An Interview with

STEPHEN DUNWELL

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Abstract

Dunwell describes the development of data processing equipment at International Business Machines in the mid-twentieth century. He discusses early data processing and cryptanalytic equipment before focusing on the development of computers following World War II. In this context, he describes IBM's Poughkeepsie Laboratory, John von Neumann's contributions as an IBM consultant, and the IBM 701 and STRETCH (7030) computers.

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DUNWELL: I'm going to give you my interpretation of the development of the computer. We were talking here to Ken Iverson about the so-called "stored program" and his objection to calling it that, to which I agree. He spoke about the fact that he wasn't concerned about the origins of these things. I am personally very much concerned with them, because I think the origins of ideas are extremely interesting and extremely important. How things develop is extremely important to science. I personally credit Alan Turing with having come up with the initial idea of using data as program, and being able to use the program to manipulate the data that would be used as a program. That is the fundamental idea that underlies all of this, as far as I am concerned. As far as I know, this originated with Turing, who, of course, knew von Neumann. Following that, as far as I know, it was von Neumann and Goldstine who formalized the thing and described how a machine that could make an important contribution might be built using these principles. They extended and formalized Turing's ideas. Turing knew the thing as he described it could not serve any useful purpose. What they did was a logical extension of that. It is my recollection that it kind of struck me as a bombshell when I became aware of the von Neumann papers and realized what we had been overlooking.

ASPRAY: What was your position at the time that you found out about the papers?

DUNWELL: I had returned from the service. Fortunately, I was in more-or-less the opposite organization from Turing. I was in the machine branch of the Signal Corps, which did cryptanalysis for our services. So I came very close to knowing Turing. I never knew Turing personally, but I have the feeling we probably were within a few hundred feet of one another on at least one occasion. But I wasn't aware of him at all, and when you're in that operation you don't talk about what other people are doing, because your great fear is letting some secrets out. You have all these secrets, and so the last thing you do is talk about them or inquire about other people. So, it wasn't considered good form to inquire about what other people might be doing beyond what it was your own business to know. So I never knew of him. After I came back from the service I went back into an operation that was called

"Future Demands" in IBM, called variously, "Future Demands", "Commercial Research." It had various names from time to time. And this organization was concerned with the practical application of -- I'll call them computers -- of systems. Prior to World War II it had been the application of punched card systems. After World War II it was immediately, as far as I was concerned, almost exclusively computers.

ASPRAY: Was that a common attitude among your colleagues?

DUNWELL: No, not particularly. (laugh) It took a little doing. No, it was not an accepted thing. We simply embarked on the project of turning IBM from a punched card into a computer business. Some of us set our minds to doing it. Some of us understood what we were doing. There was a small coterie that understood exactly what we were doing and where our problems would be, and we simply proceeded to do it. I was actively involved in the design of computers from the time I got out of the service until I became aware of the von Neumann papers, and went on and finally met von Neumann and dealt with him in a modest sort of way.

ASPRAY: Were you familiar with his original offer from IBM to be a consultant in 1946 (that I think was initiated by Wallace Eckert)?

DUNWELL: No. I was not aware of that. Not at all. I, of course, knew Wallace Eckert. I had known Wallace Eckert since before World War II. And I was not aware of that. My first actual contact with von Neumann came through the preparations for the IBM 701, and the application of the building and the application of the 701, which I was not a party to, incidentally. I had nothing whatever to do with the 701 computer. I became aware of von Neumann's work through that sort of thing. Throughout all of this, I had actually very limited contact personally with John von Neumann.

ASPRAY: When did you first see the 1946 to 1948 Princeton Reports?

DUNWELL: Well, I wouldn't be sure, but I would say probably about 1948 or something like that. I immediately tried

to get a copy and couldn't. (laugh) My first thought was, "I want a copy of that for my own records." And I couldn't get one. They weren't available.

ASPRAY: Just for curiosity's sake, how would you learn about those? I know that one of the later ones was noted in MTAC.

DUNWELL: Well, I think just through the grapevine.

ASPRAY: Were there other people at IBM that would have been looking for these reports also at the same time?

DUNWELL: We weren't really looking for them. It's very hard to discuss this thing in this way. I guess it's the only way we can do it other than to settle down and discuss the whole history of what went on, which is a long and complex thing. Maybe just a few words about what went on and how all this thing came about would be in order. Let me put a story together. This is the story of the environment in IBM, and it's not exactly what you came for. But then you can see how von Neumann fitted into the thing.

