An Interview with

CUTHBERT C. HURD

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Abstract

Hurd begins by discussing his educational background and his post-war experiences. He recalls his work at Oak Ridge National Laboratory and his interaction with International Business Machines (IBM) in acquiring the first 604 for Oak Ridge. Hurd concludes the interview with an in-depth discussion of his move to IBM, his role in developing

scientific computers, and his interaction with John von Neumann and the various national laboratories, especially Los Alamos.

CUTHBERT C. HURD INTERVIEW

DATE: November 18, 1994 INTERVIEWER: Robert W. Seidel

LOCATION: Portola Valley, CA

SEIDEL: Today is Friday, November 18, 1994 and we are in Portola Valley, California to talk with Cuthbert Hurd about the Applied Sciences Division at International Business Machines Corporation. I was interested that you began your career at Drake University in Des Moines. I haven't seen in any of the interviews that I have read any account of what impact that might have had on your career or your interests. Did you study mathematics there?

HURD: Yes.

SEIDEL: Did that have any particular influence on your decision to go into higher mathematics?

HURD: The head of the mathematics department was named Professor Neff. Professor Neff had a wooden leg and a glass eye, he was about 6 feet 2, and a plenty strong character and naturally he carried a cane. Along about the time I was a junior he called me into his office, sat me down, picked up that cane, pointed at me and said, "Hurd, you are going to get a Ph.D. in mathematics." So I guess the answer is yes, it had some influence on me. When I graduated from Drake he telephoned the chairman of the mathematics department at Iowa State College and arranged over the phone for me to go there and have a graduate assistant ship. So, it was influential.

SEIDEL: Of course this was the Depression, and I know at least in physics and I suspect also in mathematics, the prospects for employment weren't great. How did it look for you as you made the decision to go to graduate school in mathematics? Did that look like something that would pay off in the long run or was it a real risk?

HURD: At that time a mathematician either taught or went into the actuarial field, and I had taken a few courses along the way in financial mathematics, actuarial mathematics, as a backup knowing if I went to graduate school and could

not find a job I probably could go into actuarial work. But it was a risky thing.

SEIDEL: Was your family supportive of you going into mathematics? Were they interested in that kind of life for you?

HURD: Well, they were very supportive always in every way. My father and mother were both ministers and they originally had hoped that both my brother and I would be ministers. I graduated from high school in Fairmont, Minnesota, and in high school I played in the band and the orchestra, sang in the glee club and then entered a state contest for singing, and won the state contest of Minnesota. The contest was held on the University campus and the head of the voice department at the University of Minnesota then said, "If you will come to the University of Minnesota and study music, we will arrange a scholarship for you." And also Carleton College said, "If you will come here and study music, we will arrange a scholarship for you." So I had this tug between music and mathematics. The superintendent of the high school said, "Why don't you consider mathematics as a vocation and music as an avocation?" Which I did.

SEIDEL: Well, that's truly Pythagorean association between music and mathematics. I was intrigued in reading some of the interviews that you've had that you ran into a punched-card installation that Henry Wallace had installed at Iowa State. Was that correct?

HURD: Henry Wallace did his work, I think, in about 1924 and my only association from that was hearsay from a man named Dr. A. E. Brandt who was a professor of statistics at Iowa State College where I went in 1932, and I minored in statistics which at that time was just becoming mathematical. And the work of R. A. Fisher was very influential then and they had a small punched-card installation. I ran, fairly modest. Can't really call it calculations because there was not a calculator. IBM had not yet invented a calculator. They had a keypunch, verifier, sorter and a tabulator, and a tabulator would add and subtract and transfer and print. And I ran a small calculation of some kind, I can't even remember the subject, but I knew about punched-cards.

SEIDEL: Were you particularly intrigued by this method of calculation at the time?

HURD: No.

SEIDEL: It was just another machine like the old cranking calculators that people had?

HURD: Yes.

SEIDEL: You then did a dissertation in mathematics at Illinois where you got your masters degree under a man named Truizinsky.

HURD: That was a Ph.D. under Truizinsky. My masters dissertation was under Dr. Allen and was in differential equations and I showed that a certain class of differential equations could be solved by infinite continued fractions and presented that paper at the Iowa Academy of Science. I did the Ph.D. under Waldemar Joseph Truizinsky.

SEIDEL: And what was the subject of that?

HURD: It was the "Asymptotic Solution of Linear Differential Equations singular in the variable of differentiation and in a parameter."

SEIDEL: And both of these used then sort of numerical methods in terms of the calculations, is that correct?

HURD: We would now call that dissertation solution the development of an algorithm because there was a procedure for the approximation of that solution, but nobody talked about algorithms then.

SEIDEL: I know you taught for a couple of years.

HURD: After I got a Ph.D. I went to Michigan State College and while I was there I spent two summers in New York at Columbia University because the library of the American Mathematical Society at that time was in the library of Columbia University. And, if you=re interested in the punched card background, I also had determined that Michigan State College, a little-known college except for agriculture, did not have a very good method of finding positions for its graduates. I would do research in the morning at the library. In the afternoon I would call on corporations in New York and I met the general sales manager and the vice president of operations of IBM. I also met the vice president and general manager of Remington Rand and all he wanted to talk about was an electric shaver which Howard Hughes had carried when he took his flight around the world. But the IBM people wanted to talk about punched cards, one result being that when I went back the registrar named Bob Linton and I installed what may have been the first punched-card installation for registration in the United States at Michigan State. That's my second brush with punched cards.

