Interview with Dr. Derrick Henry Lehmer
Conducted by Dr. Uta Merzbach
Berkeley, California
8 October 1969

UM: Let's try the date; I think it's the eighth.

DL: Yes, October 8, 1969.

UM: I'm sorry I interrupted. You started to tell about the problem that you put on, on that 4th of July weekend. Now that was in 1946?

DL: Was it 1947?

UM: Oh, 1947.

DL: It must have been 1947. I'll have to think about it a little bit, but I guess it's 1947. It's something you could check on.*

Well, it was a very long-run problem. It ran 48 hours I think without stopping and, it was in one sense a real test for the reliability of the ENIAC. It was a problem in which the machine generated a sequence of primes and for each prime found the exponent of two, modulo the prime. If this exponent was sufficiently small, I think it was less than 2,000, we'd print out the prime and the corresponding exponent. From this we got our first real advance on the small factors of large Mercenne numbers and that sort of thing.

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* It was 1946. -- DL

John Mauchly helped design part of the program. He helped ride herd on it, or stood by during the small hours of the morning, watching and so on, and my wife and I were helping it, checking on it and taking the cards in and out and so on. But that was, you might say, the only non-lethal problem that had been run on the ENIAC at that time.*

UM: About what time did the AEC sort of cut down on its use of the ENIAC?

DL: Well, it might have been June, 1947. They were in and out, of course. They would take over for a few hours or a few days, and then they would release the machine, and so on. The problem they were running was too complicated for the small memory the ENIAC afforded. They had to make a lot of ins and outs, batch the problem up and so on. We were never told the details of the problem, but we could observe what they were doing all right.

UM: Well that's one difficulty, because I gather that still has not quite been declassified and at times, there have been references to that. It would be interesting, particularly

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* There were some boundary layer problems by Douglas Hartree being run at this time also. -- DL

because they probably had to develop some new techniques to overcome the limitations.

DL: Stanley Frankel and Nick Metropolis were the two people involved, and I'm sure they got some new ideas from this very definitely. These are the two people you'd have to talk to, if they could talk now. I've heard snide remarks since then that possibly they were doing the wrong problem but, there again, that's classified, probably. There were early days-- yes, indeed, they were.

UM: How much influence did the ... there were a couple of the committees; I think the one that Sir Archibald was still on... how much influence would you say the worker in these groups had in, well, let's say, in promoting this activity? Was it doing anything as far as convincing people that these were useful gadgets?

DL: Well, I'm not sure which committees you're talking about. There was an Academy Committee and a National Research Council Committee that was publishing "Mathematical Tables and Aids to Computation". Then, there were other committees, but I'm not sure I remember too much about them, and I would say that perhaps the government became influenced first. I would say that the IBM Company was very reluctant to get into this kind of thing at first. They believed in relays; they liked to think that a relay was either open or shut, and you could look at a relay and see whether it was open or shut. Whereas you couldn't tell what a twin triode vacuum tube was doing. At least one engineer told me that. But later, of course, they were persuaded to get into it up to their necks and, perhaps, a little further.

UM: No, I was wondering how much the professional groups influenced, for example, the decision in the government.

DL: Well, I don't know if there were any professional groups, were there? They were universities.

UM: Well, I was thinking of NRC. In that sense, I was thinking more in terms of professional scientists, mathematicians and the rest.

DL: Well, Archibald and I saw the necessity for having a section of the magazine devoted to descriptions of early machines, and this section was set up by the Bureau of Standards. They had their own little empire there, building the communications between various government agencies and the subscribers and so on. But it certainly was the government that decided to promote the machine first. The Bureau of Standards and the

Bureau of the Census were very much involved with the UNIVAC people originally.

UM: Did you get into the Bureau of Standards work through ...

DL: Well, that was during the McCarthy era, you probably remember. I felt as though I didn't need to sign the "oath". I thought I had tenure here anyway, so I tried it, and they decided that I should sign the "oath". So, I decided that I would work for the government instead. I was boycotting the University for two years, but then the government got worse than the University, and so I went back to the University.

UM: I hadn't realized that.

DL: Yes, but that wasn't the only reason, because there was a very interesting group being set up there by John Curtiss in Los Angeles, and I was interested in trying to get these theorecticians into the "deeds, not words" kind of mathematics that interested me. So, I saw this as a good chance to do that and also get a little more experience with more modern equipment than the ENIAC was.