First of all, I had been in the IBM Laboratory. I started in IBM in 1933, and I went with IBM full-time in 1934. My assignment that I took was in modifying the IBM equipment to meet individual customer's demands. That was just the kind of thing that I wanted to do and liked to do. For several years I was in the laboratory doing that. I had been a ham radio operator. I'd had my own ham station, built my own ham station, in the 1920's. I was constantly searching for ways in which we could use electronics in the business. And it was very, very difficult to do. I personally set myself apart as a person to whom all applications involving computers and electronics would come. I took on that mantle myself because nobody else wanted it. So, there was a time when I was the only person involved in electronics in the IBM Corporation (laugh) -- believe it or not! In their laboratory it's hard to believe, but it was true.

At that point, for example, a request came in from Wallace Eckert for some equipment to make. He'd been given a set

of IBM equipment at Columbia. He wanted some special equipment, so I grabbed the order and I went down and I saw Wallace. I was at the Pupin (?) Laboratory where Wallace had his equipment in either 1937 or 1938, and I wouldn't know which of those two years it was. I designed and built some equipment for him. He couldn't do very much. But he was concerned with the computation of lunar orbits, and it is a good problem. Nicholas Murray Butler, who was president of Columbia at that time, was a good friend of Watson. There was a guy named Ben Wood, who was a professor there, who was always gigging them on IBM getting into computation. Columbia was a very different organization then from what it is now. Up into the 1930s it was one of the leading contributors to engineering and communication in the world. I don't know how well people realize that, but, for example, Armstrong, who developed the super-heterodyne and he also did FM. He was associated with Columbia. Pupin, who did the repeater coil, that made long distance telephone possible, was there. Charles Dunning, who literally holds the patent on the atomic bomb, was there. That was all an earlier period. So it was quite a prestigious place to be if you were into engineering.

Anyway, that's where Wallace was, so I'd had that contact with him down there. In 1938, we got a request in to put on a show at IBM headquarters on what was being done in electronics. I didn't know it at the time, but Ben Wood had gigged Watson at dinner on why he wasn't doing more in electronics, and Watson called his laboratory saying, "What are we doing about electronics? Why aren't we doing more?" The guys up there said, "Of course, we're doing a lot here, and we'll show you." And he said, "Well, bring it down." They said, What are we going to show him?" And they called me in (laugh), and I took a dog and pony show down alone to New York on the thing in 1938. That was kind of the level at which the thing was. So it was very, very small, and didn't really amount to anything. There was one other person, Ralph Palmer. He was the second in command of the laboratory in the place. He was second in command of the electrical section in the Laboratory, which was three or four guys. Mostly what they did was sample the metals that came in -- metallurgical testing and things of that sort was mostly what went on in there. Ralph and I were pals, because we were really the only two that had any interest in electronics. Then, in 1938, I was asked to come down to New York to what was then called World Headquarters. I went down there and went into the application end of the business trying to figure out how to apply the punched-card equipment better. I stayed there until the advent of World War II.

At that time they broke the organization up and I came back to the Laboratory and worked on the thing that was called "radio-type." I don't think many people realize that before World War II IBM had a radio-operated typewriter that they actually sold to the services. At the beginning of World War II the Army had this theory that they could not use commercial communication, because if they used the commercial cables somebody might cut the cable, and they'd be totally out of communication, say out of the Philippines, you see. So instead of that, they had this relay network, and everything was relayed by radio around... But the radios weren't powerful enough. You had to relay them. You'd send from the Philippines to Hawaii, and then from Hawaii to San Francisco, and then from San Francisco to Washington. It was a several-leg operation. All of this was done with what was called a "balmy system." This consisted of literally a pen that wrote up-and-down squiggles on a narrow strip of paper. Then they would have enlisted men who would look at those things and transcribe that back into signals that they could then send out again to the next point in the relay. This was all manually done.

When I came into the Signal Corps as an IBMer and looked at this thing, at this appalling collection of antique equipment it was unbelievable! It was what Samuel Morse would have understood very, very well, and might have designed. That's all they had. It was extremely inefficient. IBM developed what it called "radio-type." Now this came about because of the fact that there was another man at Columbia University, a man named Walter Lemon. Walter Lemon, as a graduate student, had invented the single dial tuning for radios. Up to that time you had a whole row of dials, and you might have several stages of amplification in your system. If you had a real fancy radio, you had two or three stages of amplification. This was before the super-heterodyne. You had to tune each of these and get them all lined up; and if you didn't, you didn't have any signal. Walter figured out and got a patent on a method of doing this with a single dial. It was a little tricky. There was some padding and things that went into it. It wasn't quite as obvious as it seemed.

But anyway, he had become a very wealthy man through that. He had interested Watson in having his electric typewriter operated by radio. This was thing that appealed to Watson. Watson was more far-sighted than some of us realized. Among other things, they sent one of these gadgets down to Antarctica with Byrd. It was supposed to

have worked down there. I never knew for sure whether it did. Some of these things it's hard to know whether it really did work or not. But anyway, Byrd had it with him. He had it packed on his ship went he went down to Antarctica. When World War II came along, all this stuff was grabbed. We put it in a laboratory. We implemented it and put it into the field. That was the next thing I did before I went into the service. I was one of the people on that team. I handled the service end of it. We had to train 50 people to maintain this equipment. They were all over the world, and they were going to get out there, and they were going to have no help when they got there from anybody. I selected the guys for it. I only took ham operators, incidentally. You had to be a ham, and you had to have certain levels of knowledge and intelligence. If you passed those tests then we put you on, gave you training and shipped you out.

