

An Interview with

J. C. R. LICKLIDER

OH 150

Conducted by William Aspray and Arthur Norberg

on

28 October 1988

Cambridge, MA

Charles Babbage Institute  
Center for the History of Information Processing  
University of Minnesota, Minneapolis

Copyright, Charles Babbage Institute

J. C. R. Licklider Interview  
28 October 1988

Abstract

Licklider, the first director of the Advanced Research Projects Agency's (ARPA) Information Processing Techniques Office (IPTO), discusses his work at Lincoln Laboratory and IPTO. Topics include: personnel recruitment; the interrelations between the various Massachusetts Institute of Technology laboratories; Licklider's relationship with Bolt, Beranek, and Newman; the work of ARPA director Jack Ruina; IPTO's influence of computer science research in

the areas of interactive computing and timesharing; the ARPA contracting process; the work of Ivan Sutherland.

## J. C. R. LICKLIDER INTERVIEW

DATE: 28 October 1988

INTERVIEWERS: William Aspray and Arthur Norberg

LOCATION: Cambridge, MA

NORBERG: The date is October 28, 1988. We are here in the office of Professor J. C. R. Licklider to talk about his experiences, both in the MIT community and with the Advanced Research Projects Agency in the Department of the Defense. Professor Licklider, can we begin with the years at the Lincoln Laboratories? We are curious to know how you became associated with the Lincoln Laboratories back in the 1950s.

LICKLIDER: I came to MIT from Harvard University, where I was a lecturer. I had been at the Harvard Psychoacoustic Laboratory during World War II and stayed on at Harvard as a lecturer, mainly doing research, but also a little bit of teaching -- statistics and physiological psychology -- subjects like that. Then there came a time that I thought that I had better pay attention to my career. I had just been having a marvelous time there. I am not a good looker for jobs; I just came to the nearest place I could, which was in our city. I arranged to come down here and start up a psychology section, which we hoped would eventually become a psychology department. For the purposes of having a base of some kind I was in the Electrical Engineering Department. I even taught a little bit of electrical engineering. I fell in love with the summer study process that MIT had. They had one on undersea warfare and overseas transport -- a thing called Project Hartwell. I really liked that. It was getting physicists, mathematicians -- everybody who could contribute -- to work very intensively for a period of two or three months. After Hartwell there was a project called Project Charles, which was actually two years long (two summers and the time in between). It was on air defense. I was a member of that study. They needed one psychologist and 20 physicists. That led to the creation of the Lincoln Laboratory. It got started immediately as the applied section of the Research Laboratory for Electronics, which was already a growing concern at MIT. They got an old temporary building -- Building 22, which had been around for many years. We took it over and built a laboratory immediately, and then started to get one built out at Hanson Field. So, before I could get started worrying about this psychology section, I was really wrapped up in air defense. It turned out that there was a lot that needed to be known about the presentation of

information in the community (?) control system. Here the engineers were bringing radar, and computers and everything together. Then there was essentially a big display and control problem. The upshot was that a young engineer named Herbie Weiss (who has gone on to be a great expert on big antennas; he's the big antenna man in the world) and I created a group in the Lincoln Laboratory, which was about half experimental psychologists and half electrical engineers. It was supposed to work on two aspects of the presentation of information problem: building the stuff and then getting it to work, making a good interface with the user. To get the psychology part started, I went to what I thought were the ten best ten graduate schools and tried to get at least one, and sometimes two, people from each, people who were just then getting their Ph.D.s. So almost immediately we just came by one of the best groups of psychologists there ever was. At that time, nuclear explosives were getting to be a big thing. That was 1950. I think Hartwell was in 1951 and Charles was in 1952 and 1953. So it was right at the crucial time, when the Russians were getting the... Everybody was kind of excited about that. I remember psychologists were wondering whether they should go into more practical, military-oriented things, or stay in research. One kid was saying, "Well, I don't want to write the last journal paper." There was some feeling of imminence. Also I had come and gotten into the acoustics laboratory here. So I found myself spread three ways: I was in the acoustics lab and had an office over there which was in research, I was working on this psychology section thing, and I was working at the Lincoln Laboratory. I think I really made arrangements to get both George Miller, who had been a colleague at Harvard, and Walter Rosenblith down here. George and I wound up doing the Lincoln stuff jointly. We were "co-whatever" we were. When we moved the Lincoln Lab out to its new building, George and I flipped a coin to see who would stay with the Lincoln Lab, and who would stay with this lot. We both thought we lost. I stayed with the lot. He went on with Lincoln Lab.

NORBERG: What was the nature of the problem of the presentation of data as you people saw it in the early it in the early '50s?

LICKLIDER: Well, it was essentially that, here for the first time we were coupling computers with sensors like radar, and sonar and so on. It was possible to do things now that just couldn't have been thought about before. We had

plan position indicators, and A scopes and stuff like that. But there were many new dimensions now. For instance, in air defense we wanted methods of saving the air situation for successive seconds, and plotting tracks, not blips, and coloring the tracks so that could see which was the recent information, so that we could tell which way the thing was going; try to get some way of neutralizing the ground clutter, which was relatively stable from sweep to sweep -- all sorts of stuff like that. We wanted to process signals so that we could get a noise out of the signal with electronics better than we could visually, although we could not take into account its sophisticated relationships. Display was about 80%; control was about 20%. The light gun came out of a group in Lincoln. Then we turned the light gun into the light pen. We never did have mice in Lincoln.

NORBERG: Was there a close relationship between the work in the Acoustics Laboratory and the work in psychology?

LICKLIDER: No, I guess I would say not. We had a different set of problems in the Acoustics Lab. There was a relation in my mind, because I was, I guess, primarily interested in the theory of hearing and what went on in the brain. It didn't really envision what goes on in the brain as closely related to what goes on in here. So, it didn't matter to me much what the vehicle was. I was doing acoustical work, and I was doing visual work. Also, I was just playing with human communication. There was one place where they touched -- auditory radar. Auditory radar was really an exciting thing to me. Harry Shector, who is a statistician connected with the Cambridge Research Center of the Air Force, and Bill Huggins, who is an electrical engineer originally connected to that (he's been a professor at Johns Hopkins for years), and I put together oral radar -- it didn't really take development -- and got the military to fly us around in airplanes. We could literally count the blades in a jet engine and could tell quite a lot from the sound. That had been played with during World War II and then largely forgotten about. We had a wave of it and then it got forgotten. Then it came back, and I think that today there may even be a classified sector of R&D on that.

NORBERG: Is there a characteristic effect of an individual motor, such that one could identify a plane on the basis of such information?

LICKLIDER: Well, we could tell the type of airplane, but we could not distinguish between two copies of the same engine of the same type. It was possible to tell when something was going wrong with the engine. That was the thing of great interest and excitement to the military: here is a way of detecting instantly.

ASPRAY: In your visits to the universities recruiting personnel, what did you look for in the students?

LICKLIDER: Orientation to theory and experiment. Psychology, at that time, was pretty well divided between experimental and clinical. We were really not in the market for clinical people. We were looking for experimentalists. My bias was toward very bright people. I did not care very much what they knew or what they were interested in. I even made an agreement with all these kids: I told them that if they wanted to keep on working on their Ph.D. thesis project, they could do that up to 50% of their time. But I told them, "You won't want to do that after six months, because this is really an exciting thing that you're getting into." And it was true. They all started off working on their Ph.D. Then everybody left it; there was much more excitement in the new study.

ASPRAY: How did you decide which schools to go to?

LICKLIDER: Well, it's pretty well known, I guess. Everybody in any field can rank the first several universities. Let's see, in psychology it was Michigan, the University of California at Berkeley, the University of California at Los Angeles, Harvard, Stanford. Where else? I think I went to Washington University in St. Louis, because I had gone there myself (laugh). I've forgotten what the itinerary was. But there were 10 or 12 of them. I didn't have to be exactly right, because the main thing is to have good ore. You may be lucky and you may get a little unlucky. I constituted myself (?) at testing locations for the Miller Analogies Test and the Graduate Record Examination, and gave all these guys these tests, if they hadn't already had them. I had a kind of a rule, "Anybody who could do 85 or better on the Miller Analogies Test, hire him, because he's going to be very good at something."



NORBERG: Who were some of the people you were able to encourage to come?

LICKLIDER: Well, Bill McGill, who later wound up being president of Columbia; a fellow named Joe Bennett, who, unfortunately, died early. He had a bad heart, but he was brilliant and fluent. Bill Harris, who is still a staff member of the Lincoln Laboratory -- one of the few who's still around. Herb Jenkins, who is at McMaster University in Hamilton, Canada. Let's see. Well, that's all I'm going to think of right now.

NORBERG: Yes, that's fine.

LICKLIDER: I do not know whether there were 10 or 12...

NORBERG: Okay, we interrupted you just at the stage where you and Mr. Miller had flipped a coin to see who would go to Lincoln and who would stay. What happened after that?

LICKLIDER: We got some Ph.D students -- five of them -- and started to develop a psychology department. Looking back, I was incredibly naive. It did not occur to me to tell the faculty we were starting a psychology department. (laugh) We actually got around to awarding Ph.D. degrees. Then the dean found out about this and he said, "Oh, no, no, no!" We had brought Bill McGill and Herb Jenkins down here from Lincoln. Alex Babilus was still here at MIT from the wave of dynamic psychology when Kurt Lewin set up a big psychology show here in the generation earlier than what we are talking about. So, Alex, and George Miller, and Herb Jenkins, and Bill McGill, and maybe we have one or two other people ? [a person's name],... the Harvard professor, I can't remember his name. Anyway, we had quite a show going, had research contracts -- so we really had lots of money. We had the whole basement of the Sloan building, which is where the faculty club is. We were doing pretty well. The dean, or the president -- one or the other -- saw that we had a pretty good thing, even though it was on a kind of informal basis, and he talked to Don Marquis, and Smitty Stevens and maybe somebody else -- the two or three really well-established, trustable psychologists. They essentially told MIT "You've got a really good nucleus of a psychology

department." So we were going to turn this thing into a legitimate activity, and go tell the faculty about it. About that point I came to realize that my interests just couldn't be furthered. I had a big analog computer lab, because I was modeling brain stuff, and I realized that I could not do what I was trying to do by analog computing. I did not know digital computing, but I could see in all this Lincoln context that there were digital computers everywhere. I started to go tinker and learn how they work. Wes Clark got a hold of me one day and gave me a lecture about it. He said, "Why don't you take a couple of hours everyday and go work at this TX2 computer that we put together?" So I saw that I had really got to do that, and I also saw that I was not going to do that trying to build a psychology department at MIT. Just about that same time, George Miller got an offer from Harvard to go be a professor. We started to fall apart. I developed an agreement with Bolt, Beranek and Newman that I would go there, start a psycho-acoustic laboratory, and probably also start a more general experimental psychology communication lab. But mainly, we would buy a digital computer, I would master the digital computer, and we would go into the computer business -- the software side.

