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Computer Oral History Collection, 1969-1973, 1977

Interviewee: Herbert Grosch
Interviewer: Richard R. Mertz
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Tape 2, Side 1

GROSCH:

Of course I'm jumping ahead a little bit in the story. Because my actual decision to leave the Watson Lab didn't come until 1950. But the fact remains that many of these ideas or many of these considerations were in my mind all through the period ending, I mean beginning perhaps in '47 or early '48.

After I taught my course at Columbia a couple of times and had had a chance to interact with the new industrial people that were coming to ask for help and so forth, why these began to get in my mind.

Now one thing that occurs to me for instance, as a partial example, is that I've always been very ambitious. Very much an empire builder. You don't see this in a graduate student or in a person whose just running a little optical computing bureau with one assistant or something.

The fact remains that I could see, looking back on it, I could see the pressures were there then too and they should desire to control the emotions of others and so forth.

Now what this means is that in your early days you end by of course, having equipment go somewhere else where your boss will be less restricted. Where your job is a little more permissive or a little more expansive or what have you.

But in later years, it tends to be more of an empire building sort of thing. You stay where you are, but reach out for more authority and more responsibility and so forth.

Certainly I began to feel sort of at ease in the Watson Lab in the middle years. Because several things began to be quite obvious after a year or two as the, when the SSEC was in and running and things were sort of settled down for a while.

First of all it was obvious that we weren't getting a great deal more equipment at the Watson Lab itself. I don't mean to imply that we were cut off. We frequently got one of a new machines, but in each case I had to ask for it. Press for it.

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Quite frequently it wasn't volunteered very readily or at the time that I wanted it. Usually I didn't get much backing from Eckert in it. So you weren't going to have large additional supply of computing equipment at the Lab.

And it's that class of equipment the free standing punched card machine, even after it had been electrified or full of relays, didn't have anything like the power of the SSEC or the bigger machines that were obviously coming along.

So even if you did get a 604 or a (test going?) machine or something like that, it wasn't going to add a great deal to the total capability.

Meanwhile, Wallace who had, who had pulled back essentially from getting any further involved with the SSEC down town kind of activities, was using a greater and greater share of the Watson Lab facilities for his celestial mechanics work.

He was sequestering the customer engineer part of the time to build his measuring engine. His assistant, Becky Jones was doing a lot of lunar theory work in batch form, parallel processor form on the punched card machines. It was not an ugly or selfish domination of the scene. But nevertheless, his load was increasing and by the time you added to this the very substantial amount of work that we were doing with students and with walk-in young researchers from Columbia and the adjacent areas, there just plain old wasn't much time.

If I had wanted to do my own research, not only this table making sort of thing which I carried on, but some work on efficient methods of inverting matrices. And I still continued to think that I could do optical design in one way or another by differential correction methods on punched card equipment. And had presented a paper to this effect to the Optical Society in '48 or '49.

If I wanted to do that sort of thing, I obviously was going to be rather squeezed for facilities. Something was going to have to give and since Eckert had pulled back from down town, there was no feeling on my part that I could, in spite of the fact that I was sort of running, supervising the actual computing, that I could expand it to fill additional requirements as they appeared.

My wife and I quite frequently did our work from midnight to four or five o'clock in the morning, simply because that was the only time we could have a substantial chunk of the equipment to ourselves.

So I was in that sense of the word, dissatisfied with the empire that Wallace had, Wallace Eckert had at the Watson Lab in which I was merely a fairly important satrap.

MERTZ:

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Was there any relationship between this and the economic horizon so far as...(voice fades out).

GROSCH:

Oh, I think it wasn't a great thing with me. I've always been able to spend everything that I earn and a little bit more besides. I was buying full boating and camp equipment. And going out boating in the Delaware River. I was already very deeply interested in meeting my European acquaintances on their own ground instead of in the United States.

MERTZ:

Had you been to Europe yet?

GROSCH:

I had not yet gone to Europe. No. As a matter of fact, at this time with the exception of my cross Country trip to get engaged in 1940, I had not really had a major exploration of the United States.

But the hunger to do so was definitely growing on me. Many of the people that I went full boating with were Europeans. And almost all of them had been in the ski troops or something like that. Which meant that they were, they were well traveled, quite cosmopolitan in an entirely different way from the academics.

Then of course, astronomers were traditionally travelers. They are always going to eclipses or going to international astronomical meetings which were held every three or four years around the World. And they always had acquaintances in every continent.

And, you know, I was waiting for all this to happen and it didn't really seem to be happening very quickly, so to speak.

Now in 1948, I was invited to the dedication as were many other astronomers. It was not an honor or anything. I was invited to the dedication of the 200 inch telescope at (Palama?). It had been going for a while but there hadn't been a formal ceremony because of the War and the fact that it was, it was being tuned up year after year to get still better results.

My wife Dorothy of course, is a former member of the Mount Wilson staff, was very anxious to do this too. So we took a trans-continental trip in our first new car. We had bought a 1947 Buick Road master convertible. The best car that money would buy in those days.

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In fact, it was easy to buy a very good car than it was to buy an economical car, simply because you know, as always, the dealers were prepared to sell you an expensive loaded car even though they didn't have any economy models.

MERTZ:

You were both working at the same...(voice fades out).

GROSCH:

I think Dorothy was still working at the Interchemical Corporation at that time. But she stopped somewhere around there. She may very well have quit that summer in order to take this trip. I'm not sure. But at least it was around that time that she quit working there.

She'd been a valued member of the staff at the junior professional level. Had been in charge of their (Spec ?) Lab using techniques that she learned with (Magners and Skridner?) at the Bureau of Standards, before.

But I think that the, it was not so much a question of the money, as the inconvenience of the thing. We would liked to have <illegible> continued to have her salary, but on the other hand, it was an awkward business for her to have to work every day when she was also trying to concern herself with some of my work and amuse herself with these irregular hours was kind of hard. If you work until three o'clock in the morning, it's hard to go to work at nine the next morning.

So somewhere around that time she quit and became a housewife. And it was shortly before this that, shortly before she quit that we had moved into town from Hemstead. Sold the house that we had bought at the beginning of the War at a very nice profit indeed, and rented an apartment in Chelsea on West 22nd Street.

So after that, while it was easier for her to get to work, it was also very much easier for her to come up to Columbia and work with me. So it was about that time that she quit, and began to be a sort of an unpaid assistant for me.

MERTZ:

She was an unpaid assistant for how long?

GROSCH:

She got no salary from Watson Lab whatsoever. It was for one thing, a rule against nepotism except for Watsons. (LAUGHTER).

MERTZ:

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Also you mentioned I think, a rule against married...(voice fades out).

GROSCH:

Exactly. I was going to come to that. There had been a hard and fast Company rule before the War that no married women were employed anywhere in IBM. Widows and so forth excepted.

I think I shouldn't say so forth. Widows excepted. Since divorced people were also not approved of. To the best of my knowledge in any situation. Including the factory and what have you.

I think this is because there wasn't much female employment except of unmarried girls in the '20s. And by the time the '30s came along, that was just depression situation. And you remember I said that Watson kept all his people at work but cut back on the working hours and so forth. Well obviously, this same kind of thinking leads you to refuse employment to married women on the grounds that you're putting husbands out of work.

When the War came of course, IBM did not ask for draft deferments for any of its people. And in fact, only received draft deferments for very, very few. They had to use more and more women. So they put this rule in abeyance. And in fact, at the time that I came to IBM, in 1945, and it was ultimately processed through the down town personnel office, there were several very attractive senior women at work as executives down there.

Lilian Schumm, SCHUMM, was there. Was the personnel woman for, personnel officer for (?) Headquarters only. Not for the whole Company, but for just the main Headquarters building. And there was a woman vice president named, Ruth Leach. Who was so attractive that she's immortalized in the IBM legends as Peachy Leachy. A very, very nice girl indeed.

Both of these were officially required to be unmarried. In fact, it turned out that after the fact that Miss Leach finally quit in order to get married and that Lilian Schumm had been married all along but had kept her husband carefully concealed under the table somewhere.

And when he showed up at her funeral a few years ago, why everyone was quite astounded to find that this chap who had frequently escorted her to IBM functions was indeed her husband.

This happened also at the Watson Lab, when Margorie (Severy?) got married and became Mrs. (Herrick?). She was required to actually sign a piece of paper by the Company that she would go away when requested without making a big fuss when they reinstated this law.

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And when a year or so after that, in perhaps 1947 or so, they did indeed reinstitute it, Eckert managed to get a waiver for a few months, but in the end she accepted it, a job at the University of Wisconsin and departed with husband for Madison.

At that time, Miss Eleanor (Crownitz?) who I've already mentioned as being very good at running the relay calculators, took over. And Ely got married shortly thereafter but simply didn't bother to tell any of us about it. Although we were all pretty sure that she was not living in sin.

And I believe before she actually became pregnant, and had to admit that she was officially espoused, the rule had been lifted forever and no longer applied.

MERTZ:

I would say that such a rule possible promotes living in sin.

GROSCH:

It does indeed. But of course, Watson was going on the assumption that no IBM employee could possibly live in sin. However, that consideration was brushed aside. Charlie Kirk, the Executive Vice President, came aboard in '45, is popularly supposed to have died of cirrhosis of the liver which is entirely aside from the fact that no IBM executive ever was seen to drink in public.

Yes, there were indeed some pressures in the IBM Corporation that balanced many of its very good things. There was a good deal of compulsory social activities in most of the Company towns. Notably Endicott and of course to a lesser extent, Poughkeepsie.

