An Interview with

ROBERT M. FANO

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Conducted by Arthur L. Norberg

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Robert M. Fano Interview
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Abstract

Fano discusses his move to computer science from information theory and his interaction with the Advanced Research Projects Agency (ARPA). Topics include: computing research at the Massachusetts Institute of Technology (MIT); the work of J. C. R. Licklider at the Information Processing Techniques Office of ARPA; time-sharing and computer networking research; Project MAC; computer science education; CTSS development; System Development Corporation (SDC); the development of ARPANET; and a comparison of ARPA, National Science Foundation, and Office of Naval Research computer science funding.

ROBERT M. FANO INTERVIEW

DATE: 20 April 1989 INTERVIEWER: Arthur L. Norberg

LOCATION: Cambridge, MA

NORBERG: Can you review your professional activities in electrical engineering in the pre-MAC period, let's say 1955 through 1962 or so, when you were at the RLE?

FANO: Yes. My activities had nothing to do with computers. Basically my activities at the research level were concerned with information theory. Now, I do not know how much you want me to go into this.

NORBERG: Go ahead. I have read a couple of the publications, but I would like to hear some more from you about what you were doing.

FANO: Yes, I started out in information theory way back. Essentially I started working in that area immediately after I got my Ph.D., which actually was an ScD, in June 1947. So I really started thinking about information theory right after that, in the summer and the fall of 1947. There is a bit, just a bit, of history of my relation with people in the preface to my book on information theory, and I will tell you more about it.

NORBERG: This would be 1961. [*Transmission of Information* (MIT Press)].

FANO: That is the preface and that is essentially something about my early relation with Wiener and Shannon.

NORBERG: Yes. Did that relationship continue with Shannon after, say, 1950, 1955?

FANO: Oh, yes. Perhaps I had better tell you how I got involved in information, and about all those who were there.

NORBERG: Please do.

FANO: Norbert Wiener was a peripatetic. He walked around the Institute and just butted into the offices, said a few words, then wandered out, and got into another office, and so forth and so on. So he started wandering into my office. He came in and said, "You know, information is entropy." But he never explained why. So I started thinking, why does he say that it is entropy? I cooked up my own answer to that, and the answer came, I think, in March of 1948 while walking to the station to go to the IEEE meeting. IEEE at that time was IRE and was meeting in New York. Having tossed his idea around in my head, I finally saw the light in the form of encoding, which would then result in the entropy expression. While I was on the train I worked things out. At the IRE meeting, Claude Shannon was presenting a paper. I did not know him. I knew of him, but I had never met him before. After the paper I went to see him, and I asked whether I could go to see him at the lab. He was obviously very bored with the idea, but he was a very kind person so he said yes. So the next day I trotted over to Murray Hill, and I walked into his office, and he still had the bored expression. Well, I told him what I had done, the idea, and his face lit up, because I had essentially reproduced some of his work in a somewhat different way. I had arrived at what is called the first Shannon theory on noiseless coding. So that is how I got started. I could not tell you about the work in all its glorious detail except that by 1950, I started teaching a graduate subject on information theory, and one of the students was named Dave Huffman, who wrote a term paper. I had given a number of possible topics. One of them was that while I developed the form of encoding, that did not assure that the coding would be optimum. Shannon, who at that time was at Bell Laboratories, was not sure. So I raised the question. I said, "It would be nice to know an optimum way of encoding." All of which Huffman developed as a term paper that he published, of course. I think at that time, either that or the next term, I also wrote a first set of class notes. I taught that course at least once a year until 1962. That was my graduate subject. And I kept in touch with Shannon. I was interested in inviting him to become a visiting professor here [MIT], and then persuaded him to stay here. In 1961 there was kind of an end for me in various things. I finally got around to writing that book [on information theory]. At the same time, I was developing a course in electro-magnetic theory for undergraduates. The book came out about the same time as this in 1961. So I decided to take a sabbatical in 1962. Not only that, I also decided that I wanted to change fields -- I had done that long enough. I thought about getting into computing. I took a sabbatical at Lincoln Laboratory, thinking that while I was there I

would learn about computers. It was a very good group there.

NORBERG: Who was there at the time?

FANO: Well, it was the time when the TX-2 was done, and Wes Clark, the designer of the TX-2 was there.

NORBERG: It will come to you. Go ahead.

FANO: There were several people. This was the advanced design group. Then, of course, there was the whole operation about the air defense system, although that already, I believe, had moved to Mitre. I believe that Forrester was already on campus by then. There was quite a lot of activity there, particularly in application.

NORBERG: So what sort of project did you work on while your were at Lincoln during that year?

FANO: When I was at Lincoln Laboratory, actually I didn't get involved in computers, because I started thinking about the coding technique and decoding technique. A student of mine, Jack Wozencraft, had developed sequential decoding. Eventually, I came up with a much better algorithm for decoding, which was tested on the computer. Actually, I wrote a paper at that time, which I had already been committed to; it was more like an invited paper, and I stuck this algorithm at the end, because it fit in very well. I said that I eventually would write a paper describing the mathematics proving it, but I never did. I never did, and the reason was that by the fall -- I had written the paper basically during the summer -- when I came back, all the events that led to Project MAC got going, and so I knew I had that...

NORBERG: And so we are talking about the fall of 1962.

FANO: Yes.

NORBERG: And prior to that, you had no association with computing on this campus.

FANO: Really, no. There was some point, probably around 1960, when Gordon Brown, who was...

NORBERG: Dean of Engineering at the time?

FANO: No, he was still here at the department. He essentially volunteered a few of his senior faculty to take a special course that was taught by Corbato and McCarthy. So I learned a bit about things. I wrote a program that worked.

NORBERG: Why do you think Gordon Brown wanted you people to do this?

FANO: Oh, because he thought the computer was going to be very important. So he wanted the senior faculty to get familiar with the use of the computer.

NORBERG: Do you think this had anything to do with your subsequent decision to enter the computing field to get away from the information theory?

