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Interviewee: Herbert R. Grosch
Interviewer: Richard R. Mertz
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This is the second part of an interview conducted with Dr. Herbert Grosch in his office at the National Bureau of Standards in Gaithersburg, Maryland, on the 24th of August, 1970. The interviewer is Dr. Richard Mertz.

MERTZ:

Would you please continue with an account of your academic years in Michigan and Harvard?

GROSCH:

I'd like to talk just a little bit more about my intellectual and academic environment in general and then go back and pick up the increasingly important terms of computer work as it began for me in those years.

One of the major things that I want to mention is that I was interested, even in those years, in the social problems that nowadays are so prominent in the computer field. I read a lot of Lewis Mumford, The Cultural Cities kind of books, a lot of economics, a lot of history and so forth but on a basis that it was always oriented toward the technology sort of thing. Not the broadest ideas of humanism but stuff that, you know, science interacted with--city planning, city deterioration, rise of transportation facilities sort of thing. And, of course, a lot of this was due to the continuing interest in science fiction that I mentioned. While all of this was going on, I was still reading Amazing Stories, Astounding Stories and anything else I could get my hands on and I still had a very large collection of unbound science fiction magazines which gradually leaked away during the years and which I no longer have. But, at that time, I had them and reread them for ideas when summer vacation time permitted and things of this sort.

MERTZ:

Do you recall any particular authors?

GROSCH:

No, I can recall stories more than I can authors. Yes, there were early authors like a chap named Mauer, Jay Brewer and David Keller, whom I remember were sort of the harbingers of the social concern kind of science fiction. I remember a story, for instance, in which, by some mysterious means, the hero was introduced into a culture that was already in smooth existence, which seemed to be almost entirely a hedonistic one, from which the adult male members disappeared mysteriously on occasion. And he found by snooping around in back of this culture that these men had to go to the other end of the country and work in a huge, ugly, highly-automated, mechanical city with smokestacks belching and robots running around in the streets which was, of course, supplying the living materiel of the hedonistic culture at the other end of the country. And the snapper in all of this rather lengthy story was that after further investigation revealed that the work they did in the control rooms of this highly-automated city was, in fact, negligible; that they were pushing buttons, pulling levers, adjusting knobs and so forth but these were not connected. In fact, the giant mechanical brain was actually doing everything and just keeping these guys busy because it realized that they would break down under the strain if they realized they were completely worthless. And this has disappeared from the literature. There's no anthology, to the best of my knowledge, has picked this one up. It must date back to the early '30s and it was reasonably well done, although I'm sure it would appear rather crude in the context of the modern socially-concerned science fiction that we have today.

And I can remember a great many of the adventure type stories, too. The penetration into the South American jungle where one discovers a city with a mixture of scientific artifacts and ancient historical practices. Usually there's some human sacrifice and a beautiful, not very completely clothed girl involved in the thing somewhere. These, of course, have some of the flavor of Conan Doyle's <u>Lost World</u> but at that time I wasn't able to get my hands so much on the ancient classics of English fantastic and science fiction literature as I was on the current stuff that was coming out. And, in fact, it wasn't really until I went to Harvard in the summer of '39 and the summer of '40 and got access to the stacks of the Wiener Library that I discovered that one could read the complete meanderings of H. G. Wells, and Jules Verne and the science fiction stuff.

I, for instance, remember reading the (?) America of the Deep, which Conan Doyle's bathysphere story first at Harvard in the summer of 1939 not knowing that it even existed from my Amazing Stories of science stories kind of reading and later discovering that it had been reprinted in one of the early pre-Amazing Stories, Gernsbach magazines to which I had not had access because it was before I was 10 years old. Now a lot of those have been reprinted either as scientific curiosities or for their plain old fictional value but, in those days, they were very rare and you had to push them a bit.

Well, getting off the track, what I meant to say was that partly caused by the science fiction and partly by the social excitements of the time which included, of course, the Spanish Civil War, the considerable feeling among the more socially oriented students against the Hearst newspapers and that sort of things. I was immersed in the world but it didn't impinge on me nearly as much as it did people who were studying in the humanities. My own personal life was a very monastic one and I wasn't in any danger of being snapped up by the Lincoln Brigade and leaving the next day for the Spanish battlefields or anything like that. But, I was aware that others were very much concerned with reading the stories to this effect in the very good college newspaper, The Michigan Daily, and so forth, which I read religiously every day. The Detroit newspapers weren't much and the Ann Arbor newspaper was, of course, the normal small town deadly dull sort of thing. But I managed to increase my awareness of the world, an awareness that had started through debating in high school and so forth but not to a sharpness where I felt impelled to leave astronomy and go out with a flaming brand to cure the world. In fact, in those days I think almost everyone who was working in advanced scientific fields still had complete faith in the ability of science and technology to do everything. We were just damned sure that if we just did enough good astronomy, enough good physics, enough good chemistry that ultimately we'd create a human paradise, you know, just like the science fiction stories that I was reading and all we had to do was to make sure that the ugly old remnants of the pre-technological society were brushed aside. I don't think it's quite that simple any more but that's how it looked to me as a young man in my teens and early twenties.

I mentioned the Widener Library. One of the most attractive things that happened to me all through these years, and something I want to mention before going back to the computer scene proper, was the fact that I always had access to very good libraries. I mentioned that when I was in high school I used to have an adult card at the Detroit Public Library. I used to bring home great stacks of weird stuff. By weird, I mean things like the pharmacopoeia, books on naval architecture as distinguished from the things that I should normally have been interested in and, of course, great rafts of detective stories and so forth besides.

I graduated from that to the general library at the University of Michigan where I managed, after only I guess about my sophomore year, to get an unrestricted stack permit which I got through the Astronomy Department by the time that I was sort of a simulated graduate students as a sophomore.

So I always had access to open shelves of very, very good material. In fact, it improved my access in the sense of the Detroit Public Library was pretty much a call slip operation; just as a university library is for most undergraduates. Whereas when I got my stack permit, I was able to get right in there and browse around in the book shelves just like a graduate student in the humanities would. I remember being intensely disappointed to find that the pornographic and nearly pornographic, erotic material was all locked up as it tends to be in all large libraries and I spent a large part of my time trying to figure out how to get a permit to read that which, as I remember, I never succeeded in doing. But, with that exception I think I had pretty free access to just about everything in the university collection, which is one of the great university libraries of the country, and included rare books of the sort that interested me although not enormously. I never had the antiquarian interest.

When I went to Harvard in '39, one of the first things I tried to do, since I was living in the Harvard Yard at Weld Hall right across the way from the Widener Library, was to get a stack permit there and with the help of Harlow Shappley who was in a sense my host, although pretty far up above my poor graduate student level as perhaps the best known astronomer in the country at the time. Harlow Shappley helped me get this by filling out a couple of appropriate pieces of paper so that entire summer of eight or ten weeks, I had unrestricted access to the stacks in the Widener. The course work I was taking out at the Garden Street Harvard College Observatory was not so demanding that I couldn't spend a good deal of my time roaming around in those stacks. I remember reading a great deal of mountaineering, for instance.

This was at a time in which my actual access to mountains was zero but it was I think probably the year in which I began to be interested in reading about climbing mountains and others who had climbed mountains. And, of course, they had magnificent geographical files in the Widener including many, many expedition records and complete sets of the Alpine Journal and so forth that one couldn't get even in the University of Michigan General Library. So that was an interest and I remember also reading a great deal about fancy bookbinding which is something I never actually did but which I always thought of as being something I'd like to do with my own hands some day. You remember in the previous interview I mentioned that while I always have been excited about books that I was never particularly interested in the antiquarian or first edition kind of thing but I have been interested in the fancy binding and beautiful edition sort of thing. And, in fact, somewhere around the early '40s joined the Heritage Club, for instance, for several years until it became too popular and not sufficiently specialized to continue to attract me.

Well, those are some of the general intellectual influences that I was under at the time. They were mostly rather solitary; mostly they were things I could do myself. I didn't share many of them with Allen Maxwell, for instance, who tended to be extremely narrow in his interests and to care only about eating, sleeping--both of them on a rather low level--and doing his astronomy work. And he, in turn, used to tell me as his favorite and really only graduate student that I'd have to work a great deal harder and spend much more of my time if I hoped to climb the esoteric heights of theoretical astronomy. I think that probably I agreed with him in those days. I no longer do but I probably agreed with him in those days but I simply found these other intellectual diversions sufficiently exciting that I did them anyhow. However, between the astronomy, and the mathematics, and the fancy courses I was taking, and the summer schools, and these other intellectual interests, I didn't have any time left over for social stuff which was just as well because I wasn't all that attractive socially at the time anyhow. I didn't have much money. I didn't have much of a way with girls and I wasn't interested much in drinking or at all in smoking. The usual social graces had so far at least passed me by. So I didn't have more than 24 hours a day to invest and I managed to invest them in the ways that I've outlined.

Now I'd like to go back to the computing side of it. You remember that I said that I started writing papers and doing calculations with Professor Maxwell in 1936 and that, in fact, I published my first paper as junior author with him in that year on an asteroid that had wandered in close to the earth and had caused a good deal of excitement among the orbit computer people. Well, at that time we were both using pretty much printed tables--six place logarithms, six place logarithms, addition and subtraction logarithms, a few specialized printed tables that were required by the works of theoretical astronomy. The equivalent of programming in those days consisted of drawing up forms which you would fill in as a result of specific calculations. So that, for instance, the calculation of the orbit of a comet might consist of a series of perhaps 20 or 30 hectograph pages made up of perhaps eight or 10 different kinds of forms, some of which were repeated on more than one page. And with blanks left for the specific numbers to be evolved in the course of a particular orbit. Well, you'd get the observations of a newly discovered comet, for instance, by telegraph. In our case, usually from the Harvard College Observatory. You fill these in on the appropriate early blanks on the form and then with the, following the directions implicit in the form, saying take line 6 and multiply it by line 9 then subtract line 11 and fill it in on line 12, you would gradually work through this form until at the end you would get a calculation of the elements and predicted future positions of this comet. This was called elements and ephemeredes of the thing.

Now in a sense this was a program and as we today are always tinkering with computer programs so, in those days, we were always tinkering with our forms. One of the things that I did most for Maxwell as his team, undergraduate and graduate student, was to draw up more and more of these forms for him. Throw away the old ones, unused in many cases. We were always making a hundred of something, using the first three copies and then revising it which required, of course, throwing away the other 97 or 98. He was exceptionally good at doing this sort of work and I still have many of his old computations and informal books of notes that he prepared for his students, which are lavishly illustrated and such stuff. I learned from that not only a considerable degree of precision in actually performing these hand computations but the idea of preparing in advance and revising meticulously the schedule of calculations. This is very different from the sort of thing that many other people doing hand computing in those days used to do. It was quite conventional, you know, to just scratch down numbers on a blank sheet of paper, making a few notes as you went along to remind you of what the numbers were for. And then, of course, when you got to the bottom and found that your answers were not correct, you then faced an almost insuperable task of finding the mistakes. On the contrary, when you have a printed form of this sort with everything labeled and the specific directions given on how to do something, it's then possible for a second person to check the calculations on the first person. And, again, one of the things that Maxwell and I were always doing was making parallel calculations on identical forms, in many cases for a newly discovered comet or something like that. Checking back and forth every few minutes to see if our numbers still agreed within reasonable limit. And this idea of check calculations, duplication, of the acceptable degree of error between two repetitions and so forth, of course, run all the way through modern scientific computation but were a rather esoteric sort of thing in those days. And, remember, I was learning this at a time when I was not yet 20 years old so that it got me a good head start. Got me a felling for accurate manipulation of data which came in, for instance, extremely handily later on when we were trying to do extremely complex calculations with primitive tools, like punch-card equipment, because in punch-card machines again the plug board wiring and the careful handling of small decks of complex card inputs and outputs was not a very different thing from the kind of precise hand computation that I was learning in the late '30s.

Also, in order to do this sort of work economically, it was necessary to use the most advanced numerical analysis techniques that Maxwell was aware of. And he was always reaching out for new articles on computing methods, including some that were written by statistical people and so forth, not just astronomers. Although, in general, he did not penetrate into partial differential equations or anything of that sort. But he had, for instance, taken courses while he was a student at Berkeley from Charlick (?), the well-known Swedish statistical astronomer, and Charlick (?) who wrote books in celestial mechanics, which were what he had been originally exposed to, also had written books on statistical calculation of stellar motions and things of this sort. And this gave Maxwell an interest in statistical calculations and in accuracy. He didn't follow it up himself but he passed it on to me as part of what I should know as a student. And then I, in turn, took statistical courses where indeed there were still other elementary forms of hand computation. I began to be aware of the fact that there was administrative data processing done in the University of Michigan on old-fashioned punch-card machines, and, in fact, at that time became acquainted very slightly with a young professor named Dwyer, who later wrote several rather important books on linear arithmetic--algebraic calculations with matrices and determinants and so forth--which he was beginning to think about as a consequence of having access to statistical and administrative data processing on these other punch-card machines. But, aside from a couple of times visiting the room in which this work was done in Weld Hall, I had no knowledge of the use of machines at that time for astronomy work.

Now, as I was learning to be a desk calculator man; a logarithm table calculator; a constructor of tables also of small specialized sorts; I was at the same time, of course, required by my course work and urged on by Maxwell in my intellectual interests of becoming acquainted in a more general literature of astronomy and particularly celestial mechanics. Well, this involved, for instance, learning not only the work being done in the United States which was not very extensive at that time--most of the American astronomy being astrophysics--but also work that had been done in other countries. And I remember starting out, for instance, to translate the first volume of LaPlace's <u>Machinique Celeste</u> (?) only to discover that it had been done 100 years before by Baldage(?) but that there was no copy of the ...translation in the Michigan library system. In fact, I saw it a couple of years later at the Widener in Harvard or, I guess, more precisely at the Harvard College Observatory Library out on Garden Street. So, you know, these sort of things keep a young man busy and excited.

In the process of probing around in what the British were doing, I encountered articles in the monthly notices of the Royal Astronomical Society, Published in the late '30s and just before I left Michigan in about 1940, describing the use of ... (?), that is the equivalent of IBM, punch card equipment at the Greenwich Observatory to verify and utilize the grounds, tables and the motion of the moon. Now this was an enormous set of printed tables, filling several volumes; which had in turn been calculated by hand by Professor E. W. Brown of Yale in the early '30s. And there was some question of whether there were perhaps small errors left in these tables and also the work of interpolating positions of the moon from these tables was an enormously painful and difficult one which had to be done each year for very many dates, as I remember it, for several times during each day because the moon's motion is so irregular. So that they had to do it perhaps, I'm just guessing, four times a day for each day of the year before publishing the ephemeris. I discovered at this time that the great national observatories--like the Naval Observatory in Washington and the Greenwich Observatory in England and, to somewhat lesser extent, the French and German National Observatories--each did a part of the work of calculating these annual ephemeredes and then they exchanged them. They reset in type in each country to form a national volume but, in fact, one country would be responsible for calculating the motion of the sun, another for calculating the motion of the moon and so forth so that the work was not duplicated. In fact, there was even some exchange with the Russians and so forth.

Now Comerie had undertaken in the late '30s to do all the lunar calculations for many years ahead from these tables predicting the motion of the moon and finding this an enormous task to do with desk calculators and logarithm tables had either rented or borrowed--I think he actually paid rent but not on a continuing basis--had actually gotten a set of tabulating and sorting and keypunching machines from the British Tabulating Machine Company; which was then the sort of independent British company associated, however, with a patent exchange sort of way with IBM which exploited ...the patents in the United Kingdom. And he had used this small set of machines, which probably cost him, you know, only a hundred or 200 pounds a month rent. If, indeed, he paid rent, he'd used these machines to do a big one shot job on the motion of the moon and had published his methodology and the precision and economy of the results in the monthly notices of the Royal Astronomical Society. Moreover, in these articles, he expressed excitement about the fact that this was also being explored in the United States.

Well, I began to poke around at this and discovered that there had been published around 1940 a book called Punch card Methods in Scientific Calculation by a young astronomer named Wallace J. Eckert. Now it turned out that was a graduate student who had studied under Brown--remember the man who had done these tables of the motion of the moon at Yale--and had then come as a young instructor to Columbia University. He still retained, of course, many of his intellectual links to Yale, just as I was retaining mine to the University of Michigan, and had hoped to duplicate some of this work of…And I discovered that right at the end of my stay at Michigan--just the last year that I was there, which would be in 1940, after I had come back from my first year, my first summer, at Harvard--that this book was in print and could be secured. I finally managed to get to the local book store to get me a copy and, in later years, Eckert always laugh and said that I was the only person, the only private individual that had ever bought a copy. Because the number of people who were interested in the subject at that time was, of course, numbered in the dozens other than libraries and observatories and so forth. The libraries and observatories, of course, had bought copies when it was reviewed in the appropriate places. But all the individuals who were otherwise interested in it, he'd given copies to them. I was the only individual that he knew of in the whole world that had actually been unknown to him and had actually had to buy a copy. And I still have that one to this day. It was, I suppose you might say, my first book on mechanical computing. The first one that I ever acquired. It was not, by a long shot, the first one written but it was the first one that I personally had held in my hands and, certainly, the first one that I ever bought with my own money.