And most of the executives lived fairly close to each other in the Greenwich, Connecticut area. For those of us who lived in town or who were at a remote site like the Watson Laboratory, this was not an important consideration.

But at the time of the famous family weekend at the Waldorf, why we discovered that if one entered the larger IBM family, that there was a surprising degree of sameness and a considerable amount of social pressure. It wasn't just the song book among the salesmen, but a considerable uniformity required of all the senior executives.

MERTZ:

In what did this uniformity seem to consist?

GROSCH:

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The example I was going to use was a trivial one. But I think perhaps it will be indicative in a very remote way. Let me start out by describing this famous weekend at the Waldorf because it's connected with the success of the selective sequence electronic calculator.

After the calculator had been dedicated, Mr. Watson in one of his typical expansive moods, decided that he should do something to thank all the wives who had permitted their husbands to work 18 hours a day, seven days a week on this project. This has drawn in as I said, a substantial percentage of all the engineering and technical talent in the Endicott area and really for most of the Company.

Things hadn't been completely shut down, but nevertheless there had been a great deal of effort expended on this thing. So on very short notice, I remember I received my telegram for instance, on a Friday afternoon at the Watson Lab. But many people did not receive them at Endicott and Poughkeepsie and so on, until Saturday at their homes or hand delivered from the office after they had appeared in their offices where there was always a watch of course, available to do this sort of thing.

These telegrams invited technical people, essentially engineers and the very few scientists that then worked for the Company, to get together with Mr. and Mrs. Watson for an old fashioned family weekend, for an old fashioned family week, not weekend, which was to start at the Waldorf Astoria, Monday morning.

Now if you can imagine the flurry at Endicott when all these women had to rush madly out and get baby sitters, because children were not invited. It was understood that this was to compensate the wives with a big week in New York at IBM expense. For all the sacrifice that they had made with their husbands on the SSEC and on the War work in general.

And this was patterned after a famous private train which Watson sent out to the San Francisco World's Fair somewhere around the beginning of the War in 1938 or whenever the San Francisco Fair was on, which was known as the 'dry train'.(LAUGHTER). Because although it took three days to get from the East Coast to the Fair, why drinking was verboten except in private compartments. And when the booze ran out, why things got to be pretty hectic on the train.

Porters would fan out in every direction whenever the train stopped to bring back some supplies. (LAUGHTER).

Well, this was all before my time. And in fact, I didn't hear about this until the weekend of the Waldorf, week at the Waldorf produced these reminiscences.

MERTZ:

Do you recall what week, about roughly when...(voice fades out).

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GROSCH:

No. But I still think I have a program for it. It was, I would imagine that it was in late '47. It was sort of winterish as I remember.

MERTZ:

Well after the dedication...(voice fades out).

GROSCH:

Well after the dedication, yes. It did not follow the next week or anything like that. It was perhaps three, four or five months later which would put it in the fall of '47 I guess. That's probably about right.

And they took over the Wedgewood Room I remember for a lunch. And there was a lot of very, very fancy dos involved. And the ground rules were carefully announced that it was almost entirely social. That indeed there would be an address by Mr. Watson and all that. But nothing formal, nothing official. The women were in charge. They were to go out and shop and they were to have a good time.

Theater tickets, restaurants and so forth were all on the Company. No mention was made of booze, but it was popularly supposed that one reason that they went to the Waldorf was that the Waldorf charged bars bills under the name restaurant. So that many of us had eight or ten restaurant charges a day on our bill. (LAUGHTER).

In spite of my remarks about Mr. Watson's privy purse, the fact is that there was still an Accounting Department. There may not have been a budget operation, but there was an Accounting Department. And in fact, a couple of months later, Wallace Eckert was asked about the fact that I had signed a 165 dollar dinner ticket one night as a part of this thing in the Waldorf as it turned out, and Watson and Wallace was able to say that, yes this was for a party of Watson Lab people, and that he was present, and that they were indeed all certified IBM people and their wives.

No camp followers or trollops had been involved or freeloaders.

MERTZ:

And no customers could...(voice fades out).

GROSCH:

No customers. This was indeed sales free. There was no problem on that. There were very few salesmen present. What they did was, at one luncheon, and this brings me to my point of conformity, one luncheon they did invite all of the World Headquarters

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executives. Most of whom were ex-salesmen or salesmen and their wives to come in from, to come to lunch and to bring their wives in from Greenwich and so forth, to attend. And welcome the people staying at the Waldorf.

The fact that I lived only you know, walking distance from the Waldorf, didn't matter. There was a room there for me and I had to be in the room. I didn't want to go, but I had to be in the room. I don't know that they actually ran a bed check, but the idea certainly was lurking in the back of many of our minds.

Well, we looked out over this sea of beautiful men and women at this luncheon. There were gifts for the ladies, you know, something simple like a linen handkerchief or something like that. Everything done very beautifully. And a magnificent meal. Crab meat, with caviar heaped on top for an entree and all that sort of thing. Beautifully done.

And we looked out over this sea of gorgeous people and there were only two women in the whole place, not wearing a hat. My wife and Thomas' wife. And of course, as far as they were concerned, you know, fussy hats were a lot of nonsense. But not to the IBM executives wives. Or to the IBM engineers or scientists wives. All of whom had been told by their executive friends that hats were...

The Watson Lab was not told in the first place. And in the second place I don't think either Mrs. Thomas or my wife would have paid the least bit of attention anyhow. Not that they were, not that they were frumpish or anything, but they simply thought that New Yorkers didn't wear fancy hats to luncheons unless they, unless it was followed by a bridge club or something dumb like that.

But the IBM rule was, everybody wears a hat. Incidentally, in this time all the salesmen and IBM executives wore hats too. Although I'm talking of men now rather than women. Because, you know, they had a prominent hat business up in Bridgeport, Connecticut or something and they weren't going to go calling without a hat. (LAUGHTER).

I suspect that in earlier days they wore spats and gloves. But that was before my time. But at least they were still wearing hats.

MERTZ:

It was reminiscent of John Edgar Hoover.

GROSCH:

Yes indeed. Yes indeed. In fact, in the '50s when I traded in my first IBM salesman at G. E. Evendale, he was called back to World Headquarters to talk to Tom Watson Jr. himself and was told that if he would buy a hat and sell his fancy automobile, and buckle down to being a real man, that he had a great future in the Company.

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So he sold in Lincoln, restored Lincoln Continental and bought a hat and became Account Executive for the transportation industry or something. And died of a broken heart a few years later.

And probably of the liver.

MERTZ:

How does one die of a broken heart?

GROSCH:

He used to go with a circus in the summer time. That was his great crime. And I don't know whether his wife blew a whistle on him or what, but Watson Jr. as all IBM executives, know everything about their, at least their sales employees, was aware of this fact and was trying to straighten him out and make a useful citizen of him.

They straightened me out as you may remember, several times also. Except that due to the resilience of the raw material, it usually involved my departure from the Company. (LAUGHTER).

We were talking about the limited facilities though of the Watson Lab and so forth and the fact that I wanted to expand and didn't really have the capability of expanding under Eckert which was certainly one of the things pushing me.

MERTZ:

It was (formed?) then in which you felt that at this point your ambition was going to...(voice fades out).

GROSCH:

I think I needed to expand independently at the forum. I think that if I could have done more and more table making, more and more optical design, more and more even some of my old Jupiter's eighth satellite stuff which my wife was doing partly on desk calculators at the time, I think that would have kept me going for a while. Although I think this discontent would with the quality of my contribution, would have recurred.

But, you know, as in (?) dynamics, if you are allowed to expand why worked can be performed even if maybe it isn't optimum work in the human sense.

MERTZ:

That's another expression of the (?) principle.

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GROSCH:

Yes. Exactly, exactly. I was reaching for my level of incompetence and I was being held down from attaining it. Yes. That's it, that's it.

MERTZ:

In this particular instance though, you did then begin to look elsewhere or you have...(voice fades out).

GROSCH:

Elsewhere in the IBM organization first.

MERTZ:

I see.

GROSCH:

But later as I'll tell in future tapes, later I began to look outside the Company and in fact, ultimately left. But my first inclination was either to do something more outward oriented in the Company itself or to try and establish some service type of operation within the Company that didn't yet exist.

Now this brings up the question of the kind of people who were coming through the Watson Lab and with whom I consulted and interacted by this time. And it's hard to put, you know, the cart before the horse in this matter. But I mentioned Kraft. I've mentioned Cecil Hastings of Rand.

There were dozens of organizations now in the Country who were using IBM equipment or a very, or partially using IBM equipment for technical computing. Every major aircraft company. Los Alamos of course, and other parts of the emerging atomic business. General Electric and Westinghouse and some of the larger electrical companies and so forth.

And we were becoming increasingly aware that as we were having this discussion about actuarial stuff, we were beginning to be increasingly aware that places like insurance companies were trying to do technical computing now as distinguished from just statistics and bookkeeping.

So in every direction, wherever you looked. And operations research was coming along. The predecessors of PERT and so forth, this had not yet been invented. But the linear programming kind of thing, George (Danzig's?) work and so forth, was beginning to be talked about. It was clear that there was going to be a great many such installations.

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Well, every single one of these outfits had something to do with the Watson Lab. In the early days because of its name. Because it was the place in the Company that salesmen directed them to if they'd ever heard of it. There was no concerted I think, attempt to tell all the salesmen that we existed, but through the medium of the 100 percent clubs and public appearances by Eckert and by me we were about the only two that ever did it.

MERTZ:

Did you appear before the sales people?