FANO: No, I think something else had more to do with it. I forgot about that... I am trying to think of the date now. It was around 1960. Let me see if I can remember the exact date. There was a great deal of concern on the part of the administration about computer facilities at MIT. So they appointed a committee to make a recommendation consisting of Al Hill (I think he was chairman), Phil Morse, and myself. Various records also show Jerry Wiesner was a member. But I do not recall -- he probably left for Washington. But he did not really play a part. We promptly appointed a technical committee, which consisted of the various computer types at MIT: Corby, and John McCarthy, and Minsky, and Doug Ross, and Jack Dennis were there. Herb Teager was chairman. Now by that time the idea of

time-sharing was already moving, and they proposed the development at MIT of a time-sharing system.

NORBERG: They, meaning the technical committee.

FANO: Yes. At that time they were thinking in terms of using a special computer or whatever. Now there was some argument in the committee. I really do not know the details. Herb wanted to present his report and it was that thick [indicates several inches]. Eventually the committee said, "No." The report that eventually came out was essentially chaired by John McCarthy. Now, that was when I first learned about time-sharing really. I also got to know Corby. There was a little more connection there, because when John McCarthy and Marvin Minsky were getting going as assistant professors on the faculty, they were jointly sponsored by RLE and the Computation Center. And in RLE I was kind of in the role of senior faculty talking to them. The fact was that my field was the closest thing to computation in which there were any senior faculty, with the exception of Phil Morse. That was the significance. I was by then a full professor. I became a full professor in 1956. Besides being a full professor, I was playing a significant role in the department. I was a member of Gordon Brown's kitchen cabinet, as he called it. So I was the senior member of the faculty who had some kind of connection, as feeble as it was, to computation. Information theory was still the best and had some of the same ideas, and so forth and so on. So also there was... what was it? 1959 or 1960... when John McCarthy spoke in the... Do I have it here, or...? The book edited by Greenberger.

NORBERG: Yes, as I remember, the presentation was given in the spring of 1962.

FANO: That is right, yes. I was involved there, too, as a commentator on one of the papers. I certainly was very much impressed by John McCarthy's view of computing. He introduced that terminology there.

NORBERG: Can we go back to the committee for a minute? Once the technical subcommittee had presented their proposal to the committee...

FANO: We supported it.

NORBERG: You people supported it. ... what was the reaction of the administration to the proposals being made?

FANO: It was certainly favorable.

NORBERG: Why do you say "certainly"?

FANO: You see, by the time that that report came out, before the administration had a chance to do much about it, so to speak, the idea of Project MAC was born.

NORBERG: Yes. Well, there was about two years in between though.

FANO: No, not really. Let me see if I can get you the dates.

NORBERG: Okay. [Pause] We can actually look up the dates at a more efficient time.

FANO: Yes, I should have prepared myself in this but I did not. I do not see that I have a copy here. You see, that report was part of the first proposal for Project MAC.

NORBERG: It certainly became part of the proposal, yes.

FANO: I made it a part. At the moment I do not remember the date of that report to steer me to remember.

NORBERG: That is all right. I am sorry I did not bring it with me either.

[INTERRUPTION]

NORBERG: Okay, in 1960 a long-range computation study group report dated April, 1961.

FANO: So you see, there is not that much time in between.

NORBERG: Yes, right.

FANO: The administration was certainly favorably inclined, particularly with Morse who supported that. But at that point it was a question of a lot of money.

NORBERG: Right. That is what I was leading to. Where would the money have come from?

FANO: So that they were not moving very fast on that. So that was April 1961. I went on sabbatical in the summer of 1961.

NORBERG: 1961. Okay, so you were gone during...

FANO: Yes, that is right. So now another facet of the matter... Let's see, in RLE there was something called the Center for Communication Science that Jerry Wiesner had started. But he never really did anything, because Jerry went to Washington. So it was kind of a paper entity. Just before I was appointed to this computer committee, I was also chairman of the committee for the Center for Communications Science. I was basically trying to find a focus and to get people excited about something. Instead I found that the various groups that had formed RLE in the early days had kind of settled and the interdisciplinary fervor had disappeared. Everybody had found his own niche. They were just not interested in any interdisciplinary activity, which is what this new center was supposed to push. So I was rather frustrated about that.

NORBERG: Yes. That also helps to explain the shift.

FANO: No, my memory comes in bits and pieces. So these were all sorts of things that were going on simultaneously. Also, Lincoln Laboratory had donated, or loaned -- of course, it could not donate -- to the department the TX-0 computer. Then DEC donated a PDP-1 computer. I do not know whether it was model serial number one or two. So there was a committee in the department, of which I was a member again, trying to set up policies...

NORBERG: This is in electrical engineering?

FANO: In the Electrical Engineering Department. So I had all these connections. I suppose there was something else that was significant that in my work in communication theory. I had become convinced that one ought to start thinking about communications no longer in the form of, "How can I put together certain communication components?", like an amplifier, or oscillator, to make a communications system. That was the primitive thinking in communications. Then why not start thinking in terms of more general function, since computers were here, and any function that was physically realizable at the logical level could be programmed into a computer. I remember, I gave a talk in which I expressed that view very strongly. I think it was at some place in New Jersey. One of my motivations for moving into the computer field was a natural development from my work in communications, because one ought to think in terms of a function that could be eventually implemented with a computer. So it was kind of a mixture of things that happened in my life. But, you see, I came back from Lincoln Laboratory, where I spent my sabbatical in the fall of 1962, planning to get busy and learn about computers. Then things started to happen. What was happening was that Lick -- Licklider was by then in Washington -- came around talking about starting a big laboratory on computers. You know, kind of pushing. He obviously very much wanted a big computer laboratory to get started at MIT. You realize that Lick was on our faculty, of course.

NORBERG: Yes. How much association had you had with him before this?

