SAMUEL SEELY

An Interview Conducted by

William Aspray

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Aspray: This is an interview on the 11th of June 1991 with Samuel Seely. The interviewer is William Aspray. It's part of the MIT Radiation Laboratory Oral History Project. Could you begin by telling me about your background, your education and career before you came to the Rad Lab?

Seely: Yes. I studied electrical engineering at Brooklyn Polytechnic, Class of '31. That was during the depths of the 1929 Depression. Job opportunities were limited, but I was fortunate to be offered a scholarship at Stevens Institute of Technology. So I attended Stevens for one year and earned an M.S. degree. Then I continued my studies at Columbia and received my Ph.D. in 1936.

Aspray: In electrical engineering?

Seely: No, in physics. That was the basis for one of the rather interesting aspects of the discussions that always went on at the Radiation Lab: the fact that our country had to turn to the physicists to undertake engineering activities. I always tried to defend the engineers on the grounds that at that time, in the 'thirties, there was no graduate work in electrical engineering. One was always comparing a Ph.D. in physics with a B.S. in engineering. I didn't feel this was a fair comparison. But the fact was of course, that most of the scientific people and most of the serious work done at Radiation Lab was done by physicists.

Aspray: What areas of engineering and physics were you trained in?

Seely: I did my doctoral work in diamagnetism. But after I finished that, a physics teacher at City College where I was teaching at that time and I did nuclear physics work at Columbia. Thus I was engaged in nuclear physics research for four or five years. When in 1940 or so, as you no doubt know, Prof. Rabi called on all of the Columbia Ph.D.'s and said, in effect, here's the situation. Special activities for the government exist: you have two choices: You can either go to activity 1, or you can go to activity 2. And being more or less an electronics type, I chose Radiation Lab rather than what turned out to be the atom bomb project.

Aspray: Right. So when did you come to the Laboratory?

Seely: Nineteen forty-one.

Aspray: What was the status of the Lab when you arrived? How large was it by that time?

Seely: It was all contained in one major room, as I remember it, with a little activity on the roof. I would guess that we might have been a group of a couple hundred at the time, at most.

Aspray: To what group were you assigned? What were your duties originally?

Seely: Originally I worked with Zacharias (I knew him from Columbia). The only thing I remember that we did together was to establish the magnetic field requirements for optimum output of magnetrons. Then I went to the synchronizer group, as it was then called. Thus I was involved electronics circuit development at the outset: wave shaping of one sort or another to meet the synchronized needs of other components being developed elsewhere in the laboratory. I was with that activity for possibly several years. Then I joined the roof group and was involved

with complete system development. At some point I was charged with installing radar equipment into a truck. At this point I essentially got into the complete systems design; this involved promoting a modulator from the modulator group, an indicator from the indicator people, and other groups something else. You eventually secured all of the necessary pieces, then it was required to put them together with this mobile unit stationed on Deer Island. We conducted some preliminary tests at that time, the Seacoast Artillery was interested in radar could be employed by them. So I spent some time with Colonel McGraw from Seacoast Artillery while he was assessing what a 10-centimeter radar with a 4-foot dish could do for their seacoast responsibilities. These studies led to an order by the Seacoast Artillery for some preliminary models. These were made by the Rad Lab associated Research Construction Company.

Aspray: Yes.

Seely: The original order was for 50 of these units. I was essentially the project manager of what turned out to be the SCR-582. These units were installed at many remote spots in the world. The original book, Five Years, shows a few spots where these 582s were installed. I had occasion to go to Panama to visit two 582 sites, one on each side of the Canal. My visit was a rather fortunate occasion for the technical sergeant in charge of the installation on one side of the isthmus. His equipment's performance was compared with that on the other side of the isthmus. This performance was that to be expected under normal atmospheric conditions; for the location of the equipment, the normal range was 20 to 22 miles. However on the other side of the isthmus, they always seemed to have abnormal atmospheric

conditions, and their range was from 50 to 70 miles. So this poor soul on one side of the isthmus was taking a terrible beating because he couldn't make his equipment do 50 or 60 miles. It was only when I got there and assured the people that his unit was actually operating normally and the other was operating under abnormal conditions that they got off of his back.