At that point the Signal Corps came to IBM and said they'd like to interview several people for possibly going into the cryptographic operation, which was then down in Washington, and later went to Arlington Hall station. I was one of the people that went into that. I went into it and stayed through the entire period, and got Legion of Merit for it, and that sort of thing. I wound up running the thing. By the end of the war I was running it. We were doing very, very excellent work there.

There were two general ways of approaching the cryptographic machinery problems. One of these was to build special machines, like the bomb that was used, the sort of thing that was used by Turing. I was aware of the bomb. I wasn't aware of Turing, but I was aware of the bomb. I had seen the bomb, and I knew it was there. But there were a couple of things that happened. First of all, the work was always divided up. The work on the German ENIGMA went to the British and to the Navy, because the Germans used it for submarine warfare. So it was considered a Navy problem. We simply worked on other problems. So I never worked on the ENIGMA, but I was well aware of it, and I knew how it worked, and how they were solving the problems, and all of that sort of thing. So, in our own organization we had two groups. One group made special machinery like the bombs for various things, and the other used punched-card equipment. We came out about on a par with the thing. If anything, our punched-card equipment came out better. Whereas on the other equipment you always had to take the problems and go through them step-by-step as you logically might, we were able to sort and collate and organize the data. By organizing the

data, we would often get a hundred-, thousand-, million-to-one improvement in the probabilities of our getting what we wanted. So, you take a machine running at a hundred cards a minute, we might beat out a machine that was running 10,000, a hundred thousand times as fast, because it was going through a lot of stuff that we didn't have to do. By sorting, collating, measuring, and matching, and things of that sort, we did very, very well.

In the course of all of that, I had gone to IBM and arranged for some array processing equipment to be provided for us, and for some equipment to be provided which would allow us to read a punched-card and punch back the decrypted (?) result in the same card. All the processing we did during that time was array processing, because in cryptography everything is done with arrays. It's by the use of arrays that you are able to ensure, first of all, that you have no black holes in your process. In other words, if you start out with an array and manipulate arrays in some orderly way, you know that you don't have any codes that are duplicated, or any that are omitted, or anything like that. That's a general approach to cryptography and cryptanalysis. Our machines to a very high degree were array processors. I had 14 punched-card machines built by IBM that would read a card and punch the result back in the card with the computation being made as an array computation in a very short space of time with relays. Sometimes we had a thousand, ten thousand relays attached to a single unit, a single processor. By these processes we'd sometimes get 10,000, a hundred thousand to one advantage over not having it.

During World War II, a gentleman named Halsey Dickinson had been working throughout that period in the IBM Patent Department on electronic computers. He had designed an electronic device that would multiply. It didn't add, subtract, or divide. When the war was over I was held over for nearly a year. I didn't come out until quite late, because I was simply given orders that until I got the organization back in other hands... the general said, "You can't walk out the door until you can assure me that this place will run without you." I was about a year getting out after other people did, which meant that I didn't get back until August of 1946. I got a call from Halsey, who knew that I had had these things made, and he asked me for one of these devices -- one of these machines that would read a card and punch the result on the same card. I arranged to have one of them that IBM had made for the Signal Corps which we had been using for cryptanalysis -- we didn't need them all anymore -- sent up to him. The first calculator that IBM ever made that was electronic was Halsey's multiplying machine. We all knew the thing wouldn't do very useful

work, but Watson Sr. was much more far-seeing than people in general credit him with. When he saw the thing he said, "Well make 50 of them. We'll market them." We made a batch, if I remember correctly, it was 50 of them, and a few of them sold. But it put everybody on notice that we were going into electronics, and that you'd better learn what a vacuum tube was and how it worked, and a few things of that sort.

So, really, the first work was done by Halsey Dickinson and used that equipment. Then, by the time I got, out Ralph Palmer had set himself up here in Poughkeepsie (he used to live down the street here) and had set up a laboratory. The Poughkeepsie Laboratory, which was set up in a residence here, started out to make an electronic computer that would be marketable. By the time they got into that I was out of the service, so I did the system design on the thing, because they didn't know how to do that. We only allowed ourselves 50 numeric digits of storage -- 200 bits of storage. (laugh) All we had was 200 bits in the whole thing. It's hard to make a commercially useful machine out of that. It was quite a challenge. The program was all on the plugboard. We used a plugboard for the program, and did our computation electronically. That became the IBM 604, which came out in 1948. By this time I was back in IBM, but I was not up here. I was down in New York. I was in Headquarters with the job of guiding the design of electronic equipment here and in Endicott. We had a firm understanding that none of the people over in Endicott were allowed over here. The whole group here was new young people. I should say that Ralph Palmer had been in the Navy counterpart and had worked on the Navy's version of the bomb. He brought with him a group of people. Of course the SRA people were all people from the Navy group. They all came out of the Navy Opt 20G group in Washington. I sort of knew them, but I had no personal direct contact with them. But of course I was aware that they were there, knew what they were doing, and that sort of thing, in a general way.

