LEO BERANEK

An Interview Conducted by

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Center for the History of Electrical Engineering

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INTERVIEW	Leo Beranek
INTERVIEWER	Janet Abbate
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PLACE	Cambridge, Mass.

Abbate: To begin, why don't you tell me a bit about your education and how you first got interested in electrical engineering? I guess it was radio at that point.

Beranek: My interest then was certainly radio. In grammar school days, I lived with my parents on a farm on the outskirts of Solon, Iowa, a town near Iowa City. My only sibling was my brother, Lyle, who was seven years my junior. My father bought our first family radio in 1923. It was a one-tube Crosley, with three sets of earphones. I took a great interest in it, first stringing an antenna (wire with insulators at each end) between a distant tree and the house and then devoting most evenings to tuning in distant stations. I had a natural bent for anything electrical or mechanical. My folks said that I could fix anything. My grammar and junior high grades in math and physics were excellent.

My mother died in January 1926 when I was eleven and in seventh grade. My father sold our farm a year later and we went to live with his parents in Solon. My father remarried in 1928 and we moved to Mt. Vernon, Iowa, a town of 2500, sixteen miles east of Cedar Rapids, where he had bought a half share in a hardware and farm machinery store in partnership with his cousin, Gilbert Beranek. The store was called Beranek Hardware.

In the fall of 1928 I entered Mt. Vernon high school as a sophomore. The hardware store also sold radios. RCA and Atwater Kent were the two popular brands of that era. Cornell College, a liberal arts school then with about 700 students, was also located in Mt. Vernon. The store hired a Cornell student named Francis Pratt, to help them install radios and repair sets that were brought back not working. My father, knowing of my interest in radios, had the idea that with training I could learn to be a radio repair man. He purchased a correspondence course on radio in my name in the fall of 1928 from the International Correspondence Schools. The course was good for its time. I studied hard and took their examinations and mailed them to the school for grading. I ended up a year later feeling that I understood a great deal about radio. The radios were simple then, the cheapest used only triode vacuum tubes and the most expensive (RCA) used tetrodes. I even built, as part of this course, a crystal set, which is the oldest form of home receiving set requiring no power source and capable of producing sounds in one set of earphones.

My father had another good idea in the fall of 1929. He spoke to Francis Pratt, who was now a senior at Cornell College, and asked if he would take me on as an apprentice without pay, so that I could learn to repair radios and do repairing at the hardware store after he left Cornell. He agreed and this began my real training in radio.

I can remember that Pratt had a little shop where he'd do his repair work. I'd go with him in his car to pick up radios, and in his shop would watch him diagnose the trouble. I would often solder the required replacement parts into the set. All during our repair periods, we would listen to RCA-Victor (78 rpm) recordings of opera, generally from the Chicago Lyric Opera Company, because he was a native of Chicago. This was one facet of my learning to love good music. At the end of the school year, I felt capable of repairing the radios of that time. Pratt graduated from Cornell and I never heard from him again. Thus, in the summer before my senior high school year, I became the radio service man for Mt. Vernon and Cornell College.

My father and his partner gave me a small room above their hardware store which I fitted out with some work tables, a desk and a couple of chairs. The store carried a stock of vacuum tubes. With dad's financial help, I bought a small stock of parts for radios. I frequently had to order parts by mail. I did all the radio installation work for the store and repaired radios for anybody in town. In those days, most farms in Iowa had no electrical power, so their radios operated from automobile storage batteries. Most owners also bought an electrical propeller-driven generator, which had to be installed on the ridge of the roof of the house. The generator would produce enough electricity to keep the battery charged when used with the radio and one 25 watt light bulb, usually installed in the kitchen. Reading in the farm homes of those days was by gasoline lamps, with mantles like those found on the city streets before electricity.

As time went on, I became better at diagnosing troubles and gradually, I could afford better test equipment and tools. The store had a vacuum tube tester and I had meters and tools.

I should interject another bit of history. When I was in eighth grade in Solon, Iowa, my father thought I ought to learn to play a musical instrument. I had taken some piano lessons, so I could read music. The high school in Solon had a band, and they needed musicians. I talked with the music teacher, and he recommended I try a trombone. I didn't like it. Next he suggested a bass horn. Grandmother didn't like it. Finally, I took up drums. My father bought me a complete set of trap drums as well as a marching snare drum. The teacher arranged for an instructor and I practiced at home on a slanted rubber pad to save the family's ears. I took to drumming and played in the band. When I moved to Mt. Vernon, a local dance band leader learned that I played drums and engaged me to play at dances, usually on Saturday nights in Cedar Rapids. This provided me with some additional income.

But then disaster hit Mt. Vernon and the U.S. The big stock market collapse of 1929 produced a depression. By the summer of 1931, after graduating from high school, I had saved a little over $400 from radio repairing and dance band playing. This was enough to pay my first year's tuition at Cornell College. In the middle of August, 1931, I decided to remove $400 from my bank account and take the money to the bursar's office and pay my freshman year's tuition. I walked into the local bank and asked to withdraw $400. The teller said she would have to call the manager. He asked me what I wanted the money for. I said to take to Cornell to pay tuition. He said, "If you wanted it for any other purpose, I would not give it to you." I paid the tuition. The next morning, repeat, the next morning, the bank did not open its doors. I lost what remained in the account. The Beranek Hardware business was getting worse, and there was not enough income to support two families. One year later, the summer of 1932, my father sold his half of the business to his partner and moved to Cedar Rapids. Then dad's bank failed and he lost everything. He became a realtor, his specialty was farm land, in Cedar Rapids. I did not move to Cedar Rapids, but lived for the remaining years at Cornell College in a succession of cheap-rent places. I kept my radio repair shop over the Beranek Hardware store and continued to play the drums, so I completely provided for myself from then on.

I enrolled as a freshman in Cornell College in the fall of 1931. I already knew that I was to major in mathematics and physics because they were an adjunct to my radio business. I also played snare drum in the Cornell College Band and tympani in its orchestra. I continued to repair radios, so I was able for two and a half years to pay my tuition ($400 per year) and my other living expenses. But half way through my junior year (January 1934), I couldn't make ends meet, so I sought full time employment. My radio business could be turned over to another student who was two years older and who had just transferred to Cornell in the fall of 1933. He was an amateur radio operator, and knew radio very well.

I wrote a letter to Arthur Collins, the president of a small radio transmitter manufacturing company in Cedar Rapids, Iowa, asking him for a job. He hired me for the period from February to June as an aid to that part of the company's sales department that sold sound systems. I traveled with one salesman most of the time, selling and installing sound systems in funeral homes. I tired of that job by the end of the summer, and went to Arthur asking him for a more challenging job. He offered me a job as a technician in the engineering department.

Just a word about the Collins Radio Company. Arthur Collins was a young man, a college graduate, with a great interest in radio. His father was wealthy (did not lose his money in the depression) and he set his son up manufacturing radio transmitters using some unique inventions that made Arthur's transmitters attractive particularly to radio amateurs. The Collins company's big break came in 1928 when Admiral Richard Byrd chose one of his transmitters to take on his famous expedition to Antarctica. His business grew from that point, until it has become today very large and is now a division of the Rockwell International. Almost every commercial aircraft used and still uses Collins or Rockwell radio and navigation equipment. When I joined Collins, it had approximately 150 employees and a very small engineering department.

Abbate Where was this?

Beranek: Cedar Rapids, Iowa, 16 miles from Mt. Vernon. I worked at Collins from February 1934 until January 1935, when, at the recommendation of M. H. Collins, Arthur's father, I returned to Cornell. He convinced me that there was no future for a radio engineer without a college degree and some graduate work.

In Collin's engineering department, I first assisted an engineer who was designing a "mule-pack" radio transmitter for sale to the military in Colombia, South America, for use in the mountains. I took his paper designs and assembled them in "breadboard" form for preliminary testing. Later, I helped make the prototype and I did advanced wiring for the engineering department.

That year was very important for me. I not only worked with brilliant engineers, but I came to understand the competitive nature of running a high-tech business. Collins was competing with RCA, the largest manufacturer of radio transmitters in the world. They threatened to sue him for patent infringement, but Collins had some patents that they wanted access to, so they compromised by cross-licensing.

I remember one incident. The engineering department had just developed their first 1000 watt radio transmitter. Arthur Collins, holding an amateur radio broadcasting license, wanted to try it out. So he put me in charge of getting it installed in his office. It was about the size of three of today's refrigerators. I arranged for laborers to move it into his office and for others to erect a broadcasting antenna outside and bring the lead-in from it to the transmitter. We had it working by the next day, and I placed the microphone on his desk. I was there when he turned it on for its first time on air. As is customary among amateur broadcasters, he sent out the request "CQ" which means, "seeking you to talk to." An answer came from another amateur in Mexico City. The fellow said, "You are coming in loud and clear, above everybody else, what kind of a rig do you have? Arthur answered, "Just something we threw together today." Of course, it was one of the most powerful amateur radio transmitters in the country.

I went back to Cornell in the middle of my junior year, February 1935. I learned that I could not complete my degree until August of 1936 because some courses that were necessary for graduation were not available until summertime. Harold Ericson and I joined together, and not only repaired radios, but we set up a store on the second floor of a building on main street and hired two people to work for us full time. We also wired houses, because under Franklin D. Roosevelt's presidency, low interest loans were available for rural electrification. By working almost forty hours a week -- radio, wiring, and dance band -- I not only paid my $400 a year tuition and my living expenses, but I saved about $600 in addition for graduate school by graduation day in August 1936.

Toward the end of the second semester of my junior year at Cornell, I began planning for graduate school. My meager savings were not sufficient to pay for both tuition and living expenses, so my only hope for graduate school was to obtain a scholarship. I decided to apply for scholarships to all the schools around, including the Universities of Iowa, Illinois, Minnesota, Missouri, Michigan, Wisconsin, and Kansas. Then came my big break.