FANO: Very close, yes, he was in RLE. I knew him very well. As a matter of fact, he was responsible for my writing a paper. Once at the faculty club, I remember very well, he asked me what seemed like a little question. The question had to do with the relation between autocorrelation functions and power spectra. When you had only a sample of the time function to use, you know, a finite sample, to compute the autocorrelation function, and you measure the spectrum with a filter of finite bandwidth. I said, "Well, you should get approximately the Fourier transform of the other," but that started me thinking. So I went home and started fiddling (?) around, and eventually I wrote a paper in the *Journal of Acoustical Society*[1], because it turns out that you had a time independent relation between the two -- the finite length time function and the power spectrum measured it with a finite filter. But only when you have a time independent relation between the short time autocorrelation function and a short time power spectrum. But only when the correlation was measured with an exponential weight function, and the spectrum was measured with a tuned circuit, did you have a time-independent relation between the two. This question from Licklider spurred me to develop that.

NORBERG: Were you aware of what he was doing out at BBN in timesharing in 1961?

FANO: Oh, yes. I was aware. You see, the history of time-sharing dates back to January 1, 1959, with a memorandum by John McCarthy to Phil Morse. Now what happened at MIT was that, again, Herb Teager started working on that.

TAPE 1/SIDE 2

NORBERG: Teager is kind of peculiar? You mean in his approach to problems...

[1]"Short-Time Autocorrelation Functions and Power Spectra." *Journal of the Acoustic Society of America*, 22 (1950): 546-550.

FANO: Well, his approach was more to people, to working. He had a lot of very good ideas but he could never carry them to completion, because he was unable to work with other people. Most of his ideas required some sort of collaboration. He had a fixed view of things. So at a certain point Corby jumped in, who was by that time an associate director or a deputy director of the Computation Center, because he did not think that Herb Teager would get anywhere. He picked up the ball and ran separately on his own. You see, Herb Teager was concentrating on input/output equipment. Corby's view was that IBM has got a kludge of a communications unit. Why not use it as a kludge, and work on the central operating system, which is what he did. So I was familiar with all of this. But I was not a computer person. I knew really very, very, very little about computers. I was essentially living on the fringe, and having friends that knew a lot about computers, and I needed help. And this is what I intended to do in the fall of 1962. But you see, Licklider came around and I got to feel that, my God, MIT should do something very important for the future; very important, therefore, for MIT.

NORBERG: You thought that MIT should do something in computing.

FANO: Should do something, but I could see that nothing was happening.

NORBERG: Why not?

FANO: The reason was Phil Morse was the only senior faculty with any involvement in computers. He had his hands full. He had his fingers in so many pies that he clearly said he did not want to put his finger in another pie. People like Corby, I think by then he was an associate professor, had just been appointed. Marvin Minsky was also an associate professor, possibly had just been appointed, or was still an assistant professor. John McCarthy had left for Stanford. He was impatient. Things were not moving at MIT and when they offered him a position at Stanford he went. There was nobody that was in a position to organize a big project with the trust of the MIT administration. I looked around, so to speak. It was clear that of all the people that the MIT administration would be willing to

consider, I was probably the only one that had an interest and was close enough.

NORBERG: Was there some discussion about that point between you and the administration, or not?

FANO: No, there was none. The thing moved very fast. I knew Licklider's goal. You know, I knew Licklider pretty well. When he came around, I talked with him. As a matter a fact, once when he was around I went with him to see the time-sharing system that was already in operation in an elementary fashion. So I saw the people sitting at the typewriter, working. Well, I had this thought. You know, I felt bad that nothing was happening.

NORBERG: Yes. Can I get something clear, Bob, if you do not mind? Did Licklider come to MIT from Washington and go around to people's offices to talk to them -- people like you, or did he try to accumulate a group to discuss this in sort of committee-like fashion?

FANO: No, not in a committee-like fashion. He kind of went around. He was at home at MIT, you see, so he was not...

NORBERG: Certainly no stranger.

FANO: He was friends with all sorts of people. So he just came to visit and see what was going on. I do not think that it was a completely organized thing, or anything like that. He even worked with the administration. He was just trying to get people interested.

[INTERRUPTION]

I think the critical time was just before Thanksgiving, and I mean just before Thanksgiving, of 1962. There was a meeting at the Old Homestead in Virginia organized for the Air Force by the people at Mitre on communication and

computers, and all of that. I went there to chair a session on communication. I mean, I was asked to organize a session on communication that I chaired. So I attended some other sessions; particularly some sessions on command and control. I came out with the feeling that everything was a mess, which was Licklider's point.

NORBERG: In what way was it a mess?

FANO: Licklider's point was that those things ought to be done with a time-sharing system. I agreed. In other words, they were trying to use batch processing to handle those kinds of operational things, and that was just nonsense. Basically that was the idea. So, you know where the Old Homestead is?

NORBERG: Yes.

FANO: Yes, it is a long, long ride on the train to Washington, DC. Well, when the meeting was over, this was the day before Thanksgiving, Licklider and I took the train to Washington. So we talked and talked and talked about the situation, what needed to be done, and the man-machine interaction. He had written this paper on man-machine symbiosis by then. I was familiar with what he was doing at BBN, and incidentally we stopped talking about that. You see, John McCarthy was a consultant at BBN, and he got them started on a time-sharing system there too. And Licklider, at that time, was on a library project at BBN. He started using comp uters as part of that project. He got enamored with computers. He got a PDP-1; I remember seeing it. So, I came back to Boston...

NORBERG: Can we finish the train ride, though? Did the two of you cook up a scheme?

FANO: No, we just talked and talked and talked. We did not cook up anything yet. The next day was Thanksgiving, and I kept turning, turning, turning over these ideas in my head. At a certain point I said, "Well if nobody else is going to do it, I am going to do it." What got into me I do not know, because I always in the past shied away from administrative responsibility. I had been one in the beginning of Lincoln Laboratory: I was the head of a group. But

aside from that, I was not interested in these things. I realized later that I was willing to do it when it was a matter of starting something new. But still, I do not know what got into me on Thanksgiving Day when I said, "I ought to do it." Well, the next morning I had an appointment with the provost, Charlie Townes. The appointment had to do with the Committee for the Center of Communication Science. I wanted to report the situation to him. So I said, "Look, I had this idea yesterday of starting what Licklider wants. Do you want to think about it and let me know whether you think it makes sense?" And he thought a moment and said, "No, that's all right. Go ahead." He just said that, just, "Go ahead."

