

An Interview with

WESLEY CLARK

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Conducted by Judy O'Neill

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Wesley Clark Interview
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Abstract

Clark describes his research at Lincoln Laboratory and interaction with the Information Processing Techniques Office (IPTO) of the Advanced Research Projects Agency (ARPA). Topics include: various custom computers built at the Massachusetts Institute of Technology; timesharing and network research; artificial intelligence research; ARPA contracting; interaction with IPTO directors; the work of Larry Roberts at IPTO.

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O'NEILL: I would like to begin with your background, before you went to Lincoln Labs.

CLARK: I went to Lincoln Labs from the Digital Computer Lab under Jay Forrester. That laboratory was absorbed by Lincoln as one of their major divisions to put radars and computers together in the cause of air defense - the so-called SAGE systems, Semi Automatic Ground Environment system. Lincoln Laboratory was formed to put computers and radars together and ground communications so as to help defend the country against what was then perceived to be the principal threat from the Russians, which was airborne bombers over the pole primarily but not exclusively. And a very large enterprise eventually, out of which a fair number of important developments came. And Lincoln Laboratory took the Digital Computer Laboratory (then in existence for only about three years I think, perhaps), which was also at MIT, as one of its main components. And the other main component came from the old Research Laboratory of Electronics, which was an early wartime, World War II time establishment built to develop radar as much as anything, and to help the British to start with and then very heavily into the war effort in our country. And the communications part and the radar technology came from that side, and the computer stuff came from the Digital Computer Laboratory.

O'NEILL: So you were working in the Digital Computer Laboratory.

CLARK: I was working in the Digital Computer Lab.

O'NEILL: And when did you start there?

CLARK: 1951 I think, and I had gone there from Hanford where I had worked a couple of years and did some experimental physics - actually on leave of absence from the University of California Department of Physics.

O'NEILL: Had you gotten a degree from there?

CLARK: Actually, no; in fact, I am a dropout. I was working on this degree and got tired of it and confused, and went off to adjust my perspective, as the interviewer said, for a couple of years at Hanford, following which I was expecting to go back and complete my work. But I became interested in computers while I was there at Hanford and applied to MIT's Computer Lab, which was trying to staff-up new role in air defense.

O'NEILL: What is Hanford?

CLARK: Hanford is the Atomic Energy site, then run by General Electric Company. It's a plutonium factory. It's in the news a lot these days because people are tracking down the records of folks like me who were exposed to radiation during that period to see if anything has happened to them. And they shut down a large part of that plant now; it no longer functions. I was there for a couple of years anyway, where I learned a bit more about computers and then left to go to MIT.

O'NEILL: What kind of... I can't imagine there was a lot of computer activity.

CLARK: No.

O'NEILL: Were these analog machines?

CLARK: They had analog machines; that's where they started. But work was just beginning with some punch card machinery from IBM. You needed them to figure out when it was safe to shut down these big reactors. It turns out you can't just shut down a nuclear reactor of that type any time you want to, because of its delay reaction characteristics. Sometimes if you shut it down at the wrong time you won't be able to keep it at zero power or whatever level it goes to. It will begin to become reactive again all by itself, and that's not good. What you want to do at all time is keep nuclear reactors under control. But they weren't doing very much with computers. But such as it was, I became interested in them. And I had also become interested in following what for me was a very influential article in *Scientific American* by Edmund Berkeley, later a figure in setting up the Association of Computing Machinery. He had written about a simple relay gadget called "Simple Simon" I think, which was quite fascinating to

me. So I became interested in computers and went back to MIT where I started programming the WHIRLWIND computer, the main engine of the computer laboratory. And then later worked with the Memory Test Computer - MTC, it was called. Something that was designed and put together in a hurry by Ken Olson and some associates to test out the first magnetic core memory.

O'NEILL: Okay, so that was a direct spinoff from the WHIRLWIND?

CLARK: Well actually, it was part of the Digital Computer Laboratory program, and as soon as the memory had proved itself... and indeed, it was dazzlingly good compared to anything we had had up until then, and in particular the memory that was then operating on the WHIRLWIND machine. It was taken from the MTC group and moved to WHIRLWIND. The MTC later got a second one, a different one, a larger version. And that MTC group pretty much formed the core of a subgroup of the Digital Computer Lab division of Lincoln Laboratory when the whole thing moved out to Lexington, which as you know is to the west of Cambridge, and became the Advanced Computer Development Group. That's where I spent the rest of my time at Lincoln.

O'NEILL: Okay, so the Advanced Computer Development Group was responsible for the TX-0?

CLARK: Yes: the TX-0, the TX-2, the L-1 (so-called), the FX-1s film memory machine. When they had machines like that they started the LINC. Then they had to move that for political reasons to another operation outside of Lincoln Laboratory. The history of that part, by the way, has been documented already. You would find that in *The History of Personal Workstations* book the ACM put out that Adele Goldberg wrote.

O'NEILL: Okay. What kind of work were you doing when you were working on the SAGE? Were you programming that?

CLARK: I never did work directly on the SAGE system except in an advisory capacity as a member of the Advanced Development Group. I guess I had done some pilot studies as a member of a team - feasibility studies, mostly sales studies, salesmanship to the military when Jay Forrester was trying to persuade the military to fund the MIT operation as opposed to the Michigan operation, which was a competitive operation built both around the quest to

have some help by the government, and the air defense problem. And I did some programming in that connection. I think of it as a large demo or something. It didn't last more than a week, although it was a very difficult, very busy week. But once the SAGE system had gotten under way, SAGE system and development had gotten underway then I did participate a bit as a tackle from time to time with various other permanent groups then at Lincoln who were essentially the rest of the Digital Computer Laboratory. Except for the group that formed the small subgroup that formed the Advanced Development Group, everyone else in the Digital Computer Laboratory became committed to the air defense program. We thought we were fortunately isolated so as to pursue some advanced work.

O'NEILL: What was your role in the Advanced Development Group? Were you a manager of that group?

CLARK: No, I started designing logic, gradually shifted from programming activities to logic design. The group when it was formed was under the direction of a fellow named Dave Brown who subsequently left and went to St. ?

Institute, turning the direction of the group over to William Pakin (?). He was the fellow who was a graduate student who put together the first magnetic core memory array that was tested on the Memory Test Computer, the MTC. He and, let's see, Ken Olson, who I guess you wouldn't know, and I and, let's see... also some work in magnetic core, physics and magnetic materials, and two other people - John Goodenough primarily. And another group interested in transistors... in circuit theory (this was pre-transistors) pretty much constituted I should say the principles of that group although the group perhaps numbered 40 or 50 people; I don't remember. Perhaps not right away; I think it started very small.

O'NEILL: Do you remember what year that was?

CLARK: Well, that would be approximately 1953. And more details of that can be found in the history of the WHIRLWIND project, which I think you have seen documented - Redman and Smith.

O'NEILL: Redman and Smith, yes.