GROSCH:

Yes, I began to make speeches and stuff about this time. I went to the 1949 Hundred Percent Club for instance. And we on the program and Eckert, I believe, had been there the year before. And I was beginning to make outside talks. And they were now beginning to be places like the ACM and so on to do it at. And we were often sent to customer locations. I remember an early trip for instance, to the Cornell Aeronautical Lab where they were trying to do wind tunnel data recording using IBM recording devices, and stuff. On which I gave a seminar as well as just, you know, sat and talked to the people with the problem.

So these number of contacts grew amazingly. And IBM began to use this in a sales way in the sense that they began to put on meetings at which customers gathered together and exchanged their information. Praised the name of IBM and were of course, incidentally, shown the newest items in the sales line and encouraged to go back home and place orders.

They were usually accompanied unobtrusively by a salesman from their local organization who stayed in the background. Sat in the back of the room with a straight face and listened to this unintelligible gibberish that his customers were producing.

I edited the first hard cover proceeding of such a forum which I helped stage in 1948. Eckert had sort of passed the job over to me in an unofficial sort of way. And I did not in fact, have much to do with the physical organization of the meeting. That was done pretty much by the Education Department so called, in Endicott.

They made sure that hotel reservations and things of this sort were all handled right. And that the room was set up and the slide projector was available. But I was sort of the program chairman.

MERTZ:

How was it made known to people? Did people solicit...(voice fades out).

GROSCH:

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No. It was done through the sales organization. What happened was that the various district and, district and city sales offices were circularized. And IBM was always extraordinarily diligent in carrying out this sort of thing.

They are a conventional company you know, if you send out a sales notice to all salesmen or three quarters of them will not read it. And of the remainder, many will disregard it. But not at IBM. Every word from Headquarters was regarded as at least part of the (?), if not of the Bible itself.

And every salesman who had any connection was told by his local manager that there was a possibility of sending one of his customers to this fancy meeting. Well then the scrap was carried on at the local and district level until a sufficient (willowing?) had been achieved that the quota of customers to be sent to this meeting was exactly met.

If the word went out you know, to the Southern California district that they were to send six people. Why they would send six people. They might either send six from one installation. And if their contribution had been over-estimated or one man from each of six installations if it had been under-estimated.

In any event, it would be six. This however sounds cruder than it really was. Because with people like John McPherson and so forth, interested in this back at Headquarters, in fact, the number of people to be sent was reasonably well apportioned. In fact, it was a pretty sophisticated list.

However, you had the problem that if there was a customer who was really kind of dunder-headed, but who appeared on the verge of ordering some equipment from a competitor, I can't think at the moment what competitor it would be, but, let's assume it was Eckert, Mauchly or something like that, he was likely to be invited anyhow, in spite of the fact that he didn't have much to offer.

And if he insisted on getting a paper, not all of the people attending of course, gave papers, he would be permitted to do so. So the task of an editor was essentially one of preparing for publication and refining the recorded material and so forth. Rather than of genuinely being able to reject the entire paper on the grounds of lack of value.

Well the Watson Lab group in the sense of Thomas and Eckert and I of course, were present and I believe Rex (Ziber?) came up for a day or two. The Havens, Walker, Lentz type of people did not involve themselves in this. But these meetings began to be a very important artifact of technical computing fraternity in the Country.

Remember that this first one in '46 that was almost entirely statistical and educational people and was at a very much lower level, had been even before the invention of ACM. And the one in '48 was the same years that ACM held its first meeting. The one at

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Aberdeen. And so we're talking about something that's going along with the professional society.

MERTZ:

I was wondering, was there any relationship between the personnel who were sort of the charter member of ACM and the participation in this?

GROSCH:

It intersected in industry. The aerospace guys, the General Electric guys and so on, belonged to both things. For instance, Kraft was one of the charter member of ACM. He worked with or reported to, or was associated with, Charlie Concordia of GE, who ran the differential analyzer and some punched card equipment at the General Engineering Labs in GE in Schenectady.

And Concordia was the first treasurer I believe of ACM. And then working under Concordia was Frank McGuinness who was another one of the charter members. So there's three charter members for General Electric alone.

There were charter members from the West Coast. Although not so many because of the New York location. From the aerospace business, Lockheed, Douglas and so forth.

MERTZ:

How about people who were at the time potential or actual rival organizations but who might be looked over as possible (groups?)?

GROSCH:

They didn't in generally get invited to the IBM meetings. And most of these at that time were academics. Because most of the rival equipment was (one off?) equipment being built at Illinois or some place like that. Or Michigan or some place like that. They didn't get invited to the Michigan, to the IBM shin-dig. But they of course, all joined ACM.

It was the, it was really the industrial guys that were on both sides. Because they were both conducting calculations with whatever they could get. Namely in those days, IBM punched card machines and card program calculators.

At the same time trying to, trying to get something better which meant that they were involved with the academics and so forth, too.

MERTZ:

Was Cuthbert Hurd a member of IBM at that time?

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GROSCH:

Cuthbert at that time was working at Oakridge. His brother was associated with Oakridge in some kind of an executive capacity. He worked for Union Carbide or something. And Cuthbert had a somewhat lower level job. He'd been a, he'd been an instructor along with Dick Haming at Michigan State before the War or in the early years of the War.

He was aware of the work at Bell Labs. And he was aware of the work in IBM because of the presence of IBM equipment at Oakridge. And he'd heard some of the stories about Los Alamos and so forth.

So he interested himself in this and presented himself as a candidate for employment somewhere upstairs in IBM. Not to Wallace Eckert. Perhaps to John McPherson. But more likely to Watson himself or to some senior vice president.

Oh, along in late '48 or early '49. He actually reported for work sometime in 1949. And I had the dubious pleasure of teaching him how to use a sorter. Because it turned out that he never had his hands on a punched card machine when he actually reported. Although he was hired as a great expert in the computing field.

Well, it didn't take long to be a great expert in the computing field in 1949 since there wasn't much of a measure. And even today, it's possible to learn a great deal about the field in a few years just simply because you don't have the hundreds of years of background material that you need in the humanities or even in the older scientific disciplines.

So I shouldn't be invidious about it.

MERTZ:

(?) the machine language?

GROSCH:

No, the main thing was, he knew how to come in at a high level. He knew how to come in at a high level. He had somehow managed to wangle his way in to see somebody like Douglas, one of the vice presidents of IBM, or something like that. And was hired at a level comparable to Eckert.

And since I would have liked very much to have done that sort of thing myself, my nose was very much put out of joint. However, because he had that connection down town...

MERTZ:

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When did he come in?

GROSCH:

He came in at World Headquarters, yes. I don't remember what his original title was because just within a few months he'd started what was later to be called, what was almost immediately called the Applied Science Department. And became Head of it.

And he connected together...

MERTZ:

Was it essentially the kind of thing you would have liked to have done?

GROSCH:

Yes, I would. Although I must say in all fairness to Cuthbert, that I'm not sure I had it clearly in mind until he started doing it. I had the general feeling that customers were not getting sufficient interaction with the Company through our rather limited resources at the Watson Lab. But I think I didn't have a name or an organization clearly in mind to do this.

It was a general feeling of unrest rather than a specific one. But he moved in very quickly. Took over most of the programmers at the SSEC as the nucleus of his applied science operation. Took over the holding of these, of this forum in Endicott which he renamed a seminar instead for the year 1950 at least. I think even maybe in the year 1949.

Incidentally, that defines his emergence. He must have been around in early '49. Because the Computation Seminar was in mid '49 and he ran it.

MERTZ:

Is this Computation Seminar Proceeding '49?

GROSCH:

Yes. And he was already, he already took away the editing of that from me. And I never was deeply involved in it. So he must have done it at the time the thing was held.

He had much more the IBM sales impression about him. He didn't go to sales school, but he wore the dark blue suit, the vest, the striped tie and so forth. And had a general air of stability and diligence about him which I probably didn't have and certainly didn't aspire to.

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Anyhow, that was certainly one of the things that I would have liked to have done. And more and more as I saw the fantastic proliferation of this thing. Because this was probably a period at which the burst was at its very steepest point.

In terms of percentage increase per year, these periods of '48, '49, '50 were probably the greatest. Because we were right on the verge of getting mass production of electronic equipment. And I'm sure that the number of people using it for technical and advance calculations was at least doubling each year during this period.

We still of course, have a fifteen or more percent growth rate in this Country per year. But that's still a long way below a hundred percent. And the excitement was just almost unbelievable.

Back at the ranch, IBM had withdrawn the old 603 or ceased to make any more of the 603. And primarily under the supervision of this man Ralph Palmer at Poughkeepsie, had began mass production of the 604 calculating punch. Which still read one card and punched the answers on that same card, except by very tricky unusual wiring on the plug board.

But not only did it at 6,000 cards an hour, that is at 100 cards a minute, but in addition did quite an elaborate sequence of operations on each card. Including division for instance.

It had a flexible (word length?). That is one could couple counters together to get big numbers or little numbers according to your requirements. Did not have built in floating point or anything like that. But nevertheless had, had the storage capacity for perhaps 60 decimal digits in various size chunks and so forth.

MERTZ:

Excuse me. In connection with the speed of sorting and tabulating equipment of IBM, had they over the years, particularly in the immediate post-War period, substantially and significantly in any way increased the...(voice fades out).

GROSCH:

No. At this time nothing had increased in speed except the multiplying function. The tabulators, the 405 that I had in the shock wave calculations had been succeeded by the 402 and 403, which were very much handsomer and more flexible machines.

MERTZ:

402A machine?

