Interview of Wesley (Wes) Clark

Interviewed by:
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James Pelkey: Thank you for your time. You were at Lincoln Labs when you first got involved in the whole idea of interactive computing and the kinds of things that have led to this need for data communications, it seems to me to have gotten started in that environment at Lincoln Labs and SAGE.

Wes Clark: It actually got started with Whirlwind a little earlier than that, but certainly the SAGE system—there were 2 parts, the Cape Cod System that made use of Whirlwind and stayed at Cambridge, as opposed to the SAGE system which developed at Lincoln Laboratory out in Lexington--made great use of that interactive stuff, but the interactive ideas really came out of Whirlwind and Whirlwind activities, some of which actually slightly pre-dated the Cape Cod stuff, which in turn preceded the SAGE stuff, or was kind of coincident with it.

Pelkey: Whirlwind and Cape Cod, those are new to me.

Clark: Ah. Whirlwind was the machine built by Jay Forrester at MIT under a Navy contract in the late '40s and into the '50s. When I joined Project Whirlwind in '51, the machine was beginning to work, although it had a very bad memory; but it had very good displays. It was one of the fastest machines that had been built. It was a vacuum tube machine, and in fact, the story of Whirlwind is quite well told by Redmond and Smith, whom you can look up -- the history of Whirlwind and the early SAGE and its relationship to SAGE and so forth. There's also a book on SAGE itself and its history, which I am sure you have seen, so Whirlwind would figure there. Whirlwind was the first machine fast enough so that you could do interactive display work in real time, fast enough so that you got your displays back while you still cared about them and were lively enough so that you could see things going on. That work did continue at Lincoln. It continued in the Advanced Development Group, as well as in the SAGE project. Lincoln had this Advanced Development Group, of which I was a member, along with some others -- Ken Olson, the most famous of them -- related to SAGE, but as a peripheral activity, an advisor to the rest of the SAGE activity at Lincoln Laboratory, but kind of off to the side pushing new technologies, so we developed transistor circuits and new architectures for machines, and explored new applications and so forth. It was in that context that the interactive stuff really got going, I think. Interactive work was also going on in the MTC, Memory Test Computer, which was done in about ’53-'54, as I recall. It was a machine built to try out the magnetic core memory idea that Jay Forrester had, and around which the whole industry turned for the next couple of decades. That machine was a small machine, vacuum tube gadget, with a magnetic core memory that had been put together by Papian, whose name you will come across in the literature, and the machine itself designed -- sorry, engineered -- and put together by Ken Olson. That was a machine that, from the user's point of view, looked like a PC. It had a keyboard and a display, and it was also very fast. It tried the memory; memory worked just beautifully, much better than the bad memory Whirlwind was operating with, so of course, immediately it was taken over by Whirlwind -- the memory -- and new ones were built for MTC. Larger ones were built for MTC, and MTC then continued to operate -- subsequently moved out to Lexington, as a matter of fact, for a number of years, and that was a very interactive machine. So that explains the interactive part. Meanwhile, in the Advanced Development Group, there was the TX-0 and the TX-2, which were fairly big computers. Sorry, the TX-0 was a very small computer in architectural size and power. It was a very primitive machine; probably one of the first RISC machines, I guess we'd call it now -- very primitive.

Pelkey: What year was the TX-0?

Clark: The TX-0 was about 1956, '55.

Pelkey: So that was funded pre-ARPA?

Clark: Yes, ARPA was not in the picture.

Pelkey: ARPA was founded in '58 or so.

Clark: Right. Then the TX-2 was a very ambitious machine. It took a large number of years to build, only a few compared to the other things we had done. It took a long time. Anyway, TX-0 and TX-2 were engineered, that is the circuits and the general engineering, under Ken's direction, under Ken Olson's

direction, and I was responsible for the architecture and the design of those things. In fact, I proposed both of those machines. So those machines were built, and they were very interactive gadgets. It was on the TX-2 that Larry did his dissertation work, for example, and others -- Ivan Sutherland.

Pelkey: As I understand, Larry and Ivan timeshared it at night while they were working on their dissertations.

Clark: They may have done that. That would have been after I had left Lincoln Laboratories, but I'm not sure. Some of that history has been recounted in a recent paper, by the way, which is about to be published in The Proceedings, in book form, of the conference held out in Palo Alto about two years ago called "History of Personal Workstations."

Pelkey: Gordon Bell showed it to me.

Clark: Ok, so I have a paper in there that sort of ties this TX-0, TX-2, LINC, and the MTC stuff together. I mentioned LINC. That was the last thing that I did at MIT, starting with a small group of -- what should we call it -- a small computer with only those things in it that you needed and nothing more -- for complete application in the laboratory. That machine was then made in some quantity and was apparently influential. So that's the interactive story. Now, Larry did some network stuff -- oh, I was mentioning Danny Cohen. I think -- I don't have the time exactly right, but I learned this from Chuck Seitz, who is at Cal Tech at present, and he was at MIT at about the time that Danny Cohen was at Harvard completing his degree, and Danny, as far as I know, may have been one of the first to tie a number of computers together with telephone line hook-ups and use them simultaneously, so he is well worth talking with. You'll find him at the system development concern at the University of Southern California computer activity at Santa Monica.