When the war came I had a very low draft number and I did not want to be a private so I scurried around to try and find some place where I can use mathematics and found out that there was a vacancy at the United States Coast Guard Academy in New London, Connecticut. I went to the district headquarters in Cleveland, Ohio, was interviewed and sent to the Coast Guard headquarters in Washington, D.C. I was appointed a lieutenant junior grade in the United States Coast Guard Reserve and went to New London in June, 1942.

SEIDEL: And that's where you worked on LORAN tables and put in another punched card installation.

HURD: Yes.

SEIDEL: This was the second one then that you had been involved with installing. Did you work closely with IBM in putting in that installation?

HURD: Well, in the sense that we got our priority to get the installation and the manager came up a couple of times, but mostly we got the manual and did the work ourselves. And I had what we used to call yeomen. That was the classification of Coast Guard personnel who did clerical kind of work. So we did that. We started on firing tables and then did some work towards improving entrance examinations like correlations and one thing or another and that was it.

But by this time IBM had a multiplier which would not only add, subtract and multiply but also divide. Imagine that.

SEIDEL: I'm intrigued by your story that you installed this machine or this installation yourself because I remember the stories that are told at Los Alamos that the machinery arrived and they were under strict instructions not to do anything until the IBM representative also arrived and Feynman and company went ahead and installed it before he arrived. But it sounds as though this was not that uncommon because you did the same thing at the Coast Guard Academy. IBM didn't particularly object to your doing that sort of thing?

HURD: No.

SEIDEL: So you think the story at Los Alamos is apocryphal or was there such a restriction? Were you supposed to wait until a technician arrived to set it up?

HURD: I think the restriction was you were not supposed to take the covers off.

SEIDEL: Oh, I see, you could take the machines out and connect them up but you just couldn't take the covers off.

HURD: The machines each had a special function. You had a box and you had a control panel for that box and you had a manual which told if you could put a plug in this hole and connected the wire to the plug in this hole, such and

such happened. But the stories I've heard about that . . . let me see if I can think of some of the names. At Los Alamos at one time there were six Ph.D.'s and they had the IBM installation in a circle. It started out with the sorter, the reproducer, the collator, the calculating punch, and the tabulator. And apparently they had some work tables around but when they were making those calculations, apparently each one of these fellows would take a small deck of cards out of his machine, look at it, perhaps rearrange it manually, hand it to the next man, and around and around they'd go until they finished that calculation. Now I don't know whether Klari von Neumann was one of those persons or not, but she learned punched cards at Los Alamos and met John von Neumann at Los Alamos. Among those at Los Alamos were Bengt Carlson, Eldred Nelson, Preston Hammer, and Nick Metropolis.

SEIDEL: Well, Feynman has the story that when he had his special engineered detachment recruits running that installation -- the IBM punched-card installation during the war at Los Alamos -- the implosion calculations would take several months to do, and he figured out a way to do three at a time by running them in series through the machines and the way he did that was after he took charge of the group -- I think Donald Flanders had been in charge of the group before when it was chiefly staffed by women and beginning early in 1944 they began to staff it more and more by men from this special engineered detachment -- and the women had just been given first before the machines came hand calculations that they would all do and then this transferred over to the machine calculations and the men were told what part of the process they would do but according to the principle compartmentalization you don't tell them what the whole process is about. Feynman, of course, thought this was ridiculous and he said, "This is what we're trying to do. We're trying to make these calculations, to devise an implosion weapon so we can win the war." And once they understood what they were about, then they all got together and they worked out this method of running three calculations at once rather than one calculation at a time. So that may be some of the background of what you observed after the war. And so that story leads me to believe that what you saw after the war was some continuation of that process. Now at that time probably Metropolis wasn't back yet. I don't know at what point you visited Los Alamos in 1949. Richtmyer would have been around as head of the theoretical division, Carson Mark would have been around. Metropolis was just about coming back then too because he had been hired to build MANIAC, sometime early in 1949 or late in 1948.

HURD: Coming back from where?

SEIDEL: The University of Chicago.

HURD: You talked about Metropolis' modification of the ENIAC. My belief is that von Neumann had the idea of using the vast number of registers on the ENIAC to store permanent instruction and that then Metropolis and I don't know who all else followed up on that idea. Now if you'd ask Eckert, he'd say the opposite, and if you'd ask Mauchly, Mauchly would say the opposite. And I don't really know. I tried many times to get von Neumann to say if did he or did he not invent stored program and he would never say. He would say, "I noticed that..." and then he would talk about using the registers to store the numerical data and he never said explicitly, "I invented stored programming."

SEIDEL: You spent a couple years teaching after the war and then you went to work at Oak Ridge, at the Gaseous Diffusion Plant, and became involved with calculations of a more efficient through-put process for that plant. You talk in some of the interviews about designing new plants including the one that was finally built at Portsmouth. I would suspect from my own knowledge that new plants were being thought of probably more after 1950 than the late 1940s but were there already thoughts of making new plants in the late 1940s?