GROSCH:

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No, not A. 402 and 403. The A was a 602A which was a calculator punch. Right. But the tabulators were now the 402 and the 403 and these were either all numeric or alphameric respectively. And they tabulated at the same speed that the older equipment had tabulated at. But it did indeed, print a full alphabet at a somewhat faster rate.

Printing was slower, just tabulating. It would feed the card, as I remember, at a hundred and fifty cards a minute, but printed only a hundred cards a minute.

Now these were impressive speeds, but they been reached by the same class of equipment before the War. So there wasn't anything wonderful about that.

MERTZ:

So there was really no substantial...

GROSCH:

Sorters were a pre-War design. They stepped them up a little bit to maybe by the end of this period, to about 400 cards a minute from about 220 when I started. But again, a trivial increase.

Now, there was a whole burst of new equipment coming of course. Far better punched card equipment also. Especially a better key punch. You know, a 26 or 29 key punch and so forth. Which of course, was made in hundreds of thousands, ultimately.

But these were still in the development shop. Because it takes a long time to build a mechanical gadget of this sort. And most of them didn't really get started until after the SSEC was finished.

There was no pressure you see. The Remington-Rand was no challenge. And they were able to lease out every single machine they had built. The waiting line for a tabulator for instance was at least eighteen months if you didn't have a military priority or a very, very fancy private standing with Watson.

Like Prudential maybe got it a little faster because they had just finished loaning out IBM a hundred million dollars or something. So if they wanted another key punch tomorrow, why they usually got one.

But for the average customer, eighteen months wait for a tabulator. And this was another reason why we had such a troop of aerospace guys and so forth, coming through the Watson Lab. They needed to plan a long way in advance. And one of the ways they could plan more effectively, was to try and find out from us what they might be able to get 24 months from now if they got in line 6 months from now. Since it would take their companies that long to put through the purchase order sort of thing.

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MERTZ:

Was the anticipation so, would that be in terms of any substantial increase in efficiency of the traditional IBM card punch (?)?.

GROSCH:

Except for the multiplying function.

MERTZ:

Except for the multiplying function. And conceivably, some other development in the electronics field.

GROSCH:

Yes. But we were able to assure them that with the exception of the SSEC, that there was no such development. In other words, when they came to see us, one of the questions they'd ask me is, well now, you know, you're going to build all this wonderful stuff yourselves aren't you? And to which the answer was, we certainly are. And we are working hard at it at the Laboratory. But we have no commercial machine that's comparable to these (one off?) gadgets like the EDSAC and the EDVAC and the BINAC and so on, that were being talked about outside.

Of course, internally there was much discussion about what to build, when to build it, how to fund it. And a lot of this story has been told elsewhere. Watson Senior was quite, quite negative about this.

He didn't really think there was going to be much business for the large machines. He was willing to build them for the glory of science. Or for the sake of the Government. Or on a (one off?) basis perhaps by special order. But to set up an assembly line to produce UNIVAC I just didn't seem logical to him. And of course, that was one reason why they turned down Eckert and Mauchly when they applied to IBM for affiliation.

MERTZ:

You are saying that in their judgment then there was no real commercial market.

GROSCH:

No major market. Now of course, people like me were completely convinced.

MERTZ:

This was '49?

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GROSCH:

'48, '49, yes. We were completely convinced. In fact, most of us I think who were a little remote from the battle field didn't even realize that there was this negativism upstairs.

Eckert would come back with word you know, that Watson was going to undertake the construction of the NORC, Havens' machine. You know, it could cost us several million dollars and etc., etc. And to a remoter person like myself, that sounded as though Watson Senior was just gung-ho for the whole thing.

But in fact, it has turned out later by people who were closer to the decision process, that he wasn't. And that it was Tom Jr., John McPherson and people like that who were pushing for the authorization to go ahead and build, to put substantial investment into a line of large machines.

MERTZ:

That was the conventional wisdom in...(voice fades out).

GROSCH:

Yes. It's interesting because Watson had been ahead of his time in almost everything. But he was not a young man by this time. He had a big career in National Cash before he started IBM in 1918. As a salesman. And he sold, sold, sold all the way up through the War.

Moreover, by this time you know, he was interested in electing Eisenhower as President of the United States and incidentally, first President of Columbia. He was interested in being Chairman of the International Chamber of Commerce and whether or not to build a new Metropolitan Opera House and so forth. He's getting medals from all sorts of foreign governments and so forth.

The amount of number of hours a day he spent brooding about whether or not to start a 701 or not was one of the, he just wasn't thinking about it. And he might very well have had a serious set back. It would never have ruined the Company. It would still have been an enormously successful business machines company. In the same way that National Cash Registers remains an enormously successful business company today. And would be so if it didn't have an electronic tube in its line.

But might have had, nevertheless, a severe set back in the fantastic IBM growth period if it hadn't been for a few younger people who forced his hand as such.

Well, I repeat that this never...

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MERTZ:

This seems to be a mirror of the time in '49, particularly in '49 and '50 commercially of potential competitors as well.

GROSCH:

As a salesman...

MERTZ:

The patenting offices commercial patent offices found that in their judgment in several very important samples, the official patent attorneys of MIT found that there was no point in patenting devices in their judgment because there was no commercial application in sight for the magnetic core memory. This was a decision written in (1950?).

GROSCH:

Yes. I didn't know that. That's something. Well you see, I lived in a different world. There were already thousands of (demesnes?) of this world. You could live your whole life and never look outside of it. I did. Later especially. But at that time I didn't do much. Aside from my full boating friends and social, very small social contacts, I was thinking and living twenty four hours a day within this world.

And within this world it was taken for granted that everything was going to be computers. That we were going to have electronic monsters capable of doing everything from writing poetry to dancing the fan-dangle.

Probably, you know, to be delivered next year. Well our ambitions were a little over leaning. But it never occurred to us that everybody didn't agree with us. Of course, we tut-tutted, you know, the Jim Rand of Remington-Rand kind of person as being a man of little vision compared to our Tom Watson.

What we didn't realize was that our Tom Watson wasn't all that much convinced either. But you see the general pattern of good support of development and heavy salesmanship and acceptance of special tasks and so forth, was overwhelmingly obvious. What we didn't know and what I never did see really with my own eyes, was the fact that the special problem of building a commercial line of large machines was going slowly.

I just took it for granted that since they were developing new tabs, new 604s, new this, new that, and new the other thing, going into military work and converting the typewriter factory over into electronic production and so forth, that obviously they were going to build a lot of big machines too.

MERTZ:

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Your impression was at the university there was a great emphasis of this...(voice fades out).

GROSCH:

Yes. Moreover, there were things going on behind the scenes that nobody really know about to this day I suspect. I doubt if any of your tapes are ever going to really tie this down. Certainly they won't get it from me. And that is the very clear hints and gossip that you pick up that never can be proved, but there were restricted conversations about this sort of thing.

That Watson was encouraging people like General Electric for instance, not to enter the business. Because you know, they bought an awful lot of vacuum tubes from General Electric and you know, no one ever is going to know about this sort of thing I suspect.

It was probably illegal anyhow. And if it wasn't illegal, it was certainly nothing that people are going to boast about. But I think that there may very well have been some adjustments made at the very highest levels of the establishment.

The fact that the Bell System somehow or other decided not to carry on a line of relay and electronic computer systems.

MERTZ:

Although in 1946 the decision was made by Western, by AT& T as one of the co-managers with (Tom West?) that in 1946 it was decided that the entire telephone system under the Bell System, would be converted to electronic and computerized.

And R&D work was debugged in the Bell Laboratories in '46 and '47.

GROSCH:

Yes. I'm sure that's so. I've met many people...

MERTZ:

It was abandoned.

GROSCH:

Is that right? That I didn't know. That's amazing.

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MERTZ:

The R&D was.

GROSCH:

Yes, the R&D work. Yes, yes, that's amazing. I've met people who were associated with this. But of course, they were associated on the side I was closest to. Like the use of conventional equipment to support such work. As distinguished from the work itself.

And that of course, was going ahead at a great rate. That's when Dick Haming for instance, came in the picture.

MERTZ:

Yes, but he actually is...

GROSCH:

But he was helping the people that were doing this. Not doing it himself. Exactly so.

MERTZ:

The man in charge of it at this point was Clarence A. (Lovett?) who lives in Virginia.

GROSCH:

Yes. Don't even know his name. No, don't even know his name.

MERTZ:

He retired in 1949.

GROSCH:

Isn't that something. I met the Sam Williams and the George Stibitz's of course. But that's a different category entirely. They were the leftovers from the relay generation, not the beginners of the electronics generation. Very interesting.

MERTZ:

Well, you might wish to include that all of this is not really related to (your work?). You were discussing some of the people who did pass through the Watson Lab.

GROSCH:

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I think I ought to get the aerospace guys about this point. Because they are important and you are not likely to get them in too many other connections I think.

The earliest, the very earliest use of punched card equipment for anything more than bookkeeping in the aerospace industry, was at Douglas. Before the end of the War they brought in this old quasi astronomer, Ernest Claire Bower, from the Griffith Planetarium. And he tried out some of his ideas on how you might use punched card machines for things like weight and balance calculations.

Whenever you design an airplane, for instance, every time you change the design wing flap, or put in a heavier pilot seat or something like that, you change the position of the center of gravity in the aircraft. And you always have to keep track of where this is and you may have to control it and so, as well as just keep track of it.

So there is this enormous amount of just plain old sum of (A sub I and B sub I?) type of thing to be done.

MERTZ:

Right. This concludes Side I of Tape 2 of August 28, 1970 interview.

MERTZ:

This is the commencement of Side 2 of Tape 2 of 28th August 1970. An interview with Dr. Grosch.