NORBERG: [laugh] What did you propose to him though, that he would so quickly respond?

FANO: It was the sort of thing that Licklider wanted, namely a computer science focus on time-sharing. Well, that was Friday. During the weekend I wrote a two-page memo outlining what I had in mind, which probably you have a copy of in those documents, because I wanted a tie with education because I always thought education was the primary goal of MIT. So I wrote this and I distributed it to key people, like my department head, who was Peter Elias at that time, and then to Gordon Brown, who was Dean of Engineering, and of course, Charlie Townes, and Stratton. I do not know how many more. Phil Morse for sure, and I think Al Hill. I do not remember who else. I found out later that Gordon Brown told his secretary to file it under "FF", meaning "Fano's Folly". [laugh] Well anyway, on Tuesday I met with Jay Stratton and basically he asked me only one question, "Where are you going to do it?" Now, as Phil Morse later said, "MIT was caught with its buildings down." But I had already heard a rumor, which turned out to be true, that the top two floors of this building [545 Technology Square] that had been leased by CEIR might be available. CEIR had financial problems. It had been planning to put a STRETCH computer up on the ninth floor here and had rented the eighth and ninth floors. Eighth for office space. CEIR was looking for somebody to take over. So I mentioned to Stratton that I had heard that, and I was going to look at it. In fact, I looked into it and it turned out to be true. I looked into other possible spaces, and there was nothing close to this. This was practically ideal, because we were going to have a big computing installation and the ninth floor was already planned for that purpose. Well, anyway, that was a Tuesday, and on Thursday Licklider came to town, for whatever reason. So we all gathered in Stratton's office -- he, I and Phil Morse -- and we shook hands. That was the decision -- in one week --

Thursday to Thursday. The decision was more than in principle. All the parties concerned decided that yes, we are going to do it. Now of course, there was a lot of work still to do. We had to present the proposal, so I got the committee together, the computer guys who were about to be parties to the proposal. They each wrote a little piece. I wrote the introduction. And the proposal was presented, I believe, in the very beginning of 1963. It was dated in the early days of 1963. It was Licklider's idea to have a summer study. The reason was that he really wanted to create a national community interest in this. The summer study would be the right place for people to get to know each other. Now, I should say at this point that Licklider's idea was to create what he called a Center of Excellence. He and I had a very similar image, which was the image of RLE. I think that, as I told you on some other occasion, RLE was a very, very important development on the national scene in the sense that it not only produced very good research, but also created a lot of very good people. It was very heavily involved in education, although this was not its official function. It was a research lab. But the sponsor of the three services, had very much in mind that the laboratory would be breeding. You know, the place where you breed very good genius. So we both had this in mind. The original plan of funding was the key to that. Basically, Licklider had in mind three million dollars a year with a commitment for three years renewed every year, so we would always be three years ahead. Now, the first year, he did not have enough money to provide the full funding of three million, so it was 2.6, I do not know, I do not remember exactly what it was. Well, that was what he had in mind. This notion of several centers of excellence around the United States was approved at the very top. You know, the top of DOD. I know that Gene Fubini, who was then assistant secretary for science and technology, whatever it was, was very much behind it. So, of course, was Ruina, who was director of ARPA at the time. So that was where we stood. I think the image of RLE was very, very much in our minds.

NORBERG: Now, did you have to encourage anybody to come along with this project, or did they immediately see the value of it?

FANO: Now, this is the game that I set up. People at MIT, of any significance, were part of research laboratories of one sort or another. RLE, the Electronic System Laboratory, the Computation Center, and there were people in other

offices. Now, the attitude at MIT was that you would be a member of a department, of course, and a member of a laboratory. But you would not be a member of two different laboratories. There was a loyalty to one laboratory that was assumed. So if I had created a laboratory, or even the name laboratory, it would have prevented people from participating, because if they had to leave the laboratory where they were working in order to participate, probably they would not have done it. That was why it was called a project. The ideal project was a collaboratory effort of individuals and laboratories. You see, that was really critical. With the notion of a project I had no trouble. As a matter of fact, more people were interested in participating then because working with us was a source of money.

NORBERG: Did this idea of a project develop only after you ran into some difficulties?

FANO: No.

NORBERG: Or did you understand that beforehand?

FANO: I understood that. I knew the climate. I knew the attitudes of people. You see, the situation at MIT was that there were quite a few people interested in computing, but they were scattered all over the place. So what I tried to do was provide a focus and money and an organization, so that they could pull together. And this soon happened. This was clear from the very beginning. It had to be organized as a collaboratory effort. You could not pull people out from where they were.

NORBERG: Now, had all of these people been funded on other projects, say from the Air Force...?

FANO: Oh yes. They were all funded in all sorts of different ways. There was the work in the Computation Center. There was NSF, and ONR; the work in the Electronic System Laboratory... Let me see, I think it was the Air Force. It was a continuation of the APT project. It was called Computer Aided Design. Lincoln, you know.

NORBERG: Why was new money needed then, if that was the case, if there was such support available already?

FANO: Well, computational facilities were scarce. That was why that committee was set up. We were very limited and people wanted more computer time. Furthermore, of course, although they were supported they always had an idea of doing more if they could get the money. From an MIT standpoint and the computer center standpoint, this was the opportunity to really push time-sharing and experiment with it and improve it out of the constraint of supplying the traditional batch processing service to the MIT community. So everybody had some stake in it.

NORBERG: How did you decide who got money and who did not?

FANO: I suppose in the end I just dickered with people. I made the decision. It was not really a question of who got it and who did not, because, you know, people with interesting ideas were not so many. It was more a question of how much, and what will they produce, in my evaluating the ideas. Initially I was rather free with money, and then I tightened in some cases where I did not think they were getting anywhere. I remember one of the goals of the Air Force was to get the whole community involved in time-sharing. So most of the money that was supplied, let me say, outside the core computer science effort. You know, there was a user community, and a community of computer types -- regardless of where they were physically. Now the user community was very important. It was important in my mind, and it was important in Licklider's mind to get people hooked on time-sharing, man-machine interaction. So the biggest contribution to the community was in the form of computer time, which initially was free. There were lots and lots of people. As a matter of fact, it not only was free but we were pushing it on people, because we wanted to get them involved.