ASPRAY: Were you familiar with the other people doing analog computing at MIT?

LICKLIDER: Oh, yes; although I did mainly psycho-acoustic research. I used the analog equipment mainly for generating stimuli, collecting responses, analyzing them, and so on. It was having analog computers, and finally learning how to master them, that led me to do modeling on them. I knew the digital differential analyzer. Indeed, since I needed less precision and was more tolerant of faulty equipment, I got a lot of their stuff after they thought it wasn't any good any more. I had a wall full of electronic counters and stuff like that. There was a Mr. Verzuh, I think it was.

ASPRAY: Frank Verzuh.

LICKLIDER: He was very helpful to me. There was a young guy in the acoustics lab -- I forget his name -- who was an electronic genius, and he taught me to build circuitry and the like. I could do the electronics and so on for my

purposes. Indeed, I had two full-time technicians working in the lab. My lab was administered by the Research Laboratory for Electronics, and they let me have their senior electronic technician, who was really a genius at putting this stuff together. The technicians had to learn that you do not really want blueprints, and you do not want a thing finished too well, because tomorrow it's going to change. That was a shock for them.

ASPRAY: How did you learn about Bolt Beranek and Newman?

LICKLIDER: Well, it was a spin-off of the acoustics lab. I worked in the acoustics lab with Bolt and Beranek, and Jordan Baruch, who was another member, and Sam Labate, who was another, and they started spending more and more time with their company. They could see the computer was going to be important for them too -- they didn't know just how. Here I was really determined to go learn it.

NORBERG: Let me go back and see if we can unravel some of the threads that are in the fabric you've just describe. Can you review for us what some of these contracts were, and with whom the contracts had been made that you were doing in the psychology department?

LICKLIDER: Yes. Mainly Air Force. There was a thing called the Human Resources Research Laboratory, which was physically located at the Naval Research Lab, although it was an Air Force lab. I had contracts with them to do essentially psycho-acoustic research. The research was pretty diverse, I guess. I can't keep this all separate. Part of it was basic research related to auditory theory. Part of it was in the psychology section of the Sloan School, part was in the acoustics laboratory, and I did some part of it at Lincoln. I think I also had a little money from the National Institutes of Health.

NORBERG: How did all this money come about? Were you all trying to raise these funds?

LICKLIDER: A young MIT professor really had to divide funds into a lot of little fragments unless he was a member of a big lab that did all that for him, and since there wasn't any big lab they would do the administrative things orally. But they were not about to go to raise money...

NORBERG: Did you hire Bolt and Beranek and Baruch, or had they been around as students?

LICKLIDER: Oh, no, they were senior to me. Bolt and Beranek were two or three years older, I guess. Baruch was maybe a year or several years younger. He was a graduate student at first. But Beranek and Bolt were instrumental in getting me down here from Harvard. As a matter of fact, my first office at MIT was in the acoustics lab. There still is a big space back there that isn't full yet. They simply built an office for me on top of Leo Beranek's office.

NORBERG: Had you had any contact with the people in the Servo-mechanisms Laboratory, and the work that was going on with Whirlwind?

LICKLIDER: Yes. I knew Gordon Brown slightly, and I had read his book. I guess he was the only person that I really knew there, but I did spend a little time there. If I had problems, I would show up. I was not embarrassed to go ask people.

ASPRAY: He was also the director of the computing center at that time, was he not?

LICKLIDER: No, Phil Morse, the physicist, was head of the Computation Center, and Fernando Corbato was the Associate Director. At least somewhere along in there Corby had a contract with Office of Naval Research, I think, to develop a time-sharing system. That started before there was a Project MAC. It is conceivable Gordon Brown was director of the Computation Center at one time, but when I knew it, it was Phil Morse.

NORBERG: I guess what I was trying to get you to say was something about how you became interested in digital machinery, and moved away from the analog machinery.

LICKLIDER: Well, there was tremendous intellectual ferment in Cambridge after World War II. Norbert Wiener ran a weekly circle of 40 or 50 people who got together. They would gather together and talk for a couple of hours. I was a faithful adherent to that. When I was at Harvard, I came down here and audited Wiener's ? series in ? -- a couple of things like that. Then there was a faculty group at MIT that got together and talked about cybernetics and stuff like that. I was always hanging onto that. Some of it was hard for psychologists to understand. But Walter Rosenblith was understanding and he did a lot of explaining to me. As a matter of fact, he audited a course down here two weekends ago. There was a young fellow named M. Fred Webster. Routinely we'd talk about it on the way down in the car, and then listen to this stuff. Then on the way back, Walter would more or less explain it to me. (laugh) Digital stuff was big in all of that. And there was Whirlwind. And I knew about and then visited Howard Aiken's lab. Once I had the pleasure of riding on an airplane with Aiken, and got to know him enough to be somewhat inspired by that.

NORBERG: In what period did you get to know him?

LICKLIDER: It was just about the time that Jay Forrester was getting Whirlwind I up and running.

NORBERG: 1950 or so?

LICKLIDER: Something like that, yes. I was interested in speech as well as hearing, and was interested in the phenomenon that you could take speech waves and reduce them to rectangular waves that crossed the axis when the speech wave did. That is, you could construct what we called multi-vibrators, the flip-flop circuit. You listened to the output of it and got a surprisingly high degree of intelligibility. Then it went further in, quantized the time scales, so the switching had to happen at the nearest pulse. I wanted to build a device that would let me set up

arbitrary patterns -- just one bit per decision. But I wanted at least a second of speech, and preferably five or ten seconds. So I was really in quest of ways to do that. At that time, unhappily, the circuitry was expensive. We got our flip-flops, and I think they were about \$25 apiece. There were two vacuum tubes and some resistors and capacitors. I could not afford enough of them to do what I wanted. But I could sneak into some of the digital computers that were being built, and program them. That was really, I guess, how I got my hands on a computer first.

NORBERG: Is this also about that same time, when Whirlwind One was...?

LICKLIDER: Yes, I do not remember the timing exactly, but Lincoln built a thing called TX-0, which was a computer with happily only four basic instructions. That was out at ? Defense (?). Then they built TX-2, and they built a thing called the Memory Test Computer to test for memories that Forrester had done.

TAPE 1/SIDE 2

NORBERG: You were just mentioning when the Memory Test Computer was constructed, along with TX-0 and TX-2, and so on; and your association with these.

LICKLIDER: Well, at Lincoln, one would get to get his hands on a thing like that a little bit. But it was more just establishing in my mind the concept I had to have one of those. I did not get hours on it. TX-0 was essentially given to MIT by Lincoln when it was no longer needed for its purpose there. But, more to the point, what we had done at BBN was buy the cheapest computer we could find, which was the LIBRASCOPE LGB30. It turned out that that was just totally inadequate.

NORBERG: Why?

LICKLIDER: Well, maybe I will spend a minute about the LGB30. It was a vacuum tube machine. The signals recirculated through the vacuum tubes. I think they went through three times in order to get a different frequency. They were shifted in frequency. It was a trick that people knew how to do. But there was no core memory, of course. There was no vacuum tube memory either; there was just a drum. And for some strange reason the drum had 29 bit words on it. The accumulator was 32 bits. It was AC coupled, and they had forgotten that if you get all ones or all zeros, eventually you will lose your signal, because nothing would fluctuate. So they took one bit off the accumulator to make it about halfway between a zero and a one, so there would always be some AC. So, you had 29 bits in memory and 31 in the accumulator; and you had to feed things in through the accumulator. The first thing we did was multiply three by four. The way that went was you had to shift it over two bits to get it in the right place to go into memory. You multiplied the three by the four and got 12; multiplied the 4x4 and got 16; multiplied 12 and 16 and got whatever you got, and divided that by 4 (laugh) and, indeed, it worked, but it was ridiculous. The darn thing made mistakes. It just had machine errors and it was very slow. So I programmed one of the models I had running on a G.A. Philbrick analog computer. The analog computer solved the problem 30 times a second and displayed it, so I could twiddle my knobs and see what was happening. This darn thing took two and a half days to run the program. In the process it made many mistakes, so you had to checkpoint yourself. All there was to checkpoint yourself on was paper tape, and the paper tape was unreliable. So it was fun to play, and I learned to write programs and stuff on the LGB30, but we were an order of magnitude worse off from what it was going to take. At that point, or before that, Ken Olson, Norm Anderson, and their group had spun off from Lincoln to form DEC, which was a maker of plug-in modules -- digital modules. One day, they saw that, my God, we can build a computer. We've got enough parts here. All we have to do is put them together. And so they made this thing. Bolt, Beranek and Newman bought the very first one they ever made. In fact, they let us have the prototype while they made the first production machine. So we wound up with this PDP1.

ASPRAY: This was when?

LICKLIDER: The negotiations were in 1958 and the machine was delivered in 1959, I think. The second one was delivered down to MIT and went in along side TX0. Jack Dennis, I think, was head of the group that had it. The BBN people and the MIT people knew each other pretty well. I had got Minsky and McCarthy to be consultants at BBN and had a bunch of MIT graduate students: Kane, Raphael, Bobrow -- maybe two or three more. So there was good communication, and we always knew we had to have some bigger memory in this PDP1. So I think it was kind of a joint effort to get Bryant Chucking (?) to build a drum that would work. Somewhere along the line when I was early at BBN, Ed Fredkin came to see about a job. It was obvious from the beginning that here's a young genius. So, we got Fredkin working at BBN. At that time he was having all kinds of psychological problems about getting work done. We had fantastically interesting and flexible arrangements, like figuring out at the end of each month what his salary for that month should be. But he was absolutely marvelous. He designed up this drum with MIT in just a few days. Anyway, that was a serious computer. You can make an argument that, although it had better graphics, in other respects it was almost exactly a Radio Shack TRS 80-100 computer. It had about that computing power, a little less memory, and it ran a little slower. Everybody connected with it just sat at the console and did on-line interacting programming and since I was of the first one, I got most of the time. There were enough of us; we kept that thing running day and night. We even built a time-sharing system on it, although it was more just an exercise: because it was such a weak little computer, there was nothing really to time-share. But, just to prove it could be done, we divided the scope into four quadrants and let each person have a quadrant of the scope.