It was mid-August, 1935, and the temperature in the shade must have been over 100 degrees. In those days, the principal highway between Washington, D.C. and San Francisco was the Lincoln Highway, known as Route 30. It ran through Mt. Vernon east and west on Main Street. I was walking along Main Street, when I came upon a Cadillac Sedan, parked at the curb. It had a flat tire. A well-dressed man beside it was looking sadly at this catastrophe, because it was unbearably hot and the tire was dirty. Seeing his plight -- I must have been wearing jeans -- I said, "Could I help you?" His answer was an enthusiastic, "Yes, you certainly could." He opened the trunk and we got out the jack and the spare tire, and I changed the

flat. Then we got to talking. He said, “What do you do?” I answered, “I go to the college up the street here, it's called Cornell.” He said he had heard of it and he asked, “What are your subjects?” I told him about my majors, math and physics, and added, “Particularly, I want to study radio.” He said, “That’s interesting, that’s my field.” Then he asked me, “How are your grades?” I replied, “Fine.” Next, “What are your plans?” I answered, “I want to go to graduate school,” And I explained my plan was to apply for scholarships at various Midwestern universities. He seemed interested and asked, “What is your name?” After I told him, I asked, “By the way, what is your name? He answered, “My name is Glenn Browning.” I said, almost incredulously, “You are Glenn Browning? I read one of your papers in a technical journal in the library this morning.” Instantly, I had made a friend! Browning was one of the leading radio engineers in the Boston area. He had a company in Winchester called the Browning Laboratories, that existed through World War II, after which he sold out and retired. He had gone to the Graduate School of Arts and Sciences at Harvard. He next asked, “Have you thought of going to Harvard?” I answered, somewhat stunned, “No, that’s a rich man’s school.” He said, “They’ve got more money for scholarships than any of the places you’ve named.” He added, “I’ll tell you what. I’ll give you the names of the people at Harvard to whom you might write. The dean of the engineering school and the head of the radio program are both friends of mine. You ask for application and scholarship blanks and when you return them, use my name as a reference.”
I wrote down the names he gave me and when the blanks arrived I used his name as a reference, as well as my professors’ names at Cornell. I also filled out applications to the other schools as planned.

Then came April and May, 1936. One after another I received letters from the admissions offices at the Midwestern universities. Every one turned me down with a note, "Your qualifications are excellent, but there are so many applicants and not enough money for all. We are very sorry." The last letter of all was from Harvard, offering me a full scholarship. My motto now is, "Don't pass up a chance to change a tire!"

I got to know Glenn Browning very well. I was invited to his home in Winchester as soon as I had arrived and settled down. He, his family and I became very good friends. After he sold out he and his wife moved to New Hampshire where they eventually both passed away. His two daughters are in the Midwest.[1]

Harvard was both scary and challenging. Fortunately I had good teachers at Cornell so that entering graduate school was hardly a break from my senior year at Cornell. Some of the very early radio work in this country had been centered around Harvard's Cruft Laboratory. I got to see all the early radio equipment there, such as Tesla coils. The laboratory shared in the experiments of Marconi. The first transatlantic broadcast took place at Wellfleet on Cape Cod. The early Harvard Cruft Laboratory professors had all known Marconi.

One of the most important inventions in broadcasting and telephony came out of Harvard's Cruft Laboratory. The crystal oscillator was invented by Professor George Washington Pierce. Before his invention, there was no way to hold a radio station's signal precisely on its assigned frequency. A station roamed around on the dial, and, frequently, two stations would roam to the same frequency. The crystal oscillator changed that. It made it possible for every radio station to stay fixed on the dial at its proper frequency. What became more important was that it made possible "heterodyning" of audio signals. This meant that a number of telephone conversations, instead of only one, could take place at the same time over a single pair of wires. The Bell Telephone company tried to hire Pierce away from Harvard. Instead he arranged a licensing agreement with them and became a multi-millionaire off the royalties from the crystal oscillator patent. Pierce also did important work in underwater sound which was used in World War I and World War II. I enjoyed my friendship with him, and after I received my doctorate, I spent the summer with him at his summer home in New Hampshire. He was writing a book and wanted me to help him with the typing and drawings.

Now, let us go back to Harvard. In the first year of graduate school I got straight A's in my courses, and I was laying the groundwork for what is today called "electronics."

Abbate: This is the physics department at Harvard.

Beranek: Actually the Cruft Laboratory, where I was. It was a halfway station between engineering and physics. In fact most of the professors there have (or had) the title of "Professor of Physics and Communication Engineering." Today, there is an applied physics department in addition to the physics department. It has taken the place of what was then only the Cruft Laboratory. The engineering school was abolished about 1950.

My first year at Harvard was very trying physically. My meager savings barely paid for my transportation east, my room and board, my books and my clothes. Actually, I could afford only two meals a day, breakfast and dinner and my budget at Harvard Square restaurants was fifty cents a day. I lost a lot of weight and was lucky to get out of that year without health difficulties. But my subsequent years were much different.

Professor Frederick V. Hunt, whom everybody called "Ted", obtained money from the dean's office to hire an assistant. His field was acoustics, and he had never before had an assistant. He had learned during the year that I was adept at handling radio circuits and that my grade record was among the best. He asked me if I would be his half-time assistant. I could go to graduate school the other half time. I badly needed the money, because my resources were down to the vanishing point. Now I had a decent half-time salary and I could again eat three meals a day.

I was Hunt's assistant for two years and during that time he invented the first light-weight phonograph pickup that transformed audio recording and playback. Before that time phonograph records were played at 78 RPM and were three minutes to a side. With the new pickup, long-playing records became possible, 33 RPM and plastic. The motivation for this invention was the tercentenary celebration at Harvard, its 300th year. Recording of all the events at Harvard were made on vinyl records. The recording device they used cut a continuous sliver of material from the disc, which left a groove that the needle on the playback arm could follow. But the playback for 78 records weighed several ounces and would have ruined the vinyl discs with one playing. It was absolutely necessary that a light weight pickup be invented if they were ever to be played.

As Hunt's assistant, I ended up doing a number of things. First, during the development of the light-weight pickup, I had to run between him and the skilled mechanics who were building the pickup and then help him test them. But we needed good equipment for testing the equipment. He had me develop a very good loudspeaker system. I designed and had built from plywood a folded bass horn, using Jensen loudspeakers as the driving units. The folded bass loudspeaker enclosure was in two parts, each with dimensions 5 x 4 x 3 feet. We were able to buy a high frequency multi-cellular horn which was placed on top. Then I designed and built a power amplifier, because none were available on the market with the power we wanted. The phonograph pickup turned out very well and was featured on the cover of the magazine *Electronics* in 1938 or 1939.

In addition, Hunt was interested in architectural acoustics. A professor at MIT, Phillip Morse, who lived in the apartment below that of the Hunts, had written a book with a new theory for sound in rooms, called the normal mode theory. We knew the theory was valid for small rectangular rooms (3x2x1 ft), but we didn't know its applicability to large rooms (20 x 15 x 9 ft). We had in the Jefferson Physical Laboratory, a room of this larger size which was suitable for acoustical experimentation. My job as Hunt's assistant was to try to apply the Morse theory to the acoustics of this room with and without the presence of large areas of acoustical tiles and to learn whether the predicted values were confirmed by the measured data. He had already developed the necessary measuring equipment, and put the room in condition for the measurements. We worked together closely.

At the start of my second year at Harvard, fall of 1937, a Chinese student from the University of Peking came to the Cruft Laboratory. His name is Maa Dah-Yuh, or in the American way, Dah Yuh Maa. He was interested in extending Morse's theory. So Hunt, Ma and I worked together to apply the new theory to the larger room. We eventually wrote a paper jointly that was published in the *Journal of the Acoustical Society of America*, titled, "Acoustical Analysis of Sound Decay in Rectangular Rooms." This paper came out in July 1939, at the end of my research assistant position with Hunt.

Hunt never received any financial benefit from his light-weight pickup, because he tried to merchandise it to the companies that made juke boxes. The largest was the

Wurlitzer company, that had record playing machines in restaurants or other places where young people congregated. It turned out Wurlitzer didn't care that the heavy pickups wore out the records, because they kept changing the records as new ones were issued. They just threw the old ones away. The Fairchild company sensed that music lovers would buy high quality phonograph equipment, and they invented around the Hunt pickup and took over the market. Their pickup and its successors made possible the 33-1/3 and 45 RPM records. Thus, the whole recording industry was changed.

Certainly Professor Hunt liked my work. Somebody, probably Hunt, recommended me for one of the university's top fellowships, the Parker Fellowship. Without any warning, I received a call from a Harvard dean one day who said, "Are you Leo Beranek?" I answered, "Yes". He said, "Mr. Beranek I am happy to inform you that we have awarded you the Parker Fellowship for this year." I stuttered and managed to say, "What does that mean?" He answered, "It means you can continue your studies full time and can travel anywhere to study. You are not confined to Harvard." The money--I forget how much it was--was a fair amount for that time. It was about equivalent to what you'd earn as a new graduate engineer. I did not travel, because of the war in Europe, but it was a great push toward my doctorate. I stopped being Hunt's assistant, although regretfully because it had been an opportunity from which I learned much as his assistant I had developed a close friendship which lasted during his life. Under the Parker Fellowship I worked on my thesis. I did nothing else. My thesis had to do with a new apparatus and method for measuring the acoustical properties of materials used for absorbing sound, usually in rooms. I received my D.Sc. In June 1940.

Abbate: I'm wondering if your dissertation work was more on the electrical side in terms of the measuring equipment, or was it materials science? What was actually the focus?

Beranek: My studies constitute a case where the electrical side and the acoustical side merge. What we were doing was creating a sound field. I had to develop a new kind of loudspeaker for this purpose. That required electroacoustic knowledge which is always a major concern of studio radio engineers. I had to develop a type of microphone that was very small, because when it was inserted into the apparatus I did not want to disturb the sound field. The measuring equipment to the eye was a long tube with a sliding piston in it; a microphone that could be moved back and forth along the axis of the tube; a loudspeaker at one end, and the acoustical material at the other end riding on the moving piston. Outside of the measurement setup, I had to have audio amplifiers and electrical equipment for generating the signals that would create the sounds from the loudspeaker. Finally there was equipment for amplifying the output of the microphone and sending it to the meters and a cathode ray tube. In summary, I was using a combination of the latest electrical equipment, a newly-designed microphone and a special loudspeaker invented exclusively for this purpose, all produced for investigating some of the fundamental principals of acoustics.