NORBERG: Did anyone refuse to participate that you remember?

FANO: There are some funny stories about that. Like the story of... Well, maybe to preface this, there were lots of people at that time that thought that time-sharing was for the birds. The general attitude was, you know, when you

compute something then you have got to look at the result and think before you do anything else. So this man-machine interaction was a waste of computer time. As a matter of fact, that went to an extreme. Hamming at Bell Laboratory said to me something that I cannot quote perfectly, but he said it was criminal for anybody to program on-line. You should write your program, try it, and then debug it, think about it, and then go back on-line. It was a waste of a sacred resource. That was the attitude primarily in the field. That attitude was largely shared by Jay Forrester -- it may surprise you to learn that. Let me tell you what happened. One day when Project MAC was already going... it was already in operation, although I cannot say how much after the beginning it was. If I look in the record I can find out. Jay Forrester called me and said, "I have got so and so coming here tomorrow and he is interested in visiting Project MAC. Could you show him around?" "Unfortunately," I said, "I will not be here tomorrow. Dick Mills, my assistant director, will be glad to do so." Then I said, "Why don't you come along too? You may be interested to see it too." He said, "Yes, maybe I will come along." Well, it was a little bit of a conspiracy, because his chief programmer Pugh -- was it Pugh? Yes. -- who had developed the language for industrial dynamics, and the name of the other, right now I do not recall, was well aware of time-sharing, and he would have loved to use time-sharing, but you know, Jay Forrester has his way.

NORBERG: I see.

FANO: He had already developed a compiler for a time-sharing system. So he could run a compiler on the timesharing system and see what resulted. You see, and Dick Mills knew that, because they knew each other quite well. So when Forrester came here with a visitor, Dick Mills demonstrated this whole affair. I was away that...

TAPE 2/SIDE 1

NORBERG: You were saying you were in the office at 9 the next morning.

FANO: That is right. As soon as I sat down at my desk the telephone rang. It was Jay Forrester, "Can I get

computer time and, by the way, can I get it at a terminal at home?" (laugh) I said, "Sure."

NORBERG: Just like that.

FANO: Just like that. I'll tell you where we failed. We put a terminal in Claude Shannon's home, and he really never used it, although his wife, Betty, did. I just could not get him to play around with time-sharing. We had a lot of terminals in homes, which was a decision that I made, and it turned out to be the right decision -- but a lot of people found it was not right.

NORBERG: Was not right?

FANO: Yes. But you know, it was in peoples' homes. Look, you have got to bring yourself to that time. The reason why I did it was because I was very well aware of how hard people worked. They were willing to stay here even in the evening for the purpose of using the time-sharing system. That inhibited their family life. So I said, "My God, it's cheap." I only need the terminal, probably, the telephone line -- a private line to people's homes.

NORBERG: Were the lines effective as a communication device, though?

FANO: What?

NORBERG: Were the lines effective as communication channels at the time?

FANO: Oh yes, fine. Oh, of course, you had some errors, but you could still use them. So we had a large number of terminals, all the system could then have. Plus there were a lot of other people. It was quite effective.

NORBERG: Let me ask you one more question, and this will prepare us for tomorrow as well. Did Licklider come

around very much during the four or five months at the beginning of 1963 to see how things were going?

FANO: You mean before Project MAC had been fully set up?

NORBERG: No, while you were organizing Project MAC, from the time the proposal was submitted in January of 1963 to the time the summer study began.

FANO: Not very much. No, of course he was around there, but not very much. He was not breathing down my neck at all. He had the proposal. We organized the summer study. I have to say something else here. The major problem in getting the contract worked out was that MIT was part owner of this building. So the lawyers had a field day in all issues of a legal nature about the ownership -- statements of facts and interests, and so forth, you know. Everything had to be put on the table with the result that the contract was finally signed the last day of June. It was a lot of work. Of course, the other thing was that this was clear space. It was open, not a partition or anything. So I had to get an architect and fight with him. I am puritanical. When I asked the architect how much something would cost, he would say, "Oh, there is not much difference between this and that." So I took him at his word, and the plan went way, way over what he had estimated. I hit the ceiling. I had him go over the plan and tell me specifically what this cost and what that cost.

NORBERG: So that provided an additional delay, I assume.

FANO: No, not an additional delay, really, because I got him to move. I was here and there all the time. I was not going to throw away government money. I am that way. So that was all done. But there was a lot of work to be done, just plain getting ready, and organizing the summer study. Oliver Selfridge, who had then become associate director, helped me a great deal in organizing the summer study. That went very well.

NORBERG: That is where I would like to pick up tomorrow, with the summer study itself. But I have one more

question about Licklider. I have seen in the Project MAC memorandum that Licklider wrote a nine-page summary of what he thought the time-sharing project was in April of 1963. Do you recall this at all? It's downstairs where we can look at it tomorrow if you like.

FANO: For ARPA, you mean?

NORBERG: For consideration at a meeting which was to occur here in early May of 1963.

FANO: I do not recall that at all.

NORBERG: It is Memorandum 23. If you could look at it on the way in in the morning it would be interesting to talk about it.

FANO: Memorandum 23?

NORBERG: Yes.

FANO: Published by whom?

NORBERG: Project MAC.

DATE: 21 April 1989

NORBERG: All right, we just looked at that memorandum downstairs in the library. Do you remember it now that you have looked at it?

FANO: I remember the content now. I do not remember the specific memorandum that clearly was distributed during the summer study.

NORBERG: How did the summer study list of people come about?