ASPRAY: This was when?

LICKLIDER: Well, there's a paper about that time-sharing system. I do not know whether it was 1960 or 1962. It was probably 1960. I have a copy of a memo from Minsky to some of his colleagues here, chastising them for letting us get ahead of them in demonstrating the time-sharing system. I simply do not know what the precedent was there. But the BBN one was quite special purpose. It was a time-sharing system for writing and debugging programs.

ASPRAY: Do you know the origin of the time-sharing concept in this larger community?



LICKLIDER: Yes, probably the first person who wrote about it was a young Britisher. He mentioned the concept at a computer meeting in Paris, I think in 1960 -- sponsored by UNESCO, maybe.

NORBERG: Sounds right.

LICKLIDER: Yes. That's the first writing I know of about it, and his mentioning it then was probably the first I had heard. But very soon there was pretty much discussion in the community very soon. People were saying that what they wanted was interaction and memory sharing. Processors were so expensive that for a time it was really just barely making processing available for more people. I think John McCarthy was probably the source of most of the motivation and action. I think he rather stimulated BBN into doing the time-sharing system. At that point he was, I believe, head of the thing called the Long-range Study Planning Group here. He did not administer that very well. So, eventually they made Al Hill, who had been director of the Lincoln Lab head of it. Al was a very soft-spoken but driving administrator, who would get things done. Al got the report to come out about what the requirements were. The powers that be thought McCarthy's requirements were preposterous. McCarthy wanted a million bit memory -- not a million words, I think it was just a million bits -- but at that time, of course, that wasn't responsible thinking. Now it would not be responsible thinking either. Anybody that did not want more than a million words in a memory would really... (laugh)

NORBERG: Let's go back to BBN. What sort of problems were you people working on besides this time-sharing and trying to make the PDP1 work in this way?

LICKLIDER: Well, one of the things was libraries -- The Council on Library Resources gave us the money to explore what the computer was going to do to libraries. We did some things, but they were never practical at that stage, because we wanted to do something where the text and diagrams of all these books were going to be stored, but we had enough room to have three short technical articles -- or something in this direction. It took a pretty big step of

imagination to see the relationship between what we had and what we wanted. But we did make a system for studying technical documents. Even for now, the concept was just right. We wanted to analyze the scholarly process -- reading and studying documents, tracing references, and so on -- and build an interactive man-machine system, or a person-machine system, to facilitate that. Raphael, Kane, Bobrow and I actually wrote a paper about such a thing. About two years ago I was visiting Xerox PARC -- walked in on this group, and they said it was not a put-up job. They were working on exactly such a thing. And they had a copy of the paper, and indeed, there was Bobrow. So, there were several things like that. We did not work very hard on how to use the computer to control circulation and stuff like that. We got out a little brochure, I think, that presented workaday ways to use the computer in the library, but we were much more interested in knowledge bases -- how to store information. We played with pointer schemes for storing relations in them. Ed Fredkin, at that point, published a paper called "TRIE Memory," which presented an extreme storage scheme in which there was nothing but pointers. If I recall the scheme it went something like this: suppose you want to work with alphabetic things. You have 26 cells. If you have something beginning with "m" the pointer goes from the "m" cell to another one of these. And then the pointer from... But you can use each one of these several or many times. All words that begin with "m-a-n" use the same first three pointers. Then there's a thing that says, "This is the end of the line," so that you can have one thing stopping there at the word "man", and then another one going on to something that begins with "man". Well, I remember Ed simulating that thing. We managed to get eight hours on a 7094 or maybe it was a 7090 at the Lincoln Laboratory. Ed ran this simulation, which showed that when you get a big enough thing, this becomes efficient, and interesting and promising. And it was so good that we talked Lincoln into giving us another eight hours to run it. And there was a random selection process in this test. And Ed forgot to differently initialize the random number generator. And the second eight hours gave exactly the same numbers as the first eight hours. And that was really quite a big event in my life. I frankly didn't believe that... exactly the same... (laugh)... pretty unhappy not to get any ? today.

ASPRAY: Who was supporting this research?

LICKLIDER: The Council on Library Resources was. The head of it was a man named Verner Clapp, who was an eminent figure in the librarian and scholarly book world. Although he was tremendously suspicious of all of this stuff with computers, he did appreciate that we were serious and enthusiastic and working hard on it. He really supported us. He got a steering committee for us. I do not remember who was on it. I do remember the president of Colby College was a very helpful guy who steered us through a lot of troubles. But to keep this bunch of engineers honest, we would have to report to the steering committee about once a month about what we were doing. When we got all through, I still had to write the final report of this project even though I had moved to ARPA by then. I remember that occasion very vividly. I was at some meeting in Las Vegas, and every day went out by the swimming pool for about three or four hours and dictated on that report -- and, indeed, did get the thing done. I had to look up some references when I got back home, but Verner liked the report so much that he talked MIT Press into publishing it as a book. So here is this book called *Libraries of the Future* that tells about what we did. I have always been pretty happy with that. It was not a terribly expensive thing, as those go. I think it captured a little bit was an early appreciation of the coefficient in the exponential increase in the power of computers. We figured that every two years the cost effectiveness of computer hardware was doubling. Considerably later -- towards the late end of the 1960s -- Larry Roberts really went into that and plotted a lot of data about the capability of the computer according to a couple of formulas he had for figuring the capability as a function of announcement date. If you don't change the technology, I guess the coefficient within a system was 1.4, which means doubling every two years. If you take the best thing of any kind, it was 1.56, I think, which was even faster. I have felt that that is a very significant number. It tells you that if you're going to do research on something that you hope to see in practical applications in say eight years, you ought to figure that you've got four doublings. So what's that -- 16? So you better get yourself a machine 16 times as good as you would be able to afford for the practical application in order to have a space for developing it.

ASPRAY: Any other projects that went on during that time?

LICKLIDER: Oh, yes. We had a project with the Air Force Office of Scientific Research to develop the systems concept. Now it's corny, but then it was an interesting concept. We were trying to figure out what systems meant to the engineering and scientific world. That involved some meetings in which we brought [together] good thinkers in several fields. We wanted a kind of miniature Wiener circle. I do not remember that we really contributed that much to that. I don't know what came of it. But we put a lot of hours into trying to do that. I think Tom Marill joined the group at BBN, and he had a contract to develop semantic networks -- one of the very early things, probably earlier than Tom was. At Carnegie-Mellon there was one paper on Semantic Networks before Tom's, but I think that was pretty terminal I am not be able to say that guy's name.

NORBERG: How large was Bolt Beranek at the time [in] the late 1950s.

LICKLIDER: Well, when I went there I think there were probably about 40 people.

NORBERG: They could not all have been working on the Council of Library Resources.

LICKLIDER: Oh, no. BBN was mainly an acoustical consulting firm. It moved into computers pretty fast once we got started. Jordon Baruch got excited about computers. And he ran a company jointly owned by General Electric and BBN which did medical applications. I am trying to think of what I did with myself. I think my support was largely from the Air Force and the Council on Library Resources. But I believe we also had Office of Naval Research support. We were running pretty much like you would run a university. We were not building things, and selling machinery or anything. I could probably go find reprints.

NORBERG: No, that's all right. We can certainly do that. Was there any thought at BBN in that time -- I am still talking about the late 1950s -- of simulating any sort of acoustical activity in an attempt to do better design work, or better consulting?

LICKLIDER: I do not know the answer to that. Leo Beranek was deeply engaged in concert hall acoustics. We had a kind of a debacle with Lincoln Center, if you may recall -- it was pretty famous.

NORBERG: Yes, that's what I was thinking of.

LICKLIDER: This had to have been discussion of simulating concert halls. But I really do not remember the details of that. Oh, aircraft noise was a big thing. There was a whole sector of the work that had to do with how to measure the noise. It had to deal with the effects of noise on people. Karl Kryter came and started up that work, and wrote a book called *Effects of Noise on Man*. I saw him the other day and he is, I think, doing another revision of it. He spent his lifetime, really, on noise effects. It was pretty big stuff, because in industry at that time there were many manufacturing situations that made a terrible clatter. People were just beginning to realize that that was all much worse on hearing than anybody had suspected. And there were six or seven billion dollars worth of lawsuits stacked up at that point when I was at BBN. So there was a practical side of things there. I do not think anybody tried to simulate the nerve degeneration process in the computer. But the computer was certainly useful in a kind of epidemiological approach to the...

NORBERG: This is about the time you wrote the paper on man and information, is it not?

LICKLIDER: The thing called "Man Computer Symbiosis?"

NORBERG: Yes.

LICKLIDER: Yes, that was in there somewhere. I do not know what the date of that was. 1960...

NORBERG: 1960. Did that come out of the Council on Library Resources work, or was that some larger problem you were thinking about?

LICKLIDER: No, that didn't come out of any particular research. It was just a statement about the general notion of analyzing work into a creative part that seems to be heavily involved with heuristics and routine programmable parts that you could see exactly how to get a computer to do. That was really based really on some measurements I made on myself. I tried to keep schedules and see how much time I spent doing what, and I was pretty much impressed with the notion that almost all the time I thought I was thinking and working, I was really just getting in the position to do something. And I had an experience with Jerry Elkind and his manual tracking experiments, which he had done as a graduate student under my supervision, (although he was an electrical engineer). He was pretty clear [that] there were some relationships in his data; he had all these things plotted on graph paper. There was a stack of them, and we could never see them all at once, and could not tell what was going on until we put big heavy blobs wherever there was a datum point, and went down to the Sloan building where I happened to have an office at the end of a little mezzanine where you could stand and look down on the floor below. So we redirected traffic a little bit, and put all these graphs down there, so we had a hundred or so sheets of graph paper. Then it was obvious what was going on. And I was pretty much impressed. That happened frequently: you do a lot of work, you get in a position to see some relationship or make some decision. And then it was obvious. In fact, even before you could quite get finished, you knew how it was going to come out. "Man Computer Symbiosis" was largely about ideas for how to get a computer and a person thinking together, sharing, dividing the load -- mainly heuristic versus algorithmic.