So, really, it was the experience in cryptography and cryptanalysis in World War II that set up both SRA and IBM. It was those of us that had been in that thing, because we came out with the absolute determination that we were going to make these things work somehow -- not knowing quite how it was going to be done. Then the IBM 604 electronic calculator was followed by the Card Program Calculator. We all fully understood the inadequacies of the thing, but a very large number of those things were made because it was a thing we could hang together. It included a 604, plus some storage devices and things that we had lying around that we could kind of cobble together and use. It was at

that point that we led into the drum calculator.

By this time the staff over here in Poughkeepsie was beginning to grow. The Poughkeepsie group started out on what was called the Tape Processing Machine. We decided that we would build a machine. I was still not up here. I was still in New York, but I was the guiding hand on the thing because I was spending all my time out in the field looking at applications and saying, "We can do this." and, "What can we do?" and trying to figure out how we were going to apply these things. They looked to me to give them general guidance on that. I handled that end of it. There was a strong group developing here in Poughkeepsie -- all of new people. Nobody had any prior experience in the IBM organization, except for Ralph and myself. It was just forbidden for anybody else... keep them out the door. So we had that developing dichotomy and it finally turned out to go in three directions with the developments on the West Coast. We had then the Tape Processing Machine. That served to give us a lot of experience, but it was very clear to anybody looking at the thing that we were far removed from anything practical, that while we were learning real fast much of what we were doing was not going to get us anywhere. For example, the tape drive on the Tape Processing Machine had only one track on it. It recorded a single bit instead of a single byte. Obviously, we had to go back and get something that would line up the heads. It was a design trick to get heads that would work, where you could take a tape from one tape drive to another one and have it line up. We didn't know how to do those things, you know. There were a lot of things we didn't have any idea how to do. We didn't know how to make magnetic tape, for example. Nobody had any idea how to make good magnetic tape. The BINAC had used metal tape because they didn't know how to make plastic tape that could be magnetized reliably.

TAPE 1/ SIDE 2

DUNWELL: At about this time, Cuthbert Hurd came into the picture. Cuthbert saw as we did the need to coalesce all of this work into useful things. He was very restive (?) with the direction in which things were going, because my own efforts were largely toward making commercially usable machines. Of course, that's where our business had been and that's where our customer set really was. That wasn't the general direction in which he had an acquaintance. Also, the engineers felt that the sorts of things that were being asked of them were sort of

unreasonable, that there must be some simpler way.

ASPRAY: I don't understand that.

DUNWELL: Well, I mean that must be an easier way to get something on the market that would be marketable. The engineers wanted to build something, and as we looked at the Tape Processing Machine all we could see is problems. All we could see were unsolved problems, which were truly there. So at about this point a decision was made to build what became the 701, which was known as the Defense Calculator. They were going to make a number of them. Cuthbert was sent out to see if he could sell a number of them. He came back with orders for, if I remember correctly, 17 -- some such number as that. What they decided was that these machines would be straightforward von Neumann machines, that they would in every way minimize the problems of design by saying, "We will make 17 JOHNNIACs as best we can. Of course, we will do a few things that they haven't done. But that's our design; that will be enough. Cuthbert can sell them." And that was it. So that was done, and that was very, very successful. The last thing they wanted was me around reminding them of all the deficiencies of this sort of thing. They didn't want me as a part of it, and I found other things to do during that period.

I actually had no part in the 701 and its development, marketing, and so forth. I was privy to many of the things, but I was not a direct participant. The whole thing was done by this group of new, young engineers -- Nat Rochester, and Werner Bucholtz, and others in that group. The machine, basically, was a von Neumann machine with printer output and tape input, and some of the things that were obviously necessary to get it off... It was a von Neumann machine with the necessary wherewithal to make it communicate with the external world, and that was it. An effort was made to minimize changes of design unless they were absolutely essential to make the thing work. I'll come back now to your question, as to when von Neumann became involved with IBM. I don't really know. But we were involved with so many things and we had a pretty good idea of where we wanted to go. We knew that machines, like the successors to the JOHNNIAC, were going to be the thing we would use; but it was a question of how to get there, how to market them. Many people didn't believe that you could take a machine that sold for $15,000 or $20,000 and sell it. That seems ridiculous now, but that seemed like an awfully large sum of money, because everything

