

An Interview with

NICOLAS C. METROPOLIS

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Conducted by William Aspray

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Nicholas C. Metropolis Interview
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Abstract

Metropolis, the first director of computing services at Los Alamos National Laboratory, discusses John von Neumann's work in computing. Most of the interview concerns activity at Los Alamos: how von Neumann came to consult at the laboratory; his scientific contacts there, including Metropolis, Robert Richtmyer, and Edward Teller; von Neumann's first hands-on experience with punched card equipment; his contributions to shock-fitting and the implosion problem; interactions between, and comparisons of von Neumann and Enrico Fermi; and the development of Monte Carlo techniques. Other topics include: the relationship between Turing and von Neumann; work on numerical methods for non-linear problems; and the ENIAC calculations done for Los Alamos.

NICHOLAS C. METROPOLIS INTERVIEW

DATE: 29 May 1987

INTERVIEWER: William Aspray

LOCATION: Los Alamos, NM

ASPRA Y: This is an interview on the 29th of May, 1987 in Los Alamos, New Mexico with Dr. Nicholas Metropolis. Why don't we begin by having you tell me something about when you first met von Neumann and what your relationship with him was over a period of time?

METROPOLIS: Well, he first came to Los Alamos on the invitation of Dr. Oppenheimer. I don't remember quite when the date was that he came, but it was rather early on. It may have been the fall of 1943. I'm not certain about that date, but that's a matter of record and you can check on it.

ASPRA Y: All right.

METROPOLIS: Robert took him around and introduced him to a lot of people. Of course, his reputation preceded him. I knew he was a very close friend of Dr. Teller. Since I was working with him, it was very natural that I would be able to see a little more of von Neumann than perhaps some of the others. We worked together on problems when he would come to Los Alamos, from time to time. Each time it was always a very intense kind of interaction. His time was somewhat limited, and yet he was able to discuss a problem in a thorough manner. He was that kind of a person. We worked together on some shock phenomena. Then, owing to the relationship that he had with Dr. Teller, we were able to see quite a bit of him socially whenever he was here. This often took place in the form of going to Sante Fe for some New Mexican food that he relished very much. And another thing... I don't know whether this is relevant, but I think it does reflect on him. He was interested in such things as seeing the Valle Grande at least once every time he came here. Somehow or other, he just liked the idea very, very much. In scientific discussions he was always very precise, very formal, and very clear in his exposition -- both while he was just having a discussion as well as when he would give a talk. He gave talks frequently, because he always came with such a wealth of information, owing to the fact that he was involved in so many different things. My interaction with him was over and above the

hand computing problems that I was doing. There was the question of his becoming more familiar with the IBM machines that were being installed here. He was very much interested in just those things, so we spent a great deal of time discussing not only the problems that were being put on but the general aspect of computing.

ASPRAY: Yes. Over what period of time did you have these kinds of interactions? How long did they last?

METROPOLIS: They would last, maybe for one or two hours, and certainly at lunch time there would be conversation. In the evenings, socially, we would get on to those topics as well. We were always very much interested in what information he brought to the laboratory from his many travels elsewhere. For instance, there was this proposal to do a problem on the Mark I at Harvard. This came at a time when we were just getting started with the IBMs here, and comparisons would be very useful. So there was actually a comparison problem run on Mark I. The most important thing, however, was when he brought us news, as I was mentioning yesterday [in a lecture at Los Alamos National Laboratory at a conference on the transfer from wartime science to peacetime research], of the ENIAC existence in early 1945. Of course, that was most exciting. He gave talks about it. He viewed it from the general point of view rather than giving any details, because at the moment he was not privy to many of the details. Obviously from his conversations with people at the Ballistics Research Laboratory (BRL) he would know many of the general things certainly, but he would also know some of the particulars about ENIAC.

ASPRAY: You mean about how fast the machine was?

METROPOLIS: Yes, and how many tubes there were in it, how many solder joints, and what the general organization was. Frankel and I went with him in the spring of 1945 to view the physical surroundings at the Moore School (University of Pennsylvania in Philadelphia). There we met Eckert and Mauchly, and Herman and Adele Goldstine, and the engineering staff. I must say, it was a marvelous experience because they were all very excited about their machine. For us, it was a double kind of thing, because there was all this activity back here -- and then suddenly to be put into the ambience of this computer development that was going on, that was truly spectacular. We didn't need to look at our work in retrospect to recognize how terribly important it was... And so it was there. I mean, here

was electronic computing going on, at least potentially, because at that first exposure things were still under construction. But it was quite clear that there were two new disciplines, and we happened to be privy to each of them.

ASPRAY: What can you tell me about the problem that was run on the ENIAC?

METROPOLIS: Well, I think it's permissible to discuss it at this time, because I've seen references to it in the open literature, although at that time it was highly classified and the people there did not know what the problem was. All that they knew was some of the mathematics and how we were planning to solve those differential equations. So it was throughout that whole experience. Those people were helping us with the programming and with the engineering side. It was an arrangement that von Neumann had made with BRL and with Los Alamos, because our problem would tax maybe 95% of the controls of the machine, rather than what I suppose might have been only 20% or 25% when calculating the firing tables, even in the research interests that were then prevailing. By so doing, we were able to provide them with a much more thorough test of the full machine. It was the very first problem that was done on the ENIAC.