Now, that was an interesting time when I think of how few people were excited about it then and how hundreds of thousands are now excited in even just the scientific side of it. It's hard to remember that it was only, you know, years ago.

Now I mentioned that we used to get our astronomical observations, our notices of the new astronomical discoveries that could be computed by these desk calculator methods that Maxwell and I used from Harvard College Observatory. This was one reason why I wanted to go to Harvard in '39 and '40. By this time I had met, at astronomical meetings and so forth, the men who sent out these observations. The two men, who were, in a sense, rivals of Maxwell and Grosch--although somewhat superior to us in their access to information and their academic standing--were named Whipple and Cunningham. Fred Whipple, who today is the Director of the Smithsonian Astrophysical Observatory, was at that time a young assistant professor or instructor at Harvard College Observatory. And Cunningham was his disciple in the same way that I was Maxwell's disciple.

Since they always had a few hours start on us, always getting these observations from the Japanese, the Austrians, the Russians and what have you, by cable and then distributing them to the other observatories in the United States, they almost always managed to beat us in the calculation of the orbits. Their methods were as good as ours, they had a head start and I always was inclined to think that Cunningham was also a better desk calculator operator than I was or at least a harder worker. But, we always managed to put this aside and assume that the real reason was that they had a head start and, moreover, if we managed to beat them and send in the results first that they'd hold them up until theirs were also ready and publish them simultaneously. So, you see the Watson double helix kind of thing even extended into the very, very, very dim realms of orbit computing.

And in the late '30s, there were others working in this rather specialized area. Such calculations were performed in England by people working for…and Greenwich. They were performed by an individual named Paul Herget at the University of Cincinnati Observatory and will appear in this story extensively at later dates. And all of these people were even known to me by correspondence or by personal meeting at astronomical and regional neighborhood astronomical meetings in the '30s. When…came to this country, for instance, I had an opportunity to meet him at one of these meetings and several other English astronomers who were aware of his work. So even in those days, I had begun to acquire small international association.

One of the great attractions of astronomy and, of course, of the other more theoretical sciences also was that it was heavily international. Even with the war coming, even with the tensions in Germany and Spain and so forth, astronomers all considered themselves to be members of a great international family and one wrote back and forth without the least hesitation to Observatory in Russia or to the Cape Observatory in South Africa without thinking for the moment that there was any political tensions.

In fact, as I began work on my doctoral thesis, I remember receiving some observations or some information about how to reduce some observations from the Cape Observatory to which the letter had been opened by either the South African or the British censor before it reached us.

And this was the first time I'd ever seen military censorship on a piece of correspondence. That must have been 1940 because it obviously would not have happened until the British were at war, but it was before Pearl Harbor. So the world impinged but it didn't impinge very much. The international flavor was strong. I had begun to meet not only orbit computing-type people but I had become aware of competition in my own area overseas.

When I began to search around for a topic for a doctoral dissertation, for instance, I began first of all to calculate the orbits of two newly discovered satellites of Jupiter--Jupiter 10 and 11, which had been discovered by an astronomer named Seth Nicholson at the Mount Wilson Observatory with the big 100 inch telescope. One, in those days, corresponded very carefully before choosing such a topic. I remember when I went to Harvard in 1939, lived in Weld Hall and went out to meet Shappley and others for the first time that one of the things that I was interested in reporting on was that I had found that there was a case of multiple solutions in calculating the orbit of this--I think it was the tenth satellite of Jupiter--and I wondered if this might make a suitable doctoral subject. However, I began to get hints from Whipple and Cunningham that others were working on this also. And, finally tracked it down through them and through Nicholson at Mount Wilson to discover that Paul Herget the man at the University of Cincinnati, whom I knew quite well by then, was intending to keep on working on these and would probably not welcome the intrusion of a junior person into his regime. So at the suggestion of Dirk Brower with whom I was taking a course that summer at Harvard, I then wrote Nicholson again and asked him if I could perhaps work up the much larger ... of observations of Jupiter's eighth satellite, which he had discovered as a much younger man 30 years before but which was at least temporarily lost because it was such a highly perturbed object that all of the orbits finally began to wander off and at the moment, because it was so faint, so distant and so infrequently observed, we didn't have any idea where Jupiter 8 was. suggested that this would be a good idea and, in fact, that was what I chose in the end as a very long and tedious, but rather rewarding, dissertation.

I mention this not only because there was rivalry within the United States but I discovered that the next best orbit of Jupiter's eighth satellite, the one which was no longer sufficient to recover the object each year at the opposition of Jupiter, had been worked up by a woman astronomer at Leningrad Observatory. So here were the jolly old

Russians and here were published papers in the Cyrillic alphabet, which I was unable to read, and a general flavor of internationalism which attracted me.

MERTZ:

Was this a temporary study?

GROSCH:

Yes. That had been done--I'd have to look at my thesis now to see when but that had been done in the early '30s and it had been sufficient to observe the satellite again at two or three years. And then as it later turned out, from my calculations, there had been a period in which the attraction of the sun was a larger percentage of the attraction of Jupiter than usual. And, as a result, there had been higher perturbations and the old orbit that this woman at Leningrad had worked on, this old orbit had gotten away so to speak and no one had been able to find the object for a while. The gal's name was Boeva, I remember, and the work had been incompletely described even when I had the Russian description translated, why it wasn't clear what intervals she had used in her calculations and so forth. So I was pretty much on my own and it made a suitably independent topic but it also made one that involved thousands and thousands of hours of desk calculator operation, more than I really understood when I started out on it. And, in fact, the tenth or eleventh satellites would have made much nicer topics because having been just discovered, there weren't very many observations and you didn't have to extend the orbit very far in either direction in time to cover all those observations and get the most that you could possibly get out of the astronomical material.

MERTZ:

How many observations had been made?

GROSCH:

I think of the tenth and eleventh satellites, there are only a dozen or so each. Of the eighth satellite, I remember collecting a large file drawer full on those. Some of which I never got around to using.

I think, let's see, I've gotten my story a little distorted. I remember now, thinking back on it, that the eighth satellite was not discovered by Nicholson. He had indeed discovered the tenth and eleventh and, as a young student, he had discovered the ninth but the eighth had been discovered some years back at a European observatory, Greenwich or something like that. So there was a long series. He had contributed to the series in the middle and, as the discoverer of the new satellites, was the world authority on the subject. Nevertheless, he had not discovered Jupiter 8. It was discovered back in 1908 or something in Greenwich or some such place. But it could only be observed photographically. It's a little chunk of rock, extremely highly perturbed and right out of the limit of Jupiter's gravitational field. In fact, it remains, I think to this day, the most highly perturbed regular satellite of the solar system; that is, not counting little chunks of rock that no one's discovered yet and not counting artificial satellites.

And, incidentally, this was one of the reasons why when the space program came along, I had some immediate interest and access to it because much of that work on preliminary orbit calculation, and especially the orbit calculations for satellites, was highly specialized. There were only a few people in the world that knew anything about it. Of whom, Herget and I were two and presumably on the Russian side, this girl, this woman Boeva was one. Although I never saw any connection in the literature between her and the Russian space program. But, there were certainly only a few of us and it made for an immediate interaction between me and the space program from the beginning.

Well, I'm wandering again but now I should mention this man Brower because he is a link between the astronomy at the Watson Scientific Computing Lab at Columbia in later days. Brower was the inheritor of Brown's celestial mechanics theoretical work at Yale. Eckert who had started to be still more junior member of this coterie, in going to Columbia had somewhat attenuated the link. Brower who was a Dutch astronomer who had immigrated here from Holland, ... as I remember, became Brown's number 2 man and ultimately, years later, the head of the department after Brown's death and he remained the most important man in the United States and I guess ultimately in the world in theoretical celestial mechanics.

Who was junior also to…as well as to Brown became more and more concerned with the implementation of this theoretical celestial mechanics in practical ways by the construction of tables and by the use of punch-card and large computers.

MERTZ:

Was there much work done at Yerkes?

GROSCH:

No, Yerkes Observatory--I had, of course, connections with them. It was at that time almost all astrophysics. They were doing extraordinarily complex numerical calculations on the structure of the interior of stars. This work was being done by a Hindu named who is still alive and still very important today. And the English authority on this, something corresponding to the man ... in the celestial mechanics and theoretical astronomy that I mentioned was the famous Sir Arthur Eddington. And Eddington's internal constitution of the stars and ... corresponding book on stellar constitution were two of the Bibles that I studied in day and night in my non-Maxwell work at the University of Michigan.

For a year or so I seriously considered not going into orbit computing because it did seem such a dull and unpopular part of astronomy to most people, if not to me. And going instead into the stellar constitution work and, in fact, I had a very good seminar in this with a chap named Louie Hopkins, whose connection with celestial mechanics was that he had been a student of the famous F. R. Molten of the University of Chicago in the early days.

He was only teaching it, however, not doing research in it because by this time he was dean of the summer school at the University of Michigan and this was a full time administrative job except for a couple of seminars a year. So I took celestial mechanics from him as an undergraduate, again a couple of years early, and then the seminar on stellar constitution and relativity theory later on. And he was a useful, positive influence on me but not as intimate a one as Maxwell. He, in turn, tied me back to Molten whom I met only once or twice, he was then very old and died during this period. Molten was the man, who during World War I, had worked out most of the hand calculating methods for ballistics calculations at Aberdeen Proving Ground. These hand calculating methods, being used at the beginning of World War II, then transferred to punch-card machines because it was so much to do and then when punch-card machines and hand calculating was not sufficient generating the contract under which ENIAC and the major relay calculators was later designed at the end of World War II.

So…in that very long and circuitous route brings you into it. And my connection with him, as I say, was through Hopkins and through astronomy meetings which he attended shortly before his death.

MERTZ:

Did the people at Harvard have any particular influence on your selection of your thesis topic?

GROSCH:

I think in the sense that I discussed it with…and Cunningham as co-workers and rivals but they were not themselves interested in that particular part of the orbit computing business. They were interested more in asteroids and comets. And this was due to the fact that…had a continuing interest in meteorites. He was at that time, in addition to his computing stuff that I've already mentioned, he was writing books on the physical kind of meteorite thing and operating the patrol cameras which Harvard College Observatory used to look for random meteorites in the sky and to record meteor showers. So he was at that time one of the world authorities in meteor business and meteors are connected with asteroids and comets, not with satellites of Jupiter. So he had much interest in the things that Maxwell and I were doing, less interest in what I was doing directly. But it was a small, tightly-knit group. There weren't, I suppose, 20 or 30 people in the whole world earning their living in this orbit computing sort of thing counting the Russians and the French and everybody. A few Englishmen, a few Americans and that's about it, a few Dutchmen, a few Germans so everybody was pretty much aware of what everyone else was doing. This was how I was informed that ... wanted to hang on to Jupiter 10 for instance and all it took was a quick, one paragraph letter to him to verify this fact. The kind of fluency that we have today in a much larger community in which one would pick up a long distance phone and call him simply didn't exist in those days. The thought of making a $3 or $4 telephone call from Harvard to Cincinnati to ask him whether or not he was going to go on with this would never have occurred to me. You could have hung me up by my heels like a Smithfield ham and it would never have occurred to me. Simply too much money, too informal and I didn't need to know that fast. Things are different now. Now we get him on the real time computer link and type a message to him on a time sharing system, I suppose.

Well, …was certainly an interesting person and he gave an advanced course in celestial mechanics at this Harvard Summer School thing in 1939 and it was to attend that that I made my preparations and it was with the money that I had saved from this large $1000 fellowship that I'd had the year before at Michigan that I was able to do this. I got no support from Harvard that year. I took, at that time, an exceedingly interesting course just to sort of fill out the summer in the aberration theory of geometrical optics from a very brilliant young man named James G. Baker, who also appears very briefly in the computer history a little further down the road as being the first person to ever put an honest practical problem on ... Mark I IBM automatic sequence calculator. He later became the world's greatest optical systems designer, president of the optical society, advisor to many large commercial companies and he's still a very vigorous and active force in optics to this day. But, at this time, he was a member of the Society of Fellows, a graduate student at Harvard and one of the few people in the world interested in higher orbital (?) optical aberrations and this, in turn, required a great deal of desk calculator kind of calculation so that he was an expert at doing this and had learned many of his practical tricks at doing it. As I had learned from Maxwell, he had learned them from Whipple and Cunningham. Although he himself was not interested in orbit computing but he was a member of the astronomy graduate student team and, in fact, ultimately wrote his, had intended to write his thesis in astrophysics, not in optical design at all, but he had acquired this specialized knowledge of optical calculations. Because there was an element of calculating in it, because I was always interested in the instruments of astronomy as a summer observer and so on, I'd used them and I found there, in the science fiction thing also, pulled me into it. You know science fiction heroes always had access to 1000 inch telescopes or electronic versions thereof. So that I had an interest in the subject and I did take this course also from him.

MERTZ:

Were these fairly large courses in terms of …

GROSCH:

This was a thing called, as I remember it, the Harvard--the word square was in it somewhere--Hall of Square Conferences in Astronomy or something. And the intention was that they limited attendance. This wasn't very serious because there weren't very many people who could afford to come actually. But, they limited attendance to the number of people who could sit around a large hall or square conference table and sort of operate on an overgrown seminar basis. It wasn't as small as a typical graduate student seminar but it wasn't as large as a big lecture series either and there was an opportunity for give and take. Now, Brower, who was very much of European temperament, pretty much lectured solidly and you just took notes and were good boys and girls. But Baker, who was himself a graduate student, actually no younger than many of the people around the table, and there were, by the way, senior people at these meetings, too. They weren't all graduate students by a long shot. In fact, I would say that the graduate students were probably only 30 or 40 percent of the total. Most of them were young professors or, as far as that goes, professors, senior professors from smaller colleges who were trying to bring themselves up to speed in the typical re-education way that we do today. Well, this group in Baker's class really did interact with him and there was considerable give and take during it except that the subject matter was so specialized and esoteric that most of us didn't have much to say at the beginning until he got further into it. As he got further into it, he began to tell us more about the new kinds of optical instruments for astronomy, especially Schmidt telescopes, and his own particular advanced version of the Schmidt telescope system, which was just beginning to be discussed in the world of astronomy and hardly at all in optics but which became very important in later years and also important during the war for certain military instrumentation. So that was a favorable interest and here I had access to Brower, I had access to Baker. I met, for the first time on a continuing daily basis, the Whipple-Cunningham group at Harvard and I had a chance to meet Shappley and Menzel (?), the senior people who were all very kind to young people in those days. There were fewer of us and we were precious to them. We were going to carry on their tradition and it was very nice indeed.

Moreover, there was a feeling, there was sort of a monastic feeling about it all because nobody was getting rich at astronomy in those days. Shappley probably had the highest salary in the whole field, in the whole United States and maybe in the whole world and I'm sure that he wasn't getting any more than a good Harvard professor of history got which was twice what a Michigan professor got but still not enough to go out and buy a yacht by a long shot. And the people from the smaller schools, the people who were not so high in the profession, like Maxwell, and the people who were breaking in, like me, all assumed that we would never become rich; never become able to really maintain the kind of living that a used car salesman could maintain. We hoped, only at best, to be like professors.

Now that gives you a feeling of unity, too. You're all going to be poor and dedicated together so we'd play Shappley's Gilbert and Sullivan records and eat inexpensively together and, all in all, have a very fine time. A time which still goes on, of course, in fortunate university environments but which has been lost in many others. The…of poverty, so to speak, disappeared many years ago; is only being resumed involuntarily by…

MERTZ:

Along with that of being depressed (?).

GROSCH:

Exactly so.

[End of Tape 3, Side 1]

[Start Tape 3, Side 2]

GROSCH:

Well, I went back from that wonderful summer in 1939, a summer with a great deal of intellectual excitement and not only because I'd met Brower and Baker and had these two wonderful courses, met all these extremely distinguished Eastern astronomers and foreign astronomers and so forth but also, as I said at the beginning of the other tape, from the pleasant reading I'd done in the Widener, the mountaineering books, the bookbinding books, the volumes of Wells and Verne that I had not been able to get before and a general aura of pleasant intellectual activity.

I went back and plunged full-time into my doctoral work. I think by that time I had finished all of my course work, even the disastrous Quantum Mechanics course and so forth. Aside from a seminar or two, I really wasn't doing anything with teachers anymore. I was working 10 hours a day on Maxwell's desk calculator grinding out the orbit of Jupiter's eighth satellite.

MERTZ:

You had completed a language requirement at that time?

GROSCH:

I was never very good at languages and it's been a continuing hindrance to me since. However, in those days, they were very generous with you. If you could read a little bit in your subject matter field in French and German, they would pass you and I just about managed to squeeze through, just minimally. I'd been--the only really bad grades I'd had as an undergraduate were in German and I hadn't been very good in French in high school. And I really wasn't much in it, unfortunately. I think this was something I should have driven myself to and during the time that I was interested in stellar constitution, I did indeed read in French and German a little bit. But when I dropped back in this orbiting computing thing, it was essentially my own work that was involved, not very much of other people's work and it was more important to grind away eight, 10, 12 hours a day to figure out how to economize by better numerical analysis, to figure out how to draw up a better computing form than it was to keep up my reading in foreign languages so I just barely got through.

