

## **A TEI Project**

# **Interview of Leonard Kleinrock**

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## **1. Transcript**

### **1.1. Session One (May 20, 2013)**

FIDLER

Let's begin with your early life and background. How did your parents arrive in New York, and what was their background in Europe?

KLEINROCK

So my mother came over first. She was four years old. She came over in 1911. Prior to that, her mother, who was widowed, took her eldest son, came over to New York to basically create a small livelihood, generate enough money to bring them over. They were extremely poor, living in a city in Poland called Rzeszow. My grandmother worked for over a year, finally was able to call the children over. The eldest daughter was in charge of those that were left behind in Poland. The eldest daughter was my Aunt Minnie, and my mother became so attached to Minnie, she was convinced that Minnie was her mother and had a difficult adjustment to her true mother when she met her back here in America. In fact, when they started to come over, they got to the port in Europe to come over on the ship, and they got turned back because my mother had some kind of a problem. She had a little hole in her head, actually. It was some kind of disease. So they all had to go back. My grandmother had to generate some more funds. Finally brought them over. So they settled in New York, and my mother grew up with her other five siblings, very poor, extremely poor. I'm not sure exactly where they were living. Part of the time, I think, was in Lower East Side, but it was in Manhattan, in Jewish neighborhoods. My grandmother was a midwife and she was—that's what I remember about her. But the children all went to high school but, for example, my mother did not go to get an academic high school degree. She went to get a commercial degree and became a secretary. By the way, she was also a beautiful woman, says I, and she actually did win a number of beauty contests, and she beat out, in one of the local beauty contests, the woman eventually became Miss America. So I'm very proud of my mother. Now, my father, his story is a little less fortunate. He was born in 1905, and when the war broke out in 1914, his father was taken away into the army. My father relates a story that one day his father was brought to him in a wagon with other people, and his father said, "Son, I have to go to war and I may never see you again." And, in fact, my father never did see him again, and it was traumatic for him. He was born in a city called Rudniki in Poland.

It's now the western Ukraine. He was now living in a larger city called Podhajce, where he was sent to live with his grandmother where he went to elementary school. But when the war broke out, everybody started running away from the Russians. He was living with his grandmother and his uncle. It was just too difficult. They turned back to go back to Podhajce, and the Russians were very good to them. But as the war worked on, by the time 1916, there was extreme poverty and almost famine and disease, and his grandmother passed away. And then his uncle, who he was living with, was taken off to war. His uncle managed to place him—I'm probably going into too much detail, but—

FIDLER

Not at all.

KLEINROCK

His uncle managed to place him with a neighbor who promised to take care of my father, and within a couple of months, threw him out because he got into a fight with the other kids there. So now my dad was a waif in war-torn portion of Poland, which is being passed back and forth between the Kaiser's troops and the Russian troops, and he lived with a gang of other kids, watering whatever horses from whatever army came by and feeding them. In the summers, he was sort of okay. In the winters, they were freezing. Soldiers were good to them, but he was just a wild kid on his own, no education, Jewish kid, of course, but the whole area was Jewish. He managed to get by. At the end of the war, his uncle came back, full of shrapnel, in 1919, and within two years his uncle decided to go to New York, where my father's aunt, the uncle's sister, had already settled. So he came here in 1921 when he was sixteen, and he was a tiny tot of a guy because he was seriously malnourished, and on the boat over, everybody was seasick, but not him. There was a lot of food left over, so he filled himself up pretty well. [laughs] He was thrilled. So he came here and he started living with his aunt. He started working as a grocery clerk and attended night high school—well, elementary school; he had no education—and very quickly raced through and got into high school. But the effort was too much, working long hours, very heavy-duty work, you know, carrying heavy boxes, long hours. And by this time, he was also thrown out of his aunt's house. He knocked out one of the sons. I'm smiling because I guess I'm proud of that, but I shouldn't be. [laughs] So he was now living on his own, and he fell in with a bunch of guys who he spent time with and he palled around with them. That was the phrase he used, "palled around" with them. And you could see how much he needed companionship, having grown up in this environment where he had no stability. So he dropped out of school and started hanging around with them, and it was shortly thereafter that he met my mother. She was a secretary at the time. And she asked him what he did, and besides being a grocery clerk, he said, "I go to Columbia," and that was true. He was going to Columbia to go to the gym to work out. [laughs] But they hit it off and had a long courtship. But, you know, Dad was working his way up as a grocery clerk, but it was very difficult. They were very poor. So that's probably enough of that early history.

FIDLER

Was it a particularly religious or political family that you grew up in?

KLEINROCK

Neither, neither. They were very Jewish, very ethnic, and the language from my grandmother was Yiddish. My father spoke perfect Yiddish. But, no, it was not a religious family at all. It was not a political family. They were struggling to get food on

the table, make ends meet, and try to put together a small life. And what my dad did is he—probably around the time—my parents got married in 1930. My sister was born in 1932. I was born in 1934. It was around that period when my dad joined with one of the fellow clerks, and they decided to get enough money to buy a grocery store on 163rd and Broadway in Washington Heights. And they struggled, and he grew that store and made a success of it. He was extremely ethical, honest. He had a reputation for being straight, and that helped him a great deal. He was able to borrow a little bit of money from his accountant, which there's a long story there as to how his partner said to him, "Listen. We can't continue as partners. Either I buy you out or you buy me out." And he knew my father had no money, so he figured he'd be able to buy my father out. My father went to his accountant and borrowed, I think, about \$1,000. And when the time came, the partner said, "Can you buy me out?" My dad said, "Yes!" And he bought him out, and so he had his own store, and he worked in different stores and did very nicely. And around that time, I was born in 1934 in the midst of the Depression, of course.

FIDLER

Can you tell me about the neighborhood that you were born into and grew up in?

KLEINROCK

So I was born in a hospital in Harlem, but we lived in Washington Heights. That's not far from the George Washington Bridge. It was a very poor neighborhood. We were on the wrong side of the tracks, if you will. Up in that area, if you were living west of Broadway, it was the good neighborhood, and east of Broadway was the poor neighborhood. We went from apartment to apartment, and the one I remember most is we lived on 175th and Amsterdam Avenue, and it was neighborhood where myself and one other kid were the only two Jewish kids in a dominantly Catholic neighborhood. In fact, there was a Catholic high school across the street from where we lived. It was very difficult because there was a lot of discrimination, and I was constantly running away from gangs. They would sic their German Shepherds on me. But I grew up as a kid, you know, you're in the streets there. You grow up and you learn your way around, and I had to grow up tough, take care of myself, and learn to work the streets, you know, be careful where you go, how you get cornered, etc., but this is all pre-school. I very much enjoyed my youth. It was very poor. I didn't know we were poor, if you will. My mother was home all the time. She was an extremely warm, loving woman, thought the world of me, and that turned out to be very important. But it was difficult. My dad was working. He was hardly ever home, and he was very strict, and he came from the old school. Remember he grew up in that tough arena, no parents, a bunch of wild kids, and the whole social milieu of that part of the world was that children obeyed their parents, and as soon as they could, they would have to contribute economically and physically to the support of the family. And so my dad demanded a lot of obedience. My mother was a softer one. But my dad was hardly ever home. He'd work from seven a.m. to eight p.m. I'd be asleep often when he came home. Except on Sundays, he worked a half a day. And yet I admired him greatly. He was a strong guy. He had a large chest, terrifically strong arms, and I remember he always had a pack of Chesterfield cigarettes in the pocket of his T-shirt. But it was a difficult neighborhood. We were poor. My sister started going to elementary school in the bad part of the area, so they decided that when I would become elementary-school age that they'd move to a slightly better area, because by now my dad was doing better in the grocery store. So we moved just west of Broadway, one block

from the elementary school, PS 173, which I attended. So I did attend that school, and that was a nice mix of smart kids, really smart kids in, if you will, gifted classes. So I was in many ways separated from the harsh environment of the tough, unruly, disobedient, and nasty kids. A lot of Jewish kids, a lot of non-Jewish Christian children, all smart, and it was a terrific school. I really loved school. Until I went to school, I'm told I was a really wild kid. My mother couldn't take me into a store because I'd break things, you know, bumbling around, touching things. They would tell her to take her business elsewhere. But somehow when I got into elementary school—I remember this, and it's strange, where I somehow changed and I became milder, more obedient, because I was now in a structured environment where there were clear expectations put on me by the environment at the school, you know, no nonsense. And I liked it because it was structured, there were things to do, there were challenges, it was clear what was expected of me and what I had to do, so I somehow warmed up to that very well. And, you know, I was with a bunch of guys, but I still was, quote, "different" in my own mind because I was poorer than most of the kids. They had been living in the good area all the time, they had nice clothes, and they had an environment where kids and other adults would come to their apartments. It was not the case in my family. There wasn't a flow of people. I never brought my friends to my house.

FIDLER

And this is during PS 173?

KLEINROCK

During PS 173.

FIDLER

Because another question I was going to ask was how did you understand your socioeconomic background as I child, and I assume that changed somewhat over the years. But by the time you were at PS 173, you did understand something about having a particular background, a particular status.

KLEINROCK

Yes. I always felt poorer than the other kids. I saw what they had. I saw the things they did. I saw their parents were more engaged. I remember after school some of the fathers would come out and they would toss a football around or play punch ball with their kids. I had none of that. And occasionally I'd visit their homes, and it was different. It was neater; it was organized; it was sane. Ours was a little more hectic and unruly in a way. But I did feel different because most of the boys were very much into baseball, professional baseball. They would know all the players and the stats and the games and the scores. And not only didn't I know, I really didn't care. I wanted to play softball, you know, go out and play football, not talk about it. So we had little clubs. I was in a group. We called ourselves the Avengers. We had a baseball team. It was actually softball. Had a great time. But they spent most of their time talking about those stats, exchanging baseball cards, and somehow that's not where my interest was. I was more interested in reading books about astronomy. I'd go to the library, sit up all night. I was interested in science very early on. In fact, the things that interested me then were gadgets, puzzles, games, and comic books. I was really into comic books a lot. I just knew I had a different level of interest and a different socioeconomic background, as you say. And then I was eleven, I was in fifth grade, my father got extremely ill. In fact, we didn't have a separate bedroom for the kids when we were living on Broadway. My parents slept in the

bedroom. My sister and I slept in the living room. My father got extremely ill, and he was coughing all this one night. Next day, I went to school and when I came home, I saw them taking my father out into an ambulance and was taken away to the hospital, and he had a serious, serious case of asthma and bronchitis, and it really took him down. I was eleven years old. He had a grocery store by that time, as I said. He owned one, and he had done very well. This is now at the end of the war in 1945. So he'd done well in the war, but suddenly he was earning nothing. We had to live on whatever savings he had. He couldn't work. He had to sell the store. My mother was not working. Shortly thereafter, we moved back on the other side of Broadway to St. Nicholas Avenue. We lived there until I left home, spent many, many years there. It was in an apartment with one bedroom, which my sister had, and I slept in the living room on a fold-up cot. That meant every morning I'd have to fold it up again. My dad was at home ill. In the first apartment, there was a bedroom that my sister and I shared. I remember that. I was in the cot in the second apartment. And their bedroom was the converted living room with no door, so he demanded absolute quiet. So imagine young kids having to be very quiet. It was really oppressive, but we had to obey. There was no money coming in. They were getting worried about money. It was leaking away. And yet I was taking violin lessons, had to practice an hour a day. I was a lousy violinist, by the way. And four o'clock every afternoon after school, I'd go to Hebrew school. They sent me to a religious Hebrew school, even though they were not religious. So I had from three to four in the afternoon to pal around with my friends, and then from then on it was all Hebrew school, violin lesson, homework, off to school again next day. And because they needed some money, I started working as a stock boy in a butcher store and then in a children's clothing store early on. All the money I made, of course, I brought home and gave to my parents.

FIDLER

And about what age did you start working?

KLEINROCK

I can't say exactly. It was probably twelve, thirteen. It was after my dad got sick and before my mother went to work. She went to work a few years later, which was a big blow to us, my sister and I. You know, she had been home all the time. She went to work in Barton's Candy Store as a clerk and very quickly rose to the level of manager. She was a wonderful worker. And when we'd go to visit the store, she'd give us samples. [laughs] And we didn't mind that one bit. So the environment, it was very hectic. I didn't even have a desk to do my homework on, and I was working a lot at that point now. By this time, I was in junior high school. From elementary school—I loved elementary school. It was terrific, learned a lot. Junior high school was a kind of place where all the elementary schools fed their students, so many elementary schools per junior high. This is back in the bad part of the neighborhood, and it was a hellhole.

FIDLER

And this is Humboldt?

KLEINROCK

Humboldt Junior High School 115. It was 177th between Audubon and St. Nicholas.

FIDLER

When you went there, just to back up slightly before we get into Humboldt, your group of friends, did they change when you got to junior high?

KLEINROCK

No. We went as a group, so I was with many of the same kids but many others as well, and you had some really wild kids there, you know, gangs all over the place. When I went back there just a year or two ago, that whole place is boarded up with fences and guards. It's still a bad place. It was hard, but the education I got there was okay because we did have these advanced classes, nevertheless. But it's just the lunchtimes and the physical ed, and the breaks, that were hard. You had to watch out what you were doing.

FIDLER

And did you have any choice at all in terms of your eventual move to the Bronx High School of Science and Mathematics, or was that an automatic—

KLEINROCK

Oh, no. For Bronx Science, I was destined to go to the public high school. Well, they're all public high schools, but the one I was destined to go was George Washington High School, where my sister went. But there were these specialized high schools in New York at the time, for which you took exams. By then, I was very much interested in electronics, and so I and a number of my other classmates took the exam to get into Bronx Science, an exam you had to take, and I was amazed that I got in. I didn't expect to get in. It was two subway trips away to get there instead of a bus ride up to George Washington, and I was thrilled to go there. A lot of other kids in my class who I thought were smarter than me didn't get in, and I was surprised. But it was a great move. But by then, I was very much into electronics. When I was much younger, when I was just entering elementary school, as I told you, I was very much interested in comic books, and I saw in the middle of a Superman comic how to build a crystal radio. In fact, I was about six or seven years old at the time, and there was a description in the centerfold which talked about how to build this crystal radio. What fascinated me were two things: one, I could build it out of parts that I knew I wouldn't have to pay a penny for because I could find them around the house; and secondly, it claimed that I'd be able to hear music out of this crystal radio without any batteries, any electricity. It was sort of all free. So I decided to do that. I loved gadgets and puzzles, and this looked like a challenge. So what did I need? I needed an empty toilet-paper roll, which I could easily get at home. I needed some wire, which I could find in the street, to form an inductor, wind it around the toilet-paper roll. I needed a crystal. Well, they explained you can make a crystal out of your father's old razorblade and a piece of pencil lead, instead of buying one. That formed what's called the diode, which you needed for this. And then I needed an earphone. Now, I didn't have an earphone, but I knew in the candy store across the street there was a telephone booth, and I knew you could unscrew the earphone of these telephones, headsets, handsets, and take it away. So I stole that earphone. Still free. Then I needed one more part, which I knew I didn't have around the street or the house, and that was something called a variable capacitor. So I had my mother take me down to the place in New York where they sold all the electronics parts, down to Canal Street. I walked up to the first electronics store with my mother, and I banged my fist on the table and I told the proprietor, "I need a variable capacitor." And he said, "What size?" And it totally blew my cover. I explained to him exactly why I needed it, and I was a little embarrassed that I didn't know what size. He knew exactly what I needed. He sold it to us for about a nickel. It did cost a nickel. Brought it home, wired it up, and, lo and behold, I heard music as I tuned that capacitor. I could change stations. And I was amazed. I mean, this was literally magic. It still is magic, by the way. Anytime you have force at a distance, it really is magic.

Electricity, electromagnetism, radio, it's a wonderful gift that nature has given us. And so I spent the rest of my life trying to figure out how that thing worked. I understand it, but there's still something magical about it. So what I did at that early age is I began to collect old broken radios and cannibalize them. My cousin had a good friend who was in charge of a television store, and there had been a repair shop back there. They had a lot of old broken radios. I got them all. I filled up a closet full of these old vacuum-tube systems, and I unsoldered them and I started making new radios. So I was very much into electronics as a kid.

FIDLER

And was that pursuit a solitary one, or were there different people from different parts of your life that would encourage or participate?

KLEINROCK

That's a great question. Pretty much most of my life as a youth, I'd been a loner. So when I started doing this radio work, it was all on my own. Now, it would have been far more natural to find some other kids who were interested in this and form a club, share ideas, share parts, experiences. Didn't do it that way. I used to build model airplanes a lot, too, on my own, not with other kids, which is interesting. Again, this notion that you referred to earlier, I felt isolated as a kid. I didn't feel part of a group. When I built the model airplanes, for example, I could look out the back window of my apartment into a courtyard, and on Sundays these kids would come by with their model airplanes, but they could afford gasoline engines, and they'd test them out there and they'd spin those propellers and go [demonstrates]. I couldn't possibly afford an engine like that, so mine were all rubber-band-based. Similarly with the radios; it was on my own. I think I missed a lot. But as a result, when I went to Bronx Science, I didn't realize I was destined to be an engineer. In fact, I was resisting the idea because I like to keep my options open. And I was a little reluctant to go to Bronx Science, figuring I would now be channeled to become a scientist, instead of possibly becoming something else which I couldn't even imagine, but I decided to go because it was the best school in the country at the time. So I went, and one of the first classes I took, among others, was a social studies class, and I was thrilled because now I'm going to get more than just science, except when I walked into the class the first day, the teacher said, "We're going to study social studies using the scientific method," and that really worried me. But in fact, it was a regular social studies class full of the real stuff.

