An Interview with

ARTHUR W. BURKS

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Arthur W. Burks Interview
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Abstract

Burks describes John von Neumann's contribution to the development of computers. In this context Burks discusses the ENIAC, the work of the Moore School of Electrical Engineering, and the development of a computer at the Institute for Advanced Study.

ARTHUR W. BURKS INTERVIEW

DATE: 20 June 1987 INTERVIEWER: William Aspray

LOCATION: Ann Arbor, Michigan

ASPRAY: This an interview on the 20th of June, 1987 with Arthur W. Burks in his home in Ann Arbor, Michigan. Let us begin by having you tell me something about the connections between von Neumann and John Mauchly as regards the problem of weather prediction and weather calculations.

BURKS: The main computational problem that Mauchly worked on while he was at Ursinus College was a weather problem. John thought that by analyzing weather data with Fourier analysis, that he could find periodicities in the weather that were equal to or integrally related to sunspot cycles. For this purpose, he built at Ursinus a 12-ordinate harmonic analyzer -- a machine made from resistors and manual switches. And he was planning to build a 27-ordinate harmonic analyzer. These machines were called harmonic analyzers since they found the harmonic components of periodic waves.

ASPRAY: He was going to do this at Ursinus, or at Pennsylvania?

BURKS: He did this at Ursinus, and this was the source of his interest in computers. And when he went into digital computers, initially his interest was in digital methods of doing Fourier analysis. On one occasion, when von Neumann visited us, John met with von Neumann for a while and explained his ideas for "solving the weather problem."

ASPRAY: Now, this was at the Moore School.

BURKS: This was at the Moore School.

ASPRAY: At that time.

BURKS: This would... Well, von Neumann first came to the Moore School in the summer of 1944. So this would have probably... This would have been after that -- somewhere between the fall of 1944 and the end of 1945.

ASPRAY: All right.

BURKS: Von Neumann's own idea for using computers to do weather analysis was to find certain simple differential equations governing the mechanics of the weather, and solve those. So this was very different from John Mauchly's method. And von Neumann was not much interested in what John Mauchly had done.

ASPRAY: Now, you told me before we were on tape that they had had a meeting. Can you tell me what you know of that meeting?

BURKS: I remember John Mauchly telling von Neumann that he wanted to explain his weather ideas to him. And Mauchly and von Neumann went to Mauchly's office, at which time Mauchly explained his ideas to von Neumann.

ASPRAY: You said this was about a half an hour meeting.

BURKS: Well, I'm not too sure, Bill, about how long. I doubt if it was more than an hour, but this was a long time ago.

ASPRAY: All right. You weren't present, so you don't know exactly what happened. But you did get reports from Herman Goldstine, I understand.

BURKS: Herman told me that von Neumann was not much interested in what Mauchly had to say, because von Neumann didn't think that that was the way to solve the weather problem, that is, it was not the way to use computers to predict the weather.

ASPRAY: Yes. Were there any subsequent interactions between Mauchly and von Neumann on this question?

BURKS: Not to my knowledge.

ASPRAY: Okay. Staying on the topic of weather and von Neumann, but leaving Mauchly aside for the moment, I understand there was a famous incident relating to weather with the people from RCA. Can you tell me about that story?

BURKS: I can tell you what I saw of it at the time. There was a report (I believe in the New York Times) of a meeting in Washington that von Neumann participated in. And someone from RCA; I believe it was Zworykin, is that right?

ASPRAY: Zworykin.

BURKS: All right. And it was announced in the New York Times that these people had addressed a group in Washington, advocating the construction of computers. And one use of the computers would be to solve the -- more simply, to predict what the weather would be. And no mention was made in the New York Times of any background work done at the University of Pennsylvania. No mention was made of the ENIAC. No mention was made of the planned EDVAC, and the thinking at the Moore School on electronic computers was clearly far ahead of anything that RCA had done in this respect. So the people at the Moore School -- Eckert and Mauchly and the rest of us -- were surprised and a little shocked at this. Von Neumann came to consult (I believe it was the next day), and he was surprised at this article in the New York Times. And he tried to call both Washington and Princeton to talk with people as to how it occurred, but there was a telephone strike at that time, and he wasn't able to get through.

ASPRAY: Was this, as far as you know, the first time that he and Zworykin had talked about this topic?

BURKS: I don't know when he started talking with Zworykin. Von Neumann knew Jan Rajchman. Jan Rajchman and RCA had, early on, made a preliminary study for the government on the possibility of using electronic technology to build electronic fire control equipment. We knew that; and the people from the Moore School had visited RCA, learned about their work, and, in particular, learned about a counter circuit that one of the workers at RCA had developed in some connection. So we knew that RCA had done work on electronic computers. This was our first knowledge that at that time they were working on electronic computers.