Aspray: [Laughter] I see. Did you have other responsibilities while you were at the Laboratory?

Seely: Before answering this, there was one other interesting aspect of my trip to Panama: I visited a radar site that was using a Canadian-built 200 megahertz radar unit with a large antenna array that could be rotated automatically. Unfortunately something went wrong with the antenna control system. As a result, to effect rotation of the antenna they had soldiers on the roof of their radar building day and night pushing this antenna assembly around amid mosquitoes that must have been at least an inch long. At the time was a signal corps officer, a Captain O'Donnell. I asked, "Why don't we help these fellows out?" He said, "Well, why don't we try." We first checked to find that a thyratron circuit was being used to drive the antenna assembly. I was pretty well up on thyratrons and how they operated. (I had the technical sergeant). Looking at the corrections diagrams of the antenna drive, I had the technical sergeant measure the voltage at designated pair of the circuit. I finally said, Okay, now, what you must do is replace this particular potentiometer. A new one was installed. Suddenly there was a scream from up on the roof: "Hurray! The antenna is rotating." Evidently this poor tech sergeant had been trying to correct this problem for some time. I recall saying to

Captain O'Donnell "Let's get out of here while we're still batting 100 percent." [Laughter]

Aspray: Right. Oh, that's funny.

Seely: At about this time arrangements were being made for Radiation Lab to provide help to the Australian Radiophysics Laboratory, on microwave radar, a new frequency region for them. I was asked to head a small group to go to Australia. Our group numbered seven scientific persons, most from Rad Lab, but a few from outside companies. We took an airborne 3-centimeter radar set with us and a variety of radar since the purpose of our visit was not totally clear. We decided to use the 3-centimeter equipment and build complete ground-based radar equipment. While we were working with Radiophysics Laboratory staff, the actual structural group involved was the Australian Railways since they had a complete shop. We worked with the Australian Railways in planning a ground-based equipment that had the performance characteristics of what at Radiation Lab was a set called Li'l Abner. The antenna design of Li'l Abner resulted in a beavertail shape that had a very narrow beam in the vertical and a fairly broad beam in the horizontal. Because of the narrow beam, the antenna pattern could flap up and down with limited ground clutter. The equipment that we built essentially "followed" Li'l Abner, and we felt it should be known as Daisy Mae.

Aspray: Daisy Mae. Right.

Seely: So this was the "Dizy" May (Australian pronunciation). [Laughter] The Radiophysics scientists were interested in pursuing 25 centimeters at the time, as I remember it. We worked with them at this wavelength. Some of the 10-centimeter

components we had sent to Australia served well as examples of the character of microwave plumbing at their 25-cm range. Of course all of the equipment we had with us was left behind. I don't know how much was used. On the whole, I think we did a creditable job in our joint endeavors. Because we were in the Australian Theater, we were attached officially to General MacArthur's headquarters. When he moved from Australia to the Philippines, we felt that we had to leave. We went to Australia in August of '44 and came back in March or April of '45.

Aspray: How did such a cooperation get set up in the first place?

Seely: I don't know. I presume much like the British-American arrangements. There must have been either an inquiry by the Australians, or an inquiry by MacArthur's headquarters, or an inquiry by the British speaking for the Australians. I never heard how those negotiations got started.

Aspray: How much did you find that the Australians knew about radar when you arrived?

Seely: They had a small technical group. My recollection is that they were just really getting started, although they had some experience with 100-200 MHz radar I presume because of having read about microwaves and thinking in terms of physical sizes they adopted 25 cm for study. The deciding factor may have depended on being able to secure magnetrons at about 25 centimeters. This matter was never discussed with us.

Aspray: Had they employed good scientific and engineering talent?

Seely: Yes. They had good people. They were from their Radiophysics Laboratory staff.

Aspray: One thinks of the Australians as being very good in radio physics. It's one of those areas that they're noted for.

Seely: Yes. They had done quality work in radiowave propagation. I remember one fellow, Joe Pawsey, who was a pretty astute individual, also a most competent fellow by the name of Mills. Those with whom we worked were good people.