Abbate: You were really integrating the theory of acoustics with the development of equipment for investigating the properties of acoustical materials.

Beranek: And the study was using electronic equipment of the latest type. Most of the electronic equipment of that day was bought from the General Radio company which was nearby on Massachusetts Avenue and was the leading maker of test equipment.

Abbate: I am interested in knowing your relation to the IRE at this point. And did you become a member early on?

Beranek: I became a student member while I was a senior in high school. The situation was that there was no library in Mt. Vernon or Cedar Rapids, Iowa, that had a subscription to the *Proceedings of the Institute of Radio Engineers*. And the country was in the midst of a depression, so there was no way to influence any local library to buy such an esoteric journal. I needed the IRE journals to understand the new developments in radio. However, I was one year short of the required age for student membership. To obtain the IRE *Proceedings*, I took the liberty of stretching my age one year. I cannot understand why the IRE had an age limit anyway. If a person was a student, and could understand the magazine well enough to want to pay a membership fee, why should he or she be prevented from joining? My minor deception was beneficial to me and it certainly did not harm anybody. There was another important technical niche in my life. At Cornell, the speech department wanted to record the voices of the speech students, both at the beginning of the course in the fall and the end in the spring, to see how much they had improved. The speech department bought a recording machine that embossed grooves on aluminum disks. In those days, there was no other kind of recording equipment except those used for motion pictures, which were very elaborate and expensive. The speech people wanted somebody to operate and maintain this equipment. I was paid a few cents an hour, but I first had to design a small recording studio. The studio needed to be no larger than a respectable closet, but the space needed to be quiet during the recording and the normal reverberation of the room had to be eliminated. So I went into the Cornell library and found information on studios in a popular magazine. I designed a studio, set up the recording apparatus outside the studio, put a microphone inside and made the recordings and played them back to the students, one by one. Audio and recording was a very important part of radio in those days.

Abbate: All right. Why don't we go on.

Beranek: I completed my doctorate in June and had already submitted a paper to the Acoustical Society of America for publication in May. It appeared in the July issue, indicating that the editor of *JASA* read it himself and probably called Professor Hunt to make sure that he thought it was ready for publication. Of course, the editor knew that a thesis committee at Harvard, which included G. W. Pierce, F. V. Hunt, J. H. van Vleck and E. Leon Chaffee, had reviewed and passed the thesis. Thus, I suspect that it was not sent out for peer review.

After my doctorate, my plan was to stay on at Harvard. I was appointed Faculty Instructor beginning July 1, 1940. Because Harvard had abolished the category of Assistant professor for several years around that time, I was named Faculty Instructor, with all the privileges of an untenured professor.

Then started defense research nationally. In the summer of 1940, Franklin D. Roosevelt gave permission to start the National Defense Research Committee. The NDRC was set up partly at the request of Albert Einstein to get scientists seriously involved in the war effort. Because the United States was not at war, it was called defense research. The first NDRC project was the Radiation Laboratory at MIT. The second project set up was under my direction and was called "Research on Sound Control" at the beginning and later re-named the "Electro-Acoustic Laboratory."

The politics surrounding my appointment as director of the laboratory is probably worth recording. The project came to the NDRC, probably in September 1940, from the United States Army Air Corps soon to be renamed the U.S. Air Force. The request stated that the pilots who where flying B-17 bombers on missions from England to Germany, were returning from their flights so fatigued that each landing was endangered. They thought that this fatigue arose from noise and vibration in the airplane. They wanted this suspicion investigated and steps taken to reduce the noise.

Karl Compton was president of MIT and also chairman of the division of NDRC that looked after general projects like this one. Compton spoke to Professor Phillip Morse, who was also at MIT, and asked him what he would recommend to meet the request. Morse apparently replied that the project could be set up at MIT, that he would direct it, and that he would try to get the person most knowledgeable about the properties of acoustical materials to work for him-- probably having me in mind. Nobody knew the size of the project. Some thought that specifying more quiet in an airplane would be easy. Morse called me on the telephone and said, "Leo, I have just received a project, a government project, and would you come to MIT and be my assistant? The project is about quieting airplanes and it requires the use of acoustical materials. This is your field. I think the project will be exciting and it's very important. I'd like you to be my assistant." I was very flattered--I had the greatest respect for Professor Morse--and responded, "Of course I will. I am concerned about the war in Europe and I think this is a wonderful chance for me not only to work with you but to do something important using my thesis knowledge. I'll be glad to join you."

I immediately went to Professor Hunt's office to tell him what had happened. Hunt blew his stack. He said, "Now look, this isn't fair. MIT already has the Radiation Laboratory. What are they going to do, take all the laboratories? And what's more, you've done your work here and the work should come to Harvard as a result. I think that I instead of Morse should be the one who supervises it."

Hunt went to MIT to talk to Compton. He repeated what I have just said. Compton must have listened and told Hunt he would contact him later. I learned that Morse did not want to give up the project and that he already had my promise to work with him. He felt that there was nothing magic about where the work was to be done. Compton called back and told Hunt what Morse had said. I understand that Hunt replied that he was not going to give in, and if necessary he was going to Washington to make a bitter complaint about their giving all their research money to MIT. The next thing I heard was that Compton had said to them "A plague on both your houses. We'll let Beranek run the project at Harvard."

So at the age of twenty-six, I was given a war project to direct and the first contract that Harvard had ever had with the government. I was asked to appear at a committee meeting called by NDRC and the Air Corps at the American Physical Society's Headquarters in the Flatiron Building in New York in October 1940. The NDRC appointed an oversight committee for the project with Professor Morse as chairman, and with Hunt, Professor Hallowell Davis (hearing expert at Harvard), Harvey Fletcher (director of physics at the Bell Telephone Laboratories), some military officers from the army and navy and somebody from NDRC headquarters in Washington.

Morse asked me to prepare a first year's budget. I reasoned that Harvard would pay my salary. I would need an assistant and some shop equipment. I would need some more electronic measuring equipment. The budget I presented was something like $3000, equivalent to about $60,000 now. The Army Air Corps officer who had requested this project, Major Frederick Dent, was in attendance. He eventually became a general. He told us how import this project was and that it had to move rapidly. I presented my budget and the room became quiet. Major Dent said, "Look, you are not going to make progress at that rate. We are going to multiply your budget by a factor of ten and make it for half a year instead of a year. Your annual budget will be $60,000. [2]

It immediately crossed my mind that this was going to require some major changes in my life. First of all, I would have to hire staff to work with me. Previous to now, I had only supervised three men in the radio company in Mt. Vernon, Iowa. I would also have to arrange with Harvard for space and would have to talk about financial matters. I returned on the train to Boston from New York with Professor Hunt. I told him that the first thing I would have to do would be to locate a central office for myself and to install a telephone. His response was, "Now Leo, take it slow." That remark was based on the fact that all the professors in the Cruft Laboratory had only one telephone for their use, in the central secretarial office. We were summoned to a telephone call by a Morse code signal that sounded throughout the building. My previous experience had taught me something about salaries, about how people think when they are working for someone, and about equipment.

My first mission was to find somebody in the Harvard administration to learn about their requirements. I was told by the president's office to see the vice president for financial affairs, a man whose name was Lowes. He listened to me, curled the ends of his mustache, rocked back and forth in his chair a few times and said, "We have never had a government contract before. I don't what rules there ought to be. For now, I think the best thing will be that we do not write down anything. We will simply set up an account in the bursar's office. It will be called Anonymous

Research Under Leo Beranek. We will accept and pay your vouchers for salaries and equipment and so on. Each month you bill the government and when the money comes in you deposit it in the anonymous account. We'll keep no books in the central administration. You keep simple books yourself."

I established a simple ledger and put it in charge of the senior secretary. On one side she wrote down the expenditures (vouchers submitted to the bursar's office) and on the other side incoming money from the government. No detailed records were kept. The laboratory grew, and the informal bookkeeping went on for almost three years. Then one day the treasurer of Harvard, William Claflen, discovered this large growing account. He came over and said, "Where are your records?" I showed him our simple ledgers and told him he didn't have to worry, all the vouchers and invoices could be checked. There had been no danger of any theft. Well, he didn't ask to check the records but he said, "From now on we're going to set up proper accounting over in the bursar's office and we're going to deal with this like a business." That changed the way we kept books and handled money and other things. The research laboratory was big enough now to be a business.

I hired my initial staff mostly from young men already at Harvard working for their advanced degrees. As the project grew, we hired young people from a number of universities, and a few older more experiences persons, including a good illustrations department. The space in the research laboratory necessary to accommodate the Electro-Acoustic Laboratory was available because many of the professors at Harvard went off to work first for MIT in the Radiation Laboratory and later on many of them went to Los Alamos. I was able to expand our efforts by taking over their laboratory space, and I didn't have to build any new buildings or find new buildings as the project grew. Harvard was willing to let me operate that way.

Now, what were the activities of the project? First we were to look at quieting airplanes, and this meant first off we had to make measurements of the noise in airplanes. Second, we had to examine the most common bombers and see whether we could quiet them, that is to say, whether acoustical materials would do the quieting. Except for traveling from Iowa to Harvard in 1936, the first big trip in my life was to visit the airplane companies: Douglas Aircraft in Los Angeles, Convair corporation in San Diego, Lockheed corporation in Burbank and Boeing company in Seattle. My visits to them were designed to learn how the airplanes were configured, where the personal were located, that is to say, the pilots, side gunners, tail gunners, etc. Both the Army Air Corps and the navy had their own planes, designed for different purposes. The Army Air Corps was buying B-17's and lending them to England. It didn’t take long to learn that quieting aircraft was not going to be very successful. There was no place to put sound absorbing acoustical material. Quiet for the pilots was the big goal, but they had windows all around. The sides of the cockpit were covered with radio and navigation equipment and there were no areas exposed for acoustical treatment. We stated, “We will go forward with our studies of light-weight acoustical materials and tell you how to build quieter new airplanes, but it's not going to be possible to do much with the older ones.” What really came out of the project was advanced information on how to build commercial passenger airplanes after the war.

Our primary finding--and what made the laboratory grow--was that voice communication was impossible when the aircraft reached altitudes of 30,000 feet or more. Remember that airplanes prior to the end of WWII were not pressurized. At altitudes greater than 12,000 ft, personnel in aircraft had to wear oxygen masks because of the reduced air pressure.