FANO: It was suggested by a lot of people. There was a technical committee. Now I cannot quote the membership for sure. They were generally the people at MIT in the computer field, like Doug Ross, Jack Dennis, and Corby, and so on. Then of course, Phil Morse is the one, for instance, that brought Maurice Wilkes here for the summer study. He knew Wilkes quite well. So he invited him. As a matter of fact, Wilkes was here before the summer study on a visit for some reason. I met him then and invited him. And he came. He was quite interesting, because he started out being very skeptical about time-sharing, like Hamming and most other oldtimers. He completely switched sides during the summer, and became one of the strong protagonists of time-sharing. He has visited here many, many times since. As a matter of fact, recently he was in Cambridge for two or three years. Yes, he was visiting Harvard. He was also an adjunct professor here.

NORBERG: How did the summer study work? What went on during the day?

FANO: It was very informal. As you saw, people wrote memoranda to each other and presented orally. Quite often we had some kind of meeting. I remember going around the corridor with a cow bell (laugh) to call everyone to come. There was not any specific focus and tangible output. Really, what took place was two things. People got to know each other and form a community. That was a major result. The second is that we had two time-sharing systems available to the participants. One was the SDC time-sharing system.

NORBERG: SDC?

FANO: System Development Corporation.

NORBERG: Oh, I see. Out in California.

FANO: Yes, it was coming in through the telephone line.

NORBERG: I see.

FANO: The other was the CTSS, the computation center's system for awhile. Mac did not have its own installation yet. So the people had a chance to try those, compare them. There was one thing that became immediately obvious, that the CTSS was much more useful to people for one reason. It had a disk file, which had just been installed, while the SDC system worked on tapes. So CTSS was relatively fast for any user trying to get hold of a file or using any material in the system, while SDC took quite awhile, because they had to search the tape. This certainly made it clear to me that there were two technologies that were essential to having a useful time-sharing system. One was a disk file, and the IBM 1301 had just arrived. And the other was the transistor. The vacuum tube machines were not up long enough. So those were two clearly critical technologies that helped time-sharing physically.

NORBERG: What do you think the summer school accomplished?

FANO: Well, I would say, the first was getting people together, exploring time-sharing. I think basically those were the two things accomplished. Originally a report was supposed to come out. When I started thinking about it I did not have a thought about what had... It just did not make sense to make a report of the study.

NORBERG: Why not?

FANO: Because there was no recommendation that came out of the study. It was a different thing. I tried to get the group focusing on problem solving and making recommendations. You will see the memoranda had bits and pieces.

But really, it was not the thing that people intended to do, and in a sense there was nothing much that could be done. So instead, I wrote that status report, which you have, as a substitute for it. That was a mimeographed thing.

NORBERG: Was there much interaction with company people during either the summer study or immediately after?

FANO: There were a lot of company people in the summer study, who were specifically invited. Yes, if you look at the list you will see a lot of company people, including IBM and DEC, Xerox, General Electric. Joe Weizenbaum was from General Electric. Ted Glazer was still at Burroughs. There were government agency people of various kinds. The Lincoln Laboratory people. During the summer study, Dave Evans was maybe still at... I don't know, whether he was at Berkeley yet. I think he moved to Berkeley shortly thereafter. He may still have been at Bendix. There were quite a few.

NORBERG: After the summer study then, did the work just continue on on the same problems, or was there some reorganization?

FANO: Well, the summer study was mostly to launch the ARPA program. That is what Licklider wanted. It served a purpose in the sense of getting the various people that were involved or thinking of getting involved with the ARPA program together. John McCarthy was here from Stanford, and Carnegie people were here. Perlis was here. Who is who in the ARPA program was here. That was really what came out of it. Certainly the people did get together and got to know each other better, if they already knew each other, and started a general interaction. When you see, and when you read that memo from Lick...

NORBERG: Number 23.

FANO: Yes, there is a lot of that flavor of getting a community together, and at the same time, for most of them, it was the first chance to try a time-sharing system. Lincoln Laboratory, you know, was involved.

NORBERG: What was your interaction with DARPA after the summer study? Do you remember observing the activities inside the IPTO office, especially after Licklider left?

FANO: Well, that was quite a bit later.

NORBERG: In 1965.

FANO: Well, I can not say that I was in the office much. I could see the result of their policies.

NORBERG: How about procedures, though, in getting more money from them?

FANO: Okay, this is part of a bigger issue. I pointed out yesterday that when Project MAC got started, Licklider and I shared the vision of RLE as a model, besides other things. What was different was there was no laboratory; there was a project that was supposed to involve the whole MIT community as users in this. But we shared that model, which included a great deal of emphasis on education. Well, when Lick left... well, I do not know whether he had just left... the attitude in the Pentagon changed slowly, moved away, while up to that time, since World War II, it was generally acknowledged all over the place that the university research had two purposes. One was to do research of interest to the military. The other was to generate competent manpower of a high level, highly educated manpower. So there was this emphasis on education. I do not know whether this was the result of the Mansfield Amendment, or was something else, but by the time that Larry Roberts was the director, I remember distinctly -- I was still director for MAC -- that he was here and I outlined for him what my plans were for research and education, because I considered that as the main thing by far. So I decided to turn to education. He cut me off abruptly. He said, "ARPA is not interested in education."

NORBERG: This was Roberts.

FANO: Larry Roberts, yes. Now, I really do not know whether that was his view, whether it was the result of the Mansfield Amendment, or...

NORBERG: How did that show up then, Bob, in terms of money?

FANO: Oh, it did show up, because all through my tenure as director, the support from ARPA was for Project MAC as a whole. Sure, we presented the proposal, but we did not dicker about which project we were in. It was an overall support. The only difference was that, for some reason that I never understood, artificial intelligence was funded under a separate contract. It still was Project MAC.

NORBERG: There seemed to be two parts actually. There was a contract for activities across the campus...

FANO: Project MAC.

NORBERG: ... and then there was some in Project MAC.

FANO: No. It was the general contract for Project MAC, and then there was a separate contract for the artificial intelligence. Somebody someplace was worried, quite incorrectly, that I would use the artificial intelligence money for something else.

NORBERG: This was people in the Institute, or people at DARPA?