previously had been tied to punched-card speeds, and, therefore, the marketability, the price you could ask, was limited by the speed of input-output. That was the environment in which we found ourselves. Now, it became apparent that such a machine was not really going to be commercially saleable. So I was brought back into the act on the commercially equivalent machines -- the 702, and the 705, to try to find some way of making something that would be sufficiently compatible with commercial requirements to be a saleable product. I got into that. My first contacts with von Neumann were in that period in which we were trying to rationalize these two product lines. We wound up with two product lines. We wound up with one product line that was the 701, followed by the 704, followed by the 709, followed by the 7090. We had another product line that consisted of the 702, which was a decimal machine, and the 705. And there seemed to be no way of fitting these things in together. No one could see how to rationalize this in a single product line.

ASPRAY: What sorts of features were different about the two?

DUNWELL: Well, one was a binary machine that dealt in words of fixed size. The other was a decimal machine that worked in fields of variable size. And It seemed to be very difficult to take commercial data and compress it into the more rigid format of the numeric machines. The machines, like the 701, 704 when they finally put floating point on, dealt beautifully with mathematical problems, but if you tried to throw at them names and addresses and things like that, it was very hard, but not impossible. But we're dealing with things we have to market, and other people do the software and things of that sort. All the software was very weak in those days. It was not the software that we have today. By comparison it was very weak. We had this problem of rationalizing it, and if I remember correctly, the first contact that I had with von Neumann was in one, at least, and possibly more -- it seems to me there were two or three -- discussions we had with him with regard to how these things could be rationalized and what we might do. How would I happen to do that? I wasn't even in the Laboratory. But I was a man of courage, or whatever. So, when it came to having a meeting with von Neumann, I was willing to be chair of these meetings. [laugh] You know, I had enormous difficulty communicating between my level of intelligence and his. Anyway, I wound up chairing up at least one, if not more, of these meetings in which a number of engineers would sit around the table with him in our 701 building over here and talk about our various problems.

ASPRAY: This would have been what year?

DUNWELL: This would have been about 1953, or so, if I remember correctly. And Cuthbert Hurd was the one who arranged the meetings. He would arrange the meetings, and then the word would go out from the head of the laboratory who would say, "Von Neumann is going to be up next Tuesday afternoon at 2:00 and who is going to conduct the meeting?" You know, that sort of thing.

ASPRAY: Do you know how he had been hired as a consultant to the company?

DUNWELL: No. There are only two or three people who can tell you that: Cuthbert Hurd, Tom Watson, Jr., and Vin Learson. That's the level at which that thing was done. Nobody in the Laboratory was signing any contract with von Neumann, or saying, "We've got to talk with von Neumann tomorrow about things." Really, to be quite honest with the thing, the impression I have now, and the impression, if I remember correctly, that I had then was that the IBM Corporation felt this enormous debt to von Neumann for what he had contributed. It was very well recognized, this enormous debt to him. He had created the foundation of everything that we were doing. We saw all of this very, very clearly. And the corporation wanted to compensate him, at least in some small way, for this. And hopefully, something would come out of it. But if something didn't come out of it, nobody cared because we owed him enough of a debt. We were in his debt no matter what happened. Now, I'm sure that also there was some vague hope that something would come out of these discussions. Quite frankly, my recollection was that nothing did. I think that it would have been asking much too much to have had that happen.

I want to step back and say something again about the general environment that we had here. We had moved in a period of a very small number of years from a time at which everything was done by hand with hand calculators to the point where we were doing things with electronic calculators at a very much higher rate of speed. This exposed an enormous number of mathematical problems that I'm sure everybody realized were lurking there, e.g., the problem of dividing by zero. All of sudden something would go to zero, and you're dividing by zero, and what are you going

to do? How do you cope with these things? So the big and really fundamental problem in those first years on the mathematical side was how to provide a mathematical foundation that would work at these speeds and with these problems. The problems that all seemed very tractable in an ordinary simple sense would blow up on when you actually did them. Previously, you just had a kind of a theoretical foundation for it. But as I said, something would go to zero, now what do you do? Of course, von Neumann's answer on those things was, well, you reformulate the problem a little bit, and everything will be all right. I might say this of von Neumann, that he had this enormous ability to do mental computation. While the rest of us poor souls would be sitting around discussing whether something could be done, he'd be sitting there quietly and come out and say, "It won't work. It won't work because this parameter isn't sufficiently well controlled," where he went through it in his head. He was that kind of person. So, what I wanted to say was, for the rest of us souls, we were all moving to a new intellectual plateau level as a result of electronic computers. They were exposing all sorts of problems. He was very busy contributing to these things, advising the various organizations on how the problems that existed could be formulated in a way that a computer would handle them. For that he was enormously valuable. When it came to the level of language problems that we had, he was not very helpful or very sympathetic. He insisted that only a few thousand words of memory was all you needed for any problem; and if you couldn't put it in that space, there's something wrong not with problem, but with you. You're the problem. That was his view of the thing. While we knew there that there was some truth in that, we also knew there wasn't some truth. But he went to his grave believing that sort of thing. I know he would say, "Just get smarter people. What's your problem? Get some smarter people in here."