The key person behind our expansion into improving voice communication at higher altitudes in unpressurized aircraft was Lt. Commander Hanson in the navy department of aeronautics. He addressed urgent letters to the NDRC [3] asking that we be given the task of solving this problem. The problem came to our group because we were the only air-borne sound (as contrasted to underwater sound) laboratory in the USA. The Bell Telephone Laboratories converted their wartime efforts to underwater sound and radar.

Again, like the quieting problem, we had to learn all about the properties of current military voice communication equipment for use in aircraft. This required close cooperation with the war-time Psycho-Acoustic Laboratory under the direction of S. Smith Stevens, or Smitty as we called him. The basic test for a voice communication system that they perfected was called the "speech articulation test." First, at sea level in the laboratory, a person, having donned an oxygen mask fitted with the available mask-microphone combination would read words from a standardized list at a constant voice level. The signal from the microphone would be sent through an airplane audio amplifier, and a group of listeners wearing aviation helmets with available earphones, would hear the words and write down what they heard. This test was run in noise fields produced by loudspeakers that produced around the talker and listeners the same noise levels as we had measured earlier in the military aircraft. The psycho-acousticians had to design the tests, prepare balanced word lists, learn how to monitor the intensity of the talker's voices, learn how many tests would have to be made to obtain reliable results, determine when subjects became fatigued, and how to keep us their interest and morale

Abbate: So that's what psycho-acoustics is?

Beranek: That's what the Psycho-Acoustic Lab did, in part, during World War II.

Abbate: I knew Lick (J.C.R.) Licklider was involved, which is maybe getting ahead of this story.

Beranek: Well, he was in the Psycho-Acoustic Laboratory as a senior research man. The PAL was financed in parallel to me so I always said we were like a dumbbell, EAL and PAL laboratories at the two ends bound together by a common contract with the OSRD. We worked together very well. It is one of the well documented instances in the history of research where a hard-science laboratory and a soft science laboratory worked extremely closely and effectively. Everything we developed or tested, physically, at the Electro-Acoustic Laboratory was sent to the Psycho-Acoustic Laboratory for articulation testing using the best techniques known for psychological testing using human subjects.

I formed a very close personal relationship with Smitty Stevens. Our laboratories were only a block apart so it was easy to go back and forth. For the talking and listening tests he needed young men (like those in the military) who would remain at PAL over an extended period of time. The solution by Stevens was to obtain permission to use conscientious objectors as the test subjects. This gave them a way to contribute to the war effort under the excuse that the work would lead to post-war improvements in communication. Many people in the Selective Service system were concerned as to what to do with conscientious objectors, and this was one project that they were glad to endorse. Of course, there were many more c.o.'s in New England than Stevens needed.

We had to come up with a solution to the high altitude communication problem quickly. We needed to learn how present-day equipment worked in a reduced air pressure, like that up to 50,000 ft altitudes. For this purpose, we developed under great time pressure, closed chambers, 6 x 3 x 3 ft in size, which could be evacuated to reduce the air pressure. The existing microphones and earphones were then tested both at sea level and at reduced pressure corresponding to high altitudes. When we were sure how the existing equipment treated the different frequencies of speech, we would send them over to PAL for articulation testing at sea level in the high noise levels of military aircraft that was produced by loud speakers.

But the problem was not as simple as it sounds. How do you measure the frequency response of a carbon microphone, which was what the military was using? How do you measure earphones to determine how much sound they will produce at the eardrums of listeners? The Bell Telephone Laboratories had developed some equipment for testing earphones and microphones at sea level, but they had never studied their performance at altitudes. In addition, they had never tested telephone equipment in high noise levels like those in acoustically untreated military aircraft.

We at the Electro-Acoustic Laboratory realized very soon that nobody knew how a person's hearing or voice changed as the air pressure is reduced to that of high altitudes. The overall question was, how much of the deterioration of speech intelligibility at reduced pressure and high noise levels was due to changes in the microphones, the earphones, the voice, the hearing system? To test the changes in hearing and voice levels, we rapidly developed a special one-third-octave electronic filter setup to measure the frequency response of microphones and earphones. While that was going on we arranged with the Harvard Public School of Health, then headed by Dr. Drinker, to use their low pressure chamber, which had been developed to study bends that personnel encountered as they were exposed to reduced air pressures. This chamber was about 20 ft long and about 8 feet in diameter. It was fitted with benches along the sides where people could sit. Again, the conscientious objectors were the subjects. They wore oxygen masks when in the chamber. To test their voices at several high altitudes we would have each, in succession, stand in front of a test microphone (on a floor stand) and he would take off his mask for 15 seconds and speak a test sentence. Each talker would repeat this action about a dozen times, using different test sentences. On successive days, the air pressure would be reduced to lower values, corresponding to higher altitudes. The output of the test microphones were recorded. A dozen c.o.'s would go in the chamber at one time and each would go through the same procedure. Of course, we had competent medical people stand by in case any subject became ill or suffered any adverse effect, like severe bends.

We learned that the voice changed a lot at higher altitudes. It became much weaker, because the vocal cords were not pushing as much air back and forth in its thinned state as would be the case with denser air at sea level. We repeated these tests for hearing. We learned that for the same sound level at the eardrum, the ear does not change in sensitivity at high altitudes, provided the Eustachian tube remains open. So a person's hearing was not a problem.

Moreover, PAL's tests showed that the high noise levels like those in military aircraft were a serious part of the deficient voice communication problem. Principally, the noise entered the communication system at the ears of a listener. The aircraft personnel wore helmets and the earphones were mounted in pockets in the sides of those helmets. The sound got to the ears in the air spaces between the earphones and the ears. Stevens set up a development project in PAL to produce a "doughnut" cushion sewed to the inside of the helmet that would fit over the ear and produce a barrier to the entry of noise. The earphone, of course, remained mounted in the sides of the helmet and the doughnut was centered on the earphone. This proved to produce a tremendous improvement. But it was far from enough. There was, of course, nothing we could do about the weakening of the voice at high altitudes. But we found from experiments in the 6 x 3 x 3 foot test chamber that the performance of both the microphones and the earphones deteriorated with altitude. New carbon microphones and earphones had either to be found somewhere in the U.S.A. or developed from scratch. Finally, we developed a pressure regulated volume adjuster for the amplifiers, so that they would amplify the voice signal more at high altitudes to compensate for the reduction in voice level.

Usually, it takes a long time (often years) to get anything developed, approved, manufactured and into military service. Even if a satisfactory product has been developed, usually, the military sends out the specifications of that product for bids, then time is required for selection of the lucky bidder, and finally the manufacturer has to take time to tool up and to produce the product. Sending a newly purchased product to the distribution warehouses of the military and having them issue it to the personnel for whom it is intended also usually takes a lot of time.

Smitty Stevens and I were aware of this row of bottlenecks toward getting a vitally needed set of improvements into military use. We asked everyone we could how things could be moved along rapidly. We decided that we had to make a special pilgrimage to Washington and determine if there was any way to circumvent this long-delaying chain of events. We went to see Commander Hanson. Surprisingly, he had an immediate path to explore. He said, "You know, this is an interesting time to show up. We have just formed what's called the Joint army, navy, British communications board dubbed the ANB. One of the areas of its responsibility is to improve radio communications, which of course involves the voice, hearing, microphones, earphones and noise levels. This Board will have its first meeting tomorrow and I will take you over to the meeting and ask them to schedule you the first thing to explain how serious the voice communication problem is in high-flying aircraft and what it will take to change things for the better."

That afternoon Smitty and I crammed like mad to prepare for the presentation that we would have to make the next morning. When we arrived at their meeting place in Washington, we learned that indeed they had never met before. They didn't even know each other. Their meeting had been planned as a "get-acquainted" occasion and they had no agenda. We suddenly became the "agenda." They focused their full attention on us.

We explained that somewhere in the country we had to find a good microphone and earphone for operation at high altitudes. We also needed to find a fast manufacturer for producing earphone "doughnuts" and having them sewed into air personnel's helmets. We explained that there was no time to develop new equipment from scratch--the equipment would have to be available or developed to the point where minor modifications would make it suitable. We proposed that the ANB finance a trip that would include one of us and such military technical and or procurement people as they thought necessary to scour the country looking for potentially satisfactory equipment, which we would then bring back to the EAL for testing in the 6 x 3 x 3 foot altitude chamber. There were about six companies known to be manufacturers of this kind of equipment. Obviously such a trip would have to be completed in a hurry. The Board's response was enthusiastic, "We'll set up the highest priorities. You can use the airplanes with the highest priority usually only given to generals [4]. You give us the list of companies and we will call them and say you are coming and that they must give their full attention to you--and in turn, you must move fast." Well, we moved.

A man named Tennenbaum--I think he was with the Air Corps--and I visited these different companies and we found that there were only two with potentially good earphones and only one with a potentially good microphone. The earphone manufacturers were Permaflux and Western Electric and the microphone manufacturer was Western Electric. The Western Electric Company assured us that the Bell Telephone laboratories would make the adjustments that our tests showed were necessary. Permaflux agreed that changes to their earphones would not be difficult.

The military instructed Western Electric to get samples of the microphones and earphones to us as soon as possible. "Let's not negotiate anything. We need this equipment now." Our measuring equipment in Cambridge was ready to go. The Bell Labs made some modifications in practically a week's time. The microphones were carried by a Bell Labs delivery man to the overnight train in New York and one of us met the train in the morning at South Station.

We had to standardize on one shape of earphone for all three of the services. We had to choose one design of oxygen mask and request the military to have it modified to take the W.E. microphone. I remember a Sunday in this rush period when we sketched out the shape of the earphone on my desk in the physics department with the army, navy and British staff watching and agreeing. One hour's work had decided the earphone shape for the remainder of the war and well afterwards. Both W.E. and Permaflux made their earphones conform to the new shape. The oxygen mask company set about making a new mold and sent the new mask to us for test with the new W.E. microphone. Very soon, our tests were complete, the companies made some further small alterations and production lines were set up at both companies. Production samples were sent to us for final test to make sure nothing had gone wrong. Procurement followed immediately, and in a month or so time they were being sent overseas to the British to go on the B-17 bombers. All three services had them very soon, and they were in use by 1943 around the world.

