Haskins Laboratories

An Oral History of Haskins Laboratories

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December 2006

Haskins Laboratories
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USA

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This is the first disc of 16[1] covering the early history of **Haskins Laboratories** as remembered by the laboratories' founders and other early participants. Most of the recordings were made in the fall of **1988**. The last recording was made in **February 1991**. The session on this disc began on **August 12, 1988,** the occasion of **Caryl Haskin's** 80th birthday. Three senior members of Haskins Laboratories were present**: Caryl Haskins, Franklin Cooper, and Seymour Hutner**. Caryl was invited to begin with a memoir of his early life and the events that led to the founding of Haskins Laboratories. He initially completed the task in little more than 5 minutes, much to my dismay and fortunately others too. Frank saved the day by gently suggesting that Caryl needed to be much more forthcoming, and it was just about at this point that I noticed the red flashing light on the tape recorder and realized that the machine had not been recording. And so faced with the facts, Caryl good-naturedly agreed to begin his memoir once again, this time in greater detail.

PWN

[1] Number refers to recordings sampled at 44,100 per second. With MP3 compression the complete set of recordings can be stored on two CDs.

Pat: It is 9:34 a.m. on Friday, 08/12/98, and this is the first of a series of informal oral recollections, which we began a moment ago, that have restarted. Today, we have **Caryl Haskins, Franklin Cooper, and Seymour Hutner** together here. It is the occasion of Caryl's 80th birthday (as it happens) and Caryl is recorded on channel 4, Frank on channel 1, and Seymour on channel 3. I would like to put a question to Caryl, I would like to learn something about his upbringing and what were the major influences on his life that made him the man that he was in the 1930s, the man who met Frank and Seymour and formed the Laboratories.

Caryl: Okay, I was born in Schenectady, as you know Pat, and went to Schenectady Public School actually, and then to the Albany Academy in Albany for secondary, and to Yale.

Pat: May I ask you, your parents were interested in science also?

Caryl: My father was a vice-president for research in General Electric and an electrical engineer and particularly interested in meters, early development of meters like speedometers and other kinds of metering instruments, but he died when I was three, died very suddenly on a trip to the West Coast in Salt Lake City at a very early age of 44. So, I was brought up by my mother and I was the only child. So, the two of us lived in Schenectady. First in the house, later occupied by the Coolidges', by William D. Coolidge[2], and then we moved to a very small house in upper Schenectady until I was five and six, I guess, and then my mother built a much larger house to which we moved when I was about six to seven and that was where essentially I grew up. I was somewhat in and out of General Electric and knew a number of the General Electric people fairly well, knew Langmuir[3] for example and knew William Coolidge and knew, as Frank was saying, Lou Steinmetz[4]. Steinmetz was a neighbor actually and lived not very far away from our house and had a wonderful greenhouse particularly with the southwestern cacti, all kinds of southwestern cacti, and a pond, the goldfish pond, where I could go downstairs and look in from the side and that was a source of wonder, and probably hooked me on to fish for life, but I didn't actually have anything to do with General Electric except those secondary contacts.

Frank: Your sleeve is caught in your microphone cable.

Caryl: But I always had a feeling I would like to work in a research laboratory and did make a couple of calls when I was quite young, when I was still in school, (taken around the house of magic and so on) which rather deepened that impression. But I went then to Albany, as I say, for four years, but we kept our house in Schenectady and commuted. And then I went on to Yale and probably have an all time record in Yale, because I majored in medieval French and Chemistry.

Frank: Why chemistry?

[2] William D. Coolidge, born 1873 and died 1975. Served as Director of Research at GE from 1932-1940.
[3] Irving Langmuir, born 1881 and died 1957. Nobel laureate in 1932. Worked at GE 1909-1950.
[4] Charles P. Steinmetz, born 1865 and died 1923. Credited with inventing AC power transmission. Worked at GE 1893-1902.

Caryl: Why chemistry? Chemistry was the easy one. Took a Ph.D. there and you couldn't take anything better Ph.D. with that kind of a preparation. Then, we moved back to Schenectady and I applied then for a job in the research laboratory and applied to William Coolidge who was at that time the vice president. As I say, it was in the late phases of x-ray work of high (what was then regarded as very high) voltage x-rays work (million volts x-rays) they were just developing that, and rather freelanced for a period. Did some work what they call 'down the line' in plating processes and some metallurgical processes and things of that sort. And then Coolidge asked me to set up a program for biological effects of x-rays. There was nobody around at that time who was doing much of that sort of work and they were very interested to know what the shorter wave length x-rays, what sort of biological effects they would have. So, I did set up with him and with the help of Willis Whitney[5] who was senior in the laboratory at that time, a program on radiation effects and heredity and got so thoroughly hooked on that that in the second year we got some (of course it was a primitive field at the time), we got some marvelous things out in a primitive frame work. Then I thought I ought to go back and get some more training in the field and went back to Harvard and enrolled for a Ph.D. in biophysics and was very very lucky in some of the teachers I had, but at the same time Coolidge let me continue to use some of the higher voltage x-ray equipment in General Electric. So, I did my class work and so on in Cambridge during the week and came back weekends and worked with radiation material. And I was very very fortunate in two contemporary graduate students I met at that time and we all were together through**:** William Arnold[6], who had done some really outstanding work in photosynthesis with Emerson[7] at Caltech and had recently come to Harvard, and Henry Cohen who was on the medical side. We had common interests and actually my own thesis work (and that of Arnold's) which I did on photosynthesis rather intersected, but it was possible, primarily because of the equipment that Coolidge made it possible to use at General Electric. So, then after the degree, Conant[8] got interested in this work and said we could continue to use the lab (that I had used for the graduate work) as a post doc situation. And then, while in the second session at General Electric, was when Frank and I met.

Frank: May I interpose a question Caryl, you had worked at GE as a regular employee before you went to Harvard?

Caryl: Yes, that's right.

Frank: So, when you came back to GE, it was essentially picking up a continuation situation?

Caryl: Picking up. That's right. And picking up a question in quite a different field.

Frank: Right. Right.

Caryl: Yeah, I went around the world in an around-the-world trip when getting out of Yale.

Frank: This would have been in **1930?**

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[5] Willis Rodney Whitney, born 1887 and died 1985. First Director of research at GE from 1900-1932.
[6] William Arnold discovered delayed light in photosynthesis. Received a Nobel Prize in Chemistry in 1932.
[7] Robert Emerson shared the 1932 Nobel Prize in Chemistry with William Arnold.
[8] James B. Conant, born 1893 and died 1978. President of Harvard and wartime head of the NRDC.

Caryl: 1930 to 1931. Right? Oh I guess November of 1930 to April of 1931 and got back in 1931 and that was when I went to Coolidge to see if I can come to the laboratory for first time.

Frank: Right. While I am into this, your mother died some place along this line?

Caryl: She died in 1933, in the spring of 1933, before I went to Harvard. That the first session in GE was when she died and then Coolidge pressed me very hard to go back to Harvard and take a doctorate, which I had not originally planned to do; I planned to go directly into industry and stay there, but thanks to Coolidge and thanks to some advice from the man in the corner there, which I will never forget.

Frank: I didn't know you then?

Caryl: All right, I may have skipped a year.

Seymour: How about the elephants on the Mohawk trail, you used to joke about it?

Frank: Later, later

Caryl: You mean driving over the Mohawk trail. Yeah, so you see we were driving every weekend or better and the famous mouse story comes out that of course. The actual mice were there, but Frank has always insisted that it was a hallucination.

Frank: I should provide enlightenment about this.

Caryl: Alright, you provide the enlightenment. Be sure you tell the correct story.

Frank: I shall, but you won't agree.

Caryl: Let's see, this is a rather tangled narrative.

Frank: I interrupted you at about the point you were coming back to GE for your second tour of duty and I think were with **Marshal**'s group at that point.

Caryl: That's right. Working directly with **Ralph Monde** on the lubricants.

Frank: Something very exciting.

Caryl: Seemed a little too exciting after the biological work and so made the decision at that time to leave the company, because this of course was (19)33 and with the depression on, they simply couldn't afford to continue the biological work. So that was the combination. I wanted to get the extra training in Harvard and also probably decided about that point not to work for GE because it would have meant being an industrial chemist indefinitely. So, then this ties in with the story I was telling earlier at Harvard and MIT, where Frank and I met the following year, I think.

Frank: In the summer of 1935.

Caryl: The summer of 1935, and Frank shifted his thesis work, which had been in spectroscopy into this kind of thing.

Pat: How did you meet?

Caryl: We met because we were working on different floors at General Electric and I was commuting back to Harvard, weekends and Frank was commuting back to MIT for his first doctoral year at MIT. So, Frank had heard that there was this chap who had a broken down Ford and was going back and forth and asked how about a ride. So we had the ride, except the ride lasted all night in fact, much longer than we expected and went into some of these problems and went into some things you might be able to do about it. And really that set up the idea of the Laboratory, I think as much as anything. Frank didn't join the Laboratory till sometime after his own Ph.D. and he went back to General Electric for a period before joining the Lab full-time. So, at MIT after Frank's graduation and Frank had couple of graduate students also including one Orlin Morningstar.

Frank: Let me interject.

Caryl: Please do.

Frank: This (what you have been talking about) was the summer of 1935.

Caryl: Okay.

Frank: I believe you had already had your Ph.D. from Harvard that spring?

Caryl: That's right.

Frank: Spring, June of 1935, and then had come back to the laboratories working full-time again in chemistry.

Caryl: Correct.

Frank: Marshall's group, I do not know what you were working on really at that point.

Caryl: Oil Oxidation.

Frank: Right, then we met and set up my thesis problem at MIT for the 1935-36 academic year. We met that summer and talked the problems through and decided that I could handle the physics but we needed somebody to do the biology.

Caryl: So, we looked around for somebody and found the magician to do it, up in Cornell, concealed in a Cornell basement, and I managed to pry him out and so here he is.

Caryl: Over to you, I think?

END OF REEL 1 SECTION 1

Seymour: Well, I had a weird background in that I had an idyllic Brooklyn, New York childhood. I come from an extraordinarily intellectual family with the most improbable pair of parents one could imagine. I have to tell about that because it taught their lesson in intolerance. My father was a rare combination of poet, idealist, and engineer and he came here as refugee from the Czar's Army in 1906.

Caryl: I never knew that.

Seymour: Yeah. We have lately traced our ancestry. There is a von Drisenstock. We were court Jews or we intermarried for money quite efficiently, as I understand it, and so we picked up a lot of defective genes, Huntington's chorea, seven-seven consecutive cousin-cousin marriages to keep capital in the family.

Caryl: Including Huntington's chorea?

Seymour: Huntington's chorea. But something that passes for intelligence. My mother was an extraordinarily aggressive businesswoman with whom I felt utterly uncongenial. My father never ordered me to do anything, but he avidly read my textbooks and annotated them to know what kind of a world he was living in, so we were equals from say age seven or eight. He never gave me an order. I was completely at his orders, whatever he wanted, I did. My brother was a very handsome outgoing person, he became a very successful physician, very much of an outdoor man and he took me along hiking and camping with an official in the boy scouts and so I was very much...

Frank: How much age difference, Seymour?

Seymour: Oh, a lot about 12 years, but he was an exemplary brother. When he would take me along it would be very annoying because people, girls who wanted to date him, thought I was his son. The difference was that great, but fortunately, I did very well with my memory, which was a marvelous substitute for a first rate intelligence. I began to realize that my father was a good poet, which meant he could pluck metaphors and similes out, and he wrote very good comic poetry, extremely good. But I want to tell this story because I learned something in childhood. Here were two people as opposed in temperaments as could be and yet we had a quite harmonious family, excepting we all read newspapers at the dinner table not to get into quarrels, I among them. As I now realize, it was a most extraordinary family. And my cousin, for example, my first cousin, is Lord Solly Zuckerman[9], who combined some of the more obnoxious traits of my family that I have been amply given to understand.

Caryl: I gathered it.

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[9] Solly Zuckerman, born 1904 and died 1993. Physician, polymath, UK science advisor and FRS. Became a peer in 1971

Seymour: I must tell this story, because it cleared up a mystery. With my memory, the puzzle of my parents pursued me all the way into college. I came home from Cornell, and there were my mother and father laughing, my mother did not have much of a sense of humor and they were both laughing. First time, I had ever seen them laugh together. Then, I said "what was so funny, please", my mother said, "you know your father is a very shy man." I said, "Yes." "You know how I am? " "Yes". We were talking about how we got married. I said, "I wondered." "Well, you know your father also likes to have a good time with friends." "Yes". "He was invited to a mock wedding. When they got there, it was a real wedding and I was it, and I had organized it" and rather than spoil the party, he went through with it. You see the lesson, get along.

Caryl: Yeah

Seymour: We were quite successful. Now fortunately, I had virtually no complexes because I was very good in school, effortlessly until I met Frank then I realized my limitations. I mean this quite sincerely, entirely undisciplined, but very silent, but not other shiners I was observing. I was known in Hebrew as a cold spirit. I would be very polite then nod and then they would learn that I was unimpressed as if I were a novelist in the making. I was also an extremely, for my size, good athlete and there were, though I would always and I looked like little Lord Fauntleroy with all those golden curles.

Caryl: That is quite amazing.

Seymour: I have a photo of it. Well anyway, it meant that without any personality who am I, what am I, none of that, but my father did a most extraordinary thing, which came in extremely handy in later life, particularly at Fordham. He said I want you to study the Bible. This was when I was about five or six, I taught myself to read with matchsticks, alphabet cards and toothpicks, but I did not think anything of that, and I said why dad, or pa (we did not say dad in those years). I want you to know what kind of nonsense people believe and to know it thoroughly. I said, "think it's useful?" He said, "It has also many good stories." All right and I learned the Bible. So, many many years later with my memory when I was a professor at Fordham, I astonished my Jesuit friends by knowing the Bible better than they did. Now, I will skip high school.

Frank: There must have been more than divine providence behind that bit of advice.

Seymour: It astonished them. And they would say have you studied it lately for Fordham, no I just remembered what I did in childhood. Even, I was beginning to be aware that I had a truly extraordinary memory, but he insisted I also learn the Old and New Testament, but really he didn't order me, he just suggested. I didn't ever take kindly to orders until I met Frank. I knew this sincerely. Now, Frank was a very decided influence on my life. I was quite undisciplined because everything came easy. I went to an elite high school, boys, that was easy, but when I hit City College, I hit that most famous generation in the history of higher education. City College in the **1930s** (as is well known) has churned out more scientists, professional people than anything comparable, any university, anywhere in the World. I did not think of myself as extraordinary in any way and because things came easy, I found myself on probation the first year. Barely survived. I couldn't get by on memory. Well, I learned how to study a bit and I had a superb friend from high school, one of the finest minds I have ever known. He is a physician today. We keep up with each other. I had a state scholarship and I went to Cornell and I think I had the most extraordinary luck of any scientist, except Caryl and Frank that I have ever known. My mentors were wonderful. Professor Reed, for whom my son was named, he sized me up much better than I knew myself. He was writing a textbook and from time to time, he would say, will you do me a favor and look up this or that. Look up *euglena*, I can't understand it and then I met Professor (James) Sumner[10], later a Nobel laureate for the discovery that enzymes were proteins. He didn't order me. He said I am having trouble with catalase. Do you know what? I told I know of a man in Paris who has a bug that likes pure haem. I can write him for it and that man was André Lwoff[11] and where I felt down right stupid was first my botany class in City College under a Joseph Copeland[12], a weedy youth, just beginning to work for his doctorate who later became President of City University and his knowledge of botany for his age, I had never seen the like of it and he took us on field trips and since there were pretty girls in the class, he saw to it that we went up to our necks in swamps. That kind of thing. He had a devilish sense of humor that way, that bad, but he knows the plant that we had to get to . But anyway I thought I did poorly and in competition with my generation, I thought I was if anything average. I had not realized that I was in a cluster of the most brilliant people, Avery Lee Barr became Head of Pathology, acting Dean at Yale Medical. Cabot became, as you know, professor, a great immunologist as he is today. Until I really felt myself was quite extraordinary, but now I have to correct your word brilliant. About six or seven years ago, I was giving a talk at the New York Botanical Garden, I asked Sir Joe Copeland retired from the Presidency, to be there. He had followed my career quite avidly and lent me books on algae in German. So, I learned German casually, quite casually, and in the conversation and Margarita[13] was sitting by the rear of the table I said Joe, I only thought there are two puzzles in my life now and I think back that's one reason I ask you. I felt I did rather poorly in your class. Also, I had never understood why Barbara McClintock[14] took so much interest in me because I thought I had done poorly in her class. He had a blank face. He said "Joe did you know her?" "Yes". "Know her well?" "Yes". He intimated though this has to be censored because it might embarrass Barbara if she had asked him if he was married. He was the one man probably who would have been her match temperamentally. "What did you say to her? " Joe knew he was cornered. I said "keep an eye on Seymour Hutner. He is something special", and then I learned from another when I asked Joe considers that in the 40 years of teaching, I was his favorite student. Then I asked why? What did Joe said then? What he saw he wanted, he went right forward and the hell with the consequences. So it must be that Bernard Nabel, whom I did not know, had gotten to know me from Barbara McClintock.

Caryl: That's what did happen. I know. Bernard Nabel got the message from Barbara to us about you. That's how we headed for you.

Seymour: Well, the department that had the lab had very wicked people and they were the ones who had termed Barbara as unemployable.

Caryl: Several people did.

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[10] James B. Sumner, born 1887, died 1955. Biochemist and 1946 Nobel laureate with Northrop & Stanley.
[11] André Michel Lwoff, born 1902, died 1994. Bacteriologist and 1965 Nobel laureate in Physiology or Medicine.
[12] Joseph J. Copeland, born 1907, died 1990. Botanist who became City University of NY faculty member in 1928.
[13] Margarita Hutner. Biologist and faculty member at Columbia University. Seymour Hutner's second wife.
[14] Barbara McClintock, born 1902, died 1992. Geneticist and 1983 Nobel laureate in Physiology or Medicine.

Seymour: And one by one, I picked them off to my best ability like the Count of Monte Christo when I became a staffer.

Caryl: Good work.

Seymour: So much so that Bob Morrison **(**at a public meeting where I chaired, my major professor was retiring) in public, offered Margarita and I between us take our pick of five departments, including the old botany department, to rebuild it from the ground up. That is a Count of Monte Christo!

Caryl: Sure enough.

Seymour: Well I couldn't do the young man's job because Bob forgot that he had showed me by example that to give away $20 million a year is extraordinary hard work, he was forever traveling. He didn't use resumes. He visited people. But now I will have to correct what you saw.

END OF REEL 1 SECTION 2

Seymour: Why I accepted the job on the spot. I was really an outcast in that department and it wasn't my personality. These were vicious people but there was one professor who gave me space E. F. Hopkins. They got rid of him on the grounds he was a poor teacher. He was one of the finest I have known in my life. He was a challenge to them. He was the one who discovered the essentiality of manganese. Later on, I was able to help secure a job for him in Florida; he died happy.

Frank: But are you talking about the Biology Department at Cornell?

Seymour: No, this is Botany, it was entirely distinct. When I came back…

Frank: All right.

Seymour: There was no communication.

Frank: What group was it that you....

Seymour: Plant physiology.

Frank: Plant physiology.

Seymour: They had done a good work. They had done excellent work on orchids. And now you forgot to mention you hold plant patent number one x-raying the regal lilly on the roof of G. E.

Caryl: There is a painting of that lilly somewhere.

Seymour: Yeah. While, there is some moral to this. I would work at night, because I could not discuss my work with anybody. I really couldn't. I just felt I was in the presence of pure evil. I took their courses and despised them, but kept my mouth tight shut. And I saw them destroying people. They had a Hindu student, they wrecked his career sneering at his manners. And, they had a young Abe Lincoln who they were trying to destroy because he could not write. But he was magic with his fingers. But he was a young, Abe Lincoln without sense of humor, physically and mentally, but he could not write. And I wrote his master's thesis one day, when he was in the deep trouble and then I realized (that was the first time I realized) that I had a versatility in writing that was extraordinary. He said that he had not seen anything like it. Are your experiments okay? Yes, let me see them. All right. Well, I will let you. But anyway, I knew I had to get out. But now they tracked me down to that lab and I had a home made fractionating column and I said this green algae, *euglena* needs something only found in animal materials. I cannot talk to anybody here about it. And Frank and Caryl had my notebook. I said here is my notebook and see for yourself and they went through it, page by page by page, and quizzed me about it. What was my evidence. Now fortunately I had had pure plant proteins and pure animal proteins. I had purified animal protein like casine given to me by Professor Sumner. I just mentioned that I was doing something. They were the first people to take me seriously as a researcher, the first, and they quizzed me, did I know what I was doing? I had to remember that homemade fractionating column, which I made myself. Well, so the word I used in that biographical sketch is I was *parachuted* into the physics. I met Frank and for the first time I learned I had to meet deadlines. I had written my doctorate thesis in ten afternoons casually, and it had to be read. I would shelter in the bacteriology and return from, but I knew then that I was an extraordinary writer and extraordinary memory, but I did not think my intelligence rated brilliant but I really didn't. But when I came to Frank I had to learn to discipline myself to be on that I learned another lesson. I thought fern spores were **a** good bet. And so I had the MIT machine shop build a big light cabinet and promptly Roy Whelden[15] talked me out of it. In Roy, I met the utter quintessence of a Yankee.

Caryl: That's true.

Seymour: And, I loved him and of course unasked. And Professor Reed was a Yankee too. But between the two I have acquired a grasp of the New England character. The best side, possible of it. And Roy took me in hand and showed me those fungus spores. Well I struggled with it, while they were struggling with the apparatus. And I can now tell you what was one of the highest points of my life. First of all, when Frank says, "get away from there, you damn fool" when I touch some high voltage. Later I found the equivalent when Abraham Lincoln went to the parapet when nearly six miles away from….

Frank: Washington?

Seymour: Washington. And the young captain, Oliver Wendell Holmes says, "get down you damn fool" and you know what Lincoln's remark was, do you recall it?

Caryl: Yes, I do.

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[15] Haskins Laboratories employment records show Roy M. Whelden as having begun his association with the Laboratories in 1934.

Seymour: "There's a man who knows how to talk to civilians." But there was one another incident. So I felt that I had never met anybody as hard bitten as Frank in my life. I mean, of goodwill and hard bitten, and that was a new combination. You were more peripheral. Frank had the day-to-day stuff.

Caryl: Oh sure.

Seymour: I was polishing my technique and Frank criticized that the results were little irregular – and do you remember why it was, Frank? Do you remember? My technique had become so good that the aga plate with the spores showed a bit of a spiral, like a DNA. It was the exact projection of the filament. Do you remember that?

Frank: Yeah.

Seymour: I said nothing. Frank told me we will put in a jiggle factor.

Frank: Giggle factor.

Seymour: I had proved myself. That was a truly high moment in my life. I had disciplined myself to tackle the problem. Frank was the disciplinarian. I had never really had to be that… Even when I was on probation initially, I just turned the ratchet up a knotch or two and made myself mostly an English major where I knew I could get enough A's effortlessly to carry through the exams that I wanted. Well, I learned then that my mind was unique among physicists. I dropped in casually in those afternoon teas at 4 o'clock, 20 minutes, and I met people like Norbert Winer[16] and all the physicists. And I discovered that none of them could it do what I could do. Like they could take Maxwell's equations or anything and derive all the things, but they could not remember 10,000 unconnected details. They could not pull them out and then I thought I must have inherited that from my father, the comic poet, to be able to think of lines of metaphors.

Caryl: The associations?

Seymour: And Norbert Winer would pop in about every other day with a new theory of immunology and I would just send it crashing, it'd be wrong. And then I found myself for the first time in demand. Evans would ask me for examination questions, how much does a mosquito weigh? People would ask me seriously when will we synthesize life, the incredible naivete that that ran through them all.

Caryl: Oh.

Seymour: Then I learned a lesson. Nobody at MIT had the slightest interest in what I had been doing at Cornell. As far as they were concerned, I had, in effect, faculty status. I was doing the work of a faculty member and I could conduct myself as a faculty member. And yet the department that had failed, so that I had to be plucked from the outside instead of hating me, gave me the warmest cooperation knowing it was their own doom for staying at MIT. Only one survived**:** Irwin

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[16] Norbert Winer, born 1894, died 1964. Mathematician at MIT. Coined the word "Cybernetics" and wrote several books with the word in their titles.

Pfizer the size of my handball shark who measured me my fall-wall handball (*indecipherable*). It was a first I had met a truly large-minded department. The most humane institution I had ever met, MIT, and that was the last place by its outside reputation one would have expected that. When I went back to Cornell, I had learnt that...

Frank: Maybe, Seymour that reflects the kind of man that Slater[17] was.

Seymour: Cate Slater was marvelous.

Frank: I knew him not well, but what I did know was that good.

Seymour: And the old timers had, like Nickerson, enough sense not to make waves. They had been discredited by the fact….

Frank: Slater brought in a new department.

Seymour: A new department and they were wonderful. Frank, Moss, I remember them because they would ask me man-to-man questions from biology and I would fail them but…. Well this as I meditated over that I now realize that they had paid me about as high a compliment as I have ever had in my life. I did the biology part of Frank's oral as well as some of the paragraphs and nobody thought that was extraordinary and the only one who really dove into me was Prescott[18], the Head of Biology, and it did him no good. I remember all that exam almost word for word and questions. Did I ever looked at it… hardly, and he was considered out of order by his colleagues.

Caryl: You mean, Prescott?

Frank: That's my understanding too.

Seymour: Another thing amused us all that should be mentioned. We had in effect enormous success, whose magnitude grew on me over the years. So they came up with a curriculum for biophysics and it was a composite of Caryl, Frank and me. We began to add up the credits. It would have taken about eight to nine years to get through.

Frank: A fairly parochial view of the situation, Seymour.

Seymour: Well. It was composite of our backgrounds and the requirements the cross requirements. It was called curriculum 14 I think it was. I remember our amusement and would often very seriously. Well anyway, I went back to Cornell for my doctorate, but in the biology department, microbiology or bacteriology dairy industry, in the depth of the depression. I spent the summer at the University of Rochester, from MIT.

Frank: Probably, it was 36.

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[17] John C. Slater. On leave from Bell Telephone Laboratories in 1944, developed and promoted the concept of the Research Laboratory of Electronics (RLE) that ultimately evolved from the earlier Radiation Laboratory at MIT.
[18] Samuel C. Prescott. Head of the Department of Biology at MIT from 1921 to 1942.

Seymour: 36 and I went with Dave Gotten at Rochester and we found ourselves complete temperamental opposites. We truly wound up with hatred between us. He tended to be authoritarian. I tended to be skeptical. He was very mathematical, that bored me. He was proudest of his circular slide rule. I said what results are you getting, and we then wound up in tight-lip silence, but he drove me back to Cornell and did not; we stayed that way till the end of his life. But I learned the lesson.

END OF REEL 1 SECTION 3

Seymour: But I learned the lesson. I told nobody whatever at Cornell what I had done at MIT. There was no way of bridging it. I never told anybody. I got my doctorate there by working on a blood poisoning strep of others using my chemical knowledge. In spare time, when waiting for the voltage to become steady, which meant usually late at night when people had switched off, I would wander down to the chemistry department and give some of them a hand with their experiments. I learned a tremendous amount of chemistry, but it taught me how bad the chemistry at Cornell was on theory. Well there was a deep philosophic difference between Frank and Caryl's approach to it that was essentially statistical, they were looking for the sensitive volume of radiation. Frank was a little bit peeved because I did not mention that in the sketch in the cytology book last year and I concentrated on what I construed to be a more striking and contemporary thing. Could you use photoelectrons to anticipate what million volt x-rays would accomplish? And there we succeeded I think.

Caryl: That of course was implicit.

Seymour: Yes, but not the sensitive volume. I can now tell you the fact volume never had a chance, but mitochondrial DNA might well be as important as the nuclear DNA.

Caryl: Right, I think it is true.

Seymour: There have been too little. There would have been one sensitive volume for the nucleus and another for the mitochondria and the results would have been...

Caryl: Actually you might have several sensitive volumes in terms of gene groups.

Seymour: And in the membranous unidentified DNA wandering about.

Caryl: Sure. You remember that DNA wasn't invented at that time?

Seymour: And also nucleolus has DNA and why then bring up...

Frank: Well now, Seymour doesn't the history of this say that the people who were doing cellular work and radiation damage to it (x-rays, ultraviolet, and so on things that you get in) believed that the nucleus, because you could see it divide, was the essential element?

Seymour: Yes.

Frank: And there must be something there that was...

Caryl: That was the essence of it.

Seymour: Yes. But that was….

Frank: That was the belief.

Caryl: That was the belief at the time. You are right. It is quite true.

Seymour: But then I asked further as a biologist. All right, you have got that information so what. It took Hershey[19] and Chase[20] a while later, and this wasn't intuitive on my part, a different approach altogether and I thought we couldn't bridge that castle. I had fulfilled my obligation, but I felt the future would be with that not statistical for a while, but pure reductionism in chemical terms. And that to me, well the unbridgeable gap between us that had nothing to do with temperament. I left profoundly grateful to Aaron Enzmann who became almost a companion. His family took me in and I became a sort of alternate father to his children that were running wild. Duncan…

Caryl: Well, still are.

Seymour: And for all their forbearance because I could not, anyway I looked at experimentation be more different. I wanted hard biochemistry, which I picked up by myself pretty much. Still the war was coming it was inevitable and I stayed in touch with one memorable visit that I remember extremely well, the Rooseveldian visit I think of it, the Schenectady branch of Sagamore Hill[21], because I think of it now. And it really, I remember their forbearance with me. Frank, I have got to get this written and I have got to get this report in and I had to be kicked and shoved. And I think Frank must have been quite often at the limit of his patience, but maybe white lipped with fury, but not in my presence. So anyway I learned to get it done somehow. First time in my life, I really had a demanding boss. Well, I learned the lesson when I got to Cornell I got my thesis done like that. No problem.

Frank: I am beginning to learn that there are virtues in being an ogre.

Seymour: I am the product of your ogrehood. We stayed in touch. And with war coming, I was in a very bad place – technically very good, and spiritually extremely bad. The Division of Laboratories and Research of the State Health Department, it was a vicious place, comparable where the headman ground up people and destroyed them.

Frank: Was it the man or was it the system?

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[19] Alfred D. Hershey, born 1908, died 1997.Was the 1969 Nobel laureate in Physiology and Medicine shared with Delbruck and Luria.
[20] Martha Chase, born 1928, died 2003. With Hershey, performed the 1952 experiment showing that DNA rather than protein was the material of heredity.
[21] The home of Theodore Rooseveldt, 26th President of the United States Now a National Historic Site.

Seymour: The man. He used the system. The system could be used for great good or great evil.

Frank: It was a civil service-type system.

Seymour: Yes, utterly inappropriate for that place. Well, I was one of the two strongest people I thought they would turn into alcoholics. The women were turned into lesbians. I was on the carpet for preferring to associate with the technicians who were untouched by this rather than the staff who were walking wounded. My recreations with Reina[22] would be to row up and down the Hudson. We'd go to the headwater at Troy, or all the way down and we knew every bit of the terrain or hiking. Reina said those were the happiest years for her.

Frank: Now, you and Reina met at M.I.T.

Seymour: Oh, oh I used to sleep on her desk, because she would work late with her graduate students and I had to be awakened when Frank would say that he thought...the line was ready or the apparatus was outgassed. That the… we are getting the hard clicks from the motor, which I remember that sound extremely well.

Frank: With cycle pumps**.**

Seymour: Old pumps.

Frank: Cycle pump.

Seymour: A cycle pump. And she would be working late and often alone. I would stretch out on her desk and go to sleep. So now I have to interject something else. I now realize that my marriage to Reina was organized by the Physics Department. They saw to it that we were filmed together.

Frank: When were you married?

Seymour: 1940.

Frank: 1940, okay.

Seymour: She was subject to fits of depression. Her family knew it and they knew it. And I was the most cheerful one around and they saw that I was temperamentally on a very even keel. They had sized me up and somehow, although I later learned she had had six proposals of marriage from eligible people. We were thrown together and couldn't too much to be coincidence, like a cloud chamber. And I think I had the background she wanted that way, English, literature, and all the things that could have been neglected in her upbringing. It was a very happy marriage, you know, very happy. We learned…

Frank: Where were you working at that time when you were married? Were you at Albany?

[22] Reina Hutner, Seymour Hutner's first wife and mother of his son Reed.

Seymour: In the vet school. I had gone back to Cornell and she had a job working in the department store. There were no jobs for young physicists. And we married, when I found I had a job teaching.

Frank: Where was she working at this point? Was she still in Boston?.

Seymour: She was still at MIT doing odd jobs. She came over, we married on her $75 and I had a job in the offing. We married then.

Frank: And you lived in Boston at that time or in the Boston area.

Seymour: Okay, my chronology is a bit confused. That came later.

Frank: Sometime...

Seymour: Yeah. We lived at 342 Commonwealth Avenue… no, that came later. Wait a moment, wait a moment.

Caryl: I remember you were living in Albany, which is, I think, at which time I met Reina.

Seymour: We were married in Ithaca. The moment I knew I had a job in Albany. Now it comes back clearly. The job was secured for me by the veterinary people, postdoc. I got my doctorate. There were no jobs and I was… people thought I was an object of ridicule for saying that a green plant (in print) needed an unknown vitamin, only found in animals. I published it without authorization from the Botany people thinking I had nothing to lose. They would do their best to discredit me no matter what I did. I just submitted it to Germany, no American editor would accept it, but nobody had bothered to visit me as you had done, Frank. I did extremely well with the veterinary people. I was solving problems for them. I was the only protozoologist. I had troubles with infectious abortions and my faith in human nature, which had taken a rather bad beating both in that Botany - plant physiology, was quite restored by one incident which is vivid. We were outsiders. They simply offered hospitality and very little money. About three days after we were married, we trudged up to the lab as usual and we discovered that the faculty wives had organized a wonderful party for us. They were under no obligation and the warmth I experienced, we both experienced, gave us sort of an emotional capital which we drew in the Albany days, because by that time, we knew the difference how to diagnose a bad lab from a good one. No matter what the externals were.

END OF REEL 1 SECTION 4

Seymour: Well, with war imminent, the story becomes familiar that I was invited to rejoin the lab.

Frank: You were in Boston at that time.

Seymour: I was teaching in Boston.

Caryl: And you were teaching at, oh, a College?

Seymour: at Middlesex[23], the forerunner of (*unintelligible*), I had good people and again my faith in human nature was reinforced in a very striking way. I continued research I had begun in the vet school on two nasty diseases and I had promised the vet school if I ever got it. They were wrecking havoc with the animals at Cornell. I had solved trichomonosis for them. Then, I had done extremely well in that nitty-gritty pathology course. They invited me to stay on to become a professor if I would take a year or so of that work. I loved them but I was willing to gamble and Margarita was willing to gamble, which was quite a stunt, I had a class of 74 students there and some of them were brilliant**.**

Frank: Middlesex?

Seymour: Middlesex. I liked it even though it looked terrible. I had a free hand and I was fresh from the state health department and I knew every public health procedure and I taught it to them as a young MD assistant. They vaccinated each other, they tested each other, and they loved it and I dropped everything.

Frank: They lived?

Seymour: Oh, two of them were hospitalized. Vaccination, they all loved it. Of course, then that became a class exercise. I teamed them up.

Frank: You do not need an exam to quiz on that.

Seymour: I was very proud. The students inquired around and found that I was giving the best medical microbiology class in Boston, better than Tufts, better than Harvard, better than Boston U. They deeply appreciated it. As the semester ended a committee of them called on me and said Professor, we know that you have given up your research to teach us. We are very appreciative. We have an easy schedule, can we help you? Ah! And they did. That was a Listeria "**aerosysteflicks**" paper, which led to the vaccine against **aerosysteflicks**, which was marketed by Lederle which was introduction to learn. You know I never told you that.

Frank: There were strings underneath the world.

Seymour: The students whom I thought I had tortured and brow beat formed a committee and so we turned out a really first class paper. And they didn't ask for credit.

Frank: Beautiful.

Seymour: Well, now I will skip to many many years later, my brother encountered one of my ex-students as a fellow physician and he couldn't refrain from asking well how was my little brother, and I got the best recommendation I have ever heard of a teacher in a practical subject again. "He was a son of a bitch, but he sure knew his stuff."

Frank: Our, son of a bitch, all right.

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[23] Possibly Middlesex Community College in Springs Road, Bedford MA.

Seymour: Oh, I will just say. I had the temperament of a naturalist to pick up all the details I could find about what I saw whether with geology or trees, I know every tree in Prospect Park, then I was reaching out for Central Park. I knew every grass or shrub I could identify. Any dents I tried to run down. I remember I struggled with blach leaves, and collioptera of Indiana. So, I had the temperament and I had gone rowing, my brother gave me a microscope. I got to know everything; 99% I could run down, thanks to the books that Copeland had lent me in German. Now, I can tell one more. I am thinking back about professor Reed who held court every afternoon at 4 o'clock in the basement that is where the trial began, and I would be there grinding coffee. We didn't have instant coffee or Martinsen's coffee, you ground your own coffee, and I was lost. And he was a terror because he gave the German exams. And he taught me something about dry New England humor and I have to tell them they are good stories although I've not been asked to give them. We had some very maidenly ladies on the faculty, Quaker ladies that is very gentle.

Frank: Shockable?

Seymour: Extraordinary, they took full advantage, extremely this is all dead-pan. I have to tell this story. I learned some New England humor from him, a lot of it. One of them said, Oh! professor Reed, I have been so impressed by Wiseman's experiment with the rats cutting of their tails for 60 generations and they were born without tails and so Professor Reed at the head of the table. This is straight out of Wendell Holmes "The Autocrat of the Breakfast Table" Exactly. And the book I wish I could get it, its out of print. He said, Oh! I thought it was stupid Huh! Unnecessary Huh! The Jews had carried out a similar experiment much larger with similar negative results. Wait! She didn't quite grasp it. That it's actually in Shakespeare. We all know that the fuse on edge of a story, we would wait and said, Hamlet, Act 2, scene 3, "there is a destiny(sic)which shapes our ends. Rough-hewn though they may be.". And then, I served the finest blood I have ever seen. He was wearing a rather red **Eleanor McDowell** and the blood rose as if it had been poured into them. With no chuckle, with the air of a serious seeker after wisdom and truth, a complete Fred Allen deadpan. A Jack Benny, deadpan, only there I saw a master… I think one more. So it's the same Quaker lady, Oh! Professor Reed, would you please tell us how you learned German so well. I don't know, please do, I recommended it. Huh. These are Womans Christian Temperance union and Quakers all superimposed. "When I knew that I would have to, I was going to take my doctoral work in Germany. I knew no German. Then I felt that I would get there early, get a place and learn German. I found a nice pension, but somewhat to my dismay instead of my learning German, they were learning English from me. And in an utter despair, I got into the habit of going through the beer garden every night and from playing with barmaids I found, I picked it up."

Frank: I love it.

Seymour: I joined the table after having ground-enough coffee beans, and he said Well (conversation had flagged for a moment) Well Hutner, read anything interesting lately? I said yes, I have been going through Erinberg's ………………..(*title in German*). I didn't think anything of it. Anything specially interesting, he said. Well the description of *euglena* is quite good. Can you remember? I quoted. And what else? Congratulations, you have just passed a German exam. My son is Reed, named for that professor. I learned that that was mid winter and his portrait was being presented and he did almost no research but people came from all over the world, filled that building, that he had inspired them to be great teachers and great researchers. So, I know that, these rare individuals, that don't do research but have….. And, then later I found words for it, somebody had written. Mediocrities can only recognize mediocrities, but talent can recognize genius, and that's what he had.

Caryl: That's a nice one.

Seymour: That's what he had. He would get them out so that he devoted a fair amount of attention to me, genius or not. What he did as a teaching device was to pretend to be writing a textbook with all kinds of charts. So, one day he said, I am having trouble with *euglena*, would you look it up for me. That's how it began, casually, not an order. Complete functional, casual offhand. Now, that to me, is an iron command, I am asked to help somebody who is deserving of help and some had pulled the same stunt. I am having trouble with, they had sized me up and then I learned later of course there was an informal pack of club in the faculty, of which Barbara was one of the members and they had me taped completely much better then I understood myself. Now, I think that when people ask me, how come you are so brilliant in this, I say, oh, knock it off, I had the best choice of mentors that anyone I have ever heard of ever had.

Caryl: But that doesn't come by happenstance either.

Seymour: Well, it passed along from Murchesson to Copeland, but the point is that it was all done so casually that I never caught on. Well, I am back in this area now.

Pat: You have brought us up to 1940 at this point, is that right?

Seymour: Well, shall I go on a bit?

Caryl: Maybe, we have to go back Frank and get him from 1935.

Seymour: We had these makeshift quarters in Grand Central and they were terrible because it belonged to the era in which the only use for electric power was one bulb per room, you remember? There are no sinks to speak of and so we looked for……. Frank and Caryl found a beat-up aloft building, glad rag where they made cleaning rags and downstairs, neckties. They had a freight elevator. And that was converted into a lab.

Caryl: Frank discovered that.

Frank: Well, actually that was Derrick Gallagher's[24] work.

Caryl: I guess it was, yeah.

Frank: How is your tape supply, Pat?

[24] No employment record for Derrick Gallagher has been found in the personnel archive. An employment card for his wife, Anne, shows that her association with the Laboratories began in 1937.

Pat: Ample I think at this point. Would you like a break at this point?

Frank: It would be a good point for a break.

END OF REEL 1 SECTION 5

Pat: We are here again, I think that it's your turn Frank.

Frank: My turn, well, I will start with childhood as the other two friends have. I was a farm boy, raised on a farm for as long as I can remember, since my father and mother were separated. Well, I was still quite young and I went back to her home to live with her parents, my grandparents.

Caryl: It was their farm, was it Frank?

Frank: Well, it was my grandfather's farm, 220 acres of it in Central Illinois, about 200 miles South of Chicago. My grandfather was a respected figure in the neighborhood. He had been away to college for, I think, most of one year at Miriam College, which was 20 miles away, and was about the only college man in the community. So, that when it came time for the old settlers' picnic, and things like that. He was one of the people who helped organize it, and sort of a senior figure around the community. Well liked, well respected, only moderately successful as a farmer for reasons I do not fully understand. But anyhow, I had a very pleasant happy childhood growing up and roaming in woods, trapping when I got a little bit older. Eventually, the gun I could go hunting with and all the things you read about in books for farm boys. A great deal of time with myself, a reasonably good library in the house for those times and those circumstances, old but highly diverse, I suppose a couple of hundred volumes, which was quite a lot, and I read all of it. School was of essentially uneventful. I had very good teachers in the fifth and sixth grades who kept me in line and taught me things and a very poor teacher in the seventh grade, who was good looking and uninterested in teaching and unable to teach, but who ran a successful box dinner to raise money and bought a new library full of books, which I then proceeded to read. So, while she was not teaching, I was educating myself to pretty good advantage in terms of breadth of reading. In the eighth grade, I had an ogre as a teacher, and I mean an ogre, but a darn good teacher. So, I got some discipline and some education out of that; the combination was pretty good. When I say ogre, I mean that I once glanced out the road when a car went by and backfired and it was definitely against the rules to lookout the window and I stood by my desk for the rest of the afternoon. So, you can see that he was a disciplinarian. He typically did not do much beating, but he used switches occasionally, and to good effect. So, I guess it was also when I decided to become a physicist or at least I discovered that I had an interest in things, mechanical and electrical. I came across in my grandmother's home a textbook on natural philosophy that had been my father's physics textbook and proceeded to swallow it and enjoy it very much indeed. Then, I can remember still taking books of that general ilk to the fields with me so that on the rest stops, I'd have a few minutes to read. This was probably along the high school days rather than grade school, but I annoyed my eighth grade teacher a little bit. He loaned me his physics book, high school book, and I read it. And I thought that I had him pinned down one day on a non-physics problem, but it occurred to me as something that even he probably didn't know the answer too. I asked him, "how do worms breathe?" He performed the simple act of pointing to his nose and that was it. I do not know that I believed it, but it was a fitting response to an unfitting question. So, he had a sense of humor as well. High school was seven miles out in the country from Palestine, three miles from Flat Rock, which was the town to which we typically went to sell our farm merchandize and to buy food groceries, things like that and to the local church. I was a Bible student too but for rather different reasons. I could remember a number of times taking the buggy once a week to deliver our grandmother's butter to selected, chosen customers around the town – good weather and poor weather alike. But back to high school, that was the situation. There were two boys about my own age in the neighborhood**:** Royal Buchanan, who was a close friend, and Raymond Holder, who really did not go to high school until later I guess at least. Royal's family had a small Ford Roadster, the kind that jumped puddles. We helped to buy the gas for it and I rode, two of us rode together in their car to high school back and forth seven miles. Four over dirt roads and three over gravel and in the wintertime, the dirt roads were something. One time while coming back, his father had to come and met us at the end of the gravel with the team of horses and dragging the car through the mud roads broke the double tree, so the mud was there. High school was for me a good experience though. It was my first time really to get contact with a lot of new ideas and people who were interested in teaching. The one room country school is an educative process, but not intellectually particularly. I had a dedicated science teacher who tried to interest me in Biology and did not succeed; I knew as much physics as she did, so that of course was easy and chemistry was all right. It was a small high school, small laboratories, and so on, but in general it was a good experience. At the end, well skip that, the shaping influences, I think you said.

Pat: I think that's really what we want. To try and see how you got together and what kind of people you were?

Frank: The fact that I was interested in science anyhow and regardless of what the difficulties were, I was bound to do what I could with it, I learned electricity, for example, by illegally (and surreptitiously) plugging into the taillight of the family automobile and getting hold of the six-volt battery's electrical supply for my experiments. Fortunately, I did no great damage.

END OF REEL 1 SECTION 6

Frank: Well, this is after the interruption and I guess I was, where ?

Caryl: You were through high school, I think.

Frank: Through high school. Alright. I suppose an English teacher was as formative a character in my life there as anybody else and tried to teach me good literature primarily nothing else much, except a little bit of writing and after high school, I wanted to go to the University of Illinois, but it was not at all sure that that could happen even though I had a scholarship to go on. There is a certain element of education involved there. During the high school year, while I was a senior, I had taken the county exams. At that time, Illinois had a scholarship system that each county was allocated one full tuition scholarship to the university based on the outcome of a competitive examination held in the county seat sometime in the early spring, in the senior year for the people.

Caryl: You took an examination first?

Frank: I took an examination for this. It was an all-day written exam and came off number one. So, I had the scholarship to go to the university, but I didn't have the necessary money to do anything about it. So it was a hard decision, but I finally decided that what I would have to do would be to stay on the farm, teach school for a year, which was the common way of earning funds for people of that age and lack of experience and I managed to get a school-teaching job for seven months, I guess it was for I believe $70 or $80 a month mostly, teaching a one-room country school. It was only three or four miles from home, so I could either ride horseback or walk, which I did most frequently and live at home. No problems there. Not very many students, nine I think to start with and two promptly dropped out, for which I was thankful. It was as grim an experience as I have been through in all of my life, but I lived through it and...

Caryl: Grim, because the students were noncooperative?

Frank: It was I didn't enjoy it. The students, five of them, were from one family and a well-to-do family. And one of the five, on the first day at school, I started asking, what's your name? And the little chap gave me his name. I said, "what?" He gave it again. I couldn't understand him. So his next older brother tried and the next brother, and finally I got it from the fourth one. It was that kind of situation. No intellectual. No drive for schooling at all. The other two were pretty good. One of them was a fairly good to average kind of student. But, in general, it was just a hellish experience. I was too young. I didn't manage. The discipline problem got out of hand and it was just a bad experience all the way through. The next summer, I spent not on the farm, but managed to go to Chicago and get a job with the Commonwealth Edison Company through the intervention of a friend in the Chicago area with whom I stayed. That was a good summer by and large. I saw much of the loop district of Chicago instead of eating lunches and managed to do the job satisfactorily. It was not a very inspiring job there was a card file of meters in all the substations that had gotten too dog-eared to be used and they needed them to be copied onto new cards with an old Olivetti typewriter. It went clunk, clunk, clunk.

Caryl: I remember the Olivetti.

Frank: This one was a special card-writing typewriter and worse than standard form. Nevertheless, it was a very interesting experience. And for a country boy immersed in the city it was extremely stimulating. I saw the first sound movies and George Bernard Shaw walking down to his garden gate and declaim during that summer. That would have been the summer of '27, I guess. I went to the university that fall. During the summer I had gone back to Robinson (the County Seat) to retake the examination against all odds because the brightest lad they had at the **Robinson** High School was scheduled to get the scholarship, but he didn't, and the total scholarship was important in those days, it was worth a total of $210 over a four-year period.

Caryl: You mean, total four years or $210 per year.

Frank: $210 total. $10 matriculation, $50 per year, $25 each semester, tuition for a resident to be sure. So, it would have been more for an outsider. But, in those days that was a good deal of money. I graduated in '31, which was, I guess, in the middle of the depression. Wasn't it?

Caryl: Yeah. Very much so.

Frank: Very much so. No, wait a minute. We were talking about college. Let me get my dates straight. High school graduation '26. Worked the winter '26-'27, college '27-'31. So, I got out of college in the middle of the Depression and had no chance for a job, but my university record was good enough, not high honors, but honors to get me an offer of a teaching fellowship at the Physics Department. I had gone in as an electrical engineer, but decided that the foundry course was a bit grimier than I was interested in and did not have as much science involved. So, I shifted to engineering physics and graduated in engineering physics. A pretty good old fashioned physics department at that time, though they had a new man coming in who livened things up very considerably in the same way that Slater did it at MIT and a lively new professor came in while I was about the end of my freshman year. Well, about this time, I got into graduate work. So, I spent the years '27 to '31 at graduate school, '31 to '34 in physics. First year or two, I was teaching as a laboratory assistant in the optics course. I had done a good job in the optics lab and got that kind of plum for my teaching assistantship. The third year there I was given the chance of getting some teaching experience and taught under Floyd Watson[25] who was as grand a man as I have ever known. The physics for doctors two physics 201AMP, I think something of that sort, anyhow it was the dumb down course for the MD premeds who had to have a physics course, and they hated it. I taught the what-you-call sessions. Watson gave the lectures and there were quiz sections and laboratory sections. I had one or two quiz sections and one or two laboratories. I don't remember a number of the details. I do remember one thing about it that I shared their frustration with a course that had no conceivable relationship to medicine from the point of a young premed student and why should they be taking it in the first place. I thought there probably were reasons because I liked physics and thought it was interesting. So, I took some of the better students who would have made good grades in the course anyhow. I said, " look, if you want to skip these dummy experiments, you may do so provided you will read and give me a brief report on some subset of the following references." And I tried to pick out, went to some trouble to pick out, examples of articles, which showed how physics had been put to use in interpreting muscular motion and a variety of other things including radiation physics, biological effects of x-rays, and biological effects of fast electrons. One of the authors that I decided on, it was a paper just out from the GE by Caryl Haskins and some other people. I don't know I remember, on the effects of electrons that can be used essentially in x-ray tube with a beryllium window across the end of it. So, that you fire out through the window and radiate the seeds and what not. This was just one of several papers, but I found it interesting and thought that they would, and some did**.** I nearly got my ears pinned back for this departure from teaching orthodoxy, but nevertheless, I got away with it. I had a chance to work for the last year at least (while he was there) with Kruger[26] who was back from a National Science Fellowship, NRC fellowship, in Germany. And he had brought back with him a freshly made vacuum spectrograph. This was in the hay day of spectroscopy when people were looking at the emission and absorption lines of spectra to see what the external parts of an atom were like. And I worked with him quite closely in setting up the instrumentation one summer and running it all the following winter. He was a good teacher. It was a hands on with proctor experience and one of the best learning experiences I have ever had. I nearly got my head blown off. We had the 100 kV and a huge condenser bank driving the vacuum spark and I was pretty sleepy one night and got close enough where I felt my hair going like this and slightly backed off,

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[25] Floyd R. Watson. Professor of Physics at the University of Illinois. A pioneer in the field of architectural acoustics.
[26] P. Gerald Kruger. Built, in 1936, the world's third cyclotron and the first to have an external beam.

but, besides the system was driven with a motorized interrupter, that went click, click, click, click, click. We had to run it at night for several reasons; one was that only in the middle of the night could you get enough temperature stability that the thing would stay in focus. And, also it generated enough high-frequency energy through all the wiring in the physics building nothing else would work at the same time.

END OF REEL 1 SECTION 7

Frank: The night hours were compulsory. Well, Kruger was planning to go to MIT in the summer of ‘34, I guess, and he offered to pick me up, part way if I wanted, so I hitchhiked out to Schenectady to be there when he was there, as he was stopping in to talk with Charleton whom he had known and I do not know in quite what context, maybe, he had worked with Charleton[27], I don’t know, at one time. But anyhow the General Electrical Company had given him most of the high-voltage transformers and electrifiers and what not for the system that we had been installing and I had been trying to get a job with the General Electrical Company since I was out of high school. I remember (I don’t think that I still have that) a letter I wrote to them just out of high school asking for a summer job. I got back their typical blue stationary with a very nice little letter, I think, from Dr. Whitney saying that they appreciated my interest and so on, but unfortunately they do not think my experience would quite meet their needs. This seemed like a good chance to see the laboratories and meet the people, and indeed it was. He went on to MIT to spend the summer and I rode across the Mohawk Trail with him and his wife to MIT and spent most of the time helping out that summer in the work he was doing on using the MIT equipment to measure up and reduce spectrographic data that I had been taking and that gave me a chance to talk with some of the people there and I decided that I would like to be there. And I wrote to them (I guess with Kruger’s blessing all right) asking for an assistantship and to my surprise, got it. So, I moved out to, hitchhiked out in the summer of ‘34 and spent ‘34-‘35, ‘35-‘36 at MIT, in the physics department, but I am afraid I went perhaps under the false impression that I would continue to be a graduate student slave in spectroscopy. But I’d had enough of spectroscopy. They had a new man, Robby Evans[28] coming in from the West Coast with a good reputation in medical radiology. Studying how do you measure the radium content of the bones of people who had radium poisoning and he was setting up his rebuilt equipment at MIT and I had the job of helping him do it. That was the first year. It was quite an assembly of stuff. He wanted it to run automatically because you had to run the thing all night for essentially a week in order to boil the nitric acid extraction from the bones to get the radium out. It was automatic all right, if all the relays worked. There were about 50 relays, telephone-type relays, cross-connected in ways that you wouldn’t believe and did not always work. That was partly my fault because I had used soldered paste in doing the soldering and the paste, if it gets in between the terminals makes a moderately good conductor. So, there were a lot of hidden short-circuits. Well, the thing wasn’t working appreciably by the time I had applied for and was very lucky to get a summer job with Charleton for the summer of ‘35 in GE. But Evans was not happy about the equipment not being ready to go, so he bore down on me to come back weekends and work on it. That was how I happened to be inquiring about how I could…, (somebody mentioned that I could to go, and they said, “ well there is a chap here

[27] E. E. Charleton A scientist in General Electric, he was a consultant in much high level planning and administration leading to the development of a 100MEV betatron during the period 1942-48.
[28] Robley D. Evans was a graduate student of Robert Millikan at Caltech. He joined the MIT Physics faculty late in 1934 and conducted research into the medical uses of radium.

who regularly goes over weekends, maybe you could ride with him instead of hitchhiking. I could not afford it any other way). So, I bummed a ride with Caryl Haskins, I figured that there was something fishy when we got to Albany and he stopped for a couple of milk cans – what turned out to be fish literally. Then it began to dawn on me that maybe this is the Haskins that I have given this paper to. So, I asked him, he said, "Ya it was", which led into a conversation about the biological effects of electrons and so on and to our mutual appreciation of the fact, that (I think not a view commonly held) that if you wanted to probe a cell, x-rays were not the way to do it, because they go through everything and you don't know what you have hit. If you could do it with electrons, you could do it more nearly selectively at least you could go in known penetrations and that it was feasible to do this, in all probability with something if it would live in vacuum and maybe this could be done. I think in fact you had an old paper from some German Laboratory, an attempt to do this ...

Caryl: Yeah. They used bacteria or something like that.

Frank: It was a poorly conceived and poorly executed experiment, but it was on the same the idea. The idea of bombarding with electrons was there. I think the intent of using it as a tool to explain x-ray radiation effects was not implicit. It was something you and I generated in that conversation you referred to on the way to Boston. Well, that trip was repeated several times during the summer and the conversations elaborated a thesis problem that Caryl said well now, if we can convince Evans that you could change your thesis for this and if we can find somebody to do the biological work, maybe I can get it done at Harvard and something like this, it might be a feasible thing to try. I had been planning to do a thesis on, what is the name of the people who made the counter tubes, the tube-type counters, a little cylinder, wires strung down in the middle….

Pat: Geiger?

Frank: The mechanism of Geiger Counters, and that was less challenging certainly than to try to bombard living organisms with electrons. Evans was willing. Evans was an odd man in many ways. He was good to me and bad to me, and I do not know how the balance came out. I am afraid I was not a very grateful student at the time I left. He was hard driving for his own purposes, but a good teacher and permissive as a thesis supervisor. I think he dropped into the room once or twice and asked how are things going and that was the total content of his supervision. On the other hand, I did a lot of work for him. He, not very much later, published a large book on radioactivity with a number of drawings in it, which I made using my drafting skills from the engineering school. It was sort of extra work, but I learnt a good deal of nuclear physics, well it was called, I guess, Atomic Physics at that time, and it put me on to an interest in that field. Which had interesting consequences later on. This is up to '35, '36. That years' thesis work Seymour has explained some of the….., it was done under forced drive. It was a sizable undertaking and it did not really, like any piece of physical experimentation, you work, work and work building, aligning and setting up and debugging the equipment. And the experiments go like "click", "click", "click" and you are through, but we didn't get through until just a few weeks before the thesis was due. Then came interpreting the data, writing the thesis and getting it in, which, by virtue of many all-night sessions, got done just under the wire. It was important for me to do it because just before I left the University of Illinois in 1934, I met Edith, whom I had known as a girl I had dated earlier on and then she went off to teach school, but came back to visit and we had a date, a pleasant evening and she drove off into the fog and ran head on into somebody else and got hospitalized and this made me realize (what I had not quite realized before) that I really cared for this woman. Then we were engaged by the time I went to MIT. She taught school for part of that first year and then came out and joined me in February. Then we rented an apartment in February '36, – '35 I guess it was.

END OF REEL 1 SECTION 8

Frank: I think you were in that apartment, you came to visit us there.

Caryl: No

Frank: May be not

Caryl: I think first in your house in Schenectady….

Frank: That was before. You met Edith in Schenectady. We already had been married some months at that time. And we made do, but with my $80 a month from MIT and a little bit that she managed to earn on some odd jobs we managed to live, but we did not make much headway on paying off my educational debts, her educational debts, and several medical bills that she had run up at this time. And I was going through our correspondence recently and found recently lots and lots, lots and lots of letters to people saying, " we really mean to pay that bill, but it will be a little while yet, would $2 help" – that general sort of thing. They were lean years, but good years. Summer of '35, I had written for and got a summer job at GE and Edith came out by bus and I hitchhiked. In the first week we had a total of 13 cents between us to get to that first pay check, but Edith was very persuasive with the grocery store and we lived on credit long enough to make it. The work with Charleton that summer and it was on the basis of that summer's work that I was invited to come back for job at the end of my degree work. It was on designing a high voltage generator based on the Cockcroft[29] and Walton[30] circuits. It was a paper type job, but I guess it was a fair test of whether I could do an imaginative job of extrapolating engineering data and seeing whether it was feasible or not. I came off with the conclusion that it probably wasn't. At least nobody ever did it. So perhaps that was the right answer. By the time, I got back, they were busy building a resonance transformer for about 3 million volts. Well they still had the old 1 million volt prototype out in the back room. I am jumping back again, now I guess to the thesis days. I had broken off by saying that it was a good thing that we managed to get that thesis under the wire, because I did have a job to go to and if I had not finished the thesis, I would not have had a post at MIT, I would not have had very good prospect for job later on and I would have had a wife and no income.

Caryl: Well fine.

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[29] John D. Cockcroft, born 1897, died 1967. Nobel laureate in Physics awarded jointly with E. Walton in 1951 for the construction of apparatus for generating 800kV and used in the first proton accelerator.
[30] Ernest T.S. Walton, born 1903, died 1995. Nobel laureate in Physics shared with Cockcroft.

Frank: Well you got the thesis in. That is the only, that is the only answer. So, I have mixed feelings about those last few months at MIT and if I was a bit of an ogre, it was because the devil was writing in bed.

Seymour: Frank, I thoroughly understood that you, all of us were under that pressure to get done.

Caryl: Pretty high pressure. Still remember your picture remember that. Picture of the group.

Seymour: Yes the group.

Frank: And, we looked as if we were under pressure. I have one recollection there that I think you probably share, the faculty lounge for the Physics Department was just down the hall from my fourth floor room. And it was forbidden for it to be used other than for faculty purposes. But, it was the only soft place to sleep in the whole area. And every once in a while, the guard would catch one of us there. He learned to be tolerant.

Seymour: I had Reina's desk.

Caryl: You had Reina's desk.

Seymour: She had a soft desk.

Frank: It did not happen very often.

Seymour: And she was working on her thesis.

Caryl: So, she was not occupying the desk at that point of time.

Seymour: That little bit of alcohol like indentation…..

Frank: Well let's see back to

Caryl: I almost remember physically how those paragraphs shaped up in the thesis. I did this paragraph… I did this paragraph! You did this paragraph

Frank: It was a composite. Reasonably well received, wouldn't you say

Caryl: Oh, very, I think very well, very well.

Frank: Except by Prescott, whose biology department had been left out and whose nose was therefore out of joint.

Caryl: I think it is fair to say Frank, that it was a watershed in MIT biophysics.

Seymour: It was.

Frank: Well it was the first, it was the first thing

Caryl: Pfizer told me lot about it, but I never bothered. Till it was evident from the way that they submitted that whole curriculum for…. Pfizer had been a fellow grad student with me in Harvard.

Frank: Well, I would like to, since Caryl and Seymour have both talked at some length about the earliest Schenectady days, skip ahead to the end of that period. That period for me is the summer of '36 to the summer of '39 during which time I was working on high voltage insulation and with Charleton in the GE research labs and becoming increasingly interested in the possibilities of using nuclear methods to get at radiation therapy problems. A paper that finally resulted in some papers that Paul Zahl[31] and I published in ***Radiology***. With what is to my knowledge the first published reference to a practical use for U-235, there may be others, but I do not happen to know them – in December 1941. But my work at GE was mainly putting high-voltage rectifiers in oil tanks and finding out why they broke down and taking them back out of the oil tanks.

Caryl: As I remember, developing a method that was never picked up by GE.

Seymour: Sulphur hexafluoride?

Caryl: No.

Frank: No. Well, I guess may be that, if you want the shaping of a research career, that had a lot do to with it because that was a very educational experience that bore on what happened in the laboratories few years later. You see, I had come through the University of Illinois with one good teacher who had taken me by the hand and said, "let's build some equipment to do an experiment." A hard driving man, but a good one and very fair. I had had an experience of being somewhat used, but allowed to go my own way at MIT in a totally new environment, but both of these were academic environments where you picture a problem within the department's area, pretty much, I had violated that for the work that Caryl and I had set up to do in biophysics. That was tolerated in the physics department, but not more than that. And then I hit into an industrial laboratory, at that time one of the best in the country – in the world – and a totally different ethos. You were told what the problem was and if this is the problem to be solved, and never mind all the interesting bylines and never mind what it takes to solve it, that's problem, go to it, and my assignment had two parts. It was really pretty simple. It was to repeat an experiment that had recently been published in ***Comptes Rendu*** by somebody who had noticed that when he spilled a bottle carbon tet (*carbon tetrachloride*) into his static machine, he got much higher voltages than he had ever gotten before. Now, is that interesting? Well Dr Coolidge and Dr Charleton thought yeh it might be interesting we'll try it. So, repeat the experiment. That was the formal assignment. The informal assignment was to go down to the basement and find the old static machine, that the company had put down there sometime before, which turned out to be a wooden cabinet larger than that conference table over there but enclosed so that you could contain vapors in it. And then come back up to Charleton's workspace and find a place to put it but unfortunately it had to go inside the high voltage cage and the high voltage cage was completely and I mean but completely full of past experiments some of which had been interlocked by the wooden benches on which they had

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[31] Paul A. Zahl, born 1919, died 1985. Scientist and author of several books describing his travels. He joined the Laboratories in 1936 and personnel records show him to have transferred to part-time status in 1958.

been put through the rungs of other wooden benches. My job was to disassemble this, save what needed to be saved for historical reasons and then try this experiment of putting carbon tetrachloride in the static machine. Well it did increase and the next question of course is why, and what would do it better. That led to some interactions with some of the people in the chemistry, although by and large they were not altogether that helpful, a lot of literature work and very quickly the idea that if you were going to use this it would have its application not in that context exactly but in terms of finding a compressed gas that would be better than compressed air – which was already a pretty good insulator and was being used fairly extensively.