CLARK: So I started out with a small group of people looking into various architectural improvements to computers that could be made. Ken and I then proposed the construction of a fairly ambitious machine to the management built

around some new logic that I had worked out and some circuit ideas that he had worked out with vacuum tubes. Actually, I think this was on the edge, going from vacuum tubes to transistors, which were just beginning to appear. And that machine would have been far too much for us to undertake, especially as a fledgling group at that time. And it was turned down by the management and we didn't do that. I then backed off and said, "Well, all right, let's build the smallest thing we can think of," and designed the TX-0, which was very primitively structured and quite small, quite simple - small for the day. Not physically small - it took lots of space; it still took a room. And then I proposed that that be part of a continuing program. It was the first step of getting toward the machine that looked like the one I really had in mind the first time, and that would constitute a component of that machine, perhaps a front end to it or something, I didn't quite know. But it didn't matter. And that plan was approved and pursued. So we did build the TX-0 and started work on the TX-2, which was the name we gave it. We had called this proposed machine that was rejected the TX-1. We decided not to use that name since we had already had some documents around in the files. So the TX-2 was the second machine to follow the TX-0. The TX-2 was big. It was, I suppose, the biggest machine in the world at the time - logically biggest (in terms of its central characteristics, not its peripheral equipment). But the SAGE system was coming along at that point and that was really big stuff - dominated by the peripheral equipment. Operating consoles for military operators and things of that sort - very big stuff. And extensive drum memory, cabinets to provide enough storage. The central memory though was still very primitive, a very small magnetic core memory. That's all we had. That work and activities more directly related to communications and the radar stuff was what most of the rest of Lincoln Laboratory did. They did it out in Lexington, with radars all over New England.

O'NEILL: Did you have to sell the TX-2 to anyone besides your management? How was the project funded?

CLARK: Well, the Laboratory... Lincoln was funded by a combination of military funds - primarily from the Air Force, and then budgets were worked out below the level of the Laboratory to the various operating divisions. I think there were about six or seven divisions, one of them a service division. And the computer division was division six at that time. Communications was division two. The funds were justified of course as military but the military was so anxious to have the help that funding, I think, was never a serious problem. It had been a serious problem for Jay Forrester when building the WHIRLWIND machine a few years earlier - quite a significant problem. And that's well-documented by Redman and Smith. But funds were never a serious problem on something the size of the machines

that we were beginning to develop them experimentally. The Advanced Development Group did take special justifications.

O'NEILL: Okay, were you involved in doing those justifications?

CLARK: Not so much. That was really part of the group management's problem. That was just at the point of the transition of the leadership of that group from Dave Brown to Bill Pakin. And that was not something I got involved with very much. I justified things technically of course. But the part of the then-proposed costs... Back then the funds were rationalized at a higher level. We didn't see that part of the process.

O'NEILL: Did you have an on-going connection to the TX-2 once it was built?

CLARK: Oh, yes. The TX-2 took several years to build. The TX-0, being much simpler, went together fairly rapidly, although Ken had to design some new transistor circuits for it. And the TX-2 was a very large machine by comparison and very complicated - far more complicated than a small, competitive business, getting involved with given the technology of wired circuit boards and so forth, as opposed to the printed circuit boards and nice little circuit packages that we have now. And it did take a very long time to make; several years. But we began operating it as a working machine long before it had even its arithmetic elements, because it had a small arithmetic element tucked off to the side that wasn't really part of its main engine but helped with the dressing. And it was quite enough to give the machine full generality so we did a considerable amount of programming even before it was finished. And it was never finished in one sense; it was, as an experimental machine, always taking on new evaluation tasks with new pieces of equipment that came along, or new ideas. For example, the magnetic film memory, which was thought to be a strong contender for a very fast, but smaller memory, and which was also developed in the Advanced Development Group was first tried out on the TX-2 as one of its piece. As a matter of fact it was a very important piece. It held the program counters and index registers, as they are called. And other pieces that were special, we designed a special tape unit, for example, and other pieces like that were added to the periphery of the machine to try out various other ideas, and improvements, and so forth. It was not intended to be a machine that was finished and used by all comers. It was really something that was run fairly tightly. And in fact, I pretty much managed the assignment of time and allocation of resources on the machine the remainder of its life... for the remainder of the time

that I stayed at Lincoln.

O'NEILL: Were there a lot of people wanting to use it? Was it difficult to schedule the time between various people?

CLARK: Well, I had to fend off some charges... some claims on the time by research groups not part of the Advanced Development Program. But on the other hand, I let in lots of friends who seemed to have interests that were relevant to what I considered good things for the advancement of the computer art. And it was kind of a mixed story. I did not want to see the machine operator as a large resource to which people could come and bring problems. I preferred rather to have them become involved with some interesting program already in place or planned within the Advanced Development Group, and then worked with members of the group comparably to carry out the research. And that made it possible to keep the club of users small enough so that people could be assigned fairly substantial blocks of time. I mean, this hands-on operation which was necessary on the TX-2. This is not something which to use you first prepared a stack of cards for, submitted to an operating bureau to run. You know, you had to do it all yourself. Let me just look at my record here to answer that question more completely for you, about users, because I do have a stack in here somewhere of timesheets as it were for the computer for the stated period. Well, they are not in that box, to my surprise. But I do have them. They are interesting; they show what groups used the machine over what period of time. They are sort of sign-up sheets, or assignment sheets made out once a week. But they are not in this box, I'm surprised. Perhaps I will find them and send them out to you.

O'NEILL: You said that you prefer to have people working on projects that were already being worked on, on the machine.

CLARK: Yes. We have, for example, speech research as one fairly large activity, and that was of interest to some people in the group as well as people out of the group. And they worked together in various aspects of that problem for a number of years.

O'NEILL: Can you describe what the reaction was to the hands-on approach to the computer that the users were permitted with the TX-2 versus punching cards and sending them off?

CLARK: Well, oh, as opposed to punching cards. The members of the group, for the most part, never punched cards. So that was not a novelty for... The group now came out of the WHIRLWIND, MTC background - the small, hands-on, paper tape stuff. The MTC, for example - as did WHIRLWIND and TX-0 and TX-2, had a large number of switches and so forth that you could throw by hand, with your fingers, and in some cases had to, that could be used to control parameters of operation of all the things in progress. Keyboard interaction may have started with WHIRLWIND; I'm not quite sure, although that wasn't the principle mode. And then all the stuff from the SAGE system was pretty much hands-on. That was coming along at the same time. And so that was pretty much the tradition for a large part of the people, and very nearly all of all of the people in the Advanced Development Group. It's not surprising. And the only other mode at MIT at the time would have been the mode of the Computation Center and that was an IBM installation, as I remember. And that was the punch card and batch processing mode of use with 24 hour turnarounds and things of that sort. Turnaround meaning the time between submission of your deck and seeing the first proof that the program didn't work.

O'NEILL: So the two communities really weren't very intermixed. There were the people working on the TX-2 and the people working at the Computation Center.

CLARK: No, not at all. And this of course was pre-Mac by several years. There was quite a bit of experimental work still going on in connection with the WHIRLWIND, still in operation down in Cambridge and still formally under the control of the Digital Computer Laboratory. And experiments of very interesting kinds were tried there, and I know that Doug Ross has described many of those activities very engagingly. That's reported in Adele's book.

O'NEILL: What was your view at the time on interactive computing, the man-machine interaction, or man-machine symbiosis, as it has been called?

