Computer Oral History Collection, 1969-1973, 1977

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GROSCH:

This was, of course, pretty hard to do in war time. We had only, as I remember at that time, A-ration cards. We had very little gasoline to drive up to the Railroad station in, and so forth. Would you like to check that?

MERTZ:

Uh, it's o.k.

GROSCH:

It's o.k.?

MERTZ:

Yeah.

GROSCH:

Fine. Uh, so, you know, it involved considerable handicaps, and then of course, in typical way that all working women encountered, she had to do most of the housework. Of course, I tried to help, and not having children there wasn't a great deal to do. But nevertheless there was a house that did get dirty. We had a coal furnace, for instance, which made for internal dirt.

MERTZ:

This was kind of long to commute?

GROSCH:

Yes, indeed. We used to get down to the station called Asar(?) Boulevard, which was the last stop before Hempstead, and it was not one of the great lines, and sometimes you'd have to change in Jamaica, you know, if you got on the wrong trains, and so forth, at the

wrong time, or that the trains…So it was pretty awful. The equipment was old, none of it was air conditioned, lots of breakdowns and stuff. And I remember, very fondly indeed, a twenty-six inch snow, which I think was after the war, but before the equipment improved, which tied up the whole of Long Island for, oh, God, just days. But it was alright because it took me that long to shovel out, so I needed the time. That would have been something like 1946, or so…

Dorothy worked quite effectively at Endochemical(?). One of the things they worked on, for instance, that was interesting during the war, not as exciting as the Manhattan Project was that they were designing so to speak, the paint for the Black Widow Fighter that Northrop built. And they were the people who discovered the shiny black paint that made it harder to see this fighter from the ground in the search light beam than dull black paint, because the black paint, was specular - the shiny black paint was specular and it chopped reflective beams off in all sorts of directions, probably back to the eye of the observer, but, of course, black shiny was better than white shiny. So they developed a very shiny, very black paint, indeed, and I remember she did some of the pigment analysis on that stuff. Of course, the fact that she had been involved in the Manhattan District didn't mean anything at the time, because the very name of the project was secret and she didn't know she was working on it. All she knew that she had some rather curious uranium specimens and that they were looking for rather odd impurities like boron in it. Well, of course the boron was the fission product that would occur after the uranium had been in a pile, and would damp down the reaction by poisoning the neutron production of the thing, so that it was necessary to have a source of extremely boron free, and other impurity free of uranium before they build this stag pile. But she didn't know any of this. All she knew was she was analyzing uranium. Meanwhile, down at the Navy Department I remember a physicist named George Monk who was a well known author of textbooks in the optical field coming to my optical office and soliciting my help at selecting a periscope - submarine periscope for some unspecified use at the University of Chicago. And, of course, I knew Monk as a textbook name, and I was able to help in selecting the appropriate periscope, and, of course it occurred to me how - what a peculiar project he must be working on if he not only needed a submarine periscope, but could get one at a time when, you know, the periscope was one of the most precious single items of War production - almost next to the submarine itself. And, of course, the answer was that he was going - he was going to watch Dorothy's uranium melting down in the stag pile through this periscope. But, again, the words "Manhattan District" were not mentioned and the utter secrecy of the whole thing was preserved.

As an old science fiction reader, I must admit I had some doubts about Dorothy's uranium. I was pretty sure that somebody was monkeying around with atomic fission, because, of course, astounding stories and other magazines had been dealing wholesale in atomic power, and atomic weapons for many years on a purely speculative, fictional basis. But, one had one's own problems to worry about in those days. You didn't - you didn't go around poking at other secret elements. And you remember at the Watson Lab we ultimately did the calculations for it, and, of course, then the whole thing began to fall into place. When I was told millions of degrees of temperature, and so forth, then the fact

that it wasn't necessarily - uranium wasn't mentioned, the fact remained that obviously you had a nuclear device, or as we then said, an atomic device of some sort. But, even then, I hadn't put the - I didn't put the periscope - George Monk's periscope into the picture until after the Report was published, and it turned out that this had been done at Chicago and etc., etc.

But, you know, one got a thrill out of working in new fields, in any event. And Dorothy found it pretty challenging, indeed, to develop her laboratory skills, which had not been considerable until she came to Washington. I still have some pictures of her in the lab coat looking very harassed, indeed, at the - I think Grady's microscope of the Endochemical Corporation. And I remember she was working up something on that they were involved in, and that this was a difficult problem because it was essentially the newer kind of spectroscopy that you were looking for absorption bends, rather than for sharp metal lines in the spectrum. But, again, in fifteen years of not talking to her about it, it's a little vague, now, as to what was involved.

I remember, however, that I was drawn into it to a certain extent because I did know a good deal about optics and was working full time in the field during the war, so I went to a couple of the Endochemical parties and gave some free advice, so to speak, on some of these things. And I also remember working up a computing form to be done on a - on a desk calculator, not on a punch card machine, for the failure of absorption - I can't remember the exact details, I think it beers off, as in beer the drink, that has something to do with the addition of absorption in absorbing media or something. I worked up a little computing form with logarithms and stuff for them to use in this amino acid work.

Well, came the end of the war Dorothy decided to resign from that just about the time that we decided to leave Hempstead and move into Manhattan. So during the time that she was in Manhattan living in 22nd Street - West 22nd Street - she had time free, and she spent most of it at the Watson Lab, learned to run the punch card equipment and to wire the simpler plug board, but not to do the more extreme, scientifically oriented stuff that was involved later in the card program calculators, and even in the early days in the wiring of the complicated tabulator board. And then, as I say, she also did desk calculator work at the time when I'd given it up, trying to do…

Another thing I remember she helped me on, after the war was over, and I think after we moved into New York, was that I did a small amount of- nope, nope, not possible. This must have been while - this must have been while I was still engaged in optical work, ergo, it must have been still during the war. I did a small amount of consulting work for an ophthalmologist in New York City, named William B. Rose, MD…the problem with doing this is extremely difficult, because of the problem of holding the eye still. But, apparently ophthalmologists were aware of this problem, and for looking at the outside of the eye with high magnification they fixed the eyeball by putting a sort of an oversized contact lens on it that they could sort of hang on to…since I don't know much of the detail of this - didn't even at the time. Now the question was that if the eye could be fixed relatively motionless by this oversized contact lens - this big chunk of glass with a fitted

hole for the cornea to fit into, so to speak - if this could be done with anesthesia, and all that sort of thing, could you now look in through this contact lens and examine the retina at very high magnification - to get high magnification in a microscope system requires a rather sturdy colon of light - light coming out of the retina, diverging, so to speak, until it hits the microscope objective. If that cone isn't pretty sharp - a pretty large apex angle, the resulting power of the system will not support the empty magnification that lots and lots of fancy eyepieces and stuff might give you. But, the end lens - the front lens, so to speak, hemispherical objective lens of the objective of the microscope is only a millimeter or two in diameter. Therefore, if you have a sturdy cone of light coming from something, that objective lens must be very close to that thing, or some of the light will miss the objective entirely. And, of course, you see this in ordinary - using a hand magnifier. If you have a ten power hand magnifier you have to get right down close to the object you're looking at, whereas if you have a one or two power one, why, you can stand off eighteen inches, or so…little help. So, what I proposed to Rose was, well why don't we build a relay lens that would be very large in diameter, like an inch or an inch and a half in diameter, which would take this cone of light and re-focus it on a one to one basis to make it image out in air that you could get at with an ordinary microscope, so you wouldn't have to redesign the whole microscope objective, which is a many thousands of dollars job even in those days, and which I was not capable of doing either for reasons of time, or for experience and skill. So I designed him this relay lens which I think that he never actually had built, but we did try to patent it. And she did almost all the calculations, almost all of the retracing on that, which, of course, I was doing even with my own hands, or with other girls helping me at Ferand Optical on the Sperry gyroscope of which I was too tired of, and too busy to do during the intervening period. So she worked during the intervals of her working at Endochemical, or on weekends. She worked with an old, rented mathamacon machine, I remember, which was very much an old, German, cast iron machine still going strong after about twenty years, which was very much like the then Freedman Calculator, with the little separate ten-key keyboard for introducing the multiplier. Probably a predecessor, patent-wise, of that machine. And she worked with this old mathamacon, which must have weighed fifty pounds, and which we paid I think forty dollars a month to rent, or something, at home, to do the retracing and then I'd suggest, well, why don't we change this curvature, why don't we do this - and then she'd work on it some more, and finally we got this design finished. In the process an interesting little side light on Rose and on Dorothy - for several years she'd been - had a low grade infection of some sort that we couldn't lick. It was a fever, an infection, sometimes in the urinary tract, sometimes just general in the body. She never seemed to be able to find any medicine. This was, of course, before the great range of antibiotics that we have now. Never seemed to be able to lick the darned thing. Never seemed to be able to get it diagnosed, correctly. Some people, you know, would try and change your eyeglasses, or diet, or something like that, and nothing seemed to work. And Rose, I think at that time, thought that maybe the eyeglass solution would work, that she was having eye strain from too much computing or something. So he insisted on examining her, and she was a little nervous about this because she didn't like taking her clothes off in front of a business acquaintance, so to speak.

Well, Rose had an interesting - I suppose this is faintly libelous, but Rose had an interesting way of doing medicine. He would read in the professional journals, especially he was very fond, I remember, of the Lancet, about interesting new diseases and treatments…

MERTZ:

Uh, reading…

GROSCH:

The Lancet is the British Medical Association…

MERTZ:

L-A-N-C-E…

GROSCH:

L-A-N-C-E-T. And, I think actually, he got this idea out of Time Magazine, or something, rather than the Lancet, but anyhow, whatever he read about that week he would treat all his patients for. I mean a patient would come in, you know, with a broken arm, or glaucoma or something, but he'd treat them with the newest prescription for head colds, anyhow, because that was what he was treating everyone for this week. I exaggerate slightly, but it was-roughly speaking, it was correct. And the week that he saw Dorothy he had an enthusiasm, as I say, derived from Time Magazine or something, for a thing called "multifever". Now, multifever is one of the gruesilosis(?) diseases, and it comes, at least in Malta from drinking infected goat's milk. And so he was treating everybody - he was testing everybody for grueseler(?) this week. And it turned out that this was, indeed, what Dorothy had, so he had a great success there…(Laughter)…One of the funniest things, I think, medically I know of in my whole life. He - the chances, you know, of hitting a doctor who was enthusiastic this week about gruesilosis, especially when the guy was professionally an eye doctor, strikes me as being rather low…(Laughter)…

MERTZ:

It might be matched by two sets of twins who…

GROSCH:

Yes, exactly, exactly. Well said. I think Dorothy must have attracted some rather curious medical attention in some respects. Anyhow, there was a white cell blood count method of testing for this thing, and he did send her to a specialist who did this, and he was sending all his patients, including those with the broken arms that week, and, indeed, she had it. All the rest, of course, tested negative. And then there was some chemotherapy possible at that time. I think this was before biotherapy, but chemotherapy possible..... or something; which did, indeed, make a very great improvement in her health condition. It apparently had nothing to do with the miscarriages. But at least it got rid of this long, drawn out fever, and urinary infection, and stuff. And she was a much happier girl, there. But, it's a very strange way to succeed in diagnosing and curing an obscure disease like this one. Anyhow, she did this sort of side thing for me too. I did a few other small consulting jobs at that time, usually for rather small amounts of money. I considered several jobs at various times that would have taken me away from New York. I think one would have taken me to Charleston, West Virginia for a small optical company that was making optical glass out there. And I turned it down partly because she felt that this sort of location would take me away from science and the excitement of the big university. Of course she enjoyed the Columbia University ambience very much, and enjoyed the dinner at the faculty club, and all that sort of thing, much - really more than I did. My - I think my taste already at that time had gotten to the point where I would really rather have had enough money to go to the 21, you know, or a really swanky downtown restaurant rather than the rather stuffy old faculty club.

MERTZ:

You stayed downtown? You lived…

GROSCH:

We lived - when we were first married - let me go through our residences for you very quickly. When we were first married we had a place on McArthur Boulevard, then called, I think Conduit Road here in Washington. A one room apartment. From that, at the time that I was still at the Naval Observatory and that she had begun to work at the Naval - the Bureau of Standards - we bought a house out in what is now called Kensington. I guess it was Kensington, then, too, but it wasn't as much developed as it is now. And so she had to have a car pool to go to NBS, and I had to ride a horrible series of buses to get down to the Navy Ordnance job that I had in the old Navy Building that's just been torn down on Constitution Avenue. Then when I left the Government and went to work for Sperry Gyroscope we moved up to Hempstead, Long Island, and just before the - before housing became impossible, we bought one of the last of the pre-war tract houses up there for about $6,000 in an area near the Nassau Boulevard Station. And we lived in that for about three years. Sold it after the end of the - it must have been a little longer than that - about four years, maybe - sold it after the end of the war for about twice what we paid for it. But instead of socking that money into IBM stock, in which case I would have been a fairly rich man today, why we spent it mostly in the overly large rental for a three year lease of an apartment in downtown New York in Chelsea, West 22nd Street. This was so

that I could go to work at odd hours at the Watson Lab working into the middle of the night, and so forth, which, of course, I was accustomed to doing as an astronomer, anyhow, which gave me access to the punch card machines at the time when no one else was using them, and still avoided the necessity of driving all the way in from Hempstead to do this because, of course, the trains didn't match - the trains and the subways didn't - trains didn't match the requirement. So we fritted away that six thousand dollar profit in over the three years that we had this house in Chelsea, and ended up no richer than before.

MERTZ:

This was an apartment?

GROSCH:

This was an apartment - a two-bedroom, very, very attractive converted townhouse sort of thing, but in a neighborhood that did not, really, go back up again. It was intended to go back up, but the Puerto Rican invasion of that part of the city had begun; people were moving out of Spanish Harlem down into that area. And while I, personally, found the Puerto Ricans more attractive than the tough old Irish that had lived there before, they did clash and there was a good deal of ugliness and dirt in the streets even in those days. This is, of course, talking about 1948 to 1950, roughly. Then, when Tom Watson told me to go down to Washington we were moved in very lavish fashion, indeed, by the IBM Company using, as I remember it, their are movers, whom I'd had quite a bit to do with through Mary Novell Smith, the art collector now - moved us down to Washington, you know, wrapping each teaspoon in four layers of paper, you know, as if it was Chilini(?) heirloom. And we got a place through the intervention of Red Lamont, the man who ultimately fired me from IBM, and then Executive Vice President of IBM for Washington Affairs. We got a beautiful little row house, for rental, on - over near the Parkway, roughly Q and-Foxhall village, I think it's called - roughly Q and 24th Street. I'd have to look up the exact - the exact…

MERTZ:

Foxhall Village?

GROSCH:

Yeah, umm hmm. - No, not Foxhall village that's another…

MERTZ:

Is it Burley?

GROSCH:

No, it's an old, 1920-1930's row house development. I think, maybe, it is called Foxhall village. In spite of the fact that it's not on Foxhall Road.

MERTZ:

Half Timbers?

GROSCH:

Half Timbers, yes.

MERTZ:

…(inaudible)…

GROSCH:

Well, possibly I have the name wrong, but I never heard the name Burley before, and we lived there for six months, so I think I would have heard it while I was there.

MERTZ:

It's bounded by Georgetown University on one side and Foxhall Road on the other.

GROSCH:

No, this is on the other side of the ravine.

MERTZ:...(inaudible)...

GROSCH:

We could look across the ravine to the back of the Georgetown Observatory in the winter time when the trees were bare.

MERTZ:

Oh, well that...

GROSCH:

...Q street...

MERTZ:

...is sort of a shopping area...Foxhall...I believe called Burley.

GROSCH:

Umm hmm, could be. Well, that may have come along since...

MERTZ:

...Half Timbers...

GROSCH:

That's the place, yeah. This belonged to a rather rich Washington friend of Red Lamont's, who was putting his daughters through...

MERTZ:

That would be around '38, or so...

GROSCH:

Then it couldn't have been 24th, could it? Umm hmm. I'd have to look on the map to be really sure. Of course I have it all in my security questionnaires and stuff, you know, so I know the exact...

MERTZ:

But it's on the other side of the Georgetown University?

GROSCH:

Right, right...

MERTZ:

From Georgetown?

GROSCH:

Yes. And we loved that place, dearly. That was wonderful. The rich surgeon, or something, who owned it was putting his daughters through coming out parties, and stuff, and had temporarily taken a bigger house. After he got his daughters all married off he was going to move back into the place because it just suited his wife and himself, so he

didn't want much rent. He didn't want to fix it up. He wanted to redecorate it completely after he came back, so we had to do-some fixing up was required at our own expense, but since the rent was something like $80 or $90 a month for a place that now would cost $250, why, it was still a great bargain. And we did, indeed, paint about half the house, and papered the living room, the dining room, and a few changes like that. We bought some nice furniture for the first time.

About this time Dorothy realized, of course, that she wasn't going to get me back into science, and she wasn't going to have any children, so there began to be some tensions between us which were never, really fully resolved right up to the time of her death in 1955. But, nevertheless, we were still a good team, and she helped as much as she could in the stresses with IBM and the final departure.

MERTZ:

This is in...

GROSCH:

This is in 1951, yes. Now during this time, remember, I was President of the American Rocket Society, so that there were a good many social functions - the annual meetings, and so forth that she had to help at. And, again, she was very good at this. She - her - her clothes sense wasn't as highly developed as in some gals. She wasn't a real clothes horse, but she had good taste, and she always had very nice - very nice things, and she always looked very good at formal dinners and things like this. I remember her helping me entertain Alexander - Archibald Alexander, I think it was, who was then Assistant Secretary of the Army or something, but he was our guest speaker at the ARS annual dinner at Atlantic City or something. We had great problems of getting him from the airport, or getting him dressed and up on the stand, and so forth. And she had to carry a lot of the ...(inaudible)...she did very well, indeed.

So, she enjoyed that and she - as I say, she had realized by this time I wasn't going back to being a college professor or an astronomer again. Then when we left Washington, we lived for six months or so in a very elegant rented house - three story, old fashioned frame house at Belmont, Mass, that I rented from a professor who was going on a temporary Government mission down in Washington for a while, and wanted to sublet his while he was gone. And...

MERTZ:

...(inaudible)...

GROSCH:

He wanted to take most of his furniture with him, but to leave his rugs, and drapes, and stuff, and so forth. And I was able to drive into MIT that way. And when we left - when I was drafted, really, actually, from MIT by the GE people to start this big shop in Evendale, why then we moved to a small, rather unsatisfactory rented house in Cincinnati for about another six months. So poor Dorothy was kept pretty busy, you know, just tearing down and building up, you know.

MERTZ:

...(inaudible)...

GROSCH:

Yeah, yeah. And she was a homey type gal. She liked to have nice curtains and all that sort of thing, so you know there was quite a bit of work involved. And this kept her pretty busy. She wasn't missing the blue-stocking activity very much by this time. And then we finally found an old farmhouse - one that had been remodeled many times, and was quite modern, on three and a half acres of land in a suburb of Cincinnati called Wyoming, which was quite close to Evendale, quite close to the Industrial suburbs.

MERTZ:

Was that north of...

GROSCH:

It's north of - it's north of Cincinnati and it's inhabited largely by Cincinnati family people that live out in the sticks, and by executives of Proctor and Gamble. There weren't too many GE people - a few but not very many.

MERTZ:

So...(inaudible)...

GROSCH:

Yes, yes. A little more to the west. More on the direction toward the northwest rather than straight north. Straight north was…But Compton Road was the name of the country road that was involved. And we were very happy there for a while. By this time Dorothy's parents had visited us several times, and they came to stay with us almost - oh, for a couple of years during the three years we were in that house. And, among other things, he built Dorothy - the father built Dorothy a nice greenhouse, and she began to do very advanced gardening. She was raising primroses from seed, and things like that, which is quite complicated to do anywhere, and very difficult to do in Cincinnati. And I was in trying to clear more of the land which was more meadow than elegant lawn and

stuff - it was a very attractive - extremely attractive house. And our little idol there came to an end, really, by - before her death by the fact that the very rich man named Phillip Myers next to us decided to cut his much larger, hundred acre farm, up into a subdivision, and all of the - all of the red clay from this excavation washed down into our little brook and completely ruined all of Dorothy's garden, and so forth. So,...

MERTZ:

How long were you in Cincinnati?

GROSCH:

We were in Cincinnati four years, almost precisely. But the first year or so - the first fraction of a year was in this rented quarters, so we were really only in Wyoming about three. But I was at Evendale for four years solid. I came - I came to GE, ostensibly, to work at North Station in Boston where they had a Model 1 card program calculator. One engineer named Eugene Gettel, G-E-T-T-E-L, and five very - very fine girls - semi-professional assistants, all of whom had good Bachelors and the first non-military ones in the world. And so the thought of, you know, sending the data back and forth rather than the engineers back and forth, was completely foreign to the people who planned this in the late '40's. So, one reason or another, they had already made the decision to go to Cincinnati, and the question was timing and where to locate the computer, and so forth. For instance, in think it would have been quite reasonable to have put the computer in Lynn, Mass., even though half, or three quarters of its technical customers would have been in Cincinnati, because then you would have had access to the MIT kind of novelty, and excitement. And the co-sponsor of the machine was the median steam turbine department, which was also in Lynn, and which, from the day that I started, was - contained a large number of people who were struggling to get their own equipment, even though ostensibly they were delighted to have access to the machine. They really wanted their own in a typical organizational way. So I spent a large part of my time sitting on their heads and keeping them from conceding, as they indeed did, after I left.

So, putting it in Lynn would really have been a good idea. But I was one of the people who said put it in Cincinnati, long before I'd worked for them, and one of my main reasons was to get this man Paul Hergert to run it, because in those days there were ...

MERTZ:

How do you spell his name?

GROSCH:

H-E-R-G-E-T. He's still going. He's Director of the Astronomy Department at the University of Cincinnati to this day. And I didn't think that there were, you know, ten really competent people to put that machine in the whole United States, and he was the

only one that wasn't, you know, in line to put one in already. The others like the ones on the West Coast at North American, and Lockheed, and Douglas, and so forth, were putting them in. So you would have to buy one of them away, which seemed to me to be kind of dirty pool. Also it wasn't clear that most of them would leave Los Angeles to go to Cincinnati, whereas...

MERTZ:...(inaudible)...

GROSCH:

...Hergert was already in Cincinnati and wouldn't leave. So that was ...

MERTZ:

I gather, from what you said earlier, he was more academically oriented...

GROSCH:

Yes. His view was that he would do this if they would not only pay him a very large salary, and all the benefits of being a GE employee, but in addition that they would let him do his celestial mechanics calculations on the machine, too. And that, I think, more than the salary, or any other reluctance, killed the deal. He wanted, in a sense, to go on being an astronomer and the other. And, of course, I'd made that commitment years before, and was not that risk. But I had not considered myself for the job at all. At that time I expected to stay on at the Watson Lab, while I made the negotiations. And GE politics, trying to get them into the computer field and so forth, which involved lots of trips to Schenectady, and in Mass...

MERTZ:

Why ...(inaudible)..... ....

GROSCH:

Well, they took over the very large Government owned plant that Curtis Wright had used to build piston engines in during World War II. And this plant was available to them at very, very low rates. Moreover, GE at that time was at the height of …They were very much involved in struggles against the union. And the unions were very weak and - not weak, it's not the right word for it - but were conservative in Cincinnati. They were present and all of the factory people were unionized, but they were reasonable in the sense that they didn't have the graft and inefficiency of the Mass. unions, and the belligerency of, say, the Michigan unions. Now, I don't know how this compared with California, but I guess that the availability of this huge, really enormous plant, ready built pretty much, and mostly owned by the Government was the major factor, and the union thing a secondary one.

Also, there was the question of the length of - they couldn't move their test cells. Their test cells to test the engines remained at Lynn, Mass. And the sheer distance, remember the idea of telecommunications and all that was in its infancy, then. In fact, when I put in the transceiver lines for digital data to go between Lynn and Cincinnati, they were laboratories of their own, big computers of their own, etc., etc. But that, of course, was only in the very initial discussion stages, and just as they were going ahead, why, GE came and wisked me away, almost without my consent. So Dorothy was a little unhappy about this, and neither of us cared too much for the idea of going back to the middle west - going to the middle west. She knew it from her childhood in Sioux City, which had not been happy; and I knew it from Royal Oak and Ann Arbor, which I considered pretty deadly as, you know, geographically, although very - although Ann Arbor had been very attractive intellectually. So neither of us particularly wanted to do it, but we were both very pleasantly pleased with Cincinnati. It's a very attractive town. There are good restaurants. There's summer opera. There's many of the social advantages, are, I would say, considerably superior than Detroit, in spite of it being a good deal smaller city. It has a stability about it that attracted Dorothy. She was a more conservative person than me. And, of course, I was already beginning to do this enormous amount of traveling that I still do. And in those days it was propeller travel, so it took longer to go anywhere than it does now. So I spent a good deal of time away from home, so if there were limitations in Cincinnati it didn't matter too much.

The aerospace boys, or as we then said the airplane boys out of the West Coast, so that I was going back and forth to Los Angeles almost - literally, almost weekly. And when not that I was monkeying around in Masters Degrees in Mathematics, but were regarded by GE as sub professionals in the sense that they couldn't become mechanical or aerodynamic engineers without further training. So this group of six was running this card program calculator, largely on turbine stress and turbine performance calculations for the jet engine department. And I say ostensibly because, of course, I was hired to move that group - dissolve that group, and move it and its equipment, and so forth, down to Cincinnati as quickly as possible, and this also allowed GE to pay my moving expenses rather generously at a time when this was not all that common. Later, of course, everybody did it. It wasn't all that common in those days - for a new employee, that is. It was, of course, common for IBM or GE or anyone else to move an existing employee from one place to another. Well, after I'd worked for a month at North Station, I was an existing employee, and they moved me quite generously to Cincinnati.

The thing that disturbed us there was that, again, Dorothy had seen a chance for me to become a MIT professor. In fact, at the time that the - that Craft and the other GE people came over to pick me off and to take me away from Cincinnati, I was negotiating with Gordon Brown, the then Dean - the then head of Electrical Engineering, later Dean of all engineering, I believe, at MIT, to join the faculty, although it meant a fairly substantial cut in salary compared to the Industrial Corporation Staff, because I could see very clearly that it was the people who had faculty status at MIT that really ran things, and they were going to have if not there somewhere else in IBM, so I didn't consider myself

as being one of the available types, either, anymore than John Lowe, or Jack Stronomers(?), in my view, available.

So, that was one of the real reasons that I thought they should go to Cincinnati, and what with one reason and another they did. And my location in Wyoming was very attractive. I could buzz across in less than ten minutes from this very nice, calm country community to the Industrial ugliness of Evendale. And, of course, park inside the gates, and all the other good things that an executive was allowed to do.

MERTZ:

And what did they have?

GROSCH:

When I got there they had empty space in the basement, period. I had processed through a couple of orders, even before I left MIT, for a couple of card program calculators, Model 2, to hold them off until the 701 arrived. And, what with GE's exceptional connections with IBM, because GE was at that time IBM's largest single customer except the U. S. Government, and my own knowledge of where to push, and where to shove, why we got these - we got the first of these very quickly, indeed. In fact, it arrived, virtually the same day I did. And this, of course, was the same machine that I'd had in Washington, except it was a production version, so it didn't have quite as many bugs in it. I was familiar with the huge plug board, and intricacies of the innards of the machine, and was able to, quite quickly get Gettel and the girls to work on it. Now, of the five girls, four transferred to Evendale. One refused to come because she was deeply involved in Boston. As a matter of fact, I think she...

MERTZ:

This was the North Station?

GROSCH:

This was the North Station group. Gettel had already been committed to moving, but was not to offended at not being given the whole machine. He was a little afraid of the size of step up. He was - he felt barely able to cope with the Model 1 CPC. Well, this was because there was no one to lead him. He was actually a very capable man, indeed, and became a very experienced computer man, later.

MERTZ:

Was this G-E...

GROSCH:

This is this G-E-T-T-E-L, umm hmm. But he was primarily a mechanical engineer, and not a numerical analyst, or a computer data - in the full sense of the word. So, he didn't really want it. A man who worked with him at a little higher level named Bert Sells, who was the other one along with Craft who gave me the word that I was being drafted, was interested in thermodynamics and so forth, and he was more interested in running the machine, but had so much pressure on him to do what he was doing, that he wasn't really free to be considered.

MERTZ:

Was this Bert Sells?

GROSCH:

S-E-L-L-S. He's pretty well disappeared, I think. I don't believe you'll find him here. I'm sure he's still working for GE engine work, but you won't find him...

MERTZ:

...(inaudible)...

GROSCH:

Of course. And his - his advice was pretty much, you know, to move to Cincinnati, because he was moving to Cincinnati and he might have access to the machines. So as far as he had a part in the committee activity, why, he did so. Craft, of course, was working for Schenectady...steam turbine, and wasn't going to get the machine one way or the other. He'd just as soon come to Cincinnati as to live. He'd rather, I suppose, have had it in Lynn for..... Hmm?

MERTZ:

...(inaudible)...

GROSCH:

Yeah, but in those days it wasn't that way. It was a question of trained communications as much as anything else. Now, you could come from Schenectady to Cincinnati on the Ohio State Limited very nicely, indeed. It was a very, very attractive run. And actually to go across the Berkshires on the Boston and Albany was a pretty rough, bumpy ride. Hans very seldom took it, as a matter of fact, he ...And those were great trains. I used to quite frequently take - go to New York from Ohio on the train. You'd get on the Ohio State Limited at the suburban station, something like Silver Spring would be here, a very nice suburban station, very practical suburban station, and you'd have a really good dinner on

the train. You'd get on at 4:00, 4:30 in the afternoon and you'd have a very good dinner on the train with a couple of drinks. All expenses, of course, paid by GE. You could work in your roomette, or compartment quite effectively with papers spread out and so forth. And I worked harder while traveling then, than I do now. I tend to read a book now, you know, or think dire thoughts about things. But in those days I had a lot of routine, heavy paperwork to do. And, you know, you get off the next morning with a clean shirt, refreshed, and happy. And you could either get off in Albany, which, of course, took you to Schenectady, or you could stay on a couple of hours longer and go all the way down to New York City and be at the headquarters. It was really easier to do that than it was to go to work. Flying was all right, but as I remember it, for instance, when I first went...hell, I don't believe you could fly nonstop from Cincinnati to Boston. At most you had to stop somewhere else, like Albany, and in most cases, or if you flew to New York changed planes.