END OF REEL 1 SECTION 9

Frank: And that led to as intensive a search of chemical compounds, mainly inorganics as I have been through for exotics and knew that they had to be low boiling point, high vapor pressure, preferably nontoxic and because you then put them out in general circulation, not too corrosive, and somehow available. Well, I did get one good suggestion from... Well, I zeroed in myself on the carbon fluorine compounds. Carbon tetrafluoride is not a very good insulator though and many of the other compounds were at that point unavailable. There was only one company in the country that dealt with fluorine and making compounds from it and they made them primarily as refrigerants for the Westinghouse Company. In order to get some CCL_2F_2, the company bought a Westinghouse refrigerator, and drained the refrigerant out for me. That's how I got my materials. Some of the others had to be bought. And then the chap who was stone-deaf, a chemist?

Caryl: Oh yes. Starts with a B, I think.

Frank: What ever his name was. Said well, have you tried fluorine compounds, they are pretty corrosive, but some are quite stable. What about sulfur hexafluoride, which is very stable and sure enough sulfur hexafluoride when I finally managed to get some, it had to be made specially and had little small two-pound, two of it for a couple of hundred dollars, I guess, turned out to be a very good insulator, which had almost all the properties desired and in fact was used, so far as I know might still be used, as a high voltage high pressure insulator. About your chemist, there was one anecdote that I think I will have to get onto the tape. Caryl introduced me because he knew me by this time, we were well into his interests and mine. He was still at the laboratories that first summer I was there. He took me up and introduced me. When we got to the door, the man was sitting across his laboratory in his little office and then Caryl did a whisper, "he is stone-deaf".

Caryl: He was the one incidentally who worked with selenium compounds for baldness, remember? Among other things.

Frank: I remember that.

Caryl: Norstad, doesn't begin with B, something like Nostrum or Nostad, I think.

Frank: You are probably right. I don't think I would recognize it even if I heard it, but I will never forgot that he was stone-deaf, as I was cautioned.

Seymour: I am often reminded of Frank's rep because I am on a program in October in Albany on what to do to get the PCB's out of the Hudson River.

Frank: Yes that's right. That was in the hay day of PCB's they were being used for condenser cans and so on.

Caryl: I was maintained for two months in Washington by Whitney to go through the patent files. I went through the patent files and missed all the patents. At least I got good canoeing in the river.

Frank: Well, the GE experience was one in focussing on the problem not on the particular background and discipline that you have been trained in. I was never trained in inorganic chemistry or high voltage pressures or anything of that sort, but that's what you had to do, solve the problem. That carried over into the Laboratories' philosophy very considerably. Alright, I have here some notes about it. I will pass the copies around you so that you can go along with me. These are for dates that came off of , or maybe better let me have that, off Edith's scrapbook of clippings in that strange Haskins' way and the experiences there. In 02/1937, Caryl Haskins was appointed as research professor at Union College and will move Haskins Laboratories into the physics annex as soon as renovations are completed. So, I presumed that – there was a long newspaper article about that – I presume that dates the move to Union College was at about the middle of this period. My recollection of your laboratories in the garage goes back to summer of '35 or early fall of '35, or maybe even, I think you have probably still had it intact in '36.

Caryl: Yeah. When we moved into that building.

Frank: But this Union College building was a four-room affair, two stories, two rooms downstairs, two upstairs, with its back against a woodsy area. That was the point, I think at which you were feeling your way into building a personal laboratory and I don't know quite how the origins of that go because that…

Caryl: Well that goes back to these conversations with Coolidge and Loomis[32].

Frank: Coolidge and Loomis. Right. So, you had tried to do that in part by retaining space and providing employment for Enzmann[33] at Harvard.

Caryl: That all sort-of grew up out of the thesis material.

Frank: Later, Whelden.

Caryl: Yeah.

Frank: And a year later, '37 I guess, Zahl.

Caryl: Yeah. Right.

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[32] Alfred L. Loomis, born 1887, died 1975. A financier who founded a private scientific laboratory at Tuxedo Park, NJ and had a major role in establishing and operating the Radiation Laboratory at MIT during WW II.
[33] Haskins Laboratories' personnel records show that E. V. Enzmann was employed in 1935 and retired in 1948.

Frank: And then, Union College came through with this good space close to home.

Caryl: Right in the middle of that, you know, but we retained the MIT and Harvard space.

Frank: Retained the MIT and Harvard space and started moving into Union College. Well, you had your office in the right hand room downstairs. I don't remember this room. Upstairs, There were Derrick Gallagher was recruited at some point, and I don't know exactly when, to come in and build an up-to-date version….

Caryl: Something of a replica of yours.

Frank: Of the electron raying experiment at MIT to carry on that kind of work and Paul (*Zahl*) undertook with you to do some monomolecular film work.

Caryl: That is right.

Frank: Which was Langmuir's …

Caryl: Encouraged by Langmuir.

Frank: …great interest at that particular moment.

Pat: These were oil films?

Caryl: Monomolecular fatty acid films.

Pat: On a water base.

Caryl: On a water base, the kind of thing that Langmuir was doing.

Frank: The real trick is to get a clean water surface from which to start.

Caryl: Then he had of course all sorts of paper bridges, paper balances, and so on to measure forces under compression.

Frank: Paul was a free spirit who went wandering off and the entry from May '38 says PAZ (*Paul A Zahl*) reports 3000 foot waterfall.

Caryl: Which got into the movie news, remember. What was that movie news at that time?

Frank: Movietone, I guess. Anyhow, it was for a few months the highest waterfall in the world. He had gone off to Venezuela, …

Caryl: Paul's birthday, I think.

Frank: …inspired by the lost world.

Caryl: Pretty much by the lost world and some marvelous photographs he got on the top of it. I also might add that I learned that Paul got lost for quite a long period there and did not show up anywhere, but a letter from him to me came through with a dollar bill still attached to pay the postage!

Seymour: And he wrote a book about it.

Caryl: And he wrote a book about it, to the lost world.

Frank: I think he made a second trip down there, didn't he?

Caryl: Oh yes, he made a couple actually.

Frank: Yeah. Here is another entry from March 1939**,** a picture essay on Paul Zahl and the giant ants that he brought back, all those Schenectady Union stars.

Seymour: Did they get loose?

Caryl: It took me three years before I could get any ant permits after that one.

Frank: There was a feature story about "Paul Zahl and the lost world" – Alice has transcribed it here as "The *last* world."

END OF REEL 1 SECTION 10

Caryl: This is daily breakthrough, routine breakthroughs, routine daily breakthrough. Anyway, back to Frank.

Frank: I think that is the end of that particular bit of chronology. The structure of the Laboratories at about 1938 – the winter of '38 and so on – was essentially that you had two groups working in Boston, one at Harvard with Enzmann. Zahl by that time had moved to Schenectady, I think, and was living in the house with his Great Dane

Caryl: With his Great Dane.

Frank: At MIT, the electron bombardment work had been carried forward a year by Otto Morningstar who got his thesis on it, I believe, and went off to take a job. Did Charlie Buchwald come in to carry on that job or did he come to Schenectady? He came into the picture someplace along…

Caryl: I think he came to Schenectady to work on the Schenectady accelerator.

Frank: Maybe, Alice gave me a set of….

Seymour: Oh, I am sure he did because he bought a house in Schenectady I think at that point.

Frank: Yes, I guess it was Schenectady and yes that's what I am looking for.

Seymour: It's his wife who was the really famous one.

Caryl: She was something.

Frank: She got skipped.

Seymour: Running all the daily routine of the department all out of her head, effortlessly.

Caryl: You're thinking of?

Seymour: Christine Buchwald. Do you remember Christine?

Frank: I think, you got her mixed up.

Caryl: I think you got her mixed up.

Seymour: No not Anne Gallagher[34], Christine Buchwald handled personal affairs of the students. She was a sort of an auxiliary secretary while she was married to him and would find apartments….

Frank: You got the people all mixed up Seymour. Christine was secretary of the physics department.

Seymour: Yes, yes

Frank: While I was a student there. Her father married us at his home in Belmont.

Caryl: Oh, for goodness sake.

Frank: She and – whose name doesn't come to me at the moment – who lived in the same apartment house on Harvard Street that we did in the 1935-36 academic year. We were married some time shortly after that and he was at MIT running the John Trump, wasn't it? No I've got my stories mixed too.

Caryl: No, John Trump was a governor.

Frank: But she married a chap from MIT and whether the name was Buchwald or not, I cannot at the moment remember, but it is not the Charlie Buchwald that Caryl and I are talking about, it was a quite different person.

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[34] Haskins Laboratories' employment records show that Anne Gallagher was hired in 1937 and retired in 1970 when the Laboratories moved to 270 Crown Street, New Haven, Connecticut.

Caryl: No, I do not remember much about Mrs. Charlie Buchwald, except she was in the Schenectady area.

Seymour: I remember her very well.

Caryl: Sure, her name is Buchwald.

Frank: Well, back to the Laboratories. So, at MIT, had George Scott come by that time?

Caryl: He came to do the proton accelerator work at MIT.

Frank: Right.

Caryl: That was after you in were Schenectady. I think.

Frank. That was after I was in Schenectady. All of this was after I was….

Caryl: That is right. I think he overlapped on Morningstar a little bit.

Frank: He may have. I don't think he was there more than one year or there were two or three years to be accounted for there. At some place along the line, 1938 or 1939, George Scott, who is covered in, your write up, I think, came in to do the same kind of job on biological materials that Caryl and I had started with electrons, but do it with protons. This was obviously motivated by the growing interest in the neutron in nuclear physics.

Caryl: Actually, then at Berkley, the first cyclotron or the second cyclotron was starting up.

Frank: Was starting up, and you remember that you were in fact offered by somebody the money to build a cyclotron.

Caryl: That was Strauss, Lewis Strauss[35].

Frank: University of Rochester.

Seymour: That one.

Frank: That's interesting. He turned up in this bomb book, you remember.

Caryl: Very much. He turned up. I saw him quite a bit in Washington subsequently.

[35] Lewis L. Strauss, born 1896, died 1974. A financier, who had a major role in the formulation of US nuclear policy and who quarreled with J. Robert Oppenheimer over the development of the hydrogen bomb. He was a member of the U.S. Atomic Energy Commission 1946-50 and its Chairman 1953-58. However, the lack of a known link to the University of Rochester suggests the possibility that a different Lewis, or Louis, Strauss was being referred to.

Frank: Alright and then there was the MIT, Harvard, Union College then Derrick Gallagher had come in about 1938, I think, I don't recall for sure, shortly after the Union College facility became available, to redo the electron bombardment work.

Caryl: And Charlie Buchwald.

Frank: Charlie came later I think, I believe so. And you needed a personal secretary by that time and she came in and worked in a room downstairs, which was your office, as a personal secretary and eventually took over the lab accounts and a whole lot of things and eventually took over running the Laboratories.

Caryl: Pretty much.

Frank: She was quite a girl. For that organization, at that time, she played essentially the same role that Alice (Alice Dadourian[36]) plays here.

Caryl: Yes, very close and very much similar in lot of ways.

Frank: Very similar in a lot of ways, temperamentally very different.

Caryl: Yes.

Seymour: Very beautiful.

Caryl: Very beautiful and very wonderful person.

Frank: Very wonderful person. They had become interested in color photography and had played around with color printing. I remember their little house out in Scotia, I think it was Edith and I, had gone out to visit them for dinner some time and Derrick showed me his developing color prints, how to process them with the dye offset I think, which is a God awful process, but he was gung-ho on color pictures. Had visited this little company in lower Manhattan, ***Lerochrome,*** and discovered that the man was quite an inventor, but one of these restless spirits, who was always moving on to something else and had a good camera, but who was uninterested in making a business out of it. And the philosophy of the newly forming Laboratories was that if it were to grow it would have to find some means of providing income other than what Caryl could provide because he had provided out of his own funds the necessary salaries at MIT and Harvard and to Union College. So, it looked as if perhaps, the general pattern for doing this (Caryl and I discussed it at great length) would involve something like the ***Arthur D. Little***[37] pattern of a consulting laboratory where you try to pull together a small pool of compatible people who could do the things they wanted to do research wise (and thought where interesting exciting problems) part-time and go out and help people solve their industrial problems another part of the time, the monies

[36] Alice Dadourian joined the Laboratories in 1968 and served as Franklin Cooper's secretary and continued in the role of the Laboratories anchor person until her retirement in 1995.
[37] An industrial consulting and research organization. ADL born 1863, died 1935 was a chemist and industry advocate who founded the consulting company in 1886.

from which could help to keep the whole operation going. That as I remember it was the philosophy that had evolved about that time.

Caryl: We had a long discussion with Jim White[38], you might remember, on that.

Frank: Right. Who was inclined to be a little more worldly-wise than we were too.

Caryl: Definitely.

Frank: But willing to go along with these eager young beavers.

Caryl: And believed in the lab.

Frank: Believed in the lab, believed in the people, I think.

Caryl: Oh yes, very much.

Frank: Definitely and was willing to be a party to it.

Pat: May I ask you at this point. Was the practice of setting up small private laboratories unique at that time or was it, let us say, as common as setting up communes in the 1960s?

Frank: Much more nearly the former.

Caryl: Much more nearly the former. In fact, the only case I know was the Loomis experiment.

Seymour: Well, I can comment on my reading about why the Edison Lab failed, because ours was the first essentially democratic lab, Edison's was authoritarian and he was mean-spirited. Ours was the first lab with a Willis-Whitney tradition that recognized people in the lab as more than tools. And that was unique. Edison had put by adroit publicity his imprint on his lab that it was all Edison and so it collapsed on his death completely, but the lesson had been learned by **Kettering** not to run the things that way, Kettering had learned. But these were large-scale labs.

Frank: These were industrial labs.

Seymour: And so Haskins was the first small scale that you might say carried an academic equivalent of the Kettering Group of Dayton.

Caryl: And also it borrowed a lot from the Loomis one although the Loomis one differed.

Seymour: Yes. You might have mentioned somewhere that it was the birthplace of supersonics.

Caryl: It was also the original radiation lab.

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[38] James White was a lawyer and member of Haskins Laboratories' Board of Directors. He resigned his Directorship in 19…

Frank: Well, it was a gentleman's past time that also wasn't it?

Caryl: In a way, although Loomis was so intense.

Frank: He was a very intense person and he worked in it. And he was also very active on the national scene.

Caryl: Of course, this was before that.

Frank: Of course this was before that, but I mean.

Caryl: Later on, he was critical at….

Frank: I was trying to link some of the roots of this back to the English tradition of the gentleman scientist.

Caryl: That may have been true with Loomis while he was in business too.

Frank: I think it may have been.

Seymour: Spiritually, it went back to Cavendish[39].

Caryl: Yes, sure.

Frank: Yes, to some extent, your own roots were in that tradition.

Caryl: Yes, I think so.

Frank: When I first met you, you were a very hard-driven …...

Caryl: Puritan?

Frank: Well, puritan in that sense, i.e. one didn't deserve the things one had. One had to go out and earn them even if he had them. So, here was the young man who had, I knew, he had plenty of money, but I didn't know how much, and I didn't care to know. He had enough to do things he wanted to do and what was strange and interesting to me was that he didn't want just go live it up, he wanted to do something. He wasn't taking the soft way out. He was harder on himself than he would have been, I think, had he not had the funds to do otherwise and I think that's correct.

Seymour: But, I think, another cultural strain entered that your account made me think about. Teddy Roosevelt made a big thing about the strenuous life that doesn't have to justify itself. That

[39] Henry Cavendish, born the son of the 2nd Duke of Devonshire in 1731, was a wealthy eccentric and a scientist who became famous for work on the composition of gasses and the mean density of the earth. He died in 1810. William Cavendish, the 7th Duke of Devonshire, endowed a scientific laboratory in Cambridge, England in the year 1870.

you could also have fun while doing it but you, in the end, had to justify your wealth to the community at large, and he hammered away at that point.

Frank: Yes, he did and that was very much in the air.

Seymour: That was very much in the air. That must have, I thought, influenced you.

Caryl: Well of course, I came to know the family, though somewhat later.

Frank: But one of the topics that Caryl and I talked about early on and we had many long talks, was his own inner grapplings with the problem of whether he should stay with the General Electric and be a wage-earning member of society, if I can put it that way, or whether he was free to do what he was able to do because his parents had left him some money and what he wanted to do and what he believed it worth doing, but it was so much against the conventional pattern that he dassent (*dare not*).

Caryl: It was Frank who took me over the watershed on that, I well remember it, the most important piece of professional advice I ever had in my life.

Seymour: I came in with another stream altogether different. My entire generation was weaned on Arrowsmith[40] and microbundles – of the lone researcher which was based on Pasteur. And that stream was rich and full and I hit it at its utter peak and it did inspire us.

Caryl: It was still carrying from the Pasteur one if you will. It is interesting the way they all converged.

Seymour: I say to people I had it lucky, I lived the life of Arrowsmith really, but I didn't have to retreat to the woods of Vermont. I have a gorgeous lab at the edge of the financial district.

Caryl: But, society has changed though.

Seymour: In the last few months there's a very brilliant young MD with the Arrowsmith characteristics except that he has a sense of humor, Steve Neshnic, and he got into exactly that kind of conflict in the Cornell medical facility. We all called him kid Arrowsmith and he knows exactly …, but that was a such a...

Caryl: Ya, that was very strong and remember the old Vallery-Radot[41] "Life of Pasteur" that came out at that same time.

Seymour: Oh yes, it inspired my generation. We have all read it. And his experiment that you went across fields, that you took a pair of tweezers and separate the, L and D Rochelle salts and

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[40] "Arrowsmith" is the title of a satiric novel by Sinclair Lewis published in 1925. It describes the conflicting pressures upon a struggling physician to make money or to pursue a life of scientific research – in Vermont.
[41] René Vallery-Radot "The Life of Pasteur", Garden City Publishing Co. Inc., Garden City, NY. An English translation published in 1926.

then you spoke before the Academy of Sciences, and said, after a vicious attack by the MDs, "I am only a chemist." We learned that all.

Frank: Yes, indeed, tartaric acid.

Seymour: Yes, tartaric acid, potassium and sodium tartarate and that was not part of Frank's background or Caryl's. I think, you were insulated from it by the sheer day-to-day busyness of the Lab.

Caryl: Well, I had got into the Pasteur one earlier on because of the old Garden (*Garland?*) chemistry awards thing I'd got into. They featured the Pasteur very heavily of course.

Seymour: But also, Caryl is there more, that you might say, and Frank, about the character of Willis Whitney.

Caryl: Well, probably Frank could.

Frank: He was leaving GE, as I came.

Caryl: Yes, to Yale.

Frank: My own recollection of Willis Whitney is that he drove an automobile that had a hood that stuck out seven or eight feet in front of the windshield, very long old Pierce Arrow or some thing of the sort and he had, being the inventor that he was, a little right angled mirror mounting where the radiator cap usually went so that he could look around corners.

Seymour: Let me interject myself how I learnt how to meld all these things. I may never have told you about it. I was a principal, one of these speakers, at a big conference organized by boss Kettering at Worcester College on trace elements, I remember, and there we were, he was sitting right there in the middle of the front row and I had, I was telling about B-12 and cobalt and the philosophy of it and I was about third or fourth on the program and the biochemist just taught biochemistry and he was fighting, I could see, fidgeting, then I got up and I realized that he was unhappy, he didn't understand a lot of it. I switched gears and threw away my manuscript and gave the essence. For every point I used an automobile or motor metaphor, I reached in to say my father's poet reaching. I did it all in analogies and automotive and he perked up and brightened, at last, profoundly respected by the other people there. I thought nothing of it, but then we had a wonderful house farm wives lunch and he seated himself next to me, asked about the Lab, I explained that began industrially, but a sort of an academic tinge here and there, and he said, I have an institute, how would you like to be the director, here at the Yellow Springs, at Antioch[42] in the photosynthesis lab and intimated, at last he found some one he could talk to, and that was so … I thought what was barely…I thought here is this man being essentially snubbed.

Caryl: Yes, isolated.

Seymour: Isolated.

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[42] Antioch College 795 Livermore Street, Yellow Springs, OH 45387-1697.

Frank: Well, we got the story almost up to the point where the Laboratories as a coherent enterprise as distinct from a set of groups working on slightly related problems and supported by an individual scattered around universities took shape, because that taking shape happened at the formative stages of the discussion of it, I recall, were from around December 1938 up until, it actually got underway by mid 1939. Derrick Gallagher figured in it very prominently because it looked as if his ***National Photocolor Corporation*** would be the industrial probe, that we would try to use as a money maker to support the other research and that could be…

Frank: Are you rushing off Seymour?

Caryl: Are you coming back?

Frank: Coming back, okay.

Seymour: I will be right back.

Frank: I remember personally very well the evening with Caryl at our house and he and I had a long talk and he asked me if I would be willing to leave GE and join the Laboratories.

Caryl: I remember that evening too.

Frank: And I decided that was what I want to do and it was a gamble worth taking.

Pat: It was a gamble?

Caryl: It was a gamble indeed.

Frank: Because I had a job in one of the best laboratories in the country, doing the kind of thing I liked doing, but this sounded….. I'll say one other thing that's interesting, I think in a broader way about my motivations there, that they were perhaps colored.

Caryl: Yes, he is.

Frank: Pat, do you want to shutdown just a minute? Well, that evening's conversation of course set my life pattern. Oh, yes, I was mentioning one aspect of my experience at GE that, it would not have led to a decision, but that made it more attractive. I had wanted to go to the Physical Society meetings, I guess it was in New York, and was told kindly and correctly, I think, from the company's point of view that. "Well, we don't send our people unless they are giving papers and also we don't…," I said, well, may I take time and go and pay my own expenses? "Well, no we like to pick the people who will be seen there as representing the company and so on. We don't encourage our people to go to these meetings unless they are presenting a paper that is in the company." Well, that made the cage very obvious and I decided that it was a nice cell, but it was a cell and I had noticed even before that that there was nothing, no particular progress that had ever been made that didn't originate at GE. I had been finding leads to high volt insulation and had had to read a lot of the German literature and there were some very interesting things going on there, but you wouldn't know it from anything you read or heard about it in General Electric.

Caryl: Yes, that's intensely interesting, as I got the same impression.

Frank: It was very parochial and enforcedly so, so that said this is not…. I had an earlier experience of the same general kind in the summer between high school and college when I worked in the Commonwealth Edison Company in Chicago. It was a nice, soft job and I had very pleasant people with me. They had been with the company for essentially all their lives and would be for the rest and were going nowhere. I realized that there are organizations into which you could become a cog, but from which you do not emerge as an individual, ever.

Caryl: I think this may be endemic to some of the big industrial laboratories because…

Frank: I think, it is probably necessary.

Caryl: It's probably necessary.

Frank: Yeah.

Caryl: Even though, because I remember being so impressed one time talking about this sort of thing with Bill Baker[43] and his saying, "well you know, we give them the right problems to do and when these people stray away we give them the fixed stare."

Seymour: And Newan shocked me and others by saying that working outside of 9-5 was discouraged, and I thought GE had no future.

Caryl: Well that was true too, you punched a time clock.

Seymour: A few, but they did not perfect.

Caryl: And that only because of Langmuir.

Seymour: And they were affected by finances. I thought, under no circumstances, will I ever buy GE stock.

Caryl: You've got something there.

Frank: Yeah, so this was the chance to do something I wanted to do and enjoyed doing with a free enterprising spirit with an open horizon instead of a closed one, however, pleasant. That was my personal motivation. Well, the formation of the Laboratories went fairly rapidly from that point.

Caryl: That was the key.

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[43] William R. G. Baker, born 1892, died 1960. Began working for GE in 1916 on radio for military and commercial applications. He became President of the IRE and was instrumental in creating the NTSC television standard.

Frank: The purchase of Lerochrome, its renaming as ***Technochrome*** and then we got into a hassle with the Technicolor people, about names, so we had to change it to National Photocolor Corporation. Derry and Anne, as I recall it, moved down to New York in the spring of 1939.

Caryl: 1939. I think that's right.

Frank: I helped him look around for, but he selected the space at Grand Central Palace, 480 Lexington for the National Photocolor Corporation. It was perhaps 800 square feet of essentially office space.

Caryl: Office space and long and narrow.

Frank: Long and narrow.

Seymour: Marble flooring.

Caryl: Yeah.

Frank: It was the Laboratories. We had one long workbench.

Caryl: From the corridor.

Frank: And a small office at the end and there was a wider one for a studio and a workshop at the back and Derry's office and he went to work redesigning the camera, building it. Paul Zahl came down and developed the pellicle mirrors, which are the heart of the device.

Caryl: The half-silvered mirror kind of thing.

Frank: And there he did a pretty good job of going to trade shows and selling a new camera and getting a business started. In fact, put up enough competition to the…., I've forgotten even the name of the other camera company now that was manufacturing one-shot 3-color cameras.

Caryl: One-shot cameras.

Frank: One shot cameras. I did have essential….

Seymour: Carbro? Was that the name of the company?

Frank: It wasn't. It may come back to me sometime, but not now. Anyhow, they had essentially a national monopoly on this very limited market and there he began cutting into it sufficiently, that they closed up the New York branch and concentrated their efforts on the West Coast and Derry managed to sublet the space that they were vacating in 305 East 43rd Street.

Caryl: So, that's how that first came.

Frank: Which was on the fourth floor.

Caryl: Seymour's old lab.

Frank: Seymour's old lab.

Seymour: And Paul's.

Caryl: And Paul.

Frank: That happened about in 1942, I think.

Caryl: I would have said a little earlier. One I think.

Seymour: End of '40 or.

Caryl: Because you went to Washington, and I did.

Frank: I know, but I have one recollection that pins us down and that is by that time both of us were working in Washington.

Caryl: Yeah. I went in 1940.

Frank: I was still commuting to the Laboratories and to my home in Hastings and I remember very well listening to the radio the evening of Pearl Harbor at the desk at 480 Lex.

Caryl: I remember that.

Frank: So, it was after December of '41.

Caryl: I can put another one in that too because we listened on the radio to Hitler's march into the Rhineland.

Seymour: Yes, the Rhineland really crystallized it in your mind. I think.

Caryl: Yeah, that's right. Listening to that in Schenectady and I think also ……

Frank: So, I think the move to 305 by NPC was around 1942.

Caryl: May be.

Frank: And was about the time that it became immediately obvious that color cameras had no future during war time and Derry very soon thereafter got himself a job at Western Electric.

Caryl: Except, remember the British Metal Box Company?

Frank: Well yes, but it was a small application and financially unrewarding.

Caryl: Definitely, unrewarding.

Seymour: Metal Box was a huge company despite that name.

Caryl: Remember, they did use our camera in skin transplants. And also at Yalta, we have got the pictures that were taken with our camera. I have got a print of the….

Seymour: One of my contributions to the war was to find an antiseptic growth for microscope lenses and (*indecipherable*) that would keep them from locking. It worked.

Caryl: That's right, for tropical use.

Seymour: It depended on my memory of fungicides. Do you remember them?

Frank: I did not remember that.

Seymour: Everything rotted in the cell.

Caryl: Yeah, I think I do recall.

Seymour: And Nick Langen[44] put it right to me. He had nuts and spoiled cameras and spoiled lenses were coming apart from particular fungi.

Caryl: They were etching them.

Seymour: Yeah and I forget, I think I had told them to use a little copper napsylate or something, it did the trick, that's when they took us seriously in that way.

Caryl: Probably, saved the whole lens supply.

Seymour: Yes, yes. I think it went into the aircraft and the aircraft lenses too.

Caryl: May have. May well have.

Frank: You were going to wind us off I think.

Pat: Yes, I think I will have to very quickly, but I just wanted to ask one last question. When the company bought the Lerochrome process, it was simply a process at that point. The development of the cameras came within in the period of Haskin's ownership. Is that correct?

Frank: No.

Caryl: No, there was a camera.

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[44] A card bearing the name of Nicholas Langen is present among the employee records of Haskins Laboratories but no starting and ending dates are available.

Frank: No, there was a camera called Lerochrome camera and the company got along with more goodwill than it needed to have bought, a few of the old cameras. But some of them were two-color separation and some three. The threes needed some redesigning, particularly for manufacture and Derry did most of this. It was a job working with the machine shops and new alloy work and so on.

Caryl: That is some new alloys.

Frank: He got that straightened out and a new camera too, a small size and a large size a larger 4 x 5 plate developed. But Leroy, who owned and operated the Lerochrome Company, had developed the process, had developed a way of making pellicle mirrors (but it didn't work for us) and had built some cameras. But it was the goodwill at most that we got out of that was not very much.

Caryl: The Geographic cut such a deep swath that at his death…(*This fragment from Caryl is part of a conversation with Seymour that erupted during Frank's monologue. The conversation appears to be about Paul Zahl's contribution to camera development. The following remarks made by Seymour relate to that thread of the narrative.*)

Seymour: I indulged in a soliloquy with Pat when he called me I couldn't (*indecipherable*). What made our Lab unique as a small lab that I have never seen its counterpart, (may be outside of group seven, Caltech or the like) is that we all had very strong personalities. We all had very different backgrounds.

Frank: And we all like to talk at the same time!

Seymour: And some how with color last.

Caryl: Absolutely.

Pat: Gentlemen, I am afraid I have to wind up.

Frank: No permanent clashes.

Pat: The time is now 12:45 and…

Frank: We used up a lot of the roll of tape.

Pat: …we must retire for lunch. I am sorry. If we could come back.

Seymour: I can.

Pat: Absolutely splendid.

Caryl: I may have a kind of a problem with that. I don't know….

Frank: I think may be.

BREAK

Pat: This is session 2 held on the 19th of August, now the time is 9:32 and Frank you are going to tell us something about…..

Frank: This is the 19th of August 1988 for the record.

Pat: Did I say 1980?

Frank: You didn't say any year.

Pat: Oh, I am sorry.

Frank: Very good, time goes on. I have just been going through documents that had dates except years and I find it very frustrating. Well, at session one, we had talked about getting the Laboratories assembled from its various places in Harvard, MIT, Schenectady, and localized in New York. The Harvard and MIT units were still operative, but they were, at this point, definitely outlined units and eventually dried up largely because of the war. The move to New York into 480 Lexington, the Grand Central Palace, was motivated by Derry Gallagher's interest in color photography and the purchase of the Lerochrome Company which had developed a camera for one-shot photography, beam splitting type camera. So, we, Derry, Caryl and I formed up a company, moved it into 480 Lexington with about two-thirds of the space given over to the camera company and the other third to setting up a small base laboratory in the Grand Central Palace. It was not long before we outgrew that space and moved the camera company to 305 East 43rd Street, which was a manufacturing loft building owned by a manufacturer of neckties. He had his own operations on the first floor and rented out the upper three floors to various people. The fourth floor was then rented to the camera company, whose name I cannot recall, that was the main competitor of National Photocolor Corporation in the one-shot camera business. Their main operation was on the West Coast and they wanted to move out. That left a well-furnished set of darkrooms, sinks, sales room, and what not, exactly suited to National Photocolor and it would leave the space which was very tight at Grand Central Palace. That move occurred in 1941, I think. So, the Laboratories then were left with rather more space in it, needed at Grand Central Palace, not particularly well suited to laboratory work and a related operation three or four blocks away at 305 East 43rd. The Laboratory was still at Grand Central Palace. I remember one incident, that is the Japanese attack on Pearl Harbor came on a Sunday that I was on my way to Washington, but stopped in at the Grand Central Palace between commuting trains and the Washington train and so I knew that we were there at that time at least. We were still there, I think, when Seymour came, but we were just in the process (this would now be a year or two later) of moving the Laboratories into a part of National Photocolor space primarily because National Photocolor was being put on ice for the duration. It didn't take very long after the end of the war before it became clear that the advertising business was itself on the shelf, that the cameras it used being made of aluminum and German lenses were not going to be producible in any case. Derry Gallagher, the main moving spirit in this enterprise got a war job and went off to work in vacuum tubes at Western

Electric. But in 1940, I guess it was, Caryl saw many of the, well we all saw the war coming, England of course was heavily involved by that time and was in the summer of 1940 that you went to England and you and Edith were married.

Caryl: That's right.

Frank: I think it was.

Caryl: I had been back and forth, of course.

Frank: You have been back and forth several times. You also realized that you yourself needed to be involved in the war effort in some way and were casting about for an appropriate point of attachment. That, I think, is a part of the story, which you want to pick up and carry on.

Caryl: Well, Edna and I were married in this country actually, engaged in Britain, moved and married in Virginia and honeymooned on the West Coast and moved directly into Washington. This would be now October 1940 and at that time, Bush was just beginning to set up the National Defense Research Committee. I went around to him and asked if there was a job available at P Street and he eventually said there was ….

Frank: He was Vannevar Bush?

Caryl: Vannevar Bush. That's right. I am sorry, Vannevar Bush, not the current Bush and so I went to work primarily as an Liaison officer assisting Carroll Wilson[45] and this would be in November, 1940. In the meantime, going up and commuting up and down to the labs on weekends and so on and we took an apartment in Tudor City in New York, by way of being close to the Lab, to that old building, which we still held on to and then simply lived around in hotel rooms in Washington during that period. That job for me lasted about year-and-a-half. Frank came down. Carroll Wilson became Vannevar Bush's primary assistant. Frank came down and we worked together in the Liaison office for a considerable period. Frank did a massive reorganization job in the Liaison office, as I recall and that became quite a large operation, because it was charged with keeping track of all the records of civilian evolved military research through the country and shortly after that, Roosevelt asked Vannevar Bush to set up a larger organization, which would include the committee on medical research to take care of medical matters and the old NDRC under the headship of Conant continued with its contractural work in the physical and chemical sciences. At about that point, we took two weeks at our little camp in the Adirondacks and I got a call in the grocery store while I was on the lake that Conant wanted to talk to me. So, I talked to Conant from the grocery store and Conant asked me over the phone if I would come in as his assistant and deputy in NDRC, which I said I would be glad to do. So, Frank took over as senior Liaison officer and continued in that post right through the war, I think.

Frank: Just about the end of the war.

Caryl: And I didn't see as much of that after because I worked with Conant, although we were all working still in P Street so that we were spatially fairly close. That leads up, I think, to the

[45] Carroll L. Wilson was a wartime assistant to Vannevar Bush. He later became the General Manager of the AEC.

beginning of the Lab's program because Bush had been in and out of the Lab several times in New York and he had known about it and we had talked about it, and he was quite familiar with it. So, when the decision was made at the very end of OSRD to let a series of contracts for rehabilitation work particularly for the war-blinded and deaf, Bush thought that the Laboratory might be a very appropriate central laboratory to do this work and talked about this with Newton Richards[46]**,** who was then the head of the committee on medical research. He was the Dean, you remember, of the Medical School in Pennsylvania. Result of that was that Richards set up a Committee on Medical Research under the chairmanship of George Corner[47].

Frank: Committee on Sensory Devices, Caryl.

Caryl: I am sorry, CSD. Committee on Sensory Devices. Corner then asked the laboratories to serve as a Central Research Body for the committee. Plus contracts, development contracts with ***Brush*** development…

Frank: Brush development, ***Hoover***…

Caryl: GE…

Frank: GE, I don't recall.

Alvin: ***RCA?***

Caryl: RCA.

Frank: RCA operated independently. I think, they took no contracts from us.

Caryl: Is that so?

Frank: They put their own money into it. It was ***Stromberg Carlson.***

Caryl: Stromberg Carlson. Yeah. So that was how that structure got started and it was at that point that I think it was just almost at that exact point now that you came first with the Lab. You remember, we started out with some sonar direction-finding devices including a bowling device.

Alvin: You had already done that before I came to the Lab. That was not being worked on any longer.

Frank: This may be an appropriate place to backup a bit on the Laboratories' history at 305 East 43rd.

Caryl: Right.

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[46] Alfred Newton Richards, born 1876, died 1966. Professor of Pharmacology at the University of Pennsylvania.
[47] George W. Corner M.D. Chairman, Dept. of Embryology, Carnegie Institution of Washington.

Frank: To which it moved into space there on the fourth floor, which was now largely unused because National Photocolor had been put on the shelf and Derry was gone. So the laboratories moved into the space with National Photocolor and set up a reception area out front, and an office, a series of work rooms that had been originally used for photographic development plates and so on in the back and then the general purpose area off to the left for mechanical work and camera assembly and what not. The Laboratories took over part of the office space and most of the area in the back all, but one darkroom, converted it into a so called chemistry laboratory, which is where Al first had a place to work, and into eventually an animal room and another room that Seymour used for microbiology. Now, at that point…

Caryl: Seymour was moving at that point and was away from you're the old radiation working into…??????????

Frank: Right, he had been away as he told us, I think in session one.

Caryl: Yes, he did.

Frank: And came to the Laboratories just about in 1942, and 1942 is my recollection of the time, which we moved up there, late 1942 or 1943. So that leaves the Laboratories and National Photocolor on the fourth floor of 305 East 43rd Street with Caryl in Washington most of the time, I was in Washington much of the time, at the Laboratories about a day or week at the most, and Seymour and Paul Zahl were the people doing experimental work there. We had Clara Bjerkness[48] as a laboratory assistant to Seymour and Paul and Anne Gallagher ran the show as secretary, treasurer, and general factotum. On the camera side, it was down to one or two people, Arnold Garnet, I remember well, a very competent and skillful Negro chap, who had spent much of his life in photographic work and I think, there was another chap who had worked on cameras for Derry, but he left us not long after that.

Caryl: Charlie Buchwald was entirely in Schenectady. Was he not?

Frank: I believe so. He came down while we were still in 480 Lexington briefly, but I think did not actually work there. I was working on a color meter, color temperature meter for photographic work at that time and he was going to take that over, but it did not work out that way. This thing eventually just died. Paul and Seymour worked fairly closely together, Paul in a continuation (in a way) of the work started at MIT on radiation work and carried on to some extent. By that time, it was clear that the radioactive materials… I have to start this over again. Do you remember Caryl whether the work that Paul was doing was on mouse cancer?

Caryl: That was done partly in collaboration, you remember, with Columbia.

Frank: That was done in collaboration with Columbia. These were implantable mammary tumors for mice and one of the things that was striking there was that the tumors, it's common knowledge I guess, developed a whole new vascular system around the tumor and it occurred to him or to us or perhaps in the general knowledge at the time, that if you get materials to be eaten up by the white cells, they would congregate where the activity was around the tumor. Now if those dilated

[48] Clara Bjerkness was an employee of the Laboratories from….

or otherwise engorged cells, could then be radiated with something that interacted with the engorgement. You could get localized radiation at precisely the point where it would do the most good.

Caryl: Right.

Frank: And that was where the cooperation with Columbia came in. Paul worked with Dunning[49]. At about the time Dunning was starting his work, which we didn't know about, that led to the atom bomb. At that point, he was using a cyclotron and so on to provide a neutron source that would give you a sole neutron beam into a mouse tumor that had been engorged with some kind of a lithium compound – or boron was the other that he was trying. The trick was to get something that would be particulate, but essentially colloidal and nontoxic enough that it wouldn't kill the animal and that the white cells could latch on to and carry with them to the site of action. The results were not un-encouraging. I remember, my old professor at Illinois, Kruger, was trying to do some of the same sorts of things and we compared notes at one point in a Physical Society meeting of his work and ours in essentially parallel tracks, he had built a cyclotron at the University of Illinois. That work resulted in a paper in radiology on considering what kinds of things were possible in this way and there were some other papers that cited the actual experimental work. It was again one of those areas that we were not in a position to pursue without a nuclear capability, if you want to put it that way, and that was beginning to go underground. So, the work with Paul and Seymour came in, I think, to try a slightly different attack on this, which was to see what the effect of gram-negative toxins would have on the same tumors …..

Caryl: Right. Blood engorged cells.

Frank: Well, not engorged cells in this case, but it was known that some microorganisms cause massive bleeding in newly growing cells. Here was a cancer implant that had a vascular bed around it and was sensitive to this kind of thing. For example, the typhoid vaccination is a very dangerous one for a pregnant woman in early stages of pregnancy because it can cause the fetus to abort. What was the mechanism involved and what was the range of organisms that carried this kind of toxin and was it possible to separate the toxic factors from the hemorrhagic? That led Paul and Seymour to do a good deal of work on the so called gram-negative because of the way they stained microorganisms. If I remember, E. coli is one of the standard representatives of that group and they were motivated also by (everybody at that point was interested in) what can you do that may have war relevance and certainly the toxic effects of organisms and the healing of wounds was a clearly related thing. I think we made some efforts to try to get this work involved in a direct way with things, but that was a very small laboratory and it was a large program and it never had any contractual arrangements and it simply didn't develop. We tried, we I say, mainly Paul and Seymour, though I was involved in the following one which was to look at the medical syndrome shock – a traumatic shock in which simply the person's blood pressure goes way down he gets cold, and if you don't take care of him, he dies. It's very common in the battlefield, no particular injuries that you can detect which should have killed him, but he just passes out and goes down hill in a matter of few hours and is gone. A couple of Canadian workers had come up with an

[49] John R. Dunning of Columbia University did much early research into uranium separation by gaseous diffusion.

experimental method for inducing traumatic shock, they thought. It was called Noble-Collip[50] Shock from the names of the two workers, Noble and Collip and it consisted in tumbling rats. I believe, Seymour described some of this the last time around, but we finally nailed that down not to traumatic damage, but to overexcitement of the autonomic nervous system by vibration and you could pat a rat very carefully and shake him to death just as easily as you could tumble him to death. It could be alleviated by autonomic depressants such as atropine and its derivatives. This enterprise also did not attract support from the medical research council.

Caryl: CMR.

Frank: CMR, Committee of Medical Research, but we carried ahead as well as we could, it was rather difficult to maintain a draft deferment for Paul on the basis of the medical relevance of this work. Seymour, I think, had a heart condition or something, was out – had his 4F rating I guess. But they did both stay throughout the war on this. That was about the kind of thing they were doing when you came to the Lab, as I recall.

Alvin: I think so. Yes.

Frank: The traumatic shock work, Noble-Collip Shock went right on till just about war's end, as I recollect it. Now to the early work for the Committee on Sensory Devices after the Committee had been formed and had accepted the Laboratories as a contractor. The Laboratory's role there was perhaps not too unusual, but somewhat. The Committee (made up of psychologists, physicists, biologists, the five people, you probably can recite them, I don't think I can) provided general guidance and the kind of governmental supervision that was a necessary formality. But they wanted the Laboratories to come to them with recommendations for research undertakings including contracts with other companies under the Laboratory's general management. That was the central laboratory for the committee and they said we will not set up physical facilities, but will hire a laboratory to work under our direction. They were quite generous in the freedom they gave us to do this and our general guidelines were to look at anything that worked, but mainly for the problems of helping a blind man to get around and also to help him to read. So, the guidance device work and the reading machine work quickly became the two main thrusts of that enterprise. Some of the earliest incidents that were moderately interesting, the committee suggested that we look at the problems of returning blinded veterans who were then coming to…

Caryl: Avon.

Alvin: Avon, Connecticut.

Frank: May be. There was, I thought, another place, but we were told that one of the things they would like to do – that there had been one or two people who had bowled a great deal.

Caryl: That was a bit earlier.

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[50] Noble, R. L., and J. B. Collip. A quantitative method of production of experimental trauma without hemorrhage in unanesthetized animals. *Q. J. Exp. Physiol.* 31: 187-202, 1942.

Frank: And that was bit earlier, I think. I cannot remember the name of the government hospital for returning veterans who were blind, but it was some place in Virginia, as I remember.

Caryl: Oh yes, where they received them initially.

Frank: They received them and took care of them for six weeks (or something like that) and then dispersed them out to friends or other hospitals or what not. It was a sort of initial rehabilitation for returning blinded veterans. So, there was some point to trying to find something that would help these chaps make the adjustment to blindness. So we said all right, let us see if we can lash together a device that will help a blind man bowl. I took a hearing aid in order to get a very light weight compact amplifier, made it belt worn, brought a cable up to a head piece that could be strapped to the forehead and this consisted essentially of a cube of Plexiglas, but made up of laminae, which had light absorbing dyes between the laminae and with polished front and back faces, so it acted not like a lens, but a very narrow gate for a light beam. If you had the light beam lined up with the lamina, then it would come on through to a photocell. Anything to the side even a few degrees, was heavily attenuated. So it made a highly directional optical system in a very compact mode, small photocells, as small as we could get at that time, constituted the input to the hearing aid amplifier and what you heard would then be the buzz made by an intermittent light source. We mounted a camera box out of MPC stores, mounted a small fan motor inside it with a projection light behind it and replaced the fan with a phonograph disc that had holes cut around it. So, when you spun this, you got a lowish frequency, a pretty nasty sounding buzz, if you took the emerging chopped light and put it through the photocell and an amplifier. Result was if you were wearing this thing, Paul and I tried this once, Sunday morning was the only time we could rent a bowling alley in the nearby area, because they were busy other times. We rented the whole alley for Sunday morning, took this device over, put the box over the kingpin, Paul put on the head gadget and took a bowling ball and set up. I might say that we had, in making arrangements with the owner, gone over and played a few games and neither of us were excellent bowlers, but we did reasonable scores. That Sunday morning though, Paul looked up and I can see him shake his head back and forth, I knew what he was hearing, because I had tried that. The buzz got very much louder or softer as you went off axis. He zeroed in on it and threw the ball across two alleys to his right. The owner of the bowling alley was a little perplexed by this, and so was Paul. He tried again and threw it across a couple of alleys the other way. I said wait a minute Paul, let me try that, there is something going on here, and I tried it too and what happens is that your head becomes disengaged from your body. I don't know how else to put it, but you know perfectly well that your head is pointing exactly at the kingpin but you don't know where your body is and you don't know where to throw the ball. Well, this was not very encouraging to us, but we had the gadget all made and we even, I think shortly thereafter, took it to the Washington to show it to the some of the Committee members, but with some severe reservations of our own as to whether it could do any good for a blinded veteran. The meeting was held in the Carnegie Institution building and the device would not work when we were about to demonstrate it. The motor got very hot and it turned out that the Carnegie building had DC and not AC. Well, in any case, the device did go down for trial by the veterans and they had no problem with it. They had had experience by this time with the relationship of where pieces your body are and where your sensory mechanisms think, things are. I tried myself one morning wearing blinders from the time I woke up until noontime and one does adjust rather quickly to a world that is limited to the length of your arms, there simply is no world beyond that and you soon learn where it is and whatnot. So, this was not a problem for the blind man, though it was terrible problem for Paul and me.

END OF REEL 2 PART 1

Frank: The guidance device work, we quickly got out. We clearly agreed that we should contract out the production of guidance devices of one kind or another. We would do the testing and we would do the initial development work on reading machines. So, we got in touch with Hoover, whom Caryl and I had known through the Industrial Research Institute, they were a good company not overly involved in defense work and glad to take this on.

Alvin: This is the Hoover Vacuum Cleaner Company.

Frank: Hoover vacuum cleaning. They built a device, which as I recall it was a kind of horn structure that had a hammer in it, mechanically driven hammer, to make a loud noise and then receivers. You point it around and get the noise back. There was a lot of naiveté on all sides in undertaking these things. As Dr. Corner pointed out in the review chapter that he wrote later on for Paul Zahl's book on blindness[51]. The device was all right, but pretty clumsy for use. I have to say though that the naiveté had to do with what it is you want to give the blind man and what he can do with it. The psychological problems are far subtler than we realized. We thought we wanted to give him an indication, that something was there and how far away it was. We should have realized – by hindsight we do – that for most things, how far away it is (so long as he isn't about to bumping into it) doesn't matter. That's not the world he lives in. So ranging was a consideration. We talked also with people at Stromberg Carlson and then I don't quite remember how that contact came about. I knew some of the engineers and perhaps that was it. Anyhow, they came up with the idea of using supersonics and built a device, which had two wooden horns, one was an emitter and the other a receiver and you sent out a supersonic signal that is swept through a certain range. The returning echo then would be modulated and if the echo was caused to beat with the outgoing signal, the beat note then was in the audio range and if the signal came back very quickly, the beat note was a low frequency. If it took a long time to get out and back, the beat note would be a high frequency. So that as you held this thing in your hand upon exploring around, you would find that the floor gave you a low, but fairly strong thump, and a nearby wall would give you a low frequency strong sound as long as you are pointing directly at the wall. If you switched off the angle little bit, it almost disappeared until you got to the corner and you had a corner reflected and a very loud signal coming back from little bit farther way, a little higher frequency. Well, it is unfortunate, but that arrangement of frequency and distance is not the intuitive one. It would have done much better, if physics were built, so that it would come out reverse way. But that was a persistent idea in the field later on, and eventually developed by an Australian, I guess, an Australian engineer whose name Pat, will remember, …

Pat**:** Kay[52].

[51] Blindness: Modern approaches to the unseen environment. Ed: Zahl, P. A. Princeton University Press, Princeton, NJ, 1950. Reprinted in 1963 and 1973 by Hafner Press, New York, NY.
[52] Leslie Kay. A British engineer who moved to Canterbury University, Christchurch, New Zealand and set up a company to manufacture sonic aids for blind mobility.

Frank: Kay, into a fairly useful and merchandized device, the Kay device. But that's a later story. I don't remember what, those are the only two that I really remember the other contractors some how fell by the wayside pretty fast and the thing centered down to the matter of testing the **Hoover** device and the Stromberg Carlson supersonic device. Now, I don't know how much you were involved in that Al and whether you …

Alvin: Well I want you to go back and say some other things, but I don't recall the Hoover device at all. I do remember the Stromberg Carlson device very well and working with it and testing it. I also remember that somewhat later, and I can't now recall how much later, the Signal Corps came through with an optical device and we had one of those and we did test it. That was a bit later.

Frank: That was optical, used a chopped beam and triangulation to pick up the different frequencies of the beam. Well, let me say a little bit about the testing program, because that was a substantial part of our undertaking. It required a first test, you want to set up an obstacle course and see how people got through it, what the difficulties were. Of course, many of the difficulties were that the device promptly broke down. And the other difficulty was getting blind subjects to come into the center of Manhattan to be tested. The latter one proved not to be as hard as I had supposed it would be. Because there are a number of young, able bodied, blind people out in the community, some of whom use dogs, some of whom use what-they-call facial vision, reflections, echo reflections to get themselves around even in a city like New York and we had young chaps coming in from out in the Bronx, or thereabouts by subway across town to the Laboratories and they would appear on schedule to be tested. They were doing better than any guidance device we ever developed. Some came with sighted assistance. I don't recall that we had any people using guide dogs, maybe we did.

Caryl: We did. Yes we did.

Alvin: I recall… I recall the guide dogs.

Caryl: Some of them got fouled up in the wiring, I recall.

Frank: Some got fouled up with the ultrasonics.

Alvin: But, I recall one remarkably young man, his name is on the tip of my tongue, who used practically nothing, except so called facial vision.

Caryl: There was a one from MIT, I remember the….

Frank: Cliff Witcher[53], …

Caryl: …the radar engineer.

Alvin: No, this was the young man, who lived in New York City who came in and worked with such a…

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[53] Clifford M. Witcher. Employment records show him to have been hired in December 1945 and discharged in June of 1947. He continued to work on Sensory Aids at MITs Research Laboratory of Electronics.

Caryl: He was the one who guided himself by the Needix store and so forth. That was quite extraordinary. And also remember, we had visits from ***St. Dunstan's*** during that period.

Frank: Yes. That is right.

Caryl: And we had…

Frank: I myself visited St. Dunstan's to see their work.

Caryl: Yeah. And the then head of St. Dunstan's, I took him[54] out for lunch at the Century and he walked …. He had no guidance of any kind. He walked ahead of me. I couldn't keep up with him. Over all the intervening streets and to the Century with no hesitation at all. He had been blinded I think in World War One.

Pat: Was that Peterson?

Caryl: I am trying to remember his name, well, this would be about this time in the program I think.

Alvin: I don't know what. I don't know whether this is the time to say, that I think perhaps it is, because it can be said very simply and put it aside. But from my point of view, the greatest problem we had in connection with the guidance devices was not just that they broke down frequently, but that we didn't know what our tests meant. That is, you recall, we set up these obstacle courses we made these artificial obstacles and we had this large room in the Lab, an obstacle course and we start the subject that blind subject at one end with the guidance device and his job was to get to the other end and we carefully recorded the amount of time it took him and the number of collisions and so on. And we thought well we could use this to evaluate various devices to see when and if improvements have been made, but of course the problem is the validity problem. I mean, what does all this mean? So, I mean anybody, even the first crack through, a sighted person could put on blindfold – I could put on a blindfold and get through the obstacle course without hitting a single obstacle, might take me quite a while. But then what did this mean about whether I would want to use this if I were blind out on the street. Because, it was tying up my ears, right, which I want to use to get around, we really couldn't afford that. We considered at one point I think delivering the information tactually for that reason. Also these devices especially the supersonic devices were very bad at detecting down steps. The reflection is specular and you know you got a big three-foot hole there, you know, you are going along and you really don't get much information about that and ...

Frank: Or you detected a crack in the side walk, which wouldn't bother you in any case.

Alvin: So the whole reading machine program was very different in this respect. With the reading machine, you know what you want to test and it has *a priori* validity, can the person learn read and at what speed and you know with what difficulty and so. But here, we just didn't know what

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[54] Lord Fraser of Lonsdale. Blinded in World War I, Ian Fraser was an English aristocrat who headed St Dunstans, an organization for blinded ex-servicemen in Britain.

to test. The only test in the end is to get a blind person to use one of these things in real life, and then you don't know what to measure and well.

Frank: Yes, we have talked about our naiveté in this thing. I must try to give us a little…. Look, we were reasonably imaginative and practical.

Alvin: Yeah, but we were naïve about that too, certainly I was. Here we have an object, we have this obstacle course and we can measure all these things and then, you cut to the chase. What do these measures mean?

Frank: It does not fit the laboratory paradigm.

Alvin: Right.

Frank: Well, you know, Paul was good at that. He was responsible for taking several of the students around Tudor City, which is a sort of closed three- or four-block dead end area, apartment houses, and watching them. Eventually made a 16 mm movie of these actual tests, which I think we perhaps have around some place, I don't know, it did exist. We also wanted an outdoor environment and Caryl offered the use of his bowling alley at Westport, and we made one or two expeditions to Westport with two or three people and the devices and let them use that.

Caryl: One of the things that showed up there of course was the ease of distinguishing water from land and concrete from lawn.

Frank: Also one of the campuses around the blind school, here in New York. So there was practical work, another thing we did was to collect information from the men themselves about how they got to the Laboratories. That is what kinds of information did they use and find useful in getting around without a device. A good deal of that is in the article, the ***Physics Today***[55] article that I wrote sometime during this work. Well, the course of events was that the blind aid work got started pretty promptly. The contractors built the devices and we were engaged in testing them.

Alvin: You are talking about the guidance devices?

Frank: The guidance devices. Sorry what did I say?

Alvin: Well blind aid. And more specifically….

Frank: Yeah. More specifically guidance devices. That work rather began to run down and the emphasis began to build on the reading machine problem where the Laboratories was attempting to do the basic research and leading into design – we wouldn't have designed the device I think. but nevertheless, what did the device have to do and, as Al said, that is, we thought, better define the problem. Shall we leave guidance devices at this point, do you think is that...

Alvin: Yeah, I don't think there is. Certainly, from my point of view, there is not much more to be said. I would only add to what you said about the testing out in the real world. That, even there,

[55] **NOTE:** Find the reference to this publication.

it seemed to me that it was very hard to get any reasonable evaluation, sort of the cost benefit ratio here, there was a cost to it, because it was tying up the auditory system and so on and the question was always it seemed to me, what did they gain versus what did they lose? I was never satisfied that we could get a good fix on that really. My impression at the end of the whole business was that the blind would be well advised to learn, to rely on dogs, to rely on a cane, to rely on their own ability to detect obstacles. By the way, we did start a little research program on the side designed to test the ability of a couple of these skilled blind people. These young men who had been blind for sometime to test their ability to detect obstacles just on their own, you know, whatever technique they used snapping their fingers and making sounds of various kinds and we never really published that information. What we found was that they did quite remarkably well. You know, they could detect obstacles and we….

Caryl: Discouragingly so …

Alvin: And it was at about that time, I think that somebody I cannot now recall his name, someone at Cornell published a study about so called facial vision in which he showed that it is all acoustic, auditory, and so on that they are somehow responding to it.

Frank: At about this time the bat work came up, right.

Alvin: Yeah. Griffin and Galambos.

Frank: Go ahead Al.

Alvin: I was just going to say I would like you to back up there. There are a couple of things that...

Frank: Well, while we are on this particular topic, let me add a couple of anecdotes. One had to do with a young man, a subject in the guidance device program and in the particular experiment that you described of asking people to walk blindfold, but leading them backwards, disorienting them and saying now go find the wall and watching what they were doing. One chap who had used, I think, a dog or a sighted assistant only, caught on to what was going on with the effect of the echoes in the course of this experiment and within the matter of, as I recall it weeks, was traveling quite well through New York city on his own auditory basis. That is, it was a clear demonstration, a beautiful demonstration, that this is largely a matter of insight if you can latch onto the information that is there in the incoming signal you can use it, but it is sufficiently obscure, that unless you get the insight into what to listen for, you live for a long time without realizing it. We didn’t really make a careful test of the training procedure that would be necessary to duplicate the happenstance experience of this one person. Another anecdote is more personal. It had to do with the supersonic devices or one of the other guidance devices. As with the bowling alley experiment, I said to Paul, “here let me try that thing” and I must say that I had thrown the bowl across several alleys too. I decided I must find out, what is going on here, when I watch these people do such strange things, as get turned around come right back out, where they went in. Well, I tried it. It is again a very strange experience but I can tell you with complete candor that I found myself totally surrounded between an obstacle and the wall against which it was heading. I couldn’t get out.

Alvin: This was with one of the guidance devices?

Frank: One of the guidance devices. It is hard to understand, but it happened.

Alvin: Well I take it it is hard to represent what is going on, let's face it.

Frank: The representation of what you are listening to, in terms of a special construct was not possible. I knew what the device was doing very well and it was doing it, but I was caught in the corner and could not get out.

Alvin: I remember in this connection that you frequently, Frank, characterized the kind of representation one was getting from these devices as being equivalent to a Soda Straw vision and I remember we were very much worried about that. It occurred to us, you see, that in the best case one was in fact looking at the world through a very-very small bore Soda Straw, because you couldn't get any pattern or whether the question was how much pattern could you get remember, we worried about that. This became an interesting theoretical issue for us in a way. Suppose the device works very-very well in exactly the way it's supposed to work, you don't have to worry about specular reflection and all these other kinds of things even so. How well can a person integrate the information if he has to have it distributed in time? And, in fact, I remember, we even talked occasionally about trying to create the analogous visual situation, in which one would present optical information sort of point by point in time as you scan across it and the question then was how useful would that be. In the best case, how useful would that be, to what extent could a perceiver integrate that and come out with a picture, and say yes that is a chair or that is Caryl Haskins. We never really did that, but that became a sort of interest, that was one of the kinds of things that began to preoccupy us after we came to appreciate the limitations of these devices and it came home to us that at best we were going to have that problem. At best we were going to have that problem.

Caryl: And I seem to remember the testimony of the chap from MIT about how he found this desk and as I recall it, I think you asked him, because he was on the third floor, I believe, and he used to find his way up there in that maze of desks and find his own desk with no problem at all, without any guidance device at that point. I think you asked him how he did that and to draw a map and he very indignantly said, "you sighted people think we work by maps?" If you ask me to draw this kind of thing linearly, I will get there I will have no problem, but you think, we have a map. You think, and we don't.

Frank: I might say Cliff Witcher was a physicist by training, had worked in the Radiation Laboratory at MIT and wanted to work with us in this problem, he had maybe an interest in it. One of the things, he wanted to do at the Laboratory was to make some of his own devices and use the lathe and so on in the machine shop, which did not strike me as a good idea. He was a persuasive chap and liability insurance problems were not what they are today. I finally gave him permission to do it at his own risk and he did it quite successfully.

Alvin: Yes, he had a number of special devices that he had designed though I cannot recall now.

Caryl: He had been assembling radar sets, I think.

Alvin: But these were little things he used himself to help him.

Frank: The whole field was full of gadgetry at that time and indeed a lot of St. Dunstan's work was a matter of fitting gadget to individual. Their philosophy, and I found it interesting, was quite the opposite of the Committee's and the Laboratories. It probably goes back to their origins in World War I, but the philosophy was**:** you get the man and you see what he needs and you equip him with the best things you can manufacture to fit his particular needs. The Laboratories used to say all blind people have certain problems and two of them are guidance and reading, and we will attempt a general solution, a totally different philosophy. I don't know whether that holds well for St. Dunstan's now, but it did at the time, I was over there toward the end of the war, or just after the end of war, and talked about it. Guidance devices. I think that pretty well covers that.

Alvin: I think so. But before we go on. Excuse me. May I ask about whether or not we should fill in certain gaps that I see.

Frank: By all means.

Alvin: Perhaps you covered this in your last session, but I wonder if you said as much about the National Photocolor Cooperation as you might have. For example, its relation to the Lab. What was the idea behind going into this? It seems to me that was very interesting perhaps you covered that last time.

Frank: I think we did.

Caryl: I think pretty well.

Frank: The idea of a group of scientists who wanted to do their thing in science, but needed money to do it with,

Alvin: but also could use their knowledge and skills and so on in connection with the commercial enterprises. I think that is interesting and important.

Frank : Our final conclusion on that after we finally closed down Photocolor was that you cannot worship god and mammon. It just doesn't work.

Caryl: In just took too much research time.

Alvin: Did you also get around to saying how complex an undertaking it was to develop one of the pictures.

Frank: Well, that's all the photographic literature. The Carbro process is a beautiful process, but it takes a master technician to operate it and artist besides and it is very difficult. I will say a little bit more about that, Derrick Gallagher in the year or two that he was operating National Photocolor, did a fairly remarkable job, not only of re-designing the camera and getting it into production as a device. But also in building up a production facility for Carbro paper, which is a good grade of paper, you collect the three dyes that are used, (it is a subtractive process) and you cook and mix them, get them ground in a colloid mill to grind them very very fine. Mix them with gelatin and coat them. So it is a photographic emulsion process, except that it's not photosensitive and its chemically sensitive to a photographic negative that has been hardened and impregnated and then pressed against the Carbro paper, and the gelatin on the Carbro paper is hardened or is not hardened in accordance with the optical pattern on the photographic negative. Then you wash the Carbro paper in warm water and the un-hardened gelatin washes away leaving you with an image in one color not on a piece of paper. The trick now is to take the other two colors and superimpose them and strip their paper backings away so that you build up literally a sandwich of one piece of paper and three layers of hardened gelatin in thicknesses that correspond to the photographic registrations.

Pat: A most improbable process.

Frank: Most improbable process.

Caryl: And the registration problems involved.

Frank: It produces the finest color pictures I have ever seen, no question about it.

Caryl: Well the one taken at Yalta, for example, which we have, a print is absolutely… of course, they are all mineral dyes and do not fade.

Caryl: I was going to add one piece of trivia. Frank was speaking about his temperature and color balance device, which is, I recall, was greatly relished by the whiskey ad people, who insisted that their whisky had to be very red, you see, so this made it possible to make it very red and, as I recall, this print turned up on the sub ways... Third Avenue.