We found that the old methods of calibrating microphones that the Bell Labs were using were in need of change and new methods were developed. We were running the only real scientific airborne laboratory, in the world, I'd say it turned out. The rest of the world had shut these things down, were all doing underwater sound. That included Japan and Germany as well as England and France—France was probably taken over by the Germans by then.

This then led to another point. Remember, we were dealing now with things that really belong in the radio engineer's field. We were dealing with microphones and earphones and communication. A major paper came out of this work and was published in the *Proceedings of the IRE* in the September 1947 issue.

Abbate: Was that the paper titled, "The Design of Speech Communication Systems?"

Beranek: Yes. It could have been published a year earlier but the Bell Labs objected, because my paper included some basic material that they had done first. It was right that they should get their paper out a little before mine. They published in the *Journal of the Acoustical Society of America*. But my paper became the basis for American national standards that were written later on. My other major publication as a result of this war work [5] was my book *Acoustic Measurements* [6]. My other big contribution was a book titled *Principles of Sound Control in Airplanes* (Harvard University Press, 1944) which was written with several co-authors from EAL. The laboratory had grown by then to be around 100 people. It appeared for a short time that our research was coming to completion and that EAL would have to be closed down. Then came the Kamikaze airplanes!

Abbate: This seems like a major change.

Beranek: The Japanese were losing the war in the Pacific by the summer of 1943. I read that one of their officers thought up the Kamikaze concept which he speculated might turn the war around in favor of Japan. As we shall mention later, they almost did. A Kamikaze was a relatively small bomber carrying a large bomb, piloted by one aviator from the Japanese corps of volunteers who was willing for his country and its emperor to fly on a suicide mission. The pilot was instructed to fly low over the ocean water and crash into the superstructure of any American ship that they found in their flight path.

The Kamikazes created havoc with our destroyers which formed the protective rings around our aircraft carriers. The difficulty was that our radars did not detect aircraft that were flying low until they loomed over the horizon. At that instant they were only a relatively short distance away. The radar screens were located in a Combat Information Center located below decks, usually in the center of the ship. The CIC's procedure was to take the radar data appearing on their display tubes, translate the position of an enemy aircraft into longitude and latitude and convey this information to the gun turrets by telephone. The guns had to be swiveled to the correct direction and open fire on the incoming aircraft. But the time delay involved was too great. The Kamikaze aircraft would strike the ship before the guns could start firing. The common expression was that the best defense against a Kamikaze airplane was a Marine on deck with a rifle.

I was approached by the Bureau of Ships, that part of the U.S. Navy that designs, buys and maintains all the ships and the equipment on them. They asked if my laboratory would be willing to take on the problem of speeding up the radar detection and gunnery system so that the ship could shoot down Kamikazes before they hit. I talked this over with Smitty Stevens, and we decided to set up a third entity which we called the Systems Research Laboratory under my direction and to staff it with personnel from EAL, PAL, the Radiation Laboratory at MIT, from the Bell Telephone Laboratories and with time and motion and other experts from wherever we could find them, including pertinent officers from the navy.

The Kamikaze problem was growing more serious weekly. The Kamikaze pilots aimed at ships' superstructures, usually those of destroyers, which contained the radar and the command posts. Thus, instead of sinking ships they rendered them useless.

Our plan to help the navy soon evolved. The first step was to build a "ship" on land. This was a one-story building that would have a "superstructure" that would hold the radar antennas normally carried on a destroyer, a Combat Information Center inside the building where information from the radars was received, assembled and interpreted, and a gun turret which would receive the firing information from the CIC and swing into position. We needed a location for the "ship" that would have a clear sweep toward the horizon, that is to say, no buildings in the direction that the radars would point. Fortunately, the navy controlled the southern tip of an island in the Narragansett Bay called Conanicut Island. Its southern tip was called Beavertail Point. The advantage of this location was that there was a clear view of the sea within an angle of about 125 degrees. This was where we designed and built the "ship on land." The Turner Construction company was hired by Harvard to do the construction. The floor in the building on which we were to mount the CIC equipment (and stand on) were made of half-inch-squares of plastic covered wood (like kitchen counter-tops), each square being 2 feet by 2 feet. These squares were mounted on metal supports about a foot above the structural floor. These removable floor sections enabled us to set up equipment with the cables running underneath the floor. Then, equipment could be moved from one part of the CIC to another without rewiring because the cables underneath were free to move to new locations.

A problem early on was recognized by the navy. There was a strict directive that no late-design equipment could be shipped to any land-based facility. It had to be sent to the fleet where it was desperately needed. The navy solved this by commissioning (informally) our lab the "U.S.S. Beavertail". A Naval Lieutenant was put in "command", although I was the actual director of the effort. The vital radar equipment needed for our investigation was shipped to the USS Beavertail via the Quonset Naval Air Base in nearby Newport, Rhode Island.

While the building was under construction, the navy prepared official naval orders for four of our staff to go to sea on two ships, the USS Canberra, then a heavy cruiser (10-inch guns), and the USS Lexington, an aircraft carrier. A member of our staff (transferred to us from the Bell Telephone Laboratories) and I went out on the heavy cruiser and one of the EAL men and a staff member from MIT's Radiation Laboratory went out on the aircraft carrier. Remember, we were at war and these were active ships. We installed a battery of Dictaphones on each of the ships, and "taped" the twelve most important communication wires. The object was to trace the flow of information through the ship and to determine where any bottlenecks in communication might be located. This enabled us to understand the ships' operation and the way commands were issued, acknowledged and acted on. Each of us was at sea over a month. I remember that the USS Canberra put in at the "Port of Spain" harbor on Trinidad Island on Christmas Eve. We were given twelve hours leave from noon to midnight and we all headed for the Queens Park Hotel, where we had drinks, and where several of the crew demonstrated their ability to play the piano. We all sang Christmas carols. A nostalgic evening.

We came back with all of the recorded information and a good knowledge of life and activity on a ship, including the handling of the guns, the operations of the CIC, and the interactions with the Captain's deck in the superstructure. The recordings were immediately transcribed at the EAL. A 20 foot long chart was made of the conversations on the twelve communication lines, with time in hours and minutes running across the top and the activity on the lines in twelve rows beneath. We learned a lot about the problems of shipboard communications, about the difficulty of hearing commands while the aircraft batteries were in action, and about troubles with the radar equipment. We also observed the behavior of the crew assigned to the CIC and easily detected some of the reasons for time delays in receiving, interpreting and passing on radar information.

When we arrived back at Beavertail, the laboratory was under construction, and the radar equipment had been ordered by Washington. Preparations were being made to install it when it arrived. In due course, we arranged, through the commander-in-chief's office in Washington, to have a squadron of airplanes from the Quonset Air Force Base raid the USS Beavertail, flying in low within the 125 degree clear radar detection angle. The times it took to send information through the system was recorded and the time-and-motion engineers recorded the activities of the "crew" members. Thus they were able to determine how to position the equipment in the CIC so as to minimize time delays. Also deficiencies in the operation of the equipment and the communication system were isolated and understood.

In the meantime, the navy was having deep troubles. The Kamikazes were very effective in putting dozens of destroyers out of commission. The repair facilities at all the navy ports on both coasts and Hawaii were full of ships undergoing reconstruction. It was said informally in the commander-in-chief's office that the November 1945 planned invasion of Japan might not be possible because of the shortage of Naval vessels. Our project now assumed the highest priority. Unfortunately, it had taken time to get the equipment to us, time to install it, time to get the "crew" trained to use the equipment, time to arrange for the air raids, time to conduct the air raids, time to analyze the data, time to prepare the initial reports and deliver them to the navy. The process up to this point took over a year. Our initial reports were printed and delivered to the navy during 1945. A big meeting of top officers from the navy was planned for mid-August 1945, where the results of our experiments and the manner in which they were conducted would be studied by the command officers. Orders were intended to follow for modifications of all ships that would be in port or could get back to port.

But a few days before the meeting date, President Truman decided to approve dropping the atomic bomb. The meeting in our laboratory never took place and a few days later the war was over. I remember the difficulties we had on surrender day getting something to eat, because we had no restaurant of our own and all the restaurants on the Island closed down to celebrate. We all took the ferry from Jamestown over to Newport, and we found restaurants in several of the hotels still in operation. Everywhere, there was shouting, fire engines were running around town with their sirens sounding. There were some fireworks. There was dancing in the streets. The war was over.

I had been working closely with the navy commander-in-chief's office and had access to all secret information about the fleet. I knew where every ship was in 1945. It was my opinion that one of the reasons Truman approved the dropping of the bomb was that he knew that very many ships were damaged and out of action. It seemed to several naval personnel I talked to that the November invasion could not take place. Truman may have figured that if the invasion were delayed, the loss of lives over the next few years would be enormous. My surmise was backed up by one article I read after the war written by one of the admirals in the Pacific (maybe Admiral Halsey), who mentioned the lack of ships needed for a successful invasion of Japan. The laboratory continued doing systems research for the navy for another two years. Then the project was moved to Johns Hopkins University. The navy disposed of their holdings on Beavertail Point, and nothing of those days remains.

Abbate: Why don't we talk about the formation of the IRE Professional Group on Audio, because that would have occurred very soon afterwards.

Beranek: That would have been in --

Abbate: '47 or '48

Beranek: You must have a copy of the report that I prepared for the IEEE dated September 1 of 1947, titled "History of the Beginnings of the Professional Group on Audio." The Profession Group on Audio was officially formed in 1948.

Abbate: Right. I'm interested in what kind of concerns you had. The point at the IRE was to allow the sub-specialties to concentrate their efforts and learn about each other, because radio was becoming such a broad field. Which kind of things did the audio group want to focus on?

Beranek: The audio group felt that loudspeakers and microphones and recording were related to radio. At least it was a branch of the radio work. They certainly had to have, in all radio broadcasting, loudspeakers and microphones. Recording was maybe more related to the movies, but it was moving into also being important in the broadcasting studios. So broadcasting belonged to radio. It didn't belong to electrical engineering. At that time, it was radio engineering.