Aspray: After Australia, what happened to your own career?

Seely: Well, I got back from Australia in mid-'45. There was the realization that things were winding down. I was assigned to head the Publications Office to ensure rapid productions of reports. There's one part of my Rad Lab career that I'm trying to remember.

Aspray: Yes.

Seely: One of the photographs in the Rad Lab publication, Five Years, shows me and the publications people. I had previously been involved with reporting and preparing the lectures by W. W. Hansen for internal publication. The picture shows me and the Publication staff of the bevy of girls.

Aspray: Since we talked about that off tape, would you say a few words about the Hansen Lectures, and what importance they had, and what your role was in them?

Seely: W. W. Hansen was professor of physics at Stanford University. He was involved with the Varian brothers in the development of the klystrons. The Varians were his graduate students who were working on the resonators and the mathematics of the klystron. Also involved in this work was Professor Webster, who did some of

the analytical work on the bunching process. Together Hansen, Webster, and the Varians really developed the klystron. During the war Hansen spent a great deal of his time with the Sperry Corporation, which was then making klystrons.

Aspray: Was that on Long Island?

Seely: Yes. Once a week, Hansen would take the Eastern Steamship Lines from New York to Boston. He enjoyed the trip for it gave him a place to sleep, and he could work in a quiet place. He prepared lectures on electronics and microwaves. He covered an amazing array of topics. These lectures continued for several years. I was the principal reporter and editor for these lectures. This material was reproduced and made available to the Rad Lab staff. The total output amounted to 3 or 4 volumes each of five or six hundred pages. These notes included most topics and concepts important to radar including details of magnetrons, strapping of magnetrons, the klystron, antenna theory, and receiver design.

After the war, the Hansen Lectures were reviewed to see what part of it was original and what part might have been of survey nature. I always felt that he contributed a great deal by consolidating and interpreting this enormous quantity of information, and presenting it in understandable and usable form. But when the surveyors looked at what he had done and its relation with what others had done, the feeling was that they couldn't isolate enough individual material to give him credit for any essential part of this work. But from the point of view of Radiation Lab, his efforts provided an enormous educational experience for lots of people.

Aspray: Did this run the whole period of the Laboratory?

Seely: No, but I think it must have run for at least two years.

Aspray: So your responsibility was writing these notes up?

Seely: I attended the lectures, took notes and then transcribed all of these notes so that they were available to the Laboratory personnel essentially in book quality.

Aspray: Were they routinely distributed to some set of people?

Seely: I think so. They used to be prepared in ditto form. We used to run quite a lot of them. One other activity I got involved with--I don't remember the timing of this--was with the trainer group. That was with the supersonic trainer. You may have talked with someone about that. Did you?

Aspray: I've heard about it, but I haven't really discussed it in any detail.

Seely: It made use of the fact that the velocity of sound waves through water turned out to be just about 1/200,000th that of radio waves in air. That meant if you could get a supersonic transmitter in water, then the world would shrink by a factor of 200,000.

Aspray: Yes.

Seely: The British had been working on this, and they had developed a supersonic trainer using these facts. The head of our group was Ray Garman, and I was the associate head. Garman went over to England to see what they had. Then we carried out our own work and ultimately produced a supersonic trainer operating at 15 MHz, the water tank of which measured roughly 3 feet by 10 feet, and represented an airborne area of roughly 60 x 200 miles. Among other problems was the development of transducers that is, hydrophones to accept electrical pulses and

produce the supersonic sound. We also developed a computer, which controlled the two-dimensional movement of the hydrophone. This could move forward and slide back and forth so that the movement simulated the movement of an airplane or a ship. The hydrophone would rotate at radar scanning rates and scan a model plate to yield a typical radar pattern. Our glass plates were made to stimulate ground patterns by using pebbles and sand. As a special super-secret project, Paul Rosenberg, as I remember it, was assigned the job of making the plates to represent Tokyo Bay. Thus when General Doolittle and his crews bombed Tokyo, they had already had a rough idea of what Tokyo Bay would look like on a radar screen from the supersonic trainer.