Frank: They may well have that. That was of course the market for this kind of thing, the advertising profession.

Caryl: The advertising profession.

Frank: And another anecdote gives a good insight into why the Carbro process survived as long as it did, and it may still be in operation for all I know though I think it would be a very limited one. One of the tobacco companies wanted some ads and here was a matter of good color control on the leaf tobacco. This was an auctioneer holding up the kind of tobacco that these cigarettes were made out of. Now a good ad would have a larger leaf than great nature grows for you and if you do a composite operation on this, you can have a nice large leaf, because you will not only build the images, but you do a cut out and superposition so on. It gets to be very tricky business to do, but you can't do that kind of thing with the dye process or Kodachrome.

Alvin: Other cameras don't lie.

Frank: Other cameras don't lie in the same way.

Caryl: And of course that also became practically very important using it for things like the skin transplants as the British did.

Alvin: But Frank, weren't there other technical developments in connection with the camera that you and people at the Lab…?

Frank: I had mentioned the color temperature meter. If you are going to get accurate color registration on accurate separation on the three negatives, you have to be very careful with a lot of things, one of which is the color temperature of the light that you are using to take the picture. Then another thing you have to do is to control very carefully, the development of the negatives, I shortened up the process of making a Carbro. You have to develop the negatives, which are photographic plates, used in order to maintain registration. The plates are then projected on paper and it is the paper that is appliqued to the pigmented gelatin in order to get the pigment image. All of the photographic processing has to be done under very tight control. Grey scale in the image was essential at all stages, right down to the printing on the paper and that means you have to have a densitometer in order to measure the grey scales, which you are using for your control exposure development control. So we designed and built a densitometer, which was actually a rather good device. It used a, there was a standard name for the circuit, which I don't remember, but anyhow, you stack two vacuum tubes on top of each other, use pentodes so that you got a very large plate voltage swing corresponding to a very small grid swing.

Pat: Was it a Darlington circuit?

Frank: Yes, Darlington. Yes. Then put your input signal into the appropriate grid and the output will swing violently as you go through the balance point. I used a magic eye to indicate the balance point and a photographic wedge, which was both variable density and variable aperture to control the amount of light that went to the photocell and calibrate it in density units. It is little clumsy, but it worked and worked well.

Alvin: Was that the same device we used later on the spectrograms?

Frank: Yes.

Alvin: To get the relative intensities.

Frank: Yes. Later devices are better, but there is a lot of history between those things.

Alvin: How about the mirrors, Frank?

Frank: The mirrors, the pellicle, casting the pellicles was the trade secret involved in this thing. The methods we used were partly inherited from Leroy, but mostly worked over again by Paul Zahl, back in the days of 480, Lexington. You got a very good piece of plate glass, floated on mercury to get a flat level stable surface and pour a slurry of nitrocellulose in syrup consistency on to the thing. Leroy spun it in order to get it distributed and I don't quite remember how Paul.

Caryl: Paul did not spin it, I think.

Frank: He did not spin it, but managed to get a thin layer, which would then set under controlled atmosphere and give you about a mill or half-mill thick celluloid sheet, now adhered to the glass. You now have to get it off the glass, and that takes high humidity and you can with care peel it off. You build a metal frame of aluminum and let it set long enough after it has been machined so that it would stop warping the machine surfaces. Well, you lay this membrane down over it and carefully cement the edges and if it is laid on when it is a little limp from water content, it will then dry out and shrink up tight as a drumhead. And if the frame is flat, the mirror will be an almost perfect mirror and the test of its quality is certainly its flatness, which you can do with a long beam reflection. And the other one is to hold it up and see whether it has got any dense spots, that would tend to give you distortion so you held it in the beam of a fluorescent lamp and you got an interference pattern, which ought to have four or five fairly uniform and parallel fringes running from one edge to the other, that means very little wedge effect and no local inhomogeneities to distort the light beam. The camera alignment, the alignment of the three mirrors in the camera behind the three color filters is an equally tricky operation and essentially it is done by back projection, you set it up in a camera that has got adjustable mounts in it and you project a master plate on to a screen and get all three masters superimposed in their projected form, lock it down, that is it. Not difficult in principle, but in practice, it takes a good technician and this is where Arnold Garnet came in.

Alvin: How do you manage Frank to arrange it so that it will reflect as much light as you want and transmit as much light as you want? Is that a problem, you didn't want it to reflect all the light?

Frank: I am sorry, I gave a wrong impression of the process. The camera has one lens, two pellicle mirrors set at angles, so that you get the through beam and two reflected beams into three plate holders. Now the mirrors have to be located in such a place that a single subject will be the same size on all three. If you move the mirrors around a little bit, the images will not be all the same size and when you come to register, your Carbro print, it won't register, this is where registration process comes in. It is controlling the positions of the mirrors. So these mirrors themselves though, the percentage reflection, both of them are semi-reflecting mirrors that reflect and transmit, because the through beam by that time gone through the lens and two mirrors. That had to be carefully controlled, but that was a matter of specifying what you wanted and arranging with the coating laboratory to do the job right.

Alvin: You mean in coating the mirror.

Frank: Coating the mirror. That was evaporation. That was separate from ….

Caryl: We did not do that.

Frank: We had to send that out.

Caryl: And the ratios.

Frank: The ratios were specified and tested, but not controlled.

Alvin: One other question. Weren't these mirrors used or found to be useful later in bomb sites?

Frank: Yes. They were in fact produced all through the war and afterwards for period for the ack-ack guns that were used in shooting down Japanese kamikaze planes. And the trick there was to use a small mirror set at a right angle within the gun site, so that the operator who is sighting with his telescope and operating the guns at the same time could see the plane, because the mirror was semitransparent and he could see the image of a cathode ray spot on the tube mounted off to the side. The cathode ray spot controlled from radar should follow the plane's image and when the plane went behind a cloud, you kept on shooting at the cathode ray spot.

Pat: How many of these pellicles were produced in the course of month or something and what was the production rate and was the National Photocolor the only producer?

Frank: The only producer, sole producer.

Alvin: Did **N**ational Photocolor produce them for the war department?

Frank: For the Navy.

Alvin: For the Navy. Yeah! They did. The Navy did not contract it.

Frank: Well, no, let me be sure about. I am not sure. I think they went to a separate contractor who built the gunsights.

Alvin: But it was National Photocolor technique that was being used?

Frank: The National Photocolor product was used. We shipped to the assembler of the gunsight and they built them in and we were under considerable pressures at the time. This, I do not remember with accuracy, but it was a one-man operation and of the order of 20-50 to 100 a month would have been maximum production for a matter of two or three years. Arnold Garnet was the man and one of the jobs I had to do is defend him from his draft board on the basis of the importance of his war work. That took me to Harlem late one night to sit with a group of people to whom I explained these important considerations and they didn't understand or believe either a word I said. So they drafted him, but he was a 4F and it didn't stick and the Navy got its pellicles.

Caryl: Literally, it would not have.

Frank: It would not have; Nobody else knew how to do it.

Alvin: I remember one other name in connection with National Photocolor that has not figured in the discussion today, Ralph Wareham[56]

Frank: This came later

Alvin: That came later

[56] Ralph E. Wareham was hired by the Laboratories in September 1945.

Frank: And while we are on National Photocolor let's finish it off in that sense. It was put on ice during the war and after the war, we thought well, let's do it again. Ralph had been a young accountant, I don't know precisely what. He was not a technically oriented man.

Alvin: No, he was not.

Frank: He was on the business side of the things at General Electric.

Caryl: But, oriented to quality control.

Frank: Oriented to quality control. I guess that was his orientation certainly. He wanted to move out, or the company was changing its structure, I don't know what. In any case, he became available to us. A very engaging and enterprising young man and he took on the job of making National Photocolor and National Photocolor Carbros. I believe that – I will come back to the Carbros thing, but they were two parallel enterprises. He took on both of them and ran them quite well to make the cameras according to the original designs. I think, he soon gave up the making of Carbro paper, that was a separate and rather tricky operation, and made it a paying proposition for about a couple of years – two may be three. During which time, the cameras sold reasonably well to the advertising profession because it was essentially the only way you could do things like the tobacco leaf operation or that you could get the color quality that photo engravers insisted on having if they were going to do national magazine color plates. The actual making of Carbro prints for that trade was in the hands of probably half a dozen freelance people around the city (around the country really) one of whom was Nick Langen, a very competent Russian immigrant, and Gerry Wind[57], who he trained. And they teamed up, I think, with Derrick Gallagher before he left to set up a separate company called National Photocolor Corporation. Which meant that these two men came in and used the Laboratories' facilities and to some extent, the Laboratories' marketing to supply completed Carbro prints to advertising agencies, that was the main business. So, the company was into two things then, manufacturing cameras and some accessories, and making prints for the advertising agencies. Well,… What does Kodak call it dye process – not the dye process, its color prints?

Caryl: Kodacolor.

Frank: That's not the right name.

Alvin: Kodachrome.

Frank: Kodachrome…. Anyhow, their color process for making color prints. They started out with a dye transfer process and they went on to something different, and I don't know the details. Well, using a coated paper that has got the dyes in it, it is essentially the present process for making color prints. But that process was in its early development stages at the end of World War II and it came pretty well to fruition so that it was an acceptable product by discriminating users by the 50s and it was in that period that National Photocolor had a window of opportunity, which it took,

[57] Haskins Laboratories' employment records show Morton Gerald Wind to have been hired in December 1945. No discharge or resignation date is given.

but that window began to close pretty rapidly. No comparison in the cost of the processes and Ralph had kept his contacts on quality control throughout that period and simply let that part of his life increase and left the Laboratories. So, the National Photocolor went back into a quiescent state now with the Arnold Garnet the only employee and not much business, except some residual spares on the gunsight pellicles. It was eventually sold, I think, I mentioned it in the earlier reel, to Bud Schwartz in Westport, who found other uses for pellicle mirrors and developed it in quite a nice little business as a first one-man operation. He had since sold it and retired and I do not know what has happened to it since. Whether you can buy a pellicle mirror nowadays or not. It won't be from the National Photocolor Carbros Corporation, though because that one was dissolved and National Photocolor name was sold to Schwartz. It was an interesting experience in trying the industrial adjunct to support research and on balance did not work, it almost worked in terms of being financially rewarding enough.

Caryl: But, awfully costly on research.

Frank: Awfully costly on research and administrative time. It was a special niche type of product and the niche disappeared and so the company disappeared.

Caryl: Ralph Wareham, incidentally I got news on just three or four days ago. He is now living in the Florida. I think, retired.

Frank: His wife is now dead. Is your reel about gone?

Alvin: Could we take a break now?

Frank: So let's turn to the early story of the reading machine part of the CSD enterprise. I think we have said that the guidance devices occupied most of our time and attention early on and reading machine was sort of in the thinking stage, one aspect of which was to get a good look at the past literature, which is fairly extensive actually and we engaged a young woman with the name of Katherine Auchincloss[58] to do this for us as a library reference job. While she was working on it, one of the early things we tried was a demonstration for the still newly formed Committee on Sensory Devices. I remember the preparations for that demonstration, I am not even sure that it wasn't before we were in fact signed in on a contract to do the CSD laboratory work and it had been the part of the initial stages. It looked to us to be the case, that what you would have to do to make a reading machine for the blind was to provide a kind of handheld stylus or slender cigar like thing that had an optical pickup that some how if you scan it along the line of type with or without manual guidance would convert the black-and-white on the paper into some kind of variable audible signal the blind man could listen to. The matter of transforming visual patterns into auditory patterns. And the simplest thing, certainly seemed to be to take a narrow slit and for trial purposes, make it so that you didn't have the problem of providing the illumination and use reflected light, but you use transmitted light through film. Scan this device across a series of letters and look at the total light output, which for the letter "H" would have a large hump and then a small amount for the crossbar and then another large hump. Capital "M" would have a large hump in the certain gliding or more or less constant amount as you go across the "V" part of the

[58] Possibly Katherine L Auchincloss, one of two sisters of William P. Bundy who was a foreign policy advisor to Presidents Kennedy, Johnson and Nixon. He died in 2000.

capital M and then another big hump. Would they be very different, well depend on the type font in that particular case? Any how, it was a simple and straight forward first try kind of thing that an engineer or scientist would think of attempting and this is what we tried. A single slit device is, I think we came to call it, and at least that is what I will call it at the moment. We attempted to simulate that for the committee in terms of a sound motion picture for which purpose, we recorded the soundtrack with, I don't remember correctly. We actually used a 16-mm projector to illuminate the moving picture. In any case, we ended up with a film that had the letters going by Times Square wise and the sounds as they pass the center of the field view. I do not quite remember how we put this together, but it was optical soundtrack. I do remember with great misgiving that the early morning the day it was to be presented, we got the prints back and started to (we did not develop them ourselves) to project them and they wouldn't project. Freshly developed film has to dry out a little bit before the sprockets come into the right place. So, this had to be strung out to dry before the meeting later in the morning. At the meeting, it projected and went on fairly well and the committee was impressed, except one member who was an audiologist, who wished we could keep the volume down a bit. We were proud enough with him. I was sitting by the projector. I kept turning the volume up, not realizing how loud it was for everybody else. Well, this gave a series of squeaks, I believe, there are two ways to present this obviously. One is simply to make a single tone that goes up and down in intensity with the amount of light that was coming through the slit as you scan. The other way is to convert the amount of light into a variable frequency and let the constant amplitude tone go up and down. You want to be able to squelch it when there is nothing, so that you do not have a continuous tone. And there is a question about what the frequency range ought to be and whether it ought to be linear or not and so on. So, some optimizing factors to be done there. Those were the early attempts that we made in an experimental way to develop a scanning device. It was about that time that Al joined the team as a psychologist to help us see how you evaluate this and how you check it out against something that might be somewhat better than that, if you could try. There are a variety of obvious ways of manipulating the signal before it gets through the listener's ear. I do not know whether you want to do what Caryl and Seymour, and I did last time, and introduce this with a slight sketch of your life to show what the influences were that affected your... I think this will be very interesting.

Alvin: Well, very briefly, but I think it would be appropriate for you first to say something about the Optophone and how that differed.

Frank: Well, chronologically, I think the work with the Optophone came somewhat later.

Alvin: In, our own work.

Frank: Our own work

Alvin: Yes.

Frank: The device itself was very much earlier. It had an influence on us, but as I recall it, not much of an influence in the early stages. It was only after we began to run into the problems after Katherine Auchincloss had come up with her report and we were already well into the work here, that we began to think about the past history of the field and the comparisons.

Alvin: Well, I think, in the beginning, we assumed that certainly we can do better than had been done in 1920 or whenever it was if only because we had better technology.

Frank: In fact, that was a part of Dr. Bush's motivation in setting up the whole Committee on Sensory Devices, that an enormous amount of new wartime technology and why couldn't some of it be used for the rehabilitation of blinded veterans. That was the underlying philosophy for this whole thing to build. Well, since you mentioned the Optophone, let's go back and pick it up, though I think chronologically, it will be a little later in our own story. It was a device that was invented almost as soon as the Selenium cell had been discovered – changing conductivity of selenium with exposure to light. Fournier d'Albe was, I believe, the inventor of this particular device that he called the Optophone and it was essentially in development throughout World War I roughly in 1913 to 1918, or 1920. Immediately after World War I, the British firm of ***Barr & Stroud*** undertook the engineering and production of… Pat can you can tell me if Fournier d'Albe was in fact the inventor, or was he involved in some other context. I think he was the inventor.

Pat: I think he was the inventor, yes.

Frank: I believe that is correct. Anyhow, the device itself was about the size and shape of a small portable sewing machine and built with much the same kind of skills. That is, there was a small rotating disc that had five rings of holes in the disk differently spaced, so that when the disk was spun by a motor it would interrupt the light beam giving you five spots of light, differently modulated at different tones, I think.

Alvin: And, lined up vertically.

Frank: And, lined up vertically and that array of five scanning spots could be imaged on the line of type. And, there was a mechanical system for moving the scanning system back and forth along the page. The book was put upside down on a curved glass frame so as to maintain a constant focus for the scanning beam. The movement of the scanning head along it involves some very fine machine work and so on and the whole assembly then was used by the blind person to first place the book and line it up so that the lines of type were parallel to the direction of the scan of the beam and to set it up first to find the lines and scan them. The signal you got out was some subset of the five musical tones generated by the light chopping operation, because they were not really musical tones, each one of them had its own string of harmonics because of the chopping operation. But, you could distinguish five pitches quite easily. So, a letter would elicit a capital "H", for example would not pick up the lowest spot, which was designed to find descenders on letters like a small lowercase "p", but would pick up the four higher ones and you get a chord and then a middle tone corresponding to the bar across the "H" and then another four-tone chord. Capital M, to go back to that example, would give you four-letter cord and then an arpeggio down from the highest to the next lowest, back up again, and another chord. It had therefore better pattern resolution than the single slit device that we had started with. The question was how well it would work. And this was, I'm reasonably sure, the motivation that is the starting point of Zworykin's[59] group at RCA who wanted to show that they could do it better, and built a modernized version of the Optophone. They stuck with, maybe they went to seven tones, I do not remember, but basically they were using a series of…… I should back up on that. They were

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[59] Vladimir K. Zworykin, born 1889, died 1982. A Russian immigrant who invented the cathode ray tube.

sweeping with a beam on. They thought if you can use specific tones you can do better, if you provide the full range, so they did a sweeping beam device, that would scan a letter from bottom to top or top to bottom, and then repeat it again at a rather high repetition rate, but still well within the audio range. The result was when you listen to it, that the sweep frequency came through loud and clear and the frequencies that they wanted to get out were fairly heavily masked by the sweep tone. That alone was enough to damn the device completely.

END OF REEL 2 PART 2

Frank: Now do you want to say anything about white reading?

Pat: Well, it's just a minor note. I was commenting a moment ago that the Fournier D'Albe original invention was a white reading instrument whereas Barr and Stroud's production in 1920 added the balancing photocell, which transferred the image, as it were, auditorily into a black reading instrument.

Frank: Right. That was typical of the care with which they had engineered it. The device was, for it's time, well done. We wanted to find out something about that device and found that the American Foundation for the Blind in New York, had one in their museum collection and with a certain amount of persuasion, they agreed to lend it to us. It was no longer operative for a variety of reasons, though it was essentially in its original condition. The selenium photocells were dead. They could, however, be replaced with other photocells and we lashed that up and used it essentially in its original form with only minor repairs of that kind to generate some Optophone signals and recorded them. Then it was a comparison, eventually along the line. We did a study of the comparative readability of the original otophone, the RCA device 3 or 4 devices that we had concocted in the meantime, and a kind of control device, which we called ***Wuhzi,*** which amounted to this**:** it amounted to a human being reading a set of four letter words, if I remember, which were transliterations of the same four-letter words that had been used in the other test materials. Transliterated in such way that they were pronounceable.

Alvin: Wuhzi meant the word 'have'.

Frank: Wuhzi was the word 'have.' So that this was spoken language totally unknown to the listener, but otherwise English. To provide a basis for evaluating the machine signals against human signals. Wuhzi won, by a large margin. The RCA device, as I recall it, lost by a considerable margin too, well by some margin, to both the Optophone and to the devices that we generated and all of them clustered at a substantial level of performance. That curve, I think, is part of Paul Zahl's book on Blindness if anybody wants to get it.

Alvin: There was one device that worked worse than any of them, even the RCA. That was the one that I designed.

Frank: Well, why don't you take credit for that? The philosophy behind your point is I think…...

Alvin: Yeah, it was an interesting bad idea. I mean the fact that it turned out to be such a bad idea is interesting.

Frank: Yes, indeed. Well go ahead.

Alvin: Well, I don't know how relevant my background is except that it did determine very much influence the assumptions that I made when I joined the Lab as a part-time consultant. So, it is perhaps appropriate to say that I had my graduate education at Yale at a time when the only subject really that was studied by students in psychology at Yale was learning – the Psychology of Learning. Everything was learning, and it was all a very simple minded kind of learning an associationistic approach really, behaviorism, stimulus response, whatever you want to call it, the scene at Yale and indeed in all of psychology at that time was dominated by Professor Clarke Hull[60]. At all events, this is I did not consider myself a Hullian and I had not done my dissertation research under him. I had worked rather for **Donald Markowitz,** or he had been my supervisor. That turns out to be relevant because it was through him that I learned about you people. At all events, though my dissertation research was not Hullian in any proper sense, it was a simple-minded conditioning approach to learning and particularly to discrimination learning, which also turns out to be relevant. But then, after I got my degree, I joined the staff of what was then called the Yale Aeromedical Research Unit, a group then at Yale, which had undertaken to investigate the bends, decompression sickness, and anoxia this was in 1942. I believe the motivation for all this at the time was the assumption then being made that sooner or later our flyers would go to very high altitudes, altitudes where bends was a possibility and where anoxia was also a possibility since even if you are breathing pure oxygen at altitudes above 32,000 feet, the total pressure is less than the total partial pressure of oxygen. At all events, this Aeromedical Research Unit was created. It had a decompression chamber, which enabled us to simulate altitudes all the way up to 150,000 or more and very low temperatures at $-70\,^{0}$ Farenheit and 30 miles /hour gales and so on. Well, I worked there as a staff member, doing experiments at very high altitudes, and simply looking out after people who were subjects in these experiments and I did that for about three years. At the end of about three years, sometime in 1944, as I recall, the project began to die, I suppose, because by that time it was clear that either they were not going to be flying that high or they were going to be doing it in pressurized environments. So, the project sort of died a natural death sometime in 1944.

Frank: Was that funded by NDRC, do you think?

Pat: Yes, it was an OSRD, I am quite sure.

Caryl: Yes, I think it was.

Alvin: I do not think it's very relevant to talk about the research except to say that it was extremely frustrating, often painful, but it had something in common, I should say, with what we had to say a little while ago about the guidance devices and that was again the problem of the validity of our tests. We were in connection with our concern about the effects of anoxia. We wanted to know how are people affected by various amounts of anoxia, but the question is what effect. I mean, you know, if you look at critical for fusion frequency or something like that you can get effects if you go three feet up practically, but you know what difference does that make and, yes, it is the same

[60] Clark L. Hull, born 1884, died 1952. He became a Professor of Psychology at Yale in 1929 and developed a mathematical theory of behavior.

problem. We could do all kinds of objective tests. We'd get all kinds of beautiful data. We could get wonderful functions relating the performance to the oxygen saturation of the blood for an example. But, so what. I mean, there is no structure.

Well, I mean it is a serious problem and that was my first exposure to it. My experience with the guidance devices was my second. The research on bends was really quite different. That is simply a question of who is sensitive to bends, who gets bends and how can he prevent it, for example, by breathing oxygen for a long time and washing out all the nitrogen in the lungs. In any case, sometime in 1944 (my recollection is fairly late in 1944) Donald Markowitz, who had been my thesis adviser and was then chairman of the department at Yale, told me about Caryl Haskins and Frank Cooper whom he had met in Washington, These very interesting gentleman who had this very interesting laboratory and they were engaged in this very interesting research and were looking for some psychologist to be a consultant and he described the research and I thought this was just wonderful. So far as I was concerned this was a straightforward problem in discrimination learning, I mean, what was speech after all – speech was a set of sounds, you know, like the letters. There is a acoustic alphabet and there is an optical alphabet and here we had a chance to develop an acoustic alphabet and the only problem is as I saw it was first of all to make the acoustic signals, the sounds, the elements of this acoustic alphabet sufficiently distinct and different from each other that a person could learn it, and then to use everything that I knew about learning in order to devise ways of teaching it, and I was very-very cocky about this. I thought I knew all the answers, I thought the questions were quite simple, at least to me because I had all the answers. I thought it could be done and I thought this was a wonderful opportunity to make use. I had not been able to use anything in the work that I done at high altitude. I had not been able to use anything that I had ever learned, but here was a chance to use some concepts and principles that I had learned and so I was just very eager to. I thought this was a wonderful opportunity of a lifetime and I guess Donald Markowitz spoke to one of you and somehow we got in touch and I came down to the Lab and talked to Frank. I think you weren't there at time Caryl?

Caryl: Yeah, I wasn't there.

Alvin: I met Caryl and we talked about what was going on and then somehow it was agreed that I would be a consultant at the Lab. I think it was two days a week or something like that in the beginning.

Frank: Probably so, I don't remember, it was not long before you were working nearly full-time.

Alvin: Yeah.

Caryl: And you were still at Yale at that point?

Alvin: I was still at Yale at that point. Well, that's the way I got there.

END OF REEL 3 SECTION 1

Alvin: The frequency modulation device was already in existence, as Frank described it. My recollection is we continued to simulate the signals in that same way, didn't we Frank? Before we actually built that table and the thing that Eve Frankel used.

Frank: Yes, that was part of the philosophy of the whole enterprise was that you don't build devices where you can simulate the output and test the output. We were quite in agreement that the acceptability of the signal to the subject was a critical point. I do think we understood the complexity of the psychological problems, but we understood that there was a problem and that furthermore we were not a good organization and not well equipped to do mechanical designs or device design work in any case. We never expected to do that. That was true for the guidance devices as well and built a very complicated machine that never got used for stimulating guidance problems, but we'll skip that. We did indeed simulate using, as I recall it, a motion picture projector with a non-intermittent film motion to move the 16 mm film on which were images of the letters so that you had your text laid out in the same style as a Times Square sign and you moved it past any slit or any structure that you wanted to use up in front at a slow constant rate. Behind the structure in front where you could make that a single slit or you could make it a slit that's split down the middle so that you got upper halves of letters versus lower halves of the letters or we could have simulated the Optophone in that way though we did not, I think that's so. We did it with the actual instrument. Once you had more than a single signal coming through, there were various electronic manipulations you could do on it as to what you are trying, for one, I think the one you are referring to is what we call the FMFM system, not only the base frequency generated by the upper and lower halves of a slit were used, but also the signals were frequency modulated to add spectral complexity.

Alvin: Well yes, I think there is a little theoretical background for it. A little theoretical background.

Frank: I know. Mine is the electronic side.

Alvin: I remember you had grave reservations about my ideas, but you were broad minded enough to let me go ahead with it. Well, at the time that I came to work at the Lab, we were in a position to simulate the simple frequency modulation single slit machine and I think the Optophone shortly thereafter and the obvious problem was to figure out not so much whether this would work, but how well it would work and use that as a baseline then for further improvement. And again of course, there is sort of the question of validity I mean what kind of test is valid for this purpose. Obviously, we were not in a position to have people practice with one of these machines, four to five hours a day for three years, and so we had to use, what shall I say, simple paired associate learning problems. I remember we set up tests with something like 8, 10, or 12 words, each of which was scanned. So, we would have the acoustic signal followed after two or three seconds by somebody speaking the correct word and these were presented in various random orders to the subjects and subjects' job was to learn which word went with which set of the acoustic signals.

Frank: That was primarily a search for the best signal generation method.

Alvin: Yeah, you know, that was just 12 words, 14 or whatever it was and of course we had enough sense to appreciate that this did not give us a lot of information, but it ought to permit us to make comparisons and that is what we did. I think the one really good idea we had early on (and I think perhaps the only really good idea we had early on, I believe it was Frank's idea) was Wuhzi that is the notion that we ought to evaluate all of these machines by comparison with what could be done with human speech in a situation which the human speech would be an enciphered speech or transliterated speech. And **Frank** has already described how we did that. So for each of these tests, we would make an equivalent form of somebody giving the Wuhzi equivalent then followed by the correct word. And that proved to be very revealing because as **Frank** has already said, it was apparent from the very beginning that no matter what kind of projection one might try to make from these simple tests to what would happen after three or four years of practice. There was just a vast difference between what people could do with the Wuhzi and what they could with any of these signals just up vast, vast difference. As for the comparisons among the machines, as **Frank** has already said, it became clear fairly early on, and I think it was a valid conclusion in fact, that the single slit FM device was not a significant improvement on the old Optophone. It was, as I think **Frank** said, probably better than the RCA device.

Frank: The RCA device had this fatal defect of the scanning tone dominating everything else.

Alvin: But we were a little bit shaken to discover that now in 1945 or whenever it was, we really could not do any better than it had been done in whatever in 1918 and so we then worried about this. My very simple minded notion was that the problem with the FM single-slit device was that the signals were not sufficiently different from each other. That is the kind of pattern that one would get scanning the letter A was not sufficiently different from the pattern one would get scanning the letter B. And of course, the patterns were those, as Frank has described, a pattern of pitch change in time corresponding to the amount of black or white seen under the slit as it scanned across the letter. So my notion was simply to increase the number of differences between the signals for the various letters by in effect complicating the signals and introducing some independence in their controls. So I suggested that we split the slit that we use the something… Oh I don't remember the details, but I think it was the sum of the amount of black or white (these were white reading machines) the amount of white seen under both halves A&B to control the frequency. Then to use the amount of white seen under the top half to control the audio frequency at which that frequency was frequency modulated. And then to use the bottom half to control the frequency at which this was amplitude modulated. What else did we do? Did we do anything else? Was there anything else to do?

Frank: Well, that report on blindness, mentions a the number of these things and with very brief descriptions.

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Alvin: I was satisfied. It was my idea and I was satisfied after carefully listening to these that they were very complex indeed. I had never really had any experience with frequency modulating signals at audio frequencies and all kinds of weird and wild things happen, as I am sure you know. So that there are sudden shifts I mean the side bands take over beyond a certain point and so the whole pitch shifts, the quality shifts, everything changes. All the auditory characteristics of these signals were changing all over the place as the machine scanned. So, I was satisfied that these were just wonderfully complex signals and of course my assumption was that the listeners would be able to find something among these variations that would enable them to distinguish unitary discrimination. Well in the event, it was just so complex that it defeated everybody, could not make any sense out of it. My recollection is it really worked very very badly.

Frank: Did not score high.

Alvin: Did not score high. So we began to think then, and I think especially Frank, that there was going to be a fairly low upper limit on the way these machines would work. I should say at this point, and I say this with all due respect to Frank because I do not know what his assumptions were, but it did not occur to me until I had had my nose rubbed in this for couple of years that if I just simply remembered what I had read in an elementary psychology textbook in the chapter on hearing, I would have known that these machines would not work all that well because of limits on the temporal resolving power of the ear. I mean, if you are going to have an acoustic alphabet, then, obviously you've got a problem because if you think of how fast we read, reading at 300 words a minute which is not all that fast, and you think of the number of letters per second, make that calculation quickly, it is quite a few letters per second that you are getting and you think now that each of those letters has to be represented by a unit sound and these sounds are coming along at 20 a second, I mean that is going to be a buzz, you are not going to be able to resolve that let alone distinguish these things. But I at least hadn't thought about that. Perhaps Frank had. Maybe that was the basis for his assumption that something like 40 words a minute would be a limit to this. I hadn't thought about that in connection with speech that it never occurred to me. To say to myself, let's see now people can talk at a rate of 300 and some words a minute if you speak rapidly and if you take now the average number of "phonemes per word" and you multiply the one number by the other, you come out with somewhere between 8 and 20 of these per second and it just hadn't occurred to me that you could not possibly do that.

END OF REEL 3 SECTION 2

Alvin: That speech therefore could not be an acoustic alphabet. That speech had to be different.

Frank: My recollection now is slightly different on the chronology of these revelations. I cannot remember quite when we first latched into this line of thinking. I think it was only after we had begun to run into trouble with doing the straightforward engineering. I assumed that he could handle the psychology if I could handle the engineering and it turned out we were both wrong.

Alvin: It is just the wrong psychology, the engineering was okay.

Frank: The engineering was not so hot either. My recollection is that the rate problem began to engage our attention. At about that time in our actual experience, we were having real problems.

Alvin: Because we scanned various rates and it was just very obvious listening to this when you got up to some thing like, I don't know what we tried to simulate at the time, but let's say 70, 80, and 90 words a minute.

Frank: Not nearly that fast. We did not even try that fast.

Alvin: But, just a buzz you know.

Frank: That set us to thinking, as I recall it, of Morse code. Here's an acoustic code on which people have had lots of practice.

Alvin: Years, years, and years.

Frank: They did it quite successfully. How fast do they do it. That is not the easy information to come by, but when you do come by something… Well, you knew right off what the beginners rate is. You can get a license if you go at 13 words a minute. A commercial operator is supposed to take 35 with no problem and roughly twice that is a contest rate. So, well alright but that's a very simple-minded code, we can certainly do better than on and off with a single tone. So, there is some hope, and there is the other point to, that the need of the blind subject was supposed to make up for a lot of disabilities in the device. That is to say, he would be happy enough to be able to read at *ad lib*, that he would put up with a substantial lack of performance, a slow rate, difficult to learn signals and so on. But, I do not remember that we were making comparisons with human performance in speech until substantially later with the one exception of using some kind of high performance device as a control, mainly Wuhzi, but the thing we did not think about, or I do not remember thinking about any how, was the comparison of the rates the reading machine was generating and the rates that humans were generating when they talked. That came as a real shock and came late to my recollection. Another thing I might add here is that by today's thinking, it might seem odd that we didn't even consider the possibility, didn't even talk about it, of generating something that would not require the subject to learn a new language, that is to say generate synthetic English.

Alvin: We couldn't have done that anyway.

Frank: It would have been impossible to have done it, it was so impossible that the concept simply did not emerge.

Alvin: But, it was impossible because there were no optical character readers, I mean you would have to have an optimal character reader in order to….

Frank: There were a lot of reasons, the whole technology was in a sufficiently primitive state that we did not, and I am sure from my discussions with other people, a decade later, at the conferences that the Gene Murphy[61] organized that nobody there had considered this possibility or even was much interested in the idea that some kind of speech is necessary.

Alvin: But you know in that connection, I did try something that I would like to put on the record, because again it indicated how totally naive I was about this. It did occur to me more or less in connection with our thinking about Wuhzi to wonder how well the system would work if in fact we could synthesize speech. I don't know whether you remember this at all.

Frank: I don't really remember this at all.

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[61] Eugene F. Murphy served the Veterans Administration from 1948 until 1983. He was a tireless traveler, advocate and program administrator of VA supported prosthetics and sensory aids research. He died in the year 2000.

Alvin: I did it, Frank. So, I synthesized the speech in the following way. I recorded b, a, and g – that is I recorded a spoken equivalent for each letter. I recorded these on film and I got busy with my razor blade and glue and started gluing these things together. Taking them apart and gluing them together. Thinking well now I'll see, you know, this is a kind of Wuhzi, this is now going to be speech, but of course, it was not speech and that was an eye opener to me. I really could not understand. It just sounded awful and it didn't work.

Frank: That was an idea that got quite a play in 50s.

Alvin: Oh yes, there was a man who worked on that for 15 to 20 years.

Frank: On the west coast. What was his name?

Pat: Metfessel[62].

Alvin: Metfessel that's right. Continued to work on that.

Frank: His "spelled speech" or whatever it was called. Yes, we had the recordings around one time. I built another machine to do the experiment more thoroughly, but by that time, it was clear that we didn't want to do the experiment.

Alvin: At all events there came at time. This I do remember quite well that, for whatever reason, we had decided that these kinds of machines we were working on would not work very well, but that they might work at speeds we did not know exactly what, perhaps, 40 or 50 words a minute, but they might work fast enough and well enough that, as Frank was saying, blind people would find them useful.

Frank: I might add there Al, we had some evidence. You know the woman's name Pat?

Pat: Jameson[63].

Frank: Jameson in England had used the Optophone at 40 words per minute, I believe was her claim. It was extremely difficult to even try to find out what the real basis for that information was, but I have no doubt that she performed with unusual skill whatever her numbers were, but the numbers we had to go by were 40 words per minute and that seemed to us a high target, but one which had been achieved.

Alvin: One worth trying to achieve again, and therefore, we decided not to drop specifically the frequency modulation single slit device. And we thought it was important then to get some people to work several hours a day for a long period of time to try to get some idea of where this would asymptote out. You know, could they get 40 words a minute, 50 words a minute, 30 words a minute? And so Frank built another simulator. It was a table. There was a glass on the top.

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[62] Milton Metfessel was a Professor of Psychology at the University of Southern California. With support from the VA, he conducted experiments on spelled speech as a potential output for reading machines for the blind.
[63] Margaret Jameson was an elderly blind woman living in south London, UK who had owned and used a Barr & Stroud Optophone since her teens in the 1920s. She was known to be an expert performer on the instrument.

Frank: It was a box for examining films.

Alvin: You transilluminate the film and had a scanner and the subject could actually scan across the page, the photograph, there was a negative.

Frank: It was a negative made of, as I recall it, third grade reader magnified so that the slit height was about a quarter of an inch.

Alvin: And, our plan was to have several subjects begin to practice with this consistently everyday until they leveled off. And we started off with a young woman in the Lab who was working as a technician. Shall I give her name?

Frank: I think you might as well.

Pat: Eve Frankel was her name and we talked to her about what we wanted her to do. She understood and she agreed to work, I do not know how much time each day. And one of the young men in the Lab was asked to monitor her on this and she was given all this material and the idea was that they would spend an hour or two each day with her practicing. I don't recall now how long she had practiced before the day that I remember very well, the first day that I wanted to tell you about is the day that I was sitting at my desk writing a report of some kind to somebody about what we had been doing and our progress to date. And, I recall that I had just written that we had reason to believe that while these machines might be useful, they would not be all that useful. That the maximum rate would be no where near normal reading rate and I probably put down something like 40 words a minute as our estimate of the maximum rate. At all events, I recall that about that point, the young man who was monitoring Eve Frankel's performance and her progress, stuck his head in the door and announced that she had just hit 65 words a minute, I mean, something significantly beyond the figure that I just written down as the maximum.

END OF REEL 3 SECTION 3

Alvin: Well, she went on and on. She got better and better, and better and she was producing a lovely learning curve. We had data on the errors that she was making, on her speed each day, each week, and she was just getting better and better. She was sighted. We had thought that it was difficult for her, and indeed anybody else, to sit there for an hour with her eyes tightly closed. So to make things easier for her, we had given her a pair of Welder's goggles, as I recall, whose lenses had been just blacked out or replaced with opaque cardboard or something so that she would not have to sit there with her eyes closed. Well, she got better and better and I recall that at one point, she was demonstrated to the members of the Committee on Sensory Devices. The committee had included Karl Lashley[64], Stacy Guild[65], Zworykin, and then that famous physiologist from the University of Chicago, what was his name?

Caryl: Yes, I know who you mean, but I can't…

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[64] Karl S. Lashley, born 1890, died 1958. Director of the Yerkes Laboratories of Primate Biology in Orange Park, FL

[65] Stacy Guild then worked in the Otological Research Laboratory of Johns Hopkins Hospital, Baltimore, MD.

Alvin: It's a Swedish name…

Caryl: Swedish name[66].

Alvin: He was quite elderly at that time, very well known. At all events, this distinguished group of scientists and she was demonstrated. I cannot recall the rate that she went at in this demonstration, but do you Frank, I would say it was over 100 words per minute.

Frank: I wasn't here. I will add to this one when you get through.

Alvin: Pardon.

Frank: I was not here.

Alvin: Okay. I think it was over 100 words per minute that she was doing at that point. This was quite impressive. Well I cannot tell you exactly what happened after that except that someone – and I think it might have been one of the committee members – wrote and asked if we had really made absolutely sure that she was not peeking and I remember thinking that it was absurd to suppose that she would be. This was not an experiment that we were doing, nevertheless, it was reasonably well controlled. We were keeping track of all the things, that track should have been kept of. And it was in a perfectly reasonable kind of way that we were proceeding and we had controlled everything except the obvious thing. We had not controlled against the possibility that she was cheating. It never occurred to us, certainly never occurred to me that she would cheat, that there would be any motivation to cheat, why on earth would she cheat. For one thing she was bound to be found out. Indeed, at that point, we had already started, I think, two other people working on this. Though she was way ahead in time, I mean she had been at it for several months and they had been at it for only a couple of weeks. At all events, I think that one day when she was reading shortly after somebody said, "why don't you make sure", I think Paul Zahl got out one of his old…

Frank: I can fill you in on this one.

Alvin: Okay go ahead.

Frank: It shows how people's recollections of events tend to vary over the years, but I can anchor mine pretty accurately. I spent part of the early summer, well very shortly after the VE Day[67], in England and a day or two on the continent winding up the affairs of the liaison office overseas branch in London and we later had a branch in Paris. As head of the office, it was appropriate (and it was a nice junket) to go over and see what had happened there. So for roughly six weeks, I was out of the country at the time that this experiment was underway. I think I had a note from you that the things were going swimmingly and was delighted……

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[66] Probably Anton J. Carlson, born 1875, died 1956. At the time, Professor Emeritus of Physiology, University of Chicago.

[67] Victory in Europe Day was May 8th 1945.

Alvin: Not swimmingly. I was kind of put out that she was making a liar of us. I mean… I mean… She was… She was showing us we were wrong.

Frank: By the time I got back, I got this roaring report of what she was able to do – that the Committee had seen it, was impressed, and I just had to go watch this. It was wonderful and I was delighted. I walked back and sure enough and for no reason that I can remember, I reached over and picked up a sheet of ordinary paper like this or cardboard and just slipped it between her eyes and the board and the reading stopped. A little later that same day, I was about to go up on the elevator to the floor above and somebody tapped me and said, "gee, the radio just came on with an announcement that we have dropped an atom bomb on the Japanese". I said, "Oh! and went on up the elevator". So, I can anchor it to the exact date.

Alvin: We have the date right, but my recollection of the event is little different in two respects. I had thought it was Paul Zahl who came up and put a…

Frank: Well, I recollect doing it myself, but may be I didn't and he did but…

Alvin: Putting an old photographer's hood over her and the reason I remember that is that I recall that her reaction was that she began coughing and complaining and got very upset, and got up and ran off upstairs. I also remember that just a few minutes later, apparently the person she was working with got her to start again and she did read for him. But, then he got upset because he thought he noticed that she was reading a word on the next line before she got to the next line. And he apparently said something to her about this and this was I think just after this other business and she simply confessed to him that she had been cheating and she left the Lab. She had been cheating, as I recall, by leaning against her fist, putting her fist up against her cheek this way and pushing the flesh of her cheek up and thus raising the bottom of these goggles and just peeking underneath, but of course she had been very clever about the thing because she had produced a nice learning curve, she got just a little bit better each day didn't overdo it each week. You realized that she wasn't reading, you know, she could read at 400 words per minute, but she wasn't trying to do that. Well she left the Lab. I'm going to ask Caryl to pick up and tell about how he went around and told the Committee members about this.

Frank: You had to mop up the….

Alvin: Yes, you had to mop up.

Caryl: Explain

Alvin: But, I would just add that I waited two or three weeks and then called her and made myself seem as friendly as I possibly could, which wasn't easy, and I told her that all was forgiven, which wasn't true. And I said that it was important for us to save as much information as we could about this. So, would she answer a few questions for me. She said "yes". I asked her if she cheated all the time. She said, "no". I asked her can you read without cheating, she said, "yes". I said how fast do you think you can read without cheating, she said, "well I don't know, but I can read". Then I asked her if she would mind coming back to the Lab so that we could test her and find out how fast she could read and what kinds of errors she made, what letters, what words, and so on were giving her trouble, and she agreed provided she said, she could come back at a time when there would be only two people at the Lab, **Bill Mekes** remember? and me. So, we arranged a Sunday meeting. She came to the Lab. Bill and I were there and we were very friendly, and we gave her coffee and cookies and we chatted for a while. Then we invited her to read and she started reading. My recollection is that she started reading now very slowly may be five words or ten words a minute something like that and making errors. You know, I was taking notes. She was wearing the goggles again and at that point, I didn't know how she was cheating with the goggles. Then to my recollection, Bill Mekes walked up with a piece of cardboard and slipped it between her and the print and she stopped. Well, at that time we just gave up and we sent her out of the Lab and we've never seen her since. But I do recall that we did not conclude – whether it was you or Paul – we did not conclude on that basis that she was cheating. That is, there was some other hypothesis that was possible that she was upset because we did not believe her something like that. She became upset, I remember, at Frank, and ran off…

Frank: I think the sack over the head may well have been just the way you describe it, but that it happened before I got back. My recollection is that this was the first day I was back in Lab and this was really exciting news and I recall very clearly whether it's a fact or not. Nevertheless, I recall it clearly having been just skeptical enough to say well can't hurt to try this, we are trying it and it did stop and I at least drew the conclusion that this was cheating at that point and was very deeply disturbed by it.

Alvin: Well I did not (whatever had happened) I did not conclude that she was cheating. I concluded that she was upset and it wasn't until later when – I can't remember who it was, who was working there – came running down to say that she had confessed.

Frank: I think you should add to her accomplishments. She had more difficulty when there were occasional foreign language words introduced into the text. Than when there weren't…

Alvin: I don't remember that.

Caryl: Had trouble reading them.

Frank: This is hearsay not recollection. I was not here while all this was going on and at the time, I came back, the story sounded very rosy indeed and it was very shortly after that that it sounded extremely dismal to my recollection of intervening events that I've just given. **Caryl,** do you want to…

Alvin: Why don't you?

Caryl: Well, I would like to turn back to you for a moment, Frank, because we were talking about it in the layout and you were saying that you thought it had quite an influence on Murphy…

Frank: Yes, I'd be glad to pick it up.

END OF REEL 3 SECTION 4

Caryl: Well I simply went to the Committee and went to George Corner[68] particularly and explained it. They were sympathetic, but still skeptical and that's all I recall of that. I left the story with them and they did not take any specific action in respect to us about it, were tolerant, but sympathetic. I think that is all…..

Frank: It is my recollection that you went personally to every member of the Committee….

Caryl: I guess I did.

Frank: And explained just what had happened.

Caryl: Yeah.

Frank: And because we knew very well that the reputation of the Laboratory was….

Caryl: Really was hanging.

Frank: Gone!

Caryl: That is true and they were very very tolerant and sympathetic. And that it was one of those things. Will you want to go on.

Frank: Well, that should have closed the story all right, but actually a thing like that hangs around for years…

Caryl: Yeah.

Frank: You don't know about it because people don't talk to you about it…

Caryl: Nobody says anything…

Frank: But they will talk to each other about it and I got evidence for that from Gene Murphy, who in the early 50s (this, what we are talking about, happened in '45) was starting to get the Veterans Administration into sensory aids research. And he ran several conferences on guidance devices and reading machines. Strangely, it seemed to me, we were not invited. There was one in New York that I pretty much invited myself. They were open to the public, and went to it. I talked with Gene (either that day or some other time shortly thereafter) roughly about the things and about the point of view that, by that time, we had come to at the Laboratories. That the only thing that was really going to serve as an output for this was something that was at least speech or speech-like – I think the way we were phrasing it at that time. And I was telling him about our research and synthetic speech – how it supported this point of view. And he was decent enough to say, "well now look, I was sort of skeptical about this because I have heard reports about a cheating incident at your Laboratories and I don't like to take that kind of thing seriously, but you have to understand that, I must consider it." I sat him down and gave him the complete story as it was just

[68] George W. Corner was the Chairman of the Committee on Sensory Devices of the Office of Scientific Research and Development.

laid out. And he understood and was a gentleman about it and was a friend of, and believer in, the Laboratories work from that day on. But it was a rumor that had to be laid to rest explicitly…

Caryl: In fact by conversation…

Frank: That is one particular case. It had consequences. Whether the same thing has been a rumor that has had consequences I don't know about, I don't know, but it is not unlikely. It is a fact of life that that kind of thing can ruin a small organization's reputation. It happens all over the world all the time. That is one reason I think we are having it on the record here that this is not something that we are sweeping under the rug. Nor is it something that we make a great deal of, but it happened and it had consequences.

Alvin: We never did. What we should say, at all events, is we did not go on, as I recall, trying to find out what the upper limit was. We just abandoned it.

Frank: We abandoned it.

Alvin: We decided that we were convinced ourselves that this was really not worth pursuing, that this was not the way to go, that my early assumptions at least had been wrong and that there was something very special about speech. It was not just that people had had a lot of practice with it. It was just that there was something special about speech and that the thing to do then was to turn our attention to speech to find out what made it so good. Why did it work so well? But with the notion that once we answered that question, we might very well be able then to design a reading machine for the blind that would work. But, we really had to abandon devices for the time being. Is that a fair statement?

Frank: I think that's fair…

Alvin: …and do some basic research.

Frank: I think it is also fair to say that we were still laboring under what we now consider an illusion that there were ways of encoding the information…

Alvin: Oh! Yes.

Frank: That could be used in speech, but could be simulated by a machine.

Alvin: Oh! yes. Without speaking?

Frank: Without speaking. That speech-like was what we had to shoot for. There was, to my recollection, no concept that you could really do it with speech. The technology just wasn't there and that speech-like would probably suffice, I think we would not accept that point of view today, but we had to learn a lot more about speech before that was clear.

Alvin: I would agree Frank. I would put it from my own my point of view a little differently (and I think perhaps from yours too) and recall for you some of the early things we did. We were agreed that there was something very good about speech. Obviously these signals we were producing were not in the same ballpark as speech. But, we were willing to consider the possibility that speech had developed in such a way as to take advantage of certain principles of good organization ….

Frank: Auditory organization.

Alvin: Right. That were not necessarily specifically for speech, but were principles of auditory or even more broadly of…

Frank: Perceptual.

Alvin: Perceptual organization, something like the gestalt ideas. You know, there are conditions of good figure, good continuations and so on, and that we knew that, in fact, if one looked at all the research that had been done on auditory perception it was really all auditory psychophysics. That we could not find any studies in the literature about the perception of complex sounds. That there was nothing in the auditory literature corresponding to all these gestalt phenomena that had been so much talked about.

Frank: It was totally visual.

Alvin: Yeah. All of the gestalt stuff, all that talk about organization was all visual. So, one of our early ideas was that we wanted to investigate the perception of complex sounds, of which speech happened to be the subset to find out what these underlying principles are. And the idea then was that was once we knew what those principles were then, as Frank was suggesting, we could probably design a device that would not necessarily speak, but that would produce signals that would conform to those principles and therefore be… be…

Frank: Be learnable.

Alvin: Be learnable and dealable with and in fact, we were really quite explicit about that that in one of our first publications (one that I think we would now have to say we are not proud of, we are just very happy that nobody has really read it) was one in the ***Proceedings of the National Academy of Sciences***[69] and it is called the "Interconversion Of Audible And Visible Patterns". We were considering the possibility that there was a set of principles of perceptual organization, if you will, so general that they cut across all modalities. And that, in order to find these principles and make them work across modalities, one had only to find the right transform. And so we said, aha! The spectrographic transform is approximately the right transform. Yes, I mean you are substituting. The Y-dimension of space becomes frequency or timber or something like that. The X-dimension of space becomes time and you've got a third a dimension of intensity and that is what you have in the spectrogram. And so, we started doing little experiments. We never really carried out terribly formal experiments in which we would draw… The question was this, given that one has in vision, a pattern, which is recognizable as a particular kind of pattern under various

[69] Cooper, F.S., Liberman, A.M. and Borst, J.M. (1951) The interconversion of audible and visible patterns as a basis for research in the perception of speech. Proceedings of the National Academy of Sciences, 37, 318-325.

transformations, rotation and so on, then how do you transform this into sound, so that this invariance is maintained? Okay.

Frank: Size invariance and orientation invariance.

Alvin: So, we actually drew triangles, squares, and circles of various sizes and various rotations and orientations and so on and recorded them on the old Playback.

Frank: And you can see that in the CBS[70] film.

Alvin: That's right… that's right. And the question that was asked of the subjects – as I say we never really carried out terribly formal experiments, but we did make some observations in which we asked people to group the sounds. And the question was, would they group the sounds in the same way that they would group the optical displays? That is, would all the triangles be put together regardless of size and so on, and the answer was partly yes and partly no. But our thought at that time was, if it did not work perfectly, it was only because we did not have the perfect transform. That this was only an approximation to the right transform and that you know that may be had to be worked on. I do not know how long we entertained that idea. I think we abandoned that fairly quickly, but we did not….,

Frank: In the mid 50s I think…

Alvin: I am sorry to say Frank that we did not abandon the whole idea. I think we abandoned the idea that these principles might work across all modalities or at least we weren't going to pursue that. We did not pursue that in fact,

Frank: We did not pursue that.

Alvin: But I think we… we held on for a long time (a rather long time) to the notion that there would be principles that were general for the auditory modality. Nothing special about speech you see, just speech was special only because it was smart enough, if you will.

Frank: That there was an auditory gestalt psychology that hadn't been developed yet.

Alvin: That's right. And that somehow people spoke in a way that caused these principles to be obeyed – that produced acoustic signals that obeyed these principles. I can't tell you. I do not know. I can't remember how long. In fact, it is impossible to date these things because these ideas slid, you know, we were holding several of them at the same time, much of the time, and they just sort of merged from one into the other.

END OF REEL 3 SECTION 5

Alvin: It was at least a little while later before we got around to the idea that it sort of had to be speech and that was really the …..

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[70] A movie of the CBS network feature in the *Adventure* series hosted by Charles Collingswood which was filmed live from the Laboratories.in 195…?

Caryl: That was a breakthrough.

Alvin: Well… yes.

Frank: A wear through.

Alvin: Pardon? A wear through, yes. But, even then, I think there is another story to be told here. Our ideas about that have undergone considerable evolutionary changes

Frank: Yes.

Alvin: Considerable development. But I think that really sort of goes back to our first notion about what came to be called the motor theory and, you know, I remember vividly how that notion came about. We had done one of our first formal experiments. But, in a way, the first formal experiment we did was that ridiculous experiment on the bursts. I say ridiculous because we were producing these absurd sounds. We had made seven steady state vowels, two formant vowels, that really weren't very good vowels and there is a big background to this, which I won't bother you with. We had observed… I think I am getting ahead of the story here, but ….

Frank: I think you are getting ahead of the story…

Alvin: So let's leave it at this that there was a period during which we were looking for principles of perceptual organization not necessarily specific to speech and indeed, as I said, in one case for a while, so general, that they would cut across modalities. And that was sort of a period in our research.

Frank: Now if you take it back to the beginnings of that understanding, they came rather late in the total CSD enterprise, which began in '44, if I recall, fairly early in 1944.

Caryl: I think that's right.

Frank: And was totally wound up by mid-'47; contracts having…

Caryl: Contracts having expired and…

Frank: Simply expired. It was a period of very intense activity. The Laboratories grew to have some 20 or 30 people in it working on the combination of testing reading machines, testing guidance devices, and working on various aspects of the reading machine problem. Over a period of six months or so, most of which time was spent in writing two very voluminous reports, the funding lapsed and the whole enterprise had to be canceled. The Laboratory dwindled back., Al came down at weekends I think. John Borst[71] was there part-time a couple of days a week since

[71] John Borst was an engineer who was hired in December 1946, discharged in June 1947, rehired and retired in 1965.

he had another job he could afford to operate on that basis. David Zeichner[72] and Bill Winter[73] in staff positions and I and Paul were the only people left who had any contact with this work. But, before that happened, at about the beginning of the drop off, which was, let me say, precipitated not so much by the fact that the work was not really getting any place in the sense of short-term results. Though, I think it had become clear to almost everybody including the Committee that this was a long-term operation, not a short, quick enterprise. It had been laid on for long term thinking but, in the back of everybody's mind was the idea that here are these returning veterans and you ought be able to do something for them. So, there were short-term expectations and when it began to be clear that they were not going to be realized, that you are looking at something 10 or 20 years ahead instead of one or two years. We and the Committee began to lose enthusiasm and what really ended it though was the end of the war and the OSRD closed itself down. Dr. Bush began doing it even before VE Day, definitely terminating contracts, winding up and so on. This one was one of the ones that dealt with post war problems that held on and it got financing for a short time thereafter from the Surgeon General's Office and eventually that was through some lined up operations from funds from the Surgeon General's Office to the National Academy, if I remember it correctly, there was a National Academy Committee, but by that time our operation, except for report writing, was essentially already done. One thing, from the Laboratories point of view of residual value, was that we had in the meantime acquired a very substantial volume of electronic equipment and other laboratory tools, which we were allowed to retain for the sum total of $8000, I believe. Which was less than 10 cents on a dollar, substantially less than original cost. So, the Laboratories did have a good collection of then current electronic equipment, though nobody to use it. Another thing that was a residue of it (and it happened about the time that you were talking about the disillusionment with simple minded signal generating techniques) was that through the OSRD liaison office had passed some documents that I happened to see and which influenced my own thinking quite clearly. One of them was a brief description of the sound spectrograph, which was then a highly classified instrument used for breaking Japanese voice codes. But I think the code breaking aspect was not implicit in the documents I saw, but anyhow, a sound spectrogram as a display for speech and an early attempt that R K Potter[74] made to get a patent for this by showing that it contained the speech information. He had built some kind of Pattern Playback, in other words, they had made a sheet of this stuff, passed it in front of a light beam and claimed that they got intelligible speech out of it. I have never heard the speech and I doubt the claim, but nevertheless, it is I think in the patent literature. I have tried in several publications to see to it that there was credit given where credit was due for that basic idea of a Pattern playback device. But I doubt that they made it work and I know that my first attempt, which was motivated by that, did not work. I am not even sure I expected it to work, but I tried essentially the same thing. I wrapped a sheet of film, made up something like a spectrogram, and went to considerable effort to manufacture a spectrum film that had individual frequencies, intensity modulated on bands like this and wrap that around the same cylinder. Used a line filament down the center of the thing and drove the whole thing at speech rates. In other words, about the same rate that a sound spectrogram drum goes in the record mode and with photocells out in front to pick up the sound. I never saw the belt device. I have asked Potter about it and he said, "Oh

[72] David Zeichner was also an engineer hired in August 1944. He retired from the Laboratories in 198…?
[73] William C. Winter joined the Laboratories in January 1946 and resigned in January 1970 occasioned by the Laboratories' move to New Haven, Connecticut.
[74] Ralph.K Potter was an engineer at the Bell Telephone Laboratories and the author with George A. Kopp and Harriet C. Greene of the book entitled "Visible Speech" published by D. Van Nostrand Co. Inc, N.Y., in 1947.

well, it barely worked " and we put it on the shelf and lost all interest in it" and I have heard that from other Bell people too. It was purely a patent operation, but it was obvious why it didn't work and since we, by that time, wanted very much to work in this area, I set about (still at the end of the CSD operation) to build first a spectrograph because, in those days, we wanted one. You either had to be at Bell or you had to build it, besides I wanted one that was photographic that would generate photographic transparencies rather than blackened paper

Alvin: We also wanted higher fidelity.

Frank: I wanted substantially higher fidelity in terms of the dynamic range of the amplitude to density dimension and also a Playback device, which was designed…. And that is why you may have heard it referred to as PB2. It was literally the second playback that we tried to design. The first one failed totally because of signal to noise ratio. You just can't do it that way. I don't think you can do it by current technology that way. What had to be done was to disengage the tone generating operation from the spectrum scanning operation so that you could use a large tone wheel that went very fast and therefore get a (*large*) area to put the light through so that you get enough light that you don't get killed by photocell noise. And then you use the best photocells you can with a good light collecting system and you scan separately so that the two rates, the generation rate and scanning rate, are unlocked. The present Pattern Playback is what came out of that. It was a combination mechanical, optical, and electronic device. The electronics were fairly simple. Though there are some balancing circuits and what not, that take a little doing to close down. The optical scoop shovel was, as far as I know, novel. Certainly, it probably is not a patentable idea, but it was an answer to a problem, if I can put it that way. Well, those I think the lathe beds were built and some of the machine work had been done on the Playback, much of it had been done on the spectrograph, by the time the CSD program dropped. So, that was another (residue of the CSD program, we now had) both the idea of wanting to work on speech and we clearly had changed our objectives from working on reading machines to working on understanding how humans understand speech, what are the cues for human perception of speech and (we had) a good deal of machinery with which to go ahead with it if we could only get the manpower to do it; that was another gift from the CSD.

END OF REEL 3 SECTION 6

Alvin: If I may say so Frank, I think that it would be interesting to put on the record that there are some interesting ideas behind the idea of building the Playback that is, where do we stand at this point. We had arrived at the conclusion that there was something very good about speech and that we did not know what that was and we could not find out by looking at the literature. We did not have a theory. We did not have an hypothesis really at that time about what was special about speech what we did have a theory about, or at least what Frank had a theory about, was how to go about finding out and I think you should not scant that. I mean, he did not just build the Playback. I mean, there was a sort of a theory about the method if not a theory about exactly what we were going to test. And that theory had a very…

Frank: And that theory we did not inherit from Bell.

Alvin: That's right and that theory had certain interesting aspects, for example, that one needed something like the Playback in order to be able to experiment with the dynamic aspects of the signal. That, in fact I made some notes here, we wanted to be able to manipulate. There is always a question, manipulate what? Okay? well…

Frank: That was indeed the question.

Alvin: Well yes, but we had to answer part of that, I mean, you know, we knew that we wanted to manipulate something like these formants that showed up on spectrograms.

Frank: Yes.

Alvin: I mean that's… that's already a kind of a hypothesis. But even more interesting I think and I think we, and especially Frank, were quite explicit about this at the time. Our situation was this. That we did not have any specific hypothesis to test that, but what followed from this was that we would have to do a lot of trial and error work. What follows from that is that you need experimental convenience and you need conceptual convenience in order to do that work. Now, that turned out to be just terribly, terribly important. Let me get ahead of the story a little bit and remind you that after the Playback was built and operating, several roughly similar devices were constructed, but none of them was ever used very much for experimental purposes because in one way or another they lacked conceptual convenience and experimental convenience. It was not possible with them to run 200 experiments a day.

Caryl: We could by others.

Alvin: That is right, by others. We could never have done what we did with the Playback, if in fact it had taken us, let us say, two hours to generate each pattern, we just never could have done it. I mean the number of patterns drawn and tested over, I do not know what the first seven or eight years, was thousands. And I don't think we could possibly have done the job otherwise. So, I think what is very important here is that the Playback was designed for experimental convenience I mean the right aspect ratio so that it was easy to draw what you had to draw, that the paint would dry rapidly, that you could erase it and change it without scratching the cellophane and all kinds of experimental things like that. But then also I think the notion, which was very explicit at that time, and especially I think in Frank's mind, that it would be important to be able to think about these patterns in simple visual terms.

Frank: That is still a part of the interconversion problem; not that we are looking for the interconversion principles, but that good patterning was good patterning.

Alvin: That is right.

Frank: And if you can….

Alvin: Now I discovered how important that was when in 1962 I worked in Gunnar Fant's[75] lab for a couple of months using OVE II. Now, one of the problems I found with OVE II was that all lines representing formants had to be continuous. And you had to indicate on a separate control down at the bottom when you wanted the sound on and when you didn't want it on. A very simple consequence of that was that I could never look at what I had drawn and see a spectrogram. It just was not possible to look at it and say oh yes, I did not turn this down far enough, or it starts a little too low. I never knew where it started and, you know, I have to study and say, well, let's see it if it starts. That turns out to be just terribly important. It isn't important if you know exactly what you are doing. If you have a particular hypothesis that you want to test, what does it matter if it takes you two days to create the signals that you are going to test. So you spend two days creating a couple of signals and then you test them. But if you start out not having the foggiest notion what you do want to test (and that was exactly our situation) then you can't make progress that way and in fact that is not the way we made progress. The way we made progress was once the machine was up and running we did this copy synthesis. We took these 12 Harvard phonetically balanced sentences or whatever they were called, you know, "the birch canoe slid on the smooth planks", "never kill a snake with your bare hands", and so on. And we just simply copied them and simplified and went back and forth by trial and error until we had simplified spectrograms that when converted into sound were reasonably intelligible and then we asked the question what have we done. I remember ...

Frank: At that time we had a little bit of guidance out of the Potter, Kopp & Green book and a little bit from Joos'[76] book on Acoustic Phonetics.

Alvin: Yes a little, but still not much.

Frank: not much.

Alvin: Not much, because I remember vividly that the first phrase that I recall that we had actually succeeded in copying was "never kill a snake". And I remember asking the question about "kill". First of all, where is the L? And, you know, with the Playback we could turn it through by hand. You could stop it and go slowly and I remember going through that pattern looking for the L and not finding it. Yet when I let it run it said 'kill'. But then, when I went through it looking for the L, I could not find the L. In fact I could not find the "i" and suddenly the light dawns. I mean really the dynamics are important. Now say what you will about how Potter Kopp & Green talked about the hub and all these kind of things, it did not register with me at least. Okay?