MERTZ:

Did IBM supply personnel for this?

GROSCH:

No. For two reasons: One was that I already felt that I was equipped to pass on whatever IBM had. That I had a full measure of IBM's capabilities already stored away. And where I didn't have personal ability, when I, you know, faced specifics of high powered detail, I knew who to call at IBM since I'd only been gone, you know, six or seven months, I knew who to call at IBM in the Applied Science Department and stuff. So, what we agreed to do from the beginning was, essentially, to operate with the local talent. Now, of course, when the 701 was coming there was a team trained in Poughkeepsie to go with the machine. That team had been selected before I appeared on the scene, I think. In any event it was completely satisfactory to me, in fact, it was a very good bunch of fellows. I still have their pictures and some of their names.

MERTZ:

This 701, now, you had had some involvement...

GROSCH:

Yes. I had been a sample. See, one of the things that I had over people like Hergert, and even the West Coast men like Lowe, and Strong, and Dorothy had known here at NSA, and so forth, was that I had been…in the 701 development. I'd actually worked with the machine long before it was publicly announced, so I knew an awful lot about it that the average employee - the average new employee wouldn't have, or even the average old timer ...So that was nice from my point of view and from theirs. And it was more important to be able to call up Buchholtz, or Astraham, or Nat Rochester, or somebody like that in Poughkeepsie and ask him a question directly than to have a man in

Cincinnati who was extremely able to do this on your behalf, because I knew them directly and didn't have to work through an intermediary.

Now, the man that I dealt mostly with in IBM, Cincinnati, was an interesting person, himself. His name was Farwell. And Stan Farwell - F-A-R-W-E-L-L - was the nephew of Fred Farwell who had been a very big figure under Watson, Sr. in IBM in the early days - a vice president at IBM, in fact I think, perhaps, even a - what we would now call a senior vice president, or executive vice president level. And he'd been very active for IBM in going into Latin America and a few things like that. Very distinguished guy. He later became president of Underwood, I think it was, and still later became executive vice president, or senior vice president at BCA, in which job he died some years ago. And Fred was one of the few people that apparently stood up to the old man. Apparently there were several times in which the old man and Farwell got into arguments about what IBM should do in a given situation, and Farwell, you know, said this is the way it ought to be, Mr. Watson, and then the old man would either, you know, buckle under or transfer him. And after he'd been transferred a couple of times they fired him, although not in quite as vigorously as they fired me since he was a higher level - much higher level executive. In any event, he was one of the few people that were really - that were really high up in the IBM hierarchy, and then were extruded, as I am referred by my peers - extruded somewhat more peaceably than in my case.

Well, Stan, his nephew, had already gotten a good job in IBM through his uncle's interest. I don't mean favoritism, exactly, but I mean his uncle steered him toward it because Stan was a very personable sales-type person. Then he went off to war and fought in the Rangers and got himself all cut up very badly, indeed. IBM, as you perhaps know from other interviews, ever requests a draft deferment for anybody during the war. In other words anyone who was asked to go ought to go. And, in fact, even when I was brought in from Ferand Optical to do this Manhattan District thing, IBM's condition was that they would not ask for a deferment for me. And, in fact, the Manhattan District asked for the deferment. And by the same token, the Armed Forces themselves had deferred many IBM custom engineers and so forth, a few engineers - not many, but even a few engineers at Endicott who had, for instance, worked on that magic cryptological equipment, and so forth. But in each case the request from the military and not from IBM on behalf of their people.

MERTZ:

Now, in...

GROSCH:

So Farwell didn't get-to finish the sentence - Farwell didn't get deferred, and went into the Rangers and fought right on the front lines.

MERTZ:

Uh, the connection with the 701 - what model - what number, do you know, was this...

GROSCH:

Number six.

MERTZ:

Number six?

GROSCH:

Number six. Number one went to Los Alamos; number two went to Douglas, Santa Monica; number three, I think, I'm a little vague about this - I think to the Rand Corporation. And I - we have to go back to the old share records to remember them all, now. Strangely enough, this was a very vivid thing to me at the time. I could reel off all twenty - nineteen or twenty of the machines in order of priority at the time, because it was a very much a prestige, and status sort of thing. But it's faded from my mind, now. I think I don't value it the way I did then.

MERTZ:

All right. How - now, so the first CPC that you...

GROSCH:

I had, so to speak, number zero of the Model 2's. That is, it was a prototype machine, not a real, full production machine. And then the two that I finally had in the basement of this Building 300 at Evendale, oh, I would guess that probably the one that was delivered when I arrived was probably on the order of the twentieth or thirtieth. And then the one which came later, just before the 701 came, was very much further down the line - perhaps number 100 or so.

MERTZ:

Now, how - what was the time span between the time of your arrival, which was approximately the same as the first CPC, and that of the 701?

GROSCH:

Uh, the 701 came in April of 1953, and I arrived on the scene in Cincinnati - that is, actually moved, physically from Belmont to this rented house in July or August of 1952 - '52, yeah.

MERTZ:

...(inaudible)...

GROSCH:

I went to MIT in - I went to MIT in November of '51, and I joined IBM - I joined GE in late May of '52. And then I had to fiddle around North Station, and commute for a couple of months, so it was early fall, or late summer of '52 when I and that CPC arrived in Cincinnati. And it was - it was in April, that's too soon - it was '53, but I think not April of '53. I have to look in my scrapbook to find out. In fact, if you turn that off for a minute I'll... (Tape turned off)...

O. K. The dates are a little clearer, now. The date of installation of the 701 in Evendale was in the late fall of '53, and I'm just looking, now, at a story apparently published in the Herald Tribune in New York City, dateline November the 16th, 1953, describing the installation of the equipment in Cincinnati. That would probably be a few days after it actually got running.

We had the problem of check out, of course, just as you do now with the more modern machines, and in order to check out our initial work for the equipment, we went up to Poughkeepsie. And I remember that we shared the machine time on the non-deliverable IBM 701 with Los Alamos and with Charlie Baker, and John Lowe of Douglas, Santa Monica. So we were, apparently, earlier - we were apparently ready earlier to make our check out, than the number six position would have, otherwise, warranted. We were apparently about number three in terms of check out.

MERTZ:

This concludes this side of the tape.

[End of Tape 11, Side 1]

[Start Tape 11, Side 2]

GROSCH:

I'll start by talking a little about Serehan and...

MERTZ:

This is continuation of the interview with Dr. Herbert Grosch, and it's on the 30th of March 1971 in his office in the Administration Building at the National Bureau of Standards in Gettysburg, Maryland. Richard Hertz is the interviewer. And I think that's about the .....

GROSCH:

O. K. While I was putting together and attempting to operate this Technical Computing Bureau in downtown Washington, I interviewed a chap named Bernard Sereban, then at the Naval Research Laboratory, who was concerned with the building of a one off computer who's name is no longer stored away in my memory. He actually ended up by joining IBM after I was fired, still works for them today, and, I think, was all in all a fairly important acquisition for them. I seem to remember that after a slight interregnum following my disappearance, that he took over this bureau, but by this time I was so deeply involved in General Electric affairs and all that I really wasn't watching very closely. The bureau was not a major success. It stayed at a low level for many years after my departure. I think the fanciest machine it ever had was the 650, and that was a good deal later on in time than the 1951-52 era we're talking about here. The man who actually stepped in to sort of hold the thing together hasn't been mentioned in these talks. His name was George Petri. He had a PhD in mathematics; he was one of the people that Cuthbert Hurd brought into the Applied Science Department at IBM quite early in the regime; he was a very large man, both vertically and horizontally; and he stepped in to sort of hold the thing together after I left. As I remember the personnel involved; Mayhill went back to work for Herd in New York; Rothman left the company in disgust at the operation and went out, I think, to the Rand Corporation; Libby Linberg, I forget her married name but whatever it was Libby's young husband in the State Department got another assignment, so she would have left in any event, and her departure had nothing to do with my - with mine. So that the thing went from five or six rather highly motivated people to essentially zero in a period of just a couple of weeks. As I remember it Rothman and Mayhill agreed to stay on for a little while until Petri sort of got some replacements. But it's somewhat faint in my mind, now, as to whether they managed to keep the work with the Air Navigation and Development Board, and the Federal National Homeowners Association, and other things that I had started on.

Well, that's kind of dull history at the time, and really the fact that it accounts for the presence of Bernard Serehan in the business is probably as important as anything. The collection of books that I insisted on having, and the rather nice little office that we put together, I suppose dwindled away during intervening period, but I haven't any idea, really, what happened to them. The important thing for me was that I was unhappy at leaving IBM, although not really too surprised. After my interview with Tom Watson in New York the year before, when he gave me the alternative of leaving at that time or going down to Washington, I couldn't avoid the feeling that I wasn't exactly on the upward track, you know. But I still didn't want to leave and I was clinging to the door post right to the last moment, as they chopped my fingers loose, one by one.

MERTZ:

We might want to back track just a little bit, and at this point you were married?

GROSCH:

Yes, umm hmm.

MERTZ:

You haven't said anything, I think, about your family life.

GROSCH:

Sure. Be delighted to do so. Well I was still planning to be a young astronomer without any particular specialty in the computer area. I had gone to Harvard College in the summer of 1939 to a thing called the Harvard - something like Square - Howell Square Conferences in Astronomy and Astrophysics. These were sponsored by Howell Shapley(?), then Director of the Observatory. They were, at that time I guess, the most prestigious way that a young graduate student or young professional astronomer could spend his time. They were, of course, extremely cheap. The idea of the summer tuition in those days was $500 - was $50 or $100 instead of the $500 or $1000 that they have nowadays. I'd gone in 1939, and I had enormously enjoyed the experience. I'd stayed at Harvard Yard in Weld Dormitory, which was the first time I'd ever been in anything quite so…or of such faded elegance. There was a fireplace in my room, for instance, and a place to have both coal and wood in the clothes closet. All in all it seemed to me a very fine way to live, and I've regretted...

MERTZ:

...(inaudible)...

GROSCH:

Weld, W-E-L-D. It's one of the oldest ones. They used it in those days for freshmen, and then you graduated from there into more elegant and more modern dormitories later, or joined the various houses further out of the Yard. But, for me it was a wonderful experience. The lectures included some - by some outstanding, internationally known astronomers like Yon Oort -O-O-R-T - of Blyden Observatory in Holland, and were very impressive to me. Well, I enjoyed it and I think they enjoyed me, too. It was the first time that I met Fred Whipple, for instance, although I had been competing with him with my old professor Maxwell in the orbit computing days that I described several lectures back - several interviews back. So, the social aspects of it were not to be sneezed at. I had saved

enough money from my big fellowship to do it this first time, and I was all in all very happy with the results.

Well, in 1940 I decided to come back again, but my fellowship was not quite as rich that year. I wasn't quite so sure of the financial problems involved. You realize this is a time in which one traveled by Greyhound Bus, and where, you know, fifty cents per day was the most one could expect to spend, except on lavish occasions. So I wrote Shapley, with whom I'd-had become friendly on, you know - cat may look at the king basis the year before. And he gave me a small fellowship which covered the tuition in this 1940 Howell Square Conference; and also gave me a thing called the Dowes Institute Lectureship, which was essentially to give a public lecture one night to a non-professional audience...

MERTZ:

Which institute was it?

GROSCH:

Dowse - D-O-W-S-E, I believe. It's a little - the spelling's a little vague in my mind at the moment. These were the first non-Michigan awards I'd had, so I was quite proud of them. And, indeed, the total amount is something like $100 involved, probably made the difference in my going or not. The two courses that I had an opportunity to take in these conferences were the best that I had had, not only in those two years, but in many respects in my whole academic career. There was a magnificent one on celestial mechanics, by the late Derrick Brower, who was then...

MERTZ:

Bre...

GROSCH:

B-R-O-U-W-E-R...

MERTZ:

O-U? That's right O-U-W-E-R.

GROSCH:

A man who later formed this triumvirate with Wallace Eckert and Gerald Clemenson, had so much to do with large scale computing of celestial mechanics data in the late '40's and in the '50's. And this was my first opportunity to study under Brouwer, and I also - more to fill in than for any other reason - took a course in optical - essentially advanced optical operation theory from a wonderful old man named James G. Baker, then a member of the

Society of Fellows at Harvard, and later to become the designer of the Baker -None cameras which were used so successfully for optical - as the optical arm of the space program, and probably for many years was the world's greatest optical designer, although this promise was only just beginning to become evident at the time that I worked with him.

So, it was a wonderful summer, intellectually, and the other thing was that there were, for the first time, some opportunities to meet some girls at this meeting. And the two in particular that I had an opportunity to meet, I met on a double date with Frank Edmonson, who was then an associate professor in astronomy at Indiana, but a young, lively type who had left his wife behind him while he spent the summer at Harvard. And we had a double date with two girls, one of whom - named Kitty ...(pause)... I can't remember the last name, now - later married a young astronomer named Gerald Cromes, so her married name now is Katherine Cromme - Gordon, Kitty Gordon, Katherine Gordon - now Katherine Gordon Cromme. Jerry was, during the war, one of the...

MERTZ:

...(inaudible)...

GROSCH:

K-R-O-A. Jerry was a Milwaukee boy who was already quite well known in the field of photoelectric measurement in stars, and during the war became quite active in the development of photoelectric devices for military purposes, such as infrared scanning devices, telescopes to see the enemy with in darkness, and that sort of thing. He was not, however, attending this meeting. Katherine ended up dating Edmondson, and the other girl with the party was a girl named Dorothy Carlson, who was from the staff of Mt. Wilson Observatory, the semi-professional staff of the Mt Wilson Observatory, who had saved her money to come to take that course from Cecilia Payne-Gaposchkin the famous woman astronomer who...

MERTZ:

How is her last name spelled?

GROSCH:

That's a good one. Payne, P-A-Y-N-E - Gaposchkin - G-A-P-O-S-C-H-K-I-N, roughly speaking. You'd have to look it up to be sure. Cecilia was an English woman married to mad Russian named Serhan(?) Gaposchkin, and she was extremely well known at Harvard and Internationally for her veritable star work, and Dorothy had been doing some work with assisting astronomers at Mt Wilson in the area. So she came work with Cecilia, and I came to work with Derrick Brouwer, and we got into this double dating situation with Edmondson and Katherine Gordon, and had a very, very wonderful time.

This was somewhat expedited by the fact that Katherine's parents were quite rich, and she brought a very handsome Packard 120 convertible with a rumble seat, believe it or not they still had rumble seats in those days- remember, this was the summer of 1940. And she was bored with driving it, and I was mad for driving nice cars in those days, so normally Dorothy and I sat up in front while Edmondson and Katherine were in the rumble seat. And it's a very luxurious and pleasant experience. Well, I got very deeply involved with Dorothy during that summer, although these conferences were only a matter of - I think, six weeks long. And we kept up a very ware (?) correspondence after she went back to Mt Wilson and after I went back to Michigan. And that Christmas - which would now be six months later - this is the Christmas of '40–'41, I saved my money again, very diligently, indeed, and took a Greyhound bus trip all the way out to California to get engaged.

MERTZ:

...(inaudible)...

GROSCH:

Uh, I did, indeed, and just as I like to travel nowadays in rather circuitous routes to see more of the countryside, why I took a long route. I remember it took me five changes of Greyhound express buses to get out there, with changes in places like Memphis, Tennessee, routes through Arizona and all sorts of things. Well, it was a great thrill for me. It was, of course, the first time I'd crossed the country. Later on I did a great deal of American traveling for recreation and for speech making, both. But this was the first time I'd ever gotten beyond the Mississippi River, which I had reached in Minneapolis when I was going up to see Allen Maxwell in the summer of '37 or '38 I think it was. This was the first time I'd crossed the Mississippi, and the first time I'd seen great mountains, although I'd already become very much interested in reading about mountain climate, and mountain adventures. I'd never actually seen one with my own little eyeballs before. The Berkshires, which I had crossed on my way to Boston weren't the same thing at all compared to the thrill of seeing the Colorado, and the California mountains. So the whole thing was a very glamorous adventure for me. I stayed with Dorothy's parents on El Moline Street in Pasadena. And, of course, I'd already been in correspondence with Mt Wilson's professional staff, because Seth Nicholson had been one of my people that I'd consulted with in trying to decide what to pick for a Doctoral Dissertation. So Nicholson welcomed me as well as Dorothy's bosses, and we spent, for instance, one rather carefully chaperoned night up in the Captain Cottage up in Mt. Wilson looking out over the city lights all the way out to Catalina, as one could do in those pre-smog days. And we went to Griffith Planetarium with one of Allan Maxwell's old buddies was the lecturer. It turned out that he had just come back from breaking himself all up on an attempted climb of Mt…in Mexico, so this reinforced my mountain interest. And all in all in was a very great winter, and I remember it very vividly, indeed. But, of course, the most important thing was that I got engaged to Dorothy. And we decided that we would try and get married the next year, if the economic and scientific circumstances permitted. In those

days the idea of getting married before you finished your college work was a very difficult and hazardous one. And, although I was, by this time, two and a half years into my Doctoral Program, why it still wasn't the sort of thing that one undertook very easily. My parents were - would have been if I had, you know, discussed it with then in advance - I'm sure quite upset about it, although by this time I was essentially independent with my fellowships, and so forth, and didn't receive much financial support from them, still I was very close to them...

MERTZ:

What kind of background did you have...(inaudible)...

GROSCH:

Her parents were Swedish - Swedish American. They had both been born in Sioux City, Iowa. The generation before that, however, had been immigrants from Sweden. And like so many people who lived in the Middle West, and a substantial proportion of them, in fact, were Scandinavian, they had immigrated to the California - Southern California area some years before. John Carlson, the father, was a - in essentially the same line of business as my own father, cabinet making, elegant house construction sort of thing, I mean in the sense of millionaire's houses, rather than in the sense of tract houses. He had attempted to be an independent contractor and had not been very successful. In fact his last job before retirement many years later, was as a architectural draftsman for J. C. Penney while they visited us in our home in Long Island at a later date.

So, there was considerable propinquity. Not only did Dorothy and I both have some European orientation to our parents - mine English, hers Swedish - not only did both the father work in essentially the same trade, but we were both in varying degrees professional astronomers. She had taken a Masters Degree in Mathematics - no, I think she had taken a Masters Degree - a Bachelors Degree in Mathematics at the University of California in Berkley, where she'd been quite an academic success, and then had gone on to take a Masters Degree in Astronomy, or some courses toward it, and I think was trying to finish it, partly by this Harvard thing in 1940. It's a little vague in my mind, now - she died in 1956 - so it's been fourteen-fifteen years since my memories of her childhood and academic career have been refreshed, but as I remember it she didn't quite complete he Masters Degree, and thought of going into teaching, but since she lived in Pasadena, even at the time when she was going to school in Berkley, she was very much attached to the Mt. Wilson kind of tradition, was well aware of it, and when she took some courses in astronomy and liked them at Berkley, it gave her the idea of applying at Mt. Wilson for assistantship. And she was immediately - I don't know if immediately, but she was hired as quickly as one could be in those days of Depression, and short funds, at some ridiculously low salary, like, you know, $1000 a year, $1200 a year for a twelve month period. And, among other things, she worked - she measured some of the original red shift(?) plates for Hubble. Now, a job for a semi-professional in those days, and many of them were women - there was no - in fact there was a preference for women in such

posts, but no prejudice, really, against them rising through. I mentioned Mrs… In addition to that there were several other very distinguished women in American and overseas astronomy; Julie Venhansen(?) in Copenhagen, Enie Cannon(?) at Harvard, and many other people, so that there was an open door for advancement for women in astronomy that probably wasn't duplicated in any other major science at that time.

MERTZ:

Which part of astronomy would you say ...(inaudible)...

GROSCH:

Well, I think that they tended to follow up from a semi-professional career, most of these people. I never knew Professor Venhansen in Copenhagen. I mentioned her name because she was in celestial mechanics and orbit computing ...(inaudible)

MERTZ:

...(inaudible)...

GROSCH:

But in the other cases...

MERTZ:

...in computers...

GROSCH:

No, not so. In the other cases most of the people that I knew of - most of the women that I knew of came to this from doing exhaustive measurement or examination jobs. Amy Cannon, for instance, probably looked at more objective prism specter with her own eyeballs than any other person that's ever lived. There was also a very distinguished woman, who's name escapes me at moment, at Yale who never really got all the way into being a full professor, and teaching classes, but who was more responsible for the famous Yale Astrographic Catalogs than any other single person. And she just spent her whole life, you know, sitting in front of the giant measuring microscope in the carefully air conditioned, insulated room, just measuring, measuring, measuring, measuring, measuring. The sort of work that would now be done by mechanical devices, but you remember I mentioned the...

MERTZ:

...astrophysics, for example, ...(inaudible)...

GROSCH:

Either the old fashioned kind, the measuring of stars ...(inaudible)...old fashioned astronomy - allies and celestial mechanics, and so forth. Amy Cannon's objective prism worked, for instance, in astrophysics. It serves to characterize the spectral types of the stars, and so forth, and is much more important to stellar constitution studies, and evolution of the stars, and structure of the galaxy, and so on. So either one of these - there was no particular women's prejudice. I did not have the feeling, though, through the whole length of the fifteen years of our marriage, that Dorothy ever really was driven to become a full professional. She would have welcomed the opportunity to move up, gradually, but I think if I had left at Mt. Wilson and she had not married anyone else, she would have ended up like this very fine woman at Yale, who's name escapes me, a full member of the staff and all that, but not have become a teaching professor, or a very famous research name.

Anyhow, she was well liked. She had been a member...

MERTZ:

...to Mt. Wilson...

GROSCH:

Oh, we drove up in - I think I borrowed the Carlson family car, as a matter of fact. Another similarity was, as I remember, that both families had the same 1937 Plymouth car at the time - same model 1937 Plymouth car. And, in fact, when Dorothy and I got married my dad got a slightly better car, and gave me this old one as a wedding present. That was the car that I had to drive all through the war because, of course, we couldn't replace them in those days. I ended up with 126,000 miles on it, or something like that, and held together with bailing wire.

Well, to get away from the astronomy thing, and to get back to personal things, we did get engaged. We continued to correspond, of course, very vigorously, indeed, having had become deeply involved that winter. I went back to Michigan and worked feverishly on my Doctoral Thesis, and Dorothy began to put together the typical middle class girl's trousseau, you know, and all that sort of thing - quitely, because she wasn't announcing to her friends what she was going to do until we knew what it was. But she began to get ready, and to soften the blow and so forth to her parents, who were, of course delighted, because by this time Dorothy was, oh I guess about twenty-eight - I'd have to think a little bit about her actual age. Yes, it would be twenty-eight. She was born in 1912. She was six years older than I was, and, while a very attractive girl, had not really thought an awful lot about getting married in the last two or three years. She'd assumed that she was going to be an old maid as many of these astronomical characters were. In fact, she often used to laugh that she got her money's worth a hundred times over from her trip to

Harvard. Which, in a perfectly feminine sort of way, I suspect, was one of the main reasons that she went to Harvard, that she hoped to meet some attractive young man, and so forth, as well taking the course from Cecelia Payne-Gaposchkin.

Well, anyhow, I began to think about how I might tackle this. My Thesis was going along well, as far as the research aspects of it was concerned. I was punching away on my Marchant Desk Calculator day and night.

MERTZ:

Was this the same one that...

GROSCH:

Same Marchant ACT 10M that we talked about a few interviews ago, yep. I was doing enormous numerical integration, carrying this eight satellite of Jupiter step by step, back in time, trying to get back to a period where there was another cluster of observations and, hence, get a very accurate orbit, indeed, which I did succeed in doing, and which I was very pleased about. Now, there was obviously a matter of six months work writing the Thesis up, and getting it approved, and making editorial changes, and so forth. And it seemed to me by this time that since I wasn't going to need the desk calculator, that I wasn't going to need the hours, and hours, and hours of steady grinding that one had to do to do this calculation, that I was in a position to take a full time job and - you know, in the evenings, and especially, with Dorothy's help, do the writing of the Thesis and take my examinations at a later date. And, of course, I was somewhat driven to this by the desire to get married. Also, I'd been in that situation a long time. Most people, I think, very happily go to a different institution for their undergraduate and graduate work. And, looking back on it now, I wish that I had realized that I was as well favored academically as I was. If I had known it I would have gone to Harvard as an undergraduate, and to Cambridge, or overseas or something for a Doctorate, and been a very much more - a very much more well traveled, and intellectually broadened boy when I got done. But, to come out of Royland High School in Ann Arbor was a pretty broadening experience in itself, and I guess I didn't realize that I might have made a still larger jump if I had tried to do so. And in 1934 one took even a jump to Ann Arbor as being something pretty hard to...

MERTZ:

This is about seven years...

GROSCH:

So I was in Ann Arbor for seven solid years, that's right - seven solid years, from September of 1934 to May of 1941 to be precise.

MERTZ:

...(inaudible)...

GROSCH:

Right. And all during this time I had extensive help from the Astronomy Department in the Graduate School, and even from the Undergraduate Scholarships. So, it was an - you know, a difficult thing to decide to leap off onto another course. I wish, now, that I had done so. But I suspect, long term, it wouldn't have made a great deal of difference. I think I would still have been drawn into the early days of the computer business. Every astronomer interested in that sort of thing was, and I suspect I might very well have been an astronomer in any event. It still draws me to this day. I still find it one of the great intellectual adventures of ...(inaudible)...and a very pleasant and happy one. It's also an interesting point, aside from the main crux of this interview - it's also an interesting point that many of the people who stayed essentially in astronomy, with an interest in computers and an interest in the space program and all that, but nevertheless primarily continued to consider themselves as astronomers have arrived, essentially, at the same point that I've arrived at, by an entirely different route. Fred Whipple's an outstanding example. He's head of the Smithsonian Astrophysical Observatory. He's got a lot of computers at his disposal. He's been on as many fancy committees in the space program as I have, and so forth. And, you know, he got there by remaining an astronomer, where I got to the same point by going way off into that field, and circling around in entirely new areas. So I wouldn't be surprised that even if I had gone to Chicago, or Harvard, and if I had gone overseas for a Doctorate that I'd be sitting right here in this office saying the same thing to you today as I am.

In any event I did see clearly the - I did feel, very strongly, a desire to get out from this seven year rut, happy though I was at the way I'd been treated. No resentments or anything. I'd been treated wonderfully, wonderfully well, especially in view of the fact that even in those days I was kind of a hard head. I mean, I'd take a course and drop it, or accept a low grade in a subject where I thought the professor wasn't, you know, wasn't drawing my attention away from my private research. And this is not the way to get ahead in large Graduate Schools, anymore than, you know, saying…is a good way to get ahead in a bureaucracy or a big corporate organization. So I was obviously a hard headed kid, even in my teens, not just after I got, you know, squeezed a little bit by large corporations. So, with one thing and another, I can't help saying that they treated me very well, indeed, were very sympathetic with this hard headedness and independence. Professor Curtis, especially, the head of the Observatory, I think, looking back on it now, I must have been a very different type than the usual graduate student that he dealt with ...(inaudible)...wonderful. He gave me a couple of his books, even though it broke up his library, and in general it makes me remember him very warmly to this day.

Well, anyhow, what with the marriage business and coming to the end of the Thesis, and the seven year rut that I wanted to get out, I began exploring on the possibility - writing

letters as one did in those days. There was no real market. At Astronomical Society meetings one would - one picked up hints that possibly they were hiring an instructor at Dennison, you know, or something like this. But it was nothing like the organization of hiring and job exchange that came along later in all fields. It was very much more low-key sort of thing. You couldn't come right out and ask, bluntly, you know, you got a job for me sort of thing. That was very déclassé indeed. But one wrote carefully phrased letters, and asked about opportunities, and I turned up a couple of small colleges, and in the process assessed what the going rates were, and it turned out that the going rates in those days were about $1600 a year for a ten-month year for a young instructor in astronomy with his Doctorate in hand, or very, very close - guaranteed, I guess is the word I want to use - if he was not married, and maybe $1800 if he was married, with very little prospect of summer work. He'd have to do something else in the summer, or work for nothing.

MERTZ:

...(inaudible)...

GROSCH:

Right. At the same time it turned out that there was a Civil Service Examination for the U. S. Naval Observatory kicking around. I took that and got best or second best marks in the country. And that paid $2000. So, by accepting that I was not only able to get one of the highest paying, $2000 a year, jobs that were in the astronomy profession that year...

MERTZ:

...(inaudible)...

GROSCH:

That was a twelve month year, right. Very true, so per month I wasn't getting anymore, but at least a guaranteed something to work, so to speak. So, having been offered and accepted this, I then, you know, didn't of course telephone Dorothy, because, you know, that might cost two or three dollars for a conversation in those days, but I wrote her an impassioned letter saying, "Let's do it, Babe", or words to that effect. And we decided to - that I should, indeed, leave Michigan before the very end of the academic year - I was no longer taking any course work, so it was a matter of giving up one month's scholarship payment, or something like that. I moved down to Washington on my own, and that she would then come independently from California in August, and we would get married in Washington privately. We had - neither of us had any relatives there, so it was just a matter of standing up with a couple of friends before - well, at the church.

Well, we did this, and she announced her engagement and had the usual showers given her by her personal friends, and her business girlfriends at Mt. Wilson. And I shook hands all around at Michigan with very good feelings on all sides and left for Washington. I got a little basement apartment on 29th Street, here, in a rooming house knowing that I was only - a basement apartment, a basement room - knowing that I was only going to be in it a few months, began looking for an apartment for myself and for Dorothy.

MERTZ:

This is in 1941?

GROSCH:

This is 1941, May - June - I must arrived here the about end of May. She came in early August, and we were married, I think, a week later. I remember she stayed at the - at the Professional Women's Club College - American Association of University Women, I guess it is. It used to have a little sort of a clubhouse down in downtown D. C. someplace. They've since torn it down, I believe, down near the Army-Navy Club somewhere. And they had a few rooms, and she had one of those, and she...

MERTZ:

YWCA...

GROSCH:

Yeah, I guess that's - yeah, right. Anyhow, she stayed there for about a week. We were married by a clergyman in the August down on Lutheran Church, because that was her family religious connection. She was somewhat more religious than me, but I was a confirmed achiest and she was, I guess, probably an agnostic, but she wanted to be married in her parent's church even if she wasn't, herself, very keen on it. And I remember that we were feeling very solemn about this sort of thing, kids taking it more seriously in those days than they did now, until it turned out that the minister, who's name I forget now, looked startling like W. C. Fields, so we had some problems keeping a straight face through ceremony.