CLARK: I'm not sure I had a view except in contrast with the modem use on the other machine with which I was aware. I never knew anything else. After all, I had worked with WHIRLWIND and MTC, and that was just the way you did things with computers as near as I could tell. The more nearly you could control them, the better they worked - that is, the better you worked, the happier you were. We became pretty fussy at times. It was necessary to set up switches in some cases before anything would run. That was a nuisance on the memory test computer for

example, and access to some of those buttons and knobs and so forth was pretty much proscribed in the case of WHIRLWIND because they were dedicated to holding test programs and things of that sort that you weren't supposed to tamper with. But the interactive use was just something... we didn't know anything other than that.

O'NEILL: In the early 1960s there started to be a lot of talk about time-sharing. What were your views on time-sharing? Do you recall?

CLARK: Yes. I'm one of the oldest continuous floating (?) objectors in the business. I still think it's a bad idea. Time-sharing, I assume, in your question means capital T, capital S - Time-Sharing, as defined by Project Mac (approximately). The term "time-sharing" came out of the early work with WHIRLWIND and the SAGE system where it meant something far less grand. The SAGE system was built around a pair of computers - one of them serving pretty much as a backup. The SAGE system per node... I mean a network of computer nodes around the country built around a pair of computers - one primarily a backup, and yet accessed by hundreds of operators through display consoles in dark rooms looking at radar pictures and computer re-representations of the important parts of radar pictures and tracks of airplanes and the military hardware at hand to defend, and all sorts of other things that went into the air defense problem. And using the machine in that way is what was originally meant by time-sharing. Actually I think that's probably a transitional sense in which the term was used. It was first used in somewhat more of a constrained sense, simply to mean the moment-by-moment carving up of the time of a resource - among two or more cooperative users of a resource. It was used in that sense by Bob Everett, for example, who was one of the principle designers of the WHIRLWIND machine, and by the engineering staff at WHIRLWIND, to describe the mode in which parts of the resource were dedicated instant by instant to one thing and then to another. But the overall program or resource at the time was serving the function of only one principle task. And this was the matter of resource allocation to best meet that task without using too much equipment. That is a very limited sense. Then the SAGE system, I guess, took that a step further by having so many different kinds of operators for their little display terminals that had to do different kinds of things. But nonetheless, they were all working on the same thing, meaning the air-defense operating system that was then in force. The Time-Sharing - capital T, capital S notions which sort of grew up following observations of the behavior of the SAGE... well, the behavior of ideas that are on the SAGE system, and perhaps the TX-2 and TX-0 as well. But more likely from the campus part of MIT, the Cambridge people realized that... who could not fail to realize that access to the machinery, hands-on access, was a

very happy thing to have. In that sense, it was quite a different one. It meant the competitive use of the same resource for different purposes. And that's quite a different thing from the carving up of resources to fill out the needs of one task. And that competitive stuff requires a great deal of coordination to keep matters straight, from one user to another. And unfortunately, the idea of time-sharing became a bit confused, I believe. The access part was a great idea. Everybody should have good access to good computers; nobody can deny that. That was hardly a whim. But the trouble is, people generally wanted to use machinery that wasn't around. So they had to share in one way or another. And there was so many people that it was thought that the way to provide the access was to carve time up into little pieces and then alternate from one competitor to another, and to do that so fast in fact that no one realize that... or no one would have the perception that there were any other competitors around. And in fact, all of those competitors had to be kept in good order with respect to one another without their knowledge by the coordinating program, and the operating system, the time-sharing operating system for the resource. And that was going to take away a great deal from the efficiency of the machinery. So it's a very big price to pay. And in the time-sharing paradigm the more users you have the better.

TAPE 1/SIDE 2

CLARK: So the time slices get very small. And the number of coordination steps goes up, so the overhead costs become very, very large to run a time-sharing system like that. And what is perhaps worst of all, the idea was based on taking the largest machine that you could pay for, and then spread the costs around among all these users. But unfortunately, that meant that regardless of the need that the user might have, he had to pay for the entire machine during that fraction of the time that he was using it, whether he need it or not. And I guess to give you an extreme example, if he's running only a small program that has small memory requirements for data - limited input/output requirements and so forth - he is nonetheless running that on a machine that is inappropriately large, ill-matched to the job - an enormous machine compared to the size of the problem to be done. And he has to pay for that whole machine, for all of the time that he uses it. And the time is scattered into tiny chunks, of course. But the money has to come from somewhere, and that's exactly where it comes from. Not quite, because of Licklider's perception that there should be an IPTO that ought to fund something like that, with the result that that tradition was established in the subsidization of such systems, and is still the case in the university world. There was a study group formed - a long term computation study group, or something like that.

O'NEILL: Yes, I can't think of the exact title either.

CLARK: Okay, I was a member of that group. That was formed at just about the time that Licklider set up the IPTO or whatever it was called at the time. And it was clear that that group already had in mind just what they wanted the final report of the study to be. They wanted a greater access to large machinery. And here was Licklider saying, "Gee, this would be a neat thing to try." Sure enough, that's what the report came out to say. The Chairman of the committee, a fellow by the name of Herb Teager, and I both declined to sign the final report.

O'NEILL: While this committee was coming up with the report, did you have discussions with the people on the committee about these issues? I mean, was it active...?

CLARK: You mean, were the committee meetings active? Well, yes and no. As I recall the sort of case study proposals of how well things would work if we only had this kind of stuff, and so forth. I tried to point out that it was going to be very hard to do real-time work, or even non-real-time, but display work, for displays. You see, the image in mind was that of a typewriter, or teletype machine actually, as the principle means of interacting with this time-sharing machine. And that's very limited. But there was other work going on at the time with these displays starting with WHIRLWIND and through MTC and TX-0 and TX-2, all of them display-based machines - screen display, CRT. And that was going to be nearly impossible to do, even on a very small scale, on a few displays, on a time-sharing basis with the technology we had at the time. And I thought that would be a loss because I knew very well that that is your principle means of interacting with a computer if you start to do any kind of interactive work. But the committee was not interested in hearing stuff like that.

O'NEILL: Did they respond at all to your objections?

CLARK: Not so much. I think they were simply swept aside or tucked in the, "Yes, that's an interesting consideration," department.

O'NEILL: Did Teager have the same objections?

CLARK: No. He thought that the community wasn't being visionary enough and that time-sharing could do far, far more than he believed the things that they were proposing for it to do - tying the community together and that stuff. Project MAC, as you know, for awhile didn't quite know what the MAC was an acronym for. Multiple Access Computing or... What was the other?

O'NEILL: Machine Added Cognition.

CLARK: Machine Added Cognition, right. That's possibly the Licklider influence, I don't know, since he was a psychologist. I remember when I first met Lick he was in the same basement wing of the Lincoln Laboratory in which the TX-2 was operating. And I wandered down the hall one time, and off to the side of this dark room, way near the end of the hall, sort of... I think that's where the stock room was and that point, or part of it anyway. And off to the side was this very dark laboratory and I went in, and after probing around in the dark for awhile I found this man sitting in front of some displays and doing some things. And he was doing some kind of ... a piece of psychometrics, or preparing an experiment for such a study, perhaps by himself. And we began to chat, and he was clearly an interesting fellow. And I told him about my room, which was sometimes dark down at the other end of the hall, and invited him to come down and learn about programming, and he subsequently did that. Well, Machine Aided Cognition, Multiple Access... The problem with those time-sharing systems is their enormous inefficiency and the cost to user. If the same resources that have gone into development of time-sharing had gone into making smaller computers easier to use then we would certainly have quite a different instrument.