Aspray: Ah! I see.

Seely: That was an interesting development.

Aspray: Can we talk for a couple of minutes about interactions between the Laboratory and the military. You've already referred to several of those. Maybe I should just let you speak, but I'm interested in knowing what was the style of interaction? Was it that they came to you with a set of needs and you tried to satisfy those? Or were some developments within the Laboratory of things that they'd think would have promising use for the military? Or what?

Seely: Well, in my particular case, what came to be the SCR-582 and then the next version of it, the 682, was essentially Laboratory-built. Initially we had invited the military saying here is a device which does thus and so. Is there a military application for such a radar? This development was unlike the 584, which was, I think, an assigned project, there being a recognized real need for anti-aircraft fire

control. Actually the original assignment of the Laboratory was to develop radar for an airplane to permit a night-fighting capability.

Aspray: Right. With the 582, had there been military contact along the way so you understood the design parameters?

Seely: No.

Aspray: Not at all?

Seely: This was an internal development to explore the capabilities of microwave radar. A similar sequence of events involved shipboard radars. Laboratory people would get together and say, "Well, this looks like a need. Or this looks like an objective that can be met with our equipment. Let's see what we can do with it."

Aspray: What was the reaction of the military when you first displayed your prototype of a 582?

Seely: My recollection was that the military group that showed interest was Colonel McGraw from Seacoast Artillery. He was very interested in knowing what such equipment would do. After being checked out on the operation of the equipment, he did all of the testing by himself. This was not a case of my turning the knobs and demonstrating. He wanted to make sure that he understood all aspects of it, and he did.

Aspray: Had there been attempts to interest other military groups in this?

Seely: Oh, I would think yes; the notice of this and other roof group projects or the systems projects went to all military. Microwave radar was of interest to the

Navy. Ray Herb became involved with Raytheon in building (I've forgotten the official Navy number) shipboard search radars. This development has become the predecessor of practically every radar that you now see on almost every boat of 20-feet or larger.

Aspray: I see.

Seely: One other thing resulted from this 582 project. Once you had the equipment, then of course you had to have an instruction and maintenance manual. As far as I know, mine was the first complete technical manual that came out of the Laboratory. It was subsequently used as the basis for the training programs that were carried out by the training group. I was quite acutely aware of the need for a complete review program because when I got to Radiation Lab, there was a little group here, and a little group there, each doing its own thing. At no time was there ever any indication of who was doing what and for what purpose. So when I wrote this technical manual, I tried to make it clear that at least there was a coordinated picture of what radar was about, how it operated, and what problems might arise.

Aspray: Did individuals in the Laboratory have that overall picture?

Seely: I'm sure there were a few at the outset, but on the whole I would guess that at the outset, not many. As an example, if you were with the synchronizer group you were supposed to build equipment that provided a number of waveshapes: for example, a square wave, or a sawtooth, and they had to be movable with respect to fixed positions. Usually that was all that was said, without any indication of who wanted such waveshapes and what was their function to be.

Aspray: My own specialty's in computing, and let me take an example from there to ask you this next question. During the war, one of my friends Bob Campbell worked for Howard Aiken at the Harvard Computation Labs. We were looking at the manual for the Harvard Mark I computer one day.

Seely: Oh, yes.

Aspray: He said, "Notice in looking at this manual, how much different it is from today's manuals. You look at page 1, and there's no overview, no systems approach to it, that you get in the first paragraph the starting description of some component in this thing." Was there a sense of systems design, systems development, within the Laboratory, and how pervasive was that kind of understanding?

Seely: I'm sure there was, but early on it was not very pervasive around the Laboratory. I think there was a small control group at the top who had a good understanding of what was involved. They would perhaps do planning, and then they went to this group or that group, and pointed out some of the problems. But on the whole, I would say that the overall understanding by the Laboratory people was pretty limited, except for a small group. This shortcoming would improve materially with time.

Aspray: I see. Did that cause many problems in people getting their work done? I mean, did a solution to one part of the overall project mesh well enough with the needs and solutions of other parts?

