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[Tape 2, Side 1]

GROSCH:

Watson Senior had enough vision in '46 to realize that the dedication of the ENIAC at the Moore School was an extremely important thing to be taken account of at IBM planning. He looked into the IBM patent and engineering records and discovered that there was prior electronic knowledge available in IBM. That they weren't completely behind the times. But the work that had been done in IBM going back into the late 30s and a little before was pretty primitive by the real ultramodern electronic standards.

The most advanced of the IBM inventors before the War was a man named Halsey Dickenson. I think Jr. But I'm not real sure. And I met Halsey in '45 and had quite a little bit to do with him for the next two or three years after which he sort of slipped away. His picture is in the SSEC brochure and there are quite a few other records of him, but he isn't too prominent a person in the IBM records. He had fundamental patents—I guess I don't know enough about patent law to say fundamental—but he had important patents in the use of the Eccles-Jordan flip flop circuitry for straight forward punch card kinds of calculation. By punch card kinds of calculation I mean something you could put inside of a 601 instead of a mechanical counter to actually perform the arithmetic operations. I don't mean the sophisticated control circuitry and stuff that we have nowadays in computers. Just very primitive arithmetic circuits based on the Eccles-Jordan flip flop.

Well it didn't take very much investigation by IBM engineers to find out that this was just about as hotsy totsy as anything in ENIAC. ENIAC actually built by the Presper Eckert Moore School kind of people wasn't very advanced electronically either. It's just that if you do anything with electronics it tends to come pretty fast compared to doing it with mechanisms or with relays.

Two attacks were mounted. In the first place, Watson gave orders in '46 that there should be as quickly as possible an electronic item in the IBM line. This was supplied within I think less than eight months by a machine called the 603. Now the 603 was a small gang punch, I think a, 517 or 514 gang punch, I'm not quite sure of the serial number, a machine with only a single feed in it but a simpler form of the reproducer that I described a little while ago.

Tied to this was a big box that looked like a space heater or a large radiator. It was rounded and in those days almost all IBM machines were pretty square and angular, so the fact that it was smoothly rounded was in itself interesting. I'll tell you what it really looks like, it looks like one of these big Samsonite suitcases, you know, that have sort of rounded corners and look like they're made out of fiberglass. If you can imagine one of those about twice as large in all dimensions you've got a pretty good idea.

There was a grid around the sides and top of it, the seam where the suitcase would open, so to speak. Which allowed the hot air to rise from the tubes inside. And inside there were something on the order of several hundred—I use the term several because my inclination is to say 300 but I really don't remember any more how many. I think I have a brochure for it that tells us but it's hidden away in my archives somewhere. But several hundred large sizes, inch diameter tubes which performed the multiplying function.

All that this machine did was that it would read two numbers, two six digit numbers from a punch card. And before that punch card moved from the brushes to the punching station one card width away it would multiply these two numbers together, round the result off to six digits and punch it in the same card. It would not add or subtract. It would not accumulate the totals from card to card. It would not even punch all twelve digits of the answer or allow you to control rounding. All it did was A times B equals C. But it did it at gang punch speed which was 100 cards a minute, 6,000 cards an hour. And 100 cards a minute was pretty close to 10 times as fast as the 601 would do the same operation. Five or six times as fast anyhow. And it rented for approximately the same price as the 601. They made about 20 of these machines. They mass were a mass production item but it was small mass production. The main purpose of them was to get them out as fast as they could so they could say that they had a standard item in the IBM line with a regular serial number and all that, type number which was electronic. And they used Dickenson's circuits to do this.

Now while all this was being done in Endicott everybody concerned of course realized that this was just a primitive unimportant long term project but they wanted to get something out fast.

Before I go on I should say these machines were rented commercially—it was before IBM sold equipment—they were rented commercially. One I know was run by Ben Ferber at Convair a couple of years later. And I believe that one went to Boeing. So at least two of them were used for technical computing as distinguished from business data processing. The main use for them in business data processing was an inventory sort of thing. You had a whole bunch of jobs in inventory control where you wanted to multiply the number of units by their unit price or something like that and punch the answer in the same card. And for that it was a commercial success. Most people wanted to do something more complicated than that and of course the machine was not capable of that.

MERTZ:

One question in connection with Dickenson and the patents on the variations or the subsequent refinements of the Jordan flip flop. Do you happen to recall what dates these...

GROSCH:

These went back to '38 I think. Yes.

MERTZ:

Were they held by IBM?

GROSCH:

They were held by IBM. Assigned by Dickenson to IBM. And they are quoted I think in later law suits and so forth as being the earliest important IBM electronic patents. I'm sure they had earlier ones going back into the '20s, but those would be for you know card reading, amplifying circuits on brushes, and things of this sort, where this was a calculating circuit that I'm talking about.

Now I'm going to put aside for the purposes of this afternoon at least the modern electronics that we were trying to start at the Watson lab based on MIT Radiation Lab people and so forth. This ran through the NORC computer and so forth, and at the same time the same sort of people but hired down at the Watson Lab and elsewhere at IBM were developing the 604, the 701, the 702 and so on up through the line. But I'm going to put all those aside for later recitation and talk about the evolution of the old fashioned electronic circuits that Halsey Dickenson had. Because at the same time that we were trying to build up a modern capability with ultra high frequency pulse technology and so forth, and at the same time that the regular old product development lab was grinding out these 603s, there was an extraordinarily interesting intermediate venture that's not only historically important but gives you a very clear view of IBM as it then was, especially Watson Senior as he then was. This was called the Selective Sequence Electronic Calculator, SSEC. It was the second great electronic machine and the second great IBM machine.

The first great IBM machine of course being the MARK I, the first electronic calculator of course being ENIAC.

Sight after the announcement of ENIAC Watson Senior put together a small committee which included John McPherson and Wallace Eckert, and later was dominated by Frank Hamilton, who you remember had worked on the MARK I machine as sort of No. 2 to Lake. And this group was instructed to come up with the specifications of a machine much more important much more flexible, much more rewarding to use than the ENIAC, and which would have IBM's name plastered all over it.

It's important to note that there was never any question of giving such a machine to anybody else. It was never considered that this would be given to Princeton or Harvard or to Aberdeen Proving Ground or anything like that. IBM claimed all along that it made the Aberdeen machines at an extremely low rate or even perhaps almost gave them away. This may or may not have been true. But there was no question of giving away the SSEC.

Within one year specifications were originated, approved by Watson, the machine was laid out in architectural form, designed in full detail, manufactured, put together in Endicott in the engineering labs in Endicott" and tested, torn down and moved to New York City and re-erected in Galactic Headquarters and dedicated. In one year. The people concerned worked on it a minimum of 18 hours a day 7 days a week.

Every resource that could be brought to bear on the project was brought to bear on it. Now this is trivial compared to the resources available today, you understand. A couple of hundred people at most. An enormous effort for IBM in those days.

MERTZ:

Was the committee summoned in February of 1946 or was it...

GROSCH:

I would guess that it was at work in March. I think that it took a few days for people to digest what had been shown at the Moore School and to decide that something ought to be done. And I would presume to call it to Watson's attention. That I don't know. That was over my head. But I was aware of it from the beginning because I was very close to Wallace Eckert. I was his number 2 man. And he was immediately involved in this. In fact, was probably the single most important person in drawing up the specifications for the machine.

MERTZ:

Lake - didn't participate in this?

GROSCH:

Lake did not. He was an advisor to the project, but very early in the game it was decided that he was a little past running such a crash program and Hamilton was chosen for it. The brochure for this machine however has Si Lake's picture in it and so forth, and of course thousands of his relays were used in it, so his final monument in a sense was the banks of Lake relays all over the place.

Dickenson’s circuits were used in the arithmetic unit, right. He himself is prominently featured in the brochure. He didn't have an awful lot to do with the detailed work. He

had had other assignments during the War and wasn't actually doing much electronics at that time. He later became a sort of a part of the patent operation in IBM. He was an advisor to the patent office.

MERTZ:

That would mean that by February or March of 1947 it was functioning.

GROSCH:

Yea. The dates on all this are available. It isn't a terribly deep secret or anything. It's that you know I'm doing this orally and I don't have it all at my fingertips.

MERTZ:

The reason why I'm pinning down the beginning and the concluding dates is that in between the establishment of this committee or group and the completion of the SSEC, a rather important series of lectures took place, I believe at the Moore School.

GROSCH:

That's right, and to which IBM people were specifically not invited.

MERTZ:

Which would be interesting to comment...

GROSCH:

Yes. To the best of my knowledge no person who was employed as part of the old IBM organization attended those lectures although there were several people attended them who later became IBM employees and brought their sets of books and stuff with them. I believe Hilleth Thomas who was just about to join the Watson Lab staff at that time went to one or two of them. I know he had a set of the books. Eckert and I were most annoyed not to have been asked because we considered ourselves an important part of the computer fraternity.

MERTZ:

Is there any particular reason why IBM was excluded?

GROSCH:

Yes, I think so. I think these people were trying to keep it for themselves essentially. Aiken and his group were trying to make their fortune by selling more machines to

people, and I have no doubt that Mauchly and Eckert had already thought of doing the sane thing.

MERTZ:

There were many people from potentially rival computing organizations...

GROSCH:

Were they at that time? Most of them who were...

MERTZ:

WHIRLWIND personnel ...

GROSCH:

But there wasn't any WHIRLWIND at that time.

MERTZ:

1946?

GROSCH:

Don't think so.

MERTZ:

The project was in existence since'44.

GROSCH:

OK. Anyhow, the point is that those were all military projects. Now you must remember that the Aberdeen machines that I told you about were strictly IBM commercial contracts to design and build a machine and sell it or give it as the case may be. They weren't development contracts in the sense of do research. They weren't funded by the earlier equivalent of ONR or anything like that. They were not NSRD projects or anything like that.

MERTZ:

Well, would you say there was any particular reason why the Moore School of Electrical Engineering which was the school which sponsored the ... would have any particular ill feelings or...

GROSCH:

Oh I don't think in the sense of it carrying over Aiken's difficulties at all. I think it was simply that the academics just didn't think much of IBM. IBM was regarded as a crass bunch of salesmen that made punch card machines. Which was true, but they were a great deal mo»e than that besides, and that they didn't get credit for.

MERTZ:

Would this might have been a result of perhaps lack of much contact, communication...

GROSCH:

The contact was certainly available for those who wanted it because the people who were building the ENIAC obviously visited Aberdeen frequently, and there at Aberdeen were some relay machines built by IBM doing real ballistics calculations that they hadn't been able to do yet. That was worth something it seemed to me. But it's fairly easy for an R & D type to look at it and say gee, it's just a bunch of dumb old relays. We are working in modern electronics ...or something. Although I repeat that the electronics in ENIAC wasn't all that spectacular. That ring of ten vacuum tube circuits that they used so much of was
crude by radar type electronics standards.

MERTZ:

Was there any particular reason why the Moore School would think of IBM in that connection. I gather this committee on this crash program...this wasn't general knowledge.

GROSCH:

The internal activities in IBM were a deep secret, yes. Very, very few people knew they were doing any of this.

MERTZ:

So conceivably the Moore School would not have known of any interest or assume that there really was any interest.

GROSCH:

Not so. The people like John McPherson and Wallace Eckert were known to be senior people in technical computing work. And the military, after all this joined together at Aberdeen. It was Aberdeen money that was building the stuff. Leslie Simons certainly

knew everything that IBM was doing in this thing. But I think the point is that IBM was regarded as a commercial operation and was doing it for dirty old money and these guys were doing it to advance the cause of something or other.

Anyhow, I resented it and I resent it to this day. It was very poor business not to invite us.

MERTZ:

So when did the IBM people first get relatively....

GROSCH:

Right after they read it in the jolly old New York Times, I would say. The New York Times published a full page on this stuff.

MERTZ:

Yes. I was thinking now of the course notes.

GROSCH:

Oh, they weren't used at all in any of the stuff IBM worked up.

MERTZ:

When did these materials become available?

GROSCH:

I'm sure if Hilleth Thomas brought his to the Watson Lab, and I'm pretty sure he did or procured a set sooner or later, I doubt if anybody else in IBM ever looked at them. I'm sure that others brought sets to Poughkeepsie, for instance, a year or two later. But not long after this Von Neumann became a consultant to IBM, so any question of special knowledge that was in his magnificent dome became moot. And I'm sure that one reason he did so become was that Wallace Eckert saw very clearly .having this strong academic connection around the Columbia Princeton kind of circuit, I'm sure Wallace Eckert saw very early how important Von Neumann's quality of thinking was to the development of computers.

You see, the thing is that Mauchly and Eckert were about to start their own company, and they intended to start it just as soon as they possibly could. So I would attribute at least a little to that. Without as I say...

Now the guy that could tell you this, the person that you really ought to ask about this is

Urban Travis. Travis was an important factor in this. J. G. Brainerd is another one. Travis and Brainerd were both important people at the Moore School. Travis is now I think with Burroughs. Brainerd is in process I think of retiring from the Moore School right now, having been the Dean. And they must have been first hand participants in the whole thing.

MERTZ:

I think they both, one I'm sure of, if not both important in that work.

GROSCH:

But I can say from first hand knowledge, not second hand, that none of that kind of information got into the SSEC project at all. There literally wasn't time. They had to build with what was on the shelf.

MERTZ:

So it was a matter of putting together preexisting...

GROSCH:

Yes, it was done very much faster remember than the MARK I. The MARK I, partly because of Wartime delays and partly because of certain reluctance on Watson to spend all that money in one great lump took probably four years from Aiken's original commitment to the delivery of the machine. But the SSEC, one year. Fantastic.

MERTZ:

Would that machine have been a pretty costly item?

GROSCH:

Oh, well over a million dollars. And I'm not at all sure what the accounting procedures were on it. In other words, whether, for instance as an example, most of those people who were working on it, I quite seriously say working 18 hours a day on it, I'm sure for instance were not paid overtime. They were professional employees. People who were doing wireman's work and stuff were, but the engineers and so on I'm sure weren't paid overtime. I'm sure that in the long run their careers at IBM and their additional emoluments much more than made up for it. If you sort of back fed that into the books you'd probably get a couple of million bucks.

MERTZ:

What would you say was the primary motive of Watson Senior?

GROSCH:

To get out in front of the parade again.

MERTZ:

After the Moore School...

GROSCH:

He always regarded, in spite of the fact that Aiken didn't agree with him, and that most people had already begun to call the MARK I the Harvard MARK I rather than the IBMASC. He always believed, I think, that that machine established IBM as the world leader in scientific computers. And indeed it did so establish it, but not necessarily in the eyes of the man in the street or even in the eyes of most IBM customers. They read the Harvard press releases and thought it was a Harvard machine. And with all Watson's well known euphoria, at least some of his executives knew this, too. Well, along comes the ENIAC, and there is no question whatsoever that the ENIAC is the world's greatest computer. It's a thousand times as fast as the MARK I. And just as big, and it's modern, it's electronic. And IBM didn't have anything to do with it. Well, no, even that isn't true. Did you know that the input devices to that machine were IBM card punches, which they gave them free of charge and didn't get any credit for it?

And incidentally, it's an interesting artifact that having done this and IBM claimed that they gave it to them for nothing. I don't know if they really did. They claimed they had. IBM was always claiming to give things away which in fact they had actually gotten some jolly old cash for some place. But it's an interesting fact that when part of the machine was IBM and that there weren't people invited to these various shindigs that we're talking about.

Just suppliers. They probably didn't invite the guys who supplied the vacuum tubes either. The point is that they could have gotten the vacuum tubes from several places, but there was only one place that made those card punches. And later experience has shown this, that it's harder to build the card punches than it is to build the vacuum tubes. I don't mean the tubes themselves but the circuits. Anybody nowadays can make central processors, but try and make a good high speed printer. It isn't easy.

MERTZ:

Reliable vacuum tubes are not easy either.

GROSCH:

Ah, but they didn't have any reliable vacuum tubes in ENIAC. Nor in the SSEC, I might

add. This was one of the things poor Hamilton had to do was to chase around and figure out where to get a large supply of vacuum tubes. Now this was still, remember, just the end of the War. And he finally found I think it was something like 12,000 surplus 25L 6s, and most of that machine is built out of 25L6s, in spite of the fact that they turned out not to be very well adapted to the purpose. That was all he could get a large number of, you know, tomorrow. So that's what he used. That machine contained 12,000 tubes as against 18,000 in the ENIAC, and the mean time to failure was four minutes. Every four minutes it broke down and you had to do something about it.

MERTZ:

It wasn't modularized for trouble shooting?

GROSCH:

Not any more than they could do without slowing up the design. The chassis turned out to be about a foot to a foot and a half square^ and they contained you know several dozen large bottles sticking out at rather peculiar angles. And there were only of course a few types of such chassis, and they were aggregated together with control circuits and such.

MERTZ:

Did they do any kind of preventive maintenance?

GROSCH:

It really wasn't necessary. You were doing specific maintenance a lot, and for preventive you know they went around and dusted out the card punches and things like that. The machine had many interesting things, a lot of which did have to be developed. For instance, since magnetic tape and magnetic wire was not yet available and ordinary paper tape Bell Labs five channel teletype paper tape, was not particularly attractive they built a very peculiar paper tape unit which was a 78 channel paper tape. What it was a roll of IBM card stock slit so that the length of the card, the 8-inch dimension was the width of the roll. And so you might look on it as a 80-column card with an infinite number of rows on it, or with many hundreds of thousands of them...