GROSCH:

The Douglas Enterprise didn't flourish partly because you know, the equipment used was in the Accounting Department which claimed it had much more important things to do. And this was war time. This is in the like '44 or maybe very late '43.

And Bower certainly wasn't the kind of person to make much headway in a group of hard rock airplane engineers. Especially the kind of airplane engineers that they had in those days, who were essentially you know, sheet metal boys that had started out in the glider business.

He might do a little better at the space capsule kind of thing nowadays, but not then. So his place was essentially taken by a very interesting young man named John Lowe, LOWE. Lowe was a completely self-educated man and I don't believe he ever finished high school. He was an orphan or nearby and had practically no advantages whatsoever. Among other things, he had a leg lopped off as a young man and wore a rather ill-fitting wooden leg at this time.

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And he was a person of fairly difficult, rather dual personality. But he was a very expert indeed at the very low level routine work at an Accounting Installation. He was a, to put it bluntly, one hell of a sorter operator.

And he took over this sort of thing. And over a period of the next two or three years began quite effectively, to explore the use of very ordinary machines, for what we would now consider, very much down to earth engineering calculations. Notably, this weight and balance sort of thing that I was talking about. And semi-bookkeeping kind of things.

In the meantime, he was teaching himself not only a little bit of straight forward algebra and trigonometry and so forth, but actually somehow or other became self-educated in matrix arithmetic which in those days was not by any means a simple thing to do. They just weren't the books that you could take out and read. Nor, if rather (?) books existed, was there any way to find out that they existed except by you know, contact with someone who was already in the art.

But there were a lot of fairly distinguished engineers around in Douglas in spite of the sheet metal thing that I was talking about. There was a good deal of aero-dynamics. A good deal of flutter analysis and so forth being done by applied mathematical techniques. Although not on computers. At least, perhaps on analog computers, but not on punched card machines.

So, contagion was possible and it took place in John's case and he became really toward the end of the period that we are going to be taking about, pretty powerful practical applied mathematician, as well as an outstanding computer room supervisor.

Well, he was running a specialized small shop at Douglas by the end of the War. At Lockheed Aircraft a small group of people headed by a man named William D. Bell, BELL, now deceased, was doing the same sort of thing, a little more sophisticatedly, but a little further along in time than Douglas.

At North American, an aero-dynamics engineer, perhaps I shouldn't say that. But a professional aeronautical engineer named Paul Bisch, BISCH, was doing the same thing. And I presume this was also going on at Boeing, although at that time I did not have the direct contact to find out.

Each one of these people, Bisch actually was replaced rather quickly by a well-known figure today in the trade, named Jack Strong. And no longer attended IBM meetings after this first one in 1948. But these people began to be good friends of mine and to call frequently at the Watson Lab to lay out new problems and to discuss the addition of equipment.

We always discuss for instance, whether they could afford to have their own tabulator which was a thousand dollars a month. Or whether they would have to trot down the hall

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to the Accounting Installation to borrow the use of their tab since you wouldn't use the tabulator as much as you would use the multiplying functions. And a thousand dollars a month looked like an enormous investment.

Most of these men were earning in the order of a thousand dollars a month in salary or less and their total budgets were you know, of the order of perhaps a hundred thousand a year including salaries to run one of these shops. The thought that within a few years we would be paying you know, some of us would be paying a hundred thousand dollars a month in IBM rentals alone, was just undreamed of.

Although we all agreed that it was coming, we just didn't see how steeply the curve would rise. Probably I had more ideas of this sort than almost anybody because I was already beginning to plot up growth on (?) paper and things of this sort and seeing you know, that we were indeed doubling every year.

MERTZ:

Do you have any accounts of this?

GROSCH:

I think that the famous piece of paper on which Grosch's Law was essentially discovered, is probably still in existence. It's probably in one of these boxes here in this office. But an awful lot of this was communicated verbally and the scratch paper thrown away. Rather than immortalizing typical scientific publications.

Certainly this business of doubling every year sort of thing and so on, was not only not published, but I probably didn't even keep the scratch paper on which I did it. And yet I'm sure I made several speeches about it you know. At meetings and so forth and regarded it as a very important observation, simply that there didn't seem to be any place to say it.

MERTZ:

It would be interesting if you do have any documents.

GROSCH:

I remember for instance, the 1950 Endicott meeting.

MERTZ:

There are several people...(voice fades out).

GROSCH:

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Right. The 1950 Endicott Meeting, I said something that was precursor, not an actual announcement of the economy in the square root law. Although I didn't actually print the thing in an archive or publication for about another three years, two and a half years after that. And in fact, for the anecdotal value, you might be interested to know that Grosch's law, the points that Grosch's law was established by included you know, hand calculation in the sense of multiplying a number out longhand. Logarithm tables, desk calculators, 601s, Aberdeen relay machines and so forth. And I think the penultimate, the ultimate point was probably the predicted speed of the 701.

It was not, in other words, done by plotting up the fancy electronic computers to which it's now applied. It was a rule that I had observed for all kinds of calculating using everything from counting on your fingers on up.

And the universality of it, the fact that it survived as long as it did, is either due to some very interesting property of the geometrical mean or the square root or more likely to the fact it's a self-fulfilling prophecy. And the people now price equipment using this rule which has the effect of making the rule continue out very effectively.

Anyhow we will get to that a lot further down the road. I meant to say at this point anyhow, that we've got people visiting from Lockheed, Douglas, North American and shortly thereafter, Boeing and so forth.

The eastern places like Republic and Grumman were moribund at this time. Grumman was trying to make a living making aluminum canoes and I don't quite know what was happening at Republic. But they were a long way behind the West Coast outfits.

Rand Corporation or its predecessor, subsidiary organization within Douglas Aircraft, was in existence. They were working in a bank in down town Santa Monica, with their punched card machines down in the vault or something, but they existed. General Electric, I've mentioned Concordia and McGuiness, the first calculations that I ever saw made on punched card machines at General Electric, were the vibration calculations for turbine shafts, steam turbine shafts made by William Rankin. Whose name is in that gray bibliography book let that I loaned you the other day. That first of the IBM Watson Lab bibliographies that was published in an ASME or some such publication. And I had a reprint or two of it.

That was one of the things we distribute from the cupboard at the Watson Lab, for instance. But I think that there were no, I think there were no acknowledgements to the Watson Lab for that. Because I believe he did that completely independently with the help of a local IBM salesman in Schenectady.

But then, when he discovered that there was a world of other assistance available, he and later Hana Kraft, his aero-dynamics co-worker, began to come down to the Watson Lab. And then when he began to climb the executive ladder further, he sent his calculations

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over to Concordia and McGuiness to perform, and they became a part of the act, and as I say, were charter members of the ACM.

Soon there was a group of a few industrial people. I mean a few old-fashioned industrial people. A few aircraft people. A few atomic energy type people. And they circulated around at these meetings. The Bureau of Standards was slightly involved. Although not a great deal.

MERTZ:

Were you at this time, ever visited by any of the members of the or staff associated with the Advisory Board of Simulation of the air force, prior the army/air force which was under the aegis of Walter Barkey?

GROSCH:

I knew Barkey because he was a celestial mechanics man in civilian life. And I seem to remember having a vague knowledge that he was involved in this and being slightly jealous of the fact that he had a series of contacts that I didn't have. But there was no joining between the two. I think that the closest thing that we had in common was probably that he had relationships with some of the Columbia University people like Frank Murray.

MERTZ:

Well, one of the sub-groups of this Advisory Board on Simulation was a mathematics group devoted to the problem of essentially doing a survey and an evaluation of the available digital and analog computing facilities in the United States.

It was, for a while under the aegis of an astronomer Thorton Page.

GROSCH:

Thorton Page was an old friend of mine and he later taught at Wesleyan University and I saw him last, oh maybe ten or fifteen years ago. And I do remember him coming and talking to us about something or other. Probably partly this inventory.

I think it only fair to say, I've been talking almost exclusively about how wonderful IBM was and all that. But I think it's only fair to say that there was also a universe outside which was very much convinced of the power of the computer and the power of electronics and the power of both analog and digital computation, who regarded IBM as an insignificant artifact of the scene.

Their view was, you know, that it was just a bunch of dunder-headed punched card salesmen. And that when the great machines were delivered by the universities and the new companies, why IBM would just wither away and that would be the end of it.

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I make it sound invidious, as if they were gleeful over this. They were not. But the Barkeys and the Pages simply regarded IBM you know as just a grubby outfit. They put it in the same class as the (Marchant?) calculator people, I would say. Larger of course, richer. Able to have an art collection and to pay Wallace Eckert a good salary and help the theory of the motion of the moon. But not as the outfit that was going to dominate the supply of the equipment that they were talking about.

MERTZ:

Well, so far as aircraft simulators were concerned and the computational equipment associated with it, there was first of all in the analog field there was nothing IBM was interested in that I know of.

GROSCH:

I think the answer is, that we know of. I don't either. To the best of my knowledge IBM had almost no analog capability. There may have been something hidden away that I didn't know about. McPherson might have known for instance, of something.

MERTZ:

And secondly, such a thing as the SSEC was not in any sense of the word, adaptable or amenable to real (time?) problems.

GROSCH:

But you see, that's the problem. You could look ahead. If you plotted up how rapidly the multiplying speed of IBM equipment was improving, you'd see that you know, within just a few years it was going to be capable of handling real time problems.

But you're quite right. Most of people concerned with this did not see this and this included people for instance, in GE that were using digital equipment for these (roller?) calculations, but a man in the next room practically, was designing a special super cost all electronic differential analyzer sort of thing to do a different problem on the grounds that those guys next door will never be able to tackle what I want.