UM: Yes. Where would you say was any work done that was of significance in relation to theoretical mathematics? Not the theory, but mathematics for its own sake.

DL: At that time you mean?

UM: Yes, at that time and, then, subsequently.

DL: Well, we're talking about now, I suppose, the 1949-50 era. These were hard times, actually, for the developers of computing machines. They hadn't quite come out yet, and they were having trouble being every two or three years behind on every machine practically.

UM: Yes.

DL: We had these two interim (so-called interim) machines the Bureau of Standards put out: the SEAC and the SWAC. And these two were the only really operating, even partly operating computers, except what was at Aberdeen. And that was really, I guess, just about that time that the EDVAC or the ORDVAC came through. Well, there wasn't much done. Von Neumann and Goldstine did one or two things at Princeton on their machine on the early model-- experiments with continued fraction expansions of algebraic numbers of various kinds and the so-called Kummer problem. They ran that. That was written up in MTAC, I believe.

UM: Yes, I think there's a short...

DL: Yes. Well, elsewhere it was largely small groups of card program-calculator installations that occasionally came out with some little thing. In those days it was pretty rudimentary.

Well as these machines became installed and running for about a year, they ran out of so-called practical problems, and occasionally, they would let someone else use them on an idle-time basis, on some different kinds of problems. There is a wide variety of things they've done now.

UM: Yes, yes. I've wondered, I haven't been able to find any specific point or location on which you can say: well now, here was a group that was intent on putting this work for say, mathematics. Would it have been a more diffused ...?

DL: It was diffused, yes. Well you know, these projects became a sort of a gleam in the eye. Then another couple of days or so, some person suggests how to program it, or maybe somebody helps somebody program it. Then, there's one for a night or a weekend or something and, then there's some kind of a notice put out about the results. I have no idea except by looking in the periodical literature how you could present the growth of this thing. I'm not sure what you should do there. In math reviews occasionally you see, in those days or in the fifties and early sixties even, you see in a review one sentence that mentions a machine being used.

UM: Yes, yes. That's right.

DL: And then, even this is disappearing from the reviews now. I always sort of understood that if there was any real numerical work involved that it must have been done by a machine, right. People don't do things by hand much anymore.

UM: Am I right in thinking this may be because I don't know that much about the question of the SWAC? I've had the impression that there was more work on the SWAC which tended to be more theoretically oriented.

DL: Yes, I think that was true. One reason for it was that we got instructions from Washington that we should not compete in any way with private enterprise. I guess this came from Nixon originally. Some small person in the computing, what computing that was being done in the Los Angeles area with what we called CPC's, some person had a CPC installation, and he heard that some work was being done for the Army or for the Navy at our establishment and this was bread out of his mouth, you know. So he went to the Little Business Bureau and he stormed to, I guess it was Homer Ferguson in those days. Ferguson did some phoning and we finally got the word that this was socialism. So we couldn't work on what you might call practical problems, although they could do it in Washington, I understand. Somehow the distance of three thousand miles made a difference, although we were only too glad to be three thousand miles from Washington in those days.

So we were driven or we were apt to do problems in support of the group that we had there, and we had a very fancy group of people: Lipman Burrs, for instance, and the really good analysts and algebraists of that time came through the INA, and stayed for a month or a whole year in some cases.

So, the first priorities went to supporting their research and that's why a good deal of pure mathematics on experiments in methods and so on, partial differential equations and linear equation solving systems and that sort of thing, was worked out there. I have a big file of things that were produced at the Institute during my tenure, and I'd be happy if you'd take it off my hands, actually.

UM: I may take you up on that if you're serious.

DL: Yes. That explains why, since the Institute was not really in business to supply service to government agencies, although in a sense, that was in our letter of intent somewhere, I think.

UM: Yes. This is the curious thing because I mean it was part of the Bureau of Standards.

DL: Oh yes, but the Bureau didn't have much money in it.

UM: And particularly that was the function of the Bureau.

DL: Yes, yes. It was a new idea for the Bureau, really. Mina Rees of the ONR in those days was very influential in promoting this idea of getting mathematical research done at a reasonable price. And although the Bureau of Standards had no money tied up in the thing, really, except they'll probably say, "yes, they must have had", it largely came from the Air Force and the Navy. They were handling the money that way, and the Air Force and the Navy, through Mina Rees, wanted this kind of work done.