HURD: Yes. This came about. When I was at the Coast Guard, the commanding officer was a remarkable person in that he was not only an excellent officer but I would call him a humanist, and he and I started out to revise the curriculum which we did. So if there was a dean at the Coast Guard Academy, I was it. At least I don't want to be called the president. I got this job at Allegheny College as a dean, and I was a terrible dean because I was so idealistic, you know I thought that they were all interested in the good, the true, and the beautiful, and they probably weren=t. So my brother, Frank had worked with Al Nier at the University of Minnesota, got his Ph.D. under Nier.

SEIDEL: Do you know Nier recently died?

HURD: Yes. And therefore Frank learned about mass spectrometry. When the war started he was employed at Columbia University on the atomic energy project, and then when they decided to build Oak Ridge, I think he was the first person there.

SEIDEL: Did he work with Dunning's group at Columbia?

HURD: Yes, he worked for Dunning. And had charge of building the Works Laboratory at Oak Ridge. What he learned from Nier was vitally important to him. They had batteries of spectrometers trying to measure very high purities. And by the time I went to Oak Ridge the Atomic Energy Commission (AEC) became very concerned about possible unauthorized diversion of highly-enriched uranium.

TAPE 1/SIDE 2

HURD: And they had Arthur Squires, and Manson Benedict who went on to be the professor of nuclear energy at MIT.

SEIDEL: Manson Benedict?

HURD: Manson Benedict. Benedict and Squires started worrying about this problem as had all the management in the AEC. And a man named Cuthbert Daniel had been there working on it from a statistical point of view and he left. And my brother knew I had had some training and a minor at Iowa State and Illinois in statistics, so he called me up. And I went down there and started to work for a man named Dr. George A. Garrett who later became, I think, at least the U.S. expert on the whole uranium cycle along with Benedict and Squires.

SEIDEL: Did Garrett go up with Benedict when Benedict went to the operations analysis staff at AEC headquarters in 1950?

HURD: No.

SEIDEL: He was not part of that group.

HURD: My, you know a lot. How can you, so young, know so much?

SEIDEL: Well, one of the things I'm doing is writing a history of the Radiation Laboratory at Berkeley and as you know they were involved in Y-12, and as a matter of fact right at the moment I'm writing about the M.T.A. -- Materials Testing Accelerator. Manson Benedict put an end to that project because when he was head of the operations analysis staff at the AEC in Washington.

HURD: There's a question of words here, but this man George Garrett at Rice had a Ph.D. in differential equations under Professor Evans who went from Rice to UC Berkeley.

SEIDEL: Oh yes, Herbert Evans.

HURD: George had worked as a geophysical prospector, went around on a crew, made hand calculations, etc. and when the war started was recruited at the MIT Radiation Laboratory and worked on some of the first airborne radar, I think in the Navy section.

HURD: Anyway, right at 1946 Benedict learned about George Garrett and had him recruited to go to Oak Ridge and start what was really an operations research organization. Operations research at the time was a very popular term because the Operations Research Society had just been started. I think that George Garrett was certainly the first person I knew about in the atomic energy complex that did that kind of work. And George had, of course, a large room, all women, all hand-driven calculators, and they were calculating stage by stage calculations from the bottom of the plant to the top of the plant, etc. And I came down there primarily to look at this problem of accounting for

materials, all of which was being done by hand. Whenever a barrel or something would leave this place and go to that place somebody would fill out a receipt, both the sender and receiver would sign it and it would have the weight and the assay, etc. and you would try and keep track of it in that sense. Now, immediately I got a punched-card installation and set that thing up so at least you'd have the transfer document become a punched card, and you could always track something if you wanted to, and so on. And we actually made a material balance, my facility. If there was any material in the Works Laboratory where they were assaying it, when that material went in, when it went out, results in, etc. etc., and we found ways to produce an entire material balance for the plant together with some precision calculations so we at least knew what we had.

George Garrett and I and a man named Jack Kelly, said, "Well, why don't we try to automate these hand calculations," and we started that. In the meantime I had become friends with von Neumann. I'd known him as a mathematician but didn't really get acquainted with him until 1947. I was trying to get him to come to Oak Ridge and to help me. I got acquainted with him, learned about his plans for the machine in Princeton. Alston Householder also was traveling around the country and interested in computing, etc.

SEIDEL: Can I ask you about Householder's interest? Why was he interested in computing particularly? Do you know?

HURD: Well, he was in charge of the mathematics section at the Oak Ridge National Laboratory and originally while at Chicago, possibly with Weinberg but I'm not sure of that, he had been interested in medical questions and in particular the application of the technology of physics and mathematics to biology and medicine.

SEIDEL: That=s a very similar interest Warren Weaver=s, who was with Rockefeller Foundation and promoted the application of physical and chemical techniques to biology and medicine.

HURD: And I promoted the use of mathematics at the Rockefeller Institute by hiring for Detlev Bronk, the President

of the Rockefeller Institute, also the president of the National Academy of Sciences, a mathematician named Kogbetliantz[1]. He attracted Mark Kac and others and away they went. But to answer the question, I don't know exactly what Alston thought comp utation could be used for although he was always interested in matrix theory. I can't tell you. If you want to know, there's a man named Gene Golub at Stanford much younger than Alston but an intimate friend.

There was a meeting in 1948 at the Institute for Numerical Analysis. Householder and I both attended that meeting in Los Angeles. Goldstine in his book claims that Householder introduced me to computing.[2] That was false because we were friends. We were, in parallel, interested in computing. The one thing about punched cards is that that group of people -- Householder, Flanders . . . I have to remember the name of that man who was at Illinois and who built the ORDVAC. He sort of pooh-poohed . . .