Pelkey: At ISI?

Clark: At ISI. Right. A marvelous fellow, by the way. So that's the interactive story. I don't know what more you want to spend time on.

Pelkey: Was it natural at that point in time to be thinking about interactive computing?

Clark: It was natural for us in our group. We came out of the Whirlwind environment in which hands on use of the computers was the style. Of course, I fostered and pushed that for work on the TX-0 and the TX-2, and LINC was designed specifically for that kind of use, and made small enough, as a computer box, that it could go into somebody's own room, or his laboratory, or his house in some cases, and be used as a personal computer.

Pelkey: Now, at that point in time, as you recall, was the concept of tying computers at different locations together.

Clark: Sure. The SAGE did that. SAGE had to do that in order to tie the preprocessing computers out at radar sites to the central machine and, later, to tie SAGE sites to one another. Actually the SAGE system development would have been the first remote -- about 13 or so remote radars around the New England area tied together by phone line. In fact, the group that became the Data Transmissions Group, or some name to that effect, at Lincoln Laboratory under Harrington and others, really had to build the first digital modems to do that job. So they are important figures as well.

Pelkey: So they built modems themselves, or did they --

Clark: Yes. There were no modems in those days.

Pelkey: Right, although --

Clark: They had to develop the techniques and to find out how bad the telephone lines were -- and they were very bad -- and learn about transducers and companders and things of that sort that are all these gadgets in the telephone system that mess up digital signals.

Pelkey: My understanding is that the government turned to AT&T during this period of time --

Clark: I don't know anything about that history, but I'm sure they got involved because of the SAGE work, if for no other reason that they would had to have been brought in.

Pelkey: But SAGE was the place that modem technology was developed, as was multiplexing? They did frequency division multiplexing, if I understand correctly, as well.

Clark: Well, yes, but I'd be very surprised not to find multiplexing in other developments, or in part of SAGE, not necessarily at Lincoln Laboratory. For example, the Air Force Cambridge Research Laboratory sponsored work of that sort I'm sure, Rolm Air Development Center. Probably there was stuff going on in England as well, coming out of experience in War II.

Pelkey: At some level, is it correct to say that a lot of this was just really -- not aftermath but -- the continuation of the progress that was made during World War II in theories of information technology? The need for computers had been known before World War II; but it really got accelerated by World War II in terms of the number of people that came in contact with the ideas and the imperative to press forward.

Clark: Certainly, but you put World War II into the history that the computer development is built upon, sure. I'm sure you do that. How much momentum is carried forward, I don't know, although the Rad Lab, the famous MIT Radiation Laboratory, which was set up to --

Pelkey: Vannevar Bush.

Clark: Right. That continued to function after War II into the early '50s, and then the technical staff there, such as remained there at that point, was pretty much absorbed to become half of the Lincoln Laboratory. It became the communications group -- communications groups and I think there were physicist in those groups -- and other technical components of Lincoln Laboratory. The Radiation Laboratory, together with the Whirlwind computer laboratory of Forrester's, really became the two major components of the Lincoln Laboratory, so in that sense --

Pelkey: So the Lincoln Laboratory was really pulled together to do SAGE, if I understand correctly.

Clark: That's right.

Pelkey: Then later, SDC was developed to do the software, because in fact, people came to realize that was a much larger project than originally envisioned.

Clark: Yeah, I think there were a couple of spin-offs. First they started to work with Rand, and found the problem was even bigger than that, and they set up SDC. The problem was even bigger than that, and it took a long time. It was very hard. The work of Forrester's didn't necessarily come out of War II, in the sense that your suggesting it might have. Forrester was -- when I joined the Whirlwind Project, the thrust was on air traffic control as the motivating force for pushing high-speed computer development, which was what the laboratory was established to do. I don't think that went very well. I don't think enough support was there for air traffic control or airport management, that kind of thing, but it was the only other thing after simulations that they could think of to do with Whirlwind, and it was in that early period -- about '52 or so -- that they possibility of putting radars and computers together to form an air defense system came up. The need for that wouldn't have arisen without War II and the Russian involvement and so on.

Pelkey: It was the emerging aviation industry in this country -- the need for radar up there at commercial sites. At some level, the government turned to AT&T to build modems for the commercial side -- for the domestic aviation system -- if I have my facts correctly. At that point in time it was called telemetry.

Clark: Well, I'm really not that familiar with how modems -- what the thread of modem development --

Pelkey: I understand, and that's not the purpose of this conversation. So you knew Larry and Ivan a little bit when they were doing their graduate work.