I give you an example out of my own later history. When I was Manager of Investigations at Evendale, towards the end of my stay in General Electric at Evendale, and where the equipment that I was then running was IBM 704s, there was an enormous project in Schenectady in the General Engineering Lab, to build a special purpose simulator for aircraft engine work. To be installed in Evendale but to be built in Schenectady by this analog group there.

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And I wrote and still have, copies of the reports by the way. This is, this I do still have. I wrote several formal letters to my boss saying that long before this thing could ever be finished, it would be outmoded. And before the catch-up work was finished, it would be outmoded again. And if this process indeed, ever converged, it would not converge until a time at which digital speed had overwhelmed the analog business and that the whole thing should be thrown away and done digitally. Or other alternatives.

The digital point being of course, then if you change the programs so to speak, you could do it without junking the machine. Which was not true in the case of the analog device. And that analog in fact, was built at enormous cost and paid for by (Wright Feel?) and installed in Evendale and indeed, never did really do anything very useful. It just sort of died.

So I was right to that extent. But this is, I'm now talking about 1955. So we are a long way in the future compared to the present.

MERTZ:

In 1949 and or around '50, Reeves took on someone who was, whose role was to design and build digital computing equipment for Reeves to get them into the market which they had established (?) and the analog.

GROSCH:

Sure, sure. Was this Sammy Lubkin. I forget who it was, yes. Sure I knew Sam.

MERTZ:

One of the questions is, did he have any contact with, in as much as his task was to try to put Reeves into the commercial data market, did he have any contact with (Watson)?

GROSCH:

No. None whatsoever that I know of. I knew Sam very well. We met at ACM meetings and AIEE meetings and so forth. Not at the Watson Lab. He did I think, have contact with Frank Murray. But then Frank Murray would go to Reeves to do that, rather than the other way round.

And Murray himself at that time by the way, was interested in analog equipment not only because of his connection with Reeves, but this man Bob Walker was building a small analog simultaneous linear equation solver for Murray which was analog in nature.

So that, as a matter of fact, I should go back and say that that's the only analog thing that I know of, the only analog, general purpose analog that I know of in the IBM Company in those days. That would be '47 or '48 vintage.

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He was still struggling with it actually in '49 but we'd given it up as a useful, as a useful gadget.

MERTZ:

Had he gone back to MIT to examine some of their...

GROSCH:

No. My guess is...

MERTZ:

...linear simultaneous equations solvers which MIT had?

GROSCH:

There were others too, you know. There was a publication in the Journal of Applied Physics which I'm sure is in one of the later IBM bibliographies. Some guy that RCA Princeton Laboratories that was building a pot-twiddling simultaneous equation solver.

I was contemptuous of these things even then. Because it was obvious to me that once you got the cards to run through the machines fast enough, you were going to get very much more accurate. These were convergent approximations you remember. You got a sum of the square residuals and then you twiddled the pot until it was reduced and then you twiddled the pot some more, and after a while they got down to zero or there about. So the whole problem of (determinacy?) of course, and so forth, just kills you.

And when you could run six figure accuracy, or eight figure accuracy at the same speed as two figure accuracy, on the digital equipment, it just seemed obvious to me that that was ultimately the way to go. The question is, when would the machines be fast enough that they would into a matrix in a few minutes?

And I, when the 604 was delivered, I found that for a 12 times; 12 matrix for instance, I could beat Walker's and Murray's knob (twiddling ?). Oh, I guess that's too much. But for a 8 times; 8 matrix, I could beat Murray and Walker's knob twiddling right then and there and get six place accuracy to their two place accuracy.

So after that I lost interest in that class of equipment. But certainly, there were many other people who disagreed with me. I remember Vance, who was then I think, at RCA, at one of the very earliest Western joint computer conferences. Before they were Fall and Spring, they were East and West. At one of the very earliest Western joint computer conferences describing some elaborate analog that he was going to build. With dozens of, hundreds of integrators and multipliers and so forth. And I remember rising in the

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audience in typical abrasive Grosch, latter day abrasive Grosch fashion, saying this was a lot of (?) and if they'd spend a very small fraction of that effort on little numerical analysis and digital work, they would get the same results and with full accuracy etc, etc.

And all the analog people in the audience booed. And all the digital people in the audience clapped and that was the end of that. And he just went ahead doing what he was doing and I went on doing what I was doing.

MERTZ:

There was an actual competition run on the SSEC about this time which was programmed by Richard (Havens?) I think it was. The computation of ballistic test. A set of ballistic, specific ballistic data which was run at one fifth the cost of the SSEC compared with the...(voice fades out).

GROSCH:

Doesn't surprise me a bit. This was not, however, a point on Grosch's law.
(LAUGHTER).

But you know, the thing that counted in all this was that not only was there a rapidly expanding community of people and each one of the members was rapidly expanding his intellectual as well as economic installation, I mean the applications of his installation as well as its size. Not only was this so, but that the people that I was in frequent contact with in this, were very much down to earth types.

These were not the academics who were talking or who talked later about Turing's hypothesis and all that sort of thing. These were people who were assigned to design a lighter wing spiral or a stronger wing or a more compact nuclear weapon and let's have it tomorrow buddy.

And as I've said in hundred of speeches since, they had to face up to reality. And that was the tool that was available in quantity. If all of a sudden you wanted to double your capacity, you could call up your friendly IBM salesman and a whole bunch more of 604s and CPCs or 650s or what have you, would shortly arrive.

True, shortly might be several months, in the case of the bigger machines, but on the other hand, if you went to see one of the academics, he would tell you, yes sire, we are going to have the most wonderful machine ever known to science here called the (ELIAC I?) or what have you. But you know, it was always going to be ready, as Von Neumann used to say, two years from any given moment.

So if you really had to grind out the stuff, you really had to come back to IBM. And it was the division between the people who had time to wait and those who just had to do it

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right now that I observed. And I found myself very much happier with those who had to do it right now.

After all, if you look at my own personal history, I'd struggled along with huge celestial mechanics calculations with a most inadequate tool. And I'd done shock wave partial differential equations on a 601. It seemed to me relatively luxurious to have some of the equipment like a 604 available.

Whereas to the people who were just entering the art, it just seemed absurd to just sit there and feed cards through a machine for several hours just to invert a little old itty-bitty matrix, when they could write out the inverse of the matrix you know, in thirty seconds symbolically.

The trouble is, It didn't do them a damn bit of good when it came to a (?). They had to have a numerical values which they didn't have any way of getting.

MERTZ:

Well, to get back to your own career and your decision to look elsewhere within IBM. Roughly when was this, about '49 or so?

GROSCH:

I'd say it was '49. And I think that a large element in this was probably pretty straight forward jealousy of Cuthbert Hurd. Because all of a sudden he had many of these people that had been students of mine or associates of mine at the SSEC working for him. And he was sending them out all over the Country and giving speeches about how wonderful the Applied Science Department was. And in fact, being very welcome indeed in the main IBM organization.

With which I was still very much impressed. And as you know, I'm still very much impressed today. And I was still sort of stranded in this back water at the Watson Lab. Now it was easy enough for me to..

MERTZ:

Had you reasonably well progressed in terms of...

GROSCH:

No. My title when I entered was Senior Staff Member of the Watson Laboratory and five years later it was Senior Staff Member of the Watson Laboratory, period. And my title at Columbia was Research Associate, Astronomy Department, first year and last year.

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I didn't mind this a great deal. My salary increased and all that. I doubled my salary in that five year period. On the other hand, there were many, there were many people nearby who tripled and quadrupled their salary in that period. And as I say, I've always been able to spend anything that was lying around loose. So I would very much liked to have been earning more money.

Clearly Havens and Lentz for instance, who had been brought in as I remember, at 8400 to my 5200, they were probably earning, Havens especially, probably earning 16,000 by the time I got to 10,000. Well Havens was worth 16,000 to IBM. He was worth a great deal more probably than 16,000 even in those days.

And when I get to the story of how the NORC was designed and built so forth, I think you will agree with me. But the fact is, I thought I could be worth that too, if somebody would put me somewhere where I could get my, you know in the Archimedean sense, if they would give me a fulcrum I could move all sorts of things.

MERTZ:

Where was your (field ?) at that point? Something like what Cuthbert Hurd turned out to be doing?

GROSCH:

No, because by this time I had begun to see something a little different. And it was a continual thread through my career for the next ten or fifteen years.

I'll back off a little bit and mentioned that pamphlet I gave you the other day from the Scientific Computing Service Limited which I said was the World's first software house or scientific computing laboratory. The man Bill Bell at Lockheed in the late '40s persuaded Lockheed to release him and the people working for him and let them take away with them most of the IBM equipment that was installed in their scientific technical computing laboratory there.

Of course it was rented, so that all that Lockheed did was turn it back to IBM which then rented it to Bell and start a thing which was then called Tele-Computing Incorporated. And that was the first American service organization and the first scientific punched card bureau, the first scientific punched card service bureau.

MERTZ:

What you might call a spin-off.

GROSCH:

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A spin-off of Lockheed, exactly. Lockheed wanted to cut back in this activity and yet many of the engineers said, but we've got to have it even if we don't have to have it every day. So Bill and his cohorts, the actual president of the new company was Ward Beman, BEMAN, but Bill was really the key person in it and the actual Head of computing, as I was Head of computing at the Watson Lab.

They actually made a substantial success of this and Bill, among other things, wrote the first management book on computers called, "The Management Guide To Electronic Data Processing" published by MacGraw in the mid '50s.