SEIDEL: Ridenour?

HURD: Ridenour. Amazing. He sort of pooh-poohed punched cards and they all wanted something more powerful even though . . . you remember the von Neumann constant? Even though the von Neumann constant was a fact. Everybody would always say well, six months from now I'd have something which was the von Neumann constant. I don't know how he got interested. Anyway, to go back to it, Dr. Garrett and Jack Kelly and I, started then to work on these calculations which were really simulation calculations, K-25 was already built and then K-27 and K-29 was built based first on the Benedict-Squires-Garrett kind of thing, and the calculations were done more and more on punched cards. Shortly after that I left and joined IBM .

SEIDEL: Out of this early work that you did while you were still active at Oak Ridge and not when you were a

[1]Ervand Kogbetliantz, 1888-.

[2]Herman H. Goldstine, *The Computer from Pascal to von Neumann* (Princeton: Princeton University Press, 1972), p. 329.

consultant but before, were there technical reports produced that would describe that work? Was it written up in any way? Because I've looked in a preliminary fashion at some of the early Oak Ridge stuff but haven't run across it.

HURD: Well, I can give you two names and if I go in the house I might be able to get some of them, but Dr. Garrett might know. His memoirs are soon to be published in the *Annals*.

SEIDEL: But you were writing technical reports at the time.

HURD: Yes.

SEIDEL: So somewhere in the system those reports exist and Garrett and you would have been among the authors.

HURD: Yes, and Jack Kelly.

SEIDEL: You've also said in your interview with Evans that Thomas Watson, Sr. expedited the delivery of this punched-card installation to Oak Ridge because he had a particular interest in atomic energy. I was wondering what the nature of that interest was?

HURD: Well, first this doesn't have anything to do with it, but during the war John McPherson who at the time was vice president of engineering at IBM made arrangements to make some modifications whose details I'm not familiar with for machines which were sent to Los Alamos. So he was interested, but Mr. Watson I think was primarily interested in the atomic energy and made a trip once to Knoxville and was taken to Oak Ridge and may have seen a punched-card installation at Y-12 which was under the management of a man named Fred Uffleman.

SEIDEL: So this preceded your installation?

HURD: Yes. And then when I went to the IBM seminar in Endicott (Eckert, Grosch) in the summer -- it must have

been 1947 -- I was invited on the way back to Oak Ridge to visit World Headquarters and became acquainted with a number of executives as a result of which when the SSEC was dedicated in February 1948, I was invited and saw some more of these men who told me that the 604 was being developed and I asked them to deliver the first one to Oak Ridge.

SEIDEL: Now this is after the original punched-card installation was set up?

HURD: Yes, the original punched-card installation and the 602A. The next thing that happened was that there was a project to design a nuclear airplane.

SEIDEL: NEPA.

HURD: Yes. And Wendell DeMarcus (?) was in charge of shielding calculations and he came over and we tried to figure out how well the punched-card installation could make these shielding calculations. And it was very laborious and slow. In the meantime I knew about the SSEC, so I got in touch with R. R. Seeber, Jr., who managed the SSEC and said "put this calculation in the queue." But I was now face-to-face with a calculation which I understood and from which punched cards, even with the 604, were very inefficient. So now I knew there had to be something better which helped me decide to write a letter to IBM to apply for a job. And the sequel to that is that after I joined IBM the calculation was actually run on the SSEC.

SEIDEL: That interests me because you were in Oak Ridge in early 1948 and you've talked about several applications of computers at Oak Ridge. It seemed to me that there would have been (and we know later on that there is a computer built for Oak Ridge although it's built up at Argonne by Chuan Chu and others), an opportunity at Oak Ridge as there was later at Los Alamos to develop computing, and yet you chose to go to IBM instead. Is that because you thought the action would really be at IBM? And I ask this, of course, because I know from your later comments that when you got to IBM you found a lot of resistance to going beyond the SSEC and as I take it you

were hired there essentially to provide an applied science program for the SSEC, not to develop new computers.

HURD: Your question is why when we had problems which needed better than punched cards, why not stay in Oak Ridge and possibly with Householder go up to see von Neumann and try to copy his machine. I am a "do it now" person and I did not have much confidence in any university building a computer which would have general usefulness because I thought the mission of a university was research. I didn't have any doubt about their ability to build a computer which would work if anyone could, but I assumed that having built it they would want to experiment and make changes, improvements, etc., and I did not think that was the best way to serve the overall need and demand for computing which I was just coming to understand. Also, partly through NEPA I became acquainted with people at Northrop, Douglas, etc. and knew what they were trying to do. So that's why I chose to go to IBM.

Now, your second question, did I go there to form applied science and work with the SSEC? No. I went there and had no assignment except that the people who interviewed me, including Tom Watson, Jr. who was probably a vice president and not yet executive vice president, and Charlie Love,who was the general sales manager, and Wallace Eckert, and John McPherson. At least Watson and Love had some general feeling that I could help IBM participate more fully in scientific computing. They didn't know how and I didn't know how, and so I went without an assignment, without an office, without a boss. And then within the next six months, I got the card programmed calculator going, and started to hire people. I had an unlimited budget. If I saw a person to be hired, I would hire that person and somebody would take care of finding the money. And then in December after the second 1949 scientific computation forum which Tom Watson, Jr. listened to without coming in . . . .