Clark: Sure. Larry was a graduate student, came to my group at Lincoln as an undergrad in fact. We had a number of graduate students from MIT in that group; in fact, some undergraduates as well, and Ken and I both had courtesy appointments as lecturers in electrical engineering at MIT so that we could supervise the students officially. So students came, and they worked in the group. Larry was among them, many others. Ivan came along. Larry did his masters thesis first on the TX-2. He did some work on image analysis; he was interested in that.

Pelkey: In fact, I understand from Leonard Kleinrock, Leonard told me a great story about all three of them demonstrating their thesis on the same day.

Clark: I love that.

Pelkey: Very nice story.

Clark: My main problem in running the TX-2 was to keep the group small enough so that I could preserve this hands-on interactivity kind of stuff, because the pressures were considerable to use that big resource for a variety of small problems in the usual way, and I didn't want to see that happen; so I had a scheme whereby people could sign up for periods, on the order of hours, a week in advance or a day in advance or something like that, and just sort of fill in the schedule which was posted on the board, and they could fill it in themselves and claim this and that. There were a few reserved times for maintenance and a few other kinds of things, and the unavoidable shut-downs which occurred from time to time -- hardware development and check-out, because it was a developmental machine and we had to try new things, but it worked pretty well. We had some major activities there. One was a neural network modeling, which was an area that I was interested in and worked with Belmont Farley on. Another was in speech recognition, which was work under Forgie, and that's how I think Licklider became interested in the TX-2 in particular.

Pelkey: Was that post his going to Washington?

Clark: Pre. Yes, he had wandered down the hall to the TX-2. The TX-2 was in the basement of Building A in the Lincoln Laboratory, and I had wandered down the hall farther to the end, and there was another laboratory around the corner there, and behind some equipment I found this fellow, and I introduced myself, and that turned out to be Lick, and he was clearly a very interesting guy. He was doing some psychophysical experiments. He's a psychologist, as you know -- or he was a practicing psychologist in those days. I don't know how he considers himself these days. So we got to talking and explaining to one another what we were up to. I invited him to come down to the TX-2 and learn how to program it, so he ended up doing that.

Pelkey: This was before he went to BBN and all that.

Clark: Yes, long before that.

Pelkey: So your relationship with Lick goes back some years then.

Clark: Oh, yeah, a long time -- '54, '5, something. Does that bring us up to some --?

Pelkey: Then you left before Larry and Ivan and Leonard --

Clark: Well, Ivan came in and started his work. I was delighted to see him start that. I felt --

Pelkey: This is the Sketchpad work.

Clark: Sketchpad work, yes. I felt this was the man I designed the machine for in the first place. He was right --

Pelkey: It must have been a very enjoyable experience to see someone who had the vision and energy and competence to be able to do something like that.

Clark: Sure. I didn't get to watch him very much, but it was indeed, and I did come back later and see what he had ended up with, but that was after I had left. (He got me in one evening for a show and tell.) I did leave. I took my group -- a piece of my group -- that had been doing the LINC development out of Lincoln Laboratory and moved it to another pro tem group in Cambridge to finish the development and get an evaluation program -- a national evaluation program -- going (that story is told in the History of Personal Workstations) and Ivan was still working, at that point, back at Lincoln Laboratory. Larry was still there as well. Len, I think, had come and gone.

Pelkey: He just stayed shortly, and then went to UCLA. You stayed in touch with Lick when he went to IPTO.

Clark: Sure, well, yes, and at BBN too. I think he was at BBN in the meantime.

Pelkey: He was before. That's right, he went from BBN to IPTO.

Clark: The reason I say Lick was interested in getting the group (the LINC group), when we left Lincoln Laboratory, in the possibility -- interested in getting us interested in BBN. He was there, and we explored a number of places in the local area to move the group too, and ended up back at MIT in another laboratory more suitable for completing that work. Lick and I had been in various little workshops organized around what we now call the Artificial Intelligence theme for a couple of years. At about the time Lincoln was being established, Farley and I participated in a series of informal evening discussion-group meetings at a Lincoln site with Lick, Fred Webster, Oliver Selfridge, Bill Huggins, Gerry Miller, Nathaniel Sage… on the subject of Informational Theory in relation to learning machines, epistemology, very preliminary “AI”.

Pelkey: During the period of about '60 to '64, the concept of linking computers together and the issue of what had become data communications -- the Arpanet and so on --

Clark: Well, the first idea was timesharing. The first thrust was toward timesharing, and MIT became very excited by the possibility of using big machines interactively. They couldn't do that interactive work. We could do that at Lincoln. They were, I think, sort of jealous of that facility, but it meant that you had to have a small group of people working with the machine. You couldn't spread that around too much. They liked that interactive stuff, and the timesharing, which was a SAGE notion that came out of Whirlwind, seemed to be a good thing to do, so Lick, who was then at ARPA, pushed MIT to think this through. MIT set up a study committee. That's the usual university response to something like this. You set up a committee, and everybody with the least interest in computers or using them got on this committee. So we met for a few times and it was clear to me from the start that the purpose of the committee was to put its blessing on the timesharing idea, but I didn't think it was a very good idea. Fredkin, by the way, had also been at Lincoln Laboratory briefly. He worked with Frank Heart's group, and on his way out, through to BBN, I believe he -- did he go to BBN? I'm not sure. I think he went directly to his company that he was setting up in LA, but you can check this with Frank.