You ran a great drill of this which was so heavy that it had to be hauled up on the machine with a chain fall. You ran a great roll of this through a punching station and a series of reading stations which were of course taken right out of IBM reproducers and card punchers and stuffy and you punched 78 columns of digital information in columns 2 to 79, and columns 1 and 80 you punched a round hole sprocket hole to feed the thing with. And great pieces of this tape were the equivalent of little pieces of magnetic tape that they have today.

Now of course you couldn't erase it. You couldn't rerecord it. But having punched it you

could read it later at these stations that were further down the machine, and the loops of tape between these machines could be of variable length. Each brush station had its own feed, so you could be feeding fast at one station and standing still at another one and the loop between them would lengthen.

There were three such machines in the back of this big unit, and behind them furnishing the primary storage of the machine there were 15 foot high banks of Lake relays, tens of thousands of these multi position relays. The arithmetic units were on one wall, and as I say included 12,000 bottles. And free standing with cables as in later days connecting units under a false floor were especially built consoles, and especially cased in card punches and card readers and printers and things of this sort. I still have the dedication material and the brochures and the operating manuals of this thing. Again, the operating manuals were never printed, they were only blueprinted. Many plug boards, lots of stuff in it.

MERTZ:

So once it was unveiled for the world to behold. What were some of the initial problems that were run on it?

GROSCH:

The biggest and most famous problem that was run on it and the one that was... Oh, I should say, by the way, talking about hardware isn't enough; we should also talk about software. On the day that this thing was unveiled it was doing a big useful problem. Specifically it was doing Wallace Eckert's evaluation of Brown's lunar theory tables, the same job that had done in 1938. But this time as a continuous sequence in which you put a date in the beginning and got out a position of the moon at the end, so you evaluated a single position of the moon in one fell swoop. It was doing this job in a relatively well debugged program the day the machine was dedicated.

MERTZ:

How fast was it?

GROSCH:

Multiplying times as I remember it was somewhat inferior to the ENIAC. I think in spite of all their instructions to outdo ENIAC and the MARK I in every respect I think the actual multiplying time was 6 or 8 milliseconds as against I think it was 3 or 4 for the ENIAC. But it was so much more flexible from the very beginning you could not only have alternate modes of operation and change from one problem to another very quickly^ but as the name Selective Sequence Electronic Calculator implies, you had the branching instruction.

Now you remember I said the Bell machines had that already, but neither the pluggable Sequence Calculator on a large scale basis nor the MARK I had it. Nor did ENIAC have it in the early days.

MERTZ:

And this was programmed by?

GROSCH:

Programmed by plain old human beings who ended up by punching up program cards, one card per instruction as I remember it, and feeding that into a card punch connected to the machine for program control. As I remember it, and I'm not absolutely sure about this anymore, but I still have all the information but not in my head as I remember it there were two card punches, one of which could be used to read program cards, and that was installed in the console or something, and then there was another one as an output for the machine which produced new cards. However in general those new cards were not program cards. They were normally output data.

In this machine as in the earlier machines, including ENIAC at that time, and including I believe the Bell machine also, data and instructions were still kept separate. You didn't have the stored program or the modified program concept yet. It was in people's minds but it hadn't been embodied in a big machine yet.

MERTZ:

It certainly was in people's minds by 1947

GROSCH:

Well, you know many of these things, for instance Sam Alexander was here at the Bureau of Standards by '47. And people were diligently engaged in lying out and building all sorts of stuff. But it wasn't working yet. This thing actually was specified, laid out, designed in detail, built, checked out, torn down, and reinstalled, and I repeat a problem programmed that checked out on it, all in not much over a year.

It seems to me that the dedication date was early June of '47.

MERTZ:

This was a decimal machine...

GROSCH:

This was a decimal machine, yes. I don't remember off hand whether the Dickenson

patents were decimally oriented or not, but I suspect they were since they had been designed for 1930 level machines. But certainly that machine was decimal. And it was not decimal and alphabetic in our modern sense of the word. It was just plain old ten decimal digits and sane signals.

The great thing was it was beautifully designed while these guys up in Endicott were slaving away building the physical machine, other parts of the IBM team were designing with the aid of outside people beautiful casings for it, plate glass fronts, and modifying a very fancy building for it.

I told the story at the 20th anniversary of the ACM here at the Bureau of Standards a couple of years ago about how Watson inspected the machine a day or two before the famous, the press conferences and the opening ceremonies to which by the way all of the people who had not invited us to their openings were invited to, including Johnny von Neumann and people of this sort. And I think Einstein was invited but didn't come. And I had something to do with the visitors' list, and I believe I still have the initial material in my files somewhere from which we finally derived the invitation list.

Well, this was a beautiful thing with big sliding panels, everything beautifully done, an inscription from Mr. Watson engraved on a plaque, you know, expand the boundaries of the universe and all that, and when he toured this to make sure that everything was right for his distinguished visitors a couple of days later he was disturbed to find that in the room which had been taken over to install this machine there were two giant columns, maybe three giant columns. These had been carefully cased in beautiful cylindrical pillars, modern as was the feeling of the rest of the room, but they certainly were right there, running right down the middle of the room. So he turned to his vice presidents who were hovering around him like John MacPherson and Wallace Eckert and so forth, and said have those pillars taken down before the opening day.

Well, since these pillars held the building up and there were 20 stories up above that depended on them this didn't seem very practical, nor did it seem practical to do it in the next couple of days. But they did the next best thing. They recalled the brochure which was very elegant and beautiful but only in one color, as I remember it, in monotone. They recalled the brochure and had the columns retouched in the central spread picture of the installation. And I still have a copy of that brochure showing no columns and no trace that they had ever been there. But also I have the Saturday Evening Post story on the machine published a year or two later which has a plain old SEP photograph a beautiful color photograph and the columns are still there. And in fact if you go into that room today it's gone one through very many peregrinations but the building is still there and the columns are still there. Great story, that.

The power supplies and the air conditioning equipment, because by this time you needed air conditioning, unlike the Bell machine and the Aberdeen relay calculators you needed air conditioning for this kind of monster, just as you did the ENIAC. That was all placed in the basement. The building chosen was the one that's around the corner on 57th Street,

now a part of the IBM Galactic Headquarters. At that time IBM leased the rest of the building. The ground floor was occupied by the French Bootery which 13, was set shoe store now across the street from them. As one of the minor little things that they had to do, and remember this was in '46 and '47 when it wasn't easy, they had to buy out the French Bootery lease which presumably ran for the next 618 years. Get them out of there, find another place for them to move to, because they wouldn't just move until they had a better place; do all this remodeling and persuade the New York Electrical Union not to tinker with the thing. The Electrical Union of course insisted that it would have to rewire every chassis in the machine before it could be installed in Union headquarters. But by some devious means the IBM Corporation persuaded them not to do it, presumably involving the expenditure of vast sums of schmere. I also understood from John McPherson that it included showing them what they would have to do if they were asked to do it, and the Union membership itself decided they didn't really want to do that, fellows.

IBM at that time and today was one of the largest organizations in the world without any union. It has no union whatsoever in Endicott or Poughkeepsie or anywhere else. So there were certain additional tensions in such negotiations.

Anyhow it was all done. The damned machine worked on opening day as it had been ordered to do, and everybody concerned was tickled pink with the whole thing. The next major job that was put on it after Eckert's lunar theory, and in fact the lunar theory was a sort of a back up job that ran on and off for years and pears and years until we scrapped the machine, the next major job that was put on it was David Hill's work on the hydro dynamical analogy of nuclear fission, theoretical which is a rather well known/study on the division on the liquid drop, which is supposed to have something to do with the fission of the uranium nucleus. And Hill's work was supported by IBM. That is, he got free machine time to do this, and it was rather widely published thereafter. This of course partly came because of our connection with Los Alamos through the work that I'd been doing at the Watson Lab.

Then I was able to get a problem on the machine on optical design. I still retained my interest in optics that had led me to correspond with Eckert in 1945. I had had time on the punch card machines at the Watson Lab to in '46 and '47 to try to do some ray tracing in parallel on the 601s. And now to a much higher degree of accuracy and in a one ray all the way through the lens in one fell swoop kind of method, I was able to do the same thing for much more complex lenses and many more rays on the SSEC. And I have abstracts on the two talks I gave on this at the Optical Society but no finished papers. I was too busy to ever work them up into finished papers.

These were however two of the, the ray tracing on the punch card machines was the first substantial work done on automatic calculating in the optical field anywhere in the world, I believe. And the only exception to this was that James Baker, the man I had taken the course from in 1940 at Harvard had traced one skew ray through a lens system on Aiken's MARK I as part of the dedication ceremonies of the MARK I, after which Aiken threw

him off and never let him get back into place again. So he never designed any lenses with it or had any mass experience. But certainly that ray antedated anything that I did. So Baker was the first, but I was the next.

And the SSEC work which I never carried through to a synthetic method, I finally turned over to Max Hertzberger of Eastman Kodak when he began to do spit diagram calculations with later machines and tried to use it as a means of improving the geometric optical theories that he was connected with. I was never good enough to do that sort of thing, but I had this enormous mass of results that I had obtained from my experiments.

Now one of the things that are interesting about the SSEC was that that was really the first group to have a programmer team in that sense of the word. People who ran the MARK I including Grace Hopper and so forth, you sort of had to be next to the machine most of the time. I don't mean to imply you set the problem up by flipping switches or anything. You did go away and punch holes in paper tapes and things of this sort. But there was no clear distinction between an operator and a programmer, or even really between a machine designer and an operator and a programmer. They just sort of tinkered away until they got something running.

Certainly in ENIAC where you really sort of had to retire the machine every time you used it with these big trays of coax cables—ENIAC in the early days also didn't have a separate programmer group. Now later on when Von Neumann and Clippinger modified the concept of the ENIAC so that you could set numbers in on these dial switches and read those as program instructions, then you had people who wrote down lists of such settings in another room so to speak and in that sense of the word were real programmers.

Well I think that the gang that did this sort of thing for the SSEC probably came pretty close to being the first professional team of programmers. And it was recruited from young people, partly at the Watson Laboratory and partly hired for the job* many of whom are now in their older years still members of IBM. Bill McClelland who is an important part of the IBM marketing organization today was one of them. Betsy Stewart was another one. She came to them from the Watson Lab. They were organized together by a sort of a straw boss named Kenneth Clark who retired just the other day as a sort of administrative assistant of the Watson Scientific Computing Lab in its third location on 115th Street. Ken was an old IBM employee who came to me and Wallace Eckert at the Watson Lab for a while, and then went downtown to do the administrative organizing of this group down there.

The man who was in charge of the machine and who had a great deal to do with the final stages of its design and debugging was Rex Seeber. I said Robert a little while ago. If I remember it his name was Robert Rex Seeber Jr., but everybody called him Rex. I don't know how I said just Robert a minute ago. He was one of Aiken's team on the MARK I during the War and had come to us shortly after the end of the War. Eckert had chosen him to be the supervisor downtown for the SSEC when it got in place and so he spent a great deal of time working with Hamilton, during that year at Endicott.

Rex was a very quiet man. He is currently working on associative memory designs, mostly from a software side. He and Hamilton became great friends. Hamilton was a big impressive master of men type. Rex was quieter but had the numerical analysis and the knowledge of how to use the machines for technical purposes which was required; and they made a good team.

Many of the other senior people in the IBM engineering operations at the present time, Frizell was one, for instance. Several others worked in that group, the 18 hour day business.

So that was an enormously exciting time for the IBM engineering operations. An awful lot of work had to be subordinated to this crash program and this of course caused a certain amount of unrest among those whose projects were slighted. But in fact IBM was already big enough that they didn't have to shut down everything else. In '45 IBM's gross business was about 140 million dollars a year worldwide, which is one fiftieth of what it is today. It was increasing at the rate of over 20 percent a year. So by the time we get up to 1947 we are pushing 200 million a year. And to put out a million or two million depending on how you keep the books, in this one project was not... you know, it didn't cripple them. Although we are talking about 1 percent of their gross and probably 10 or 20 percent of their profits for that year. But IBM was always high profit organization and it didn't distribute dividends in those days at all, and now it distributes very small ones. So it's just a question of plowing a little less back into the business that year than they normally did.

MERTZ:

Did they get any income from this?

GROSCH:

To the best of my knowledge, no. I think that towards the end of its career commercial or semi commercial work was put on it, but that was after I left IBM I think. I think they didn't start doing that until about 1950 or '51.

MERTZ:

I thought there were some government problems that were run on it, but they were all run gratis or what?

GROSCH:

I think they were all run gratis, the three that I've mentioned including specifically David Hill's certainly was free. Now I remember at a later date one of my only connections with the General Electric Company was that we wanted to program a big job

for the turbine engineering group in Schenectady and I believe that that was run in the period that I was in Washington for IBM. And I'm pretty sure that if it was so run that they charged for it. And I think there were some other military or AEC kinds of jobs done too.

Bit it was toward the end of the machine's career. You see what happened was that the tide of development swept along so rapidly that by the time they had satisfied their commitments for free work to a fairly substantial number of exciting and important people, the machine wasn't really particularly competitive for commercial work. If you really cost it out, that is, on a realistic basis, if you considered the fact that they had to have a four or five man maintenance crew standing by day and night, and the machine ran two and three shifts, because there was an enormous demand for it in the early days, and if you considered this special team of programmers and if you considered replacing whatever it was, 25 of these bottles a day, and all the other raw materials and so forth, that were used, I would think that a realistic figure for its use would have been of the order of 500 to 1000 dollars an hour.

Well now, by '51 or '52 people were beginning to talk about getting deliveries on UNIVAC Is and IBM 701's in which for half that much money you were going to get ten times that speed. And a stored program machine with all the flexibility that that term includes.

So it never really was a very competitive commercial machine except in the very earliest days when it was the only game in town. And then they gave it away.

MERTZ:

Now who was on this group of the early group of programmers?

GROSCH:

Well, I mentioned Betsy Stewart and I mentioned Bill McClelland. There was Ruth Mayer, Harlan Mills, most of who are still working in IBM at this time. A handsome Southern gal named Edna Wammel, who was named for an insurance company, I believe. Two or three others. These were all of course very close personal friends of mine. Many of them were students in my class's full time or part time.

Other people who came into that group but passed through it very quickly without really becoming full time members of it later became prominent in the IBM applied science department, which most of them ended up in at a later date. I'm thinking of people like Elmer Kubie and Listen Tatum and others like that who came early to help Cuthbert Hurd start the Applied Science Department and who spread the IBM technical computing story all over the United States and ultimately all over the world. Many of them spent a few weeks or a few months in that machine.

MERTZ:

If we might backtrack just a little bit in terms of teaching, formal instruction in the use of both punch card equipment and later computing equipment as an academic thing that is formal training apart from company in-house training...

GROSCH:

Yes, one of the things that Wallace Eckert wanted to do from the beginning of starting the Watson Laboratory was to start an academic program at Columbia. And not only because he was a professor a type deeply committed to that kind of thinking because he had many friends on the faculty who would welcome his doing this instead of trying to keep him out the way that in later years the typical academics tried to keep out the burgeoning computer groups; but also because IBM itself had a deep involvement with education as a whole and Columbia University in particular. IBM had always had an enormous amount of in-house education for its own people and for customers. But this was trade school stuff.

What's more you really had to take some of it too.

I remember I was essentially ordered to take a course around 1948 or 49. And having looked through the catalogue of IBM trade school courses and having been prohibited from taking a course on circuitry which I later managed to force our way into, ended up taking social dancing. I learned to tango in the IBM cafeteria, at IBM orders. Free of charge, of course, And with a very expert instructor.

So there was that idea that you all had to study all the time. Everybody had to take courses. Customers had to take courses. Education was a good thing, and so forth. Deep part of the Watson philosophy.

MERTZ:

When did you get to the course in circuitry?

GROSCH:

'49 I believe. I only managed to do it for one year, although I still have the text book materials in my files. I took customer engineering circuitry for the tabulators, I think. And beautifully prepared stuff it was, which of course they used in Endicott to teach their customer engineers, so it was very elegantly prepared and very beautifully done.

Now on top of all this of course, one reason that the Watson Lab had gone to Columbia in the first place was that Watson Senior was a trustee of Columbia, and in fact in many respects the most important trustee of Columbia in those days. So with all this going for us it was clear that we were going to have courses at Columbia. And in fact they. Began

to teach them in 1946.

MERTZ:

Was that part of the original conception when they set up the Watson Lab?

GROSCH:

Yes. The original arrangement with the University which I believe Nicholas Miraculous countersigned, but which was really drawn up by Fackenthal and presumably Mr. Watson or a representative and Wallace Eckert, was that the Watson Laboratory should be built or remodeled is the appropriate word, should be established by IBM and that it then would be given by the corporation to Columbia University. Columbia would then maintain it in the sense of janitorial services and power and light and so forth. And IBM would be free to put equipment and
so forth in it which did not however belong to Columbia. To put staff in it and the senior staff people would be regarded as research associates or professors on the Columbia faculty without pay.

MERTZ:

They would be selected by IBM and be IBM employees?

GROSCH:

The question of selection was left rather loose. It was understood that Wallace Eckert wouldn't cross them up by putting any dunderheads in. And since he had been a full professor at Columbia before he left, why he had tenure and was trusted by the other faculty...

MERTZ:

But they would actually be paid for by IBM...