NORBERG: While your were out at BBN were you still teaching here at the Institute?

LICKLIDER: No, I was around the Institute a good bit, but I was not teaching.

ASPRAY: One last question about BBN. Were there people that you got to know well at BBN that you then were in a funding relationship with at DARPA?

LICKLIDER: Well, there are two parts to that. One would be funding BBN from DARPA. There what happened was that, in order to avoid conflict of interest I studiously avoided funding BBN. Leo Beranek did not like that much, and went down to ARPA to complain. I said, "Well, okay, you deal with the director's office, but not through Licklider." I do not think anything happened very soon, but later when it came to building the ARPA network, ARPA picked BBN to be the principle developer and builder of that. And that set BBN into the network business. I think it has been very effective in the network business.

NORBERG: Had you had anything to do with MITRE during this period?

LICKLIDER: Yes, MITRE was spun off from Lincoln. I do not remember the date, but a lot of the people I knew went with MITRE. MITRE ran an annual conference, for a few years at least, in Virginia or West Virginia -- I have forgotten the name of the place now. One of those big hotels...

TAPE 2/SIDE 1

LICKLIDER: Through the work at BBN I got acquainted with people in the computer world. Also, through the work of Lincoln Laboratories. So I guess I knew pretty well that there were people at MIT, and Harvard, and UCLA -- I remember George Brown ran the Western Digital Network, or something like that, under IBM funding at UCLA. I had a number of notions of where to go. It was not starting absolutely cold. But most of the time that I was at BBN I did not know that I was going to ARPA at all. It had happened rather suddenly.

NORBERG: How did it happen?

LICKLIDER: Jack Ruina was director of ARPA, and was given responsibility for a command and control project that got set up from inside the office of the Secretary of Defense. The contract was let by them through ARPA to System Development Corporation to do some command and control research. It was envisioned that that would grow. There

would be more to it than that one thing. So Jack wanted somebody to do that. Simultaneously, there was a recommendation from the Defense Advisory Board that the Defense Department look into supporting some behavioral research, particularly through some institutes that would be set up to bring behavioral researchers together with better facilities. Jack wanted somebody to look into that; maybe do something about that. Fred Frick, whom I first knew at Harvard in the Psychology Department at Harvard, was in all of this at Lincoln. Fred was, by that time, at Lincoln. Fred and I went jointly to talk with Ruina. We were both interested in what he was talking about, but neither one of us wanted to leave what we were doing. I guess Ruina got Gene Fubini to give us a sales pitch and we decided, yes, this was so important that one of us would do it. But which one? Again we came to a coin toss.

ASPRAY: Who was Fubini and why were you influenced by him?

LICKLIDER: Well, Gene Fubini was general assistant or associate to the Assistant Secretary of Defense. I had known him before that. I do not know exactly how. He was in a research laboratory out of Long Island, and I remember spending some time with him. I was tremendously impressed with the guy. He was an immigrant from Italy -- a wonderful European technical education; very sharp, incisive, impatient kind of guy; quite eloquent, and really dedicated to this job at the Pentagon. He made Frick and me feel that the world needed at least one of us and we should do it. So, it wound up that I was going to take off from BBN and do that. I went on leave, but before too long had decided that I would not go back to BBN and went off the leave.

NORBERG: Let's go back a little bit. Do you recall what sort of arguments Fubini used about ARPA and its objectives that might have helped to convince you?

LICKLIDER: Well, part of it was when we got talking I started to wax eloquent on my view of the thing: that the problems of command and control were essentially problems of man computer interaction. I thought it was just ridiculous to be having command control systems based on batch processing. Who can direct a battle when he's got



to write the program in the middle of the battle? Fubini essentially agreed 100% with that and so did Ruina. We started seeing that. Here this whole military thing was not developed right. The kind of computing almost did not exist, but up in Cambridge everybody was excited about making it exist. Why didn't we really develop an interactive computing? If the Defense Department's need for that was to provide an underpinning for command and control, fine. But it was probably necessary in intelligence and other parts of the military too. So, we essentially found that there was a great consonance of interest here, despite the fact that we were using different terms we were talking about the same thing.

NORBERG: Had you had any experience with command and control before this conversation with Fubini?

LICKLIDER: Well, yes. The Air Force systems contract at BBN was really essentially command and control, yes.

NORBERG: Who was working on that at BBN?

LICKLIDER: Well, I was a little bit. Jordan Baruch was; Tom Marill was; Jerry Elkind was. Everybody had a corner of it.

NORBERG: Was this under support from the Air Force?

LICKLIDER: Yes. Somewhere along in there in the mid-1950s I was a member of the Air Force Scientific Advisory Board. They picked me to run a committee to explore from a human factors point of view the relative merits of one-person and two-person interceptors -- essentially can one person run such a complicated thing? It was a time when there were F-86s, F-89s, and F-94s. F-86 was a one-person machine; the other two were two-person machines. I had a committee, partly military people and partly psychologists and engineers -- I guess six or eight people, including one colonel who could somehow commandeer airplanes and fly them. (I remember it was the fall that Eisenhower and Stevenson were running against each other and I covered more miles than they did. We went everywhere.) We were

going to report on that, but it was kind of a shocker to the Air Force Research & Development, ARDC, because we were really very critical about a lot of things. They did not have any place to test tactics; they did not know whether these things would work or not. When they finally did get around to testing them, nothing hit anything except the F-89 shot off 104 rockets, and one was out of control, spinning off like that, and it hit the target. (laugh) So I got pretty deeply involved with military people about stuff like that. And the Lincoln Laboratory had been essentially command and control -- factoring aircraft to make interceptions and stuff like that. So I was pretty well coded up on it. I had read a lot and was intrinsically interested. But I did not go to Washington and do anything about command and control studies as such. It was the development of interactive computing which we all claimed would be a basis for command and control.

NORBERG: Okay, let me understand something that you said before to make sure I got the point. You mentioned when you and Frick went to Washington to talk to Ruina, and he sent you on to Fubini, that there had been an interest in the Department of the Air Force (I think you said through the Air Force) to develop institutes, to study these behavioral questions. Now, had you two gone looking for funds to set up one of those institutes, or bring the work to BBN?

LICKLIDER: No.

NORBERG: You had been invited to Washington, then, for this conversation?

LICKLIDER: Yes.

NORBERG: Okay. I wasn't quite sure what started it.

LICKLIDER: I was going to say, the institute thing never really did work, because Behavioral Science didn't want to move. They wanted money where they were, and did not want to move together just for facilities. They were not

very convinced they wanted any facilities. In my view every research enterprise like that ought to be centered about a big, time-shared computer.

NORBERG: Yes. What sort of man was Ruina?

LICKLIDER: In my view, brilliant; for a scientist, amazingly competent in finance and all kinds of fiscal administration. As a manager, it seemed to me that he had spent long enough to decide he understood what I was trying to do, and that I would probably work hard to do that, and then just wanted to have a report periodically -- was I on track or not? He had much bigger fish to fry; this was a small part of his life. I've told him since that I've felt that that was a kind of benign neglect.

ASPRAY: Was he an MIT man?

LICKLIDER: I do not know whether he was before or not. I knew him first when he was in the Pentagon; the first time I ever heard him give a talk he was in the Pentagon. He came to MIT afterward. I do not know whether that was returning to MIT or not.

ASPRAY: How well did you know him when you went to that meeting with Frick?

LICKLIDER: Not very well. I had heard him give talks. I do not know whether I knew him personally then or not.

NORBERG: ARPA was five years old at the time you arrived. What sort of situation did you find there? How was it set up? What sort of people were active?

LICKLIDER: That's hard to answer. I do not remember extremely well what the organization was. There was one part that distressed me. ARPA had gotten in... Were we in the Vietnam war then?

NORBERG: Not quite yet. We were there, but it had not expanded.

LICKLIDER: There was a kind of a cloak and dagger part of it.

NORBERG: There was?

LICKLIDER: There was a fellow named Bill Godell who, it seemed to me, was always trying to get control over what I was doing. I could never tell what he was doing, so that part made me nervous. Other parts of it... I am not sure whether it was organized with the Strategic Office, or the Tactical Office, but it was something along that line. A lot of it was re-entry physics. I had the feeling that there were some real geniuses in this, but there were also some guys who were essentially bureaucrats and managers, so I sort of stayed out of that as best I could. I couldn't be totally uninvolved. I had one project that I wasn't cleared deeply enough to know what was, and that made me nervous.

NORBERG: Let me try and ask the question a different way...

LICKLIDER: It turned out, I could say I was paid for digging a hole in Lafayette Square.

NORBERG: ... And that is, as we look over the history of ARPA that has been presented to us by the Barber Associates when they did their analysis of the management style there, which they prepared in 1975, it seems there were about six different groups active in ARPA at the time. One of them was the missile group, which had been the primary activity from 1958 through about 1962. Then there was the commanding control group, which seems to be relatively small in comparison to the missile group. There was some activity in behavioral psychology and one or two other things now which escape my memory. It appears as if the command and control and the behavioral psychology groups ? groups offices were linked...

LICKLIDER: I was director...

NORBERG: ... Whereas the others were not linked and did not seem to be associated with command and control and behavioral psychology. Is that a fair assessment?

LICKLIDER: Yes. I did not deeply understand what they were doing. I thought that the motivation here was really that there has to be this research on reentry physics, and the secretary didn't want to give that to one of the established military departments, because they knew how to spend money. ARPA would do it for one-tenth the money, and they did not want that to be a development project. They wanted that to be close to a research project. But I did not know anything about that; I had got problems of my own.

NORBERG: Okay, let's put that aside for the moment. Regardless of what your agenda was, what was it you were told that you were being brought in for, to run Command and Control and Behavioral Psychology?

LICKLIDER: To run Behavioral Science. I do not think I was ever told to run Command and Control. I was going to be in charge of the SDC contract, but mainly, I was going to set up more stuff, and maybe even decrease SDC a bit, to spread it further, and do interactive computing. At least, I said that. Every time I had the chance to talk, I said the mission is interactive computing. I did realize that the guys in the secretary's office started off thinking that I was running the Command and Control Office, but every time I possibly could I got them to say interactive computing. I think eventually that was what they thought I was doing.

NORBERG: Well, why? Why did you want to push interactive computing so strongly?