Frank: I am not sure it registered with them.

Alvin: I don't think it did not register with them and so you see we had no idea what we were doing. I mean we were looking for the word. We were obviously expecting that L would be somewhere near the end of that acoustic syllable and it wasn't. Okay? I didn't know what I expected about the K, but I do recall that I assumed we had drawn a little burst which we had

[75] Gunnar Fant, born 1919. Currently, Professor Emeritus, Royal Institute of Technology, Department of Speech Communication and Music Acoustics, Stockholm, Sweden.
[76] Martin Joos, born 1907, died 1978. The author of "Acoustic Phonetics" published in *Language Monographs*, 23, Baltimore: The Linguistic Society of America, 1948.

copied from the spectrogram and I remember saying well, obviously, that is the K. And so I erased it and played it. It said kill. Oh! My God, what is going on here? That is not the K or at least you don't need that for the K. So, I mean all of this was just starting out from nothing, but you see if we hadn't had something like the Playback, we could never have done that. We did know what to test. Now, imagine think of this Pat, think of trying to do this working from oscillograms. I mean in principle, one can imagine doing that yes (it is impossible, in practice it is impossible). So, what I am trying to say….

Frank: You mean like the chap in the Canadian film labs, you may remember his name, who did do some music and even speech-like events by manipulating sound track.

Alvin: Yes, Oh what a job! So, what I am trying to say is that there were some interesting ideas that lay behind the whole notion of the Pattern Playback. They are not trivial… They are not trivial at all.

END OF REEL 3 SECTION 7

Pat: Much of speech research in that period (just to try and sketch in the context here) was interested in the problem of bandwidth compression. I mean, these were pre-satellite days and there was some concern about that. So, your contemporaries were working in an entirely different area when they were using speech synthesis. Why was it that you went in the direction that you did? Why did you have the interests that you had at that time that were so different from everybody else. Were you in fact totally isolated from that other activity? Did you feel…

Frank: At that time, I did not know much about it and I don't think Al knew.

Alvin: Oh! I didn't know anything about it. I mean the bandwidth compression, it is true that the people in bandwidth compression were among the first to be interested in some of our work.

Pat : Some of these other synthesizers were designed really for experiments in that field.

Alvin: Well perhaps.

Frank: I think there is an interesting point in that Pat, and it turned up in the guidance device thing too. That an instrument needs to be designed for the problem it is to be used for. This one was, as Al said, designed with some care to be able to review the transmission or reflection of spectrograms. Big enough that you could change them because we clearly intended to do some changes on the things.

Alvin: We wanted to experiment.

Frank: That was the whole essence of the thing. It was to be able to look, change, and listen. And use the perceptual system as your guide and it was going to be very difficult to make a machine that would meet those needs and still provide pitch inflexion and so we said well let's leave pitch out.

Alvin: Yeah, we took a chance.

Frank: It is undoubtedly important in the speech, but one can understand monotone speech, let's simplify the problem and keep the machinery useful. I am sure that for **Walter Lawrence's** device, I am less sure but think it's true for Gunnar Fant's, which were the two principles synthesizers that were built following that (*the Playback*) by the early 50s. The idea that the Playback was a monotone device, was a fatal defect, and (the other synthesizers were motivated by the notion) I can do better with better engineering. And both of them brought better engineering to it.

Alvin: And better speech as a matter of fact.

Frank: And better speech. But they were both for one reason or another largely useless as research instruments for the problem we wanted to research which was what are the acoustic. We said acoustic cues at that point, because we were still thinking in this transform mode that there are critical aspects of the signal that determine its intelligibility and that seemed to be supported a little later by the things like the Hub, as an observational invariant. And, it turned out to be in fact a real advantage to have not had a pitch inflexion capability in the old Playback. It made it much simpler and it made the problem of interpretation easier as we discovered when we later on built our own version of a pitch-inflected device using a vocoder. Pitch only complicated the problem. It led you into different problems.

Alvin: We didn't have a noise source either. We only had 60 harmonics.

Frank: That is right. How do you get noise out of it?

Alvin: Well, we took our chances.

Frank: We could randomize the amount of time and occurrence of tone burst and …

Alvin: Make them twitter…

Frank: Make them twitter. If you do it adequately it's fair substitute for noise. Well it is noise. But of white noise.

Alvin: Well I think there is an interesting point there Frank, a general point to be made. Another assumption that I think we weren't quite so explicit about at the time (at least I don't recall that we were, but it is nonetheless interesting) is that the success of this method depended upon there being an almost complete orthogonality between what you might call naturalness, on the one hand, and the phonetic information on the other. Because we must have known, we had to know, we certainly discovered early on that the speech that we could produce would never sound to anybody like natural speech and we did not have noise. And in fact, Frank says well okay. We could make these little dots and create noise. That is certainly true. But, if you do that and then play that by itself it really sounds like birds twittering. I think, yet if you put that in a proper context with formant transitions after it and a vowel then it will say 'sa' or 'sha'. Now, the interesting thing is that we have never had reason to modify any conclusion that we drew on the basis of these highly artificial, not human sounding sounds. When later, it became possible to make them more nearly human. You follow me? That is to say, the results that we got from these twitters are real. You have to probably listen to them. The results that we got from these twitters hold up when you have real noise. I mean, you investigate the difference between sa and sha, sa twitters, and sha twitters and it turns out to be exactly right for sa noise and sha noise. And that is interesting. There was a complete orthogonality there. And, in fact, the very first experiment that we did was one with these crazy bursts and steady state vowels that I mentioned to you. And I recall making the patterns, randomizing them and then playing them for Frank and Pierre and me. And we made judgments pa, ta, ka. And I went into my office, I analyzed them and they made sense. I mean, there were patterns emerging from the data. I had great excitement and I remember I showed this to Frank and Pierre, who also said well yes, but, you know naïve listeners will never do this. I took them up, these ridiculous sounds, I took them up to Storrs and I played them on an old '78 phonograph, a little one, portable, in a great big barn of a room and I asked these subjects to listen to each of these. I said, "I am going to play you some syllables and each syllable is going to be either P, T, or K followed by some vowel and I want you to write down P, T, or K and guess if necessary. I remember saying "now, I will play a few of these for you so you get the idea" and I played a few, and there were roars of laughter.

Caryl: Chuckles.

Alvin: They all thought it was, you know, a joke of some kind. Or I'm a psychologist and I'm really doing something else, you know, psychoanalyzing them or something. But, they did it and I remember they passed though I couldn't hear a thing. I took the sheets home and I tabulated them and got the same results that we got for the experts. Now, the data were noisy, but there were clearcut patterns. I think it is fair to say that those data still hold up. Amazingly, they are still true. They are still right. So, there is a very interesting assumption that at least in my thinking was never quite explicit. I think I began to worry about it a little early on and I remember a lot of other people worried about it. I recall people coming to the Lab and commenting surely this won't work for real speech. They would listen to this obviously machine-type sound and then say well that is all very fine what you are doing, but what has that got to do with real speech. It turns out interestingly that its got a lot to do with real speech. There is another assumption that we had to make that I do not recall ever making explicitly and that was that the cues (if I may call them that) had to be largely independent of each other. Or else, we were in an impossible situation, because we could not possibly vary everything at the same time. So, you know, we would vary the second formant transition and we would get a result then we would vary the third formant transition and get a result. Now, if it happened that by putting those two things together you'd get a totally different result, we could never have gotten anywhere. And in fact, fortunately, it turned out that these things are sufficiently independent that we were able, as a practical matter, to study them one at a time. And that is an interesting characteristic of speech.

END OF REEL 3 SECTION 8

Alvin: I guess what I am trying to say is that, while we did not have a theory of any kind about the nature of the relation between the speech signal and the phonetic information it was conveying, we did have a kind of theory (sometimes explicit and sometimes not) about how to go about finding out. Because, there were many other ways. In fact, other people had been studying speech perception for many years by many other methods, for example, filtering.

Frank: At this time the literature consisted mainly of a book by Fletcher[77]**.** I do not remember the title of it but it was a classic text at the time, with a chapter devoted to speech. He was of course much concerned, as the whole Bell Telephone System was, with what you have to convey to let people be satisfied with the telephone and still not cost too much. So the upper and lower frequency bounds, the dynamic range and all that kind of thing, the frequency flatness, phase shift had to be carefully considered. And there was a small chapter on speech, which went back to early work around the 20's and so on, with Helmholtz resonators and what not. So the vowel system was reasonably well understood. There were one-formant vowels and there were two formant vowels and that accounted for everything. It was a sort of steady state tone and then there were consonants, and with a whole one-page table, as I recall it, of the various frequency and intensity type characteristics of the consonants. That was what was known about speech at that time. We were not totally in the dark about vowels and here in the spectrograms were dark bands, that equivalence was in the air. Everybody who looked at all knew that. Beyond that nothing and I don't not remember the publication date on the Potter Kopp & Green book though I think the Bell Laboratories papers on the spectrograph came out in 1946 or 1947 or something like that. We already had a spectrograph so nearly ready to go that that was rather little more than confirmation of what we were trying to do. It was not much.

Alvin: There were two other things, excuse me Frank, I think, should be mentioned**.** One**.** As I started saying before. There had been a great deal of research done largely by psycho-acousticians including the group at Harvard. And some of these were done, during the war, on speech intelligibility studies in which they filtered the speech in various ways and measured the intelligibility. And they developed very elaborate theories about how to measure intelligibility. They did not learn an awful lot about speech. It is very interesting in this connection, I think it would be, I have not done this for a long time. I think it was in about in 1952 (or thereabouts) that the handbook… I have forgotten exactly what it is called, but it was the one that S. S. Stevens[78] edited (Handbook of Experimental Psychology) came out and there was a chapter in there on speech by, I think, J. C. R. Licklider and probably George Miller[79]. I mean that group. This chapter represented largely their work and it is very interesting. It would be very interesting to go back and look at that, because that's what was known as late as 1950-51 and 1952 and that was just simply about these intelligibility studies of how intelligibility, just sentence intelligibility for example or word intelligibility varies as a function of high pass filtering and low pass filtering and middle pass filtering, all kinds of filtering.

Frank: And probabilities of the information theory type.

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[77] Harvey Fletcher, born 1884, died 1981. Author of the book "Speech and Hearing", D. Van Nostrand and Co Inc., N.Y.,1929.
[78] S.S. Stevens, (editor): Handbook of Experimental Psychology. Wiley, N.Y. 1951.
[79] Licklider, J.C.R. and Miller, G.A. (1951) The perception of speech. In S.S.Stevens(Ed) Handbook of Experimental Psychology, pp. 1040-1074.

Alvin: Yeah. Yes. Yes. That was a sort of theoretical base for it. Then there was one publication that, I think, was probably more useful to us than anything else. And that was the Martin Joos' monograph.

Frank: That was a very insightful job.

Alvin: Yeah. He had many interesting insights. It was very frustrating to read it because it was not clear where his data had come from.

Frank: He even had an experimental method for discovering what the glottal pulse was like – you cut off the head and record.

Alvin: But that was a fairly insightful kind of thing that he did. He dropped it. Apparently, he had been at Bell during the war and had made these observations and then after the war published, them but without much support.

Frank: Whether he was at Bell or not I don't know. I think he was at NSA.

Alvin: Well. Okay may be NSA. I am sorry.

Frank: I think that's where his experience came from.

Alvin: Well. It was not called that then.

Frank: No, it was the parent organization. I am almost positive of that because I picked up trails of him talking to people down there. There is one other large glaring omission in our discussion so far and that is Pierre Delattre[80].

Alvin: Oh. Yes but we have not come to that.

Frank: He came in fairly early.

Alvin: Fairly early. Yes.

Frank: The position here, as I recall it, you can check me on this, is that we had a grant from the ***Carnegie Corporation,*** which made it possible for us to move ahead on finishing the spectrograph and the Pattern Playback. They set up $10,000 a year for five years. With the set up over the first two years in your hand and then come back and if you are making good progress, the remaining $30,000 is there for you. Though we had a deadline of late 1949, as I recall it, as the end of the first two years to convince them that we were making progress and we took that, I think, to mean that we could indeed manipulate speech that the machinery would work. Well I think the sentence that they got generated for their initial $20,000 was "eat at Joe's". It was done on the basis of an early version of the spectrograph in which there was simply a sheet of film wrapped around a drum so the spectrogram lasted about as long as…

[80] Pierre Delattre, born 1903, died 1969. He joined the Laboratories in June 1951 while he was a Professor of Linguistics in the University of Pennsylvania.

Alvin: Eat at Joe's.

Frank: And a very crude scanning operation, but it worked, proved out that the system could be used. So that is how I think we can date that particular period and it was about that time that we gave a paper at the Acoustical Society. Now the thing gets a little fuzzy right there. But very soon after the Pattern Playback was operating at all, so here was a way of making synthetic speech, a group of people from MIT came down. And with them came Pierre Delattre who was a Professor of Phonetics at the University of Pennsylvania, who had been commuting from Philadelphia to Boston to work with the speech experimental group there. And I don't quite remember what he had been doing there.

Alvin: He was an advisor to them

Frank: But I do not remember what their experimental program was at that time. There was in the Air Force Cambridge Research Laboratories, a group under a German professor who had been in communications work in Germany during the war, who came over and was working on a variety of bandwidth and other type of compression problems – speech engineering work. In any case, Pierre had found interests that took him back and forth to Cambridge occasionally and brought him into the Laboratories for a kind of demonstration. So there was a group there that was interested and he was interested and I still remember after the lunch, he sort of pulled me aside at the corner of 43rd Street and Second Avenue and said I've got to work with that machine. As simple as that. So, he started coming up quite frequently. How frequently, depended on the progress we were making in getting the machine in such a state that he could use it. But he was involved in that work very shortly after the first set of sentences had been put through in transmission spectrograms and I think…

Alvin: That's not quite the way I remember it.

Pat: 1949 would that be?

Frank: 1950, I would think this was. You go ahead and I will see if I can find anything here.

Alvin: My recollection is first of all about the paper at the Acoustical Society – you read that paper Frank – it was a meeting here in New York. You gave the paper before PB2 had actually been built. It was not really performing. You described the plan of how it would perform.

Frank: How it would perform?

Alvin: I remember that one of the reactions afterwards and I think it was from, perhaps I should not say who, but one person who later became very well known came up and wanted to know why on earth we wanted to do this. He could not understand why we wanted to do this. As for Pierre, I remember very well the day he came and I remember exactly as you do his saying on the way back from lunch that this was exactly what he had been looking for and I remember his saying why he had been studying nasal vowels in French. He had some ideas about the acoustic basis for nasality or the nasalization of vowels and this was the way to test it. But as I recall it, Frank, at that point we had produced the copy synthesis sentences. That is about all we had done. We were just at the point … I mean, PB2 was up and working.

Frank: PB2 was up and working.

Alvin: We had already produced, may be not the full set of the copies, but some of them. We had copy synthesized a couple of sentences at least. It was then that Pierre joined us, I think.

Frank: I think you are right on that. You did the initial work on both transmission and the early copy handwritten and drawn.

Alvin: That is right and Pierre started working on nasalization. We were working on other problems but within a very short time we just spontaneously started to work together. The time is getting short but I just thought of one other…

Frank: To date that, Pierre joined the Laboratories group in June 1951.

Caryl: That matches it

Alvin: The Playback was up and running by then because… oh yes. I think we had done all the sentences at that point. I think you did not say, Frank, that initially we did not know, in fact, that we would be able to use painted spectrograms. That is why you designed the Playback to work from the negatives of the spectrograms.

Frank: Well actually it is designed to use either.

Alvin: Either. That is what I said. But we never did anything with the real spectrograms, remember, because we discovered that in fact we could do the copy synthesis. But my point is simply that to begin with we thought we might have to work with the real spectrograms or the photographic copies of the real spectrogram. And that would have been of course very inconvenient because we would have to have copies to make changes. And it would have been very hard because I remember how delighted we were when we discovered, in fact, that we could copy off what seemed to be most apparent to the eye and we could make it sound like a phrase.

Frank: Well one of the very first demonstrations that we used, I think in the paper that you are referring to, was a “never kill a snake” in three versions, and that is undoubtedly still around.

Alvin: I have a recording of that.

Frank: One was a transparency.

Alvin: A transparency.

Frank: One was a careful hand copy.

Alvin: Copying the details.

Frank: And another was the stylized version.

Alvin: And the stylized version seemed the best.

Frank: The stylized version was the best of all.

Alvin: But you see that was something we hadn't known in advance and in fact I had not anticipated. I did not think it would be anything like that easy and so I would not have been willing to bet that in fact it would have been as convenient and easy as it turned out to be. But the fact is that once we discovered that we could do it with these painted patterns we found no use for the original spectrograms at all.

Frank: I think the original one was used perhaps twice in the whole history of the thing. Once was the very first test was done that way and we soon discovered that... *(loud hammering is heard from an adjoining room*) Is that somebody wanting to attract our attention, do you think?

Caryl: Sounds like a carpenter.

Frank: You want to sign off and see what's going on?

Pat: Well it is getting fairly late anyway.

Caryl: We should take a break.

Alvin: We should take a break, have lunch. There is a lot more. I don't know how far we want to go, but there is still a lot more about the early days, I think.

Frank: Yes, I have a feeling there is. Though I think we have left Caryl without much to say for sometime now.

Caryl: In this area, I am learning.

END OF REEL 3 SECTION 9

Caryl: Well, it was suggested that I say a couple of words about the original philosophy of the Lab and perhaps some of the things that have contributed to its coherence and success that we were talking about. I think right back almost to the foundation of the Laboratory; when it was no more than a one- or two-person operation. We resolved on a couple of things. First, that it would pick its problems along borderline fields, whatever they were. The biophysics, early work qualified in that respect – of course the speech work much more so. And the second one was that the work be project-oriented, of course, followed from this – multidisciplinary and project-oriented rather than discipline-oriented. So, that the whole psychology would be that various people in the Lab were concerned with the state of the field rather than their own status with respect to the field. And, of course, that meant that the people who were recruited into the Laboratory (or recruited themselves) had to have essentially the same philosophy as the people who are already in it. And this, I think, is followed out very much to this day. The other one was that there be real freedom in terms of the directions the research would take. And, hopefully, that the staff will be such that freedom would be allowable and could be encouraged and that problems of greater difficulty and import were as welcome as shorter range problems. And, therefore, publication in a given time was not a merit and there need be no schedule for publication. So that, in the genesis of a promising field, it might be five, six, or seven years before that would payoff – and the Laboratory was fully in accord to support it during that period. Well, I think those were, as I look back on it, sort of the principle structural philosophies that underpin the Lab. And of course, a number of things made it possible at the beginning. One was the very smallness of the Lab at that time and another one was, perhaps, the state of American science or at least in the areas we worked in at that time. There were so many areas that were flexible, were exploratory, and that had not been trodden down so to speak with a lot of more or less conventional research. The other was in the financing which had to be extremely modest at the start of the Lab, but which at least we could control and set the patterns and we have often wondered whether one could indeed do that today. I think it is questionable, but once having done it, and the pattern having crystallized, then it is obviously maintainable. And with the leadership the Lab has had and the kinds of problems it has had, have been the other great guarantees I think for where it is going and why it is going in those directions. It does represent, I think, a rather basically different attitude toward the whole research enterprise when you get in (*compare it with*) most research institutions across the country. One of the things that always seemed to us to be quite intrinsically superior to much of the university structure in the country is the lack of departmentalization and with it the lack of workers feelings that they are identified with a particular either discipline, department or field. And some of their own very natural feelings, their own fortunes, rest with what they do in that particular field and they must not change fields because if you do that, you will hazard half a life's work before you get back to the same reputational status – even if you do well with it. And of course, at the Lab this is never a consideration. So, the people are not bound in that way. Well, I suppose there are a few institutions in addition to ours. I do think of the Carnegie because, although I had not heard of the Carnegie Institution in the early days of the Laboratory, the longer I have been associated with it, the more convinced I have been that its departments, in very many ways, parallel the Lab very closely. I think as distinct even from a place, perhaps, like the Rockefeller University and certainly as distinct from the Institute for Advanced Studies, as we were saying at luncheon, so those seem to me essentially the frameworks that have brought us here.

Frank: Where would you put the Melon Institute in that?

Caryl: I find it hard to make a comparison. It is quite different.

Frank: Quite different.

Caryl: I suppose it is not really a research institute, is it? It's much more of an engineering and applied.

Frank: It's an industry service kind of thing.

Caryl: Industry service type of thing. Not too different in a way from, maybe A. D. Little. It looks a little more commercial.

Frank: Melon could have gone on this path, but didn't.

Caryl: But didn't. It was well, almost well on the way to I think, and I thought that when it joined (one then had Carnegie-Melon, of course) that (*it became*) a closer approximation in a way I suppose to this. But it is just very hard to think of institutes of that sort. Which seems to me makes us that much more important in the country from a structural standpoint as I think both we, and the Carnegie, are. And, of course, going with this is a point of decentralization as we all know so well. Certainly, there are groups in the Laboratory that cluster around particular prospects and challenges. But, they are not disciplinary clusters in any sense nor are they impermeable. So people can move from one to another. I think these are at a very deep level, these are guarantees. Now what to do in the future we have often wondered, whether something like the Lab could be started in the present day. I think it could be quite difficult for all the obvious reasons, which makes it that much more important I think for it to be maintained.

Alvin: May I ask in that connection whether why you think that is? Is that just because of the cost, because while it is true that we weren't spending much money in 1930's and 40's by comparison with what we would have to spend now. If we make the appropriate allowance for inflation, it is a lot more.

Caryl: It's not too different.

Alvin: Well I do not know. That is the question, I guess. Has the cost of setting up something like this operation increased beyond inflation?

Caryl: My guess would be it has. My guess also would be that, what you might call the cultural space or something of this sort would be much more difficult to retrieve now than it was then. I think a part of it also may be the much more organized state of science and, I am sure, the big funding agencies have had a great deal to do with this. It's the obverse of what they have made possible of course. But, I often wonder whether you can have that massive and that rapid and specialized growth and not have these very rigidities as an inevitable obverse to it. I suspect that this is true, which again I think puts quite a premium on smaller size. And I am interested again, as a comparison, that the Carnegie has come to exactly the same conclusion. They have not increased in size despite a lot of pressure to do so. They may have a bulge for a year or two with fellows in some particularly lush field, but the departments are essentially the same size as they were 40 years ago and that has been deliberate. This book we were talking about the "Breakthrough" (Robert M. Hazen[81]**:** "Breakthrough**:** The race for the superconductor"), I think, elucidates that kind of philosophy in a very interesting way. Hazen himself is of our philosophy very much and he is at the end of this exciting story of the time of the Woodstocks and so on. He has a couple of sentences saying at the end of all that, "we have gone back to our usual and more relaxing job, hopefully, not having been fooled of the idea that any one person controls the field or that we are more than a quite efficient working team in that field and all we want to do through rest of our lives is trying to continue our research with maximum flexibility." Of course, the other thing I think is not only the cost of establishing, but the cost of maintaining on the large scale. I

[81] Robert M. Hazen a Professor of Earth Sciences at George Mason University. Author with Margaret Hindle Hazen of "The Breakthrough: The Race for the Superconductor", Summit NJ, 1988.

mean, because our fates have been so well managed with respect to the grants and because the reputation for absolutely first-rate work was established so early with places like NIH and NSF, so that we now have a relatively secure position with them actually. Without those things, I think we would have to be scrounging even more than we are for current grants competitively. And that this again carries a tremendous temptation, obviously as happens so often, to exaggerate possible results or even to put them forward. All the things we see. I think just humanly that sort of thing is awfully hard to avoid so that, I believe, we just have been enormously fortunate and enormously wise in our position and that it is indeed something precious.

Frank: Because I wonder too about essentially the same question that Al was asking you; just why you think it would not be possible to do again? I wonder – having asked the question and let me answer –.

Caryl: Please.

Frank: …if an element of it isn't that the motivation that launched the Laboratories, isn't still rather uncommon. That is to say to pull together a group of people who are interested in a particular problem and consider that their primary interest, and going ahead with it rather than building an institution or creating a structure, I'll leave aside the financial entrepreneurial motivations…

Caryl: Yeah!

Frank: …which are working. We mentioned Melon among other institutions. SRI, at the time after we were well started, was launching itself and chose to go for size and growth *per se*, tailoring its opportunities to what would attract the dollars. That would be an unduly critical judgment?

Caryl: No, I think that is certainly that …..

Frank: Well, I think that is essentially critical.

Caryl: Yeah.

Frank: That was not an underlying motivation in this Laboratory.

Caryl: No, indeed.

Frank: Without making value judgments, I wonder though if that is not, perhaps, a particular aspect of some significance in the conclusion, correct or not, that there is not much likelihood of other organizations like this coming into being.

Caryl: I think that is absolutely true plus the fact that…

Frank: Not that they couldn't, but they aren't likely to…

Caryl: They aren't likely to. One isn't likely to get that kind of a group of people because of the extraordinary pressures on people of that quality to go into other places with other motivations.

Frank: The attractiveness of the bottom line.

Caryl: Yeah! I think it was really the extraordinary wisdom of Laboratory people and the extraordinary vision of Laboratory people from very early on.

Frank: Namely? The three of us.

Caryl: Absolutely. Self-evident.

Alvin: Quite apart from the amount of money that is required, people have now become habituated to getting this money always from no matter what, from the NIH, from the federal agencies – and we all know how hard it is to do anything novel or to get money from them to do something novel.

Caryl: Do something novel. That I think is very important.

Alvin: And, so, the question then is given that everybody expects (by everybody I mean the University expects) its people to get them money.

Caryl: It is a figure of merit of their staff.

Alvin: I mean, if they are NIH eligible they are expected to get NIH grants. And, it is very hard to do...

Caryl: It is a tremendously corroding thing of course.

Alvin: It is very hard to do anything.

Caryl: And the other side of that is, one admires at various times, the NSF staff and NIH staff but they are not apt to include the most creative people.

Frank: They could not.

Alvin: But it's not the staff, it is the peer review.

Caryl: Well, I think that is right.

Alvin: No matter what their ability is, they play it safe. They tend to play it safe.

Caryl: And there is every pressure to play it….

Alvin: And often they are very explicit about it they will say critically about some proposal that has not been, "Why haven't you done it?" you know, "Where is the pilot data?" "Where is the proof that you can do it?"

Caryl: Yes, exactly.

END OF REEL 3 SECTION 10

Caryl: And, is that proof? And it extends also I think into recruiting personnel with many institutions. I have heard at said at Rockefeller University, but I don't know how rightly, that they put great pressure when they are recruiting people on getting Nobelists or getting people who are already high in the showcase. Of course, I think they are just in fatal mode. But, it all comes back to, I think, an acceptance of a short-term popular judgment. Same thing you have said, which thinks bigness is very good, which thinks that a staff of entirely nobelists is a great thing. And these are again commercial. Really, sort of a bottom line, least common denominator commercial values. I think that is one of the hazards of science having become a dominant activity in the country and a dominant activity in people's consciousness. We now do have ***Science Times***, and various other things of course, which was not true at all when we were founded. It was even less true with Carnegie in 1902. People who did that were thought to be cooks who love to do that kind of thing, but they weren't doing any harm and that is a good position to be in.

Alvin: What was it like in the universities before NIH and NSF. Where did the research money come from?

Caryl: Well the only experience really I have there of course is the graduate experience in Harvard and I happened to be extremely happy there. I was just very very fortunate in the people I met first both the Delaros of this world and the Henry Cohens of this world and my research professors. They were not under any great – and particularly, since this was an inexpensive kind of research – there were not under any great financial pressure at that point and I well remember still being bemused by the way I got admitted at Harvard. I talked this thing over with **Kroger,** I had of course been at GE and I wanted to continue the GE work and the man I had wanted to work for had been killed in the laboratory just two weeks before I arrived in an accident banging his head on the refrigerator. So I did not have anybody to go with, I was already signed up with him. So I signed up with Kroger who turned out to be an ideal person to sign up with and when I had asked him and told him the circumstances, just out of the blue one day I had never met him, and said why would I thought it would be interesting and fun to do. He said, "well, I have two graduate students in the basement at the moment working who are interested in much same sort of things you are, why don't you talk to them". So, I went down and talked to them. They were Delano and Henry Cohen. I came back up and he said, "well, how do you like to come and work with us". I said, "I would sure like to, but what will I have to do". He said, "go down to the registrar's office and tell him I sent you". That was literally it. Can't imagine that happening today.

Alvin: But, then the University supplied the funds for the laboratory.

Caryl: The University supplied the funds. That's right, entirely. There were no grants involved there at all. If you served your department and the department requested those funds and if the administration approved they got them. And I was never short on funds. I was a very light spender, of course, my work lasted there for two and a half years, So I took my Ph.D. in 2 years till I worked in GE during the summer. During that whole 2 years, I was never questioned and I cannot remember it entering my head as a matter of fact through that all time.

Alvin: What about yours?

Frank: Well my experience at Illinois was, of course, right in the depth of the depression. The department had very little funds for research. They had to scrounge them up from the University as I recall. Of course, it is a State University. It had a new physics head and he did manage to get some funds to buy spectrographs, things of that kind into which area he was leading the department. But, I was working with Kruger on vacuum spectroscopy and we had to use special photographic plates on very thin flexible glass, which were made only in Germany. And we had enough money in the department budget to buy two dozen plates and breaking a plate, which was very easy, was therefore something of a tragedy. And the department had been used to allowing its students to have free pencils and paper just as a matter of course because they didn't pay them much salary. Even that had to be cut back. They had either a very definite allotment or none at all. As I say, that was in the midst of the depression or the time by which the depression hit the University schedules. At MIT, I did not really have any feel for this because I was there only two years and I worked for a new man, and they had made some outlay for him and I was not really concerned about it.

Caryl: How about other free funds for other supplies apart from things like the plates? I mean was that fairly free.

Frank: No, it was very tight… There were only a couple of research professors there and the rest of them were from the older generation, were just phasing into retirement and they taught their classes and that was all they were expected to do.

Caryl: Of course, I was very fortunate in the Harvard situation. I did not really need much equipment as compared to what you did and it was not expensive equipment, mainly laboratory glassware.

Pat: Foundations were still making grants at that time were they not?

Frank: There were not many foundations around during this.

Pat: Didn't the Lab receive foundation funds before the war?

Alvin: Oh. Yes.

Frank: Well, no, wait a minute.

Caryl: No

Alvin: We were funded. We got a Carnegie Institution of New York.

Caryl: And, Seymour got a Rockefeller.

Frank: Seymour got a Rockefeller grant. One of the things that helped the Laboratories in about the time Seymour was joining – at about the time we made the move to 43rd Street – was that Phil Pillsbury was simply a personal friend through the Industrial Research Institute. And I think he invested, out of the goodness of his heart really, in the Lab research. I don't know, $10,000 to $15, 000 a year for...

Caryl: One of the guys was going to be done for Pillsbury.

Frank: We did do a little consulting for him, but not much. And that literally equipped the chemistry laboratory that Seymour had. There would not have been one if he had not done that. It was a small grant, but it was critical.

Caryl: And, the other of course was Alfred Loomis who made several grants of $10,000 or so each, I guess about $15,000 probably a total of about $30, 000.

Frank: I didn't remember those, but ….you know.

Caryl: Mainly for Seymour. Of course, he had done a rather similar thing, as I was mentioning in our first tape, in the Loomis Labs. Except the structural difference was that he invited people he felt were interesting to come there and work and would build their equipment. He built a lot of equipment for them and of course there were no constraints. But, on the other hand, that did not built up any permanent staff for the group because the people would be there for eight to nine months and expected to leave. And, of course, later he packed all that up and transformed it into the core of the Radiation Laboratory at MIT. But those were aberrations. There were just not many of them around.

Alvin: The first equipment that I had to use as a graduate student was a good deal, I think, simpler than what you people needed. But I had to build all of it myself even in the case of my master's thesis where I was going to use bromide paper to make shadow-graph recordings of an eye blink. I had to build the room in which I would develop this bromide paper and I had to build the racks – several dozen muslin covered sheets to dry them on and frames to hold them. From ground up to the roof. My professor said, "there is your space build it".

Caryl: Hope you took them away with you in your lab.

Alvin: I spent a greater part of the year building the equipment, building the room.

Caryl: And, no other way to do it.

Alvin: And no other way to do it.

Caryl: What was your impression on the funding side?

Alvin: Well, I was going to say that my experience was different from yours in one respect. I did my Ph.D. work at Yale. The department was part of the Institute Of Human Relations and the

Institute did have some money that had come from the original Rockefeller grant, I believe. So that there was money to support research assistants that I think came not from Yale University, but from the original Rockefeller grant, which was then running out. And indeed, during my first year as an instructor at Yale, I was told by the then director Mark May that I could have $700 with which to hire a research assistant and that was, you know, it was a gift. . I didn't apply for it. He called me up and said that it is available to you. But it was also true at Yale that I built all my own equipment for my thesis research from scratch and that much of the time that I spent working as a research assistant for Donald Markowitz was spent in building equipment. He was interested in measures of muscular tension. Somebody had suggested that the standing level of skin resistance correlated with muscular tension and so I tried to confirm this and failed. But every time I failed I had to suppose that it was because measuring skin resistance is a rather tricky thing and needs non-polarizable electrodes. And, I wound up making silver-silver chloride electrodes. You know, I learned to do electrolysis, I think. He said, make em! I said how do I do that.

Caryl: All the training. Variety of fields.

Alvin: And that money, that was also institutional. I mean, to the extent that I had to buy a little bits and pieces of things, I am quite sure that was available through the Institute not through the University, and certainly not on a research grant, not...

Caryl: No, not a formal thing…..

Alvin: No, no one was applying. There weren't any grants.

Caryl: There weren't any granting bodies.

Alvin: But I suppose the equivalent of what would happen now to somebody in my situation is that he would not spend time building the equipment, he would spend time programming. That would be sort of the moral equivalent for those long hours I spent with hammer and saw.

Caryl: You may be more fortunate on that one.

Frank: Well, building equipment was my own experience at MIT. Everything except the glass blowing and the machine work on the main instrument, I simply had to do myself including building the Helmholtz correlators; the frame for the whole vacuum system doing the glass blowing on….

Caryl: The vacuum system itself and the ….

Frank: That is a part of the training we had to have, ought to be a part of the training of….

Caryl: It should be and I doubt that it is barely mentioned now.

END OF REEL 3 SECTION 11

The following recording session took place on August 30th, 1989. Those present were Franklin Cooper, Caryl Haskins and Seymour Hutner. Seymour posed a question just before the tape recorder was started.

Caryl: Well following Seymour's question, my radiation interest began originally during the period at GE when Dr. Coolidge asked me to set up a program for testing the biological effects of million-volt radiation. They were just then developing the million volt tubes and didn't have much idea about what the biological effects would be. So I did set up a program of this sort, which turned out to be quite interesting mainly on plant mutations in a variety of crop plants and that sort of thing and some bacterial work. This was from 1932 to late 1933 and I decided to go back to Harvard and try to get some real education of the state of the knowledge at the time in radiation effects. So I did, as I mentioned I think, in the last tape apply there for Ph.D. work, it turned out, with Crocher. But, I was very very fortunate when I called on Crocher to be introduced to William Arnold who, as Seymour says, was very basically interested in processes of photosynthesis, and had worked with Alfred Emerson at some length at Caltech. His history there was interesting because he went in as an undergraduate student majoring in mathematics and physics and completely forgot that Caltech required a course in biology before you could graduate. So, having discovered that in his last year, he decided to take a course with Emerson in photosynthesis for his biology requirement and they developed during that period (and largely due, I think, in that particular case to Arnold) the techniques for measuring light time and dark time in photosynthetic reactions. So, Arnold had just come then to Harvard to continue that work for his Ph.D. and happened to be there on the day I got there and after talking with him for a couple of hours I was fascinated in his work. So we decided to sort of combine forces between ionizing radiation and photosynthesis working originally with *chlorella* and we divided up our work there**.** Arnold setting up the *chlorella* experiments during the day. I flew to Schenectady at night because GE had generously still allowed me the use of the tools to get our samples irradiated. Never forgotten the comment of Arnold that " if there was a plane crash what's going to happen to that tube of *chlorella*?". So, we worked through that year for Arnold's own thesis on irradiation effects, on "Dark time and light time, photosynthetic reactions in *chlorella*" and then I had a pretty strong genetics interest even at that period and decided to repeat this kind of thing (sensitive volume calculation) which Arnold was doing for a "photosynthetic unit" in *chlorella*. Our genetic interest in *Drosophila* and the strategy here was to use an x-linked eye color mutation in *Drosophila*, which, because it would be represented once in the male, would produce actually a white eye mutation in the male and a light eye colored mutation in the female. So you could do frequencies in mutation from light eye color to white in the male and from normal color to faint color in the female, and do these sorts of statistical calculations on genetic "sensitive volume". Now of course quite irrelevant in terms of related DNA, which was not known at that time. So I did my thesis in that area and Arnold and I took our degrees together, one on the photosynthesis side and one on the *Drosophila* side, and following that then I returned briefly to General Electric, but stayed only a very short time before we started the Lab as we have recorded in the first tapes. Arnold and I remained friends for a lifetime and he is still working at Oak Ridge still on photosynthetic problems. So I think that…

Frank: You introduced me to Arnold by making it possible for me to go out to Van Neil's[82] laboratory for a summer course.

Caryl: Which was the time you did that. That's right.

Frank: Arnold was out there at that time.

Caryl: Yeah.

Frank: I remember one incident that they all told me. He said he got really very much interested in Van Neil's trick of using enrichment cultures to pick new bugs out of the soil and so on. Bugs that would eat almost anything, however improbable. They all said, well now why shouldn't they eat electricity, so he took a couple of wires and put them into a bottle of water with just a little bit of salts and a pinch of earth, which was then Van Neils' standard procedure for seeding his culture, and let it go. Sure enough one of the poles came up with a nice growth of hydrogen eaters.

Seymour: Remember how crestfallen you were when you tried to astonish me with that and I had the answer.

Caryl: It was a rich hydrogen source that way.

Seymour: Convenient, highly…

Frank: I am curious about one thing. The article that I remember having referred my physics premed students to, of yours, while you were at GE had to do with cathode ray irradiation…

Caryl: Right.

Frank: Of something or other and genetic effects on seeds, I think it was.

Caryl: Yes.

Frank: Can you say a little more about that?

Caryl: Yes. That… that I carried on contemporaneously. There was a fairly powerful cathode ray tube with an electron transparent….

Frank: Beryllium window, I think.

Caryl: A beryllium window that's right. And this was simply a case of radiating as part of the program. Irradiating seeds and some bacteria directly with cathode rays, so I think that was the one that was in the article that you picked up.

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[82] Cornelis B. Van Niel, born 1897, died 1985. An educator and pioneer in general microbiology. Emigrated from Holland in 1928 and worked at the Hopkins Marine Station of Stanford University in Carmel, California.

Frank: Right. Tell me about the sensitive volumes. Did the genetic and the photosynthetic come out anywhere near the same size?

Caryl: No they didn't. Well, I concentrated on my sensitive volume. As a matter of fact they both came out reasonably. They both came out…

Frank: They fitted the cell?

Caryl: They were inside the cell and they were about the size of a protein molecule, whatever that told you. And Bill had a similar experience. Now what about Clara Bjerknes?

Frank: Well, yes. This is skipping quite a few years. The Laboratories focused in on New York City, 480 Lexington, the Grand Central Palace. In the context of moving down to New York to set up the color camera company, National Photocolor and the two were set up in the same rather small quarters at first. The camera company quickly outgrew its space and moved into its competitor's space over at East 43rd Street leaving the Laboratories in full possession of a little bit more room than it needed. Paul did some work there on pellicle mirror development and I did a little bit on a color temperature meter and on a densitometer, which was later manufactured by National Photo Color. And, about the middle of this period, this would have been into the war years I think (Seymour can supply the dates because they fit into his biography) you and I had talked about biological warfare as a potential problem or possibility. More a problem than anything else and worked up a memorandum for Dr. Bush on it, which promptly got so classified that I couldn't see it.

Caryl: I have never seen it.

Frank: In the course of doing this, I went up to Boston to see Seymour and talk with him about it. This was about the time that the work on transformation principle on Pneumococci was coming out and we discussed the possibilities of tailoring bacteria to do the kinds of things you wanted to do if you were building a biological weapon and this went into the memorandum as well. Then, a little later on, not in that context at all, we followed up with Seymour and just decided that may be the Laboratories should move in with National Photocolor since it was being closed down by a lot of war pressures. Since space was there, facilities were there and Phil Pillsbury[83] had given us $15,000, I think.

Caryl: 15 as I recall.

Frank: With five of which we set up a "chemistry laboratory" and a biological laboratory for Seymour. Since he was…, I don't now remember what the conversations were that led up to the decision that he should join the Labs, you can fill that in, I think, Caryl better than I, but anyhow it was decided around 1943 or 1944 as I remember it that Seymour would join us. The Laboratories at that point were still at 43rd Street. And Paul and Seymour and I were sitting in Derry Gallagher's old office because it was the only one that was big enough for three or four people to sit in. And it was decided that Seymour and Paul would need a technician. They interviewed a young woman

[83] Philip Pillsbury may have been a member of the Pillsbury baking family. The family is engaged in philanthropy. Philip is an intergenerational name.

who came in response, I guess to an advertisement with the name of Clara Bjerknes, a very attractive and competent appearing young woman and we said, "well, we'll think about it and so on". She seemed interested. She was going on a holiday. Seymour said, "Yes, she would be just about right", and I said, "well, if you want her go catch her," which he did just before she got to the elevator. She stayed in the Laboratories for quite a long time and I think that is something you can fill in, Seymour.

END OF REEL 4 SECTION 1

Seymour: She worked primarily with Paul and then as Paul's Camera duties became more extensive, she gradually shifted to me. She was perfection, but very Norwegian in manners. I remember her always wearing a white lab coat. The other girls were in shirts.

Caryl: How long did she stay? I can't remember that.

Seymour: She must have stayed about 20 years maybe, and then she got the job in Brookhaven. Commuting from Smithtown as the traffic increased on Long Island became more and more onerous, but we got along fine.

Frank: She was an excellent person.

Seymour: Excellent, we liked her, very fond of her.

Frank: We still have Christmas cards from her, perhaps, you have.

Seymour: She came to my party on my 65th birthday. She remarked some thing to the effect that I had weathered pretty well. She thought that the years had treated me more kindly than I deserved. I agreed.

Caryl: Not necessary deserved.

Frank: I think that just about coincided – just barely preceded – the move out of 480 Lexington to 403 East 43rd.

Seymour: We moved, yes, she saw it coming and did not like the idea of readjusting her life. She is quite happy at Brookhaven I guess and, once in a while, sends a Christmas card or something. She's probably retired now[84]. I know of her existence through Leonard Hamilton whose a neighbor.

Frank: The set up at 43rd Street when we moved in**:** Derry had bought out the facilities and had rented the entire fourth floor from another camera company that was his competitor. I do not now know or remember its name. They retreated to the West Coast and then the war came and the cameras became non-war effort and the whole thing went into a deep freeze and Derry went away

[84] In November 2003, Clara Bjerknes held the position of Archive Manager for the Southold Historical Society on Long Island, NY.

to work on vacuum tubes in Western Electric. The facilities there comprised a front office that had been nicely decorated in modern style. A little corner office that Derry had used and then one office behind that that later became Paul's office. Behind the brick wall, which divided the total floor space into a front two-thirds and a back one-third was a row of rooms three or four in total. The right was one that became an animal room for Paul's use and in front of it a dissecting room that he and Andy Novack used extensively. Down the hall was the center, fairly large, room that was made into Seymour's microbiology laboratory. The principal adaptation was to put asbestos covered sheeting on all the work benches, that was a requirement on Seymour's part. The next room to that was a small narrow darkroom, which had been fitted up with heavy wooden sinks by our predecessor and we kept it as a darkroom. Later it became the operating center for the National Photocolor Carbros. Nick Langen made his Carbro prints in it and in the back corner was the chemistry laboratory, which was fitted up like a college chemistry lab, and I felt very elegantly. It never got used for that purpose and eventually the elegance all got dissipated. But it did grow a hood in one corner of the room, which Seymour used occasionally and it was general lab space, though I remember as the CSD first started that was about the only place for Alvin to go and sit. Here he was sitting in the alleyway between a couple of chemistry benches trying to do psychology.

Caryl: This was all on the fourth floor.

Frank: This was all on the fourth floor. Then in the front of – I've accounted for the back third and the right hand half as you come in the front – the left hand part of the front two-thirds was given over to camera manufacture and alignment. It was mostly sort of empty space, work benches that sort of thing except for Arnold Garnet's set up for doing the actual alignment of the pellicle mirrors and testing the cameras. That was the physical set up of the Laboratories all through the war as I recall it. We had on the fifth floor, above us, the Gladrags Company. They manufactured the kind of flannel cloth that is impregnated so that you can rub your military brass and shine it up. It has a distinctive odor. If you ever smell it you know it is Gladrag and if you have felt the kerosene dripping through the roof, you know what solvents they use and these drips were quite frequent. Below us on the third floor was a chemical manufacturing company. I think they packaged vitamins if I do not misrecollect, pills of some kind. When we finally took over the space, it was some of the filthiest space I have ever seen. The first two floors – so we had the third, fourth, and fifth – the first two floors were given over to the owner of the building who was William Kees, who manufactured in the Kees and Lockwood necktie factory. They did not like the kerosene coming on through, they did not like our spills from the photographic sinks, which would sometimes spring leaks or sometimes run over if the water was left on with the plug in. But otherwise we had very amicable relationships with them. They did not bother us, except once in a while they started stamping out things; their big heavy stamping machine shook the whole building. There was a freight elevator in the front, down which one of our employees once fell to our mutual disadvantage. Let's see. Well this is jumping ahead two or three years now because the fourth floor served both National Photocolor and in its frozen down state and the Laboratories until after the end of the war. At that time, Ralph Wareham, you recruited him in GE, he came down and reorganized the National Photocolor and put it on its feet and actually on a paying basis for a year or two or three, set up a Carbro Printing Company with Nick Langen in charge of it and using some of the space. They also – no, that was a little later I guess. And, the Laboratories undertook the work on sensory devices, the Committee on Sensory Devices work on guidance devices and reading machines for the blind, while the total laboratory space was still on the fourth floor. And I remember that the Committee thought well, may be these people can handle it, but they've got mighty little space to work in there with all these other things going on around them. Indeed, the only space we had was what was left over from the camera registration and assembly area, about twice the size of the studio we are sitting in. Nevertheless we did some of our first experiments there, and I built the little headpiece we used in the bowling experiment. I think I gave you the anecdote about that. Tried quite a few things and did our first demonstration motion picture of a slit scan across letters using a motion picture projector, sound projector, and so on. But it very quickly became clear that we had to have more space so that we went first I think to the fifth floor of the Gladrags Company. I do not know what happened to them, but they relinquished their space, we rented it and renovated it somewhat, put a machine shop in the left hand front part, a general work area in the right hand front, and in the back third, which was a long arrangement, built a testing space for guidance devices. This was simply an open floor, uniformly illuminated for using optical devices and with movable baffles set up so that a person, a blind man or sighted person blindfolded, could be given a device at one end of this hall-like area and timed as he made his way down it. I think, Alvin and I described some of the experiences with that. The right hand part of the front section was given over to a machine shop. This was during the demobilization period with war factories shutting down and their machine tools being dissipated and so on. We got some of that equipment, but not very much. We did get an excellent machine lathe from MIT and a milling machine that was a superb one, which we still have indeed brought it up here. I think both the lathe and milling machine came up here to New Haven. Some other things were kind of junk, I spent a whole day going through one of the long sheds, so long you could hardly see the other end of it, lined with milling machines and heavy production tools on both sides all the way down. It was a remarkable sight, reminded me of that New Yorker cartoon of the banker, who looked at such a sight and said, "My! You certainly get a lot for $100 million dollars, don't you?".

END OF REEL 4 SECTION 2

Frank: Where do we stand at this point? Well that accounts for the fifth floor, which was used mainly for the Committee on Sensory Devices work, the blind aids work. Eventually, in the middle of that, we expanded to take on the third floor as well for the same purpose. Let's see, I am a little vague on that one. It was fairly open space and we did rather little to divide it up.

Caryl: Taking the office space, of course, with your office and mine, and Anne Gallagher's.

Seymour: And a conference area.

Frank: Yeah! that's right. The back part of it had two offices by the right hand windows, one for Caryl and one for Paul and Seymour. Then, I had a laboratory next to that and there was another laboratory on the extreme left to the back third that became Luigi's laboratory. And, in front of that, I just don't remember how it looked.

Caryl: In the front corner, I think, was Ann's, as I remember.

Frank: You are thinking the fourth floor, I think, but I am not sure.

Caryl: I am not sure either.

Seymour: Anne was on the fourth.

Caryl: Anne was on the fourth floor. O.K., yes.

Frank: Now we set up the front corner as you came in off the elevator. You came directly into a tiled area that led back past one office and then a library and conference table. The government put up a little objection to our putting carpeting on the conference room floor, at their expense, but they finally allowed it. And, then the right hand half from the elevator back to the left front was used for research space of one kind or other and that does not come clearly to mind just what it was now. So, we had then by the time the work on blind devices and the war reached their peak and started terminating down, fourth, fifth, and third floors for the Laboratories and rather more space than we actually needed once the CSD work and its 20 or 30 people went away. It left Caryl, who was by that time there only occasionally, Seymour, Paul, Clara Bjerknes, Andy Novack, and Anne Gallagher. I think from the CSD staff, we retained two people, Bill Winter and Dave Zeichner. John Borst came in occasionally to help out with the electronics on the Pattern Playback and the spectrograph, and Alvin came down occasionally, perhaps, every weekend or every other weekend to work on the work we were then beginning to undertake on understanding speech synthesis and analysis. So that is the physical picture of the Laboratories up until about the 50's. Somebody else's turn to talk.

Caryl: We have a chapter of Luigi's coming.

Frank: Yes why don't you take that?

Caryl: That's Seymour's. Because Seymour found Luigi.

Seymour: Well, he looked me up through the good offices of André Lwoff. He needed a job and without very particular difficulty we got him a job teaching at St. Francis College.

Pat: May I ask Seymour how did Luigi come to this country?

Seymour: Easily. He met Rose in the university studying in Paris. She was an American citizen of Italian extraction.

Frank: An artist.

Seymour: An artist, and they fell in love. But there were difficulties because as an Italian with a university degree, he had to be an army officer in Mussolini's army. But he managed to wangle an act of Congress.

Caryl: Of course, he had been at Camerino during the Nazi period….

Seymour: Camerino. A Nazi by law. So we contrived, you did I suppose with Frank. These Acts of Congress were very popular then. It took an Act of Congress to make an exception to the law. It was written into the law. And then he taught at Brooklyn College.

Frank: I might add there, Seymour…

Seymour: I got him that job.

Frank: …that he came over as a war bride.

Seymour: Yes that's what I mean. Because she was an American citizen now and the Cable Act[85]. That was the law, which has since been amended. As you know, you cannot use that trick any more. In fact, there is a movie about it, "I was a Male War Bride" by Cary Grant or something. Well, any way, Luigi brought with him something extremely precious. I believe a *euglena* that grew like mad whereas the one that I had used was a slow grower. Now that I thought I would pick up the thread of it, but first we have got to get some techniques and Van Neil had entered the picture and Stan Carson. So over to you about Stan Carson. Because you were the prime mover there.

Caryl: Well I met Stan Carson via Bill Arnold because they had been together in Van Neil's place and came to know him during the time that both Frank and I spent with Van Neil. And Stan was of course very much interested in Seymour's organisms and in the microbiological workup of the Laboratories in general and so he became the first Haskins fellow. He was not a member of the Laboratory, but he was working at, let's see he was at Burke on a Haskins Fellowship. And from there went to Oak Ridge, I think he went directly to the Oak Ridge.

Seymour: Somewhere in between was an episode of Van Neil's. Somewhere centered around there.

Caryl: An episode, yeh.

Frank: He worked on enrichment cultures with Van Neil. The idea was that this might be a way to generate useful chemicals along the lines of the Laboratories trying to commercialize. It was about the time that Pfizer was doing….

END OF REEL 4 SECTION 3

Seymour: He was one of the most underestimated people I have known. He and Jackson Foster. They share the patent that changed penicillin production from bottles to the pilot plant fermenter by getting a germ proof bearing and Stan – that is how I met him through Jackson. We became very close friends and that is when I learned something about engineering. Stan and Jackson both used to laugh about how they had to convince the American Engineers that 99.9% stability wasn't quite good enough and they held the patent and that is how they missed that in the book on

[85] The Cable Act (September 22, 1922: amended 1931) stipulates that a foreign woman who marries a U.S. citizen does not automatically become a citizen, but must go through the process of naturalization. In addition, a woman who is an American citizen does not lose her citizenship if she marries a foreigner unless she chooses to renounce it.

penicillin last year and I tried to tell everybody. Then Stan invented a very large part of the technique of doing metabolism by taking apart labeled compounds. But he was overshadowed by Shawnheimer at Columbia and the dramatic death of Rubin when a tank blew up on him. And Stan was a generally modest fellow who was terribly underestimated, brilliant, and self-effacing.

Caryl: Self-effacing, never put himself forward.

Seymour: I understood him very very well. We became fast friends, but then I knew Alexander Hollander very well I had met him at Woods Hole and so it was natural for Hollander who also had a little more drive to…. He must have been the one who snatched Stan Carson.

Caryl: He was.

Seymour: I took that for granted. It was inevitable.

Caryl: He also snatched Bill Arnold.

Seymour: My knowledge of Van Neil was very dramatic in a way as a graduate student. Cornell had a cozy little library for biology where the secretary on duty there sold chocolate bars and apples and I would buy my lunch from the secretary, (Haskins style lunch) put my feet up on a desk, and read the **Archiv fur Microbiologie** where Van Neil had his papers. I thought, this is genius and this is the opposite pole from the arrogance of Wallberg, which was how I learnt it.

Caryl: He was the opposite pole of Wallberg altogether.

Seymour: This was the man who was calling the shots. He didn't draw attention to himself, but every sentence is chiseled, is poetry. I remember volume 3, volume 4, and volume 7. I waited for each issue and he was almost unknown. He attracted no particular attention.

Caryl: And wasn't trying to.

Seymour: And I said this is…. So I wrote to him for a fellowship. He had no money. I learned he'd come from Stanford, but I knew nothing else about him. My idea was this; Van Neil had the most peculiar shortcoming as a scientist, extremely precise as an analytical chemist. Utterly traditional. But all his recipes had a pinch of yeast.

Frank: Yeast extract.

Seymour: Made by some esoteric formula, which was really a secret formula. He never published the details of how he made his extract.
He would guard it preciously. It amused me because what was missing totally from Case's keys was medicine from which he recoiled in horror, genuine horror. He blotted it out of his mental landscape. Well I was thoroughly at home in it. It is a different story.

Caryl: That is interesting.

Seymour: Very comfortable with medical affairs, and what I had written about. Let's run down this pinch of yeast. I have done my doctorate and nothing happened, but I finished my doctorate after MIT, but then the MIT episode intervened so that was a dead letter and I realized that this was the future, that animal work as we knew it, was like anthropology today. You look for an untouched culture, so you could do a doctoral thesis. In those days, you looked for a disease so you could work out a serum and become a hero like Pasteur. It was going to be biochemical and here he had the methods and the organisms. He didn't know how to go up the pinch of yeast and I thought I did my doctorate and I worked out. When you saw me, I had all fractionation methods, which I picked up from the literature mostly and did my doctorate on it. Well, here were photosynthetic organisms that didn't produce oxygen and I knew from the geology bits of it that we had begun by being anaerobic. And I said that, if we are ever going to get the secret of life we were hunting at MIT, let's start with as primitive a manageable organism as one could ever find, Van Neil's purple bacteria, green bacteria, and red bacteria. Each paper was a consummate masterpiece, which I followed much more avidly than the Wallberg papers. I felt that Wallberg was picking his data. The arrogance of his theory that he had it that I felt that while Wallberg did not cook his data, he selected. Case was clear, called the shots, as they were. He pointed out limitations. Now, when we came back to rejoin the Lab. At first, I worked with Paul and you know in an informal way, I was very content to be Paul's assistant at first. We hit it off, yet our backgrounds were so utterly different.

Frank: Paul at that time I remember working on finishing up his work on tumors, wasn't it?

Seymour: Yes.

Frank: … or doing tumor work?

Seymour: And I like that very much and I had never seen such deftness and ingenuity. I admired Paul enormously. We really never had any differences of opinion, and then I had something that Paul admired. I could write better than he could – the scientific paper ordinarily. And that to him was a shock at first as we never had any differences about any issue. We agreed, even though we would argue, but it never involved personalities or who comes first. That issue never arose ever. Whoever did more work went up front.

Caryl: Was the senior.

Seymour: We had some good girls like Drasher, Marian Drasher who drifted in. Well anyway, when I wasn't needed anymore for that and incidentally that little memo along with a parallel memo from Elvin Cabbet was a program selected for bacteriological warfare. We coincided bug by bug. And that was the origin of Camp Detrick[86] and the man who was the head of it, Riley Housewright, whom you must have met from time to time. He knew exactly of my partner, but of course, as Frank said, I was never cleared to read my…, but I called the shots extremely accurately and now… But, that thought made a common sense was often lacking because abruptly all papers on those subjects disappeared, which of course gave the game away. We showed a trickle of papers, harmless papers and I thought don't take intelligence for granted at the upper echelons and

[86] Camp Detrick was the site of the U.S. Biological Warfare Program established in 1941 and discontinued in 1972. Fort Detrick, as it is now known, is operated by the DHHS and is the site of a Cancer Center.

I landed another one at Albany. That's an another story, but mostly irrelevant**.** Albany taught me how not to run an institution and how to destroy people with purely sadism and fascism, with outward efficiency, but with inner rot, dry rot. But anyway, the moment I was clear, and Luigi brought me that *euglena*, I said I don't want to tackle that *euglena* head on, so it was a very subtle growth factor. I will work out Case's pinch of yeast, that is big stuff, obvious, and I knew Ward Starr at Brooklyn College, he sent me all the cultures, 120 of them. And as Sumner used to say, " low and behold", I spotted each case species had a different vitamin requirement and that stunned him as it did Roger Stanley, here was a table of species vitamin and agreed. But what I did not tell him that those two or three strains that did not have vitamin requirements were mixed cultures. They were sloppy, but I knew that Case's whole edifice of pride rested on his ability to have pure culture. So Ward and I laughed about it and said we won't talk about it. Its an honest mistake. Case never did find out. The shock would have been too much. His life was founded on the isolation of pure culture. Well, there were the conventional vitamins, biotin, thiamin, para-aminobenzoate. In no time at all we had them, meaning my techniques were good, which meant that biotin and para-aminobenzoate were very subtle so that my techniques I knew were well ahead of the time to avoid biological contact with materials, crystallize and re-crystallize the chemical, make a better still, for that we went to the *Euglena*. It converged on pernicious anemia factor. Thanks to the sensitization of my having taken pathology in the Web College where I did extremely well on top. And so much so that after spending a year taking a wet culture they offered me a professorship where I was solving problems all over the place and one of the old problems I solved was that… Were you aware that almost any cut on a coral reef in South Pacific festered?

Caryl: No.

Seymour: I knew about it. It was *Swine Erysipelas* and I worked out the culture medium and literally marketed the vaccine made and I went from blood serum to ivory flakes to Oleic Acid as a vitamin and which stunned the people at Merc and others that I had just gone boom, boom boom.… I didn't say anything about the experiments that didn't work and I have the curves. And so the head of the animal products then, after the "Wooden Indian" Subrow book, on a production, end was a man named Schroeder. And he built the vaccine out of my medium and it worked commercially and that is how the introduction of Thomas Jukes[87]. We saw the convergence. Jukes had come from University of California animal products that was not the medical part of Lederle. Lederle was originally a veterinarian outfit founded by a veterinarian.

Caryl: I didn't know that.

Frank: You are up into about the mid 50s now, aren't you?

Seymour: About **1949-1950,** no, not the mid. You see the groundwork took a lot of time to work out these techniques. We didn't publish much. We did small papers that were finger exercises mostly, but we concealed the strategy and our objectives, which said nothing.

END OF REEL 4 SECTION 4

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[87] Thomas H. Jukes, born 1906, died 1999. He worked in nutrition and chemical research at the Berkeley and Davis campuses of the University of California as well for the Lederle Company.

Seymour: Now I will come to the dramatic thing. Merk was struggling desperately because it had put all its effort into penicillin during the war as a patriotic thing and so it had no patents to speak of, it was unpatentable. They were just in dire straits. Lederle had given Merk all the info on folic, but Merk was ahead on B-12 and wouldn't give it to Lederle. That, we resented very deeply. Merk actually stole the B-12 from **Lester Smith** of Glaxo. I had become a consultant to Lederle – I will jump a bit – they asked me to testify against the Merk's patent of adding cobalt to B-12 fermentation, and I said, "I can break the patent, but it is not in the national interest" and right then and there, **Lederle** and I parted company. I wouldn't be a company man, I would not to be bought and Merk knew I had done that. The chelation paper would have broken. At that time, the Supreme Court believed in the stroke of flash and genius doctrine, if you recall, that was a hay day of flash and genius. Well, we got some pure B-12 from Glaxo and had perfected the glia curve. And that night, I couldn't sleep because we were going to go from utter obscurity to the command of the field. Our techniques were so advanced. We knew how it had been stolen. **Gladys Severson** had visited Smith, he'd pointed to the purple red area on the industrial scale chromatographic columns, and said I think, it says… And they covered it by using a lactobacillus assay as a cover. We knew all that, everybody knew it. But the field agreed with me that it was dirty pool to attack Merk when they were defenseless. So patriotism, they had really thrown everything into the war effort. Their chemists had all tried to synthesize penicillin, as you know, that was a $30 million failure and it was Stan & Jackson with a little bearing that saved the company. It wasn't nothing, that was how… Well, anyway. We knew exactly how to play it. We wrote the paper immediately, we got the curves and we put in that killing word with Jukes, and the whole team, Luigi, for 'routine' use. That was the most malicious sentence I ever wrote. The paper took us, I still remember, two-and-a-half hours to write and of course, it was a classic. Well, we have the technique what do we apply it to. Well, Luigi, we thought well if *euglena* is long B-12 and the purple bacteria photosynthesize, what is going on in the ocean here and there? And, we got a $100,000 Rockefeller Foundation consolation prize, which was more than the Nobel at that time in money and while legally it was five years. Actually, it was *carte blanche*, do what you want to do.

Caryl: No time constraint.

Seymour: There were a few pages of memorandum, no great details. We have done this and now, we want to go into different fields and so for two years, this is something incredible today. We, knowing **Negreli** and Mrs. **Mrs Minor Younus** and her husband we set up a New York Academy Program where we gave everybody our techniques and cultures on only one motivation**:** that everything was to go into that New York Academy Program, two-year lead time, altogether, complete access to all our data, cultures anything, only do it then, then its public property. Nobody violated the agreement and do you remember that probably all start…., You will never forget that Barbizon Plaza effect.

Caryl: oh!

Seymour: When we kept parading up to the platform…….

Caryl: I do indeed.