Standing up for us were a young couple named Mikesell, M-I-K-E-S-E-L-L, whom - I think the male member of the family, Al Mikesell, had been a fellow student of Dorothy's at Berkley, as I remember. He later became head of the Time Service at the Naval Observatory and has - still has a fairly distinguished, although rather low keyed career in old fashioned astronomy here in Washington. He got quite heavily into electronics, crystal parts and that sort of thing, but at this time was a rather lowly Junior Astronomer in the Time Service, as I had become a Junior Astronomer, as I said in earlier interviews in the Astrographic Division. And his wife, who was quite pregnant at the time - they

were Quakers as I remember it - his wife and Al stood up for - Mary I guess it was - Mary and Al stood up for us, and that was our total wedding party, just the four of us.

Mikesell, I remember, had the authooloing(?) interesting experience that later that summer, that when his wife went in the hospital to have the baby, it turned out to be twins, which wasn't always as predetermined then as it is now. Moreover, in the same waiting room was another man who's wife was also unexpectedly having twins, and it turned out that his name was also Mikesell, spelled the same way. These two were not connected in any way, it was a rather unusual name, and it turned out to be a front page story in the Washington Star or something. The extreme degrees of coincidence Anyhow, that's about Al's greatest claim to fame, except that he and Mary, as I say, being Quakers were great on kids and ended up by having many of their own, and adopting a raft of others besides.

MERTZ:

Be ye fruitful and multiply.

GROSCH:

Being fruitful and multiplying, and I'm all in favor of the adoption business because they're there already, I'm not so sure I'm all that keen on the fruitful business.

Dorothy was not able to have children. We had made several attempts after a year or two in which we avoided the problem, and in each case these were terminated by miscarriages, one of which was almost fatal. It was a complete separation of the placenta, that if she had not managed to stagger off to a neighbors and be carted off to the hospital, might have resulted in her bleeding to death. That was in 1950 - oh, '49 or '48 or '49, after we'd been married about eight or nine years. And after that we didn't try anymore. So, that accounts for the fact that that first marriage did not result in any children.

Dorothy was an enormous help to me in a professional way, as well as very pleasant person to be married to. She used to work at the Watson Labs in the evening, and the early morning with me running cards through the punch card machines, running the desk calculator for me. She extended some of my Doctoral Thesis calculations later on when I was too busy supervising other people's computing work, and finding out about new computing devices, to do it myself, and she was reluctant to have me give it up. It was her general feeling that after the war was over, and, of course, she'd been very happy to support me in my optical stuff and engineering work during the war, but it was her feelings after the war was over that I should go back to being an astronomer, and she never really relished the kind of, if I may use a word I use occasionally nowadays in informal talks, the hotsy-totsy kind of business that the computer field, even in those early days, promised to be, her ideal was that I should be a dignified astronomer or Astronomy Professor, and, you know, occasionally...take a foreign trip, or make a speech, why fine, but it should be a highly intellectual and professional sort of thing. And the

kind of thing that I do so much now, charging around talking about social responsibility and saying the programmers are dopes, and so forth, was quite foreign to her preferences. But this is so far back in time that there wasn't much conflict between us on this matter in the Watson Lab days. Most of the people that you met were pretty intellectual at that time. The raveling salesman sort of thing wasn't nearly as far advanced in technical use of computers as he is now. He was out raging around selling punch card equipment, all right, and the ancillary services there unto appertaining, but his victims were, you know, Bank Presidents, and department store owners, and so forth - more Government bureaucrats, indeed, and the Internal Revenue Service, and so on. He wasn't pestering the astronomers and the aerospace boys and stuff much, because there wasn't much obvious trade there.

One of the things that was interesting about the marriage was the way that Dorothy adapted to it without really approving of the IBM thing. I remember, distinctly for instance, that the famous IBM family gathering at the Waldorf Astoria in late 1949, or whenever it was, the famous weekend at the Waldorf, that she and Hillef Thomas' wife were the only two women in this entire aggregation that didn't have, not only - that didn't have any kind of hats, let alone the magnificent, fancy, brand new hats, you know, purchased for just that luncheon. And these two gals huddled together with their husbands at a fairly remote table while the flossier types were bouncing around down below showing off their new headgear. And yet, Dorothy enjoyed IBM and fit very well into those many quasi-social events that the old man offered to the central staff. I still have a big photograph - big panoramic photograph taken in the hall of stuffed birds at the American Museum of Natural History, showing me in black tie, and Dottie all dolled up fit to kill sitting next Lillian Chalm, and a very distinguished man who, I believe...

MERTZ:

Is that...

GROSCH:

Uh, Schumm. She was a sort of successor to Rose Leach as the IBM frontwoman.

MERTZ:

...(inaudible)...

GROSCH:

Leach, yes, Rose Leach was the vice president of IBM and front woman, just as they had some very distinguished looking Negro and Jewish employees who were prominently featured in all group photographs in those days. And, Rose Leach left the company to get married, because in those days it was impossible for a married woman to work for IBM, and Lillian Schumm sort of took over as front woman. She was Personnel Director for

World Headquarters, and this photograph in the Hall of Stuffed Birds shows Lillian sitting with a very handsome gentleman, indeed, who used to frequently accompany her as her date on these company affairs. Lillian died recently, much to my sorrow. She was a very pleasant and considerate person. As it turned out she'd been married to this guy all this time...(Laughter)... He showed up at the funeral as, you know, as Mr….so to speak. I shouldn't laugh about such a tragic thing, but it made my day when I read the article, nevertheless, because Lillian had taken the obvious path out, and though it's interesting to see a devoted IBM one, and a personnel director as well doing this. And married the guy quietly, was presumably Mrs., whatever his name is, this whole time.

Well, Dorothy used to laugh with me at the IBM idiosyncrasies. She was a little impatient at some of the more extreme sales efforts, but I repeat, adapted very well as long as I didn't get too corrupted myself. Nevertheless there was always a small amount of pressure there with the family with why don't you go back to being a real scientist again. And when I made the - even if it could be, say, in the computer field, rather than necessarily all the way back to astronomy, or half way back to optics. And whenever I got too egregiously involved in public relations gesture, why she would scold me about it. But it was a quiet pressure, and when I went to General Electric from MIT, and she got involved in General Electric managerial-family sort of thing, which also controlled a considerable amount of the family life of senior people, she adapted quite well to that too, in fact, was very popular, indeed with the other managers wives, and very cooperative in the social life that was required. So, it wasn't an inability on her part to adapt to it. She was very good at entertaining, very good at doing the things that contributed, but I think that down underneath there was always that feeling that it would have been nice if I could have become a staff member of Mt. Wilson and we could have explored the universe together, so to speak.

MERTZ:

Well, then, she was with you - she had other jobs I think you - during your...

GROSCH:

Yes. Now during the war it was, of course, patriotic for everybody to work. It wasn't just a matter of the finances, but there was a shortage of skilled people. So many of the young men, of course, in spite of the draft exemptions that were possible, were taken away. So she used a rather novel ability of hers. When she had come here to Washington in '41, and we had gotten married, she wanted, as I got through with my Doctoral Thesis, and her share in the thing, the typing and the - some small amount of calculations, and so on, dwindled, she wanted to keep busy, and after all a one room apartment with no children, and war pressures as well forced one rather closely in this direction. She had many connections through her Mt. Wilson seniors with Dr. William F. Meggers, M-E-G-G-E-R-S, here at the Bureau of Standards - or I should say at the Bureau of Standards - it was not here in those days, but in downtown Washington, Connecticut and Van Ness. Meggers was the world expert in what you might call very fundamental metrological

spectroscopy, who later - he later defined the length of the meter in terms of the wave length light for the first time. But he was also involved in astrophysical standards. It was his laboratory work which the astrophysics people at Mt. Wilson and other people used to calibrate their spectrographs and to make their fundamental measurements. So, Dorothy introduced herself to Meggers with appropriate formalities from the Mt. Wilson end, and was offered a job as a sub-professional, as they used to call them in those days. I was an P1, and she was an SP, something, I don't remember what it was because she didn't have a doctorate or - and didn't have a degree in Physics, and this was Physics rather than astronomy that she was doing. Learned a good deal about instrumental spectroscopy at the old Bureau of Standards Plant. Now, one of the things that she did was to work on the spectroscopy of uranium, and of course the reason for this was that Meggers was one of the people who, way down beneath the scenes, was working on the material for the stag pile in...

MERTZ:

What kind of spectroscopy was this?

GROSCH:

Well, this was what they called a mission spectroscopy rather than he new fangled kinds of absorption stuff, and so on. You take a chunk of metal, for instance, and draw an arc from it and look at that arcare flame with a spectroscope, and you get the hard lines - the hard lines of the solid material.

MERTZ:

They didn't use defraction...

GROSCH:

Yes, they did use defraction ratings, although she became accustomed to doing both grating and prism spectroscopy. Astronomers, at that time, used almost entirely prisms. But...

MERTZ:

This is...

GROSCH:

... were coming in and, in fact, Meggers great friendly rival was Harrison of MIT, who was building a ruling engine to make better defraction gratings at MIT during the war. Of course Harrison was a very great figure in NDRC, the National Defense Research work, and I wouldn't be at all surprised if it was Harrison who drew Meggers into the

Manhattan Project work, But that is - I'm sure that's all a matter of record, now. I simply don't know it.

Anyhow, Meggers and his people, including Berden Scribner, who is on the staff essentially in that job, now, here at Gaithersburg...

MERTZ:

Is this ...

GROSCH:

B-O-U-R-D-O-N first name, Scribner, who's brother, I remember, was the Pan Am Captain, one of the early transatlantic pilots in flying books. Scribner was a bachelor at that time and he and his brother cut a pretty wide sway. He's a conservative married man, now, but I envied them at the time.

Well, anyhow, Scribner and Meggers used Dorothy as an assistant in some of this uranium spectroscopy work. And because astrophysicists are more commonly involved with these oddball substances like uranium, and so on, in the stars, then the earthbound spectroscopes were, in those days, why she was an apt assistant. So she developed considerable knowledge in the day to day laboratory work of spectroscopy, and after we moved to New York and lived on Hempstead, Long Island while I worked at the Watson Lab, she used to commute in on the Long Island railroad, often on the same train that I did, and she worked as an Industrial Spectroscopist for the Interchemical Corporation on 45th Street in Manhattan.

MERTZ:

Right. At this point I think we probably had better change sides.

[End of Tape 11, Side 2]

[Start Tape 12, Side 1]

(Initial statements very low and inaudible)

GROSCH:

O.K. Let's go back to the 701 and the prototypes and so forth. The original 701 concepts were developed with a machine called the Tape Processing Machine, which I have a whole set of the original order codes, and so forth, handwritten by Nat Rochester, and I guess blueprint oscillated, I guess, for reproduction method. And those date back to the very late '40's, I think, with the beginning concepts of the order code and so forth that

probably date to something like the fall of 1949. Well now, the actual circuitry to be used, which was closely related to the circuitry in the 604, which by that time was very much a mass production electronic calculating machine in the punch card line. The circuitry for this was tried out in a machine which looked like a couple of 604's sort of back to back, but attached to which was a version of the Williams tube electrostatic memory to provide the working store of the machine. By this time IBM, unknown to me - unbeknownst to me - had negotiated a license with Williams and Cobol patents at the University of Manchester, and so forth, and was designing this memory in the Poughkeepsie Laboratory. And, true to the old man's theory of secrecy in these things, even though the Watson Labs was very much involved in the decisions to go into the computer field, I didn't know about the Williams tube memory until six months, at least, after the licensing had originally had been made. John McPherson, of course, whom I've mentioned several times, knew all about it, he was probably one of the signers of the agreement, but it was his duty not to talk about it to people that weren't supposed to know, and there was no reason for me to know so - in his judgment - so I didn't. Well…

MERTZ:

How many of these storage tubes…

GROSCH:

Uh, the pattern of the system, as I remember it now, was thirty-six of these tubes, each one of which stored one bit of the later traditional thirty-six bit word. It was his decision to adopt the thirty-six bit parallel structure that really created thirty-six bit word size for the whole industry. It was not the size of the UNIVAC I, whose word size at the moment as a matter of fact I can't remember. The UNIVAC I was a different character oriented machine.

Now I got a little off the track because I meant to say that the original idea of the Tape Processing Machine, the thing that Rochester and Buchholz, and others were working on - the intention of that was to build a commercial data processing machine which would revolve around the passage of material back and forth on magnetic tape. Remember at this time magnetic tape was not a success any more than this kind of tape that's being used on this recording machine, at least plastic based was not. The magnetic tapes to go into the UNIVAC were on thin ribbon of metal, phosphor bronze as I remember, some kind of bronze, and the SEAC here at the Naval - the Bureau of Standards, which was working by that time and using a wire - a magnetic wire recording, not too successfully. So this was a - this was really quite a novel adventure; one of the more novel adventures that IBM has done, because in general they don't tend to do ultra-extreme pioneering with technology in their production machines. That is, they do wonderful things, of course, in their laboratories, but in their…

MERTZ:

(inaudible)

GROSCH:

Well, now by this time we were in the 1950's. We're still not up into the '51, '52 era yet but we're in the 1950's. Now about this time the pressures of the Korean War caused a fairly substantial number of people to call on IBM for better calculating equipment than the card program calculators. Now the 650 was under development, too, at this time, but it had been slowed down because Cuthbert Hurd and others on the sales side of the company had said that there wasn't much market for it. So you have the curious circumstance of the card program calculator, a very old fashioned way of doing calculations, Frank Hamilton's prototype 650, and the 701, a very much more powerful machine using a very elegant kind of storage and so forth, all coming on at really the same level of development. Simply because the CPC was a lot smaller, and used a lot more existing equipment, it came out first. But the prototype stages, the three machines were almost abreast of each other in three different parts of the of the continent. It's startling to think of it in those terms. Actually, they could have aggregated the CPC Model 2 and brought out an earlier version of the 650, a hundred times more powerful machine with magnetic drum storage, of course, as distinguished from the funny old electromechanical counter storage of the CPC just about as quickly. But they would have had to put many millions of dollars into Hamilton's engineering development, which they were not at that time prepared to do.

Now, at the same time pressures were being exerted by people like Jack Wolsor(?), the President of Northrop Aircraft, Donald Douglas, Jr., of Douglas Aircraft, and so forth - I don't know all the names - on a man named Ned Douglas, Vice President Edward (something) Douglas of IBM to produce something a lot bigger - the 650-they had probably been leaked information on - they probably had seen Hamilton's prototype, and the answer was, you know, we need something a lot faster than that, a lot more flexible. We need something that doesn't depend on optimization of programs by their location and relocation on the surface of the magnetic drum that's only turning at mechanical speeds.

MERTZ:

Did you - you mentioned that you were exposed to the 701…

GROSCH:

Well, this was…

MERTZ:

…CPC, but how about the 650?

GROSCH:

Uh, the 650 I saw only by accident. I wasn't supposed to be in on that argument either, but - and the fact that Hurd was - in fact if I had been in on it officially. I might very well have countered Hurd's prediction that they were only going to sell fifty of them in the life of the machine, which is what suppressed the speed of development. Because I would have said hundreds, I mean I would have laughed at it - but, of course, I was always wildly optimistic compared to most of the rest of them. But it happened that not only did I know Frank Hamilton very well, personally from the SSEC days, when he was the Chief Engineer of Math, but that his physical operation at Endicott was right across the hall from where the CPC Model 2 was being debugged. So while my engineer, Carl - I gave his name in an earlier interview - well, my…engineer, Carl, was rewiring the back of the control panel for this CPC Model 2, I was across the hall talking to Frank Hamilton about his adventures, and being privately and probably rather illegally shown the prototype equipment. Now, in those days, for instance, they hadn't been able to make a good…at IBM, believe it or not, and as a result the prototype in the machine that I saw had been made by winding magnetic wire - that is wire capable of being used in magnetic recording like Alexander's SEAC wire - on the surface, very tightly winding it on the surface of a substrate on the drum bracing it in position, and then grinding it off to make a metal surface. Almost unbelievable technique. Of course, not a bad one for a small drum, but this was quite a large drum, so it was very difficult to do. Now, by the time they came along with a drum like the 701, they developed plating, or probably more likely licensed plating techniques from others, and were able to make a good coated drum which worked very well with a wire wound ground off drum which dropped, of course, and was dropped also from the 650. But I repeat, I probably wasn't really supposed to know about the 650, and I certainly didn't know about marketing arguments and so forth. When I talked to Hamilton about it, his view was you know, it's going to be a couple of years before we'll be able to go ahead with this. But I didn't say to somebody, you know, why can't I have the 650 down in Washington. Moreover, I hadn't the faintest idea when we'd get enough business to fill it, anyhow. I wasn't even using the CPC Model 2 on a full shift when I was fired.

Now, of course, the G. E. situation was entirely different. G.E. had work backed up to the rafters waiting for these machines, and the reason they were only using the Model 1 CPC one shift was that its limited programming capabilities were such that they couldn't tackle much of the jobs at all except on a sequential basis that they didn't have time to set up. Well, now Douglas responded to the pressures from people like Jack Northrop, and so on, to build a much bigger and fancier machine, technically engineering oriented in other words, not a UNIVAC I which was intended from the beginning to be at most a statistical and in general a business data processing machine - insurance and actuarial work and stuff was as fancy as they expected it to get. This was intended from the beginning to be very advanced work in nuclear design, in - I'm sure, for instance, that there was pressure from Los Alamos. I don't know who brought it but I'm sure that there was pressure from Los Alamos - I would guess that it was Bradbury, but I don't know that for a fact. So, Douglas, then, took these pressures upstairs to Tom Watson, Jr., and Tom Watson, Jr.

persuaded his father to put twenty four million dollars into this program, and to change the Tape Processing Machine concept over to a thing called the Defense Calculator. And GE in the person of the then Vice President of Engineering, Harry - I don't remember his last name, but it'll come to me later, or you can look it up in the GE records. First name, Harry, he was then Vice President of Engineering - corporate Vice President of Engineering - he signed an agreement with Ned Douglas of IBM to be one of the first recipients of this Defense Calculator. And I have a copy of that agreement in my files.

MERTZ:

(Inaudible)

GROSCH:

This would be - now we're into '51. This would be in the early '51, at the time that I was just getting ready to leave the Watson Lab and go down to the - to go down to Washington.

MERTZ:

…Washington…

GROSCH:

Um hmm, to go to Washington, um hmm. I - again, I have a copy of that agreement in my files somewhere. I could tie it down to the nearest day, but I hope it's not important - the fact is that it was in '51 sometime, and it was still called the Defense Calculator, and this was the - presumably about the sixth order they received. It doesn't necessarily follow that they might not have received a Los Alamos order, for instance, later and still put it ahead on the grounds of national necessity or something like that. But it was not later than the sixth order, you see, certainly. IBM, at that time, followed a policy of first come, first served in order to avoid having things thrown into military priorities. So, you had to line up as early as possible in order to get machine. And, in fact, I've had to practically snoop on joggle board of that Model No. OCPC Model 2 to keep somebody like Northrop Aircraft from taking it away from me. Of course I never had a formal order for it since I was inside the company, it was sort of an engineering model that somebody else could have sold for external money if I hadn't, as I say, snooped on the joggle board at night.

Well, about the time that public knowledge about the possibilities of this Defense Calculator came around, why, the Watson Lab began to hear about it, too. So - and, of course, I know Rochester, and Buchholz and these people, and taught in company classes in Poughkeepsie that they and their men had attended, and so forth - evening lectures on, you know, - this is back to the time when you gave lectures on the binary number system. Seriously. Not that Rochester didn't know all about it, you understand, but a lot of the

young engineers working for him were still counting by tens, believe it or not. It's hard to believe, but that's the way 1951—1950--1951 was. So I had begun to pickup information about this, and along about the time when there was this feeling of sort of dislocation that I was either going to get fired out of the Watson Lab, or go down to Washington, I think more as a gesture of - a peace making gesture than anything else, Cuthbert Hurd asked me if I'd like to join two or three of his people, who were sort of a team of visitors up in Poughkeepsie cooperating with Rochester and Buchholz, Sera - Asterhed(?), and so forth - be careful about that - we started the actual physical construction of the Defense Calculator. So I said, "Of course, I'd be delighted to", because I wanted to be up there on occasion, anyhow, to see what was going on, and I had friends there, and I had nothing better to do while I was waiting for Endicott to produce the prototype Model 2. So I spent, on and off, I would say two thirds of my time for about two months in what was then called the Kenyon Estate. It's now the guest house in Poughkeepsie, out in the country, beyond the Research Laboratory; and I shared a - I shared upstairs quarters, which meant that I actually had my space in a rather spacious, converted bathroom, in which they'd taken out the water closet, but had left the tub, for instance, and I had a big five-drawer file cabinet sitting in the tub for lack of space, oriented so that you could pull the drawer out lengthwise, you know. And I was sort of a simulated customer. The Cuthbert Hurd boys - Bill McClellan, who is still active in the company was one of them, were working rather more closely with the hardware men, simply because there needed to be further work on assembly programs - it wasn't really even assembly lane, which in those days it was really just relocation, you'd write your program in terms, and then relocate it into absolute location by little simple software package of a few dozen instruction, which no one had written yet. Rochester had written one, but it was for a non-existent machine, and to write it for the actual working machine required some software orders, and people like McClellan who had written really non stored program programs for the SSEC, some five or six years before, were pretty senior people in this. So while they were doing that, they needed someone…

MERTZ:

(inaudible)

GROSCH:

Relocation of instructions…The instructions were written in symbolic form, and then somewhere along the-line you had what we would now call a very elementary assembly program that would put two or three of them together, and store them away in memory at specific locations, and then print out an absolute program.

MERTZ:

Did you, in your instruction, have to give… (inaudible)

GROSCH:

Not - only in the sense of relocating blocks - relocating the first instructional block of instructions. But the fact that they had to be in an adjacent block is, presumably, the way that you wrote this symbolic program. You assumed that they would be adjacent, but you wouldn't know where in the memory they would be, in this tiny little assembly program, stuffed them all together so as not to waste any space. That way you didn't have to worry about how big they were when you started. Because, you see, you only had about a thousand and twenty-four words. And instructions were half words, so as to contain a maximum…

MERTZ:

Was this machine - this was…

GROSCH:

This was '51 at that time.

MERTZ:

Was the machine working?

GROSCH:

No. Oh, no. There was no machine…

MERTZ:

(inaudible - both men talking at once)…

GROSCH:

This is two 604's back to back with a prototype Williams tube thing was installed on the first floor of the Kenyon Estate. And you'd go down and tinker with it. But the programs that you wrote for that would not work on the machine that was being designed down in the - further down the road. So, that's why I say that the ones that Rochester had written worked, but they didn't work on the 701 - the worked on this test assembly. And the…type fellows were re-working such software to work on the production machine. And as they did this the Rochester's and the Buchholz' kept revising the instruction bank, so…(laughter)…Just as they do nowadays in a much more elevated all software fashion. And I have, you know, versions of these instruction manuals - about five of them altogether from the earliest handwritten ones, up to the final fashions.

Well, they wanted someone to write an application program to run on the 701, and in a moment of madness I decided to write one which was essentially double accuracy

arithmetic, 72 digit - 72 binary digit arithmetic; add, subtract, multiply, divide, exponentiate sort of thing, you know…

MERTZ:

…(inaudible)

GROSCH:

…double ordinate. Unheard of in these days. None of these machines had floating point, for instance. So, not only was - I think was writing a floating point program, or maybe Rochester, himself, was, but I was writing the double accuracy program. And, of course, wasn't anywhere near done. I can see, now, looking back on it with the experience that I've gained since that I was looking at something that was like a two man-year effort for a very skilled programmer. And I was never a very able programmer in the sense that McCracken(?) or Don Shell(?), or people like that were exceptionally good. And I spent, I suppose, a total of two and a half months on it of the three month period. But I still have much of the paper…(inaudible)…And it was, you know, attractive looking. It's just that I only scratched the surface, especially as in a typical astronomer's way I didn't try to generalize and write an interpul(?) accuracy program instead of the double accuracy program, and that would have been a five year thing. I had done a good deal of work in numerical analysis in the intervening five years at the Watson Labs, so that I had very clear ideas pertaining to the difficulties of this, and the multiple accuracy thing attracted me because this man Hans Craft had tried to use hundred digit - hundred decimal digit accuracy, or had planned - I guess I got my sequences wrong - had planned to use hundred decimal digit accuracy on his enormous compressible flow steam turbine problem, if and when somebody built a machine that would be able to do it. In fact…

MERTZ:

…(inaudible)…

GROSCH:

…he was eagerly awaiting the 701 to try this out.

MERTZ:

…(inaudible)…

GROSCH:

He had a problem that resisted - in which nature was resisting solution. You know, some problems in physical science where no matter how clever you are, you always run into difficulty. This is because it's an inherently resistant problem. It's something that nature

doesn't want you to solve. And in his case compressible flow in lattices of…right up near the supersonic - the sonic limit in steam apparently was one such problem. Nature just didn't want him to solve it, and no matter how cleverly he transformed the problem mathematically a new difficulty would always arise. He had an extremely able applied math consultant named Stefan Bergmann, S-T-E-F-A-N B-E-R-G-M-A-N-N - no, I think just one "N", who was, I think, at Stanford University at the time, who transformed the problem enormously into the entirely different kind of problem in integral representation, complex plane, and a so-called…plane. Beautiful, beautiful work, but it turned out that when he did this that the normal difficulties of lack of convergence in the series calculations that people had tried to do by hand were transformed into lack of accuracy that you ended up by having to subtract two enormous numbers, one from the other and a little itsy bitsy difference was what you were looking for. So, every time he'd do this he'd get no answer, and finally decided that he had to do it to about a hundred places to be really sure of getting anything left over. And, in fact, many years later - not many years - some years later he did this on a 705, which was character oriented in which would do hundred place calculations if you waited long enough, just from a simple instruction. He did this on the 705, and in fact lost, you know, ninety places of accuracy as the theory had predicted, and got some results but I believe, even there nature defeated him and they never used his design method to make turban checks(?).

Anyhow, that counter, to a certain extent, for my interest in multiple accuracy, and intuple? accuracy arithmetic, because I was thinking of Craft, and of course it was a gorgeous problem to program. It would have been useful if I could have done it. Anyhow, it gave me real feel for how the machine worked, what instructions were powerful, and which were to be avoided. And when I went to Cincinnati, of course, I had all of this in my head, along with advanced copies of all the literature, which none of the other customers had because, you know, it was illegal to hand it out until everything was all cut and dried in typical IBM fashion. So I was a good buy for G. E. No wonder they wanted to draft me - brief cases full of secret documents that they couldn't get any other way. And, of course, Los Alamos probably had a set, too, sort of leaked to them by IBM. But Douglas, for instance, where John Lowe was a very close friend, knew very much less about the machine than I did, right up to the point of formal announcement. Then, of course, they got everything I got and we were all even again.

Now, the name 701 was not given to the line until much later. Formal announcement - I'd have to look into the books to - perhaps even - no, I don't think it would be in here. I'd have to look into some of my old IBM files and stuff to see when IBM 701 formal announcement was made. But It was not very long in advance of the first deliveries, because they were really reluctant to make it. The old man didn't really want to make it. And Tom, Jr. really twisted his arm and got - twenty-four million bucks was a lot of money for a development project for any company in those days. And you've got to remember that IBM was in the sub-billion dollar a year category, now. Not much so, but some so. As against seven or eight billion now.

MERTZ:

Did you have any opportunity to talk to the 650 people at this time to find out (inaudible)

GROSCH:

I'm sure I did. It isn't clear in my mind anymore. My guess, off hand, would be that I must have gotten a lot of inside dope on this from people like Bill McClellan, and Ted Codd - C-O-D-D- and other people from applied science that came up on and off, or worked full time in Poughkeepsie on the 701 on behalf of Hurd, because they knew both sides of the story, and they knew the 650 turn down. They were in day to day contact with customer who wanted something smaller than the 701, but bigger than the CPC. So they must have had all this information. And I'm sure that the relations were very friendly, very intimate. We were together all day, and ate together at night, and stayed in the same hotel, and, as I say, they'd been in my classes and stuff, so we were all good, close friends. But I don't really remember, nevertheless, the specifics of it. I remember the general feeling that the 650 should've been pushed faster, and that there was going to be, you know, there was going to be more demand than they were able to satisfy.

MERTZ:

(inaudible - both men talking at once)…

GROSCH:

These were under Hurd. These men all reported to Hurd.

MERTZ:

Well, they, in effect, disagreed with Hurd's view of the situation…

GROSCH:

Uh, I think they didn't know. Cuthbert was a relatively conservative person. He wore the almost black IBM navy blue suit, high buttoned vest, starched shirt collar kind of costume. I doubt very much that these youngsters were really completely privy to what his opinion was. They knew what the company policy was, which was not to announce the machine for a while, and not make very many of them, but I don't know that they really knew that it was Cuthbert's decision. And, really, my knowledge is really second hand. It's simply one of those things that's brooded about in the trade when Hurd was shelved some years later. I've never asked him point blank if it was so, and I never really talked to someone like, for instance, John McPherson, who must have been present at the conferences where the decisions were made, and said to them, "Is it so?" It's generally thought to be true in the trade, but even to that you can't be absolutely certain. But I know this, that many times when there were complaints about lack of deliveries on the 650 in, for instance, parts of GE after I got my 701 several other people, of course, later got 650's

and they would complain bitterly about this. Hurd always defended it on the grounds - now this is first hand knowledge - I mean I actually heard him do this - defended it on the grounds that there really wasn't much market for this kind of equipment, and, you know, the company only went into this reluctantly. Now, he might have been defending a Watson, Sr. decision that he objected to, but he did defend it at a time when everyone sort of agreed it was wrong, I think, indicates he probably had been…(inaudible)…Because he was the key focus of all the technical computing work in the whole country at that time on IBM equipment, which was, as always, most of it. UNIVAC was not used much for scientific work, and even the data processing work didn't go ahead very well in the early days. And anybody in the aerospace industry that wanted to do more computing automatically either asked Hurd to come out and see him, or came to New York and saw Hurd. So Cuthbert really was the key person.