FIDLER

And why did that worry you? Can you expand on that at all?

KLEINROCK

As I say, I like to keep my options open. I wasn't committed to be an engineer, nor even a scientist. Maybe a doctor, maybe a lawyer, maybe a pilot. I wanted to be a pilot, by the way. Maybe an explorer. Something else not channeled. And yet little did I realize that by that time in my life, it was already ordained that I would become an engineer. You know, all my interests were going in that direction. I remember in Bronx Science one of my classmates and I were on an elevated train platform, waiting for the train to come. He was a real nerd from Bronx Science. And I looked down. We were probably about 100 feet high on this elevated platform, and I said, "I wonder how high we are." So he said, "Oh, just a minute." He reached down into the gravel, picked up a stone, dropped it, timed it, and told me exactly how high we were. I said, "How'd you do that?" He explained a little

physics principle, and I found those kinds of practical uses of science just intriguing. So I took physics as one of my first courses at Bronx Science, way before chemistry, which is the opposite order, by the way. And physics, well, I just loved physics. In many ways, you know, electrical engineering and physics are coupled. They're the same kind of basic studies. But you're right, not only was I a loner, but I wasn't even socially—I won't even use the word "advanced," but socially adept. Give you an example. In high school, I wore orthodontures, braces for my teeth, and I remember I was probably a junior in high school, one of my friends said to me when he saw these braces on my teeth, says, "How do you kiss the girls?" And my answer was, "I've never kissed a girl." [laughs] So I was really backward and socially backward at the time. I lived in a kind of not economically protected, but a kind of sheltered environment from—you know, I was taking care of myself, managing in this world I found myself, but not participating in it in a way. And by the way, just to jump ahead, when I look at the colleagues that I later interacted with in my professional life, I find many that had similar backgrounds. They were not the guys who traded baseball cards. They weren't into sports statistics. They weren't those kind of collectors. They were often on their own, doing their thing, doing it well, and not making a shared effort out of it. And I actually got a lot of gratification out of that when I realized later I was not unusual in that sense.

FIDLER

I was going to ask how this outsider status affected your intellectual development, but that seems to answer at least a bit of it.

KLEINROCK

Well, it also made it important to me to succeed based on my own wits, and not asking other kids how they did their homework. I remember later in college when we did lab reports and you had to write them up, and they were a long and tortuous thing to do, I never took data or advice or information from other kids. I wrote all my own lab reports—the other kids were copying them and sharing them, and there were older ones they used—only because that's the way I felt I had to learn, and I just didn't like to use other people's material. I needed to do it myself so I'd learn.

FIDLER

Did you continue to work part-time through your time at Bronx High?

KLEINROCK

Oh, yes. I was working many, many hours. I took a job as an usher because we needed the money. So when I'd get home from Bronx Science, I'd immediately rush off to usher. I was an usher in two movie theaters at the same time because they were owned by the same corporation, and I'd work there typically from about four o'clock until seven or eight in the evening during the weekday, and on the weekends, longer hours. So I didn't have much time for play. It was not a difficult job, but it was time-consuming, and I learned a lot about life in that job. Again, there were a lot of gang members, also, in my fellow usherette, and I befriended them and I learned about how they operated, how they lived, and in some ways, they protected me because I was sort of a buddy of theirs. One of the usherettes was a prostitute, and she ran tricks in the balcony during the theater. And just to tell you what was going on there, we'd have to get out of our city clothes and get into these ugly ushers' uniforms, and we just would strip down to our shorts in an open area with the girls and the guys, and every so often this usher would show me the fancy embroidery on her girdle. One day she and I had an argument. She was older than I

was, quite a bit older. And then she looked at me. She said, “You know, I would have given it to you for free.” And my response to myself was, “What is she talking about?” [laughs] I was that naïve at that time. I was really not with it.

FIDLER

And this is during high school?

KLEINROCK

Yeah, during high school. But I worked the whole time in high school.

FIDLER

And between, for example, the advanced classes at Humboldt and your classmates at Bronx Science, was there a shift, do you remember?

KLEINROCK

Considerably. When I got to Bronx Science, I was surrounded by all smart, somewhat nerdy, aggressive, middle-class, good-background kids who were prepared. They had the kinds of backgrounds I was not privileged to have. You know, they had parents who were helping them with their science work and their projects, made sure they did their homework. I felt like an outsider there, and they were richer than I was and they were clubby, sometimes to exclusiveness. I was scheduled to graduate in January of 1952, but I knew that if I'm going to go to college, entering college in the middle of the school year is a bad idea. So I decided to accelerate and skip one grade, so I would graduate in June of '51 and be on schedule for proper school entrance. That was a mistake, because not only was I not close to the social groups, but even then I shifted my class, so I went from one group to another group. I was neither fish nor fowl in either group now, and so, again, I had one or two friends, but never in a close-knit group. Anyway, as soon as school was over, I had to rush home and do my work as an usher. So it was an isolated, independent, do-it-yourself kind of existence I had there, and learn whatever you encounter, but not by the experience or the benefit of how others had done it, which was unfortunate and made it more difficult.

FIDLER

Can you tell me about your time in the Scouts and in particular your experiences getting to and being an Eagle Scout?

KLEINROCK

It's a wonderful time. When I was a kid—it was shortly after my father got ill—I was very interested in joining the Sea Scouts. Now, to join the Sea Scouts, you had to be four-foot-ten, and I was not. So every Friday night, I would go to their meeting and asked to be measured, and I became a bit of a nuisance. I remember one day when I went there, it was snowing, so I had to wear galoshes, and when I got to make my measurements, I decided to stand on my toes inside my galoshes, and I passed the four-foot-ten mark. They let me in. I became a Sea Scout, and I loved it because you could march around with wooden rifles in the Armory. I really was interested—you know, this was just after World War II, and it was a time of the military, and the whole patriotism and the glory was a big deal for kids my age. I did know all the fighter aircraft, all the planes all flying overhead.

FIDLER

What year was that, and about how many times did you get measured before you—

KLEINROCK

I'd say five or six times. I'm calculating it was just before I was twelve years old, because at twelve you could join the Boy Scouts, and I think Sea Scouts allowed you in just as you were approaching twelve. So I got in and I loved it. Oh, I learned about knots. I had a sailor's uniform. It was a complicated outfit with all the buttons, the stripes, and the sailor's hat. And you learned about ships and knots, and we actually got to march in the Saint Patrick's Day parade up Fifth Avenue. Now, that was a big thrill with the bands and all. It was a big kick. But that Sea Scout group disbanded about a year after I joined, and I was disappointed, because I just loved it. I decided to now get into the Boy Scouts. So I came in, as everybody else, as a tenderfoot, and the reason I wanted to join these things, not only for the military side, but I knew the Boy Scouts did camping. Growing up in New York City, on the concrete sidewalks of New York City, I loved nature. I wanted to be Tarzan as a kid, and if I couldn't be Tarzan, I wanted to be an American Indian. I wanted to get out there, ride a horse, shoot a bow and arrow, climb a tree, and there was none of that. But I knew the Boy Scouts did go out there. They'd go hiking, they'd go camping, they'd make fires. So I joined the Boy Scouts, and I just loved it. I loved it for a lot of reasons. First of all, again, just like school, it was structured. There was a clear order of procedure. There was a ranking. And just like in school, as you progressed, you had marks of achievement, and those were important to me, clear milestones. You've achieved this, you've achieved that from tenderfoot to second class to first class, and I worked my way up the ranks, and I became a member of a patrol, then I became a patrol leader, then I became the senior patrol leader. So I basically was in charge of all the patrols, which is a wonderful experience in learning about management and executive and leadership, which is what the Scouts were all about. So I became what's called a Star Scout, which means you've earned five merit badges. I really enjoyed camping. All the other kids had sleeping bags. When we were camping, I had a blanket, froze my behind off every night.

FIDLER

Were you doing that, again, as an outsider, or were you finding a sense of belonging by this point?

KLEINROCK

No, I felt a real sense of belonging with these kids. Every Friday night, we'd meet, and I had great leaders. I looked up to them. I became a leader. I got the respect. I got the chance to take a leadership role and, if you will, a collaborative group experience, which is excellent, and I just loved going out camping overnight and doing physically challenging things and gaining skills. So when I became a Star Scout, my Scout master, whose name was Mr. Skinner, I remember very well, he said to me, "Len, if you want, you can become the first Eagle Scout in this troop. It's not easy, but I have faith that you can do it." And I was very flattered that he said that to me, so I decided, yeah, I'm going to try it, and that was a goal that was basically just outside my grasp. You'd have to get twenty-one merit badges. In those days, it was an arduous task. There were no Eagle Scouts in my troop. They were like the glorified, honorific people in the whole scouting tradition. I said, "Why not," because I love Scouting. So I set out to do it. I became a Life Scout, which is ten merit badges, and then to get the other eleven merit badges—actually, it was before I became Life. I knew it was very slow progress getting merit badges. Most of the merit badges you had to get out in the wild on a camping trip. So I decided to go to Scout camp one summer, and it was a two-week camp, and in that two weeks, I got to

thirteen merit badges, which was literally unheard of. I was busy all the time, working like hell. So I got all but one of the merit badges I needed for Eagle. The one I couldn't get because it just took too much time was Bird Study. That meant you had to go out every morning. You had to see, understand, identify, know all about forty different birds. By the way, they've dropped that requirement now from the Eagle Scout. So I had to go back the next year just for that one merit badge. I got it, became an Eagle Scout. That was a great milestone in my life, because it was an example of something that was clearly not easy to get; it was a challenge. It was, as I say, just outside my grasp, so I had to really reach to get it, and I achieved it. And when I did that, it was different from getting 100 on an exam. Other kids get 100, but I was the first to get that Eagle Scout, and I realized that if I set my mind to it, I could achieve the things I wanted to get, arduous and difficult though it may be. That sort of set a—not a standard, but an approach, a kind of confidence, that, yeah, if you want to do it, you can do it. So while I was in high school, I did become an Eagle Scout, and it was very important to me. In fact, that Eagle Scout achievement means so much to me, that since then in my professional career, I've gathered many, many other honors, my bachelor's degree, my master's, my Ph.D. degree, all kinds of honorary societies and best papers and fellowships and fellow members. I've got a collage at home where I took all those diplomas and I put them in a collage, and the one on top is the Eagle Scout, and another one that's peeking out is my Karate Black Belt, my Nidan Black Belt. So it's those kinds of nonscholastic achievements that mean most to me, but the scholastic I did, but it's those others that represent a side of me that I see myself as, not as the professor, but as the all-around person. In junior high and high, I studied Latin, and there's a wonderful phrase in Latin—I'd have to look up the Latin. I forget it, but what it means is "A sound mind in a sound body," and I took that to heart, and it's true. If you keep yourself physically fit and capable, it clears your mind. They work together and they each benefit each other. So, end of Eagle Scout story.

FIDLER

You've mentioned elsewhere that prior to your Eagle Scout status, you were a good student but not a top student, and this helped you achieve more scholastically, in fact.

KLEINROCK

Yes. I was a very good student in elementary school, junior high, high school. I was never the top of the class. I was right up there, but I didn't see myself as a super achiever. When I got the Eagle Scout, it made me think differently. I knew I could do a lot more, and later in life I was able to become the best of class, if you will. I remember one rather interesting story in elementary school. I was really good in math and science and not so good in English and penmanship or conduct. I've seen my report cards since. It says "Needs improvement in conduct and penmanship." But the math and the science, I would ace those things. So I remember one day the teacher in elementary school gave us a kind of verbal arithmetic puzzle, and she would say, "Take this number, add such and such, multiply it, divide, add," but a long sequence. Then she went around the room in order and asked, "What's the answer?" She went up and down the rows. She got to me and I said, "Naught." She went around and around, and everybody else, she says, "Wrong, wrong, wrong." She got to me, she said, "Wrong." She got to somebody. He said, "Zero." She said, "Right." I said, "Wait a minute. I said naught." She didn't quite understand the word somehow. [laughs] Now, I don't know why I said "naught," but I was pleased that I got it right. Whenever you score well, it just builds your own confidence.

FIDLER

Let's move to your time at the City College of New York. What was it like being there in the 1950s?

KLEINROCK

Let me talk to you about how I got to City College. I was doing fine in Bronx Science, made sure I graduated in June of '51, but I knew that if I'm going to go to college, I really wanted to go away to college and live in a dormitory and have the full college experience. It was kind of idealistic—see, most of my classmates at Bronx Science were planning to go away to whatever, the Ivy Leagues or liberal arts colleges, even Columbia, and some were going to City College. But I decided the only way I can go away to college is if I get a full scholarship, because we just didn't have the money. My dad was not doing anything and my mom was struggling to earn a living. So I wrote away to almost every Chamber of Commerce in the United States, and I asked them, "Which of your schools do you have scholarships for," and what kind of pay were they giving. And I got a whole bunch of scholarships. I wrote away and I applied. The trouble is none of them was sufficient. Typically, they pay for room and board and tuition, but the sad fact is there were still some expenses, the travel there, the travel back and money to help the household. Couldn't make it happen. So I decided I'll go to City College, which was basically free and a fantastic place. City College was one of the best colleges in the country at the time, but those were the golden days when all the immigrant kids, those who couldn't afford to go to Columbia or NYU or out of town, went there, and those immigrant kids or children of immigrants, as always, were highly motivated, very, very studious, disciplined, goal oriented, and they weren't going to fool around. So it was a great place to go. So I was all set to go. That summer of 1951, I took a job as a lifeguard—I was on the swimming team at Bronx Science, by the way. I just loved that, sound mind and sound body kind of thing, and I took a job as a lifeguard in New York City. The pool that I was a lifeguard at was in the Lower East Side, by the way, which was rather interesting because I broke up more fights than I saved lives, and I saved fourteen lives. But it was, again, a life experience. I remember the head of our lifeguard group, he was an experienced lifeguard, and this particular pool was full of gangs. It was just full of gangs. It's 1951. So the first thing he did the day we got there is he walked over to the leaders of the two top gangs, and he made peace with them. He said, "Look, I'll give you some rope, but you've got to help me keep order and discipline here." He made an alliance. Boy, was that something to learn, really an experience. So anyway, I was a lifeguard there, and during that summer I was set to go to City College that fall. Toward the end of that summer, my dad took me to visit one of his cousins; that was one of the cousins in whose home he lived when he came here to New York with his aunt. He's not the one that my father knocked out; it was another one. This guy was a brilliant electrical engineer, and he had a very small firm called Photobell Corporation, which was building industrial electronic devices. My dad took me down there, and Abe Edelman was the owner, my father's cousin. He showed me around. It was a fascinating place. They were building electronics from scratch. They would make the chassis, cut the holes, put the vacuum tubes in, wire up the resistors and capacitors, conductors, put in the power supplies. It was a fascinating place. And little did I know that my father had an ulterior motive; he wanted me to work there instead of going to school. And Abe offered me a job. When my dad and I came home, my dad basically said, "Len, you really should

work there.” And I said, “Dad, I’ve got to go to college.” He said, “Well, you know, we really need the money.” So he convinced me to go to night session instead of day session, and work at Abe’s place. Now, that was a huge blow to me, but I didn’t even realize it at the time. I sort of took it in stride and said, “Well, I guess I’ll do that.” I didn’t realize in some sense the danger I was being put in, because most kids didn’t graduate evening session. They’d drop out or fail out, and I feel especially bad about that because he had the same experience. He couldn’t get through high school at night for the same reason, the pressures of work and all the rest. But anyway, he sent me there, and I have a deep resentment for that situation. My mother in some sense didn’t object either. She didn’t push it. So I went to evening session, and happy-go-lucky, I was ready to take on anything, and so I did. So I went to work there, and it turned out to be an amazing experience. I couldn’t go to college full-time, obviously, but I decided to push it as hard as I could, so I went three-quarters time and every summer and worked daytime, so I’d be up at six-thirty a.m., down to work, get to school by close to six p.m., take my classes, finish typically at eleven at night. And then, just because I was desperate for some break in that schedule, I participated in what substituted for fraternities at City College; it was called House Plan. They were clubs. They were brownstone buildings next to City College. We’d go there and sort of hang out with guys and gals, and there were guys’ clubs and gals’ clubs, and it would typically close at midnight. So I joined this group called Dean House, a bunch of Jewish and Italian guys. We had a great time, wild guys, you know; I was young and vigorous and full of spit and polish at the time. Then I’d get home at about twelve-thirty, one a.m., and get up early in the morning. Now, I did my studying on the subway going to and from work. So what was this environment? I was, at night, with a very strange group of people, a bunch of losers who couldn’t get into day session, who would drop out, a bunch of poor kids struggling, some of whom could make it and some of whom couldn’t, and a bunch of GIs, having come out of World War II on the GI Bill, and these guys were really motivated to get a degree. They knew what life was about. They knew they needed this degree. And there were some kids like me who were too poor to go day session. So it was mixed bag, and I felt sort of out of it, in a way, because I wasn’t on a track to go to college like anybody else. Now, when I was in high school—I told you I studied the violin—I was in the high school orchestra. It turned out one of the first mathematics classes I took at CCNY at night—it was the first class—was taught by my orchestra teacher from high school—you know often music and mathematics go together—and I was so thrilled to see him. It was like having a friend in an otherwise foreign environment. So that was a certain sense of familiarity, encouragement. So those are my classmates. These are a lot of hardworking kids, some of them smart, some of them not, but in electrical engineering you didn’t find a lot of dumb kids. They’d be in the softer classes, the kids who couldn’t handle it. There were a lot of gangs preying on evening-session classes. I still have my calculus book from a class I took on the ground floor of Shepard Hall at CCNY. It was a hot summer night class. The windows were open. A gang came by with buckets of water and tossed water into the class, and my pages of the book—it’s over here, actually, those pages are still warped from where the water hit. But there weren’t any physical encounters except going to City College at night. It was in Harlem, so you had to be careful where, what time, what street you used. And all during that period, and still to this day, I tend to carry my briefcase in my left hand, so my right hand is available. So anyway, that was at school. Now, at work,