GROSCH:

All the salaries were paid by IBM. Columbia didn't put a dime in except for some janitorial services.

MERTZ:

[End Tape 2, Side 1]

[Start Tape 2, Side 2]

GROSCH:

... Eckert. I'm sure if an IBM engineer had been brought down to run the thing, even someone as smooth and ax well educated as John McPherson this would not have been possible at all. But Wallace had been a full fledged faculty member. He knew all the department heads as equals. He had been, I guess, Director of the rather small Astronomy Department for a few months before he went down to Washington. And his return was welcomed, believe me.

Then he in turn, when he brought other people aboard, made sure that the senior Columbia people looked at his prospectus before he actually hired them. I was an exception. He assumed because of my astronomical PhD and the fact that I, you know, I was his kind of guy, and also because of the urgency of the war thing, that I would be all right period. And there wasn't any question about it. But thereafter all the people that he hired he sort of passed in front of the people like Robby Schult, the head of the Astronomy Department, Paul Smith, the head of the Math Department, and so forth. We would have lunch together at the faculty club or something like that, and if anyone was ever blackballed;—I don't believe it ever happened, I certainly never heard of anyone, but if anyone were ever blackballed by that group I'm sure that Eckert would not have made them an offer,

MERTZ:

Now when Eckert made an offer to them did they have any faculty status as...

GROSCH:

It varied from person to person. When Eckert and I started, and we both in a sense came together when the Watson Lab began after in June or July or something like that, in terms of the signing of these agreements and so on which of course I was not then party to, we were called Research Associates. We were both research associates of the Astronomy Department since he was a former head of it and I was a professional astronomer.

When Rex Seeber joined us, and I believe he was the next employee, he was made a research associate I think of the Math Department. Somewhere around the first or second year Wallace's Research Associate was converted into a professorship, but without pay, so he had a faculty appointment, faculty status but did not receive any pay and therefore the question of tenure was moot. But I think he could have for instance attended the senate meetings if he had wanted to do so.

About the next person we hired were three senior people from the Radiation Lab whom I will return to in later parts of the story. They were given Watson Laboratory appointments but I believe at the very beginning did not have Columbia appointments at all and ultimately got research associate ships, two in electrical engineering and one in physics.

When Hilleth Thomas joined us shortly thereafter he had a very elegant PhD from Trinity College Cambridge in England, and publications and a genuine high level professional reputation. He was made a research associate in physics immediately, and in fact was assigned a couple of PhD candidates right away, one of whom was Robert Jastrow, who is now very important in the NASA Columbia University research business. And his appointment was converted to a faculty position somewhere around 1949 or 50.

Mine never was. I left in 1950 still a research associate in the Astronomy Department. I would like to have had the professorship but it was not...even in those days I was not all that keen on academia. It was not an important thing with me.

MERTZ:

When you say research associate that usually implies no teaching involved

GROSCH:

We did teach. And in fact one of the things that were specified from the beginning in this agreement was that the teaching would be with the consent of the departments concerned. So you had to shop around to find a department to sponsor you. You did not give your courses "in the Watson Laboratory" although we did put out our own little brochure in the Columbia University format, with a Columbia University cover on it and so forth. But the courses labeled in it were clearly labeled as being Astronomy 201 or Graduate Engineering 16, or what have you.

Eckert taught a course with the assistance of several of our junior professionals, taught a course essentially in machine operations. We'd call it a hardware course now, but it was a hardware-operations course not a hardware design course. He normally gave this once a year but sometimes it was given semesters. That was in astronomy because his appointment was in the Astronomy Department.

However, when I wanted to give my course which was in Numerical Analysis, which he was not particularly interested in doing—he wasn't really very hot in numerical analysis, his specialty had been celestial mechanics, and he knew only as much numerical analysis as wax necessary for that which wasn't much—so I gave the numerical analysis course and the Astronomy Department didn't feel that was a good place for it, and the Math Department was far too proud to let any grubby little astronomer teach his grubby old numerical analysis course in it—also they had Francis J. Norrity of analog computer world and Typhoon and Hurricane fame who wanted to give his own kind of numerical course at some later date when he got it organized. So I ended up in giving mine in graduate engineering, which was very flattering since it made it a graduate course but was improper because I taught it at roughly the junior level and it shouldn't have been at the graduate level. But those were so esoteric in those days that you could put it anywhere you wanted to.

MERTZ:

Was this a course in programming?

GROSCH:

No. Numerical analysis per se. Finite differences. Matrix arithmetic. Chevy approximation. . .

MERTZ:

Didn't they have such a course?

GROSCH:

Hardly any university in the country had such a course at that time. We look back at it now through a fog of 20 years of...

Not in numerical analysis, no. I don't mean to imply that there weren't math and statistics people who knew this sort of thing, but they'd picked it up on the side. For instance, down here in Washington there was a guy named T.N.E. Grebble who was very, very good at fancy difference work and so forth. He was in the Census or something. But you only had a course of that sort where there was a professional numerical analyst to teach it, and where he was sufficiently encouraged by his university and/or free in his research time to do it.

So that was one of the probably half dozen numerical analysis courses in the whole country at that time. And of course those were the first courses in what you might call computer operation anywhere, in the United States for formal university credit.

MERTZ:

When were these...

GROSCH:

1946 was the first year. Fall of '46. I still have the brochure. I used as much as anything, Whitaker and Robinson's *Calculus of Observations*, but I only required the students to buy it if they were really interested in the subject, because I lectured almost entirely. Wallace didn't have a textbook at all. His own Punch Card Methods book was out of date and it wasn't a textbook anyhow, so he worked almost entirely from notes. These notes were worked up for him and about half of the lectures were given by junior people, the head of which was Rebecca Jones who was his co-author later in a book called FASTER, FASTER which describes the NORC calculator. And she was a former astronomer from Harvard, I believe, Becky was. Stayed with him for many years and finally left after

much service to get married. And then Marjorie Severie Herrick and later supervisors in the shop also helped. And of course we used the punch card installations as the teaching laboratory.

The tradition at that time was that you lectured about one a week in such things and then the students all got downstairs and wired plug boards and ran cards through to see how it really worked. So the heavy job of shepherding them through the punch card lab and making sure they didn't spoil the work that was supposed to be going on at the same time devolved on the machine room supervisor who in the first year was Marjorie Herrick. Still Marjorie Severie I guess.

All during this time there was also this large installation almost exactly the same as my own except with a poorer tabulator and so forth over in the Puppine attic doing this B-29 fire control work. These were the machines that had expanded the Thomas J. Watson Astronomical Computing ______,

Well, somewhere around the second or third year after we were well ensconced in our new building it was decided to shut down the Astronomical Computing Bureau, for keeps, that its functions were now handled five times over by a small part of the Watson Lab when we shut them down we took over almost all of those machines and put them in another room in the new Watson Laboratory. So I then had two punch card installations almost identical.

And the same problem arose as before, that you really needed people to run the, to actually shovel the cards through the machines. It wasn't enough to just have an overall manager type like me. So we brought along with that second batch of machines Lillian Feinstein Housman, the gal who had been Eckert's first supervisor in the late '30s,' who had married and who had been running the machines all through the War.

But we did not get Everett Yeow. He got his PhD and went off, and as a matter of fact in the end he ended up in California working for Computer Research Corporation when it split off from Northrop. And then went in with the Hawthorne plant of National Cash Register, and for a while was sort of manager of scientific sales for National Cash. I've lost track of him since, but I think he's still in the business.

So not only ... the - machines came over, we got maintenance engineers because we had twice as many machines. Then when the Aberdeen relay calculators arrived we got rid of most of the extra 601s, cut back to about 6 of them. Then we got rid of a couple more of those when I got that 603 I was telling you about. I got the prototype of that. I didn't get one of the production numbers but I got the prototype.

And then finally when better mechanical machines began to come out of the IBM line, the 602 and the 602A, we replaced our 601s with those and got rid of our last 601s. So over a period of about three or four years we built up from 8 to 14 601s and then down again to zero. And at the time I left the Watson Lab we had the two Aberdeen machines,

the 603 was moribund we had a prototype 604 which had since been built, and we had I think a 602 and two 602As or something like that.

I won't bother to describe all these gadgets. They're electrical mechanical machines with the exception of the 604 which I will describe later, and they're all in the catalogues and so forth...

MERTZ:

What kinds of problems were being run now on these?

GROSCH:

Well, now let's back off a little bit and look at the problem of teaching this way. By the way, I should add to these courses that after about a year when we acquired L. H. Thomas, Dr. Hilleth Thomas, he began to give a second semester of, I think he called it something like calculations of mathematical physics or something, but it was really advanced numerical analysis, with heavy emphasis on the solution of ordinary and partial differential equations and error analysis. He was extremely good at this, better than I was, and we were both delighted to have hia do some of this. The first year he offered this was '48. But he was thinking about it in 1947. He came to us in '46 or '7 and he was thinking about it and worked up his notes and actually recruited some students even in the spring of '48 or the fall of '48 I'm not sure which.

But then after I left he took over, I think, he took over my more elementary course as well. Then he usually gave, once and a while, every other year he'd give a course in real mathematical physics, by which I mean not computationally oriented but the Watson's Bessel functions kind of thing. And I gave one semester in celestial mechanics which also was not offered in the astronomy department, but I offered it in Astronomy because that was an acceptable subject. And in that course I had for instance among others Walter Ramshaw who was trying to be a professional astronomer at that time but who later became one of the ten or twelve founders of the Share Organization that was my rival in charge of the heavy computer work at Pratt and Whitney in United Aircraft in West Hartford.

So as I say Harrison from Aberdeen came up and took a numerical analysis course and several other well known names in the business. So it was a nice place to be at. Then there was something else about this and that is that not only because of the machines I mentioned already, and not only because of its position as by far the largest data processing company in the world by a tremendous margin, but also because of Watson Senior's reputation for social activities, for you know, being the head of the International Chamber of Commerce and the Metropolitan Museum of Art etc. We drew an enormous number of visitors into the IBM outfit, most of whom got funneled if they were anywhere in New York City got funneled through Eckert's office.

And then Columbia University of course is one of the world centers of advanced science, especially at the end of the atom bomb sort of thing, drew an enormous number of visitors. And remember the physics and math and astronomy people and to a lesser extent the engineering people were proud of our presence there, so they funneled people over to us.

So over that five year period from '45 to '50 when I was there, just about every single person concerned in the digital computer field came through. The very few that didn't come through we visited somewhere else. An example would be for instance that we didn't see much of Mauchly-Eckert-Moore School kind of bunch, although we did see them on occasion. But we'd see them at scientific meetings and the initial meetings of the ACM and so forth.

The organization meeting of the ACM was held at Columbia, for instance, although not actually in the Watson Lab. And I no longer remember why it was. It just seemed as though New York City was a good place for it, I guess. Probably because Ed Berkeley was still working at Prudential at the time and Prudential didn't have a suitable meeting room. So we met at Columbia University in '47 and started the ACM of which I was one of the 60 or 70 charter members.

MERTZ:

Were there any particular times with ERB?

GROSCH:

No. Eckert had some knowledge of some of the people, I think perhaps Jim Lakeland and we all got copies of their famous book *Something Computing Techniques*, or something when it first came out. But I think that the ties there were rather remote and probably went back through the magic project and furnishing punch card units to ENIAC and things like that, rather than through the Watson Lab proper. I knew none of those people at that time, Bill Norris or any of those people. I got to know them later.

MERTZ:

How about magnetic drums?

GROSCH:

Drum development was a moot question at IBM. Frank Hamilton after he got through with the SSEC built a working drum which was ultimately-- dimensionally at least--was the one that went into the 650, but he built it by winding magnetic wire on a cylindrical surface, brazing it in place and grinding it off smooth. So he got a magnitizable surface without plating and without coating, both of which were pretty difficult in those days.

We looked with some interest at the efforts of Eckert Mauchly with the BINAC and of others to produce a really good drum, but by this time IBM had started pretty substantial development work in Poughkeepsie. And I think that...I simply was no longer able to have an eye poled into every laboratory and to know everything that was going on.

One of the things you should know about this is, and I think that history ought to record it is the way that the old man used to run his development organization. This was especially true when there was just this engineering laboratory in Endicott--the so-called Research Laboratory in Endicott. By the time he started them in many locations he himself was not able to keep track of everything. But during the 30s and the 40s the method that was done for control of research was very simple. If you wanted to do a development job you asked Mr. Watson's permission. And in many cases he suggested it to you because the planning organization would report to him and make suggestions to him, and most customer requests came in to him in one way or another.

One way or another a project would be started, and it would be assigned to a senior engineer like Lake or Hamilton or Halsey Dickenson or what have you. There were many other senior engineers besides them, perhaps as many as a dozen. Pete Moon later became a senior engineer, but more in the Poughkeepsie area than Endicott.

Each one of these senior engineers was characterized by having his complete operation. He had his own drafting room/ his own offices; his own experimental machine shop, his own test area. And he was required by company secrecy not to let anybody else in there.

So his men were not supposed to say what they were doing to the men from the adjacent area where something else was being developed. In fact, the very project being developed was supposed to be a secret. Now in fact because human people are human there was a pretty clear knowledge around Endicott of who was working on what, but only in general terms. Since no one in those days really ever left IBM to go to work for anybody else, not much of this was carried away to competitors. Nor was there any competition worth mentioning anyhow since they had 95 percent of the business.

The old man, for instance, might very well authorize three simultaneous printing developments. And I believe at one time had in adjacent laboratories one guy working on a better bar printer, that is one like the 405, 402, 403 tabulators where the printing bar went up and down and a slug in that bar was struck against the paper with a hammer; a wheel printer like the 407 and like many of the modern high speed printers where a rotating wheel spins continuously and the paper is struck against it at the proper time; a chain printer like the one in the 1401 and the 360 where the motion of the type unit is horizontal; a pin printer where little ends of wires come out and form a. . . from a matrix come out and form a character in the way that the Flow 26 E punch does it; and in later years maybe a xerographic or non impact printer. Each one of these would be under the direction of a senior inventor and his group. Each one would be costing a vast amount of money. There would be a great deal of waste motion in the sense that each guy would have a machine shop for instance so only a few gadgets in it would be busy at any given

time.

But on some fatal day when he wasn't running the Museum of Modern Art or the Metropolitan Museum of Art or something the old man would walk down the line and go in each door and look at what he saw and say let's carry that one through to commercial development. The others would close down and be available for other projects. And the chap who had painted his machine the most attractive color or who had the best shaped cover on it, or for all I know had the best engineering principles embodied in it, would then go into commercial development.
You can only do this in an autocracy. You might very well say how he could make good decisions. I don't know the answer to that. But in fact the prosperity of the company indicates that it probably didn't matter all that much...he probably could have carried any one of through to a reasonable commercial product. Nor were all the decisions good. A few machines were carried to commercial offering and then withdrawn. The 602 was a case in point. That was the successor calculator punch to the 601, electromechanical in nature. I got one of the first ones at the Watson Lab. We played a few games on it. Ours worked fine, but when they started making them for outsiders in large quantities they turned out not to be maintainable or something. I no longer remember what the problem was, but all of a sudden a slightly improved version called the 602A came out.

It was very clear to anyone using the machines including outside customers that the 602A was a completely different machine than the 602 in spite of that A suffix. It wasn't just made out of titanium instead of stainless steel. It was a completely different layout. The arithmetic flows in the machine were different. It was obviously the No.2 candidate for successor to the 601, and when the one that Mr. Watson crowned as the choice turned out not to be any good they went back to second choice and carried it through instead. I didn't know the names of the inventors concerned on those, but obviously the system was not perfect.

But the demand was so great, the substance of the company was so good, and the individual senior inventors were each so capable, that almost anything would indeed be profitable in the long run.

MERTZ:

Now to get back very briefly to course work, these courses were offered more or less under specific departments?

GROSCH:

Always under a specific department. Thomas1 in physics, mine in astronomy or graduate engineering, Eckert's in astronomy.

MERTZ:

Was your course offered regularly every year?

GROSCH:

Mine was offered the first semester of each year, yes. Eckert's was always offered first semester and sometimes was either duplicated or extended in the second semester.

MERTZ:

Were there course notes written up?

GROSCH:

Yes, for his. Not for mine. I think Eric _____ finally produced a set for his. But not for mine. I've been notorious all my life for not doing much of that sort of thing and I still don't do it.

MERTZ:

Do you have any lecture notes?

GROSCH:

I have a syllabus still left over and reading stuff and so forth and copies of my final exams.

MERTZ:

And you continued that throughout the time you were at ...

GROSCH:

Yes. I gave it five years I remember, '46, '47, '48, '49 and the fall of '50 before I went down to Washington. I think I had to curtail it and go down before final exam time that fifth year.

MERTZ:

No one at that point specialized as such in this field... if you could call it a field.

GROSCH:

You see, we weren't calling it computer science and that's for several reasons. First of all, I don't think there is such a thing as computer science, or if there is it's a fairly trivial Quantity of knowledge. And the second thing is that there wasn't any point in doing this

because in a manner of speaking there was no demand for it at the far end. What you needed were tools with which you could use computers in physics, in aerodynamics, in astronomy, or ultimately of course in business administration.