I was quite jealous of this, but I think, looking back on it, that I didn't have the company loyalty, or the sales orientation that it had taken to do the job Hurd did. I think he did, actually, a very good job. I think he lacked a little long-term vision that I might have supplied, but for IBM needs at that time - what they really needed was a bang-up salesman that had a technical degree, and an understanding of atomic energy and aerospace and stuff, and he was that. So he paid his way a hundred times over. Perhaps did better than I would have. And certainly got into a lot less fights with top management than I would have, of course.

MERTZ:

But, this particular decision was not exactly one of his more - if it were…(inaudible)…

GROSCH:

Oh, I think Cuthbert made a lot of bum guesses, but that was a big bum guess. I mean, you're talking about a difference between many hundred machines, and something like fifty, and it's typical, of course, that any hardware development that it only has a certain viability. You bring out a machine two years late, and you've cut two years off it's rental period, and off the total number of sales. You can't stretch it out two years more at the far end. And that's especially true as there began to be a good many competitive machines for the 650 at a time when there still wasn't much competitive equipment against the 700, the UNIVAC I, and later on the ERA equipment being about the only things that really put much of a crimp in IBM's push. So what you saw on the one hand, was IBM recovering the lead in the big computer field, which, for a time, they could…very easily have lost Eckert and Mauchly and Remington Rand. Recovering that at the same time where they were really being challenged in the medium sized business machine, when they should have been dominant. When they should have - so, it was - I'm sure there were some very strong feelings about this in the more intelligent part of the company. I'm sure the old man, Watson, Jr. and many others were very about this, indeed. But it's guess work. I wasn't actually present when anybody got dressed down. And, moreover, I was

then working for GE, so, it wasn't that I was being ignored or passed over. By the time these decisions were really…

MERTZ:

You were simply out of the picture.

GROSCH:

Yeah. I was out of the picture. I was a customer. I was a very valued customer, and very much of an insider customer, but nevertheless, I was on the other side of the fence. When I went to Poughkeepsie for a 701 check out they used to assign people to follow me around to make sure I didn't see anything. I couldn't open a closet door without two IBM salesmen suavely appearing, suddenly, and saying, you know, "Nothing in there. That's just a washroom" sort of thing, you know. Of course, what I was looking for was stacks of, you know, of manuals or blue-prints or something, I would have stolen them without the least hesitation.

MERTZ:

Of course, they were rather aware of this.

GROSCH:

I don't know, they knew I knew a lot more closet doors to open than the average person, so that's - In fact, for a good many years in the early days of the machines, and of the sharer invitations and stuff I was a valued member, because I knew where more bodies were buried than the average member and I could tell them how to put pressure on IBM, what specifically to ask for, or what man might know if you could get him off and give him a drink somewhere. One of the advantages of being early in the game, of course. And then, you've got to remember too that in those days very few people were leaving IBM. Now, of course, it's regarded as a great reservoir of skilled talent. But in those days, once at IBM always at IBM. And a few people like Fred Farwell and myself that left the company-were extruded from the company, were - well, I must say, regarded as pariahs, not only by IBM but by a great many IBM customers on the ground that who would leave IBM. Now, in my case, when I became a valued customer, instead,-GE, of course didn't care about this - when I became a valued customer, instead, why I restored a great deal of my viability in the organization because practically no one at that time was entitled to say "boo" to the customer. I could always put on my customer hat and get in where I couldn't get in…

MERTZ:

(inaudible)

GROSCH:

Exactly, exactly…

MERTZ:

(inaudible)

GROSCH:

Yeah. But of course I had a lot of friends in the outfit that would have quit, too, if they'd had the gumption. And, so they were always friendly. So I had good - I had good always had good sources for fresh information.

So, what would you like to talk about, now? Shall we go on with the 701, or go back to G.E., or Fred Farlow…

MERTZ:

(inaudible)

GROSCH:

Umm hmm, umm hmm. Well…

MERTZ:

What - in this regard I assume that, from what you've said, that John McPherson would be one of the…

GROSCH:

Enormous reservoir of information.

MERTZ:

of being an insider at the executive level…

GROSCH:

Yes, yes, yes. I think that the best interview you could conduct would be to make about a hundred tapes out of John McPherson. He must know everything. But he probably won't talk very much.

MERTZ:

(inaudible)

GROSCH:

Discreet, discreet. And he's completely honest. In fact, one of the reasons that John never rose higher in IBM, he's got a bad habit of blushing when he tells a lie. He'll lie for the company any day of the week, but, oh, he gets all red in the face while he does it, you know, and that just…Very conservative, but a great guy. Maybe after he retired, but I don't think so. He's deeply loyal to the company - deeply, genuinely. He probably won't ever say "boo" about them if he lives to be 118 year old. But I sure wish you could do a true panning(?) operation on him, because he really knows, he really knows. And the further back, the more percentage of what when on he knew, because that for a period in the late '40's there he was really the key guy. He was the only one in the top management of IBM that knew boo about computers. Watson, Jr., of course, talked very bluntly about him, very glibly about him, now, but he didn't know computers for toggle switches when he came back from the Air Force - from the Army. And it took him some years to learn. But McPherson knew - he knew about magic, he knew, you know, about late '30's. He used to edit the funny old book called Pointers, which was a collection of tips from the users on how to wire ordinary, old fashioned electro-mechanical punch cards machines. They used to issue them in loose leaf form, and every once in a while they'd bring out a - a couple of times in history they brought out a bound volume. And he wrote a lot of it himself. So this takes him back into the late '30's. (inaudible)…great source…

Well, McPherson was certainly involved with, for instance, the priorities of the 701 orders. For instance, he would be a person that would be quite far inside the national security agency's security, for instance. Of course, in those days it wasn't even admitted that such an agency existed. I don't know what they worked on, let alone where they were or anything like that. This is all pre-publicity, pre-defector, pre everything. And I'm sure that John had all sorts of high level clearances there, and was aware of their needs in terms - specific terms, because, of course, he did in the magic thing, which was the same field, ultra high secrecy sort of thing during the war. Now, that meant that when people like Dottie Bluement (?) at NSA came trotting down the road in the late '40's to represent NSA at - to represent "the federal government", which is usually what their badges said, or Washington, D. C. at one of these conferences on how to use the CPC, or how to use ordinary punch card equipment, why they would talk - undoubtedly talk in fairly classified terms from the first about their needs. And…(inaudible)…

Now, somewhere around this part I ought to mention those conferences. Did we ever get, in any of these interviews, about the three or four conferences that were held in Endicott in the late '40's. You know about them, I think.

MERTZ:

The ones on application…

GROSCH:

Yes. Scientific computing forum, '48, '49, and two in 1950, and one in '51, I think.

MERTZ:

If I'm not mistaken they came out…(inaudible)…

GROSCH:

That's right. Much to my annoyance. Yeah, yeah.

MERTZ:

(inaudible)…Hurd was at one time…

GROSCH:

Yeah. I was editor of the first one, and Hurd managed to get it away from the Watson Lab and edited the rest of them.

MERTZ:

Well, actually…(inaudible)…

GROSCH:

Nope.

MERTZ:

…didn't this go back to an older series of publications that dealt with applications…

GROSCH:

Uh, applications of punch card machines, but not the scientific problems. There was an educational forum, for instance, in '46 at which I gave a paper. But that didn't pre-date me, that post dated me. And then, there was one, I think, in 1940 or something that was about hospital problems or something. I have copies of some of these.

MERTZ:

Much earlier…

GROSCH:

That's right. But the flavor…

MERTZ:

…(inaudible)…

GROSCH:

Well, it depends on who you're talking to. If you talk to a guy that was a professional editor type, who was concerned with continuity of publications, he'd take that viewpoint. If you talked about somebody who was interested in the content, I think he would say that the '48 one was really a pioneer adventure. Certainly when Eckert set it up it was our idea that it was something very new, the word scientific had never been used before, for instance. And the kind of people who came were very different. The '46 one that he and I participated in was essentially a bunch of guys doing educational statistics and stuff, on which we drafted a couple of rather novel papers. I did one on harmonic analysis for instance, which is certainly not something you do in educational statistics. And Eckert talked about his celestial mechanics work before the war, and the sequenced punch card machine at the Thomas J. Watson Astronomical Computing Laboratory. But the rest of them were all how we compute means, and standard deviations test scores. That is demonstration of the test score machines, for instance. Now, all of a sudden in '48 you have an entirely different program with papers on all sorts of "hotsy totsy" scientific engineering work by people from the aerospace industry in Los Alamos, and the National Bureau of Standards, and so forth. So I would say it started in '48, and I'm not just boasting when I say that. Then Hurd took it over and managed it for a few years, and, as you say, the title changed every year, and then after a while they quit putting it in hard covers, and then after a while it sort of slipped away, and they started doing many more…

MERTZ:

(inaudible)

GROSCH:

That's right. Von Neumann was on one of the two that were held in 1950. They had two that year. One was for big shots like Von Neumann, and one was for working stiffs(?). I went to both of them, but I think gave a paper to only one, and I think didn't write it up in such a fashion that it was included in the book. I've always been hard put to put things down on paper, and when I was editor of the one in '48, I had to do my own, so it's there. But in 1950 I think I did one on Primary Approximations, and also announced, verbally, the -what's now called Grosch's Law (inaudible)…but it didn't come out in print for a couple of years thereafter.

And the interesting thing about many of those meetings was that just about everybody in the whole country who was using a big IBM machine for scientific work sent a representative. And, in fact, they sent just about everybody, you know, every senior person working; because, you know, in a place like Douglas there'd be maybe three people who were really above the Indian level, and the whole thing was that all three of them would come if they could get permission. IBM usually tried to limit them to two people from an installation, but sometimes they'd sneak in a third or something, and that usually would be most of the people. So, in one room, you'd have, perhaps a hundred people who constituted about eighty percent of the scientific and engineering computing experience in the United States; the rest being either ERA customers, or one off machine customers, or people who were just too poor or too embarrassed to come. Now, of course, there was a great mass of people on the outside who wanted to get in, but they didn't have equipment yet. They were planning to be engineering and scientific computing people some day, but they didn't have 701's, or 704's, or even 650's.

MERTZ:

(inaudible)

GROSCH:

Yes, but it was mostly a matter of waiting. You see, in those days the thing was that the waiting list was the thing. It was eighteen months to get delivery, and really, you know, it took you that long to get ready unless you were an unusually able person.

MERTZ:

I don't know whether there was anyone there from the Whirlwind…

GROSCH:

This is right. And when I got to Whirlwind, of course, I used to complain about this, but it was really more that they had their noses in the air than that IBM wouldn't have let them in. If Forrester, for instance, had wanted to come, they would have made him the guest speaker, I'm sure, without any hesitation because someone like McPherson would have been very much impressed - undoubtedly knew what was going on, and would have been very much impressed. But there was no question that there was a strong rivalry, as you say, between these various factions. But the point was that of the various factions only the UNIVZC - ERA Eckert and Mauchly boys - were doing anything in mass production. Whirlwind was the one machine, and, in fact, of course about the time that you might have said that, you know, interest in going to these meetings began to drop off IBM was picked by Forrester to be the manufacturing agent for the Sage(?) Computer. So after that the relationship got very tight, but, of course, by that time the meetings that we're talking about were over.

MERTZ:

(inaudible)

GROSCH:

Handley(?) used to go, sure, sure, sure.

MERTZ:

(inaudible)

GROSCH:

Sure, sure. Well, I have a few of them up here, but most of those boxes over there are all full of that stuff, too. All the early conferences, - I went to not only the IBM ones, but most of the others, which I try to today, in which there's thousands of them now any large percentage.

MERTZ:

But it was possible…

GROSCH:

It was possible to go to all of them. We used to have the so-called Eastern and Western Joint Computer Conferences in the spring and fall, and I suppose I haven't missed more than two or three out of the whole series of thirty or forty of them. We didn't have international meetings, to speak of, in those days. International operations were very limited…

MERTZ:

1950 is getting very close, also, to the very first joint…

GROSCH:

Yes. The so-called joint computer conferences were started by a committee of the IRA, the AIEE, and the ACM, of which I was an early but not pioneering member, not a charter member. And it included people like Nat Rochester on behalf of the IRA, and John McPherson on behalf of the AIEE, and Dick Hamming on behalf of the ACM, and people like this. Now I was a charter member of the ACM, and I was a member of the JCC on behalf of ACM. But later on…

MERTZ:

…the very first meeting of this organization…December of 1950…

GROSCH:

I think that's right. I remember a Western joint computer conference, that was not by any means the very first one, in the Los Angeles area, at which Vance of RCA talked about analog computers, and I made some cutting remarks that I later regretted. That would be about 1953, for instance. So, if you assume that that was the third year - second year, maybe, that makes the Western joints go back about 1951, and the Eastern joints had started before that…(inaudible - both talking at once) but they were very small.

MERTZ:

(inaudible)…to chair these conferences, the chief engineer

GROSCH:

That would be…

MERTZ:

(inaudible)

GROSCH:

Oh, Norm Taylor. Yes, I guess that's right. He was strong in the AIEE, which was the founding father of the thing. That's right. The AIEE in the early days had an Executive Director type, not for the whole thing, but for the computer activities which, the name escapes me-but he was a very much older man, and because his maturity tended to have a somewhat disproportionate effect on the operation, they finally managed to unload him in the mid '50's - sort of retire him. But because he was senior, and had been through the wars in other areas of the electronics field, earlier, he tended to make the AIEE more important than its younger members…I had become a member of the IRE somewhat around this time, sort of…There had been an IRE computer - electronic computer committee in the '48–'49 era, and they put out a bibliography, and Bob Serell of RCA, that's the IRELL of RCA, actually produced this bibliography. And I did so, and sort of got co-opted onto the committee until somebody discovered that I wasn't a member of the IRE, so they sort of made me a member in spit of myself. I had to pay the dues and all that, but I didn't really have to do much in the way of applying. And they made me a senior member right away, which I've been ever since without ever doing very much…(inaudible)…

MERTZ:

Well, we probably…

GROSCH:

O.K., O.K.

MERTZ:

Unless there are some things that you want to mention in connection with your participation in the organizations…activities such as the ACM.

GROSCH:

No, the ACM charter meeting, as far as I was concerned, we held at Columbia University in '47.

MERTZ:

Possibly some of the…

GROSCH:

Um hmm, um, hmmm. There had been some correspondence and telephone calls, and I think a meeting in Ed Berkley's office in Boston - at Berkley's office, period. I'm not sure it was in Boston. He worked for Prudential, I think, probably in New York.

MERTZ:

Is this before Berkley…

GROSCH:

Oh yes, long before. He was far down the insurance company for non-production on the UNIVAC I, as I'll tell in a minute. And Ed had sort of gotten this group together, but the first time that outsiders, other than half a dozen friends, had gotten together was in 1947 at Columbia University over in the Chemistry Building, as I remember of the journalism - Wallace Eckert, I think, provided the room. Berkley presided, and Charley Concordia(?) of G.E. was there, I remember. I'm not sure but I think, perhaps, Hans Craft was also there. Concordia later became the Treasurer of ACM for many years. He's dropped out now in retirement. Oh, lots and lots of figures for there. But many of the downtown IBM'ers were not represented. I can't remember, for instance, that any of Cuthbert Hurd's people, or Cuthbert himself was present. It tended to be more university professor types, not very many data processing types, and not very many engineering types, mostly because they didn't know the meeting was being held. Concordian was an exception. And

we got together and agreed on a charter, essentially, for the society. And I believe that the sign up list from that meeting and from the few people who would have come but were prevented by local circumstances was about seventy-eight members. Berkley still has a copy of that, and I should think our twenty-fifth anniversary, next year, will undoubtedly produce a more formal version of this. The problem with that association was that there wasn't really any history of meetings and stuff to support it as there is, now, of course, so that the first real meeting was at Aberdeen Proving Ground, and the next year, in '48, at which time Von Neumann, for instance, presided at one session I remember - John Mauchly talked about the magnetic tapes of the ENIAC - I mean of the - later became the UNIVAC Mark I, and so on.

MERTZ:

Right. I think that at this point I'd better reverse the tape.

[End of Tape 12, Side 1]

[Start Tape 12, Side 2]

GROSCH:

Now the IBM people tended to join the IRE, as they were the young people that had been recruited from MIT, for instance. This included, of course, Havens, Lentz, and Walker that I'd spoken about. Although Walker was the AIEE type, as you might expect being more conservative, and more old fashioned. Again, Rochester and Buchholz and those who were working on the 701 later in Poughkeepsie, were all IRE members; but McPherson, the fellows who did the relay calculator work for and so forth, tended to be AIEE types. But none of them gave much of a…to the ACM. They joined much later, and some of them have not joined to this day. ACM tended to be numerical analysts, and especially college professor types of numerical analysts. And that group has maintained a pretty strong position in this society to this day, going from - whatever it was - seventy-eight members to twenty-five thousand. It's still that little coterie of very high brow college professor types, that has traded…

MERTZ:

…(inaudible)…

GROSCH:

?Well, this is right, but you see the thing that really gets you year after year is the control of the society by the council and by the editorial board. And the editorial board tends to be dominated by people who want to have hotsy totsy publications, and these are generally college professors, because publish or perish. And the council, year after year, is dominated by regional representatives who tend to be appointed by the local chapters

for duties performed, and the local chapters tend to be dominated by university types. I wouldn't say dominated - highly populated by university types, and by school chapters. And so about half of your regional council members, year in and year out, are college professors who can't understand why all this commercialism is in colleges…My opinion, of course, is that the colleges are much more crass, and corrupt, and commercial than the average profit making organization. But this is not a popular view among college professors.

MERTZ:

This does provide, perhaps…(inaudible)…

GROSCH:

Yes, it's true, except that the science pyramid in the United States is quite a steep one. Most of the public appearance, and most of the control of scientific and technical organizations in the United States passed around among a very few people. And this turned out to be true, also, in the ACM. If you list the powers of the ACM they tend to be forty, fifty, sixty people who - most of whom have been in the things since the very early days and have been passing the duties around pretty intensively. They tend to include people like Ted Capney(?), and George Forsythe, and so forth. There have been somewhat younger faces appear and disappear, but the old timers stay up there pretty - stay up there pretty well. They do tend to dwindle. I mentioned Charlie Concordian, who's name is no longer talked about very much in the trade. Well, Charlie was a hard rock electrical engineer from General Electric, who became Treasurer of the organization for many years and was always very important in its councils. But, on retirement, he disappeared entirely. I don't know whether he's still alive. Another person with the same story for instance, might be - escaped my mind, now - I was talking about Concordian I had another case in mind, but it's disappeared now. The people who were concerned with designing and building hardware, in most cases, joined the other organizations, or were already members of other organizations. That long string of people like…

MERTZ:

Well, IRE…

GROSCH:

Snider and Eichman, and all like that. Yeah, um hmm.

MERTZ:

…(inaudible)…

GROSCH:

Umm hmm, umm hmm.

MERTZ:

However…(inaudible)

GROSCH:

Well, the theory of the ACM, of course, was that it was to deal with the construction and use of the machines. And I think it would be a much healthier organization, today, if it had been closer to what the British Computer Society did in later years, and drew in both the business people and the scientific users; and both the hardware people and the software people. Instead it has always been strong in software, and especially in scientific and technical software. There's a lot of FORTRAN'ers, and not so many COBOL'ers so to speak. And the people who are interested in hardware tend to be those people who want to head special hardware, coordinate special applications of hardware, rather than those who are interested in…If the whole thing could be subsumed into one big organization, which I would like to call the American Computer Society as paralleled with the British Computer Society, essentially all of the organizations that are…with special interest groups within it, you have a two hundred thousand man society, today, and bigger in the future, which is what we ought to have to represent such a divergence in trade and divergence in professions.

Anyhow, as you can tell from my remarks, I continue to be interested in this from the very beginning. I'd always been a joiner in the sense that I joined the Astronomical Society, and then I became an Astronomer; and the Optical Society the minute I became an optical man. So I joined the Computer Society the minute that there was a Computer Society although I was a computer man first. And I've belonged to the British Computer Society for some years, and the IRE, as I say, co-opted from those early days of the early fifties, when they found out that I wasn't already a member. And I've joined various applied mathematics organizations, and so forth at various times, usually dropped them when I found that I hadn't been able to contribute to them. I don't believe in belonging to a society just to receive the publications because I have all the reading that I can do, already, and access to all the publications through professional institutions; libraries, and so forth. Moreover, I think that in every case that the three things that these societies are supposed to do have been bought. They have not been anywhere near as unified or as adequate in the sphering of the new professions - the new - professions, I don't want to say - the new skill, as they should been. They tended to follow along behind the IBM salesman, and the mavericks who had been making the real advances. They haven't been adequate, in my view, to represent the people in the field of the guild. In other words, they haven't put together the codes of ethics, the professional qualifications, the stratifications of membership, the examinations for proficiency, and so forth that a profession is presumed to have in that context of the word. And they certainly have not interacted with the rest of society adequately in informing congress, and consumers, and

so forth with the hazards and the beauties of the art. So the three things that they might have done, I think they've done a very poor job in the trade. And I've said so loudly…(inaudible)…twenty-five years. Of course, this hasn't gotten me any additional friends, but…

MERTZ:

…(inaudible)…

GROSCH:

Well, is it a profession, or isn't it. I think that's the basic question. If you define a profession in the sense that it's a thing like doctors and lawyers are, then you observe that that's not a definition but simply an example or a parallel, then certainly we aren't anything like the American Medical Association or the American Bar Association. We are a thing that you have to belong to with almost absolute pressure in order to call yourself a doctor, or to call yourself a lawyer. More than two-thirds of the doctors in the United States belong to the American Medical Association. And a sizeable percentage of those who don't belong either do it from spirit, or because they're the wrong skin color or something like that. On the other hand, a very small proportion of the people who like to think of themselves computer users belong to the ACM.

MERTZ:

Well, isn't there a distinction - a fairly basic one in terms of…(inaudible)…working out procedures…

GROSCH:

Well, to give specific names, the guy developing large primers(?), would be Derrick Laymer, one of the initial members of the ACM, still professor of mathematics at Berkley - L-E-H-M-E-R. And a typical guy who was interested in new storage media would be someone like John Rhachman at RCA Labs, - R-H-A-C-H-M-A-N - or Williams and Kilburn over at Manchester. Now, the fact of the matter is that those men were further apart at the times when they were pioneers, and this would be in the late '40's, than they are today. The equivalent people today find that hardware and software overlap enormously, and that in addition to that the man doing the design of the new storage medium has to know a lot about computer programming in order to design his new storage medium, on the computer. And the guy meaning to look out into the future of applications work needs to know a great deal about new kinds of storage economics, and things of this sort which are coming down the road and that he must almost select from the salesman's offerings by applications-type knowledge. No, I think that on the contrary that there is more unity between the two builders and the two users today than there was in the beginning, and I've been preaching the overlap of hardware and software the whole time. I've always seen this coming. And, moreover, you don't necessarily all have to be in

bed together. A guy in the Society of Automotive Engineers is interested in body design, and who's special interest in hardware is in methods of fabricating every weird fender shapes can still belong to the same society as the guy that wants to do suspension design on a computer or design better automobile tires, or something like that.

MERTZ:

What I was saying…

GROSCH:

Because they have a common professional interest…

MERTZ:

Most professional societies presuppose (inaudible)

GROSCH:

Well, this is the processing information…(inaudible)

MERTZ:

(inaudible)…one which, as in medicine, all physicians…

GROSCH:

Well, I think a great deal of this depends - depends on the degree in which - the degree and the form in which you look at professionalism. If you look on a profession as a guild, something which essentially keeps the outsiders out, and keeps the compensation up then the - there's considerable unity in the various parts of the computer trade. If you look on a profession as something that has a common body of knowledge - I claim that body of knowledge, to a considerable extent, exists in the information field and is growing. The fact that the doctor shares a body of knowledge about the human body, whether he's the surgeon that cuts one open, or the biochemist that figures out how the endocrinological mechanisms work differs from the computer field only in the fact that our body doesn't stay constant. The human body is the same now as it was in Egyptian times. The 701 and the ENIAC 4 are quite different.

MERTZ:

(inaudible)

GROSCH:

This is right. I have always resented the implication that there is a deep field of information science parallel in size of importance to, say, physics or biochemistry. I've never believed this. I've always felt that the little nubbing of new information involved in the term "information science" is easy by comparison. So I have to go along with what you say in that respect. But you see I don't tend to define profession in that term. I tend to define profession in the operational sense of it being a guild, a thing that gets together to benefit the guild, and to explain the interactions of the guild and the rest of the society. And to that extent the computer profession can be used - pardon the ugly term - is just as specialized as the doctor or lawyer, or more so. Because he hasn't been around long enough for people to have adjusted to him (inaudible)…

Then, of course, there's something else that militates against other definitions of the professional. One reason the doctor and lawyer tend to be freer in their guild activities is that most of them are self-employed. So, you know, a doctor's worried about his own billings. He's worried about his own survival in the financial jungle. He can be pretty crude privately about his financial motivations and still blossom over with a nice veneer of super professionalism, and it's for the good of all you people that I double my fee next week, sort of thing. But the computer professional tends to work for salary, and that means that when the boss says hop, he's got to hop.

MERTZ:

(inaudible)

GROSCH:

Yeah, but there's a tradition of independence, of free speech in universities which, while it's challenged year after year, it still has a considerable body of support. But you try to find me a computer professional who would refuse to write a program on the grounds that his boss is a crook and, boy, you've got an unusual man. That's why I think when we talk about professional ethics in the computer field, that you just laugh and say it's not that kind of a profession. Of course, I've been one of the people speaking for that view. I'm in favor of people refusing to do varied things with computers, but I find very few computer artists willing to actually do this. But, of course, the selection principle thing is difficult, because unlike the doctors there's a sizeable - a very large percentage of computer people who think this nonsense. Now, you can always find a doctor to do an abortion, too, or any other kind of even more illegal operations, but you may have to scrape around quite a little bit before you'd find a guy that would find a guy who would slip you some heroin or something. Whereas, I think, in the computer trades there is, you know, a large percentage of the people that wanted to work on anything that was presented to them by the man who owned the computer. They simply feel themselves divorced from the consequences of their actions in a way that doctors through the generations and generations of Hippocratic and, in many cases hypocritical tradition, has…

MERTZ:

Well, what other kind of …a high school graduate in six weeks be trained in computer programming, or the eighth grade student…

GROSCH:

Umm hmm, umm hmm. Not if the computer programs aren't worth anything, which is what they aren't. If you train a kid to play tick-tack-toe in the eighth grade, that's no harder than training a kid to zip open a problem with a scalpel, but that doesn't make him a surgeon.

MERTZ:

GROSCH:

See, that's still another definition of professional.

MERTZ:

GROSCH:

Exactly. I couldn't agree more. If you require a certain level of skill before you could become a full member of the ACM for instance, or become a full member of the Data Processing Management Association, then you'd have something, you know, where zipping open the frog with the blunt does not make you a surgeon. And if the computer trade is to be a profession, then I think there's no question that such criteria are necessary. But you observe with great interest that we don't move very rapidly in that direction in the ACM.

MERTZ:

Possibly one suggested explanation for this might be the variety of disciplines for those (inaudible)…They came into their own specialties…

GROSCH:

Right. Well, my theory is that what you need is a catch all association. This I'm putting in true terms, I'm not picking it out carefully or making a full position, but my general rough theory about this that you need an overall catchall association, that within that association you should have a group that's concerned with professionalism, and that this would have some groups that would be concerned with ethics, that would be concerned with educational practices, the whole area that a true professional, if there is such a thing, would be concerned with. Quality of work, quantity, safety of the work, etc…

MERTZ:

The IRE…

GROSCH:

…but that this would not subsume the entire organization. It would be perfectly appropriate to have thousands and thousands of members who are just, you know, high level computer installation operators, who want to take an occasional course, or read a journal that's made from another computer installation operator, and who would still have a form of membership - maybe even full membership if they spent enough years, or worked in an important enough position, but would simply not consider themselves as professionals. Somebody said ethics, boom, they'd say (inaudible)…

MERTZ:

There possibly are (inaudible)…

GROSCH:

Umm hmm, umm hmm.

MERTZ:

(inaudible)

GROSCH:

Yeah, umm hmm, umm hmm.

MERTZ:

…for developments…

GROSCH:

Of course, yes.

MERTZ:

…which gets…

GROSCH:

Well, I don't regard electrical engineers as much more professional than the computer field - much. They do meet the requirements of a large, specialized body of knowledge, and a more stable one than the computer trade, per se. But…

MERTZ:

…(inaudible)…committees that pass on the qualifications…

GROSCH:

Well, but the British Computer Society also has problems, and also had a committee which passes on them, and yet at the same time it also has a large number of members who would be, in our context members of the Data Processing Management Association, people that work in the large computer installation, for a reasonable enough salary, enough day to day continuity of interest that they are willing to pay a fee to get into the…

MERTZ:

I don't usually say this, but I suspect that less than five percent of the members

GROSCH:

Oh yes, much less I should say.

MERTZ:

…speaking of maybe ninety-five percent of the people…

GROSCH:

I think one of the great differences that needs to be brought out, and if you like this - I'm sure that you're personally very familiar with, is that there is a difference, say, between the automotive field, where you have automotive engineers who design often rather advanced techniques - who design automotive equipment, and say, truck drivers who drive them up and down the highways. It's not true that the guy who's running a computer shop, I wouldn't necessarily say a man who had just changed his tapes in an installation, but a man who's managing a computer installation, or a programmer installation is like a truck driver, because the problems are new everyday, and the art of writing a program or debugging a program is essentially a development art, very much like the business of designing a landing gear for an airplane, or modifying a race car. There's a standard way of going about it. There's a - it isn't research, it isn't the creation of brand new knowledge or anything like that, but it isn't production in the sense of just standing there and pulling a handle, or tightening the same screw in the body of an automobile in a production line worker, to the extent that using a computer is essentially a development process, and the user as well as the hardware, or software designer is sitting…(inaudible)…A high lever

tradesman, and a quasi-professional. He may not fill all the requirements of a professional, but he's certainly more than a truck driver. Now in every installation there are also people who are just truck drivers - the guy who sits at the front desk, and just joggles cards and puts them in the rack for the programmer to remove - or a high level operator to remove. But he doesn't want to join the ACM, or the American Computer Society, either. So the act of requiring him to put out $25 bucks or more a year of his own money, in a sense, dwindles him out. So the truck drivers who are also present are employed in the computer trade, all right, but they're not members of the trade anymore than the clerk in a store considers himself a member of the Retail Merchants Association.