what did I have? I had electrical engineers, technicians, a really tough boss—Abe Edleman was a tough guy—an inept manager. There were about eight to ten of us. He never could make a strong living at this. He wouldn't charge enough for his product, but he was a brilliant engineer. I learned a great deal from him. In fact, I remember when I first met him, he gave me a little engineering problem. I won't describe it to you, but he asked me how do you calculate the resistance of this thing when you move this around. And I got it. But what I had, they were engineers doing electronics, and I was learning at an enormously fast pace. I learned what the real world was about. The things you learned at electrical engineering school you could apply here and vice versa; things you learned on the job you could bring to the classroom. One of the first things I did when I was still a naïve student, having just gotten out of high school, working at Photobell, I'd taken an excellent class in descriptive geometry at Bronx Science. I really knew how to draft and do descriptive geometry, which is really intersecting solids and seeing what the intersection is, very well. So Abe, as one of my first jobs, said, "Look. Take this phone plug, solder a resistor inside of it, put it together, and make a drawing of what you did." Simple enough. So I did that and I made a drawing, but I made a draftsman drawing. I measured every dimension, put in perfect arrows. It was a very nice eight-and-a-half-by-eleven picture of this thing. I gave it to Abe, and he nearly killed me. He said, "What are wasting your time doing that? I just wanted a sketch. You wasted about an hour that you took to draw that." So I quickly learned about the practicality of the real business world. The mix of that practical experience and the theoretical learning at school, whereas at the same time, those professors themselves were in an environment like me, they were working during the day and teaching at night, and they brought to this class their practical experience. The example I like to use here is I remember one of my teachers came in and he held in his hand a funny little device. He says, "See this? This is a transistor." They'd just been invented. And he said, "They're used as amplifiers in place of vacuum tubes." And he said, "However, they're better thermometers than they are amplifiers because they're so heat-sensitive. And what you do because they're heat-sensitive is you have to put this into the circuitry to combat that heat variation." Now, I know that no day-session professor would ever have understood that issue about the heat sensitivity or explained to the students how to deal with it. So I had the advantage of practical experience, good theoretical people teaching me, getting the practical experience at the work, around GIs who set the tone for what you really are in school for, not to play, because there was no play. It was a serious business, and I basically set my course and I managed to graduate in five and a half years, which is kind of an achievement, because the program for day students was five years. In engineering, you took five years. It was 142 credits. I did it in five and a half because I was going every summer, taking as heavy a load as I could. When I first got in, I joined Dean House, as I told you I was just out of high school on the swimming team, and I decided, "Look, let's make an evening-session swimming team, and we'll challenge the day-session swimming team." And so I got this ragtag group of guys from Dean House, who were not really swimmers, and they all volunteered, "Sure, we can swim!" And I was a good swimmer. So we put this team together. We never practiced. We challenged the freshman team. Little did I know the freshman team is enormously strong. So we had this meet, and I was a sprinter, so I swam the 50-yard sprint and I think I came in second. One of our guys says, "I can swim the 220." That was the long-distance race. So when we got to do it, he says, "No, I can't." And nobody else

would volunteer. So I said, “Well, I’d better do it.” So I sprinted the 220. Well, you can’t sprint the 220. By the time I got to 150 yards, I began to fade, and by the time I got to 200, I was almost going backwards. So we lost that meet. But the point is, I did put together a swim team. I had some of the social aspects. I even became evening-session student president, and I graduated first in my class, day and evening, of electrical engineering. So, I managed to do well.

FIDLER

The house plan and the swim team, that would be the group of people outside of classes that you would interact and associate with?

KLEINROCK

Well, the house plan and swim team were the same guys. We had a great, great time, and my social life was basically the house-plan guys and, again, it was a bunch of—a mixed group. There was, I think, one other engineer. There were some guys who dropped out eventually. These were very suave, sophisticated, social—and I don’t mean in the high sense. I mean these guys knew their way around. We had rules. When we had a dance with a girls’ house plan, we couldn’t stick with one girl. We had to circulate. There was no ownership of these girls. So they had these rules. We were really hip and suave, and we knew our way around. And we met a lot of fine Italian girls, so I decided I’d like to be able to speak to the Italian girls in Italian. So at work we had an Italian guy, and he taught me some high Italian. So he said, “When you first meet an Italian girl, you say, ‘tanto piacere,’ which means ‘There’s much pleasure in meeting you.’ Then you say, ‘vuoi ballare?’ which means ‘You want to dance?’ And then, ‘Si balla bene,’ which means ‘You dance well,’ and you go from there.” So we went to this dance and I met this very beautiful, clearly cultured Italian girl. I went over to her with my Italian. She laughed at me, because what I had said, I didn’t say, “tanto piacere.” I said, “tanto pisciare,” which means “You urinate a lot.” Well, that was a good beginning, and we got along just fine. We had a good time. We went on these escapades on the weekends. We’d raise hell. But during that time, at one of these dances a girl I knew came up to me and said, “This young lady would like to meet you.” And she introduced this woman named Gail to me. She was in high school at the time. We started dating, and I was able to marry her. I married her while I was in evening session at night because I was earning money. By the way, when I was earning that money, it all went into the house. I started earning \$35 a week. Then eventually I started earning—at the end, it was \$100 a week. So it was \$5,000 a year, which was a big deal. In November 1954, I married Gail in a civil ceremony so that we could apply to live in a project in New York City. (You had to be married.) Then she went home to her home, I went home to my home, and in January of ’55, we had our formal wedding. Then we took an apartment. So in some sense, I realize now that was the only way I could justify leaving my folks’ apartment and stepping out of that environment where I was kind of a slave, if you will. It was this rigid working, going to school, all the money piping in. So we took an apartment, and from January of ’55, I put Gail through high school and college. She went up to Jackson College, which is part of Tufts University, came back, went to Hunter College, and I kept working at night putting her through college, and myself. So I got married quite young. I was twenty years old when I got married, which was too young.

FIDLER

Maybe you can tell me about how you got into MIT.

KLEINROCK

Sure. So I was at City College, decided I did want to get a master's degree in electrical engineering. It was a worthwhile thing to do. I was thinking about different graduate schools, recognizing, again, I need to get a very strong scholarship to go there. I now had a wife. [laughs] I heard that on a certain day, there was this representative from MIT. In fact, MIT Lincoln Laboratory was going to come by and describe this wonderful fellowship scholarship program at four o'clock one afternoon. So I spoke to my boss and I said, "Look, I need to leave work a little early this day. I'll make it up during the week." He said, "Fine." So I got up there for the presentation, and this guy from MIT Lincoln Lab described what they call their Staff Associate program. It's a fantastic program whereby you become a student at MIT as a research assistant, you spend your summers at Lincoln Laboratory as a researcher, you spend one semester as a full-time student, and you get a master's degree. They'll pay your full tuition, you earn some money as a research assistant, as a summer employee, and you'll go to MIT, but you have to apply for this. This was sounding like nirvana to me. What could be better? MIT, the best place to go to. So the gentleman at that lecture said, "And if you want an application, go see the professor in the back of the room when my lecture is over." The lecture was over, I went back there, I went to this professor and I said, "I'd like an application." He was an electrical engineering professor. He looked at me and says, "I don't recognize you. Who are you?" So I told him my name, and I said, "I go to evening session." He said, "Evening session? Get outta here. You can't have an application." I said, "What are you talking about?" He says, "Get outta here." I couldn't believe the attitude. So I wrote away to MIT Lincoln Lab, I got the application, I applied, and, happily, I was the only one accepted because I was first in my class in day and evening, the credentials were good, and I presented well. So I managed to get that application. By the way, I won't tell you the name of the professor, but his name basically was appropriate for that personality he exposed in that little interaction.

FIDLER

Can you tell me about your time at the Servomechanisms Lab?

KLEINROCK

Sure. Oh, by the way, true to my idiosyncrasy of trying to keep my options open, even when I was accepted at MIT, I wasn't 100 percent convinced that I should go there, which was idiocy when you think about it. I was thinking, well, where else might I go, what else should I do, should I really get a master's degree. And my dad, at this point, was very clear. He said, "Don't think twice about it. That's the place to go." So bless him. By now he was working as a clerk again. He had gotten enough health back, so he was working at that point. So I had a choice of which laboratory I go to. I got up there in January 1957 and started working at the Servomechanisms Lab. The reason I chose that laboratory is because I had taken a course in servomechanisms at City College and I was fascinated by these automatic control systems. They were wonderful systems which, with the feedback loops and the ability to adapt to behavior, were fascinating. And along with servomechanisms, you learn a lot about analog computers, because in some sense a servomechanism exploits the abilities of analog computers. So on my application to MIT, I said, "I'm interested in servomechanisms and analog computers," explicitly leaving out digital computers. That was before I finished City College. I had one more semester to go, and the last semester after I made that application, I took a great course on digital

circuits, a book called Pulse and Digital Circuits by Millman and Taub. I still have that book. It was a great book, and one of those authors was teaching at City College as well. So I went to the Servomechanisms Lab, and the first group I was with was in a group doing a digital flight simulator, using digital circuits to simulate flight in an aircraft, a pilot in an aircraft. The group I happened to be working with were all from Texas. The students were all from Texas. Now, I figure when I go up to MIT, I'm going to pick up a Boston accent. These guys all had a southern drawl, and they kept talking about things I couldn't understand, like they kept talking something called a "dowd." Turns out a "dowd" is a diode. [laughs] So little by little, I picked up a Texas accent, Boston accent, and my good New York accent. I did some very interesting work there. Now, here I was in an environment, all graduate students taking their courses, learning to work with them, sharing the experience with them of going to class, doing our research, finding problems. It was a wonderful experience. These were really smart guys. MIT typically took the top student of almost any school, and they really filtered in some great people. I had a supervisor of the laboratory named Frank Reintjes. I'm trying to remember the head of my digital flight simulator group. I think it was Connor. And I had an advisor whose name was Gardner, Murray Gardner, Professor Murray Gardner, and Gardner was also teaching the first course I took on linear and transient systems. I was told by my people at the Servomechanisms Lab that it would behoove me to do well in this course because this is the course where they separated the men from the boys. And guess who was teaching this class. Murray Gardner. He was the author of the book. So I strolled into class and I did my work. Took the midterm exam. A grade came back, and I got a 50. I had never seen anything south of 95 for years. This was a terrible blow to me. The other students did okay. I eventually got the score up to 70. It was totally unacceptable. I remember driving back and forth to MIT those days and shouting, "Fifty? This is unacceptable! You can't do this!" And I realized a couple of things. First of all, I realized that my study habits were not sufficient to deal with MIT and the students with whom I was competing. In addition, my education at City College, as good as it was, didn't nail down certain understanding of the things he was teaching which would have helped me in that class. So I decided I better buckle down, and I worked like a dog, and I got an A in the course. So I like to refer to this as a wakeup call. It was a wakeup call, and you have to realize that with a wakeup call, you do one of two things: either you get crushed and you go away, or you rise to the occasion and you change what you're doing to correct and make sure that doesn't happen again. So I did the latter, of course, and I got an A in the course, and that was a rather interesting thing. So that wakeup call made me realize that this collection of people with whom I was sharing courses was a very special group. These kids were smart and they were driven, and they were weird, nerds upon nerds, but brilliant. Each one had a different kind of approach to work, nothing standard, so you really had to stay alert and do your best and do the work, do the hard work. So I took a bunch of courses with these kids, began to know them very well, and I was doing my research. After that first semester, in the spring of 1957, I spent that next summer at MIT Lincoln Laboratory, as was supposed to be the case, working in a group which was involved with a transistorized computer called the TX-2, with a bunch of brilliant engineers who were there full-time and with some summer students as well. This group I was with was, again, a great privilege because these guys were powerhouses. They had designed these machines. In fact, this group had conceived the machine, designed it, built

the hardware, wrote the software, did the compilers, wrote the applications. This machine was part of them, and it was an innovative machine. The TX-2 happened to be a 36-bit word machine, which you could break into four 9-bit segments or one 18 and two 9s or two 18s or one 36. You could do parallel computation. The instruction set was unbelievable. It was one of the first transistorized computers. The kind of work going on, it was headed by a fellow named Wes Clark, Wesley Clark, and he had a bunch of people working under him, and under those people were people like myself, students. One of the guys I was working with was a fellow named Jack Raffel. If I'm not mistaken, he had also come out of Bronx Science, actually, and he was very much interested in thin magnetic films for storage. But even before I worked for him, now as I recall, I actually started working for Ken Olsen. Now, Ken Olsen you probably recognize as the gentleman who founded Digital Equipment Corporation. I started working for him that summer of '57. That's right. He had me studying the behavior of gamma rays on transistors because he was concerned about the impact that those gamma rays would have on the functioning of these transistors when they were part of a computer circuit. So I did that, and I got some information that helped. Then he had me design a very simple device, a digital device called a pulse delay amplifier. What you do is you have a little circuit and you dial in a number like 100 milliseconds, and this box, when you hit the trigger, nothing comes out until 100 milliseconds later, and then a little pulse comes out. So you could decide when you wanted a pulse to come out to use as a test for many other circuits. So I built this thing for him and it performed very well. When I brought it to him, he wanted it to perform better. He was a wonderful supervisor. He said, "Len, try a little more. Here, try this." He was very encouraging in a soft way and, sure enough, I made significant improvements under his tutelage. That summer, as I was working for him, he said, "Len, I'm going to form a company, and I'm taking some of the people from here with me, some of the key engineers, and I'd like you to join me," because he liked what I did for him. I said, "Ken, I can't do that. I'm on this wonderful program. I'm going to get my master's degree. My graduate work is more important." So it was fine. So he went off and formed DEC that summer, went to Maynard and he went the Barn, as they called it, where they formed this group. He took people like Ben Gurley and Tom Stockebrand and a few others, and formed Digital Equipment Corporation. One of the first things they built was this pulse delay amplifier, because they started out selling modules. They sold flip-flops, they sold amplifiers, they sold a variety of things, and eventually they sold computers. But DEC, it turns out, has a rather checkered early history because Ben Gurley, one of the brilliant engineers who designed the systems for them, got shot to death in his breakfast room while having breakfast with his family, shortly after DEC was formed, by a disgruntled employee at DEC. Another fellow, who was one of their technicians, when he came out of his home, on his way to his garage, somebody came over and shot him in each arm and each leg and then left him. He survived and he never told who it was. There was intrigue going on there. These are the way these young companies would get into it—it was on Route 128, the famous Route 128 around Boston. That could have been the Silicon Valley had somehow the environment fostered the kind of environment that they did at Stanford. At any rate, so I continued on. Then I started working back at MIT as a student, and I was looking for a thesis, a master's thesis, and some of the work I was doing at Lincoln Laboratory under Jack Raffel, which is after Ken left, was on thin magnetic films. A thin magnetic film is a

very small dot which you deposit in a vacuum chamber, made out of magnetic material, and it can magnetize one direction or the other, which represents a bit, either a one or a zero. You read it out the way you read out from ferrite core memory. You flip it and you see which way the magnetism goes. So it was a pretty standard but a very compact memory. However, there was another way to read it, and that was by shining polarized light on this magnetic dot. If the dot was magnetized in one direction, the interaction between the polarized light and the magnetization would cause the polarization to shift in one direction or the other, depending if it was a one or a zero. So here was a way to optically read out the contents of memory without disturbing it. It was a passive readout. So this was the topic, on Jack Raffel's recommendation, that I do my dissertation. So I started studying this. There was a well-known effect called the Kerr, K-e-r-r, Kerr magneto-optic effect. By the way, there was a well-known, something called the Faraday effect, where if you shine polarized light through a magnetic field, that'll also rotate the polarization, depending upon which direction the magnetic field is. This was reflecting as opposed to transmission, and the effect was small. So the challenge was to somehow do the physics to add something to the magnetic film which would enhance the shift of the polarization to be a more dramatic readout. Now, this was not quite electrical engineering. It was more physics, depositing thin magnetic films, putting layers on top of them, but it was fun because I could create my own apparatus. You needed to basically shine the light through a collimator onto the magnetic film, and look at it through a polarized filter, and there was optics and telescopes and some engineering, and I made my own little apparatus. I was used to building things from Photobell, from a kid with the radios, and this was right up my alley. So I built this magnificent structure—you can see it in my master's thesis—and I did find a way to enhance the effect. But while I was at it, I couldn't help myself, and I was surrounded by my other classmates who also work in a variety of things, some on magnetics, some on electronics, and a lot of the work we were all doing was trying to add some mathematics and some logic as opposed to just physics to these problems. So I decided to see what would happen if you reflected the light beam off one magnetic film, and it bounces up and bounces off another magnetic film in a sequence. What would be the resulting magnetization, what would it mean? And I created a logic, basically a digital logic, where you could do multiplication, addition. It was a wonderful logic. It was part of my dissertation, my master's thesis, but the effect was not really big enough to make this practical. I did enhance it, I did the mathematics and the logic, and I handed this dissertation in. My supervisor was the head of the laboratory, Servomechanisms Laboratory, this fellow Frank Reintjes. Frank Reintjes was one of the greats of the era of World War II when MIT had something called the Radiation Lab Series, and they created these wonderful books and technology all about radar for World War II. So he was a well-known man. He was very impressed with what I did. So I went through the sequence. By the way, the sequence for this staff associate scholarship was you're at school for a semester, research assistant my first semester. That summer, you're a full-time employee. Next semester, you're research assistant. Next semester, you're full-time student. Next summer, you're full-time employee at Lincoln Lab. And now you've gone through three semesters and two summers, and you have one more semester to go, but you should be done with your thesis by then and have only one course left, and that course you're going to take by commuting from Lincoln Laboratory to MIT, which was a twenty-mile bus ride away. They had a shuttle going back and forth. So I was all