Benjamin Olney was with the Stromberg-Carlson company. Benjamin was very interested in the audio problem. I went to MIT in February of 1947 after the war and became a tenured professor there. I stayed at Harvard until 1947 in February on their faculty. And we were in communication with Angavine. We had our own section we'd set up. We called it the Audio Section. It was a section of the Institute of Radio Engineers, and its main interest was audio. Benjamin was interested in this out at Stromberg-Carlson. We communicated with each other, and we decided to communicate with the staff. We learned that there was talk going on already about the groups. They had appointed, on October 7th, 1947, R. A. Heising as chairman of a planning committee, and he said that there were already six specialized meetings of radio engineers going on, mainly in broadcast engineering. The possible groups envisaged at that moment were tube electronics, television, broadcasting, wave propagation, transmitters, receivers, microwaves and audio. This committee was asked to prepare bylaws and develop plans for inauguration of the group system.

On March 22, 1948, the committee had progressed far enough that a news article was prepared and proposed for publication in the *Proceedings* on the concept of professional groups. It mentions audio engineering, broadcasting and wave propagation as possible early groups.

Benjamin said he'd take care of putting the paperwork through. And so he learned, in talking with headquarters, that they would consider setting up professional groups, and the provision was that the groups had to first have a petition that was signed by the required number of IRE members, and secondly that we'd adopt some universal bylaws and code of operations. Benjamin submitted this petition sometime about April 1, 1948, and on June 2, 1948 the IRE formally established the audio group.

In answer to your question, our feeling was, in Cambridge at least, that audio was important enough in broadcasting that there at least ought to be activity in the field so that broadcast engineers could keep up to date. We didn't feel that IRE ought to drop that field and let it all go to the acoustical Society, which was the only other one—there was no Audio Engineering Society then. We felt that this was an important thing to do, to keep our own members informed and to keep them active in the audio field. Well, at almost the same time the Audio Engineering Society was formed. The Audio Engineering Society decided to take over recording as their big thing, and in the process they took over broadcasting, really the audio part of broadcasting—the studios and the microphones and whatever they needed audio-wise, such as control panels and so on. So it turned out then that audio had a short life, really just until it became obvious that what was important was signal processing, and then that gradually took over.

Abbate: I'm curious about that transition. We've talked to other people and we've got the impression that in the early to mid-'60s there was a real move away from the traditional audio concerns and toward new FFT techniques and digital techniques.

Partly what I'm wondering is, was this a matter of applying new techniques to old concerns, or had the actual applications changed? Was it still to do with recording and telephony and broadcasting or did it really move away from those traditional audio concerns?

Beranek: Well because this was a gradual shift both occurred. The first uses of signal processing that I encountered were in the analysis of audio signals, whether the signal was noise or speech or music signals. The early signal analysis equipment was a series of parallel filter bands, such as I described for the high altitude experiments we made during the war. These equipments could, for a steady signal only, determine the power (or intensity) in a signal as a function of frequency. Each of the parallel filter bands was generally 1/3 or 1 octave wide and could cover the range from about 20 hz to 20,000 hz. The difficulty with this was that it could not deal with transient sounds, nor with sounds that varied from one part of a minute to the next. The application was in the continuous analysis of the noise signal as a function of time.

In this period, from 1947 to 1963, the IRE publication was a Newsletter, run off on a mimeograph or a copying machine, to the best of my knowledge. The first IEEE publication, named the *IEEE Transactions on Audio* was started in 1963 and continued for two years. It was published twice a year.

About 1965, the use of fast Fourier transform analyzers was beginning to come into common use. With the FFT, measurements could be made of fluctuating and transient signals, and continuous analysis was almost possible. In this period the name of the IEEE publication was changed to *IEEE Transactions on Audio and Electroacoustics*. I have no record as to why electroacoustics was considered different from audio, but the result was a wider readership and more attention to signal processing.

By 1970, the words "spectral analysis" were common. At this time reliable analog-to-digital converters were available. The FFT was thought of as a method for digitally simulating a bank of filters. Swept filter analyses were more common, and their uncertainities were better known. Speech recognition was raised to a new level in this period. The concept of Cepstrum, a method of analyzing speech signals was devised. Whereas, earlier researchers were trying to determine the properties of the human speech production apparatus (vocal cords, resonant passages, tongue, teeth, lips) and to translate there variations into control signals for producing artificial speech, statistical methods began to be used for developing models of speech. Speech recognition became more interesting than speech synthesis. The result was another change in the name of the IEEE publication, *IEEE Transactions on Acoustics, Speech and Signal Processing.* This publication was issued six times a year and continued for sixteen years. But in this expanse of time, *Acoustics* articles appeared less often in the publication, and speech recognition and signal processing, with all the associated mathematical methods, took over. Words like "Hidden Markov Models" and "CAT" were common. Speech was considered as a sequence of "states", described statistically. The computing machine would analyze many samples of a speech sequence and a good model of speech evolved. That is to say, audio speech got lost in a sequence of "states." Large data bases were assembled and these bases were massaged to that speech could be recognized from its "states."

In 1991, the IEEE publication took on another title, which has existed to this time, *IEEE Transactions on Signal Processing*. It is issued monthly and has a large readership. Because my interest is in acoustics and audio, I don't even receive this publication. The publications that interest me are the *Journal of the Acoustical Society of America* and the *Journal of the Audio Engineering Society*. Parenthetically, speech synthesis is the property of the Acoustical Society of America, not the IEEE.

Abbate: How long were you at MIT before you started BBN?

Beranek: A year and a half. I came to MIT in February of '47 and we started what became known at BBN in November of 1948. Actually, for one year only, its name was Bolt and Beranek.

Abbate: And how did that develop?

Beranek: There were two factors that caused Bolt and me to start a consulting business. One of them was the immediate factor and the other was a continuing factor. The immediate factor was that a permanent headquarters for the United Nations was being planned in New York, and Harrison and Abramovitz were selected as the lead architects. The project was too large for any one consultant to handle. The other factor was that both of us were individually doing acoustical consulting work, and our practices were becoming too large to be carried in spare time by two professors.[7]

For many years, starting right after the war, MIT had a large acoustics research facility, called the Acoustics Laboratory. It was adjacent to the research laboratory for electronics. The Acoustics Laboratory was one of the leading research and teaching laboratories in acoustics in the nation. Other laboratories were at Brown University and UCLA. There was a small acoustics research activity at Harvard after I left for MIT. Apparently, many architects thought of MIT as a place to go to ask for acoustical consultation. The United Nations job came to Richard H. (Dick)

Bolt through President Killian's office. Harrison and Abramovitz also asked Professor V. O. Knudsen at UCLA to bid on the job. Bolt bid lower than Knudsen because there would be no days lost in travel. Also, being younger, his rates per hour might have been lower. In any event, distance is a barrier to the close collaboration that is necessary between an acoustical consultant and an architect. Bolt got the contract. When the preliminary drawings came in, he saw immediately that the job couldn't be handled by one person, so he asked me to join with him. We so notified President Killian. Shortly afterwards Killian came to see us and suggested that we set up a consulting firm formally, because he foresaw the upcoming building boom and he did not want MIT in the consulting business. He even offered to rent us two rooms at MIT in which we could house additional personal. He stated, however, that if our needs grew beyond this, we would have to seek space outside the Institute. We first asked Robert B. (Bob) Newman, then a graduate architectural student with a masters degree in physics, to join us part time. As we became busier we employed two other graduate students part time, Samuel Labate and Jordan J. Baruch. Within a year, we realized that we must hire more staff, so we located in offices in Harvard Square. After a year, Newman received his architectural degree and we hired him and the firm became Bolt Beranek and Newman, a partnership.

Bolt was from the physics department, I was from the EE department and Newman had a part time appointment as a lecturer in the architectural department. These disciplines gave us a wide range of skills, and our business prospered. We received commissions for music schools, municipal auditoriums, conventions centers, office buildings, etc. from architects. We also received requests to assist in noise quieting of machines and factories.

BBN was incorporated in 1953 and I became its first president. We moved to larger quarters in Harvard Square, and then several years later moved to a still larger building at 50 Moulton Street in Cambridge. We opened branch offices in Los Angeles, New York, Chicago and Washington, D.C. By 1970 we had built two additional buildings and a few years later leased two more very large buildings all in the Fresh Pond area. When we took over the larger building in Cambridge, I dropped to half-time at MIT, I resigned completely, except for teaching summer courses, in 1958.

Our first businesses, as I said, was consulting on building acoustics with architects. The advent of the jet aircraft age got us involved in a big way in noise control of intense noise sources. Our first jobs were design of large test cells in which jet engines could be tested in production. Then, a big opportunity came, when NASA had us design the largest muffler in the world to quiet a supersonic jet engine's noise development at their laboratories in Cleveland. Our next big job was with the New York and New Jersey Port Authority. We helped the Port Authority develop a standard of permissible aircraft noise reaching neighborhoods surrounding the airport during landing and takeoff of jet aircraft. That was a political battle as well as a technical one, but it worked out well. The help we gave the port I have described as "shoe-horning in the jet age."

Abbate: That's an overlooked issue, I think.

Beranek: Setting external noise standards for jet aircraft was one of my most important life activities. Then came the predecessor to Internet.

Abbate: Yes, I want to get to that. So you started out with building acoustics, then came jet noise control and standards for neighborhood noise levels from aircraft?

Beranek: My feeling as president of BBN was that acoustics would turn out to be a limited field, especially since the building boom would someday come to an end. I felt that Bolt Beranek & Newman ought to continue to grow. So I thought, "What's the next place we ought to be? Perhaps we should be in human factors. Machines are going to become more complicated with time, and they ought to be maximally usable by humans. That means connections between the five senses and machines. I think we should have human factors man in the firm."

I had worked with J. C. R. (Lick) Licklider during WWII at Harvard. He was primarily in the Psycho-Acoustic Laboratory, and he was interested in speech articulation in high noise levels and at high altitudes. After I got to MIT, in 1950, I got MIT to hire him as a professor in the EE department with his office in the acoustical laboratory. By about 1956, he was being courted by Hughes Aircraft in California. I took this opportunity to talk him and his wife Louise into staying in Cambridge and working for BBN. I actually spent time trying to convince his wife Louise into staying in Cambridge and I think she was of great help in my getting him to join BBN. He arrived in 1957.