MERTZ:

There are a fair numbers of…

GROSCH:

Oh yeah, sure. Sure, lots of people…I think another thing, too, and perhaps the supreme definition of a professional is the same as the definition of the Jew. Who is a Jew? The answer is a Jew is anybody who thinks he's a Jew. And so to a similar extent a person in the computer profession is just about anybody who thinks he's a computer specialist. It's true that in the process you get some confused truck drivers, but you also get some confused Jews - I guess I should say some confused Christians. I think that the main thing that you get in any professional association - I'm not saying profession, now - in any professional association - I'm not saying profession, now - in any professional association is self selection. Now persons participating in a most recent issue of Datamation of Computer Decisions, I read both of them last night, I can't remember which one it is - a marine officer writes in and says while he's a marine officer first, and considers himself a computer person, too - considers himself a computer professional also. Well, that's an interesting introduction in itself, because not many marine doctors would write in and say that they're a marine first and a doctor second. So, in a sense, he's self selecting himself. He's a professional marine - a professional military man. But he, you know, you don't have to be exclusively, he could also be a computer professional. Now he says, Isn't it a mark of how unprofessional the ACM is that nobody's ever come around and asked him for membership. Well, does he have any idea that in the - that the bar association, or the medical association comes around and solicits young college students. They certainly don't go out and beat the bushes for doctors that are already in practice that don't belong to the AMA. Not at all - they think, by God if this person is any kind of doctor he'd be here beating on the doors trying to get in. And it's our duty to keep him out if he isn't a good doctor. So, old Mr. Marine, it seems to me, accuses himself out of his own mouth. And there's two ways of not being a computer professional: One is that he didn't have enough gumption to get in on his own; and the other is he says he puts another devotion first. Since I don't really think it's a profession, I don't object to him being a marine first, and a computer tradesman second. But he says it's a profession, or if it is a profession that that's his requirement.

MERTZ:

What do you think of the impact of this tendency to be perhaps more academic …(inaudible)…

GROSCH:

Umm hmm. Well, I think…

MERTZ:

…(inaudible)…

GROSCH:

Yeah, I think the answer is that there have been very few people who have resigned from any of the older IRE, AIEE especially, organizations in order to join ACM. They didn't feel that their needs were now met by the ACM, and they could now relax and quit going around the communications engineers, power engineers, traction motor designers, and what have you. They kept their old memberships. They may in many cases also join ACM. And I think in the British Computer Society that would not have been true. I can't speak from first-hand experience. I don't go anymore, and I might be overstating the case, but it seems to me that many men who have been in the Institution of Electrical Engineers, the British equivalent of the AIEE, and so forth, and who've worked in the computer field, found that their needs were met by the BCS and they didn't have to continue any others. At least - I guess I don't know that. I guess I don't know that. At least the hypothesis occurs to me, I guess is what I want to say. I suspect, for instance, that Tommy Kilburn still belongs to the AIEE. But the, you know, a man of that stature tends to belong to a lot of organization because he's a fellow at all of them. I mean, you're not going to resign something you're a fellow of.

I just resigned from the Astronomical Association the other day after thirty years. It made me feel very bad, but then I said to myself I'm never going to go to another astronomy meeting; I - when I meet an astronomer in the street he's some young guy I've never heard and who never heard of me; and why should I go on, you know, cluttering up their roles, and paying for a publication I no longer want, when I'll never by an astronomer again. So I resigned, and everybody said, "But, thirty years, how can you do it?" The answer is easy, I don't even listen.

MERTZ:

Well, have you had any particular…

GROSCH:

Well, I think my major concern in the very earliest days was twofold: One was, essentially, negative - inimical, perhaps, is a possible word for it. I had been - this is, I hope, a minor one - I had been very much concerned with breaking the grasp of the academics on this field. I was very much opposed to the fact that most of the progress, and most of the interest in the field is not in the universities except for the very earliest days, and that they have a disproportionate control over the actual workings of the society; or as I used to put it in my fairly vivid terminology, that I was a member of the dollar not the scholar party. And I think I've always been, probably, the loudest and one of the leading voices in that non-academic, or anti-academic party. Not essentially negative majority. On the positive side, I'd also been a very loud voice from the very beginning on the interactions of the ACM and its trade profession or whatever it works with - whatever it represents - with the rest of society. I've always wanted them to have social position, to testify before Congress, to…

MERTZ:

(inaudible)

GROSCH:

…in society and tell them what we had to offer. Yeah?

MERTZ:

(inaudible)

GROSCH:

Yes, yes.

MERTZ:

(inaudible)

GROSCH:

Yes, yes, yes, exactly.

MERTZ:

(inaudible)

GROSCH:

And for instance, I was using terms like "invasion of privacy" ten or fifteen years ago. Yeah…at a time when a lot of people thought this was just nonsense. I've been defensive, although not concerned, about accusations of displacing people, or…kind of argument. I've been concerned with the positive powers of the machine to simulate social problems and help solve them, transportation assimilations, and things of this sort many years ago. I was just trying to write a letter to Tom Watson, right now, on a matter of the changing structure of the Board of Directors of the company, as a private citizen, you understand, not as a Government employee. And in doing so, I've been revealing a thing that I did for him in 1961 on social applications of the computer revolution. And there's a whole raft of these things which are now all…that I was pushing on IBM as a consultant ten years ago.

MERTZ:

Yes, I saw that.

GROSCH:

Umm hmm, umm hmm. (inaudible)

The - those two things, I think, are the two things that have characterized, certainly, my ACM record and a very large number of snide remarks that I make in my many speeches, and so on, are directed in those two directions. For instance, the major resources of the ACM, from the beginning, have gone into an increasing program of highly technical publications, which the average member does not appreciate. And which, often, is so esoteric that even the technical, the academic membership complains that there isn't enough about their part of the discipline, and too much about computability theory, and that sort of thing. Well, now to counteract that, you put something like the ACM Communications, and then it turns out that they then want to put a whole bunch of archival publications in the Communications, and cut down with running you short, cut down with news items and the advertising - not cut down the advertising, but has the effect of cutting down on the advertising since the advertisers don't want to advertise anything except college textbooks, and a book that's devoted to highly academic jargon. So then they say, well, why don't we start a newspaper. Well, why should they start a newspaper when "Datamation"-in view of their past deficiencies - Datamation and other magazines already exist, some of them on a weekly basis. So we stopped from doing that, and then they tried to take revenge on us by cutting the news items out of it…that if we didn't want a newspaper, we obviously didn't want any news.

MERTZ:

(inaudible)

GROSCH:

Well, that management is how old, now? Two years. The previous manager was Bernie Gallaher, and Bernie's as academic as they come. And the management of the editorial board is greatly improved, I think, by Elliott - Elliott Ness, is it? But the previous one - Eric Fineman was as academic as they came, and fought tooth and nail for every dollar of money he could bleed out of the society.

MERTZ:

(inaudible)

GROSCH:

No, Morris is even more academic than the average academic.

MERTZ:

He's not…

GROSCH:

Yeah, sure he is. He's an academician that doesn't have to teach classes. Rand Corporation is right in the Bureau of Standards. It's a softer touch for the academics. You don't have to publish as much, you don't have to teach classes, but you're just as academic and cloud…as they come.

I wrote a - there's a Datamation article called "Who's Yellow" that I wrote some years ago which describes a - in passing and in a vengeful manner - a fight I had with Mario Juncosa about the time when he was editor of the Communications - or was it the Journal? - the Journal…

MERTZ:

(inaudible)

GROSCH:

Yeah. And they published an article about automatic abstract - automatic indexing, which was an obvious extension of the work by H. P. Moon, that I mentioned, on automatic abstracting, and didn't reference Moon in the bibliography. So I wrote Juncosa a harsh letter saying, you know, this is a crooked damned business, and how about an apology. And he writes back and asks me for a bibliographic reference to Moon's work. That's pure academic, boy. That's the old academic still at it…(inaudible)…I don't mean club in the sense of association, but in the sense of budget. So, a year later when I had a chance I struck it from the other end…(inaudible)

Now, I would like…The academic type that I knew was my old professor Allen Maxwell. Maxwell a vow of poverty. He was going to earn $3500 bucks a year for the rest of his life, he wasn't going to be able to own a fancy car, and a wife, and some nice clothes, and a vacation, and a desk calculator, so he chose the desk calculator and a second hand car, period. He gave up the wife, and nice clothes, and the vacation. And, in return, he could work on any damned thing he wanted to work on. If he wanted to compute comet orbits, dull though it appeared to most of his…he was free to do so. True, he had to buy his own desk calculator to do it efficiently, but…And he didn't have to spend much time teaching other men's classes; he taught a course in orbit computing in which two people would come every two years. And, of course, the practical astronomy, because it was required for the PhD, attracted a somewhat larger version and then he took his share of the common load of lower level classes.

Now, to a person of monkish behavior, which I guess I really never was, but which I thought I was going to be in those days, that was a way of life. Now a guy goes out and bucks for a fifteen million a year military contract so that he can dragoon a whole bunch of graduate students into being slave labor, and let himself build up to a twenty million dollar contract. Like, for instance, Dr…I don't rightly care for that kind of academics. And that's the kind of academic that most academics want to be. Not the humanists, not the historians, but increasingly even the economists, the political scientists. And all of the physical scientists, and most of the natural scientists.

MERTZ:

So the field of physical science…

GROSCH:

It costs a lot to build a two-hundred inch telescope. So we built one for the whole world. And it took years, and years, and years, and year, and years, and years, and years for them to kiss enough feet to get that one.

MERTZ:

(inaudible)

GROSCH:

I don't like the way they changed. And I don't see any indication that we need a three-hundred GEV accelerator in Geneva. I don't want to know that much about particle physics. And I don't think society wants to know that much about particle physics. But they've got a goddamned effective particle physics lobby, nationally and internationally.

MERTZ:

Well, aren't you proposing, though, that one of the problems of the guild and computer professionals is that they don't have a lobby…(inaudible)

GROSCH:

I don't want that kind of a lobby.

MERTZ:

What are – what…

GROSCH:

I want a - essentially a section kind of lobby, and I want to protect society against us, more than us against society. For instance, if I were a doctor - if I were a medical doctor the thing that I would want to do would be to use the AMA lobby in Washington to, for instance, work against insecticides.

MERTZ:

Well, the basis of the way…

GROSCH:

Wrong use of insecticides.

MERTZ:

…lobbies so it can work in Washington and elsewhere - could you, without being charged with being excessively…reasonably expect such a lobby of computer professionals to represent precisely the area of concern that you…

GROSCH:

No, but I would rather have a lobby that started out to do something in that direction, which I could then hound to stay honest, than not to have a lobby at all. Now, I guess I would say as a third choice would be a lobby that was in the wrong direction. Like one that was in favor of the ABM, or in favor of more computers in the Air Force, or something like that. But there's very little worry about that. That lobby is provided by the Department of Defense and the manufacturers. You don't have to have a professional group…

MERTZ:

Well…

GROSCH:

I think what I was talking about was not a lobby. To a certain extent you put that expression in my voice - in my mouth. What I really said was that I wanted the computer people to interact with society.

MERTZ:

(inaudible)

GROSCH:

Well, but then you've said yourself that this is a new technology, and a new kind of interaction. And they're also new kinds of organizations that don't act this way. There are scientists concerned with social matters, and the Federation of Atomic Scientists, of which I was also a charter member, now the Federation of American Scientists, was a case in point. They knew sin. They blew up a lot of Japanese and felt bad about it.

MERTZ:

Well, have you participated in capacities for the ACM for which you would have a chance to influence…

GROSCH:

Not too many. I've been, primarily, a lone wolf. I'm sort of a party of one in the sense that from the very earliest days I've been a very popular public speaker, in great demand all over the country and world wide as things got bigger. I've always been independent enough that I could say what I thought. I've never had to play the IBM line, or the G. E. line too much, but never the ACM line, whatever it is. So I felt free to say what I wanted to say. The result is that since a large part of this has been negative; for instance, we're talking about the ACM right now, and since a large part of my time in discussing ACM has been filled with saying ACM is a bunch of schnooks. I haven't normally been invited to get very close to the management controls. Now, for instance, some of the people that I've attacked-who's attitudes I've attacked strongly, have, nonetheless, been big enough to ask me to do some non-controversial things. John Carr, who's as academic as they come, for instance, appointed me - when he was president of the ACM - appointed me one of the ACM representatives on the JCC, as I mentioned an hour or so ago. And that, in spite of the fact that he knew that many of my views were very different from his, but as far as the business aspects of running a giant computer conference were concerned, he felt I'd do a good job of representing ACM interests, which I like to think I did. So he appointed me to it anyhow. But it was only recently that I was made a member of the Council, in spite of the fact that I got an enormous vote when my name was finally put up. And I think that what this means is that - not so much that I'm less of a lone wolf, but that

simply the pressures of society, the positive thing that I've been looking for all these years, are becoming obvious even to the average ACM member; reluctant though he is to admit it, he's aware of the fact that, you know, society's concerned about him. And the guy who's been, for many years, getting up and speaking very vigorously, indeed, against the bad aspects of the trade with the good aspects, finally comes into his own.

I was told by Paul Armer - I've never seen the actual vote - but I was told by Paul Armer…

MERTZ:

A-R…

GROSCH:

A-R-M-E-R, that I had - when I was elected member at large of the Council, I had the largest single vote of anybody, including virtually uncontested other positions. And certainly larger than Crouse…because he had…running against him, which is not a very strong opponent, but nevertheless, one who drew away a few thousand votes. But the member-at-large business, of course, everybody could vote for them if they wanted to, and also vote for other people besides. So I got a very large vote, indeed. Well, that's flattering after all these years. I must say I think I deserve it. I've struggled for a long time to maintain these positions. So many people thought I was a kook. And now most people still think I'm a kook, but, you know, they regard me as a friendly kook rather than a misguided one. And this is only the beginning. I don't know how much more I'll be able to do, personally, but It's very clear, indeed, that there's going to be a much greater involvement in technology as a whole in society, and that society's going to demand a great deal more - in many, many cases more than the professionals, the engineers and scientists can perform, from these guys, and they damned well better get some people that are aware of this. Now, Carleson, for instance, you mentioned the fact that we have a better management - a non-academic management of ACM, better by my standards…

MERTZ:

I don't believe I've made any - hopefully, any value judgments…

GROSCH:

O.K. Well I have, as you know. I'm an old value judgment…

MERTZ:

(inaudible)…not my place…

GROSCH:

And Carleson is certainly very much more concerned with this - very much more concerned. But, this comes from involvement with the Department of Defense, and senior industrial positions with Dupont and with ACM. He's had to - in the words of Sam Johnson's woman, he's had to accept the universe.

MERTZ:

Dupont and IBM?

GROSCH:

Dupont, Department of Defense, and IBM in that order. When I first saw him he was a planning executive of Dupont - Data Processing Planning Executive of Dupont…His service with the Department of Defense was, I think, not particularly liberal, but it was certainly intelligent, and gave him a little perspective on social and Government problems; and so he's an able guy for…They have the association now, compared to - for all these obvious good points. Bernie Gallaher, who's a professor, wanted to promulgate certain kinds of…

MERTZ:

I was going to ask, what are you - what would you, in a very short, capsulated form, describe as the major shortcoming of academic preeminence

GROSCH:

Oh, in a single word - selfishness. The academic - the academic wants it for himself. True. The thing that has disturbed me so much about academia, since the end of the war, is that if you get a committee meeting going, for instance, that's going to proportion out some resources, first thing that can count for is that every academic on that committee is going to try and get some for himself. It never occurs to these guys to take a broad view, you know, of social good, or optimal total performance, and what have you. And the reason is simple, they're all running little private businesses. In some cases they're running real businesses in the sense of stock issues, and treasuries, and so forth. In other cases they're running private businesses in the sense of Draper Resistance Rotation Lab, which, in any case, they feel nobody's out for them except them.

MERTZ:

(inaudible) software…

GROSCH:

Sure I do.

MERTZ:

Well, you consider that academic?

GROSCH:

Well, when they're founded by professors. Particularly if professors get a grant from the United States, and with it he develops something, hardware or software, and then he goes off and starts a company and makes profits with it. He doesn't plan to pay Uncle Sam back, you understand. So, in a word, selfishness. And as a guy that's pretty good at being selfish, himself, I think I can recognize it when I see it.

MERTZ:

All right. Now this will conclude this side of the tape.

[Tape 13, Side 1]

GROSCH:

The whole idea of the Evandale Enterprise changed rather quickly over the four years that I was in charge of it. It changed in magnitude and it changed in direction and so forth. The idea originally was that there would be an IBM 701 Defense Calculator, that it would probably be needed more than one shift but not an awful lot more and work to be done on it would be brought to the machine by the Medium Steam Turbine people in Lynn, Massachusetts, the small aircraft engine department, part of my AGT (aircraft gas turbine) division also in Lynn and to a somewhat less extent by the Large Steam Turbine Department in Schenectady, notably this man, Hans Kraft. In addition to that…

MERTZ:

K-r-a-f-t.

GROSCH:

K-r-a-f-t, yes. Also a member of the ACM for some years but he's disappeared. I believe he's dead now. I've tried to find out but I haven't met anyone who would know for some time.

He was a pupil of--oh, heck--Von Carmen, Theodore Von Carmen in Germany and immigrated from Germany because of his distaste for what he saw coming there quite a long time before Hitler. He named his son, for instance, Franklin Roosevelt Kraft which

gives you a fair view of the thing. And, Hans was a great guy with this enormously difficult compressible flow problem always in his mind year after year after year. And he especially intended to bring work, and did in fact bring work down to the 701. But, there, of course, was supposedly an enormous amount of design and routine work both to be generated in the Evandale area.

I should explain that at this time was almost the height of the General Electric theory of decentralization and professional management. The President of the company was Ralph Bordner. His philosopher in residence, actually a staff vice president was named Hal Smiddy and these two between them were formalizing the organization of this enormous 200,000 man company in an almost unbelievably rigid fashion, not necessarily erroneously rigid.

It was an extremely interesting experiment in organization conducted with real money and real people, not the theoretical thing done in a college laboratory. I think in the long run, like most extreme positions, it turned out to be a little too extreme but the ideas of decentralization, of separate profit centers, of clearer responsibilities and authorities and matching sizes certainly have great validity.

I have the advantage of having my major climb in management from the lowest levels of supervision up to a fairly sizeable managerial job under those auspices so I was formally taught to be a manager although not all of the education actually penetrated but at least I was exposed to this in a very formal and straightforward fashion…the bookshelves and the Bibles of the thing and blue books at the end of that shelf named General Electric's Organization, General Electric's Skills, which one had to master in the same way that one mastered Granville, Smith and Longley's Calculus in order to become a mathematician when I was a college student.

I went to work first for a--let me see if I can describe the motivation behind the 701 since this is really about computers. The idea was that the two divisions had similar general tasks. They both dealt with rotating machinery, with fluid flows through that machinery--the gas turbine and the steam turbine. The fact that one of them was going to fly around largely, would fly around in airplanes and the other one would largely sit in large power stations for 50 years at a time, of course, did differentiate them. The fact is that you still had the problems of fluid flow and heat transfer, and strength of materials under high temperatures, and so forth. Both of them were interested in the long run in numerical control of machine tools, both for different reasons. Both of them had the power problem of reducing enormous quantities of test data and, if possible, very quickly so that they could replicate the experiment or tear it down and go on to the next one with minimal losses.

In Lynn they had an enormous set of test cells for the jet engines which were powered by essentially most of the steam driven electric power generating capacity of the city of Lynn and, in fact, could run only at night because during the daytime the rest of the city needed power. I can no longer quote the horsepower but we're talking in terms of, with

all the cells running--they couldn't all run at once but with the maximum amount of cell power being drawn anyhow--60,000 horsepower. It may have been several times that amount. I forget exactly. I remember that particular figure because when they began to talk about expanding the capability, I discussed the possibility of their getting--I recommended the possibility of their getting a bunch of used steam locomotives and getting the steam from them. And, in fact, in the end I believe they did get a couple of mothball destroyers and board them in the river near the cells and used them to generate steam, used their boilers to generate steam.

So, we're talking about major enterprises, extreme expense, tremendous business and a high degree of rapidly changing technology. Now, for instance, at that time the jet engine business, which was my parent organization, the one that actually paid my and which owned this Evandale facility, ran this Evandale facility, was tossing back and forth the combined commercial and military jet engine business of the United States government and a small amount of overseas business with the Pratt & Whitney Division of United Aircraft who, by the way, got the 701, number 8, or something like that (?). A great rivalry between us somewhat accentuated by the fact that the young man who was number 2 in the Pratt & Whitney operation had been one of my students. In fact, one of my two prize students at Columbia when I was teaching numerical analysis at Columbia.

Well, this rivalry was important commercially as well as technologically because on the swing of a little engineering performance and a little salesmanship, there was about $600 million a year of business at stake. And this would swing back and forth from a couple of hundred for GE and four hundred for Pratt & Whitney to the reverse from year to year. And it was relatively apolitical. I don't mean to imply that there weren't some very high level pressures exerted through senators and all that sort of thing but, by and large, the efficiency of the engines and the price at which you could manufacture them…

Now this accounted for the fact that both GE and Pratt & Whitney were perfectly willing to get a several million dollar computer--to rent it, of course, in the ambitious stages--and to build up a large and expensive organization and to spend a lot of time on software and going to meetings, letting me play my little games around the country. If I could change the efficiency of that engine, or the cost of that engine from a fixed efficiency by one percent, it might make a difference of $100 million a year in gross and several million dollars a year in profit. And, of course, the cost of doing this was, at least in the military cases, all charged off to cost plus fixed fee, anyhow. So--or even in some cases, variable percentage fees--they really didn't stand to lose very much even if it wasn't a good experiment and they stood to gain a very large amount if it was a success.

The Aircraft Gas Turbine Division was divided into three departments, each of which was a separate profit center. And there was a sort of laboratory department called the Development Department. Later I believe the name was complicated to be the Aircraft Gas Turbine Development Department, typical GE professional management lingo. There was a manufacturing department which was called something like the Large

Engine Department and there was a Small Aircraft Engine Department. Later they added an Aircraft Accessories Department and made things like fuel pumps and very small jet engines that didn't go in airplanes but for auxiliary power and stuff. And there was associated with it in Evandale--the latter two, by the way, remained in Lynn--the Aircraft Nuclear Propulsion Project which was part of a different division of GE which was trying to make a nuclear reactor that would fly around upstairs and this, of course, required a plane the size of Howard Hughes' wooden seaplane to carry it under the optimal conditions.

MERTZ:

This wasn't the Dinosaur?

GROSCH:

No, that was a different name. Dinosaur was a space program. This was an airborne, air breathing system. The reactor would take in air, breathing system. The reactor would take in air, for instance, or working fluid, I guess I should say more specifically and heat it enormously as it passed through the reactor which, in turn, would keep the reactor from melting down or--And then this hot air got squirted out the back of the airplane in a thing like a jet engine in order to produce thrust except that the detail design of the jet engines never really proceeded very far because they never could get the reactors to the point where they would be light enough to fly around in then useable aircraft.

Anyhow, the AMP (?) Program--although not one of the two partners of this to support the 701 was nevertheless a major user of the facility. Like all the other organizations I mentioned, it wanted to get its own machine or even bigger machines at the earliest possible moment. And, in fact, I managed to fight them off to the point that they finally got a 650 to do data processing and continued to use the 701 and 704 for scientific work. But it was a continuing battle in which I was reasonably effective.

Now I was hired first as a first normal supervisor and I was called something like Unit Supervisor, Numerical Analysis or something like that. I had this man Gettle (?) and four girls under me. Within about two years I was made a second level manager, a so-called sub-section manager and about, I guess it was about a year and another year after that I was made a third level manager, a so-called section manager reporting directly to the general manager at the same level as the chief engineer and the head manufacturing guy and the chief accountant and so forth and called Manager, Investigations Aircraft Gas Turbine Development Department. And by the time all that had happened, my boss was--bosses changed during the period but during the last couple of years when I was a full-fledged section manager--David Cochran, who's working at a high level for GE here in Washington at the present time.

And Dave was one of the first people to be sent away to the In Residence Professional Management School that GE opened in Crotonville, New York around this time under

this man, Smiddy's auspices. This was a private estate with 150,000 volume library, classrooms, lecture rooms and all that stuff. And you had to go for 13-14 weeks at a time, leave your wife behind, leave your job behind and when you got back, you usually still had a wife.

Anyhow, Cochran went to the first class and came back fully imbued with the glories and beauties of the GE philosophy which he then passed on to us. And he still had a job although I really think if he hadn't gone, he'd probably be higher in GE today than he is. But, he was a great guy and he genuinely believed this stuff. He was, of course, development oriented and he couldn't see any reason why you couldn't use development tactics in managerial matters as well as technical matters and I agree. I don't either but not all the GE experiments were too successful. Notably the one on entering the computer business at a somewhat later date.

Anyhow, Dave put me through the proper psychological tests which included professional interviews with the psychological corporation representative and so forth. The manager of Employee and Community Relations at that time was a woman, the highest woman executive in the General Electric Company, which indicated a certain flexibility of approach. Her name was Miss Marian Kellogg and she's still with GE in Management Development at headquarters. And, after Marian put me through the hoops, why I got made section manager and joined the small family of six or seven people who reported directly to Cochran.

Now this, of course, by this time we'd switched to a more advanced family of computers and were putting fancier techniques into action. But, it was not so much I think a reward for the technical capabilities of the machines, which I'm sure they would have secured just as well under a more diligent mathematician who was less interested in politics and the expansion of the art, as the fact that I had sold the concept of technical computation pretty widely across the GE Company by this time. It was also in contrast to the fact that about the same time they decided to get the 701 for the two combined divisions, the Steam Turbine and the Aircraft Gas Turbine Divisions. They had also decided in an entirely independent and decentralized way to put a UNIVAC I in into Louisville to be the first UNIVAC in data processing service anywhere in the world.

Chuck Rieger, then division manager of the Appliance part of GE in Louisville, decided to spend the money required to do this and I believe he had to buy the machine. I think he put a million dollars or so in cash into the UNIVAC I. He got UNIVAC I, number 3 or 4. This is touted as being the world's first data processing machine but, in fact, that's not true. One of the early IBM 701's--in fact, I think that's one of those that preceded mine. I think number 4 or number 5 was the 701 that went into the Air Navy Aviation Supply Office in Philadelphia and it was used for inventory control, which was a very straight forward data processing installation and that would be installed in '53. A little earlier in '53 than mine and a year at least before that the Lions Electronic Office in England was doing realistic alge…production and inventory control calculations for the Lion's Tea

Shoppes. True, at that time, it was a one off machine but they've--successive identical machines. So I would say that UNIVAC I was number one. It was number three.

Now, to get back to the point however. That was at least a temporary catastrophic failure. Years went by and they never succeeded in running the payroll. They wrote an 80,000 word absolute octal program to do this enormous payroll.

MERTZ:

This is the one in Louisville?

GROSCH:

That's the one in Louisville. Never were able to de-bug it either in chunks or as a whole and finally had to throw the whole thing out and rewrite it all--40,000 words the second time. And I think relocatable the second time. The combined efforts of Arthur Anderson and company, the consulting firm, the Remington Rand division of Sperry Rand and all of GE Louisville's resources just barely sufficing to run this stupid payroll.

Meanwhile, starting with scratch we got into business exactly on schedule. In fact, only a few weeks after the machine arrived and had much more difficult programs running on it immediately. Part of the difference being IBM. When you needed help, IBM would supply it. Partly that I didn't need the darn help very much, that I was an experienced operator of computer equipment for many years and people they had in charge in Louisville, you know, didn't know a wall switch when they started out and were mostly retrained accountants. But, for one reason or another, the enormous contrast between these two things did me a lot of good. There's no question about it. I offered over and over again to try and help the Louisville people. I routed business data processing people that came to see us down to them because I didn't need them and they did. And they usually refused to interview them. I sent visitors down there to talk to them. People like Bill Bell of telecomputing and so forth to give them good advice. Tom Tillitt who got one of the early 701's at the U. S. Naval Ordinance Test Station, China Lake (?), California…

MERTZ:

T-i-l-l…

GROSCH:

T-i-l-l-i-t-t. And again one of the share founders. He got 701, number 9 or 10, something like that. And they were very courteous to most of those people on the grounds of "what are you bothering us for. You have an IBM machine" in spite of the fact that the problems that they faced were actually simpler than the problems that these men had already conquered and were well aware of in general form.

Well, for one reason or another I had a big success and they had a big failure. The vice president and general manager of the whole Aircraft Gas Turbine Division was at this time Mr. C. W. LaPierre, Cramer W. LaPierre, who later became the number 3 man, the Executive Vice President of GE. And Jim, as he was called, was a very forward-looking guy and he was very much impressed not only with this equipment but with the flair that I showed in showing it off and selling the people as distinguished from just the detailed results we got from it. Now I've always thought that my rapid rise in the GE management was at least partly attributed to the fact that the week the 701 was coming out of its crates before we actually had all of the power on--this means we're talking about something like November of '53--I finally sent everybody home, even the IBM customer engineering team that was killing itself 24 hours a day trying to get this thing going. And I sent everybody home and on Sunday morning, about 10 o'clock, unable to stay away myself, I got a push broom from the janitor closet and was trying to clean up the machine room from all the messy stuff that was lying around when we opened crates and pulled off wrapping paper and so forth. And as I am standing there in my ragged T-shirt with this push broom they had and not particularly clean. I mean, of course, this is a clean area so it isn't like I've been shoveling coal or anything but still I'm not exactly ready to receive visitors either. In comes LaPierre with Assistant Secretary of Defense, Donald Claw (?), I think it was, in tow. At least some Assistant Secretary of Defense showing him through his laboratory facilities on a Sunday morning when nobody else was around. And I've always thought that when LaPierre heard me give my lecture, push broom in hand, on the glories and beauties of the computing machine, it did me a lot of good. Anyhow, I always had a good relationship with all of those people and this business of being a little vigorous, which I've been accused of before and since, seemed to fit in pretty well with the vigorous nature of that business. It was one in which there wasn't a great deal of phoniness permitted. When you got done, you built the damn engine and you measured the trust and it either did what you said or it didn't. And different from management information system where you can't tell whether it's working or not most of the time. Or as far as that goes, one of these giant military command and patrol systems which you only get to test once. Afterwards, you're either dead or not.