Seely: Not really. Early on when you look at what subsequently became the case with the availability of instructional material. For example, initially there was a

synchronizer group, and an indicator group. Actually, when you do the electronics of these groups these are essentially a single problem. But initially there were two separate groups with very little mutual interaction.

Aspray: How would problems manifest themselves? If these two groups are working separately, who would understand their interaction?

Seely: Usually the heads of the groups would say, "We need so-and-so," generally without very much explanation of what the purpose was.

Aspray: I see. Okay.

Seely: After the war, I sat down and wrote a most successful book that was called Electron Tube Circuits, I took most all of the circuitry that was used at Rad Lab and other sources and put it in a book form. While the book didn't tie things together into systems, nevertheless, it said: "If you want a sawtooth wave, here are N ways to make such waveforms. If you want to do clamping, here's how you do this." Such considerations of a very wide variety of circuits were undertaken. I wasn't competing with the Radiation Lab series, but this book did cover most of the wartime circuitry.

Aspray: To what degree were you familiar with the work that was going on at other places, for example, at the Army Signal Corps in Fort Monmouth, or at the Naval Research Lab, or in Britain?

Seely: There were weekly seminars, every Tuesday evening, and there were always speakers from at home and abroad talking about what was being done. There was a real effort made to acquaint us with what was going on. And W. W. Hansen

was available for discussion of non-Rad Lab work.

Aspray: Outside of the seminar, did you have any direct contact with some of these other laboratories?

Seely: I didn't. I'm sure other people did. It was just that the work I was doing didn't really involve what others were doing.

Aspray: What about industrial concerns? Did you have contacts with Raytheon or Westinghouse or AT&T or whomever?

Seely: No, I didn't. These organizations were involved with separate groups. I know that the magnetron group was intimately involved with a GE group in Massachusetts. No. I guess I lived a kind of an isolated, internal existence.

Aspray: How did you feel the Laboratory responded in getting you the equipment or the money or the staff you needed?

Seely: I never had any problem. The money part we never knew about because this was always handled by top management. When we went to Australia, I was given a supply of travelers checks to take care of the group. We had our assignment and we lived isolated lives trying our best to carry out our assignment. We reported regularly.

Aspray: Did you have trouble getting qualified staff when you needed extra staff? I mean, not that they were unwilling to give them, but were people available? Was that a problem?

Seely: Oh, you mean internally?

Aspray: Internally.

Seely: No. I think you always could get the support that you felt you needed, even if you borrowed somebody from one or another of the other groups for a short while to do a special job.

Aspray: Can you tell me a bit about your experience of the way that the Laboratory ran? Management of the developments and procedures and informality versus formality. Those kinds of questions.

Seely: I think one of the real strengths of the Laboratory was its essential informality. I think the general feeling of top management was that most of the scientific staff, most of whom were Ph.D.'s, were all pretty well qualified. It was assumed that with an understanding of the problem you were free to work on it with your Division and Group guidance. I think that this attitude pervaded the entire laboratory. Now as far as the larger picture is concerned, that was always done above the administrative level at which I operated, but the channels were always open for consultation.

Aspray: Okay. How did you communicate with others in the organization? Were there formal mechanisms for doing so? Did you have to put in periodic reports? Did you have an internal seminar where each of the staff groups reported on what their progress was? Was there that kind of procedure?

Seely: As I remember, no periodic reports were required. If you had anything to report, you would write a report. You weren't hidebound by any requirements of memos. And if you wanted to talk to Joe Blow in one of the other divisions or other

branches, you just went over and talked with him. Completely informal.

Aspray: Was there any constraint or compartmentalization because of classification problems?

Seely: I don't think so. Everyone on the staff had been subjected to security clearance. Of course there was a scientific staff and there were the technicians and the secretarial pool and so on. And these were all pretty well recognized, but I don't recall any communication constraints.

Aspray: If you were working on a project, you didn't have to be careful not to tell somebody in another group about the details of your project?

Seely: No, not at all.

Aspray: When you did need more money or staff or whatever, did you have to write a report to get that?