FANO: DARPA, probably, which was absolutely not the case. As a matter of fact, we always put MAC money from the general MAC contract into artificial intelligence in one form or another, either into space or something else. The contract was always late in being signed. The general Project MAC fund had to tie over the artificial intelligence

work. I do not know why, because I was down here. I didn't think it was worth trying to find out... But I was still pretty much in control.

NORBERG: I am still thinking about Larry Roberts and the attempt to sever education from this process.

FANO: I do not know. I have no idea.

NORBERG: What was the implication here as a result of that? How did you fund graduate students, and junior faculty, and so on? Did that stop?

NORBERG: No. The money situation was the same. There was no change in that. I mean, there still were research assistantships. The view was that DARPA was not interested in what was happening in education. While, to me, from the very beginning, education was a crucial aspect. So I just did not talk with him about it. But money for the research assistantships and support went on. He just refused, so to speak, to take credit, because essentially our computer science program in the department was born out of all of this. Almost immediately we started planning an undergraduate program -- almost immediately. So in our view -- certainly in mine -- the academic development in computer science was part of the goal. In fact, all though my tenure as director, I was as much director of Project MAC as, in a certain sense, the protagonist in the department for the computer science faculty and education in computer science. I was playing a dual role, let me call it an associate department head, which did not exist then. For computer science I became such. I had that job later on, but at that time it did not exist. But I was essentially the department head, a right hand person for anything academic having to do with computer science.

NORBERG: What was your attitude toward Licklider's successors in terms of their ability to run the DARPA program? I am thinking of Sutherland and Taylor.

FANO: Well, I think Sutherland did quite well, and aside from this quirk, I think that Larry Roberts did quite well. I

had some misgivings with Bob Taylor about the development of the ARPANET. As a matter of fact, I got in somewhat of an argument with him.

NORBERG: Why? What was the issue?

FANO: You know, we had these meetings of all the principal investigators, and there was one -- I don't know, it was 1967 or something like that -- when he was trying to force the group to make technical decisions about the network. In other words, he was trying to design a network by committee, which to me was nonsensical. He was trying to appoint a committee for various aspects of the network. Larry Roberts, who was then his right hand man, was sitting next to him and saying nothing. At a certain point, when Bob Taylor was pressing for a decision by the group, I said, "This is a madhouse." Bob Taylor never forgave me that. Sitting next to Bob was Larry Roberts, who became the network chief engineer. He engineered the network. Not only that, he hired BBN to do most of the work, which was the right way of doing it. So you see, as different from the other directors, Bob Taylor did not have an engineering background, a technical background. He was a psychologist, a bright guy, and I think he has accomplished quite a bit later. He learned, I suppose. But he just did not understand what went into design. The ARPANET was not something that you could do by committee. Now, in terms of funding, all through my tenure the funding was always a general funding. I started getting some NSF funds, of course. I do not think anything... Well, it was NSF and ONR, and I'll tell you in a moment.

TAPE 2/SIDE 2

NORBERG: The NSF was for the theoretical work?

FANO: Yes, more for the theoretical work. I persuaded NSF to provide kind of an umbrella fund, rather than a specific one. They did that for a year or two, but it did not last.

NORBERG: They wanted you to shift to project-related work?

FANO: No, NSF operates on each little project. They did not like the idea of umbrella funding for an area. So they switched back. The ONR funding came, in a sense, in a strange way...

NORBERG: This is ONR that is different than the ARPA management contract.

FANO: That is right. This is ONR money, not ONR as a conduit for ARPA money. The head contract man in ONR played a major role in setting up Project MAC -- I am starting way back, because he -- I wish I could remember his name but I do not -- and Paul Cusick at MIT -- Paul Cusick was the controller. He was basically the chief financial officer of MIT. He was not the treasurer, but the chief financial officer. But he was the one that signed the contracts. Now, he and this ONR man had a very close, long-standing personal relationship. They worked together for years. They had total trust in each other. That was why I was sitting in my office on the eighth floor before the contract was signed, because MIT had gone ahead and rented the place, furnished it, did all the work, and committed itself before the contract was signed. Not only that, but we were authorized to spend money as early as April 1, 1963, again, before the contract was signed. There was a tremendous amount of trust, and really, all the administrative matters were handled with such speed, and we were able to move all sorts of things. There was also a commitment to IBM to get the installation. It could be done because of the mutual trust of those two men. Well, at one point the head contract man from ONR probably signed the contract, so it should be there someplace. Essentially he said, "Well, we are putting a lot money in Project MAC. I had better go and visit it." This was in the fall. I was standing there when he saw a demonstration of time-sharing, the things that were close to his interest, his eyes popped. He wanted the stuff. (laugh) So he gave a contract out of ONR, for the development of a business system. Now, when you talk about interaction with industry, there were people from everywhere that came to the lab. Very quickly it became like a shrine. You know, the number of visitors from all over was just incredible: technical and other people, and reporters, and you know. But there was one thing that stood out, however. When you talk about really top level executives of major corporations, the American companies did not come, but the Japanese were all over the place.

Officers of the major electronic companies of Japan were arriving with gifts, interpreters and everything. What a huge difference. There was the first year a book -- it must be in the records someplace -- of all visitors to the site, yes, with their signatures. It may be worthwhile to see who is in it. There is the first page of this on the wall in the offices of LCS. I remember that one of the first people to sign was Nat Rochester from IBM. That is somebody that you may want to talk with. I have some suggestions. Well, I may as well give them to you.

NORBERG: Go ahead while you're thinking of it.

FANO: Yes. Nat Rochester is somebody, because he was in from the beginning, and was very sympathetic and understanding of the objective, while IBM in general was not. I think you should talk with Ken Olsen. From the very beginning he was very sympathetic. You know, he was over several times. Some of his key people were very heavily involved. The designer of the PDP-6 that was shot... I keep forgetting his name.

NORBERG: He was shot?

FANO: Yes, he was shot while he was having dinner at home. He was a good friend of Fredkin. As a matter of fact, he left DEC and joined Fredkin and then he was shot. He was a member of the Technical Committee that advised me on Project MAC. So we had very close relations with DEC. And Ken Olsen's view may be important. On IBM's side, you ought to talk with Bo Evans, because he was managing the whole 360 system. They were just not suitable. Do you know Bo Evans?