MERTZ:

Did you have any contact with the computing facilities at the Wright Patterson Air?

GROSCH:

They were very much inferior, extremely so. I gave lectures and visited all of those places all over the Middle West at one time or another. There was a special reason for an interest there though and I can mention one in about 1955. And about 1955 I was asked by some people in Syracuse, GE Syracuse where I was also known at that time, to visit the project in Syracuse and they were building a computer for Wright Pat. That was the ORAC, Office of Air Research Automatic Computer, which was to be built as a copy of Aiken's Mach Ill by the General Electric Company, more or less from scratch with only a small amount of re-engineering, however, mostly to permit the use of GE geranium

diodes, which weren't any good as it turned out, and a few things like that. And some redesign of the bearing structure and so on this enormous drum. It was an enormous drum machine. A drum as big as a man's torso practically.

And I went up to Syracuse and saw that and wrote a report, of which I still have a copy, one of my informal letter type reports, cutting them up one side and down the other as insufficiently supported which was management's fault, insufficiently trained which was their fault and much over-burdened schedule-wise and so on which…

So, in the process, I got to know something about the customer departments at Wright Pat, which were not all that serious in the research and development end where this was to go. Now, do you remember the name of the guy that was in charge of that? If you mentioned it, I would remember it but I'd have to dig pretty deep to find him now. Little, Clarence Cook Little, former President of the University of Michigan. I think there was a Little…

MERTZ:

Is this at Wright Patterson?

GROSCH:

Wright Patterson, yeah. This was research now and in addition to that, Wright Pat, of course, had enormous problems in logistics and stuff--inventory and that sort of stuff. And, there I did have some knowledge but not a great deal. Most of our jet engine stuff interacted with an inventory control system at Tinker Air Force Base in Oklahoma, Oklahoma City and I lectured there sometimes. In fact, attempted to work up a computer to computer. They also had a 701 or 702 perhaps and was also interested in trying to get a tape to tape exchange of data with them which was not done at the time I left although I think it was done later.

MERTZ:

Did you hear of ILIAC or…

GROSCH:

I know Meager (?) as a friend, yes. I never visited the actual shop. My introduction to Meager were backwards, as a matter of fact. I started out by knowing his English visitors. He had visitors there like David Wheeler, Maurice Wilkes, Sandy Douglas and several other pioneers of the British thing.

MERTZ:

Much?

GROSCH:

Eric Much, yes. Eric and I were very good friends. He's dead now, unfortunately. I still know Margaret, his widow and as those guys went through there, they normally came to the Watson Lab as part of the grand tour and I got to know them. Then Meager, who didn't tour very much, of course, was best friends with all of them. So when I finally did meet him and had a meeting or two, why, we had immediate rapport. But, I never worked for him, or with him or very close to the project.

In general, I was at this time both at the Watson Lab days and at the GE days and variances…indeed about the idea of mass production, experienced exchangeable family of machines. In other words, I was first to jump in favor of the 701. Secondly, other IBM machines and thirdly, other production machines like UNIVAC I, and the ERA 1103 because I felt, you know, that the training problem was so much easier. You'd get a junior supervisor from another installation to be your senior supervisor when you started out. These one off machines with no prospect of mass production seemed to me to be essentially self-defeating and even in those days, the ideas of the academics were concentrating on this rather than on how to use the machines more efficiently. It's one of the things I was objecting to. But, as you know, I'm always objecting to something so I won't try to rehearse my old animosities.

These were friendly days in general. Everyone was going ahead so well that, aside from a little sniping between the one off system, like SEAC and WHIRLWIND and ILIAC and so forth, and, of course, there was a certain amount of commercial rivalry between the two or three major companies, the professionals--if you'll pardon the expression-- were all trying to help each other. No question about it. We were all known to each other. There was only a few hundred of us--same general goals. Our measurement factors were a little different in some cases. New knowledge on the one hand, political advantage or bigger grants, or better jet engines, or better--cheaper life insurance policy issues what have you.

Incidentally, that life insurance policy thing brings me back to something I want to go back to on another tape. We talked about how ACM got started and I started to say that Ed Berkeley had a curious history in this. Let me drop out of the jet engine thing for just a minute and go back to Ed Berkeley.

Ed was an actuary for one of the big insurance companies. I'm almost sure it was Prudential but it might have been Metropolitan. It might have been. Anyhow, he was a reasonably respected actuary who had done some work on logical, mathematical logic. He tried to find ways of refining complicated legal contracts by mathematical logic so that contradictions could be exposed short of a law suit or something. And this had gotten him interested in computers and so forth. And what with one thing and another and learning about the Bell System relay machines and so forth, he finally had gotten to the point where he persuaded his company, whichever it was, to invest in a contract for an

early Eckert-Moffley machine before Eckert and Moffley sold themselves to Remington Rand.

MERTZ:

…EDVAC type machine?

GROSCH:

No, it would still be what later became a UNIVAC I but it would be a UNIVAC I at the time when they were trying to make it themselves before they sold the company to Greenberg and before, in turn, Greenberg died and they had to resell it to Remington Rand but it would still be what we would now call a UNIVAC I. And the trouble is, you know, that that machine changed every few months and it never really came to fruition until '52 or something like that. And I'm talking now about '46 or '47. Well, '47 or '48.

MERTZ:

Forty-six was when they first gave their plans to EDVAC.

GROSCH:

This is right. OK. But, this was not an EDVAC because that remained at the Moore School and they had already left the Moore School and were changing it over into what later became UNIVAC I.

Anyhow, I guess it was the first commercial contract they had, even earlier than the totaller saver (?) sort of thing. And, of course, the insurance company put up--I remember the figure--$50,000. This was advance payment on this which, again, looks like a lot then. Now, you know, just buys a few days programming but it was thought of as a lot of money then. And, of course, no machine ever arrived so after a while, Ed Berkeley wasn't working for this insurance company any more. And he turned out, because of his rather strange way of doing things what were then regarded as odd enthusiasms and a rather prickly personality, he turned out to be essentially unemployable as an actuary. So he became, he founded Computers and Automation and became a magazine editor instead and began selling computer kits and all sorts of things like that. And, of course, has survived quite nicely to this day. Meanwhile, of course, his main interest was founding and running the ACM and when his interests became so different from those of the academics that I was inveighing about a little while ago, why, he was finally shoved off as secretary and put aside and is now just an honored charter member.

OK. Back to the jet engine business. I mentioned OARAC. Another outfit in GE that was building computers at that time--I think it was a machine that ultimately came to be CMYVAC. And I can't remember what the acronym was but O was for Ordinance and this was a group that was building heavy, like Navy, ordinance equipment in

Schenectady--Aircraft and Ordinance Systems Division, I think it was called. And it was within that division that GE spawned the space business at a later date. For instance, at that time one of the departments was located at White Sands and was shooting off the old B-2s, headed by Richard W. Porter, general manager, who later became, who followed me a couple of years later as President of the American Rocket Society and is now a very distinguished consultant to GE in headquarters, New York City. But, in parallel to Porter's rather extreme work out in, in firing off B-2s and bumpers and so forth out in White Sands, they had some very down to earth guys making Navy ordinance fire control system out of solid bronze you know, with analog computers that stuck down inside. Big three dimensional cams and designed to stand the shock of gunfire all around.

And these guys somehow got the idea that they, too, should have a digital computer and with a enough support from the Navy or something, they got the funds to build a one off computer which they finally finished and which was called OMYVAC. And, OMYVAC was also kind of a catastrophe. It wasn't a commercial catastrophe in the sense that OARAC was because they never intended to build more than one but it's the typical story of the industrial one off computer. That by the time it was done, they could have bought a 701 for half the price, had it delivered and running and, of course, would have had an enormously more capable machine.

OARAC was wrong for many reasons. One is, in a rapidly moving technology, to make a Chinese copy of another man's machine is ridiculous. Secondly, of course, Aiken was already falling behind in the field technologically for lack of resources and lack of imagination by the time he started the MARK III. And, thirdly, the GE thing was an attempt to buy into the business on the cheap with the idea that ultimately they would have a commercial product. And, of course, they not only had no product at all but it certainly wasn't commercial in the sense of being duplicate able. And they had no follow-on. There was no R&D program going on in the research labs. There was no mass production set up ready to make more OARACS so the whole thing was just an utter fiasco and the only reason they didn't lose billion of dollars was that they only invested a couple of million in the first place. There wasn't that much to lose. Utter ridiculous disaster.

Then there were a couple of study teams in the parent organization in New York City and Schenectady that were trying to do paper studies of the industrial electronics business and I interacted with every one of these at one time or another. OMYVAC the least but I tried to give them advice and we went to internal GE meetings, which were conducted in those days and of which I have transcripts and so forth.

MERTZ:

You had a model 2CPC and 701.

GROSCH:

I had a second model 2 by the time that the 701 came, yes. And after the 701 came, I let model 2 go because the second one had a few more bells and whistles on it. Kept the second one and the 701 going in parallel for about another year. Finally got everything over on the 701 and discontinued the CPC entirely. By this time I had added several pieces of auxiliary IBM equipment, however, that were also quite informed. I had gotten an electronics statistical machine, and IBM 101, which was a great, great gadget and I'd gotten Stan Rothman back to work for me again. Remember he was the one that caused the blow-up in LaMott's area in Washington in '51. Stan had come back to work for me again and was doing some simulation and that one of the jobs I wanted him to do was to explore that 101, not for simulation but for other reasons. Never really got anything out of that.

MERTZ:

(inaudible)

GROSCH:

Oh, this would be about '54, I think.

MERTZ:

Now, was any consideration ever given to the 650?

GROSCH:

Nope, had no interest. I had already originated Grosch's law, you see, years before so my theory was you got the largest possible machine and they kept you busy so I never really wanted to do that. The 101 was a highly flexible counting sorter and since it physically rearranged data, there were things that it could do, physically rearrange cards, that the other couldn't.

One place I did break that rule was that about this time--the time, in fact, that I traded in the CPC, I acquired the electronic box of a CPC, slightly modified so that it would run alone called a 605. And a 605 was a Hoover capacity 604 essentially with a bigger plug board and I used that to do the department bill (?) which was 1200 people. We did this in an hour or so a week and we did a few other things on the machine--data preparation and simple data reductions and stuff simply because that was a card manipulating machine. And, remember the 701 did not have off line peripheries so while you were running a card reader on the 701, everything else had to stop. So if you wanted to do something to each card, like read a card and punch another one over in the card punch, you could do it just about as well for a simple program on the 603 and save the difference between about $1000 a month then and $15000.

Incidentally, I should mention cost. The 701 cost $15,000 in one.

MERTZ:

What did the CPC amount to?

GROSCH:

I've still got a sales manual. I could look it up but it was between $2 and $3000 I think. It depended a good deal more on accessories. The 701 that you originally got--the first models--were a set complemented (?). You got exactly the same equipment whether you were Douglas or Rand or GE. You got two double tape drives--that is, each box contained two complete tape cars unlike the later machines which contained one. You got one drum which I wasn't too enthusiastic about but which we, so to speak, had to take. You got one card reader and one card punch, one tabulator, one box of Williams tube storage and one central main frame. That was it with a console swinging out from it.

Now, later on when you got three standing peripherals, of course, there was juggling to be done and my general rule, in ordering later equipment, was to get the most of everything that I could tack on. That was always my philosophy and even in the 701, I tended to get all the little optional bells and whistles, like selectors in the card punch and stuff you could get even though they were trivial.

The point is though that no matter how you twisted it around, you paid about $15,000 a month. And I remember that my maximum bills in the early days, when I still had the CPC and still had some of this experimental punch card equipment and, of course, key punches and transceivers (?), ran around $20,000 a month per show. Then that was around the time in which you paid 50 percent extra for second shift (?), too. So we kept careful records of our shift stops and had many accounting arguments, some official and some unofficial, keeping it straightened out. These arguments were always pleasant because I conducted them with this fellow, Sam Stan Harwell, that I mentioned who was in Rangers in World War II. When he came back, he was really sort of unfit to be a real honest to God standard IBM salesman. He'd seen life, so to speak. Of course, he got his job back not only because he was a Harwell but also because he'd gone off and gotten himself almost killed in military service and he was a beautiful boy besides--tall and handsome and an ideal IBM salesman type. But, he used to do things like run off and work with the circus in the summertime, for instance, and he had a beautiful Lincoln Continental that he was restoring, an antique car that he was restoring at great price. Twenty coats of hand rubbed lacquer and all that sort of stuff for exhibit around the country. But, he didn't wear a hat. There were a lot of other things, ways in which he didn't conform to the IBM sales pattern but he was an ideal IBM man for me because, first of all, he let me do what I wanted instead of trying to scare me which was typical IBM sales method. And, secondly, he did have, because of his family connections, a lot of good connections back at the center of IBM that were different from mine and that I could explore.

And so when I had problems with IBM in accounting, in charges, in late delivery of specialized equipment, problems of retaining our very good customer engineers--extremely good customer engineers when they wanted to split off and send one off to each of the new 701 shops and so forth-- Stan was a great help to me. I have whole books of pictures showing him installing the 701 in the machine and all that maintenance thing at a time when it was very glamorous…and I remember that he's in all of them…with considerable pleasure.

The physical installation of that machine was interesting. The original plan, when I was hired, was that it was to go in the basement of a laboratory building called Building 300 in Evandale. And, indeed, I put the first CPC in there. The trouble with that was that I soon found that the telephone company had refused to install a very unimportant little minor switchboard in the basement of that building on the grounds that every few years they expected Mill Creek to flood and that building would probably fill if the flood got to be too serious and the basement could fill.

Moreover, it was our intention to chomp up the heavy concrete slab of the basement floor to make channels in which we would run the cables. This was before flexible flooring, of course. And I was somewhat--since the building was otherwise Pretty full--confused as to how I would go about continuing to operate the CPCs in an atmosphere of concrete air hammers. So I went to my boss--remember I was a first level manager and a first level supervisor--and I said, "I would like to put it on the second floor because I had surveyed the building and there were some people on the second floor that could go in the basement although they would be very, very annoyed if they were forced to do so." And he said, "No." So then I went to his boss, a chap named Neil Burgess, who later became general manager of a larger division. He is now, I guess, the top GE jet engine salesman for Hawaii and I asked Neal and he said, "Yes." I guess that's wrong. I convinced my boss and then his boss, Neal Burgess, said, "No." And then I took it all the way up to LaPierre, which was the equivalent of putting my head on the block in the old fashion and LaPierre finally said, "OK, Grosch, you're the guy we hired to do this. If you say second floor, it's second floor." Well, there were no elevators in that building that would take the weight of this equipment. In fact, there were no elevators in this building, period. The second floor was light laboratory equipment in general though the floor loading slabs would hold the machine. I'd explored that and we decided to chop holes in that floor and put the cables in channels in the laboratories underneath which while…not as disruptive as it sounds because there were only a few holes and they could be done rather quickly on week-ends. Putting the cables and stuff up and then putting canvas underneath to catch the debris. So the whole thing sounded very good and we were safe from floods. The only thing was halfway through the life of the 701, we had a flood on the second floor anyhow. The reason for this being that we had to air condition with GE equipment, under the conditions I made of getting later generation equipment was that we not do this. These GE units were hot-rodded. They had originally intended to be 10 tons capacity per box, two five ton compressors and one single set of Edex (?) change cards and stuff and trans (?). And they had been hot rodded up to 7 & frac12; tons capacity each by running them at higher speeds and so forth commercially but they weren't able to run 24 hours a

day like that without breaking down rather frequently. And because these broke down so frequently, we had to have a third box for a total ostensibly of 45 tons of air conditioning equipment installed in this upstairs machine room in order that when one was down, why, we'd still have enough left to run the machine.

While they were installing the third one, some dumb plumber opened a two inch pipe and got 75 pounds of water pressure staring him in the face. The union and the plumber up in the air--all the water bounces off the ceiling and comes down into the machine which was running at the time. So we were off the air 24 hours. We had a wonderful maintenance crew--wonderful. And then all the water ran down through the holes in the floor and ruined the laboratory. Oh, how they hated me. Analog computers, wouldn't you know and I was a loud voice against analog computers. To be more precise, in favor of digital computers of this type and then I ruined the whole analog computer lab. Oh, how they hated me. I told them they should hate the plumber and they agreed they should but the fact was they hated me, too.

MERTZ:

But the machine, the 701…

GROSCH:

Worked fine within 24 hours. We were off for a while. They not only had to dry it out but, of course, there were quite a few, these were vacuum tube machines so there were quite a few cracked bottles and stuff. But, no serious damage. We had no major trouble at all.

And fortunately, the storage box, which was really delicate with these Williams tubes, was hardly touched. It was shut down but…

MERTZ:

Were many of the tubes…

GROSCH:

I seem to remember 6000. I still have brochures. I seem to remember 6000 bottles in that machine.

MERTZ:

Total how many Williams tubes?

GROSCH:

Oh, Williams tubes--36. Maybe a couple spares. I don't remember that but roughly 36. One billion…

MERTZ:

Did they do any--keep any record on the…

GROSCH:

Of course, there were two overflow bits which I invented when I was the 701 consultant at Poughkeepsie, E and Q bits (?) so there must have been at least 38.

MERTZ:

Did they keep any record…

GROSCH:

Yes, but IBM kept it and not me. Yes, that's known.

MERTZ:

(inaudible)

GROSCH:

Very satisfactory toward the end of the period. They were pretty touchy at the beginning but by the time the last people were just starting in the trade reports (?), a few people actually had been replaced by Ford. People who kept the 701 for two generations. By the time the last ones were replaced, they really ran for hours and hours and hours at a time. But they were sensitive. You know, a big vibration in the neighborhood or a failure of the air conditioning or humidity control, or a flicker in the power supply and you were in trouble. One of the things that I was fortunate in was that from the very beginning I spent an awful lot of personal attention and had a lot of good advice about facilities.

So that I always had a better facility than everybody said you had to have and usually it was barely enough. Those early days they really were terrible. They really were.

Now also I had very, very good customer engineers. The manager of the group, the supervisor of the group before that we had at the beginning was named Chuck Mitchell and he's still quite active in high level customer maintenance in IBM today. He was just great.

And then, of course, they sent a special crew down with the machine to help install it to work with and who were right from the design group. And one of the men in that was

Carl Christensen, who later became one of the great figures in IBM magnetic tape development and has graduated to full engineering rank. Very high…so that was a great crew.

Now to get the stuff in we had to lift it--I remember exploring all the alternatives and finally finding a place in one of the big stairways at each end of the building that had an enormous landing and we cut a hatchway through the landing so that we wheeled the machines in underneath this dead spot underneath this landing and lifted it through this hatch up to the level of the second floor and then ran a steel track across the ceiling--bolt it on, of course, on a permanent basis with a chain claw, ran it over and land it on the upper landing and roll it down the hall to the stuff. And I remember the day that the machine was installed, a little IBM man who had ridden in the cab of the furniture truck in which this was delivered, standing underneath the tabulator--the heaviest part of this machine--as it was going up through this hatch and I don't remember his last name but I said to him, "Pete, stand out from under there. If that thing falls, you'll be crushed." And he said, "If that thing falls, I want to be crushed."

MERTZ:

Do you have any recollection of how much down time there was for the machine. How long did it take before you got…

GROSCH:

Beyond preventive maintenance, less than 10 percent even at the very beginning. We had very, very good results from the very earliest days. Practically all the failures that we could really look at were either really casual--in the sense of a single tube going out or a Williams tube storage tube failing or else GE air conditioning failure. We had practically no things like drum bearings going out or tape drives having to be rebuilt or whole sections of the machine.

I do remember one single persistent error in the very early days that caused us a hell of a lot of trouble, a long time to figure. In fact, I think they sent men in from Poughkeepsie--I think we were down for two or three days solid on it and it turned out in the end that the power supply box, which was a separate box on the 701, had a whole bunch of little fuses in it, masses of fuses for the different currents in the thing. And each fuse had a little mechanical widgie that would pop out when the fuse blew. If you were looking at the end of them and the widgie popped out and what had happened was that one circuit had gone dead, due to a blown fuse and the little old mechanical widgie hadn't popped out. So when they searched the panel for a blown fuse, none showed and that was really a tough one. But I think that was the only time we were ever down for more than about six hours except for the flood in the history of that machine. And that was very early in the game when you could really forgive them for it.

[Start Tape 13, Side 2]

GROSCH:

We started off, of course, with the intention of operating about a shift or, at most, a shift and one-half.

MERTZ:

This November-December?

GROSCH:

This would be November or December of '53, yes. And the time from arrival of the machine to official acceptance when you're actually paying rent was on the order of two weeks. True, they worked practically day and night--over 100 hours at the very least per man per week on this--but the fact was it was still a very primitive performance. I think it was probably the fastest installation that they'd of the first six. Afterward, of course, they got to the point where they could practically plug them into the wall sockets and run.

But, environmental problems were serious. I know there were several companies, for instance, Lockheed-EI Segundo which got about number 12, was one where the machine simply didn't work for several months after it arrived because they just hadn't built a suitable building for it or a suitable room. Or, in some cases, they had one coming but it wasn't finished in time. So, we were relatively successful in that area.

And then the engineers were so good and the crew that came down to help install it worked so diligently that after they left and we were on full rental, we were completely satisfied. We never had a bit of objection to it.

MERTZ:

What were the type of things that you did in the way of selection of place other than get it out of the basement?

GROSCH:

Well, I was intent on sound conditioning and air conditioning beyond what people normally talked about in those days. But, there was quite an elegant installation manual, which didn't exist for most of the competitive equipment at that time, which showed you how much space you had to have around the machine and gave humidity limits and things of this sort. This is, of course, where IBM's heavy experience with just ordinary equipment helped because by this time they'd installed a lot of electronic machines although not computers, not computers and the plug in units and the tubes that were in the 604 were the same as those in the 701. In fact, it's this sort of equipment here. That

was what you pulled out and tested on a special separate testing machine which was furnished at the rate of one per customer.

MERTZ:

Excuse me--these were modular (?)…

GROSCH:

Right and, in general, they had a work bench and the boys would rebuild one of those or fix it after they found what was wrong. But, meanwhile they had a spare waiting so that they could plug that in and replace that very quickly once they had identified that one of them was wrong. And the early diagnostic tests, which people like Nat Rochester had pioneered, were already good enough that they could pretty well point out which one of those eight two modules was at fault or, at least, common failures--like a failure in the arithmetic unit or in buffer (?) or a main channel. So you made a quick substitution and then if the machine ran, why, the customer was satisfied and then they went off in the back room and rebuilt the unit. They didn't just throw it away the way they do now but they went back and switched tubes changed resistors and, generally, rebuilt it.

So we were getting along very nicely on one hour of preventative maintenance. I remember one of the accounting arguments that Harwell had to help them solve was that we were allowed to define our working shift as any eight hours practically that we chose. And we tried to choose it in such a way that they could do their preventative maintenance within, what you might call, comfortable plant and working hours without intruding on the time that we wanted to use the machine and there had been some argument about what this did for the lunch hour and this sort. So, we had some controversies and some arguments but, all in all, it was a very happy and successful relationship and the machine ran like a charm from the very beginning.

Now we soon found, especially with the development load of trying to find better ways of using the machine…improving the tool program as distinguished from the applications program, we were up to a shift and one-half and going up fast. Well, I was always good at extrapolation. I plotted up all my budgets and time estimates and stuff on semilog paper, for instance, much to the consternation of the accountants who had never seen this technique used before. And they said, "But, you know, you can't do that because if you look five years ahead, you'll have to have 1000 times the capacity you've got now." "But," I said, "I expect to have 1000." "Where will the work come from?" And I said, "Where did work come from. Only a year and one half ago, we were using a Model I CPC one shift." So they said, "Well, yeah, there's that." And, of course, in the development group, where this kind of argument was used in other areas also, it was possible to have such an argument but I'm sure that this was the one of the reasons that they had so much trouble in Louisville. The guys down there were keeping books on washing machines manufacturers rather than on nuclear aircraft, propulsion of aircraft. And when somebody said they were going to need 1000 times the capacity in a couple of

years of four or five years, why, people just said forget it. But, in our place they didn't say forget it although they gave you a pretty tough argument. So I was always able to look ahead.

For instance, at the time I was up to about two shifts, which still gave us substantial…

MERTZ:

How long was this after you started?

GROSCH:

I can't remember any more but, assuming that it took us three years--assuming that it was linear on semi-log paper and it took us three years to get up to 30 hours a day, which was our maximum, I would guess that we probably got halfway in about 40 percent of the time so it was probably under two years. Probably on the order of the middle of '55. Probably a little earlier than that…it must have been spring of '55 because at this time I said that we were going to have to have more equipment and more…before that new equipment because we were undercover we were finding out with IBM after the 704 coming along, then called the 701a though we hadn't been officially announced yet. I said, "Before this comes and before it can be installed, even if we get the very first one, which is my plan, we'll still run out of 168 hours a week 24 hours a day." So I said, "We've got to make arrangements for that." Well, by this time IBM had put its old prototype machine--the one that we had used for check out purposes at Poughkeepsie--into galactic (?) headquarters, 590 Madison. And was operating on a service bureau basis so I signed up for the third shift on that machine to begin six months later on or something like that. For a whole shift--no one else had bought a whole shift at that time. A few hours here and a few hours there plus a good deal of eleemosynary…me for polysec (?) and celestial mechanics and other academic problems. But I signed up for a whole shift in advance and, sure enough, when the time came that we need it, everyone else was clamoring for time on it, too, and I had a whole blankety-blank shift.
Then, of course, we had to cut back to two and one-half shifts--two shifts or two and one-half shifts--at Evandale and ship a whole shift of work off to New York because that cost $400 an hour, as I remember it. Maybe it was $600--anyhow, it was a commercial rate--maybe it was $600 and I was going to have to pay for it whether I used it or not whereas the rate back at Evandale, by the efficient use of the extra shift that we were working was down to about $250 an hour. So I got some static from our accountants on that but, again, I brought it up through LaPierre and so forth and got him to agree to it. And, in fact, I have a Business Week story clipping floating around here somewhere called "Computing 30 Hours a Day" showing how we were using transceiver networks to send work from Evandale to New York City and back and also from Lynn, Massachusetts and Schenectady to Evandale and/or New York City.

And, that brings me to another first. Very early in the game, we began to use card to tape and tape to card punches which were modified keypunches…that IBM made. We make, from a pack of cards which came out of the 701--getting ahead of the story.

We take a pack of cards that came out of a keypunch in Lynn, Massachusetts with raw data on them; make them, in an automatic machine, into paper tape--five hole paper tape; put the five hole paper tape on Western Union lease line and have it reproduced, errors and all, at Evandale. And then put that into a tape to card machine which would make it into a deck of identical punch cards again plus a few errors that have been added since and then we go to that 605 I mentioned and to, in the early days, the card program calculator then to the 605 and this 701 and other equipment and, as much as possible, eliminate the errors that Western Union and so forth had made by checks--longitudinal and cross checks, redundancy hamming (?) code kind of checks.

MERTZ:

Oh, with the original…

GROSCH:

And then put, what we hoped, were corrected cards into the 701 and reduce the data. Then get the reduced data out of the 701, reverse the process and send it back to Lynn on a 24 hour cycle. Pretty sexy but, God awful, hard. Terrible hard. Errors in every step of the process.

MERTZ:

Yeah, what was the general efficiency?

GROSCH:

The efficiency was that the experiments being run in the test cell frequently were unsuccessful and you couldn't tell until you plotted up the reduced data. And when they reduced the data in Lynn with desk calculators and slide rulers, or even the CPC, it took a couple of days or so to do it. Meanwhile, your expensive experiment had to sit there in that 60,000 horsepower test cell, not running because you thought it was done but not daring to tear it down because maybe you weren't done. So, all the expense of this thing, including $1500 a month of lease line expenses on Western Union, were trivial compared to the savings.

You see that's why it was easy in the early days. Everybody tells you how hard it was when this trade was new but, in fact, it wasn't. It was easy because you had all that beautiful cream to skim off. Also you had very bright people because the dumb people hadn't come yet, huh. So you had very bright people and nice, clean--not clean but nice, obvious problems with big savings to be achieved--great. Paradise.

MERTZ:

Wasn't this, to some extent, justification for leasing this much more expensive…

GROSCH:

Yeah, yeah. You see, I was actually cutting back on the $250 an hour time in Cincinnati for a while. Of course, ultimately it all built up again and before the 704s came, we were running 190--I can't remember. One hundred ninety--no, no, more than that. Theoretically 280 hours a week. In practice, we had enough casual breakdowns and inefficiencies that it was about 200 hours but we were running 200 hours a week by the time the 704 came. And I got a lot of medals for that because, you know, not big medals. Nobody took me up and gave me a Coffin (?) award or anything but the point was that I foresaw the trouble and it was a serious one.

Pratt & Whitney was just as foresighted, just as intelligent people running the shop but they weren't able to get their management to back them quite as well. It's a bigger company and it was a little stodgier because they had been in the piston engine business for a long time before they built jets. And Walter Ranshaw, the man who had been my student and his boss, Duke (?) Crossman, were at least as clever as I and my people were but they couldn't get their bosses to move as quickly as I could get my bosses. Maybe they weren't as good salesman. But, one way or another, they got resistance so when they came down to buy a shift of time at IBM Headquarters, it was gone. I had already bought it six months before.

MERTZ:

When was it that the 704…

GROSCH:

Well, the 704 was announced formally--that is, we were all called up to a meeting in Endicott to have this unveiled to us. This would be, what we would now call, the share organization and it wasn't quite formalized at that time. It was the people who found a share within a few months but it wasn't actually. The name hadn't been chosen yet.

That would have been in early '54. I remember the other machine had only just been delivered in late '53 but it would have been in early '54 nevertheless that they did this. By early, it might have been summer but it was, you know, 54, not '55. I got delivery of the number 3 machine in January 1956 and it was more than a year between formal announcement and the first delivery. And the first delivery was a couple of months ahead of mine so working backward from that, I would say the first delivery was probably November of '55. And, therefore, the disclosure was probably like September of '54, something like that. I remember it was a good time of year.