MERTZ:

You had mentioned that you had jointly with Maxwell published quite early on in your student days at Michigan a paper. Did this relate to a series of orbital computations or comments?

GROSCH:

The reason that we published this in fairly formal fashion was that it was an unusually interesting object. It was an asteroid called the Delport (?) Object, named after the Royal Observatory of Belgium. A man at I'll call the Royal Observatory of Belgium, who had discovered it, which turned out after the orbit was calculated to make at that time the closest approach any celestial object had made to the earth. So Maxwell published the orbit computations that we did on this in the formal publications of the observatory of the University of Michigan, a series which was mostly devoted to astrophysics. And that came out, as I say, in early '36, the work we'd done in the fall of '35 and early '36. But most of the things we did after that, that was the very first object that I did much work on; the thing that he broke me in with. After that, we did mostly preliminary orbits of comets, newly discovered objects--not following them with great precision through their whole career but just figuring out where they were going to go so that people could pick them up and continue to photograph them. And these were mostly published in so-called Harvard Announcement Cards. These were literally postcards printed at Harvard Observatory--some of which gave the observations of newly discovered objects and others of which gave the orbital elements and ephemeredes predictions resulting from calculations and these were put out pretty much by Whipple and Cunningham.

MERTZ:

Whence arose the competition?

GROSCH:

Exactly so. But this was nevertheless a formal method of publication. It was listed in the annual publications indices of the astronomical profession, The Astro;(?), and it counted in one's academic record. This was just as well as any other form of publication would. It was the proper form of publication for preliminary orbits and I had a series of perhaps seven or eight of those over the next three or four years with Maxwell. And then afterwards, when I left his sheltering bosom and went off to the Naval Observatory, I published one or two independently, still in this form. In addition to this, these calculations would frequently be reprinted in somewhat more detail in, oh, semi-popular publications like Popular Astronomy, for instance, which was read by astronomers but was really a teaching magazine rather than an original research publication. By the same token, much of the work done in Great Britain of this sort was published first in these Harvard Announcement Cards or their international equivalent which were printed in

Paris--I'm now vague on that. There was an International Astronomical Union Announcement series on a white postcard matching the Harvard buff postcard which, of course, because of steamer mail being used in these days, usually came a week or so late for our purposes. They would occasionally reprint what we did and that would occasionally also carry the calculations of French or German or English astronomers. And these in turn would be reprinted in more detail in things like the Journal of the British Astronomical Association, which was the amateur and teaching society, parallel to the Royal Astronomical Society which was the one the hot shots belonged to. The Royal Astronomical Society had no useful method of publishing things like preliminary comet orbits because their publications were too slow as well as too dignified. So, there was an equivalence on the two sides of this.

So, my first eight or ten publications were first ness (?) after this asteroid one in the publications of the University of Michigan Observatory and then eight or ten divided between the Harvard Announcement Cards and Popular Astronomy.

Now my thesis, of course, was to be published in something more important than that if I could help it. It was conventional in those days to publish one's Ph.D. thesis in the astronomical journal, which was the non-astrophysics professional journal for the United States, or in the astrophysical journal, which was the one for all the others, if you could get it in. Failing that, you might publish it in your own observatory's publications where you could always get it in. If they gave you the degree, they'd certainly publish your thesis. But I was ambitious from the beginning to have mine published in the astronomical journal. And, indeed, with some delay due to the war, I finally ended up by doing so. It meant that you had to write it twice because having written the big, thick thesis and having it accepted by the graduate faculty and getting your degree, you then had to cut it down in size so that it would be attractive to a professional journal which didn't have much dough.

MERTZ:

You mentioned that this took an enormous number of computations which you performed on a hand machine, Marchant (?)…

GROSCH:

An ACTIOM…Calculator. I can remember it to this day. I'm sure I could identify it if I found it in a used shop.

MERTZ:

How did this or does this relate in any way to the impact of this book on punch-cards and…

GROSCH:

A very cogent question.

It had an enormous connection because as I sat there doing these repetitive calculations…most of this, by the way, consisted of numerical egression of the differential equations of motion of this perturbed object so you were making a step in time. I'd gotten it to the point where with very, very elaborate preparations, I could make one of these steps in 18 or 20 minutes. While these steps carried the satellite ahead 10 days, or sometimes back 10 days, in time, but when the object got very highly perturbed, I'd sometimes have to do it over two or three times. I was very conscious of the fact that with this punch-card equipment Eckert (?) could have done something of this sort for families of asteroids and had made these steps in a couple of minutes apiece using punch-card equipment and he described this process very completely in the book so that there was no question that I could have duplicated it if I had been at Columbia. And, of course, the thought of just picking up and going to Columbia did occur to me but I was dependent on the University of Michigan for my fellowships and so on. I was, in a sense, a captive although not, in any way, an unhappy captive of my circumstances.

Moreover, looking back at it, I'm sure that I didn't feel that--had this sophisticated observation at that time. I'm sure looking back at it I would not have taken any less time because I would have had to do, what we would now call, the programming which, in that case, would have involved figuring out a very, very detailed sequence of operations on punch-card equipment and wiring many punch-card plug boards. And, there would have been simple financial hindrances. For instance, in order to do this sort of work on punch-card equipment, as I found out nearly 10 years later--six or seven years later, to be more precise--one has to have these boards pre-wired. That is, you have to have a series of these plug boards sitting around, each one with a certain pattern wired into it with a large number of plug wires and these boards, at that time, cost $50 to $100 a piece.

Now, in work that I did for Los Alamos later on, we had 28 plug boards wired up, sitting on shelves that we switched around from machine to machine to get our time down. Well, where would I have found $1400 worth of plug boards. I didn't know that much detail about it at the time and, of course, the plug boards at Eckert's laboratory at Columbia were presumably free. But, there were probably only a dozen of them, too, and he probably had 10 of them to use for his own work. So I probably wouldn't have saved a moment if I had gone somewhere else but I frequently say in speeches and in reminiscences today that I could now do that 2500 hours of computing in one under a minute on a modern machine but that it would probably require 2500 hours of programming.

MERTZ:

What led you to the National Observatory?

GROSCH:

Well, first of all, it was already obvious to me that the only place in the country that was doing any extensive amount of research in all forms of old-fashioned astronomy was the Naval Observatory .... Yale was doing the fancy work in celestial mechanics. There was still a tradition of orbit computing at Berkeley, and I should mention that, by the way, before I forget it, and two or three other places. But, in each one of these cases, with the exception of Yale, I guess I should say, in each one of these cases, the old-fashioned astronomy was submerged in a much larger mass of astrophysics and modern physics and so forth, or cosmogony or what have you. Yale was an exception but Yale was clearly not only very theoretical celestial mechanics in which I was not yet very well-trained but it was also a very narrow milieu. There was enough money for Brower and a couple of disciples and that was it. And Brower and his disciples were there already. You could count them. The chances of getting an instructorship, for instance, at Yale I'd already assessed to be about zero to four decimal places. There just weren't any opportunities.

The Naval Observatory, on the other hand, had a very large staff of dozens of people who were doing all kinds of old-fashioned astronomy. They were making star catalogs. They were preparing this huge annual ephemeris. They had a very powerful photographic telescope with which, for instance, one could pick up, as I did later, two satellite. They had the time service with world-famous time measuring machines. They were developing crystal flocks. Almost everything they did was old-fashioned. In fact, astrophysics was done hardly at all and what little was done was subsidiary to the main one. So it was a natural thing.

Another thing is they were paying about $200 a year more than colleges were in those days. An unmarried instructor got about $1600 a year for an academic year in those days. A married one might hope for $1800. The Naval Observatory was paying $2000.

MERTZ:

How many trained astronomers did they have?

GROSCH:

Oh, I would say 25 anyhow. We could, of course, go back and count them. Yes, they had, I would say, not only in the plurality but probably the majority of old-fashioned astronomers in the United States in that one place. Now, in addition to that, there were a fairly large number of people who were quasi-professional who were life workers at the Naval Observatory but who were not recognized in the astronomy profession as being real astronomers simply because they didn't publish, they didn't have Ph.D.'s. But, they were, nevertheless, if you counted fairly routine old-fashioned astronomy, they were doing a great deal of it. So, you had actually a group of more like 50 people who were working at, what you might call, directed and undirected astronomical work. I'm not counting stenographers and people who just, you know, tend calculating machines without knowing what the numbers meant. There were many of those in addition. So, it was a very substantial establishment--one of the two or three biggest astronomical enclaves in the world. And I've always been attracted to large organizations. I've always felt you have more scope, more things that you can draw on in a large organization than in a small one although, indeed, the restrictions on your doing so are, of course, considerable, as I found out when I got to the Naval Observatory.

But, I think at this stage of the game I wasn't really thinking too hard about it. I was really still thinking about getting that damn thesis out of the way, grinding away another 1000 hours or so. This somewhat changed when I went to Harvard the next summer because I had enjoyed the '39 summer enormously and, while I no longer could afford to go entirely at my own expense this second summer, I still wanted to go back. I was getting awfully tired of Jupiter 8 for one thing. So I wrote Harlow Shappley--I think I still have the letter, as a matter of fact--and asked him if he could help and was given a very small tuition sort of a fellowship and a fee to make a public lecture. A series of public lectures were given each summer and I think the fee was like $50. But, you know, $50 was a lot of money in those days. I could live for several weeks on $50 so I think I got my tuition and that $50 fee, as I remember, and that by the way was the first money I ever earned making speeches. I had many, many, many very juicy honorariums since then but that was my first so I remember that with great pleasure. And I remember Harlow Shappley and his kindnesses with very great pleasure also. Considering that I was not one of his students and that I was working in a rather old-fashioned astronomy under a man that he didn't have much connection with, I was treated very, very well indeed. Of course, I'd been there the year before. They knew who I was and how I acted.

This time I could not afford to stay in Weld Hall in the Yard and had to stay in an ordinary boarding house but it was a pleasant place.

I said that it made a difference in the way I regarded my thesis. Well, the main difference was that one of Nicholson's people had come from Mount Wilson for the 1940 summer. This was a girl named Dorothy Carlson, who was one of the people who measured spectrographic and direct photography plates at Mount Wilson and did a certain amount of desk calculator work. You wouldn't call her a full-time computer in the modern sense of the word but she was an astronomical assistant at the Pasadena offices in the Mount Wilson Observatory. And she'd decided to spend her summer there urged on partly by Nicholson who was aware of how much fun I had had the summer before. So she came at her own expense and we went around together during most of the summer and, in fact, I married her a year later. While this, of course, made a difference to my personal plans, I was no longer content to go off somewhere where, you know, there was wonderful astronomy being done. I wanted to be able to take Dorothy along also. So, for that reason also the extra few hundred dollars a year of the Naval Observatory opportunity attracted.

You also have to remember that we're talking about the year 1940, the fall of 1940 and the spring of 1941 and there were very, very few jobs available at the professional level for young astronomers in the whole world at that time. This was still the end of the war. Before the war on the United States, Europe, of course, was at war but in the United

States, there were still considerable remnants of the Depression. So I considered myself very fortunate to do well on this competitive examination and ultimately to be offered a position. I think I ranked second in the United States of all those who took the exam. This was an old-fashioned civil service exam but one that was specialized--(question by RM inaudible)

Competitive examination in astronomy, not a general knowledge thing in astronomy. You had to do calculations with funny old logarithm tables and so forth at which naturally I was much better than anyone else who took the exam. And answer technical questions about observing methods and so forth at which I was no better than a good student or at least equally good. So I did well on the exam and was in the end offered a job.

Now, by this time something else had happened behind the scenes and I was slightly, but not completely, aware of it. And that is that as a result of the initial work that Eckert had done on his punch-card equipment at the Columbia University laboratories, he had been brought down to Washington. And I think this is a good time to tell that story. I'll finish off my own little part of it by saying that I got my appointment to the Naval Observatory in something like May 1941. This was just before Pearl Harbor. I had finished all of my calculations for the thesis and I had drawn most of my graphs and I had gotten Maxwell's approval on what I'd done and it was enough to publish. But I hadn't written the thing and this is a fairly substantial effort in itself. I had to pull together, what actually turned out altogether to be, nearly three year's work, 2500 hours, as I say, of calculations and so forth. And this was obviously going to be hard to do but I preferred to take the job at the Naval Observatory, get married--which happened in August--and then finish my thesis in after hours. The Naval Observatory, in those days, wouldn't permit you to do such work during the day but Dorothy, of course, was going to be a big help to me since she was a member of the profession also and, in fact, did all the typing and that sort of work for me. So it looked to me like something that I could go ahead and do so I left Michigan, as I remember, a month before the end of the semester, giving up one month of my ultimate fellowship, university fellowship, to do it. I went to the Naval Observatory and settled in, began work there in mid-May 1941.

MERTZ:

Did your wife return to Pasadena?

GROSCH:

She went back to Pasadena at the end of the summer I went out to visit her the following winter, the winter of '40, '41, and had a wonderful time at her parent's house at Pasadena. We got engaged then and made our plans to marry as soon as I could get something going. I had no thought of coming to California at that time. It looked like the Promised Land to me in 1940 but there was nothing in Southern California in my line of business.

MERTZ:

(Something about Mount Wilson)

GROSCH:

Mount Wilson had no people of the sort that I wanted to be at all. They were entirely astrophysical. Nicholson was an observational ... astronomer taking direct photographs and doing things like this satellite business but he was only permitted to do so really because they wanted little of that done and because he was a very senior member of the staff but they weren't taking on any youngsters to do it. They wanted youngsters like Jessie Greenstein and people of that sort, not like Herb Grosch.

So I thought of Berkeley--one thing that I should mention before going on to the Columbia Naval Observatory story is that Maxwell, you remember, had come from Berkeley. And at Berkeley he had studied with an old German astronomer named Neuschner (?). Well, Neuschner remained in close at Berkeley after Maxwell left, got his degree and left, and had built up around him a small group of old-fashioned astronomers, of whom Maxwell had been a temporary member. It stayed down in Berkeley and it kept alive pretty much by teaching astronomy where the fancier astronomers of the University of California lived up on Mount Hamilton at Lick (?) Observatory. They could not teach at the University because it was an all day trip to get down from the observatory to Berkeley. And, in fact, only a few years ago have they actually moved down even to San Jose. They now work in San Jose and go off on the mountain at night in the same way that the Mount Wilson astronomers and the Palomar astronomers do. In those days, they actually lived 365 days a year up on top of the mountain so there had to be a separate group teaching and Neuschner…

MERTZ:

You couldn't locate the group at all?

GROSCH:

In final practice, it wasn't done. In final practice what happened is that all the fancy, modern astronomers went upstairs where the fancy equipment was and all the old-fashioned ones, who could work with paper and pencil, stayed down in Berkeley.
And a member of that group--Neuschner, of course, died about this time--but a member of this group was a man named Samuel Harrick. And Harrick ultimately became fairly active in space work, became a consultant, is still a consultant, in fact, to some of the California aerospace companies and was one of the early influences in the business.

One of Harrick's associates was a man named Ernest Clair Bower, who was sort of an unsuccessful astronomer. One who, although I believe he had the doctorate --he had not been able to do very good research or somehow or other hadn't directed himself right--he had tried to join the Lick (?) Observatory group and, in fact, his few publications were publications of the Lick Observatory. But, one of the things was that Bower, like me, had picked up this excitement of Comerie's punch-card work and he had published, and I had read, two or three things of the Lick Observatory publications on how he would use punch-card equipment to do certain rather advanced astronomical calculations if he had some. He didn't have any. So, it was pretty hypothetical. Nevertheless, his ideas were good.

Now to jump around a little bit. One reason for mentioning this is not just that Harrick went into aerospace part-time and is still a well-known figure in international space society stuff and has written a couple of books on the specialized celestial mechanics of artificial satellites and that sort of thing. But, in addition to that, this man Bower was the first person to do technical computing in the aeronautical industry later on. He was the first man to actually use punch-card equipment for aeronautical calculations at Douglas Aircraft, as a guess, perhaps 1943. So that little group of people doing what I was doing at Michigan, what Herget was doing at Cincinnati and what Whipple and Cunningham were doing at Harvard. All of these, in some way or another, contributed to the early days of the computer business. I might also say…

MERTZ:

Did you know any of those people?

GROSCH:

I knew all of these people. Now I met Bower for the first time when I went out to get engaged in the winter '40-'41.

MERTZ:

Well, you went up to San…

GROSCH:

Nicholson --no, by this time, he had come down and was a lecturer at the Griffith Planetarium. I told you he was sort of an unsuccessful astronomer. He'd given up his connection with Lick where he had published these things that I had known about and was a lecturer at Griffith Planetarium, which is considered a second-class job for astronomers even in those days. Nicholson had told me I should meet him and so Dorothy, who was working partly for Nicholson in those days, took me across to Griffith, which I wanted to see anyhow. It was my first planetarium. They were pretty scarce in those days. Second planetarium, I had seen the one in Chicago. So I met him and we talked about this. I remember he was banged up from having had an accident climbing Mount Popocatepetl. So that was also the first astronomer I met who was a mountain climber; an unsuccessful mountain climber, in this case, since he had fallen off or whatever it is one does on Popo.