But Eckert and I regarded the computer as a tool although it was a tool that we proposed to devote our careers to. But in the same sense that I don't believe Columbia would have considered in those days having a PhD in Spectroscopy, for instance. Spectroscopy is something that you use to make better steel with or something of that sort. That doesn't mean that if you invent a new spectroscope or become the world's greatest expert in some particular new form of spectroscopy you couldn't make a PhD thesis out if it or a career out of it, but you wouldn't have a department of spectroscopy...

MERTZ:

How about graduate students? You mentioned that he was allowed to have...

GROSCH:

Neither Eckert nor I ever had really full fledged graduate students in the sense of having them come over and work only for us in the way that I had worked only for Alan Maxwell, and he had worked only for E. W. Brown. There wasn't that much of a curriculum and so forth.

Thomas did have a couple, but his were really in mathematical physics in which he was a world authority. And he worked up curricula for them requiring many other people's courses besides his own.

I was on two doctoral committees, one for Jim Mulligan who is now the secretary of the National Academy of Engineering here in town and is going to be the head of the IEEE next year. And one other, I can't remember who it was. But these were men who were taking their degrees under other people.

I was just a member of the committees. I read the thesis and patted them on the back and that was it. Mulligan didn't take a course from me, for instance.

I might very well have been head of Walt Ramshaw's committee if he had stayed on in astronomy. Ramshaw was a curious person. He had trained as an undergraduate as a naval architect, but gotten tired of working for a naval architecture production company during the war, and had decided to take his savings and become a pure scientist h so he chose astronomy. He liked my celestial mechanics course and in fact probably would have become sort of as I had an orbit computer celestial mechanics computer type guy except that his money ran out and pressures to come and make a fortune as a full time computer man began to mount. So with my very genuine encouragement he went up to Pratt and Whitney and ended up in a very fine career there as a professional computer operator and manager.

Since then however he's found the commercial strains of this a little much and is now I think working in the Veterans' Administration or something like that in a semi- do gooder kind of operation. But he was one of the dozen guys who started the Share Organization and a very valued man in the middle range of the computer business.

MERTZ:

In this time were there any academic problems run in the facility for departments...

GROSCH:

I think that one of the interesting things about Columbia was that in general the departments with which we had the most frequent social intercourse and with whom we were most wired in at faculty club and so forth, the tables at which we ate lunch at the faculty club and so forth had absolutely nothing to do with us on a business basis. There was no physics, no chemistry, absolutely no mathematics, not a smell of mathematics work ever done in the Watson Lab during my five years there.

Frank Murray, Francis J. Murray used to come over and talk to us on occasion, but it was really his consulting work for Cyclone or whatever it was that was involved, rather than his regular mathematical stuff.

On the other hand, there were other departments of the university that did make quite a bit of use of our equipment, either in the sense of sending over somebody who we'd teach either officially or unofficially to use the stuff, or in occasionally sending over a problem which we'd run for them as our personal staff permitted.

One of the most interesting ones was what is now oceanography, a chap named Morris Ewing, an internationally known figure, the head of the Columbia Oceanographic and Seismology up on the Hudson River now. I forget what they call it, the Hudson Laboratories or something like that. And an internationally known figure. He's the discoverer of the Atlantic Ridge and a bunch of things like that. Well, I remember doing work for him with my own hands for a while, at night after the major pressure of the Teller and Marshak was off, on the propagation of sound in what was called the sound channel in the ocean. This was a warning device in which you fired off dynamite, for instance, and because of temperature layers in the ocean it turned out that the sound waves would remain within a certain stratum of water. The point being that the temperature distribution in most of the oceans made--the sound refraction made--the refraction of the sound waves such that they would go up until they got close enough to the surface and then would curve downward. And when they got far enough down they would hit a different layer and curve up again. So the sound didn't dissipate in three dimensions but only spread out in two dimensions. And it was possible it turned out with hydraphones to detect an explosion hundreds and hundreds of miles away, if you fired it at the right depth. And this was, of course, thought of during the War as a special device

and carried along as a warning system afterwards. Well, the calculations of that sound channel were done on the old 601s and 602s with my own fingers for Ewing. Later on he sent over a full time graduate student who had not taken our courses, but whom we trained to do just the routine work of running the machines and they had done their own numerical analysis. He'd carry on into the small hours of the morning with these calculations.

I remember some earthquake work that we did for structural engineering people on the question of fundamental frequencies of skyscraper frames in California. I remember a chap named Gordon who came down Cornell through astronomical connections and did some calculations in radio astronomy for us.

There was always a substantial supply of astronomical people coming through from Wallace Eckert's connections and from my somewhat lesser prominent connections. The major job done on the Aberdeen relay machines, for instance, was for a chap named Louis Green of Haverford College. And as I understand those calculations are going on today although not at IBM. They are going on at NYU at ______ Labs or something. Atomic physics essentially but of an astrophysical nature.

Incidentally, along about this time after Mrs. Herrick went to the University of Wisconsin, after IBM wouldn't employ married women anymore, and she went off to Wisconsin; and after Lillian Hausman decided that she ought to have a baby or something, we had a third supervisor named Eleanor Krawitz, a very, very beautiful girl and extremely capable who was our third machine supervisor. And she did a great deal of the Aberdeen relay calculator work herself, wired the plug boards and ran a lot of the work herself because it was so intricate. Finally trained Eric Hankton to do it. He was subordinate to her during those years. And Ellie did a great deal of this work for Louie Green on these Aberdeen machines. These intricate calculations.

Now for instance at that time one of the astronomers at Columbia was Martin Swartzhill who is now one of the great figures in international astronomy and the senior man in this balloon program, the balloon astronomy thing running out of Princeton. Well, Martin and his wife used to come over at night and run astronomical calculations largely stellar constitution sort of stuff, through the machines. And & I remember, another one of my anecdotal things that I like to talk about, I remember the first time they ran a substantial bunch of cards through the Aberdeen machine, they prepared their cards very carefully according to Eleanor's instructions, but they hadn't actually run any with their own hands, and they had a machine jam and the cards started to pile up inside the machine. They didn't know that they were supposed to come out as fast as they went in. And ultimately the covers burst and great gobs of torn up cards started pouring out of the machine. And when I scolded Martin for this the next day he said, "I thought it was storing them." It took us the best part of the day to untangle all these spindled cards and restore his data.

So there was a lot of that. We worked really around the clock. My wife and I, for instance, used to often come in late. When I didn't have classes or anything she'd come

up and help me and we'd work until 3, 4, 5 o'clock in the morning.

MERTZ:

Was she still...

GROSCH:

By this triune she'd quit Indo chemical and we'd moved into town and were living in New York City so as to be more easily...

MERTZ:

Morningside Heights?

GROSCH:

No, I never wanted to be up there. We lived in Chelsea at that time on West 22nd Street. We could come up on the subway, a direct route. It wasn't as dangerous to wander around on the streets late at night as it was later. And we didn't need a car. We sold our car. Just went afoot. With her scientific training and her interest in astronomy she carried on some of my Jupiter 8 calculations for me for a while. But by hand, not on any of the punch card machines, because she didn't know how to run them at that time. Then when I taught her a little bit about how to run the elementary machines she did some stuff for me on matrix inversion. And I remember I tried to do some predictive analysis tried to predict the results of horse races by factor analysis. We were talking about Harmon and Thurstone, I tried to apply some of those techniques, and this involved a lot of matrix arithmetic. It turned out you couldn't make any money with the horse races, but at least it was a good practice....

MERTZ:

Did the Watson Lab at this time get involved in any meteorological .

GROSCH:

No. We were aware of meteorological interest but it centered mostly downtown at the SSEC. Now the SSEC was not truly a part of the Watson Lab. Seeber had an appointment at the Watson Lab and was called a research associate in mathematics at the University and so forth, but he practically never came up to the place after the machine got underway. He didn't teach any courses. I think he taught Eckert's course for him about half the time the first year before the machine really got underway, '46 or '47, but after that he didn't. And that was sort of a hollow relationship.

The result was that ultimately the SSEC was regarded as part of the downtown World

Headquarters Organization. And finally it was switched over to the Applied Science Department when it began in 1950 or something like that.

MERTZ:

And they did run some meteorological…

GROSCH:

They talked about it. I don't believe they ever actually did it. I'm not sure whether they did it or not, but they certainly didn't do much until 1950 at least.

MERTZ:

You mentioned some problems that were run in the Watson Lab. What other departments were represented?

GROSCH:

Oh, very many came over. We did some work for chemistry, George Kimball, I remember, had a girl graduate student who worked with us for two years.$ I never really knew much about what she did, but at least it was quantum chemistry of some sort, rather involved calculations.

MERTZ:

How about the engineering? Were there any civil engineering or...

GROSCH:

The earthquake thing was civil engineering, but the two or three most important people at the University were not interested in digital calculation at that time. These were, oh, a chap with an Italian name...and another chap named Ray Mindlin, and both of them were very distinguished, in engineering/ one in civil and one in mechanical as I remember it. I don't remember which is which. And both of them taught courses in engineering calculations and so for, but with emphasis on analog computers, transient responses and things of this sort, rather than my old fashioned tabulators.

MERTZ:

How about the school of business?

GROSCH:

Almost no connection. It was kind of a doddery operation at that time anyhow.

Remember this was the time of the interregnum, the Nicholas Murray Butler interregnum. I'll tell you though, one outfit that does come in, and this is how I knew about Thurstone and Harmon; Hotelling had split off from mathematics and become mathematical statistics. We didn't get anything much out of him, although we knew him socially. Abraham Wald was there and we knew him socially but he didn't give us any business.

But at the other end of that sort of stuff, Irving Lorge, and some other people like that had sort of picked up the Teachers' College kind of statistics,' the test and ... educational statistics... and that's where I came across factor analysis and some of our matrix work. And we did do some work with them. I can't remember very much about it. It was very early in the game while we were still in Pupine and the next year after that. But we did do quite a bit of work with them.

MERTZ:

How about psychology and testing...

GROSCH:

We knew people who did this through Lorge. There was a chap who was at the 1948 forum. Tucker I believe his name was, from the Educational Testing Service. But the main thing that we did for them was that for about a year we had a mark sensing machine, a test scoring machine more properly called, I believe, that mark sensed large sheets of paper on which Educational Testing Service exams were given in those days, and actually fed these big sheets in. It wasn't a punch card. Actually fed these big sheets in, and in the average machine it showed the results of the test as a meter reading. I never quite understood whether that was an analog meter or what, but it actually...a meter went over and stood on 62, and a human being wrote 62 on the sheet of paper as it came out. And what we had was a prototype machine in which this was actually punched on a card or something instead, digitized down inside and punched on a card. And I can't really remember what happened to that. I can tell you where in the building it stood, but I really don't know what happened to it in the end. It was gone before I left. It just sort of disappeared one day without anyone saying anything.

And that tied us to ETS and some of the other stuff. IBM built that equipment for the Educational Testing Service, but it was always a special equipment basis.
Now one of the things you should remember about all this is that while this was going on at the Watson Lab there were many other activities going on at IBM and even at the Watson Lab that were not under the direct Wallace Eckert-Herb Grosch kind of thing. Or at least wasn't under mine. Wallace Eckert was head of the whole thing, and there were other activities that he was involved in that didn't affect me much.

Now specifically, he was always deeply involved in the SSEC downtown, although it was not formally in his responsibility. I mentioned that among the people that we brought into the Watson Lab in '46 - '47 eras there were three boys from MIT, and they had set up

electronics laboratories in the Watson Lab and were actually building equipment, building hardware with their own hands. And he was over them, but they were not under me in any way or associated with me in anyway. This culminated in the NORC.

Now let me go back a little bit and talk about the physical circumstances of the Watson Lab. The Watson Lab during the time I was there was a little narrow fraternity house, 612 W. 116th Street, across the street from the end of Claremone Avenue where it comes out of Barnard and dead ends on 116th. On both sides there were huge multistory old fashioned apartment houses, and this little stone fraternity house was wedged in between. I don't remember what fraternity it was anymore, but they shut it down during the War for lack of men to belong to it. IBM bought it, remodeled it very extensively including tearing out most of the insides and putting in steel beams under the floors in order to support the weight of equipment; and then turned it over to Columbia as Columbia University property, which it is today, some kind of a language institute or something today.

We moved our equipment over there from Pupine sometime during 1946; I don't remember the exact date anymore. It would be pretty hard to tie down although Eckert might remember it. We had a double basement. In the very basement, basement, basement, way down in the bottom we had a huge vault which they had apparently kept the fraternity gold in--the crown jewels or what ever it is fraternities have--a great big huge walk-in vault. And I gradually filled that with the output cards from this dumb shock wave calculation, 300,000 of them in the end.

Next to it was a space where there had been a big coal furnace. One of the things we did was to tear that out and put in an oil burner, which was not a minor point since this was done at a time when there were no priorities on all this sort of equipment. Then in front where the coal bin had been we put in a small experimental machine shop to be used by these three men from MIT. But I also had a key to it and had a lot of fun down there making things and modifying gadgets of my own. Nothing to do with the Watson Lab business at all. Wallace Eckert believed that people ought to be able to have books and machine shops and things like that to tinker around in, and I certainly agree with him 100 percent. If you have a creative group of people it helps to do that sort of thing.

The floor above us had a machine room in back looking out onto a court. And it was in that room for instance that we had the 603, this big radiator full of 300 vacuum tubes I talked about. The 602s and the 602As when they replaced the 601s. And it was into that room that we put the machines from Lillian Housman's astronomical Computing Bureau when they arrived.

Also in the front of that floor we had a room that was devoted first to storage and later to the construction of Eckert's automatic measuring engine which Bennet and he were building and which was to be a great big huge old fashioned measuring engine loaned to us by Yale Observatory—remember his connections with Yaler onto which we were putting photoelectric equipment to find the position of a star and punch card machines to

guide the photo cell to the approximately correct area from a previously punched star catalog.

So that was going on on that floor. On the main floor, the living room of the fraternity house had been converted into a beautiful reception lobby in which the scene between Watson Senior and me on the dedication of the building was played. And behind that another large machine room full of punch card equipment.

Above the living room on the second floor was a beautiful library which I pretty well furnished and for which I picked the books, with matching celestial and terrestrial globes at several hundred dollars apiece and multi thousand dollar walnut conference table that we found in the IBM art collection. And oil paintings of distinguished scientists and inventors on the walls, and so forth.

And on the second floor behind that we had an electronics lab for these men originally, later converted into a classroom when they moved out to build the NORC elsewhere.

On the third floor in front was an office for Eckert and behind were two offices for me and ultimately Leon Briolon whet the well known theoretical physicist who lived with us for a while. On the fourth floor in front was Hilleth Thomas, and behind were the offices of the three electronics boys that I mentioned. Seeber by this time was downtown at Galactic Headquarters with his machine.

On the fifth floor they ultimately built the beginnings of an electronics lab out of which they I were going to build NORC. There was no air conditioning so there were some problems in summer weather. But it was a very attractive place. It was all done W. and J, Sloane furniture. Everybody who wanted it had built in bookcases. Most of us had fireplaces which however no longer worked. There were pictures of Mr. Watson add think signs and so forth everywhere, but they weren't forced on you.
And I was allowed, since Eckert wasn't interested in such things, to interact with the IBM fine arts department and secure a great many beautiful engravings and oil paintings and pieces of ceramics and sculpture and so forth to decorate the joint with. So it was a very beautiful and attractive place.

MERTZ:

I take it the think signs were the standard issue?

GROSCH:

The think signs were standard issue, although as our people began to travel they began to bring back reflections and so forth from other parts of the world. But when you came to IBM to open an office like a new sales office somewhere
there was a standard package which was shipped to you and you got one executive package and 18 salesmen packages or what have you automatically if you had one

manager and 18 salesmen. And the salesman package consisted of a small picture of Mr. Watson, and a small picture of Charlie Kirk, and a small picture of somebody else, you know. If it was Washington it would be Louis LaMotte or something. And a think sign. And he had to have those up in his office at an appropriate place. And if he was in a big bullpen it was a little confusing because you might have eight or ten sets of these things around the walls of the bullpen.

An executive one of course was a great deal fancier, frames and stuff. We had a beautiful gold framed portrait of Mr. Watson over the fireplace downstairs, and that was the only one that I had encouraged around the place, although a few of the engineers stole a think sign or two out of the closet where I kept them and put them up because they were cute or patriotic or something.

MERTZ:

We are just about to run out of tape. This building was to remain. . .

GROSCH:

This building was the main part of the Watson Lab for many years. By Havens, one of the three men I described moved around the corner onto Broadway onto upper story space to build the NORC. And then that was closed up entirely. And about the time that that closed up a much larger building at 612 W. 115th Street was purchased. And this I believe was purchased by IBM. I don't believe that is part of Columbia. That's the Watson Laboratory today; the part that belongs to Columbia has reverted to a college? And the Watson Lab today I think is less integrated with the university. That was after I left, so I would say that the bigger building was probably '52...

MERTZ:

And that was the one at which Von Neumann was to speak. . .

GROSCH:

I think that's right. I think that's right. Von Neumann was often the guest of honor at IBM shindigs. For instance, he was I think a guest of honor at the dedication of the NORC, which was at the...