LICKLIDER: I was just a true believer. I thought, this is going to revolutionize how people think, how things are done. You know, yesterday or the day before I heard a talk about productivity and about improving it 7/10 of a

percent per year, or something. I thought we were going to double it or triple it, or multiply it by four or ten or something; and I still feel that way.

NORBERG: Why did you believe this?

LICKLIDER: Well, I was one of the very few people, at that time, who had been sitting at a computer console four or five hours a day -- or maybe even more. It was very compelling. I was terribly frustrated at the limitations of the equipment we had, but I also saw how fast it's getting better. So I was just a true believer in my own propaganda, I guess.

NORBERG: Who else did you talk to about this question of interactive computing before you went to ARPA?

LICKLIDER: Oh, all the guys around here -- Minsky, McCarthy, Fredkin (maybe especially Fredkin. He is a tremendous enthusiast), Tom Marill, Wes Clark, Larry Roberts. Ivan Sutherland

ASPRAY: Did they share your view?

LICKLIDER: Oh, yes.

ASPRAY: Was there a disagreement at all about this in this community?

LICKLIDER: Well, people like Minsky and McCarthy were primarily interested in artificial intelligence, and tended to view man-computer interaction as a neat and convenient thing to make it possible to write AI programs, whereas I thought there was going to be this interval between man's thinking about himself and machines taking over. I do not know how long the interval was, but it looked like a considerable interval, when working with the computer was of the essence. So, in short, I really believed it, and quite a few people in the area here thought that something really great

was going to happen. Belmont Farley was another one of the fellows. He worked with Wes Clark on the Memory Test Computer.

NORBERG: Was there a staff waiting for you when you arrived?

LICKLIDER: Well, I had a secretary. I went in my first day, which was in October of 1962, and met my secretary and she said, "Well, Dr. Licklider, you have just one appointment today. There are some gentlemen coming from the Bureau of the Budget, or whatever it was called then, to review your program." And, indeed, they came. They were amused when they found out it was my first day on the job. We had a really very pleasant talk. I told them what I was excited about, and that turned out to work greatly to my favor, because they got interested in it, and when we did have a meeting on it it was very favorable; they did not take any of my money away.

NORBERG: Was the only contract at the time the one with SDC?

LICKLIDER: That's correct, yes.

ASPRAY: And who had let this happen?

LICKLIDER: I do not remember, exactly, but I am afraid somebody from SDC had spent a year in the secretary's office, and had managed to get that set up -- something along that line.

NORBERG: If the people from BOB, I guess at the time here in the budget, came in to talk to whoever it was running this office, and you turned out to be the person, there must have been a budget associated with the office.

LICKLIDER: Yes, I think that there was nine million dollars in that contract, and I think that there was something like 14 million dollars total that... Ruina could have moved to any place he wanted to, but I think he had had a good move sitting there (?).

NORBERG: In comparison to the contracts you had had before, what sort of budget was BBN running on, for example, in 1962?

LICKLIDER: Well, if I take just the part of BBN related to computers and systems and stuff like that, I guess we were a total of 10, or 12, or 15 people. I do not know how you turn that into money, but our whole thing was small stuff compared with... Fourteen million dollars seemed like a lot of money to me.

NORBERG: It sure does to me, especially for that period. What I am leading toward is to try to see whether or not you thought about it in terms of, how do I spend so much money, or, this is not going to be enough. Between the ends of that spectrum, where did you sit at the time?

LICKLIDER: Well, there was indeed also a little behavioral science money. I thought the computer money was going to pay for about ten laboratories. It was going to take more than that to make a movement, but I could settle for it if I just had ten. I thought I would like to have at least one that was well over a million dollars. I thought that about 300,000 dollars is as low as you can go and still do something, because you've got to have a computer, and you've got to have people to make it run, and so on. So it was just back of the envelope calculations. I felt that I could be happy if I never saw any more money than this. But probably the most important thing I could do was get some stuff started very fast; get things committed so it can't go away. The other thought was that I really wanted to set this up on three-year funding, because I was not going to be here too long and I wanted to get something set up. I would have liked three-year... where you keep moving one year... That essentially divides the amount of money I have got by three, and that makes it look pretty weak.



NORBERG: Why ten places?

LICKLIDER: I had this picture that Cambridge is a good reinforcing community. There were enough different places doing related things that if you just had a single group doing something, nobody knew whether to believe it or not. But if Harvard had something, and MIT had something here, and there was something at the Lincoln Lab, and BBN had something, and maybe Otto Burnett had something at MGH [Massachusetts General Hospital], it reinforced. I would have liked to see that happen in Cambridge; I'd have liked to see that happen in Los Angeles or the San Francisco area; maybe one other place. There was always in the back of my mind the idea that if we could ever get time we would see if we could not do something in Texas or the Midwest, and create a cognitive center where there wasn't one. I never did really get around to that, although ARPA has tried a couple of times.

NORBERG: So, with just you and a secretary to do this, how did you proceed?

LICKLIDER: Well, I tried to select a colonel with the aid of the computer systems. I talked to a lot of people in the Pentagon. They were very helpful. I said, "You want to get somebody who's really credible to the military. If you can, find a war hero. Also, he's got to know something about science, or mathematics or something." I wound up with a guy who was a war hero. He had escaped from captivity by the Germans twice. He was a flyer. He also had run the computer center in the Pentagon, but the records did not say that. It turned out he had hated running the computer center so much that he tore up his records before he left. But I finally got him anyway. He was an astronomer by trade.

ASPRAY: What was his name?

LICKLIDER: Buck Clevon -- a very interesting guy. Well, it would have been a folly to try to do this, except for one very important fact. ARPA did not write contracts; it wrote ARPA orders, and then some agent went and turned that into a contract, and the agent supplied a monitor to be sure they turned in their progress reports and all of that. I was

already friends with the people in ONR and the people in the Air Force Office of Scientific Research. And I knew people in the Army Research Office a little bit. So we had an infrastructure, and the infrastructure was ambitious and did not have enough money in its budget, so it was very happy to join ARPA and make a team.

ASPRAY: Were these contracting organizations already contracting for ARPA for other programs?

LICKLIDER: Yes. In addition, there was something called Defense Supply Service Washington (DSSW), which was a kind of a captive of the Secretary's office. If you needed something to happen in a hurry, it was possible to get it to happen. I believe I had one from concept to contract in two weeks.

NORBERG: (laugh) We did not get it in two weeks, as you know.

LICKLIDER: Things got much, much, much, much worse, I am sure.

ASPRAY: How much of this budget was allocated to the SDC people?

LICKLIDER: Well, I think it started off nine million, or seven million dollars; I have forgotten which it was -- probably seven.

ASPRAY: Did you have some control over how much they got of that, and when, and what they used it for?

LICKLIDER: I do not know how to deal with this. To be frank about it, I was not impressed with some of what they were doing and cut them back a bit. But I did not do too much of that, because I think all of us are convinced it is easier and better to get more money than it is to take money away from people who spread it around. So they came out of it pretty well; they were a little mad at me, but not terribly.

ASPRAY: Can you tell us what they were doing?

LICKLIDER: Yes, they were doing command and control research. They were doing data base research. One of the really good things they were doing, I think, was that they had the first research, the first thoughtful approach, to how to deal with large data bases. They were interested in complex programming, and had a project (which I probably will not remember the name of), which was essentially just an excuse for making an extremely complex program, that allowed them to learn how to make them stay on top of complex programs.

TAPE 2/SIDE 2

LICKLIDER: They had some work on displays and controls. One thing I liked best of all the things they did was the transparent or translucent desks with projectors that allowed the computer to draw stuff on this. It had a light pen, sensing equipment, so the computer could tell what you were doing. One thing they demonstrated was proofreading. You would make proofreader's marks, and the computer would recognize the marks, and respond to those commands. So this thing would readjust. That was better than any editing with a mouse I have ever seen. It was early technology, so you had to turn the lights off to make it work, and then you could not read the book, but the concept of it was just beautiful.

ASPRAY: What did you not like about the work that was going on there?

LICKLIDER: Well, essentially I did not like it because it was based on batch processing, and while I was interested in a new way of doing things, they were studying how to make improvements in the way things were done already. They had what I thought was a pretty great asset. They had one of the four new SAGE computers that the Air Force decided not to go forward with. So there were these four machines, and they had one of them. I hated to see it sit there being used as an old batch processor.

NORBERG: So you have a philosophic difference there as to what is going on. Why not encourage them to change, and provide them money for the change?

LICKLIDER: I did exactly that. I even let a contract with a little firm that Fredkin had set up just to give Fredkin time to go take the Cambridge word and transplant it to the SDC. Of course, there were drawbacks about that. They did not like to be told what to do. But it did work, in fact. There is a fellow named Jules Schwartz, who is the father of the language called JOVIAL. Really good programmers are fairly easy to spot, especially for other really good programmers. I will not claim to be one, but I certainly was associated with them, and I spotted Schwartz as a really positive character. We supported him to the hilt. We got him to turn a machine into a time-sharing system, which I think he enjoyed doing. So, it went well. I did not really have a bad battle with SDC, but I was aware that this was cheating a little bit. I would insist on my philosophy, my vision of what I wanted to happen here, and these people had every right to have their own vision.

NORBERG: That's fine. I am just trying to set up an extreme contrast to get you to remember some of these things more clearly.

ASPRAY: You were clearly committed to this time-sharing, or interactive mode of operation. Did Ruina have the same kind of commitment, or would he have been happy if he had developed computing for command and control in the existing way and improved upon it?

LICKLIDER: Well, I think his attitude was something like this; that I perceived that command and control really needed some work. It was recognized as one of the three or four main foci of military effort, but we did not know how to do it.

NORBERG: This is 1962 you're talking about now.

LICKLIDER: Yes. Well, we needed to get some work going. If we had just done command and control studies, we'd done them well, nobody ever asked too much about them, and then maybe we got a little military interest in them, we would have been reasonably happy. I think he saw that, "My gosh, maybe there is a big theme here. Maybe it is possible to develop a kind of computing, called interactive computing, that will make a significant change in it. That would be great. That's what I'd like ARPA really to be doing." And so, without being convinced it was feasible or anything else, I think he said, "Let's go along with it." I think he also had a feeling, "I have got 50 or 100 million dollars in getting these nuclear warheads back in; why can't I have 10 or 15 in command or control without..." I think he saw it as no big deal, and possibly it would work out doing it the way I wanted to do it.

NORBERG: Does this suggest that there were presentations to Ruina, and perhaps to others, by you in the early months that you were at ARPA?