Anyhow, that was a friendly relationship which didn't mature into anything very important but which continued gently for many years thereafter. I believe Bower is now dead; I'm not positive of that.

I might end up the Berkeley thing by saying that in addition to that, the man Cunningham, whom you remember I said in informal conversation was the man who wrote the specifications to which ENIAC and the two big relay computers at Aberdeen were built. He left Aberdeen at the end of the war and went to Berkeley, not back to Harvard but went to Berkeley. And to this day, sort of carries on that Neuschner tradition of orbit computing and old-fashioned astronomy at the teaching department of astronomy in Berkeley so that's what has happened to Cunningham, so to speak. I think he has access to some small computers out there. I think he even ran them for a while but has sort of backed away from that and now simply uses them. I've lost track of Cunningham, I must say.

I might mention that this man Cunningham, who has an important role to play in the early days of computers but who is very, very little known. There are just a few dozen people in the whole world that know he even existed in that field really. Cunningham was a self-taught, a strange self-taught individual. He was, at the time I met him in 1939 --met him face to face for the first time in 1939 --he was a bachelor and I believe, I think he remained a bachelor. I'm not sure. He was a large man, blond, plump. He had been, I think, a low level technical worker for the telephone company in Massachusetts. This is beginning to be a little vague in my mind. One, of course, could find out from Whipple. But he had, somehow or other, become interested as an amateur in astronomy; never had a college degree or anything like that. Where most people who became interested in amateur astronomy either became veritable star observers, which is very dull stuff, indeed; something like bird watching to the minus fourth power. Or went out and made amateur telescopes and started to do observing in their own right or both. He had, somehow or other, gotten interested in calculation and had apprenticed himself in an informal fashion to this man Whipple, who was a very distinguished, professional astronomer. A younger man than Cunningham, as a matter of fact, and had, as I say, become a disciple of Whipple's in the way that I had become, much more formally, a disciple of Maxwell's. So he had a strange background. It was not a common way, even in those days, to get into a science. In fact, to a certain extent I suppose it was less common then than it is now because the astronomy profession was a very narrow one then. There was no space enthusiasm. There were no major rewards in it and there weren't very many places where you could do it. So, there didn't tend to be many amateurs that succeeded in becoming professionals.

MERTZ:

And he wound up at Berkeley?

GROSCH:

And he wound up as a professional in Berkeley. I believe in the end, I think he managed to get himself some kind of a doctorate. At the end of the war, people who had done distinguished service in an intellectual environment for their war work sometimes were given sort of almost degrees by acclamation. An outstanding example of that being the man Baker that I mentioned. Baker during the war became pretty universally accepted as the best optical designer in the United States, if not in the whole world. Did a great deal of very original thinking. Was responsible for some very unusual gadgets, including some that used artificial fluoride as optical elements and all sorts of very difficult things. And when he went back to Harvard and tried to get his degree, they essentially just sort of, you know, struck him on the shoulder with a sword and said, "Arise, Doctor Baker." It isn't quite that simple but he was certainly not required --they took some of his secret oral reports in lieu of a thesis or something like that. And, I believe Cunningham got some sort of consideration of this sort later but I'm not absolutely sure of that. Maxwell was not a good source of information and as I got deeper and deeper into --of gossip type information --and as I got deeper and deeper into the computer field, my connections even with him began to get pretty slim so I've never been absolutely sure.

Well, let's go back to the main theme…

MERTZ:

That was the winter then of 1941?

GROSCH:

That was the winter of '40-'41 and then in the spring, I went to the Naval Observatory and, so to speak, never came back.

MERTZ:

(something about Ann Arbor)

GROSCH:

Came from Ann Arbor. I'd already decided that I was going to get married in the summer so my parents gave me their old used one, bought a new…as a wedding present and I drove down with all my belongings, intellectual and otherwise.

MERTZ:

Well, it was a premature wedding present?

GROSCH:

Right. A premature wedding present. It saved me the problem of getting all my stuff to Washington. Nowadays, as you can see by looking around this office, I carry a good deal of intellectual apparatus around with me. Well, it was true in those days, too, but only relatively, of course.

MERTZ:

You wouldn't get it all in one car.

GROSCH:

I wouldn't get it all in one car now, no, indeed, I wouldn't.

MERTZ:

Well, then your fiancée was…

GROSCH:

She stayed at Mount Wilson, had her showers and her girl-type operations there and joined me in late July, then we got married here in Washington. Neither of us had our parents with us at the time. She had a friend who had left California where she had known… She had gone to Berkeley and had come to the Naval Observatory so this friend, Mr. … and his wife stood up for us. It was just a very simple, informal wedding at the August Anna Lutheran Church, as I remember. I only go to churches to get married. You can delete that or not, if you choose.

MERTZ:

That's not necessary. One doesn't need to go to church.

GROSCH:

No, it isn't indeed. But I always seem to marry people that want to go to church. Although, they also turn out only to go to church to get married.

MERTZ:

Did your wife then also have a job?

GROSCH:

She did not originally. It was our intention that she would work almost full-time helping with my thesis but, in fact, once the war started she came here to the Bureau of Standards and worked for a year and one-half here in a way that involved me later on more deeply in optics. You remember I worked in optics during the war and this pulled me higher up in the optics profession because the people that she worked for here at the Bureau of Standards were high up in the optical society professionally so they pulled me up as a friend of the family, so to speak.

MERTZ:

But you were, at that point, working at the Naval Observatory?

GROSCH:

That's right and she would come in the evenings, for instance…

MERTZ:

(something about position)

GROSCH:

I was junior astronomer in the astrographic department. The astrographic department being the one that actually operated the observing equipment --big telescope, the big Ritchey-Cranian (?) telescope, the big 26-inch retractor and a couple of smaller instruments.

MERTZ:

Did you have a rating of some sort?

GROSCH:

P-l, junior astronomer, I suppose that's about the equivalent of a GS-5 now.

MERTZ:

(comment inaudible)

GROSCH:

Now I want to back off and describe what I would like to have done there although I didn't actually succeed in doing it. I would like to have worked in the nautical almanac office, which was the largest department at the observatory. And the reason for that is that this is where Wallace Eckert now was.

Now when he became a disciple of Brown and read these articles by Comerie, he became quite excited about the use of punch-card machines. When he became an instructor at Columbia, he discovered that Thomas J. Watson, Sr. was a trustee of Columbia and, of course, he knew that punch-card machines came from IBM in this country. And, actually, in fact, Watson, Sr. was a member of the Board of Directors at BTM (?) in England from whom Comerie had rented his machines so there was no question about his being in Watson's company just as it is today. So, although he was only a young and rather quiet academic, he managed to promote a relationship as follows. At Columbia University, the astronomical society and the IBM Corporation would go together on an experimental punch-card laboratory to be called the Thomas J. Watson Astronomical Computing Bureau. This is the precise and exact title. It gets blurred in many documents that have been published since. Thomas J. Watson Astronomical Computing Bureau and this is described, among other things, in the appendix to this book. He got this going in the late '30s and the book was written afterward. This, as you can imagine, consisted of Columbia providing the sponsorship, the astronomical society asking IBM to do something about it and IBM giving the equipment, just as frequently is today.

A very early venture, on IBM's part, in the support of scientific work. However, not the first and I think here I should mention the existence of old Doctor Ben Wood of Columbia. Also probably because of Watson's earlier previous connection as a trustee with Columbia, Ben Wood had asked for help in the statistical vicinity, so to speak, some years before and, in fact, in the late '20s or early '30s was given a specialized tabulating machine, one that had been modified in rather complicated ways --what would then be considered complicated ways --and had used this for work at teachers college and in Columbia University proper on statistics. It was a specially modified machine but not an interestingly modified machine. By that I mean it didn't perform long sequences of operations or do anything that we would call computer-like now. It simply had more capabilities than the average tabulator.

MERTZ:

Roughly was this the late '20s?

GROSCH:

Late '20s or early '30s. It's in the literature and I can give you references to it. Some of the elements of it are described in a very old book called Punch-cards in Universities and Colleges, which you probably haven't seen but which is a curious old tome. But, this is not that far back. This was published in '35 and here's Ben Wood down here as the author of one of the articles. I haven't read that…for 20 years and I don't know how much it describes of the thing but I have other references also of what he did.

Now Columbia and Watson, Sr. were used to working together, you see, in this way. So when another kind of professor came along and asked for something a little more elegant and when I'm sure Watson checked with BTM to see what had been done in Britain, the thing fell together pretty well and IBM indeed said yes. Now they furnished a set of equipment to Wallace. This included what was then called a horizontal tabulator. That was a funny old tabulator that had visible dials and you could actually see your numbers flipping through and storing up in the totals of these counters. The counters are not nearly as flexible as they are today and there were all sorts of curious, old-fashioned limitations on the machines. These were machines that had been designed in the '20s and used practically without modification from that time on. The number of that machine was the 285 and I still have descriptions of it in my files. He had more modern keypunching equipment and so forth.

For the calculating device, he had what was then called a 601 calculating punch, which was a device that read a punch-card end-wise in a modified form of the keypunch bed but then having read the numbers off the card, the card was held before going under the punch unit. The card was held, calculations were performed in electromechanical counters inside the machine and then the result was punched on the same card in a pre-determined field as the card was elected and the next one was fed. Now this 601 calculating punch was the only thing in the IBM line at that time that actually multiplied. Everything else added, subtracted, listed and so forth but didn't actually multiply.

MERTZ:

What kinds of things could that do while it was being held --multiply?

GROSCH:

Well, it could add, subtract and multiply. It could do a very small sequence of these things. For instance, it could multiply two numbers together and add a couple more numbers to the product or subtract a couple of numbers from the product.

MERTZ:

It could do more than one?

GROSCH:

Yes but a very limited number of things. For instance, there was practically no storage in the machine. You had four or five specialized counters into which parts of the calculations were done. If you hooked them together in certain ways through plug board control, they would do additions and subtractions. In other ways, they would do multiplications. If you were very ingenious, you could get them to do maybe a couple of each. Only one multiplication, I shouldn't have said that, only one multiplication but maybe a couple of other additions and subtractions.

However, the choice of addition or subtraction was pre-determined. You could do an A times B plus C minus D maybe. But if you wanted to do an A plus B plus C plus D you had to change a wire on the plug board.

Now the numbers that a man like Eckert wanted to use, of course, were likely to have algebraic signs.

MERTZ:

(something about multipliers)

GROSCH:

Exactly. You could, for instance, do minus A times B but you had to change a wire to do make it minus AB as distinguished from plus AB. There was no control of the algebraic signs of the quantities whatsoever. It was assumed that the numbers were always positive. This was because they were developed, of course, for accounting purposes. The typical use of this machine was for inventory, for instance. Or for payroll.

MERTZ:

Could you necessarily vary the multiplier?

GROSCH:

Yes but not from card to card. Only by changing the wires in the plug board.

MERTZ:

You had to rewire the plug board?

GROSCH:

Right.

MERTZ:

So, in other words, you must put in a particular batch of cards to form a uniform?

GROSCH:

Right.

MERTZ:

Whatever it was, the same thing was done to all cards?

GROSCH:

Right. For instance, suppose that you were trying to do A times B, plus C and plus D, but that you knew that a, b, c, and d might be positive or might be negative, as they frequently would be in scientific work. The way you'd have to handle that would be to sort the thing down into 16 different piles according to whether a, b, c, and d were positive or negative. Now some of them could be combined. For instance, A minus A, A plus B, could be combined with the plus A and the minus B and the plus A and the plus B and the minus A and the minus B could be combined because of the algebraic sign rule but you would still have to make 16 piles to start with and then recombine them in certain ways and then rewire the plug board as each small package went through.

MERTZ:

(inaudible)

GROSCH:

That's right and remember this is all batch, in a very restricted sense of the word, batch. You not only have to do it all at once and bring the work to the machine but you had to run all the calculations through on a given plug board and then change the plug board and then run them all through the next plug board and so forth.

MERTZ:

Well, this would hold the card and then would automatically…

GROSCH:

It would hold it for quite a while. If the computation was lengthy, it might hold the card for as long as six or even 10 seconds.

MERTZ:

And then its release will trigger the next one?

GROSCH:

Right. It was almost impossible to hold a result over from card to card except in the sense that you could, by not clearing one of the counters, you could have an accumulating total, for instance, punched onto the card.

MERTZ:

Oh, you could?

GROSCH:

You could. But then when you got done, you had to clear the counter manually, for instance. And there was, in fact, some buttons on the outside of the machine so that if you were accumulating something in a counter, you actually when you were done, you'd push this button and the machine would clear while you watched it go flop, flop, flop, it would clear. And then you wouldn't have to rewire the plug board to clear it, so to speak, and send one card through.

Now in order for Eckert to do complicated work…

MERTZ:

Do you know how many cards it could run?

GROSCH:

A typical operation for a fairly complicated scientific work was 600 cards an hour, six seconds a piece. I sure can. I remember vividly. I later ran dozens of them.

MERTZ:

I was wondering if there was a quantitative limit to the machine itself?

GROSCH:

I can tell you everything you want to know about the machine. I've got detailed operating manuals for it.

MERTZ:

I see. Was there any way of verifying?

GROSCH:

No, to verify you could either go back and do it exchanging fields around or if you mistrusted, for instance, the fact that you'd still be picking up the numbers under the same brushes for instance. Each of these little wire brushes that read the card, the best way to do it was really not to try and do it on one machine but have two machines. And, in fact, although Eckert only had one, all the labs that I ever ran for him later on always had anywhere from six to eight of these machines at once. Two of them were always broken down. Sometimes all six or eight but you could always have identical plug boards and if your plug boards had been wired --you know, if you had two plug boards, each of which had the same thing on it then you weren't even worried about there being a defective wire in the plug board or something. You have to be very thoughtful about this. For instance, if you only had one machine, one way you could check was to put the card in with column one and column 80 interchanged. Then you could or in the case of the 601 --I'm sorry that's not right --row 12 and row 9 interchanged and then crisscross the wires on the plug board so that they would pick up a nine and read it as if it were a 12, this would have the effect of having a second run of the cards use different brushes to pick up the same holes. So you could do very complex checks if you had to but it took a great deal of very messy, ugly…

MERTZ:

Are these cards?

GROSCH:

No, these are the old-fashioned 80-column cards. The same kind you see today.

MERTZ:

Oh, I see but those still have an angular cut, don't they?

GROSCH:

Well, that wasn't used in those machines. It was an 81st column that you could use that thing for but that was mainly used so that you could jog and make sure you didn't have some backwards.

MERTZ:

You didn't know about…

GROSCH:

The machine didn't know if you put it in backwards. You did but they didn't.

MERTZ:

No attempt was made to assign anything like a sign?

GROSCH:

Well, yes, but for tabulating purposes. In a tabulator, for instance, you could make a count of what we called an X-punch --a punch above the zero position --which would indicate that this was a credit instead of a debit and you could then control the tabulator to subtract instead of add. But no such feature was then available on the 601. The 601 was assumed to be used for payroll or for inventory where all numbers were positive.

Now when Eckert tried to use it in the late '30s and 1940, of course, he had to have not only this negative capability but he wanted to do several things in a row so IBM modified his machine and, in fact, I think it's in one of the museums --one of IBM's museums today. They put a vertical shaft in between the legs of the machine where there was some empty space which contained a whole bunch of circuit breakers. Now circuit breakers in that old class of IBM machine was a big disk of Formica, Bakelite, this is what I want, with bumps on it, the cam design. And as this shaft rotated, the bumps opened and closed a relay; a substantial circuit breaker type contact. And by wiring from the plug board to the individual relays on this stack of cams, changing the cams on the shaft and rotating them on the shaft with a set screw, you could get certain circuits on the plug board to open or close at certain times not necessarily during the course of one card. You could, for instance, have the thing rotate slowly so that it would only rotate once every three cards or something like that. Then you could replicate what you wanted to do on every card three times around the cam but when you wanted to do on every card three times around the cam but when you wanted to do something over a cycle of three cards, you just had one notch on the cam and only every third card would have something happen.

MERTZ:

This is obviating and necessitating rewiring the plug board?

GROSCH:

That's right and this was to the best of my knowledge the first time that IBM made a true sequence control for any of their machines. And I'm knocking now --of course, it's a question of definitions. Obviously, these circuit breakers just came out of a standard old machine anyhow and in that machine they were also doing sequences. It was always a same card sort of thing and it was built in to the machine in its original design and never modified and you couldn't rotate the cams on the shafts except as a maintenance thing, you know, to make it timed more precisely. So I think it's fair to say that that was really IBM's first sequence control calculator. This is a long time before the Mark I, you understand, and it was, in that sense, a landmark machine although certainly not one that you'd think of as being as important, any acronym like that, but it was a landmark machine. And Eckert had the only one in the whole world and I'm sure that Comerie would have liked one in England and so forth. But there wasn't the community of people interested in this thing to pass this kind of word around. For instance, I didn't realize, until I got the book, that this laboratory even existed, let alone that he had what equipment he had in it, let alone that he had this special modification. By the way, the machine is described --that I don't have the detailed description of although Wallace Eckert probably does. But there's a short description of it in the back of The Punch-card

Methods and Scientific Computation book so by the time I bought the book, I knew it existed.