ASPRAY: Let me turn to a rather different question now about the design of the Institute for Advanced Study computer. Can you tell me something about the kinds of ideas that von Neumann had? How did he arrive at the design features of... the basic design features of the machine? Were there particular engineering needs, or particular scientific needs that shaped the design of the Institute computer?

BURKS: Yes. This can be simply answered by saying that he wanted to take the technology that was available and the technology that was being developed, and make a machine that was as powerful as was reasonable, given that technology. There was a plan for the EDVAC. There was now the possibility of a much superior memory using cathode ray tubes. This memory would be superior because a large number of bits could be stored on the surface of the tube in such a way that any position could be accessed as easily as any other position. Therefore, one could have a random access memory. And this would be much faster than the mercury line memory, where there was the problem of waiting for the information the machine needed to come out of the line where it would be accessible. One got an additional factor of speed by using the random access memory in a bit parallel manner. When we stored forty bits in forty tubes, a word of forty bits could be removed or written all in parallel. Well, given those constraints, one had a machine which was far more powerful than the ENIAC, the only operating electronic computer at the time, and one that would be considerably more powerful than the planned EDVAC-type machines using the mercury delay line as a memory. Von Neumann had a wide experience with different problems, partly through his consultation with Los Alamos, but partly through his consultation with other agencies in government during the war, and because of independent interests he had in physics computations, weather computations, and so forth. So this new machine would be a powerful tool -- would enable him to solve mathematically and numerically lots of problems that couldn't be solved at that time.

ASPRAY: If I understand your answer to this question correctly, you're suggesting that engineering feasibility drove the overall design. In some sense, that here are some good ideas that could be put into effect rather rapidly, and would produce results. And that it was this that was the overriding consideration, rather than that we have the following need... scientific computational need, or engineering computational need for Los Alamos, or for meteorology, or for whatever, that has the following parameters that this problem has to solve. Is that a correct statement of your...?

BURKS: I think so, Bill. That is, it wasn't the case of saying, "Here are these problems, and we can't solve them unless we have a machine so big. And let's do the development work that will enable us to build a machine that big. It was rather that we could start with the available electronic switching and storage technology, which we knew at this time could be operated in essentially a million cycles per second, and proceed with what seemed like a relatively easy development problem, that is, developing the cathode ray tube into a memory. So with a manageable amount of development we could feel pretty confident of having a machine in two or three years. Now actually it turned out that in the case of the memory, much more development was required than anticipated. So these machines of the random access type weren't really available in two or three years.

ASPRAY: But clearly some decisions had to be made. You did have a choice on how much primary memory there was, what the level of precision of the machine was, and so on. Those choices did have to be made on some basis.

BURKS: Yes. Well, the choice on precision would be made on the basis of what was known about numerical calculations, but we really didn't have enough experience in the ENIAC, at that point, to guide us. It did seem that one should have at least ten decimal digits and preferably more. So we were talking about 32, or 36, or 40 bits per word. Then when you looked at the instructions that we had in mind, you could probably get two instructions in a word of, say, 36 bits. But you weren't quite sure of how many you would need for the instruction, and to be on the safe side with accuracy, to be on the safe side with how many bits you'd have for an instruction, a word length of 40 was chosen. And of course, that 40 would allow you to have a memory larger than 4000 -- if at the next stage you wanted to have a memory, say it was 8000 or something like that, so there is flexibility with respect to the follow-up machine. You could say, "Well, let's have two of these memories." Yes, you could have two. The point was, though, that with one memory of 40 cathode ray tubes, each with 4096 bits (which is what was hoped for), one would have a reasonably large machine, and that seemed like a reasonable goal.

ASPRAY: What kind of experience was there with the ENIAC at the time that the IAS design was being made... word numerical problems and the kinds of computational needs you needed... you had on your machines?

BURKS: There wasn't very much experience with the ENIAC yet. The choice of how large the memory would be, how many bits we would have in a word for the Institute for Advanced Study machine, was made in early 1946. At that point the ENIAC had solved the Los Alamos problem, which was not really a typical computational problem. And there was little experience from the Harvard Mark I, which had solved problems -- but again, the ENIAC, and orders of magnitude superior to it in computational form. So it was basically general computational experience derived from hand computations and IBM-type computations made during the war that gave whatever information there was that was relevant to the choice we made.

But there really wasn't much experience; that's right. And part of von Neumann's interest in this was to have a machine so the problems could be run and one could learn these matters that were important and would have to do with how best to do numerical calculations with a powerful electronic computer.