ASPRAY: What was his attitude about this commercial versus scientific line? Do you recall?

DUNWELL: I don't think he ever understood it. I don't think he ever saw any problem. I don't think that any problem was visible to him at all. The level at which he approached these things didn't permit considering the problems of some very ordinary people dealing with things. Anybody who couldn't compute in binary as easily as they could compute in decimal to him was the wrong person to be dealing with.

ASPRAY: Did he have any business acumen about these issues?

DUNWELL: Not that I ever saw. I really think that it became a problem really of two things: one, of putting things in a context in which people related to them. The people that used them related them to the things they knew and did. That was a part of it, and a very important part. Then, we had some things we had to learn about design so that these things seemed natural to people. But it was not his milieu. I think if he were alive today he wouldn't be interested. It isn't a question of his not being able to understand. To him, it would be the kind of trivia which should be better left to lesser people.

ASPRAY: It was quite clear this was a fundamentally a scientific instrument to him, and he was interested in only those problems.

DUNWELL: Yes, I think that's right.

ASPRAY: I've heard various stories n[th] hand about his attitudes towards early attempts to do automated programming, formula translation, and such. Do you know anything about his attitude?

DUNWELL: No. No, I really don't. We were talking to him about these things. We didn't have to be told that you had to use the computer to program the computer. That was very evident early on.

ASPRAY: Did IBM have a group that was working with their scientific users that would be very interested in questions of numerical analysis, scientific computation, new ranges of problems that could be done on the machines, new features or specifications for new machines for that body of users? Because it seems to me that that would be the place that he would be ideally suited to be helpful to the company.

DUNWELL: Yes. We have to bear in mind that he was ill for awhile before he died. I don't know how long he was ill. I don't know how many months it was before he died that he became aware...

ASPRAY: He discovered his illness in mid-1955.

DUNWELL: Mid-1955. So there was a two-year period there. Cuthbert Hurd was very instrumental in the development of IBM's response to the scientific use of computers, and his influence in that period is not to be underestimated. After he went out with one or two other people and sold all these machines, he looked at the thing and said, "Well, what have I done?" He then brought in around him a group of young people who then went on to provide the support for these machines. So we had a technical support group that reported to Cuthbert Hurd that supported these first machines. Then that branched out and turned into what were called Science Centers. We had a Cambridge Science Center. We had another science center in Los Angeles. There were others at other places. Those two were the ones that come to mind as key organizations. They provided much of this sort of assistance. They came along a little bit later. It would be my impression that they did not exist until after von Neumann died, or at least until after he was not fully functional. So I would doubt that he ever had any direct contact with them simply because of his early death.

ASPRAY: Yes. There were the IBM computation seminars.

DUNWELL: Oh, yes.

ASPRAY: Did they somehow serve the same function of trying to at least find out the scientific problems that the community had?

DUNWELL: Yes, or mostly to get people to describe the work they had done. Cuthbert Hurd was, again, instrumental in organizing these. I went to most of them. That's correct. That was a part of it. But again, you can only accomplish so much in a meeting when what you've got is a machine sitting here that costs you more money than you can imagine, and it's not doing what you promised it would do. Then you better get it going. And it's all because you don't understand well enough the problem that you're trying to solve. It goes this way, zigs this way and zags that way, and causes you great difficulty -- not all of which you can see or understand. That took people

right there working on these problems for many hours. While you got encouragement from going to a conference, and it might tell you who you should call up on the telephone when you got in trouble next time, the real front line was the direct contacts between IBM and the customers, and the customers contact with one another, which Cuthbert pretty much handled.

ASPRAY: Yes. Is there any chance that von Neumann had a significant role in design specifications for the scientific line of computers: for the 704, the 709, or the STRETCH?

DUNWELL: Well, he certainly didn't for STRETCH. On matters of that sort, you would really have to ask someone like Nat Rochester. Nat was one of the key people in the development of the 701, and the later extension of that (the 704) and the 709. He and Gene Amdahl would be the people that would give you more insight into how much, if anything, von Neumann had to do at that later time. He had made this very fundamental contribution in the fact that the machine built at Princeton was copied in the 701, and the 704 was a copy of the 701, and the 709 was a copy of the 704, because they had to run the same programs. They extended the programming, but the 709 would run the programs pretty much that had been running on the 701. So they were successors in a line of equipment, like annual models in the automobile industry. They did not depart in any radical way from the initial design. So, I think one has to say that von Neumann, Goldstine, Burks, and Pomerene, and others that did the machine at Princeton in large part defined that machine. I think Nat Rochester will confirm that. But now, if you come and say, "Well, did anybody consult with von Neumann on how to do floating point to a 704?" I have a hunch that you'll find the guy just went ahead and did it.