O'NEILL: Do you think that the committee at the time was influenced by Licklider?

CLARK: Absolutely.

O'NEILL: And do you think that it was also the influence of having the IPT available to do the financing of such machines?

CLARK: Well, I am sure that was an opportunistic consideration. I have the impression that MIT was in the habit of

defining some major goal or other and expecting money to come as a matter of right, from whatever source. That may or may not be so... may not be true, but that's my impression of how MIT operated, especially in those days. It was not a major concern. But it is nothing that I wondered about very much at the time in any case because I was working in a very well-protected and well-funded laboratory with all the resources that I needed, for the most part. I wasn't very much interested... I'm still not very much interested in money. Otherwise I would be off in industry I suppose.

O'NEILL: Do you know why you were on the committee in the first place? Was there a connection with the TX-2?

CLARK: Sure. Yes, the TX-0, TX-2. I think both Ken and I were made lecturers in electrical engineering. I know we were. I think it about that time... earlier than that time, as a matter of fact. The motivation was primarily to have an official academic tie at Lincoln Laboratory that made it possible to supervise students coming from MIT and doing their work at Lincoln. And so I was a logical person to participate on that committee. And the members of the committee, or some of them... It was a fairly large group as I remember - some 30 people maybe... 20 to 30 people. Some of them had been visitors to Lincoln or had known about the activities of the TX-0 and the TX-2. There wasn't a great deal of traffic between the Cambridge people and the Lincoln Laboratory people - not very much. Just in general, and among that group of people, very little.

O'NEILL: After the committee prepared its report which you declined to sign, did you have any further discussions about time-sharing with either that group or other people?

CLARK: Well, sure. One couldn't avoid it at that point, because it was supposed to be a hot new topic around which the field was going to turn for decades. And it did in fact. So other talks of that kind could not have been avoided - discussions, just in the air, all around - couldn't avoid it. But we hung on to the simpler use of the TX-2 throughout all that time. Ed Fredkin wanted to turn the TX-2 into a time-sharing computer for which it would have been pretty much a natural actually, owing to the particular sign of its input/output control system, which made it possible to switch back and forth among different users easily. But in my conception of that thing in the first place, those users were pieces of equipment primarily, not humans. And they were working cooperatively on the same task. I called that mode of operation (very badly); I called it multi-sequence operation, but it's something closer to multi-tasking. I

guess multi-tasking is what it would be called today. A bad choice of words on my part. But the idea was that those various users, pieces of equipment, would be working together on the same task and would be programmed in a coordinated way by a single programmer who was a real human on the system in that case, who had to design his single, multi-tasking program with all of the tasks in mind, although the detailed execution of the tasks, sub-tasks in their own time sequences, would be independent and under the control of the various devices involved - each of which would have its own time characteristics.

[INTERRUPTION]

CLARK: Now, after that interruption I am not quite sure what point I was trying to make, but let me reemphasize, if I haven't said it enough already, that the difference between the stuff that was going on with the TX-2 and what was proposed by that group and did become Project MAC was the difference between cooperative parts of a single program and non-cooperative parts of competing elements under a single operating system. And making that operating system was a bit of a trick given the hardware that they had. The TX-2 would have been better at it, given its multi-tasking structure. And so, for example, Fredkin was quite put out with me for not seeing the beauty of the time-sharing idea and supporting it wholeheartedly but I didn't want to do things that way. The TX-2 time was used in blocks ranging from... Oh, a typical block of a half an hour or something like that, and then many blocks of time for, oh, four and six hour runs. And that meant that the number of users in the club was necessarily small - on the order of a dozen people or something like that, not the hundreds visualized by the time-sharing alternative. It wasn't really an alternative because you could not have done the same kind of work that had been done on the TX-2 on any time-sharing system then conceived. Well, for example, Ivan's Sketchpad work which was done on the TX-2 required enormous blocks of time.

O'NEILL: So, it couldn't have been done on a time-sharing system because...

CLARK: There were no displays.

O'NEILL: Oh, because they just had the teletypes?

CLARK: That is a graphical system. In a couple of years they did try to add displays to the Project MAC system. Something called a KLUGE was added as I remember. And that was a very large box of equipment that practically ran itself. It didn't need the computer; there was so much stuff in it. But you couldn't do graphical work with the Project MAC stuff as conceived. So they made their points in other ways with symbolic programming and stuff that was not input/output limited. In fact, they made a virtue of the observation that a teletype keying operation was a very low data rate operation limited by the rate at which people can type keys. And that that meant there were many, many opportunities from one competitive user to another in between keystrokes.

O'NEILL: Was Sutherland one of the 12 people that you mentioned?

CLARK: Oh, yes, sure.

O'NEILL: Was he working with you as a graduate student?

CLARK: He was a graduate assistant in the laboratory, as was Larry Roberts for example.

O'NEILL: I wasn't sure whether your time overlapped with Roberts.

CLARK: Oh, yes, it did. Larry in fact wrote the operating system - what was it called? - MARK IV for the TX-2 which was used for many years. Well, he was a graduate assistant, but formally a student of Claude Shannon, Sutherland was. Claude Shannon or perhaps another member of the faculty who were his primary mentors. But that work was done on the TX-2. It could not have been done on any other machine in existence at the time.

O'NEILL: Where does the LINC fit into this?

CLARK: Oh, my! Now you really do need to see the LINC story, in that red book.

O'NEILL: The History of Personal Work Stations.

CLARK: Yes. That is all fairly well laid out there. I can fill in some details if you could be a bit more specific.

O'NEILL: Well, I was wondering if that had influenced your ideas about time-sharing because you were more focused on individual machines, a personal computer idea.

CLARK: Well, I guess that since I knew that time-sharing wasn't going to work... You may find that a strange statement but it never has worked. And because I wasn't interested in it in the first place and was already embarked on programs of my own I didn't really give it much further thought. The LINC was motivated by another series of interactions that a number of colleagues of mine and I had with yet another group at MIT called the Communications Biophysics Laboratory, then under the direction of Professor Walter Rosenblith who was subsequently the provost at MIT. And that laboratory tried to apply various techniques, and increasingly, computer techniques, to the understanding of the behavior of organisms - animal structures in particular: brain tissue or nervous system tissue. And a number of members of that laboratory were users of the TX-0 and TX-2. And in fact, Charlie Molnar, who had been a graduate student of Walter's, was one of the foremost users of the TX-2. He was one of the fellows who helped design and debug some of the later parts of the machine, and spent many, many ten-hours blocks overnight, I am sure, trying to complete his dissertation on that machine, as no doubt did Ivan, who was a student at the same time.

O'NEILL: What do you mean when you say time-sharing has never worked? Is it the fact that it has always had some inefficiencies?

CLARK: Yes, it has always been too expensive. We have over the last 20 or 30 years wasted a good fraction of the computing resources in this country, in my view, simply because those same resources could have been applied differently with much greater effect. There was certainly a need for better access machinery in those days. But there was no need to make such a virtue of competition.

O'NEILL: Did you ever discuss this with Licklider when he was at IPT?