set to do that that summer of '58. And if you realize what was going on, when you finish this program, you get hired by Lincoln Laboratory in a fine research position, as a research engineer. So my wife and I, we were married—yes, I told you. We decided, “Look, let’s start a family. We’ll have our first child in the summer of ’58.” Of course, then I’ll be full-time at Lincoln Lab, I’ll be commuting, having a full-time salary, ready to go. So my son, Martin, was born in August of 1958, August 21st, 1958. However, Frank Reintjes had other plans for me. He said, “Len, you did a really impressive master’s thesis. I want you to pursue that master’s thesis and make it into a Ph.D. dissertation, get a Ph.D. out of this.” I said, “No, I can’t. I’m scheduled to be at Lincoln Laboratory. My son is born. I want to get the full-time salary. I want to be an engineer. I have my life plan set.” He kept twisting my arm and twisting my arm, and finally I said, “Look, if I’m going to do this, I want to do something that’s going to have significance, not just piddle around with something,” because, you know, I enjoyed my master’s thesis, but it wasn’t earth-shattering. I did my first paper on that, by the way. I did the paper in Chicago, but that’s another story. I’ll tell you later if you want. So I said, “Look, I’ll do this.” I spoke to my wife, Gail, and we agreed, “Okay, let’s do it, but you’ll need another Staff Associate program to support you.” And sure enough, Lincoln was willing to extend my master’s to a Ph.D. It was the first time they took a master’s Staff Associate and extended it out to a Ph.D. So I was very honored to have that, and the money was good enough. And I said, “If I’m going to do this, I want to look for the best professor at MIT. I want to do something which is challenging, fun, and has impact.” So that was sort of the deal I made with myself. So that summer I decided who’s the best professor I know, and the best professor at MIT that I knew, the most exciting was Claude Shannon. Claude Shannon had come to MIT in 1958. He had come off a career at Bell Labs, having created this magnificent field called Information Theory and Coding Theory. The man was world-famous. He’s a brilliant mind. I had taken his classes. He was now at MIT, and I watched the way his mind worked and the way he worked. He had this wonderful mix of great intuition, physical engineering intuition about objects, and mathematic prowess, and he could merge the two. So he could look at a system, decide what are the underlying principles, use the mathematics to extract them, exploit them, and then implement them. And he was fast, one of these really brilliant guys, and he had proved himself with this classic paper he produced in 1948, which became the entire field of Information Theory and Coding Theory. He and Norbert Wiener basically were contemporaries, and Norbert Wiener was also at MIT. So I was surrounded by really brilliant guys. I said, “Shannon’s the guy I want to work for.” So I called him up that summer of ’58, and I said, “I’d like to work for you on my Ph.D.” So he said, “Look, why don’t you come out to my house this weekend, and we’ll talk.” At this point, I was working with Jack Raffel that second summer, and I told Jack and a number of my classmates and colleagues there, “I’m going to visit Claude Shannon this weekend.” They said, “Well, that’s terrific.” And Shannon had told me do I mind that he has a houseguest while I come to visit. I said, “Of course not.” Here I am, this miniscule graduate student talking to the giant, Claude Shannon. He said, “I have John Pierce here.” So I told my Jack and my other friend, “Jan Peerce is with Shannon.” Now, Jan Peerce, as you know, is a famous opera singer. They all said, “Well, look, why don’t you ask Jan Peerce to sing ‘The Bluebird of Happiness’ or something,” as a joke. So I visited Shannon that weekend. The man I saw as his houseguest did not look like Jan Peerce. Jan Peerce was a

somewhat rotund person, and this gentleman was a thin, scrawny-looking guy. And I realized later it was John Pierce. John Pierce was a colleague of Shannon's from Bell Telephone Labs, and John was a curmudgeon and a cynic. And here I was, summer of '58, right after my son was born, I'm bubbling over with life and joy and a new child, and it came up in our conversation, and Pierce says, "Bah. Children. They're selfish. They're horrible. All they do is disappoint you and exploit you." And I couldn't believe this man had this attitude, and here I am just optimistic, so it was a clash of world view, certainly. But I spoke with Shannon, and we were out on his porch, I remember very well. I'm sitting on a chair facing Shannon, and we're chatting, and every so often his eyes glance above my head. So finally I wonder what's behind my head. So I spun around, and his son was on a hammock, and the hammock was swinging, the rod of the hammock was swinging within inches of my skull, and had I'd leaned back, I would have been bopped on the head pretty hard, and Shannon, I think, was just waiting for that to happen. [laughs] He was a bit of a—what shall I say—a mischievous guy. We talked about a lot of things. He told me about the automatic lawnmower system he had. He had wired his lawn with an underground wire, and the lawnmower would follow the wire. It was one of the first robotic systems. He told me about this school bus he had converted into a family bus. It was sort of one of the first minibuses that people go camping in. And he told me about the project he's working on now. The thing that was really exciting him had nothing to do with Information Theory. He was working on a chess-playing program with another professor at MIT, John McCarthy, who is one of the great artificial-intelligence people. Shannon was interested in getting a program to play chess, basically the first chess-playing program, because Shannon was a chess enthusiast. He had these wonderful chess games. He would buy any automatic chess game where the pieces would try to move automatically. So I said, "I'd love to work on it," because this was a challenging situation, and I wanted to work with Shannon. So he took me on as a research assistant, and the deal was this. John McCarthy and his student were going to create what's called a legal move generator. Given the position of the board, what are the possible legal moves? Then Shannon would come along with me, but mostly Shannon, and decide what's the right move to make. And we wanted to study the middle game, because the beginning of the game is sort of pat, and end game is sort of strict, but the middle game was where the really interesting strategy takes place. So we wanted to build some interesting rules into this program. So Shannon handed me this book by Fred Reinfeld called 1001 Winning Chess Sacrifices and Combinations, I think it was called. On every page there was a position shown and it would say, "Black has a brilliant sequence. Find it." Next page, "White has a brilliant sequence." And you'd look at this. It was really hard to solve these things, but in the back of the book were the answers. So Shannon said, "Go to the back of the book, look at the answers, and find out the following: what is the most common first move of a brilliant sequence. Because I want to know that move so I can build it into the algorithms and the heuristics of my stress strategy. Find out what is the second most common first move of a brilliant sequence, what's the most common second move of a brilliant sequence, etc., classify them, get those, and we'll make our strategy based on that." Well, I did that. It was easy to do, and it turns out, by the way, that the most common first move of a brilliant sequence is check. You totally constrain the opponent. There's only a few moves he can make, and then you can take a long sequence in your mind. And I think the second most common move was a capture. Again, you're nailing

the guy. So we built this, and we created the first chess-playing program. I worked on that for a while, but I decided this is not really what I'm particularly good at. And I was taking more courses at the time, and I was getting ready to take the qualifying exam for the Ph.D., which you have to pass before you're allowed formally into the Ph.D. program. I took Shannon's courses. I had taken some. I took more. I looked around me and I saw so many of my classmates were working on—guess what—Information Theory and Coding Theory, which was Shannon's forte, and why many of the students had come to electrical engineering. And by the way, I was no longer at the Servomechanisms Lab. I was now at the Research Lab for Electronics, RLE, with a different group of students. And by the way, one of my classmates, who was also on the Lincoln Laboratory Ph.D. Staff Associate program—he hadn't got to the master's, but was pursuing the Ph.D., was Larry Roberts, and another one was Ivan Sutherland. These names will come up later as we go along. So I looked around. I saw most of my classmates were working on Information Theory and Coding Theory, and I also saw the problems they were working on. We were in a bullpen of graduate students all mixed together. I would talk to all of them and learn what they were doing, and the problems they were working on seemed to me to be really hard problems. And in some sense, Shannon had done most of the critical and important and fundamental work, and what was left was not only hard, but, in my mind, was of relatively of little significance. And I said, "That's not what I signed up for. I signed up to work on something which would be challenging, fun, new, and would have real impact." So I decided not to pursue that avenue, and I decided that the chess-playing was not really where I shone or really where there's the kind of thing that I wanted to do. But having spent time at MIT Lincoln Laboratory, surrounded by computers, and here at MIT itself, surrounded by computers, I recognized that sooner or later, these computers are going to have to talk to each other. These were isolated computers doing their wonderful work and their wonderful projects, but each one had projects of interest. They probably want to communicate. You may want to go from one computer to another and find out what they were doing when you're on the first computer. So I said to myself, well, how are these computers going to communicate? What was going to be the underlying networking technology? And there was no networking technology, no communication technology which would allow that to happen. I thought, "Oh, this is interesting. Here is a problem that I can see is important, nobody's working on it. So what kind of network would allow them to communicate?" Well, there was a network around at the time; it was called a telephone network. Telephone networks were everywhere, all over the world, but I realized that a telephone network technology would be inadequate for connecting computers together, because in a telephone network, when you're sending voice, you're talking most of the time, and the way you connect is you dedicate a link from you through a network of links to the person you're trying talk to, and that sequence of links is dedicated to your conversation. On the other hand—and you're talking most of the time. You're silent maybe a third of the time, which is a small amount. When computers talk to each other, I noticed, you sit at a computer, you type a few characters, then you scratch your head or you pause or you think. A little while later, you type a few more characters and maybe something comes back. Most of the time, you're sending nothing, so there's no way you could dedicate a sequence of expensive communication links, data communication links, for this sporadic, bursty communications. In fact, if you characterize what kind of communications computers

engage in, it's what's called bursty communications; namely, they almost never want to talk to each other. When they do, they want immediate access, but they don't warn you when they want, and they're going to send a tiny bit of data. So how can you possibly have a network which would allow that crazy kind of demand? I won't tell you when I want it, I almost never need it, I only want to send a tiny bit, but when I want it, I want it immediately. What do we do? Certainly the telephone network won't solve that. So I said the only way to do that is to have a highly dynamic adaptive system which provides communications for the bursts only when they're there, but doesn't dedicate the path to that communication, but allows other people to use the resources when you're not using them, some kind of a shared dynamic, adaptive, demand access system. So I said, well, how can I possibly create a network which would deal with that? I seem to understand it's got to be highly dynamic, highly adaptive, but coming out of the MIT tradition, not only can I conceive of such a network, but I have to be able to predict how it's going to perform, I have to make a model of that network, and then I've got to try to design it and maybe optimize it, which means I need a fundamental model of what's going on.

FIDLER

And we're speaking now about your Ph.D. proposal.

KLEINROCK

I'm thinking about putting together a Ph.D. proposal. You're exactly right.

FIDLER

And before we go further with that, is there anything else to wrap up with the influence of Shannon and his prior work on your thinking about these problems?

KLEINROCK

Absolutely. Absolutely. Shannon was my role model then, and he still is. He had a way of looking at systems, as I said, merging his mathematical excellence with his intuitive understanding. You'd walk in this man's office, he'd have a differential gear in one hand and a Swiss army knife in the other, unscrewing it. This mathematical genius was physically involved with everything he did. And the work he did on Information Theory took advantage of a very important phenomenon in statistical systems, and the phenomenon is this, that if you let nature take a good crack at you, she will expose her average behavior if you let her work on you long enough. So, for example, if you toss a coin, a fair coin, half the times it comes up heads, half the times it comes up tails. If you toss it ten times—this is Shannon recognizing this—toss it ten times and it comes up six times heads and four times tails, is that an unusual outcome? The answer is no. There's a reasonable probability that could happen. You expect five and five, it's the most likely situation, but six and four will happen. Suppose you tossed it a million times, and it came up six in a thousand heads and four in a thousand tails. Is that reasonable? The answer, absolutely not. That cannot happen. With infinitesimal vanishing probability, it's going to be very close. With high probability, it's half heads and half tails. In other words, if you toss it enough times, this average behavior of half heads and half tails will expose itself. It's a kind of emergent property. Let nature run an experiment many times, she will guarantee to show you her average behavior. And there's a name for this; it's called the law of large numbers. So I knew that in systems if you mix enough things together or let them behave long enough or have enough numbers, there will be emergent properties that you won't see if you have small systems or small trials. So I said to myself, if I'm going to study these networks, I want to study large networks where these emergent properties

will come out and maybe expose some principles of network behavior. In fact, when I started working on this, I said these have to be large networks I'm going to study. In fact, my thesis proposal, the original title of the proposal was "Information Flow in Large Communication Nets." And the reason that word "large" is in there, for what I just said, to expose the emergent behavior. That's Shannon's influence right there. He said, "Give me long sequences of code words, and I can predict the way they're going to behave, and I'll put in just the right amount of protection to combat the noise," said Shannon, "and I'll be able to undo any errors that come in. If you give me the right amount statistically, I can predict what's going to happen." Large systems are predictable, even though they're random and statistical. So that was a very strong influence on how I decided to think about this problem.

FIDLER

So in the process of deciding to and how to look at networks, we've got the ideas of Claude Shannon and we've got this context of being around computers and thinking that maybe they're going to need to talk to each other. Is there anything else to add to that?

KLEINROCK

Yes, definitely. I needed an approach. I needed some tool which would allow me to take this system whereby these bursty demands are coming at you all over the place and they're trying to use the system somehow. How can I predict the way these unpredictable demands, unpredictable in length, unpredictable as to when they're going to make their requests, how do they behave? What tool is there out there that I can use? And it turns out there is a statistical tool out there, and I was lucky enough to find it, called queueing theory. It studies the way queues behave. What is a queue? Think of a queue at a bakery store. There are clerks behind the bakery store counter, and customers arrive when? Well, you don't know. Unpredictable. And when they come in and they occupy a clerk, how long will they occupy the clerk? You don't know. But queueing theory studies these unpredictable statistical systems and shows you how to predict the behavior; how long will the queues form; how long do the buffers have to be; how much storage space do you need to store customers in the store; how many servers do you need; with what efficiency will they work; what's the throughput of the system? So these metrics that I just mentioned—throughput, buffering, delay, efficiency—these are exactly the terms you want to be able to solve when you have these bursty demands of computers talking to each other. How many messages per second can I send through? How large must I make those communication channels? How many clerks do I need, if you will? How many buffers do I need to store the messages while they're waiting to get served? Queueing theory provides exactly the right metrics to study and provides a mathematical structure to pose and answer these questions. So I said—ah!—I've got a mathematical approach. I've got a need. I've got a new problem. There's a lot of low-hanging fruit here. It's clear this will have an impact. And by the way, when I started to look into the queueing theory, it turns out there had been some work just in 1957 by a fellow named James Jackson, who happened to be a professor at UCLA, and he had studied a job-shop model, where work would go from one station to another station to another station in the job shop. First you cut it; then you saw it; then you drill it; then you mill it; then you put it together; then you paint it. This flow of jobs goes from hop, hop, hop, from storage place to storage place. That sounded to me like messages passing through a sequence of nodes on their way to the destination. He had some mathematics in queueing theory. So I had a real

approach with an example of how to use that mathematics and a problem which is important, was current, low-hanging fruit, and one that people hadn't studied. Boy, this was right up my alley—just what I was looking for. So there I was, and I started looking at that problem.

FIDLER

And in what other lineages do you place your work?

KLEINROCK

So besides Shannon and besides Jackson's work, which influenced me very strongly, it was a very important part of my own proposal, my thesis proposal for the doctorate, the whole concept of queueing theory is there's a long tradition of queueing theory which is wonderful stuff. Now, there is a father of queueing theory. His name is A.K. Erlang, and did his work in the early part of the twentieth century. He was an engineer in the Copenhagen Telephone Exchange, an engineer, just like Shannon, with wonderful mathematical and intuition capability. In telephone systems you have the problem that you have a building, say, a commercial building that needs telephones. Population may be fifty people, each with a telephone, but how many live telephone lines do you need leaving that building? Because most of the time people are not on the telephone. So he needed a way to calculate how many lines should they supply to a building or a facility. So he had to make a model, and he created basically a queueing theory model, because when you place a telephone call, it's like an arrival. If somebody's using a telephone line that you want and you have to wait, but that's not good, okay, so he wanted to make sure there was no busyness, no loss. When somebody comes in and they can't get a telephone, they're lost. He said, "I want to provide enough lines so that almost surely whenever somebody wants a line, it's available to them." So he developed a model, an intuitive model, in which he wrote down equations of how these systems would behave. He studied both lossless systems, where there's always a line, or even systems where you queue up and wait. He created queueing theory and he created a mathematics using a kind of engineering approach to solving the mathematics, and he turned out to be right on. His mathematics was correct. It was not rigorously derived. It wasn't until years later in 1937, when William Feller, the great probabilist, created something called the birth-death process, where he was able to show that Erlang's work could be put on a firm mathematical basis, and his equations were correct. So here again we have an engineer solving an engineering problem using his mathematical view of the world to write down equations, solve them, and come up with numbers which allow you to build systems. His approach to life was very influential on me, because I always consider myself an engineer, not a mathematician, a physicist rather than a mathematician, physical stuff that you build and make work, and you make it work efficiently. So after Erlang, there were many, many others who produced a theory. I happen to have this book here, which is the Life and Works of A.K. Erlang, which is a wonderful book. It's a very precious book. It's very old. It was written in 1948, but it begins the work of Erlang in 1917, I believe. At any rate, this man, his approach to life matched my view of life, which was not to come at it from the mathematics point of view only and leave it there as a sterile set of equations, but to come at it from a problem that needs solving, bring together the tools you need, be it the engineering tools, be it the mathematical tools, solve the problem, and then implement it. Shannon did the same thing. And I admired that so much in both of

these men, but I didn't know Erlang, but I certainly knew Shannon. So the answer to your question is Erlang, the father of queueing theory, was very influential in my thinking.