I remember I drove to Endicott. The disclosure was at the old IBM--where those conferences and forums used to be held--Education Building and I remember I drove in my beautiful Chrysler convertible, which had Ohio license plate GE-701 on it, and found all the GE prefixes were reserved for the GE Cleveland plant that made light bulbs so I applied to the guy that had had 701 for years and got it…He sent me down his plate. So I used to drive around the country with GE-701 on my car causing quite a shock when I got to Poughkeepsie, rather Endicott, and parked it in front of the hotel. Besides the fact that it was also the best car there.

Well, any who (sic), we got to this meeting which was to disclose the operating characteristics and price of the 701a, later to be the 704. And it was an all week meeting and we were there for a day and one-half and they kept fussing around giving us stories about how wonderful the time clocks were and how wonderful the IBM research program was but no story on the 701a. What was going on behind the scenes, of course, was a knock down, drag out marketing battle between Hurd and the accountants and Tom, Jr. and half a dozen other major forces on the final details of the announcement. This was before the anti-trust consent decrees had…so there was no legal reason why they couldn't…

The simple fact of the matter was that they hadn't quite decided what to tell us and whether to tell us anything. But, here we were--all taken away from our expensive jobs, coming from all over the country all over the world practically to hear this news. So, 25 of us got together representing I think it was something like 18 of the 20 existing installations--the one in New York City, of course, was still in IBM and, I think the weather bureau wasn't finished yet. But the rest of us got together and I drafted a telegram to Watson, Jr. which John--took around and got everybody to sign. And I still have a copy of that telegram somewhere. I don't know exactly where but it essentially said, you know, that we represent something like 10 percent of IBM's gross business and will you get Cuthbert Hurd out of this supposed bed of his. He was supposed to be sick with the flu or something. Get him out of bed and get him up here with that 701a information…Sent this off in the late afternoon and the next morning, here's old Cuthbert, white as a sheet, passing out the pamphlets. I tell you we didn't pull any punches and that was the reason the chare was founded. Not that particular incident but that sort of thing. The purpose in SHARE is not to share programs. That's a lot of crap and…

Inaudible Comment

GROSCH:

Well, it's a good time to do it anyhow. It's an amusing story but it's an exact replica of the troubles that we frequently had that IBM would pull one of its old IBM style tricks on us, you know. And that was, of course, an egregious one because of the expense of the attendance and the fact that we all came on such great hopes of hearing all this wonderful

news because we were all dying for more equipment. There was hardly a soul in that meeting that didn't need--that wasn't running two shifts and needed a lot more.

However, there were problems in those days. One was the problem of pricing. Obviously, $15,000 a month had not anywhere near covered the cost of operation, especially the heavy maintenance on the Williams tubes, especially the large staff of men required. When we were running around the clock, we had I think something like eight maintenance engineers full time. The original crew was four. They didn't need quite that many for each shift. But, on the other hand, when you're running around the clock, you still have to have two guys. Both sides had a rule of not having one man, one engineer alone, in the installation probably because of the danger. He might grab 1000 volts and kill himself. There were high voltages in the Williams tubes box especially and partly that, you know, you frequently needed a guy to hold the wrench here while the other guy hits with the hammer so there were always two men. And by the time you're running four shifts, which is what it takes to run 168 hours, you're talking about eight or ten men even if they work some overtime. So there were extra costs.

MERTZ:

Doesn't leave much margin for profit.

GROSCH:

That's right. In fact, IBM, although they got a tremendous reputation out of this machine and, of course, it had in turn bloomed their 702 commercial machine sales--and the 702, by the way, GE put one of those into Hanford (?), for instance, which ran also before the Louisville UNIVAC I ran. The 702 was supposed to be the original tape processing machine that I described. The machine that Rochester and Ralph Palmer (?) and others…had designed before it turned into the defense calculator. It was character oriented so that you could address a single character and it was alphabetic in nature and there was a prescribed binary code for each alphabetic character and so forth. So the 702s had been priced a little higher and they had gotten, of course, an enormous commercial advantage from their regular machines out of the publicity and all that.

The fact remains that when you're losing, you know, several thousand dollars a shift on 20 machines and they're all running three and four shifts a week, you know over a year or a couple of years you're talking about substantial amounts of money. So they were anxious not only to get more machines out because they decided to terminate the 701 but they wanted to get them out so that they could get some money back for a change.

Now the termination of the 701s is a case in point. This is still a first generation machine, mind you. The 704 was one and one-half generations. It was not transistorized.

MERTZ:

By that, you mean…

GROSCH:

It was a tube machine. But it was a successor to another tube machine so it was, in a sense, one and one-half generations. And the reason they terminated the 701 was not that they couldn't continue to use the 701. It was not outmoded, nor was the 704 all that much faster. It was just that the 704 was going to call twice as much for not much more machine.

MERTZ:

Well, what was the rental?

GROSCH:

Oh, $30,000 a month roughly. It depended on the machine. I got a very large complement of stuff. I got all the tape drives and a couple of printers, and all the bells and whistles and I think mine was $43,000 a month so it's almost three times as much and I ordered three of them. Not one but three--boom.

MERTZ:

Three for Cincinnati?

GROSCH:

Three in general and the intention was to put two in Cincinnati and one in Lynn but in the event what happened was I put one in Cincinnati then took one with me out to Phoenix and that the third one, indeed, was installed elsewhere in GE but I think not in the building I left behind me after I left the advance slowed up a little.

But at the time I did it, I was designing building a new building that would hold two of them and I intended the first two to be delivered almost simultaneously and both go in there. Then when I discovered that my workload and cost of maintenance, and so forth, wouldn't justify that, I decided to put the second one into Lynn. Oh, no, that's where it went. I used it. I forgot. I installed both of them and I took the third one out to Phoenix with me. Forgotten how many machines I've known.

MERTZ:

Let's see--one was in Lynn, one was…

GROSCH:

Yeah. Let me see, let me finish the share because it's only a few more sentences. Then we'll come back to these other machines.

The share organization was started by about 12 guys, almost all of whom were associated with aerospace in one way or another. That includes a couple who were not like the National Security Agency and Los Alamos but otherwise it was two engine companies--Pratt & Whitney and General Electric, Evandale--all of the major aircraft companies--Boeing, Lockheed, North American - two plants, Douglas - three plants. They had Douglas, Santa Monica, the pioneering one, Douglas, El Segundo and Douglas, Long Beach and there's North American, Englewood and North American…Any who, and Boeing out in the northwest and this added up to…

MERTZ:

(something about St. Louis (?))

GROSCH:

Not yet nor did Drummond nor Republic had equipment of that size yet. They came in the 704 area. McDonald not yet, either. No, this was before McDonald.

MERTZ:

Did Hughes?

GROSCH:

No, Hughes didn't have the 701 and neither did Northrop in spite of the fact that Northrop had pioneered the CPC. They never quite got a 701, as I remember. But, anyhow I came pretty close to 12 there and there were probably a couple I did forget. Almost all aerospace, as I said.

And we got together in I think it was John Lowe's or Jack Strong's motel room in front of the Western Joint Computer Conference or an ACM meeting and we were grousing about this experience that we'd had on the announcement of the 701a. And we said, "Why don't we get a little more formal organization together."

The SHARE minutes would show it, to be precise, if you want to look at that as part of your historical activities. I would guess offhand it would be quite early '55.

MERTZ:

And this was on the West Coast?

GROSCH:

This was on the West Coast I believe that we had this thing. It wasn't a real meeting in the sense of a constitution and bylaws and officers. It was just a bunch of guys saying, "We ought to do it." And those were the real founders of SHARE. Now the group then got together more formally. In fact, I wasn't present at the more formal one. I sent a delegate and they elected a slate of officers and a group of men to write up a formal agreement of some sort. And Fletcher Jones, who's now the Board Chairman of Computer Sciences Corporation, founder and Board Chairman of CSC, was the first secretary. And he was the secretary in the sense of keeping minutes and Harding working amanuensis; not a secretary in the sense of executive director.

We did not, at the first meetings, have a regular constitution, bylaws sort of thing because we didn't think it necessary. But, later on General Electric and Pratt & Whitney lawyers forced us to do it on the grounds that we were competitors and that by getting together in a non-formalized organization that we were subject to anti-trust criticism. And it was those two companies--mine and Stu Crossman's--that we really pioneered in doing this.

One of my sub-section managers, Donald Shell, was one of the most active early members in it and probably could have forced his 704 compiling assembly techniques on the rest of the trade instead of waiting for IBM to deliver them if he'd wanted to be more forceful about it since we had developed our own software for the 704. And I'll come to that in a moment. But, he was also one of the founding fathers. I was only one of the suggesters. I was never really too enthusiastic about any of the formalized SHARE activities with the single exception of putting pressure on IBM.

MERTZ:

What was the purpose of SHARE?

GROSCH:

Screw IBM up as much as possible. A unified front against our great white father but also our very rigid supplier. There were so many occasions in which we would find, for instance, that one party had negotiated a letter agreement with the local managers permitting him to do something or other that the rest of us didn't know about. And when you asked for it, the word was, "Why, no, we can't do anything like that. The company rules say so and so." And if you could then say, "But, here's the agreement that Lockheed just signed with you people two weeks ago," you, of course, invariably got the same treatment. And to a considerable extent you were told rather bluntly by the local manager that this wouldn't be…the district or local manager in Cincinnati that, you know, this was contingent on your keeping your damned mouth shut. If you started telling all your friends about it, the way Lockheed obviously had done, why they might have to withdraw the agreements and everything. So by having the formal, not formal, but by having a universal organization, one in which we all belonged, why there was a place in which we could dump these experiences and when we got into a really nasty bind…like…bundling

thing nowadays would be a perfect example. We could have expressed a single opinion on bundling versus unbundling. And that was the peak of customer influence versus the IBM organization. There was no time…

MERTZ:

This is quite a threat to IBM?

GROSCH:

Yeah and, of course, they responded in the obvious fashion by taking over shareholders (?) they dominated for the last ten years. But, that was a time in which that group of people--those 20 701 customers--allowing for the other parts of their organization, which had enormous quantities of punch-card machines and smaller computers--650s and so forth and so forth--not represented by those men particularly but known to those men and subject to influence by those men. Those 20 companies or organizations put together constituted 10 percent of IBM's total business which was already in the billion dollar a year category. Or you could see this from the fact that at that time the GE bill, for instance, which I, of course, did not control single-handed but which I nevertheless represented in that outfit was $14 million a year. Now we were the largest but you multiply that by ten other organizations, or 12 other organizations almost as big and you have pretty much the $100 million mark and then you add the other eight or ten who didn't help found share but who joined right away and then other outfits, like 650 users and such, that wanted to get aboard in some associated fashion which we didn't permit at first and you're talking in terms of much more than 10 percent of the business. Now, of course, no group that small represents that much of IBM's business. And, in fact, I think probably all the SHARE customers probably don't represent it anymore but SHARE has been taken over so that it's not able to speak with that…

MERTZ:

How did IBM manage to take it over?

GROSCH:

Oh, by being nice to us.

MERTZ:

Killing you with kindness.

GROSCH:

Killing us with kindness. They said, "Whatever it is you want, we'll do. Now, we'll send representatives to the meeting and you tell us. And, of course, you wouldn't object if our

own people who run 701s join up, of course." We wouldn't dare-nice people, you know. They also have problems. They tell us their problems, we'll tell us our problems. Well, pretty soon, you know, you had a substantial number of IBM guys inside and IBM paying all the bills and doing all sorts of free things for us, publishing all sorts of special magazines and exchanging newsletters and sending us free copies of all the brochures and then one day, you all of a sudden discover that, you know, there isn't any fire left in your organization any more. And all they're doing is trading programs around at IBM's expense. Nobody sends any telegrams to Mr. Watson anymore. Well, they do it in really wild cases. They expressed themselves at the time of the unbundling action and so forth and so on.

MERTZ:

(something about recent)

GROSCH:

Much more recent. Moreover, IBM is now so huge.

MERTZ:

Do you want to describe that now. You've referred to it.

GROSCH:

Yeah, yeah. Let's go back to the 704 thing instead…I'm not sure you even want to do it as part of the history because it's so recent.

But the 704 thing is interesting because it marks the transition period from the unusual IBM equipment to everybody having IBM equipment sort of thing.

The 701a, or the 704 as it became known, was essentially characterized as follows. It was a hell of a lot more expensive for value performed. It got them back on the Grosch's law or theorem (?) essentially. It had independent peripherals which meant that you could do a lot better job of data preparation and data analysis and stuff or for commercial applications on the 701. Now that's not so important for most of the people--most of those 20 customers weren't doing any kind of mass production data processing. It was almost all scientific and technical work but I, for one, and many others, to a lesser extent, wanted to do inventory control and fancy stuff with a lot of input-output. And we also looked forward to a day when we'd operate an assortment of machines, some of which would be 701 types and some would be 702 types and we'd exchange tapes and all that sort of stuff. So, this being clearly in mind we were all concerned to have more efficient peripheral equipment operations, especially we wanted to be able to run the printed diagnostic films and things of this sort without tying up the whole machine because debugging was getting tougher by this time and you didn't want to waste the time on the

main machine holding up memory printouts and stuff like that. So, this was welcomed, in spite of the higher price, with great eagerness.

Also, of course, they introduced this time the magnetic core. The Williams Tube died at this point in the regular IBM equipment and with all of the good experience that I have had, there were many other installations where the Williams tubes had been a continuous source of trouble. They were delicate. There was no question about it and if you didn't have really hotsy-totsy air conditioning and stuff, you really did have a lot of …

MERTZ:

Now, this is the 704?

GROSCH:

So, the four was fours (?), larger in memory size, double memory sized.

MERTZ:

Now this meant what?

GROSCH:

Well, I was operating a double Williams tube storage by then. I had the first, as I remember it, double Williams tube memory so I was already at 2048 words. And then they announced that the standard core size for the 704 was to be I think it was 4096 so that was four times as much for many users and twice as much for me. Moreover, they then announced a little later than that--just before delivery of the first machines-- we could have two boxes of that so that you, in fact, had 8192 words and that's 16,000 instructions if you used it all in instruction. And you could have a large number of magnetic tapes, for instance. You weren't restricted to a set number. You could have half a dozen or a dozen magnetic tapes. Only one to a box…and they ran faster. And you could have a couple of printers, which I ordered…So it was much more flexible.

MERTZ:

Any of this terminal equipment new?

GROSCH:

Well, the tapes were an entirely different design and much tighter packing. I no longer remember the figures but they were at least twice as many discs per inch. Still the same old…and only one drive per box.

MERTZ:

How about the printers?

GROSCH:

I don't think the printers were changed much, as I remember. In most cases, they were remodeled 407s, re-cased 407s, which were quite satisfactory but, you know, held up the main machine when they were integrated. Much of this is available in the literature. It's just that, you know, it's vague in my mind now after 15 years.

But, we made a very close study of this and decided that, indeed, we wanted to get lots and lots of everything. Of course, that was my style but my bosses and my managers working for me both agreed on this and we had a little plan…

Now it was my intention to order a lot of them and it was my intention to get the first couple so I ordered three with the specifications that I should get numbers 1 and 2 and then a later delivery when I needed the third one. Now there was a story behind this. And it's a good yarn.

This was still in the days of, what I call, the Oklahoma land rush technique. The first guy to give them an order got the first machine. Well, when I attempted to order the first two, the answer was "It's too early yet." And then the next answer was "Well, we sort of promised it to some other people, too, but they haven't given us a firm order and you have." And then the next reply was "Well, you know, there is some question of military priority." And then the next reply was, "Well, we're going to keep the number 1 machine ourselves" so we're getting pretty wild. Meanwhile, the same thing was going on at Pratt & Whitney, and Lockheed, and Rand Corporation and several other places. So, pressures were mounting pretty fast.

I was angry with them because I had attempted to get a graphic output device, a big cathode ray tube screen that you could plot graphs on, like that balancing raw figures on there. (?) And also with a camera device to photograph a high intensity small screen for permanent record for much of our test data records to keep very nice.

MERTZ:

Had you seen any of that equipment at Rand or MIT?

GROSCH:

Yes, indeed.

MERTZ:

They had them.

GROSCH:

Sure, sure.

And so I'd attempted to order this only to find out that they were building one for dirty old money for the Rand Corporation. Rand had given $50,000--and for all I know a bigger contract--to build an experimental one. And I was mad as hops because I'd never been given the opportunity to do this. I said, "I would have done that. I wanted one of those. You never came down and asked me. You went to Rand." So, finally to calm me down, why they agreed that I could have the first production one…

MERTZ:

The first…

GROSCH:

Cathode ray tube output but it turned out that it was never offered as a production device on the 701. It was offered on the 4 so I ended up by getting one on this early 4 instead.

MERTZ:

704?

GROSCH:

704, yeah.

Now I ought to go back a little bit and mention another thing. I started to tell about remote data reduction on the Western Union lines on card to tape, tape to card machines and the terrible number of errors and stuff.

MERTZ:

You were using standard teletype tape?

GROSCH:

Yeah. Five channel teletype tape with regular leads to Western Union telegraph line, not a voice line. Not a Telex or fancy line. It was the best you could get in those days from Boston.

So I discovered through my underground connections that they were building these special gadgets called a transceiver which was a combination of a sending and receiving

machine that looked like an old 29 keypunch…modern looking equipment which was going to use voice frequency division techniques for communications like touch tone telepathy which was then available which would use telephone lines which would have a great deal of error checking built into it. So that while you might not get absolute perfect results, it would be 100 times better than the poor old things that I'd been using so far.

But they were making the first hundred or so for the Air Force and then maybe after that they would take orders. And, in fact, they…commercial announcement.

Well, by this time I had gotten this card to tape, tape to card punch thing pretty well known through the trade and IBM was quite keen on it. So I said to IBM, "Now, I'll make you a deal. I'll put you on publicity for this thing. I'll get the GE publicity people, if you want, to take charge of the thing. We'll have a big press conference and we'll show a GE vice president feeding some data into a transceiver at one end, like Lynn, Massachusetts or Schenectady" and, in fact, the guy I produced in the end was Glenn Warren, the vice president of the Steam Turbine Division. And I said, "You produce Tom Watson or some equivalent and we'll have him taking the data out at Steubendale (?)."

MERTZ:

(inaudible)

GROSCH:

Yeah, that's right. Well, you know. I knew what I was saying, anyhow. You're quite right and, in fact, that's what they did and I still have the flat sheet of pictures showing Glenn Warren standing by while they feed data in Schenectady and Tom Watson standing by while we take it out in New York or something. The condition was, however, that I get a couple of these transceivers, three of these transceivers right away, right out of the Air Force production. Of course, I paid for them but, you know, these were the ways that you could screw up the system if you knew your way around.

Now the guys at Pratt & Whitney didn't even know the name of the PR man that I went to but I knew it.

MERTZ:

This wasn't shared with SHARE?

GROSCH:

This wasn't shared with SHARE. No, I gave them a copy of the photographs. Well, you remember I said I was always a maverick. I wasn't all that keen on SHARE really. I helped and I'm glad to say I was a charter member and all that but I could do pretty well

without SHARE. SHARE needed my kind of needle and I was perfectly willing to provide it but I used it on my own behalf privately as well.

So I had this transceiver thing going and I'd gotten very good publicity for IBM and for GE on that. And that's in Business Week in 30 hours of…and I needed it, of course, when I started shuffling this data around between the extra shift and (?) the 704 as well.

MERTZ:

Were you still using that prototype 701 in New York after you got the 704?

GROSCH:

Yes but there was one problem on that. The prototype machine in New York had been rigged up to use a double box of memory like the one I had in Evandale. The one reason that we needed to go there was there were only three or four machines in the country that had double memories and some of our programs would only run on double memories. And when we were halfway through the use of that shift--just about the time the 704 was coming--darned if they didn't break off one of these boxes without notice to us and send it off to former 20th machine for the U.S. Weather Bureau or something. So I sent one of my famous letters to…and Tom Watson, Jr. saying, "What a bunch of schnooks they were at…and how I was going to do this and I was going to do that…And in the end I closed down the second shift operation, the third shift operation there and bought time I think from General Motors in Detroit which was a one box machine and which did have some time to spare. And I sent only one box programs up to it. And I managed to stagger along and I used that after the delivery of the 704. But I did not use the New York machine after the delivery of the 704. I used an extra machine but not the New York one.

And we were very successful in getting our stuff switched over to the 704. The 701 load dropped very rapidly to less than two shifts within just a few weeks of the 704 arrival because we carefully laid out two or three of our major time-burning problems and checked them out on the 4 before delivery so that when we got those transferred, our load dropped way down on the 701. And then we could take our time about transferring the others, Because the 701 actually did digital multiplication just about as cheaply as the 704 did. It's just that we had run out of time on it, because of its lower rental rate, which is still a very economical way of using equipment. And I had building to building by this time so I didn't need the space, physical space, as many of the customers did.

OK. Now we get back to the Oklahoma Land Rush of the 704. I needed this diversion to point out the fact that I had gotten pretty skilled at manipulating IBM but, you know, other people were also learning these tricks, too. So it became obvious that half a dozen other customers were trying to get those first one or two machines. So IBM said, "New set of ground rules, fellows. We've just let ourselves get too confused. It's our fault. It's not yours but we've just taken too many letters of intent, too many this and too many that and we made too many promises that we can't keep so we'll start over again. We're going

to have a certain day on which we're going to announce the firm, final prices which are slightly different than the ones we've leaked to you. We're going to announce these at the opening of business on Monday morning and the first person after that time who gets to his local IBM office with a firm legally committing purchase order with the figures and everything on it--not just a blank purchase order--properly filled out, properly signed and so forth gets the first machine. And the second one gets the second machine. And if you all agree to this, we won't have to throw it into military priorities and we want you to make sure that nobody disagrees." They were thinking particularly of Pratt & Whitney which was roaring mad by this time…fate of the whole United States government depended on the machine. Mostly because they were mad at me, I think. So we said, "Fine."

And I had at that time--Stan Farwell (?) had left to become a big shot possibly (?) and wear a hat and gloves elsewhere in IBM. Tom Watson, Jr. had called him in and told him, "Stan, you're a wonderful guy and I love you personally. And you have a wonderful career ahead of you in IBM but you've got to shape up." And he gave him a list of things he had to do--like wear a hat, for instance, quit running off with the circus or to be a lumberjack in the summertime and sell that damn Lincoln Continental. And Stan stood there like a little man and said, "Yes, sir. Yes, sir."

MERTZ:

This is Tom, Jr.?

GROSCH:

Tom, Jr. And Stan came right back and wept all over me telling me the story. And I said, "Why didn't you tell him to go to hell?" And he said, "He would have fired me." I said, "He won't fire you." And he said, "Well, he might have." And I said, "Yeah, he might have." So he said, "Yes, sir," and became manager of transportation, department of transportation. Later on they went and made him branch manager in Denver…

Well by this time they thought enough of me that I was asked who I wanted for the next salesman. So I said I'm looking for a person who will have lots of good channels into Cuthbert Hurd's organization, the engineering organization, research lab which was then building up. I said I know how to use a salesman as far as filling out the forms are concerned. What I want from you now is a guy that can funnel in information that I'm not getting out of date on and would like to have refreshed. Fine, they said, we will send you Cuthbert Hurd's number 2 man which was the best. So they sent me a guy named Liston Tatum. Liston had a degree, a doctor's degree, but it was in something like public speaking not in physics. A wonderful guy. Clean living, decent, honest, ideal IBM salesman. He believed every word of those lies that they made him tell and a very, very fine fellow. And I liked him very much personally. I've known him for years. He's a wonderful person--a wonderful choice. So he and his wife were moved to Cincinnati to be my full-time salesman--soothe me and funnel information into me and a wonderful

guy and I was very happy with their choice. And I think it was good for him because he was being pushed aside by more sales oriented types in Hurd's organization. At least, he said he was happy.

Well, now Liston was very much aware of the dangers of this Oklahoma Land Rush approach so he hovered over this. He was making phone calls every hour to friends all over the Applied Science Department who were trying to get a machine themselves, by the way, and tapping all sorts of sources in the company. And while I was away making a speech somewhere on a Friday night, he got word that the rules were changed. And when he opened this envelope containing these new secret prices Monday morning, he was going to find some new rules on top of them. And he was told what the new rules were. The new rules were that because of the time zone problem, which had been called to their attention, that the first customer to get a signed contract in the New York office--that's not the exact phrasing. The first person to get a signed legal price contract in New York after the opening of business on Monday morning would be the head of the list and, moreover, to make this possible that you could open the envelope on Saturday or Sunday, or what have you, you know, so that everybody would have time to get their vice presidents out of bed and get them signed. Imagine getting a vice president to sign a purchase order on Sunday night, you know. Well, the enthusiasm was such at that time that a lot of them were willing to do it. The thing is that my boss has delegated authority to me so I had the authority to do this and I had already pre-signed all the forms. All that Liston had to do was type in the numbers on the spaces provided.

MERTZ:

If he went to New York.

GROSCH:

He said, "I've got the prices. I've typed them all in Cincinnati and I got all the papers legal and here I am in New York waiting for business to start tomorrow. Moreover, I read this thing over 50 times and it says New York. It doesn't say World Headquarters and the Wall Street office opens at 8 o'clock so I'm going to be at the Wall Street office at 8:01 and hand them the papers."

And he did and at 8:30--the main office opened at 9:15--Pratt & Whitney is standing outside the door with their sheaf of numbers. And to make matters worse, they get in and the guy says, "You can have numbers 4 and 5." He said, "What do you mean 4 and 5?" He said, "Well, Grosch has got numbers 2 and 3 down at Wall Street at 8 o'clock."

[End of that story]

[Tape 14]

GROSCH:

…There were still some small adjustments made in the long run. In spite of all my chortling about this, I didn't get the number 1 machine. In the first place, IBM did reserve the number 1 machine for themselves for check out…And they said, "We'll take a production machine this time so the check out can be more reliable." And then Pratt & Whitney threatened to throw the whole thing into military priorities because they were so extremely angry at this change and so forth. They really were in this case more angry at IBM than at me. I remember from conversations in which we laughed heartily about my scheming…because they said, you know, they had no business changing their signals the second, third and fourth time. It was bad enough they changed it the first time. So they did threaten to throw it into military priorities and Cuthbert Hurd personally came to Cincinnati and essentially got down on his knees and begged me to reconsider. And I said, "Listen what's the difference in delivery date." And he said, "We can squeeze the delivery time to number 2 and number 3 machines to a difference of only about two weeks." And I said, "All right. I'll take number 3 instead of number 2 and you can give number 2 to Pratt & Whitney. However, I want the first double memory. That would 8,192 words…the second box of fours. I want to be the first machine that has that and I want to get the first production cathode ray tubes device that you've been promising me on and off for the 701. And I want lots of assistance from you on publicity on getting the thing in. Moreover, I may want some special favors from you on check out time because we're very anxious to really get going. We really have an enormous load. I know Pratt & Whitney does, too. They're not conning you but we, too, have an enormous load and I want to transfer it as fast as possible because I'm running out of 701 time in Detroit." And he said, "We'll do everything you want, Herb. I'll give it to you in writing just let Pratt & Whitney have the number 2 machine." And I said, "I will." And I did that on my own without the bosses and everything…

Well, actually what happened was that due to the extreme skills of a man named Donald Shell, who's still in GE but not in the computer trade anymore--not in the computer part of GE anymore--we actually worked up a whole tremendous…by those standards--small now but what I regarded then as our own tremendous software package in advance of delivery of the machine in the intervening year. This was the General Electric compiler and assembler program.

MERTZ:

(something about name and what was the delivery date)

GROSCH:

I knew you were going to ask me that and I can't tell you for sure. It was roughly the winter of '54 or '55. I would guess maybe January of '55 but I'd have to look in the books to really tie it down.

MERTZ:

…Would it be three or four months after the announcement…

GROSCH:

The 701a conference at which we were all screaming and so forth which was in late summer or something like that did not disclose prices or anything like that. Remember, this was before the anti-trust action so they disclosed the structure of the machine, the characteristics of the various boxes and how many boxes you could have but not the prices, not the…final delivery dates. And, in fact, then they leaked us prices on and off in the next few months subject to change and not for everything at once and so forth.

MERTZ:

(inaudible)

GROSCH:

Well, I remember that my machines--the big machine, the first one I got--was, with all the bells and whistles and all the maximum tape guards and everything, $43,000 a month as against $15,000 for the 701. But, of course, that was more equipment. We got more stuff for it and that was for a double memory again. That was without, however, the double memory. That was 4,000 words, not 8,000.

MERTZ:

And which model now--which memory was that?

GROSCH:

That was--they didn't have models in that sense of the word. They were all just called 704s.

MERTZ:

Right. Number…

GROSCH:

Well, what I ordered was this. My original plan was when I placed the order was to get three machines of which the first and third were to be maximum capabilities and the second one was to share some of the peripheral equipment and so forth of the first one. So that the three didn't add to $129,000 a month. They added up to about $110,000 a month, something like that. And I was able to sign off on that because my boss had given

me advanced authorization in connection with the building we built to house these machines, he'd given me advanced authorization to sign off according to GE management principles, you understand, nothing special. But he authorized up to the limit. I think it was $250,000 and my view was I had gotten--again an example of the kind of politics that you had to play or the kind of maneuvering you had to do, I had a signed letter from the local IBM manager in Cincinnati. Remember, this is from the time when they had a fixed contract so it was very difficult to get any answer from them (?). I had a letter from the IBM manager in Cincinnati confirming the fact that as a GE national account of long standing that I could discontinue any and all of my IBM equipment at one month's notice. Normally, it's 90 days or was in that case. I had a 30 day cancellation clause by special agreement with the managers. Since other IBM equipment would go on elsewhere in GE and they always permitted you 30 days notice on small changes in the accountant work (?), something like one keypunch or something like that. So he extended this to the three 704s when they came in. The result was that when I signed off on that purchase order for those three machines, I was only ordering $120,000 worth of equipment--half…The fact that we had to build $650,000 building to house them and that we were doing half a million dollars worth of software to get ready for them and that we were going to be tied to them for the next three years…I followed the rules.

MERTZ:

(inaudible)

GROSCH:

In fact, I asked my boss, Dave Cochran, to countersign the purchase order and he did. But, he just laughed and said, "It's not necessary. You just want to hang me, too." And I said, "Yeah, the thought occurred to me."