CLARK: Oh, perhaps... You mean, state my feelings about it and possible objections and so forth?

O'NEILL: Yes.

CLARK: Oh, I am sure. I can't remember ever a detailed debate with him or a another member of IPT on that subject.

O'NEILL: Okay. I just wanted to be sure that you still had the same views on time-sharing.

CLARK: I'm pretty sure. I don't think I have changed much over the years. There is still a lot of good stuff I do not mean to put down. I mean, it focused attention in a very nice way. It helped centralize funding, and it helped facilitate funding through that centralization. It did provide better access for a large family of important work, of projects that had to go out... that wanted to go out. And that was a plus; no doubt about it. But then a mystique sort of developed that somehow there were other values less easy to measure that had to do with community - things of that sort. Well, yes and no. It turns out that there are many, many communities that develop around any kind of a shared resource. Some of them are directly due to the fact that a single resource is being shared, but many of them are not. They have more to do with the fact that interests are held in common with respect to a given body of work. But the access even through a teletypewriter was certainly better than submitting punched-card stacks to a batch operation. That was a step forward, but you could certainly have had multiple access systems, better accesses systems, without time-sharing.

O'NEILL: Okay. I guess I would like to move forward to the macromodules project...

CLARK: Wow! Okay.

O'NEILL: ... unless there is something in the gap there that I should know about?

CLARK: Well, the LINK, but that's already been pretty well...

O'NEILL: Yes, I didn't want to go into a lot of detail about that because that is available. Was the macromodules project the first project that you had gotten DARPA funding for?

CLARK: I think so. Let me see. Yes.

O'NEILL: Can you give me a short synopsis of how and why the project got started?

CLARK: Well, in the formation of the Center for Computer Technology and Research in the Biomedical Science (I think I have got the name right), which was put together by Walter Rosenblith and Bill Papian when it was necessary to move the LINC project from Lincoln Laboratory and set up shop in Cambridge, at Kendall Square... began to map out its program. It was necessary to say something about what would follow LINC. It was necessary to tell that story to potential sponsors and to potential... what should I say? ...supporting institutions in the area who were all to join together and to find the rationale for centralizing something of this size. That was the premise, that research institutions within a couple hundred miles or so of MIT (with the exception of Yale, of course) would be invited to cooperate in some way, to sponsor a research and development program of this kind involving both computers and biology and medicine. And in painting a possible future, I set forth the idea of macromodules without quite knowing what they were. There was a great deal of handwaving. But it seemed to satisfy the need at the time. So when everybody seemed to be happy then I went back to finishing the LINC. Then we moved out to Washington University subsequently, and that operation fell through at MIT. And I was fired for the second time for insubordination. At Washington University the program did take place. And in large part the substance was provided by Severo and by some other colleagues who moved with me from Cambridge, Michelle Stucky, and Charlie Molnar who had by then finished his degree at MIT and joined the University, and others. Then things began to take place at Washington University and we did look for ARPA support because it wasn't going to be possible for NIH, the agency that would have been the primary sponsor of the Center for... etc. at Cambridge found it unlikely that sufficient funds would be forthcoming following the MIT fiasco. So we looked around for other sponsorship, and so DARPA (ARPA at the time) was invited to help support the work. And Ivan, who was then running the IPT, said that that was one of the most difficult decisions he had ever had to make, whether to fund that St. Louis operation, because he thought the work technically was terrific but he had the conflict of interest problem.

O'NEILL: Because he had been so closely involved?

CLARK: Sure, with the TX-2, not with the LINK. He now feels, by the way (if you were to ask him I'm sure he would say this) that the government got more than its money worth in the single study... sub-study that fell out of the Washington University program having to do with the... what's called a glitch in bi-stable or multistable elements, synchronization failure and things of that sort - work done by Charlie Molnar as much as anybody, and some of his colleagues who are still there at Washington University.

O'NEILL: Why did you go to Washington University?

CLARK: Well, we picked that University... Charlie and I pretty much made the final decision privately, although nominally the coordinating... The dissidence among the coordinating committee of the Center for... etc. were charged with deciding what was going to happen, where to go, as a body, trying to hang together as much as possible. And there were many contenders. We had looked at universities all around the country. The University of Pennsylvania was a very strong possibility. Washington University, because we had had some contact with Jerry Cox at that university through the LINC program. He is now chairman of the computer science department there, but at the time he was a research scientist with the Central Institute for the Deaf, one of the Washington University affiliated operations, and had just established a bio-medical computer laboratory at the Medical School at Washington University. But we did see a number of universities: Chicago, Northwestern, a strong possibility at Rochester. But eventually we elected to go to Washington University in St. Louis - quite an upheaval for a lot of people.

O'NEILL: How many people went with you?

CLARK: Oh, there were about 10, but we were joined by several more subsequently. And Severo was one of the people to go immediately.

O'NEILL: Once the project was actually established at Washington University, do you recall how much of the funding was from NIH and how much was from DARPA?

CLARK: Yes, it was about 60/40 - 60 NIH and 40 ARPA.

O'NEILL: Can you give me some idea, if there is any, of the difference in the funding techniques of the NIH and of DARPA?

CLARK: Oh sure, you bet I can. You must really have better data on this than I can give you.

O'NEILL: Well, I am interested from someone who is actually involved in getting money from both of these organizations - how you viewed them.

CLARK: Well, you must remember, that was in the 1960s. It was kind of a heyday of government funding. NIH and ARPA had budgets that were growing. I almost felt as though I was called up from time to time to see if I wouldn't be willing to take another quarter of million dollars off their hands. You see, it wasn't quite the same sort of thing. The ARPA work was on contract, the contract mechanism and the NIH funding was through the grant mechanism, and those are quite different kinds of things. The NIH has considerably more paperwork associated with the grant mechanism than it does from its own contract mechanism, which it also has or did have, but we did not use. And ARPA operated only on the contract mechanism. On the other hand, in the ARPA system once you were in you were a member of a club, a stable of supportees by the IPT, with the early meetings in exotic places (preferably, but not always), and a pretty good sense of community with other people who were receiving support from that office. And of course, IPT also had a sense of mission that the NIH people generally did not manifest, although they may have had. And so, for example, Larry's thing was networks and Ivan's thing was, I suppose, graphics, not graphics per se. He did not support... innovation, I would say. Lick's thing was clearly time-sharing. Bob Taylor's was computer science education, and so forth, the early directors of the laboratory. And Al Blue, steadfast fellow through it all that he was, was assuring that enlightened civil service would always be applied to the general problem in funding good science and technology with this unique opportunity of IPT.