Pelkey: I'm not sure. I will.

Clark: But he had proposed to me, just before leaving Lincoln, that we turn the TX-2 into a timesharing machine, and I didn't like that idea at all. I thought that that would immediately mean you couldn't use the displays interactively, and do stuff that involved really wide band communication with the user. I was much more interested in seeing how far you could go with all the power at the disposal of any one person at a time, and timesharing is just too costly to do that.

Pelkey: That involved only terminals at that point in time, because of communications linkage?

Clark: Yeah, although Fredkin, who is an enormously imaginative guy, may have had some wider band timesharing in mind. We didn't discuss it any further. I think he was disgusted with me when I said no, and then left. So, yes, Lick went off to ARPA -- set up ARPA -- set up the IPT.

Pelkey: This process -- this MIT committee -- that eventually became Project MAC.

Clark: Yes it did.

Pelkey: Who else was involved at that point in time?

Clark: Oh, gosh, it was a very large committee, and there is an official report, which you can see. The chairman of the committee was named Herb Teager, “was”, I say, because I think he has since died. He left MIT after a while and went to, I think to manage something in one of the other universities in the Boston area, although I'm not sure of that. Herb Teager was the official chair. I was a member of that committee, of course. I think Fernando Corbató was a prominent figure on the committee; I don't know if Fano was involved at that point or not. Fano was sort of a late-comer to the computer game because he had so much going in the information theory business, and he may have been a member of that committee, though I kind of doubt it. I really can't recall all the other folks, but the committee was certainly on the order of 20 people, so there were a lot of them in there, and you'll have to check that out. Frank Verzuh would have been another figure at MIT.

Pelkey: That must have been a very exciting period of time at MIT. A lot of ideas were starting to come forth, and people starting to see things relative to what was happening at Lincoln Labs and where computing could go.

Clark: Well, it was exciting at Lincoln, but at MIT I'm not sure, because I didn't have all that much contact with the general computer activities down there. There weren't so many activities down there. That stuff pretty much moved out to Lincoln, so at MIT you had people who were becoming increasingly frustrated using the central facilities, which were sort of these sealed-in, strictly hands-off, mostly batch-processing kinds of things, and long turn-around times to get work done; definitely not interactive, and exploring mathematical aspects of computers. Other kinds of things going on at that time, I'm really not sure about, so you'll have to check that out with people who were on the campus during that period. Whether there was a lot of excitement there or not I don't know. There certainly was following the push to move toward timesharing, and the resulting Project MAC.

Pelkey: At some level, those conversations were seminal conversations, in terms of what happened later, in terms of getting MIT into timesharing with Project MAC --

Clark: The Long Range Computation Study Group conversations you're referring to?

Pelkey: Right.

Clark: Yeah, I think they were, although I always had the impression when I went into those meetings, that all of the other people were in pretty good contact with one another, being on the campus, instead of out in the sticks, and that a lot of conversation had been going around informally before these formal meetings. So I don't know what you'd find in the report, but you might want to see it. By the way, as I reported somewhere, both Teager and I declined to sign the final report. We're the only ones who didn't do so. Lick was delighted with the report, and then money appeared, and MAC got started.

Pelkey: And then he goes to IPTO.

Clark: Lick set it up.

Pelkey: Yes, he set it up, and he goes to IPTO shortly thereafter.

Clark: I think in those days it was something called IPT, and it was simply called ARPA.

Pelkey: It was ARPA IPT.

Clark: I think that's right.

Pelkey: And then, as things progressed, he left, and then Taylor came in. Sutherland came in for a short stint, and then Taylor came in, and you were still in touch with these people. This was a relatively small community. You all knew each other and had come in contact with each other, although Bob was relatively new to the group, having come out of NASA.

Clark: Right, but we became pretty good friends in a hurry.

Pelkey: Yes, I'm sure you did. Then Bob had this notion that he came up with of linking -- taking what he refers to as these timesharing communities and building a larger community from them, both for economic reasons -- there was more and more pressure to get resources and it was another big project that would have demonstrated --

Clark: You'll also want to talk to Ivan about the timing or the sequencing there, because I seem to recall -- I was in St. Louis at that point in time (’64 – ’65) -- that Ivan approached me, suggesting that communication networks were going to become important, and what did I think about networking the stuff. I told him that I didn't think that really great stuff could be done until the bandwidths were wide enough, great enough, because I didn't see how you could get interactive -- my old argument -- that you couldn't get enough interactivity with remote machinery. Then I didn't hear any more from him, but I had the impression from him that -- when he told me this I don't know -- that it was coming down the pike, and I think that was before I heard anything about this from Bob. So it may be that there was that going on down at ARPA, and Bob took that to its next logical extension and developed these ideas about interactivity among shared resources on networks.