LICKLIDER: Yes. I even went over to the CIA and gave them a pitch. I had to tell them, "Look, I do not know what you're doing about this. I hope you are doing the following. But let me tell you about what I am doing, and then maybe we can figure some way to talk about what the relations are." I guess it turned out they were not doing it, but I did not know that they were not. I tried to make my presentation so it wasn't too parochial in case they did know about it. Similarly, at NSA they really needed what I wanted, and Fubini thought they had it. One of my early jobs was to get in to see that they did not have it.

NORBERG: Do you have any idea why he felt they had it already?

LICKLIDER: Yes, they had quick turn-around batch processing, which was very helpful. I do not know what they have now. I have not been to their place for a long time. We should not talk about it anyway, but I'm morally sure that...

NORBERG: Did any of your thoughts about command and control in this period of 1962 harp back to things that you had observed while on the Air Force Scientific Advisory Board? In fact we might even ask, how long were you on the advisory board?

LICKLIDER: Well, I do not remember that, but I would guess five, or six or seven years -- something like that.

NORBERG: Beginning in, say, 1955, or in 1956 or 1957?

LICKLIDER: Well, I went to a meeting of it after I went to ARPA, although I came off the Scientific Advisory Board because I was now a government employee. So it was leading up to 1962.

NORBERG: So you would be familiar, then, with many of the problems that the Air Force saw in Research and Development with respect to their objectives.

LICKLIDER: Yes, I think that is right. I was very much interested in sensors, and sonar rays, and radar rays, and all kinds of stuff like that from the Lincoln Laboratory.

ASPRAY: Were there other computer type people on the Advisory Board? I know that von Neumann had been on it earlier, and was off, presumably, by the time you were on it.

LICKLIDER: Oh, sure, several computer types. I am trying to think of just which ones.

NORBERG: I would think that J. Forrester was on the Advisory Board in the middle 1950s.

LICKLIDER: That is right. J. Forrester was on. I think George Brown was on it; I am not sure.

NORBERG: Is it possible to separate out military interests from the interests of this community around Cambridge in the use of computers and meeting objectives?

LICKLIDER: I think of it this way: you can't make any clear cuts if you look at the thing in terms of big block diagrams, because what the military needs is what the businessman needs is what the scientist needs. But look more sharply -- look ahead a block. Take speech understanding, for instance. Here, the scientist wants continuous discourse, wants not to transfer the individual person. The military person, or the intelligence person wants to recognize a few critical words -- "We'd like to be able to pick out Secretary of Defense" -- but is less interested in the dictating machine. So when you get down to the specific task they're really quite different. Or take a project done around here: making a computer simulation of a Morse Code operator, so that you can hook the computer in the net with people. That's an artificial intelligence problem, and academics get tremendously interested in it. They are simulating in it the planning capabilities of the person, as well as the reception of Morse Code. Military people want something that will work, and not something that will advance the theory of how to do AI. But they will both be happy with exactly the same project if it has both facets.

NORBERG: I see. So, did you suggest that people build in both facets, or were you the person who was doing the building in?

LICKLIDER: My first tour at ARPA was too short to build in anything. I did not feel much pressure to make a military case for anything. I tried to stay at the top level of the block diagram, and tried to convince people of the philosophy that in general the same thing is needed. If I had stayed longer, I would have had to be specific. I feel a little bit like Bush and Dukakis.

NORBERG: All right, back to the office then. You arrived; you had this money; you had only one contract that you needed to worry about with SDC. How did you go about finding people to do work in interactive computing?

LICKLIDER: Well, somebody gave me a little talk, "Lick, you can not just go write proposals for people. You're a government employee now; you've got to respond to suggestions." So I took advantage of the presence of those SAGE computers. I could take the initiative of going around and talking to people, "Do you want one of these things, and what would you do with it if you had it?" People were pretty sensible. Nobody wanted one, but it did lead to a lot of discussions; and discussions lead to proposals and so forth. So, I was able to get proposals out of MIT, Harvard, the University of California at Berkeley, Stanford, UCLA, and, oh, where else? Quite a few places; I have forgotten how many contracts there were, but they came pretty fast.

NORBERG: To do what? What were these contracts specifically about?

LICKLIDER: Well, some of them were to build time-sharing systems, because we needed to have time-sharing systems before we could do man computer interaction research. So, for instance, Berkeley built a time-sharing system. SDC built one; MIT built one. That may have been it. There was not really very much graphics. We didn't have the facilities for doing graphics. There were contracts on displays, controls, data bases, organization of the main computer interface. Stanford Research Institute did work on these. Doug Englebert was already working there before I got there, but he was working on a shoestring, and we funded him pretty well.

ASPRAY: How did you identify these centers? I mean, MIT is obvious, and maybe one or two others are, but you listed a range of places, like UCLA and Berkeley.

LICKLIDER: Well, partly it's people; partly the reputation of the university. I had been going to computer meetings for quite a while. I'd heard many of these people talk. I do not know how to deal with that question. There is a kind of networking. You learn to trust certain people, and they expand your acquaintance. I did a lot of traveling, and in a job like that, when people know you have some money it's awful easy to meet people; you get to hear what they are doing. (laugh)



ASPRAY: Right. Well, let me ask it in a different way. You do not map one to one onto all of the established computing centers at the time: there is a fairly large center already at Wayne State; there is one at University of Michigan; there is one at Georgia Tech. But I do not see money coming to those places; I see them going to other places.

LICKLIDER: Well, I am surprised about Michigan, but I guess that I spent behavioral science money at Michigan. It seems like I was in Ann Arbor a quarter of my time.

ASPRAY: I do not know for a fact that you did not award money there.

LICKLIDER: Well, I think there was money at Michigan -- probably behavioral science money -- but I am not really sure. I was there a lot. I knew some people there -- especially Tanner, Swets, and Green of signal detection theory. I am really amazed if I did not fund something of that.

ASPRAY: But, am I interpreting what you say correctly when I say, the individuals were more important than the existence of a well-developed computer center and program at an institution?

LICKLIDER: Well, the thing is that we had computer centers all over the country even then. There was a big computer and arrangements for putting in your deck of cards and getting out your printout. But in most of those places there was not much promise for the research I wanted.

NORBERG: One of the things that seemed to emerge from the meeting over the last couple of days here at MIT was a sense, on my part at least (I have talked to Bill about this as well), that in that period, somehow, MIT -- that is, the community here in Cambridge and Boston -- was thinking of computing in a way essentially quite different, maybe even new, from people in other parts of the academic community -- certainly different from people in industry. It was trying to strive for some sort of discontinuous circumstance where a new world of computing would develop out of

the research. What is not clear to me, first of all, is whether others did not have a similar sort of vision. That we would have to inquire about. But secondly, whether or not enough of the required pieces to make that work -- the displays, the interfaces, the expanded memories -- were seen as part of an overall plan at that time, each piece of which would need to be attacked in order to achieve interactive computing. The way you just described your visits to the various groups suggest that such a vision might have been in your mind, but it isn't clear to me whether it was a clear vision at the time.

LICKLIDER: Well, it surely was not clear. Take this area here. Here's a mass; here's a keyboard. A fellow named Herb Teager thought that you get rid of that, and you put a sheet of plastic down here, because it was an inductive coupling. Go out to Rand and it was the same thing: "It is a sheet of plastic; it is capacitated coupling." My thought was, "It does not matter which they use, but we better have two projects, because people tend to goof up on these." And then the Mouse... Engelbart had a mouse. There was some difference of opinion about how many buttons there ought to be. "Oh, that's trivial. Why don't we get somebody to go in to do research on how effective a thing like this is. Is this a function of off-set, and comparing this with light pens?" and so on. "Herb Jenkins does that at the Lincoln Laboratory. Maybe if Engelbart wants to do some of that, that's fine. But let's try to think of all of the schemes -- horizontal and vertical scopes, languages. Let's not get into the language business, but let's see if there are essentially new ideas about languages." And so on. Well, I used to draw big sketches on big sheets of paper. Then I would lose them. But I had pretty well wrapped up in me all of the topics that it would take to put interactive computing together. I deliberately talked about the intergalactic network, but deliberately did not try to do anything about netting them together, because it was becoming very difficult just to get them to run. The concept that would say: this is pertinent, this is relevant, and this we can let alone for now; so I would create a network of contracts in which one place might do some subset of things that did not necessarily fit together to make a total system. But if I was going to be successful, I had to have some kind of a system here. Maybe have one place interact with another, get these guys together frequently, and have special Computer Society meetings. We would get our gang together, and there would be lots of discussion, and we would stay up late at night, and maybe drink a little alcohol and such. So I thought I had a plan at that level. I could talk about it to people in ARPA. It was easy to have plenty of topics

in the outline carried down three or four levels if I found a guy who wanted it that way. I even invented a way to keep books on how much money was being spent on what, that did not say, you know: "Here's this contract and we will put this in this category." Instead it said: "This contract is dealing with these issues, and the money is..." I could take the whole pile of things and say how much money went to each part, and what the interactions were. That blew the minds of the accounting people; they had never seen that. Still, they didn't heckle me much about being the manager there. It was a different dimension. "Oh, go talk to him and he will talk your arm off for two or three hours about that. Better avoid him." So, to come back to your question, it was not a clear vision, as certainly, not that: "We'll plug them all together, and that will be the system"; but, rather, it was a matter of getting a supply of parts and methods and techniques, and different people will put together different systems out of it.

NORBERG: Okay. Were you using advisors at the time, either unofficially or officially?

LICKLIDER: I do not think there was ever anything official. This was a very small operation in the Pentagon. I did not have that much time to talk with people, to bring them in and spend the day with them. But there was lots and lots of talk in the contractor community, as soon as they got some contractors. I think we even told them that the future of this program was very much going to come out of how they suggest things.

NORBERG: How much overlap was there between your procedures in running the command and control and later IPTO, and other operations within ARPA? Did you talk with the other program managers, for example?

LICKLIDER: Yes, a little bit, but not really very much. They used computers, and I talked with them about their uses of computers. Occasionally we'd try to dream up some joint thing. But I think at that time nuclear explosions were being monitored, and that was generating a tremendous lot of data. I had talks with them about what we could do. In 1967, I think -- or was it earlier? -- IBM got around to delivering this trillion bit... I forgot the relation of the trillion bit also. I think I may be into my second visit to ARPA. That is the trouble there. I was excited about really big stores and going back to the library.