TAPE 2/SIDE 1

CLARK: Well, the GRANTS mechanism, as you know, takes a lot of paperwork and takes site visits, and formal reports, and wrangling over the apportionment of a little bit of the pile of money, especially these days - although even in the early days, by peers of various kinds who volunteer to serve on such committees. And, together with

standing committees at a slightly higher level, finally make the awards. And then, if you don't really blow it badly in the course of your one-, two-, three-, four- or five-year, seven-year award (whatever it turns out to be), then the renewal can be obtained by pretty much the same process. And if you don't do as well as expected, or if you don't, on consecutive grant periods, do as well as you are expected to do, why then, that money dries up, as it rightly should. Other than that, of course, then, you had this fairly loosely-defined body of work that you undertake to do under the terms of grant. And if you do a job well, even if it's not exactly what you described, then you will continue to be blessed. On the contract mechanism it is quite different. You specify what it is you are going to do and there are deliverables that you must send to your sponsor. The macromodular program part that was funded by ARPA took a considerable deliverable, the final report. It was written by the laboratory under Charlie's direction after I left in 1972. And now it occupies, well, at least three or four of those blue boxes up there - a fairly substantial report on the macromodular program over the entire period of the contract. That would also have been sent to NIH, I am sure. That was... not solely for ARPA. But IPT's policy was not to specify contract tasks so specifically, in such detail, that they could not be discharged by lots of means. Essentially, they were funding research with fairly loosely defined objectives. And the idea was to help them, whenever they needed help, to justify the work you were doing with respect to their sponsors in turn, the Department of Defense in general. And that became more of a problem after Ivan, approximately after the first few turns of operation at IPT. That became more of a problem... That is, it became necessary to justify things to the military a little bit more specifically than theretofore had been the case. And in fact that has taken another wrench or two up over the years since. So that now, very goal-directed work, and only that, apparently the stuff that is sponsored by IPT. I have lost track of exactly how IPT does its thing since I haven't used that resource for ten years, or whatever.

O'NEILL: Okay. I would like to go back now and talk about something you mentioned in passing, which is the group that you become part of as an ARPA contractor, and the principle investigator meetings. Do you remember when you first went to a principle investigator meeting?

CLARK: No. I would have gone to the first one after the establishment of a contract in 1964, 1965 - approximately one of those dates. Probably 1965.

O'NEILL: The 1967 principle investigator meeting is the one I would like to focus on. There was, at that meeting, a

discussion of networking.

CLARK: Yes. Well, I am not sure of the date, but if you say that is the meeting... Do you remember where it was?

O'NEILL: Michigan.

CLARK: Michigan, okay.

O'NEILL: Does that sound right?

CLARK: I think so. That is, I remember a network meeting in Michigan but I don't remember the year. The meeting was pretty much run by Larry, although Bob Taylor was then Larry's boss, as I recall.

O'NEILL: Had you been asked about the networking plan prior to that meeting?

CLARK: Yes, I think so. In fact, before Bob Taylor took over from Ivan after he worked as an assistant to him, you know how that organization paired its way up. Ivan asked me, or pointed out to me, that it would be important to start thinking about networks.

O'NEILL: Ivan Sutherland?

CLARK: Yes. The implications of networking and so forth, because he could see some of the requirements in the military coming down the pike - long-term, having to do with networking. And I told him that I didn't think it would be very powerful stuff until they had a much wider band than was being talked about. That is about all I remember of that conversation with Ivan. But then, the networking stuff prior to that meeting was subject to much conversation in various places. Don't forget, there had been two or three preliminary meetings, or committee meetings, or something, in preparation for that contractor's meeting. Perhaps there had been an organization... not an organization, but a group of people who met to discuss networking - folks like Len Kleinrock possibly (although I am not sure), but others who were interested in seeing networking happen, or who had been turned on to it by Larry

already, individually, before a coherent program had formed and things of that sort.

O'NEILL: Were you part of that group?

CLARK: I think I may have gone to a meeting or so; I'm not sure. I do remember having seen... By and large, no, I didn't; I wasn't part of that group, because I wasn't very much interested in networks. I do remember (I think it was about that time) seeing some report stuff from various people that were beginning to appear, people with various observations, or something like that.

O'NEILL: I was under the impression that those all started after the PI meeting.

CLARK: All of the reports?

O'NEILL: Well, that is what I am trying to find out, to get the whole chronology straight.

CLARK: Oh, I see. Well, I can't give you any hard facts and figures because it's only an impression that I have. that there was talk about that stuff prior... No, that meeting did not kick off the network work at ARPA.

O'NEILL: Well, no, Roberts was discussing it with the host community at that point and had talked about it previously, I guess. I just wasn't aware that there had been any set meetings or a lot of work prior to that.

CLARK: Well, Larry, when he was about to take over from Bob, asked me what I thought he should do about the TX-2. What should he turn it to, and he named some possibilities. TX-2 had been involved in some network studies at that point - network experiments. And Danny Cohen... Is he on your list of people to talk to? Danny Cohen.

O'NEILL: No.

CLARK: Oh my goodness, put him on your list! Unfortunately, he won't be able to say much about ARPA, I think, from that subject period, but you must talk to Danny Cohen, who was at the time, as my friend Chuck Sites said later,

the brightest guy in Cambridge. He had tied together a number of computers, more or less in passing, while he was at Harvard, and got them all to operate as a small network, in some way, very limited, to be sure, but quite a tour de force.

O'NEILL: Do you know when, approximately?

CLARK: You know... Very early 1960s, I would have to say. No, mid-1960s.

O'NEILL: Okay. I have not see anything about this at all.

CLARK: No, I think it was under Portage (?), but not under March (?). There had been experiments tying the TX-2 to... I think that's right. Let me see, was the TX-2 part of the network in L.A, or...?

O'NEILL: There had been an experiment between the Q-32 out of SDC and the TX-2...

CLARK: And the TX-2, okay.

O'NEILL: That Tom Merrill and Larry Roberts wrote up.

CLARK: Okay, that's right. Well, yes, I am sure that it is a case that after that, Michigan meeting... if it was '67 or so... after 1967 there were various specific committees looking at different things like network protocols, or network connectivity, which interested Kleinrock, and things of that sort.

O'NEILL: You mentioned before that Roberts talked to you about what to do with the TX-2.

CLARK: With the TX-2. Yes, I am sorry, because he said networking was one possibility, graphics was another. I recommended strongly that they continue the graphics program support at MIT because the machine was uniquely qualified for that at the time. There weren't any other machines around that could come close. And I don't remember what he finally did about it. And there was another project in speech research - one that defined a goal that turned

out to be unbeatable - but a number of six or seven of their projects around the country were funded under a common program - no doubt, again, as part of IPT's interpretation of the military requirement, of some kind, put to them. And Lincoln had a program in speech research, with Jim and Karma Forgie and others. The network was part of the program. I think BBN may have had some of that; I am not sure. I don't remember where it was, but in any case, yes, I recommend it to Larry - strongly recommended - the graphics stuff. I did not play any particular role in the early planning in networking. I mean the early enthusiasms, because I just wasn't enthusiastic about it.

O'NEILL: Can you tell me what you remember from the Michigan meeting?

CLARK: Oh, yes, I mostly heard all of these people talking about the various problems, and maybe for the first time and not with that much zeal, and was quiet for most of the meeting, if not all of the meeting. But toward the end, just before we broke up, I do remember suddenly realizing what the meta-problem was. They hadn't quite realized what they had, all of the proponents of the network. And so, I must have lit up in some way that caught Larry's attention. No, I passed him a note; that's what I did, I passed him a note saying that I thought I saw how to solve the problem. So they collared me once we left the meeting and wanted to hear about it. I just suddenly realized the fairly obvious thing that they had an n -squared (n^2) interaction problem within computer nodes, and that that was the wrong way to go about it. It would be hard to fund and control, and everything else. And so, the idea was to simply define the network to be something self-contained without those n nodes... without those n ARPA-supported big computers, they had a number of PDP-10s, as I remember. I think that was PDP-10s time, I am not sure. But, in any case, leave them out; put them outside the network. They weren't part of the network, the network was everything from there in, and that should be around a single, common message-handling instrument of some kind, so that the design of that instrument and all of the lines were under central control - ARPA central control. They could fund it, get projects started to design the parts, define its finest characteristics and so forth. You had from n to only one interactions, translations, or protocol translations, or whatever to get on the network, and one more to get off, instead of n -squared. Because they were all talking different requirements, and timings, and concerns and so forth.