Pelkey: Do you recall conversations with Bob around these issues as well?

Clark: I don't recall any, I think. Let me see. I really don't. I don't know how that developed. I know that we had these Principal Investigator's meetings and it probably was at one of those that the push towards some kind of networking first came up. Of course, I had been following what Larry was doing at MIT -- I mean with the TX-2.

Pelkey: You were familiar with those exercises?

Clark: Sure, I stayed in touch with that, but I wasn't familiar with it. I just was aware of it. I knew they were doing these - - doing communication across country, as I recall, and then I was told later about Danny Cohen's local stuff. I don't know exactly the details of that, but I think he had machines at Harvard, MIT and Lincoln --

Pelkey: I'll have to contact Danny, because that's new information to me. There was also an experiment that was going on under Gerald Estrin at UCLA to link 7940s with 360s. Were you familiar with that exercise?

Clark: No.

Pelkey: That was stillborn. Money was spent on it, but it never got anywhere. Do you remember anything specifically about this Michigan meeting? I understand that Bob had approached you beforehand to think about this concept of what the architecture of this network might be?

Clark: He may have. I don't really know. I think that I was simply at the meeting because -- was the PI meeting a general one, or did it have this specific topic?

Pelkey: I think it was the PI meeting in Ann Arbor.

Clark: Yes, but it could have had a theme of networking, because as I recall, that's about all that was discussed.

Pelkey: That I don't know. So your recollection is that the theme was networking.

Clark: Yes, pretty much. At least, that's the only thing I can recall having been talked about. It was very -- the discussions were lively and varied, and there was a lot of argument, and the problem was that as the session wore on, people were trying to have -- each of the investigators with his own big machine -- was trying to figure out how to get it to talk to all the other big machines --

Pelkey: Without having --

Clark: -- while at the same time pushing his own approach to the kind of communication language that you want to have, so there were N machines, and that meant N squared potential interactions. There weren't N different machine types. There were fewer than that number. There were only a few machine types around. PDP- 10s were just coming along, but people had other machinery, and they wanted to tie it together, and there was the problem of hooking all that stuff up. It was toward the end of the meeting that it became clear to me what the problem was that they were fighting, so I slipped a note up to Bob, who was chairing this -- it was Bob and Larry together, as I recall. I slipped a note up to him and said: "I see what to do." I don't think he looked at it until after the meeting broke up or something, but in any case, we then shared a cab back to the airport. Who was in that cab? In addition to the people you named, there was Dave Evans, who later formed a company with Ivan – “E&S”, Evans & Sutherland. I think also Al Blue was along on that ride. Now, Al Blue is a name you ought to know. One of the best things Licklider did when he first went to Washington -- as I recall it was one of the first things he did - - was to acquire Al Blue to work at . . .

Tape Side Ends

Clark: Al Blue was the administrative manager for the IPT.

Pelkey: And who brought him in?

Clark: I think it was Licklider who brought him, and he stayed through Ivan and Larry and Bob. He may have left it about the time Bob Kahn took over that operation, and retired, probably. I think he may -- I've lost track of him. I don't know where he is now, but Al Blue was a marvelous gentleman. He was the one who provided the continuity through director after director of the IPT, and a marvelous gentleman. He was on that ride, as I recall, and Evans and Bob and/or Larry.

Pelkey: I understand both of them.

Clark: Both of them and me. I don't recall whom else. I have a feeling there may have been somebody else, but it must have been a big cab or a limo or something. I can't quite picture this.

Pelkey: I can't either. You guys were really close, weren't you?

Clark: I remember riding in a station wagon once, on the tailgate practically, back from one of these meetings, though I think that wasn't the Michigan meeting. It was really crammed; but I think this was a cab ride. In any case, I just told them what I thought the idea was and --

Pelkey: Do you recall what prompted this idea?

Clark: Oh, yeah, I think it was fairly obvious. I think somebody else would have come to it with a little more time. I just happened to be the first, because these other people were so busy figuring out how to talk to one another on an N squared basis, and the idea was to interpose a small computer, just big enough for that job, to handle the message routing and all of the other stuff. Let those little computers talk to one another and serve as terminals to which the big machines would talk. In a sense, that took the big machines outside the network. The concept up to that point was that the network was the big machines plus all the interconnections, and my sudden realization at the meeting was that you wanted the machines outside the network, not inside it.

Pelkey: Could you elaborate a little bit?

Clark: I can draw you a picture. It's easy enough to do. [Drawing in the notebook].

Pelkey: It's obvious in retrospect, but at the point in time --

Clark: Ok --

Pelkey: That's the N squared problem.