ASPRAY: What was the nature of your problem?

METROPOLIS: Our problem was with one dimension in space and one dimension in time, of course, to study some of the thermonuclear possibilities. These were the first realistic, semi-realistic tests. There were, of course, lots of thoughts about the analytical approaches; but there, the simplifications were even bolder than we were making in our attempts; it was a very complicated problem and we had to make a lot of assumptions to contain the problem within the capabilities of the ENIAC. Nonetheless, it used many, many of the controls -- like 95%.

ASPRAY: Can you give me some idea of the size of the problem? I've read in journalistic accounts, of which I'm somewhat skeptical, that there were very large numbers of punched cards shipped to do the problem, and that the problem took on the order of six weeks of computation time.

METROPOLIS: Well, let me just remind you that the ENIAC had very limited dynamic storage. It used punched card inputs and punched cards outputs. And it was a problem wherein each cycle represented the state of the solution at that moment.

ASPRAY: At that stage, yes.

METROPOLIS: In space for a given time interval. So, we would run the problem; each cycle took about a minute to run at electronic speeds. [See page 6 for elaboration of this point.] I remember vividly that the engineers, whenever they were making a demonstration, would use our problem. But this was no violation of security because nobody could possibly unravel what it was that we were doing, even if they had access to the punched cards; because it was a very complicated thing. The information that was available was absolutely beyond -- even we had difficulties keeping things straight. So even by looking at the output you could not possibly infer what it was. But the point I wanted to make is that there was a cycle generator on the ENIAC, which was variable; so just by turning a knob you could reduce the cycle time. And in one of the demonstrations they would tell their visitors, "Let us run it at the speed of the Mark I," which was an electromechanical computer. It had introduced the radical new idea of combining a lot of sequential operations instead of, in the old fashion, going from one machine to the other to do individual operations.

ASPRAY: But a relay just can't match an electronic speed.

METROPOLIS: That's right. I was wrong in saying that it took about a minute for a cycle time. What I should say is that the output punch would go along when it was running our problems as click, click, click, click -- maybe about a second between clicks. But this is the situation at each space point for each time cycle.

ASPRAY: Right. So to get a whole one, you have to run that whole set of points.

METROPOLIS: Right. So the overall punch time was about a minute. But the rate at which cards were being punched was at about a second at a time. Now when they turned the oscillator down to Mark I speeds, it took about a minute of computation. You never were so impressed by the difference between a second and a minute as waiting for that thing. In fact, the very first few times we thought that the machine had stopped, but eventually Bingo! ... would come the punch. You got a very vivid impression of the difference in computing speeds.

ASPRAY: Was there any hand processing that needed to be done between running one to the next?

METROPOLIS: No, that's in contrast to what was done here with the IBM machine. As I indicated yesterday in the talk, the simple operations could be done, the highly repetitive operations could be done using the IBM. But then there was the question of shock fitting at each time cycle.

ASPRAY: Right.

METROPOLIS: That shock fitting had to be done external to the process and then punched in to the machine as the new initial condition for that operation, that particular cycle. That was not true with the ENIAC. We did not treat shocks necessarily, but there was nothing that had to be done external to the machine except the initial conditions that we posed for the problem. We had to punch out a set of cards that corresponded to the initial condition.

ASPRAY: What was the value of this computation that was done on the ENIAC?

METROPOLIS: Well, apart from the marvelous returns that we got from just having Eckert and Mauchly around to talk about things. The moment that bugs were discovered, we tried to track down where they were, and then be able to tell the engineers, "Look, there's a cold solder joint here, or a tube has gone out," or something like that. While the engineers would be fixing it up, we would be waiting there. Whenever Eckert and Mauchly had an opportunity they would drop around to find out how things were going. Usually that included a period where we had a chance to chat about things. So we would talk about this earlier history of the evolution of the ENIAC as well as what the future

was possibly holding. It was a marvelous opportunity for an indoctrination. It was absolutely priceless, almost.

ASPRAY: What about its insight into thermonuclear problems?

METROPOLIS: Oh yes, well then we ran a whole series of problems corresponding to different situations, a different set of initial conditions. We tried to run a set whose ensemble would enable us to make certain inferences about what the prospects were for the possibilities of thermonuclear ignition, taking into account a lot of the simplifications. We tried to take into account these simplifications in our extrapolations. I must say that it was Edward Teller who urged us to continue running these problems even after the war was over. Indeed, we stayed on until the very early part of the following year and then came back to Los Alamos. Edward wanted us to document all of the results, and when documented that we would have this conference of some of these luminaries to listen to the results. That's when the director, Norris Bradbury, Carson Mark, Johnny von Neumann, and Stan Ulam were involved. And Bob Richtmyer, I think was present. Of course, Turkevich and Stan Frankel and I were there. There was a description of the computer as well as the results.