TAPE 2/SIDE 1

HURD: Tom Watson, Jr. and someone else whom I don't remember came up to Endicott and the sound was piped into another room and he listened -- I don't know to how much of it -- but, of course, was impressed -- he never told me this -- but had to be impressed by the fact that people like von Neumann were there, and Flanders, and people

from Oak Ridge, people from Los Alamos, all the aircraft companies. John Curtiss was there, Mina Rees was there, all these people were there, and presumably because I knew them and they were friends and that's why they came. Well, I get back down to New York and was called into Watson, Sr.'s office and the Applied Science Department was established on the spot.

SEIDEL: Was that your intent in calling this conference, to sell the company?

HURD: Partly.

SEIDEL: You were also trying to learn what possible scientific applications were out there.

HURD: Yes.

SEIDEL: Who invented scientific computing, you or von Neumann? Because this is something that's rather new at this time, isn't it?

HURD: You mean the term?

SEIDEL: The concept of using the electronic computer for science.

HURD: Well, Vannevar Bush wrote a proposal in around 1939 to the National Cash Register Company and whether he used the term scientific computing, I don't know. But he certainly intended to build a digital calculator of some kind because he knew, I presume, what the analog computer would and would not do. Now I would think Vannevar Bush didn't get the money and then you'd have to say, "Well, how about our friend at Harvard, Howard Aiken". He came to IBM, I think, in 1939 and made the proposal which lead to the MARK 1 Automatic Sequence Controlled Calculator. But I doubt if Bush and Aiken had anything to do with each other, but I don't know. They were three or

four miles from each other, but I don't know. I never thought to ask Howard. But I don't think there's any question about the fact that the decisions IBM made were responsible for the principal impetus in scientific computing. I think that's incontrovertible, but I would not say that they originated the idea.

SEIDEL: How important was, in your view, the fact that there was a large interest in the atomic energy labs that, of course, you represented in a sense, von Neumann in a sense represented, in terms of IBM recognition that -- now, talking about applied science -- that computers were going to be an important area there? Obviously they're bringing you from Oak Ridge in part because you were at Oak Ridge and you were applying computers there. Von Neumann has a great interest in this field. This is when the whole thermonuclear weapon calculations are being written and they are waiting for machines to run on. The SSEC's second problem was one of these big calculations -- the first moon orbit.

HURD: Was it "Hippo"?

SEIDEL: I believe it was "Hippo," but I'm not sure.

HURD: The first calculation was a moon calculation which Eckert was interested in, and then it took at least six months, possibly nine months, to take that problem off and get Richtmyer's problem on. But you were asking a question and I interrupted you.

SEIDEL: I just wondered in terms of a market for this what your doing, applied science, one could see the military it provides a certain market. They have developments in big analog computers. They are working on some big electronic computers. They're responsible for ENIAC. Then you have, and I think here a more scientific audience in the national laboratories. These are people not only interested in practical applications of computing but they're also scientific applications of computing that go beyond what we might call applied science to fundamental science, and certainly these people are well represented at this 1949 symposium that you hold as are the military people so there

seems to be a mix there and I was wondering how important were the AEC labs in that? How important were the military? You say the universities you expected would be doing a different sort of thing, and they're not very well represented at that meeting. There aren't a lot of people from universities there. So you're doing sort of a market survey here in 1949. I wonder what the market looked like to you at any rate.

HURD: It looked like atomic energy and aerospace, and I do not think there was a single representative of the military in those 1949 symposiums. It's also interesting that even though a number of government laboratories got CPCs -- there were 250 CPCs -- that not a single one of them outside of atomic energy got a defense calculator 701. Not a one of them. Wright-Patterson tried to order one. John Curtiss at the Bureau of Standards Institute of Numerical Analysis tried to order one. The Cornell Wind Tunnel -- I guess that was not government, that was a university -- tried to order one. And there was an Air Force laboratory somewhere in upper New York state -- Rome-- tried to order one and were not able to get funds, so even though James Birkenstock and I, pestered the Pentagon and we might have gotten a couple letters of intent but no orders to be delivered from the Pentagon or the Navy or the Air Force or the Marines -- got none. And I think it was a bureaucratic delay and not a delay in interest because certainly John Curtiss and Mina Rees knew as much as anyone in the world how these machines might be applied, so it had to be primarily aircraft and atomic energy, particularly because of the Korean War and the name "Defense Calculator" and the need for physicists to obtain parts. And, of course, the atomic energy was closest to research. I'm wrong. Because one of those machines went to Philadelphia to that Naval supply depot and one went ultimately to the Weather Bureau and one went to General Motors.

SEIDEL: But none of these were used really for scientific computing, the last three that you mentioned, particularly the 701s.

HURD: No, unless you equate scientific and engineering.

SEIDEL: Now, the 701 you have said in various places was essentially a von Neumann type computer. If you were to

compare it to MANIAC, what were the significant differences if any on the von Neumann type computer using advanced Williams tubes, etc.?