Seely: I don't think so. I think what you did was to explain your need to the division head or section head and then things generally got done. From that point of view, it was really a marvelous organization. As I say, it was understood that the scientific staff were a bunch of competent people, and when they understood what the problem was, they were allowed to proceed to solve it, and with whatever support services they required.

Aspray: What about in task assignments? How much were you directed from above, and how much were you just given freedom to develop things on your own?

Seely: As I recall, it was both. Occasionally there would be an indication that a particular

need required attention. In terms of how you did it, this was up to you. Also if you wanted to try something original, you did that. As an example, Ralph Meagher in the roof group, was a real good circuits man. He became interested in indicators. As a result there were two different attacks on the indicator problem; there was an indicator group of perhaps 30 people, and there was Ralph all by himself, doing his own thing, his own way. Some of the ultimate indicator practices were the result of his work.

Aspray: I see.

Seely: As far as I know, there never were any complaints about being experimental and original.

Aspray: How did you come to leave the Laboratory? We started on that path and didn't really finish that story. What did you do afterwards?

Seely: I had completed my doctorate at Columbia in '36. I was teaching at City College from 1936. And it was in 1941 that I left City College and came to MIT. After the war I got a job at the Naval Postgraduate School, but I stayed for only one semester.

Aspray: In Monterey?

Seely: No. This was before they moved to Monterey. I decided I didn't want to go to Monterey.

Aspray: I see. Where were they then?

Seely: They were in Annapolis. In 1947 I joined the Electrical Engineering staff at

Syracuse University. I had been an instructor and assistant professor at City College, and I was associate professor at the Naval Postgraduate School. I went to Syracuse as full professor. I was at Syracuse for nine years, I think, and the last two or three years I was department head. Then I was approached by Case Western in Cleveland. They wanted to completely upgrade their engineering program, and that appealed to me, so I moved to Cleveland. I think we stayed in Cleveland another eight or nine years. But the interim period was an interesting period for me, because I took two years leave to become head of the Engineering Division of the National Science Foundation.

Aspray: Oh! Uh huh.

Seely: Then I accepted a one-year Fulbright Lectureship, and I spent a year in Greece. And that, too, has a bit of interesting history. From my experience in Greece, I went back to Case, but at that time my wife said, "We must move back East. I don't like the Cleveland weather." So we moved back East. I decided to try visiting professorships. I spent two years at the University of Connecticut, two years at the University of Massachusetts, and then seven years at the University of Rhode Island. I guess in the period between the Connecticut and the Massachusetts appointments I spent a year in Sweden as visiting professor there.

Aspray: How would you say that the Rad Lab experience affected your career?

Seely: Oh, very materially. I had very little electronics experience prior to Rad Labs days. Jack Millman and I (two physicists) had written a book on electronics that came out in 1942 (we wrote it in 1940, before Radiation Lab days)--a very good book, by the way. It was actually in print for about 25 years, used more by

physicists than engineers the last ten or 15 years. But except for the little bit of electronics that was in that, I didn't know very much electronics circuits; I had no real experience. The Radiation Lab experience was a complete educational eye-opener for me, and particularly the Hansen Lectures which essentially covered all aspects of electronics. Since then, I've written 16 books on one or another phase of electrical engineering.

Aspray: All of them in related areas?

Seely: Not completely. But a good many, yes. However, the term engineering circuits can be inclusive involving electronic circuits or general circuit theory, such as filters electrical machinery, feedback control. Little of this was included in my Radiation Lab experience. However, my experiences there gave me a technical background in the basic science behind electrical engineering. This has stood me in good stead ever since.

Aspray: What haven't we talked about with regard to the Rad Lab that we should?

Seely: The only thing that comes to mind was the attitude of the technical staff toward each other. Everyone had tremendous respect for everybody else. There was a certain amount of backbiting and a certain number of people trying to climb on the shoulders of others to ascend in the hierarchy. But on the whole, my recollection is that the staff were just a bunch of nice people working together who recognized that there was a job to be done, and who tried to do meet the job. Few looked upon this job as other than a temporary activity. It was a marvelous experience. There may have been others who don't feel the way I did. My associations were at an intermediate administrative level. So perhaps I had a

freedom that some of the other staff didn't have. But from my level it was a real wonderful experience.