ASPRAY: Was there evaluation of designs of IBM equipment done by von Neumann? There weren't very many to evaluate at the time.

DUNWELL: None that I know of. No effort was made. I don't know how he could have done that. The big question of evaluation is a question of evaluating the machine for the application for which it's intended, and nobody knew what they were intended to do. We knew what we wanted to do, but we didn't really know how well. And the

applications were not well formulated. They were not well defined. You had to install the machine before you found out what the problem was, really. You only had a vague idea of what you were trying to do. You were talking about the STRETCH machine, and I think a little bit might be said with regard to that. The STRETCH machine came about through a succession of steps. And it started with a series of coffee klatches with Gene Amdahl, and myself, and Werner Bucholtz.

The three of us would get together over a cup of coffee and do a blackboard exercise in which we talked about these various problems, tried to gig one another, and see what we could come up with in the way of solutions. We came up with a number of proposals for different kinds of machines. So we gave these things different names and worked along with them. Finally, one summer, Ralph Palmer, who was head of the laboratory here, sent the word out to a series of people. I came to the Laboratory in 1954. He passed out the word to about eight of us that he wanted us to forego our summer vacation and all get together and have a two-week session in which we would decide what's going to be done. Amdahl was one of them; Werner Bucholtz was one of them; I was one of them; Ralph Palmer too -- a total of about ten people. We got together and started out really from scratch and said, "What can be done in hardware, in systems design, and everything of that sort?" We came up with the conviction that, in fact, we could put together a machine which would serve both scientific and business purposes -- that we could meld these two, could bring the two together, and that we could also build a machine that was very much faster than any existing machine of any kind, and that this would be a very desirable thing to do. We needed to have some proof that we could make the transistors, put them together in logical circuits, and make the memory -- the high-speed ferrite memory. By then we had ferrite memory that would be required for it. I have it around here somewhere in the house the original timing chart for those memories, incidentally. I took it off of the blackboard and took it home. I said, "That's a good thing to keep." And it was. We decided we needed some money for it, and so we immediately went down to what was then the National Security Agency. That was the combined group that had taken the place of the one that Ralph had been, that I had been in. We went down and told them what we felt we could do, and that we needed some money, and would they give us some support for this? And they did. Because IBM wasn't about to support any hair-brained thing like this. So, we got, I don't know what it was, a half a million bucks, which was a lot of money then, something like that -- maybe a million; I think more like half a million. And we set about the design --

both software and hardware design. That is systems design, I should say -- not software... a systems design for a

system that would do the kinds of things we wanted. We were well into this thing when we got an invitation to come

out to the West Coast and present there to the Atomic Energy Commission a proposal for a machine. Sperry Rand

had proposed the LARC, and we put in a counter proposal. I wrote the proposal, and I have that here in the house

too.

ASPRAY: This was when?

DUNWELL: I've got the proposal; I could look at the date on it. It was about 1954; maybe it was 1955 by then.

ASPRAY: Von Neumann was a commissioner at the time?

DUNWELL: Yes, that's right. So, at that point this was presented to Teller. Nat Rochester was involved in the thing;

I was involved in the presentation to Teller; Werner Bucholtz was involved in the presentation to Teller. We were

told what they felt was a better proposal, but that since Sperry Rand was promising to deliver earlier and they were in

a big rush for equipment and they would have to take the one that they could get first. So, at that point Cuthbert

took the matter to von Neumann and said, "This is what we want to do." Von Neumann was a commissioner. And he

was the commissioner who had the responsibility for computing. He was the computing commissioner. His word

was the final word. If he thought something ought to be done by the AEC in computing, the rest of them took his

word for it. They left it to him, with good reason. So we presented the plan to von Neumann. He never went through

the whole thing in all its dirty details. But we were asked to go out and present it at Los Alamos and so we did that.

TAPE 2/SIDE 1

DUNWELL: We went to Los Alamos, got agreement with them. We went to the National Security Agency and got

agreement with them that they would get a machine tailored to their specific requirements. This was to be a copy of

the machine that went to Los Alamos with additional features that they needed for their particular classes of work.

At that point it remained to get von Neumann's approval. Now, he did not participate day-by-day in these discussions at all. And I don't know exactly how much contact he was having with his own people, but adequate to satisfy him on the thing. Then finally there was a meeting with von Neumann that I went to with Cuthbert Hurd -- the last time I time I ever saw von Neumann -- at his office at the Atomic Energy Commission in Washington at which we laid the plans out before him. And he said, "Okay, let's go." That was the last time I saw him. At the time he was ill, and I knew he was ill, and knew what his problem was, and so forth. We got his blessing on the thing, which he conveyed to his own people. And we had it out. So you see, my personal contacts with von Neumann were really very limited in total.