FIDLER

Now perhaps we can move on in the context of your dissertation, even, to the relationship between queueing theory and packet switching.

KLEINROCK

Okay. So the underlying concept that allows computers to talk to each other is the fact that I shouldn't give you a resource, an expensive resource as a communications path or link, until you need it, and only as long as you need it. So the idea of this network I was trying to create had to provide communication links, data links, which were dynamically allocated to messages only when they were there to be transmitted. I'll give you a homely example, by the way. Do you own an airplane?

FIDLER

No.

KLEINROCK

But you need a seat on a jet plane every so often.

FIDLER

Correct.

KLEINROCK

But you can't afford it. It's too expensive.

FIDLER

Let's go with that.

KLEINROCK

You can't afford to own the airplane.

FIDLER

Correct.

KLEINROCK

Because if you did, you'd almost never use it. So what do you do? You only ask for it and get to use it when you need it. You reserve it.

FIDLER

Yes.

KLEINROCK

It's allocated to you. It's yours. Get off the plane, someone else gets on that seat. That's exactly what we needed here. These expensive seats were the communication, the data links in a network. Make them available and let people grab them when they need them, and if more than one person tries to grab it at the same time, they wait on a queue. Three people need it; only one person gets it. So the idea of a queue forming in front of these important links was natural. These queues are going to form and shrink. People are going to move up the queue, get served, etc. So the question is, how can we dynamically allocate these resources? The answer is using the notion of a queue. You arrive, either it's there and you use it, or you wait, your turn comes up, and then you use it. Nobody owns it ahead of time. It's not reserved ahead of time. So how can we do this? Well, throw messages into a network, and they go hop, hop, hop from node to node. When they're received at a node and they want to go to the next hop, somehow it's decided which next link they should follow. If that link is free, zip, you transmit over it. If it's busy, you wait in a queue. So the idea of queues forming in this network as these messages compete for

lines was an important piece. It was a natural network of queues model, similar to the one that Jackson had laid out in his job-shop model. But the issues are now what path do you choose in that network, who controls that path, and what do you do if there's a very long message in front of a very short message? You certainly don't want the short message to wait a long time for a long message. If somehow you could reverse it and let the little one go first, he causes a small disturbance of the big message instead of a large disturbance to the little message. Well, how are you going to figure out which the short messages are?

Well, it turns out—

FIDLER

Is this related to implications of a queue not being reserved ahead of time?

KLEINROCK

Exactly. Nobody owns it. When somebody gets there, if the guy in front of the first one to use it is going to occupy that channel for a very long time, that's not efficient use for the little guys behind. Now, it turns out this exact problem had been faced by the computing community years before in the context of time-sharing. In a time-sharing computer, the resource is the processing engine, the CPU. In a network, the resource is the communication channel. Going back to time-sharing, they had a problem. Jobs would come in, they want to use the CPU, and they want to serve the short jobs in front of the long jobs, so the short jobs don't have to be delayed a long time. So how do you find out which the short jobs are? Well, you can ask, but every job is going to say, "I'm a short job." So what time-sharing did is to say look, we're going to ask you to prove that you're a short job. We'll give you the processor, you get a little slice of time. If you're a short job, that'll be enough to get you through. If not, you say, ah, you lied, put that job in the tail of the queue, and let another job try to prove he's small. And so as you go round and round—this is called round robin—as you go round and round, the short jobs will be filtered out early on, they'll expose the fact that they're short; they'll get through. The long jobs are going around more times, which is fine because they're being delayed by a small amount by the little jobs. So the idea to implicitly find out which are the long jobs and which are the short jobs comes out of this round robin. I said, well, that's an interesting thing. Why not incorporate a similar idea in the networking model? A long message comes in, chop it into fixed-length pieces. Each of those lengths corresponds to the amount of service they would have gotten in the time-sharing system. Let those little fixed-length things, which we now call packets, make their way through the network, so nobody gets stopped by a big thing; they only have little packets in front of them. And your long job, your long message becomes a sequence of packets which flows through the network, gets collected at the other end, and shipped out as a message. So in my early work, when I presented my thesis proposal, I said, "Look, we want to get an analytic model for what a data network looks like." Well, a queueing network model is the right one, and we know how to solve that. We want to decide how to route messages. Well, we're going to look at a variety of routing procedures, including fixed routing, adaptive routing, dynamic routing, so we don't force all the control into one node. We're going to deal with large systems so that they get the statistically deterministic behavior, and we're going to look at the queueing discipline. As messages go hop, hop through the network, I point out in my thesis proposal, let's study various queueing disciplines because that will help us decide on the efficiency by which these messages get passed through the network. So I started studying that, and very soon after the proposal was

submitted, I recognized that this adaptive routing, this dynamic routing, this resource sharing, and this idea of using the right queueing discipline was very important in order to get the short jobs through quickly, and so I analyzed all these systems and, in fact, I was able to get an exact analysis for this time-sharing, this round robin time-sharing system, and I was able to prove mathematically that the short jobs did do better than the long jobs did. The long jobs did worse than average, short jobs did better than average, which is exactly what you wanted in these data networks. And so I included that among the possibilities as to how to deal with the queues in my thesis. So packet switching came out of that thinking.

FIDLER

And to get there, there was an Independence Assumption that you introduce.

KLEINROCK

Oh, yes. So I set up this analytic model, started working out the details of its solution, and in order to get an exact solution—oh, by the way, the other thing I included in my thesis proposal and in my results was a field of study called network flow theory. Network flow theory talks about graphs and links and flows through these networks to determine how much can you push through a network with finite capacity links. Well, guess what. Shannon, among others, has solved that problem. He came up with something called the max-flow min-cut theorem, which tells you exactly how to predict the maximum flow you can get through a network of finite capacity links. Here's Shannon again. His fingerprints are all over this domain. So basically what I did was to set up an analytical model and then try to solve it exactly. Now, it turns out if you look at the mathematics and you look at the exact equations, this turns out to be an intractable model for two reasons: one, the statistical side drives you nuts; and secondly, the network flow side drives you nuts. So I had to find an easier way, some way to either give up on this wonderful problem I have created or do something to adjust the model so I could break through the analytics, and I decided what I'm going to do, I'm going to make an approximation. I'm going to make an approximation so the thing which is making the dependence in my statistical model so difficult and impossible to evaluate is removed and allows you to push through to a solution. And that particular assumption I called the Independence Assumption. The problem is that as messages move through the network and packets, they maintain their same length as they move through, and when they bump into each other, they cause a dependence in the mathematical equations which you just can't solve. We still haven't solved that problem today. By making an Independence Assumption, it allows you to remove this complexity. The Independence Assumption, to be specific, says that every time a message encounters a new node on its way, hop, hop, hop through the network, its length is randomly chosen again, independent of what its original length was. Two comments. One, that is clearly a false assumption. Comment number two is, does it affect the solution? Does it affect the things you want to solve for? And what do you want to solve for? You want to solve for how long it takes messages to get through the network, how many can go through, what the efficiency is, etc. So I was of the impression that this Independence Assumption, first of all, would definitely allow me to do the mathematical solution, and I had a suspicion that it wouldn't alter the results very much. So I had the following issue. I can put the mathematics through and get an answer, but then I have to prove that my assumption is a good assumption. Now, how are you going to prove it? You can't build a network. It would take years and millions of

dollars, but you could simulate a network. So I decided what I'm going to do was to use my knowledge, that at MIT Lincoln Lab there's a great computer out there I can use. I can create this massive simulation of very large networks and simulate them in two cases, with the assumption, and without the assumption, measure the things I want, like the response time, the delay, the throughput, and see if the results are close or not. If the answer is they're not, I have a problem. If they're close, I've got a miracle. And so that's exactly the path I took. What I did is I was able to solve the mathematics, extract certain principles from that mathematical solution, which I can talk about in a moment, and once you have an exact analytical solution, you can then optimize things like where to put the capacity in the network, how much to put, how to route things, what kind of topologies to use to minimize the important things like the response time. I was able to do all that, but I still had the job of proving that this was an accurate enough solution. So I decided to write this simulation program and run it on the TX-2. So while I was doing the analytical work, I also did the programming work, and this is part of my work at Lincoln Laboratory. So I wrote this very large simulation program, and, of course, my classmates at the time, both at MIT and at Lincoln Lab, people like Larry Roberts and Ivan Sutherland and a bunch of others, were present at the time. Larry wrote the compiler for the TX-2, which I happened to use. I wrote a 2,500-line program without ever debugging any of it. This is not a good approach, by the way. [laughs] And then I set out to debug it all at once, and it took me four months of grueling work to test and debug that program so it would run. If I wasn't able to debug that program, I would not have gotten a Ph.D., because I couldn't prove what I needed to prove. It was a worrisome period and, in fact, what I had was access to that TX-2 for hours at a time in the time-sharing mode, and in it's the signup mode. I could get that machine seven hours a night, from midnight to seven, four days a week, for four months running. Well, unfortunately, they weren't contiguous days. It was like a Monday, Wednesday, Thursday, Saturday. So my sleep habits went to hell. My sleep habits went to hell in evening session. I hardly slept. So I could manage that, and I spent all that time debugging this program. I remember one night I was there late at night debugging this thing and, you know, this is a very expensive computer, a million-dollar computer, all to myself at night with the printers and the tape drives and the CPU and the thin-film memory and all the sound and the whistles and the whispers and the cycles, and you get to know every sound. And by the time seven a.m. comes around, you are grubby and you've got a beard and your mouth tastes lousy and you're tired. And in comes one of your classmates. He's on the seven a.m. shift, he's bright, he just had breakfast, he's shaved, he smells great, and you curse him for coming. This was the cycle I was in. So there I am late one night, about three or four a.m., and I'm working. I'm trying to debug this damn thing, and I hear a sound I'd never heard before. It was pssst pssst, and I really got frightened, because I figured something was about to break in this million-dollar machine, and if it did, it's my fault. So I'm looking around, I'm looking at the console. This was an experimental computer, a TX-2 transistorized experimental computer, and pieces of the machine of the control panel were missing every so often because they were being repaired. And so my eyes glanced over various pieces, and they glanced up to an empty hole in the console, and in that empty space I saw a pair of eyes looking at me, and it scared the hell of me. And who was it? My good friend, son of a bitch, Larry Roberts, and he was back there trying to scare me, and he scared the hell out of me, going pssst pssst.

FIDLER

Actually, let's speak a bit about your relationship with Larry Roberts and also Ivan Sutherland.

KLEINROCK

So Larry was on the same program I was. He was working for some professors, and one of the professors on his committee was Claude Shannon also. Larry was working on a picture processing program, 3-D picture processing, trying to find hidden lines in 3-D objects. He and I were both using the TX-2 for our dissertation, as you see, I was for my simulation. We shared an office at Lincoln Lab. I met Larry first in 1959 when I started in the Ph.D. program, and we went through the whole program together. We were very close buddies. We spent a lot of time talking at MIT and at Lincoln Labs. We went on camping trips together. We went to see an eclipse, total eclipse of the sun, up in Maine. We were very close. We shared everything. I knew everything he was doing for his research. He knew everything I was doing. We shared ideas. We recommended things to each other. In fact, that was the modus operandi for these graduate students. We shared offices, and we'd always talk to each other about what we were working on, independent of the professors. If we'd have a problem breaking through, we'd get their advice, they'd make a recommendation, we'd critique what they were doing. We'd teach each other things we knew that they didn't know, and everybody was highly cooperative and highly competitive. It was a wonderful honing operation, where you hone your skills. You had to prove your mettle to your classmates as well as to the faculty. These are the people you spend the rest of your professional career with, in very diverse fields. I could go down a list of who my officemates there. It's a rogues' gallery of the giants of communications and networking technology.

FIDLER

And how did you know Ivan?

KLEINROCK

Ivan was also a Staff Associate. He came in from Caltech with his master's degree, whereas most of us had gotten our master's degrees at MIT, which was very useful to get your degree at MIT, your master's, because that prepared you for the qualifying exam to get into the Ph.D. program, because the problems on that qualifying exam were based on the material of your master's degree. However, the questions were not straightforward. In that year when I was taking that exam, they were all trick questions, and if you didn't get the trick, you'd fail that exam. So you had to be alert, clever, and knowledgeable. So talking about Ivan Sutherland, he came in from Caltech. Instead of having two years of preparation, he had like two months. I remember Larry and I and Ivan took it at the same time with some of the guys. Ivan scored top. Pure raw brilliance, wonderful approach. I really admire him. He is one of the smartest guys I know, and his name you don't hear much about. He's kept a low profile. He doesn't give interviews. Brilliant guy. His personality was that of a nerd. He'd get on the phone with you and he'd begin talking to you about a mathematics problem or a science problem instead of saying—I'd usually stop and say, "Ivan, just a minute. Say hello." [laughs] You know, a moment of chat. Now let's get into it. But he's since, of course, mellowed quite a bit. So Larry and Ivan and I and other classmates were very close. In fact, as a master's student at the Servomechanisms Lab, I was forced to start smoking a pipe. It was what you did as a graduate student. I hated smoking a pipe, but you just had to do it. It was an open bullpen.

So you sort of joined the group, you did what they did, but it was an unbelievable experience. Anyway, that's how Larry and—and Larry actually suggested while I was doing my dissertation that I apply it to road-traffic theory because road traffic's also a network of queues. It's a queueing network. I never did. Actually, more recently I have started to do that, but back then we never did that. Larry had on his committee Claude Shannon. Ivan Sutherland's supervisor, I believe, was Claude Shannon. Claude Shannon was on my committee. He wasn't my supervisor; he was on my committee. So we all shared Claude Shannon, so we had a kind of common philosophy, approach, tutelage, and we'd all reinforce each other.

FIDLER

Let's switch gears and talk about your move to UCLA and the beginnings of the Network Measurement Center, the first ARPANET node.

KLEINROCK

Sure. We can get into the UCLA story, but before I do that, I'd like to say a few words about the research I was able to conduct in that milieu.

FIDLER

Great.

KLEINROCK

So what I was looking for, as I said, was a way to capture not only the behavior of these large networks, but what is something fundamental that emerged out of that study. So just to reiterate, what I did is I created an analytic model for what a data network should look like. I put in Independence Assumptions so I could solve that model exactly, and, in fact, I have an exact equation without the Independence Assumption which can't go further. Then I use the Independence Assumption and get a complete solution, and once you have an analytic solution, there's a few things you want to do. One thing you want to do is you want to do an optimization. Now that you know the behavior exactly, how can you alter the behavior by playing around with the way you assign the channel capacity, how much capacity you put in the network, how you do the routing procedure, how you do the control, how you do the topology, to minimize the critical and important performance criteria like response time and maximized throughput, and I did that. But more importantly, having done all that, what principles, what do you learn from all of this? Are there things that come out? And by the way, that approach to not only solving but understanding and extracting principles has been the way in which I've conducted my entire professional career, and, by the way, is the complaint I have about the way a lot of research is done today. We can get back to that later, if you'd like. So what was the principle I was able to extract? Well, for one thing, it became clear, as I said, as I started out, that you want to use demand access, shared resources, not assigned in a reserved way and wasted, but dynamically assigned in a dynamic fashion, in an adaptive fashion so they're efficiently used. So that resource-sharing was critical. It turns out that if you share dynamically large resources, you get enormous benefits in terms of all these metrics, high efficiency, better response time, more throughput. So a large shared system was a principle that emerged out of this. The other thing is getting back to the original title of my proposal, the idea of information flow in large communication nets. I said if I'm going to design large networks, there's no way we can put all the control in one node and have it control tens of thousands of other nodes. It's too much of a load on one node. It's too vulnerable. It takes too much traffic in and out of that node to collect and distribute

the control information. So it had to be a distributed control system, which meant that you delegate the authority, the control, so every node participates in this control function, no one node is doing it all, which builds in an automatic and free robustness against nodes failing. If one node fails, no problem. Everybody else is helping in the control. And in addition, it portends a philosophy which emerged later when I got to UCLA in the ARPA story. The idea of delegating authority and letting everyone contribute is a fundamental principle that turned out to emerge in the management and in the architecture of the network as well. So the idea of distributed control was important. So, distributed control, large shared systems, dynamically allocated resources were all critical principles that I was able to find. I was also able to discover that if you try to concentrate the traffic into a few large shared systems, large shared networks, then you get the economies of scale that I referred to before. So, topology information, routing information, capacity assignment information all played together to say the same story, statistically: get deterministic behavior out of it by merging together a lot of things, put a lot of capacity in there, distribute the control, don't reserve, but, rather, dynamically allocate resources were all the principles that emerged. Okay. So now we can get to the UCLA story.

FIDLER

And in particular, the factors that led into your decisions to go to UCLA in the first place. You left the East Coast.