MERTZ:

Then after the ordering…it was over even before you got…

GROSCH:

Yeah. During this period three people--Donald Shell, Jim Porter who is still very active in the GE G-Coast (?) software out in Phoenix, and a woman named Dorothea Clark, Mrs. Dorothea Clark, who still works with Jim out in Phoenix--about 2½ man years of effort, three people…little less than full time… for the whole year. Two and one-half man years of effort, not only wrote this whole new software package to accommodate itself to the intricacies of the different machine. Parts of the word size when we'd written other software stuff before that and we weren't forced to do all the peripheral maneuvering and stuff. IBM was furnishing that but this was a compiler and an assembler, balance subroutines for generating mathematical functions and so on borrowed from our earlier 701 work. Floating point, by the way, was wired in now so we were able to drop out the

floating point that we had to write for the 701 and so forth. And then on top of that they not only did that and checked it out on the sample machine quite late in the game but, in addition to that, they wrote the applications program to go on top of that to be assembled and compiled by that software for our two major jobs which were jet engine performance calculation and data reduction.

MERTZ:

Well, isn't there about this time some impetus for obtaining…

GROSCH:

Well, this is where SHARE came into its current form. What we did with SHARE, or what they did with SHARE because it was mostly Don Shell who represented me on this problem and I myself. I got into the social act and that was about it. But what Shell and the others did with SHARE was to specify things like these peripheral programs and so forth that they thought would be needed for the 704. And then IBM agreed to write them to their specifications and, in fact, at the time the 709 was announced, which was the successor to the 704 a couple of years later still, the 709, the whole software package called the SHARE Operating System, SOS, was…I forget the acronym it means anymore was done by IBM to the tune of 100 man years of work to the specifications written by the SHARE Organization which had also specified most of the pages in the 709 hardware. And it was a fiasco. IBM didn't write the software well and the specifications for the hardware turned out to be poor. IBM built them all right but a poor set of…

MERTZ:

Well, wasn't there…

GROSCH:

So that sort of …SHARE up for keeps.

MERTZ:

Well, yes, but wasn't there some possibility of--I'm not sure about this--that after time the proposal was made to IBM which indeed they had all agreed and what already had been done and had said to IBM, "Well we've done it. You don't need to do it." This might have altered the future course of…

GROSCH:

Yes, I think this is right. And to a certain extent, Donald Shell was responsible for that because he had done more than any other person, he had done and checked out and was

trusted to have done more than any other single installation there represented had done so far. But, Don was perhaps humble is not exactly the right word. He's an unassuming kind of guy. He's not bashful. He's very sure of himself in what he's done but he doesn't push himself. And I left it in his hands. I told him "Don, this is up to you. If you fish for it, you're going to have a lot of extra work. I'll support you. I'll give you people and all that but you're going to have a lot of extra work so I'll leave it up to you." And he said,

"Don't worry…it's good. If it's good, it will catch on." Well, it was good and it didn't catch on. And without him pushing it, the others wouldn't get behind it and, moreover, the others who were coming along later were more divergent. John Lowe, for instance, didn't order a 704 at all. He skipped the 704 generation so he was…the others. And Jack Strong and Randy Porter at Boeing and several others had alternate schemes. Randy was using very fancy interpretative scheme called Bachache (?). Yeah, Boeing Aircraft Corporation Aid to Engineering Calculation or something. Anyhow, he had Ted Richards (?) and…Grems and it was a great scheme, too. And he was pushing for it in one sense…The unification needed for any standardization project didn't need this. It wasn't de facto standardization and there wasn't a strong enough drive for it and IBM I think didn't care at that time. They weren't trying to divide the SHARE units at that time. They didn't realize that that was the way of doing it yet. So there was really no force. If Don had been a charismatic personality and had gotten up and said, "You know this is the greatest thing since the invention of…in the way, for instance, of…or something why…IBM would not have gone as far in substituting itself and then have pressed the one hundred man years of the 709 didn't occur. But, …this is water over the dam. And I never blamed Don for it. I gave him full responsibility and he chose the path…

I think later that he himself felt that what he had done was better. Certainly was more concise. It was supposed to be concise by 2½ man years.

MERTZ:

Now you were still in Cincinnati?

GROSCH:

This was all Cincinnati, yeah.

MERTZ:

1955, right.

GROSCH:

Right.

MERTZ:

(inaudible)

GROSCH:

I was still living in Wyoming. Dorothy was still alive at the time. No, she was not working. We had a fairly heavy social commitment. The GE--along with this GE decentralization was a lot of entertaining and stuff. Cochran, for instance, would give a formal dance at the…City Club and appear. And his wife would attend the…of Bob and all of Cochran's section managers would be invited and they would all come in black tie and we would dance and drink and have a magnificent six course dinner and stuff. Then each of us in turn would entertain in our own houses and invite the others to come and it was quite a bit of competition as a result. I remember serving canned whale meat and…frozen in a--incidentally, can't get whale meat anymore. It's dropped out of the market. I must have used the last can. I'd bring stuff back from Europe, you know, and so forth. That's right. Yeah, it's great. Like dried beef but you know not very many people had had it. Rattlesnake meat was…chocolate covered ants--ECH but whale meat was good. And I used to serve a lot of Termadamic (?) and cheese which was new in those days and it's like discovering Scandinavia. And I could then bring back from New York, you know, and I'd bring back Lindy's cheese cake. But it was a fairly time consuming operation and Dorothy and I used to do ours at least without servants…And we used to entertain fairly well and I'd begun to make European trips by this time--'64 was my first really large lecture series overseas. I lectured at Cavendish Laboratory at Cambridge, for instance, shops, more formal British banks and visited the National Physical Laboratory…computer and all these guys that I knew over there…so I'd begun to internationalize myself. So I was very busy, needless to say. And Dorothy was busy with the social side of the thing and then, as I say, she was doing this gardening and greenhouse. Well, after her garden was ruined and after her death, why I was pretty much anxious to uproot myself. By that time I had the 704s in place and I'd been invited to join the Phoenix group so this all fit together personally.

MERTZ:

This was '55?

GROSCH:

The trip to Europe was the early fall of '54. I came back and built the building. I hadn't gotten the building yet. I came back and built the building during '55 and dedicated the first 704 and the building at the beginning of '56. We dedicated the Lynn 704, which I hadn't come…in the late spring of '56. Dorothy died in June of '56 and I left for Phoenix in August of '56. So it was a pretty tight year--'56 was a very good and very bad year for me.

Now, let's see. Let's do the building first and then Lynn. I was, of course, terribly cramped in this second floor operation in this building 300, this laboratory building. We had done things like, for instance, I had built my own partitions to put between desks. This was bullpen-type operations because by putting the desks face to face with the fiberglass opaque partition between them, I could save every other row of chair space. It was literally that tight. Toward the end we actually had junior people sharing a desk and you can't do much worse than that. We tried to keep it happy.

I had, by this time, acquired as an executive assistant, an illegal job in General Electric professional management, but effectively he was my executive assistant. Charlie Asmuse (?), who is now the executive secretary of AFUS (?)…

MERTZ:

Spell that.

GROSCH:

A-s-m-u-s and Charlie is actually Homer George. Needless to say, Charlie. Charlie had been a junior accountant at Evandale with no training in computers and he came to work for me, worked up through the computer trade, worked in other parts of GE after I left and later became executive secretary of AFUS (American Federation Commission…) and is the man primarily responsible for the physical success of the JCC Conferences.

MERTZ:

At that time…

GROSCH:

Yeah. He's executive secretary of…

MERTZ:

He is?

GROSCH:

He's executive director and they put him over Charlie and ruined…Charlie's the one that built it up. Bruce was the one that's running it down.

MERTZ:

I see.

GROSCH:

All Charlie wanted to do was to run the conferences. He's essentially a business arranger and he runs the conferences, makes a nice profit, runs a nice entertainment suite with lots of free-flowing booze. Gilforth (?) wants to rebuild, reform the computer business which is fine except he's not able to do it. And in the process, he's paying (?) spending all that good money to HEME (?). This is current politics.

MERTZ:

Yeah. To get back to his…

GROSCH:

But, he does introduce Charlie Asmus to their computer training. He was a junior accountant when I picked him up in 1953, helped me get the 701 going with some wonderful help and all through those years, learned an awful lot about the trade without ever himself being a programmer, or installation operator or something.

MERTZ:

Who managed personnel?

GROSCH:

He helped with personnel. I always chose the people in the days when there were few enough of them that I could talk to them all myself.

I should mention the names of the rest of the people--Eugene Gettle (?) I've mentioned remained a direct report for me until I left for Phoenix. Don Shell was in charge of, what we would now call, software development. I literally don't remember the exact sub-section manager title. Jack Hughes, John B. Hughes came to me from a number one job which was anticipating acceptance of the 701 at the General Motors Research Lab. This is the job now held by John Hart, who was his number 2 man. John Hart's the man who's so active now in computer age design. John Jack Hughes was married to a Cincinnati girl who wanted to come back home. She had some family problems. She wanted to come back home and this was the best thing Jack could do. He would have had, of course, the top job if he'd stayed with General Motors. He had the top job and had a 701 of his own but by coming down to me, he was my chief operations man and my number 2 man really. And then when I left to go to Phoenix, he lost some faith in GE, didn't want to come with me because of the Cincinnati commitment and he went to Proctor and Gamble which is very unusual because P&G promotes only from within and is now their chief programmer, chief supervisor and very happy with…spent a year in Brussels on their behalf studying up on computer shop foreman and Brussels…very, very useful position in life.

And Charlie Asmus was…assistant. I seem to remember one other face but I can't remember at the moment who it was.

Dan McCracken (?) was aboard by that time but he was the nearest supervisor reporting to Hughes at that stage of the game--first level supervisor. He had been a sub-professional when I took him aboard, paid by the hour.

All of the girls that I had brought down from Boston I made full professionals and with the help of Miss Kellogg and with great resistance from the conservative GE management, they were the first full professional women in the computer field in General Electric…The others had been thought of as aides to the engineers. They ran slide rules or desk calculators. For instance…wonderful girls in Schenectady but they helped him. They didn't resist independently. In fact, one of the four girls that I brought down--Shirley Hanaford--became a first level supervisor which is also a first. By this time Miss Kellogg was a third level supervisor but still she was the, this gal was in the engineering shop where Marian was in personnel activities, personnel community relations. So I did very well by the youngsters or the girls and I would have done very well by the Negroes and the Jews and so forth except they didn't have any.

As a matter of fact, I just had a reunion the other day with Bob Smith, who was an old hand from the IBM conference days in Endicott and who was Negro. He was the operations manager for a CPC installation in the Bureau of Mines in Pittsburgh. And I saw him the other day in Boston. He'd come all the way up from Pittsburgh to hear me talk and we had a great reunion and I love him dearly and I wanted him to come to work for me in Cincinnati in the worst way. An enormously capable fellow. I wanted him before we even got the 701. I wanted him to help me with the CPCs but he felt uncomfortable in a strange situation. Cincinnati is a little further south than Pittsburgh and felt that he wouldn't be as welcome in either GE versus the Bureau of Mines or Cincinnati versus Pittsburgh. He turned me down. In fact, he's still at the Bureau of Mines…He was literally right and left hand man to one of the pioneers of those conferences--a man named Stuart Brinkley who did thermodynamic calculations on the CPCs. Brinkley had blown both his hands in a chemistry lab explosion and gone into computing afterwards and, of course, wasn't able to shuffle cards or push buttons or anything very effectively with artificial hands so Bob Smith was literally his right and left hand man. He learned everything that Stuart knew…so I would have been nice to some Negroes if I had any. And I did have the girls and the other under-privileged types in pretty good shape.

By this time I'd built up to about 100 people--almost evenly divided between men and women. I had not only the majority…Ruth Koch, half a dozen other very able gals, Janet Henchie who was a female engineer type and who knew a good deal about jet engine design that we taught computers thinker (?).

Mostly we used the buddy system and the swimming pool method. The buddy system consisting of looking over a friend's shoulder and the swimming pool method consisting of being shoved in and, you know, if you don't drown, why you become a…I don't apologize for this because we had no time to do anything more formal. But, later we began to give internal courses, cooperate with the University of Cincinnati, all of the good things that are standard. We pioneered a lot of all the good things that are standard. Of course, some of the things we had to teach ourselves. Like, this remote transmission of data, the use of a transceiver and stuff. We had no help either from IBM or… operated from.

Don Shell's compiler and assembling stuff--I think as far as I can tell, Don was just a natural genius in this sort of thing. And, you know, when I first knew him, he didn't know a desk calculator from a slide rule practically and within six months he was one of the world's leading software experts. And questioned how this happened but he did. McCracken was the same way. I got McCracken from Hanaford (?) and he was a good young sub-professional out there with a second rate chemistry degree from an expensive college in Washington or something. And within just a few months, you could just see parts sparks coming out of his eyes…computer programming book and on his way to being the best known programmer author in the business. So I was happy, delighted with the guy so he did all of the work. I floated around giving speeches and politicking and…a great team.

But, of course, I'd done a lot of this stuff in my own hands earlier in the punch card days and we made calculators and so forth so it wasn't just chicanery. I'd graduated from the day to day work and…

MERTZ:

Now this was done in the…

GROSCH:

Well, we're getting up to the point now where we've got an organized group of about 100 people. We're running out of space. We've got three 704s running and I went with Cochran to the board of directors of the General Electric Company in New York City with Jim LaPierre's concurrence and asked permission to build a building right next to Building 300, rather a handsome operation to be
completely devoted to computers. It was to be called the Computation Building and it was to house two 704s and all the associated auxiliary equipment, keypunch rooms, transceiver rooms, big punch card operation, power supply, air conditioning, etc., etc., and lots and lots of space and lots and lots and lots of people--250 people.

MERTZ:

And directors, too.

GROSCH:

And a very nice director's suite, yes. Furnished, as a matter of fact, by special permission with Herman Miller furniture which I'm very proud of and was replaced the week after I left by grey metal.

The building was painted in 26 colors of paint and paint doesn't cost any extra no matter what color it is unless you choose some outrageous which we didn't. The styling to make sure that the right color got on the right walls were given us free of charge by General Fireproofing on condition that we buy their furniture, which Charlie Asmus schemed for it. So we bought all nice new General Fireproofing furniture and left the old, scarred stuff behind for the analog computer place. Moved over to this new building…

MERTZ:

How long did the building take?

GROSCH:

Less than a year. I designed it myself in the sense that I started out with sketches on a piece of paper and I specified a lot of oddball stuff. We used heat extraction (?) air conditioning. Great big 110 ton units that ran from…that this plant had underground so that we didn't have to develop…We ran everything in duplicate--this was at a time when people didn't think much about security and so forth. We ran two major power lines in to switch gears. We had everything duplicated except the main step down transformer. We had a pad ready for a second one and an agreement made if we ever blew one that it wasn't my fault and that we would put a second one in immediately on one week notice and so forth and then…We had switch gear capability…We put in an enormous pacipitron (?). We were moving 220 tons of air conditioning when both units were working at full capacity and on 110 degree day in Cincinnati with two 704s and 200 people and a flat roof, we needed help (?). But, most of the time we could run on one which provided…these things are almost failure proofed…We duplicated all the blowers and all the pumps and everything. We just had a single cooling tower out back out even there we had two sets of motors and pumps to spray the water up onto the cooling…And when the entire thing was built in less than a year with detail design and the detail design was wrong and Jack Hughes and I found that the air conditioning really did work just as they were about to start cutting tin because of a misunderstanding of having air conditioned computers and we got it changed in time and when the thing was installed, it worked beautifully.

And the only place in the whole system where we didn't have duplication was one Y (?) where the two pumps from the cooling tower funneled into a single tube and then from there went through the ground and into the building and opened up again into the double

stuff inside, damned if we didn't get a stoppage in that Y the first day we checked the system. You know you can't be too careful but, in general, it was enormously successful.

MERTZ:

How much did the…

GROSCH:

Six hundred fifty thousand without furniture, 27,000 square feet. I've got a whole volume of photographs…Of course, the detail design was done by…We still didn't have flexible flooring in those days and instead of digging trenches which is what the architects wanted to do, or piercing holes through a diaphragm floor which I'd done before, I specified a floor made like a bowling alley out of strips of maple on edge which you could saw holes in with a compass…And the minute I left they put…They just couldn't bear to have anything--and painted the 26 colors of paint pea green in six months. Couldn't bear to have anything out of the usual. Didn't cost a cent extra, not a cent. The only thing that cost extra in the building was my Herman Miller furniture in my own office, which wasn't all that much. It was a small office and two tanks of tropical gold fish in the wall and three tanks of tropical gold fish in the walls in the lobby instead of using rubber plants. That's the only thing I did that was wasteful and they took out the tropical fish and threw away the Herman Miller furniture and painted all the walls the minute I got out. They were trying to tell my something I think.

MERTZ:

You say the building was dedicated the same time the 704.

GROSCH:

We got the building running and put in the punch card equipment from the other building and a few things like that and got people started moving over about two months before the actual arrival of the machine. But in--starting about two months before the arrival of the machine, we always claimed in our promotion literature that actually since the first weeks were just shuffling, you know, nothing really was done. No real work was done in the new building that from the time that we began to do work in the building until the time that we installed the 704 and unexpectedly got the second box of memory that I had agreed to and it arrived before the opening day, got the whole machine checked out including the second box of memory and the first production graphic display device, checked out…checked out both of the applications program--you know, finally since they had been checked out in advance in Poughkeepsie as much as they could without…equipment--and put on a great big show as part of a company wide celebration for the Air Force and so forth with seven consecutive showings for people like the President of American Air lines and Vice Chief of Staff of the Air Force and Tom

Watson…and so forth and so forth. And the worst thing that happened on our part was we ran out of paper on…Mostly due to Shell, Hughes and Asmus. I supplied the…

That was about February of '56. I still have the brochures and stuff…

Now, meanwhile, there had been another excitement going on. All through this period there had been enormous forces from the people who helped us get started on the 701 to get some equipment, notably a man named Allen Keller, who had been the father of one of the paper computer studies in the company before HORWAC (?) and OMYVAC (?) when he tried to get GE to build him a computer. He was a shining light in the medium steam turbine department. Enormously intelligent man. Too devoted to work. The kind of guy who would go home at night, have dinner, kiss his wife, go in the study, open his briefcase and work for five or six more hours.

MERTZ:

An academic…

GROSCH:

The former type of academician, yes. The present day one goes to a cocktail party but, anyhow, the old kind of academician. Enormously intelligent. No sense of humor whatsoever. Had some very devoted young men working for him. They were more afraid of him than anything…And he wanted to use the 701 primarily for system studies of steam turbines. In other words, where the turbine itself is only a block on the system and you also have…towers and piping and valves and heat exchangers and all the other stuff that's in a big power plant, and boilers and stuff.

And he made systems designed very advance thinking. Beautiful stuff. And he compiled these in a sense at a time when you didn't use compilers by having sub-programs reported on little decks of pure binary punch cards which he could sort of assemble by hand into a system and then he'd poke them all into the machine and they'd sort of compile up into a system description. Now this sounds pretty fancy nowadays and he was doing this in '53 and '54. Really a brilliant, brilliant man. But, on the one hand he wasn't a politician or the organizer to really run a big machine or even to get permission to get one really. On the other hand, he always underestimated…He and two of these young men tackled each problem and then he had 18 other people who would have to get in and bail him out. Some of which…He didn't for a moment disguise the fact that his ambitions were sound and his abilities were enormous but I could go past him to the general manager of the department, a man named Chandonnet, who corresponded to my Dave Karpin (?) and I would talk to Chandonnet like a brother and say, "Look here, you know, Allen is going to trunk you all up if you trust him. You've got to give him more people. You've got to give him more time, more money." Or sometimes I'd say, "Don't let him do it, you know." …too far ahead of himself or it's not economical and Chandonnet would, by very cleaver means, usually steer around whatever it was that was the problem. But, the pressures for

them to get their own machine continued to mount and, indeed, they did begin to use more and more of my equipment. Now I would show them if they did this instead of that and didn't transmit over the transceiver and didn't do this, they could do it in half of a shift. Now you don't want to get a machine for half a shift, do you, sort of thing. But I was well aware of the fact that sooner or later they would get one. The small aircraft engine department, part of my own division in Lynn also were trying very hard to get a machine and ended up, as I say, by getting a 650 or something for accounting work which I would gladly have done in Evandale but I simply couldn't pry loose. And I felt more and more that the engineering load, which was small, was going to get away from us sooner or later. So I finally proposed to Jim LaPie…re, who was just leaving as Division General Manager and going to be replaced by a man named Jack Parker, who is now one of the three co-presidents of the General Electric Company. I proposed to both LaPierre and Parker that we should put the second one of the 704s not in the second machine room of the new building 305 but in Massachusetts and I said, "We'll continue to run it in the aircraft development department under Karpin," but I said, "it's not that I'm trying to get another machine." I said, "It's simply that this way we can use the knowledge that we've got here. We don't have to have somebody else start off and have a whole different set of problems, a whole different set of programs and a whole set of operating conditions which I know Keller will do and which I suspect a small FS (?) engine force will do. Moreover, Keller won't do any work on a small aircraft engine or at least they won't trust him to do it. Small aircraft engine won't be allowed to do any work for Keller so you'll end up with two machines, each under-utilized, and there is still this data reduction load in Lynn that I'm sending down over the transceivers. Why don't we add that to the package, out off the transceivers or maybe put them in mothballs for dial up type of transmission in case of emergencies. Then when the machines themselves get turbulence and the line charges that's costing us the money." Well, I sold the package anyhow. It took a lot of effort.

So then I went to Los Alamos and I hired a man named Alan Benson who had been the number 2 guy to Williard Bouricius, who's still I believe in IBM research lab somewhere and is one of the third generation of human beings to run the Los Alamos Computer Lab. The first generation was in the punch card days Nick Metropolis (as in large cities) and Stan Frankel, who had security troubles later. These two guys did the shock wave calculations on the 601s that I did on a smaller scale at the Watson Lab. And they were succeeded by Preston Hammer, who is the old boy who looks like Santa Claus now with a beard down to his waist at Purdue or Indiana, one of the middle western universities, and Dick Stark, who was for many years the operator of computers for General Electric Atomic activities at San Jose, California. And then they were succeeded by Williard Bouricius and Alan Benson. They, in turn, were succeeded by the fourth generation which is run by a Swede named Ben Karlsen who was hired on for the last 10 years. In fact, in the last…each one of these guys has gone through a couple of generations of machines and has done beautiful work but security keeps people from knowing as much about it as I'd like to know.

Anyhow I got Benson away from there because he had a changing marital situation. He'd remarried after a rather messy divorce and his first wife continued to live in Los Alamos so in a close community like that he used to see her every day at the drugstore so he said--more likely, knowing both of them, at the liquor store--so he remarried to a new gal named--gone. Martha or is that pertinent? Anyhow, he was looking for a change in atmosphere. I'd approached him to work for me before and he turned me down. This time he said yes and I got him to come with the understanding that he would be pretty independent either as a sub-section manager under me but he'd be pretty independent at designing, installing and operating the 704 in Lynn. But, first of all, we had terrible trouble finding a physical place for it because all of the space was owned by somebody else. The medium steam turbine people had some, aircraft accessories had some, small aircraft gas turbines had some and so forth and we got shuffled around from place to place and the remodeling cost was enormous. Finally we ended up by taking an old shed that I think Edison must have invented the light bulb in and put in a whole new inner building into it with air conditioning and so forth, not quite as lavish or as imaginative as the one in Evandale and certainly not as nice to look at from the outside but with the usual glassed in entertainment passages and so forth and with nice punch card equipment and all that sort of thing. And at a considerably lower price because the external structure didn't have to be built. And Benson did a fine job of getting that going. He hired a lot of good people to work for him; among whom are Charles and Harriet Meadow as in hard grass. Charles is the author of two outstanding books on information retrieval and has been working for me here at the Bureau of Standards recently until my change in title comes down at the Office of Science and Technology. And his wife used to work for IBM here in town and Gaithersburg and Rockville. I think now working at the Smithsonian in beetle collection or something, some museum of natural history around here programming beetle gender or something. Both extremely good people--Harriet a brilliant programmer, Charles more a large scale systems analyst. Ed Spiegelthal who is now one of the consultants to the National Security Agency. Rudolph Koenig who is a well known figure in the Washington small scale software business and several others. He recruited them in the Boston area, got the shop going on a very fine basis. We didn't have quite as an elaborate opening ceremony there as we did in Evandale.

MERTZ:

(inaudible)

GROSCH:

He was on the air before I left for Phoenix so he must have been on the air in May or June of '56. It's somewhat masked in my mind by the fact that this was the time that Dorothy died so that my first experience…unhappy also my first experience in…But it was certainly before I left for Phoenix. It might even have been a little earlier. I may have gotten my time moved back. But it was very compressed in those days.

Well, now I had two 704s at work, each in a new building. The fortunes of the jet engine business were such that the load was not building that much faster than the 704 could handle. In fact we were thinking, although not yet done it, of discontinuing the 701…

MERTZ:

Did you move the 701?

GROSCH:

We never moved it to the other building. No, the prospect was thought of several times. They, of course, wanted the space in the other building desperately but I kept saying, "Why bother? We'll get the stuff off it." And, in fact, in the end we got it to the point where only people in that original building were using the machine--that is, the only customers for that machine were people who lived in that building. And then we let them fight it out among themselves. When they said, "You've got to get out of here." We'd say, "Well, why don't you go down the hall and see Dick Whozit whose programs are the only things that are keeping us alive." And so forth, "Get him to reprogram, get him to pay for reprogramming and we'll get out tomorrow" sort of thing. And finally we did indeed get out but I think I'd left by then.

I recommended at the time that I was offered the Phoenix assignment--well, let's see, it's more complicated than that. First of all, when Cocheran (?) came back from his visit to…for professional management ideas, we had the building, we had everything going. He thought he ought to do some reorganization and according to the precepts of GE professional management, all the laboratory stuff ought to be in one place. And I said, "Fine. You add on the metallurgy labs and the testing labs and so forth and I'll run the whole thing…" Well, I don't think they liked that and I said, "You're right. On the other hand, if you put me under some other section manager I won't like it either. In fact, I'll leave and so will most of my people." Well, that doesn't seem right…Well, it doesn't, does it. Why don't you leave Things the way they are. No, no, no. The security…So we horsed around with this for a while and meanwhile I was exploring—and this is a story in itself--in Syracuse what now no longer looked like morbid attempts by GE to get into the business. It began to look as though they really meant to do it. Although I disagreed with almost every single parameter…decision but I agreed…So I finally, although I had been turned down for general manager of the job, I finally went to the men who were in charge--Olefield and Lasher--whose names will come into the story later. And said that I would like to come out to Phoenix--that was unknown at the time. If they went to a nice place, I would like to come out with them in spite of the fact that I couldn't have the top job and be, what we would now call, manager of software … computer applications…And so after Dorothy's death and after the threat of this dumb reorganization was upon me, why I decided to do this.

MERTZ:

Did they have some standing offer that you could come out?

GROSCH:

Yes and the thing that was holding it up--it wasn't really a standing offer, it was a tentative offer and the thing that was holding it up was that they couldn't spring the organization in Syracuse. They couldn't get official authority to start the dumb department even though they decided to go into the business and they'd spent a lot of money and had essentially chosen the people but they couldn't get an actual move to do this. Well, now this was W. R. G. Baker, father of electronic spark in Syracuse and one of the great figures in electronics manufacturing and development in the country. It's for him that station WRGB is named for instance in Schenectady. Doc had built this enormous manufacturing facility for some research, not much, some development and research in Syracuse. He was lord of it, absolute lord of it. He kept a coiled bull whip over a fireplace in his office. He had the only fireplace in any office in…and the coiled bull whip had been presented to him by his managers as a moment of jest. But, the rumor was that he took it down frequently nevertheless. And I sat in his office several times although I was many levels below him. He was sort of bigger than the division manager. He had three divisions under him…In spite of all this professional reorganization, they couldn't chop up Baker. He was too tough so they had to retire him or drive him away in some way.

Baker didn't want to go into the computer business. I sat in his office three times. Each time presenting an impassioned plea to do something or other and each time have him tell me, you know, in…possible terms, "Forget it. Get lost," nicely. Not in an ugly fashion in the sense that he was nasty to me but he was nasty to my project. And he was the king of electronics and computers were electronics and that was it. It under him that they had this ORAC (?) fiasco which was an utter, utter fiasco. And he told me…he said, "Grosch, it cost me $25,000 a year in my personal incentive compensation to put the General Electric Company into portable televisions" --portable but black and white television which hadn't yet come on the market but they'd been developing them for two or three years at that time. The Japanese beat them in the long run I believe. He said, "I have to put development money and profits into that year after year," and he said, "after I've retired, the company is going to make a mint on that." And he said, "They didn't even give me a dollar except in the sense of my GE stocks that I hold will go up a little bit." He said. "Meanwhile, I get $25,000 a year as compensation. If I put that profit in the company's pocket, it's going to build …" He said, "Now, you want me to spend 10 times that much on computers." And I said, "… 100 times." You know, he didn't hear me. And he said, "I can't do it. Can't afford to. Forget it. Let IBM have the business. We'll sell them tubes." Which is true although by that time the first solid state was coming along, it should have been germanium diodes and transistors but he thought in terms of tubes.

Well, Baker nevertheless was forced by the top management of the company to sponsor a couple of studies. One was called the Industrial Electronics Study. It was run by George Metcalf who later became the government vice present for GE here in Washington and

the chapter on computers was written by a man named Claire Lasher who succeeded Olefield (?) as general manager of the Computer Department in about '59 or '60. And I talked many times--oh, many times is an exaggeration--I talked several times to Lasher about his predictions and his statistics. Each time quoting him two, three, four times low even for, you know, present countable computers. They knew nothing about IBM. How they could investigate computers with the little bit of knowledge they seemed to have about IBM I don't knew but then this was one of those typical committee investigative operations where the results were treated…It didn't really matter what you put in the study. Unfortunately I don't have a copy of that study. It was highly confidential. Jack Parker, who was then division general manager at AGT, was given a copy as were all division vice presidents and he loaned me his and I took some notes from it which I still have but I don't actually have a copy of that study. I'd love to have it. People who are investigating the history of the trade are all still trying to get a copy but it's disappeared…every copy has been burned. Ridiculous low estimates on everything including the cost of getting into the business.

Anyhow, I was conversant with them. They weren't taking my advice. They weren't going to make me general manager but they were going to get into it.

MERTZ:

Right. I think this ends this side of the tape.