Clark: Right. I'll put all the connections in, even though -- and this is page 'Clark' in your book. Alright, so there's big machines one, two, three and four, and the concept that was being discussed was the concept in which surrounding all of this stuff was the network, and this leads to N squared potential interactions. Well, you still have N squared interactions, but they're among potentially different machine types and languages and ways of doing things and everything else. Larry and Bob's job was going to be how to manage this thing and get all these people cooperating with one another and get the standards developed and all that other stuff. It looked like a very hard job, together with the technical problems of working out the channels themselves. So my understanding was this: that the big machines be taken outside the network. I think my comment to them in the cab was they had things inside out, but that isn't quite the right image. So we now have a network of smaller machines that topologically looked exactly like the previous large picture, each of these small terminals talking to the big machines, but this smaller interior thing's called the network, and that meant that this could be done entirely without regard to the kinds of approaches that the different PIs would take to the interaction problem. ARPA could manage this, let contracts for the development of this as a network that had terminals on it that you could connect to. In fact, I then told them that when -- I think Larry asked me, Larry or Bob asked me how to get these things built, these little gadgets here, these little small computers here that ran the network, I said I thought there was only one person who could do that job in the country, namely Frank Heart.

Pelkey: Why did his name come to mind?

Clark: Frank is my oldest and dearest friend, and he and I had worked together for many years at Whirlwind and Lincoln, off and on. We both went out to consult to Lockheed once on a communication problem involving multiplexing, for a couple of weeks or three weeks or so, learned one another's approaches to system design had in fact just about the same kind of attitude toward how you partition big problems into small manageable ones and so forth, and his engineering sense is always superb. He's just a great system engineer. I said there isn't anybody else; if you want this job done, you go to Frank Heart. Well, they had to write a formal request for --

Pelkey: Right, and RFQ and so on.

Clark: Interestingly enough, after a few months, when that was done, I went back and happened to be visiting the ARPA office, the IPT, for some other reason, I think, and they were about to move out to -- go out to another meeting in California and take along the proposals that had just then been received and evaluate them. I think there was some other business as well, but I'm not sure. There was Al Blue in his office measuring this stack of proposals with a ruler, and I thought this was very enlightened civil service. Anyway, they went out to LA and they evaluated them and they came back and said: "Yes, BBN's was the best proposal," so they in fact ended up doing that. Severo Ornstein, who had worked -- is that a name you know?

Pelkey: Yes.

Clark: Well, Severo had worked in my group starting with the LINC activity group at Lincoln Laboratory, and following to Cambridge when we moved there; following from Cambridge out to St. Louis.

Pelkey: Oh he came with you? He came to St. Louis with you as well?

Clark: Sure.

Pelkey: And he went from there to BBN?

Clark: He went from there to Harvard, briefly, about the same time Ivan was at Harvard, and they taught a course together for a while. Did he go to Harvard? I think, no, he went to BBN, but he taught at Harvard, some courses, along with Ivan. He went to BBN from my group at Washington University in St. Louis.

Pelkey: Now, did you know Bob Kahn during this period of time?

Clark: No, not until a little bit later.

Pelkey: But Bob and Severo were the two that were asked to really do the design of the network, underneath Frank, as I understand.

Clark: That's right. I had forgotten that Bob had been at BBN. Also: William Crowther, a man who is on speaking terms with nearly every working Bit in the universe! In any case, Severo took on the design of these little things, which were named IMPs. They weren't IMPs at that time when I proposed this approach. They became IMPs, and Severo did that part. Frank managed this activity, but there were many other actors on the scene. It was a very hard job.

Pelkey: Now, you drew this quite consciously. At that point in time, did it still look like it could be an N squared, and only later became a ring, if you will?

Clark: It's not a ring. The Arpanet is not a ring.

Pelkey: It's not a ring, but it isn't N squared.

Clark: No, no. I just drew in all the possible connections, but it was a rich network, and the network design was something that took a lot of study, and this is, I think, when Len Kleinrock did quite a bit of traffic analysis; very exciting stuff. So that's only the general picture.

Pelkey: Did you stay much involved after that?

Clark: No, not at all. In fact, I even declined, once this was going -- started to go -- I even declined to have my -- anything I had -- my site at St. Louis put on the network. My own lab in St Louis, in fact, had no big computer to offer as a network resource. Later, Bob suggested that St Louis would be a good place for a dedicated Arpanet Service Computer, but I declined to push the idea. The Arpanet Service Computer, or whatever it was called, was set up by Uncapher at ISI instead.

Pelkey: So you were one of these people who they had difficulty encouraging to participate in the exercise.

Clark: I was not encouragable. I was incorrigible, in fact.

Pelkey: That was a big part of the process, just getting all these other sites to give up some of their resources and some of their attention, to deal with connecting to the network and writing the software and making the network available to sites other than your own.

Clark: I'm sure that's true. I hadn't really thought about that, but it must be so.

Pelkey: And you are an example of it.

Clark: Well, I'm not quite sure. Many of them, I'm sure, leapt in with enthusiasm, because that's exactly what they wanted to do. It looked like the sort of stuff they wanted to become involved with, development of networking and interactive ideas -- networking connectivity ideas. I just happened not to be interested in that, that's all. Others were encouraged in or demanded in, and if you didn't have full enthusiasm for it, then I'm sure there was quite a bit of pain involved in that.