We trained a lot of very fine people that way too, and when the axe finally fell on the Institute from Washington, the crowd of people that deserted the ship, you might say, permeated not only Southern California, but the whole United

States, and they spread the word about what could be done, how to do it, and so on. In that way, perhaps, to fire everybody was a good way to disseminate the information. We all went somewhere else and tried to do likewise. It was a little rough on a lot of us, because the laboratories we went to or the campuses we returned to were not supplied with the hardware that we needed, of course, and it took another ten years for that to show up at every university.

UM: Jumping back to that first machine of yours which you said actually goes back to the mid-twenties, do you remember what made you do it?

DL: Oh, yes, I had a lot of problems that could be solved by a sieve process, and I'd been doing this sifting by hand, using large sheets of paper ruled in squares and movable strips. This became too hazardous and too time-consuming, so I decided I'd mechanize that part of it. I was just a student here at the time. My father was also interested in these problems and so I knew I had a source of problems, anyway. I put the thing together, and it was sort of a homemade job using bicycle chains, actually, for the periodic elements. I was able to run quite a few problems before I went to Chicago and had to abandon it.

UM: Bicycle chains?

DL: Bicycle chains, yes. I used bicycle chains of different prime lengths with little pins that you could put in, and contacts that they broke as they went over the top of the shaft. Yes, it's explained in this Monthly article.

UM: You say that that's in the Monthly?

DL: Yes, I can give you a reprint of that I think; I'm pretty sure I have them.

UM: About 1927?

DL: Well, I'd have to look it up, yes.

I was able to do three thousand words a minute which seemed awfully fast after doing hand work, of course. Then, that was stepped up by a factor of over a hundred on the bigger machine.

UM: Photo electric?

DL: Yes, yes. That was the first example of factors of a hundred in speed in computing. Now, factors of ten are gratefully received, I guess, and we're getting near the top, but there has been a terrific speedup, of course, in everything. If there was anything wrong with the machine, they always speeded it up and that made everybody happy again. They didn't always worry about the logical structure of it too much. But another factor of ten and speed was always the thing to do.

I like Hamming's way of putting it. He says: "The fastest way you can get back home is only about a hundred times faster than a brisk walk." But we have factors of ten to the sixth (10^6) of speed now; that makes all the difference of course. It makes your whole reason for computing different. The whole thing, the whole problem, the whole outlook is changed, of course.

UM: Again, going back, something that has puzzled me, maybe just because I had thought of him in a different context, and that is Archibald's interest in math; apparently it was strong.

DL: Oh, yes!

UM: Was it something that developed, or did it come about through association, or what?

DL: Well, historically, he was a historian as well as mathematician, and he had, perhaps, the world's finest collection of mathematical tables at the Brown University Library. Every trip he made to Europe, which was nearly every summer, he would not go to the resorts; he would go to the bookshops and look around for old things that should be put in the library at Brown. Well, then he wrote in Scripta Mathematica especially, lots of articles about old tables and old numerical methods and so on. Then he decided that there was enough material there to, enough new tables coming out as this was the golden age of tables, I guess you'd say, so that he could start this magazine. A number of us who were also interested in tables and mathematical analysis and so on, helped him on the editing and running of the Journal. This was a sort of a hobby of his, I suppose you could say. He was strongly motivated in this direction. Then, as the title "Other Aids to Computation" indicates, he was also interested in old machines, and more or less modern machines.

Comrie, in England, was also a pioneer in adapting commercial desk calculator things to mathematical problems, and so he and Comrie sort of hit it off and they had that section of the magazine. Then, as the electronic things started to come out, and even the early relay machines, he decided, of course, to incorporate those and then, they became more newsworthy, you might say. Then, the thing burgeoned under the aegis of the Bureau of Standards and developed a really effective Journal.

Then, of course, other journals took on; the ACM of course has this magnificent collection of literature now, and the Germans have this Numerische Mathematik.

UM: And what about Zeitschrift fur ...?

DL: Well, that too. In those days, the Germans were obsessed with analog equipment.

UM: Yes, that's for analog.

DL: Right, right. It took them a long time to learn digital. M.I.T. was also analog minded in those days.

UM: Yes.

DL: But that's how Archibald got into it. He's been in it all of his life, really, in a sense of tables and methods of computing.

UM: This analog business is ... you know I have found that in many instances, well the Bureau is an exception, but looking not only at M.I.T. but also the Moore School of Electrical Engineering at Pennsylvania and places such as Cambridge, the places that were associated with the early development of the electronic digital computer were analog headquarters up to that time.