Well, he was quite successful with this. Not only was the book published but he got several good publications in the astronomical journal and so forth…

MERTZ:

I was going to ask you what the product was?

GROSCH:

There was a product. But before the product really became broadly known because, you know, the publication route is a slow one even today and it was very slow in those days, especially as no one was really watching, so to speak, except Comerie and Herget and a few people like that. Before this became really very well known IBM…

[End of Tape 3, Side 2]

[Start Tape 4, Side 1]

This is interview #3 conducted with Dr. Herbert Grosch in his office at the National Bureau of Standards on the 24th of August 1970.

GROSCH:

We were talking, you will remember, about the Watson laboratory and I said that we were now getting to the point where we could begin to talk about people and equipment rather than just the original project.

We had started out almost entirely with conventional punch-card machines. The only unusual thing being this algebraic sign control circuit that I mentioned. By the end of the first month, that is, by the end of June, we were not only fully under way with the actual calculations but we'd acquired some people. Of those who had later influence in the computer field, I think the most important is a Miss Marjorie Severy, who was recruited by the downtown IBM organization directly from Wellesley College. Marge married a year or two later when her young man got out of the Army and her married name was Herrick. And somewhere around 1948 or 1949, she left the Watson Lab when IBM re-instituted its no married women employment policy that it had had before the war, went out to the University of Wisconsin where one of the very first university computer laboratories was being set up and became supervisor there. So she not only was my first computer room supervisor but was also one of the very first university teaching computer laboratory supervisors at Wisconsin and among her probably the most famous of her co-workers and students was Freddie Greenberger, who became quite influential in the art of describing computers on the West Coast. Among other things, he was the founder of Computer Newsline, the news sheet Computer Newsline, which is moribund at the present time but was influential in the '50s and early '60s.

Well, Marjorie came to work as my first supervisor. She didn't know anything more about the punch-card machines than I had when we started but she had a very quick native intelligence and a great deal of willingness to work and learn. She had working with her two or three other people. A young man from the IBM Accounting Department, whose last name I've forgotten but whose first name was Cliff, who knew much more than the rest of us about the plain, old day-to-day idiosyncrasies of joggling cards and wiring plug boards but who had very little knowledge of the machines.

Shortly thereafter, I would guess probably four or five months thereafter, we had a Miss Elizabeth Ward join us. Liz was primarily a blue stocking a daughter of a rather rich family in Red Bank, New Jersey --but she had had mathematics in school and was looking for some contribution that she could make to the war effort and she did the desk calculator work that I referred to in the earlier tape: the calculation of the position of the moving shock at which we had to make an adjustment of a partial space step in order to finish each time step of our calculating process. And at a somewhat later date, two or three or four years later, she also operated for me the first bibliography of computer literature which the, what the institution now called, IEEE then the AIEE (the American Institute of Electrical Engineers) put together. I'll probably forget to do that when I come to it so I'll say that that was under the supervision of Doctor Bob Serrell, who was at that time at the RCA Laboratories in Princeton, New Jersey. He co-opted me with the help of a chap named Havens, who will appear on the scene shortly, into the AIEE activity called the Computer Committee and the first thing that I did for them was to operate this bibliography in a punch-card context and Liz Ward actually did the work on that.

OK, those were the very first people that we got. We had frequent visits not only from the higher level people like MARSHAC and BETA and so forth, and Ira Mayer and Teller but also from people who worked in the immediate level of work out there; notably Richard Fineman. Doctor Fineman recently got the Nobel Prize in Physics and was already regarded as one of the great theoretical young men of the time even in the mid-40s. He had the reputation of being a young Oppenheimer essentially and I distinctly remember his showing me, for instance, how to wire a chain of selectors on a 405 plug board. It gives you a measure of the times that a future Nobel laureate was concerned with the finger-to-wire technique of plug board wiring simply because it was urgent that these calculations be performed and the lower level people --the people who nowadays would run such equipment --hadn't the faintest idea how to perform such work. Sooner than train up a whole generation of lower level people, it was very much easier to do the work yourself and, in fact, we did so.

For instance, the actual embodiment of these calculations that I was describing was something on the order of 28 standard IBM plug boards, mostly one panel plug but a fair assortment of three panel boards that went into a 405. The 405 was a big tabulator, the most modern one that IBM had at that time, and one this we produced the printed pages which were airmailed, of course, registered, to the mysterious post office box in Santa Fe, New Mexico, which later turned out to be the drop box for the Los Alamos Laboratory. Among other things, we were told that they had several hundred copies of Astounding Stories, the science fiction magazine, all being mailed to that one box and that this was the giveaway to the subscription department of Astounding Stories that something interesting was happening in New Mexico.

Well, this 405 was the summary machine, so to speak, of the process and we had several plug boards for it. But, in addition to that, most of the actual calculations were performed on this bank of eight IBM 601 Calculating Punches, which had this particular algebraic sign control circuit installed, I believe on six of them. I think we had two that did not have it. We had a 514 Reproducer or two. We had a couple of Sorters as I remember. We had only one or two keypunches because there was only a minimal amount of introductory keypunching.

MERTZ:

How many cards were involved in this?

GROSCH:

At the time that we disbanded the classified work, I had 300,000 cards punched up and stored away in a vault in the basement of the newer Watson Laboratory to which we had not yet moved. And, in fact, the problem of disposing of these cards, which were not actually stamped "Classified" in any way but which in their ordered forms certainly contained some useful information, was a harassment to us three or four years in the future.

We were simply not accustomed in those days to the profligacy of computer use that's current today in both scientific institutions and the military and even in universities. To us, a box of cards, which actually cost only I guess $10 or $15, a box of 10,000 cards, seemed like a pretty precious object you know. And we made an effort not to waste them and so forth. This was partly due to the fact that we had all come from rather penurious disciplines, like astronomy. Partly due to the fact that Eckert's experience at the Astronomical Computing Bureau and Lillian Finestein Houseman's experience when she joined us later was very much on the side that a box of cards was a precious object. And also due to the fact that it was hard to get supplies and so forth with wartime priorities for a good time to come. For instance, those 28 plug boards were probably harder to get than one of the calculating punches because the plug boards were in great demand all over the world, including actually in the military theaters and most people could do pretty well with three or four you know, per machine and here we wanted 28 for a fairly small installation.

MERTZ:

How many people were there?

GROSCH:

I think I had at --my initial crew actually consisted of about four people --the three that I mentioned and one other person who escapes my memory almost completely now. There was still another person from the accounting department at IBM who helped Cliff and we had almost full-time attention from some very good service engineers, a succession of very good service engineers, what IBM calls Customer Engineer. None of them were quite up to the standard of this man, Richard Denna, that I had met at the Naval Observatory and who later joined the Watson Laboratory at Eckert's request but who was not there at this time. He was in military service, I believe, at this time. But they were all very capable and we had to have them because a good deal of the work that we did, especially on the calculating punches, was entirely unconventional.

And this leads me, I said I was going to talk about equipment, to an interesting little piece of history that I think I'm one of the few leads to. The plug board of the IBM 601, and on many of the older IBM machines of that vintage, had on it a sizeable number of switches. Now these switches were not toggle switches that you flip with your fingers. They were actually alternate positions of a little, what was really an extremely short plug wire that connected adjacent holes. It was called a jack plug and it was a little thing like a miniature rubber bottle with two metal prongs sticking out of the bottom. These prongs were connected inside to a little rubber plug and it was simply the shortest possible wire which could only be used for two adjacent holes. When you put this jack plug in a pair of holes marked "on", you've got the effect of flipping the toggle switch one way in the circuitry and when you put it in an adjacent pair of holes marked "off", you've got the opposite effect.

Now it turned out that not all the "ons" and "offs" on the 601 plug boards were real binary choices; that under some conditions, you could put in two jack plugs --one in the "on" position and one in the "off" position and get useful results which were different from "on" only or "off" only. The knowledge of which of these were useful things to do was, of course, an esoteric sort of thing. You could obtain it by reading the blueprints of the machine but, at that time, IBM was extremely reluctant to have customers look at these blueprints and, in fact, the people at Los Alamos, the customer engineers assigned to the Watson Laboratory and as it turned out, at about the same time, Paul Herget who was then working at the Naval Observatory in the old installation that I had seen installed in '41 were just about the only people in the country that knew about this sort of thing. Even the few adventures being done in the aerospace industry on the West Coast had not yet acquired this kind of esoteric information. So, you had to have private information about this. In fact, somewhere around '46 or '47 --a year or two after the time we're talking about --Paul Herget wrote up, either just before he left the Naval Observatory or just after he left it to go back to Cincinnati, about a 20-page coverless brochure, so to speak, or a pamphlet on how to do this sort of thing which is, as far as I know, the only "literature" in the business on how to do unorthodox things with the 601. By that time, there were several hundred people that knew how to do it and they were learning from bootlegged copies of this sort of thing. IBM had with each machine a blueprint and this blueprint was marked off with colored pencils by the customer engineers as changes were made to it; especially…(change in speed of the tape - inaudible) were added to it. If additional features were very standard ones, like additional x-punch positions on a reproducer, there was a formal blueprint that came with the pieces and as the engineer installed the pieces, he simply clipped the additional blueprint, the modifying blueprint, to the old one or perhaps substituted it for the old one and destroyed the old one. The resulting package of material was then locked in the little compartment on the side of the machine. The physical machine contained the blueprint describing its situation. It was extremely important that these engineers keep the blueprint up to date because it was the master record, at least for their own use, on what was in the machine. There were, of course, presumably accounting records back at the IBM office and back at the IBM laboratories for facts about this but the most important of all was this actual blueprint. And, if it was an unorthodox circuit, like this algebraic sign control thing, changes were made on the face of the old blueprint with colored pencil, as I started to say, describing it and notes were written on the side. Now this document was frequently consulted and one of the jokes of the trade in the early days was that you were not a certified computing room supervisor until you had stolen a copy of the key which opened the machine's compartment so that you also could look at the blueprint and try to figure out why the dumb machine was doing something you didn't want it to do or your plug board circuitry, that you thought was perfect, didn't seem to be working.

At a considerably later date --like four or five years later --I managed to secure permission for a few of our people (I, myself, was one of them) to go down to IBM headquarters and take some courses in the circuitry of the machines; courses designed to encourage people to become customer engineers but in 1945 this was absolutely verboten. The inside of the machine was supposed to be terra incognita and you had to call your friendly customer engineer to get any service. Now this was all right in our case because we were, of course, as a part of the IBM corporation and as an important artifact of the war effort, we were very well served indeed. But, if you had a small data processing installation somewhere down in the clothing district or something like that and your machine went on the fritz, you might have to wait a while before the mysterious customer engineer appeared from somewhere to fix it. He wasn't a resident engineer because there wasn't enough equipment to warrant his presence. So this business of making sure that you had good service was not entirely IBM's problem, it was also a problem of the computer installation manager.

Now we had installed this equipment, remember, in the 10th floor of the Prutine (?) Laboratory in a hastily vacated space in this physics, research and instruction building. I didn't require air conditioning in those days. The floor loading was relatively high, especially because of the heavy weight of the tabulator, but this was a big, reinforced concrete building and there was no problem about that. So as the machines were uncrated, as they came from the factory and were uncrated, they were put into use virtually immediately. There was no problem of any elaborate air conditioning check-out, of false flooring, of cable lengths to be determined or anything. We did, as I remember, have to install some extra heavy electrical circuitry but, of course, in a physics building this was trivial. It was just a question of making some changes in the floor power panels and this was done in less than a day. We did not have security with the exception of a little information that Eckert and I had, which was classified and which we kept locked up all the time and only got out to look at it, why there was no problem.

Now about this time, I began to make visits elsewhere in IBM. For instance, there was excitement due to the fact that we were about to have, or had just had I guess, the unveiling of the Aiken MARK I automatic sequence calculator. This was subject to great discord in the IBM Corporation because Aiken was attempting to secure all the credit for the machine for himself and for Harvard. What had actually happened; of course, this is second-hand information, as far as I was concerned, because I was not actually present at the conferences and so forth that were involved. But what had actually happened was that in the early '40s, Aiken had brought the specifications for the machine that he wanted to build to the IBM people, to Mr. Watson himself, Watson, Sr. himself. Watson had appointed a senior engineer named C. B. Lake the head man on the project and Lake with the assistance of several junior engineers, among whom a prominent one was Frank Hamilton (who will appear in my story with great frequency later on) had done the detail design, had altered many of the requirements of the Harvard specifications and had finally, with the natural difficulties of wartime had finally produced this enormous machine; this multi-ton machine with this enormous drive shaft running across the front of it and so forth. Aiken, who had a strong military bent exacerbated by the fact that he was wearing a Navy commander's uniform by this time, insisted on calling this the MARK I; although the official IBM name was the Automatic Sequence Calculator (ASC). And IBM gave Harvard University $200,000 to assist in the installation and use of this equipment causing me to make a joke in later years that that was the only university installation that got 120 percent discount, that is, they got a million dollar machine for nothing and $200,000 in cash besides.

Now the dedication ceremonies, as I'm sure will be by others, were marred by dissent on the part of IBM to the treatment that they were being given. And President Fillman of Harvard was forced to intervene and to alter the actual physical arrangements for the unveiling ceremonies on the threat from Watson, Sr.'s people that if he didn't, IBM would at least take their marbles and go home and it wasn't clear whether these marbles were only the individuals, included the $200,000, or might even include the machine, too, which I presume at that time still belonged to IBM. Harvard didn't put a buck into it, to the best of my knowledge, and the Navy, which later on paid for the operation of the machine and was deeply involved with it, I think at that time had also not put anything into it. This, however, should be checked by people who had first-hand knowledge of it. I'm essentially repeating IBM's stories that were told me in 1945 and '46.

MERTZ:

Now were these stories from people who had attended?

GROSCH:

Oh, yes, these were from people like Frank Hamilton and so forth who had actually built the machine so they're pretty authoritative.

The fact is, however, that they were passed through another man's brain before coming into mine and I'm pretty sure my recollection of them is accurate but, again, it may be flavored by many, many stories that have circulated around in the trade ever since about this fairly exciting time.

Aiken, by this time, had gotten some kind of weird arrangement with the Navy Department --and I use the term weird advisedly --to calculate an enormous set of tables of vessel functions with this machine. Well, we needed vessel functions at the end of the war like we needed a hole in the head. In the first place, the whole idea of a machine of that power would be that it could calculate a value of required; not search for it in an enormous prepared table done in advance. That was the kind of thinking that led to Brown's tables of the moon that I referred to as Comerie's introduction to the punch-card business and so forth. And it made very good sense as long as you didn't have a machine capable of going through an elaborate sequence of different operations and then starting over. It would have made sense, for instance, to have such a table to use on this shock wave board that we were doing at the Watson Lab because we were having to do everything in some sort of a batch form but the whole essence of the Babbage (?) idea, the sequence of operations which you could run through from the beginning, made these dumb tables unnecessary. And those of us who had some feeling for the philosophy of the machine as well as the experience and the actual production of mathematical tables were irritated almost beyond measure by this project because here they were locking up the only great calculating machine in the entire world on this absolutely asinine and useless project. And they kept on doing it for, you know, years after the end of the war. Well, years perhaps is an exaggeration --for a couple of years after the end of the war.

Now Aiken had a substantial contract to do this and he used the money from this contract in turn to set up a hardware laboratory at Harvard in which he designed and built a succession of (?) machines which were not designed by IBM or manufactured by IBM. And it's perfectly appropriate to give him the credit for the specific design of those machines but he certainly did not do the detail design of the MARK I. That was done in Endicott, New York, by IBM engineers to his somewhat modified specifications. And I think there's no question about the validity of this even though my own information was second hand. I was not a member of the design team or anything. I think there was no question that this was so. It was generally admitted in the trade.

Shortly after the end of the war, the first publication of the Harvard Computation Laboratory came out and it was an inch and one-half thick book, completely describing the innards of the ASC or the MARK I and this, of course, is a fairly common article in the literature of the trade. That was, in a sense, the first major machine description after I presume the poor little things that Babbage put out in the 1860s.

MERTZ:

Do you recall were many of the people --it's my recollection that a number of Navy officers or men who had some interest or proclivity in computers received training at the Harvard?