He wired the first general purpose CPC board which is a way of loading up a card program calculator plug board so that it will do trigonometric calculations and floating point calculations and so forth. And you can call on one or another by holes punched in the card.

The predecessor of the full card program calculator kind of concept. Because he had this going in certain ways on the 604 actually. Very ingenious, very hard working and very...

MERTZ:

He was not with IBM though?

GROSCH:

No, he was with Lockheed then, then went in his own company. And I had seen this and I had begun to equate this with the fact that IBM said over and over again and had said since the early '20s, that their business was not selling machines, but to sell service.

Along with (FANK?) and World Peace through World Trade and so on. They had many other acronymic type slogans, one of which was that 'we sell service'.

MERTZ:

Well, did IBM give him any sponsorship by...(voice fades out).

GROSCH:

To the best of my knowledge, no. To the best of my knowledge, no. He died some years ago and we can't find out from him, but I think that the answer is probably not. IBM and the sales organization was fairly hard nosed about this. That the idea of discount for universities and so forth was still there, but he was not a university. And I suspect that he had to make his way as is.

I think they probably wouldn't have (doned?) him for a late bill or something, quite as much as they would have a (clothing?) district, customer. But I think that was mostly

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because they figured they'd get it out of Lockheed if they didn't get it out of him. So he had a cushion.

Well I saw clearly the possibility of doing this on the East Coast. And my first idea was to do this as an off-shoot or parallel operation to the Applied Science Department. That applied science was essentially giving away advice. Why not sell some services instead.

Well after I talked about this enough, Cuthbert Hurd put in card program calculator in New York City and started doing it. Leaving me of course, up at ye olde Watson Lab as before. So I not only didn't get any credit, but I didn't get the opportunity to run it.

MERTZ:

With whom did you discuss this, McPherson?

GROSCH:

Yes, I discussed this with McPherson and Wallace Eckert.

MERTZ:

Hurd?

GROSCH:

Not so much. We were sort of rivals, although friendly rivals. Not ugly or nasty, but I tended to talk it over more with the Bill Bells and the John Lowe's and the Jack Strong's and the McPherson's and Eckert's than with Hurd.

But Hurd was enormously vigorous and had the ear of the appropriate people and had no difficulty at all in setting one of these up and getting it going.

MERTZ:

For IBM?

GROSCH:

For IBM, that's right. Then I talked a little bit about doing this in another city, thinking that thereby I might get away from Hurd a little bit. It was pretty generally understood that I didn't want to work for him and that, very clear indeed that he wasn't going to work for me. So there was no, there wasn't a question of starting another one for him somewhere.

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Well, I could undoubtedly have done that if I'd knuckled under. So I started poking around with that and the story is told in the Rogers book how discouraged at that. I then began to think about leaving the Company. And I went to Mutual of New York, the insurance company there near Columbus Circle, which had just opened its fancy new sky-scraper which looked like a reasonably advanced thinking outfit.

And remember, I knew that Prudential had fouled up with Berkley and I didn't have much of a line into Metropolitan, but it was already pretty clearly established that Metropolitan was going to go with Eckert Mauchly if and when Eckert Mauchly ever got going, which it hadn't get done.

So, I went to Mutual of New York and finally managed to get my way after some (?) since I wasn't very good at direct selling so to speak. Managed to get my way into a second vice president type who was charged with you know, operations and that sort of thing.

Made a pitch to him that I would take a consulting contract to survey what actuarial and similar problems could be put on something better than punched card equipment and what equipment or compliments were available for this. And then lay out a one shot type machine that they might build or suggest a service organization or methodology whereby they might share the use of a special machine or something.

And he was sufficiently impressed with this to take it to the Board of Directors. The only trouble was that Tom Watson Junior was on the Board of Directors. (LAUGHTER). I didn't even know that there was homework like that to be done. It was just a question of having done it. I didn't even know one went and looked up the Board of Directors at Standard and Poor and so forth. I don't think I'd even seen Standard and Poor in those days.

So, not knowing that there was homework to be done, I hadn't done it. And when the Board of Directors got this, Tom Watson Junior pipes up on the other end of the table you know, and says, say, whose proposal is this? Well, it's a gentleman named Herbert Grosch. I know him. said Watson Junior. I know him. (LAUGHTER). And the next day I'm on the carpet at (?) Headquarters.

MERTZ:

When was this?

GROSCH:

Oh, this would be...

MERTZ:

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'49?

GROSCH:

This all happened pretty fast once it started. No, I would guess this was probably, this was probably the Spring of '51. We are moving along now. There's a year of discontent in here in which I'm poking away at this stuff. And I think this must be, it might be the Fall of '50. It might be the Fall of '50.

Let's see now, my first, my first course at the Watson Lab, I mean at Columbia, was in '46, the Fall of '46 and the end of that semester in '47. '46, '47, '48, '49, '50, the last year I taught it then was the year '50/'51. And I see to remember that I had to curtail Spring class of celestial mechanics or something I had...No, I must have given my numerical analysis course in the Fall of '50 and then curtailed the, you know, the post-Christmas kind of thing and gotten ready to go down to Washington and take up my new IBM duties down there in the very early Spring of '51.

And I remember, I was fired in late '51, like November 1st or something like that. So that now fits together pretty well. So the interview with Watson in which he said, what the hell are you doing in Mutual of New York when you are employed at the Watson Lab? must have been January or February of '51. Probably December of '50 or January of '51.

Well, I was of course, somewhat dumbfounded to have this appear on the scene. But I told him essentially that I was trying to do this sort of thing in IBM and had not been able to do so and was exploring ways in which I might do it outside.

And he, having apparently checked in advance, before calling me in, recalled that I had suggested this idea of starting a service, scientific service bureau, in Washington. And gave me 24 hours to decide whether I would move to Washington and open this bureau right then and there or resign from the Company.

And I went home and talked to Dorothy about it and decided that since obviously Mutual wasn't going to take me up on this, that I had better accept the offer. And besides it was something that I wanted to do and had not realized that I could, if you want to put it crudely, I could blackball them into it.

Actually my appearance on the scene at Mutual had been the only reason that Watson had ever heard of this. It had not reached him before this. Watson Junior had heard of this. It hadn't reached him until, until he tripped over it at Mutual.

And having reached him he was in favor of it. Moreover, he did not put me under Cuthbert Hurd. He sent me down to Washington and I then reported to the General Manager in Washington DC.

MERTZ:

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He had done his homework then?

GROSCH:

He'd done his homework.

MERTZ:

About you?

GROSCH:

Yes. Well, I was a well known figure in the Company by this time. I was an iconoclast of course, and at a time when it was against Company regulations to wear a moustache I wore a beard. And at the time when it was against regulations to employ married women, I had a whole staff of them. And at the time when no knowing customer entered World Headquarters except in a dark suit, I would visit in a sport jacket with a plaid lining.

I think that the important point there is not only a certain iconoclast on my part, but a clear understanding of what these rules meant in IBM. They were already aware of the fact that there were some people important to them who would not conform to the typical salesman pattern.

And of course, today, they are willing to hire people who practically don't wear shoes if they have got good enough heads. But they don't accept such people in their sales organization. And actually, even in the scientific organization, if you are a shoeless type, you can never expect any kind of executive responsibility or even planning responsibility.

You may lock yourself away in your laboratory and invent the World's greatest solid state components, or write the World's greatest programs, and you may get an extremely good compensation for it and Mr. Watson himself may pat you on the back and say you are a great value to the IBM Company. But he isn't going to let you run anything.

Because people who run things wear white shirts and striped ties and salute the flag. So, there was no question I think, that I would be fired for wearing a beard, although (Willie and Chum?) wanted to have me fired for not having my tonsils out on command, which is another story. On the grounds that I was a health menace to the rest of the organization. (LAUGHTER).

MERTZ:

Did you actually have to have your tonsils out?

GROSCH:

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After some months when the pressure of getting the shock wave calculations going had subsided slightly, I did manage to slip down to Headquarters and have a physical examination.

They had a very elegant internal medical department. You didn't have to go out to a doctor on the street anywhere. And said doctor observed that my tonsils were enlarged and recommended they be removed.

Well his recommendation was their command and I was instructed that until I had them removed I could not be employed by the Company. After which I pointed out that I had been getting a pay check for some three or four months and wasn't about to either have my tonsils out or stop working.

So there was a general feeling that maybe it would be better to ignore it. I still have tonsils to this day. But it was, it was presented you know, pretty briskly that the IBM Company wants you to do the following things, 1,2,3,4,5. And it just happened that most of them were fine except that number 3 like was have your tonsils out which I said no to.

And in the same thing, I don't think that Watson was, Watson Junior or Watson Senior either one of them was prepared to run me out of the joint for wearing a beard. Because you know scientists wear beards.

But scientists don't run major IBM organizations either. Or certainly they didn't in those days. So you get that sort of (balance ?).

MERTZ:

Well that's something of a concession though...to set up something which is running...(voice fades out).

GROSCH:

Yes. Bob Walker had a moustache for instance, and he kept it. But as a matter of fact, he could not have been hired as an IBM salesman or as far as that goes, as an IBM clerk or accountant or something, in down town IBM with that moustache. And I most certainly would not have been hired if I had been passed through the standard personnel activity. I would never have gotten anywhere near Wallace Eckert if I had walked in off the street and said, I understand you're starting a new laboratory and I would like to apply. They would have given me a piece of paper to fill out and meanwhile someone would have said to somebody else, just look at that funny looking character over there with the whiskers. And when I had gotten through filling out the piece of paper, it would be neatly torn in two and dropped in the waste basket.