ASPRAY: So this would have taken place in early 1946?

METROPOLIS: That's right, in the spring of 1946.

ASPRAY: Can I go back to an earlier period, before the ENIAC calculations, for a couple of minutes. One thing that I don't know about von Neumann is how much he knew, or how much he was interested in automatic computing, before he came to Los Alamos. Can you tell me anything about that?

METROPOLIS: He was interested in the logical aspects of computing and also as it related to a lot of theoretical problems, because Godel was at Princeton at the Institute. Alan Turing had been there. He had been a student of Alonzo Church, who was the Princeton University logician, professor of logic. Turing worked with Church. Von Neumann was interested in some of these decision-making problems, as well as the Godel theory. (The Godel work

came as a tremendous shock to von Neumann because von Neumann had been at Gottingen and had worked with Hilbert...)

ASPRAY: Hilbert of the Hilbert program.

METROPOLIS: Right. There was always this challenge to make mathematical foundations complete and consistent. Both Hilbert and von Neumann independently, or maybe even jointly (I don't know to what extent they had worked jointly on it). But anyway von Neumann had taken on this challenge of producing this proof of the completeness. So I think it had never occurred to von Neumann that maybe the things he was trying to prove may have not been provable -- they may even have been false.

ASPRAY: Did von Neumann actually talk about this work, talk about Turing and Godel at Los Alamos?

METROPOLIS: In his talks and in conversations he would remark about the work of Godel and even of Turing. I have to be careful because there were some things that Turing did immediately after the war and some things that he may have done in connection with his studies at Princeton. So it may be that that was a somewhat later thing. I'm trying to remember. I think the decision problem of Turing was before the war.

ASPRAY: That's right.

METROPOLIS: Okay, so then he would have certainly been mentioning Turing's early work. But that period is kind of fuzzy in terms of accurate dates. But there is that period from 1939 to maybe 1948 that there is a lot of activity going on that I have a little difficulty with, since I haven't been thinking recently about these things.

ASPRAY: The reason I asked this is that it's well known that von Neumann offered Turing a position as his assistant at the Institute -- that was declined. But it's also stated with quite a bit of evidence in Andrew Hodge's biography of Turing that von Neumann, when he wrote letters of recommendation for Turing, never mentioned the work on theory

of computing. He mentioned minor results in other areas, in number theory and analysis for example. It wasn't clear at all from the documentary evidence that at the time that Turing was at Princeton he really appreciated the significance of the work in logic.

METROPOLIS: Well, I can't respond to that because I'm not privy to the pre-war situation, but it seems to me that the mere fact that von Neumann had offered Turing this assistantship... It was a genuine plum for Turing and it must have been very difficult for Turing to turn that down. But it was only because he was conscious of the war clouds gathering in Europe, and he wasn't far off the mark in his thinking. He felt that he had to go back. I'm sure that you're familiar with the Brian Randell report that was given here.

ASPRAY: Yes, I am.

METROPOLIS: It shows that Turing was really completely dedicated and completely competent to make very important contributions. So I would have thought, just judging superficially from my impression of Turing seeing him at the dedication exercises at Harvard of the Mark I computer shortly after the war.

ASPRAY: In 1947, January of 1947.

METROPOLIS: January of 1947. Turing had to be a very impressive guy. Von Neumann, with his perception... I'm sure he must have properly evaluated him. I don't know what the circumstances were with respect to what von Neumann wrote in letters of recommendation. He may have just thought that these had been things that would have impressed whomever he was writing to, more so than... After all, he wasn't writing an analysis of Turing's contribution as a biographical kind of thing or as a scientific evaluation, but perhaps things that might have been more relevant. I think that von Neumann was extremely fair and very careful about the remarks that he made about anything and anybody.

ASPRAY: All right. Let's come back to this question that set this discussion off on Turing. Your response originally

was that von Neumann knew about all of this work in logic and the theory of computation. What about in practical computing?

METROPOLIS: No. He learned most of that, I think, here at Los Alamos. I remember his wanting to wire up a plug board and being aware of what was the down-to-earth interaction between theory and practice. He would be conscious of how many counters there were in the tabulator, how rapidly it transferred information, and what capabilities each of the instruments had. But he was learning all of that firsthand here at Los Alamos.

ASPRAY: That would have been both punched card equipment and desk calculating equipment?

METROPOLIS: Well, I think mostly it was the punched card equipment that he was interested in. The desk calculators were so simplistic that he wouldn't need to spend any of his time. But when it came to the theoretical questions in talking about the mathematical and physical implications of the assumptions and things like that, there was lots of discussion.

TAPE 1/SIDE 2

ASPRAY: As I understand from either your lecture yesterday or from some of your writings, one of the services that von Neumann provided to you at Los Alamos was reporting on other activities in computing. Could you tell me about the places that he reported on? Which ones did he know about at this time?

METROPOLIS: Well, there was certainly the Mark I.

ASPRAY: The Harvard Mark I.