GROSCH:

Yes, Dick Block is certainly one. He's one of the early characters in the trade who is still very active. He's still deeply involved in the hardware side of the computer business to this day and he was one of them. And, of course, the sort of senior programmer or senior machine operator type was Grace Hopper, who is the most famous woman in the trade today. So there are lots of other sources to which you can refer for both the operation of the machine, the later design of successor equipment which was unrelated really as far as, you know, the architecture is concerned to the MARK I but which was done by the same people and so forth. You don't need to rely on second hand…There are many others. We later hired a chap named Bob Seeber who became the manager of the next big machine that IBM built back in '47-–'48, as I will recount, and he came to us from Harvard. He was one of that group also and he's on the verge of retirement at IBM now. He stayed in IBM for the rest of his career and, like Wallace Eckert, has many memories of these days. He was, as I remember it, Seeber was the next professional employee hired by the Watson Lab after me. I was the first one after Eckert then I think came Seeber, then some people from the Radiation Lab and then Hileth Thomas (?).

MERTZ:

Just to jump back a little bit to give the version that you heard of the scene at the dedication ceremony.

GROSCH:

My understanding is that Watson, Sr. himself said to Conant, "Either you get this thing straightened up or there isn't going to be any ceremony tomorrow." Now, I repeat, second order information and I do not know whether by that he meant I won't be there or the IBM people will pull out or whether he meant he'd take his money away or whether he meant, you know, he'd draw up the moving vans and pull out the machine. Watson, Sr. was perfectly capable of doing the latter if he was sufficiently convinced that IBM was not getting its fair credit. And speaking as, of course, a person who's somewhat prejudiced by being on the IBM side of the thing; it seems to me that he was quite justified because this entire thing was an enormously expensive project by the standards of those days --over a million dollars out of pocket IBM expense at a time when even IBM stockholders were not accustomed to eleemosynary activities.

Now it's all very well looking back on it from this vantage point to say that, of course, this got IBM into the computer business, that this paid off 100-fold or 1,000-fold in the future. Indeed, it did but how many people thought of that at that time. Only a few visionaries like Watson, Sr. and Aiken and presumably Eckert and Grosch and so forth, too. But many of the IBM engineers, for instance, thought the old man was as nutty as a fruit cake to waste his time on this thing when he could be using the same equipment, the same personnel, the same raw materials to make, you know, 100 tabulators. Actually, the machine was made to a very large extent out of standard IBM componentry; that is, many of the counters going clackety-clack inside the MARK I were the same things that I had in my 601 calculating punches, may be made double length or ganged together, you know, to allow for the very large word length that Aiken insisted on, which, by the way, to me as an experienced numerical analyst also looked grotesque. I might add that I had direct personal opinions about that and I suppose you might say I began my career of criticism of the field in which I majored for the last 30 something years by remarking that the word length was absurd; that there was no point in having, what was it, 19 decimal places or something, you know, sort of a dumb machine. That was sort of inherited in a way from Babbage. Babbage II and the Shurtsey (?) Difference Engine of Albany and places like that all had this idea that they had to have an enormous number of places. But having struggled manfully with high accuracy astronomical calculations, the most accurate calculations that anyone really needed to do. I was not at all convinced of this and regarded it as sort of a mathematical hobby that people had; akin to this business of determining pi to several hundred decimal places. So I was negative on that one from the beginning.

One other thing that's notable about the ASC that you may not have picked up already is the fact that it did not really have the ability to branch or to select alternate sequences. One could calculate away on an elaborate sequence and you could have more than one sequence in the machine in the form of perforated paper tapes at one time but the machine was not only did not calculate subsequent orders in the stored program concept idea, which was, I presume, percolating away in Johnny Van Neumann's head about this time, but in addition to that it didn't even switch back and forth automatically between these foreseen sequences. So while it went through an elaborate series of calculations, I repeat it didn't have the branching instruction that is probably the second or third most important artifact in the whole soft ware area.

MERTZ:

Do you think that Aiken received undeserved, unmerited credit in the award given, that Franklin East (?)…

GROSCH:

No, I think Aiken is the inheritor of Charles Babbage and an enormously important figure in the business but I do think he tried to hog the credit. I think that like many other great men he was personally a selfish and difficult person but he certainly deserves the credit. He certainly is the inheritor of Babbage's garment and he had the foresight to see the power of this equipment in a modern technological civilization and could conceive of its embodiment in a sufficiently impressive form that IBM would back it. No, sir, I think you ought to plaster the old guy with all the medals you can hang on him but that doesn't mean he isn't a son of a bitch which we all admit he is, too. I'm sure Babbage was worse. From everything we hear about Babbage, he was really a wild man but that doesn't mean he wasn't a very great and indicative figure in the development of technology.

MERTZ:

One question that did come up at the time that Aiken got, I believe it was, the gold medal from the Franklin Institute (?) in 1963, I believe it was, a study was made comparing his contribution to that of George... Inasmuch as they both had worked quite early on the development of parallel efforts and accomplishments.

GROSCH:

One of the things that I hope to contribute to the history of the machines is to make it very clear that there were parallel efforts going on. That much of this would have happened anyhow. I don't think there's any question whatsoever that the Bell System's interest in relay calculators, that ZEUSSA' (?) efforts in Germany, that the punch-card sort of stuff that started with Comerie and went ahead through Wallace Eckert, any one of these things would ultimately have done something and if all of them had been dead, if every single one of them had never happened, the pulse electronics coming out of the radar laboratories, notably the Radiation Lab at MIT, would have created this. It was an invention waiting to be made and there's no question about it. Nevertheless, the guy that picked up the central idea that Babbage was practically the sole proprietor of in the 1800s clearly I think was Howard Aiken, much as you may dislike him. Now…(?) was a nice, warm guy and, gee whiz, I'd be delighted to say that he was the guy instead of Aiken, but I don't think it's so. And, besides, you know, there's also the question of team effort involved in this. The Bell System, as a whole, was already operating what we would nowadays look on as a very major decentralized specialized digital computer; namely the telephone switching network. It's true that it was very specialized indeed and it's also true that the idea of the computer as a --what did some Bell Lab guy say the other day --great immortal machine and the telephone network likewise hadn't really dawned on very many people. The fact remains that there were a great many other forces in the Bell System besides Stibbitz and it was their counterparts in IBM that made Aiken's dream realizable. But Aiken, at least up to the point of specifying the machine and coming to IBM with it, was working pretty much alone. And Zeussa, from what little I know about him was doing the same thing in Germany. They were essentially following the Grail on their own and not as part of a natural corporate effort. The fact remains that if there hadn't been any Aiken, IBM would still be in the computing business and if there hadn't been any Stibbitz, the Bell System would still be in the computer business. There sure as hell wouldn't be any Zeussa outfit without Zeussa.

MERTZ:

The other interesting part of the problem, too, is the development of… what might be termed a dormant tradition on the reawakening…computers.

GROSCH:

Well, you see the English had never quite given up this kind of Comerie effort. Comerie established in the very early 1940s --I'm a little uncertain as to the exact year and it would be interesting to determine this as far as the history. Late '30s or early '40s, he established a thing called Scientific Computing Service Limited in England and it was in existence as a working entity through the war and it did war work and so forth. It had some punch-card equipment but, for instance, I remember that his method of doing multiplications was to punch out the multiplier and the multiplicand, print them on a tabulator and then have a girl multiply them on a margin calculator. And punch the answer back into a card as being cheaper considering the low cost of a calculator and the low cost of the girl than renting a 601 because he had to rent the 601, of course, full-time. And the price for this sort of equipment in England tended to be higher because it was manufactured to BTM specifications and patents rather than to the fundamental IBM ones, which meant lower production and so forth.

I remember, for instance, that his tabulator was --the English tabulator --supposed to be superior to the IBM 405. Although I never operated one and don't remember any more in what way it was supposed to be superior but there were differences.

Well, now that meant that during the war Scientific Computing Service Limited was actually doing the same sort of thing that I did at the Watson Laboratory starting in 1945. And I think it fair to say that that was the first computer service organization world-wide and it went on, and I think, in fact, didn't really disappear until the '60s at which time it turned primarily into a library service organization that would supply technical books to people who wrote in and so forth. Obviously a one man show by that time.

Comerie was President of this first thing and among his staff members were J. C. P. Miller, whom I mentioned a while ago, well-known table maker and several other people. And all of these people had had some connection with astronomy so that, you know, there was this little knowledge of what was going on floating around in the Wallace Eckert kind of circles and these people, in turn, would have been aware of the electronic skills developing at the National Physical Laboratory and other places and would have undoubtedly turned up a Zeussa-level computer effort sooner or later even if there had not been an American base to operate on. One of the things you need to remember is that there were other entirely different types of computing being pulled off at this time. For instance, the introduction of several mechanisms to the original post-differential analyzer that was going on and some of the most experienced users of differential analyzers in the world, and most sophisticated users of differential analyzers were in Europe. There had been one at Oslo, for instance, at the Institute of Astrophysics or some such name which is supposed in the folklore of the trade to have been the first thing that Hitler's troops captured when they landed in Oslo. They had been instructed to capture this before anything, even the power station or the radio station…, the man who did this, was an astrophysicist. I mean the man who was operating this institute was an astrophysicist and I had met him before the war at these Harvard summer schools. And I was aware that he had this equipment and, in fact, had investigated the possibility of using it for my Jupiter 8 doctoral thesis but, of course, soon found out that the kind of integrations that it performed were limited to two-figure, or at most three-figure accuracy where I needed six or eight-figure accuracy.

MERTZ:

Is Rosseland…

GROSCH:

ROSSELAND, now dead, I believe. He was simply copying the Bush, the fundamental van Bush ideas, you understand, but what I'm saying is that there were several of these machines in Europe and the most prominent Englishman to be associated with him was Douglas Hartrey, who is one of the great names of the computer trade in the very early days and would certainly have introduced the British scientific community, the non-astronomical scientific community, to these ideas even if the Comeries and the Greenwich Observatories hadn't existed. Hartrey was associated with Cambridge University and, in fact, his book "Computing Instruments" and so forth is one of the seminal books of the '40s in his trade. He was doing atomic physics as distinguished I guess from what we nowadays call particle and nuclear physics. He was doing atomic physics calculations on differential analyzers and because of the difficulties of accuracy was very anxious to go over to digital equipment. Had been unable to raise the money to use Comerie's Scientific Computing Service equipment but was, of course, good friends with Wallace Eckert and others and was very anxious to have relationships in the United States with people who were in this sort of thing.

MERTZ:

Financial limitations seemed to have played a major role in this.

GROSCH:

Well, the Watson Lab equipment, was-probably if rented from IBM at standard prices, would probably have cost maybe $2,500 to $3,000 a month. That means in IBM terminology, it was a $2,500 or 3,000 point installation. The Naval Observatory installation would have been about 1,000 point installation. It cost something like

$10,000 or 12,000 a year and that was actually paid. I mean the Navy Department actually paid that money to the IBM Corporation--Where the Watson Lab thing was essentially a transfer of paper within the outfit.

Now that doesn't seem like much but when you realize that a full professor in those days got perhaps $12,000 a year at most and this included full professors of the stature of, you know, Von Neumann and Oppenheimer and people like that. You begin to realize that it just wasn't the affluent society that we have now. In the next two or three years as people came to me from the aerospace industry and Rand Corporation and so on to talk about setting up larger computer labs, the first question always was whether they had enough money to get a tabulator because a tabulator cost nearly $1000 a month all by itself and it made quite a bit of difference in how you handled your problems whether you could have one of these quite sophisticated 405 tabulators or whether you'd just have to struggle along with less sophisticated equipment, old fashioned horizontal 285 tabs or something of this sort.

Now, of course, where a single computer may rent for as much $300,000 a month, instead of the $3,000 a month that I was paying for all my whole room full of stuff at the Watson Lab, why the perspective is somewhat different. But, I was at that time, for instance --I came to the Watson Lab, as I remember, at only $5 or 6,000 a year. I was, as a human aside I might say, extremely aggravated to find out later that Eckert had been raised from $12,000 a year when he left the Naval Observatory to $30,000 a year by Watson, Sr. when he came to start at the Watson Lab. It's an interesting artifact, however, that I've been informed by gossip since that he never got a raise in all the remaining years and retired just a couple of years ago still at $30,000. So, you can see that the old man thought in pretty fancy terms --to multiply a man's salary by two and one half fold was a fairly spectacular thing. Of course, at that time Charles Kirk, the number 2 man in the company, the executive vice-president of the company, was getting about $120,000 in salary plus other emollients. so this did not distort the IBM salary picture but it certainly distorted the academic compensation of the other people who were working in the field. Of course, that figure was a deeply held secret and I don't suppose a dozen people in the whole company knew what Wallace was getting or what any other senior person was getting.

Now I said that I'd begun to tour around in the IBM Corporation and that first of all, of course. I tripped over the Harvard machine. One of the ways that I tripped over this was that I began to associate on a frequent visit basis with some men at the Endicott Laboratories who were building relay calculators, which were to be installed at the Watson Laboratories shortly thereafter. These two men, with whom I had frequent dealings at that time, were named Ben Durfee and Bob Piatt. I think Durfee has retired but I think they're probably alive and they both remained with IBM continuously. Piatt was a younger man. Both of these men had been associated in some way or another with the Aiken project. I don't remember if I ever knew in exactly what way but they were both associated with. Durfee had also been associated with the construction of the special relay equipment which is referred to even now rather in gingerly fashion as having been the equipment used to break the Japanese Naval code and that was about as hush-hush as the Atom Bomb. In fact, I didn't even know about this for a year or so later in spite of the close association. The fact that this magic technique, so-called MAGIC, was used is now in the literature but it's pretty hush-hush stuff to this day. And the NSA-type people that do this sort of work today have written very little to explain how this was integrated into the war effort and so forth. However, IBM did build the racks of relay equipment that did this work and Durfee was associated with it.

Under the general direction, I believe --I don't know exactly what the organizational structure was --but certainly associated in a junior way with this man C. D. Lake, that I referred to. Lake had invented either for the MAGIC program or thereabouts a very fancy multi-contact relay, which was called the LAKE Wire Contact Relay, and was used in hundreds and hundreds of thousands in IBM equipment beginning about this time and spreading through the whole range of equipment.

MERTZ:

This time is 1940…

GROSCH:

This is 1945 still but it's late in '45 when I've begun to be able to do a little traveling instead of just staying home pushing cards through the 601s on Von Neumann's shock waves; or to be more precise, on Marchak's shock waves.

Now I want to back off a little bit and follow a thread that leads essentially to ENIAC. And this takes us back to the man Lowen Cunningham who was, if you remember, worked with Fred Whipple on orbit computing with desk calculators when I was a young student and graduate student. Cunningham had gone to the Aberdeen Proving Ground sometime during the war just as I had gone into optics. And he had, in the course of the war, begun to run the punch-card installation that was installed there for ballistics calculations. Now you remember that I said earlier that in World War I a very famous celestial mechanics man named F. R. Molten, who later I believe became Secretary of the Carnegie Institute, perhaps it was the American Association of the Advancement of Science, or something, but later became a big figure in institutional science in Washington. Molten had gone to the Aberdeen Proving Ground and had shown them how to use astronomical and numerical analysis techniques to integrate the equations of motion of a projectile, which were a little different from those of celestial mechanics because of the resistance of the air, of course. The earth's atmosphere came into it, the shape of the shell and things like that entered into it which don't bother celestial mechanic-ers or didn't until very recently.

So Molten left a tradition of this behind him and several publications --New Methods in Exterior Ballistics was one, which had some influence on my career and on numerical analysis in general. This work continued behind the scenes during the time between

World War I and World War II and specifically the people that stayed on at Aberdeen calculated the trajectory tables for all the artillery shells that were used by the Army.

Now these had to be recalculated whenever the physical shell was changed and, of course, especially when a brand new cannon or mortar or something was brought out. Something like the work at the Naval Observatory, this was kind of a restricted task at first until new things came along that made it more complicated. Where the air navigation made it necessary for Wallace Eckert to mechanize the calculations so he could produce the Air Almanac, the use of anti-aircraft cannon fire made it necessary to mechanize the ballistics calculations at Aberdeen. As long as all they had to figure out was where the shell was going to land on the earth after soaring through the sky, whether this is Big Bertha or a mortar shell, was beside the point. As long as they only had to find the terminal point, there were many simplifications that they could do in their calculations. In antiaircraft fire you had to know the entire trajectory of the thing because you might intersect the enemy airplane and your shell anywhere long the course of the trajectory so this increased the amount of calculating to be performed enormously. Also, of course, with the proliferation of weapons in World War II, you had all sorts of new stuff to calculate; all kinds of weird shape charges and so on. Now the man who was in charge of this, running a thing called the Ballistics Research Laboratory, I think, towards the end of the war was named Major Leslie Simon. Later Colonel Simon when I knew him in '45 and '46 and at the end of his career, Major General Simon.

Simon was a small, intelligent, professional Army officer who, I'm sure, deserves a lot more credit than he has gotten for his share in developing computers because he wasn't just, you know, following orders. He was being creative in his role in the same way that in later days Herman Rickover, for instance, has been creative in the nuclear submarines and guided missile field; although in a much quieter way.

MERTZ:

Hyman Rickover?