Seymour: It was a joke. We took command and we had people like **Calvin,** and Nobel prizewinners galore and I shared it with **Trager** for the pathogens. We have the first techniques done with Lederle which enabled us to isolate biopterin, which was a fundamental nerve. And **Jean Copplethwait** had joined us and **Helen Vishek** and Jean was, oh boy! I mean that, even if you win all the cups and horse shoes, it does not mean that you have got a brain waiting to be used otherwise. They simply got bored, none of them. If I would ask **Paul** who is the most memorable character in general along with Helen Vishek and Jean Copplethwait, no one knew who knew her has ever forgotten her. Even she fought down predators like **Tom Dukeswood** and I hear from him occasionally. Well anyway. So, Luigi was his genius for isolating patience, which would have driven me crazy and that paper from New York Academy was something like seventy different plankton marine fresh water and here all the vitamin table, each of which individually would have made a great paper. So, I had written on New York Academy paper on how to apply chelation and we labeled this, which was left out, but actually that was the beginning of the marine media, just proceeding that and we had that….. Oh, no, there was an episode in between. Let's go to **1946,** we'll set the stage. Oh, '**48**, rather, when was the first big post war meeting at **Cold Spring Harbor** with **André** Lwoff**,** and **Manou** and**…..?**

Caryl: That was '**48.**

Seymour: '**48**, I think and this I have enacted for you before, but I have to do it, and it was so funny. I was there, rather, he said, "Seymour, you have sent me all your papers, but one, I would like to have it. Do you remember having worked on *euglena*," then I realized a la Huxley talking to who was his companion, Hooker, the botanist, when he said, "the lord hath delivered you into my hands…" I think I have enacted it for you, the exact scene, but I remember the actor Charles Ray, who played country bumpkins and that does give you his case.

Caryl: This is Van Neil now?

Seymour: I will play Van Neil for a moment. This is a high point of my life really. The high point of my life.

END OF REEL 4 SECTION 5

Seymour: *Seymour.* You know, do you remember that gesture?

Caryl: yeah.

Seymour: He is getting ready to mold some reluctant clay. *Why so sudden?* Now I go to me. I wish you hadn't found that, we are telling that dirt like, Oh! may be I was asking you to the Mong dance on Saturday night. Do you remember those Asian movies with Charles Ray.

Caryl: Oh! Yes.

Seymour: Because I have some stuff and I think it is obsolete and I caught the biggest dust hang dog look. *I want it.* What I had impressed with paper with Lederle , the B-12, the chelation.

Caryl: Oh yeah.

Seymour: The volatile preservative and some odds and the streptomycin bleaching all together. And **Wolf** and the others were in on the secret. So I dug up the reprint and sent it to him. Something I entitled it "apprehensively yours" or something like that, idiotly, then bang, bang, bang telegram, *Send your greeting card.* Case had never been toyed with like that.

Caryl: He reacted.

Seymour: Did he react? Of course he immediately got the B-12 **carol** and everything. It was just crash crab everything blowing up around you but the idea of his being toyed with. Well about a year later, I had occasion, it must have been a year later, in California, he asked me to drop in and give a seminar and **Jack Meyers** was on the program for a seminar. And that is known as the only time when a visitor would not give a seminar because it is only Seymour filibuster; he kept me talking for 16 hours. Every detail of technique, how, how, how. Wound up at about 2 in the morning right through lunch, dinner.

Frank: Was it down in his house?

Seymour: In his house, on the rocks, until every detail in my mind had been vacuumed-clean – all the chemistry of chelation, which was new to him, which I had picked up at MIT. When we got home, we found the invitation somehow from the American Philosophical Society. It was written in about two days because all I had to do with my memory was simply organize the filibuster. It was all there fresh in my mind and I think that I can leave the story right there.

Caryl: I remember that presentation at the Phil Soc.

Seymour: Do you remember Percy Bridgeman[88], utterly fascinating ready to offer me a Harvard professorship if he had it on the spot.

Caryl: That was a sell out.

Seymour: It was a sell out. I think the rest is all documented thoroughly, but the problem we made just to bridge it from then to now, I always remembered that the secret of life which MIT professors had kept asking me at those teas and slide show on the third floor, remember, "when will we synthesize life?" I really couldn't answer it.

Caryl: That is a tough one.

Seymour: I was tempted to say never.. I thought what we really we don't understand how to answer yet and then it slowly dawned on me and from reading **André** Lwoff's papers and Schroedinger[89]. The secret of life is 'organization' and the act of analyzing it makes it unanalyzable. Which was not (**unintelligible**) to the MIT Physics Department.

Caryl: A strange idea to them, I am sure.

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[88] Percy W. Bridgeman, born 1882, died 1961. Physicist, Harvard professor, philosopher and author.
[89] Irwin Schroedinger, born 1867, died 1961. Physicist, 1933 Nobel prizewinner with Paul A.M. Dirac.

Seymour: Which means that the secret of life is repair, how to organize and repair organization and that is a phrase to be added to any definition of life. It read somewhere that life is organized not like a crystal, but surely dynamically, which Schrodinger had and then refined by Labov, we live on negative entropy.

Frank: I think **Walter Cannon** had a similar idea.

Seymour: What?

Frank: Walter Cannon had a similar idea too.

Caryl: Wisdom of the Born …….

Seymour: Not put with the force, of Labov.

Caryl: A different angle.

Seymour: Different angle. And so all this stayed in my mind, but we never had the data until I realized that the question of temperature work, which was done for really ecological reasons… For how come an organism that lives in the bottom of a cold pond on top of the penning loves American room temperature. How does it organize? So, we knew about the shock work had come from the Noble Collip. The general adaptation syndrome, which was the first clear formulation of that and so you have the review and the international review of cytology so I can say **Danny Petrolack** who was in this apostolic line of decent has a three-year fellowship to do just that after his year, they are working, he loves the organization, its a team of 35 people.

Caryl: Where is it?

Seymour: Sloan-Kettering.

Caryl: Sloan-Kettering.

Seymour: I knew that they were angling for **Devito** and they got him. Albert Einstein wanted him, but Danny knew ahead of time. So, that we have a direct channel, they know all about us, we published them all and I have got all the tools down at the lab.

Caryl: That is great

Seymour: I have got the tools and I have some brilliant people with me, you know. And **Stuart Markus** is also in charge of the clinical test of Lederle's cancer drug, which is unique. It blocks the proliferation of the capillaries that will enable a solid tumor, we know we've got to tackle the solid tumors. That is why we went for the protozoan diseases to know more about blood and the environment and that is how we got the (**unintelligible**) direct outgrowth. Because we know we could not finance the Lab by raising questions like that.

Caryl: No, it is too long.

Seymour: I forgot to imagination on the study sections, so we thought there is a vacuum in tropical medicine and that was the class I decision to go at that problem through the bugs that was so exquisitely adapted to blood or intracellular life that we get clues that were needed for repair.

Caryl: The clues changed it all.

Caryl: I was quite conscious.

END OF REEL 4 SECTION 6

Frank: You are well ahead of chronology. Can you put a date on some of this?

Seymour: Well, The New York Academy was **1953**. The transplantable tumors and the endotoxin covered roughly 46 to, maybe 50 with Paul, applied papers. We went as far as we could go, after that it turns clinical and large animals.

Frank: Yeah, I remember you being a little bit earlier, but that is…

Seymour: Then, with Luigi around, I had to develop better techniques. We knew that money that resources… what organisms should we concentrate on, which ones. **Kaiser Monads** had no medical significance at that time, but we did develop its B-12 requirement for which, as I said, regularly to people where does your money come from, and my stock after the great American hypochondriacs. We developed those vitamin assays. That was pot boiling and we knew it. Conceptually there was nothing new, but it meant a steady supply of money. Well let me backtrack to the famous word you passed on to me the Five Rockefeller Foundation years, **Bob...**

Caryl: Morrison?

Seymour: Not, oh, the Australian episode where we showed him how to do the *euglena* to plot all their cobalt deficient soils, spectacularly successful.

Caryl: Yeah

Seymour: And I was co-opted to participate in a big symposium, very secret, on how would we defend ourselves against bacteriological or counter warfare and that was a cover of the symposium by the National Academy. That was all a cover. Nobody had broken that secret, it would have been now clear enough. Well, and so I had a lot of fun writing that, calculating how many molecules of B-12 per cell in that sense, it became a fashion. But, all that covered and I had a lot of fun. So, we did a lot of pot boiling. The vitamins told us how to attack the tropical medicine because we have to use antivitamins because the ordinary dosages would be lethal. They had to be salty. We saw, and Cy was the one who had the idea of trying polyamines, because they were ubiquitous and subtle.

Frank: Now when did you come up with the operating philosophy of looking to the protozoa as a small …..

Seymour: That was intuitive from the beginning. It really goes back to the paper of Van Neil and **Stanier** of the gigantic gulf between the prokaryotes and the eukaryotes that we were wasting our time with the bacteria accepting, as **Lederberg** was doing, accepting as finger exercises on very fundamental biochemistry. We could not afford the work with rats, mice, rabbits, and dogs that we would need for cancer and the like. We simply didn't have… My favorite analogy at that time was that we would have to fill, say, Radio City with labs, if we had to make a beginning today I would say, the World Trade Center; and I felt that that we have to find the eukaryotic model and that developed naturally out of my high school knowledge of protozoa analogy. We could see the gulf, but **Roger** and **Case** put it into clear words and that was inspired by penicillin, which would not work on… You could eat penicillin because we have a different kind of cellular organization and our membranes are different. But people did not quite realize the importance of that. Even after that paper, I said this is the introduction to modern chemotherapy. But **Case** and **Roger** could not develop it because that part of their background was simply a blank. Emotionally, I would say they would faint at the sight of blood, which I wouldn't. But so that told me that work where they put **Roger** and **Case** put it together, I rank back along with Darwin, I will say the double helix at least, with the double helix of the great discovery of the century. It is emerging as such.

Pat : What date was that paper?

Seymour: That paper must have been about somewhere around **1950** something like that and that agreed and put into words that I hadn't. They were the first to see the real significance of penicillin as an introduction to the new change in focus from bacteria and seroimmune vaccine to the tough problems where the pathogens whether a cancer cell or protozoa is like us that is all the tough ones for which and that quinine was a lucky break that happens once in an era the fact that we had quinine against malaria and then my generation of course, we bet on Arrowsmith[90], and then Arrowsmith, he leaves at the end to go to work out the motor action of quinine in the Vermont Woods, they knew it wouldn't work. I knew it. They picked the wrong course. Remember the upbringing of my generation was most covered by the Microbe Hunters and Arrowsmith. We were weaned on it. Those were our heroes.

Frank: Did you ever meet **DeCroof**.

Seymour: No, but Paul told me all about him and I read his autobiography which made Sinclair Lewis a very nasty character because the agreement was a joint authorship**.**

Caryl: Yeah.

Seymour: That was criminal. **Paul DeCroof's** autobiography fills it.

Frank: That is "A strange whim" or something like that.

[90] Martin Arrowsmith is a bateriologist who appears as a character in a satirical novel by Sinclair Lewis,

Caryl: Something, but I have read it and it was a good autobiography, not bitter but just a far a bit so to lose authorship of a **….**

Caryl: Major authorship

Caryl: And worked so hard on it, it was easy for me to reconstruct who the characters were. **Gotlieb** is **Paul Ehrlich** (**unintelligible**) condensed**, Whitlow** is Northrop[91] Sumner's great rival at Princeton**. Capetella,** I don't know, I guess they were all over the place, **McGurk,** Rockefeller, an amiable Rockefeller. That was easy to piece together. And, so, in effect as I have pointed out or you, **Frank**, I am the keeper of the original flame**.** Why does a cell die or recover or not from radiation.

Caryl: Yeah.

Seymour: The issue now is "did we or did we not come from a photosynthetic ancestor"**?** If we did, how much do we carry over or did we bypass that and go to directly from the halophyles, the way that chlorophyl comes partly from heme**.** That is a precursor and these were the issues today, very very alive. Now I am being…. People are busy rediscovering me.

Caryl: Who will cycle in.

Seymour: Well, I will probably be asked to reorganize the Society of Protozoology, it is in bad shape. I had to absent myself from that.

Caryl: I remember you doing that.

Seymour: I got pressures. It was very heart wrenching, but now the Treasurer, the past President, the President, the Secretary are all my friends. They are now, I have heard, the majority ….

Caryl: That would be pretty important for the Society.

Seymour: So what I think will happen is that… Well anyway, I have it pretty well planned out. There will probably be a special meeting of just us**.** We have got a lot of power and we have got the votes.

Seymour: We?

Seymour: Old times sake… And I imagine that it might be organized in ten-twenty days after we all get back after Labor day. The editor – we have a new editor – the other was abrasive, capable, but stagnant. I didn't make, any… Personalities didn't really enter. There were some people who use the Society for the own ends, very destructively, but they're out of the picture and there will probably be an *ad hoc* meeting. We will get the whole thing right. As in all emergencies, the constitution side has been thrown out, *a la* Lincoln and the civil war That is the story.

[91] John H. Northrop, born 1891, died 1987. Biochemist and 1946 Nobel laureate with Sumner and Stanley in chemistry.

Frank: Can we come back to Paul Zahl. You and Paul worked closely together during the late **40's** on the various stages of the tumor work, which I think rather ended up, didn't it with the **mothersill**, seasick pill, that is……

Seymour: Yes**.**

Frank: …the so called Nobel Collip shock.

Seymour: We were scooped by **Sidey** and the general adaptation syndrome, but we understood the value of the work and that built… The gadget itself was Canadian, Nobel Collip and we had gotten ahead of them, that is what I discovered in the Fredericton.

Frank: Well now. This was about the time, which I recall that Paul went into more nearly straight biological things and ..

Seymour: That is right, he phased himself, he phased himself.

Frank: …began to phase out with the…..

Seymour: The reason was very clear.

END OF REEL 4 SECTION 7

Seymour: The reason was very clear. Paul couldn't handle chemistry and was ill at ease with pharmacology, which was then evolving rapidly…..

Caryl: Becoming dominant yeah.

Seymour: Chemical subject. There is a no rationale, that's the way people are, just as Reina was baffled by me, she would say, "why can't you learn math" said she and I said, "I can't, I love it, but I cannot,"…

Caryl: This is the way it was put together.

Seymour: Look its all Maxwell equations, and everything follows right off.

Caryl: If you don't write?

Seymour:: And then Reina would say, " Sushi, how could anyone learn biology, you have got five 10,000 unrelated facts. It makes no sense." I said, "Well, I have got that."

Caryl: A lesson in brain or organization**.**

Seymour: There was nothing I can do about it or that you can do.

Frank: But was Paul…

Seymour: Paul knew that he was …,

Frank: Was it a limitation or was it rather the greater attractiveness of the adventurous life?

Seymour: No it was a genuine limitation that he recognized**.**

Caryl: Well, yes, the two may go together.

Seymour: He was quite explicit. I would sit down and write and say **Paul** this is the way the molecules go, the pharmacology goes or the chemistry goes and **Paul** does… It was repugnant, but now I know why I can do it because instead of applying my talent that would have made me a first rate, (**unintelligible**) first rate and second rate novelist. To me chemicals had personalities. Just as Barbara McClintock would say, guilt is ridiculous, sensed ridiculous to me that each corn plant had its own personality, each molecule biochemical is as if it were alive. So I had no trouble. As if there was a stroll through a zoo. This is an anteater and this is a raccoon and Paul lacked that completely. They were diagrams on a textbook that he could not assimilate.

Caryl: He had that for larger entities in the living world like Anacondas…

Seymour: What?

Caryl: … and Raccoons.

Seymour: He couldn't abstract it. He simply could not abstract it.

Frank: Well now when he did began wandering away….**.**

Seymour: When Lewis Thomas[92] wrote a review on endotoxins which clearly reviewed our work so thoroughly, that we knew if we were to continue that kind of work we have to live a new kind of life that was part of a huge bureaucracy, probably government because the cost of the animal work would have been utterly stupendous. We realized it, we didn't want to….

Frank: Roughly when was that?

Seymour: About **1955** maybe, **1954**. It was born in that we had come to the end of the techniques we could use on that scale. Lewis Thomas had preempted the reviews, assimilated them, and he himself had to leave it, the endotoxins and all, which he did. The trouble is he didn't grow after that, Thomas, we thought he would. But instead he became a philosopher, which was an awful let down for Sloan-Kettering. He almost ran them into the ground, you know.

Caryl: I guess I didn't know of that.

Seymour: He didn't mind the store.

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[92] Lewis Thomas, born 1913, died 1993.Author, philosopher and biologist. President of Sloan-Kettering 1973-83.

Caryl: Well I remember that.

Seymour: So **Marks**[93] had to be called in from Columbia with a staunch friend of **Margarita's** by the way, and he made it all those scandals from not knowing. And there was a real blood bath as the same kind that he had done at Columbia Medical and he had made all the right moves at Columbia, there are no soft spots**.** It is a different spirit and so this is what **Danny** has now encountered at Sloan-Kettering so…

Caryl: same story over again.

Seymour: All the mediocrity had been pruned away**.** You know the scandal with **Robert Goods** and his entourage, that is inevitable.

Caryl: That has been clear.

Seymour: All excised away and **Danny** found a complete team spirit where each has his individuality and they are working as an all first rate; **Danny** is ecstatic.

Caryl: That is wonderful.

Seymour: He was one of them in the team so he cheerfully accepted his assignment. He has got one year by agreement, not forced on him, but he will pick up all the clinical know-how that can been packed into one year. So he is working as he tells me at capacity. Completely at capacity and soaking it up and waiting for me to come up with a new stress factor so last Saturday he came up with the first paper there he located seeing very much so, which is inevitable. I had spotted it and you need zinc for every reaction of nucleic acids and there given zinc. We have a couple of right there waiting, right at the bottom and see and there were found. So they gave **Danny** quite an assignment just to get acquainted, dig up everything on the new platinum, and do it in ten minutes. And also we all could keep up with, so **Danny** said have you gotten anything on it, here are my notebooks I picked out the cream**,** take it home. I would say, Danny, that was brilliant. He was always selective of all the abstracted and of course he added and so now they gave him another assignment. Solid tumors again, I had it because that is why we had picked up intracellular parasites. If can get through to them like **Coozy** in the heart muscle, Leishmania, and the macrophage, or get through the solid tumors. So they were depending on him to keep them abreast of the literature. I mean they rotated the assignment so they….

Caryl: He is fulfilling it.

Seymour: What? He is fulfilling it and then Danny had an extraordinarily personable fellow. Extraordinary.

Caryl: Coming back to Frank's question on Paul Zahl**…**

Frank: Yeah, why don't you fill in some of the things that you remember about **Paul** because he is not here to speak for himself.

[93] Paul A. Marks, MD. President of the Sloan Kettering Cancer Center 1983-2000.

Seymour: Paul realized that defect.

Caryl: Because Paul is half artist.

Seymour: Yes, but that's it. He couldn't think abstractly that way. Well remember the blackout on the kidney. Once he was away from a kidney and in his doctoral exam – we have referred to that from time to time, Paul would mention it – he is got to have the concrete before him.

Caryl: Yeah, he was very much artist with concrete perception.

Seymour: That too it used to tell, that preyed on him that complete blackout he had to describe the kidney. But then others he could draw the outline he couldn't remember a thing in it at his doctoral exam. A complete utter blackout and that had shocked Paul. It brought him for the first time face to face with the fact that he had a mind different from the run of academic minds.

Caryl: Well I think he was aware of that when he joined the Lab.

Seymour: Yes. Because I have never seen such ingenuity, because he could do everything **Bill Winter** could do within the machine**.**

Caryl: yeah. Very very good.

Seymour: Almost effortless. It didn't bore him. That was too easy. He can design little tricks or technique.

Frank: He could get a mouse to lie on its back in his hand while he stuck a needle in it, more easy, remember?

Seymour: I mean the deftness. The deftness with which he worked.

Caryl: Oh, yes.

Seymour: And that fineness of perception is…… And photography grew on him as a means of comprehending reality without an abstraction.

Caryl: While as Frank says he was also he was part explorer.

Seymour: Yes he was.

Caryl: Deeply part explorer.

Seymour: He did. He was not against the grain to him to abandon this.

Caryl: Oh no he …

Seymour: He knew that we cleaned out on resources and that we could not go farther. He did not have a background. He did not simply have the background of**...**

Caryl: And yet it was very important to him for years after to come to the Lab and talk with **John** particularly, (**John McLaughlin?)** , which he did at very frequent intervals

Seymour: Very frequently.

Caryl: …and lean down spiritually a great deal.

Seymour: I knew that was the friendship that I did not touch because it was out of my range.

Caryl: Well I saw him via the Geographic in that period too and he had to have that balance between exploration and……

Seymour: Paul knew that my judgment for topics was good. I may have suggested as many as twelve topics and I would do the background… I would present it to him like an advertising. I can name quite a number, the halophyles, the phosphorescent bay.

Caryl: Oh yes. These all of course had an exploratory aspect too.

Seymour: I had seen that and would say Paul, I think that can be worked out and Paul would say okay give me some background and I would dig it out. And what angles to use or the greater barrier reef for …

Caryl: And particularly if it involved field work

Seymour: And he knew that I would always come through, that it was not often idly, I had a sense of the public for it and where it fell into the science. So I guess we must have done about 50,000

Caryl: So it gave, you see, the deeper substance to the kind of fieldwork he liked to do anyway.

Seymour: He knew it was significant.

Caryl: …knew the significance.

Seymour: It was not just too risky

Caryl: We have rounded off.

Seymour: Bare breasted girls, the big National Geographic thing for a dentist's office. And he was very much concerned with bringing it up to an adult level.

Caryl: Yes and did greatly do that.

Seymour: Success with what we did. Paul was very happy that he could call on me …

Caryl: He relied on you a great deal, I know.

Seymour: We did it once in a while and I was happy to do it. I was very fond of Paul, **Joan**, and then **Rita**, they knew that they could not ask me anything I was unwilling to do. I enjoyed doing that and I would be very alert for topics for him.

Caryl: He depended on you, I knew, a great deal.

Seymour: Not a great, but he knew that we were in a tight spot, fully aware of where to go next. I usually have an idea.

Caryl: Come for consultation.

Seymour: Some thing, which would come out of my scientific readings. I remember some of the weird creatures in the amazon I'd read about. What was it, not just the giant ants, some weirdoes with…

Caryl: He was interested, for example, in four-eyed fish, anableps

Seymour: Anableps, I think that must have come from you.

Caryl: Double vision effect.

Seymour: But I think, in all, that it might have been 10 or 12 of the 50 that I might have had a hand in.

END OF REEL 4 SECTION 8

Frank: Was Paul's trip to the South America, when he found the high waterfalls, motivated at all by the movie, the Lost World.

Seymour: Very much so. It was a challenge.

Caryl: His first book, you remember, was "To the Lost World."

Seymour: Conan Doyle.

Frank: Arthur Conan Doyle. I remember that you had a copy of the film.

Caryl: That's right. I still have it.

Frank: The movie, which Paul showed in the context of talking about his trip down there.

Caryl: That was a very predominant event….

Seymour: But, **Paul** had in mind, if he would not find dinosaurs, he might find some very weird thing because it was geologically isolated so long.

Caryl: Oh yeah! Absolutely.

Seymour: That was very much on his mind.

Caryl: Very much on his mind. I still have a recollection, I guess, I mentioned it before about the second or third time he was down, when he got lost and was down for I guess three quarters of a year.

Seymour: Oh! on that, I will give you. The fact that the spoonbills are likely really living on halophyles.

Caryl: Yeah! Also, flamingos.

Seymour: Flamingos and spoonbills, their real food is bacteria.

Caryl: They scoop them out of the saline on the ground .

Seymour: And that's where their pigments came from.

Caryl: I still have a letter from Paul out there with a dollar bill attached to it saying, "I am lost, I don't think I will ever get back, but I have taken this to my **portnocker** friend – you know the diamond traders used to take him into the jungle – and given him a dollar for postage and asked him to, when he gets back to Georgetown, to mail it to you". So, I got mail with the dollar still attached. Paul arrived somewhat later.

Seymour: I don't remember whether they won.

Frank: Did you give the dollar?

Caryl: I kept the dollar.

Seymour: Yes, I think he did something in the Rift Valley.

Caryl: I do not remember that.

Frank: I do not remember that.

Seymour: Vaguely. Maybe the spoonbills or the Flamingos in the Rift Valley salt lakes.

Caryl: They are further north in Kenya.

Seymour: Kenya.

Caryl: But I think he had done a great ….

Caryl: That may be right.

Seymour: There were others, fairy tale well.

Caryl: Well, you did not greatly remember this giant flower, what's the thing it is?

Seymour: Raffle….

Caryl: Rafflesia.

Caryl: Yes, he found one of those early ones. Yeah!

Seymour: But, anyway. Usually, it amused him that I had more ideas for articles than he had ever had in a lifetime. That amused him.

Caryl: Yeah.

Seymour: And very happy that I could hold his attention .

Pat: How were these trips, these explorations, sponsored**?** Was it part of National Geographic?

Caryl: That was National Geographic.

Seymour: It was the science at it.

Caryl: And, he also wrote quite a number of articles for the National Geographic.

Pat : So, he was associated with the Geographic from quite an early….

Caryl: From quite an early period, from oh the middle 40s I should say and put an increasing amount of time in.

Seymour: And, he built up his own clientele. People used to look for his marine articles.

Caryl: Oh! yes and they were indeed excellent.

Seymour: Yes.

Caryl: And then he built in very closer to the Geographic after that of course, in fact they flew their flag at half-mast. They did for a couple of days.

Frank: Very nice.

Seymour: Very nice. Well it was a comfort to him to know I was alive..

Caryl: He leaned with both elbows at various times ... and lot more great..

Seymour: I was proud of that. He was going to come to our Christmas party, wrote me a long note that he is really looking forward to it.

Caryl: And he looked forward to getting back to the Lab for a whole variety of reasons.

Seymour: Well.

Caryl: He was a different character from any of the rest of us, but…

Seymour: Well I liked his one anecdote about the Great Barrier Reef.

Caryl: What was that, I don't…

Seymour: He went to a remote corner of it; before it was so accessible from Cairns**,** Townsville rather, and he was out of the way and there was another person did start walking towards each other. Who do you think the other one was, **Sanga**.

Caryl: Wow, this is really a new thing.

Seymour: Both had to get away.

Caryl: Confused out of same space.

Seymour: We had a very interesting time, great fun. All alone with **Sanga** on the Great Barrier Reef away from all those nasty tourists.

Caryl: Try doing that now.

Frank: Luigi, you have mentioned if I can shift the subject here. You mentioned Luigi and how you got him started into….

Seymour: I gave him Irma (**Irma Pintner**), which is the most unselfish thing I've ever done, but I knew what I was doing.

Caryl: He treasured Irma for a lifetime.

Seymour: I knew that perfectly well and we got Irma because of the caste system. That's on Kettering and Rockefeller.

Caryl: She would not tolerate that.

Seymour: She would not tolerate that.

Caryl: She is an independent person.

Seymour: But she knew her limitations.

Caryl: Yes, she did.

Seymour: And, learned to live with them. But do you know that she had a virus attack that knocked her out for six months, subtle virus that turned her into a neurasthenic. She wasn't herself.

Caryl: No, I didn't know that.

Seymour: She was very grateful that we kept her on the payroll. Do you remember that episode?

Frank: I don't remember that.

Caryl: I guess I didn't know it.

Seymour: She was simply knocked out of action. She was not herself at all, a neurotrophic virus, which is a description although. And, we held on to her that was part of her intense loyalty.

Caryl: Extreme loyalty. I think she still corresponds to Luigi quite a bit**.**

Frank: Yes I think. She does.

Seymour: Luigi and I fought pretty bitterly because my kind of mind was not his kind of mind. My upbringing was not his.

Caryl: You were perfectly complementary though.

Seymour: That was the hell of it.

Caryl: Perfectly, but does not necessarily……

Seymour: We worked it out. We finally had an open session where we did put every one of our pet peeves on the table and we each sort of shook hands on you won't do this and I won't do that and then we became closer than I ever was to my own brother.

Frank: Luigi had a volatile personality.

Caryl: Still does.

Frank: Still does.

Seymour: What happened is…

Frank: After that, we had done it a good many times.

Caryl: I remember.

Seymour: You may not realize how much we enjoyed staging terrible mock battles. Luigi enjoyed them immensely. And we had polished it like a Jack Benny radio routine. I would make a few snide remarks, he'd come back, he'd blow a top and Irma would say, "may I have another cup of tea". And each time the rumor "Seymour and Luigi are going to split up and we are here" and we both enjoyed it tremendously and Japanese would look back in horror until they caught on that it was, what do you call it, a Kabachi drama, totally rehearsed.

Pat: Seymour, am I correct in the impression that you worked closely together initially, but then your interests diverged to some degree.

Seymour: Luigi, no. Never diverged really.

Frank: You never really worked closely together except for…..

Seymour: I supplied the chemistry that he needed and then after a while I stayed purposely in the background because Luigi was working very hard on it, but my chemistry…. and it is still 10 years ahead, even today. This was a field dominated by great men like Evelyn Hutchinson[94], but who couldn't handle chemistry, again.

Caryl: Well, Luigi was a bit in that category too, of course.

Seymour: But, he knew it and he knew I was right there all the time without question and I would stay….. And also as editor, I did an awful lot of manuscripts.

Caryl: Of course, he was just learning his English at that point anyway.

Seymour: I would do all the ticklish ones, chemical and Luigi knew I was quite content. I had a full life of my own, but he could always call on me without question….

Caryl: Well, that was a tremendous resource for him.

Seymour: Which went on and I would do it quickly.

Frank: That was a good cooperation, but not a co-working arrangement.

Caryl: I guess so.

Seymour: It was not co-working because I was going back to my….

Caryl: With the fields, after all our approaches were quite different.

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[94] G. Evelyn Hutchinson, born 1903, died 1991. Zoologist and father of ecological movement. Yale Professor from 1928.

Frank: Were quite different. And Luigi was basically an individual operator.

Caryl: Very much so.

Seymour: Luigi remained an entomologist.

Caryl: Yes, that's right, at heart.

Seymour: And an agriculturist, so I could comprehend him, but he could not comprehend me, but we accepted that. But, I would always have the answers.

Pat: Then, what exactly brought you together? Why did Luigi join you?

Seymour: He had an artistic sense and a fantastic dexterity.

Frank: And, collection of the microbes.

Caryl: Collected the microbes and sorted them.

Seymour: And fantastic, he has irresistible charm with people.

Caryl: That, apparently, continues to be true.

Seymour: And, not forced really. He was gentle to the core.

Caryl: He has got the classic personal characteristics.

Seymour: And, I wondered,… see at first I thought it was pretty fake and then gradually I realized that it was really part of the man.

Caryl: Oh! Yes.

Seymour: I learnt to live with it.

Caryl: There was no faking at all.

Seymour: No faking. And, there was such an outwardly Hollywood thing about Luigi.

Caryl: Of course, Luigi was learning American via Reader's Digest readings and so on.

Seymour: Yes, Luigi was a diehard stand pat Republican who swallowed the whole Reader's Digest stuff completely. That was sacred. You could not touch that.

Caryl: That was his first encounter

Seymour: He knew what our sensitive spots were, I mean differences that were beyond reason and therefore were too rigidly exclusive.

Seymour: Emotional differences.

Seymour: And, nothing can be done about it. Nothing will ever. And, Luigi had picked up, had developed the French sardonic humor. Did I ever tell you the one about **Pat Earlbaum**, its worth it because it is said of General Sherman, by one of his generals, "the concentrated quintessence of Yankeedom.".

Caryl: That was Sherman law.

Seymour: By the way, there is a new biography in paperback and that is magnificent. Magnificent. It tells why he emerged as the first citizen in New York, and went to back of that statue at the Plaza, which I think is the finest statue in New York.

Caryl: It's a splendid one.

Seymour: That he was even much greater person than the (**unintelligible**).

END OF REEL 4 SECTION 9

Seymour: Well any way, we had a girl in the lab. This is pure Luigi – two interlocked anecdotes**.** Luigi gave – as a member of Woods Hole Corporation an advisor to the course on marine ecology or something. Gave an annual lecture which was a high point of the season, at least for that whole…., And so I got the report from **Pat Earlbaum** – gave me play-by-play. First**,** Pat Earlbaum whom I met at Fordham, was a very unhappy girl with a Jewish mother and Irish catholic father and caught between the two cultures, utterly bewildered.

Caryl: That is an explosive combination.

Seymour: We took her in hand. She was a friend of **Kits,** and we realized that there was a young man training for the priesthood who was just right for her. They are married with a bunch of kids successfully. I teamed her up with Father **McCarthy**. She did a classic paper…a classic often referred to on acid flagellates out of peat bog. She wanted to take…, and got over some of her timidity, and she wanted to take the course at Woods Hole, which she did, on marine ecology**,** given by her supervisor **Walter Harrington** of the University of Tennessee, you may have encountered him. He was a good fellow and a very close friend of Luigi, and I knew Walter very well and he knew exactly what was going on between Luigi and myself. You could not, he was very shrewd guy. So, I told Pat that I would write a letter on her behalf for entrance to the course, which was a great prize many more applicants. I say Dear Water we have a girl. I recommend without reservation for the course. Not only that, she is good looking when I understand your course could use some body like her. She said what are you doing. I said look, I don't dare show it to you unless you are accepted for the course. She was accepted. Now there was Luigi and they had kept in certain reserve and he knew exactly… he was fond of Pat. And he knew exactly what was called for and this is it, Pat appears at the door of the Lab, you know the new girl on the block, Luigi, says Pat my darling, and he folds her in a passionate embrace. He knew what was demanded. She, of course was upset..

Caryl: Wonderful.

Seymour: Well Pat, tell me how did this lecture go. She said hopeless totally. Luigi then got out, gave his great lecture. He was dressed in perfect Italian garb, sharply pressed fawn with an Ascot tie, cigarette with a long holder, looking like a Hollywood like personality, one of the Hollywood heroes. Luigi stepped out of the movies. Perfectly groomed. Of course she took three days before that lecture. There was not much work done. All the women, girls, called yelling, " Luigi is coming". They filled up the beauty parlor when they discovered Luigi is coming.

Caryl: Wonderful.

Seymour: Luigi did it pretty well and certainly you see there has been ___.

Caryl: So that is why Luigi liked Woods Hole so well.

Seymour: Yeah, oh yeah. When Luigi is there, dressed in the best Hollywood-Italian manner and he'd do perfectly well in Hollywood. Everything. The Ascot tie at Woods Hole, and you can imagine that. He can carry it all.

Caryl: New revelation.

Seymour: He was always you know thought so. He went on and so, being stupid, he did this experiment. Did not know about what could last, he did not know it couldn't last and then made a discovery. So they were ignorant or stupid or both. All deadpan.

Caryl: Wonderful. Oh wonderful.

Seymour: Do you think so? A voice in the back of a room says, the question period and he sits down to have a cigarette , he is at his ease. " Dr. Provasoli, don't you think you would get farther if you had applied the methods of molecular biology? Luigi considers it gravely, no smoke. "Go talk to the scientists, I am only a poor farmer." Perfect timing.

Caryl: Lovely, wonderful, superb.

Seymour: I recall he had not only mastered America but Hollywood.

Frank: It was a slogan for all to see.

Caryl: How lovely?

Seymour: Absolute perfection.

Caryl: Beautiful.

Seymour: They talked to him sound.

Caryl: Lovely.

Seymour: You know, what they like… Italian gestures.

Caryl: Ya, oh! Yes.

Seymour: It is perfection.

Caryl: Still got it.

Frank: My earliest recollections of Luigi were when he first came to have a place in the Laboratory and I think you introduced him to me

Caryl: May have been.

Frank: And said here is a man I think we are going to have in the Laboratory and he has got some bugs I want to work with and so on, can we fixed him up and so you must have had more to do with this than referred to so far.

Caryl: I did not meet him until… well I met him while on a visit to Brooklyn. I think that was our first meeting actually.

Frank: I forgot precisely what the….

Seymour: Allow me to say something before I forget it. Luigi got so wrapped up both with teaching at Brooklyn and at the research here that he hired **Mel Belsky** as an assistant. And Mel Belsky still remembers. He said after I worked 17 hours a day, I had to pay out of my own pocket for an assistant to help me.

Caryl: I can imagine that. We went over and had lunch or had dinner with Rose and Luigi in Brooklyn, while they were still in the Brooklyn apartment, And that is about the earliest that I can recall.

Frank: What was the particular problem that you and he were jointly interested in at that time. I do not recall that. As far as I was concerned, he was coming into work with you on a particular problem of interest to you, and we provided space for him in the back of the Laboratories.

Seymour: Might have been developing the *euglena*, streptomycin, and all that.

Caryl: That is probably what it was because I think it had just broken.

Seymour: Yes.

Caryl: And that was on that paper. He bleached *euglena*, was a trade joke.

Seymour: With genetic angle

Caryl: With genetic angle with bleached *euglena*. I think that was it.

Pat : Had he published while still in Italy?

Caryl: Yes, he had, but I think he'd published actually in entomology topics, but also of course he'd been to the Pasteur Institute.

Frank: No. He'd been with **Laboff**

Seymour: He had been **Laboff.**

Caryl: He had been **Laboff.**

Seymour: **Laboff.** I think that was it because it had been given at Cold Spring Harbor, and you felt that ground should be consolidated.

Caryl: I guess that is the story.

Seymour: When you were very active at Cold Spring Habor.

Caryl: I guess that is the story.

Seymour: Then, your friendship with Dennis was at its height. So that I had to try at a time. We felt they last with out a genuine reason to be there. I think that just hit the spot.

Caryl: Yeah, I guess that is the root.

Pat : So, at what point did he leave Brooklyn and come and join Labs fulltime.

Frank: Well, he continued to live in.

Caryl: To live in Brooklyn sometime after. In fact, he continued to do that until the Lab moved actually.

Frank: That's right.

Seymour: One of the heart warming aspects, is that when **Levoff** visited me, **Levoff** chose to ignore that he considered Luigi second rate and by his action and respect and warmth made it very clear to Luigi that this was one of the great mistake he Levoff had made in underestimating Luigi.

Caryl: Good for him.

Frank: Levoff was very grateful to me about that.

Caryl: Good for him.

Seymour: Levoff was a large, large person of the **clyvarian** stamp.

Caryl: Yeah. And I think Luigi continue to be devoted himself to Levoff.

Seymour: And Luigi was aware that his mind set was so different from of the Levoff's that there was no point in harboring any resentment whatever.

Caryl: You know they were told.

Seymour: And is aware that my superior biochemistry is what drove Levoff to his Nobel Prize. But he abandoned the field to me with the B-12 that he said could not be, and that he took up the (**unintelligible**) micro manipulator, made them, and that is how he did the single cell isolation that led to modern molecular biology, isogony single cell and then restriction enzymes. But Levoff was the real founder of molecular biology, not **Cullen** and **Boyer**, they built on Levoff. And Levoff did not harbor, if anything, any resentment towards me whatsoever, quite contrarily, he enjoyed he abandoned the field.

Caryl: He knew when it was in good hands.

Seymour: I went to poliovirus, you remember, where he picked up the technique and he did appreciate the fact that we have made a most of Luigi possible. More than he…..

Caryl: Yes, Luigi grew, as he would not have grown….

Seymour: And for that matter we made the most of the Levoff**s** early work. And it was all out.

Caryl: That was large

Seymour: They were happy about it.

Caryl: Then physically Luigi stayed in Brooklyn.

Seymour: Well Brooklyn Heights.

Caryl: Brooklyn Heights up to the time the labs moved to New Haven.

Frank: Yeah that is right.

Caryl: And then I moved in with Luigi at the end of the year.

Seymour: At first unhappy, but then very happy.

Caryl: Very happy afterwards.

Seymour: And he deeply appreciated being Lab Secretary, he loved it.

Caryl: He was very effective at that.

Pat : I was trying to pinpoint exactly when he left Brooklyn College.

Caryl: Yes.

Seymour: He is accounted…

Caryl: 50, I should think.

Seymour: …as one of the great people of the Brooklyn College history. They understood him after many years. He was a tyrant and some of his wise cracks have passed into history. I will tell one… Luigi had the Italian love of dirty stories.

Caryl: What stories?

Seymour: Dirty stories. He was a connoisseur. He began one of his lectures in protozoology, today, I will talk about tetrahymena. As its name indicates, it has a very interesting sex life.

Seymour: Luigi had a way with humor.

Caryl: Second side.

Seymour: Deadpan.

Seymour: Figure out what they call it afterwards. So he was a very effective teacher.

Caryl: I would think.

Seymour: Anyway, so Luigi was a happy man.

Caryl: Still he is, I think.

Seymour: He was very happy soul, as you see I have taken all the odds and ends leftover from **Luigi.**

Caryl: you have filled in a lot

Seymour: No, that is now in press.

Caryl: Oh! Yeah.

Seymour: That's why I tried my best to do when I had the chance to revise it, I really did it with all the technical notes on how to update Luigi spelled thoroughly. I dedicated to **Simon** and Luigi, and I said many decades of joyous and I used the word that the Japanese I are not used to.

Seymour: *Jou ers sona.*

Caryl: It's true. Yeah,

Seymour: Well I did my best to make it a masterpiece. I worked on it, polish it, kept everything. Now Luigi does not know about it.

END OF REEL 4 SECTION 10

Frank: One thing… This goes out of the chronology I, think – farther than what have been talking about – one of the people who has come out of your group and is an outstanding person is Cy Bacchi

Seymour: Now, that was a very straightforward affair. Cy was inarticulate, was my student at Fordham.

Frank: Was he always tense?

Seymour: Yeah, always. He was fond of his father. Fortunately, he had a home life, his father was a successful physician. His grandfather, a Sicilian fisherman, he is proud of that. And I could not figure him out. He never spoke, practically never. And I had to find out the ability of people. So, I gave the term examination paper into his term paper to everybody's term paper. I made a question about significant, " what you think is a significant, this or that, your term paper. Cy turned in a masterpiece of term paper, which he exhibits to this day to students. He did a masterpiece of a final exam. What do you thing of **Kit O'Connor** did. She wrote a parody of one of my papers. There was no choice either I threw her out or I embrace her passionately.

Caryl: I think that was all a dream.

Seymour: That was a parody, a vicious parody that she quoted essence of my style, my thinking, and she never let on.

Caryl: Wonderful.

Seymour: There was no little ground to think like that. So I forgot what the mechanics were. I had some money. Then I realized that Cy had a quality that I was lacking in, much as he hated it he was a damn good administrator with a head for detail and so was **Josie** to back him up.

Caryl: That certainly turned out.

Seymour: Josie is super. **Josie** is simply superb. Well but this did not tell me whether he had originality. But I needed somebody, to get going at Pace with all the engineering and **Kit** by the way was one who designed the lab.

Caryl: Oh really.

Seymour: Its more Kit than anybody else. She had that rare ability for detail, we have no sinks, no faucets, no pipes, and so she just slapped ___. She got her way, not his way. Well anyway, then Cy said to me one day. I like to work on polyamines. What about them? Well sort of a universal growth factor and I liked what they are doing. Protozoa, you have not touched them. I said well, **Seymour Collins,** who was the leading authority on them, why don't you go up and see him, he is a good friend. Cy went up there and they hit it off together. You know **Seymour Collins** quite well from the academy at least and I think a classmate of mine at that generation….

Caryl: Yes I know.

Seymour: …that fantastic generation of mine and then Cy just took off like a rocket from there.

Caryl: He certainly taken off ever since.

Seymour: Ever since.

Pat : What year would that have been, Seymour?

Seymour: I have no idea of his capacity to this day, to the limits. That must have been about **1971**.

Caryl: Right at the beginning with early on.

Frank: Well he was working with you at the Labs before you moved to Pace.

Seymour: Yes at Fordham. But they were not those problems and we could not follow through.

Frank: Now, I though he was working on, polyamines …

Seymour: Nothing like that was done at Fordham. He worked on **colon bacters** and some other things. They were difficult problems.

Frank: Well check me on this, but I thought Cy was in the Lab at 305 east 43rd for some years before we moved.

Seymour: Yes, but he was not working on polyamines at all.

Frank: Okay. Yeah.

Seymour: That was entirely spontaneous. Completely and that is why I put myself very much at the background because I realized that this term paper and the perfect exam was not a flash in the pan. I could not tell he was so reserved and intense, brittle and afraid. So in effect, he did the administration thoroughly, and slowly I began to see that I really did not know the limits and could not gauge his ability and then he came up with this. I thought, oh goody, goody. Let's see what **Seymour Collins** makes out of this. It was love at first sight there. In some mysterious way, **Seymour Collins** measured the man better than I had.

Frank: One of the things

Seymour: That was about **1972-1973**.

Pat: But his association with the Lab began earlier than that?

Seymour: 1970.

Frank: Before that, Seymour.

Caryl: Before that actually.

Seymour: Oh yeah! before that, but I had used him for odd jobs, he was my student.

Pat: Yes.

Seymour: And several of my students were… the man in charge of the entire work, **Sam McCarthy** was one of my students.

Pat: So when you first met him, what year would that have been?

Seymour: I think he worked maybe for two years before; it was about two years before we moved.

Caryl: At least that.

Frank: At least that.

Seymour: At least about 2-3 years. I taught at Fordham at '64 then I think he joined us. We had some money. I do not remember where the money came from, and I asked him to work with me, but he did not show… I think he was in awe of me, which is a bad thing for me**.**

Caryl: What?

Seymour: He was at awe of me. I think that was a new world, the world of the middle class, of the intellectual and the professional scientists that he had never been exposed to and he was feeling his way, dreadfully afraid of making mistakes. Well I could not

Caryl: I think he is still tense or nervous when…

Seymour: Well there is still a trace of it. He checks with me. He tells me why he will not be in tomorrow as if I have a care whether he would be there or not.

Frank: One of the things about the days at 305, Seymour, particularly after the war, many people commented on Seymour's chickens, Hutner's chickens.

Seymour: Well, they've done very well.

Frank: You have a remarkable skill in taking young people, and I mean very young, hardly dry behind the ears, and getting them to do enthusiastic and genuine science.

Seymour: Oh that is easy.

Frank: How did you do it.

Caryl: How did you do it.

Seymour: Oh that is easy. It goes back to my memory. I'm one of these rare adults who can shift gears and can remember what it is to be a child.

Caryl: That is pretty precious.

Seymour: And that is usually forgotten completely, people cannot reconstruct it. All I do is shift the gear.

Caryl: And get back to be there contemporarily.

Seymour: Without appearing to be, I mean, formally in authority of some kind and they get over their shyness, and begin to show themselves. So **Herman** told just the other day while **Stuart** came to the Lab and, while there – **Herman** was there and **Stuart –** said "what do you do in this lab?" And I shouted to **Stuart, "**Hey **Stuart** what the hell do we do here anyway?"

Seymour: Some reasoned academic discourse.

Seymour: Herman came up with some idiot wise crack. **Stuart** relaxed in very short order. The hardest for me to learn was that I could not do that with the Japanese.

Caryl: I imagine not.

Seymour: Luigi's Japanese students taught me a lot of their culture and the one who really understood us is **Stilawaki**. He is a great power in Japan today and we were good friends in that I am sending all of Japanese friends a copy of this.

Frank: Well I am still curious as to…, Take Luigi for example. He had Irma as an assistant. She worked well for him and later with him, almost as an equal, and that was his little domain. **Al** and I had a group of people working along, but they were mostly either independent researchers or technical assistants and you went out and dragged in high school kids and everything. It was a very different pattern of operation. Why did you do it not how did you do it. Why?

Seymour: I know what I was doing because I projected myself. I deeply resented arbitrary authority. But, once Professor Reed asked me help him with *euglena* for this page of my book, or who suddenly says I am having trouble with the enzyme. Others on the case have told me about these vitamin assays I'm having trouble. Then I could not resist. I would have resented the authority deeply if I felt there was the least trace of arbitrary position. But an offer of help whether it was Bill Pennick as I just told you "I am stuck Seymour, I cannot write my thesis". "What's the matter Bill" as I just told you.

Caryl: Yeah.

Seymour: I am have never been able, anymore than my father would ever give me a command but I would do anything that he implied he wanted done, never ever, and so I would say, almost unconsciously Let's see. I would ask them that I need some help and they would rise to that. Then the stupid ones would thing that I was a softy and that was a very good winnowing process.

Caryl: Because you selected them very accurately

Seymour: They selected themselves and then it was a Van Neil circle I knew about that the selection for Van Neil was a circle.

Caryl: Was a circle

Seymour: Around Van Neil and I saw to it that fairly that was pretty various. For example, **Lester pactor** was apprentice to **Jane Copperswaith.** Did you ever dream about modern couple?

Caryl: Oh! my.

Seymour: And you know what a moral?

END OF REEL 4 SECTION 11

DISC 2

HASKINS LABORATORIES

Continued ………

Frank: But, why did you pick the pattern of going for a lot of very young and therefore not very skillful….

Seymour: Because I purposely kept the techniques very simple.

Frank: You were in a different kind of field. That's perfectly true.

Seymour: And I knew that for this field to develop and I had money from the Rockefeller to start, I could pay the kids, and to them that meant it was serious. Even if they were brought up in a money-centered world, couldn't blame them. They were very few...

Caryl: And I think the difference in the field counts for a lot here.

Seymour: They had struggled out with very few, the person who really understood my whole game was **Fred Camino.** He admired it enormously. You remember, we were….

Caryl: Yeah, I do, indeed.

Seymour: Now I would indicate, I learned that reading about in a way by reading… who was the great English… **Barford**, the teacher of **Haldane**?

Caryl: Oh! Something like that.

Seymour: Barford. But, Haldane in his memoir said that the way he got started. **Barford** said, "will you hold this beaker for me?", as one worker to another. Do me a favor, please hold this paper for me. Somebody got me that way you see, help me, I am stuck with a damn heating metabolaze.

Caryl: Help me unstick it.

Seymour: Well, I found that I think I was fairly unconscious doing it, but I projected my own thinking. Either way they want to help or they don't want to help. And one might as well find out as quickly as possible.

Caryl: That is a deep temperamental quality of growth.

Seymour: And, that is how the English do it. Because, that total thing of **Barford,** stayed in my mind. He invented the differential….. **Barcroft!**

Caryl: Barcroft.

Seymour: Remember the differential manometer, It has been **Haldane** did it all and **JS** and then he got that from **JS.** That was **JS** who said that, he got started with **Barcroft** and that he transmitted and I met Haldane and talked to him that International Congress of Biology when he was half drunk. "I will have some of your waters." Definitely he is talking to me as one equal to another, that had a deep impression.

Caryl: Yeah, that is questionable.

Seymour: He was a star of that congress at Cornell, if you know. Well, I would never ever talk like that superior to that. Never.

Frank: I think this is irrelevant here because it is a different pattern there were at least those three different patterns, Seymour's, Luigi's, and Alvin and I, we found that some of the most productive people in the speech research aspect of the thing came to us as post docs, that is there were far enough long to be good independent workers. They were keen to spend full time working and get a publication record and they really turned out a great deal of the research. They were more productive than the university people who had a very limited time to work at it and certainly much more productive than the younger people who had no skills or experience. So, there was a concentration in that area on people who were at the peak of their training and with a year or two to give undivided attention to research. So, it then made and still makes something of a pattern for the choice of people. You went in quite a different way.

Seymour: Wait, but I had a critical mass of kids that took the strain off me. Just I as said, I apprenticed to teach **Lester** to give him some polish or he would have been thrown out of academic life all together. He was a pretty crude customer. To **Gene Coppersthwait** or **Mike Bach** who I apprenticed to **Helen Wishnack** who picked up at Queens. Now he is a senior scientist at Upjohn so….

Caryl: But Frank's point still holds.

Seymour: But, you have got to have a clinical mass or you are exhausted by prolonged intimate contact. That is exhausting.

Caryl: Exhausting business.

Seymour: It is like being a lion tamer in the circus.

Caryl: But, its also true that if you took them at a very different stage of development in fact…..

Seymour: They were still plastic. They were plastic. **Mike Bach** then was wonderful and there was a long succession. Sometime, it was shaking a model. Oh! The printed word helped. I exposed him to our publications in that era before TV and so much of …

Caryl: New experience.

Seymour: I had a tremendous experience of terror of names in textbooks and papers and so they took me very seriously with printed words.

Caryl: Well, it was their window.

Frank: There is still the….

Seymour: No, here I could talk to them in their own lingo, no matter what they were.

Caryl: And be one of them and at same time.

Seymour: And yet they knew I was a world authority, or about to be one or something of that... Oh! Another rule we had utterly rigidly. I never talked to a visitor alone. I would call in the kids, we would organize an impromptu little party or seminar. Never, meaning (in other words), that I knew that the biggest danger to a lab was partitioning. So they participated in any visit, but came any visit, whatever.

Caryl: That is very good.

Seymour: And they didn't ask and that was not the procedure where I would draw the great man into the private office.

Caryl: No.

Seymour: But, I had no private office. Well, we had a tea table, with a journal and the kids deeply appreciated and this thrilled the outside world.

Caryl: It made them a working piece.

Frank: Let me try another hypothesis. As I am interested in the philosophy behind this. Is it fair to say that your motivation, your deep down motivation on lot of this, was the training of people and the finding of them or, as for Al and myself, I think the motivation was to get the problem solved. There, one could go about these things in those different ways, if you had those different motivations that I am just wondering, you see there is a very different – a considerable difference in the pattern.

Seymour: I invested with them for their lifetime and they got the feeling that their mistakes and disasters were trivial compared with a return in a lifetime investment and….

Caryl: And it was a people investment basically.

Seymour:: I didn't pressure them, but they felt that I wasn't much interested in short-term games. I could handle that by myself, but I would tackle tough problems that required a lifetime investment.

Caryl: But, also…

Frank: Speech problem also, there is no question.

Seymour: They know that they were not being exploited, that they were not …..

Caryl: They were being open tombs…challenging problems.

Seymour: And I could always be talked to, I could always be interrupted, and I also developed a very simple device to keep myself free of frivolous interruptions. I could be interrupted anytime if they would bet on it to contradict me. Let us see among kids would be the judge, put up or shut up.

Pat: Is it an oversimplification to simply make distinction that, **Seymour**, you enjoyed teaching?

Seymour: Look, this is street smarts, which I have in abundance. The thing is they couldn't understand why I could be interrupted at anytime. It would be very easy to abuse, but if I were to reprimand him for that, that would be a deadly chill. Now, how do you meet that? I had learned how to do that. I don't want to address the idea, I think it stinks that's all. What is there that stinksLook, I don't have time, but look you think it so good. Here is a quarter. Let's say your quarter. People will judge, aa, Ya, I am not the judge, let the kids judge. You convince them. Now **Burt Goldberg** who was a most aggressive one other than the **Lester** that we ever had lost 14 bets in a row, each one successfully larger because I would conn him into a one he will never forget. I would then place him into these bets. The last one was a dollar. You know, each time to recoup it and this is how I got him where he swallowed off. I have seen one, you know. I have been meeting defense. Yeah I heard about it. Tell me still at it. Jimmy Stuart, you know, the innocent, stuttering, half-stuttering.

Pat: Seymour….

Seymour: Wait, let me finish. Just a moment. That will not take long. I'm willing to bet you cannot think of anyway to incorporate that into one of your papers, Galapagos finches. "Let's see, well no, got any money here?" I had a reference to it in one of the papers. They cannot learn that, it costs. Now to New York, can you think of a better recipe for New York kids.

Caryl: That is street wisdom.

Seymour: Or **Herman Baker**, did I ever tell you about **Herman Baker** and the dollar.

Frank: No

Seymour: See, the kids all knew it. It was very public at teatime. I had sent a paper to the Journal of General Microbiology and was turned down without any appeal—too bad to be published. And Herman said brought up to the time of again that I put in too much discussion, too much crap and he had warned me against it and he hoped I had learned my lesson. There I have the invitation from the Society for General Microbiology to head off their symposium, you remember, **Dogwood**

and at that. I tell the kids I have had enough of Herman, I need some help. **Mary Keen** wanted to be the stakeholder. Whatever happens to all the stakeholders. Well at tea times, you know, Herman take it a walk. You weren't entirely right about that. You said it would forever kill my chance of you ever being invited to write a paper or everything, I don't know, I think you over did it. Oh! I over did it, did I? Oh, Herman, I know it hurts while I still think it isn't that bad – the invitation. Herman said, Huh I will bet you. Oh! Herman I would not bet a thing on something that is painful, come on. Alright a dollar. Alright make it a buck. And there Mary was supposed to say, I will hold the stakes. She got an uncontrollable fit of giggles. She blew her line. So I said, " all right Herman**,** hand over the dollar. That is the invitation. You bet. I didn't say that I had it. I thought I carefully chose my tense. I thought it might interfered with a possibility of someday my being invited. You know, it was intricate test. That cured Herman. It took me a couple of months to…

Frank: I doubt it cured him permanently.

Seymour: Oh yes it did and **Goldberg** who is teaching very well in high school and sending us bright kids, you want to bet, you want to bet. But he uses that on his kids.

Caryl: Passed down.

Seymour: Passed straight down. Or leaves with someone

Frank: I still think we have an example here of a variety of methodology.

Seymour: Well, look.

Frank: And partly it is personality, partly it is motivation, and partly it is subject matter.

Caryl: All three are in there.

Frank: And I'm a little puzzled about the breakdown.

Seymour: I kept the techniques simple so that the drudgery was not too bad. We'd all divide up the drudgery and I would join in, they did know that it was largely symbolic, but nobody was exempt.

END OF REEL 4 SECTION 12

Pat: This recording is being made on **Tuesday, September 06**. Participants are **Frank Cooper**, **Alvin Liberman**, and **Katherine Harris.** We will begin at approximately **10 A.M**.

Alvin: Are we on?

Others: You are on, yeah.

Frank: You want to put your introductory note on this?

Alvin: My introductory note is already there.

Frank: Already there. Okay, I am sorry I held you up after that. Well, why don't you start back with just what you were saying, **Al**, that one of the threads we can follow here is the evolution of the ideas about what speech was like and what it meant to be speech like, if that is what a reading machine for the blind had to be. And how those ideas came out of the initial research results and how they eventually led to a whole new area of the Lab's research on production?

Alvin: Well, we have first to recall that our experience in trying to build the reading machine for the blind led us to the conclusion that there was something quite special about the sounds of speech, at least it was clear that we could not expect to use just almost any old sounds provided only they were discriminately different, which is the assumption that I at least had started out with, and we were led therefore to ask the question what is it that is special about speech. Because, I think we must have said at the last session that one of our early notions was a sort a gestalt-like idea that there are certain principles of good figure, good continuation, and kinds of things that the gestalt psychologists have talked about, and that what we had to do, therefore, was to find or to contrive sounds that would somehow obey those principles. In the beginning, we thought that those principles were so general; in fact, that they would cut across modalities provided only one had the right transformation of coordinates.

Frank: And indeed that the people at Bell Labs in coming up with the sound spectrograph had made the happy discovery of a pretty good transformation.

Alvin: That's right that the Y dimension of space was equivalent roughly to either pitch or timber, however you want to look at it and the X dimension space was in audition became time and the intensity was well then, Z. And that while this might not be the perfect transform, it was a good first approximation and indeed one of our notions was that through research, we could in fact improve the transform that is we could more and more closely approximate the right one.

Frank: The correct one?

Alvin: The correct one. But, we thought that this transform was good enough that we were emboldened to do some research to test it and one of our earliest studies, when I think – our first published experiment really – it was called the interconversion of audible invisible patterns. And what we did was to ask the question given that there are certain optical shapes, which people will put into a category and would say all of these were circles, all of these were triangles, and all of these were squares regardless of their size and their orientations and so on, the question then was okay, if we convert those into sound by the Playback by the spectrographic kind of transform, will they be put into the same categories. And, we did a few quasi-experiments, I would say, that partly worked and partly did not work, but at all events we did write this up and there is a paper published the proceedings in the National Academy of Sciences, which fortunately very few people have read.

Frank: It's interesting, I think, that the squares sounded like squares and the diamonds sounded like diamonds, but not like squares, and people called them diamonds instead of squares when you look at the two.

Alvin: Now that was an encouraging thing.

Katherine: Somebody read….

Alvin: It is easy to be fooled.

Katherine: Somebody read it, however, because I remember in my very early days at the Lab, some lady called up and inquired if one could not bring peace to the world, by writing peace.

Alvin: That came later, **Kathy;** that was after the adventure program.

Katherine: Was it!

Alvin: Yes.

Katherine: Because it was certainly done while people were still talking about pattern conversion.

Alvin: I will come to that, we had a letter from some executive of one of the big department stores in New York – I cannot remember now which one – who proposed this.

Katherine: Yeah.

Alvin: But may be that had had been in due course, so.

Frank: Well that idea didn't really last very long.

Alvin: No, it didn't preoccupy.

Frank: it didn't preoccupy us**.** And I think one of the to me interesting things was that we went from early experimentation, which you did, **Al,** on repainting stylized pictures from real spectrograms being the Harvard sentences. I guess those were demonstrated one of the acoustical society of meetings. The stylized ones sounded better than the original spectrograms or from the sounds made from transparencies spectrogram, which encouraged just to feel that pattern was an essential part of this, and that the reading machine for the blind aspect was a matter of finding out what are the elements of good pattern or whether it is a matter of transformation or not.
There must be things in speech that make use of these principles of good patterning in auditory phenomena. And the trick is ….

Alvin: Another bad idea and one that we pursued for quite a while.

Frank: It was a useful idea in that it generated data

Alvin: Well, there are still people who believe it, unfortunately.

Frank: well, anyhow. That idea was, I guess, the sort of origin of the term "cues" for speech perception. I would – though if I have left something out – I will come back to it.

Alvin: Like I would agree with that one.

Frank: Anyhow. You were busy in the initial experiments in doing simplified patterns and the fact that the simplified ones worked better, that is to say, were more intelligible, was strong encouragement to feel that yes, inherently, this is a simple proposition – you simply had to get your fingers on the correct aspects of it. We were aware I am sure, that there were technical reasons why they sounded better too, but that's even taking that into account they were better. About that point, that **Pierre** joined the group and about that point that he began playing with the French vowels, as I remember.

Alvin: Cardinal vowels.

Frank: Cardinal vowels.

Alvin: I came across that paper too that one in French written in phonetic symbols.

Frank: With the cardinal vowels. Yes. Well, he did quite a lot of work on that and took it, as I recall, maybe a little later, into a two format versus, one format vowels, showing that if you got into the back vowels where the two formats, the first two formats are close together you can in fact replace them with a single format that is energetically compromised.

Alvin: A phenomenon that has also been rediscovered several times.

Frank: Yeah. And it fact it had emerged in the literature even before that. That's another story. And, I don't quite remember what the sequence with events was that led you to do the pee-Ka-pooh experiment in which you took a single set of phonetic-like entities. The voiceless stop consonants and tried to make simplified patterns and do a systematic study of the variations in those patterns. Do you recall this?

Alvin: Yes, I remember exactly I think, quite exactly how that went. As we have said several times, we spent a lot of time in the beginning simply copying spectrograms hand painting spectrograms by copying from real spectrograms. Then simplifying them and then proceeding on a by guess and by God basis to make changes until the sentences that we had produced these – I think there were twenty of these Harvard phonetically balanced sentences – were reasonably intelligible. At that point, we really did not know what we have done, that is we did not really know where the information was. We could only say that somehow there was enough information in these much simpler patterns to convey a reasonable amount of intelligibility. And, so the next question was okay, where is the information. I think maybe we covered a part of this last time, I don't recall, but one of the phrases that we had – there was this one of the sentences was – "Never kill a snake with your bare hands". We had excerpted a phrase from that "Never kill a snake."

Alvin: And that was in fact the first one I think where we simplified from a real spectrogram in steps and convinced ourselves that we could not only do as well with the schematized sort of cartoonized pattern, but even, as **Frank** said, a little better, it is actually better. And we still have a demonstration of that, you listen to "never kill a snake" first from the real spectrogram played, you know, through the transparency, then from a somewhat simplified, but fairly carefully copied version and then finally from this much more simplified version. Well, I recall then asking myself a couple of questions about the word 'kill' and I just isolated the word 'kill' and I remember asking myself first, "where is the L?" And so, of course, with the pattern in Playback, one can stop it anywhere along the time dimension and so I went using it by hand looking for the L and of course not finding it. Because it is in the dynamics that was one of our really first discoveries except we didn't do anything systematic about it at that point except to convince ourselves that in order to get the L, we had to play the whole syllable. Then we couldn't, and we stopped it ...

Frank: It was an event in time.

Alvin: Yeah, an event in time. Then we stopped it at various points along the root, we simply got a succession of vowels.

Frank: The "A" in snake was even more dramatic

Alvin: Yes. So… so…

Frank: That's when I discovered diphthongs.