KLEINROCK

Sure. Well, it wasn't an easy leave. There I was at MIT. Lincoln Laboratory had sent me through my master's, my Ph.D., in the most magnificent, benevolent, and effective support, both emotionally, financially, scientifically, etc., and the environment they provided. So I was committed in my mind to take my career and spend it at MIT Lincoln Laboratories, which was a great place to be, lots of things going on there, very important research, very important, great people. What could be better? So when I was preparing to do that—in fact, this was in the fall of 1962, I handed my dissertation in in December 1962, but my graduation was in June of '63. That's when the ceremony was, and I had some things to finish up at MIT in the spring of '63. I told Lincoln Lab, "I'll be joining you, as we've all agreed, and I look forward to it." And here was the approach taken by Lincoln Lab. They said, "Look, we really want you to work here. There's a job waiting for you. Terrific. In fact, we'll give you a choice. If you want a window office, it's got to be shared by two people. If you want an office with no window, you get your own private office." I selected the private office; I wanted an unshared office. But they said, "Look, in fairness to yourself and to make your future career as productive as possible, we recommend that you look at other possibilities for where you might go before you commit to Lincoln Labs," and they encouraged me to do that, so I did. I decided to look at other places—I went to Bell Labs; they offered me a position. I went to some of the engineering companies around Boston, around New York, and out west. I went to Los Angeles and San Francisco, some of the aerospace companies, the research labs. Hughes had a Malibu research lab. They offered me a terrific job. In fact, they offered me \$15,000 a year, which was a huge amount of money in those days, because IBM, Bell Labs, and Lincoln all offered me about the same, which was \$12,500 a year. That was a lot of money in those days—those are huge salaries. So I went around and made this tour. While I was in San Francisco, it was recommended that I interview for an academic

position to Berkeley. So I went there, not thinking anything of it. I hadn't ever considered a career in academia, but I had done some teaching as a teaching assistant at MIT, and a teaching assistantship at MIT in those days was you taught the course, so I really had the students, the whole shebang. So you got the experience. I found I liked it. I very much enjoyed opening up the minds of some of these students. So I figured, okay, I'll interview Berkeley, and I did the interview, not really planning an academic career on the West Coast. The interview went very well. One of the faculty that interviewed me and I interviewed him was a professor named Lotfi Zadeh. He was a great name in those days in linear systems theory. There's his book over here, Zadeh Desoer. He was a giant in the field. He was a tall guy, bald-headed, from one of the Stans, Turkistan or Azerbaijan or whatever, and a foreboding figure of a man and personality, very strict, very stern. Took me into his office and he sat me down in a soft low chair, and he sat on a high wooden stool and he gazed down at me. The first question out of his mouth was, "Kleinrock, where do you stand at MIT?" I said I was near the top of the graduate students. Then we went through the—right away I didn't like him, with the setup and the question. Went through the interview and I did fine. Then we finish the interview, and I was not very comfortable with him. So when we walked out of his office, I saw he walked down the corridor to the right. I decided to go down the corridor to the left. And we're about 100 feet apart, and suddenly I hear, "Kleinrock!" he shouted down the hallway. It was an empty hallway. "Where did you stand at City College?" I said, "First!" I spun on my heel and I left. I never heard from Berkeley. Got back, all the other places offered me a position. Never heard from Berkeley. What happened is that the chairman of the department, electrical engineering department, stepped down, another guy stepped in, and they lost my file, I found out later. But I didn't think anything of it. That semester, which was the spring of '63, Zadeh comes on sabbatical to MIT, electrical engineering. He's walking down the hall, and I see him. He comes, and he says, "Kleinrock!" as if I'm his best friend. And we really hit it off. Really, he's a very nice guy. Just that first impression was a little bit off-putting. He thought, not surprisingly, that I wanted an academic position, so he contacted his good friend here at UCLA, Balakrishnan, and said, "Look, this is Kleinrock. He's done this work. I think highly of it. Why don't you interview him." So UCLA set up an interview for me. And while I was at it, I interviewed a few other universities. I interviewed Stony Brook in New York, I interviewed Rice University, because a guy named Marty Graham was there, a brilliant guy, and UCLA. Sure enough, they all offered me a position, but the UCLA position looked really good. I really liked the department, I liked the school, I liked Balakrishnan. It was a very interesting place. So here I have this offer from UCLA at about half the salary, like \$7,000 a year, academic year, 3,000 miles away from where I'm living, in the wild west—in those days, crossing the United States was a big deal—family on the East Coast. I had a family started. What to do? So I presented this dilemma to Lincoln Lab. I said, "Look, there's really this nice offer from UCLA. I'm willing to take that chance, but I feel an obligation to Lincoln Labs, and I'm not sure I'm going to like teaching in the first place." So, god bless them, they said, "Len, take that position at UCLA. If you don't like it, you can come back." Well, with all those options, how could I refuse? So, sure enough, I took the position in August of 1963. I drove my family across the country, heading west, wagon train kind of thing, came to UCLA, and I've been here for fifty years. [laughs] [End of May 20, 2013 interview]

## 1.2. Session Two (July 20, 2013)

FIDLER

So how did you go from having a research focus in computer networks to creating and running the Network Measurement Center at UCLA?

KLEINROCK

So when I arrived here in 1963, I continued to conduct research, both in computer networks and in other performance-evaluation areas, all of which were analytically and theoretically designed and focused, and I began to work with Ph.D. students and began to teach the subject. That was going along just fine, except all the while I was trying to get a way to convince industry that some of the work I had done would be suitable for them to take up and implement. The reactions I got from AT&T, as I think we've discussed already, were not the best. They said it wouldn't work, and even if it did work, they wanted nothing to do with it. And I called many—I can't recall, by the way, if we discussed this in the earlier interview.

FIDLER

Not to do with AT&T or efforts with industry. Maybe if you could speak a bit to that.

KLEINROCK

Okay. Sure. So I recall in the early and mid-sixties, there'd be these major computer conferences, the Spring Joint, I think, and the Fall Joint Computer Conference, and there'd usually be a plenary session with a group of people discussing networking, and typically there'd be two constituents: one would be the telephone industry (the telecommunications industry) and the other would be the computer guys. I remember being up on these panels in front of tens of thousands of people discussing this, and it would begin where I'd typically say to the telephone guys, "Look, please give us good data communications, data networks." And then their answer would be, "Well, the country is a copper mine. It's full of telephone wire. So why not use the telephone network?" And my response typically would be, "Well, you don't understand. It takes you twenty-five seconds to dial up a call, you charge us a minimum of a three-minute call, and I want to send a tenth-of-a-second of data." And their answer typically would be, "Little boy, go away." And the debate would go back and forth. And as history shows, little boy, we data guys, went away, and we created what we now call the Internet, which seriously disrupted the telephone networks' architecture, and they had to respond to it. But in those days, there was no embracing this new technology by the telephone engineers or telephone carriers, largely because there was no data to send. There was no revenue in data, since there was none to send, whereas they were making plenty of money on voice communications. So from a business point of view and from a short-term point of view, they were absolutely correct, but their long-range view was lacking. They couldn't see that data would dominate soon, and so they chose not to participate at all. So the reaction of industry was quite negative and somewhat disheartening. Nevertheless, I continued to pursue the research. I felt it was important to do, to develop technology as far as we could. Along the way, ARPA gained interest in this. What was happening is in 1957, Sputnik went up, caught the United States with its pants down. President Eisenhower said, "This will never happen again," and he created the Advanced Research Projects Agency in early 1958, which was an agency within the Department of Defense. The role of that agency was to bring up the level of expertise in the United States in the—

well, we're now called the STEM area, Science, Technology, Engineering, and Math, by supporting them in various ways, mainly in providing funding of research and development in those domains, and so ARPA was formed and began to do exactly that. In 1962, the computer group was formed under Licklider. It was called the Information Processing Techniques Office, I-P-T-O, IPTO. Lick was the first director of that office, and he articulated a vision around that time which involved something he called the galactic network, where he was talking about man-computer symbiosis and the wonderful enhancements that could occur if humans and computers did what they do individually best and collaborated to have this symbiotic relationship, thereby to produce yet further great advancements, but he had no idea how to do it technically. Little did he know that I had already published the technology from a mathematical and certain engineering point of view about how it could be done. But we didn't know each other at the time, even though we both had MIT connections at the same time. Two years later, one of my classmates from MIT, Ivan Sutherland, took over as director of IPTO, and early in his directorship, he came to UCLA and he recognized that we had three near-identical IBM mainframes. One was in the medical school, one was for the campus, and one was in the business school, as I recall. He suggested to me why don't we connect those three computers together in a small computer network, because he had seen my work at MIT. We were very close colleagues. He said, "Let's put them together since that's a very nice experimental testbed here." Well, it never happened, and it never happened because of the political jealousies here on campus. The three administrative groups were unwilling to yield any control over their machines, arguing that their machines were being used 100 percent of the time, and to share them would be deleterious to their efforts. They didn't anticipate or recognize the benefits of mutual sharing. Nevertheless, the concept of a network was now present in ARPA's mind. A couple of years later in 1966, Bob Taylor became director of IPTO. By then, ARPA had been funding computer research for quite a number of years and they had established some very excellent centers of research and development, and these individual centers became highly individualistic. As an example, at the University of Utah, they developed excellent graphics processing under Dave Evans and some of the others that were working with him. And at SRI, Stanford Research Institute, under Doug Engelbart, they'd put together some very excellent database technology. And over at the University of Illinois, under people like Slotnick, they created high-performance computing. Every time Bob Taylor approached a new researcher to join this effort and he was prepared to fund them, the new researcher—these researchers were called principal investigators—said, "Fine. You want me to do research for you, buy me a computer." And Bob said, "Fine. We're happy to buy you a computer." And the researcher would go on typically and say, "But not only do I want a computer, but I want to have the same power that each of those other sites has. I want the graphics from Utah, I want the high performance of Illinois, etc." Well, Bob recognized he couldn't fund to the full extent every site with every capability, so he pointed out to the researchers, "Look, if you were in a network and you want to do the graphics, you log on to the machine at Utah and run the graphics there. And you want database access, log on to to SRI," etc.

FIDLER

And just to go back to your time at MIT, this is similar to the situation that you were in when you thought more about the need to network computers together with the TX. Maybe you can speak to that.

KLEINROCK

Sure. One of the things that motivated me to do this research in the first place when I was a student at MIT, I recognized that computers eventually would have to talk to each other, and one of the things that made that clear to me was that there was at MIT Lincoln Laboratory the TX-2 computer, which was a transistorized computer put together by the developers at Lincoln Lab. They had earlier developed a transistorized machine called TX-0, the first of the series, and that machine, TX-0, is now at MIT. And I recognized that there would be an advantage and the eventual desire for the people who had developed the machine to access the TX-0 from Lincoln Laboratory, and vice versa, those who had now migrated to MIT might want the greater power of TX-2. So here was a perfect example, in my mind, where it was natural for people to want to, over some network, connect two computers together, and that was part of the motivating reason that I had. Well, this was a similar argument that Bob Taylor said. He said, "Look, you want access to a computer. I'm not going to give you an identical copy of that machine. I'm going to give you access off of a network." So Bob Taylor recognized that that was a good reason to create a network. The germ of the idea was already there by Ivan Sutherland. So here we now had a situation where there was a need, there was a technology that could be applied, and a significant desire on the part of the funding agency at ARPA, and so, sure enough, they put forward a plan, and they brought in Larry Roberts as Chief Scientist in 1966 to manage this full operation. Well, Larry came there. Larry was another classmate of mine at MIT, and he was a very close classmate. In fact, we shared an office at Lincoln Laboratory. He was extremely familiar with my work. In fact, he wrote the compiler for the TX-2, which is the machine I used to simulate my computer network research. Larry was now willing to take on this task. Actually, he was unwilling at first. Bob Taylor had to pull some strings to get Larry there from Lincoln. Well, Larry was working at Lincoln Laboratory at the time. But Larry recognized that this technology that I had published in my dissertation had proven that this concept of data networks was a reasonable one, that messages and packets wouldn't fall on the floor, that there'd be reasonable-size buffers, that the throughput and the response time were within acceptable limits. So he said, "Look, there's theoretical and simulation proof that this works. Now we're going to make a physical implementation of it," in the form of this thing which was eventually going to be called the ARPA network. So Larry took that on. He put together a group of us to help him specify what this network would look like. We met. We specified a number of aspects of it. For example, we had a representative from the time-sharing community there. I believe it was Herb Baskin from Berkeley. I'm not sure that was the person. But the point was made that if this network can't give us a half-second response time, then I can't get the feel of time-sharing over this network, because the idea was that somebody at one location will log on to a remote machine, and that user should feel as if he's locally connected to that machine in terms of response time in services. So Herb said, "We need a half-a-second response time." We said, "Okay, half-a-second response time is what we want for short messages." Well, it turned out, by the way, by the time we implemented it, we could easily achieve two-tenths-of-a-second, and so that criteria and that specification were easily met. Wes Clark was making the

recommendation that no way do you want to burden these mainframe computers you're going to attach to the network with the task of all this communications data networking. He said, put that in a separate machine alongside the mainframes. So the idea of this co-processor, which we eventually called an interface message processor—we now call them routers or packet switches—should be a separate machine. So that was in the spec as well. We'll have a separate machine, an unattended machine. It should not require any human intervention. It should have no rotating parts so they don't fail. It should be mechanically and physically strong. So we wanted a hardened machine to sit in a closet unattended. That spec went in. We also wanted reliability. Now, in those days, the telephone company was claiming very high reliability for their network of what's called five nines. Five nines means 99.999 percent of the time, the network will be running properly. What they didn't tell you at that spec at the time was that when the network was up and running, it would continue to run 99.999 percent of the time, but when it went down, they weren't counting that in their calculations. So we said, you know, those kinds of five-nine specs are difficult to prove and a little bit pie-in-the-sky. We wanted a more pragmatic definition of reliability. So what we said was that the network should be such that if any one piece of it breaks, any one line or any one switch fails, all the rest of the network should still be able to communicate. And that turned out to what we call in network flow theory, a two-connected topology. There's two independent paths between every pair of host machines on the network. So that specification was made.

FIDLER

Since we're on the topic here of these Washington, D.C. meetings in 1967, can you speak maybe more to the construction of the RFQ?

KLEINROCK

So the request for quotation had not yet gone out. This was an early meeting where we're trying to lay down the specifications. Larry Roberts then took that specification and created an ARPANET plan, and from that he created this request for quotation, which went out in 1968. One other point I was going to make, which answers your original question about how we went from the research I was doing to a Network Measurement Center, was that I recognized that if this ARPANET was going to be an experimental network, we needed some way to experiment with it, which means we had to put in some hooks, software and hardware hooks, to be able to generate artificial traffic, make measurements all across the network, send those measurements back to a location which eventually we were to call the Network Measurement Center, to trace packets as they routed through the network, to measure the levels of traffic and the response times, etc. So I said we have to introduce them to the spec that we're putting down, the ability to put in the hooks to run the experiments, to collect the measurement, to generate the traffic, etc., and that was specified as well. And all of those specifications that we just talked about went into the request for quotation that Larry put together, and that went out in 1968. And as a result, it was also decided that since UCLA, we had the expertise in network technology, that we would be the Network Measurement Center. We'd be the ones to conduct those experiments, to collect the data, to evaluate them, to match them against what we expected from a theoretical point of view, and then basically to try to find the limit of the network performance. The specification was it was our job to try to break the network, and in so breaking, we would find fault with the network and would, therefore, find ways to remove those faults and get the network up and running. So that's

the trace from the theoretical work I came to UCLA to do, to the physical implementation of the Network Measurement Center here at UCLA.

FIDLER

And were there any other challenges or issues involved in establishing the Network Measurement Center at UCLA?

KLEINROCK

Oh, yes. Oh, yes. This was a big issue, because, first of all, we had to get a team together that had the capability to write the code, to run the measurements, to put together the hardware, to accept this first switch. The Network Measurement Center was to be the first node of the network as well, so we had to develop the hardware to match the IMP-to-host interface, and to come up as the first node and then to talk to the other nodes as they came up. So I was a young professor at the time. By 1967, I was already an associate professor and I had my own Ph.D. research students, but I didn't have a software team and I didn't have a hardware team and I didn't have a staff of secretaries, of administrators, etc. Happily, one of my colleagues, Professor Gerald Estrin, had such a group that he was working with, and, in fact, that group was running our time-shared service here at UCLA on the Scientific Data Systems SIGMA 7, SDS SIGMA 7. And it was pretty clear at that point that that would be the ideal group to assist me in bringing up this Network Measurement Center, in becoming the first node, etc., so basically I inherited that group almost in toto, and to that group I added my own Ph.D. research students, and some of the other faculty joined with this effort as well to participate in this network effort, and so the group was created that way. The challenge was now to manage this group that I had not created on my own, and these were all brilliant people, names that we can discuss later, names that are now known in the network world, a very creative, opinionated, strong, powerful group of researchers and software developers, most of whom were graduate students, to work with them and basically create the esprit de corps and the management philosophy which would allow them to function properly and to extract the best out of their talents. So here I was, having experienced two types of control and management experience in coming from my research to this point. The first was that I recognized early on that if we're going to deal with a large network that we can't allocate all the control to a single node, and so, as we discussed, we needed a distributed network architecture. Well, a distributed network architecture means giving control out to many parts of the network. Each part basically controls a piece of the network. That's called delegating from a technical point of view. The second exposure I had to this notion of delegating authority was from ARPA itself. ARPA was composed of some brilliant program managers there in Washington. Many of them were themselves recent Ph.D. graduates or researchers themselves who were spending time at DARPA to promote DARPA's goal of basically improving the United States STEM capabilities, and their philosophy was one of also picking the best people, allowing those people to do what they felt best, without controlling them, by throwing considerable amounts of money over long-range intervals, without much control, chasing high-risk, high-payoff goals, which would then allow those researchers, those principal investigators, to conduct their best work in a free, fluid, and resource-rich environment. So I said here I was with those two experiences of distributing and delegating authority. Here I had a group of very talented staff, researchers, graduate students, and my philosophy was to follow that lead and allow those students, that staff, that group to organize themselves and achieve the goals that I

set for them in a nonprocedural way, as opposed to procedural. Procedural means I tell them how to do it. No. Nonprocedural means you tell them what you want and let them figure out how to do it. That's effective for many reasons. It allows the people you are dealing with to feel in control, respected, to allow them to try things and be creative, and also extracts the best out of them, and allows at the same time the esprit de corps that you create is one of, gee, we're participants, we're stakeholders. We're not supplicants or workers. We're managers as well. And that worked very well, and this group just responded beautifully to that. There's a price to pay from the management point of view. You now have a group that you don't control very closely, they have their opinions, and you've got to basically adjudicate some differences among them, among their approach and yours. So it's a very fine line and a very interesting experience and a very gratifying one as well, to work with such a creative, brilliant group and still extract the best from them.