[Tape 5, Side 1]

I'm ready to back off and repeat again the business with Eckert and Columbia and so forth. I think that he was indeed producing some publications, not only his book but a couple of things in the Astronomical Journal and so forth. But they weren't being distributed very quickly. There wasn't a, community that was particularly interested in

extracting this material early. And the few people, the Bowers and the Herbert's that were interested, of course, were in daily touch with him about his work. So it was only beginning to get out into the world that this Thomas J. Watson Astronomical Computing Bureau even existed when pressures began to mount. These pressures came in a different direction, and of course it would be a good idea to hear this from Wallace Eckert, himself, who's an important figure on the scene, but my guess is that it probably came from the Naval Observatory. To be more precise, it came from the Armed Forces to the Naval Observatory to IBM, and that they, in turn, drew Eckert in. I am sure I knew at one time, but if I did know I have forgotten. The problem was as follows:

The Naval Observatory, you will remember, had this Nautical Almanac Office. And, you'll remember, they had many people doing desk calculator and mathematical table type calculations, including the motion of the sun, the motion of the moon, and so forth. This generated, each year, a thing called the American Astronomical Ephemeredes which is a large, blue-bound book, about an inch and a quarter thick very technical, very abstruse, very accurate, which was used by all American Astronomers and many foreign astronomers for fundamental astronomical work.

Now, from this was deduced a less accurate book, The Nautical Almanac, Now this was a paperbound book, much smaller, which was used by sailors for navigation. And every war ship, every private ocean liner or freighter has copies of this or the British or French or German equivalent. This is less precise, much less or many fewer kinds of data are given. They don't give positions of satellites of Jupiter or anything like that but only the things that are used for practical navigation with the sextant, or of course now days with radio aids, but in those days almost always the extant. It does not include hydrographic charts; it does not include radio direction finding or anything like that. But it includes the things that you need to make sextant observations and to do celestial navigation of boats.

Now, it was obvious before we entered World War II that something different was going to be needed for aerial navigation. That in order to navigate across the ocean, or to navigate above clouds for bombing and other military missions, something a great deal handier to use--still less accurate than the nautical navigation--was going to be needed. It was going to have to be tabulated at much more frequent intervals--like every hour of the day, or even every ten minutes of the day--instead of once a day or something like that.

When they did aerial navigation up to that time, it was such a specialized thing. People trained for years before flying a Pan American Clipper or before making one of these one-of-a-kind flights, that you could expect them to either prepare a great deal of material in advance, or you could expect them to deal very efficiently with the mariner's kind of navigational aids. But when you're training thousands and thousands of young men, many of them not familiar with this type of thing at all, training them very quickly, you have to have very specialized and simple things for them to learn on. An Air Almanac, so called, was one of the requirements that were decided on.

Well, they said, the Naval Observatory will have to furnish this. But the Naval

Observatory said Aeronautical Almanac Office must continue to do the fundamental work because everything else derives from it. You have to first do the ephemeris before you can cut it down to get an almanac. And then you have to continue to do the Nautical Almanac because we have plenty of boats waiting for it anyhow. It isn't as if everybody is going to get into the air. And that uses up all the people we've got. Now sure we can reach into the Astrographic Division and into the Solar Physics people, and so forth, and borrow a body here or there, but it's going to be very difficult to do this. We should consider doing this mechanically.

So, having been told by the Navy Department that they had to do it, they then went to my assumption is they then went to the IBM Corporation and said, "Can’t you guys do something to help us"? Certainly many of the people there knew about Eckert's experiments, so it's possible they went directly to Eckert, but I believe they went to IBM. IBM, in turn, advised them, "Yes you could," but drew Eckert into the thing. And the result of it all was that Eckert was made Director of the Nautical Almanac Office, in late 1940, with the primary task of putting this Air Almanac into existence as quickly as possible using mechanical methods.

Now we begin to encounter some of the figures in computing who are still active. For instance, when I arrived at the Naval Observatory in May of 1941, was assigned a room in the middle Astrographic building. Now this contained the two offices of people who are doing observational work with the Ritchey-Cretian Telescope and the 26-inch refractor. But additionally, across the hall, it had the office of the Small Postcard Installation.

Now the supervisor of this Installation was Jack Belzer, who is now a well-known professor of computer science at the University of Pittsburgh. And Belzer was, I suppose, in a sense running the first rent-paying, scientific computer facility in the whole world.

Now, I think you have to be rather careful how you define these firsts. Clearly the stuff that Comrie had done at Greenwich was an earlier venture, but it was not a permanent one. And I'm still, to this day, not sure whether or not he paid rent, or paid for it with a grant, or what. Then, of course, both Ben Willard's statistical stuff at Columbia, which antedated the Comrie work and Eckert's Installation at the Watson Astronomical Computing Bureau were both earlier, but they were early ventures. They were not rent paying operations in that sense of the word.

And, of course, all over the United States and in many places elsewhere in the world there were statistical installations, notably in census offices and stuff doing very fancy technical work, indeed. But it wasn't technical computing the way that the word was used in the next ten or twenty years because it didn't involve the full panel play of numerical analysis and so forth. The mechanical part of the job tended to be simple tabulation. Just adding up large columns of figures, unit counting, and so forth requiring complex and demanding machinery and a high degree of the organization of the individuals and the work, but not requiring the integration of differential equations, inversional matrices, and

so forth, that soon came to characterize technical computing.

So, I guess, in that sense of the word I would say that the Comrie work was the first scientific computing done on punch card machines and that Eckert' s laboratory at the Naval Observatory was the first one to do it on a rent-paying, straight forward, permanent basis. That installation continues to this day, although of course there have been several generations of machines go through it.

In addition to Wallace Eckert, who was the Director of the Nautical Almanac Office, and who had his office in the main building of the Naval Observatory, and Belzer who was the supervisor,' there were a couple of girls who shared the work of running the cards through the machines and key punching the data to go in them. And these girls were not professionals, and they have disappeared from the scene now. I remember that the younger and more attractive of them was named Ruby and I have no doubt that Belzer would remember both of them. But I don't anymore. We used to get together quite frequently and I was always extremely interested in what was going on in this room, because it was the embodiment of the book that I had read and the work that I wanted to do on my own thesis. But it was too late to use it for my thesis work. And I had been unfortunately assigned to the Astrographic Department where there was a vacancy, rather than to this intriguing work in the Nautical Almanac Office. And I had not wanted to do the routine desk calculator kind of work that the rest of the Nautical Almanac people were doing, so I could not complain at the circumstances. In fact, it gave me a chance to watch what was going on.

I still didn't get my hands right on the machines. I went and ran a couple of packs of cards through be sorter once to see what it was like, but it's not the same thing as laying out the work and running it yourself. In fact, it was to be some years before I actually had a chance to do this.

Nevertheless, that was my nearest approach to that time to a real computer installation. I used to talk to Eckert about my ambitions with respect to Jupiter 8 and how I hoped, after the pressure of this Air Almanac work disappeared, that I could make some use of his equipment. And he used to soothingly tell me that it would be a wonderful thing of course. But before any of this could mature the war came along.

I think I should back off for a minute and mention one other item about personnel.' When Eckert installed the Thomas J. Watson Astronomical Computing Bureau at Columbia, he had had some help. To the best of my knowledge the work done at Greenwich Observatory was done by the Astronomers and the professionals themselves. But Eckert had decided since the - remember it was to be a temporary operation so that it was not advisable to take on specialized personnel for a permanent assignment when the work itself was not permanent. But Eckert set up the Astronomical Computing Bureau to be a permanent thing. He expected the continuing support from the University and from the Astronomical Society, and of course, he had been given the machines by IBM. So he hired a full-time person to help him with the more routine aspects of the job. And this

was, I suppose, in a sense the first scientific computing installation supervisor in history. A girl, unmarried at that time, named Lillian Finestein.

Now, when Eckert came to the Naval Observatory, it's important to remember that the Astronomical Computing Bureau remained behind, I have forgotten, if I ever knew, what the intension was for a program form(?) in the time, the five or ten years that Eckert undoubtedly intended to spend at the Naval Observatory. But, in any event, Lillian Finestein remained behind to run that installation, presumably on Eckert's old research projects, (although this I didn't know). And when the war came along it switched over to war work and, in fact, existed as a useful technical computing bureau through World War II, Not in any way under Eckert's direct control, but of course since he maintained a tenuous academic relationship with Columbia all through this time and a strong personal one, both with the Administration and with the Astronomy Department there was always in touch with them and knew what was going on.

I was the one, who, so to speak, went away and don't have firsthand knowledge of what went on. But I might say that during the latter part of World War II, while I was off doing optical work, the General Electric Company in Schenectady bought a very substantial computing job down to Miss Finestein, probably with Eckert knowledge. And they used this installation which by this time was beginning to grow with extra equipment, which was secured on a priority basis from IBM, and was used for the calculations involved in designing or at least in ratifying the design of the fire-control equipment in the B-29 Bomber.

The B-29 Bomber had remote control guns, machine gun turrets which were guided by several mechanisms from sighting stations in the sides of the plane - in sighting blisters on the sides of the plane. So the human beings were off in these blisters and there was no one actually in the turret with the machine guns. The connection between the sighting stations and the machine gun turrets went through an analog computing device which computed the appropriate lead for the guns? And the computation of the surfaces of the cams in the sight and other ballistic and hydraulic matters that went into the design of the system were done at this Astronomical computing Bureau, or at least the tests verifying the quality of the design were calculated there. I remember that the firings were done at Eaglin Field for instance, and the data was sent up to Columbia to be processed through this equipment.

Miss Finestein, who by this time had gotten married and was now Mrs. Lillian Finestein Haussmann, remained in charge of this all through the war and she recruited toward the end of the war a young astronomical graduate student named Everett Yowell. Everett's father was the Director of the Cincinnati Observatory where Paul Hergert had done his astronomical computing in the old days. So, through knowing Hergert, he was aware of this sort of thing and got himself recruited into this group and was, in turn, the senior operator under Mrs. Haussmann's general supervision on this GE program.

As I may remember to mention later on, I was sort of concerned with the B-29 air born

fire control system through optical work at Sperry Gyroscope Company. But I didn't know that this work was going on in such detail until after I left the optical thing and came to IBM in 1945. Anyhow, that group continued to exist at Pupine Laboratories of Columbia University through the war while Eckert was at the Naval Observatory.

Now we switch back to the Naval Observatory. Eckert's main assistant, other than Belzer, was the assigned, customer engineer. IBM always had very highly trained maintenance engineers to help in any installation that had unusual requirements; one that was either very big, or that had unusual environmental conditions- or unusually demanding problems, or which was very young. They had assigned a young man named Richard Bennett to work almost full-time with Wallace Eckert, He remained, of course, an IBM employee under the control of IBM's Washington Office, but he spent almost all of his time with the Naval Observatory. And I had a good deal to do with him socially, also.

One of his tasks, for instance, was to supervise the printing of this Air Almanac t Aside from the-tasks of interpolating the numbers from the more accurate but less frequently tabulated astronomical data, you had the problem of finally producing the physical document itself, Now this was to be a quarterly publication with, as I remember it, one double page, one facing page opening for each day and an entry for every ten minutes of the day as a line on these pages, plus ancillary pages. And to print these--let's see, it would be two times the number of days in-- it would be 180 pages in each one of these quarterly volumes, plus the introductory material and auxiliary material. To print these every three months, error free you remember because the life of not only an individual but of an entire squadron of bombers flying on an overseas mission might depend on one single misplaced digit in this thing, It was an extremely challenging problem. Eckert decided to print them from tabulator material, but the trouble with the tabulator material was that it was too coarse for the kind of sizes required. So what he finally did was to put in half-width slugs in the type bar of the tabulator. This meant that you got a rather narrow numerical character printed. Of course this is all numeric, the alphabetic material was posted in later, the headings and stuff were posted in later. A large page was printed in this way with every other digit printed. That is, if you had a six-digit number digits one, three, and five would be printed at one pass of the tabulator for the entire page, not just the column, but the entire solid page all at one run. The paper would then be started back through the tabulator very precisely and the platen moved over one-half of the normal width of the type bar or an amount equal to the width of these half -space characters and a different plug board inserted into the tabulator and digits two, four, and six of the six digit number would be printed. In order for these to be perfectly lined up horizontally and so forth, very, very precise adjustments had to be made to the tabulator. And Dick Bennett was out there day and night making sure that everything was just so and practically running each sheet through himself. The overall large sheet was reduced photographically to the appropriate size to make lithograph plates from and printed at the Government Printing Office.

To make sure that there are no errors introduced in this process, the final plates, all ready to be printed, had proofs pulled from them. These proofs were key punched pad the key

punch data was then compared in an IBM reproducer to the cards from which the material had been punched in the first place. The only human examination of these pages that was done was to make sure that there weren't misalignments or that things were sort of all…and didn't look right; But the correctness of each digit and the verification that each digit was there and in fact that it wasn't just a blank, and so forth, was done by the key punching process.

The result was that many years later Eckert could state in publications that there had never "been a single error detected in the production of these 180 pages per quarter of this very important and very greatly used data. So it wasn't something that was just put on the shelf, it was used day and night by people who's lives depended on it. That no single detected error had ever occurred, not one digit had ever been wrong in all this work. Now you could use mathematic and arithmetic checks up to the point of the printing, but the next question was to do the printing without the horrendous task of proof reading that would have otherwise been necessary.

So it was a real accomplishment, and of course it took him a good deal of time to do this. I'm describing the thing as it was working half way through the war. The first few issues weren't all that nice, but every single issue of the Air Almanac was produced automatically, there was never a hand-made version switched over.

Then the thing that was interesting about it was that very early in this game Eckert ordered from IBM on a special procurement basis with Government funding which I suspect however didn't really cover all the cost of the engineering work, a card-operated typewriter to replace this horrendous tabulator operation. This typewriter was produced toward the end of the war and is described in one of the early issues of Math Tables and Other Aids to Calculation, the NRG publication on computation of methods which came out later and it was essentially a typewriter with several fonts of numerical characters, replacing the usual arithmetic ones; proportional spacing and with the spacing under control of a separate punch card unit so that you could, for instance, program the machine to type all the digits one on top of the other or to have spacing entirely independent from the normal spacing of the typewriter and so forth. And with this very beautiful mathematical tables were produced from the middle 1940's on. And that machine was used at the Naval Observatory for many years.

It was, I think, the first specialized computer output device ever built by IBM, although thousands and thousands of specialized devices have been built since. That was done almost entirely to Wallace Eckert's specifications. The man who operated this machine, which is described for instance not only in MTAG but in the 19^-8 IBM Forum Series that we were discussing a little while ago, was at that time a Frederick Hollander, a professional astronomer assigned to that work.

During the war when it became obvious that there would be further calculations performed by this installation of punch card machines, and after I had, myself, left the Naval Observatory, others whom I've mentioned already, came down to help. Notably

Paul Hergert joined them from the University of Cincinnati and was responsible over Belzer, but beneath Eckert for a good deal of the detailed, fairly advanced work that was done at the Naval Observatory during the war. Shortly after the end of the war my old professor, Alien Maxwell joined them for a while, but did not find that part of the task congenial. He sort of replaced Hergert after Hergert went back to Cincinnati. He didn't find the work congenial and he was somewhat disappointed at the organizational problems involved. And he ended up as a professor at Howard University--from which he retired--and he died a year or two ago.

So he retreated essentially from an opportunity to work deeply in the computer field. He could have done so through the Naval Observatory connection and maintained his astronomical work. But just as I remember as I was leaving Michigan that he pulled back from the use of desk calculators because he preferred the feeling of creativity in using logarithms and printed tables. So I think he pulled back from his first opportunity to associate with punch card machines and so on, I didn't feel free, although I was always his favorite graduate student, I didn't feel free to criticize him, for this was a personal judgment. But I always felt that he had a good deal of knowledge to offer in numerical analysis and so on that was never fully utilized in the early days of the computer business, except in that it was passed on through people like me and Paul Hergert who had worked for him, mostly me.

Well, that carries the Naval Observatory along pretty well of through the war.
I had finished the writing of my thesis and was in the process of typing the tables and drawing the graphs and so forth for it, and arranging for my final PhD examination and so forth. And my wife was helping me do all this in after hours and on Saturdays and Sundays when Pearl Harbor came along. And one of the early effects of Pearl Harbor was that all strangers were banned from the Naval Observatory ground, so that my wife wasn't able to do any further work within those hallowed precincts. And the work load for the civilian staff increased considerably as military things began to come in. The Air Almanac, of course, was already planned for, but there was a good deal of testing of specialized gadgetry, navigational equipment, primarily by people who were concerned with instrumentation.

And I, myself, was drawn into this because of having some knowledge of optics by a group at Boiling Field that was trying to design a zenith camera. So I remember, as a paid employee of the Astrographic Division, helping these people reduce some aerial camera films by the use of our highly precise measuring equipment and desk calculator-type calculations to find the accuracy with which this zenith camera had been directed to the high-point of the sky. The intention being that such equipment would be used probably by parachute drop to provide fundamental points for aerial mapping of remote territories like Alaska and so forth, provide the fundamental geodetic control for this and so forth.

Well, I suggested that if this were done they should, for instance, use punch card equipment for the calculations and actually laid out some minor suggestions of this sort. This would be - by this time, I am talking in terms of the early spring of 1942 Pearl

Harbor has happened and so forth. It became obvious that I was about to be drafted or otherwise involved in direct military service unless I did something more useful than staying in the Astrographic Division.

I did not see a way clear to getting into this Air Force project, it was then, of course, the U. S, Army-Air Force, on zenith cameras. It seemed to be all military people and moving at too slow a pace. I didn't see the point of attempting to do more war work at the Naval Observatory, anyhow, which seemed to be rather a backwater in what was then a highly popular war. So I finally dug up my knowledge of fancy optics and offered ray services to a group in the downtown Navy Department, in the Naval Ordinance Activity, which was doing research and development work, or more accurately, was sponsoring research and development work, in fire control optics.