METROPOLIS: Right, the Harvard Mark I. He was aware of the activities at Bell Laboratories.

ASPRAY: Stibitz and company.

METROPOLIS: And Williams. On the relay machines there. He was very close to the work of Pitts and McCullough who were doing neural science for the first time at Chicago. So he reported on those things, which showed Johnny's very comprehensive interest in all of these things. We all know of his very famous paper -- among many of them -- on the reproductive capabilities, possibly, of some computing machine. That's a very deep question, which I think is glossed over by many people, because that's one aspect of living organisms that differentiated them from the machine world. That's why Johnny was interested in to what extent was it conceivable that a machine could be rendered sufficiently capable of even reproducing itself.

ASPRAY: Did he investigate the possibilities of using the various analog machines, for example differential analyzers at MIT?

METROPOLIS: He was very much aware of the differential analyzers and, for instance, after the war there was this development of digitized analyzers.

ASPRAY: Like MADDIDA?

METROPOLIS: MADDIDA is exactly the one I have in mind. But it was he that stressed that these differential analyzers have certain capabilities which were very valuable. But he also appreciated that they had limited precision capabilities, and perhaps it was possible to get an accuracy of one part in a thousand with care. But if you wanted one more decimal place, then you had to invoke watchmaker techniques. Okay? So he was certainly aware of them and frequently made a comparison between the digital approach and the analog approach.

ASPRAY: Had he any awareness of the machines that were being built in Dayton, Ohio to be used by the cryptologic community in Washington?

METROPOLIS: I don't know that answer, but I would like to say that I would be very much surprised if he did not. He may have known about them but never mentioned them because of the nature of the higher classification of those projects. But those are logical problems and those are the things, of course, which were of great interest to Turing. So I would be very much surprised if Johnny was not privy to them. But as far as any communication, there wasn't any.

ASPRAY: This is somewhat off the subject, but since we have introduced these topics let me ask one last question of this sort. Do you know of any interaction between von Neumann and Turing during the war period?

METROPOLIS: No, I do not. But I do know that von Neumann did go to England during the war. Whether or not that involved any interaction with Bletchley Hall, I do not know.

ASPRAY: I know that on that trip he saw Max Newman, but he saw him not in Bletchley; I think he saw him in Manchester, or Oxford, or something. There's a letter from von Neumann to Veblen talking about seeing Newman. That actually piqued my interest because there's something very vaguely worded in the letter that could have referred to Bletchley Park, but it was hard to tell. Let me go back directly to the Los Alamos activities. You've already mentioned that von Neumann enabled you to use the ENIAC. I understand that there were Los Alamos computations made on a number of other machines, like SEAC and one of the early UNIVAC machines. Did he play a role in making those available to you?

METROPOLIS: Well, I don't know to what extent. For instance, Bob Richtmyer is the principal one who was using those machines. He used the SEAC and he was at Princeton getting problems ready for the time when the IAS machine became available, but that already is in the 1952, 1953 period. But earlier, I think he had used the SSEC.

ASPRAY: In New York City.

METROPOLIS: In New York City. He was trying to use whatever facility was available. Of course, he eventually did

come to use the ENIAC, as far as I remember. I think he did because I know that people like Foster Evans, Cerda Evans, Jack Calkin, Paul Stein, and Jerry Suydam went out (after we used the ENIAC in 1948 to do our first ambitious Monte Carlo problem. They followed after that). That was after the time when the Clippinger idea had been implemented on the ENIAC, so that they could just turn knobs on these function tables.

ASPRAY: Let's turn to von Neumann's other contributions at Los Alamos. One of the things that's been mentioned to me a number of times this week is the role he played in settling the laboratory on implosion as the assembly procedure. Can you tell me about that?

METROPOLIS: Yes, the idea, as I first heard of it, was due to Seth Neddermeyer. He had this idea. Von Neumann was very quick in seeing all of the implications of a really good idea. He could just run through, like no one else could, the consequences that might ensue from any new idea. For instance, there was the stored program concept about which there had been lots of discussion as to how it came about. So it was with the implosion concept. Seth first proposed it as a way of bringing together this critical material in a short time scale. I'm not sure of this, but I think again it could be tracked down... whether it was Seth who appreciated the fact that after the spherical surface would come in it would lead to increased densities at the center, that this would cause the reactions to take place much more efficiently owing to the increase in density. I'm not sure whether Seth was aware of that or not; I just haven't looked at it. There was a time when I did know the answer to these questions, but here I have to be a little more careful. I don't know whether it was von Neumann who appreciated this first. But he certainly did appreciate it and he was able to elaborate and to encourage that this had real possibilities and thereby caused the laboratory to seriously consider it as part of its program.

ASPRAY: Now as I understand it, Neddermeyer had been working on this for some period of time before von Neumann arrived, but that there were some alternative options for detonation that were being considered. Is that correct?