DL: Oh sure, yes. Well, the electric analog was started in Germany and England, and MIT too, I guess. The electric analog certainly paved the way for interest in electronic circuitry. Even the last analog computers had some kind of pulse circuitry in them too. And we have these digital differential analyzers which were imitations of the analog and which tried to get the reliability of the pulse circuitry into the picture, trying to escape the amplitude faithful circuitry which was so unreliable, and so unpredictable.

Well, they're still talking about hybrid computers and building them for special purposes for industry. My son just started a new job in this field. I don't know how it will come out.

UM: You said that this machine you were working on before Aberdeen was essentially a hybrid machine?

DL: This was what?

UM: You used the term hybrid, I think, in connection with the ...

DL: Oh, yes. This bombing analyzer was a combination of the digital and the analog device. It gave not only intensity but the number of successes in the experiment. You had some overall success of measurement which was analog which was filling a large condenser, and another one which was counting hits above a certain tolerance, so that you had two measures of the success of the mission.

UM: Is that written up?

DL: No, that was classified. I don't know whether it's still classified; probably it is, but I don't know. (Laughter) No, I imagine it's been declassified.

UM: Well, you can't tell.

DL: I demonstrated it in Washington one time at the Pentagon, which was the only time I ever went to the Pentagon. I don't think you'd strictly call it hybrid; I think you'd say it was two machines in one, but there wasn't this interplay that you get in a hybrid machine.

UM: You say that was supported by the Carnegie ...

DL: No, no, that wasn't. This thing was Army Ordnance, I guess.

UM: Oh, well, I was confused when I asked you earlier, because that's what I was referring to. You were talking about the sieve, I guess.

DL: Oh, yes, that's right; yes. The Carnegie people funded the sieve, yes.

UM: Right. I was a little confused as to why Carnegie would have ...

DL: Yes, right.

UM: That was Army Ordnance.

DL: Yes. It may not have been Ordnance; it might have been the Air Force, I don't know. We called it War Department in those days.

Just a moment; can I see you sometime this afternoon, maybe?

(BREAK)

DL: ... mechanical part that would fit into about two squares of the file there, and then there's the electronic part which consists of three fair sized boxes consisting of amplifiers and so on.

UM: Fair size being?

DL: Well, about the end of this table and about this high. It's old-fashioned, of course. It's not done on cards and printed circuitry or anything; it had to be very carefully screened against outside noises and so on. It had terrific pickup for radio and so on. So it makes quite a display; I mean, it amounts to quite a bit of floor space.

UM: Well, basically, it is the kind of thing that I think we should have. Of course there's the question, if there would be some way. Now, we wouldn't be able to put it on any kind of permanent display, anyway, but the question is, could we at sometime put it back together to demonstrate?

DL: Oh yes, it's not difficult to assemble. The question is, if you really want to operate it, whether you want to use this old electronic equipment. Nowadays, you could get a very small, powerful amplifier which would do the job as far as running the machine is concerned, but you can't even buy the old tubes that are in there now, you know, so, it's sort of hopeless. You can run it mechanically. That is to say, rotate the gears and that sort of thing; that's all easy to do, and it has an optical system and parts of that are missing now. I used parts of it for some other optical devices. So, the optics could be fixed easily, especially with the terrific shop you have there at the Smithsonian. It was done very informally. There was nothing fancy about it. I did my own machining on it myself, and you can see how crude it was. But it wasn't all that crude. It ran nicely at 360,000 a minute. So, it's only recently you've got sieves that are running faster than that. I have one running over in Cory Hall at a million a second. A million a second! There's your factor again. And we're looking for another factor of sixteen there next year.

UM: I was going through the library and I noticed there quite a few theses that have been done on construction of sieves.

DL: Yes.

UM: Would that be something worthwhile as far as this development? Would it be worth looking at?

DL: Well, I suppose it could, yes. That might be a good idea. A lot of these were steps toward Parnassus, of course, and some of these were steps in the wrong direction even, but that's the way we get our work done here. We use master's theses for slave labor, you see.

UM: Yes. I just went through the catalogues and I haven't actually looked at the material, but I wanted to check with you whether you thought it was worthwhile.

DL: Oh, yes, some of those theses are quite interesting, and some of them are more or less routine; the early master's theses, I think.

UM: Well, I think I've taken enough of your time.

END OF INTERVIEW

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