This involved a formal Civil Service transfer from P-l, Junior Astronomer to P-2, Assistant Physicist in Naval Ordinance but remaining, of course, within the Navy Department. So it was quite easy to arrange. It involved my working downtown in the old temporary Navy Building, which has just been torn down in the last few weeks. It also involved, for the first time, my really doing something that was practical engineering work.

The instruments and gadgets that were being examined and designed and tested were practical mass production fire control instruments. They ranged from very simple things like telescopes, to aid in the sighting of fixed guns on a dive bomber, to very elaborate bomb sight optics, to very elaborate periscope systems for our submarines and airplanes and so forth.

I came to the group with a superior knowledge of theoretical optics. I was probably the only person in the Navy Department that had ever heard of high order aberrations and so forth, with the possible exception of one or two old optical designers at the Naval Gun Factory. And I certainly was the only person that knew how to calculate them.

On the other hand, I had almost no knowledge of the practical engineering aspects of optics. I remember distinctly, for instance, not being very clear on what the entrance-pupil and exo-pupil (?) of an optical system was something that has everything in the world to do with the use of optical systems for gun sights or something like that, or even for something as simple as a binocular. But I could calculate the aberrations of the lenses to a furriorel(?) Well, I gradually acquired the engineering knowledge I needed, partly by reading, partly by professional discussions and membership in the Optical Society and so forth, and partly by just the plain old school of "hard knocks".

We analyzed the usefulness of captured Japanese optical instruments as measured at the Naval Gun Laboratory and calculated by me and a couple of assistants. We laid out a few oddball special gadgets for instance, I remember inventing a rather unique reverse telephoto boracites system for a 20mm cannon, for instance, to be used on anti-aircraft mounts in the Navy. I remember having a very interesting visit with the battleship

Alabama when it was anchored in the Chesapeake Bay, and clambering around in the fire control equipment, being quite thrilled by the whole thing.

But primarily, it involved negotiating as a civilian and under the direction of some really scientifically inclined officers who had been given blue suits for the duration, on the development of more novel optics in industry and in universities. Polaroid, for instance, was developing classic lenses and had built a rather novel shore-based length range finder. Sperry Gyroscope was designing and building - was re-designing and building the Drapper Gun sights, which were the first things to come out of Dr. Eckert's work at MIT which later culminated of course, in the huge instrumentation laboratory activities that have been so actively discussed in the last year. And I met Drapper through that activity, etc.

MERTZ:

When was your transfer?

GROSCH:

August of 1942 as I remember it. I wanted to do it in the spring of '42 but it simply took a while to find a useful solution and to persuade the Naval Observatory that it was a suitable transfer. And Civil Service, even then, was a complex mechanism that did not move too fast. It took, I think, maybe four months of paperwork before I finally, actually emptied my desk at one place and filled it at another.

MERTZ:

And that also involved a promotion?

GROSCH:

Yes, although that was not important to me as a matter of fact. But it was a promotion as I remember it to $2600 a year extra. Meanwhile my wife had decided that she, too, should do something for the war effort and had gone to work with Dr. William F. Megers, the Dean of Spectroscopy, I mean the senior figure in Spectroscopy at the National Bureau of Standards, which was then on Connecticut and Van Ness. And, as it turned out later, what she was actually doing was helping Dr. Megers and his assistant, Berden Scribner, develop analytical spedsographic analytical methods for uranium impurities, with which they were actually advising the procurement of the uranium - the procurement and fabrication of the uranium slabs to go into the stag pile. But we didn't know this; of course, Dorothy was not cleared for the upper delicacies of the project. All we knew was that there was some very peculiar thing going on with uranium at the Bureau of Standards and with my science fiction background and knowledge of astrophysics and the solar phoenix(?) cycle, it didn't take very much extrapolation to conclude that this had something to do -with the use of an atomic weapon of some sort.

Perhaps not a bomb, I didn't know that I saw that, but certainly some kind of an atomic weapon.

MERTZ:

Then, well let's see, did she start shortly after Pearl Harbor?

GROSCH:

She started just about the same time I started downtown as I remember it. I think that she was probably at work in the early summer and I went to downtown Navy Department in late summer.

We were at the time living in Kensington. We had just bought a house, the first house I'd ever owned, and moved out of a little one-room apartment which we had on MacArthur Boulevard, then called Conduit Road, 4854 MacArthur Boulevard. It's still standing. So we moved out to Kensington and had this very attractive, speculative house on a dirt road, by the way. It was still possible to buy a house on a dirt road in Washington. In fact, with the war it was impossible, in fact, to pave it until the end of the war.

I worked at the Navy Department for about a year and a half. My superior there was a man named Dr. Stanley Ballard, - (Jr., I believe), who'd been a professor of physics at the University of Hawaii; and who made a very distinguished career out of his involvement with optics. He hadn't known much about optics when he got the job, and it was always a mystery to me how he got it. But as a full Lieutenant in the Navy, later a Lieutenant Commander, he did such a useful job during the war and made so many acquaintances in the professional optical field, that after he left and went back to academic life, he became more and more important in the operation of the Optical Society; of which he became Foreign Secretary, Corresponding Secretary, and ultimately President. And then later in the Institute of Physics as a whole, so he's now quite an important artifact in the professional physics world, especially in the optical end of it. All of which, I'm sure, stems from this.

One of the other men in uniform was a Lieutenant Gurley Nutting, Jr. whose father had been a distinguished optical physicist in World War I. Another one was Erna LaDelle who later became a very - I think also was a doctor - who later became a very important person in the nuclear testing program after the war, and was responsible for some of the high-level hydrogen bomb tests and so forth, I've lost track of them since, but I believe he is also a fairly important figure in physics today.

Then there were some junior types whom I've sort of lost track of. My direct superior, the person who actually signed my paycheck was a civilian named Michael Goldberg, who was an old time Civil Service engineering type. He was in charge of a large drafting room where all the civilians doing work on torpedo gun sights, aircraft gun sights, communications and fire control of systems for battle ships and destroyers and so on, all

worked.

And I sat at a drafting table just like a draftsman, but I ran a desk calculator instead of a T-square. I was associated with a couple of old civil servants who had worked at the Naval Gun Factory, one of them named Russell Banker whom I have lost track of. And it was really from them, as much as from the literature, that I began to get -some practical feel for this business of fields-of-view and exit-and entrance-pupils and so forth. And, in turn, I passed on to them what little I had that was of value in this aberration calculation sort of thing.

I should say that during the time I was at the Naval Observatory I was not only working on my own thesis and the regular work of the Astrographic Division, but leaning on the stuff that I had learned from Baker at Harvard in '40 - '39. I was trying to design a corrector lens for the - to flatten the field of the Ritchey-Gretian reflecting telescope. So I sort of had my hand in the optical thing even before the
a war started and it became useful particle. The main thing that I didn't like about the Navy work internally, aside from the fact that I was reporting to officers but was not an officer myself and similar little personal items, the fact that it took me an hour and a half to get to work in the morning and I had to be at work at 8:00 and things of this sort.

MERTZ:

As I say, did you commute and drop your wife off at the Bureau of Standards? (garbled)

GROSCH:

She belonged to a car pool that used to start in Kensington and take turns driving into Connecticut and Van Ness. So we used our "A coupon" or whatever it was for that, and she drove I think it was two days a week or something - no I think there were more than that in the car - anyway she drove a quarter of the time or something.

But I, unfortunately, had to leave very much earlier than that, walk about four or five long, cold blocks -to the end of the bus line, take a little "jitney" bus into Chevy Chase Circle to transfer there into a larger bus which took me all the way downtown to the Navy Department. This took pretty near an hour and a half. And you have to log in, you know, precise to the minute with Navy discipline or else loose 15 minutes annual leave and be scolded for holding up the war effort besides. And my drafting room was on the top floor and at the extreme back of Wing 0, which meant that I had to get into the building about five minutes earlier in order to walk to that enormously distant room where all this happened (laughter). So there were some little personal aggravations, but the main thing was that as I became accustomed to the engineering atmosphere, the practical work in optics. I found it more and more interesting, and I felt myself more and more capable, whereas what I was doing was essentially the assessment of test results and so forth that other people were creating.

MERTZ:

Had much of this work been sub-contracted out?

GROSCH:

Almost all of it. There were heavy connections with Eastman Kodak, with Bausch and Lomb with Kuflin Eshert(?) which was responsible for much of the larger range finder production in those days, and then with "hot shot" outfits like Polaroid and some of the Universities who were trying to do really novel things; I mentioned plastic optics. Most of the creativity was at those places, it wasn't at the Navy Department. And what little kind of specification creativity was involved, the highest level of assessment of "should we put our money here or here?" which would have been interesting, was being done by Ballard, not by me. In fact, I was a long way down from it.

So, I wanted to do something a. little more creative. So I managed to get myself an offer from Sperry Gyroscope in Long Island and left the Government after about a total of only a couple of years of work as a Civil Servant, not to return until 1967.

MERTZ:

Did you have a chance to visit any of these facilities when you were with the Navy Department?

GROSCH:

Yes, I began to travel about this time. As a youngster I had done almost no traveling. My parents had been very conservative about this; there were no vacations and so forth. I wasn't accustomed to it. The first major trip I remember ever taking was when Alien Maxwell asked me to go to Minneapolis one summer, I think it was about 1938, when I completed my summer observing chores at the University and then drive home with him after he had spent a summer there as a summer school professor. That was the first time, I think; I had ever been more than a hundred miles from home. Then, of course, the two trips to Harvard. Both of which I finished by spending a few days at the New York World's Fair which in '39 and '40 was on in Flushing. But those were darned near the extent of all my travels.

MERTZ:

You hadn't gone to the Chicago World's Fair?

GROSCH:

No, I hadn't gone to the Chicago World's Fair. I was too poor. I knew it was on and I had

read all about it in the papers and collected literature about it but I wasn't able to actually go. Many of my friends did, but I wasn't able to.

Now at the Naval Observatory I began to go to a few scientific meetings. I had joined the Optical Society by this time so I now belonged to two scientific societies: The Astronomers and the Optical Society. And I remember going up to New York once, for instance, to a meeting at the old Statler Hilton - what is now the Statler Hilton - the old Hotel Statler which was across from Penn Station. But it was a pretty important thing for me to make a trip of this sort. I didn't do much of it. Now when I travel 100 or 150 thousand miles a year all over the world, why it seems kind of laughable. But at the time, why, I wasn't much of a traveler. I wanted to be but I didn't have the opportunity.

MERTZ:

You took the train out to California?

GROSCH:

No, I went by bus - Greyhound bus. Five different buses, I remember, it took two days and three nights, I think to get there and almost as bad to get back. I remember I took an extra long route so I could see as much of the country as possible.

Yes, I think at this time I had had a couple of flights as a kid where my dad had paid to have me taken up for an hour or so, you know. I seem to remember riding in a Ford Tri-Motor(?) at the Detroit Airport at an air-show once, a couple of things like that. But I never had a commercial airplane flight for travel purposes only and had done little train riding - darned little. Anything I did back and forth to school, for instance, was either by bus or in my parents' car.

So I was pretty naive at this sort of thing and when I began to take small trips from the Navy Department and many more when I went to Sperry Gyroscope, they were usually to places like Eastman Kodak in Rochester, so that it was still not a great distance. But by this time, of course, the problem of war-time travel was a promise and you needed priorities in order to get an air ticket and, in fact, to get a reservation on a Pullman train.

MERTZ:

I was going to say; did you fly any of these places?

GROSCH:

Occasionally, I remember flying American Airlines to Rochester, for instance, oh, about 1943. To the best of my knowledge that was the first commercial plane trip I ever took. I seem to remember that I had already gotten to Sperry by then. The trips I had taken for

the Navy had been train trips. But somehow Sperry had a priority on this one, and we went up to Eastman Kodak and I remember I was airsick on the way back and wasn't sure I'd like it. Now having flown nearly two-million miles, that seems a long time ago (DC-3 I suppose).

MERTZ:

Was this on one of these trips that you took up to Sperry an occasion when you had a chance to discuss...

GROSCH:

I seem to remember that I did this Sperry thing by correspondence. That was one place that I had not had a personal visit) but people from Sperry had visited the group at the Navy. I knew who to write and all that, but I think I hadn't visited them.

I'd gone to Bausch and Lomb, I'd gone to Kodak, but I think I had not gone physically to Sperry Gyroscope at that time.

The thing that attracted me, I think, as much as anything was the fact that they had a mixture of advanced fire control stuff. This was Dr. Eppert's computing ________(?) in these gun sights. A very high priority because these were the main weapons that we were using for defense against the kamikaze attacks in the Pacific; and some difficult but not intricate optics.

The Mark 14 gun sight which is what we were mounting on the back end of everything from PT Boats on up to shoot down kamikaze's, was a little box full of gyroscopes and a lot of computing devices, air driven gyros as I remember, so that there had to be air pressure and filters and stuff in it. These gyros controlled an extremely light little un-severed reflecting glass which reflected a culminated image of a brightly illuminated radical down inside the gun sight into the gunner's eye. He looked through this glass and through the windows closing the box--this glass was between two windows of the box--at the enemy plane. He pulled the whole box around and then he-in many instances this box was actually fastened on the gun--so that you pulled the gun and everything around in such a way that this illuminated image of the radical was kept on the enemy plane. If knobs on the front of the box were set to the correct range and so forth. The gyros inside computed from the rate at which he was -moving- the box and the gun, in order to keep on the airplane, computed the proper lead for the guns and displaced the radical image backward. So that by keeping the radical on you never-the-less got the appropriate gun lead and were able to calculate very precisely how to hit the plane.

Now about the time I went there, the basic problem was to procure these little - this little fine piece of glass, the lenses for the radical columnator and so forth, the radical itself, and the windows in such a way that all the appropriate optical tolerances would be met. And yet the little reflecting glass had to be sufficiently light that it could be moved by

these rather small gyros and so forth. So it was an engineering problem. It wasn't a fancy design problem at all. It was a question of procurement, of inspection, of sufficient supply and so forth. And with the help of some rather down-to-earth Sperry Gyroscope people, I learned something of that part of the business.

Then Drapper went to much more complex design in which this little transparent, wiggling glass was replaced by a good deal heavier flat mirrors with illuminated surfaces which, along with a pair of fixed mirrors, constituted a sort of a dogleg optical system like the prisms in a binocular out in front of the telescope. That way you got a five power magnification from the telescope so that you could aim at a plane of very much larger distance out. This had to be put into a director-type thing that was independent from the guns and was used to point a 40mm quad cannon mount. This offered all sorts of interesting opportunities because you had a good deal more optics to procure. You had a five power telescope, you had a much more complex radical system, and these mirrors which were heavier also had to be much more precise because they have to reflect - they had to be usable in a five-power optical train, and so forth and so forth. Then there were still windows that they looked through, this thing was still inside of a case and you still had to look out-through the windows.

And, finally, about half-way along in my stay with them they decided to introduce a radar input to this. Radar, at that time, was a great secret, a very nameless classified, but it had been going on so to speak across the hall from me in the Navy Department so I was aware of this And my interest there was to feed in a signal from the little cathode ray to the radar input which could be tracked instead of the actual airplane in conditions of fog or at night and so forth.

[Start Tape 5, Side 2]

GROSCH:

The fundamental design work had been divided between MIT, where Drapper and his group were turning out the prototypes. Places like Eastman Kodak that were going to do the mass-production of course had to redesign it. And my task was to cooperate in this to figure out how to get this radar thing in which I was doing as the original creative designer, and at the same time set up and operate inspection facilities to handle a fairly large flow of these parts when they came. I, for instance, pioneered the use of an interferometer-type inspection device for" flatness and things of this sort while this was going on and had to learn a certain practicality of approach to technical problems, which has been useful to me ever since.

Well, I enjoyed that very much and I began to learn the intricacies of traveling on an expense account and the intricacies of maintaining one's exemption from the draft in war time, and so forth, and so forth. Nevertheless, I still felt that I could do something more useful. And after another year and a half there, I transferred to a place called Ferrand

Optical Company, which again still exists and in fact has built some large and complex simulators for NASA, a manned space craft center in Houston, in the last few years. At that time it was a small outfit in the Bronx.

MERTZ:

Lets, - if we might go back in chronology a. little bit,..

GROSCH:

OK, sure.

MERTZ:

This was from August of 1941 until ...

GROSCH:

That would be the Spring of 1943.

MERTZ:

Well, it was first - you were at the Observatory...

GROSCH:

Ah no, I'm sorry, I have slipped a year in there. I was only at downtown Navy Ordinance about less than a year. So it was the spring of 1943 that I left Washington.

MERTZ:

Now, let's see, that was when you left the Navy...(garbled)

GROSCH:

So I was at downtown Navy Ordinance for only about eight months, something like that. I thought there was too long a period in there.

MERTZ:

So actually you had spent almost exactly one year at the Observatory?

GROSCH:

Umm hmm, yeah%

MERTZ:

From August of 1941 to August of 1942?

GROSCH:

Umm hmm, yeah, right. And a half a year or a little but more than a half a year downtown.

MERTZ:

Spring of '43?

GROSCH:

Right. Then I stayed a year and a half at Sperry. That is correct. And that brings us to the Autumn of '44.