GROSCH:

Yeah. Rickover is a loud type. Simon was a very quiet, behind the scenes type but he also contributed personally to the development of this stuff. My understanding is --I didn't know him in the very early days of all this. Didn't meet him until about '46 or '47 but my understanding is that he had a background in statistics and a reputation in quality control. And, of course, quality control is very important in procuring shells and ordinance in general. So this led him into this particular task. He was given the command of the Ballistics Research Labs at Aberdeen and this was a time in which this mechanization was going ahead…

[End of Tape 4, Side 1]

[Start Tape 4, Side 2]

GROSCH:

I was saying that Simon had a professional expertise in quality control and a background in statistics, so when people talked to him about calculations, it made easy sense to him since he'd had to perform them himself with only desk calculators and slide rules and so forth. Therefore, when this extra load of bombs to be dropped by Army air force planes and anti-aircraft guns to be fired with whole trajectories determined, when this began to build up, he was quite receptive to the introduction of IBM equipment and so forth. I think it was probably introduced before he took over --this is something you could find out at Aberdeen --but, in any event, its use was something he approved of. And, somewhere around the middle of the war, this man, Lowen Cunningham, came down from Harvard and took over the operation of these machines with, of course, a fairly sizeable staff of less professional assistants.

Now, Cunningham was, of course, aware of the Comerie experiments and the other things that I've been talking about and he felt that it ought to be possible to do better than just run standard batch processing computing punch-card machines. So with the support of the senior military management at Aberdeen, a contract was drawn up to explore the possibility of, what we would now call, medium size computers. Three contracts were let. I don't know the details of how many bids were received; whether there were more than three offers. All of this is of great interest but I simply have never known what the other possibilities were. But, three contracts were let. One was let to the Bell Laboratories and it was essentially due to the fact that the trade, small though it was, was aware of the early work of Thorton Fry and George Stibbitz and others in specialized digital relay equipment, complex number calculators, etc., etc. In fact, there were by this time in existence half a dozen --perhaps not a half a dozen but several different digital calculators built by the Bell Laboratories and demonstrated by them. I think mostly, however, not doing very useful work except perhaps inside the Bell System. So a contract was secured by the Bell System to build a much larger and more flexible relay calculator which is well-known and well described in the literature.

Another contract went to the Moore School of Electrical Engineering.

MERTZ:

What date was this?

GROSCH:

Oh, this contract must have been in '43 or '44. Again, this must be well known in the trade but I've never known it exactly nor have I actually seen the written material. But, the reason I say this is that IBM completed its third contract, which I haven't mentioned yet, and delivered the machines before the end of the war. And they were actually at work at Aberdeen doing useful work for Cunningham when the war ended. So, assuming that they built them very quickly, as they were able to do in those days, it still leads you back to about '43 for the initial discussion. And, it might have been earlier for all I know; it might have been, you know, at the beginning of the war because much of this, of course, the description that I'm giving is of the actual production use of the machines. It doesn't follow that the senior people, the ballistics scientists and so forth, didn't see all of this coming, you know, five or ten years earlier because certainly they knew anti-aircraft guns and aerial bombs and so on were going to be used in the armed forces even if there wasn't a war. So I don't know those facts but it must have been '43 or earlier.

This second contract of the Moore School, of course, was to develop an electronic machine which was a fairly obvious idea by that time with electronics blossoming on every side.

MERTZ:

Were these contemporary contracts you're talking about?

GROSCH:

These were simultaneous contracts. All three were let simultaneously. Whatever the date was --and, again, I suppose ENIAC people know what this date was also --to the best of my knowledge all three were let simultaneously; that is, you know, the same week or the same month signed, I think, by Leslie Simon, who is by the way still alive so that one could check up with him.

The third was to the IBM Company and whether it had anything to do with the knowledge that IBM was building this MAGIC equipment or not, I have no way of knowing. As I understand it, the MAGIC equipment was paid for by the Navy and was ultra-super hush-hush so it doesn't follow that anyone in Aberdeen had any knowledge that it existed. But, in any event, the same sort of personnel were used on the project and, specifically, Durfee and Piatt had been involved in it.

Now Wallace Eckert came across this very early in his connections with IBM whether he knew it in his initial discussions before we started in May of '45 or whether he found it out shortly thereafter, I don't know. He would, for instance, have probably found this out from John C. McPherson who was his major contact in downtown IBM with what I christened some years later Galactic Headquarters then known as World Headquarters. McPherson was one of the very few people who probably knew all about this, including the MAGIC activities and so forth. He'd come into the IBM Company as a salesman in the '30s but he had an engineering degree from Princeton University and was quite capable of acting as an interface between top management and the more hardened engineering types and was so doing at the time when I met him in '45. He worked down in the 590 Madison Avenue offices and I met him probably in June of '45 after I'd been at work for a couple of weeks.

Now McPherson or others assigned Durfee and Piatt to work on this sort of thing.

MERTZ:

Excuse me. Was there much direct involvement or interest in the work you were doing up at the laboratory…

GROSCH:

Only a few of the senior IBM executives knew about it until the bomb was dropped. After that we began to be an important artifact of the company and to be featured in the company paper and all of that sort of thing. But, during the time that I'm talking about and really up to the end of about 1945, there was very little publicity. This is probably due to the fact that we weren't quite sure how the development, how the relationship with Columbia was going to develop. It wasn't completely formalized yet. All this had gone ahead very quickly under wartime pressures. Moreover, Nicholas Miraculous, old Nicholas Maury Butler was still the official president at Columbia and he was moribund at this time essentially; not a very useful person any more, although still a very revered and honored figure. And, Frank Thackinthall was then the acting, sort of acting, president and quite nervous about doing any major policy changes until a permanent president could be appointed. This permanent president, of course, turned out to be Dwight Eisenhower after a while but that's several years up the road yet.

Well, Durfee and Piatt, first of all, worked in the team that built these so-called Aberdeen machines as we called them. The official name is Pluggable Sequence Relay Calculator and two of them were built during the war, delivered to Cunningham at Aberdeen and used for actual working military ballistics calculations before the end of the war. Neither the Bell machine, nor the ENIAC were finished in time to do any real work. They did test runs but no real work; nor were either of them actually moved to Aberdeen until well after the end of the war. But, the two IBM machines, which were just, you know, big old lumps of black machinery on casters, were put in trucks and trucked down to Aberdeen and plugged into the wall socket and ran substantially before the others. Moreover, although relay calculators, they were also relay calculators, they were substantially faster than the Bell machine in actually multiplying time and so on. Although, very much slower than ENIAC. ENIAC was something like four milliseconds, as I remember it. The IBM machines were something like 400 milliseconds but the telephone relays in the Bell machine were more like a couple of seconds so the IBM machine was still faster.

Now two of these were installed at Aberdeen, I repeat, before the end of the war and Cunningham was reasonably pleased with them. However, he wanted more flexibility, more storage capacity. Very much like nowadays, you know, only it's early '45. So a further contract was given IBM to take these two machines back to Endicott and to improve them and remodel them. Somewhere along this point, Wallace Eckert found out about it and it was decided that three more would be built to the improved specifications so there would be a total of five of these machines. Two would be the original ones, would be finished first and would go back to Aberdeen. Two more would come to the Watson Laboratory and, in fact, they came in '46; not in '45 but in '46. And the fifth one would go to Dahlgren (?) Proving Ground in Virginia, which is where the Navy did its ballistics calculations completely separate from those done at Aberdeen by the Army. Dahlgren's needs were also important but they did not have as much aerial involvement as the Army since the Army was still running the Air Force at that time. And, for one reason or another, they seemed to be quite capable at the desk calculator kind of thing or perhaps their needs simply weren't attended to. That I don't know. I do know that both Aberdeen and Dahlgren had differential analyzers, post differential analyzers, and this was perhaps used rather creatively to extend the less precise work; not the trajectory calculation but the aerodynamics kind of work and that sort of thing…

MERTZ:

Hydrodynamics.

GROSCH:

Hydrodynamics, exactly. And you know they had the shape charge, the bazooka sort of thing was coming along at that time and there was a great deal of explosives work and some of it, of course, actually got involved with the Atom bomb where there were, of course, explosive devices required to drive the pieces of fissionable material together somewhere along the line. So there was other kinds of ballistics besides trajectory and while the trajectory really demanded digital calculation, much of the rest could be done with other analog or similar equipment. So, Dahlgren was a strong competitor for Aberdeen and was to get a fifth of these machines. Since I was going to be in charge of the two at the Watson Lab, I naturally began to go up to Endicott to find out what was going on. I met Durfee and Piatt. I had no control over the design of the machines, which were being done to specifications agreed on with Aberdeen, but I was naturally very much concerned in how they would work and how we would get ready to run them and so forth.

MERTZ:

Who was in charge of drawing up the specifications at Aberdeen?

GROSCH:

I don't know that but I would presume it was still Cunningham or Cunningham and Simon. I don't know that for a fact. I think I never saw the actual contract or modification contract of any of this. Although I saw many of the IBM working drawings and detailed plans on the IBM side but I didn't see the official paperwork, so to speak. I was not, of course, in any way connected with the project officially. I was simply a future customer but the company was small. The number of technical people in it was not too large and we did have a rather special position in the company at that time. So I could find out about as much as I had time to find out but I had many things to do of my own. I couldn't spend too much time doing this.

Now, this, however, made me aware of the existence of the Bell line of computers and the ENIAC. And when ENIAC was actually unveiled in February of '46, of course, IBM was shaken as if a bomb had gone off in its own backyard. Not too many of us knew that the project even existed; only those really associated with the Aberdeen machine plus a few senior executives, like McPherson and perhaps Watson himself, knew that the project even existed. It wasn't that it was so super-secret but it was awfully specialized and Aberdeen didn't talk much about it because they weren't really sure the thing was going to work. In fact, not all that sure that it did work in the early days. Breakdowns were so frequent that you could never really be sure the whole machine was going at any one time for the first year or so. There were 18,000 vacuum tubes in it; all funny, old-fashioned inch and one-half diameter big glass bottles and they replaced, you know, dozens of them a day. So that in the meantime, between failures was probably only a few seconds actually. But, when you are working in millisecond speeds, you nevertheless got more calculations done between failures than you really got done between failures on an old IBM 601. A 601 would clank away all day before it began to make mistakes and you had to call the customer engineer. But at 600 operations an hour, it only produced a few thousand calculations before breaking down. Now you could get the same number of calculations out of ENIAC if it would run, for instance, for 10 seconds without failing. Moreover, ENIAC, of course, being also a sequence control calculator was able to run through a string of rather varied calculations before it had to repeat. Whereas, your old 601 would not only do a few thousand operations between errors but they would have to be repetitive operations because that was the nature of the beast.

So those of us who knew about it were excited about it and all that but it certainly posed an enormous threat to a far-sighted executive like Watson, Sr. It didn't pose much of a threat to the guys making punch-card machines up in Endicott. Their view was, gee, those silly scientific guys, you know, while they're doing that, we're going to go back to making tabulators again by the hundreds and thousands. But, the Watsons and McPhersons were much more intelligent than that and saw the enormous implications of this thing for all their equipment.

Now that was February the 15th or so that that was unveiled in the Moore School. To the best of my knowledge, no IBM people were present. I'd have to see the invitation list to be sure. Eckert and I were not present, certainly, maybe Watson was or somebody like that but probably not. The same people who were involved at Los Alamos --the Von Neumanns and so forth --were interested in the thing and Von Neumann, who, of course, was an extremely important advisor all through the military operations of the United State, had been in on the development of the ENIAC from the beginning. So it was really at the point at which ENIAC was actually unveiled at the Moore School that the importance of Von Neumann in the electronic computer business began to be obvious. I'd thought of him as a customer for the punch card equipment at the Watson Lab and I wasn't particularly aware of his interest in the architecture and so forth of the machines. It was my thought that he was going to use them to do better physics; not that he was going to actually help design them.

MERTZ:

There was, I believe, in March of 1944 or April of 1944, a symposium on shock wave computations and theory at Princeton at which a number of people were in attendance including Albert Einstein. During which --symposium, series of meetings actually --some of the computational problems…

GROSCH:

I'd love to have known about that. I never saw a copy of any proceedings that came out of that and, of course, I was not within the proper security classification at that time to have known about it. Because I was an optic cadet.

MERTZ:

Yes, it pre-dates a little bit your involvement but it was later ...

GROSCH:

Were there written proceedings for that? It would be interesting to see them now.

MERTZ:

Yes, there were.

GROSCH:

Right. Well, that must have been the time in which many of these people got together and exchanged information. I believe that Hilleth Thomas, who joined the Watson Lab in '46 I think, might have been involved in that because he was doing chemical explosives kind of stuff at Aberdeen at that time and they were probably Aberdeen. But I have never seen a report of that thing and I don't know what its seminal influence was.

MERTZ:

Well, selected papers of Von Neumann's were his contribution. He presented a paper.

GROSCH:

Now, in addition to ENIAC, which, of course, you know a great deal about and your tapes and records will have a great deal of involvement with them was this Bell thing.

Descriptions of both the IBM Pluggable Sequence Relay Calculator or Aberdeen Machines and the Bell Relay Calculator that I think was called the MARK VI are to be found in "Math Tables and Other Aids to Computation" in the early volumes. But the Bell Machine was better documented and I think more widely visited than the IBM equipment. This was fair because the Bell Machine was very much more flexible; very much more powerful and capable of unattended operation since it worked on paper tape. You could put a big reel of paper tape in it and go home for the night. And it was so reliable being built out of telephone equipment which is pretty reliable stuff, if slow, that it would clank away all night long and when you came back in the morning, you might have some finished results. Also, the machine was sufficiently flexible that if it ran out of work to do or encountered unresolveable lacks of convergence, or what have you, in its night's work, it would switch over to another sequence and tackle another problem. And this was spoken of very proudly by the Bell people and made us grind our teeth because we weren't that good yet.

You'll observe that by this time then you have a machine that is capable of doing alternate sequences, switching back and forth, of executing a branch instruction. The Pluggable Sequence Relay Calculator did that perhaps within a single card cycle, just as all the other IBM equipment did, but it certainly didn't do it in a long sequence of operations running through, you know, minutes or hours of operation. It was essentially still a card by card machine.

MERTZ:

How did your facility operate in terms of working hours?

GROSCH:

In those days it was pretty much a restricted thing. We pretty well worked a long hard eight in the morning until six or seven at night business and shut down. We did not yet have students. We had not yet started teaching at Columbia and although we didn't have security restrictions in the sense of guards and such, it simply wasn't easy to get into the Pupine Labs in the middle of the night. You'd sort of stay around for a while, run out your string of calculations but then when you came to the end of them, you'd go home. You didn't stay around and do your own work. One reason for this being that you were pretty fatigued with the pressure of the routine work that you'd been doing. Another one was that if you went out for supper and so forth, other military work in the same lab --there was a cyclotron in the basement and things like that --made it a little hard to get up and down. There were guards on the elevators, So, in general, we went home but we went home pretty late. We stretched the day out. If there was a machine breakdown and we ran into troubles, we might run until midnight. But, to the best of my knowledge, we never did any real third shift work or made any attempt to stretch it out. We were told at the beginning that it was very important that we finish these calculations, unspecified number of steps however, as quickly as possible. Actually we had not finished them at the time that the bomb was dropped and the result was that --really if you remember, we started doing this in June, I came aboard on May 24th and we started doing real work toward the end of June and the bomb was dropped in August. So, before we were really geared up to really crank out several time steps a day, which would have required three shift operation, the war ended and the tremendous pressure to do so dropped off. We continued, I think, for almost a year doing this as our major work. I no longer remember exactly when we stopped doing it but it must have taken nearly a year and we certainly carried it over into the other building into the fancier building of the Watson Lab into which we moved in '46. But, it didn't have so much pressure that we tried to set up three shifts or anything and, in fact, when we moved into the other building we began to use the machines for our own purposes; for astronomy and optics and teaching and so forth, the same machines that we were doing in the daytime, doing these shock wave calculations.

MERTZ:

Which that equipment, of course, could easily be put to work…

GROSCH:

The only problem was the shortage of plug boards, simple as that. We had lots of machines and lots of wires but we didn't dare tear them down ...

MERTZ:

…computers and shut down…

GROSCH:

Right. One of the great advantages of the IBM equipment in general is that you can switch from one problem to another very quickly by changing the plug board. Now in the early days of the ENIAC, for instance, it took days to switch from one problem to another. That was before Von Neumann's influence, and Crippinger's (?) influence was felt and you had to take these enormous trays of coax cable and actually physically interconnect the machine in a different way. While this was a terribly slow process --and, in fact, they kept it going most of the time on single things until they were done with it and then switched off to something else, --the Bell System was even more flexible than the IBM equipment in that you could put paper tapes into the machine and change it around very quickly. But, the plug board flexibility was good and, for instance, it completely ruled out the Remington Rand punch-card equipment.