ASPRAY: Can you tell me something about the origins of the plans for a computer at the Institute. I understand these started during the time that work was going on at the Moore School.

BURKS: Yes, von Neumann came as a consultant; at first, I think, in late summer of 1944 when he saw the two accumulators under test, and saw some of the rest of the ENIAC being constructed. He came back two or three times in the fall to learn more about the ENIAC. He came in March and April of 1945 when we discussed the plans for the

EDVAC. And then he went off to Los Alamos and sent back what later became the preliminary design of the EDVAC, the famous von Neumann 1945 report or proposal. Then he came in... continued to come on into the fall and winter of 1945, 1946. And sometime, I would say late summer or early fall of 1946, I heard from Goldstine that von Neumann was very much interested in having his own computer, that Herman was working on the possibility of the Moore School establishing a center for the construction and use of an electronic computer. And Herman was optimistic that money would be forthcoming from the government to build such a computer. At a certain stage, Herman expressed interest in my participating in this. And I reciprocated by saying that I did have an interest in this. By late 1945, probably, the talk had shifted to where Herman was saying (and I heard this also from John Mauchly) that it was less likely that the Moore School would move rapidly on this, and that von Neumann might well start his own project at the Institute for Advanced Study. And sometime fairly early in 1946, Herman was definite about this and invited me to come and work at the Institute.

ASPRAY: Now, the Moore School was moving slowly because they wanted to get the ENIAC finished first, or what reasons would you guess?

BURKS: Well, I think the best way to think of it is that although the ENIAC project was at the Moore School and was administered by the Moore School, no one active in the project had any permanent status at the Moore School. I had been offered a position by the dean to continue teaching engineering there, but there was no discussion that I heard from the Moore School along the lines: "Well, here you've done something great in building this ENIAC. We want to start a permanent activity, which will be part of the Moore School, to design the EDVAC. It was understood that they would go ahead and get a contract for the EDVAC, but there was no institutional regularity suggested for this. It was going to be highly contingent on whether the funds from the EDVAC came along. And it wasn't clear that anybody involved in this would have any more status than they'd had during the war, a temporary status dependent on the existence of outside contracts.

ASPRAY: Okay. Was there talk from the very beginning that Goldstine would be associated with this project? Was he interested in doing so?

BURKS: Well, he was certainly interested in it. And he was promoting it. Eckert and Mauchly, also, were in favor of something of this sort. So it was just assumed that he would be part of it, yes.

ASPRAY: I see.

BURKS: Herman had been responsible in making the liaison between the Moore School and Aberdeen that led to the ENIAC. He, after about a year... maybe sometime in 1944, he was assigned full time to the Moore School as the liaison with ordinance for the ENIAC. So we just assumed that he would be part of the activity, and an important part of the activity.

ASPRAY: You told me before we were on tape something about the reasons that von Neumann might want to build a computer. Could you tell them to me again now on tape?

BURKS: Yes. There were many different interests that Johnny had here, and he viewed them as unified. One of the interests was in a powerful computer to do numerical mathematics, or computational mathematics. And he saw, as clearly as anyone, and more clearly than most, that with these new, fast, large-capacity electronic computers, one could solve many problems numerically that hadn't been solved before. He had a practical interest in seeing that the problems were solved, but more than that, he thought that by solving special cases, one could get insight into how physical processes worked. In particular, he was interested in the problems of shock waves and when turbulence arose; problems that today we would talk about as chaotic behavior problems -- the solving of nonlinear partial differential equations. And it was clear from what he was saying that he thought that with powerful computers one could solve particular cases and use those as a heuristic guide to constructing theories and testing theories, so that experimental computations could play a role something like empirical experimentation could play in science. So that was a computational interest. As a logician, he was interested in the architecture of the computer and the design of programming languages that would be suitable for a computer. He was also interested (and this interest he got through the meetings he had with Wiener and others) on the... how was it natural systems computed. And he thought that there would be a theory of automata that would embrace both artificial devices and natural devices that computed, that would explain certain features of information handling in natural systems, and the nature of evolution, and things like that. And that knowledge of this generalized automata theory would be useful in designing more powerful computers, and in using more powerful computers.

ASPRAY: Now, I don't mean this in any contentious way, but it's easy 40 years afterwards to rationally reconstruct things based upon a long sequence of events. Are you quite clear that this happened... that these were his motivations before the project was set up, that these were the things that he was thinking in 1945, and how...? What kinds of evidence do you have of this? Were they through your own conversations with him?