HURD: Well, these are qualitative differences: it had more high speed memory, and I don't remember how the MANIAC finally came out, but at least with respect to the Princeton machine, was faster. Everybody used to talk about you want a thousand multiplications a second. So when the 701 was delivered it had two thousand multiplications a second, and it had a drum backup memory and it had magnetic tape, not punched paper tape, and it had punched-card input/output and a printer directly attached which is what you need in a production environment whereas at least on the Princeton machine, they were content to have possibly a photoelectric tape reader, although I don't remember, and a thousand words of memory. So scientifically or technically that was the difference. Now, of equal or greater importance, when the machine was delivered it had software. The word had not yet been coined. It was called programming. To the best of my knowledge the first bootstrap program was delivered with the Defense Calculator. Prior to that you set switches manually to turn the machine on. That was written by Ted Codd. He's the inventor of relational databases and won a Turing Award. And it had an assembler. The von Neumann machine you assembled by hand. And of course it had utilities like decimal-to-binary conversion and binary-to-decimal conversion, all that kind of thing. And very quickly thereafter had an interpreter, Speedcoder, and that interpreter was a direct analog to the CPC methodology because people -- and I want to say Los Alamos again, Inyokern, and Rand--I wish I could remember their names -- developed the so-called general purpose board which would do floating point calculations, based on a three address code.

SEIDEL: Voorhees?

HURD: Yes, that's it. And there's one prior name who was involved -- Preston Hammer -- and a tall Ph.D. who wore glasses, became involved in an altercation of some kind and they both left. It was a three-address system. You'd punch three addresses and an operation in a card and feed that into the Card Programmed Calculator and you had a three-address instruction, so the first interpreter was a three-address floating-point interpreter. And people started

writing those. Los Alamos wrote one. Dick Hamming at Bell Labs wrote one. William Kern (?) wrote one. Rand wrote one. IBM wrote one. This was really the start of software as we now know it. But the point is that the day the 701 machine was delivered you had the software set and you could start using it quickly. Again, persons at the universities were smart enough but that was not yet their pressing duty.

SEIDEL: You talk about the software influence. I wonder about hardware influence -- Metropolis is, at the time you were working on the 701, was or had been working on MANIAC for some time. He made some innovations. Were you in touch with that work that was going on?

HURD: Yes.

SEIDEL: And so did IBM learn from those processes as well or was there a sharing of ideas in terms of architecture or design in the machines as well as in the software end of things?

HURD: Well, we were certainly close to Princeton and specifically to Metropolis and to [Julian Bigelow?] Herman Goldstine, the man who actually built the machine that ran. Can't think of the name. We were certainly close. Every time I went to Los Alamos I always tried to see Nick [Metropolis] and I would tell him what we were doing and he would say what he was doing, and I always communicated those ideas to our engineering people. I can't point out any specific development of Nick's that was incorporated at IBM. I can't remember any detail, and I know he and . . . who's the man who actually built the Princeton machine? Bigelow. But they were always very, very cooperative. By the time Flanders, who was a good friend, was deep into that construction, we already were shipping a machine and thinking what to do to make the 704, and Argonne did not buy a 701. The only 701's at an atomic energy facility were at Los Alamos and Livermore. Similarly, works at Rand.

SEIDEL: I'd forgotten that Livermore did get a 701. Was that after the Univac? There first machine was a Univac I and they got a 701 after that.

HURD: Yes.

SEIDEL: You had a representative at Los Alamos in the late 1940s. You mentioned that . . .

HURD: Lloyd Hubbard.

SEIDEL: How long was he there?

HURD: Several years. He now lives in Minneapolis. Lloyd C. Hubbard. He worked for IBM. Was ultimately promoted to world headquarters and reported directly to a man named Frank Cary, later president and chairman of IBM. And one of Cary's contributions, of which there were many, was that he organized and codified what I would call the product planning process. And Hubbard worked directly with him, probably as a person who knew the most details of computers, and then Norris hired Hubbard, according to Hubbard, to install that process at Control Data, and that's all I know of the story.

SEIDEL: You recruited a number of people for the Applied Science Division. You say you had an unlimited budget for personnel to do that. Did you recruit anybody from the national labs, from Oak Ridge, at that time?

HURD: None from Oak Ridge. The first one I remember from Los Alamos was Sid Lida and he probably worked for Bengt Carlson, and he came and actually wrote the first 701 manual. And then there was another one, a very productive man whose name I can't remember. I'm sorry. I can't remember it.

SEIDEL: But that was an obvious place to recruit, the AC Labs, as far as you were concerned.

HURD: Yes, but very delicate. Because IBM had a policy of never recruiting a person from a customer. When I was

joining IBM there were lots of negotiations.

INTERRUPTION

SEIDEL: Bruce was just saying while you on the phone that as you were talking about Lloyd Hubbard at Los Alamos, he's finding considerable correspondence in there from him, so you're already being evaluated.

HURD: Good, good.

SEIDEL: In fact, one of the projects we're working on at the Babbage, or thinking of working on, is a study of the relationship between the application of the computer to science and the National Labs and some of the other military labs in particular because in an early study that I did for a talk at the International Congress for the History of Science, it seemed to me that many of the significant early applications took place in places like Los Alamos where Fermi and Metropolis and Teller were available, and as soon as MANIAC was on line and the 701 comes in, and are able to do the kinds of experiments that had not been possible before, somehow sandwiching that in with all the supercalculations and other things that had to be done, and so it's very interesting to know how the relationship between Los Alamos and the other labs and IBM evolved and what they've learned from each other. Arthur's particularly interested in the side of the question that relates to the contributions that those uses might have made to the design of machines. In other words we have a general purpose computer evolving here, but as computers evolve there are certain design features that built in for certain customers that tend to lead to more serial invector machines than parallel machines, for example, so there is some feedback from the intelligent users into the machine design and so one of the questions he's particularly interested in is that question. And, of course, he's been studying the early industry for a long time and is going to be looking hard at IBM's development to see how that . . . of course he knows the ERA story pretty well, too, and the Univac side was also developing general purpose computers, and you say at one point in your testimony that that was what convinced you that there could be such a thing as a general purpose ...?... to a scientific and commercial computer was the Univac I, which is an interesting point. Because I gather you

saw the 701 and 704 as essentially scientific computers.