FIDLER

And how did you see your management philosophy relate to that of other ARPA PIs?

KLEINROCK

Okay. So the ARPA PIs were in touch with each other from their professional relationships, but even in a slightly more formal way, we had principal investigator meetings every so often, where all the PIs would get together—and there weren't all that many of us at the time. There were between fifteen and twenty of us that ARPA was supporting. The interesting thing is that these other PIs were working in fields other than your own expertise. For example, they were not all doing networking. They were doing artificial intelligence; they were doing high-performance computing; they were doing database; they were doing graphics, people I would not ordinarily have interacted with. So I could observe and communicate and discuss and debate with them how they were managing their projects, because they each had these large projects. I found a number of them had a similar philosophy. They have taken the same clue from ARPA that the right way to do this—you know, the folks from MIT, from Berkeley, from Utah, from Santa Barbara, they were all people who had enough confidence in themselves that they could let other people do great work in their environment. They weren't challenged. In fact, they were enhanced by these other people they were working with in their environment. So I found most of them having a similar philosophy, but remember they were also influenced by their experience with ARPA, who was funding them and gave them that freedom, and they saw how well it worked. But there was another group of talented people who did the same thing. As I told you, I gave my own group, my own staff and programmers and developers, lots of flexibility. Well, what did they do with that? Well, bless them, those graduate students formed their own group of graduate students, well beyond the confines of the halls of UCLA. They reached out to other graduate students at Utah, at UCSB, at Berkeley, at Illinois, at MIT, at Stanford, and they formed their own graduate students' mafia, if you will. They formed this Network Working Group. They created—it came out of UCLA, but they all participated in the Request for Comments group. They were active behind the scenes in their own distributed control flexible environment, and so that same idea, that same governing policy, beautifully permeated. They picked up the same message and they were able to implement it as well.

FIDLER

Was there anything else from your personal, professional, or intellectual background that influenced this management strategy? So far we have ARPA and we have your experience on a more technical side with distributed network architecture. Is there anything else that comes to mind?

KLEINROCK

Well, again, I spent many years at MIT and I was a graduate student, and I observed the faculty there, and it was that same esprit de corps. There was confidence and, if you will, respect for your peers, and between the faculty and the students it was a very collaborative environment. It turns out at MIT that most of the gratification came from within your colleagues at MIT that typically didn't reach out across the world or across the continent, and so there was a sense of these were Centers of Excellence here at MIT, and we're here to collaborate with, impress, cooperate with our fellow students and our fellow faculty. So there was another sense that the way to work was not in an isolated domains, not in a protective or secretive environment. It was a competitive environment for sure. All throughout all the questions you asked me, yes, we were all competitive in a very positive way, and we were willing to alter our direction if we felt somebody else had a better idea rather than fight it and be secretive and pull it back. So, yes, the MIT environment is another source of that.

FIDLER

And how do you feel that the operation of the Network Measurement Center compared to other ARPANET nodes or even Centers of Excellence at that time?

KLEINROCK

Well, that's hard to respond to because each of us had a different view as to what we were doing with our ARPA funding. It turns out that the work here at UCLA was specifically network-focused. Therefore, the network itself was the object of our studies. In many of the other cases, like some of the artificial-intelligence work, the work was not network-related. They used the network to collaborate, but they didn't study the network as an entity. So there was a difference in that sense, but the tool of the network was the object of our study. Now, in terms of the management philosophy, we've talked about that. I saw a similar distributed delegated approach.

FIDLER

Do you feel that constantly working with a distributed network architecture, as something that not only you were working on, but the graduate students as well, that that continued to inform the work and management philosophy at the Network Measurement Center?

KLEINROCK

Yes, in the sense that it was self-feeding. It was self-generating. That philosophy took hold and stuck, and it enlarged, as I say, by not only reaching out to the PIs, but to the entire graduate student community. That group was an amazing thing to watch, because in some sense it started what I might call, not to be pejorative, an underground culture. These graduate students communicated with themselves in ways that did not pass through the management of their respective universities. I remember one day going into the bullpen where many of the graduate students, mine and others were working, and I saw they were very busy doing some damn thing or other. They were on their terminals interacting with something. And when I questioned them, they were all working on news groups, these hobby-related, interest-related groups that had things to talk to each other about, be it recipes, be it photography, be it computing, be it programming. And I was

amazed. I was unaware that culture was vibrating right in the bowels of this university here, but there they were doing it, and it was the beginnings of social networking as we know it now. Certainly email, by that time, had taken hold strongly. But it was an amazing thing to observe, and to suddenly be aware, boy, this thing has reached out and has a life of its own.

FIDLER

And in the six years that the Network Measurement Center operated, that would have been one change, the extent to which email was used, this underground that you described. Are there other kinds of changes that you noticed between 1969 and 1975?

KLEINROCK

Yeah, a few things. First of all, initially, nobody wanted to participate in this network. None of the PIs wanted to put their host machines on a network, for the same reason that those three nodes at UCLA didn't want to. They all felt their machines were being used 100 percent of the time, they couldn't possibly afford to give up any of their cycles, and ARPA basically forced them to participate. "Since," they said, "we're funding you, you'll join this network." We need you to do it." As soon as they joined, they began to recognize the strength, but it didn't happen—when I say as soon as they joined, it didn't happen immediately. The early usage of the network was spotty, and the reason is that it was very difficult to use any of the external resources through the network. I mean, what do you want to use? You want to use hardware, software, applications, and services. Well, in order to do that, you have to log on to a remote machine, a machine with which you may not be familiar, so you've got to get a logon name, you've got to learn the command language, you got to learn the services, you have to learn the way to use that remote facility. Why bother? Why not use the machine here in your local environment that you're used to? Well, if they have really excellent resources, you're going to push, but there was no easy protocol by which you could access these remote machines. So in the early days, most of the usage of the network was by people who had left one locale, one university, and taken a job elsewhere and wanted to use the machine at the original site they came from. They had a logon, they knew the command language. They knew that better than the new place they had arrived at. So there was that kind of reaching back to your old environment that caused some of the early traffic. The Network Measurement Center also generated a lot of traffic, which was not true traffic; it was basically measurement traffic. It was very hard to use. So as it became easier to use the network, and what made it easier was once we introduced the host-host protocol in 1971, that we began to see sites accepting the use of the network as something powerful and people actually beginning to do something useful. So those changes took place in those early seventies when it became easier to use, and we began to get more interesting sites attached. By the summer of '70, we had already crossed the United States in the network, but we needed some more interesting sites to develop, and by the time you get some of these large machines attached, some of what we now call super computers, the big mainframes, with real resources that you really want to use, that happened through the early seventies. Those first four sites, there were some specialized resources, but they weren't really by themselves something with enough variety and capability, that you really want to learn networking. You learn networking with a lot of stuff out there, and that happened in the seventies as well.

FIDLER

Do you remember the first time that you began sending electronic mail over the network?

KLEINROCK

No, I can't. I can't tell you what the first email message is that I sent, but it was in late 1972 once Ray Tomlinson had introduced it. The amazing thing about what Ray did is, time-sharing systems had email for years. Email was nothing new, but it was locked to a particular time-sharing system, so only the users of that time-sharing system could exchange email. What Ray did, he said, "Let's put that on a network," so people at different time-sharing systems across the network could share email. The interesting thing is the way he announced the arrival of this email capability was through the first email he sent to his colleagues at BBN. He said, "There is this capability now, network email," and it was the first email, which I think is beautiful.

FIDLER

Just to go back and finish up with our discussion of management and management philosophy, is there anything that you want to add about the way that goals were set or tasks were defined, either by you or within the groups that you managed?

KLEINROCK

Okay. So there were some large goals that were clearly set early on. We needed this host-host protocol; we needed to get the host-IMP protocol going; we had to connect to the IMP when it arrived; we had to get Network Measurement Center running experiments. Those kinds of high-level goals I laid down as things we needed to do. But as I said earlier, the way we had those implemented was to have my group—the software development group was headed by Steve Crocker, the hardware group by Mike Wingfield, I headed the research group with my own Ph.D. students. We let them basically direct their own activities, largely. I remember Steve would come to me every so often with two of his strong cohorts, Vint Cerf and Jon Postel, and they'd present to me a travel budget they'd want, and it was always far more excessive than I felt they should be asking for, but more often than not, I granted it because I knew they had to travel to the other sites as they were gaining access to colleagues at their level, the network working group. So we have a discussion and we talk about what they were doing, but the details of the software that they were developing and how it was being developed, I participated—I let them do it. I didn't direct them at all. They were better at software than I was. I was better at performance evaluation, networking, etc. So I gave them a great deal of flexibility, and they took it on very well. The research group I managed very closely. They were my Ph.D. students. The measurements I conducted very carefully. We set up the experiments. Some of my own research Ph.D. students were doing a lot of the measurements themselves.

FIDLER

Can you say more about the different groups? You've touched briefly on research software and hardware, but the way that those groups interacted at the Network Measurement Center, the different kinds of things that they did, the way that they worked.

KLEINROCK

Well, in addition to the protocol aspects of it, we had people doing some programming to create the software for the measurements, so we had a programming staff as well. So the groups I identified, there was software development for the protocols; there was programming to support the Network Measurement Center activities; there was a

hardware group and that was a very small but very important group to get us connected to the IMP in the first place, maintain it, and maintain the facility we had in terms of our center here. Then there was the Ph.D. students I supervised in networking research and a few related faculty who were participating and interested in networking at the time. I remember holding a class on some of the theoretical aspects, and in the class were not only my graduate students, there were some faculty attending to get up to speed on the networking technology that was being developed in those early days.

FIDLER

And did the number of these groups or their content or their focus shift over time between, for example, the establishment of the Network Measurement Center and then its discontinuation in 1975?

KLEINROCK

Well, understand these people that I've mentioned, Steve Crocker, Vint Cerf, Jon Postel, Charley Kline, Bill Naylor, they were all Ph.D. students and they had their own Ph.D. research to conduct. Some of it was related to and coincident with what they were doing for their dissertations, some was not, or what they were doing on the Network Measurement Center. So to the extent that it was not directly related to NMC stuff, they began to work more closely with their supervisors, less with the urgency of getting some of the critical protocols and connectivity up and running early on in our work at the Network Measurement Center. So the group continued to operate as an entity, but they were each going in individual directions as they were pursuing their own Ph.D. research, and not all of those in the group were Ph.D.'s of mine. They were Ph.D.'s of other faculty members as well, quite a number. So there was a sort of separation of some activity, but the group pretty much held together. We were busy running the experiments, interacting with new nodes. The Network Working Group took on a life of its own. They were looking at things that were extending well beyond the Network Measurement Center's responsibilities, so that was not directed out of my domain; it was directed out of that Network Working Group domain. So they began to function almost as an entity in that domain on their own.

FIDLER

In retrospect, it's easy to look at the work that was done and accomplished as inevitable progress and development. Can you speak to the perception of risk of this research and even how it might have changed over time, for example, the viability of getting this network running in 1968 versus the kind of work you were doing in 1975?

KLEINROCK

Sure. In those early days, this concept of making a computer network, a packet-switched network, was a challenge, a serious challenge from an engineering point of view, from an implementation point of view, from an up-and-running acceptance point of view. In my mind, there was no question but that the technology would work, but would it be properly funded, would it be properly managed, would it be properly implemented, would it be properly supervised, were challenging questions. And all of us involved in those days took it on as a serious challenge and an engaging and exciting challenge. We recognized risk in the sense that, yes, we had to do our job well to make this come off properly, but we didn't get a sense that we were in danger of failing. It was a question of how well would it work, how exciting would the challenge be, would it gain traction, that was always a question. The original concept of this network, we originally designed the 19-

node network. That was in a spec that went out with the RFQ. And BBN, who won the contract, originally made it possible for 64 nodes only. They had 6 bits of address space. So the original implementation mentality was not one of a very broad-based network. In terms of the vision that we had, yes, we saw this growing, but the implementation direction didn't head that way directly. So the risk of failure was small, in my mind, and, I believe, in most of the other participants, the risk of failure of the technology. But failure of the impact of this on the digital community, that was always a question. Would anybody use it? We've worked very hard to make it easy for people to join. I told you originally many of the PIs did not want to join, so we, as we developed the protocols, we removed as many of the impediments to join in this network as possible, to make the protocols as easy as possible, to make the requirements for membership as simple as possible, to make the restrictions on what could be run and what could be done on the network as simple as possible. That's one of the reasons that we failed to put in any protection against adverse behavior. We knew everybody that was coming on to the network. We trusted everybody. Those early days, we had an ARPANET directory of all the email addresses, when email came in. We knew everybody who was on the network. So, as I say, we trusted them. That led to the lack or zero focus on security and protection against what later became the dark side of the network, but we wanted to make it very easy and collaborative, and so the idea of trust, openness, sharing, participation was dominant in the culture, and that is what allowed people to come in and join and make it easy for them. And even then, it was not a rapid uptake, because until the host-host protocol came in, it was very difficult to do inter-host communications. Actually, there was a rather interesting point. Bob Kahn took over as director of IPTO, and he was also responsible for creating a public demonstration of the ARPANET in 1972, in October 1972. I mention that because up until that point, many of the sites were not aware of what the others were doing. We didn't really know what other applications were in the bowels of the other universities. They were doing their own applications, really neat stuff. The challenge for this 1972 demonstration was to bring all these sites together in a public arena so that the public, meaning the other engineers and other researchers, could come and see how to use this network. So we all collaborated on putting our applications out there in ways that other people could access them through the network, and we made joint efforts with other groups to make collaborative applications as well. So we began to learn what was being done out there. The artificial-intelligence community had a lot of interesting applications, chess-playing programs, robots that could run around. The simulation world got together. We were doing a number of things. One of the early demonstrations was to run a distributed simulation of air-traffic control across many computers in a dynamic fashion. I won't go into the details now, but it allowed we PIs to understand far better what was going on on the network in terms of applications and allowed the public to see it as well. So at that point, there was a greater uptake and appreciation for what was available to us as the participants in the network and, if you will, the designers and the contributors of applications and services to the network that we never had before. So October '72 was a rather important and successful demonstration.

FIDLER

So in terms of growth and change and patterns of use of the ARPANET, October 1972 is one major shift that you saw. Were there others over this period of the Network Measurement Center where use changed significantly?

KLEINROCK

Well, you have to understand that the growth of the number of hosts on the Internet—eventually called the Internet—was exponential from day one. So when you ask for significant changes, there were no major inflection points for traffic, but there was this kind of S-curve behavior. When you get a growth of a thing which makes the network more interesting to people, that becomes an S-curve, and another inflection point from another technology, that collection gives the exponential behavior. So, yes, there was some things. As you say, the International Conference on Computer Communications, 1972, that demonstration was an important one. The host-host protocol was another one that made it possible whereby people could now gain access. The bringing on of some other networks like the radio networks, the ALOHA Network, the Satellite Network, access to Europe via the link to Norway and London, they were all important in the sense that it continued to reinforce the fact that this network was a growing network full of good things of interest, it had staying power, and the community continued to enlarge in participants, and the participants were all the researchers in the field of digital computers. That community just grew and grew and grew, and by its own growth, added to the power and the steam of this locomotive called the ARPANET, hence Internet.

FIDLER

You mentioned earlier trust and openness and sharing as being integral to both the working groups at UCLA, the people working at the Network Measurement Center at UCLA, but then also you suggested that that was being built into, I guess, patterns of behavior on the ARPANET itself, at least in the early days. Is that accurate?

KLEINROCK

Yeah, it is true. Across the ARPANET there was a sense of camaraderie, there was an implied, implicit netiquette that developed. It wasn't ever written down as a set of rules. It was an accepted mode of behavior, and people behaved well. People didn't—what should I say—blaspheme. Certainly spam was not introduced. We recognized this was an experimental research network among colleagues that we respected, where this was an engineering effort. This was not a commercial effort, this was not a financial bonanza, this was not looking to exploit; this was looking to create. And it was a golden era of creativity. You couldn't have asked for a better environment. It didn't have the trappings, as I say, of a business side, a heavy control side, a nasty competitive side, some freaky people doing bad things. It's amazing, now that you ask this question, that I look back on it and that all of those bad elements did not participate, did not manifest themselves in those early days. It was a group of really well-meaning, well-minded, well-behaved people all doing amazing work. I think the fact that we were doing good work and getting the gratification of creating and growing and seeing this thing take off kept things on course. That was enough, sort of was the container.