METROPOLIS: Well, originally it was thought that maybe the plutonium could be used in a gun. But when we

learned about the spontaneous fission of, I think, 240, then it was clear that the time scale for the gun method would not be satisfactory because there could be predetonation. So there was this other method; that's what made Neddermeyer's suggestion so valuable, because it was on a much shorter time scale for assembly.

ASPRAY: Was von Neumann involved in following through on this idea afterwards, or was his contribution mainly getting the lab moving in the right direction?

METROPOLIS: As you know, one of the characteristics of von Neumann was that for the crystallization of his thinking, he would write things down. As a consequence, this characteristic enabled other people to have some formal report available to read and reflect on. I think it was this characteristic that enabled the thrust of von Neumann's impact on the program. You have to remember that since he was also a consultant at BRL and was interested in many of the things that relate to this concept, it would not come as anything quite new to him.

ASPRAY: That precisely was going to be my next question for you. I understood he'd already been interested in questions of shaped charges; he'd done work under contract for NDRC and previously to that at Aberdeen. Could you tell me a bit about the background of that work and how it relates to the implosion problem?

METROPOLIS: I didn't spend very much time or effort on those things, so I only know them as an innocent bystander, if you will. I know that he was very much interested in ramjets, which were the shaped charges, which are slightly different from lenses.

ASPRAY: I don't know anything about this subject.

METROPOLIS: Okay. So there was this question. Well, it's not unrelated to explosive lenses, but it's a slightly different emphasis. So he was aware of these things; therefore, when the implosion question came about it was certainly correlated with some of his earlier work. That's what, I think, enabled him to make a very realistic assumption. Of course, he would have been interested in spherical shells which are imploding, that there are

instabilities associated with them because it's a free surface running in. And he was certainly aware of Taylor instabilities.

ASPRAY: Were the contributions that he made, for example in his written reports, essentially ones in helping to translate the physical problem into mathematics, or taking the stated problem of mathematics into numerical solution, or what else?

METROPOLIS: Well, I think both. You know, he knew a lot of physics. I don't know whether your familiar with the story that's told about a friend of his who wanted to really find out whether he was a physicist, primarily or conceptually, or whether he was really a mathematician.

ASPRAY: I don't know this story. I'd like to hear it.

METROPOLIS: Well, it's a nice story. It's a well-known story among von Neumann's friends. When you realize that he's been gone some thirty years, it's perhaps better known to some of the old fogies than some of the younger people. The story goes something like this: this friend of his wanted to ask him a question. He told him on the basis of the answer that he got, he could tell whether von Neumann was fundamentally a physicist or a mathematician. Two locomotives are on the same track coming toward each other at some point $t = 0$, and the trains are going at a uniform velocity. Now there's a fly that flies from the tip of one of the locomotives (at a speed greater than the speed of the trains) to the tip of the oncoming locomotive and turns around and goes back, touches the tip of the other locomotive also coming, and keeps going back and forth. The question was, "How far does the fly fly before it's crushed to death by the two trains colliding?" So Johnny thought for a little bit, not very long, and gave the answer. This guy jumps up. He says, "I knew it. I knew it. You're really a physicist, Johnny." Johnny rears back and says, "How did you come to that conclusion?" Whereupon his friend explains that the mathematician would have summed the geometric series, whereas a physicist would have computed the time that it takes for the trains to collide, multiplied by the speed of the fly to get the total distance. This was his explanation. Johnny says, "But that was such an easy series to sum."

ASPRAY: Could you tell me something about the other kinds of problems that von Neumann worked on when he was at Los Alamos, both during and after the war?

METROPOLIS: Well, there was a problem that involved linear rational transformations, which was work that he and I did together; as well as another aspect of the same problem that he did with John Calkin, who was a mathematician that was interested in doing some of this work. This was during the war. After the war the very important effort that Johnny devoted to Los Alamos was with the Monte Carlo. There the original suggestion came from Stan Ulam. I think I've tried to give some of the spirit of that effort by saying that Stan had sat in on the first report of the ENIAC results. Therefore, he was in a position to be impressed by the differences in computing speeds between the electromechanical devices and the electronic devices. So when he heard that first report, it must have been the circumstance that inspired him to think about resuscitating statistical sampling techniques. There are two aspects. I think that Stan was conscious of each of them, namely: that there was this tremendous increase in electronic speeds. Therefore, the question of getting statistical samples would have been very much encouraged to enable him to propose this resuscitation of the statistical samples. But also, he was aware of the neutron diffusion work -- that when there is neutron scattering, for instance, one has a statistical game of chance to play in the very problems that were of interest. So that there was a statistical component in some of these equations. He must have realized that because he was interested in neutron diffusion. Some of his earlier work with Hawkins and Everett must have made him very sensitive to these things. So I think he thought this was the way to go. He mentioned it to von Neumann, and again von Neumann, as with the Neddermeyer implosion suggestion, realized that maybe this is something that is very worthwhile to pursue. Once again he sat down and tried to work out what all of the details might be. He so did in a letter to Richtmyer.

ASPRAY: This would have been written about...?

METROPOLIS: 1947.

ASPRAY: 1947. That is, writing back to the lab to Richtmyer. Richtmyer was here at that time.