HURD: Well, that is almost correct, but number 1, if I had said in December 1951 that we are going to build a machine which will be equally applicable in science and accounting and management, I would never have gotten the money. But the fact that it was science and the Korean War was going on and I could demonstrate that the atomic energy laboratories and the aircraft laboratories needed something and it was science, and they [IBM] said, "Well, Hurd and his people know what they're talking about. They know mathematics, etc. So let's do it." But if we'd said we were going to build what ultimately became the 360. No chance. But on that point you earlier mentioned von Neumann promoting the field of scientific computation. He asked me to go to see Fermi at Chicago, which I did, to talk about computing and how it might be helpful. So that's when I became acquainted with Fermi. It was certainly after Teller moved to Livermore. Do you remember when that was?

SEIDEL: Teller went to Livermore in 1952.

HURD: Right. It was certainly after Teller went to Livermore and John wanted me to meet with Teller and talk with him about a scientific computer and a kind of work that Teller wanted to do. And before that time I had seen Teller once in a fairly large meeting of some kind at Los Alamos and I'm sure I made no impression on him. So I did not really get acquainted with him until after he was at Livermore, and whether before or after von Neumann wanted me to get acquainted with him, I don't know. But von Neumann was always looking for ways and certainly one of his great contributions was his general scientific and mathematical contributions, along with the fact that he was a good promoter. And Tom Watson in his book, he doesn't quite get the story straight sequentially, but I know that when I told him that I wanted to hire von Neumann as a consultant, he knew von Neumann's name.

TAPE 2/SIDE 2

HURD: In some way or another it meant a lot to Tom that von Neumann put his stamp of approval on the 701, the

Defense Calculator. I can't remember how that happened, whether I brought von Neumann up to Tom's office and he met him and that's when he asked him, I don't remember the details, but I know that it was important to Tom because Tom was leading the company off into a new direction when frankly many at IBM were very happy with what they were doing. He took a tremendous risk, really.

SEIDEL: I know that you originally had thirty some offers for the 701. The price went up and they dropped to six, one of whom, I presume Los Alamos, hung on to their order at that point, and I think you said that you estimated that eventually 19 might be built. So you were taking a risk, too, by saying something like that.

HURD: Well, the nature of the risk. I can't remember, whereas Nick had maybe a half a dozen engineers and at Princeton maybe a dozen. There were all these small groups around. We went into this and Palmer and Haddad divided the whole thing up into subsystems and hired people for every subsystem and went at it, and we didn't have any budget, you know. The treasurer, Al Williams, gave us approval. We just got the money we wanted. If we could cut a month off of the schedule we could get more money. As I said, I had no budget at all. One time, Learson who was the general sales manager, sent a telegram to every one of his branches when we were trying to recruit people and he says, "Hurd needs a hundred men." Now think of that -- a hundred men. Now I think we'd get sued because he didn't say men or women, but a hundred men. So we just, you know, we just went at this. And one result, and I've told this story before, was that Mr. Watson, Sr. called Tom, and Williams the financial man, and Wally McDowell the engineer, and probably Birkenstock, I'm not sure. He had all these figures showing how the profit was going down and all this simply because we were putting so much money into the 701 to be followed shortly by the 650, and then the 702. Dozens and hundreds of millions of dollars and this company was really at risk if it didn't go.

SEIDEL: So it was all riding on you and Tom and Birkenstock and von Neumann?

HURD: Palmer who was responsible for the 604 and the 603, had become convinced Rochester, Astrahan, Buchholz, had become convinced, not because they wanted to build the Defense Calculator, but because they knew they

couldn't do what they wanted to do which was to build what became the 702. I said, "There's no way we can get the money for it." And I had taken Palmer around including once down to Cape Kennedy, and the Weather Bureau, and so on, and just let him listen. And he became convinced. And then I asked George Brown and his boss whose name was Williams from Rand to come in to give Rochester and Astrahan and Buchholz and possibly Dunwell, a chance to listen to them. And they were almost impolite. They made fun of the proposed plan of the 702. It could not possibly serve their need. And now Rochester said, "Well, we can't build that. Let's build something." So he got on the Defense Calculator. But no question that von Neumann was a strong influence even though he did not yet have a consulting agreement.

SEIDEL: Did it make a difference when von Neumann joined the General Advisory Committee, AEC, or when he became a Commissioner? Does that increase the confidence of the company that computing is going to be an important part of Atomic Energy Commission programs?

HURD: Well, I don't think that, because by that time . . . when was it, 1955?

SEIDEL: He became a member of the Commission in 1955. He became a member of the GAC in 1952.