Alvin: That began to give us a little insight that it seemed to me that that was going to be rather hard to pursue, so I turned my attention to the question where is the K. Well we had copied this in such a way that there was a little burst, you know, corresponding to the burst in the real spectrogram a little burst and followed by the vocalic section. And so my first assumption was that the K was in the burst, so I erased the burst and it still said kill. Well, I found that a little disconcerting, but I decided maybe it didn't say kill quite as well. And maybe this burst was doing something and that led to our first really systematic experiment. And that experiment took a very simple form. We synthesized seven steady state vowels.

Frank: Cardinal vowels.

Alvin: Well, roughly yes, seven of them from e to u. These were steady state two-formant vowels then for each of these vowels, we put this thing that we called the burst that was our term, **Pierre** called them blimps, I remember. We put these little stylized bursts at each of I think it was 13 frequency positions sampling the whole frequency range really from the bottom of the spectrogram all the way up to may be 5000.

Frank: Acoustically, they were brief enough that they were burst-like, and some of them pitched.

Alvin: But they sounded awful. Very unspeech like. So there we had 13 times seven, whatever that is, patterns, which I recorded and then randomized. I've forgotten how I randomized them because we had disk recordings, I think, I do not think we could have taped them.

But anyway, I guess we recorded one at a time and randomized them that way, at all events I recall vividly calling **Frank** and **Pierre** into the listening room to be subjects and the three of us were subjects as we listened to this recording and we must have, I think, they must have been on tape.

Frank: I think you are right, the early experiments were done.

Alvin: Date must have been in **1950 or 1951**, because the paper's publication date was **1952**, I am quite sure.

Katherine: Right and it was well before this all occurred well….

Frank: Before.

Katherine: Before I arrived and I arrived in **1952**.

Alvin: I remember we started playing the recording and all of us said, "oh terrible, listen to it, it is awful", and but you know we were good sports, we had started. So we tried, we did our best labeling each one as P, T, or K, and I think we all had the feeling when we finished that the data would not make any sense. Nevertheless, I tabulated the data and plotted the data and low and behold, the data made some sense, that is to say there were lot of noise and lot of stimuli we disagreed about. But, if one looked at the distribution of responses just from the three of us and I think we went through the thing twice. One saw that there was a K peak that followed very regularly on the second formant of the vowel, that is for "E", the K peak was high and for "A" it was a little lower and for "a" it was still lower and for "ou" it was still lower, just very nicely down. It was very clear that there was a preponderance of T responses for all the high bursts anything above the K and that there were P responses down at the bottom and in any gaps that were left in between. Well, we looked at these data and we sort of agreed that we were experts and nobody else would hear them this way. And we certainly could not publish a paper with just our own judgments. So I took this recording up to **Storrs**, I played it on a portable, it wasn't an acoustic, it was an electrical phonograph. But it was the next thing too an acoustic. It was a little portable job and I played it in a great big barn of a room in an old barracks that had been converted into classrooms to a class of about 50 students. And I explained to them that I was going to present these sounds and that they were to try to hear them as syllables, a variety of vowels, each one beginning with either P, T, or K and they were to write down P, T, or K and to guess, if necessary, and …

Frank: Not to laugh.

Alvin: And all laughed.

Alvin: I didn't say don't laugh, but that is what happened. When I said now I will play a few of these for you and there was roar of laugher, well it is ridiculous. I said, "well, come on". So they all did it and I remember as they handed in their answer sheets as they were leaving, I thought it as ridiculous. I took the answer sheets home and tabulated and got exactly the same results that we had gotten from the three so-called experts, exactly, a lot of noise in the data, but there were these clear trends and that is what we published in our first real experimental paper. Interestingly, those data sort of hold up and that leads to a kind of interesting conclusion. It shows that one of the sort of implicit assumptions that we have made in all this Playback research was reasonably justified and that assumption was that the degree to which we had naturalness would not interact with the results that the only effect of having speech this bad was that we would lose many of our listeners that noise would be added to the data. But there would be no systematic shifts, any conclusions that we could draw would be approximately correct conclusions and that has turned out to be true.

Frank: I believe that was confirmed as well or better than any other experiment that we have ever done with….

Alvin: That's right, that's right.

Frank: by **Carol Schatz**, and I think that came along not long afterwards.

Katherine: It must have been a while though, because she was then an undergraduate at M.I.T., I think, oh wait a minute, she was working. No she was at **Penn**, she was working because that's where she first met Noam. Of course, she was a student of **Pierre' s**.

Alvin: That's right.

Katherine: As an undergraduate.

Alvin: The confirmation with real speech was not done in our Lab.

Frank: No. no.

Katherine: No.

Alvin: After she left. She did work in our lab for a while, I cannot recall what she did, she left with Pierre.

Frank: It was later after that.

Alvin: After that?

Frank: Well certainly.

Katherine: But certainly the experiment was done elsewhere entirely. Because I am sure that that technique was a copy of something that I had developed at the Lab and I am sure she did it quite independently.

Frank: Well, let's come back to that.

Alvin: In any case, there were two conclusions that emerged, I think important conclusions that emerged from this initial burst experiment, this P, T, and K experiment. Conclusions that are still correct, I think, and that changed our thinking and that led us really in the direction of the motor theory. The two conclusions were first….., well they are really the same conclusion in a way I know, they are so closely related. What we saw very clearly in these data was the effect of context – the effect of the vowel in this case, if you will, on the perception of the consonant. And that manifested itself in two ways. First, in the case of the K bursts, that is those bursts that were judged to be K, it was very clear that the correct K burst, the one that was heard most frequently as K, was the one just above the second formant of the vowel wherever that formant was. And since that formant was very high with "E" somewhere up around 28,000 cycles because we had only two formant vowels and very low with "U" which meant probably down around what 700, 800 something like that. So the K burst, the best K burst was covering the whole range and that was very clear and that is a conclusion that is still valid, I mean, in this kind of context effect.

Alvin: But there was another aspect of the context effect that is much rarer, that happens only once in a while and then sort of accidentally, that I think intrigued us even more and that was the pee ka poo aspect of thing. The fact that one particular burst, I remember it was one at 1440 Hz, the burst at 1440 Hz, was judged predominantly to be P in front of E. Predominantly to be K in front of R, where it was sitting just above the second formant and again predominantly to be P in front of U. And we thought this was especially interesting and especially challenging and I recall, as I said before we started this recording, discussing this with **Frank** one evening at dinner and it was, I am quite sure, **Frank** who called my attention to the fact that this was really telling. Because what we had to explain here was not that how it was that different bursts were required to give the same phonetic unit in different contexts, but rather something else, which was how could we explain how it was that the same burst in different contexts gave us different percepts. And **Frank** said well, could be that when the vocal track is shaped for an E, then if you are going to produce a burst at 1440 cycles you can only do it with your lips, but when the vocal track is shaped for R and you want to produce a burst in that same place you can only do it by pumping the back of your tongue up and pulling it away. And I think from my point of view, that's what really convinced me that what was being perceived here was a gesture and not the sound, that's my recollection.

Frank: I think that is essentially…….

Alvin: But at all events, this first study led us to that conclusion. We saw these context effects, they were very clear, they were very reliable. They took these two forms and this had somehow to be explained and it seemed to us that the explanation was very clear that what was invariant for both forms of the context effect was something having to do with the articulation and not something having to do with the sound. The other conclusion….

Frank: I don't think we jumped to articulation quite that fast.

Katherine: I don't think so. I have something to say on that.

Alvin: Now let me say one other thing and that is of course the other conclusion, which I remember we put into the paper and which quite a few other people did pick up, I recall, when we gave a talk about this at the Acoustical Society was that perhaps something more like the syllable…. We had to be careful about drawing any conclusions about how phonemes so called, as everybody referred to them then, were isolated bits out in the sound stream; that the coding unit, as **Frank** used to refer to it, was larger than that and conceivably as large as the syllable.

Katherine: I just wanted to say since I came along. Actually as the second formant paper rather than as this paper was finishing up, but it was still quite new in the lab. I mean as a piece of work that I think it's important to note that at least as I understood what people were saying to me when I came in, you had convinced yourself of the importance of relationships or gestalts if you like as containing cue structure, but you were not yet heavily invested in (even with the transition paper) the time varying aspects or the gestural pattern as being the center of the event. I think that came a little later and I think also that indeed many people today who would agree with you about relations among events as forming the cue structure, for example, Ken (**Kenneth Stevens**). They would not agree with you about the necessity of incorporating the picture of the motor dynamics. I think in many ways that's the beginning of a split of points of view. People were willing to go with the notion that there was more to the perception of complex sounds than an analysis into steady state formants or indeed an analysis into pure tones. But they were not willing to go with you as with a gestural analysis and in particular with the importance of time, the time varying quality as being important.

Frank: There was another interesting phenomenon there, as I recall it Kathy, and that is that there are some people who were willing to go along on this with the observation that well after all isn't this what phoneticians have been saying for a long time. That we make the Ps, Ts, and Ks with different parts of the articulatory anatomy. And it isn't surprising therefore that you are finding relationships that look back to the way phoneticians have always known these sounds were made. But, if you say that, are you saying anything very interesting about your sounds. Is this anything more than articulatory phonetics translated into the acoustics? And that was another line of defense against our ideas, if I may put it that way.

Alvin: Oh yeah at that time. I think we were being, excuse me...

Frank: Yeah.

Alvin: I think what you have said is correct and what **Kathy** has said is correct, but I would emphasize that we were at that stage I think, being fairly explicit about the initial version of the motor theory that is it is not the version any of us subscribed to now and **Kathy** is right. We weren't talking about articulatory dynamics and I think if you go back and read in fact our first transition paper you will see that, because in that transition paper I am ashamed to say we were talking about rising transitions and falling transitions and the categorical, you know.

Frank: A kind of plus and minus.

Alvin: Plus and minus, yes that we were doing plusses and minuses, we were not talking about the dynamics at all and I think you are exactly right. But what we were talking about was what I now consider and I think what we all considered to be a very preliminary and indeed naïve version of the motor theory, which was, but it still sort of central to the motor theory and that is, and what is being perceived in the end is something about the gesture. What about the gesture is something else again and it was naïve from our present point of view in that it was the behaviorist kind of motor theory. That is, my assumption at least was that this had come about because people had had long experience in associating the gesture with the sound – that there is not a separate phonetic system, you see, that the phonetic system works like anything else. It is just that in the case of experience with the development of the phonetic system, people have for years and years and years had a chance to associate the sound and the gesture. As a result of that, they cannot disassociate them and so whenever they hear the sound, they get back ultimately to some aspect of the gesture though not necessarily to its dynamics or certainly not to the gesture as we see it now. As I said, and as they confirm, you go back to our first transition paper and you'll see we were talking about plusses and minuses, rising and falling and put together a rising second formant and a falling third formant and you get a wonderful result on that kind of thing.

Frank: Can you sketch through the transition experiment – who did it and what happened and so on – because that was essentially the second major experiment that came out ….

Alvin: And it gave us the second kind of cue.

Frank: Well, third if you pick Pierre's two-formant vowels.

Alvin: Yes that was study. That was not an experiment.

Frank: That's right.

Alvin: It was a demonstration. It was an interesting one and an important one…. Well, the transition experiment came about, I think, because of our experience again with this word 'kill'. It did become plain to us, though I am not sure we formulated it this way in our thinking, that somehow this L was in the dynamics. And also that the K was somehow not just in the burst. I mean it was clear that when we erased the burst from the kill pattern, it still said something like kill.

Frank: Seems to me I remember that we also were having trouble understanding why the Ps were so poor.

Alvin: Yes.

Frank: That the Ps.

Alvin: The burst didn't seem to work very well.

Frank: They didn't work very well. They were what were left over.

Alvin: The K was the best.

Frank: The K was good, the Ts were marginal, and the Ps were poor.

Alvin: Were poor. That's true.

Frank: Why? Well, and then you look at the patterns and you saw these transitions and the question was do they contribute something?

Alvin: Yes.

Frank: And they were initial transitions because that's where they show up best not unlike the L in kill. My recollection is that that is the kind of observation from which the experiment took off.

Alvin: It is hard to me to recall exactly. I am not sure.

Frank: You did the experiments.

Alvin: Well, I am not sure the transitions were all that apparent to us at that point, perhaps they were.

Frank: So they were. After all, this was one of the contributions Potter, Kopp and Green made….

Alvin: They have mentioned these, Yeah, yeah, right.

Frank: …including a hub, which figures later on with respect to locus.

Alvin: At all events, putting all these things together, we did begin to wonder whether what we came to call the transitions carried any information and so we just designed some patterns, listened to them, and it was clear that the thing worked, and then it was just a matter of going back and trying to do this more systematically.

Frank: Based on the pee ka poo experiments.

Alvin: Well, yes and taking a number of vowels and then varying the transitions systematically, randomizing these and presenting them to listeners for judgment. We, at that time, tried to do both BDG and PTK on the assumption that the difference was that in BDG the first formant was rising, which turns out to be still correct really, and that for PTK, it was straight, which turns out really not to be quite right.You know, straight when the syllable starts, and we did force people to judge BDG and PTK. As I recall, our BDG results were cleaner than the PTK, but at all events, the outcome was very similar to the outcome of the burst experiment in that subjects were consistent – I mean consistent patterns emerged. Results that we then attempted to interpret not on the basis of gestures as we see them now, but on the basis of a sort of a crude acoustic basis – the transition was either rising or falling and that was the important thing somehow. And we tried to see if we could do it really on a binary basis. I haven't read that paper recently, but I am quite sure that is….

Frank: That conclusion was fairly tentative because I do not think we believed it at that time, but it was in the tradition of **Chomsky.**

Alvin: In fact, I think Frank, one won't find that interpretation in the monograph we wrote.

Frank: No.

Alvin: One will find it in some more theoretically oriented papers that we gave about that time at the Acoustical Society.

Frank: That is only place I recall.

Alvin: Actually, I think that at the time…

Frank: That is the kind of review paper that I believe I gave to the Acoustical Society.

Alvin: Yes.

Frank: Shortly after the second formant transition paper.

Alvin: Right, I think this actually came out before the experimental paper was published.

Frank: Quite likely.

Katherine: Yeah, I think that's the first time I ever heard you, I think that paper was given at an Acoustical Society, which took place, some place around one of the Harvard lecture halls.

Alvin: But as **Frank** said, excuse me.

Katherine: I remember hearing it.

Alvin: As Frank said, we were trying quite hard at that point to make our results fit with current thinking about speech and so there are binary oppositions and features.

Frank: Current thinking was analyzed with the phoneme with some reservations about the syllables.

Alvin: Yeah, but also you know that it is a set of binary choices.

Katherine: But also that it is acoustic. Remember that was a very heavy prejudice at that time, while there were people who talked about the syllable of traditional articulatory phonetics. Under the influence of **Jacobsen**, they were considered to be old fogies.

Frank: That's right.

Katherine: The big new thing was that everything had to be in the acoustic signal.

Alvin: Well.

Frank: Jacobsen was very explicit about that.

Katherine: Yes.

Alvin: The **Jacobsen, Fant and Halle** paper said rather explicitly did it not, that there were three domains really that mapped very straightforwardly on to each other, the articulatory, the acoustic, and the perceptual if you will, and that was quite explicit.

Frank: And the closer you got to the ear, the closer you were to truth.

Alvin: That's right.

Alvin: Well, at all events, from my point of view at least, that was the beginning of the conviction that there was an interesting relation between the acoustic signal and the phonetic message, that it wasn't just a simple acoustic alphabet, but that there was more to it and we knew a little about what more there was and that did lead us to an early version, a very early version, of the motor theory and ultimately then to an appreciation of the importance of coming to understand articulation better. Yet also in these two experiments managed to reveal the two basic kinds of cues. The one kind represented by the transitions that are formed when sounds originate back in the source of everything and have a chance to be filtered by the entire vocal track. Those were the transitions, for example, and on the other hand, the sounds that are produced at the point of consonant constriction of the burst of the stops, the noises of the fricatives and so on. And those remain the two major kinds of information.

Frank: On acoustic basis.

Alvin: Yes, on an acoustics basis and the speech, certainly.

Frank: It was at about that time that you joined the Lab , was it not **Kathy?**

Katherine: Ah, yes just about then. I gave some thought to this the other night. At the time I came, the results were in on the transition experiment and **Pierre** was spending a great deal of his time on what was to become the locus paper. I remember being told I think that while **Al** has just presented two kinds of cues. I do not think we were even articulation minded enough. I mean we were so, and I have come in from a psychophysics lab and indeed it is interesting that you thought that what you wanted to hire was somebody like me.

Frank: yes, I went to **Walter Rosenblith**.

Katherine: Yes, whose experience had been in a psychophysics lab. He did not go looking for an articulatory phonetician, which, I think, indicates. I mean, it always worked out all right, but I think it is interesting that you did not look for somebody with a training in articulatory phonetics, which would in many ways….

Frank: Of course, **Pierre** supplied that particular thing well, but in terms of..

Alvin: But in terms of old-fashioned articulatory phonetics.

Frank: Ya

Katherine: Right, but he was very new-fangled about this sort of thing and I wanted to remark about several things about the locus paper. Well, first I wanted to say that when I came, there was still a great deal of preoccupation with the transform. Nobody really talked about acoustic phonetics as it was even then developing, I mean nobody went back. I had read Helmholtz as it happened, but I read it because I had been at Harvard, but I hadn't – nobody around the Lab was running around the clutching a **Helmholtz** to their bosom, although it might have been wise. We were still very convinced of relational cues, gestalt-like cues and, indeed, I take the effort of the locus paper, not to have been the return of articulatory phonetics, which is what **Larry** says, but I believe historically, it was an attempt to get back to a relational cue structure.

Frank: Well it was looking for a way to rationalize variable acoustic cues as acoustic cues.

Katherine: That's right and it was a rationalization that was non-dynamic.

Alvin: Exactly.

Katherine: In conception, because.

Alvin: It was an invariant, I mean the locus was invariant where as the transitions were variable.

Katherine: That's right and so were we.

Frank: That's true.

Katherine: And further more, while we were using formants, the distinction of the reference back to the articulatory phonetics of the situation was not yet there and, in fact, nobody talked really very much about formants. And I remember you directing me to the fricatives as an example of some class which had a cue structure, which appeared to be non-formant oriented, and I therefore was convinced that if I was to take on the fricatives, which I was sort of given as a project, the way to go was to try very very hard to escape from formants. And I did the experiment that I did because I could not do it. (Also at that point, after a brief period with a horrible wire recorder that kept producing these great balls of snarled wire, we finally had the beginnings of the plastic tape recorder.)
Frank: Yes, we got one of the early Magnacorders.

Katherine: Yes.

Frank: And they were quite a prize, especially for laboratories without very much money.

Katherine: Yes. I remember that I did what was, for me from Steven's lab, the obvious thing to do, which was that if you had a two-dimensional cube, you would try to cross combine. So, therefore, given that I could now chop up little pieces of tape that is precisely what I did.

Frank: Why don't you describe the experiment because its …...

Katherine: The experiment is a very straightforward one. If you look at spectrograms of fricatives it is very clear that they have a vocalic portion and a portion of friction, which was an obvious division given the experiments that had already going on particularly the burst and steady state vowel experiment, and indeed, you could acoustically isolate them by running a piece of tape across the heads and you could, therefore, use a one sided (because otherwise you get cut) razor blade to cut the one piece away from the other piece and you had to be careful it wasn't magnetized or you got a clunk and you then recombine these pieces again systematically, I think **Al** and I both showed the effects of what conscientious and careful training in ordinary psychophysical technique in that we were both well aware that the way you constructed things was to make a matrix of combined stimuli. So that it is indeed exactly what we did and we played them to people and came to the astonishing conclusion that the transition *per se* was carrying information.

Frank: Especially for "f" and "theta".

Katherine: Especially for "f" and "theta".

Alvin: Yes, I think my recollection was that this showed two things that there were strong fricatives and weak fricatives and they behaved differently.

Katherine: Yes.

Alvin: They operate on different cues.

Katherine: Yes, and I believe also, while I didn't think of it certainly in articulatory terms, it reinforced I think all of our belief that the transition experiment had not been a phony. It also began to reveal to us that there was "the place of articulation," an old fashioned articulatory phonetic cue, that cut across manners of articulation, which was an important thing for us. And we also became convinced, and I can remember again arguing with people at MIT about this, we became convinced that there was no steady state cue for "f" and "theta" that resided in the friction only.

Alvin: Certainly not very strong.

Frank: There was manner information, but not place information.

Katherine: That's right and at that point I think (and this I think represents the beginning of dynamic thinking) at that point, we began to build a synthesizer I have forgotten its name.

Frank: Octopus.

Katherine: Octopus, in which there were controls that were aimed specifically at producing the time-varying information. I realized that you could do that by painting, but we hadn't thought, I think, that Octopus represents, in a sense, the beginning of the motor theory because…

Frank: Because it had eight arms? No.

Katherine: …because the notion that you step something through a time course and that *per se* is important was a change because with the Pattern Playback you could let it run, you could stop it. But how fast it ran was not fixed. With Octopus, we were stuck with having to choose.

Frank: Yes. You were simply lining up events that would be run off on schedule.

Katherine: Right and you, as you may recall, you choose how fast the transition went and at that point we began to think about speeds of transitions and at that point I think the bahwah experiment came along. And in some ways, of all these experiments, the bahwah experiment is my absolute favorite because I think it is really the formal beginning of the motor theory in that it makes it very explicit that the dynamics of events are central in how they are perceived.

Alvin: Well, I disagree with Katherine, excuse me,….

Frank: Go ahead.

Alvin: …a little bit. We are talking about different motor theories that is the problem. I think **Katherine** is exactly right in what she is implying here, which is at the early version of the motor theory, certainly did not properly appreciate gestures as gestures.

Katherine: That's right.

Alvin: That it was still sort of acoustically based as I tried to say before.

Frank: Articulatory invariance instead of acoustic invariance,

Alvin: That's right, what we believed (certainly what I believed) was that the acoustics (the auditory thing) was at the base of it, but that through long experience (this is the typical behaviorist kind of notion) through long experience, associations had been developed with the articulation so strongly that the articulation was also now influencing the percept. But, in the case of the locus you see I was delighted that we were getting an acoustic invariant, but that didn't alter the fact that there was still information in the transitions and that that made sense only on something like the motor theory. Because one still had to explain (for example, in the case of the D locus, which was the clearest and neatest, you know, something like 1800 Hz with all these transitions going off some rising to E and some falling to U) why a rising transition and falling transition would nevertheless sound the same you see – given that you start with an acoustic invariant. So, I think that the aspect of the bahwah experiment that should have given us (certainly me in this case because I remember it so well) insight into exactly what you are saying is that as we did the bahwah experiment was a very simple one in which we had a bah with a relatively rapid transition and then we simply slowed the transition down in steps and we randomized stimuli as usual presented these to listeners and they gave us very nice functions bah to wah. I noticed that the cut off point, the shift from bah to wah and for several other vowels too, occurred at about 50 milliseconds for reasons that were of no interest. Although it may be an interesting number.

Frank: Sure.

Alvin: Okay, it occurred at about 50 milliseconds and then I thought "Ah, yes, that's an auditory phenomenon. That 50 milliseconds is perhaps approximately the integration time of the ear". In fact, there was some evidence in the old…

Frank: Licklider's work.

Alvin: …well I remember looking in the old Stevens and Davis book on hearing that there was a study reported in which it was found that in order for a signal of 1000 Hz to be perceived as fully pitchful, you know, if you match it with steady state long duration sound that it asymptoted out at full pitch at about 50 milliseconds.

Frank: A lot of things converge on 50 milliseconds.

Alvin**:** So, I said, "Ah, 50 milliseconds is the integration time of the ear", but then my next step was to say – it was a pretty smart idea at that time – "well, if it is within the integration time of the ear then you don't hear the glide, you see you just hear an event, a knock, a burst or something and if it is longer than 50 milliseconds then you hear this glide, the semi-vowel, the wa you see." So, the difference I started saying myself between the stop and semi-vowel is that in one case, the semi-vowel, you hear the glide and in the other case you don't hear the glide. You cannot tell whether it is rising or falling, but I said to myself this is an auditory phenomenon and I remember vividly getting that idea and running into the Playback and drawing not bahwah, but thin lines at varying slopes and varying durations thinking that what I would hear was that for all of those that (they all had about the same excursion you see, previously for all of those below 50 msec, I would not be able to tell whether it was rising or falling) and for all those beyond 50 msec, I could tell whether it was rising or falling and I tried that and low behold it did not work. It did not work! I mean I can tell whether it is rising or falling. I noticed things were very, very short. I mean I could not tell whether it was rising or falling when it was vertical but all those at the slightest angle I could tell and I remembered puzzling over that and being very disappointed. You see because I had this wonderful idea. But that is fairly late in the game now. So, that shows you the extent to which I at least in spite of the motor theory was still thinking in acoustic auditory terms because here we were dealing with transitions which were really the essence of the whole argument and still I was not only prepared to entertain this idea, I thought it was exactly right and I was disappointed that it did not work.

Frank: Its amusing that we were calling them transitions all this time too.

Alvin: Well, yes. That is an interesting…, Well okay. But let me say, when I remember using this at least in talks that I gave, I do not think we ever published it. But, I remember using this as a sort of a beginning of an argument that things were really more complicated than we thought. You know? That one could not distinguish, stops as a class from semi-vowels as a class on some simple auditory basis, yet this was a fairly categorical distinction phonetically. I also found in doing this, though I did not make a point of it at that time, that as I listened to these ramps as it were nonspeech ramps, that I did not hear any categorical difference at all. I mean it was just a succession of whistles that were either rising or falling in pitch. In this sense, Kathy is exactly right, you see that we did not…

Katherine: I think it is interesting also to go back to Potter, Kopp and Green again since we have mentioned it once already, because I remember that I read Potter, it was published, I think in **1952**.

Frank: About that time.

Katherine: But it certainly was around the Acoustical Society before that but I remember they also could perfectly well have had a motor theory but didn't and they didn't in a interesting way, which was that they assumed that you could recognize patterns by their transitions. That is the way to teach people.

Frank: That was their whole orientation, how to read spectrograms.

Katherine: Right! But their idea was how you read, while we from the beginning believed that what we identified as ways of reading spectrograms if you like or as salient cues, to put it in another way, were the way the cues were represented. Their attitude was always that things like the transitions were useful to you if you were trying to read them but they did not have to do with the cue structure. They still went back, because they were at Bell, to essentially Fletcher's point of view, which was that by knowing the frequency, energy, distribution you would know what the nature of the sound was. That is, they assumed that you sort of fell in something like the S-friction patch and that was where the acoustic cue was but may be it was convenient to know what the theta transition patch, that was where the theta was, and that was what you used acoustically, but it was convenient for you to notice the transition because that would help you read it.

Frank: They were also going back to Alexander Graham Bell of course with visible speech. That is one reason that for the emphasis on reading these things.

Katherine: Yes.

Alvin: Except that his visible speech was articulatory…

Frank: Was articulatory! Indeed, very interesting, very revealing, exactly so.

Katherine: Yeah, but they were interesting people. It was interesting how resistant we and everybody else were, and still are to the notion of systematically relating acoustic cues to their articulatory origins.

Alvin: We are responsible for many of the terms that have been adopted like burst and transition and so on.

Frank: and Cues.

Alvin: Well, cues have been used in psychology for many things, I think. That is another story we can come to. But Frank said that we call these things transitions which is a little misleading of course because that implies that they don't count for much they are just a way of getting from one place to another. If you go back and read our first transition paper, you will see that we were aware of that, that there is a statement early on. We are going to call these transitions, but we do not mean to imply by that, that it is merely a way of getting from one place to another but I think Frank is right. It is not the right descriptor because it does imply that there is no particular consequence.

Frank: May be this is no more inappropriate that any other place to interject a little bit on the old Octopus as a device and what it teaches an experimentalist anyhow. It was a gadget that was intended to generate syllables and the purpose was to make it easy to manipulate the variables that went into well, perhaps two or three syllables at a time, short utterances. And the idea was very simply, that you make up a sort of time grid of eight successive events, which would necessarily follow each other because they were driven by a rotating telephone-type rotory switch. Each event could be varied in its duration and in its composition in the spectrum, that is you could pick the kind of sound you were going to generate – buzz or hiss. You could manipulate the pitch. You could manipulate three formants independently as to frequency, as to intensity, and as to the transitional course that you could come up fast and level off or come up slowly. It is as if you said, lets take a spectrogram and cut it into eight pieces that are typical of friction or transition or steady vowel or something else and then we will build a machine that can generate that kind of section in all its variability. The result was that there was a vertical line of knobs, potentiometer knobs, for controlling the variables of each event. And eight columns of these across the face of the relay rack, and your whole trick in setting up an experiment was to twiddle these, something like 80, different knobs in order to set the variables the way you wanted them for the pattern. And then you turn on the switch and, low and behold, it cycles through and you listen to it. Go back and change again, but this time you don't have to use a paintbrush. There are several things that can be said about this. It had its mechanical and electrical faults all right, but it worked.

Alvin: It worked very well

Frank: It generated better speech than we ever got from the Playback.

Alvin: Oh yes, much better.

Frank: André Malecot's demonstration was a good example.

Pat: What is it called?

Alvin: It is just a demonstration of Octopus.

Frank: It ends up with "no comment". Although, I think it was pitch inflected. But it had a fatal error. You simply cannot think in terms of 80 knobs. It is not possible. No experiments were ever done on that device.

Katherine: I did one.

Frank: Which one.

Katherine: I did another fricative experiment that is reported only in the corner of a Progress Report**.** The object of which was to show that the intensity of the friction was part of the cue structure of the light grade fricatives of "f" and "theta". Because there was another peculiar delusion in the literature at the time, which was that, what you needed was a kind of **clou tee**, that is you could always make a cue better by increasing its intensity. So it was possible. We had no intensity control on the Pattern Playback. I mean we had, but intensity control on the Pattern Playback was not very effective. And so that it was necessary to use it for this purpose.

Frank: The only intensity control that really worked on the Pattern Playback was Pierre's invention of using colored paints, which had different reflectance's.

Alvin: As they evaporated, as I recall, the reflectance is changed. But, I remember we spent a lot of time calibrating those making different greys in calibrating…..

Frank: But in a sense the Octopus taught us something. That you can make up synthetic speech on an event by event basis. You can do pretty well, a pretty good job of getting acceptable output but you cannot experiment with it very well. I had forgotten that **Kathy** had done an experiment, I thought none have ever been done with it, simply because it was unusable as an experimental tool.

Alvin: We should in that connection say that some thought it was called Octopus because it took eight arms to operate it properly.

Frank: That is not strictly what it is called after.

Katherine: There are a couple of,…. We knew that already though. I mean that had always been a point of yours because I remember again very early on I heard the Lawrence machine and you pointed out about both the **Lawrence** machine and the early **Fant** machine.

Frank: That they were not good experimental machines.

Katherine: That there were not good experimental machines and it was scarcely surprising because they had not been designed with any kind of contact with experimentalists, and I think that is correct.

Frank: Well the primary aim certainly with Walter Lawrence's machine was to produce good speech, not to produce a device that was convenient for the experimentalist.

Katherine: Well, our point that phonetic perception did not interact with naturalness was regularly and routinely disbelieved.

Alvin: Yes challenged by almost everybody who came to the Lab.

Katherine: Yes.

Frank: Almost everybody who came to the Lab raised that question. How do you know that this will hold for real speech. It was in that connection by the way another assumption that we were making that **Kathy** sort of alluded to, I think we talked about this the other day that obviously we could not investigate all consonants and vowels at the same time. So, we had to do them by classes and of course we chose to do them by the standard manner and place classes and it turned out that this was okay. Now you see if those had not been real classes, we had been in some trouble because we would have drawn a conclusion working from BDG, that would not have worked for PTK at all then if we had opened it up to include the semi-vowels it would not have worked. But in fact, we did not lose much by this procedure, simply because these are genuine categories. They do not overlap an awful lot, so that if we find the kind of information, what the information has to be like in order to distinguish BDG in a situation, which were using only BDG and subjects are not allowed to say that was wah or rah, or lah, or yah. It turns out when we do allow them to say wah, rah, lah or yah. We do not get into a lot of trouble.

Katherine: But that also was challenged.

Frank: It was challenged all the time.

Katherine: Right, but we were still picking… We did not always pick chance classes according to what was the received articulatory phonetics of the day always. Remember, **wirley** was for us a class. It is not in old articulatory phonetics texts – partly, I think, because many phoneticians were more sophisticated than we about the variants of R. But, it is interesting, we stuck with things that looked to be spectrographic classes and that's what did not get us into trouble. BDG the fricative says a class, although in some ways they are not a class. Worked pretty well for us, but we defined our own class when it came to wirley.

END OF REEL 5 PART 1

Frank: Well let me lead us in a slightly different direction. My recollection is that after the pee-ka-poo – the burst experiment and the transition experiment – that the next line of effort, more or less than parallel with what **Kathy** was doing in fricatives, was to combine bursts and transitions in the experiments and I think you might have something interesting to say on that. Shall we pick up that?

Alvin: Well, I was going to pick it up from just a slightly different point of view and talk about the experiment that you, **Kathy**, and I were authors of and that **Kathy** was a senior author of, which was the third formant of transition experiment. Speaking to the same sort of question, we did an experiment, **Kathy** did most of it as I recall, in which we systematically varied the second and third formant transitions. And I think that is an interesting experiment or at least what we said about it was interesting, because it was sort of the high point of our taking cues as cues, seriously. Because what we found, one of the results that emerged as I remember, was that in general, listeners would decide quite reliably between B on the one hand and D&G on the other on the basis of the second formant transition, but the second formant transition did not do a very good job of distinguishing D&G. The third formant transition didn't have much to do with B, but did distinguish very nicely between D&G. Do you remember that? And if you go back and you read that paper, you will see there is either that result or a very similar one, it was that kind of thing that led us then into some speculation on the question how do cues combine? That is we were looking for a model. Our model had nothing to do with articulation, really if you think about it, the model about cue combination. We were intrigued by the notion that each cue served its own particular purpose as if God had arranged it so that once he finds out that this cue does the job between B on the one hand and D&G on the other then obviously you need another one that is going to do the job for D&G. And indeed these led us to …

Frank: This was the Jacobsonian idea again. But we tried also to put vectors on…

Alvin: That I remembered, I think it was you **Frank** who suggested, well maybe we ought to try a vector, that these cues are acting like vectors.

Frank: I didn't generate all the bad ideas here, but.

Alvin: No, it was not a bad idea. But then I remember there is some very interesting plots and some very interesting speculation in that paper it repays rereading in fact. But I think that if anybody really wants to accuse us of taking cues seriously, I mean the cue story is a vexed subject as you surely know, **Pat**. On the one hand one needs a concept like cue just as a descriptive thing. I mean you have to talk…, I mean, you only manipulate certain things. What you are manipulating we called cues if it turns out that the things that you manipulate have an effect on perception. So, it is a neutral term theoretically. Basically it is a neutral term. But it becomes not a neutral term, or it can become more than a neutral term, it can come to carry a lot of theoretical freight, once you start talking about how the percept is formed by somehow combining separate cues and we did that more explicitly I think in this paper that I am talking about than anywhere else. So, if somebody wants to accuse us of having once given theoretical weight to the concept of cues that's the paper, on which they can do it.

Katherine: Yes, but again looking to that paper. Well, I remember its history pretty vividly, there is a crude model of vector combination in that paper, which I think I thought about a lot, in fact everybody thought about it. There is a crude model of vector combination in that paper, which I think was correct. It was later reconfirmed. But I think that you need the third cue kind of thinking actually is not so much in that paper as it is in the **wirley** papers because you remember you can get W, R, and Y quite convincingly from two format patterns, but you need the third format for L. Actually, a simple vector combination model would work quite well in that paper. But an interesting aspect of that paper, which shows what some of our problems were, were that we had a horrible fight, as you may recall, with **Peter Ladefoged,** the young **Peter Ladefoged,** because what we had done, carried away with vector combination and **Al**'s and my training as experimental psychologists, was to systematically combine factor I with factor II in all possible combinations. That will give you, obviously, some combinations that do not arise in the real world.

Alvin: Many.

Katherine: Many.

Frank: That was indeed one of the troubles that we had with the three-formant paper and with burst and two formants. By combining these things you generate a very large population of non-entities and they make the experiment difficult for the perceiver.

Katherine: No, they don't. That is they may make it difficult for the perceiver if you mean the member of the audience. They don't make it difficult for the listener. That is, listeners are perfectly willing to accept many of those quite impossible third formant patterns, which are sort of cross-bills, you know. Now, of course I at least was very unsophisticated probably substantially more so than you about what the patterns looked like once they had gone through the visual-to-auditory transform and one of the things we were accused of in this period was not understanding what we were doing acoustically well enough. That is taking the patterns themselves, as painted, seriously when in fact what people were listening to was something….

Frank: Was quite something else.

Katherine: Was quite something else. But these cross-billed patterns are not nonstarters acoustically that is, you cannot ask somebody "tell me, which of these patterns are unnatural" and get them to draw a sharp line around patterns that are unnatural.

Frank: I was referring rather to a dilution effect K**athy.**

Katherine: Yeah**.**

Frank: Which is there. It makes the experiment hard to do and hard to interpret.

Katherine: Okay, yeah, there is a dilution effect but it is not true that listeners distinguish between patterns which are impossible and those that are not, they do not. And in fact, part of the reason I think that we got this kind of noise at this time was because people were still talking about idealized patterns spectrographically. Spectrograms were beginning to be available. People were talking about the kinds of careful enunciation patterns of short segments that they could see. They did not recognize that in fact in real speech, there are all kinds of events, which can occur that do not represent well formed, utterances of this type.

Alvin: Well, you know it was about this time that categorical perception came along.

Frank: Yes, and in what context?

Alvin: Well, that is an interesting story I think not least because it captured the attention of the psychological world more than anything else we have done for bad reasons, but interesting reasons. Also, it has been much misunderstood.

Frank: Capture the attention, is a euphemism I might say**.**

Alvin: It has been much misunderstood. It has been unfairly maligned and, in the ways that it might have been maligned, it has not been maligned. I mean that there were problems with it. It was wrong in certain respects that nobody seems to care about and right in other important respects that people don't still understand I think. And **Kathy** was very much involved in this – but let me tell my side of it and then K**athy** can tell hers. All this got started because I was still at this time – and are talking now about, I think, what roughly **1955**.

Katherine: Yes, **1955** is right. Because categorical perception and …

Alvin: When was that paper published, **1957?**

Katherine: Yeah, but it was first given at an Acoustical Society meeting in Boston when I was pregnant.

Alvin: Right. Okay.

Frank: So we've got the date on that!

Alvin: Well, it all arose out of my conviction at that time that we were dealing with a motor theory in the old fashioned behaviorists sense. And I had learned as a graduate student that there was such a thing – if you look at the world this way – such a thing as acquired distinctiveness and acquired similarity. Acquired distinctiveness arises in the neo-behaviorist tradition *a la* **Clark Hull**. Acquired distinctiveness arises when a person has long experience attaching some acoustic signal, let us say, some sensory event out there, with some motor response. Let me start over again. One starts with two signals or two sounds, which are not very different from each other. They can be distinguished, but just barely. Now, one can presumably increase the perceived difference by attaching each of these to radically different motor responses. The point being that associations are formed between the acoustic signal and these very different motor responses and then the feedback, the proprioceptive return as it were from these motor responses enhances the distinctiveness. So, that what started out to be not very different winds up being very different because automatically, you see, when the stimuli presented the implicit motor responses are evoked and these provide distinctive feedback. The opposite phenomenon also presumably can occur, acquired similarity, that is, you start with two acoustic signals that are quite different, but now you attach them to same motor response and a consequence of that is that they are now hard to distinguish. That is what I had learned as a graduate student. Of course, there is not one bit of evidence anywhere in the literature that either of these things happens. But that didn't matter. They were supposed to happen. And it occurred to me that speech presented the perfect case because what do we have in speech? We have these acoustic continua, which are divided very nicely into phonemes. And so, all the stimuli within a phoneme category, if you will, have through long experience become attached. All the acoustic signals within that category, after long experience become attached to the same motor response. Yes? The person says B, ba. On the other hand …

Frank: The same linguistic response.

Alvin: Well, also I was thinking the same motor response. If it is with the same vowel you see you've got a bunch of…, yeah, ok the same motor response. On the other hand, stimuli that could be separated by the same physical amount – acoustic signals that differed by the same number of hertz, cycles, or whatever, could cross one of these boundaries and of course the subject would have had long experience in attaching different motor responses to those like ba and da. So, I said this ought to show up, a perfect case. And there was a graduate student then working in the Lab, doing his research at the **University of Connecticut** named **Belver Griffith**. You remember **Belver,** and so I discussed this with **Belver** and he decided to do it as his dissertation. And so, **Belver** set out to test this with steady state vowels. Steady state vowels produced on the **Playback**. **Belver** worked and worked, fortunately, as it turned out, **Belver** was very fussy. He couldn't get a set of vowels that satisfied him. Meanwhile, I became a little impatient and so with **Kathy** we set up the same experiment with stops ba, da, ga, at the same time that **Belver** was still fussing with his steady state vowels and we got our stop vowel syllables made and he was still fussing around with the vowels. And we did the experiment. By the way, I should say that to do this experiment, we devised a psychophysical procedure that had never been used before I think or way of going about this that had never been used before, that is instead of just getting JNDs. Just getting discrimination responses out from a single point, we got discrimination responses all along the continuum. And we looked at the results and low and behold, there were these fairly reliable peaks in the discrimination, as we were measuring discriminability at each point along that continuum, something that really had not been done very much before, I guess. We looked at the results we saw low and behold, these fairly reliable peaks that corresponded approximately to the positions of the phoneme boundaries.

Frank: The crossovers corresponded to the boundaries.

Alvin: That is right.

Frank: The peaks to the center of the category.

Alvin: No, the peaks to the boundaries, Frank.

Katherine: The discrimination peaks.

Frank: The discrimination peaks. I am sorry, I was thinking of the identification**.**

Alvin: So, at least my reaction to this was hoorah! Here we have acquired distinctiveness and acquired similarity. We've found it at last, you see, and we have a theoretical explanation for it. Well of course that theoretical explanation is entirely wrong. The phenomenon is quite genuine. The explanation was entirely wrong. **Kathy's** big contribution at that point, and I thought it was a very a significant one, was to develop a very straightforward model not for explaining categorical perception, but for measuring it. And perhaps you could speak about that. It is really very, very good.

Katherine: Yes, the assumptions are laid out. It is very simple – it says, the model says supposing you assume that your discrimination is the byproduct of your identification then what you do when you are presented with a trio of stimuli is to name them in sets of three. And let us assume that we ask you to say 'same' or 'different' but you do this by referring to the labels you have attached to the stimuli. So, if you have discriminable stimuli and name them differently you will say BDD and if you have stimuli, you cannot discriminate you might say DDD for the same set and if you make those assumptions…

Alvin: This is for an ABX procedure?

Katherine: This is for an ABX procedure, Yes. If you run through these things using ordinary combinatorial probability, which was again, all the crack. Associated with switching models were combinatorial probability models which we all learned in graduate school and I had it.

Alvin: I did not.

Katherine: Well, I did. I had had a very good class with **George Miller** in which we ran our way through **Feller**, which was the then classic text for this sort of thing, but you learned to figure these things and I figured them. So, we were then able to represent the effects of there being no further information (the language at that time) in the stimulus beyond that which we got from naming. This was again very much in the spirit of the kinds of models on information transmittal that **Clyder** & **Miller** had been developing at the time that I was in graduate school. So, it is a very interesting way of measuring what you get. I was always convinced that my failure to be good enough at this to be able to calculate the expected variance was going to condemn us to outer darkness. However, I note that although many statisticians of real consequence, like **Neil Miller** and so forth, have worked with these models since. Nobody else has ever come up with a model of the expected variance, so I guess it cannot have been so bad.

Alvin: But you see the great contribution of **Kathy's** model. Really, a very significant contribution made to this whole notion was that her model enabled us to see to what extent our results did indicate perfect categorical perception and the answer from the very beginning was that it was not perfectly categorical and we were able to be very explicit about that. That is a point that has never really been fully appreciated. Okay, because the extent to which the perception is categorical varies according to a large number of conditions exactly as one would expect it to. And we were able to see that and at least make gross estimates of the extent to which the perception did or did not fit what would have been predicted had it been perfectly categorical would have kept us from going too far astray. So, I say that the theory was all wrong. The phenomenon was very real one, it still occurs there is a recent paper for example by **Watson** and **Diane Kewley-Port** that came out recently in the Journal of the Acoustical Society, which shows various things. It shows that if you use a low uncertainty procedure all peaks disappeared and everything and all the auditory, you don't get peaks anywhere, but if you use the high uncertainty relatively high uncertainty procedure, you do get peaks. But interestingly there is one case in which they replicate an experiment on the **ba-da** distinction, for example, using what they call relatively high uncertainty procedure that kind of procedure we used and they get a lovely peak at exactly is the right place. And then they replicate an experiment that had been done by **Miller** and somebody on a nonspeech analogue, which had originally claimed that it got a peak in the same place and first thing that happens is they get little tiny peak and they comment on that in the paper what they do not comment on is the little tiny peak is in a totally different place. Our peak is exactly our peak and it is exactly where it should be, it is right there on the boundary. And for this auditory thing I had forgotten what the signals were, some acoustic analogue of speech, the peak is quite displaced, which is of course the point of the whole thing. The point is not that categorical perception is unique to speech in the sense we knew that when we did this experiment because there were data in the literature about color discrimination, which showed as it were, that indirectly you could see, you could infer peaks at various places along the spectrum. The question is whether the peaks that one gets in phonetic perception correspond to the peaks that one gets in the closest auditory analogues and the answer to that over and over again I think now is "No"**.** And that makes that even more interesting because what happens in the phonetic case is not only that it is not using the peaks that are there in the auditory case as for example in the most recent experiment by **Michael Kennedy** and **Kathy Best,** but it ignores them. I mean it constructs its own peaks as if it just did not give a damn about what is going on in the nonspeech sphere. That has just been grossly misunderstood and grossly misrepresented for years and years and years, it is just impossible to get it straight.

Katherine: However, let me point out that we knew that because **Belver** who we sort of left playing with the vowels…..

Alvin: Playing with the vowels. You suddenly did this experiment a different…and he dropped that and did something else. **Belver** was a co-author of that paper, remember?

Katherine: Yes, right. Following this exciting experiment, **Belver** soon saw the light and he deliberately did an experiment where one could not argue that ….

Alvin: A beautiful experiment, never published, go ahead.

Katherine: Yes, it is a beautiful experiment in which he varies second and third formant transitions which we had by this time learned to do. I mean we had by this time sort of build up some kind of a know-how around the house about what would happen on second and third formant of combinations. And he was able to show that if you put a third formant in, you would systematically shift the place the peak was. The relationship between predicted and obtained would not change. It was the same relationship, but the peak moved over and we pointed out, I think, (or **Belver** pointed out in his thesis and however, it was reported because I can remember the reporting of all this, although it was never properly published) that it was very hard to imagine that the kinds of peak shift you got from putting the third formant in there, had anything to do with the way the auditory system was constructed. It simply was not plausible that simply adding the third formant changed the whole underlying auditory mechanism and we felt very chipper about that at the time.

Alvin: I felt very sad, that we couldn't get him to write it up for publication.

Katherine: Well, but he was your problem, not mine. But we felt very strongly that we had demonstrated, that the phenomenon was we still believed experience based in some fashion and we still spent a lot time trying to think of ways of *experimenta crouches* to analyze the question further of whether it was acquired distinctiveness or acquired similarity that was still a great preoccupation.

Alvin: Still is a great preoccupation.

Frank: Why did all the heat not light get generated with respect to this categorical perception?

Alvin: I have never understood really why of all the things that we did in the first 20 years or more of our existence this attracted by far the greatest amount of attention among psychologists. May be it is because it is true what psychologists are often accused of which is that they are paradigm happy. And I think this struck people as a kind of, I don't know, a paradigm. We had used slightly different procedures. We were reporting the data in a different way. I don't know, but it certainly caught on.

Frank: Who was the chap who gave us such a bad time on this?

Alvin: Harlan Lane. There were several. Everybody I mean there was nobody who didn't…

Frank: But I have forgotten what the base of his objection was.

Alvin: The base of his objection was that this could be created for any continuum on the basis of experience. I think that was it.

Katherine: Yes.

Alvin: Independently of any motor… Well, I don't know. But anyway, that you could do this for all kinds of…, You could take any physical continuum and somehow do this.

Frank: This was essentially an attack on the speech is special view?

Alvin: I haven't thought about that for a long…. Yes and no because you see we were saying the speech is special, but I at least was also saying it wasn't because this was an example in the early days in my view of acquired distinctiveness and acquired similarity which ought to happen for nonspeech and I cannot remember ….well go ahead **Kathy**.

Katherine: I was going to say, I think the reason that the heat was a pile up. On the one hand you had an attack from people like **Harlan** who believed that this was, you remember at that time, I have now forgotten the name of the man who points out…, There had been in the 50s a bunch of experiments by people like **Jerry Brunner,** whom I once worked for, in which people had showed that experience affected perception. So, that for example I did an experiment, which was a flop as far as **Jerry** was concerned because it showed that perception was rather veridical**.** His hypothesis was that a dime would be perceived as being larger for its physical size than a nickel. That is the nickel ought to be a little smaller. Right? Well, in fact, the ratio of perceived sizes for dimes and nickels is exactly right.

Alvin: Well the claim went farther than that it was the poor kids would show a bigger difference.

Katherine: Yeah.

Alvin: In poor kids it would register for pennies, or something like that.

Katherine: Right. In anyway. So this followed some work by an anthropologist. You remember all this stuff about how the perception of the Eskimos, who had words for 50 kinds of snow, was different from the perception ….

Alvin: I can't remember the name, it was a linguist, a woman possibly at Vassar.

Katherine: It was a linguist. I don't remember. But, anyway, the idea was that experience with objects affected one's perception of them and this was believed to be a special case of that.

Alvin: It went so far. I don't want to accuse **Harlan** of this though I think he might have said it that there were some people in any case who believed that this was a **Skinner** influence, the most extreme kind of **Skinner** influence, that we can distinguish colors because we have learned to call them by different names and if therefore one found a society in which let us say blue and purple were called by the same name, people wouldn't be able to tell them apart.

Katherine: Yes, that is the way the argument goes, but then we were also getting attacks from another group who were the auditory psychophysicists who believed that all this ….

Alvin: Was auditory.

Katherine: So, these effects were all sort of cheap reflexes of the auditory system. Therefore, that the places that boundaries occurred were inherent…

Alvin: Auditory.

Katherine: Were auditory and had sort of come with the animal. And there were very great efforts made to show, particularly in connection with a later paper (the VOT paper) that where were we got the peak was exactly where you would expect it on the basis of continua made up of nonspeech stimuli entirely.

Alvin: Or speech stimuli perceived by non-human animals.

Katherine: Or speech stimuli. And that is indeed the beginning of another whole movement. But I think that one reason we were getting so much heat… We were getting sort of combined heats from two directions at once and people were not very analytic in deciding which way they wanted to bite.

Alvin: Well in fact on the one hand there were people who were questioning the phenomenon itself and saying that it was kind of an uninteresting thing, it really did not happen and if you practiced the subjects longer these peaks would disappear which is the course true because you are getting everybody you are getting a ceiling effect. So, we were attacked on the grounds that this was not a real phenomenon. We were attacked on the other hand on the grounds that it was even more valid than we thought it was. A broader phenomenon than we thought it was, you could get it everywhere. I mean you could get it with Chinchillas listening to the speech sounds, that you could get it with human beings listening to nonspeech analogues of these variations that we were dealing with. So where ever you turned, right or left, we were wrong. Either it wasn't real or it was even more real than we thought. And actually nobody even to this day pays any attention to what I remarked on before which is that the issue is not whether there are peaks or not. The issue is not whether you can reduce these peaks with a lot of training and so on, because we have never supposed that it was perfectly categorical, we have never supposed that phonetic perception cannot be improved. Okay. So the issue is not are there peaks or are there not peaks the issue is simply where those peaks are and whether under reasonable psychophysical conditions you get these peaks and, if you do and if you get them in speech and if you get them all in nonspeech, whether those peaks are in the same place. And of course the fact about the phonetic peaks is that they always occur at the phonetic boundary and we know from dozens and dozens and dozens of

experiments that the phonetic boundary itself moves as a function of context, as a function with many phonetic categories of rate of articulation as a function of all kinds of things and it cannot be that all of those are auditory that when we talk faster we adjust our articulators in such way as to make the sounds conform to a shift in the phonetic boundary that is auditory, independent of articulation. I mean that is just so implausible and yet that is what we have had to face.
People do not look at the phenomenon that way, they still don't.

Frank: Can I link this into the non-science world events, that were going on about that time. We were, during much of the early **50's** and into the late **50's** and beyond, very heavily dependent on the National Security Agency for funding for all this research. We will have to back up a little bit to pick up the beginnings of the electromyographic work and the interest on speech production *per se*, but that work and this work on.., the psychological rather than linguistic aspects of speech perception looked to us to be not the kinds of things that we could expect the agencies interests to put up with indefinitely. That is, they had their problems with speech security systems and our basic results on speech fitted into them very well. But their interests did not parallel ours out into some of these other areas, which said that although they had been very generous in their funding and in their permission to go ahead and do what you think is important to do, there would come a time when that would not work. So, we set about looking around for other sources of funding and you and I went to NIH and NSF with a couple of proposals, one had to do the early exploratory work on speech production and the other had to do with the work on speech discrimination if I recall correctly, that went to NSF. I think both of them were funded for a year or two in this way, the NIH one eventually ended up with the Dental Institute for peculiar reasons and led into long-term program. The NSF one came up for a renewal in a year or two and the renewal fell flat. And the reason it fell flat, was all this flak we had been receiving from….

Alvin: Harlan Lane.

Frank: Harlan Lane in all probability.

Alvin: We heard his paper had been circulated. It was not probability, I was told…, I was in touch with…, Oh well, go ahead.

Frank: Alright. Well these things have consequences outside the scientific domain.

Alvin: Absolutely.

Frank: It was clear that the National Science Foundation for perfectly good reasons on its part, was not willing to fund something that had an element of controversy.

Alvin: I remember vividly because it happened in **1964**, I was on my way out to the center with my family. I had stopped at my father's house in St. Joes, Missouri. We had our application into NSF, as **Frank** said primarily to follow-up some of the work on categorical perception and I remember you called me at my father's house. We had a long talk on the phone. You said you just heard from NSF that there was trouble. And that the trouble had to do with **Harlan Lane's** preprint of his paper, which had been circulated among the panel members or something like that. I recall then writing a letter immediately to NSF, sort of presenting our side of the thing, and asking….

there was going to be a meeting of the, I think American Psychological Association in California after I was scheduled to be there (I think you told me that the NSF person would be at that meeting and that I should try to see him, so I wrote to him.) I gave our side of the story and said that I would like to see him. I went to the meeting and I did see him. I sat next to him as **Harlan Lane** presented his stuff and then I got up and gave our defense, but in the end, we in fact did not get the grant and we did not get the grant and it was exactly for that reason. That was really not a good article that **Harlan Lane** wrote and there was real faults with it but I don't want to go into that on the tape.

Pat : Your response took a long period of time, because when I arrived in **1971**, you were still, I think, at the point of finalizing a rebuttal of that paper.

Alvin: Oh yes we did write a rebuttal that was published in the psychological review, which is where he had published his paper. And I cannot recall now what the problems were, I'd have to go back and read all of it, but they were all irrelevant to the main issue, I think. All of this was whether true or false, was simply irrelevant to the main issue. The fact is that it is the easiest thing in the world to get these peaks, nobody has ever failed to get them, if he uses reasonable procedures. The only time you fail to get them is if you press too hard. I mean you, you have stimuli that are too far apart. I mean there are all kinds of things you can do to get ceiling effects. Also there is one way of going about this that one could model, nobody ever has tried to as far as I know and that is to use the **Pierre DeLattre** technique which will never give you peaks. **Pierre** could listen to any syllable that you presented to him, any synthetic syllable, and tell you within a few percentage points what percentage of naive listeners would call this B and what percentage would call it D and what percentage would call it G. And if you ask Pierre how he did this, he would tell you, it is a technique that I can use pretty effectively. You try it on, you try these things on for size. You make a sort of an estimate. If it is the case that you can equally well hear it as "ba" or as "da" then you know that it is right in the middle. You are not going to get the peak. I mean you hear either **Pierre** what I say either one is categorical, but if I can equally, easily hear it as "ba" and as "da" then I am going to tell you, it is right in the middle. Well one could develop a model of that and if one uses that strategy, then one is not going to show peaks and I think that is what very skilled listeners do as a matter of fact. but that also is irrelevant. The point is we have never said that one cannot improve. Certainly, I have never said, or have not said in the last 20 years, that one cannot improve phonetic perception through practice. Okay. The question is what is being perceived is it a phonetic kind of thing that's being perceived or is it an auditory kind of thing that's being perceived. And, I think the evidence is overwhelmingly that it is phonetic kind of thing is being perceived and this does show you peaks when you use certain kinds of procedures and peaks are always in the right place and they always move as the phonetic boundary moves without exception.

Frank: Let me try to wind off this aspect of the discussion with a question. We certainly went through a lot of effort and work on discrimination. How valuable was it in getting on with the understanding of speech perception *per se* or was this a side issue that we got trapped into?

Alvin: I think it was pretty much a side issue. I mean we were trapped into defending ourselves. It is still interesting and useful to the extent that one wants to see whether there is a difference between speech and nonspeech. It is one of the most sensitive ways of detecting that. For example, I would just give you simple instance. Consider duplex perception, the classic kind in which you put the base, so called, in one ear and the third formant transition in the other. The third formant transition is the critical element that determines whether the listener hears “da or ga.” Now you know that when you do it this way, the base (all the pattern except the third formant of transition) in one ear and the third formant transition in the other, then typically subjects get a duplex percept. That is, they hear “da or ga**”** depending on what the transition was. They hear that in the ear that has the base and in the other ear they hear the chirp, that is they hear the third formant transition exactly as it sounds in isolation. Phenomenologically, these are clearly different things, but a question arises are they really different, you know or are they simply slightly different forms of the same representation. There are all kinds of questions like this that **Ignatius** and I have gone through *ad nauseam* recently I do not want to take time and now to do it. But one way to test this, one way to put this under pressure, you see is to ask listeners to discriminate and not to tell you whether they hear a chirp or speech, not to classify these things but simply to listen to pairs of these and to discriminate as well as they can on any basis at all. So here you are presenting to these listeners, these dichotically-presented patterns, the base to one ear the formant transition to the other, you vary the formant transition in small steps. Now you ask the subject what do you hear, the subject says well I hear “das” and “gas” in this ear and in this ear I hear chips. You say okay, now I want you to concentrate on what you call the “das” and “gas” and you present to him three of them. You know, two are the same and one is different and his task is to tell you which is the different one, the first or the second. There is no question about what it is, just are they different or are they not different, which one is different. And you have him do this for all these triads which you have arranged with the formant transitions. He does this under one condition where he is paying attention to the “das” and “gas.” He does it again under another condition where he is paying attention to the chirps. In both conditions, he is being fed exactly the same stimuli, in exactly the same context, using exactly the same stimulus variables, using exactly the same psychophysical procedure. Okay. Now when you do this and you now look at the discrimination function you find that the function you get when he was listening, putting his attention on the speech is a sharply peaked function, categorical perception with a peak occurring exactly at the phonetic boundary. The function that you get for exactly the same stimuli presented in exactly the same context using the same psychophysical procedure is a linear function coming right with no hit of the peak. It is a perfectly straightforward straight line, **Weber** function kind of thing. Okay.

Frank: It is an incisive technique. But it is still a technique.

Alvin: Yes a technique but this is as sensitive a way that we have of showing that these listeners are really hearing different things. **Michael Studdert-Kennedy** and **Cathy Best** used it recently in another case where it is the same stimulus being perceived. They created sine wave approximations to a continuum going from lah to rah. Now, they bring one group of subjects in and they tell these subjects that these are just chords and tones and so on, that’s the way the subjects hear them. They do a discrimination experiment. They get one kind of discrimination function with a slight peak as I recall at the flat. The difference between a falling transition and a rising transition, just as you would expect on auditory grounds. They bring another group in and they say, by the way this is speech. You listen carefully, asI said, you can hear lah and rah. Now they do the same procedure and they get a peak in a different place. So, here are two kinds of experiments in which exactly the same acoustic signal, in exactly the same context is being perceived in two different ways and by measuring discrimination one can get a relatively sensitive measure of the difference and show perhaps more convincingly than one could by other means that there really is a difference. Moreover, as I said before, the interesting thing is not just that there is a peak in the phonetic case but that in some cases there is also a peak… (there happens not to be in “dah or gah” but there happens to be in “lah” “rah” – I think there is in the “lah” “rah”) but when there is a peak in the nonspeech case it is always in a different place. So, I say again it is not only the case that the nonspeech has not used a boundary that is there in the auditory domain, but it is ignored whatever boundary there might be. It is totally ignored. Anyway that is the latest on categorical perception.

Frank: I would like to ask at a later point a comparable kind of question, how much did it have to do with the speech, about dichotic perception. It is whole topic we have not touched on so far. But I wonder if it would not be useful for the record to come back to one of the things that seemed to meet to be a happenstance discovery about speech cues that had escaped us for a long time and that was the P, T, K, B, D, G difference, all acoustic manner. That is an interesting story and I think it deserves to be recorded.

Alvin: Well, would you.

Frank: No you go ahead.

Alvin: Well as I remember it went something like this. We had tried, we being primarily **Pierre** in fact, but to a considerable extent me too and I think Frank. We did this transition experiment that I described before in which we tried to distinguish between BDG and PTK by simply straightening the first formant transition in the case of PTK and it was obvious to us that it did not work very well. Certainly did not produce convincing ‘pahs’, ‘tahs’, and ‘kahs’ and we tried for a long time to make, ‘pahs’, ‘tahs’, and ‘kahs’ and we could not. We could make pretty good ‘bahs’, ‘dahs’, and ‘gahs’, but the ‘pahs’, ‘tahs’‘kahs’ were not the right, they were not voiceless. We tried everything strengthening the bursts, putting in noise as best we could for all the transitions doing all kinds of things and nothing worked. My recollection is that the cut back, so called, of the first formant, which really does the trick was discovered quite accidentally by André Malecot . He was working on released stops but “aa..b”, “aa..d”, “aa..g”, which were simply little syllables, I mean second syllables. For some reason that I cannot now understand or remember, he omitted the whole first formant….

Frank: I think in some of them.

Alvin: …I think, in these released things. And some of them sounded quiet voiceless and I remember vaguely his coming in and telling us this and we went in and listened and it did. So, then we started working on it and sure enough it worked. That was when we did this cut back experiment. It worked very well.

Frank: Then rationalized why it should work.

Alvin: That’s right, we did not rationalize until afterwards and I remember **Fant** helped us to rationalize it in fact. It was the right rationalization. It is still the right rationalization. It is still a lovely experiment that nobody ever cites, we had everything right in that experiment. I do not think we had anything wrong. We didn’t say anything wrong except we did not talk about we did not invent the term ‘VOT’ that was **Arthur** and **Leigh** who invented that. We called it cut back, but it worked, we had a beautiful data that showed that it worked and we had an explanation why it worked.

Frank: That was pretty early on, well what, the mid 50s?

Alvin: 1957.

Katherine: Yes, because an early experiment…, We knew the phenomenon and we used it, almost before we fully explored it to do another identification discrimination experiment. Because we were determined to show, again to go back to our own preoccupation with responding to attack, we were determined to show that the BDG phenomenon was a perfectly general speech perception phenomenon and that the same physical experiment could not work for every demonstration. So, we were very interested in getting first a place cue and then to show a manner cue and the second experiment of that sort as I recall is a manner cue experiment.

Alvin: No, I am not sure.

Katherine: That is the second identification discrimination experiment is a cut back experiment.

Alvin: Oh, I am not sure about the sequences here, but.