NORBERG: I guess it was not the technical side that I was interested in exploring in that question (although you answered it quite nicely), but it was the management side. Did you discuss with them what their procedures were, and did you then apply those procedures to your own operations?

LICKLIDER: No, I viewed myself, and they viewed me as not a very good manager, but didn't think this was essentially a "manage" job. I could always say, "Well, ONR, and the AFOSR, and organizations like that are seeing to it that the progress reports get written."

NORBERG: So, their objection was a detail objection, rather than a programmatic objection about management, and whether or not you were a good manager.

LICKLIDER: No, they were always kind of amazed that I wanted to travel light. Some of my colleagues liked to have a big office with quite a few people in it. My attitude was, "Time is of the essence. It has gone by, and I don't want to take time to hire somebody. Not when we've got these good people already." There wasn't much time spent on that. The worst thing that happened to me administratively, or managerially was that there was an old-line bureaucrat in the Secretary's office, who really wanted me to spend my money on something quite different from what I was spending it on. I guess I would repress that guy's name. He got somebody to write a letter saying, essentially, that he didn't see real value in what I was doing. This kid who was picked to write the letter came to talk to me. We weren't close friends, but I knew him, and he didn't want to do this dirty trick. He came around and told me about it. I told him, "Well, gee, Russ, I really don't want that letter to get over the system." And he said, "Well, I think the best thing to do is that I forget about it and you forget about it. I'll let you know if there is any more pressure about it." And it never raised its head again.

NORBERG: What was the letter about? I don't understand.

LICKLIDER: It was just an expression of non-faith, or disbelief, that that's not the kind of research we need. I did have a kind of a battle with Wimmix (?), and I'm not sure whether that was really well-formed when I was there the first time, or whether that came later. I'm afraid I flashed out some times in frustration. I said something like you guys are just running a big batch processing center and haven't got your stuff networked enough. It was being run by a guy from IBM whom I vaguely knew. But that wasn't really much of a battle either. They were too busy. They were chronically six months behind on their programming. They didn't have time to fight.

ASPRAY: Was there a certain ethos about there being an ARPA way of management at the time, say in the other offices?

LICKLIDER: I don't think ARPA had convinced itself... There came a time when ARPA thought it was awfully good. I think the first time I was in ARPA that hadn't really emerged much. Ruina was brilliant. Sproull, in a different way, was at least as brilliant. The third floor E-ring was populated by very bright people. McNamara's whiz kids, and so forth. So, there wasn't much bureaucratic fighting that I saw. I told you about these two instances, which were the main ones that I could think of. There were problems, like some character came around with a gun that shot pellets, that shot little rockets -- 50 or 100 little rockets -- and demonstrated it in my office. These things got going and the place was left a shambles. There were administrative problems of that sort. (laugh) We were lucky not to be blinded.

TAPE 3/SIDE 1

NORBERG: Okay. When did Colonel Cleven join your group?

LICKLIDER: Well, it took some months, I guess, before he arrived. He joined fairly early.

NORBERG: What was the division of labor then between the two of you?

LICKLIDER: Well, that wasn't fair to Cleven. You can't imagine how many people, when they hear you've got money, want to come sell you something. Most of it was unbelievably irrelevant or low-grade. I just took advantage of Cleven and made him listen to all those things. He could be very charming; he was fantastically charming. Then he would say, "Now, I've had all that I can take. I'm going on a trip or something." And he would go visit the contractors. But he was a very good guy. Unfortunately, his main job was just to make the visitors feel good, and not give them any money unless he could spot that they had something -- which was one in 30 or 40.

NORBERG: Would it be fair, then, to say that he was a screening mechanism in the office, and that was his principal task?

LICKLIDER: Yes. Well, he was also fantastically personable. So if there were some unpleasant task to do, a progress report had to be turned in or something, he could drop in on the contractor, and get it done. Well, he was wonderful. He could explain about bureaucracy, "We all hate it, and so forth. It would really be helpful if you could get that thing done." He was a troubleshooter, as well as a screener.

NORBERG: Was he helpful in any way with the military side of the Department of Defense?

LICKLIDER: Yes, he knew the protocol. He knew, for instance, that it was very important to keep everybody who ought to be in the piece identified with it and knowledgeable about it. So he had lots of meetings. I remember once getting in all the people who dealt with lie detection. That was on the behavioral science side. I have never seen such a meeting in my life. These were all guys from the intelligence agencies of one kind or another, and they wouldn't talk. They absolutely would not. Happily, I had brought the guy from the Harvard Medical School, who was a national authority, and he gave a lecture (laugh).

NORBERG: In the early years of ARPA, from its beginning -- essentially 1958 --through sometime later, there was a considerable amount of sometimes open, sometimes not so open conflict between the military services and the

director of ARPA, maybe even some of the program people. At least in the early years, there was a sense of impending doom on the part of some of the ARPA people, that indeed, their mission might be wiped out in the Department of Defense. Did you sense any of this when you came, or had that disappeared?

LICKLIDER: No, I didn't even quite realize that was the case. In my area we were cheating a little bit, because when we talked about the military we talked first about ONR, AFOSR, and the Army Research Office. Well, Marv Dennicoff and ONR were close personal friends... I knew if I shook hands with him about something there was no question. Charles Hutchinson in AFOSR. Then the next step would be into operational military that didn't really care much about research programs, but where we would... We did a lot of visiting of Fort Knox and the interior of a mountain of Colorado, mainly so that we would be surely current about applications. The military development people weren't really in our circuit. I think when it comes to the Strategic Defense office, or the Tactical Defense Office, or the Reentry Physics thing, or any of those, they had real competitors in the military. But what might have been our competitors were really our agents and our friends. I felt more threat, really, from secretary's offices in the E Ring, and not too much there. There was, for instance, a guy who was in charge of inventory of some kind, I guess. He had 24 data bases and I've had a hard time avoiding the assignment of making his 24 data base interactive. So he had the picture that he was going to sit in his office at something, and talk with these 24 data bases. I studied it a while and I said, "Oh, my God! Most of them are batch-oriented. There's millions of dollars to spend transcribing records. I've got to stay out of that." It was unbelievable, but he had this kind of a terminal, not hooked to anything, and said, "Licklider, what I wanted you to do is to hook this up to my 24 data bases." He wasn't wanting my money or anything like that, but I could have taken on a responsibility in a relation to him that was just beyond my power to tinker with. I mean, I could tell him, of course, "Ten years from now, if we're successful, you're going to be in command of all of that. That's where we're going. But let's not make... Sorry."

NORBERG: Well, there was one little piece of jargon in there that I didn't understand -- the E Ring?

LICKLIDER: Oh, in the Pentagon the concentric rings are A, B, C, D, E. E is outside. The second or third floor is the most prestigious, depending on if you've got a uniform or not. The Secretary of Defense is on the third floor in the E Ring.

NORBERG: I see.

LICKLIDER: And your proximity to the secretary's office is important. Well, first you've got to stay on the E Ring if you're anybody, because they've got outside windows. Then the closer you are to the Secretary's office, the more important you are. I was in the D Ring, so nobody was trying to get in my office. But I was still close enough; I could interact with those bright guys. And they were bright. Oh, what a fantastic bunch of people!

ASPRAY: At the time that ARPA starts funding computing, the money from the federal government is coming from ONR, from the Air Force, starting to come from AEC. How did your presence disrupt or change the way that the funding patterns came at the time?

LICKLIDER: Well, there was a bad feature, because the amount of money we had, although it was a pittance in modern terms, was pretty much then and its presence could be used, for example, as a reason why ONR didn't need any more money in the computing field. You know: let ARPA do it if ARPA has got all that money. On the other hand, we did have meetings. A gang of people who had some computer money to spend got together. For a time, I think we had a meeting every month. And we were fairly factual with each other about what we had. So to try to answer the question, I think that the fact ARPA had a fair amount of money, was inhibitory on the others, but they did help ARPA spend some of its money, and so in some sense they had more than they'd have had without ARPA.

ASPRAY: Was there any effort to divvy up the field in certain ways?



LICKLIDER: Yes, Dennicoff would fund artificial intelligence. Most of the others were afraid to try it. They wouldn't be able to defend that. The Air Force Office of Scientific Research had funded Engelbart on his interactive office concepts, and continued to do more, I think, in the -- the paperless office concept, and so forth. But there was no sharp division, no charter that said, your responsibility to this is this.

NORBERG: Okay. Can you give an assessment, then, of what you think your accomplishments were in the two years in the first tour in that office?

LICKLIDER: Yes, I think that I found a lot of bright people and got them working in this area, well enough almost to define this area. I got it moving. I think maybe the best thing I did was to pick a successor, Ivan Sutherland, who was surely more brilliant than I and very effective, and who carried it on. I think that the main thing ARPA has had is a series of good people running an office, and a fantastic community. I guess that's the word. It was more than just a collection of bright people working in the field. It was a thing that organized itself a little bit into a community, so that there was some competition and some cooperation, and it resulted in the emergence of a field.

NORBERG: How do you think that emergence of the community occurred? Was there a particular mechanism that you think stimulated it in that period?

LICKLIDER: Well, let me just make claims about what I did. I think I was a good picker of people. I had always, since getting started in Cambridge, just looked for... I told you that. I used Miller analogies to ? ?

ASPRAY: Yes, you did.

LICKLIDER: Of course, I realized I could not go give a professor at Irving an intelligence test, but I was just deliberately trying to get the best people I could find, those who were interestable in this area, into it.

NORBERG: This sort of thing continued after you got to ARPA?

LICKLIDER: Yes. Then maybe the second thing would be that I did work very hard on trying to make friends and contacts in the Pentagon, and I had had enough pre-indoctrination with the military that I could at least talk with them about... But I wasn't as good at it; my relations with the world of research were better than with the military. I would make a stronger claim for having gotten good research people.

NORBERG: Yes. I'd like to go back to my earlier question, though, about what you think your accomplishments were during the first tour of duty in terms of actual developments that occurred in the research community at that time. What things would you cite from the technical field as most significant?

LICKLIDER: Oh, I'd almost rather not deal with that, because there was a real problem there. I went for one year and stayed for two, and the time scale for doing anything significant is longer than that. So I had to buy into things and finish them and make them demonstrable, so that there would be something for people to look at without realizing that I didn't do them. ONR had had Corby started on the time-sharing system and the computation center, and he came along and greatly increased the speed with which that project was going. We could never have started it and gotten it going. Berkeley had a time-sharing system running on an SDS computer before I left.