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Or filed and then they forgot about it.

GROSCH:

Exactly. Exactly. As a matter of fact, yes, there would be you would probably enter the files with a notation across, beard, obviously unstable. (LAUGHTER). Or something. There weren't all that many beards in '45, so I guess I shouldn't complain too briskly about that.

But since I came in you know, from above so to speak, and was imposed on them by Eckert and McPherson without anybody down town ever seeing me until I'd drawn several pay checks and had gotten all these machines in and running, why these normal if somewhat stilted, (soul defying?) requirements were never brought to bear.

MERTZ:

Well then, when you were...

GROSCH:

For years when I went to the elevator bank at (?) Headquarters, they would ask me to go in and register at the visitors desk because they couldn't believe that an IBM employee would look like that. And it wasn't until they knew that I was that Doctor Grosch from the Watson Lab that they quit running me into the visitors booth.

MERTZ:

At the time you were given, perhaps in January the ultimatum to go to Washington and which ultimatum you accepted, and did you go?

GROSCH:

Yes I went. And I spent 6 or 8 months down here very happily indeed. It was an enormously difficult year for me because in that one year I was President of the American Rocket Society. What was then the Space Organization. A small society of only 2500 members, but a rapidly growing one. And one which was of course, very much in the public eye.

I was National Program Chairman of the Optical Society of America. I was giving speeches about computing all over the IBM Corporation and to a certain extent, now that ACM was afloat, in professional meetings and so forth. I was trying to keep up with Cuthbert Hurd back in New York engineering organization. And I was starting a whole new shop down here in Washington under what turned out, to be rather difficult conditions.

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So, I was busy. I was busy. And in fact, the difficult conditions turned out to be the fact that while Watson had the power to send me, the people down here had the power not to receive me with any great enthusiasm. And this was not the fault of anyone in New York particularly, but IBM Washington in those days, was the personal (?) of Louis H. Lemonte, LaMotte, LaMOTTE, Junior I believe. Who is still, as a sort of an emeritus member of the Board of Directors, is still hovering around IBM somewhere. Although he must be in his mid '80s by now.

Red LaMotte and his family had substantial holdings of IBM stock. But in addition to that he was what we would now call the Washington Vice President of IBM. He was the guy that made sure that Congressional relations were good. And he went horse back riding in the morning with the Quarter Master General and things of this sort.

And under him was what you might call, the overall Federal operation. The work done actually in Washington was under a local manager named Don Gamel, GAMEL. A young, vigorous hundred percent club winner type salesman. To whom I reported directly.

But it was LaMotte's (?) and he hadn't asked to have a scientific computing bureau established. And in fact, probably had said no without my knowing it, in the earlier days. So when I appeared on the scene with my little satchel and said, I'm here to start the bureau, why there was a certain feeling of you know, get lost buddy.

In those days, there was a Washington Service Bureau for accounting work as there was in most large cities in the United States at that time.

MERTZ:

Was there no Federal Assistance Division?

GROSCH:

No Federal Assistance Division at all. No sir. That didn't exist. And this little thing had just been moved from the card plant where they used to make punched cards down on New York Avenue to its own little small location on 19th Street. Since torn down, but it was a two story store front type building. A rather nice one. Most of which was already, the first floor of which was almost entirely filled with standard punched card machines with a nice glass window so that you could see them running and all that. And a small office for Scottie, I can't remember his last name now, but he was my associate in the activity and his nick name was Scottie.

A very pleasant little Scots man he was indeed. Who had come from the card plant with this bureau to run it. Well they were doing the typical niggling service bureau work of little tiny pay roll for somebody. Or little tiny pay roll or set of bookkeeping for

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somebody. Or key punch 162 cards in 14 columns sort of thing for eighteen dollars. Very small scale operation. And I was drafted on to this with permission to install the prototype model II card program calculator then being checked out in Endicott.

And this machine alone cost about as much as all the equipment in the Standard Service Bureau. Moreover, I insisted that in order to work efficiently, I would have to have my own reproducer and tabulator and sorters and so forth, rather than using the ones in the front.

So I ended up with a fairly sizable compliment of nice shiny new equipment and quite expensive equipment. While the Service Bureau stuff in front was pretty much banged up. And I brought down with me from New York, two or three people to help me run this. Notably Stan Rothman, who is a distinguished figure at TRW at the present time, and who worked for me a total of three times in his career. And I made him an offer here once too, but he turned that one down, number 4.

Stan had worked for me in the Watson Lab while he was taking his Masters Degree in mathematical statistics at Columbia, but as a more or less full time employee. And had taken some classes from me.

Then I brought him down to Washington and later on I hired him again from the outside world after he left IBM. And he worked for me in Evendale, for General Electric. And he left and went to the Rand Corporation and then to TRW. Well, I brought Stan down as my hot-shot machine man. His task being to find out how this brand new model card program calculator worked. And to wire up the general purpose plug boards required for it.

And as the payment for getting this prototype machine which about a hundred customers were clamoring for, I agreed that Stan would also wire up the test panel, what we would call the diagnostic nowadays, the diagnostic panel to see whether or not this machine was running correctly and write a descriptive manual for the diagnostic procedure for the use of the customer engineers.

As an additional bargaining point, I insisted that if he was to do this, he should have a good customer engineer. So a man named Carl Suthert, SOUTHARD, who is now in the Federal Systems Division here in Washington as a fairly senior planner or engineer, should be my customer engineer.

He was sort of a latter day Richard Bennett. If you remember the man I thought so highly of in Washington and at the Watson Lab. Carl was one of the few people in the Company who was permitted to be slovenly.

He always had a cigarette dangling out of the corner of his mouth. The ashes from which fell into whatever machine he was fixing. He tended to leave burns on the (?) board of the tabulator from putting this cigarette down. His shirt tails were usually pulled out. And

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the shirt front was usually covered with oil. Although indeed, it was a white shirt, as required by regulations.

But he was an extraordinarily capable customer engineer. The sort of person who would diagnose the machine troubles by thinking, rather than by physical test. And would look at the machine with a sort of glazed look in his eyes which was kind of hard to detect, because he had a glazed look in his eyes most of the time.

But when he was standing in front of the machine, and indicated he was, he was diagnosing. And after a while, he'd say to Stan, Rothman, why don't you do so and so. And Rothman would push a button and would put in a pre-punched card of some sort and it would do what Southard thought. And then he'd say, fine, let's open up the gates and go in and pull out one wire contact relay and replace it with another one. Or file it, or fiddle it, whatever it is he did. Put it back, and the machine would run. Extraordinarily good.

Obvious human synthetic ability that we haven't yet succeeded in putting into very many computer programs. Believe me, diagnostic or otherwise.

Well, along with these two, Southard didn't report to me, but was assigned to me almost full time. And was too proud to monkey with the old Service Bureau equipment up front which any customer engineer could fix.

Another source of conflict between me and Scottie. I brought down a older man named John Mayhew who later went back to (?) at Fort Worth. I guess it was Consolidated (?) Fort Worth or something in those days, who had some aerospace capability and was a very precise logical sort of person who had been employed as one of the SSEC programmers.

Rothman had been at the Watson Lab. And a very handsome gal whose unmarried name was Libby Lindberger. And the married ban was off by then and she wanted to come to Washington because she had just gotten married to a handsome young man who was going to work for the State Department. And Libby was one of these bright young girls we had around the Watson Lab and the SSEC. I don't remember at the moment which one.

Aside from her physical attraction, she was a hard working and very, very intelligent gal. Many of the ones that I've mentioned already were exceedingly bright. And she was a real catch. And this little package of people came down there with me.

I managed to secure the card program calculator as promised after many, many social and managerial setbacks. And we began to get some business. We did some semi-business type calculations. Mortgage repayment sort of things for the Federal National Mortgage, (?) or whatever their proper name for it is.

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We did, believe it or not, some early simulations on air traffic control. Very simple. Ultra simple stuff for what was then called something like the Air Navigation Development Board or something like that. I'm sure you've hit it in your simulative adventures.

It was an intermediate agency that came before the FAA but after the whatever the predecessor of CAA or whatever the predecessor organizations were.

It was essentially an R&D activity rather poorly funded that had picked up a few people from the Rad Lab and a few things like that. And we were essentially doing digital simulation of the reserved air space around moving airplanes and so forth.

Very primitive sort of thing. But the sort of thing that was later done very much more sophisticated fashion and at three dimensions at Sage or before that at (Whirlwind?).

I can't remember the name of the man...

MERTZ:

Contemporary was this? Because they, their first traffic (?) began...with the Berlin air lift.

GROSCH:

Yes. Exactly so. I knew nothing about this however. Yes, I remember Forrester and Eckert saying something about this. But I knew nothing of this at the time. And the man who brought the work in and who paid for it, I think was not too well wired in on that sort of thing.

I think he was more of an old civil servant type and was essentially following orders. We had two or three other things. I don't remember what they were. Most of the time when the machine was occupied with the construction and testing of this diagnostic board and the general purpose board, both of which of course, were far more complicated than these two paying tasks.

MERTZ:

Did you more or less pick the people that you brought with you?

GROSCH:

Yes. Yes. I was given essentially, carte blanche to bring a few people. If I'd asked for ten, it would have been of course, immediately vetoed. But practically speaking, any three and it was just a matter of, well any three or four, it was a matter of being sensible about it.

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Herb Grosch Interview, August 28, 1970, Archives Center, National Museum of American History

MERTZ:

Fine. Well, now that we've established you on the scene in Washington we are about to run out of tape.

GROSCH:

Good, let's stop for today.

MERTZ:

Why don't we conclude.