Aspray: Your comments are consistent with what I'm hearing from everybody.

Seely: I think that the favorable attitude resulted from the performance of the top administration--Lee DuBridge, and Wheeler Loomis, and others. Clearly, their approach was that these scientists were good people. Just tell them what the requirements are and let them alone. I think this attitude paid off. Remember that a number of the staff members were subsequent Nobel laureates.

Aspray: Do you want to say anything about the social organization or social life of the Laboratory? What the work environment was like? What you did your off time? How much you spent time with other people in the Lab? That sort of thing.

Seely: Well, during Rad Lab days three of us, Ray Herb, Rufus Wright, and I were unmarried and we shared an apartment. We had little association other than our interests in what each other was doing and that we occupied an apartment jointly. But I still keep in touch with both of these men after all these many years. We exchange Christmas cards.

One of the activities that I remember was seminar night. This was generally a Tuesday night. There was always a group of from 15 to 20 staff members who would go out together for dinner usually to Chinatown. One of this group was Lan Chu. You may have heard his name; he was one of the antenna group and he was an MIT professor on leave. Chu would usually do the ordering.

A variety of group activities developed. There were activities that were available in Boston. There were staff people at the Laboratory who would put on shows at

one time or another. There was a varied talent available. Many of the staff engaged in a variety of family and community interests. There was a fair amount of socializing among the people.

Aspray: What about going into work? If you went in in the evenings, did you find lots of activity going on? If you went in on the weekends? What was the typical workweek?

Seely: Your schedule was largely up to you. You were doing a job, and if you were anxious to stay with it, you would stay on. I don't think there was any concern for a normal 40 hours a week of work among the scientific staff. The time schedule was quite flexible. If you had reason to leave early, you would leave early. If you wanted to stay late, you stayed late. I think that the scientific staff did not consider a week at 40 hours. For a lot of people, the technician and secretarial staff, this was just a job rather than an experience, and they adopted normal workday schedule.

Aspray: Can you tell me about the interrelationship between the scientific staff and the technicians? How did you choose the technicians? Did you direct their work? Did you hire them personally? Those kinds of questions.

Seely: In our group, there was a certain number of technicians assigned to the group. When I was working on a synchronizer, for example, I would draw the circuit design and then have one of the technicians build it. When the unit was completed we would sit down together to debug it, to make it do what it was supposed to do.

Aspray: Right.

Seely: The Personnel Office did the interviewing and hiring of all staff, including technicians, secretarial and general support personnel. Some of the technicians were remarkably good. Others were competent but ordinary. In each group we had a reservoir of technicians. If you had something to do, you'd ascertain which technician had time, and he would undertake the job.

Aspray: What kind of training did the technicians have?

Seely: Those that we had were experienced electronic technicians who could read circuit diagrams and do the initial construction. They were able to put things together in a very clean and orderly fashion.

Aspray: High school education?

Seely: I would expect so. Perhaps even vocational school beyond that.

Aspray: Moving towards closure, what would you say were your greatest personal accomplishments at the Laboratory?

Seely: Well, I don't think there's anything I would call "outstanding." I think among the things that were reasonably noteworthy was the design of the 582, our effort and experience in Australia, work in preparing the Hansen Notes. Those were probably my major contributions to the cause.

Aspray: Okay. Do you have anything else you'd like to add?

Seely: No, not about the Laboratory. I think we've covered my recollections and feelings about that organization.

Aspray: Okay.

Seely: I do have one thought. Subsequently, since I've had some government experience, it just is astounding to me that the government was able to set up an organization to do a particular job, and then when the war was over to close up shop. But of course this was readily possible because, in part, most of the Rad Lab scientific staff were on loan from institutions.

Aspray: Right. And they wanted to go back. Yes.

Seely: With the demise of Radiation Lab and presumably a recognized need for an advanced laboratory the Lincoln Lab and other labs were established. But still, for the government to start something and then subsequently turn it off, is a real phenomenon.

Aspray: Well, thank you very much.