ASPRAY: Had they already started time-sharing before you came along?

LICKLIDER: They hadn't started time-sharing, but they had a laboratory and a computer and everything else, and the time-sharing part was relatively simple. What looked good when they demonstrated it was that it had some graphics; they had applications that were interesting and exciting. At SDC, well, I think their time-sharing system ran before I left Washington. But they had a hundred or two hundred programmers, or something like that, working before I got there. We were responsible to channel that a bit, but... So I think that there's nothing I can point to with pride and say, "I did that."

NORBERG: All right, I'm going to ask the question yet another way. Did you set conditions on the awards that you made to these people?

LICKLIDER: Set conditions?

NORBERG: Yes, what were the conditions of making a contract with, say, MIT?

LICKLIDER: Well, in that one, my main condition was that they should produce a proposal that would be a statesmanly work, because it was going to be my first one, and I wanted a real good proposal. I wanted interactive computing; I wanted time-sharing. I wanted: "Computers are as much for communication as they are for calculation." A lot of these themes in it. Then I wanted assurance there were going to be good people working on it. I wanted a summer study that would bring people from all over the industry, that was going to try to shape up this field and make it clear what we were doing. I also said that I wanted a lot of help, although I didn't want that written in the proposal, I wanted a lot of help.

NORBERG: What do you mean by a lot of help?

LICKLIDER: I wanted to be able to get an MIT person to visit SDC, or I wanted people to take time off from their research to have meetings to think about how all this was going to go.

NORBERG: Did you think about giving a contract to anyone, whether it's MIT or anyone else, which would be a large amount of money that they could then decide how to distribute among the various groups that were doing research? Or was this prescribed before you received the proposal?

LICKLIDER: No, the Project MAC, for example, did not prescribe what the groups should be. The reason for calling it Project MAC instead of a laboratory was that it was going to use existing labs to a considerable extent. Existing research groups were going to come into it. MIT had a lot of flexibility in just which projects to do. But it was clear that there's got to be artificial intelligence. There's got to be time-sharing. There's got to be interactive stuff like the Teagger Tablet and graphics like the thing called the Kludge, that Sal Loventhal did. I would have been very unhappy not to have Sal Loventhal modeling protein molecules, because, to me, that's going to be a dramatic step. I really had to have that, and lots of other things, like the civil engineering Stress and Strudel and those things. I didn't really hear much whether they were in or out. They did exercises on the system and were perfectly good things to develop, but what I needed was an inventory of examples to make things concrete. Then it could be that one or another ?

NORBERG: How closely did you work with the proposers?

LICKLIDER: It ranged. In the case of MIT, Bob Fano worked the proposal. I talked with him at some length on two or three occasions. But I knew Bob very well, and he was a good writer. I felt bad, because this was just absolutely crucial to me, because it was my first one. A lot of people were going to see this, and he had to make a good one. In other cases, I practically wrote the proposal myself, because I could not beat into the head of the research guy that you've got to have milestones. Don't ask why you have to have milestones.

NORBERG: Well, the reason that I ask that question is in reading over the MAC memorandum in the spring of 1963, you were organizing a meeting to be held in Palo Alto, which was to take place in early May. A memorandum went around saying that it was going to be impossible for you to be at that meeting, and so could people shift to a week later, because you had to spend a week at MIT -- no reason given. "For externally driven reasons," was the phrase used in the memorandum. I'm not asking you to try and remember what that week was about. I think we have to look for some other records to determine it. But what I was trying to ask here is, was this an unusual occurrence, or do you remember spending a week at a time at many places, and did it have to do with proposal writing?

LICKLIDER: Well, I spent a lot of time at other places, and sometimes, I'm sure it had to do with proposal writing. I had the feeling that, having talked with all these people in the Pentagon, I knew some things about proposal writing. I had written many, many proposals in my life. When I would see one that I thought was just hopeless, that was going to cause me trouble, and I really wanted research done there, and I knew this was a bright guy but he's just not good at this particular department, I think maybe I tried to help.

ASPRAY: To what degree did you determine the research areas and projects? I mean, how much was it that you were sitting back, or visiting and finding out what these people really wanted to do, and how much was it that you had in mind that it would be nice to have a project in area X? And so, you were sort of touring, telling people that, waiting for the right response?

LICKLIDER: Well, those two don't have to add up to one, because if you visit laboratories, you can be getting a lot of great ideas and seeing concrete things, and at the same time you can be trying to explain to them what you think you need, and look for some kind of convergence, or at least some kind of harmony. In some sense, everything I knew I got either from thinking myself or from visiting labs and talking with people.

ASPRAY: Let me ask the question a slightly different way. Did you find people who approached you about doing research projects that you thought were technically feasible, and perhaps even important research, that you didn't fund because they were in areas that were outside a few that you were concentrating on?

LICKLIDER: Well, for example, a fellow came to give me a pitch about mathematical technique in engineering. I've forgotten exactly what it was, but it had to do with digital system functions. I knew enough about it to see that this was a real good idea, but I didn't know enough about it to know whether it was original with him, or whether if I talked with other mathematicians that they would tell me, "Oh, yes, we know about that." I didn't fund that, but I did try to help him get connected with people who had mathematics money. I didn't fund it, because, essentially, I would have

clobbered myself, if I got to doing too many things that I did not understand really well enough. Although, I was pretty well convinced that this was great. Another example was when a guy had a twist on the general theory of relativity -- a somewhat different approach to it that came out looking very similar. I went to the trouble of getting some physicists to study it, and try and advise me. Finally, I decided not to deal with that. But I think Larry Roberts, who succeeded me down the line, did fund it. I think by that time Larry had the excuse that he had some equipment that would facilitate doing it, and he could always say he was using this as a driver. But I don't know whether this is getting at your question. I felt constrained to the general area of interactive computing. I was not afraid to go make little scallops outside it if they seemed particularly exciting. In a behavioral science I had less limitation than that. I was funding work on pain, and neuronal work, as well experimental psychology. Anyhow, I funded a guy in Toronto that I thought was going to win the Nobel prize. He didn't, but his work was very favorably received.

ASPRAY: Can you break up the interactive into a few categories into which we can fit the research that you funded during that period?

LICKLIDER: Well, I don't remember what categories were in my mind then, but sure: operating systems, time-sharing systems and the like, graphics, data, databases, datatypes, languages, displays, controls, theory of algorithms -- it sounded like almost any computer science center laboratory, all focused on interactive computing. I probably spent more time with more energy than most of my colleagues on what I thought of as not an interface, but an intermedium - the skills and capabilities that the operator needed and could develop to get a system together that was not going to leave the situation static, but was going to improve both the computer through reorganization, and the user through making it possible for him to learn stuff as he goes. I had the idea of a motivational trap. I wanted somebody to make the computer an interactive system that people wouldn't, in fact, leave. I had one small success in that with teaching system for kids. They would stay with the machine and they would not go to the bathroom. They'd sit... But I'm afraid the list of subfields is very much like... If you'd look at Project MAC or if you'd look at the Laboratory for Computer Science, it must be fairly similar.

NORBERG: Let me ask two more questions and we can bring this to a close. First of all, did you, in your capacity as program manager, have any interaction with people in the White House? I'm thinking particularly of staff members for the President's Science Advisory Committee.

LICKLIDER: Yes. At that time there was a thing called the Multinational Force, or Concept. It looked as though there might even be people from several different countries on the same ship, or in the same group. So how could we declare war if we have got 15 heads of state. Well, I have since written about teleconferencing, and looked at the history. The earliest pieces I found were from our program at that time that we set up in direct response to the White House's request to get hopping on teleconferencing at the Institute for Defense Analyses. I mentioned psychologist Vamos at MIT -- he had gone to Stanford by then, I guess -- and got him as an external consultant to us. We ran teleconferences. It was very primitive in those days; a teletype machine was about what you had to work with. We'd run conferences face to face, and through teletype, and also, we had some video that was just simulated. So there was that. I'm trying to think whether at that time... Wiesner was Science Advisor through at least part of that time. I had been very close to him here at MIT. So he gave me some chores to do. I discovered that he was really very sensitive. My bosses in the Pentagon didn't like me working for him; I was supposed to work for them. There was another thing going on at that time. From the library work, I was interested in the flow of scientific and technical information. There was a fellow named Weinberg, the head of the Atomic Energy plant in Tennessee, who had written a document about the national effort in scientific and technical communication, proposing a lot of stuff.

TAPE 3/SIDE 2

LICKLIDER: ... ready to evaluate what had happened, and proposed what ought to happen next. So I wrote a kind of a follow-up to the Weinberg Report. That was directed to the White House connection. There wasn't, at that time, anything we had that I could deliver to them. As a matter of fact, it wasn't till one of my last two or three days in the Pentagon that I had a console in my office. It was connected to computers here and in California. When Ivan Sutherland succeeded me, he had a steady stream of military people, including generals and admirals coming to play

with that console. He found that he had to put it in a little room that was big enough for just one person, because if an admiral were sitting at the console, and there were junior officers looking, the admiral was afraid to move his fingers for fear he'd reveal he didn't know what to do.

NORBERG: And my last question is, how did you come to choose Ivan Sutherland?

LICKLIDER: Well, I had known Ivan for a while. I thought he was a brilliant person, and he was a true believer in the things I was a believer in (and, in my view, better at it). I had lots of talks with Sproull, and Charlie Herzfeld, and several of the others, about if it is going to be possible for such a young guy to make it in the Pentagon. They worked hard on how to set it up. For instance, a colonel couldn't report to this guy who was a second lieutenant, or something like that. They got enough arrangements made so that there wouldn't be protocol problems -- or not too many of them. There is one other aspect of it. Several of us in the field, who realized that Ivan was being wasted in some job in Michigan that he'd been assigned to, got him moved to the National Security Agency.

NORBERG: Was he a military person at the time?

LICKLIDER: He was in uniform -- second lieutenant. He was such a success at the National Security Agency, and developed such a following there that the ARPA people thought, this is really going to be pretty good to have. That's always been a kind of rough and rocky connection for us. So that's how come that Ivan... Also, you might say, it wasn't easy to get a successor for me. Most of my colleagues would much rather spend the government money doing research back in the lab than coast another year or two or three in Washington.

END OF INTERVIEW