MERTZ:

And that also caused you to move away from the Washington area?

GROSCH:

i Yes, I had to move for the first time to another city, and I located in Hempstead, Long Island which was about four or five miles away from the Lake Success Factory, in our Garden-City Laboratories of Sperry.

MERTZ:

I see, and did your wife give up her job?

GROSCH:

She gave up her job at the Bureau of Standards and became a spectrochemical professional at the Inter-chemical Laboratories on 45th Street in New York City. But there was an intermediate period in which she was settling us in our new house and trying to have a baby and a few things like that - trying unsuccessfully to have a baby, and a few things like that - before she started this up, so she was a little stale/'"inter-chemical was primarily a peace-time sort of outfit, dealing largely in printing inks and that sort of thing, so she did not do any more work in what later turned out to be the atom bomb, or anything like that, for the duration. But, I remember that they were working on the paint for the black-widow fighter, for instance, and this was the laboratory that was making the shiny black lacquer for that Northrop(?) plane, which turned out to be better at dodging

search lights than the dull black paint that had been used up until that period. So even there the war OCS impinged, as it impinged everywhere in those days.

The Sperry Gyroscope work had been more varied that I indicated. It wasn't just those two gun sights; they had many other things going. They had extremely elaborate Sperry bombsight which was a good deal more accurate and a good deal more interesting than the bombsight, but was not made in anything near the quantities! It was too elaborate, too delicate for mass production at that time. I had something to do with that.

And then toward the end of my stay at Garden City and Lake Success, both, I was involved in the fire control for this B-29 bomber, which was then a very super-secret project. Not yet in production, of course. This was the one which ultimately used the General Electric fire control system, the details of which were calculated at Watson Astronomical Computing Bureau, you will remember. But at that time the Sperry opposition to this consisted of a very much more elaborate analog computing device - a large box the size of a kitchen chair out of the top and bottom of which came a double-ended aircraft periscope that was supposed to stick out of the top and bottom surface of this big bomber,' so that a person standing in an eyepiece built into the front of this box would see either the upper or the lower hemisphere through a periscope telescope system. And by tracking an incoming fighter plane with that system would calculate and dispose of these remote-control machine gun turrets.

The Sperry system lost and was cut off before I left Sperry. But it was, again, a good deal more complicated, a good deal more accurate than the winning General Electric system. And was of more interest to me because it was a good deal more optics in it.

The system was already under re-design when I first had something to do with it to obtain a bigger field-of-view. That re-design was being done by some very sophisticated lens design people at Eastman Kodak, at the Hawkeye Works in Kodak in Rochester. And I began to be acquainted there with some very high-level, professional lens designers, the first people that I had met on a continuing basis that were of the same caliber as Jim Baker, for instance.

This team was lead by Rudolph Kingsley, who was then Chief Optical Designer for Kodak. He was a very distinguished Englishman who had come to Kodak before the war and is now author of a large series of books on optical design, and so forth, that he's written in his near retirement years. His wife was the daughter of Conrady, the writer of the textbook which I had used to learn practical optical designing from at Navy Ordinance before I came to Sperry. So I felt drawn to Kingsley by his Englishness, remembering my English parents; by his relationship to Conrady which was my bible at the moment, and by the fact that he was the top man that I had contact with in this important contractor. And he and his men re-designing the optics for this double-ended periscope introduced me to wide-angle eyepieces and several other interesting things and to some of the intricacies of prism design.

I felt it would be very interesting to work with a group like that. I was conscious of the fact that I was not probably going to be a life-long optical designer that I probably would go back to being an astronomer when the war was over. It didn't seem appropriate or likely that they would--that I should join Eastman Kodak, for instance. In fact I never asked to be considered, but I did look around for something that would be a little more like the work they were doing and a little less of this inspection and procurement sort of thing with which I was involved at Sperry. Not that I minded it, I enjoyed it very much, and it taught me some very practical things. But it was rather a limited universe intellectually. I wanted to do some creative work; I guess what we say nowadays, some synthesis rather than just analysis.

And so I made this connection with Herrand Optical. As I remember through an Employment Agency, I don't believe I knew about it in any detail from my - either my Sperry or my Navy Ordinance Connection, but I think I came across it through a technical Employment Agency in the New York City area. I practically had to stay in New York because the problems of relocation during the war were just almost insuperable unless you were moving to a place, for instance, like Los Alamos which wasn't of course publicly known then. There wasn't any housing available when you got there so you almost had to stay put.

MERTZ:

Did you have a great deal of difficulty finding housing when you moved up to New York?

GROSCH:

No, because that was early enough that there were still houses left, so to speak, I bought a house; I couldn't rent anymore, I bought a house but there was still quite a choice of freshly built houses. There were things like, for instance, one was unable to get gas hot water heat. And we had to heat our hot water through the war with a little pot-stove that you filled with coal the size of a pea rather than big chunks of coal, because the war-time priorities had eliminated gas heaters by that time. But, I guess in fact gas connections - no, we cooked with gas - but at least you couldn't use it to heat hot water with by that time. But the houses were still there. You could still get the houses. A year or two later after they were sold, of course, there were no more available. And when I wanted to work at Ferrand, which is in the Bronx, the only solution was to get a sea-ration card, which I had to arrange for before accepting the job, and drive all the way across the deserted parkways up into the far Bronx, which was nearly twenty miles each way each day. So I burned more gasoline during the war than 99 per cent of the people. Moreover, I couldn't join a car pool because Ferrand was a small enough group that there wasn't anyone else living that far away. So I had to have a sea-ration card...

MERTZ:

It probably took you a little longer to get there than it did to get...

GROSCH:

Oh no, completely deserted, there wasn't...

MERTZ:

(garbled)

GROSCH:

That's right, completely deserted. You could just whiz along; I think it took me less than an hour as a matter of fact, including crossing the Bronx-White Stone Bridge and everything. I remember I had to bargain for a little more money in order to pay for all the gasoline and bridge tolls, because the bridge tolls were two-bits each way, in addition. This didn't seem like much, except that I was probably making only five or six thousand a year by then, I would have to look at my income tax to see how much, but it certainly wasn't very much. I seem to remember it was $5200. (After bargaining). And I remember that after I had worked for a couple of weeks they were sufficiently pleased with me that they told me that they would pay my Employment Agency Fee, which I took as a compliment.

My superior was a man named Tripp, who had been some kind of an engineer - high-level engineer - for Walt Disney. And Ferrand had hired him because they were doing a good deal of simulator-type work where you were essentially trying to confuse the operator or the trainee into thinking that he was in a real-life situation by optical means. And so the person, who'd worked with animated cartoon operation and so forth, knew more about doing this sort of thing than an Eastman Kodak camera designer did.

My superior at Sperry, I should mention, the highest level superior I had was a man named Carl Holschuh, who later became an Executive Vice- President at Sperry Rand; at least at the Sperry Corporation. I'm not sure how he carried over into Sperry Rand, but at least at the Sperry Corporation. And under him a man named Dave Fram. And then when I began to work heavily on the gun sight work, the Dr. Drapper gyro-gun sights, and my immediate superior was a man named George Bently, who had been the Director of Research for the Hamilton Watch Co. of all things, and of course, was involved in the fine mechanism kind of business through that. But who had also been a student of Drapper's at MIT before the war, so that he had a good connection for that. He was Chief Engineer on that program. I think still under this man Holschuh, although I'm not quite sure of the organizational details anymore.

While, when I went into Ferrand I worked for this man Tripp, I worked with a man named Dr. Seymour Rosin, who was a Senior Optical Designer and I was 8 Junior Optical Designer. I had now risen to the point where I did the simple synthetic work. So

did the more complex synthetic work and the two of us together did the more difficult, high-level stuff, each contributing as he could. And by this time I had begun to have, you know, permanent help under me. I had had a laboratory assistant-type young man in Sperry, and now I had a girl named Mrs. Alice Barskey who ran my desk calculator for me, and this was my first computer assistant, so to speak. So that was the first of many, many hundreds of people that I've had running computers for me since then.

This, as I remember, was another one of those Marchant ACT-10s. I had sort of gone-down hill when I left Maxwell and the machine that I used mostly at the Naval Observatory was an old mechanical monster called a "Millionaire" which was built in Germany in the 1860's or something, and had the peculiar characteristic of performing a multiplication with one turn of the crank-instead of nine turns to multiply by nine, you only turned once. It was a gear-shift mechanism that selected an assortment of different gears that had the result of producing a nine-fold thing instead. When I went from there to the Navy Department, I think I got myself my first Freden(?), And then when I went from the Navy to Sperry, I think I had another Freden, but I didn't do a great deal of calculating there. When I went to Ferran, by golly, I had an assistant, Mrs. Barskey, and she used another one of these Marchant ACT 10's except I seem to remember it was only an eight-place machine. It was an AGT-8M.

By this time I'd gotten very much interested in the intricacies of optical calculations because the closer I got to design work, the more of this was to be performed. For instance, I still have notebooks from that period in which there's page after page of calculations, more complex than a slide-rule - more accurate than a slide-rule - and quite complex just to design a little head prism or something.

MERTZ:

The number of place accuracy...(garbled)

GROSCH:

A good deal less. But in fancy lens design not so much less. Most lens design is carried on to six places as a precaution and must be carried on to five places. This is because the diffraction image kind of thing that goes on in optics turns out to be one of the more precise physical phenomenon.

Now it's a good point here to put in that optical design was an obvious computer application from the very earliest days, because of the fact it's one of the few things that require large numbers of calculations. Where there is sufficient commercial value to doing it that you, you 'know you're not just doing it for a hobby or occasionally but doing it pretty steadily, and want to do it economically and rapidly in order to beat out your competitors and put a new lens on the market quickly, and where the system that you're simulating or calculating is as close as nature provides to reality. In other words, if you calculate a lens and say it's going to be a good one, and then make it very precisely to that

design, it will do what you predicted it would do. Now if you build something like a hydraulic pump it may perform within one percent of what you calculate. If you build something like an airplane wing, it may perform within three or four percent of what you calculated. I'm trying to draw examples from 1940-1945 not from the present day. But if you design a lens, it will perform the way you calculate to the precision with which you can make optics, which turns out with the long development of optical manufacturing to be about five places.

You can make the curvature of the lens accurate to one part in ten thousand very easily, and one part in one-hundred thousand with considerable effort. And, in fact, a typical radius for a photographic lens in an expensive camera will be specified to the 100th of a millimeter and will be like - that's one part in five thousand for typical curvature. So it's a natural place for computing to be used. And as I watched Alice Barskey grinding away, day after day, on these lens designs and when I ground away hour after hour on these prism design calculations. I couldn't help remembering how nice it would be to have that punch-card equipment in the Naval Observatory or other mechanizations available to help me.

So, here we are, we're now ...

MERTZ:

You left the Navy...

GROSCH:

I'd left the Navy in ...

MERTZ:

'43 in the spring...

GROSCH:

In the spring of '43 and I'd left the Sperry for a better job in the Fall of '44. And we are now talking about, like March of '45, The war is still on...

MERTZ:

Six months later?

GROSCH:

Yeah, less than six months later. The war is still on; the bomb is still a secret...

MERTZ:

And your wife is commuting...

GROSCH:

My wife is commuting into New York City on the Long Island Railroad. Right. I'm driving to the Bronx.

The war is obviously turned. We're obviously going to win, but the finale is not upon us. Hitler is being pushed around rather badly in Europe, but he hasn't given up yet. And I read in something like March or April, I think it was probably March, I read in Science Magazine which I was still reading fairly frequently, (in fact quite regularly) -I read in Science Magazine that Wallace Eckert has left the Naval Observatory and is going to start a new Department with the International Business Machines Corporation.

Now, I should explain before I go on with this that Ferrand was an interesting place in more ways than one. I started to say that Tripp was a former engineer at Disney, Rosin was a more typical academic physicist-type that had converted to this lens-design work during the war, as I had in somewhat less precise fashion. But the head of the whole thing, G. L. Ferrand was the man who had, in his younger years, invented the dynamic speaker and had made a small private fortune by selling the patents and the rights to this electronic -electrical equipment - electrocoustical equipment. He'd invested his money and had used, of course, Navy and other Government contracts and facilities also, to set up this rather fancy factory in the Bronx, employing about a thousand people. Essentially, I would say an intellectual hobby, I don't mean in the sense that he didn't want it to make money or in the sense that he wasn't patriotic or anything, but he wanted to do something that would help the war and be fun.

The result was that, for instance, they had an entirely disproportionate level of technology for such a small company. It was the only company of its size in the United States making a medium based length range finder, for instance, the sort of thing that was giving Eastman Kodak fits. They were doing quite well. They were doing it almost as well as Bausch and Lomb and Kushman Esher, and much better than Eastman Kodak, which were the other sources for this kind of equipment. At the same time he was doing these very difficult optical simulator devices, for instance he had a gadget where you would sit in a cart and roll around on the terrazzo floor under computer-analog computer control. Coming out of the top of this cart -the inside of this cart being footed up like a fidroplane cockpit, -coming out of this cart was a modified submarine periscope which I was redesigning the prisms and stuff, which ran around close to the--to inverted relief map, like that relief map behind your head. And you used this thing to simulate the fighter plane or dive bomber attacks on enemy territory. You put a map of the enemy airport or the enemy harbor on the ceiling of this simulator room and ran around under it, at reduced speed of course, in this cart with the head prism simulating the path to be taken, the exit-prism and entrance-prism of the head prism, the entrance pupil of the head

prism simulating the position of the aircraft attacking this harbor or running into this mountainside. And these were very intricate and interesting devices and entirely different from the sort of things that most small companies were doing in those days. Most small companies were trying to make mass production lenses or cheap gadgets of some sort but the really sophisticated stuff was being done at places like Bausch and Lomb. But Ferrand was doing it too.

MERTZ:

Was all of his work defense related?

GROSCH:

At that time completely, yes. I think that he had behind the scenes a private project on electro statically focused electron microscopes. But it was the sort of thing that he was working on personally in his own private laboratory somewhere. And all the young men and all the production people were working head-over-heels on military work. The range finder was the most important thing. These simulators were all development contracts. The range finder was a production contract. But an extremely difficult one. They made all their own optics, some of which was extremely good, and they were building an interfarometer, for instance, I remember at the time of their own, which is one of the hardest things to build in optics. And they had invented a special production machine to hog-out deep parabola’s, deep parabolic mirrors. By deep I mean deep in the sense that an old 1920 automobile headlight was deep, not in the sense of being half an inch deep, but as deep as the diameter of the mirror practically. And they were going to use these in connection with infrared search equipment which - bolometer focusing and stuff -which in those days was even more sacred than radar, although it's quite common now-a-days.

So they had a lot of very interesting stuff going on. And to support all this, Ferrand used to subscribe to all the new books and all the good magazines and instead of having a formal library he'd just route them around to all the people he thought were interested.

So it wasn't surprising, to get back to my main theme, it wasn't surprising that I would see almost every day a copy of Science Magazine, even though I was sitting up there in a Bronx factory rather in an elaborate university library. In fact, I had much more intellectual freedom and a great deal more satisfying job at Ferrand than I had had at Sperry, in spite of the fact that Sperry was then twelve thousand people on Long Island and Ferrand was one thousand in the Bronx.

So, I did see this thing. I was encouraged by the investigative ambitions of people like Ferrand and his immediate administrators, to think that I might combine activities in some way with this new venture. So I wrote Wallace Eckert a letter, addressing it, as I remember to the Naval Observatory since there wasn't any better address given in this thing, saying - telling him what I'd been doing in the last year or two since I hadn't seen him for at least since my last Astronomical Society meeting a couple of years before, and

asking him if I could, perhaps, arrange to come down at night when he got his laboratory going and try out some ideal on an automatic lens design with his punch-card equipment.

Well, this is, I repeat, March or April of l945, we could pick it up, of course from Science Magazine what the exact publication date of this thing was. I had thought of - it had been mentioned that the facility would be at Columbia University, and had not given a name to it yet. And I had thought of it roughly speaking as the same sort of thing that he had at the Naval Observatory; one multiplying punch, one tabulator, one sorter, a couple of key punches and everybody ran his own equipment. Because it was clear that it was to be a laboratory and development operation rather than something grinding out the Air Almanac. I hadn't, of course, enough experience at executive thinking yet to realize that it was highly unlikely that he'd be permitted to do this in war-time; just to wander off into the night you know, and start a nice little laboratory. This is still the spring of 1945 and we're still fighting tooth and nail in Europe and not even having a complete success of the Pacific yet at all.

The fact of the matter was that while Eckert had wanted to do this all along and apparently Watson, Sr. had wanted to do this all along, that the impetus to do this had been a request from Los Alamos, which was at that time a deep secret. The name was not known, in fact the address was Post Office Box something-or-other, Santa Fe, New Mexico was all we knew as an address for it.

There had been contact between Los Alamos and IBM all through the end of the war and they had wanted IBM to undertake some additional calculations for them because they were just frankly running out of people to run their own IBM equipment at Los Alamos. I'll come to that equipment in a minute, but they had approached IBM and said "we would like to have you do some of the calculating, not just give us more equipment".

Having the contact with Wallace Eckert, Watson, Sr. probably with the help of John McPherson had reached out to Washington and had proposed to Eckert that they set up this laboratory to handle this requirement from Los Alamos then after the war to go on and be a facility at Columbia University to be shared by Columbia and IBM, How much of this was done at the very beginning, how much it was worked out during the Fall of '45 while I was busy actually doing work there, and too busy to think about it, I don't really know. Only Wallace Eckert and John McPherson knows any more, I should think. The old man's dead and most of the other people at IBM who were concerned have retired or died also. But Eckert would certainly know.