FIDLER

So you're saying that having this ability to directly work on and contribute to this growing community is something that you saw keeping behavior in check.

KLEINROCK

I believe so, but it wasn't clear to me until you asked the question, somehow. It's sort of like a tornado that stays collimated because it feeds on itself. And there was enough gratification, enough success, enough creativity, that we didn't need other sources of diversion or whatever to make it exciting. It was exciting.

FIDLER

Now, you mentioned netiquette as an unwritten set of rules for, I suppose, conduct and behavior. Did you see a similar level of, like, unwritten practices at UCLA at this time? Was there a similar extent to which ways of doing things were developed that weren't necessarily codified, but were, nonetheless, prevalent?

KLEINROCK

Well, that harkens back to what we talked about earlier, the idea that it was not a heavy-handed management. It was a cooperative, it was a respectful, and a creative environment. Much of that was manifested in the research groups that we had, but not so much across the entire department. The entire department was not participating in network effort. These towers of excellence in the department at the time, one of which was this whole networking world, there were others as well, and it didn't cross-fertilize as much. So in some sense, this environment that we're describing was not even across the entire department, but it was across the entire community of like-minded people doing work.

FIDLER

You mentioned traction when we were talking about risk, not will this work technically, but will this be successful in certain communities. Was there a moment when you said, "Okay, this is definitely going to take off," more than just saying, "This is what we hope it will be," but something where you knew, "Okay, nothing can stop this now"?

KLEINROCK

Well, I can't tell you when I first got that sense, but certainly by the time NSF entered the picture, and NSF entered the picture in an interesting way. They created these super computer centers in the early eighties that were not necessarily connected to each other, and we were aware of the super computer centers. They were not an important part of the Internet at the time, but when NSF decided to connect them together using the Internet, suddenly the constituency of the membership in this Internet community increased dramatically from the computer scientist closed group that we talked about earlier, namely to the group who was being supported by ARPA, to a much larger community, a community of chemists, physicists, physiologists, archeologists, oceanographers, etc., all science. At the same time, it was around that period when those groups that were not being funded by ARPA, those computer science departments, for example, that were outside this chosen few, if you will, decided they wanted to participate too. So some efforts were started which were successful to allow them to participate in the Internet experiment, namely CSNET. Computer Science Net was formed, PhoneNet was formed, other ways to gain access without being blessed by the large ARPA-funding machine. So they began to come in on their own. They began to extend beyond the ARPA-selected community, and I think that was an important point when I recognized this is now growing on its own. It doesn't need the constant stoking of ARPA funding. It's reaching out. So that certainly was a very important development at the time. But I must tell you that email, which was 1972, was a strong impetus to getting other people excited about this, because that drew in people. They didn't know about networking, but they sure liked

this email thing, and they were using email without even necessarily knowing they were using a network, and so the community enlarged that way as well. But until we get the breadth of the NSF world, the community was largely computer science efforts. So when did I first realize this thing was unstoppable? The first thing when I mentioned earlier is when we went international and we went more than just wired networks. We went to ALOHAnet. We went to Satellite networks. The whole packet radio effort, by the way, interestingly enough, the idea of ground radio packet-switching, was started in the early seventies. This is very early in the genealogy we're talking about here. The applications there were broad-based. They were certainly military, but certainly in the civilian world as well, what with communications coming into fashion. That whole effort began to have a life of its own, but it was coupled into the ARPANET because it was using the ARPANET for communications as well. I could see then that this was not just about wire line machine-to-machine communications. It was allowing other things that we didn't anticipate to suddenly engage in this thing we call networking in a way which would cross continents and not just hundreds of feet or hundreds of miles. So you could get a sense that it was gaining access, the international side and the broad-based other network side, and suddenly the international community, even though they didn't engage heavily in terms of participation, because across Europe there were a number of networks that started to spring up. The Nordic Network, for example, which is a network based on high-voltage electric utilities, wanted to gain access. The French created their CYCLADES network, and CNET was the research effort in France, and the British networks and the Spanish networks and the Italian, they were beginning to emerge and then attaching as the Internet grew. So you could see this had a life of its own at that point. This is through the seventies and the eighties. So I can't say exactly when, but every indicator was up that this is going to grow.

FIDLER

Certainly. Another element of traction is program transfer, is getting the ARPANET away from ARPA at a certain point. In 1975 it went to the Defense Communications Agency. When did you see transferring the ARPANET to a different operator as being something that should happen or that people wanted to happen?

KLEINROCK

Well, it did happen, as you say. The Defense Communications Agency, DCA, took over the Network Measurement Center in 1975. DARPA was funding the whole boat for a long time, and it was clear they were eventually going to step out of that role. So other groups began to think about taking over some of the management function. Did I think it was a good thing? I'm not sure I had an opinion in those early days. I was sorry to see the Network Measurement Center transfer to DCA, not so much from a personal point of view, but because I could see that they were not picking up the role. They were not conducting the kinds of critical experiments we were doing. They didn't have the mindset to ask the right kinds of questions. They were doing network monitoring, at best, and then they stopped doing that, instead of asking questions, well, what would happen if we stressed the network in the following way, and what do we expect will happen before we conduct that experiment. So that whole approach to scientific experimentation disappeared. For that reason, I was regretful at that point. In terms of what ARPA's role was at the high levels within IPTO, within ARPA itself, once you get out of IPTO into the ARPA level, what's now called DARPA, that was beyond the world that I lived in,

and that was managed by administrators and funders and politicians and government agencies, which was not a world I was involved with and not a world that I cared that much about who the players were as long as it continued. And so, yes, the PSIs appeared, the other network operators appeared. I forget exactly when one of the access networks appeared. We had one of our own here. What was it called? What's it called? Los Nettos. I can't remember the names now of the early access providers, but it was a group trying to provide local access at high speeds, and it was local, and suddenly those things became commercial. First, it was a group of researchers putting it together. Companies suddenly went out there and became a commercial entity. You could see at that point that the financial world was now taking note of this thing and trying to make a business out of it, and certainly they did, and it was in the late eighties when these things began to take on that commercial side.

FIDLER

We speak alternately about, on the one hand, the Network Measurement Center at UCLA, and then on the other hand, the UCLA ARPANET node. Maybe you can speak to more about how those two terms overlap and how they're different.

KLEINROCK

Well, the UCLA node, the ARPANET node, was the first node on the ARPANET. Its role, besides being first node, was to run experiments, and being the first node, we were able to begin experiments from the first node on. We continued to serve as the Network Measurement Center. The fact that we were the first node was almost irrelevant in terms of there was a second node, a third, a fourth, etc., and number of the node didn't matter. The function of the node mattered. Ours was Network Measurement Center, SRI was a database capability, Utah was graphics, etc. So the point is a node was basically an IMP connected to a host. The IMP had a number: one, two, three, four. We were number one. The host provided the services and the capabilities and the applications. We were the Network Measurement Center with a host. Utah was a graphics center. So the most stark separation is that the IMP was the node and the host was the service, and yet the group of people working there, like at UCLA it was that group we talked about, those forty-odd people that I had put together to manage the network.

FIDLER

You spoke about measurement previously. Maybe we can speak about the kinds of network measurement experiments that you ran.

KLEINROCK

Sure. So the plan was to create a 4-node network in the first four months. September, UCLA, September '69; October '69, SRI; November '69, UC Santa Barbara; December '69, University of Utah, a 3-node network, a little triangular network and a stub off to Utah, and then stop for about three months while we at the Network Measurement Center and those at BBN tested out this test network. BBN was measuring the quality of the lines, some of the traffic, were the IMPs up or down. At UCLA, we were trying to send flows, traffic through the network to see the response time, the throughput, the buffer utilization, etc. So we conducted some early experiments. The most obvious first experiment was to pump data from the first node to the second node, namely from UCLA to SRI, to see how much we could pump through and how long it would take to get files through. Recognized this was in the 4-node network or whatever. And so we ran some very early throughput experiments from UCLA to SRI by sending just one connection

from UCLA to SRI and then increasing the number of connections to see how much throughput we could achieve. Those early experiments were very informative in terms of phenomena that we had anticipated, but weren't sure how they'd behave in terms of the throughput themselves. So we were happy to see that we could get good response times and good throughputs through that early network. I'm looking right here at a page from my book where I show the throughput between UCLA and—oh, it's UCSB, I'm sorry, not SRI. My mistake. It was from UCLA to USCB, from the first node to the third node as a function of how many connections there were, if you will, generators between UCLA and UCSB. And no surprise that we could begin to approach up to 50 kilobits per second as long as we had single-packet messages, but as we increased the number of generators, we began to go above 50 kilobits per second, which was the line speed connecting UCLA, UCSB, because we, in fact, forced some parallel routing through SRI, which was an alternate path. So we studied this alternate. It was very smooth behavior, and we were able to get up to about what we expected, two paths from UCLA to UCSB, subtract the overhead, and you get about what we predicted. So that was a very nice experiment, and we got reasonable response times as well. The network was functioning, there were no major faults, and we began to run some larger experiments as we connected more nodes into the network, and then we began to stress the network. We began to do a variety of things which caused the network to fail, and I can talk about some of those experiments if you like. For example, one of the things that we noticed early on, something that was predictable ahead of time by people like Bob Kahn and ourselves here, was something called store and forward lockup. It turns out that if two opposing traffic streams were heading toward each other through the network, they could occupy buffers in a certain way which would prevent either one from getting through unless one of them relaxed, and neither one would relax. In that case, we got a lockup or a deadlock. It was called store and forward lockup. We also found lockups in some of our other experiments—and once we decided that, we determined how to fix it. So we would notify BBN, "We have this lockup. Please fix it." It would take them six months to fix it because they were busy keeping the network up and running in terms of monitoring the lines and the IMPS. We ran a number of measurement experiments, whereby UCLA would send out a number of tests to various nodes, and take snapshots of what every node looked like, and send that data back to UCLA. Now, taking those snapshots was easy. Sending all that data back to UCLA all at the same time clearly placed a stress on our network. All the traffic was coming back to one node, UCLA, and, sure enough, we crashed the network. So we were conducting this innocent experiment, taking snapshots, and down went the network. And, of course, BBN noticed that because they were monitoring the status of the network, and they called us up and said, "UCLA, you just crashed the network." And we said, "All we're doing is taking measurements." So BBN brought it up again, we continued the experiment, down it went again. So we decided we'd better find out what's happening, and we found out it was due to a kind of allocation of buffers here at the reassembling site that happened to be a kind of error in the protocol. We told BBN what we suspected was the case, and they fixed it. And as we conducted other experiments—we had christmas lockup, we had piggyback lockup, etc., a variety of different types—we began to get anxious that we'd like to know what was the operating system that the IMP was running, what was the software protocols they were running, so that we could look at the code and decide how to fix it. And BBN said, "No, you're not allowed to have that code.

It's our proprietary code." And we said, "What?" [laughs] And they held on to it. So we pointed out to ARPA that this code ought to be open. ARPA had paid for it and we needed it to conduct our measurements properly. So, finally, ARPA leaned on them heavily, and BBN relented. So the next time we found some errors, we'd run an experiment, we'd stress the network, intending to break it—we could break it—then we'd find out why it broke, we'd show BBN how to fix it because we had the code. It would still take them six months to fix it. So as you can imagine, there was a kind of tension between the group at BBN, and the group at UCLA. We were breaking a network, we told them how to fix it. It was sort of, in some sense, that they considered it to be their network because they had deployed it. So how quick will they fix it? We were pressuring them, and they were complaining to us. So the relationship was one of a little bit of tension, but differing motives. They wanted to keep the network up. We wanted to take it down.

FIDLER

Right. You saw it as an experiment in progress. They wanted to have a functioning network.

KLEINROCK

Exactly. Exactly right. For them it was not a business but an engineering role, but so it was a kind of—what should I say—professional competition. It was not mean-spirited. It was almost fun in a way. We—what should I say—annoyed each other in ways that produced progress. But in terms of the Network Measurement Center, that was our role, and I was unhappy when it couldn't continue.

FIDLER

In 1974 you published a paper with Bill Naylor, "On Measured Behavior of the ARPA Network," and there, amongst other things, you found, I guess, favorite node pairing would be one way to put it, and also something that you coined "incest." I wonder if you could speak to both of those properties.

KLEINROCK

Sure. So we conducted rather extensive measurements of the traffic on the Internet. Bill Naylor was my Ph.D. student. His dissertation was all about network measurement. So we made these measurements to see what was going on in the network, and we wanted to see what we could extract out of it. Just as people do data mining today, those were the early, probably one of the first data-mining experiments ever run. So we noticed some things in the statistics of the way the traffic would move around the network. For example, we discovered that a significant percentage of the traffic at a given IMP was going between two hosts at that same IMP, namely host at a given node, say UCLA, was sending traffic nowhere in the network. It was going into the IMP and back out to another host at same location. After all, if two machines want to talk to each other, what better implementation than to use the local IMP, which knows how to connect two machines together without even going out of network. I forget the number. I think it was 21 percent of the network. You can remind me, Brad.

FIDLER

I think there was an average of around 20 percent of zero hop.

KLEINROCK

We were very much surprised. I'm checking in my own book right now, what the number was. Yeah, basically a lot of traffic was going zero hops, and I called it incest because it

was between two siblings at a given site. And, yeah, approximately 22 percent of the traffic was incestuous, a huge amount, one-fifth of the traffic was incestuous. And we found, in addition, that certain pairs of sites were very heavily intercommunicating, and we noticed that as well. So there were some favorites. For example, UCLA would have a favorite—I forget who it was—and SRI would have a favorite. Yeah, 34 percent of MIT traffic was incestuous. As an example, the favorite sites—I could look up again, not so easily, but various nodes had various favorite sites. So we began to extract some rather interesting behavior through the network. In fact, we realized that if we had links only between a given node's favorite site and no other node, that that would account for a significant fraction of the traffic reserved on the network, which is an interesting observation. You don't do much with that. You want the full network topology so that you can communicate with everybody.

FIDLER

And there was surprise when these results came in, because, on the one hand, you've gone to all this trouble of making an invisible subnet, and it should be the same cost to communicate to any other node, but then you have these patterns of local use.

KLEINROCK

Yes. Well, in hindsight, of course, it's not a surprise. There were certain sites that offered better services than other, or there were certain pairs of sites that had a lot in common. Just because a node is in a network and easily accessible doesn't mean it's one you want to connect to or one you want to glean services from. So, in hindsight, it makes a lot of sense. Some of the larger sites were more interesting. Some of those that had more interesting applications, be it artificial-intelligence programs, simulation programs, were accessed quite a bit more. But the incestuous traffic was a surprise. In hindsight, it makes sense, yes, you want two computers at a site that know each other to communicate. Fine, use the IMP. But we did not anticipate that. We thought a node was a node, and they wanted to talk to other nodes, didn't want to be incestuous, and that was rather interesting and kind of a remarkable—one-fifth of the traffic. That was a lot.

FIDLER

So that you told you about the way that this network was socially organized, I suppose.

KLEINROCK

Excellent observation. You're exactly right. If I knew somebody, I want to communicate with their machines that they were producing applications on. And as the network grew and we began to interact with people remote from our physical location, as distance disappeared, that effect goes away. I wonder what the incestuous traffic is now. On the other hand, we have so many hundreds of millions of hosts. I suspect the incestuous traffic is tiny. I mean, who wants to connect to my laptop except me?

FIDLER

And Ethernet came in as something of a solution to this local traffic.

KLEINROCK

Yes. It's interesting that it was recognized, not only from our recognition of incestuous traffic, but as the digital community matured, that more and more machines were being used at local sites, say at corporations, say at universities, say in government offices, and there was a need to connect things based on locality as well as on what services they have to offer. It would be natural that people want to communicate within a given organization or department. And so when Ethernet came in, which allowed for local area networks to

emerge in a dramatic way, that sort of provided yet more motivation and more access to incestuous traffic at a given site, where the Ethernet link was the communications among siblings at a given location. By the way, interestingly as well, perhaps one of the first really financially successes that took place on the Internet as a communications entity was Ethernet itself. I would say that Bob Metcalfe was one of the earliest wealth-accumulating individuals on the network, way before many of the other networking successes came along. I mean, IBM and all those other companies are doing great in the digital world, but not in the networking world. And this whole local area network is also interesting because for years the trade magazines were predicting this is the year of the LAN, and it kept being postponed and kept being postponed. And I'll never forget, I think it was 1982, but I've got to check, when finally the IEEE, who, was tasked with the job of creating a local area network standard, came out with their standard, and I remember very clearly mentioning this in many of the public conferences and trade-show arenas. I would produce a foil, an overhead foil, of a page in, I think it was Computer World, and the headline was "IEEE Adopts Local Area Network Standards," and the most important letter in that sentence was the last S. They didn't adopt one standard; they adopted three. They adopted carrier sense, multiple access on a wire named Ethernet, with collision detect. They adopted Token on a ring, which was the IBM, SNA, Token Ring technology. And by the way, Ethernet was on a bus as opposed to a ring. And just to be safe, IEEE adopted Token on a bus. So they had all the combinations there, but it was hilarious that they adopted all three. And for a long time, the battle was raging between Token Ring and Ethernet, and one came out of an industry proprietary standard, Token Ring, IBM's, and the other was a grassroots effort, this Ethernet bubbling up from beneath, and we all know who won. It was a grassroots effort that made it, and that's a story that's repeated over and over again. An industry-forced standard from above will almost always lose to a grassroots effort from the user population from below. But you're quite right that Ethernet exploited and recognized and made it possible for this location-based communications to blossom. [End of July 20, 2013 interview]

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