Katherine: But, it certainly was very close and it was because the cut back was a pleasure to us.

Alvin: Well because of the functions were so clean.

Katherine: The functions were so clean and also.

Alvin: They were really categorical.

Katherine: It was so clean. The results were exactly the same conceptually I mean fitted just as well conceptually and they were obviously entirely….. If you want to conceive the underlying auditory system processing, obviously we are talking about something very very different.

Alvin: I do not think we made a big point of that in the original cut back paper. And, I think that’s where **Arthur** and **Leigh** really sort of made a coup. They did make the right theoretical point, as I think we did not in the original cut back paper. We rationalized it. But we did not go on from there. That is, we did not point out as they did, and I think very effectively, that there were multiple consequences of this articulatory maneuver and the cut back was just one of them. We had in fact…

Frank: Loss of the first formant of transition.

Alvin: Yeah, that’s right the loss of the first formant of transition.

Frank: Yeah! That's right the loss of first… That was really where the visual information in the spectrogram was misleading because you could see that the first formant transition was a little weak was not there as compared with the voice tones, but it wasn't all that obvious.

Alvin: That is very interesting because you know.

Frank: If you are like **Potter, Kopp and Green** in simply describing what you see rather than what you can manipulate. You do not come at these things.

Alvin: It is the fact that not only did we not do this experiment. We did not make this discovery because we figured out what it all to be from an articulatory point of view, it is also true though we had spent a lot of time examining spectrograms that also did not lead us to this conclusion. I mean I might not have the story about André Malecot exactly right, but what is exactly right is that we did not figure it out either from looking at spectrograms or from considering what the articulation was. Now what is even more interesting about that is that as you well know of all the kinds of measurements that can be made in spectrograms that pertain to important phonetics distinctions, this one turns out to be the easiest to make. I mean, you know, if you set about trying to….

Frank: If you know what you are looking for.

Alvin: That's right, but you see, even when you know what you are looking for, if you try to get figures from data from spectrograms on the difference between 'bah' and 'dah' you are not going to do it. I mean you just cannot see well enough where that transition starts. It is all so vague down there. But once you know what you are looking for, it is possible to measure quite accurately the "cutback".

Frank: Though it's easier on the wave form.

Alvin: Well alright, but it can be done and it has been done, you know, with very interesting results. So, here was a case in which what can be seen on the spectrogram, if you know what you are looking for, was not seen by us. Even though we had spent a lot of time looking. We really were frustrated, I remember that vividly, at our inability to make good 'pahs', 'tahs', and 'kahs'. And, in fact, I remember at one point we decided that the problem was simply an inherent defect in the Playback. We really needed a noise source and we did not have a noise source and that was the reason we could not make 'pahs', 'tahs', and 'kahs'.

Frank: The Playback has had, and has, another defect and that it is a one-sided modulator and when you turn it on, it thumps.

Alvin: Thumps, yes. And, we could not make a strong burst therefore because it became too thumpy. So, we were inclined to say well, you know we will need another synthesizer, you can't ever do this with the Playback and then suddenly we discovered we could do it very well with the Playback. We did it very well. Very convincing.

Katherine: I think though that probably once the cutback experiment had been done and I think at about this time the equivalent identification discrimination experimenting had been done, I think that the great contribution in a way of the, (now let me see if I can state this properly)… The VOT experiment is our first important production experiment and also it is the first direct attack on **Chomsky and Halle** and it is the first attempt to show that phonetic categorization is arbitrary or it is the first successful demonstration that phonetic categorization itself is arbitrary, a point of view which follows rather logically from the identification discrimination experiment.

Alvin: I am not sure I understand what you mean.

Frank: I don't either...

Katherine: That is, what they say in that experiment is how you chop up the phonetic universe is arbitrary. You have multiple ways of doing it. That is, we can have a category that is long pre-voicing, that is how many phones you have along given acoustic continuum or given articulatory continuum is to some degree arbitrary, now just as you can….

Frank: Culturally, culturally determined no doubt, but arbitrary all the same….

Katherine: Yes

Frank: …so far as the animal is concerned.

Katherine: Well it may not be completely arbitrary.

Alvin: It is constrained in certain ways.

Katherine: Right, alright, it is constrained in certain ways. At the time I would have thought, though may be not now, that it wasn't constrained at all. That is, that you, perhaps, could not have by some kind of information transmission based thinking. I do not think that I thought you could have infinitely many category boundaries on the same dimension. But, I do believe that I thought, following **Leigh and Arthur's** work that you could put them where you liked.

Alvin: That is not clear. You see.

Katherine: It was not clear, but it was to me at that time, certainly.

Alvin: Well, I think in fact… I feel very frustrated about the whole voicing business because I think that provides us with a wonderful opportunity to answer a question that in fact has really never been properly answered and that is that at the one extreme the situation might be this that if you look at the languages of the world for the voicing distinction for a particular kind of vowel – let's just hold the context constant – what we would find is that there is a limited set of possible settings of the "VOT". In the simplest case three, and it is the case that there is an unmarked form, which is the essentially zero a sort of voiceless unaspirated form that is the French 'pa' that there is the aspirated form 'paa' and there is the pre-voiced form 'ba'. Okay. And that languages now choose to use these two, or these two, or all three. That is one extreme. That could be modified of course to be let's say four categories, or four points instead of three points, but be limited. The alternative at the other extreme is that lots of languages use this dimension of voicing, in fact most languages do, but you can put the boundary almost any where provided you do not get too close. Now I have asked **Arthur** and **Leigh**, I do not know how many times, you know, you have looked at all these languages, which is the answer. And we do not know, we don't know. Are there sort of natural articulatory categories, it would not be auditory? Are there a sort of natural categories and languages pick and chose, which of these they are going to…. Categories is not the right word, natural niches sort of in this articulatory continuum of voice onset time or are there not. Is it just totally a matter of choice? And we do not really know.

Frank: Kathy was mentioning this was one of the early production things and certainly we followed it up, I don't know how soon, with a straight production experiment with translumination, which has been done two or three times in the Laboratories here. Very crudely initially, but even that crudely showed perfectly well that the matter of the timing of the glottal closure was what was critical here. You could see it literally.

Alvin: Now **Kathy** was in connection with the voicing distinction or what was it with that you did this wonderful set of experiments with **Jarvis Bastian** that combined everything and was never published because we couldn't get Jarvis to publish it. Remember what we did? We had the acoustic variation. We did identification, we did discrimination and we did mimicry with the measures of the acoustic results and what the articulators were doing. Was it voicing that we did that on?.

Katherine: Yeah.

Alvin: Beautiful set of experiments and I still weep when I think that it was never published.

Katherine: No, and we did them quite carefully.

Frank: We did them carefully.

Katherine: That I spent a lot of time on it.

Alvin: You can read abstracts of these in the Journal of the Acoustical Society because we did give talks. You gave a paper. Jarvis gave a paper. I think there were three papers altogether and I remember my frustration trying to get **Jarvis** to write this up because he was really the person who should have written it up. We didn't want to take it away from him. And I recalled his coming to visit me when we were at the center in **1964,** presumably to talk about this. In fact, he brought me an introduction, but by that time his interest had shifted completely to something else.

Katherine: Whales, I remember him yammering on about whales.

Frank: Dolphins

Alvin: Dolphins, yes. Actually nothing to do with this. It was just totally irrelevant. I did not understand what was going on. But that was a lovely set of experiments and it is one we really ought to repeat sometime, now that we can probably do even better now. Yes?

Katherine: We could, I am going to look.

Alvin: We can do it much better. You see the point was to do this everything I mean to construct as I said an acoustic continuum, mimicking the "VOT" and get identifications of that and discrimination functions and then to present these in random order and ask people to mimic them as carefully as they could to see then whether the productions are categorical or to what extent they are categorical. And if they are not whether it is just random variation or whether there is some systematic format and to look at their productions the acoustic result and also… We measured the articulatory…

Frank: You measured the articulatory, the EMGs didn't you.

Katherine: We could not get at the EMG at the time. No. We just measured…, We had mimicry, but we just measured it acoustically.

Alvin: Are you sure?

Katherine: Yeah! I am quite sure, because I can remember.

Frank: I guess we did not really have the technology.

Katherine: We did not have the technology, remember we did not have the technology to examine glottal events until quite a while later. We are now talking, I think about the early **60's** and we did not have the technology to measure glottal events, I think even the transillumination experiment until the later **60's**.

Frank: Transillumination would have been too rough on the subject to have tried then.

Katherine: Right, I think.

Frank: Because the initial experiments were done with a solid rod type thing.

Katherine: Yeah!

Frank: Difficult to swallow.

Alvin: You know that **Bruno Repp** has done a similar thing with vowels within the last two years.

Katherine: Yes I do know that.

Frank: Let's take a break. Well, I think we are at end of the reel…

Pat : Yes, we are within about a minute or so.

Alvin: What else is there that we have to...

Frank: Well the whole origins of the production work have not come under discussion and how the thinking shifted on that and how there is still a divergence of interests within Laboratories between the ear and the mouth.

Alvin: Well, but then the question is how far do we want to take that because for this group or certainly for what we are trying to do now is just the beginnings of it because at some point you have to bring other people into…

Frank: Oh! Yeah! Sure, but I thank we can go from **1955-62 or 63** or something like that period.

Katherine: Certainly pre, the great trip to Japan, that is the great break point in that work.

Frank: That's great point in that work.

Alvin: I wonder if I might be excused from that. I don't know how relevant I am because I have a problem, which is that we have this meeting coming up with the **Shaywitzes** which is very important and also I have a luncheon engagement that I better get at within the next 10 or 15 minutes. So, I mean I think this is something that I do not have all that much contribution to make to, do I?

Katherine: Well, I think you might do one more thing before you go away, which is that I would like to know and I do not know at what point in all this you shifted from relational coding as the mode you shifted in your head from relational coding as the basis of speech perception and phonetic perception and experience at determining the boundaries to a truly motor theory.

Alvin: That is difficult to say. First, I think I would not want to characterize…, I certainly agree, and I have said it several times, that there were stages in the development of motor theory. There was the early motor theory and there was the middle motor theory and the most recent motor theory. I do not think that relational is the best way to characterize the one aspect of that. It is true **Kathy** that there were many times when in an attempt to make some of the stuff fit with what other people were saying, several of us in the Lab emphasized the relational character, that is we would say about the **K-**burst. Well, you see, what is invariant about that is the relationship. That is, the best **K-**burst is that burst which occurs just slightly above the second formant of the vowel and if you want to call that an invariant, it is an invariant.

END OF REEL 5 PART 2

Alvin: That in a sense does not say that there is a separate modality for speech. There is not a module for it. The big difference between…, I mean, the kind of motor theory that I now hold says that speech production and speech perception are carried out by a biologically coherent system that is narrowly specialized for just those purposes, for the purposes of producing phonetic structures that is articulating and co-articulating phonetic structures and on the perception side for processing the acoustic signal in such a way as to recover those more or less abstractly defined articulatory events. It deserves to be called a motor theory only because the factors that primarily determine how the system went were factors having to do with the articulatory system. That is, the primary constraints in the evolution of the system were articulatory constraints, not auditory constraints. So, it deserves therefore to be called motor. I think that is the big difference. Now, correlated with that of course is the kind of thing you have been talking about, whether the dynamics are important or not. For the early form of the theory, the one that says well it's a motor theory in the old fashion sense in a **Watsonian** sense then, dynamics smynamics, you don't really care, I mean, it is sort of trivial. In the later form, to me the newer form and the better form, what's happening in articulation is absolutely essential because that is what is being perceived and that is what is being produced and the emphasis I now put on the thing is that on the production side, the system is special in at least two ways. One that the gestures that we use in speaking are for the most part a distinct set. That is, they are different on the whole from the movements that we make when we chew, when we swallow, when we move food around the mouth, and when we worry a blackberry seed with our tongue. There is a rather special set of gestures that are used for producing phonetic structures and for nothing else. And second, their coordination is special, that is, they are overlapped and merged, co-articulated, so as to produce these structures, these phonetic units whatever they are at a very rapid rate, roughly ten consonants or vowels per second on the average, that's a huge rate, and yet to keep them so constrained that the underlying structures still communicate. So, it is a specialization therefore on the production side and it is the same specialization on the perception side. But, now if you look at it this way and if you believe as I do that the constraints are not primarily the auditory constraints. You know that when you make these gestures, all kinds of acoustic consequences follow and some of them are going to be discriminable and distinguishable and perceivable so you don't have to worry very much about the auditory side of things. You have to worry a lot about just what kinds of gestures will lend themselves to this purpose. What kinds of gestures can be co-articulated so that you don't have to spell, you can speak, okay. And therefore to understand the system, you have to understand articulation and you have to do, I think, exactly what we are trying to do in this Laboratory. But, you see, that is merely sort of the more extreme form of the notion that **Frank** started out with many years before when I was still holding to the naïve view (or early view) of the motor theory because **Frank** used to say, I remember vividly all the time that if you want to understand this thing, you have to understand articulation. **Frank** started saying that very early and that was Frank's reason for wanting to start studying articulation. It is not simply a correlate, which it might have been in the early version of the motor theory, but it is the heart of the thing. Well I have now come to that view too.

Frank: I can remember my own conversion and it certainly was total because I can hear myself still saying in the early days that all this business about articulatory phonetics is something I do not really want to know about because once I have recorded somebody's voice and got it so I can look at it and manipulate it, I don't care about the speaker off with his head. Well, that is not so. But, my recollection is that the many conversations you and I had on motor theory at about the mid 50s and the way you wrote it up in the paper, the review paper you did by invitation for the Journal of the Acoustical Society, in which you really spelled things… not only a resume of the work, but spelled out in this way of looking at speech as being something that is being produced rather than something that is being listened to. The main linguistic linkage has to be found at the production site. I took you seriously and I do not think you took yourself seriously.

Alvin: Well, I thought I was taking you seriously because, you know, as I think about it now, I have to say that I do not think I believe that then quite as firmly or in exactly the same way that you did because my version of the motor theory did not really require that.

Frank: Well may be. But anyhow, I took it seriously to the extent of saying, well look if that's the case, then why do not we experiment on the production side and you have always had very good taste in selection of problems and so on, but you were not really interested in following through and go look at the production if you think it is motor. And that puzzled me.

Alvin: While I was interested at looking at it only to see the correlates, that is, I wanted to see, I wanted people to do experiments, which would show that when the perception remained invariant, it was because the articulation remained invariant, that is what interested me and remember we sort of tried to argue that in the early days and rather naively as it turned out because we were talking about what happens at the periphery and of course there's not invariance there. And, that was one of the early discoveries in the articulation work…

Frank : I don't remember quite that way, but....

Katherine : I think we got zapped unreasonably. I will go on on that point sometime.

Alvin : But in any case, I would just say again that the early version of the motor theory as I understood it led me to be interested in articulation, but only up to a point, exactly as you said. Not in the way I am interested articulation now, you know, and the way the Lab is moving in articulation. But, just as another measure that had to be taken so that we would have data that could be correlated with these other data. That was all.

Frank : That is the difference….

Alvin : And that is all I needed…

Frank : …between a psychologist and an engineer. The engineer wants a model that will work and produce the result. If he thinks the model is a speaking one he wants to go get a speaker.

Alvin : No, but you see, the only model I needed for the old motor theory, I did not… even if I wanted a model of it, even an informal model, that was all I needed. But, for the new motor theory, no. For the new motor theory it is exactly what you say. Well, that's the big difference in my own thinking about this.

Frank : I would raise a slight demure about your present thinking if I may. It seems to me you are a little bit offside in putting major attention on things like modularity and nativeness of the machinery and so on, not that they may not be true, I suspect they are, but that they are not central. The central thing as I still see it, I'm afraid, is how do we account for the phenomena of language being organized in a phonemic mode using models of the speaking machinery. What's the relationship? Now that is not quite Kathy's point of view either, I realize, but let me say that the central problem as I still see it lies in finding the kind of model or machinery that will behave in a linguistic manner and of tracing through the connections between whatever linguistic units there are or may be characterized through the production reception cycle.

Alvin : Yeah well…

Frank : It's a signal… it's a signal analysis point of view, I grant you…

Alvin : I just want to say that my views about innateness are rather special views about innateness, I do not think the speech is innate in the way that term is ordinarily used, I think that what is innate is the module (which is as innate as it can be) and the difference between my view and the more common view is that the notion that Ignatius and I have developed recently is that when you have a module, it is the module that changes. The example I use is sound localization. It is obvious that as the head gets bigger, something has to change in sound localization because the head gets bigger, the interaural temporal disparity changes for the same sound in the same place. The question is how does this happen, it does happen and there are two possibilities, one is that the subject continues to hear the same thing, but just learns by association to make different judgments and clearly that can't be right. The alternative is that there is a highly specialized system that adjusts itself that has this property of plasticity. It is a module itself that adjusts and you can show this in the case of the Barn Owl for example if you plug one ear, that the map changes and the Barn Owl does not have to do any cognitive work in other words. And so I think if I have had any kind of insight about this whole innateness question, the affects of experience, it is that the experience affects modules and non-modular cognitive processes as well, but it affects them in different ways —radically different ways. And this is why you do not have to teach your child to speak and this is why you do not have to teach your child to listen and this is in fact in a paper that I have just written, and I think I haven't given you a copy. There is a big difference between reading, learning to read and learning to speak that this kind of thing has to be imposed cognitively in learning to read where as learning to speak, you got a thing in there that certainly is affected by experience, but it is affected by experience in a radically different way.

Frank: Lets go back and pick up some of the history, if we can persevere just a little bit longer, then I expect that we shall all have to break up. I came across the other night a letter from **Lovick Norso**. Do you remember him?

Katherine: Yes, I do.

Frank: He is a delightful solo investigator working in Vermont someplace. He came down to visit us and he built for Kathy and me our first electromyographic EMG amplifier. It's was a single channel device, which fed its output to an oscilloscope and it had its difficulties, but it worked and got us started and my recollection… this letter was dated in **February of 1955** so that puts an interesting time and date on this. My recollection is that you and I, **Kathy** had talked about this kind of thing to some extent in the context of the motor theory as then presented and decided that if you want to get at what the articulators are doing perhaps finding out what the muscles are doing is the most direct thing you can hope to do. You cannot get back to the neural signals by any technique that we could imagine at the time. Let us push the system as far back as we can and use electromyography, which, at that point if I recall, was a medical technique that was being developed. It was used primarily for discovering muscular or neuromuscular diseases and it had been used very little if at all for studying how the motor events operate. There was one Danish doctor who had done some direct needle insertions into the larynx, I've forgotten his name now. He simply reached down and stuck in a needle. Buchtal was it?

Katherine: Yes, I came across those papers. No, **Buchtal** was the great electromyographer. But, the man who did this kind of work in the larynx was a known laryngologist and I have now forgotten his name, I will think of it in a minute.

Frank: Well, anyhow, he had done this. But, that is about all that had been done to my knowledge. And I thought up the suction electrodes that we used for a while as a way of getting on to the tongue, which seemed like an essential piece of machinery to investigate by definition difficult to attach to. And remembering how I used to suck on small rifle shells and attach them to my tongue and tap them on my teeth. So, may be a suction electrode would do the job. It was about that time that **Patty Grub** came to work with us here and came as I remembered specifically to work in this area. Now, do you remember when she came or what the context was?

Katherine: I do not remember very much about **Patty Grub**. I do have somewhat…, I mean, I remember **Patty's** being there and a predecessor of hers whose name escapes me entirely. But, I do remember some things about thinking about this problem. We said a while back that we were worried about the funding of the Lab. And we knew that we were being supported for reasons having to do with coding secrecy. At this time, in the middle **1950's**, I had by this time had **Maud** and my mother-in-law was so overwhelmed with the fact that I had had a baby. She had long longed for a grandchild that she wished to do something that she thought would please me and she also wanted to do something that she felt was good for the child. So, she bought her first television set and one of the great attractions for small children at that time was someone called **Sherry Louis** and **Sherry Louis** used a sock puppet. She had a sock over her hand and she would move, talk and so forth and this entertained my daughter very much. But, it occurred to me to think supposing you wanted to figure out how many fingers she has. While it looks like she can do everything with this sock in fact what she has got inside is a bunch of forces that are attached to levers and if you observe the motion of this thing carefully, you could perhaps infer the structure of her hand. But, it would be far easier if you looked at the muscles and therefore why don't we look at the muscles. The structure of the muscles must be simpler than the structure of the motions of the sock puppet and what people are doing in Sidney is looking at the motions of the damn sock puppet, they should not be doing that.

They should be looking at the muscles and so I thought that would be a good idea and would tell us something about how speech was organized. We did talk about organization even then, secondly…

Alvin: Excuse me, I have got to run because of this.

Katherine: Tell me what happens.

Frank: Don't worry I will let you know. Excuse me.

Katherine: Secondly, I believed very strongly. I also had this vision of the muscle movements as a clever coding device that is in fact you could transmit muscle signals in **Louis'** speech and you would have something that was really unbreakable. And, so we talked about this.

Frank: You didn't tell NSA about that one.

Katherine: No, I should have. It never crossed… I thought about it a lot. At this point, I wasn't in the Lab as much.

Frank: That's right.

Katherine: I was home with **Sherry Louis,** but I can remember the arrival of the EMG machine. I can remember trying to get the surface resistance with skin on the arms down and us both taking emery paper, which we got from **Bill Winter** and scraping away at our arms and raising these huge bloody welts. And, I distinctly remember thinking that the origins of this notion that the muscle movements were simpler. It was not that I did not recognize that they were forces rather than positions, which was essentially what the McNeilage argument was – that we did not realize that we were looking at events that were… – It was that I felt that the structure of the free variables or at least it seemed to me that we ought to be able to tell something about what the dimensioning of the free events had to be.

Frank: Right. My thinking ran parallel with this.

Katherine: Well, we talked about it at lot for one thing. The other thing was that I believe that we could tell a lot about the relative timing of events, which following Leigh and Arthur's work on VOT seemed to me to be a central matter. I think given that we knew that, in static terms, the Pattern Playback was not a very good device, but by that time we had come to recognize it as transmitting relative timing information extremely well. So, I thought we wanted a technique.

Frank: I do not remember that my emphasis was quite as much on timing as you are putting it, though later on it certainly did. Let's see, as I remember it, **Patty** was here for a while…only a matter of months I think really and her husband whose name I don't know….

Katherine: Keith Hayes

Frank: Keith Hayes, right.

Katherine: He and **Patty,** yes, they had taught the chimp to talk or rather had been unable to teach the chimp to talk…

Frank: They were unable to teach the chimp to talk. Anyhow, he was more insightful than I had been about suction electrodes and came up with the silver beads sliced in half as substitutes for the plastic ones I had been trying to develop. They worked really quite well and you used them for quite a time. We built a screen wire cage in the corner of the upstairs room on the 5th floor to avoid electrical effects, which later turned out not to be important if you got the electronics right and got an old multichannel penwriter, which is used for, I guess cardiographic work primarily. He set it up to do some experiments. It was about that time that **Gloria Lysaught** was with us as a research assistant, rather more than an assistant, she was trained as a nurse.

Katherine: Right, but as you may recall, she, **Arthur**, and **Tom Rootes** were the result of your association with Columbia.

Frank: Yes, I guess that's right.

Katherine: She was getting a Master's…..

Frank: She was getting a Master's there. She, and you, and I were members of the team and the first experiment, which I still think was a good one, was the PBM experiment in which one puts electrodes on the upper and lower lips at the margins and measure the relative timing of events, and so on and found that essentially equivalent signals corresponded to P or B or M. That is, the lip signals were not identical, but almost indistinguishable. By that time, the cut back work was in hand and the transillumination ideas if not the actual experiments have been done, so that we knew that it was relative. We interpreted this thing in terms of relative timing of lip, glottal and velar gestures. And, came off with a set of two or three papers, of oral papers, at the Acoustical Society and I cannot quite place the dates on those. Whether they were... They were not earlier than **1958** and not later than **1960**. But, I do not know just when.

Katherine: There is a written paper also.

Frank: Well, I remember the oral papers in particular because we got all kinds of flak in the corridors, what about you people up to now. You have been working on acoustic cues and now you are talking about electromyography, what goes on here? There were three or four papers in a string.

Katherine: Yes, there was a published paper, which is **Harris -Lysaught-Spay**

Frank: Yes, that's right.

Katherine: I have forgotten, I think you published, you co-published with Leigh and Arthur on the transillumination work just about the same time.

Frank: There was a paper that I gave at the International Congress on Cybernetics in Namur which spelled out the rationale for this and talked about the suction electrode use, the methodology and so on. But was quite explicit about the relationship between a motor theory and the idea of associating the phonemic composition of the message with, in the simplest case, a combination of muscular activity that corresponded to it. Essentially, what was the term used there, I don't remember, but anyhow the idea of being that things might not be this simple, but the simplest assumption you could make was that a particular muscle or muscle set was involved in a behavior that had had a particular phonemic name. That there was a kind of correlation that we saw quite clearly in the PBM case, that is, there was a lip gesture, that was typical of the place gesture and there was information about manner that we had from other sources which, in combination, told us that this phoneme is P or B or M. We lost points unfairly on that from **Vicki Fromkin** on the basis of a much less adequate experiment that she ran and came off with a conclusion that there are some differences between the lip gestures in P, B and M and this must be what is important. Although her data overlapped so much that nobody could possibly have hoped to tell which was which from the data. Now, nature was not that simple when we got further into it. We had stumbled on to probably the only case I can think of in which the three major actuators are so far apart that their actions really are independent so that I at least was led to think in simplistic terms longer than I should have about the relationship between linguistic unit and terminal motor event. But the basic idea that you ought to look for the kind of linguistic invariance that is certainly somewhere because that is how the system is put together to manipulate units and to concatenate them into new strings and then get them expressed that some place the relationship between those units and the outside world goes down the motor path and it could be a comparatively simple coding thing. That was what we were after as I recall it.

Katherine: Yes.

Frank: That's what is explicit in that **Namur** paper, which I deliberately tucked away in an odd point, so it would not become too common too soon and it got hidden forever.

Katherine: There is an another thing that is interesting that there was another argument that we made that it is the only time we ever paid any great attention to the constancy transform. Another point we made at that time, which is pretty obvious, is that you have to have some form of motor theory, though not necessarily any of the ones that **Alvin** now can distinguish, because in fact perception is… the size transform is so easy. That is, it is so easy that phoneticians don't notice its there. That is, it has always been the case that you had to have some kind of an internalized perceptual transform mechanism simply because the transcription is indifferent to the size and shape of the speaker and it seemed to us that it was pretty obvious that the muscular organization is similarly indifferent to the size and shape of the speaker. Now, at that point **Fant** had… (by this time, of course, things were quite far advanced in acoustic theory over the days when we started, I mean, a number of things that we said in those times would not if we had been as sophisticated as the field was ten years later, one would never have said). But, nonetheless, by that time, **Fant** had begun to note that simple forms of the transform algorithms like scaling were not correct, that is, that you had to know something about how heads grew, for example the pharynx-mouth ratio was not constant for men, women, and children and so forth.

Frank: It's not constant after eight either.

Katherine: So, for all these reasons, it seemed likely that the forcing functions somehow were may be a better description of the way speech is organized.

Frank: Though **Fant** has never, to my knowledge, agreed that it is not an acoustic phenomenon primarily.

Katherine: He is a very strange man in that sense.

Frank: Yes, he is.

Katherine: He has produced in my view, and in **Michael's** too I think, one of the most convincing papers ever written on the failure of phonetic segmentation, the Santa Claus paper. And yet his view has continued to be a feature-oriented view of the phonetic units. The pattern of his thinking has never been clear to me, although I would certainly agree as to its quality.

Frank: I have one small recollection from Fant's big conference, which was in early **1960's**, I do not remember exactly.

Katherine: I was not there.

Frank: I think you weren't. It was in **Stockholm?**

Katherine: Yes.

Frank: It was a major event and the Proceedings, I would rather think, are still are in the library and occasionally referred to. The Laboratories had two or three papers there I think (**Al** was responsible for one and I was for another and I don't remember who else) on the motor theory, explicitly on motor theory and on mechanism and the direct investigation of production. **Walter Lawrence** ,a fine old gentleman he was, but a little bit slow in his thinking and yet straightforward in his expression, got up and said, "well you know, one of the things that has struck me at this meeting is this new work of the Haskins Laboratories. I thought they had done some beautiful work early on with their synthetic speech and that was probably the end of it. Now, here they are opening up a whole new field and I think that is something to be commented upon." And, from Walter I took that as high praise and perceptiveness. You took over this whole area of work as your own domain and I think should sit and talk with some of the people about it. I sort of dropped out of it because I got involved in a lot of other things aside from helping with the instrumentation on it. About the time that the PBM paper had been finished and I guess there are one or two more, I remember the slit-split one, which also involved electromyography and from thereon, it was pretty much your own show and I do not remember too much about the details of it. That would take us up to what? **62 or 63**, something of that sort I think.

Katherine: Fauberg Anderson is the name of the first man to put electrodes into the larynx.

Frank: He and his subjects were both heroic.

Katherine: It is not so bad. The Japanese do it routinely, you know.

Frank: Well, I do not think they take the long regular needles and plunk them down. They do it more subtly. And your account of that Tokyo conference and the consequences that fell from it is a major part of this whole EMG story, but that's your story primarily because you worked out the relationships for bringing the…. We had a terrible time here getting medical assistance and supervision of our EMG work and it wasn't until you had made the Japanese connection that things really took off and soon thereafter the needle electrodes replaced suction type surface electrodes, though I think we actually brought surface suction electrodes equipment up to New Haven when we came in 1970 although I don't think it was ever used.

Katherine: I should point out that some people were terribly impressed with a likelihood of our being right and **Claud Chevry Milair** (who was meanwhile a young doctor assiduously reading journals on speech production – she was interested in this Pediatric Neurology attached to speech) carefully built, an exact duplicate of the suction electrode system at **Salpetriere Hospital**.

Frank: Well good for her. I do not know, have we…. I would be interested in your observations about the nature of the division down through the years within the Laboratories between the perception work which said that there is a motor theory and the production work, which says there is perception but the two lines of endeavor have been more distinct than interactive it has seemed to me, to my sorrow, but nevertheless...

Katherine: It's may be the final thing to say on this tape. I do believe there is a simple…. I think there is simple explanation, which was very intelligently put by **Ruth Day** of all people. She said when we were having conferences up here after we had moved, by this time it has gotten to be **1970**. She said, "Oh! You people are just where the perception group was in the early **1950s**." It is very easy for us now if you want to get the answer to a production question. It is now very easy for us to think how to do it. We know a little bit about the biomechanics of the system and certainly a lot about the muscles of the system. We have not been alone in making these investigations particularly in the area of laryngeal research. We have done a lot of the now classic papers in various of these areas but really at the time that we began to do this work, the tongue was a blooming buzzing confusion, still is to some degree. There was substantial controversy about what are, in some sense, phonetic facts. I mean we knew by the end of the BDG experiment, not only the theoretical points that we have been emphasizing this morning but how to make one and essentially we then had a long swing through the job of learning how to make one. Also while in the perception research, we early on developed a very successful all-purpose tool that is not true for the production research. It is still true that we continue to try to lay hands on new tools. We did not get into a technically superior position instantly with the production research and indeed are still not.

Frank: Yes, I think that's an interesting point and let me elaborate a little bit as I see it at the moment. The old Pattern Playback was a lucky strike, in many ways. It was simple, easy to use and tapped into an enormously rich field. An articulatory synthesizer could in principal tap the same kind of field in articulatory research. We have not done it. Nobody else has done it. It is certainly a more difficult thing to do. I do not know whether it is more difficult in a hidden sense or not. That is, if you have the machinery that you could manipulate fairly easily to model the vocal tract, will perception be an equally good guide on how the articulation goes as it was on how the acoustics went. That is to say, with the old Pattern Playback, if you got the sound right, the pattern right, you heard it correctly. The relationship was nice and easy. There may be a many to one relationship between the way the articulatory synthesizer will behave and the percepts, so it may (much as I would like to see an articulatory synthesizer fill the same role for us as the old Pattern Playback filled in linking percept to production) there is always the possibility that it is not a doable job, I do not know.

Katherine: It is not doable. I think the many to one problem is not the kind of thing that people like **Atal** talk about. You squeeze one end and you squeeze the other end and they are equivalent. It is not that problem I think. I think it is because our first instinct was right that is, with the present articulatory synthesizer, we are not manipulating the variables that are meaningful. We really have begun to talk about the problems. Periodically we have a meeting on improving the articulatory synthesizer. Right? And we talk about head size. We talk about the fact that we do not have enough information (which **Pat** is now busy getting) on the shape of the pharyngeal cavity and we talked about a whole bunch of things like that but we really do not talk about the free variables of tongue shape. We really ought to be talking in terms of what the tongue can do and we do not even now (with good EMG as good as anybody has ever gotten), there still are some problems in just getting the information we need to get ourselves information which is phrased in terms of the vectors that move the tongue around. A model isn't good enough and so you now with another ten years may be it will be but you know as far as we are concerned, the tongue in the articulatory synthesizer is an arbitrary shape and there is nothing you can do to take the EMG data and say okay this is the dimension that increases as you move from this vowel to this vowel. Now let us put those forcing functions into the articulatory synthesizer.

Frank: It does not have muscles in it.

Katherine: It does not have any muscles.

Frank: That's probably a fatal defect.

Katherine: And indeed while we can still read dead anatomy on what the muscles are. We still after all this time, and as I say I think we are not deficient in good technique. We still do not get absolutely predictable data from plunging electrodes into the tongue. Every time, we do it even though it is **Larry**'s tongue always. We do not entirely, we ought to be doing more of it but it is terribly hard work and you do not get many brownie points for it. Our life is full of surprises in this respect, so that we still do not have the dimensions of the shape picture right yet. As I say, maybe we are getting there.

Frank: But I think we are getting some experience on which to set compass bearings.

Katherine: Oh! We're are getting answers to questions. They are not answering at the global level that you wanted them at in Namur.

Frank: I wanted them **20** years ago **30** years ago.

Katherine: But you wanted them on a very global basis. You were not too terribly interested.

Frank: Well, I am engineer at heart.

Katherine: Yeah, well. All right.

Frank: I want gears and I want products.

Katherine: Okay but, I think we are probably getting the answers for a good engineer of a certain kind but you were tremendously influenced by **Al** in that you wanted cognitive answers. Machinery answers, I think we're getting nearer but we still do not have cognitive answers.

Frank: Now the big problem is still ahead of us very much so, conceptually as always.

Katherine: So as the sinking sun sinks slowly in the west.

Pat: Well.

Frank: We'll gallop off to various other actives.

Pat: Thank you **Frank**. Thank you **Kathy**.

Pat: I think I should begin by saying that this is **09/22/88** and we have three people here and we have **Arthur Abramson, Leigh Lisker, and Katherine Harris**.

Katherine: I will now begin by saying a few words that make the overlap with what I was saying before. **Frank** and I and **Al** as well of course, had discussed the beginnings of motor theory, the way that led us into believing that it would be a grand idea to do research on speech production and I think I had said that if we had been going at this thing the way any natural articulatory phonetician would have done, we would have not thought of things in that order. We would have begun that way, but we were, I think I was at least – probably **Frank** as well – quite innocent of the work of people like **Sweet (Henry Sweet), Durand (Marguerite Durand) or Saussure (Ferdinand Saussure)** or any number of …the **Abbé Rousselot.** We were quite naïve about a great deal of that work. Consequently, our approach was essentially perception based and we attempted to infer production from it somewhat naively I think looking back at it. However in fact, I think we had a kind of accident in that while we were attempting to provide a motor component to the motor theory. In fact, the single piece of research that has had more influence I think on the field at large on essentially our point of view on the importance of the dynamics of production in establishing this framework for phonetics…. The piece of work that has had greatest influence is unquestionably the cross language study and I simply don't know what your motives were in doing that work – what its origins were for you intellectually – and I think it would be interesting and desirable to say something about your backgrounds and indicate how you came to this particular piece of work because it has no necessary origins in the previous work of the Laboratories. It does not represent an idea that comes from somebody else, it is sort of *de novo*.

Leigh: By the cross language work, you mean the…

Arthur: She means the voice onset time, the **1964** paper

Katherine: The '64 paper. Where does it come from?

Arthur: Excuse me, does Pat want this biographical material first, the reasons for coming in, and then tackling his question that does not matter.

Pat: If you like.

Leigh: Well I can say something about that. But shall I go all the way back? I was a graduate student in linguistics.

Pat: We should identify ourselves on the tape.

Leigh: You know who it is? **Leigh Lisker.** I came back from military service and went back to school in **1945** and I became rather dissatisfied with straight linguistics I must say because I don't know, I felt that the chances of really learning something as against engaging in polemical discourse about the nature of language which I really find ultimately not very informative.
I decided I wanted to go into physics and I almost made it but the Veterans Administration that was paying my graduate tuition suddenly slapped down on people changing course. They said as far as we are concerned, you are doing perfectly well as a graduate student in linguistics and we won't let you change to physics. So, I decided that well I will do the next best thing. I will go into the acoustics, in particular, of speech. I had started to look into the possibilities, this is before the sound spectrograph that we know became available and I was made aware of the existence of a machine that somebody in electrical engineering at Penn had developed called a sound prism, he'd developed it as a MA dissertation. And I started to fiddle around with that and there were lots of technical problems, ringing and all sorts of problems with the circuitry that made it rather unreliable as a device for measuring moving spectra. Well I was unhappy about this. I might say by the way that Zellig Harris, who was my professor and the sole person in linguistics at Penn, encouraged me to go into phonetics. He thought then that it was an up and coming field – this by the way at a time when linguists were utterly uninterested in phonetic research and people in the States who had been working in both the acoustics and the physiology of speech had to go to Europe to get their stuff published. You know, **Ashiev,** Neal and **Dales** etc. In any case, I selected as a topic for a dissertation what I thought of as may be an interesting problem namely to discover how much work F1 and F2 measurements do in predicting the phonetic phonological classification of two vowels that in American English at least are closely related that lie close together on the F1 and F2 plane. And, in order to get data on this, I selected these two vowels, used a single speaker, and chose a context that was fairly close. There were two English sentences that I used and proceeded to grind out spectrograms. At that time, there was only spectrograph available in the country and that was at Bell Labs and Gene Peterson (Eugene Peterson) was there and he was my host and I would go up once a week, ring the doorbell at Bell Labs, and wait for a while. They were under military security, this was back in **1947-1948** may be. And he would come down and usher me up to the room where the spectrograph was and I would go in and spend eight hours breathing these fumes and getting a sick headache, grinding out spectrograms. The setup was, needless to say, not very good by present standards. I would record a sentence, listen to it, and make sure that in my estimation it would pass muster from the auditory phonological standpoint and then proceed to grind out the spectrogram. I wound up with a pile of spectrograms over the better part of a year with hundreds of them and did my measuring by hand and so forth and I wrote up a paper. During this time, I got a traveling grant from ACLS to go visit the then known speech scientists operating in the States and they were Martin Joos (in Madison then) with whom I spent three days, with Don Lewis in Iowa City and Stetson who was then on his death bed in Oberlin and each one of them was very encouraging. I think I learned a lot from them although with Stetson, I must say I spent most of the time listening to him carry on against the modern structuralist phonologists, especially my mentor and teacher, Zellig Harris and, you know, it was a matter of being polite and so forth. In any case, when I wrote the dissertation it was accepted

and actually a small part of it was published as an article (that has been by my guess pretty much neglected or forgotten) in **Language 1948.** And then, since there was really nothing that I was aware of that was going on in speech research and I had to get a job I got my degree in early 1949 I think it was, I took an appointment at Penn in Indian Linguistics, I think, it was or Dravidian linguistics and went off to India figuring that there was really no support for phonetic research as far as I knew, there was no great interest in it and so I went off to India to learn something about Indian languages which I was going to then teach. When I came back, I got to know Pierre Delatre who was in the French department at that time down the hall and I had meanwhile gotten a sound spectrograph and he did not have one. What he had I remember was an old kymograph and I remember spending one day with him helping him smoke this long screen, this long loop of paper you know and I think, then I went away again to India and I had this spectrograph and he asked if he could use it while I was away and I said sure and so we wheeled the spectrograph into his bailiwick and then when I came back from India, we saw each other a good deal at Penn. One day he came to me and told me about the existence of **Haskins** and asked if I would be interested in coming up to pay a visit. So, I said sure and I went up, whenever it was, sometime in early **1953** I think and spent a day with him watching him paint and painting on the Pattern Playback and about 4 o'clock as I recall on that day, Frank came and asked me into the office. I learned of course and I guess I knew already that Pierre was planning to leave Penn to go out to Colorado because he wanted to play tennis through the year and so forth and so Frank asked me if I'd be interested in coming to the Lab. I said yes very much, but at that particular point, I was not only teaching full-time at Penn, in fact more than full-time because I was teaching some of the Indian languages and also teaching linguistics and a phonetics course, but I was also going down one day a week to Washington to handle a course at Georgetown and doing some other things, I don't know. So, I said I could only come in for one day and I think it was Tuesday then and Frank said well it would be better if you could come in more, may be after a while you can, which is what happened. So, I came in for a while just the one day. Since the Lab was in New York, this was no problem. We could get up in an hour-and-half. So I started to work at the Lab. Pierre was my mentor I would say. He told me of course that everybody coming into the Lab had to put in (I do not know how long) a year anyway, standing at this machine painting. There was no other way and I remember before the year was out I had to go to the footman and get myself fitted for artificial arches because my feet had really gone bad there. No way. It was no joke. You could not sit there very comfortably and paint. You had to stand there and paint. I remember one of the first questions they asked me to look into was this matter of the contribution made by F3 transitions to stop place and I remember because there had been all this work already published on F2 transition in particular and there was some question about F3 what it would do and I started to work beginning with patterns that were identified… two formant patterns that were pretty well identified as to place of the initial stop and I found that the effect of adding a third format was not very great and I was a little bit discouraged by this. I think the job was then, (I don't know, whether you knew) was handed over to you. In any case, Kathy, I think you got involved in that question and I learnt something from this because what Kathy's technique was, was to minimize the F2 contribution to place and under that condition of course F3 emerged as a fairly important cue you see. Now I did not pretend to have the experimental experience of you know somebody in psychology, linguists were not and have never been experimentally minded except for this rather simple pairs test that they never carryout and what the psychologists I am sure would call inadequate, you know, conditions or controls. I also learnt from **Kathy** and of course from **Al,** both of them practicing psychologists, that a view that I inherited from my linguistic colleagues namely that if a feature does not throw categorical judgments entirely from 100% to 0% or vice versa, they are to be ignored as non-distinctive you see – that life, in the linguistic sense, is a matter of really strict binary categories you know with no ambiguities allowed. You know a word like pin is either one that begins with a p or b and there is nothing in between and if you state a generalization and it does not hold absolutely for 100% of the cases, then it is no good, you throw it out, never mind the possibility that a rule might work 87.5%. That is not good enough. It's either 0 or 100 and so I think I learnt something from contact with psychologists that I would not have learnt from my fellow linguists those days who were non-experimental.

END OF REEL 6 PART 1

Leigh: Now, if you like, the question that occurred to me, maybe already in **1953** or **1954** certainly by **1955,** followed from my dissertation which was concerned with the question of how much use is the determination of just F1 and F2 so far as accounting for our vowel categorizing behavior. I raised the same question with respect to the feature of voice onset timing. I knew of course that as between ba and pa, you could state the difference in terms of a difference in the onset of voicing relative to some mark and I thought of burst onset as a fairly reliable acoustic marker of the onset of a gesture, not the onset of the gesture, but some mark that a gesture had taken place. On the basis of earlier experience with a variety of languages, I knew that what we call P or B is not the same thing as what a Spaniard calls B or P. These terms have no fixed meaning except with respect to some particular language. And the question I raised was similar to the one I raised with respect to F1 and F2 for the vowels. Except for the vowels, I didn't look across languages. This was how much work will this particular measure do so far as accounting for a category difference across languages. Would a significant number of languages be found for which this measure was inadequate. So, I think I remember that's one occasion when **Pierre** made me join the MLA, so I could give a paper on these preliminary findings, which I did. I guess I forget how long that was before **Arthur**, I don't know what his, I think you were here….

Arthur: I was on the scene when you gave the paper.

Leigh: You were on the scene certainly.

Arthur: I was probably present

Leigh: Yeah, **Arthur** suggested that he'd be interested in getting together with me on this problem. And I must say that what I had thought of is well, (I don't know how many languages I reported on, a number of them) that I would probably go on to something else. But when the two of us got together, we proceeded to collect a very much larger corpus or set of measurements for an increased number of languages and then go on to experiments and synthesis with the idea of using a master set of synthesized stimuli in order to get perceptual data to see how much agreement there would be between measurements of natural speech samples as against categorical judgments and also discrimination data. The whole enterprise seemed to, you know, mushroom beyond all rationale balance may be. Of course what we found was that, as you might expect, the particular measure was very useful, but not entirely useful, not all languages, Korean was a good negative case where just that measure would suffice to unambiguously separate categories. Though even there, there was not then and was not thereafter any good evidence to suggest that any other single measure was superior so far as separating categories. I don't know what else to say now at least upto this point.

Pat: You seem to suggest that you walked in, I know, on the very first day, **Frank** invited you to become part of the staff.

Leigh: That is true.

Pat: Was it the way it worked.

Leigh: Yeah. That's the way it worked.

Arthur: It was known then that **Pierre** was soon to leave.

Leigh: Just that first day there was never any…

Katherine: Frank would usually, I think, it was in my case as well. You would find out afterwards that when you came in your view fairly fortuitously, he would in fact know you were coming and would have done some spade work on your background. I didn't find out till years later for example that he had, a full year I think before I came to the Lab, called my department which was an obvious source of somebody for this particular kind of project and had heard that there existed some possible people and I was one and was likely to be in New York, otherwise unemployed and indeed in some ways unemployable and that therefore, I was a good bet. I mean, I thought I was paying a casual visit, but in fact I wasn't and I suspect precisely the same thing happened in your case. As you thought you were paying a causal visit, but in fact **Frank** had already gotten **Pierre** to make inquires around Penn and so forth, is my guess. So he had already made up his mind I think.

Leigh: Yeah. But there was no hint of that.

Katherine: No.

Leigh: Certainly, **Pierre** didn't spill any beans.

Arthur: He probably wouldn't have taken it upon himself.

Leigh: Yeah. Yeah. Sure.

Leigh: But, I mean, as years went on, in fact I was spending more and more time at **Haskins**. Indeed I think by **1959** just before when I again went for a year, almost year-and-a-half to India, I was coming in six days a week and at the same time teaching a full load, which meant I had scheduled my classes for early morning or late afternoon or evening and was shuttling back and forth…

Pat: That was possible on the train at that time.

Leigh: That was possible on the train and of course, I don't think my family has ever forgiven me for this. Starting thereafter every time I went to India before we came back to this country, my wife would say, no you got to spend more time at home. So over the years, there was a decline in the number of days per week until finally it was just…. Well of course other things happened. At some point, I had to agree to be Chairman of the department, which cut into things you know. So my commitment went down in terms of time although virtually… Well not all my research, I had to do things and **Indic** languages too.

Pat: What made you to take up linguistics then when you left the service? You were in the intelligence course at that time.

Leigh: I was a major in German and as an undergraduate and when I first went to start at graduate school, I was in the German department, but I happened to have taken, still as an undergraduate, first one course, then another course with **Harris**. And those were eye openers to me because every course I had had before was an old fashioned language course that is a linguistics course, old high German, or old Icelandic or something where there would be, of course, some discussion of cross language comparisons of the usual Indo-European comparative grammar sort, mostly phonological say, but never any really explicit discussion of method. I happened, many years later, in fact just very recently, to be talking with a colleague of mine that comparative linguists, then called philologists, were in principal opposed to discussions of method. Method you acquired indirectly you see by working on problems and somehow you intuited a correct etymology as against a poor one. **Harris** was of the new school of American linguists, structuralists, now they are called. Then, we just called them linguists with the penchant for making things explicit. So, I then went into military service and in the army, of course I got involved in interrogation. So, I used my German and I had some opportunity to talk to German prisoners among other things linguistic matters. When I came back instead of returning to German, I joined as a student of the newly formed indeed the first department of linguistics in the country then called the department of linguistic analysis and I was looking around thinking what might I do. **Harris** himself was not particularly interested in phonetics as I say, but he suggested, particularly after he knew that I was trying to get out of linguistics into physics, that I might look into speech acoustics. At this point, **Martin Joos's** acoustic phonetics manuscript was circulating and I had a copy and I found it very interesting. I don't know whether you read it, but it is quite an interesting piece of work, so I did that. I decided to take his advice, but once I did the dissertation work, I found absolutely no position available for somebody in phonetics in an American linguistics department of which they were then very few. Indeed the only solid job offer I had was from the University of Alabama to go down and teach Russian and German and I really didn't have the stomach to go the South. I had been in the south during the war and I really wanted no part of the South for social reasons. It was quite a revolting place to be, you know. Not that it was much better in the North, but at least people are civil, certainly at the academic level. I didn't know what to do and along came this opportunity. A colleague of mine who is a Sanskritist, said, when a new program in South Asian regional studies opened up and he had the job of starting language courses, for all the languages of South Asia or the important ones. He said, look I need somebody to help me with the teaching of Hindi and I said to him, I don't know any Hindi and he said that doesn't matter. I am going to teach a course this summer for six or eight weeks, come sit in, and in the fall, you will help me around the classes. The head of the department who is a Sanskritist was a great friend of **Harris'** and had an almost unbounded faith in the ability of linguists to handle any language. This by the way dates from the war experience, you know, that is where one of the main motivations for the development of linguistic studies in this country came from. The abysmal performance by American High School and College graduates who had 'n' years of French and who simply couldn't be used by the military for any serious purpose. I observed this myself when I was in the military intelligence service because most of the people who came to this camp in Maryland, I think it is now Camp David, were failed upon alighting from the train because they couldn't read a newspaper in the language and discuss it in the language and talk with people. They got As may be in Spanish, but they were simply no dam good and they didn't care what avenue by which you learned whatever you learn, they wanted to know could you manage in the way in which a native or nearly the way a native operated. And fortunately, I was able to do this in German mainly because when I was a high school kid, I would go to German language church to find out that sort of thing. In any case, I had no other choice and I thought well, I did the dissertation, but that is the end of any serious research except for what I can do dabbling with the sound spectrograph that I had then. So, I decided well, I'd better go to India. That was a requirement for teaching Hindi. I should say after teaching Hindi for a couple of years always with a native speaker you understand in the linguists fashion, the chairman brought over a young man a fresh Ph.D. for Dravidian languages and this man was an abysmal failure. He was a native speaker of the two languages of South India, but had no idea of how to teach the language to people who didn't already speak it. Exactly the situation that we found with respect to the teaching of English as a foreign language in this country years later. Now that's when the chairman of the South Asian department asked me if I would join with this young man, who was a couple of years older than I was actually, to figure out a course of study suitable for American students and I said, okay I will do that. I had a year working with this man – a terrible situation for both of us because he resented me and I couldn't help but feel that he had a right to resent me because I didn't know beans about his language and yet I was expected to show him how to go about teaching it to Americans. In any case at the end of that year, he was shipped home and I was asked would I accept an Assistant Professorship in Linguistics and Dravidian studies, a joint appointment in Linguistics and South Asian Studies and I said, sure. But there the requirement was that I go to India and turn myself into a proper Dravidianist. So, I did. When I came home the next year that is when **Pierre** invited me up to **Haskins** and I suddenly found that there was a chance to get back into serious, in fact more serious and more interesting, research in phonetics and speech than I had dreamed was possible. But I had already taken on the responsibility for Dravidian. So from that time in **1953** until **1970**, I was trying to fill both, you know, sets of shoes which wasn't too easy, but in **1970**, I was finally relieved of language teaching in general, but not because of the research, but because my arm was sort of twisted to, you know, become chairman. So, that I took as an excuse for giving up the language teaching.

Pat: How much direction of your efforts at the Lab was offered or imposed on you any at all?

Leigh: Well, there was one point aside from this suggestion that I might look at F3, which I obviously flubbed, you see namely, I expected that without doing anything to neutralize the other powerful cue that had ought to show up, you see and it didn't. The other thing I was asked to do was to try to codify a set of rules for synthesis and I remember this was still in the pattern PBII days. I looked through the available literature, which was of course entirely **Haskins** stuff and started to paint patterns following the directions either explicit or implicit in the **Haskins** literature and found right away that when one attempted to generalize some of these findings over contexts other than those used in the tests that yielded the generalizations that I did not get the kind of responses from subjects that I expected and so I found myself trying to figure out what cues were missing, what was wrong with the generalizations and therefore ultimately, I was side tracked from the task of setting up rules because I found that the rules I tried to set up on the basis of the literature were inadequate. So for example, the rule that would work for initial **pa, ka, ga, ba, da, ga** did not work very well really over all of vocalic contexts and the mirror image did not work in final position and did not work all that well in medial position. Of course, when I looked at the details of the perceptual data included in many of those papers, it was clear that more had to be known about the cues that I really decided that it was premature to come up with a cook book. A cook book was what was required and I guess that there must have been dissatisfaction on **Frank's** part and **Al's** part I suppose because **Frances Ingeman** came in and the job was turned over to her. Except for those two occasions, I had really no direction. I followed, you know, whatever I was interested in doing, I would say that the only restriction if you like was the salutary one exerted by the colleague on my right who had a tendency to say, come on let's stick to this until we… I passed the point where I left my own devices and moved on to something else. So, I would say that by and large, I felt and still feel that I have all the freedom to pursue whatever will o the wisp thought comes to me that I would have in a straight academic environment and that I found was a great thing about **Haskins** that it was not closely directed research.

Pat: Well Arthur, you've been blamed for providing a lot of motivation here.

Arthur: That's very good, very nice. I guess my coming to Laboratories is somewhat different. You know, I am the only person here I guess who was actually a student in a very formal sense of **Franklin Cooper**. There have been others, of course, but I am the only one now at the Laboratory. My background went through various shifts. You know, I grew up in North Eastern New Jersey and I became interested in botany. And I thought I was going to become a botanist and its sort of curious that one linguist who achieved some notoriety with various editions of his text book, **H L Gleason** was in fact a botanist, may have something to do with taxonomy or something in the later fashions of linguistics, near taxonomy, although I do not know that I was interested in taxonomy. But I interrupted my college studies to go to the war as did Leigh and I was in the army for three years, mostly in Britain and on the European continent. I have to add that although I had this interest in botany, I was also very much interested in language and languages and had had a fair amount of practical experience with languages, you know, in childhood, in adolescence, and young adulthood. So for example, unlike these people that **Leigh** was mentioning, I arrived in the army in **1943** with great fluency in Modern French and ironically even while all these people were being thrown out at the gates, and although this was clearly stated on my service record, no formal use was ever made of it. So much for the army classification system. Instead, and this does, I think, have a bearing on later developments at **Haskins Laboratories** under my career. What with my, as a turned out, risky eye sight if I were to become a rifle man which I thought was going to happen, I was instead sent for training in the medical department and became an x-ray technician. You know, went out with a field hospital for battle causalities. As such although I had some background now you see…., of course I was in so called natural science and mathematics before that early on in college, so it was not any great thing for me to shift into human anatomy and physiology and mostly anatomy of course. Then I had enough physics and maths not to be troubled too much by the business of x-rays and whatever we had to know to operate as technicians. So I did that. I was all the time in the army (except during basic training) in x-ray work in the European theater of operation as we called it. When I came out, I decided I didn't wanted to be a botanist anymore. I was more fascinated by language, but I did not even know that there was such a field as linguistics, you see. I really did not know. This would be when I came out in **1946**. So I went back to school thinking I would go on in languages, sort of shifting that way and then I discovered, when I finished college and wanted to do graduate work in languages, that there was such a thing as linguistics. I was at Columbia University and I went to see **André Martinet** who was then head of the department, had arrived there as a refugee from the war (well, he'd been in a prisoner of war camp and had a history of his own) and he was chairing the department of linguistics. And he said, yes I think you belong here and we went through the process and so on and I got in. Well at the same time, I had this wanderlust that seems to afflickt me to this day and I started looking into the possibility of going to Southeast Asia and its another story as to why I was interested in that but let's not waste time with that here. I looked into that, found there were opportunities in the **Fulbright** program, eventually got a grant to go and teach. I wasn't an established scholar. So I had to teach and used the opportunity to work on languages and so on. So, I went off interrupting my studies and also I had been doing some teaching in the schools of New Jersey over in my native village on the side until I got a, well nevermind…. I was doing that. I even had a license to teach in New Jersey and they said they would hold a regular job for me in the high schools there, you see. So, I went off to Thailand… Oh, by then, I had gotten married. So **Ruby** and I went off and wound up staying there two-and-a-half years. During that time, I had one more language to work on although I was teaching English, and spending lot of time in English, English phonetics, basic linguistics, and even methods of teaching English. So, I was anxious for all sort of things, way down in the South in Bangkok. So, I came back, that would be in the fall of **1955,** to Columbia University. Martinet had left, my adviser was the late **John Lotz** and he said to me, "Mr. Abramson", in those days we were quite formal. He said, "you are very much interested in phonetics". He said, "You've demonstrated this and had already done some course of work and you've shown a great interest" and he said, "this is a wonderful opportunity". He said, "we have **Dr. Franklin Cooper**, President of **Haskins Laboratories** and director of research there as an Adjunct Professor. Now, for the first time, Columbia University he is going to offer a course called Acoustic Phonetics. You should go and sit at his feet. He said these courses are to be offered downtown in Manhattan at **Haskins Laboratories**. Ooh! all right, I signed up for it and said, "this sounds good". It was a very bad year for me because I was also studying Sanskrit you know, having some relevance to my Southeast Asian studies and I broke my back over Sanskrit, and also over **Frank's** reading assignments. But this to me… my entrance to the Lab was very different, was an eye opener and **Frank**, the person impressed me so. He was not a showman by any means, we know that. He did not go out of his way to impress people, but I saw something in him and I became a great admirer of his. So I took this course and there were few other people at the time, **Bill Nemser** you may remember now off in Austria as a professor long years and some other people. **Jane Gaitenby** came in later. I sat through the course for a year and, while doing so, I was working on my other courses, but I had to do some research for **Frank** for the course and I had this interest in Thai, the tones, the vowels, and so forth. So, I was sort of puttering around with that in the Laboratory. He allowed me to come extra time. You know, he said, you do not have to come just when the class is in session which was in the evening every week for few hours and I would come down later. (Oh, incidentally, he gave that course again the next year and I said I want to come again so that I can do some work and I sat with him and sometimes I helped him with it.) But during that course was when I first met **Kathy**, she had come in one evening to give a lecture on psychoacoustics and the impression of **Frank** and **Kathy**, you know she was a very young woman then. But I said, my God! Look at her. She really knows all this stuff and she is established and look what she is saying. I must say, you know, if I have ever said this to you. He did not want to lecture on these topics himself too much although he assigned readings, you know, we got the standard stuff at the time, on pitch and loudness, and all these classical psychophysical experiments. But **Kathy** came in and gave us a kind of broad overview and it was very good. It made the reading a lot easier I must say because it was pretty heavy stuff some of it, you know, for me. **Frank** really put us through a lot of discipline not just in what we had to do in the Laboratory, but the reading and discussion and then some other people came in like **Jane** and others. Well later as I finished my course work…., I should add that I was in the department of linguistics in Columbia University not quite so old as one at Penn but one of the older ones with a very very European air about it. It was not all that committed to American structuralism, but this was more of the Praguian form of structuralism or neo-praguian influenced by **Martinet** and **John Lotz** of course. **Joseph Greenberg** was there in anthropology and linguistics and he was more a product of **Edward Sapir** and **Leonard Bloomfield** but he was very flexible and all that. So I had, I must say, a very liberal broad background even in linguistics and I did not feel too constrained. I even had some general phonetics. Under **John Lotz**, I had field methods and I had very broad exposure to languages and language problems and I found that I could marry this and my interest in Southeast Asia and what we then called acoustic phonetics or what we were more likely to call experimental phonetics. We were not doing any physiology then, except sort of indirectly, until the EMG project started and my contact with that was to be a subject early on for a fee. Yes, I was still a graduate student. They said well look we are having trouble, I guess, finding subjects. We are putting people in the cage with the suction electrodes on and all that. And so somewhere or another in the literature are data taken from my muscles, at least superficially. So, I used the Pattern Playback. I too stood at it, and later not long before I had finished my dissertation, **Frank Cooper** said to me…. I arrived in **1955** and in **1959**, he said, look we have something opening up here and this is again different for your experience; this is directed stuff. I need somebody. He had been persuaded to go into this business of doing the x-ray motion pictures of some several languages. **John Lotz** was involved because the American Counsel of Learned Societies had a piece of the pie, it wanted to do (because of his influence) a film on Hungarian and then the United States Office of Eduction, wanted to do it on Russian, Mandarin Chinese, and Syrian Arabic. So **Frank** said, I need somebody to coordinate all this. Someone who has an interest in phonetics, who would do the work, who would edit the tapes, and supervise the whole thing, but of course I will work with him and **Dave Zeichner** at the time, you know, was our sound engineer. Of course, we had to hire informants and we had these consultants on various languages he said, what about that, I can offer you that, stipulated a salary and so on. He said, since you haven't quite finished your dissertation, you can also spend some of your time in that. You here anyway working on it. It is an interesting topic. I was working on Thai. My dissertation was on the vowels andtones of standard Thai, acoustic methods and some experiments. So, I did a lot of that on the Pattern Playback, some of them on the old **Voback** because you could vary the fundamental frequency on that, you know using the vocoder upstairs in the place. During that period was when I met **Leigh Lisker,** not at the class as such, but when I was spending more time at the Laboratory. He seemed to be an amiable sort of fellow, you know, and very much of a kindred soul in someway not all together, but it seemed that way, and he was talking to me. He at the time as you explained before started looking at this business of voice timing and I had some interest having had experience with certain languages including most recently the Thai with three-way categories and of course there were the Indian languages, and that is where some of the complications come in. We were talking and we got going on that, but before that I was working on this x-ray project, but it was slow getting started. I was spending all my time reading literature, on cineradiography, what could be done and so forth. And we had to deal with that with the Columbia Presbyterian Medical Center, we had to use their equipment and indeed we did. So, I was doing that. In the meantime, **Leigh** was going off on that particular trip to India and he was in a terrible hassle trying to get all the stimuli made and I was spending some time with him. He asked me to get the rest of them (that he had designed) run off on the Pattern Playback and I did and shipped them out because I had some free time, I don't remember whether that was before or after I finished the dissertation which I defended in **1960**. It was at that time too that I met **Dennis Fry** who was a guest for that year at **Haskins** and became very much attached to him and that led to another long series of associations. I finished the dissertation defended in **1960**. **Frank Cooper** was really my mentor although **John Lots** was officially so for the Columbia University, but **Frank** was the one I was really working with and **Joseph Greenberg**. So, I had very good people, you know who were supporting me and stimulating me. I worked with **Pierre Delatre** sometimes because he would come in and stand at the Pattern Playback and somehow I was free to spend time with him and help him out and I learned a lot doing that. As I said, I used the Pattern Playback for at least the vowel experiments this was the synthesizer at that time. So, then I got more and more involved in the x-ray project and that really took up my time. So, I am one of the few people around from those days who served as a full time staff member, **Kathy** too, and not as an associate from an academic environment. Although I was curious to hear **Leigh** talking about spending 6 days while on the faculty of the University of Pennsylvania. It is quite a thing. So, I was there. Now, at the time, **Lou Gerstman** was there too and of course he left long ago and went off in other directions, but I have to say that he was very kind to me and helped me at a time when I was rather weak in statistical matters and some questions of experimental design and so on. He was very kind to me, somehow he took to me and helped me and of course I was at the Laboratory full time and I was very helpful in just general house keeping things, the Status Reports, this and that. One of the things **Frank** had me to do in later years was to be the editor of the first of the green series number #1 before he got someone else to take over. But before that, we had the old quarterly reports and I helped a lot with that, all kinds of house keeping things that I guess I was helpful with or efficient at. I worked on these x-ray films. There were all kinds of technical problems – of course **Frank** was very much involved. I spent hundred of hours just studying picture tracks and sound tracks, and editing boards. It was very good for me by the way because of my involvement in phonetics to look at these slow motion films and match them up and do that, but it was in some ways very tedious work. We got the films made for the contractors, so that was directed, but I had lot of freedom in how I did it. **David Zeichner** and I got to be rather close at that time because he handled the sound and we worked together a great deal and we had some of the certified engineers over us, you know, I remember old **Dave Speaker,** whom you mentioned recently, was in charge of the vocoder at the time. We were using the vocoder in order to stretch the sound to go with the slow motion film that is upto that time people had not been able to synchronize speech with slow motion film they used simply music, and so on. So what we did was to…, this may have been **Frank's** idea. We had this 22-track tape recorder, you may recall, upstairs.