Abbate: I forgot the exact dates. This was before he went to ARPA the first time?

Beranek: Oh yes, he worked at BBN until he went to ARPA in 1962.

Abbate: I know BBN got some ARPA contracts. You were one of the first with time sharing?

Beranek: We were the first site to demonstrate time sharing. We bought Digital corporation's first PDP-1 in the fall of 1958 or '59 and promptly put it to use on problems relating to research projects and hospital patient care. One day I ran on to two strange guys working at a long table in one of the large rooms there. I walked up to the first fellow and said, "Who are you?" and he said, "Who are you?" The two men were Marvin Minsky and John McCarthy, two of the most prominent figures in the emerging field of artificial intelligence. With their help, BBN built the first time sharing system, dividing the screen into four simultaneous users.

Abbate: Did you hire Minsky and McCarthy, and if so what were they asked to do?

Beranek: I don't know. Licklider hired them for a short period as consultants, according to my memory. My feeling was that their coming that summer led to the time sharing demonstration in the fall. BBN even started a time sharing business, purchasing Digital computers to do so, and the number of users was expanding fast in the greater Boston area. Soon, however, GE entered the field, adapted some fully depreciated computers to the service, undersold BBN and quickly stole the bulk of our time-sharing business.

Abbate: I would like to know more about how time sharing related to what BBN was doing.

Beranek: The story is written up in a book, *Where Wizards Stay Up Late: The Origins of the Internet*.[8] They interviewed me and many at BBN, so I don't need to look up my records. They write that Licklider thought the future of scientific research was going to be linked to high-speed computers and that this was a good field for BBN. After only a few months at BBN, he asked me for an allocation of money to purchase a Royal-McBee, LGP-30 computer, which could be programmed and data entered by means of a punched paper tape. Although Licklider could not tell me what he was going to do with it, I agreed to spend the $25,000 required. The book quotes me, "I decided it was worth the risk to spend $25,000 on an unknown machine for an unknown purpose because I had confidence in Lick." Not long after, we helped make beta tests on DEC's prototype PDP-1 and subsequently bought the first one for a little less than $150,000.

Abbate: So from BBN's point of view the idea was, "We want to have a future, we want to enter into a new technology, so let's invest in sort of promising areas and see what happens."

Beranek: Possession of those computers led us to contracts with the NIH and with ARPA.

Abbate: So once you got this first computer and started getting productive work on it, was it clear that you were going to go more into computing continuously from there?

Beranek: It became clear because we were getting government contracts. After we bought the PDP-1, the NIH became interested in introducing computers into hospitals. We worked out a joint NIH contract with the Massachusetts General Hospital to develop computers for hospitals. Core memory was to limited in those days, so we bought a UNISYS high capacity rotating-drum memory from Sperry Gyroscope. It was the biggest memory available at that time, and we used it primarily on the hospital project.

Abbate: So the computer work pretty much split off from the acoustics work right from the beginning.

Beranek: Yes, there was no special relation between them, although our work with the U.S. Navy on submarine detection was heavily computer driven.

Abbate: Now, I know that you ended up in other areas, like underwater sound. That seems different.

Beranek: Well, that really came out of regular acoustics, and also we went into space acoustics. There we were principally interested in metal fatigue caused by excess vibrations. Also, the astronauts had to have quiet so they could sleep and talk easily with each other.

Abbate: And underwater acoustics for submarines?

Beranek: Inside submarines. Our research and development goal for the navy was to make submarines so quiet that nobody could hear them with hydrophones placed in the water. Interesting though, on the other side of the laboratory we had people working on developing better hydrophone systems so they could hear them.

Abbate: Was that Cold War funded technology?

Beranek: We figured the Russians were doing the same thing and that they were going to outdo us in submarines. We were taught by the navy that the world could be taken over with submarines. In fact, the Russians were doing very well in submarines, just as they were doing well in space.

Abbate: In summary, you had buildings, jets, submarines and space. Now let me ask you a little about the ARPANET. At this point my impression is that BBN had considerable expertise in the computing group by this time.

Beranek: Here I must draw some more from the Haffner and Lyon book, which discusses BBN's part in the ARPANET. That led directly into the INTERNET. In July 1968, ARPA sent out a proposal for bids which asked for the development of an Interface Message Processor (IMP). They had conceived the idea that this device could be placed at each node of a network where there was a computer, called a Host Computer, and the IMP's would make it possible for a variety of Host Computers to communicate with each other. Frank Heart at BBN was selected as the leader of the team that put together the IMP proposal. Others on the team included Dave Walden, Bernie Cosell, Severo Ornstein, Robert Kahn and Will Crowther. BBN submitted its proposal September 6. A dozen other bids were submitted, from such companies as IBM, Honeywell, Raytheon, Bunker-Ramo, etc. It surprised everyone when, just a few days before Christmas, ARPA announced that the contract to build the IMP was being awarded to BBN. BBN went on not only successfully to develop the IMP, but we built the entire ARPA network, and operated it for many years. The control center for the entire network was at BBN.

Abbate: What was your relation to Harvard and MIT?

Beranek: Students there thought BBN was a choice place to work, because our research was forefront, and they would be surrounded by brilliant people. I always said that each new addition to the company had to raise the average level of intelligence. In the Haffner and Lyon book, they have a chapter on BBN called "The Third University." We tried to do more than the ARPANET with IMP. Jerry Elkind and I went to the FAA and told them that we could improve upon their antique methods of communication from one tower to another around the country. We asked them to give us a contract to develop a high speed network system for air traffic control They didn't see the point at all. They said, "We've got much more serious problems. We need better radars, and this communication thing in not our most important problem." The whole computer story was one of the most interesting parts of my life.

Abbate: Very interesting. Were you at the 1972 demonstration at the International conference in Computer Communications in Washington?

Beranek: No, because at that time I was the president and CEO and one of the principal stockholders of Channel 5-TV, WCVB in Boston. I left BBN in 1971 to take this position.

Abbate: Now, how did that come about?

Beranek: Channel V was being operated by one of the leading newspapers of Boston, the *Boston Herald-Traveler.* It was discovered by the FCC in 1961 that there had been an improper contact, a luncheon, between the chairman of the Federal Communications Commission and the president of the Herald-Traveler when the

Herald's bid for a license to operate the station was undergoing competitive review. There was a general belief that there had been a fix, money exchanged, but, if so, it could not be traced. A money fix had actually been proven in the case of two Miami stations in which the same FCC chairman was involved. So the FCC decided not to give the Herald-Traveler a license longer than four months, and they opened it up for competitive competition again, with the Herald-Traveler given the opportunity to reapply.

The FCC process stipulated that any group could make an application to operate a TV station whenever a frequency became available. An examiner, a judge, is appointed to conduct hearings. He makes a decision on who he thinks should get the license and presents the trial evidence and his decision to the FCC. They may or may not go along with his decision.

There were four applications for Channel 5. Our group consisted of thirty persons representative of professions, gender, minorities, and business. The hearings started in 1963 and went on for one year in Washington. The examiner took another year to make a decision. He decided to recommend the Herald-Traveler for the license. The FCC, after a two year study gave the license to us. Their reason was that they thought we would do a better job of serving the community with the original programming that we promised. We appeared to be a group dedicated to good television. The judge examiner thought that we had made a lot of hot air promises. The Herald-Traveler took the case to the US Court of Appeals in Washington. We won by the usual three-judges decision. The H-T asked for a full bench, nine judge review. We won. They took the case to the Supreme Court. The court refused to take the case, i.e., denied "cert". Then, the Herald-Traveler found some irregularities in the law practice of one of our thirty stockholders. They took the case back to the Court of Appeals, and again to the Supreme Court. We won. Finally, as a last gasp they found another irregularity with a different stockholder and took it through the same court sequence. We won for the final time. We went on the air March 13 at 3 AM in the year 1972. I resigned from BBN one year in advance of going on air, in order to hire the staff, purchase the building and equipment and deal with the final legal actions.

Why was I chosen president and why did I accept the position? First, I had been president of BBN for sixteen years, and it was getting to be more of a computer company and I was basically an acoustician. Also, I was not particularly adapted to the pace and hard sell of a modern company listed on the NASDAQ stock market exchange. So, I felt a change was in order. When we obtained the TV license, the group decided I was the only one among the thirty who had experience running a company that primarily employed creative people.

Abbate: Again, what was the name?

Beranek: The corporation was called Boston Broadcasters Incorporated. The call letters of the station were WCVB-TV and we were on Channel 5 and were affiliated with the ABC network. ABC owned no part of BBN. They paid us a fee to carry their programs.

Abbate: How long were you president?

Beranek: I was president for eight years. We sold the station in 1982, and the final two years I was chairman of the board and stayed active at the station, so that my vice president and general manager could have a shot at management We finally sold, because the thirty stockholders, most of them up to ten years older then me, were aging and we did not want to face the problems of handling deceased members stock in a non-public company. Metromedia was the buyer.

Abbate: Then what?

Beranek: I became a member of the board of directors and a two-thirds time consultant to Wang Laboratories in Lowell, Massachusetts. I was brought in to help him plan how to use some of the engineers better. The chief engineer wasn't very good and they had no new product. Actually, I spent most of my time there helping them develop a world-wide network for the management of their business. When they missed the advent of the PC computer, Wang was finished. The company went completely broke, the banks took over, held all the stock and the remaining company now only develops software.

Abbate: Was that your last active venture?

Beranek: That was my last business venture. I was chairman of the Boston Symphony Orchestra from 1983-1987. Then I served as full-time volunteer president of the American Academy of Arts and Sciences from 1989-94. Just before that, I was a member of the Board of Overseers of Harvard University from 1984-90.

Abbate: But it appears that you were now out of engineering.

Beranek: Then I went back to architectural acoustics consulting. Now I work almost full time as a consultant.

Abbate: In what area?

Beranek: Architectural acoustics. I'm consulting in the area of concert and opera halls. You have just seen my newest book.

Abbate: The book, *Concert and Opera Halls* that we just looked at. It seems that a lot of your career has uniting engineering with either aesthetic concerns or psychological or humanities disciplines. Do you think that's unusual for someone with a background in EE?