METROPOLIS: Yes, at that time and in fact was the division leader, because after Bethe there had been a short interval when Placek was the division leader.

ASPRAY: He'd resigned for health reasons.

METROPOLIS: That's right; the altitude, I think, was the problem that bothered him. And then Richtmyer was here and was made the division leader. But I think that Bob had greater interest in pursuing scientific problems than administration. He made that clear and that was why Carson Mark, who had been brought here by Placek from Canada, was made leader.

Von Neumann was very much interested in those subroutines that would be useful in implementing Monte Carlo, where from a uniform distribution of numbers you could get some other appropriate distribution. He was very much interested in just this question. He got many of us enthused about it. We would try to work out routines that would be efficient. One of the very first problems was how does one get a set of uniform distribution for the real numbers in any interval. That's when von Neumann came up with his suggestion of taking some big number, squaring it and extracting the middle digits from it and using that as the next iterate. When you exhausted those digits, you would just square that number and extract its middle. That turned out to be a subject that had a number of theoretical interests. We explored some of those things at that time as well as later, for example, to see how long these iterations would continue until they generated a number which had been previously generated, thereby closing the cycle. You don't necessarily close the cycle from the initial number. You may go along for some number of iterates and then continue further and come back to that iterate; so that the cycle had some leader and then it would start cycling from some point on. There were questions about what sort of numbers would generate themselves. Are there numbers that, if you squared them and extracted the middle digits, you would repeat the process. *Ab initio*, in other words, an iterate generated itself straightaway. Then there were questions of the distribution of the chain links, such that you could start with some numbers and find out whether their chain links would be long or short, what that distribution

was. Then later, there were the questions of the Lehmer routine, which Kronecker and Weyl had shown would generate all of the numbers before it repeated. That was used. Then there were all kinds of variants, e.g. combining two such sequences. Instead of squaring a number, you multiply a given iterate by some other iterate from some other similar process, and extracting the middle digits of that.

ASPRAY: The way that you've described this is that the suggestion for these Monte Carlo techniques came from their potential applicability to certain problems that were of interest to the laboratories, but that then there was a certain set of investigations about the formal properties of the Monte Carlo techniques. Was there also a current line of research of application of these back to these original or other problems of interest to the laboratories?

METROPOLIS: Well, yes. It was easily seen that there were many problems of the laboratory -- mathematical problems -- that would lend themselves to this kind of technique. In fact, one of the earlier suggestions was that maybe the Schroedinger equation itself could be solved by these techniques. Certainly if one wanted to use these techniques for evaluating very complicated integrals, one could use the statistical method. I think that one can just summarize by saying that there were a whole host of problems that were being done using Monte Carlo methods.

Inasmuch as the ENIAC was being moved from Philadelphia to Aberdeen, there was a small hiatus so that people were trying to use some of the slower machines that were available for doing some of these preliminary calculations. But the first ambitious tests were done after the ENIAC was installed in Aberdeen and, in fact, immediately after the background controls were made available to the ENIAC. Then there followed a sequence of Monte Carlo types by the previously mentioned Evans, Stein, Calkin, Suydam, and others.

ASPRAY: You've mentioned several times, in your lecture yesterday and in some of your writing, about Fermi's previous knowledge of Monte Carlo methods. Was he involved in these activities as they developed in the laboratory with von Neumann and Ulam and others?

TAPE 2/SIDE 1

METROPOLIS: Fermi was interested in statistical sampling as early as the middle 1930s in his neutron diffusion work at the University of Rome, and he had applied these statistical sampling techniques. He would do these things in the wee morning hours when his insomnia was contributing to the science effort that he was making. He would use a small mechanical computer to help him expedite the computations that he was doing.

I should mention that Fermi was always interested in the modern electronic computer. He was interested even in the electromechanical computing that had been going on during the war. I related yesterday that incident about how he wanted to learn about the techniques, and that he was terribly keen about knowing exactly the complexity of a problem that was realistically tractable by an existing computer system. For instance, in the case of the electromechanical machines, he knew that one could compute this rather complicated polynomial expression for the masses of nuclei (as a function of the mass number A and the charge Z) taking into account a lot of effects that he had considered. Later, when it came time for him to become involved with electronic computing, which he did with our computer here at Los Alamos, he had a much more complicated problem, which could not have been done on the electromechanical machines but could be tractable on MANIAC I. That was namely the pion scattering by hydrogen that he and Anderson and others had built the cyclotron for at the University of Chicago. One of the first experiments they did was this scattering of ions by hydrogen. He was interested in analyzing the results to get at what corresponds to the phase shift analyses. Once again, it was just an ideal problem for dealing with that situation. Then later, he proposed and worked with Ulam and Pasta, or maybe I should say Pasta and Ulam, on this famous non-linear oscillator problem. In general he was always interested in what the technology was like. I remember after the war I rejoined the faculty of the University of Chicago and saw a great deal of him. Fermi would ask me whenever he was travelling to give his lectures in quantum mechanics. This was an auditorium-wide class -- in contradistinction to the sizes of the classes that we had in pre-war times when the number was 15 to 20 at most -- that was considered a big class. Suddenly there would be a whole auditorium full because of all of the build-up of graduate students owing to the hiatus of the war, you see. He was interested, for instance, and surprised by the fact that magnetic tapes could store reliably so much information and be so effective and efficient, in addition to the kinds of things that computers lent themselves to. He appreciated that there had been problems in mathematics and in

physics that had been pushed aside because there was no capability of dealing with these problems. With electronic computing, he became very much involved with those kinds of problems.