Pelkey: My understanding during this period of time is that it wasn't clear that that many people really wanted to do this. There was an understanding that some people wanted to do it, and that Bob and the office had the funding and the vision and they were going to push that --

Clark: They had to be pushed, but I'm not sure for how long. I think enthusiasm developed around that networking stuff. I think there must have been pushing, sure.

Pelkey: A lot of these sites still didn't really have timesharing systems.

Clark: I wasn't aware of that, but I guess it does make sense that they wouldn't. They came along together, is really what it is.

Pelkey: A lot of people wanted to develop their own timesharing systems. They didn't just want to take the Tenex, for example. They wanted to do their own, thinking they could do it better, whereas the IPT office said: "Wait a minute, we solved that problem. Let's go on to solve another problem. We don't need to fund someone to do another timesharing system at this point. We want to fund something bigger and better."

Clark: Each of the directors of IPT came in with the idea of pushing a theme. Lick's was timesharing.

Pelkey: Ivan's was graphics.

Clark: Ivan's was graphics, sure.

Pelkey: And then Bob's was computer science, of which networking was a portion of it, which he then brought Larry in on, and Larry really became the champion of this.

Clark: Larry became the champion. Larry asked me what I thought ought to be done with the TX-2 after he was running IPT -- a question of management, support for it, sponsorship, came up, and I told him that I thought he ought to have it concentrate on graphics stuff, build on the advantage they had and push that as far as possible. There were other groups at Lincoln who were also involved, back then I'm sure, in connecting to the ARPA network and so forth.

Pelkey: Let me ask you a couple of general questions. This issue about networks and things being linked together, now that we're starting to get things like T-1 and T-3 and fiber optics and FDDI -- we're starting to get bandwidth that is starting to be meaningful for pushing graphics around -- we're moving in

that direction, albeit not very rapidly. I presume that you believe that that's necessary -- getting a higher speed, 100 megabit, gigabit per second networks in place is an important issue in terms of where we take computing to the next stage and the kinds of phenomena that will come from connecting computer sites --

Clark: I think so.

Pelkey: Is that a vision that you share, that that's something that needs to be done and is an appropriate thing to focus on?

Clark: It's appropriate to do, and it's going to happen whether anybody wants it to or not. I'm sorry; I shouldn't say that. I don't think that any naysayers are going to have any effect upon that activity, nor do I know of any, really. That's not quite true. I'm aware of some people who think that going to wider bands and pushing very large networks is not an especially good idea. There was a conference about this that Gordon set up just before leaving NSF, now he's at Lake Arrowhead, and Danny Cohen was one of the principal local sponsors of that conference. A number of people were there who were interested in the wide band net questions; what to do next, and there was just a bit of division about the validity of doing the thing at all. If you want to talk more about that you won't learn it from Danny, because he rejects that idea, that it's not the thing to do next. His notion is very clear about this. Gordon, possibly, he might -- he might recall more accurately. Talk to some others. I would talk to Jonathan Turner at Washington University, by the way, who is doing some very exciting stuff with the next steps to take in this kind of wide band network, in developing a suitable switch fabric for the entire thing. The stuff he is doing is just first rate, and very well regarded.

Pelkey: I will definitely look him up. When we connect these systems together, who is going to fund this?

Clark: That's the problem.

Pelkey: There still -- there may not be a lot of science left to do -- that's not clear -- but there's certainly an awful lot of engineering; just like going from those early days of creating the Arpanet to creating what we have today was an awful lot of engineering.

Clark: My impression of what's wrong with the present collection of local area and wide area networks is that we're again facing exactly the problem that ARPA had when it started doing networks in the first place, and I would do the same trick all over again. We have no common standards; we have no centralized, coordinated thing called the supernetwork to which you connect only in a standard way, as I proposed for the Arpanet. Were we to do that, a lot of our incompatibilities would go away, but it's been such a commercially hot game for so long that people don't want to do that, and also, there are plenty of new ideas to have.

Pelkey: Who should fund this supernetwork?

Clark: I don't know.

Pelkey: One of the issues I'm addressing in this book is that ARPA's becoming DARPA has had an influence on the type of research and systems level work that's being done, in terms of long term versus more immediate return, and with Bell Labs as it is now post-divestiture, Bell Labs is going to become increasingly more short-term focused. Quite honestly, in this area of communications, data communications in particular, they have been noticeably absent, in terms of their contributions, although as a national institution, it's still one of the premier places for research to be done. While there is some research at IBM, there's not a lot of it, and the NSF idea of sponsoring individuals doesn't lend itself to these big projects, and a big network is multi-disciplinary and is a large project that has to be managed.

Clark: Well, we don't have an organization like that which sponsors work in this county. IPT came close -- ARPA came close. It could do it again, but it has the wrong --

Pelkey: Mandate now.