ASPRAY: If I could get you to summarize what you're telling me about his actual contributions as a consultant to the company they would be really rather limited.

DUNWELL: That's correct. I cannot pinpoint any single specific thing that he did as a consultant, although we all acknowledged that everything we did was a result of his work. So, in a sense it was everything and nothing.

ASPRAY: Yes. If these particular visits to IBM as a consultant did happen to have some importance to somebody or some group of people at the company, who besides, perhaps, Cuthbert Hurd might be able to tell me about it?

DUNWELL: Nat Rochester, Gene Amdahl. I have already talked to Werner Bucholtz, who was at least at one of those meetings, and he says that he can make no contribution. You may want to speak to him directly. I made a point of chatting with him to find out whether or not he felt that there was anything that he could contribute and that you ought to be seeing him too. I gather you said that he didn't feel that way, because that's what he indicated to me.

ASPRAY: Can you tell me something about von Neumann the man? I've gotten pictures from a few people, but it's always interesting to hear somebody else's comments.

DUNWELL: I was never that close to him. I had few contacts with him. He would be at meetings that I was at, and I would see him in contact with other people. Sometimes you wish afterward that you had taken more bold steps. I wish that I had taken more bold steps than I had, but I was certainly not prepared to discuss the things most on his mind at the level at which he was thinking of them, and I was very well aware of that, and so was he. I did not really attempt to develop contacts with him, and had little contact. In all respects he appeared to be a very likeable person, and he was not a distant person. As I saw him, he was not a distant person, although those of us where our contacts were professional, we were a little bit sometimes put off by the difficulties of communicating at our level with him at his level. That was always a little bit of a problem. But he was always friendly and always bent over backwards to listen. He would never put you down, but he might not understand at all why you considered something a problem (laugh).

ASPRAY: Okay. In 1954 the predecessor of the National Weather Service was trying to decide what computer to purchase for its Suitland, Maryland operation. There was a choice to be made between an IBM 704 and an ERA 1103, there were some tests run, and the decision was made in favor of IBM. I had the feeling even though von Neumann was not directly party to this that he was behind the scenes manipulating this decision the whole way through. Do you know anything about this story?

DUNWELL: No, I don't. No, I don't know about that specifically. Of course, I do also know that Teller was very much interested in weather. I don't recollect anything having to do with von Neumann. But weather was one of the things that was on Teller's mind with respect to getting these computers. I guess as you get closer to the problem you get farther away in this field, but at the time I think there was a feeling that maybe if we just had a little more power we could begin to solve some of these things. But I never discussed it. Incidentally, Nat Rochester and I spent three days with the Weather Bureau before they moved to Suitland, Maryland. Before that they were in New Orleans. Nat and I were invited down about 1948. We spent three days there going over their materials and considering what could be done in the way of applying computers to their work. That would have been about 1948 or so that we did that. That was one of our inputs to our Tape Processing Machine activities in the 702 machine. But that was the "how to record and digest weather data." It was not prediction, which Teller and von Neumann were

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concerned with. Rather, it had to do with taking the facts and trying somehow to tame them. The Weather Bureau was buried in paper at the time.

ASPRAY: Do you have any other recommendations to make to me in terms of trying to understand the influence that von Neumann might have had on IBM's operations, whether it was directly as a consultant or in this more global sense that you been talking about in terms of his role as an early leader in the field and opening up the modern field of computing?

DUNWELL: You may find that someone such as Gene Amdahl will be able to shed some light on that, or Nat Rochester. I personally am not aware of it, and I think it would have been very difficult for it to be true. His intellectual contribution was at a level which was in keeping with his intellect. And it was enormous. You know, everything depended on that. But the kinds of remaining problems we had really descended essentially to what he might have thought of as trivia; you know, of writing a computer program to program a computer, for example -- things like that. Or how languages should be constructed so to make it easier for somebody to grasp what was going on. That didn't trouble him at all, because it wasn't a problem (laugh). It was a non-problem. So, I wish I could say more, and I wish there were more that I knew, but there isn't. I don't think you're going to find much in the way of digging unless you will find someone at one of these conferences who was very much influenced by von Neumann. For example, I don't know about floating points. I think the question of floating points is very interesting. I wouldn't be at all sure that von Neumann had very much interest in that.

ASPRAY: He thought seriously about it several times in design as far as its ramifications for doing scientific computation, but I can't say that he ever settled his mind about floating points, because some of his designs had it and some didn't.

DUNWELL: Yes. I would think that if there is anything that it would be at that level, because that's a meeting of the computer with the practical handling of problems of the kind that he was interested in and would naturally be thinking about.

END OF INTERVIEW