ASPRAY: Although it may not have been his direct purpose, von Neumann made a number of contributions to numerical methods. Is it possible in a short period of time to characterize the significance that these had for the development of a new numerical analysis that is computer-oriented, or how it contributed to the subsequent development of numerical analysis in the 1950s and 1960s?

METROPOLIS: Well, I think it was he who first called our attention to the Courant, Friedrichs, Lewy conditions for time and space interval relations. He knew about those. He was also aware of the Taylor instabilities. Then, of course, there is the famous application that was made with Bob Richtmyer utilizing artificial viscosity in order to avoid having to compute the shock wave conditions. In other words, the shock waves represented discontinuities; and therefore there were special conditions that had to be fitted each time. Their suggestion was that maybe one could introduce some fictitious viscosity which precluded discontinuities and enabled one to proceed with the calculation and therefore did not have to make these special computations at each time cycle. So there was this viscosity method. That's the thing that comes primarily to mind.

ASPRAY: All of these were issues that continued to be of great interest to numerical analysts after this time.

METROPOLIS: That's right.

ASPRAY: I have a similar question about the theory of hydrodynamics. These problems are fundamentally ones of fluid dynamics. It's a field that hadn't had a great deal of study prior to the late 1930s, let's say. How did the contributions he made during the war years, and shortly thereafter, contribute to an understanding of the theory of fluid dynamics?

METROPOLIS: Well, he was very conscious that there were these nonlinear aspects to hydrodynamics. One can try

to linearize things and, hence, make them tractable by analytic methods. But I think he was convinced that in order to get to the real aspects of hydrodynamics, one had to understand further these nonlinearities. I think it was his hope that by looking at some numerical examples, it might give some clues on how to make a mathematical attack on the problem. This kind of activity was going on not only in this country, but also in Russia and Germany and other places, where people were concerned with how does one deal with these nonlinearities. By looking at a few numerical examples, I think, it was his hope that it might stimulate some analytic approaches, some mathematical approaches for dealing with these problems. We were to see some fruition of these and other aspects of nonlinearities; for instance in the nonlinear oscillators. Kruskal and Zabusky were able to carry that to the limit and come up with the notion of solutions. We had some empirical information dating back from the 19th century on some of these experiments that had been noted and wondered about. Here the solutions enabled a rather interesting analysis of that problem and then later one of the byproducts of that activity was the question of inverse scattering that led to problems of nonlinear phenomena in mathematics, and rendered tractable. There was other work, like that of universal sequences, which is a direct consequence of computer capabilities enabling these *experimental* mathematical efforts. Universal sequences are simple iterations of a quadratic form that depend on a single parameter. We learned that if one takes any other quadratic form that has the same general characteristics of the original quadratic form in that it is symmetrical about $x=1/2$ and is unimodal, one would find that the sequences were exactly the same and that's why they were given the name of "universal sequences."

ASPRAY: Okay. Was this work that was done mostly at the labs?

METROPOLIS: Some related work had been done by Ulam and Paul Stein, but they were interested in much more complicated functions to iterate, in two dimensions and things like that. But later, Paul Stein and I, along with Myron Stein, who was not related to Paul but who acted as our interface with the computers, and who did quite a bit of the coding for these problems... One thing that emerged from that study of universal sequences was that we were using an on-line display scope. Without it we would have had grave difficulties in seeing what was really going on in those processes. It was one of the early applications of man-machine interaction, which I think was rather interesting.

ASPRAY: This would have been done at what time?

METROPOLIS: This was on MANIAC 2, and it was done around 1966.

ASPRAY: If I may go back to one of your remarks, you said there were others in Germany and Russia doing work at the same time that von Neumann was. Can you give me some names?

METROPOLIS: Well, I don't know what the time correlation is, but I think it was early on. And Olga Ladyzenskaya, who was in Leningrad, has been very much interested in the question of these partial differential equations that have these hydrodynamical overtones. She was one of several of the people there, and it's a name that I remember.

ASPRAY: There was a question that came up in the question and answer period yesterday that I would like to pursue again with you. It's this question of von Neumann being familiar with the computational needs of the laboratory and perhaps keeping those in mind in the design of computers that are being built at certain periods of time. It's clear that he would have the opportunity to do so with the Institute computers, since he had responsibility for overall design. Do you think that that's true, for example, through his consulting relationship with IBM? Did 701 and perhaps even 704 have some specifications that von Neumann suggested because of the needs of places like Los Alamos?