Beranek: If you had asked me the question of what do you think your principal strength is, I would have answered that my principal strength is management of creative people. I managed a war laboratory. I had many thesis students at MIT and I was technical director of MIT's acoustics laboratory. I was co-founder of BBN and I helped it grow into a successful organization managing creative people and making important contribution to the future of the information world. Then I ran a television station, which, with lots of local programming and a first rate news department, is management of creative people. Incidentally, we even made a one-hour show with Henry Fonda and Myrna Loy as the stars. Finally, I have worked with architects to this day, helping them in their creative efforts.

Abbate: Well, I think about considering the location of your activities as an intersection of scientists and artists. Boston is pretty hard to beat as a place to find a pool of people to recruit.

Beranek: Oh that's true. I hired from around the country. I hired the best general manager for the television station in the nation by almost a year of hard work and maybe a little good luck. He turned out to be superb, and without him we never could have achieved the success we did. He and I complemented each other and we built a good, strong station. And I knew how to manage that group so they were always happy. When I go back to the TV station now for a visit, personnel there talk about the former time, "Those were the happy days."

Abbate: It seems that when you were running BBN, it had a genuine reputation for fostering basic research even though it was a commercial entity. Was that a deliberate philosophy or strategy on your part?

Beranek: It was a deliberate policy, but it had to be based upon financial success. We had to have income. On the architectural acoustics side and the submarine quieting, we turned out to be the best firm in the country, so we had success. We were able to attract plenty of clients. The same thing was true of our obtaining ARPA contracts. We also won NIH contracts for the computer people because our staff had original ideas and a reputation for producing on our ideas. Today, BBN has turned into trying to make money off the Internet side of BBN's business. How that's going to play out in the long run, I don't know. The future is dependent a lot on making a correct guess today about whether the direction we are pursuing will lead to a financially viable business next year or five years from now.

Abbate: Especially in computing.

Beranek: Yes, everything associated with computers changes very fast. And if management makes a bad guess, the company can end up broke. A case in which a bad guess, or no guess, was made involved Dr. An Wang at the Wang Laboratories. When asked to look at a new computer concept called "Apple", the said, "Take it away, I don't want to look at a toy." He did not see it as a model for the future. He made the wrong guess.

Abbate: Did you approach your future by trying to get contracts that would incorporate research which would push you into the cutting edge in terms of markets?

Beranek: There was a mixture. Obviously we had good income from the acoustics side. It enabled me to say to Licklider, "You can buy a $25,000 computer." even though both he and I did not know what the hell to do with it. We only knew we were supporting research that might lead to something. Licklider said, "Computers are the thing of the future." I trusted him to make good on his observation. We did the same in several areas of acoustics. I would underwrite research from which I hoped we could develop proposals to the military or to other branches of the government or to private companies like Digital, with the hope that they would lead to large funded contracts. When one thing is successful you can build on it.

Abbate: Were there things that turned out to be dead ends?

Beranek: Oh yes. We had a lot of dead ends. We tried to merchandise a mechanical teaching machine. It bombed. It's the same story with the field of speech identification. We thought it was going to be a commercially viable area in 1965--and it is just beginning to pay off thirty-two years later. But, even today, (I was told last week by a friend at the Bell Telephone Laboratories) BBN is considered as a leader in the field. We thought that in another five years after 1965 fuel cells would be in use in practically every home. We, unfortunately, were not able to develop a marketable product that fast, or even within ten years. The cost of a fuel cell per kilowatt using our design was too high because we were using palladium as the critical element in our designs. We never got a better idea. And palladium was just too costly.

Abbate: How did you ever get into fuel cells?

Beranek: I, and several others in management, thought there was a future in fuel cells and we knew a leading man in the field who had casually remarked that he might like to change companies.

Abbate: So it did not arise directly out of some other activity at BBN?

Beranek: No, not at all. It just seemed like an area that was ripe for the kind of thing we could do, so we hired Walter Juda, who was on of the acknowledged experts in the chemistry of fuel cells. He was considered by his colleagues at Harvard and MIT as one of the most creative chemists anywhere. It was the same, I have always tried to hire the best. He got a fuel cell to work superbly, but could never get the cost per watt down to a level that was competitive. There was no space age then that later demanded fuel cells. Kennedy had just announced it in 1965.

Abbate: Now, that's an example of where you got interested in an area and then hired someone to do the research and development. Did you also have people on the staff who were already saying, "I've got expertise in this area. I thing this will be promising. Why don't we try this," and you turned them down?

Beranek: I can only remember two cases, both minor and which did not work out later by anybody else. One was a hydraulic transmission for cars. Another was a particular kind of instrumentation for oceanography.

Abbate: And then, once those people got hired and that project was over, what did you do with them?

Beranek: Like what happened to the fuel cell project?

Abbate: For instance.

Beranek: First, the fuel cell project never involved many people. Those involved simply got jobs elsewhere. There were plenty of jobs fifteen years ago. No problem. But when a person was not fitting in well with the plans of the company, my preference was to call them into my office and say, "Things are not going too well, as I am sure you can sense. Why don't you look for a job elsewhere. And I'll help you get it."

Abbate: It sounds as though that gave you a lot of flexibility. You could assemble a team for a purpose and then they could go on to something else rather than being locked into a fixed staff where it would be more difficult to work efficiently.

Beranek: Yes, I suppose so. But most were productive all their lives. Yesterday, I was over at BBN to attend a retirement party. Three of my original people in acoustics had been there since 1953, to give you an idea of how we kept them happy, and most important, productive. No one hinted that even after the longevity of their employment, they had been ineffective in recent years. Of course we had a very good retirement plan. Combined with social security, they can make as much off the earnings of their retirement money as they were currently earning at BBN.

Abbate: But it sounds as though working for BBN was a place attractive for academic people to spend some time; that gave you a unique access to talent.

Beranek: And we were close to Harvard and MIT.

Abbate: Right. It's a nice synergy.

Beranek: That was (and is) important, because we hired so many people from those two institutions. Although some came from Brown and some came from other research universities.

Abbate: It sounds as though one place where you guessed right was your work on minicomputers, because a lot of companies were guessing wrong about where the future of computers lay. There were alternatives of time-sharing machines, batch machines, or whether mini-computers were the future. And I don't think it could have been clear in 1967, ten years after you hired Licklider. Did you have a sense that small computers were going to be the future, or could you just not afford to buy a big computer. How did you make you decision?

Beranek: In terms of mini-computers, we were doing paid research for DEC all the time after our purchase of the first PDP-1, and we could see where the market was headed. Of course, we thought we would be leaders in time sharing, which we were the first to. We even developed a programming language called TELCOMP that to this day, some say was better than the programming language that the industry adopted, namely BASIC. Our time sharing business had several early obstacles: the telephone lines were not reliable; the available input typing machines were too slow -- they were standard teletype machines; there were no big memories; and there were no fast printers. Then GE undercut us. We were just too early. But we assembled a roomful of DEC minicomputers and we purchased better storage equipment -- first the big reel cabinets from IBM and later the big flat disks from IBM.

Abbate: I was wondering about the decision to use mini-computers or even to be working with mini-computers. Did you envision mini-computers as being a thing of the future or was it just chance synergy that arose from your closeness to DEC?

Beranek: There was synergy with DEC. But our network activities showed us that the business world could use something like PC's as soon as they were available -- we were already using the smallest computer made by DEC. Thus, we were able to foresee that when smaller computers became available they would be the ones used by banks, etc. on Local Area Networks (LAN), or on wide area networks, wherever a company had branches around a city or around a country. So BBN was seeing the future need, and expecting that someone, we thought DEC, would come out with the next generation of mini-mini computers. We never thought that the networks were the best means to hook big mainframes together on a national network, although at first there may have been a predominance of them on the ARPA network. If big computers were to be hooked together they would probably use wide band telephone (T1) dedicated lines. But if there were going to be packet switching networks, the most efficient use of these would be to have a lot of people with smaller computers operating through IMP's or modems. So we had a partial vision of the direction in which things might go.

Abbate: Okay. Just to wrap up, do you have any advice for young engineers.

Beranek: This is a question I was not prepared to answer. But I'll try. I think that it is highly desirable for any bright young person to gain practical experience along with his or her formal education, either by taking off a year or so or by being involved in a cooperative university program. In my case it was operating a radio business at the same time I was going to college. And, it was taking off fifteen months to work at the Collins Radio company, where I received the highest level of experience in my field. Also, in my last eighteen months of college, I was co-partner in a business that employed three people. I feel that these things added together gave me an advantage over many of my fellow students who stuck only to their books and whose only goal was passing their examinations and graduating.

This experience gave me the expertise to move easily from one endeavor to another. I could move to managing research in an university laboratory. I could write books on noise reduction and concert halls.[9]

I learned to manage BBN first as an acoustical consulting company, and later as a big company in diverse areas, even when it became large enough to go public and be listed in the NASDAQ pages. Next, I managed Channel 5 television which was different, yet the same. My specialty has been the management of creative people, and my ability to attract top people to work with, me. I learned a lot of things in running a radio business in Mt. Vernon, Iowa, that helped me run a television station thirty-five years later. Many people succeed without this kind of background, but many do not, because there aren't that many jobs in any one field. Of course, getting a job in a restaurant isn't quite the same.

Abbate: No.

Beranek: But even a restaurant job has some advantages. A person meets people and learns why they get grumpy.

Abbate: All right. Thank you very much.

Beranek: I hope I have been helpful. And I thank you for your patience and very good questions. I hope our paths cross again soon.

[End of interview]

Notes added by interviewee

1 Incidentally, one of them telephoned me shortly after this interview and said that she was in Boston, had remembered my name and had looked it up in the telephone book. She remarked that particularly she remembered her father telling the family about my having changed a tire for him." She had never contacted me before.

2 That is like $1,200,000 now.

3 Or the OSRD, Office of Scientific Research and Development, as it was later called

4 There were few airplanes in those days and only priority passengers could usually book space.

5 Still available from the Acoustical Society of America

6 Still available from the Acoustical Society of America

7 In my years at MIT, we were commonly allowed one day a week for consulitng, as well as all of our summer months.

8 Katie Hafner and Matthew Lyon (Simon & Schuster, 1996)

9 I have written six books and they have been translated into other languages, so they have been well-used and all are still in print, even the one written in '49 and which is still selling.