Anyhow, sometime between the beginning and the end of 1945 that plan was worked out. Now, when I wrote this letter to Eckert he already knew that the - that Los Alamos was in on the act. He had already talked to former friends of his on the Columbia faculty like I. I. Robbey. Well, present friends who had been former associates on the Columbia faculty like I. I. Robbey and others who knew all the details of what was going on at Los Alamos and were able, within limits of security, to tell them how important it all was and he'd already decided to leave the Navy. But he didn't have any employees. He didn't have a

single soul, not even a girl secretary to help him get started. My letter arrived out of the blue; I was his kind of person. He knew all about me; he knew of my deep interest in this computing thing; I had all sorts of high-level security clearances already so that there was no problem of security, and I wanted to do it. So the next thing I know, he sends a little man from the - what was then called the Manhattan Engineer District up to see Ferrand, himself, and tells Ferrand a little bit about what's going on. Ferrand, being a high level industrial executive, probably knew something about it already in some context or other. And I'm essentially drafted. It isn't quite as simple as that. There were a couple of little conversations back and forth between Eckert and me. But, as a matter of fact, in something under a week from the time that he received this letter, I was actually employed by the IBM Corporation and never went back to Ferrand in my whole life. I cleared out my desk and left and that was it. And in war-time you never did this. You had to arrange and dicker and dittle. (Garbled). Boom.

MERTZ:

This letter was forwarded then to Eckert in New York?

GROSCH:

It took a while to get all this going. I got an answer back from him almost immediately from Washington saying, "Herb, it's wonderful to hear from you, and I certainly think we ought to be able to work out something at least the sort that you described, but you've got to give me a little more time, we haven't got the arrangements all made." Then when he finally did get in touch with me and said "We are going to start on such and such a day, if you'd like to join up full-time and work on projects in the lab, we'd like to do it". That was in early May and I remember saying to him," Gee, Wallace, that's wonderful of you but I couldn't possibly get away. You know, I've got this to do and I've been there six months, and I've got that to do." And that's when the little man appeared, and a week later I'm at work at the Watson Lab.

MERTZ:

In May of...

GROSCH:

In May of '45, that's right. Still before the end of the war. Now, let's see if I can set the scene on this...

MERTZ:

You didn't have quite so far to commute now?

GROSCH:

No, this is Columbia now. And when I actually reported for duty I think I had...

MERTZ:

Long Island Railroad?

GROSCH:

Uh, yeah. Long Island Railroad. I used to go in with my wife, as a matter of fact, for quite a while. I used to then take the subway up from Penn Station Station one hundred and sixteenth street, walk into this shop. Now, what I found when I got there at first was that the old Astronomical Computing Bureau was still in existence, up on the top floor.

MERTZ:

Same building?

GROSCH:

Same building: Pupine Physics Lab. Lillian Haussmann, whom I had not met before, and Everett Yowell, the young man I mentioned were grinding away on these B-29 calculations, which I knew all about, as a matter of fact, because such tests would have been -were proposed for the Sperry Gyroscope equipment which didn't get the contract.

We were told that we would have some space on the tenth floor temporarily, while the new building, the building that had been assigned to the Watson Laboratories for permanent occupation was gotten ready. And that IBM was scouting around as hard as it could to produce some lower-level people to work for me. There didn't ever seem to be any discussion about whether or not I was going to run the shop. It was just taken for granted from the beginning that Eckert was going to be the top man and negotiate with the Watson's and that sort of a thing, and I was going to do the computing. So that was it.

The name was to be the Watson Scientific Computing Laboratory, not Thomas J., just Watson Scientific Computing Laboratory, not bureau. It was to be an enterprise of the Columbia University and IBM together. But the astronomical end of it had dropped out. Not only in the name, but in the arrangements. In fact, the Astronomical Computing Bureau was continued for about three more years after that - had an independent existence for about three more years after that (two anyhow). It was to be located, as I say, in Pupine, Eckert and I were to have what we would now call "Q" clearances. The word didn't exist then, but we were to have high-level disclosure of what we were to be doing. But all of the people working for us were not cleared and it was up to me to desensitize the work that we did so that it could "be done in an open shop without everything being locked up at night, and by people that were not investigated. That's right. For instance, Eckert and I were told that the unit of temperature was a million

degrees calvin(?) or something like that. But the people running the calculations didn't even know that it was temperature, let alone that the unit was a million degrees. If they had known it was temperature it could just as well been a hundred degrees because there was no inter-action obvious to a person who wasn't an expert in nuclear physics or fancy hydrogeneptics(?) to reveal this.

The work was brought to us; we didn't go out to Los Alamos. Perhaps Eckert made a trip but I don't believe so. And I never did visit - to this day I've never been to Los Alamos, in fact. The initial contacts were with Marsheck(?), who has since been President of the University of Rochester I think and is now coming into GGNY I think it is. The man behind him was Hans Boeta, whom I knew - who was his boss. And whom I knew through astronomical connections from before the war. And way up in heaven above Boeta was Oppenheimer. But I didn't even know Oppenheimer's name at that time. Boeta was present a couple of times in early discussion, but I don't believe I ever sat down with him, I think he and Eckert and Robbey had lunch at the Columbia Faculty Club a couple of times and it wasn't even clear that he was involved,. But Marsheck came to see me. And under Marsheck there were a whole bunch of people who were doing theoretical calculations at Los Alamos. Now at Los Alamos it turned out there was a substantial punch card installation which had a special device in its 601's. This was the same old calculating punch that Eckert had had one of in Washington and in modified advanced form, one of at the Astronomical Computing Bureau.

By the time that we started the Watson Lab in '45, the Astronomical Computing Bureau had added about five more of these and had a total of about six of them. One of them was this funny sequencing gadget which, in fact, they pushed into a corner and didn't use. And the other five were commercial 601 calculating punches like - which have been made by the thousands. But they had had added to them a little special circuit, not described in the main manual, which enabled them to take a count of the algebraic sine of the quantities. They still did A times B plus C plus D. But in now A, B, C, and D could either be plus or minus. An indication of whether they were plus or minus was an X punch in one of the top rows of the card.

MERTZ:

(Garbled) and an X and A Y punch both...

GROSCH:

Uh, the so called X punch was in the eleventh row, not the twelfth row. In fact, I think, in those days we didn't use the twelfth row for anything. You had to move little individual brushes around inside the machine to pick up those X punches which were on a., completely different circuit from the main brushes that picked up any one of the eighty holes of the main card. Later on that was eliminated, but in that early 601 version you had to have special little brushes for that. And these were modeled after the special little brushes that picked up X punches in a reproducing punch. The 514 reproducing punch,

for instance, had these little individual brushes and you would use that X punch to indicate whether it was a master card or not. If it was a master card you punched onto the next card, if it was - but you didn't let the last card punch onto the master card sort of thing, so - control sort of thing, they called them control punches - control brushes.

MERTZ:

And there was no use made of the twelfth...

GROSCH:

No use made of the twelfth in that machine. It was used in the tabulator and it was used in other machines, but it was not used in that machine. So they had five or six of these. Well, it turned out that Los Alamos had six of them also. In addition to that circuit. Los Alamos had a dividing circuit. So they were the first people so far as I know in the whole world to have a production IBM machine that divided.
I was not able to get that and the machines that were delivered to me, - I ended up with eight in the end - the machines that were delivered to me had the algebraic control circuit on them but not the dividing circuit. In fact, I never ran a 601 with divide in it.

MERTZ:

You didn't know about the Los Alamos people...?

GROSCH:

Uh, yes. By this time they began to tell us. After Marsheck had made a visit or two and I, as I say, I had met him slightly through Astrophysics, - after he made a visit or two. Then we had a couple of visits from Dick Fineman who sinc e won the Noble Prize, who was sort of an advisor on these calculations. And we had telephone conversations and I think one visit with the two me n that were running the installation at Los Alamos, these were Metropolis and Frenkle. Nicholas Metropolis later went to the University of Chicago and Frenkle had security troubles and ended up as a very well-known consultant in hardware, and helped design several of the electric data machines. He's still active, I believe. Metropolis is also still active but Metropolis kind of pooped out, something like Irving Goldstein sort of died but they didn't bury him, so he's still kicking around someplace, writing...

MERTZ:

(garbled)

GROSCH:

Institute of Metals in Chicago, the last time law him...

MERTZ:

But he still...(garbled)?

GROSCH:

Yes. Nevertheless, those guys did a yeoman job out there because, remember, the isolation - it wasn't just a matter of their having to do it with the burden of secrecy and all that, but where I was right in New York , the whole IBM Corporation was just a short subway ride away, they were doing this out on that damned secret... So to do what they did was really phenomenal.

Now, they had simply run out of expansion capability. They could have gotten more equipment, they could have run the machines I got out there instead, and I'm sure they could have gotten some physical space for them, but they just plain old had run out of Metropolis' and Frenkle's, _______ and Fineman's. So since they knew pretty what to do, they were essentially telling us what to do and we were implementing it.

However, they had the advantage that in my case I knew all the numerical analysis. And I learned in just a week or so what there was no…about the machines which were extremely simple then. Even aside from these little tricky things that you couldn't read up on and then someone like this chap Demmit, that I mentioned, at the Naval Observatory*- I think actually Dick was involved in one of these things, would just come in, you know, and give a little piece of glucoted(?) paper that told the story. So learning the machines was trivial, with the exception, perhaps, of the 077 collator which was always a complicated gadget. I never did understand that thoroughly. But with the rest of them, I learned the machines in a week.

I had the numerical analysis, which was the hardest thing, and I didn't need to know the physics - that had been done by the Fineman's and the Marsheck's before the problems were presented, It turned out that what we were doing was a calculation of the spherical shock, which was actually the theoretical prediction of what had happened in the Trinity Burst(?), the Trinity Experimental Burst. They'd had instrumentation on it; they made assumptions as to what happened. And we were calculating out from that assumption and ultimately, of course, would come to certain figures on pressure and temperature, should we say that we compared with their instrumentation. In close, of course, instrumentation is all vaporized, but far out they got some real readings. And we were making, I'm sure, several runs on this. It turned out that ours I believe was the final run. That is, it turned out that their guessed-at parameters were correct in our case and ours was the final run.

As I remember it, the time interval was a millisecond, but I'm not sure about that, it might have been a micro-second, I, of course had to turn in all the papers back in after the project was over. The few papers that had that sort of stuff on them were super secret and

had to be locked up in a vault in the physics building and could only be taken out to check on once in a while. But everything that laid around in Pupine was fine. There was no problem; you just left the cards sitting around. No problem at all* No FBI men, no locked doors.

MERTZ:

What about - did Finorman and. . .

GROSCH:

Finorman was in on this and this is when I first met Johnny. I had no idea of his interest in ENIAC and so forth. That was all secret in another compartment at that time. I didn't know ENIAC existed. But, I met Johnny and, of course, I knew him through his work in group theory and quanto-mechanics and so forth and respect him has one of the great brains of our time.

But he didn't appear as conspicuously as a gal named Ryonaire and also Ed Teller. Although, this was the main task that we had to do, Ryonaire and Teller brought in the next task to be performed as soon as we got done with this one. Well, this one ran into the fall and the war was over before we started the next one. And, in fact, it had much less priority as a consequence. But, in fact, it was a Teller project.

I'm not sure if it was hydrogen bomb, I've never known that much to this day, but it was a Teller project. And I had more to do with it through Maria Mayre, who I think also got the Nobel Prize a few years ago, one of the few American women to get it, than, I did von Neumann through Fineman and Marsheck.

The line, I think from Marsheck went lip through Boeta to Oppenheimer without going to Von Neumann. But I remember, for instance, that Johnny was the one that told me-that warned me about the instability rule and integration of elliptic partial differential equations, which I had not gotten Fredericks that summer that I told you about. Because it was numerical analysis rather than mathematics; and it essentially says that when the ratio of the time interval and the space interval is too big or too small, I forget what it is - there's a criterion for it - when it exceeds or is less than this critical limit that your assumption becomes unstable.

Well, as we ran through this punch card cycle to advance this expanding shell of shock wave one time step, one millisecond or whatever it was, and we rant through this. There had to be a desk calculation operation done by a girl on the sidelines, who didn't know what she was doing, just filling out one of those forms that I used to make and that I made for the purpose that told you essentially where to stop. This was called some kind of a "hugeneau shock(?)" or something, there's a professional name for it which I've since forgotten, And the position of this shock which was a fraction of a space interval. That fraction of which interval it was had to be hand calculated so that you'd stop when you

got to that point, and not go beyond it. And when it got to the certain point, you then had to change your space interval in order not to exceed or get less than this criterion this was written down at the bottom of the page, but the first time we came to it she didn't pay any attention to it and the next time interval was wrong and the one after that was wrong, and pretty soon plodding it up were voom, voom, voom.

So I called up Johnny--he was in Princeton in those days--and I called up Johnny in Princeton and remember a very wonderful conversation with him in which he explained that he told me all about this before, and I hadn't been looking. And I said "yes, I have, it's blah, blah, blah, blah, blah" and he said "that's right, now, he said, did you exceed it?" And I said, "Yes we did and he said "Now what are you calling me for?" And I said, "Listen, what I want to know is can I run a smoothing line through that, and replace - you know - replace it without backing off four steps," because each step took us a whole morning or a whole afternoon to do. We only made two steps a day. And I said, "It will save me a lot of trouble." And he said, "Nobody in the whole world, including me knows whether that will work or not. He says, "After the war we'll try it." He says, "Meanwhile, go back four steps and start over." (laughter). So I went back four steps and started over and it seems to me that Frenkle told me or Metropolis told me later that they had tried it at Los Alamos and that as far as they could tell that if you did it fairly quickly, it was an acceptable solution. But you have to do it pretty quickly. Maybe four steps would have been too much.

Anyhow, I remember that contact with Johnny, and of course, I remember him socially coming through once or twice and shaking hands all around, and we were all giving him a little ahh...because he was a great man even then, I didn't know Oppenheimer from a hole in the ground and wouldn't have been impressed. I knew Boeta was a big man, at least of Eckert's status or probably more so.

MERTZ:

(garbled) You never knew Fermi and the Chicago...

GROSCH:

I never met Fermi. Uh, I had - I saw him across the table at Faculty Club several times after that, but I never met him in the Watson Lab. Same with Oppenheimer, I saw Fermi, Oppenheimer, and Einstein at the Columbia Faculty Club but never met either one of them personally. Von Neumann, of course, I got to know later quite well, to the point that I invited him to my house a couple of times. I never succeeded in snaring him, but to the point that I felt free to do so. Boeta, Marsheck and company drifted off. They went off like Marya Mayre and so on and didn't come back. Teller I had no contact with for several years afterwards until he got to be kind of an anathema and then I tried to get him to do something or other, mentioned an IBM job I had in the late 50's and got the brush-off so ... But the important thing is that we were regarded as part of the Columbia family of relationship to the Atom Bomb project.

MERTZ:

(garbled)

GROSCH:

Yeah, I knew of Turing well, but that was because of his Columbia connection and not because of his Atom Bomb connection He had been a professor at Columbia and Eckert knew him as an old buddy. I mean, very close personal friendship. And so during the few months at the end of the war when he was still at Columbia before he went, I think it was to Chicago first, but then out to Loyola. I used to have lunch at the same table once a week anyhow, so I got to know a lot of them. And the important thing in that relationship was Eckert. Because Eckert was a full-fledged social member of the Columbia community. He was returning home in that sense of the word. He was not establishing something new. And as his right-hand man I was immediately welcomed to the physics and astronomy table at the Faculty Club, made a member of the Faculty Club the first week, and so forth.

I had the Ph D, and I had had a post-graduate student relationship at the Naval Observatory, so I was a certified young scientist. I had to keep my trap shut when Robbey and Boeta and people like that were talking. But, at least I was as good as a young instructor or a little better than a young instructor would have been. And that was an enormous advantage, because when the Watson Laboratory began to expand and we began to hire more people, most of them did not have Doctorate's, and most of them had not worked as full-fledged scientific types before the war or early in the war. They tended to be electrical Engineers in the Radiation Laboratory and so forth, and they tended to be somewhat pushed aside by the Columbia Faculty. Also, I had a lot of astronomy friends at Michigan and so, on who were close to the astronomers at Columbia.

The Chief Astronomer at Columbia was John Schult, another emigree dutchman like Brower and a very close friend of Broer-'s. And he had succeeded Eckert when Eckert went to Washington. And Schult regarded me as a member of the family, I mean, I was a young astronomer and he wanted to reclaim me for astronomy. In fact, as soon as he'd get me away from Eckert and these nasty people at IBM, why he was going to make me a real astronomer again. And, I think, probably for a few months I thought so, too. But I soon discovered that I wasn't going back.

MERTZ:

Now this was in - from about May in 1945

GROSCH:

Thru the end of the year, roughly. Everything I said happened in 1945 sometime, yes. It

would be difficult to reconstruct in detail just what followed what. Like, for instance in the fall of '45 you not only have the--want to stop?

MERTZ:

I think it might be a good point to stop because we're about to run out of tape,

GROSCH:

Want to stop for the day, too?

MERTZ:

And this concludes the second session and the second side of this tape.

[End of Interview]