METROPOLIS: I don't know that I can answer it specifically, but let me try to give you some general flavor of what I believe are his contributions. It is true that the question of utilizing electromechanical machines, namely the IBM business machines, for our computations preceded von Neumann's appearance on the scene. So it was clear to the people here that we had to invoke some of that capability in order to do some of the work that we had to do. The work was primarily on the nonlinear partial differential equations of hydrodynamics. That was related principally to the implosion. When he came, he had conversations with many people here. He also had his other consulting responsibilities. So he was putting them together. First and foremost, he was a consultant to BRL and, therefore, he

would have taken a very keen interest in the ENIAC development. In so doing, he became a part of the activity that would be subsequent to the ENIAC, namely the EDVAC. It was the intention of BRL to go ahead with the development of a next phase, and that was the EDVAC. There were many discussions as to what its characteristics should be. That was one place where he did interact. In connection with IBM, it is my understanding from Cuthbert Hurd, and I believe that this is the correct version (because I was under the impression earlier that the 701 was a consequence of von Neumann input)... But Cuthbert Hurd, who was in a position to know because he was involved with IBM as head of their research operations stated that it was from general publications and by conference attendance, that IBM found out what some of the thinking had been. A large part of it would have been von Neumann's contributions; not directly, but only through this assimilation stage that they constructed the design of the 701. Now Cuthbert Hurd can give you these facts: that von Neumann actually came on board at IBM only after the finalization of the 701 design features, which came as a consequence of all of the previous attitudes for which Johnny had a tremendous input, but not directly. But when he came on board, they were then thinking of the 704. Johnny had a salutary effect on it. So there are these contributions with respect to IBM, with respect to the BRL contact, plus the fact that Johnny was very much interested in utilizing logical principles and set up his flow diagram for formulating a computational problem in preparation for its being implemented on the computer. He had this flow diagram, which I found eminently sensible, and very precise, and quite elegant to adopt. It really set out in a manner that enabled you to see where all the structure was. They were in his writings, on the physical realization of computers that he wrote with Goldstine, Bigelow, and Burks in a series of volumes. I was sorry to see that flow diagramming didn't endure to the extent that it should have. But maybe the consequences of that kind of thinking translated itself into some of the other algebraic languages that were used for problem preparation.

ASPRAY: One last machine to ask about possible influence -- NORC. Is there a connection there?

METROPOLIS: I know that von Neumann may even have been a consultant to Dahlgren where the NORC was. He was certainly aware of it. I first learned of it through him, and I think there were some arrangements for using the Dahlgren machine for Los Alamos problems. In other words, I think it was just true that Los Alamos tried to utilize every conceivable facility for its work.

ASPRAY: I knew he gave the dedication speech for NORC, for example.

METROPOLIS: Yes, right.

ASPRAY: Von Neumann was just a consultant at Los Alamos, but he was here quite frequently for a long period of time, both during and after the war.

METROPOLIS: Not during the war so much.

ASPRAY: Not so much?

METROPOLIS: Not so much during the war. He would come frequently, but he never stayed very long. But whenever he did come, even for those short periods, he had a tremendous impact. After the war, when things were a little more relaxed, he then would come and become more involved and spend a great deal of time. He spent a great deal of time when we were doing the ENIAC problems. On ENIAC, for instance, he came down to Aberdeen -- where he would go anyway for many of his consulting activities because he was a very good friend of Del Sasso, who was in charge administratively of a certain portion of it, and of Dederick. He was also a good friend and colleague of Bob Kent, a key person at BRL. When we were doing the Monte Carlo problems, and that was being done with Klari von Neumann, he would come and spend quite a bit of time. After the war he would be here for one or two week periods. He was interacting with everybody at the lab, and in particular with his colleague, Edward Teller, because he was very much interested in the thermonuclear development.

ASPRAY: Los Alamos is a place that's filled with scientific luminaries; especially during the war, but also afterwards. What kind of recognition did von Neumann get among these very high stature physicists?

METROPOLIS: He was certainly very much respected by Oppenheimer. And obviously so by Edward Teller. I'm

sure Hans Bethe would have viewed him as a tremendous asset to the laboratory. There is a little anecdotal version of Fermi's interaction with von Neumann. I'm sure that they had many discussions together about various things. But one summer they collaborated on Taylor instabilities.

ASPRAY: Can you tell me what year that was?

METROPOLIS: That would have been 1951, plus or minus one. Fermi and von Neumann overlapped. They collaborated on problems of Taylor instabilities and they wrote a report. When Fermi went back to Chicago after that work he called in his very close collaborator, namely Herbert Anderson, a young Ph.D. student at Columbia, a collaboration that began from Fermi's very first days at Columbia and lasted up until the very last moment. Herb was an experimental physicist. (If you want to know about Fermi in great detail, you would do well to interview Herbert Anderson.) But, at any rate, when Fermi got back he called in Herb Anderson to his office and he said, "You know, Herb, how much faster I am in thinking than you are. That is how much faster von Neumann is compared to me."

END OF INTERVIEW