BURKS: Yes. (Could you turn that off?) Well, I knew in, say, 1946... by 1946 that he had some general interests. When I came to write the introductions to the theory of self-reproducing automata, I made a historical research on his activities, looked more into the meetings sponsored by the Macy Foundation, learned about the lectures he gave at the Institute on self-reproducing automata, learned about the lectures he gave at Illinois. But I remember, when I was a consultant at Argonne in 1950-51, hearing from people that heard his lectures in Illinois that he was talking about self-reproducing automata. So I knew a little at the time. And I learned a lot more when I looked into the history of this.

ASPRAY: Did he talk at all about these general issues when you were working on the reports that became well-distributed as the Institute reports?

BURKS: No, except maybe incidentally. Most of the time was devoted to developing these reports.

ASPRAY: Okay. Perhaps one piece of evidence of an early belief in the heuristic possibilities of the computer was the lecture that he gave to the Canadian Mathematical Society about the computer replacing the wind tunnel in studies of these aerodynamic problems. Were you familiar with this?

BURKS: Well, I was aware of the wind tunnels at Aberdeen. And at the time, I was aware of the possibility of using the computer to do numerically what the wind tunnel did experimentally, yes. That I would have known as a motivation for having this machine.

ASPRAY: Okay. Because I think that was the thrust of the talk that he gave at the Canadian Mathematical Society.

BURKS: Well, I don't know about a Canadian talk, specifically. But traditionally, the attitude of mathematicians to numerical analysis was that it was a one-way street in which the mathematician could give you the equation; for example, the exterior ballistics equation. They had failed to find any simple analytical formula for approximating the resistance function; that is, the resistance of the air to the shell. Well, for a particular application, such as a firing table, you solve this equation, putting in the resistance values that you get from a table. So here was the mathematics and it developed so far, and then the application was made. But now these powerful computers... it occurred to von Neumann that with these powerful computers one could take phenomena where traditional analytical mathematics had failed -- non-linear phenomena, turbulence phenomena (today, what we would call chaotic phenomena). And one could experiment numerically with this; that is, run critical cases, see the behavior of the cases, and in so far as one could have a visual display this seeing could be a visual seeing. And then get ideas in order to construct a new mathematical formulation of what the phenomenon was.

ASPRAY: Yes. Can you tell me something about the attitudes at the Institute for Advanced Study among the members of the School of Mathematics and other mathematicians who may have visited towards the computer project?

BURKS: Yes, I had felt from a few interactions I had had that his colleagues thought that von Neumann was not using his creative mathematical skills properly in being involved in computers. And I remember once going with Herman and Oswald Veblen to pick a site for the new building. And we walked through the woods, but it was clear that Veblen didn't want any trees to be cut down for the building. In the end, he picked a site which was low down, not too far away from the Institute building so it wasn't inconveniently far away. He wanted the building to be one story only, so that this would not be a conspicuous building. I understand now that the building is used as a branch of the Institute of Advanced Study, is that right?

ASPRAY: I don't know.

BURKS: I think so. Somebody told me that he was in that building when he was visiting the Institute, had an office in it.

ASPRAY: And they even put up a building in different architectural style from the main building. Is that correct?

BURKS: Yes. The financing of the building is relevant here. The government was willing to pay $50,000 to build this building. Because of post-war priorities we wouldn't have a high priority. So I said, "Well, does that mean the building is going to be made of wood?" And the answer came back, "No, because of post-war priorities, the returning veterans get the wood for their houses, so this building will be made out of concrete." And I remember also that the Institute didn't want a bare, one-story, concrete building. They wanted a brick face. So they agreed to pay $10,000 to put a brick face on the building.

TAPE 1/SIDE 2

BURKS: I guess I wouldn't make too much of these stories. The tradition of the Institute had been along the lines of pure mathematics and mathematical physics. It was non-experimental. There wasn't much in the way of any other non-science subject, although there was work in art history, for example, at the time. So it was, indeed, a kind of an anomaly for the Institute for Advanced Study to be building a computer, especially when Princeton University was nearby and it had an engineering college.

ASPRAY: On the other hand, one might say that this was the considered judgement as to what would be most significant parts of mathematics, ones that did not involve numerical calculations.

BURKS: Yes, that's right. There was a kind of purist attitude at the Institute.

ASPRAY: Would there have been any members of the faculty who would have been familiar with, or possibly supported calculation?

BURKS: Yes. Oswald Veblen. He had been associated with Aberdeen, I believe, in World War I as a consultant. And he was associated with Aberdeen in World War II as a consultant. He was a chairman of the Aberdeen, the committee of the Ballistics Research Laboratory which considered the ENIAC proposal. And he, I think, was the most influential person in deciding that Aberdeen should sponsor and finance the ENIAC. So he had an understanding of these matters. I don't suppose that he thought of these as contributing to mathematics, per se, but as being an important application to mathematics.

ASPRAY: I see.

END OF INTERVIEW