O'NEILL: So they were each talking about their own specific computer and how it would interconnect.

CLARK: Yes, the TX-2 was going to talk to whatever in California, SDS. I forget where all the other machines were,

they were all supposed to be tied in with various network connectivities. And that was a subject of great interest - exactly what their network looked like. The machines were not all the same, and certainly their operating systems were not the same. Their set of needs were not the same. And so, things were... they were more different than they were alike; that's all. So you had all of these things talking potentially to any other, n-squared interactions, or n-squared over two, whatever these kinds of protocol designs and administrative designs and God knows what else.

O'NEILL: What was Roberts' response to your suggestion?

CLARK: Oh, he thought that was nice; he thought that was a good idea, as I remember. It was in a taxi ride to the airport, and I think Al Blue was in that ride to the airport, and Bob Taylor, and Dave Evans I know was there, because I can hear Dave saying, "Yes, I guess that's right." (laugh) Maybe when Dave was saying (?) Larry thought it was a good idea; I don't know. But it seemed to be well-received. Look, how could it not be? It was a natural thing. They just hadn't seen it yet. That's all, they would have with a little more struggle, I am sure.

O'NEILL: So, it hadn't actually been discussed at the meeting.

CLARK: No, it was not discussed in the meeting at all. The appreciation that this was what the problem was didn't occur to me until just before the meeting was breaking up, so I didn't want to keep it going, I wanted to go home.

O'NEILL: (Laugh) How long did these meetings last?

CLARK: Oh, they were a couple-day meetings. That one, I think, was a shorter one.

[INTERRUPTION]

CLARK: Yes, but in fact that one may have been only a one-day meeting.

O'NEILL: Okay. Was it a normal meeting in the sense that did it have all the PIs talking about what they were each doing...?

CLARK: It was a focused meeting. It was focused on the network question specifically.

O'NEILL: Okay, and was that normal?

CLARK: No. I think that was not typical. There may have been another one or so, but that was not typical.

O'NEILL: Okay, so it was not necessarily unheard of, but it wasn't...

CLARK: No, there were mostly reports, main work and progress around the country among the members of the stable, and some reports of activities to come on ARPA's part - that kind of thing. And fun and games.

O'NEILL: So they were enjoyable as well.

CLARK: Yes, they were good meetings.

O'NEILL: About how many people are involved at this point, as PIs?

CLARK: 30.

O'NEILL: About 30, okay. You mentioned that you listened to various people talk about networking considerations at this meeting.

CLARK: Half-listened; I was kind of bored.

O'NEILL: (Laugh) Okay. How many people were involved in talking about networking? I am wondering how many were interested?

CLARK: About half a dozen... probably half a dozen principles, not more were the main contributors. It seemed to be

fairly well focused stuff among about a half a dozen people, but with many others having other things to say (that's my memory). I don't think there were 30 at that meeting, by the way. That may have been a smaller meeting. But the typical meeting was I would guess about that size - 20 to 30, in that order.

O'NEILL: Do you recall your reaction? Well, I guess that you have said your reaction was somewhat bored...

CLARK: Well, I was kind of bored. I wasn't interested in that stuff so much, and the details they were going into were a bit abstruse, and some of them were the result of protocol concerns that they had sort of made for themselves. And I just didn't follow it all.

O'NEILL: Okay. Other than the half-dozen or so who were presenting that information, could you characterize the reaction from the rest of the people?

CLARK: Reaction to what?

O'NEILL: To the network proposal that was being put forth.

CLARK: I am not sure that was... I think... it wasn't proposed at that meeting. Everyone...

O'NEILL: So everyone pretty much knew about it. It wasn't being presented to anyone...

CLARK: The ones who were already thinking in terms of networking were thinking along network experimental lines, I am sure had already been in touch with Larry and vice versa. I don't think this was laid out in the cold. What was put on was the fact of focus, that this was going to be a big new effort under Larry's direction, which is why Bob let him have the floor pretty much for that meeting, for the part of it I remember anyway. No doubt there were other general interest topics being discussed at that meeting that had been discussed earlier but the last thing, and the major thing on the agenda was the network stuff. But I think what we were getting was the announcement that the plan was that this was going to be formally... this was going to be certified as a formal program, and so on, and that they were really going to do big stuff, and so forth. And somebody would say, "Well, that is going to mean that you

will have to buy a bunch of something or others." And Bob Taylor would say, "Well, let's see, I guess we can just to that out of something-or-other program funds." And he would turn to Al Blue and say, "Can we do that, Al?" And Al would think for a half a second and say, "Yes, we can do that."

O'NEILL: Okay, well, that gives me a better idea of what was going on at that meeting. I haven't been able to find anything... any writeup, you know, after the fact, of what went on at the meeting, nor have I talked to people who were actually there.

CLARK: Well, we can name some of them, can't we?

O'NEILL: Well, Roberts and Taylor and Blue.

CLARK: Sure, and Evans; I know he was there.

O'NEILL: Okay. Frank Westervelt, I am assuming was there, since he was at Michigan at the time.

CLARK: Sure, the Michigan folk would have been there. The MIT folk would have been there, but in the form of who I don't recall. But it's easy to get... I am sure you can get the names because of the listing... through the listing of people who are being supported by IPT in 1967. The principle investigators of 1967 were all supposed to come to this meeting, if I understand this stuff.

O'NEILL: Assuming that they all came.

CLARK: Well, but you could get the names - the shopping list (laugh), anyway. I suspect Len was there, as well.

O'NEILL: No.

CLARK: He wasn't at that meeting? Okay. I just don't remember who else was there. So they subsequently... They thought it was a good idea in the cab, and the next thing I heard about it was something about requests for proposals

on the various elements. Oh, I know, they had to build this... what turned out to be named the Interface Message Processor (I don't know who named it) - IMP. That was the little nodal piece that was really part of the network to which the big local computer tied.

O'NEILL: Okay, that wasn't a term that you used, then?

CLARK: No. I do remember visiting ARPA at IPT in Washington just before they were going off to evaluate the proposals that had been received. Oh, by the way, Larry, I think, had asked me later... not during the cab ride (I am really not sure; it could have been even then). You see, I focus on the cab ride because that's really about the only little interval of time during which I had any focus on this problem. But I remember Larry asking me who could do something like that - design a network like that and get it built. And I said, "There's only one person in the country who can do that," who I named; it was Frank Heart. And I did so because Frank and I... Frank is one of my oldest and dearest friends. But that wasn't the reason, of course. Frank is just a superb system engineer who ran a very strong group at Bolt, Beranek and Newman. Frank was the fellow who as much as any other single person taught me programming when I first went to WHIRLWIND back in 1951. He worked in another part of Lincoln Laboratory during the time I was working at the Advanced Development Group. And he left Lincoln Laboratory to go to BBN and set up their computer operations. But Frank, I knew, could do that job well. He and I had done a joint consulting task for Lincoln at Lockheed once. We were sent out there for two or three weeks to help Lockheed out of a bind - sort of flag-wavers from Lincoln Laboratory but in fact we did have some technical input to an important military satellite program that they were at some impasse on... Frank was just a super engineer and I knew he was exactly the person who could do this sort of thing. He had just done networking stuff in connection with Mass General Hospital and BBN, which, through Jordan Beruch had a contract with MGH... no, I am sorry, a program with MGH sponsored by NIH to do. And it was such ? ? mention that BBN got out of the operation of that thing. So he was the right man.