Clark: -- and, as I learned at Arrowhead, there are about half a dozen major players who would like to do things like this, each of them pushing his own thing, of course, where what you'd want is some very large, unified operation. Gordon, perhaps, might have been able to change the mode of awarding contracts, in terms of size, had he stayed at NSF -- I don't know -- because he seemed to be quite interested in this question, but I think he agrees that there isn't an agency now that can do that in this country. If you do that commercially, then you have the additional problems of commercial jealousies and standards and de facto standards and things of that sort.

Pelkey: Is this issue of how we're doing research in this country, relative to information technology research, or do you think that a sufficient amount of it is being done now and of a high enough quality that the economic growth that will stem from this in 20 to 50 years -- that we're investing enough today.

Clark: No, we're not. It's very hard to find anybody who would say we are, I suspect. No, we need more money in research. We need longer term and more stable research, and we probably need better mechanisms in government to fund long-term stable research at sufficient levels. We don't do that. I know of a number of things that have had to go under or been put on hold just because there wasn't funding. Most small scale work find investigators spending a lot of their time just looking for the next continuation segment of their funds. It's really bad news. Venture people can't get into it privately when ideas are young and fragile like that, and need more work, and would at best be undertaking considerable risk when you get into ideas that are a little farther along but not yet commercial. I don't know how you do it, but in a case like this, the scale is enormous, so you need a very large organization to handle it with a large budget. You also need something with great authority, because you're going to have to say: "This is the way it'll be done, fellows, and get in or you don't get funded." That always has problems.

Pelkey: This other issue of -- during the '50s, even artificial intelligence represented a centrist notion that, if we can capture enough of the rules and have a database out there, we can in fact reproduce whatever reality we want to reproduce, be it vision or knowledge or whatever, but that as time has progressed, we've come to stand in awe of the complexity of the problems, where you apply parallel processing. Some train of thought is that if we can just produce more processing power, we can solve it. Some of this is neural network, that we can't have centrally programmed notions, but it strikes me that we're starting to have an appreciation that there is no absolute reality out there, outside ourselves, outside organizations. It comes into being as a consequence of interactions. I'm struck in writing this book about how I can talk to five people about something, and all five remember it differently, and it's real to them. What really happened is almost irrelevant. That's what history has become. When you and I look at something in the world, we see it differently interactively. Your perspective is as valid as mine, but at some level there is no permanency outside ourselves. Trying to capture it in a computer database and model it and write rules about it and so on is less appropriate for effective action, in terms of how to use these emerging networks.

Clark: Well, it may not be. I agree with you that everyone has his own reality, all valid. It's important, in systems design, though, to try to codify as much “consensus reality”, you could call it, among the potential users of a system, which you would be able to regularize and set standards for that people can agree on. Even having done so, people bring their own value systems in looking at those standards, and people won't interpret even words the same way, let alone detailed standards. But the AI people, or at least many of them, have gradually come to realize that reality is a little more complicated than they thought it would be, and I'm glad. Some people never thought otherwise. A large part of that community has always been a bit over enthusiastic about how far and how fast you could take these techniques.

Pelkey: My view, as a businessperson for a moment, is that we've always had organizations, businesses that are discrete, distinct entities, much as we are distinct biological entities. If I go back to the biological metaphor that, at the very beginning, there were these biological elements that learned how to combine, and became a cell of a much more complex structure, in which they interact with each other, and they gave up a little independence in order to have more independence by having a more complex structure, which eventually evolved into something like a person. When you put these networks together, organizations -- the difference between a customer and a vendor and a manufacturer, those are going to

begin to fade as you begin to develop much more complex interconnections and relationships as a consequence of accessing each others databases in ways that are much more natural than today. What the organization form will look like in 30 to 50 years, once we have information highways in place, is going to be very different than what we have today, and part of the change that has to take place in effectively bringing forth these networks and using them effectively is a change in the way we look at them, and understanding the different environment we're going to be operating in and managing in. Does that make any sense?

Clark: Yes, entirely. I agree with you entirely. You put things together -- cells, as you said -- and hope that they form stable configurations. In fact, you go through lots of possibilities that don't work, rapidly -- rapidly on the scale of evolutionary development of the world. You can try all sorts of possibilities -- put them together in various combinations and see which ones stabilize. That's almost what you're saying; but you haven't time for that, so you don't do it that way. You can only try a few things, so therefore you have to make sure that the things you put together "like" to be put together, and that wasn't necessarily true of the first pieces that made up cells. But those did come together and worked survived. We can't do things that fast to develop our networking systems that way.

Pelkey: I believe that the organization of tomorrow has to be marked by learning and evolution, and us learning how to deal with that is very important, because --

Clark: Well, it's almost going to be so fast that I don't think you'll be able to recognize it as evolution. The pace of development is --

Pelkey: But it's going to be vastly different than today.

Clark: It will be, but even now, it's vastly different than it was just a few years ago. These things do happen at breathtaking speed, but you can't do enough of them.

Pelkey: Thank you so very much.

Clark: It was great fun.

END OF THE INTERVIEW