NORBERG: I have met him once, but I do not know him.

FANO: Yes, he is a very forthright man. He tells you exactly what is in his mind. As a matter of fact, when he was a vice president of IBM, he made the IBM lawyers shake in their boots, because they could not control what he would say.

NORBERG: Did he come and visit Project MAC?

FANO: Oh, yes, later on; not at that time. Not in the beginning. He has been very close to MIT; he was on our visiting committee. As a matter of fact, he gave a talk about what IBM learned from their disaster with Project MAC, listing specific technical things that we were after, and we got through GE, that they could fix in their equipment, and some they could not.

NORBERG: Some that who could fix?

FANO: IBM. In the 360. He was very explicit. He is getting kind of old, but I think his mind is fine. I think you may want to spend a little bit of time with him, not too much.

NORBERG: Are we still talking about Evans?

FANO: No, with Jay Stratton, who was president at that time. Well, you had asked me a question about what MIT would have done about the recommendation of having a time-sharing system if it had been circulated within the community. He would have that answer.

NORBERG: I wonder if anything can be gained from talking to him that cannot be found from memoranda and various records of the Institute.

FANO: I do not know what memoranda exist at the Institute, because everything was done so informally. For instance, if you look at how he made the decision to go ahead with Project MAC at MIT, I doubt that...

[INTERRUPTION]

NORBERG: Well, you talked about the informality of it yesterday, when you went to see Stratton.

FANO: That is right, and, you know, besides the short memorandum of what I intended to do that I wrote during the weekend, and distributed, I do not remember any piece of paper exchanged with Stratton.

[INTERRUPTION]

FANO: So I doubt that there is any. No, I think that Charlie Townes... I never got a letter from him saying, "Go ahead."

NORBERG: Since we have so little time, I want to ask you one other question. In keeping with this list of names, who are the people who were associated with General Electric at the time who would be useful to talk to, besides Joe Weizenbaum, obviously?

FANO: Well, are you talking about after we got the computer?

NORBERG: Well, during the negotiation phase, and then after you gave them the contract.

FANO: Well, there were two people, Weil and Couleur -- they are the GE authors of that series of papers. One was hardware and the other software. Their boss knew nothing. He had just arrived in Phoenix from heading a lamp plant. You know the style of General Electric -- managers are managers are managers. He knew absolutely nothing. There were various people involved later on. You may want to talk with those two. I do not know where they are now, but maybe Corby knows.

NORBERG: We have had some contact with General Electric people, but there are no records, so we cannot look any

of that up.

FANO: Yes, I think Corby can put you on the trail, if it is possible. I do not know whether Joe Weizenbaum had much contact with General Electric since he worked there. Incidentally, I may have mentioned to you that if you have trouble finding a lot of documents about Project MAC, you will probably be able to find them in Corby's files. Corby does not throw anything away.

NORBERG: He pulled out some things for me the other day.

FANO: Oh, I wonder whether they are in some special boxes that you could feel free to separate things -- that you could just take and look at. Corby does not throw anything away.

NORBERG: I think we ought to wait and see what sort of questions we have that cannot be answered with what we have, and then at that time approach him and ask for that sort of thing.

FANO: Yes, keep in mind that he probably has most of them. He has the fullest records.

NORBERG: When you decided to step aside from heading Project MAC, what were the reasons for stepping aside?

FANO: Oh, I had had it. You see, I had never been fond of being an administrator. It was hard... Also, I did not want to spend the rest of my life being an administrator, and I felt that at some point I had to get back to being a professor. There was no particular, specific reason.

NORBERG: Okay, there was no special research project, or...?

FANO: No. I had been there a few years, and I do not think that you should spend more time on some things...

NORBERG: Did you ever visit SDC in that period? What was your impression of what was going out there?

FANO: I was just there once. I do not have a strong recollection. I remember seeing the Q-32. I don't have a... Well look, we have to quit. But there are lots of other things that I can tell you about.

NORBERG: Be my guest. Just sort of tick off a list so that we will remember them for next time.

FANO: I think the major thing is what happened in Project MAC. You will find some things in the papers about the community surrounding it. I suppose on the personal side, we were more interested in the social implications of computers and why there is the story behind it. ? ? . Of course, the whole question of what went into the General Electric system and Multics, I suppose you got a lot of those things from Corby. I do not know what you got from him.

NORBERG: In fact, we stopped just before Multics, with a plan to talk again.

FANO: Oh you did? Okay, well, I think that whole thing needs to be considered. Also, its implication relative to ARPA's act, because when Multics started being late, ARPA almost killed it, you know, cut off all the money. There was a committee -- this was after I resigned as director -- that was appointed to review.... And somewhat to the surprise of the ARPA people, and of Licklider, who was then director, it recommended they continue the funding.

NORBERG: He was then director of...? Project Mac? Okay, so there is the Multics story, and what else?

FANO: The Multics story. The community, for example, was very, very interesting, because clearly our goal was the creation of an on-line community, and to a large extent we succeeded except for people elsewhere (?). It was very tricky. In a sense the... People that were part of the community, living as part of the community office-wise, were fine.

But we never succeeded in getting somebody who was physically removed to really use the time-sharing system. This is something that at a certain point I tried to fix in collaboration -- start a project in collaboration -- with Bell Laboratory. But the laboratory backed out.

NORBERG: Would this be similar to what Rand and SDC had going in Santa Monica? They were using the time-sharing system between them.

FANO: No. The point is, how can you end the technical isolation of the individual who works at the terminal remote from other people? When he tries to do something that does not work he can't go next door and say, "Hey, what am I doing wrong?" Basically, we were trying to develop help for the isolated user on the system. Documentation is not enough. The documentation of the system was on-line, but that was not enough. At one point we gave a terminal to Jerry Wiesner, and he told me he had trouble. You know, he was in his office. He was dean at the time. He was isolated.

NORBERG: We ought to pick up with that at the next time. Good, thank you.

END OF INTERVIEW