[INTERRUPTION]

CLARK: Well, Frank was the right man. And then Larry of course said, "Well, okay. It doesn't matter. We really have to do requests for proposals formally," - as Al Blue pointed out. And that's what they did. So the various

people made proposals. I don't know who proposed, but the BBN group certainly did. And I went down to... I just happened to be in Washington visiting then... and the point at which they were preparing to go off and evaluate the proposals. They were going off to California, I think. It was in connection with something else; I don't think it was solely to evaluate the proposals. They probably had something else to do as well. But there was Al Blue stacking up proposals and measuring them with a ruler. And I thought this was really a very imaginative way to evaluate proposals (laugh). But he pointed out he was just moving them for packing cases, because they were going out to California to read them over. And BBN of course did get the proposal. They were indeed the right group to get the contract.

O'NEILL: I am a little confused on why they had to put out an RFQ when that wasn't their normal way of doing business.

CLARK: RFQ, RFP, RFQ, yes. Because, I don't know, there was a specific contract deliverable for them, they wanted to have a network built for them - a thing for them, ARPA.

O'NEILL: Okay.

CLARK: Yes, it wasn't going to be something done at a university, owned by a university, or...? ? ? in actuality, as most of those things were. This was going to be a centralized ARPA thing. It's that central control of the network that made the Arpanet work. And you have the evidence before you every time you try to send mail to anybody today, because we have all of these independent, non-standardized intercommunication structures in place, and nobody in control. And things are a mess. I refuse to use networks. Maxine uses the networks for me. Her office is on the other side of the bathtub, on the ? from the ? . ? bathtub (laugh). We have the only Ethernet in the world with a bathtub on it. Her machine talks to mine. But I do not talk to the world; her machine does.

O'NEILL: And why do you not talk to the world?

CLARK: Because I think networks are a mistake. They don't work.

O'NEILL: And they don't work because...

CLARK: Have you looked at a message? I'm talking about mail, not networking in general. But it turns out that, although they foresaw it would be an important component, they had no appreciation at the time of how big a component it was going to be. If you look at the network usages now they are predominantly mail, electronic mail, for which the Arpanet of course immediately turned out to be an enormous overkill. Nonetheless, it did work for mail; it worked well. You had a central directory ? ? , a central responsibility for keeping it all honest, and a network control center run from BBN for many years. But we presently have a babble and no central responsibility, no one to complain to, and messages that get lost, and things that take forever to get from... Well, it doesn't matter how far apart. Things should not take forever in the electronic world, and yet they do. And they scramble and break, and the headers alone quite often exceed the messages. It's a mess. And also, people do nasty things on networks, as you know. I just prefer to stay away from it altogether. So my partner does that. She likes networks a lot.

O'NEILL: It is very rare to hear anybody say anything against networks these days. Do you know if Roberts ever did anything to follow up on your suggestion of Frank Heart, or was it immediately clear that they were going to have to do an RFQ anyway?

CLARK: Oh, I am sure they had to do the RFQ, but I don't know - you could ask Frank; ask Larry.

O'NEILL: In terms of the evaluation committee (obviously you didn't have any direct connection with them), do you know who was doing that besides Al Blue and Larry Roberts?

CLARK: They would surely know.

TAPE 2/SIDE 2

O'NEILL: You had no further connection with the network after that, is that true?

CLARK: That's true. In fact, there is an interesting sidelight here. When they were trying to build the network and they decided there ought to be a node in St. Louis. That's right in the middle of the country - northern, western, eastern middle. That seemed like a logical place for a node on the network. And I said no. First of all, we didn't have any big machine resource to put on the network that all the other people were playing with big resource machines - Project MAC and PDP-10s, things of that sort - big for the time. And I was running the macromodule systems program at that time. We didn't have big machines. We designed machines one day and tore them apart the next, sometimes within the hour. That's what macromodules were for. We didn't have a big thing like that. So that was one main reason. And then later on the ARPA group (Would it have been Larry? I'm not sure) wanted me to establish a central resource for the network - some focused... not a flagship resource, but a centralized resource that would do service functions for the network, which turned out to go to Santa Monica instead, to Keith Uncapher's group at ISI which was set up in Santa Monica. Maybe it was again because that seemed to be a nice central point, but it wasn't on the network. It also may have been at the time they wanted me to get on the network in the first place. "Get on the network and build yourself, or require ? , or we'll give you a central resource, or something like that. That was so distorted from the program that I was trying to run there at the time that I did not encourage them to pursue that in any way.

O'NEILL: I see. So, primarily because it didn't fit in with what you were doing - not for any...

CLARK: And because I just didn't want to play that game; that's all. ? ? system on the same side of that coin perhaps. I don't know whether that was a good thing for Washington University or not. It simply put them into a different world than it would have been otherwise, although they have now... involved in an excellent, most interesting computer picture. That's not something that you can deal with in the way of a networked intercommunication resource as visualized by ARPA at the time, although even what they have, it doesn't work that way, mostly as a rule it works to send the mail messages, subsidized mail. If you look at pictures of the network, I have always observed that it never seems to go through St. Louis throughout any of its development phases.

O'NEILL: Okay. That covers the specific questions I wanted to ask. Do you have any general comments you would like to make about IPT, or...

CLARK: IPT? Oh, sure. Gosh! IPT was a marvelous institution. Was, I say. I doubt that it is anything like that now. My last contact with them in fact was some years ago when I took the Chinese keyboard down and demonstrated the results of my study undertaken nominally under ARPA sponsorship... under IPT sponsorship. And I took this demo down, because by then it had a number of Suns, Sun computers attached to it. And one of the staff at IPT set up this Sun for me. And so I took the stuff down, I guess the day before, checked it all out, brought it all back with me, and went down the next morning and gave a demonstration, which was a formal requirement under my contract to study transcription stuff - coding stuff. And I haven't been there since. And even then it wasn't the same place that it had been when I was under direct sponsorship in the big program. It was a fine, informal office that worked well because it had a pretty clear vision of what it was trying to do and pretty good support from its sponsors in the military. It had imaginative people like Lick and Ivan and Bob and Larry, for openers, and it had stability and know-how-to-doativity in the form of Al Blue and his group. It just worked well - very well indeed. And it's a shame we don't have a mechanism like that anymore, but then we also don't have the governmental money to devote anymore to efforts of that kind. It's too bad. Anyway, it was a fine, fine place.

O'NEILL: Okay, well thank you very much.

CLARK: Well, that was fun. We must do it again some time.

END OF INTERVIEW