HURD: Right. But by that time we were already shipping the 704 -- very successful, highly profitable machine. Maybe the most profitable IBM ever produced. I don't know. And we changed one little circuit and went to the 701 to 704, and what, doubled the price? I'm not exactly sure but we sold them like hotcakes -- they were enormously profitable. But I kept in touch with von Neumann as a friend and one thing that happened was von Neumann was a member of the Project Charles Committee, and the Charles Committee through the Air Force and MIT essentially made the recommendations which led to the SAGE Project.

SEIDEL: So you saw that coming?

HURD: Well, the story I want to tell is that I was a part of the negotiating team when we first went to MIT. Really the key was Forrester came to Poughkeepsie and here we had four or five 701s in some state of construction and that was the only place in the world you could see that. So that was really one of the keys -- Forrester's visit. Whether Valley and Wiesner came, I don't know. I remember Forrester. But along about this time von Neumann, was a member of the General Advisory Committee. The Oppenheimer thing took place. We knew the Russians= had a hydrogen bomb and von Neumann was worried. I'm sure Teller was worried much more, and others. Now von Neumann asked me to come to Washington and we spent a day, maybe two days, I can't remember, in the Mayflower Hotel in a little suite, and Convair was bidding to the Air Force on a missile. I may get the name wrong. It might have been Atlas. And for that project they needed a ground-based computer to control at least the initial launch, and we had some extras and Amdahl and Evans invented a so-called real time input channel for the 701. And we proposed that to Convair who accepted it. I may be getting off the story, but not quite. We proposed that and then Ramo-Williams came along and Bernie Shriever said, "Absolutely cancel it." Clearly von Neumann did not intervene for me and IBM.

But I tell you this story because it shows a general background and status of our unreadiness at the time von Neumann went to Washington and shows his concern. And so we spent these two days knowing at the time how long it would take to get the FSQ-7 built with all of its redundancy and so forth. We spent these two days making calculations on a yellow pad as to know if we could get the 701 in quantity production very quickly when this real time input, advising is it worth while to go into a major parallel program to use the 701 and aircraft defenses to bide time until the FSQ-7 gets up. But the point is that he remained interested in subjects other than direct atomic energy. As you know, before he became commissioner he gave the speech at the dedication of the NORC, and even though he did not agree with the design of the NORC, he thought it was a very worthwhile thing to do.

Now I come back to this time when Mr. Watson, Sr. got us all together to talk about money. McDowell and I had a meeting and we knew we had to have something better than the 700 series. By this time we'd gotten from Oak Ridge a physicist, Lloyd Hunter, who developed a transistor and also a small magnetic core. McDowell and I went up to Poughkeepsie and talked and we said, "Let's go get a government contract." And that, then, was the start of Stretch.

Well, we're still talking about von Neumann. We went to Rand and we went to Livermore and we went to Los Alamos and went to National Security Agency, and by this time (1955) Livermore was on the point of signing up for the LARC, and all we could say was that if they would wait another year beyond the promised delivery of the LARC, we would give them something that was faster. And they decided, I'm sure wisely, to take what they had and go with it. The significant thing in answer to your earlier question, I thought that machines of such supposed great power ought to have direct participation of persons who understood the problem that could be solved. So Los Alamos formed that advisory committee and you'll know the name of the man who was head of it,[3] and as long as I was in charge of Stretch I was absolutely insistent of having these design reviews.

SEIDEL: I'm very interested in that committee because you know Werner Buccholz wrote the book on Stretch and he talks about the joint group from Los Alamos and IBM that was set up to work on this machine and you just mentioned that there were these reviews going on which indicates which you sent stuff out to Los Alamos and got some feedback from them. Did they actually work together physically on the design, and how did that particular collaboration work? Today we have these CRADAs -- Cooperative Research and Development Agreements -- were we bring people from other companies into Los Alamos and they work there or Los Alamos researchers go out into companies and work there, but how did this Stretch collaboration work exactly.

HURD: I worked primarily by physical visits by people like Dunwell and Buccholz going to Los Alamos and people from Los Alamos going to Poughkeepsie and as long as I was in that program I know it proceeded.

SEIDEL: Now, Bengt Carlson was the first head of this?

HURD: No.

[3]"Of the nine LASL scientists who serven on this planning group,≅ a contmporary account identifies Athe group=s leader≅ as Bengt Carlson, T-1 Group leader, and the other members of the committee as Robert Frank, Roger Lazarus, Edward Voorhees, Mrk Well, Jack Worlton, M. Goldstein, H. G. Kolsky, and D. F. Woods. ASTRETCH, The Super Computer is Gone,≅ *The Atom* 8:6 (July-August, 1971), p. 25.

SEIDEL: Who was it that was before Carlson? I'm trying to remember.

HURD: Can't remember. I don't know. Someone who knew about the Metropolis work certainly and also knew about 701 and 704, but was not directly working for Bengt. Bengt thought he had enough to do.

But to get back to von Neumann, I went to Washington after he was Commissioner and talked to him about the Stretch proposal and also talked to him about by that time the NSA proposal. He knew about LARC and Dunwell claims that I got von Neumann to approve the Stretch contract. That is not true. What I did was talk to von Neumann about the general external design specifications and von Neumann said as he always said, "You should always build the best computers you possibly can." Now whether he then called up Bradbury and said, "Bradbury, buy this computer," I have no knowledge. It was just I went to see him in the general spirit of trying to keep him aware of things going on.

[END OF INTERVIEW]