No work of this sort was done on Rem Rand equipment, to speak of. Gabriel Korn did a little scientific work on round hole equipment, as we called it, in the late '40s and reported it in obscure places in the literature but it was awfully hard to do. And reason was that the Rem Rand equipment had to be changed mechanically. Most of the innards of that machine actually consisted of push rods and shafts, and so forth. The reading part of the machine that picked up data from the cards actually poked a little finger down through the round hole in the card to see if it was there or not; where the IBM machine stroked the card with metal brushes; flexible hair-like metal brushes to make an electrical contact with the common roller underneath. Well, the result was that all the innards of the IBM machines were essentially electrical; certainly, all the control circuits. Although the storage elements were often mechanical. But the Rem Rand machines were all mechanical. To change them from one job to another, you actually had to take a monkey wrench and alter the interconnection of the gears, and shafts, and stuff, which was perfectly feasible to do but it took hours to do where you could change a plug board in a few seconds. So the flexibility of the IBM equipment made it superior to the Rem Rand equipment and the early ENIAC but it was inferior to the Bell System equipment. Also, it broke down a great deal more frequently than the Bell equipment. But, also it could be repaired a great deal more easily, too and the customer engineers, who repaired not only my punch-card machines but later on the Aberdeen Machines that we installed, were very, very good at it and became, you know, would make the repairs so quickly that your up time was actually as good as or comparable to the telephone equipment.

MERTZ:

I take it that this laboratory had almost exclusively in the way of non-hand computational equipment, exclusively IBM.

GROSCH:

Exclusively IBM. In fact, I remember one of the Watson, Sr. stories was I remember two years later being quite anxious to get a printing calculator. By that, I mean an adding machine that was capable of doing something close to multiplication and division but would print the results on the paper tape like other adding machines. Now this is very common nowadays but, in those days, everything printed was an adding machine and everything that multiplied and divided was not a printing machine. So I wanted to get the closest thing I could to a printing device that would still do crudely, at least, multiplying and dividing and this was a Remington Rand, so-called, printing calculator which was really a sexy adding machine. And, Watson was so opposed to using anything except IBM equipment that Wallace Eckert had to go down and ask his personal permission to buy that Remington Rand calculator with IBM money and he was refused. Watson said, "Get an Underwood," which was not as good but we got an Underwood. That's a view of the IBM Corporation as it was in '46 ...

MERTZ:

Why was Underwood considered ...

GROSCH:

Underwood was not a competitor. Remington Rand made competitive punch-card equipment. The fact that IBM at that time had 95% of the punch-card business and the round hole boys only had five percent, entirely beside the point. Watson was a salesman. He wasn't going to give a dime to a competitor.

Now the Bell System, however, wasn't really regarded as a competitor in those days. It wasn't at all clear that Bell was going to go into the data processing business, or the computer business, or whatever it was called later on. In those days we referred to it as the punch-card business, which was a rather parochial view of the future possibilities. Wallace and I, of course, always referred to it as computing but the idea of computing and computers wasn't universally accepted yet.

The Bell System, however, did in fact not go very much further with the development of large general purpose machines. And there are, of course, stories around in the trade that this was by understanding with IBM. Whether this is true or not, I do not know. I was certainly not party to any such discussions. It may well be true but I don't know anything about it.

The man who, during this period, ran the Bell relay machines --specifically this MARK VI machine, the big, flexible, successful machine --after it was moved to Aberdeen in '46 or '47, whatever…the proper date, was a man named Dr. Joseph O. Harrison, Jr., who works for me here at the Bureau of Standards at the present time. Harrison later came up to the Watson Lab and took some of my courses at Columbia while on leave from Aberdeen; then went back and made still further contributions to the operation of these machines down there…then went into operations research in private industry, leaving to come to the Bureau of Standards two or three years ago. So, he's a living fossil of those days of ...

MERTZ:

What is his present position here?

GROSCH:

He's Director of our Standards Board, Chief of the Office of Information Processing Standards of the Center for Computer Sciences and Technology.

Now Harrison and the others running this actually used the relay machine, I believe, the Bell machine more than they used either the ENIAC or the IBM machines. The IBM machines were regarded, and I think quite properly regarded, as an extension of the punch-card installation. The ENIAC was under so much pressure to do sophisticated scientific work and was so difficult to actually use for routine simple ballistics calculations that, in fact, I think most of the advance work did evolve on the Bell machine. But, again, this is gossip. I don't know this first hand.

The IBM machines were never really described in much detail in the art, the article in NTAC and a few other places tells something about them. I still have the operating manuals, which were blueprinted. They were not fancy printed or hard cover books like the IBM MARK I machines for Aiken. And they, of course, since they were only made in quantities of five, why, the blueprint route was an acceptable one.

The physical machines may be of some interest. Although I don't want to waste much time on it since it's described in the literature. It was essentially a much modified reproducing punch. A reproducer, an IBM 514, was essentially a gadget that had two card feeds; one of which went under a full set of brushes to be read and the other one of which went under a punch unit for punching. So, you put in the primitive operation of the machine, you put blank cards in the punch feed and you put cards to be copied, cards which were filled up and were to be selectively copied over on the fresh cards in the card feed. And these two feeds remain separate. The cards went through their appropriate stations and came out into hoppers, could not be merged or interleaved in any way. What the Aberdeen Machines did was to attach to the back of one of these an enormous rack --by enormous I mean six feet square and two feet thick with several layers of relays within these two feet --of these C. D. Lake's wire contact relays, which worked very rapidly. The opening and closing speed of which was extremely high and the circuits, the pre-wired and plug board circuits were so arranged as to go through a pretty complicated set of multiplications and divisions--this machine was capable of dividing --and additions and subtractions and so forth. However, the storage capacity, as we would now use the terminology, was just a few words. The word length was only six decimal digits, as I remember it. The code for each decimal digit was biquinary as it was, at that time, in most of the Bell machines; a mixture of two and five base. And the maintenance (comment inaudible from RM) ... Yeah, right. The maintenance of the machine was extremely tricky simply because there was no school in which you could learn it. You had to be a specialist in keeping that machine, and only that machine, going. And to find such a specialist, Eckert sent down to the Naval Observatory --or to be more precise, to the IBM Washington office --and procured the services of this man, Richard Bennett, who had been our customer engineer on the Naval Observatory installation in 1940 and '41; the man who had adjusted the printer for the Air Almanac. Dick had to learn all about this relay circuitry because he was not a relay man. But, when he came out of service and went back to IBM for the rest of his career, he was taught that part of it in Endicott and then came down and became a very effective and valuable member of the team maintaining these two machines after their arrival. I presume there were also specialized engineers at Dahlgren and Aberdeen but I never met them myself.

MERTZ:

Did he stay on with IBM?

GROSCH:

He stayed on in his whole career, as far as I know. He published one article in an electronics magazine in the '50s… I literally can't tell within five years, somewhere in the '50s, describing a card operated --by which I mean card input, punch-card input --

servomechanism control measuring engine for large astronomical photographs; which Wallace Eckert had built at the Watson Laboratories and which Dick Bennett did the rather primitive electronic and electrical circuitry. So, that's his only appearance in the literature that I know of, but he's nevertheless a very valuable member of those early days and I liked him very much and respected his work very much.

I'll probably come back to some of that later on although it isn't really…to the work of the Watson Lab. It was Wallace Eckert's prime project in the next three or four years; prime personal project.

Bennett, however, had not joined us at the time I'm talking about when I was going up to Endicott to see the gradual creation of these machines and to try and figure out how they worked and how to wire the plug boards and so forth. He was still in the Army and we did not have plans, specific plans, for installation and operation of the equipment worked out in detail.

I might add that the plug boards on these machines were supplemented by some dial switches. Like ENIAC, you could set some constants into the machine by turning 10-position knobs on the end of the machine. I remember there were something like six figure numbers that one could set into the machine and this storage of constants was a valuable additional storage to the flexible counters inside.

Another line of equipment that was going to be available to us was also coming along at this time. Like the Aberdeen Machines, it turned out to be a dead end. It did not eventuate in a line of production equipment but it came from the brain of a man named Hans Peter Luhn. Now Pete Luhn was a mechanical engineer, Swedish by birth, for whom a memorial volume has been published by the American Society of Information Sciences. So a long story about his relationships with IBM and his personal ingenuity is available in the printed literature at the present time. That was published only a few years ago after his death.

Pete was one of these born inventors. Like Jack Rabinow (?), the inventor of the magnetic clutch and the optical character reader, who was at the Bureau of Standards in the early days and is now an important part of the control data family. Pete could no more stop inventing things than he could stop breathing. Everywhere he looked there was something to be improved. And he had channeled his energies very effectively into the IBM product line after joining the company in mid-career. At the time that I first met him, he had not gotten involved in information sciences and documentation at all; that came a little later. And, the product that he was working on was some very small specialized relay calculators, also using these Lake wire contact relays, but not in such vast numbers as in these Aberdeen Machines or in the other MAGIC, or other specialized equipment.

He took, for instance, a machine which we intended at the Watson Lab to use for the calculations for Ed Teller. He took a machine which was later, I believe, christened

Virginia --we had acronyms and so forth. To some of them, Aberdeen Machine was not a very pleasant way of describing the big one. He called his machines by female names and I believe the one I'm describing is Virginia. It was a modified key punch in which cards slid through endwise but, in addition to the normal position in which wire brushes would read the contents of the card as it slid under them column by column, there were additional flexibly placed rows of brushes. You could physically change the spacing between these rows and they were spaced out along the bend of the key punch. Now a card could be put into the --and these were beyond the punching station --machine and fed through. Intermediate calculations could be performed. The answer could be punched on that same card as it slid along and then that answer could be read again when it got to another one of these sets of brushes. The result was that you had the effect of storage without actually having to have storage counters. You simply used the card itself as an intermediate storage device. You could only reread what was punched but you could read it a few tenths of a second later when it got further along in its course through the machine.

MERTZ:

What was done with the reading?

GROSCH:

Well, the thing is that you could do a small sequence of calculations --very much in the way that you could in IBM and in Wallace Eckert's old modified 601 at the Astronomical Computing Bureau. Just as the circuit breakers turned in that machine and let you do a multiplication and then a different one, and then a different one, so in Virginia you could slide a machine through and then in the course of one-half pass through the machine, you might do two or three different multiplications. As I remember that machine also was capable of division.

MERTZ:

Each of equal complexity or ...

GROSCH:

Yeah but they were very simply. I mean it would be like multiplying two four digit numbers together and punching a rounded-off four digit answer, then reading that answer and adding something to it and then punching that and then reading that answer and dividing it by something else and punching that answer. Always on the same card. Practically no carry-over from card to card.

MERTZ:

... with the same belt ...

GROSCH:

No, there was no belt. All this actually consisted of was a key punch with multiple brush stations added to it. An ordinary card sliding through it and a small box of relays on the back. By a small box, I mean the size of a suitcase. Small enough to be strapped on the back of an ordinary key punch. This was a very compact and inexpensive looking gadget. Pete's skill, among other things, was economy. He could achieve amazing results with small amounts of circuitry and equipment.

He had a bigger machine which was strapped on the back of a 405 tabulator. The name of that machine escapes me at the moment. There was also a very bright young man who worked with him who used to come up and maintain these machines for us and modify them and fiddle with them. He was of Italian extraction and had an Italian name but I can't recall it unfortunately. A very nice, bright, young engineer, a professional circuit man.

MERTZ:

What time was this ...

GROSCH:

This was late '45. This was still at the time of which the Marya Mayers and the Ed Tellers would visit us. They had not yet been dissipated by the success of the bomb, the creation of the Atomic Energy Commission, the arguments about the May, Johnson and McMann (?) Bills and so forth; all of which came along in '46. So, this was late '45 I believe without having anything to go on, I was never told, but I believe now, looking back on the history of the Atom bomb project that this was probably part of Teller's Hydrogen bomb calculations. Mathematically very much more complicated than the shock wave work. I did not personally lay out the work the way I had before. I did not understand Virginia thoroughly and I still had other projects going in the main machine and I was getting ready for the Aberdeen equipment. So, this work was handed over to a bright young named Eric Hankam who was, I believe, a Hitler immigrant --someone who had left Europe because of the Hitler's persecutions --unmarried at that time. A bright, young boy without a very fancy degree, whom we had hired at the Watson Lab as another helper in Mrs. Harrick's (?) punch-card room. So, we handed that particular job over to Eric and he had more to do even with the Tellers and the Mayers than I did and a great deal more to do with Luhn and the machine. And he set up, after a great deal of effort, the work to do this and Then, as I remember it was terminated before we did any mass production on it. The changes that Luhn had to make in the machine were that we had to be able to carry some data over one card to another; that it was not satisfactory to just punch everything into the one card as you did in an old-fashioned machine. To carry it over from one card to another required some circuit changes, and some additions and more relays which Luhn and this young man did.

The control, by the way, instead of a plug board on this, there was a small plug board, as I remember it, strapped on the side of the machine but the major control was that in the old-fashioned key punch there was a master card which rode back and forth, which was not ejected every time like the working card but which rode back and forth. And you could punch holes in that card to control the action of the machine. There was a thing called a skip bar which was a metal bar with notches in it which controlled where the machine paused and where it punched, and where it did some other things like that. Things of this sort were done as part of the planning for the machine so that Luhn had made unorthodox use of what looked like a fairly orthodox keypunch.

MERTZ:

What became of these two?

GROSCH:

Those two machines and another machine that Pete built called the Survey Computer simply disappeared into limbo. To the best of my knowledge, they were both simply scrapped, dismantled. The Survey Computer was built on the back of an electric typewriter; an old, black IBM, fixed spacing electric typewriter which was a common article of currency in those days it was capable of doing some very restricted trigonometric calculations. A man doing plain survey work --I have one pamphlet on that, which is probably the only copy in the world. I just got a feeling that now that Luhn is dead, that there probably isn't another copy of that pamphlet in the world at the present time but it was a printed pamphlet describing this.

The idea was that you did plain surveys. You typed in the X and Y coordinate of a point on the grid then you typed in a distance and an azimuth that you had observed with your survey equipment The machine calculated the distance times the sine or the cosine of the azimuth(?) added it to the existing X and Y coordinates and gave you a new X and Y coordinate. Very primitive fixed calculation. No flexibility. It did exactly this. The accuracy was prescribed. The rounding was prescribed and so forth. And I worked out for Pete the very compact trigonometric table to be stored on stepper switches inside his box of relays for this machine but he did all the rest of it. Embodied the thing in actual hardware and did the multiplying circuits, which were taken from Virginia and so forth. So, a survey man could take this thing out into the field, plug into a wall socket, use it like a typewriter --in fact, you could do correspondence on it when you weren't doing this calculating. Disconnect the computing and do correspondence on it. And he would type out his survey report and every time he got through typing X, Y, R and theta, the machine would then go on and type next X and Y for him. He typed in the next R and theta and it would do the next X and Y and so forth. And, again, typical Luhn ingenuity and typical modification of inexpensive existing equipment but it turned out no market. I think they made a very few and sold them to the Army or something and that was it.

MERTZ:

What became of his assistant, Eric, do you know?

GROSCH:

Eric Hankam stayed at the Watson Lab for many years and he's probably still in IBM. He later became a teaching assistant at the university and in the IBM sponsored courses at the Watson Lab and in the mid-60s was still there. I haven't heard or seen him for at least five years. He was not the sort of person who went to a scientific or computer meeting so you didn't meet him after you left his vicinity, you didn't see him again until you visited that vicinity. But, he turned out, for instance, a long series of hectograph notes for classes that he helped Wallace Eckert run in machine operation, which described a lot of these trick ways of using the standard IBM machines. And, he also became expert at running the Aberdeen Relay Machine although that was never his primary assignment. And, in fact, became sort of the universal court of last resort on machine tricks during the late '40s while I was still there and in the early '50s after I'd left. He took some work at Columbia but I think never did get a Ph.D. I think he got a Master's degree or something like that and dropped off at that point. He took numerical analysis and had quite a capable finished formal education but never got to the point of being a professor himself. Nor do I know and I don't know what happened to the young Italian engineer that was doing all this good work for Luhn. I'm sure he also was reabsorbed into the IBM organization and probably worked for Luhn for the rest of his life. I simply never saw him again after the Virginia sort of thing gave up.

We had these two machines --not the survey machine which we only had visitors for a few weeks but Virginia and the other machine. We had it at the Watson Lab for nearly three years…, in fact, they were present at the time when Watson made his famous visit to the laboratory that's described in Roger's book, "Think," in which I said that I thought nobody ought to leave the living room decorated the way it was. It was those machines that caused Watson, as reported in that story, to tell Luhn that he ought to build a whole bank of relay computers down in the basement of the building which could be used remotely as sort of a forerunner of real time or even time sharing equipment as we know it today.

Now, you see you've got now all these different lines of equipment at work somewhere in the American milieu. You've got people interested in improving the differential analyzer. You have people building relay equipment out of telephone circuits and this, of course, includes Stibbitz and Sam Williams, and people like that. You have people trying to build improved IBM machines. You have the Moore School building electronic equipment and even off at Harvard University, you have Aiken laying out the plans for his MARK II, MARK III, and MARK IV. So, you're beginning now to have a computer industry. It's a one off industry still. Nobody has mass production and so forth but it's still going.

Now I want to come back to the influence of ENIAC on the business.

MERTZ:

Yes, and I was thinking that inasmuch that we're just about to run out of tape and the lunch hour ...

GROSCH:

Why don't we do that and have an early lunch and start on that again. Right.

MERTZ:

This concludes side 2 of the first tape of the interview on the 26th of August, 1970.