Leigh: The Rangertone.

Arthur: The Rangertone. And we… no… no… no not the Rangertone. This was a different one. The Rangertone was just a straight audio recorder. I've forgotten the name, but it took 22 separate channels of information. So we had enough, for I guess 19 or 18 channels on the vocoder plus a line for the buzz-hiss choice and one for frequency information and one more for synchronization. You know, I remember 60 Hz tone and 120 just to synchronize the film with the tape because we did the filming in the x-ray room in the Columbia Presbyterian Hospital. We used a tape recorder with synchronous drive to the camera, but the speech was being recorded not on the film directly as some people do nowadays, but on a separate tape and so we had to match them up later which is quite conventional in movie making. Well, here we had the problem of passing the speech of the original recording that is to say when we took the films at three times normal speed to get that three-and-a-half slow down effect, we recorded of course normally, but we then passed the speech through the vocoder to get all the spectral information in the form of electronic outputs of varying voltages of all these analyzer channels, recorded them as slowly varying control voltages onto the channels of a tape recorder and then simply as it slowed the whole thing down on the output using our synchronization signals to keep everything in line, and then put that through the synthesis end of the old vocoder. That is the speech recorder. Of course it didn't sound very natural anymore although I still got to the point where I could recognize this informant and that informant of the Chinese, or the Arabs, or what have you and we did that and that was really quite a technical task and not by means all together successful. There were voice breaks and so on, but it was very challenging. We were constantly tinkering with settings, balancing modulators, and what not. Much as you and I had to do when we used the **Voback** synthesizer worrying about varying DC voltages from the elevator and so on. That took up great deal of my time and it was sometime in that period since our first real publication. The first one was published in **1964** paper, so it must have been in the early 60s around the time I was finishing that that I really got together with **Leigh** very seriously. Although I was much interested in being associated with him when he was back from one of those trips and we got to work on the voice onset time, and he has talked about this. I got very much involved in that, but it was of course, as I say, as a student of **Frank** that I came in. I was influenced by **Frank** especially, by **Kathy** at least for the brief contacts and later more, and certainly by **Leigh** as I got to know him. Now there were other people, Uncle **Al** certainly. **Alvin Liberman** loomed large after a while. It was hard to miss him. Even then, as a younger **Al,** he was a person who made himself felt and he was gratifyingly interested in some of what I was doing and this led ultimately to my revising of the dissertation by the way as a monograph in the Indiana series that **Thomas Sebeok** was editing Linguistics Folklore and Anthropology and people asked me whether I was in folklore or anthropology or whatnot. This work was published and so I had lot of support. Also, I have to say that somewhere along the line, **Frank** was concerned about me and very nice, thinking who knows what will happen to the Laboratory, and that I should keep my eyes open for academic slots, and possibly, if I was really nearby (as he was kind enough to hope) I could be an associate the way others were and still get some security. And, it was around that time that Queen's College was looking for someone in speech science or a tame linguist or something like that and **Arthur Bronstein** and others were involved. Oh, I had been doing some teaching on the side at Hunter College and New York University Saturday mornings, Hunter College certain evenings, one year at Columbia University when **Frank** was not available for a course below his level, a kind of general phonetics course with some instrumental phonetics, and things like that. So I had been doing a little teaching on the side, but eventually I took this Associate Professorship at Queen's College and simultaneously (the way they were working in those days although there were few people exclusively at the Graduate Center that was not too

common yet **Kathy** is of course that way now) I was at Queen's College and for the city wide doctoral program in speech and hearing science not in linguistics. I was at the Graduate Center in both places and that sort of thing. Two years later after that, **Frank** and **Al** were to go off to Stanford California to the think tank to **Palo Alto** I should say. That would be, do you remember, may be around **1965-1966** and **Frank** came to me and he said, look this is your second year I guess at the City University of New York. "Do you think you could arrange for a partial leave to sit here for me and coordinate the speech section?" I had helped him a lot, you know. He sometimes used me as a shield against people coming in and what not, as a front man for this, that, and the other thing. He said, "I just need… I'll be in touch of course, but I need someone just to coordinate things not for the whole lab, not the biology section, but the speech section as you recall". Oh! you know I was so grateful to **Frank.** I'll do for this man anything he asks. So I went to my Department head at Queen's College, which was my primary seat, old **Wilber Gilman** it was, the rhetorician, a nice gentle little soul. He said, "well Arthur, I would like to help you out, but there is no way that a person who has been here as short a time as you can get by the bylaws of the Board of Higher Education at the time and take a leave of absence this soon, even a partial leave – part of the salary would have been taken over by **Haskins** and all that. I was so flabbergasted and I said, "what am I going to do?" **Frank** wants me to do this. So I wrote of letter of resignation. I am going to try various ways. So I resigned and I went back to **Haskins** because **Frank** wanted me and I spent the year there doing this and also continuing research, but doing the administrative work. It was then that I even learnt to use a dictating machine. **Frank**, my hero, used one and so I learnt. So I said, this is how to do it, you know, I took to that and it was really a lifesaver. So, during that year, they kept the position open (they had a couple of people in there, you know) and they came to me, (I guess **Arthur Bronstein** was still in the department and other people and I had been an Associated Professor) and they came to me and said, "look this year will be over, you know, in another five, four, or six months, will you come back? We will offer you a full professorship. Come back".
So I had in fact looked into various possibilities here and there but most of them were a very great distance away from New York and I rather liked the idea of continuing an association with **Haskins Laboratories**, so I accepted it. I went back and became once again an associate. So there was that feature as well. There we are… Those were the early years, a lot of that work, as you know, was with **Leigh** after the x-ray project. Other things that I did were simply by way of helping keep up the house administration and so forth and I guess to this day I have been involved in some of that, but as **Leigh** says, you know, we got together. We formed a very good partnership. In fact, for years, I've referred to him as my business partner, people have heard me say, and we kept going with all these studies. In recent years, we have been working together much less although we stay in touch, you know, generally arranging our schedules and so on and now and then have done something together anyway, but less than what we used to.

Pat: You mentioned two figures of that period. **Frances Ingeman,** just briefly and **Jane Gaitenby.** Are there any other things which you can add about what they did at the time that they arrived and they were engaged?

Arthur: Well of course, we both knew them both. Do you want to say something about **Frances** or may be **Kathy too**?

Leigh: Frances of course was here on several occasions, but I do not remember her being here for much more than a year.

Arthur: I was about right. The first time…

Leigh: The first time a year and then later on she would come for even shorter periods up to three months or so. As I said, I remember that time when she was asked to codify the existing information and I do not remember whether she was named as first author in this paper that was published in **JASA** in **1959**. She may be have been first author.

Arthur: I think she was.

Leigh: It had lot of our names attached to it, you know.

Arthur: Not mine but yours.

Leigh: The initial… Maybe one of the first papers on synthesis by rule.

Arthur: I do not know how she came. It may have been through **Thomas Sebeok** who was very close to **John Lotz** who was close to **Frank Cooper**. I do not know because **Frances** has been a student of Sebeok's in the New York University and has interest in phonetics and so on. She of course wound up at Kansas**…** still there after many many years. We know her very well. **Jane Gaitenby,** as I said before, was a student at Columbia University. I guess she had some background in anthropology moved into linguistics. She was a fellow student of mine and you may recall when I gave a brief eulogy at her funeral service, I mentioned this, you and I were there. She was quite a colorful character and clearly had an interest in language and linguistics. She also had great graphic skills. She was very good. She was an artist as it were although I do not think she did much professionally, but she had great talent. Wherever she sat listening to lectures, she would sketch things. There are still some sketches by her scattered around here and there. Well at the time some work was needed in preparing displays, I think again it was something that **John Lotz** was involved in with **Frank**. I do not remember exactly what, but she of course was a student of his just as I was and other people and I guess he put her forward as someone who could do thisSo, she was hired by **Frank** while she was still a graduate student at Columbia and she did this work, a lot of drafting, a lot of artistic work. Now, unfortunately for any number of reasons, she never really followed through on her academic career. I guess she got masters degree at one point in linguistics, but just eventually drifted it away from any formal continuation of her program and became a full time employee at **Haskins** doing all kinds of useful things, including some speech work. So she got involved very early on in this project on the reading machines for the blind, as you know. For some years, she was working in New York City as well as here later until her retirement. She, as I say, was a very colorful person. I think people liked her generally, but I think you know she had her own problems and just did not make a very great mark although it is interesting that certain pieces of her work continued to be cited from time-to-time. What comes to mind is that paper…… 'The elastic word.'

Katherine: Yes, its cited quite a lot.

Arthur: It still is.

Leigh: A paper, by the way, which was never published. The English version was published in a Russian translation.

Arthur: Exactly so. It was published in a book in the Soviet Union in Russian, otherwise was simply in, may be, our Quarterly Progress Report and yet people cited it and still do. I think I was one of the speakers for it, one of the eely-tongued ones or something… I don't know that she used that term, but people of varying speaking rates and different conditions. So, she was there.

Leigh: Well, also coming in about the same time as she did was **Tom Rootes.**

Arthur: Oh. Yes.

Leigh: I do not know whether you know his name. **Tom Rootes** was… you remember.

Katherine: Yes indeed. He was one of the pre-voicing English speaker.

Leigh: He was a subject, one of our speakers for English. He was the sole… He practiced pre-voicing and **Arthur** and I thought may be…. we tried to explain why he stood out like a sore thumb. We thought well perhaps its because he is a Californian, you see. Indeed I think at some point, either in conversation or may be in a publication, **Peter Ladefoged** said something about American English BDGs being produced in even initial position with pre-voicing and we thought may be it is because **Peter** came to California and he is biased, you see. It turns out not to be true. It is not consistent. We looked into this question of enhancement. I think we called it in the later paper of **1967**. Since then people have gathered data to refute our generalization or supposition. But **Tom Rootes** again like **Jane** never completed his academic work.

Katherine: There was also **Gloria Lysaught** who had more or less the same pattern.

Arthur: Also from Columbia. A little later on, but she was a real worker in the vineyard with you and did not pursue….

Katherine: There is something kind of interesting historically about you know all these what if questions I suppose are mildly interesting. But one of the things that I think probably was Arthur, partly because of his own conscientious and thorough character perhaps because he was a little earlier, perhaps because he was just able to make better use of the department. There was a kind of depression I think attached to that department that caused these three people. I happened to have run into **Bob Port** who also came out of that department in its dying days.

Arthur: Before he came to us in Connecticut.

Katherine: In its dying days.

Arthur: It was much later of course when **Bob Port** was there.

Katherine: But then **John Lotz** died of course. I have forgotten what year.

Arthur: That would be I think in **1970** when I was in Thailand at that time again you know…

Katherine: He was quite ill before that.

Arthur: Yes. Indeed.

Katherine: Indeed, when I first met him he was a very vital person and certainly he was not by 73 or 74. I think this seems to me that that department gradually declined and their failure to adopt the Lab, as a matter of fact, (which accompanied the decline, because it would have been a rather obvious thing to have happened) resulted so that a lot of the initiatives that were sort of begun by **Frank**, I think with a view to kind of recruiting a connection didn't work….

Arthur: The linguistic program at Columbia had been a sick man for many many years.

Katherine: Well yes, he continued to be a sick man and I think his sickness showed itself in various ways and resulted in a lot of what appeared what would have looked like very sensible initiatives on **Frank's** part if they worked. I was going to comment in that light of course the journal Word failed.

Arthur: Well, it went into the doldrums. It still exists you know.

Katherine: Yes. I know.

Arthur: It has been coming out more frequently. It has been coming out more regularly again. It is still.

Leigh: Another point may I just remind you there was another loss, it just occurs to me, and that was a loss of **Uriel Weinreich** …

Arthur: He died very young.

Leigh: …because judging from my own experience, he was very positive toward our line of work. He invited me to be Visiting Professor at Columbia for one term I think in the early 60s…?

Arthur: A little later I think.

Leigh: Whenever. It must have been a time when neither **Frank** nor **Arthur** was available, I do not know.

Arthur: I don't know. I think, I was around when he got you, sure.

Katherine: I was going to say also that beginning in the 60s in the period from **1956** to **1962,** which were the two years that I had my two children and I was quite ill in between because I had a number of miscarriages. But anyway, so that I went through a period of being less present with one thing and another. But, in this period when the Lab began… If you look on the Columbia thing, as the beginning of an attempt to find places to go to, we were all in those years, in a sense, seeking an audience and a place to go, an academic base. I think this was a real…, There was a sort of queeziness attached to this whole area. And another thing that was beginning, the whole possibility of going to California was of course brought up in that period in the middle 60s. But also another event that was stirring with **Arthur's** association at Queen's. At that time, all of those speech and hearing departments in the city were partly departments of rhetoric. They all had conventional articulatory phoneticians attached to them. For a while, **Arthur** was the one at Queens but there was a guy called **Jim Able,** for example, at Brooklyn and there were various others of these people. **Bob Sonkind** and the guy that used to do the accent work at City College (Marshal Berger). There was a sort of Pygmalion spirit in some of these speech and hearing departments, but it was killed off essentially because, I think in retrospect, in the first place, speech and hearing as a clinical discipline was growing and in the second place the notion of pejorative work on accents lasted longer in the city system than it should have but it was very dead. These people were not, except for **Arthur,** respected at all, or very much, in the linguistic community. Some of these people at least were studentsof **Martinet** but they were very peripheral to the growing linguistic enterprise and they weren't very central to the speech and hearing enterprise either. So there was a question… I remember that **Frank**…. Remember **Beatrice Jacob**? **Frank** did sort of try out these departments in what were then really very separate colleges. Again, these sort of things did not seem to be doing very well in providing a base. Then of course **Arthur** left to go to the University of Connecticut when the Lab was about to leave New York. By that time, I was a little out from under and was very interested in forming an academic affiliation myself. And, by that time, I began to be interested in speech pathology as a discipline in its own right. And it was the more logical home for me. I had long since ceased to be a psychologist. But I think it is very important in this tape to get some of these thoughts down. **Leigh** was partly I suppose because he was associated with **Zelig Harris** partly because he continued his association with the Indian languages. **Arthur** also because… **Leigh** and **Arthur** were successful in maintaining themselves within linguistics for many years, but I think it is important to get down the notion of just how tough it was to get any of this work, that we were all working at very hard to do, any kind of status at all.

END OF REEL 6 PART 2

Leigh: It was to be a place where linguists, psychologists, speech people could interact as well as of course people with technical background to produce and rationalize equipment. I don't know what the financial state of health of the Laboratory was, but certainly the Laboratory had to depend on outside funding to a considerable extent as far as I knew all along, and unlike academic institutions could not afford to provide the kind of tenure security for a wide range of personnel. In fact, from my observation (except **Kathy** when she was, you know, involved in family matters much more) the people who had academic appointments, **Al** in particular must have spent at least as much time at the laboratory as you might expect of any full-time fully affiliated person – and I did too. We would come and work right through the night. We all bunked in the sound room. I would bunk in the sound room regularly or sometimes, if **Al** was on his infrequent visits home, I would sleep on his office cot. And indeed, observing this, I would wonder how people… (only the men, by the way the women didn't spend the night in the Lab. That was verboten).

Katherine: I was supposed to disappear.

Leigh: Oh yes. Nightfall. I still remember the crisis. There were two crises about that. One was, there was a period when **Jane Gaitenby** was ill and she wanted to spend the time in the Lab and I was there and **Frank** was terribly perturbed. I think he asked me if I could help persuade her, you know, not to spend the night in the Lab. And the other was the night of the great blackout in New York City when people couldn't get home, you see, and I think **Frank** particularly was again in a terrible tizzy, you know, about all this. How can we countenance males and females sleeping on the premises at the same time. I mean it goes back to an earlier period when I can understand. We mustn't look backward from our present unbenighted (**unintelligible**) you know, back to those times. But now I forget what I was going to say…

Arthur: You told…

Leigh: I think that the people who had academic positions outside as their firm bases were spending as much time on research at the Lab as you would have expected of any 9 to 5 person. We were not 9 to 5 persons. The Laboratory lights stayed on until 3 to 4 o'clock in the morning. You know, that was the norm and of course in those days, if you are going to do synthesis work, there was only this one machine and you had to stand in line, you see, if you wanted to use it.

Katherine: I think that recording was difficult. Everything was.

Leigh: Oh yes, everything was difficult. Recording of course of the final stimuli was in the hands of **Agnes McKeon** usually. She did the final job of painting in her white gloves, you know. But then the preparation of test materials was an agony. To prepare the kind of test I can do in an evening (maybe I can still do it after we finish here) would take six months during the course of which you would be cutting snippets of tape and festooning them all over in the room outside the sound room, you know.

Arthur: And recombining them into different orders………

Leigh: Using the old bag of poker chips for the numbers in order to get your randomization. Do you remember that?

Katherine: Yes. Yes.

Arthur: Kathy is going to say something, I think, about the business of the academic base.

Katherine: Yes, I was just going to say that about this whole business. It was very very difficult to do, but also I think that we were remarkably self-motivating. It is not just the hours, but these hours were spent against an obbligato of our not being the standard thing, that is we would get up and give papers at meetings to…. We behaved like a religious sect really in that we did not give these papers, that are now considered to be standard works, to universal applause – anything but. It was very commonly believed that there were other ways of skinning the cat. That is, I remember we would go through meeting after meeting of the Acoustical Society (for some reason, I guess, **Arthur** and I were always moderately active in the Society as a society and **Frank** also to some degree) but there would be a paper. We would sit through papers of the speech session. The speech production session would be something like 7, 8, or 20 vocoder papers plus one from us and that would be speech. So meanwhile, back at the ranch, **Al** would be sort of attempting to break down the barriers in psychology. There were few people who began to be interested as cognitive psychology took over. A few psychologists began to be interested in what we were doing, but it was by no means a shoo in. Speech analysis and synthesis was still very… If you had asked people of conventional wisdom about speech analysis and synthesis during this period, they would have felt that the work that had gone on at psychoacoustic laboratories when I was a graduate student, where you found out about speech by doing band pass filtering and submitting some of these new related bits of speech to listener panels was the way to do it. It was only very very gradually that people began to look at speech signals… I was going to say atomistically, or in terms of their real cue structure rather than as things that could be decomposed in the way that analysis instruments could easily decompose them.

Arthur: I would like to say something about a point that **Kathy** was making about the slide of linguistics at Columbia and its bearing on the search for academic roots. It seems to me that I was lucky in the matter that I was still there on the crest of a wave, you see. This feeling of discontent, the friction of factionalism, the loss of key people, all took place after my day, I mean, the late **Uriel Weinreich** who died alas very young, was a very active and dynamic teacher of mine, he wasn't in this field, but he had, I would say, a great influence on me and indeed we became friends after I was finished, we were neighbors, and as you know I really felt his loss as a great blow. **John Lotz** was still possessed of some energy, **Joseph Greenberg** with his anthropological leanings had a great influence on me and there were other people in related areas there. Later of course… Now even then, when I got back from Thailand, there had been a terrible fracas there between **John Lots** and **André Martinet** and this resulted in a real schism that yielded some of these people that **Kathy** was referring to scattered around the City University who took strong sides. They were great disciples of **Martinet** and enemies of **John Lotz**, I don't know how much you are aware of this. It was constantly on their minds, you know, they were devotees and this kept going, you mentioned the near loss of the journal Word, or the long interruption. This was in fact a result of all this and I of course, as **Leigh** remembers, was very much involved in the…..

Leigh: It was this rump revolt. Right?

Arthur: Yes, which I was into.

Leigh: Didn't you propose that I be President or something like that.

Arthur: No, It was actually **Uriel Weinreich** but anyway, we won't go into that in great detail. But, I even got **Frank** to come to a meeting as a member of the Linguistic Circle of Word to vote and he was challenged. I didn't mean to embarrass **Frank** although he was a proper member for the Laboratory. There were these people around the city. Now there was this unfortunate bitterness there that I knew of mostly through these other people, some of whom you have mentioned at colleges of the City University, but these were people I knew because I would go to the monthly talks at the old Linguistic Circle of New York and in fact I was a member of the executive committee for a couple of years and things like that and yet although I knew these people and some of them were fellow students of mine like **Diana Kao,** you remember**,** who was also a student of **Frank's**. I was never taken in by them. I never was influenced very much by them. I enjoyed my associations with my teachers at Columbia and of course **Frank**. So, I think I got a lot out of it. Now these other people that we mentioned, **Tom Rootes**, and **Jane Gaitenby** were there early enough not to have suffered so much from this decline that came on. I think we have to be very careful there. I am sure they had some very private and personal problems in the matter. I liked both of them very much and **Tom Rootes**, of course, was at the Laboratory for a long time and I in fact tried to help him. He was a man of my age and a fellow student with a base in anthropology, but he stayed with the Lab for quite a while and suffered from much discontent and disillusionment and frustration and eventually wandered off. I think he is somewhere in the world and I think I have still seen his name recently on membership lists of the LSA, Linguistic Society of America. He is working I think with some publisher, I haven't seen him in years. You mentioned **Gloria Lysaught**, she had her own concerns. She was also a registered nurse and in many ways that made her an ideal person to get into the physiological project. But I think the downhill slide of linguistics at Columbia, although it may have had its germ then, really started happening later after **1960**, **61**, **62**. You know, a few years later **Uriel Weinreich** died and as you said **John Lotz** was getting sicker and sicker and very ill and I heard of his death while I was in Thailand. And other things happened, and the department was almost disbanded. It was a very sad state of affairs, but there was on **Frank's** part and **John Lots'** part, a very sincere desire, I think, to meld these two and get a base. I was rather surprised (in the context of some of the things that **Kathy** was taking about) when **Yuriel Vanraf** said to me a few years after I had, (maybe a year or so, after I had my doctorate which was in **1960** and I was at Haskins and indeed, I guess around that time, it was a couple of years after that, I had a invitation from Queens College and City University in New York) **Yuriel** said to me, "you know, **Arthur** I really wish we could have you here at Columbia". So, it was only then I had a kind of intimation that maybe there had been some maneuvering behind the scenes to actually create a faculty position that would be devoted to phonetics and linguistics because it never occurred to me in my wildest dreams that I, as a recent product of that department at least officially, would be invited back so soon, you know. It did not even occur to me and I was sort of surprised when he said that to me perhaps he thought that I thought this was reasonable thing to do and at around that time I was going off to the City University. But I had, you know, in general rather happy associations with the department from my time. You know, I thought I had very good training, very rich and broad background and of course it was there that I got led to **Frank,** you see, and **Haskins** Laboratories. And there is just one another thing I would say because I didn't use the term, but it has come out in some of **Leigh's** remarks and by implication in ours. That this rich interdisciplinary mix had a great impact on me and I guess everybody. That I didn't have to have my base only in the academic discipline of linguistics, but I could feel myself to be at these borderlines to the extent that we had to recognize disciplines between linguistics and psychology and the time of electrical engineering and eventually physiology and for me this was a very heady mixture and very good. You know, it was all very stimulating and exciting for people like me. I felt I should spend more time learning something about electrical circuitry. This was just quite hopeless, you know, you have only one life to live. But still, it was very stimulating and I felt very comfortable and happy with this group of people across these disciplines.

Leigh: What you just said reminds me of something you (Kathy) said when you were talking about the uphill fight within a department of psychology. In all the years that I have been at the Lab, I've very rarely found it at all profitable to talk with my colleagues in linguistics about any concern of mine. That is, linguists are supposed to be interested in speech from their standpoint and as you know they still don't hesitate to make strong statements about what the distinctive properties of speech are, but they are not particularly interested in the collection of data or the examination of data that people have collected in laboratories so that while I have been all these years at one institution, the University of Pennsylvania in linguistics, I have never found it particularly profitable to talk about any of my own research with my colleagues. There is only one exception and that was **Henry Hoenigswald**, who is a man of very broad interests, who happens now to be a member of the National Academy of Sciences. He is the only person I have ever found interested in talking to me about my work and from whom I could get interesting and valuable comments. But otherwise, the only people I can talk with or have been able to talk with over the years and learn something was from my colleagues here at the Laboratory. So much so that, for years, I have felt myself really not to be anymore a linguist in the sense in which many linguists define the term.

Arthur: Of course, if I may speak to this point. Again my experience was somewhat different because, along the lines **Kathy** was talking about, I went into a department of speech and hearing where I was their, you might say, captive linguist and of course, **Kathy** said something about the background to all this, and I was made very welcome. Now, there were some ways in which I was an ugly duckling in this pond because of my attitudes and background. And there was another thing too that **Kathy** would be very sensitive to as I went in over the heads of old timers there, you know in rank and so forth because of the history of the colleges of the City of New York before they formed the City University. That is another matter. But anyway, aside from that, I was very welcome and there were people who at least had some overlapping interests – mainly clinical people. The people who were perhaps least comfortable with me were the kinds that **Kathy** has also mentioned, those who are in speech correction, accent correction or the like. I am not talking about people who were therapists dealing with organic deficits, but people who were changing accents. They were not too happy with my kind of free and easy attitude you know and my desire to know what the basis was, the criteria. What do you mean by standard English or the standard variety of greater New York English or whatever. So I would raise these questions. In fact, its sort of amusing. At one time I was the chairman of a committee to look into this, and the Chairman of the Department asked me to stop being chairman because I was upsetting too many people there. But nevertheless, aside from those rough waters now and then, I did fit in very well and around that time they were beginning an undergraduate linguistics program at Queens'. There was not as yet a citywide doctoral program. The closest to it was a component of the anthropology program. So eventually, when even that was being formed, I was a member of the graduate faculty of the Graduate Center also I would attend the meetings discussing this and had a lot to say about it, but I was nevertheless in this department. It was only when I accepted the invitation to go to the University of Connecticut in **1967** that I actually went into a department of linguistics. But, I was the founding head. The groundwork had been laid by **Alvin Liberman.** I mean, without his pummeling away, pounding, knocking, flailing, arguing, it would never have been founded there. The proximate cause was a committee that was headed by **Ignatius Mattingly** who had come on to the scene from the National Security Agency, who had been a visitor at **Haskins** Laboratories as he was one of the contract monitors and wanted to go into academic life and was working on this degree again at Yale and had an instructorship at the University of Connecticut where his old buddy, old college classmate from Yale, **Homer Babbage** (the late **Homer Babbage)** was President. Babbage created this committee with a number of people from various language departments and psychology and **Ignatius** was asked to take on the chairmanship as his task and,not surprisingly, recommended the creation of a department of linguistics and the idea was that this would have very much of a phonetic bias. It was you know, for my money, when I arrived there with **Phillip Lieberman** that year and with **Ignatius Mattingly** who had been in the English department, but was pulled over. There were three of us, **Mattingly**, **Lieberman** and **Abramson** forming the Department of Linguistics and I knew that the whole name was a farce. You know, it was a Department of Phonetics. This was the bias that **Al,** I think**,** succeeded in putting over and, of course, we did gradually, very slowly expand and cover a little more of the waterfront. But we were a very peculiar, early Department of Linguistics. Of course now we have a couple of distinguished syntacticians and we are strong in phonology, not in historical linguistics, but the **Haskins** presence (of course that may be a later in the period that **Pat** has asked us to talk about) but I have to say that the **Haskins** presence was very much there at the University of Connecticut and of course it had been in the person of **Al Liberman** who, as **Leigh** was saying, would come down here and stay a few nights. I don't know how any of these families stood it – **Frank's** family, **Al's** family, **Leigh's** family. I think, from the point of view of, if you will excuse my saying so, interpersonal relationships, it was quite unconscionable. Nevertheless, these men did it and spent all the time here and they were quite dedicated and some at great physical inconvenience. **Al** coming down from Mansfield, Connecticut, **Leigh** coming up from Philadelphia and of course continuing to do so as the Laboratory moved to New Haven, which made it worse for him. But, the Department of Linguistics at Connecticut, at least in its early years, was a creature of **Haskins** Laboratory, so there we had this. And then the further infiltration into psychology, with **Al** already present, began with the arrival of **Donald Shankweiler** and then later on, of course, others who became interested as well like **Len Katz** and then eventually **Michael Turvey.** So there of course it was possible to work out a very solid relationship because **Haskins** itself had a hand in creating that Department and no question about it. No one says that officially you know.

Katherine: Yeah, but I was trying to think when the change began. Of course in psychology (and **Al** is the person really to talk about this because I was no longer really part of the psychology community by this time) it was really with the beginning of the 60s when cognitive psychology began to be the motor, the philosophical motor of Psychology Departments, people began to talk about cognitive. A good deal of the **Haskins** work on speech perception lay at the heart of the standard cognitive psychology curriculum. For example, in books like **Neisser's** book, which was the sort of classic textbook on cognitive. It was the first cognitive psychology textbook and sort of one of the most influential books I feel in recent years. The **Haskins** work on speech perception is at the heart of that book and that had an enormous effect. Meanwhile, I think that in some sense it is still uphill all the way perhaps within linguistics and the linguistics community at large.

Arthur: Less so today I think

Katherine: But it is variable from that. I could comment on my own linguistics.

Arthur: Okay.

Katherine: But, I don't know that I should.

Arthur: Well.

Katherine: Certainly in a way the **Haskins** work on speech perception certainly was successful in psychology before the Haskins work on speech was successful in remolding linguistics. It in fact has never done so.

Arthur: Yeah. I really think that is fair. Yeah.

Katherine: Within speech, the problem within psychology, and **Al** will say this as well, is that the subject matter of psychology is just too varied I mean you never need a department of speech perception because you are supposed to make up a psychology department like a Chinese menu with one person in animal learning and one person in this and that. Yeah. So that we get that effect, but on the other hand the prestige of the work – there has never been any problem there.

Arthur: Yeah.

Katherine: Speech and hearing it is a little different again. The speech perception work is very widely accepted. I think to some degree within speech and hearing departments our work is regarded as interloping work. A lot of the things for example that **Arthur** and **Frank** did together early were things that speech and hearing departments, particularly **Iowa,** were trying to do as well. Not exactly the same, but there was a whole series of papers in the 60s by Ken Moll and various students that ought to have formed the core of the kind of work we are doing now, but they didn't for one reason or another.

Arthur: I think **Kathy's** points are well taken. I demurred a moment ago about linguistics only to mean this**:** that the work has had some impact so that now we find over the decades more and more centers of phonetic research have been housed in departments of linguistics. I mean that you can count them maybe on the fingers of one hand or two, but you know Ohio state, UCLA, and other places and of course in Europe as well, in Britain and on the continent. So you do find at least a greater readiness to house this although that's not the same as saying a readiness to allow one's abstract phonological theories to be very heavily influenced by it, you see. And that's of course a seminal matter. Nevertheless, it has become a little more accepted. For years now, meetings of the National Organization, which is the largest in the world, the Linguistic Society of America, has at least been receptive through its program committee to having papers on the program in experimental phonetics. I mean one has to try to give them some kind of linguistic relevance, you know as a way of doing this, but at least it happens you know. Whereas, in the early days, it was very unusual and, you mentioned the MLA before, I too was pulled into the Modern Language Association of America by **Pierre Delattre.** In fact I was also pulled into the politics of the experimental phonetics section (that he had helped found in a smoke filled back room) to become an officer and so forth. And we all gave papers there. It was an outlet, a forum even. Most of the convention, a vast MLA convention, had to do with comparative literature and literary history and so forth. But there were still a few of us who could present oral papers on topics in speech research there. But, much more seldom I think at the Linguistics Society, (of course, very readily especially after the earlier days that you mentioned of the Acoustical Society of America until today, when it takes up a whole week of speech communication sessions). So, that I have to say, and I guess, you would agree don't you, I think that for us as linguists, it was much easier to present papers at the **MLA,** which was not a big forum, but more especially at the Acoustical

Society of America than it was to go to our own narrower Professional Association, the Linguistic Society of America. So for years, I felt much more comfortable presenting work at the Acoustical Society because people were interested, they would talk, react, and so on, than, at the Linguistic Society. But I think some of that has changed and it is now easier to find a forum there and we have both given papers at the Linguistic Society.

Leigh: May be, from what you say there has been some misunderstanding with my discomfort with the linguists, is not that I found that if I submit an abstract to a meeting of the Linguistic Society that it will be rejected – although that happened once.

Arthur: Well, it can happen to anybody.

Leigh: When **Phil Lieberman** was on the program committee. The story I heard was that he objected to it because the abstract seemed to contradict some work of my own you see, published. Yeah! I learned that from somebody. Arthur would say that's because I do not speak out straight in an abstract. But in any case….

Arthur: We have these quarrels.

Leigh: My dissatisfaction with the Linguistic Society is still, and was, that they would listen respectfully, but they had nothing of any interest to give by way of reaction. I can see because most linguists may be still are deathly afraid of anything that smacks of a machine. Whereas, if you go to the Acoustical Society meeting, you can count on somebody there taking issue. You know…

Arthur: Asking a good question or something.

Leigh: Or raising a question that you can consider serious. I still remember the first paper I gave on this rapid-rabid subject, was to the Linguistic Society where instead of using rapid-rabid I chose ruby-rupe and there was one comment, which came from a venerable member of the society of Sanskritists, **Franklin Edgerton,** who of course had been many years in India and his comment was he understood the word not to be RUpee but ruPEE.

Arthur: Which certainly was an existing pronunciation.

Leigh: A British pronunciation. But there was no warrant in any Indian language for that particular kind of use. Yes, but that was the comment.

Arthur: Well, on the other hand, look in recent years, there has been something of an audience, I think typically you do find them, there are enough people around and know what is going on, to raise some questions. I mean now adays, there will be some people, who come to that section because they want to hear these papers. I think it is somewhat better than it was and of course Peter Ladefoged… It is a funny thing you know there was this argument going among our southern friends, the **Hollien** gang and so forth, down south in Florida about the standing of holy phonetics. You know they have been much concerned and you have been an officer in this. I have too. I'm still a Vice President I guess in the International Association and this provoked some articles in their newsletter, (that little journal you know, it must be the American Association of Phonetics Sciences, that one perhaps) and **Peter Ladefoged** who must have been President or something, (you were President) and wrote an article about it. He takes all this very seriously, but he was arguing that, in fact, there is more recognition in linguistics. He said after all, there have been four recent Presidents of the Linguistic Society of America including me, you know, who have been phoneticians. He said now how could that happen? And so that was his argument but this truly is a sign of something you know.

Leigh: I don't think it's a sign of anything.

Arthur: Well he said.

Leigh: It's a sign of the fact that…

Arthur: They have run out of people to be President.

Leigh: Maybe phoneticians are willing to take on this onerous….

Arthur: Well its not necessarily onerous, it depends very much on the person, after all there is an other time when I served five years as an executive officer, secretary-treasurer and I lived through five presidents and they varied in their styles as to how much they want to be involved. I could have done all the work with the help of the excellent professional staff in Washington or turned as much of it over to the President as he wished and then people vary, they would take. No, no, the President was a honorific thing, he did not have to do too much but besides chair the executive committee if he wanted to, you know, and other committee meetings. So, as **Ladefoged** commented, these people (he listed them) did get nominated and elected, so I do not know, just something a point he made that there were people there. Whether that's an important datum or not, I would say that I think there has been something of a change. I remind you that when we first got this article out and the full thing went to print (the 64 article) it was told to us later by **Morris Halle** that he and **Noam Chomsky** had delayed publication of that book 'The Sound Patterns of English' because they felt they had to cope with those initial findings. And whatever they did, we weren't too happy with, because we wrote another article in Language.

Katherine: That is my favorite article of yours.

Arthur: In Language, yes. Nevertheless they felt that they had to handle this and we did not like the way they did it but they tried to cope with it as serious phonetic data that had to be accounted for in their model and they say so. I was reminded of this the other day… some student in the department walked by me in the corridor and said that he just read this thing, and I said yes they did take it into account. I did not talk about it but I remember at the time, hearing this, I think from **Morris Halle,** that they actually held the publication because they somehow had to cope with this set of data in the inferences we had drawn and so forth and for better or worse, they discussed it and other phonetic data. You know, their particular pose was that they were taking phonetic very seriously. I think in a sense, they were you know.

Katherine: In a sense though, you know, I have recently reviewed in fact…, I often….., The cross-language paper, my students all read, if they don't read it with **Larry**, they read it with me and probably they read it by both. But actually my favorite article in that series is the Language one and…

Leigh: More talky.

Arthur: Well it was because we were discussing these issues. Distinctive features?

Katherine: But the thing about those…, But the fact of the matter is that I think that the MIT people, in spite of my earlier remarks about somebody or other, have at least through the medium of there contact with the Acoustical Society (such as it is, through Ken)….

Arthur: Ken Stevens, yes

Katherine: …have taken this work seriously and, in a sense, the controversy as to nature of temporal representation of speech sounds has taught us what we meant in some sense. That is, the fact is that they did try to handle it. You did then write the other paper and he wrote of time and timing.

Arthur: Right. At my invitation that was.

Katherine: Yes I know, but….

Arthur: As you wrote something, also at my invitation.

Katherine: Yes, by that time, I think that controversy in the sense taught us what we meant.

Arthur: All right. That's a good point… that was good.

Leigh: I had not had the opportunity really to see what modern day phonologists do. Although we have a new phonologist, our first real phonologist if you like (in some people's opinion) in our department, and I have been meaning to sit in on his class, but I haven't so far.

Arthur: You're such a glutton for punishment.

Leigh: He is a very pleasant young man and he said he is going to be talking good deal about phonetics and I guess since I approved his appointment, I must have had some...

Arthur: Feeling

Leigh: I felt that he was a person who does have some control of the literature, so I am hoping that I won't be disappointment when I do sit down. From my examination of many of the text books that introduced the subject of phonology, I would say they are by and large pretty slipshod when it comes to taking due account of the work, not only the work from **Haskins'** but other work in production, perception etc

Katherine: There is a vast body of work, for example, much more comparative phonetically oriented than much of ours, from Ladefoged's students. Endless cross language studies from that group, in which you know, the real sort of old fashion English substance of experimental phonetics is right up there.

Leigh: But how much of their work affects the work of the non-laboratory phonologists? It is a serious question. They are housed in linguistic departments, but it does not mean that their work penetrates.

Arthur: Well, the thing is that **Peter Ladefoged** has had a kind of missionary zeal about this. He wants to be very much a part of the linguistic establishment and he has a voice that has been heard for better or worse. I mean you may not like what he says, but he has tried this and his students have gone out, they are placed in various departments of linguistics many of them and they have something to say. I mean there are people like **John Ohala,** now himself an old timer. Such people who have tried to get in even to the theorizing about historical linguistics as language changed to **acrony** and have had an effect there – they've published things, held meetings. I think they do have an effect, sometimes there are personality conflicts you know, and people don't know how to talk to one another, but with some patience and understanding, they can have an influence on each other. You see, **Kathy** was saying something about the kind of feedback from some of the linguistic arguments to us to make us think about some of these issues, and this can happen, this reciprocity. I have had some rewarding discussions with my own colleagues at Connecticut you know, in phonology and even in sort of syntax in discussing these things at least privately and I feel that we have some kind of commonality of interest in the nature of language. And although the effects may be small (that is, I don't go running off looking at these abstruse arguments about syntactic structures, I confess, anymore than they penetrated very deeply into the instrumental work or the perceptual work on speech) but I think there is an impact. There is some….

Leigh: Let me ask you this question. Let's grant that you do not pay too much attention to abstruse syntactic…

Arthur: Reasoning argumentation.

Leigh: …how much attention do you pay to the fairly abstruse reasoning that goes into the formulation of phonological structure rules. Do you pay much attention to that?

Arthur: No, because…

Leigh: Judging from your reaction to my wishing to sit on a phonology class, you say, don't waste your time or whatever you see.

Arthur: It's true.

Leigh: So, in effect, you are buttressing my argument that there is some

Arthur: There is a gulf….

Leigh: There is an impermeability.

Arthur: Here it's a question, I think, of conservation of effort, you know, you want to go and sit. I think of you as a glutton for punishment, you know, going and sitting in a course and trying to go through with it when its very hard for you to do so. That is another matter, I have some concern for you physically and all for that matter or emotionally. But I would say that yes, you are right in what you take me to task for, I do not have too much interest in following the very abstruse and abstract reasoning of the typical phonologist. Nevertheless…., Because you know, again life is short and I don't find it too engaging or too interesting. Nevertheless, sometimes one of them will sit with me and discuss things about phonetic underpinnings saying look I am trying to rationalize or justify this particular approach, this distinctive feature or that and so forth, can we talk about this from say an articulatory point of view or a perceptual point of view and, to the extent of my knowledge, I will. We can talk about it rationally and say, look, I understand your problem, you are going to have to do this and as far as I am concerned both of your solutions aren't especially intriguing – I might say that. However, given what I understand to be so about this mechanism, and so forth, it seems to me that if you are forced to choose between these two that A is closer to being plausible than B and I say why. And then I say if you could do something beyond that fine, but this is what I understand to be the situation with regard to this distinction in such a language. So we can talk. Now, how much of this happens formally in a 12-minute paper at a meeting, I don't know. How often people actually read articles, I don't know. I mean if I have to sit and wade through a very abstract piece of phonological reasoning in a journal as opposed to catching up with my back reading in the Journal of the Acoustical Society or the Journal of Phonetics and so on, I may make certain choices, you know, that things that are close to me and this is sort of inevitable. But I think there is some communication. I think it is better than it used to be, I am not saying anything like ideal and I think may be the sin is mostly in their camp. That is, the willingness to make very abstract daring statements about the sound patterns of language while presuming not to take into account the work of some of us who slop or slosh around in the slop of human speech, you know. I think that is very presumptuous. But at the same time there is the old criticism that some of the early phoneticians were quite innocent of the knowledge of language, you know, how language works, so I don't think it is sort of true anymore may be, but at one time it could have been.

Leigh: Well I have had similar conversation with some colleagues.

Arthur: You mentioned **Henry Hoeningswald**.

Leigh: Henry Hoeningswald and **George Cardona** who are both primarily historical linguists and George quite recently and a new person **Don Ringe,** who was here at Yale. They have come in and asked to discuss, you know, is there any phonetic basis that you can think of to explain a particular phonological relation or phonological development if you like and so I am quite happy to….

Arthur: Didn't **Ringe** have a course with **Al**?

Leigh: He must have.

Arthur: He must have because he went through in recent years. He may be very smart and gifted, but he also had some exposure. He had to have with **Al** and so on. He should know something.

Katherine: I was going to say that I am not surprised that it was **Henry Hoeningswald** who was interested. Historical linguistics, it seems to me, is not what people are most interested in these days.

Arthur: These days. Oh, it is true. She is right.

Katherine: And I think that is too bad. But certainly, historical linguistics as a source of systematic research on rule change has always been, it seems me as an outsider, rather more insightful perhaps than straight phonology.

Arthur: Synchronic phonology.

Katherine: Synchronic phonology in attempting to pay attention to phonetic detail in a systematic way.

Arthur: Certain sound change. And, of course, this for a longtime was the major focus of diachronic historical linguistics. You know.

Katherine: Yes.

Arthur: Of course, lately, people have been attempting to broaden it —to go into syntax and so forth. But this was, for a longtime, the thing that they could most readily work with you know. Changing phonological systems.

Katherine: Changing phonological systems. I have lately been reading a little **Weinreich –** Max.

Arthur: Max the father?

Katherine: Oh yes. **Max** the father. But he is a man of whom a lot of what he wrote about would suggest that if he were around we could talk to him profitably about what we are doing, probably.

Arthur: Yeah, in much the way that **Henry Hoeningswald** did. I guess I knew him.

Katherine: Yes. We discussed that.

Arthur: Yes, yes, but not very well, but I knew him. It's time. Shall we quit?

Pat: I hope you won't think me a philistine. I wonder whether there are any other remarks. A summing up, perhaps.

Arthur: Final thoughts or something else that I think… In the earlier sessions was something said about the support personnel? I say this because **Leigh** brought in one or two names. One name was **Agnes McKeon** and I would just like to say that in those early days in New York when I was a youngster coming in as **Frank's** student and I had to learn all kinds of procedures and techniques just how to handle things, splicing blocks (**Frank,** by the way was very good at this) but still I learned a lot of this from people like **Barbara McDermott,** the late **Barbara McDermott,** and **Agnes McKeon,** you see – in particular these people. And then, of course, the men in the shop, **Bill Winter**, **Dave Zeichner**, and so forth. And so I think that somewhere in the record should be mentioned the names of these people as people who developed into loyal members of the staff who were very very useful. I was very sad in **Bill Winter's** last days to find him withering on the vine as the need for an expert carpenter and mechanic became less and less when we were building less equipment until he finally retired, and not too long thereafter died of a heart attack. But I much appreciated him in New York and Dave and some of other technicians and of course, as I said, **Agnes McKeon** who worked right with the research group and **Barbara McDermott** who was more or less in charge at the time, with **Agnes** doing all the graphic work as **Leigh** mentioned her doing the final paintings for the Pattern Playback as she had the best control. She was a person who had various problems at that time and I remember that she felt I was a friend and, especially at the time when I was sitting in the chair for **Frank**. But I, you know, felt quite close to her and still have very warm feelings for her. And **Barbara,** of course, left after a while to go on to better things because she got a masters degree (I guess in psychology), and went on to **Bell Labs**. But still, I did learn a lot from her and these people were I think very important assets at the time, as **Leigh** has described it, we were putting snippets of tape along the edge of a work bench on masking tape in order to combine them and recombine them to get all different permutations. And **Kathy** of course, before she moved over into the budding new electromyographic and general physiological project, was immersed in all this perception testing the split fricatives and this and that and was doing the same sort of thing and we had to have a lot of help from these people and their young assistants in putting all these things together. I think I wanted to mention this. I don't know whether the earlier groups brought this up at all in the earlier oral history sessions.

Pat: Many of these names have been mentioned.

Leigh: Yeah, I can think of two additional names if they are of interest. We had for, I think maybe it was just one summer or maybe a little longer, a young man named **Chuck Cairns**, wasn't he at Queens?

Arthur: He is still.

Katherine: He is still there

Leigh: Yeah. Maybe he's a professor of linguistics by now.

Katherine: Yes, he is.

Leigh: And he became a phonologist, didn't he?

Arthur: Yes.

Leigh: And then **Erica Garcia** who was a Columbia product, a student of **Diver's** and who was with us a couple of years.

Arthur: Well, she was with us first of all in her earlier days as an assistant and **Frank,** when I was the coordinator there, asked me to try to woo her. She had come from Argentina – of German background from Argentina with a husband, **Erica Garcia.** I invited her to come as an associate, because by then she was on the faculty at Columbia. She was one person who was fed back into the system for a while, until she had to leave because of a lack of tenure. She is now in the Netherlands long since, you know, she is over there. But she was quite a striking person and, at the time when I told her we would like to invite her to come and be an associate at the Lab and do some research, she said to me in her very straight forward way, "well, **Arthur,** I will think about it, but I don't want you to think I am offering you my soul on a platter." That is the way you can image her saying this and she did. Yes, these people where there. **Chuck Cairns** was an assistant to us. He helped you and me quite a lot as a graduate student at Columbia, then it was all and then he went off and of course he is in the City of University of New York, but of course there were other people.

Pat: Few years ago I had the occasion to talk to **Newman Gutman's** wife.

Arthur: Yes.

Pat: Where did she fit in?

Arthur: She worked in biology, what was her name, **Helen**.

Katherine: Helen Newstead is all I can think of, but that is not quite right.

Arthur: Helen something, but she was some kind of biologist.

Katherine: Yes, she was a biologist. She was an associate of **Seymour's**.

Arthur: that is it.

Katherine: And she had some effect on me actually probably perhaps more than the rest of you. Because she was very interested in defining a group of identifiable woman scientists in the New York Academy and she organized a meeting of such people, which met at the New York Academy of Sciences and we would have meetings to entertain each other with our work although….

Arthur: Also professional matters.

Katherine: At dinner we would discuss our professional matters, but we always had a talk about some substantive thing.

Arthur: Some work. Yes.

Katherine: I think she had great effect on me because she was the first person that ever suggested to me that I define myself in this kind of role that is I hadn't really differentiated my problems from those of you and **Leigh** and anybody else really, although I knew that I had such problems. But **Helen**, I think, got a lot of people around New York thinking about professional advancement and tenure in this kind of a context. She was an extraordinarily militant feminist well before it was….

Arthur: It was very popular. It was really wide spread. Yes. That is an interesting point. Yes. There were other people, one name that I guess you mentioned **Leigh** was **Eugene Peterson** in fact I hadn't heard of this before, his role at **Bell Laboratories** and his welcoming of you. Because later, when he went into semiretirement of course, **Frank** brought him in as a part-time consultant engineer and he was much with us, and I mean I would see him every week. I found him to be quite a colorful character as an electronics engineer working on particular projects (that semi or super-secret thing, Peak Picking) and therefore, there was some overlap of interest because we were using vocoders for research and he was working on peak picking and we used to have lunch with him and talk with him and he was quite an outspoken person I think and had something of an impact. There were other engineers of course coming through at that time. We had to have engineers around, it was all hardware.

Katherine: But we never really were entirely successful in having a very strong impact within the engineering community.

Arthur: Within the community.

Katherine: We didn't even know we had some people like **Gordon** or like **Eugene Peterson** who had some status within that community.

Arthur: Yeah, he had of course.

Katherine: John didn't of course.

Arthur: John Borst was exclusively with us and I guess was too truculent and so forth to be much involved.

Katherine: He was an old-time….

Arthur: Radio engineer and so on. But of course he was the main stay at **Haskins** in running things and we had a long association with him and his name figures in some of the early papers especially on the instrumental methodological side until his retirement, He was a difficult person and many people had run-ins with him.

Leigh: Oh, yes. It was easy to rub him the wrong way.

Katherine: You may be amused to learn that he had this theory. Have I told you this. He believed in never labeling knobs. So we were always faced with these panels full of unlabeled knobs, because he felt that if you label the knobs, you would encourage stupid people to use the equipment.

Arthur: Oh, yes he had great contempt for all these people with little sophistication in circuitry and electronics and all that you know. We often had…

Katherine: So there were these tests like your ability to memorize all the items on a 30 knob panel with no labels at all, and they would also arbitrarily turn right or turn left. You may have forgotten this, but I haven't.

Leigh: Much of the perceptual data was gathered in house.

Arthur: That's right.

Leigh: All of us felt rather free to run around and coral all the colleagues.

Arthur: We would announce on a loud speaker, test on 3.

Leigh: Test on 3 and not only did the professional staff come in, but technicians, **Agnes**, and everybody, **Barbara** and so forth. But **John Borst,** you soon learnt better. He absolutely refused. I could never even…, Even when he came by, you know we were sort of friendly. Much of the time, his being friendly depended on the fact that when he came to me bitching about some other person, I kept quiet. I didn't chase him away or so forth, but if I asked him to tell me what he could hear, you know, he would refuse.

Arthur: Of course he was a native speaker of Dutch.

Leigh: I may have been asking him to react as a Dutch speaker. I think he enjoyed being the old curmudgeon.

Arthur: Oh yes. I think so, I think so, I think so. He had lot of problems with interpersonal relations because I know, when I was acting as the coordinator for **Frank,** much of the time was spent with him trying to pacify relations between him and the technicians working for him.

Pat: Well thank you all. That is good. That was a very informative two hours and, well, 24 minutes.

END OF REEL 7 PART 1

(This session recorded in Guilford, Connecticut at the home of Elaine & Patrick Nye.)

Pat: Time is about **7.30 p.m., on Friday, 10/28/1988**. The two re-collectors are **Irma Pintner** and **Agnes McKeon,** both of whom worked at the Labs from, I think about the middle **1950's**. Anyway, we will shortly see from their own recollections exactly when their involvement in the Laboratories really began. Things on and we are on the air, I think. Would you like to just say a word or two just to have the level adjusted correctly.

Agnes: Regarding my memories of the early days at **Haskins Laboratories.**

Pat: And**.**

Agnes: How does it seem to be?

Pat: Good.

Agnes: Okay. Well, we were just recalling that in the very early days when we were still making the synthetic speech patterns, we had many visitors who were intrigued with this new device and if they came from a foreign country or from distant places, they were immediately asked to go into our sound room and be recorded because they were adding a language laboratory. At that time, you know, we had a spectrograph that occupied one whole room.

Pat: Yes.

Agnes: And that ran in synchronization with a **Rangertone** on the outside. The tape would be played and then you saw the very large spectrogram at the end. Not 35 mm, it was 6 inches across.

Pat: Was that the spectrogram device that **Frank** made?

Agnes: The spectrograph, yes, **Dr. Cooper** designed. A bench lathe was the basis of it and then this taped transport system carried the film around and it did run in synchronization with a loop of tape and then **Barbara McDermott** who was the one in charge of all of this, it took her an hour to setup all of the instruments and the tape and the film and then in turn it would be developed and you would have what was around that loop. One lovely little girl who spoke Cantonese recorded for us in the early days. I believe they were nursery rhymes and of course the main objective then was to see the similarities in vowels and consonants of other languages as compared with American English and this was why it was such a marvelous tool for linguists and phoneticians because they could play with the speech pattern and that was my job to paint various speech patterns and Pierre DeLattre, who was the father of the Playback. When I was first hired, he was then spending several days at the University of Pennsylvania where he taught and he would just come into Haskins Laboratories a few days a week. When he did come in, the whole Laboratory came alive. He was that kind of a personality, you know, and so I was employed prior to his arrival and **Barbara McDermott** told me what I was supposed to paint on clear acetate tape and of course these symbols truthfully meant nothing to me at all, but I very studiously did what I thought I had been told and then **Pierre** came in and of course he wanted to take these loops of tape into the Playback room and he disappeared in there for quite sometime and then he came out to me and he said, "**Agnes**, they are all very well painted, but they are all wrong." Of course, I thought I was finished before it began and he must have seen the expression on his face and he said now don't worry we have been investigating speech for about 50 years and he said we will probably be doing it for another 50. Then he sat down and he explained very clearly what I was supposed to be painting and of course I was painting all the formants at various degrees change and the transitions also some very sharp, some very rounded and the same thing with the bursts for the consonants, some were further apart closer together. They were playing with the friction patterns. At what range were they still intelligible or whether they sounded like Ss or, which you could barely perceive. So then we proceeded to paint thousands of patterns upon these acetate loops.

Elaine: You had never done this before?

Agnes: No. Nor had anyone else. Because it really was a whole new idea. You know, the Playback had been built as a guidance device for the blind and there was a terrible error made during the dress rehearsal, which prompted then to give up the idea of continuing, as for that purpose and it was just at that time you probably heard this already that **Pierre** was in town speaking with someone who was a member of the Laboratory and told him of this terrible calamity and he wanted to see the device and immediately thought (because he was a phonetician) what a marvelous tool it would be. For the guidance devices they had just been painting geometric patterns on the acetate loops and he immediately knowing what a spectrograph looked like and that was the objective to reduce the speech pattern, which carries all this extraneous noise around us to reduce it to the barest minimum and still retain recognizable speech.

Pat: Was it **Pierre** who was interested in all the different languages and was making this connection?

Agnes: Yes.

Pat: I see.

Agnes: And because if you have listened to some of the early sentences, you know that he incorporated a little of his French accent in some of them, like we had **Aa la ba ma** and we had Alabama.

Irma: I remember those.

Agnes: And every once in a while some of the vowels people protested sounded more French than English, but anyway that was it.

Pat: That was one of the first jobs that you did when you arrived at the Labs?

Agnes: Yes, that was the job.

Pat: That was in **1952**?

Agnes: In **1952**.

Elaine: And **Irma** was just the other side of a wall somewhere...

Agnes: Yes, that is right, **Irma** was on the same floor.

Pat: Irma, you must have arrived at about the time that **Dr**. Provasoli started at the Labs.

Irma: Yes, in fact he was only coming in weekends and so I would carry on through the week and he would come on Friday or Saturday and he would decide what to do during the rest of the week. And if I got tired of that, I go up and work with **Seymour Hutner** for a while, we were also working together. But I came into **Haskins** sort of by accident. I had come down to see about a job at **Amherst** that **Seymour** knew about and he cornered me and got me stay, and I have been there ever since. I didn't know that **Luigi** was there at the time and I actually did my first work with **Dr. Haskins**. He was interested in **Chlamydomonas** genetics and I did little bit of his fish work with him, but that really wasn't my thing and I found that the work that **Seymour** and **Luigi** were doing was much more up my alley since I'd been trained in microbiology and I got to work with them on the nutrition work.

Pat: So, that began in **1948**.

Irma: Yes, and **Luigi** was teaching at St. Francis College at that time and just came in when he had free time, but I was there all day and then eventually he gave up the teaching and came full-time to **Haskins**.

Pat: You had presumably independent labs within the building in New York.

Irma: Yes, they were, but we used each other's facilities constantly. We had a room downstairs where **Agnes** worked, **Seymour** was upstairs in the little cubbyhole. It really was that in the beginning. It was a room about… oh about the size of this part of your living room with no windows.

Pat: Really?

Irma: And we had a little alleyway outside, which served as our washroom and kitchen and that was about it. Over the years the building was remodeled here and there and we were little bit more comfortable, but it was very very simple in the first place.

Agnes: Even in **1952**, it was still very simple.

Pat: Over the years, **Luigi** built up a rather famous collection of organisms.

Irma: Yes**:**

Pat: Do you know how they began?

Irma: Oh, it began with the cultures he brought with him from Italy when he came in **1946 or 47** and then we gradually added as years went by, we added new interesting strains and cultures of algae and seaweeds. And now that we have abandoned the collection, most of it is in Maine at the Provasoli-Guillard Center for Phytoplankton. And that is still going strong, but….

Pat: That's at **Bar Harbor** isn't it?

Irma: No, it is at Boothbay Harbor.

Pat: Boothbay Harbor. Aha.

Irma: The **Bigelow Laboratories** at Boothbay Harbor. So, over the years, we were collecting more and more cultures and occasionally we have to get rid some of them, but in the end we ended up with good collection, which people still ask for. Some of them have become important organisms for genetics, physiology in many places. But, our primary interest was always in the nutrition of the organisms themselves.

Pat: I remember hearing some apocryphal stories about the refrigerators being turned off by accident or something and you nearly lost the entire collection.

Irma: Well, that was always a terrible trouble and we tried to have a separate line for every box and then we keep duplicates once we took cultures of each strain and that way we were fairly fortunate and we didn't lose too many important cultures. So, we came up to New Haven, they put them temporarily in a cold room, which was full of molds and we lost more when we moved to New Haven than we had at any other time. I think they were storing potatoes in the cold room. That was a hazard, but it wasn't fatal. But I had never heard of an algae hardly until I came to **Haskins Laboratories**. I had been working in medical bacteriology and it was quite new to me, but all the techniques were similar and so it was quite easy to get enthusiastic about growing algae and **Seymour** of course was a pioneer in that regard.

Pat: It was **Seymour,** who, as it were, picked **Luigi**? They had common interests?

Irma: They have common interests. I think the cultures that he brought…, See, he had no laboratory at that time and someone told him that **Haskins** Laboratories had a place where he could keep the cultures. That was really his introduction actually. So he came to find a home for his cultures while he was teaching. Then that just grew from there and he became interested in what they were doing and it was closely related to what he had been doing in France.

Pat: **Luigi** was quite well known at that point. Was he already? Would you say?

Irma: Well, first he had gone through the war and he had not been able to do much work during the war.

Pat: Yeah!

Irma: But before that he had been a student of Professor **Lwoff**[95] in Paris at the Pasteur Institute.

Pat: Yeah!

Irma: So, that is where he got his training, but he did not know very many people in the United States.

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[95] André Lwoff, born 1902, died 1994: 1965 Nobel laureate in Physiology or Medicine. Worked at the Pasteur Institute.

Pat: Yeah!

Irma: So, it was a little hard to come here and start from scratch.

Pat: I see, yeah!.

Irma: But, he was very fortunate that in the early days he worked with **Seymour** on the discovery of the importance of Vitamin B12 for the algae and also for another project on the effects of some antibiotics on algae. You know, bleach green cultures with streptomycin.

Pat: Yeah!

Irma: And that was very early, in the very early days and it gave him a good start.

Pat: Yeah!

Irma: Gave him a good start.

Pat: Do you remember when you first came to the Lab, what was your sort of first impression when you arrived?

Irma: Certainly I had….. **Seymour** had all these kids that came in from high school, kids that he got in on Saturdays and Sundays. It was just sort of a beehive of activity, but it was all conducted in this little tiny room and it was amazing that anything could get done, particularly work, which was supposed to be sterile cultures.

Pat: Yes

Irma: But nothing ever got contaminated.

Pat: Yeah!

Irma: And everything worked out very well, and he raised a lot of youngsters who were very enthusiastic and worked very hard.

Pat: It was very serious. I mean that

Irma: They were not…

Pat: Playing?

Irma: They weren't playing…. They were really working hard.

Pat: Yes.

Irma: Many of them went on to be very serious scientists.

Agnes: He had a wonderful knack there.

Pat: For picking these people?

Irma: Yes.

Agnes: And for instilling enthusiasm in them too.

Irma: Many of them came from the Bronx High School of Science.

Elaine: I was just going to ask that where they came from?

Irma: Not all of them, but that was a main source.

Speaker: It's a very good school now. I presume it was then too.

Irma: And, they would stay until they were off to college or if they went to college in New York they would continue to work with us through their college years and often beyond. When I first came most of the graduate students, well they were not even graduate students, were war veterans who were working on the GI bill and they were older and quite serious and I think that they got an exceptional start there in the Lab.

Pat: Yeah! Why do think that was especially so?

Irma: There is something about the atmosphere that is hard to pin down, but **Seymour** put them to work right away and treated them as grownups and I think that we all felt that way, you know, we had to spend long hours if necessary and that...

Agnes: Yes.

Irma: ..and that was something of the spirit of the Labs at that time?

Elaine: The Labs must have been very small at that time.

Irma: Yes! it was very small.

Elaine: So, if you were working there, you would feel part of a very small group and may be you would feel that you were a real part of it?

Agnes: You did.

Irma: Well, we did not see very much of Dr. **Cooper's** group, but we were friendly.

Agnes: It was really, just amazing how the computer changed the whole world of the laboratory too and I am sure yours was effected as much.

Irma: No. Not that much by computer.

Agnes: No? Well all of the tests that we had to compile in the early days you know. We would have to get listeners who would go into the sound room and very often in the back room where I worked it was as if we had a display of grass skirts because all the tapes would be cutup into little pieces and arranged in test order and then spliced together. I do have them all hanging from masking tape along the wall.

Pat: Yes.

Irma: No, we did not have to, computerize at all even to the end.

Pat: Were your techniques very much the same then throughout the period you were working there?

Irma: Yes… Yes, they were and they were based on the assumption that we would try to get organisms free of any other organisms so that we could thereby discover their real nutritional needsFree of any bacteria or any other foreign bodies.

Pat: And, it was a technique that simply did not lend itself to new instrumentation, that would make the process simpler or faster.

Irma: It was really naturalist's work. We were old fashioned, I guess, in the sense that we were working with the organisms themselves and we would give the cultures to others who were working with more sophisticated systems. And, they often were unable to grow them themselves and they found great difficulty. Chemists or even biochemists often were just not handy at keeping these animals alive and...

Pat: There was a knack then?

Irma: It was partly an art as well as a science, and then I do not know. But we did uncover a lot of interesting things and then we started at the lowest end of the food chain and started to work up first with a single cell algae and then acinic algae and then acinic small copopods and invertebrates, which would be made bacteria-free and then grown on bacteria-free algae, so that you had your whole chain free of any…

Pat: So each part built on work that was done before, refining and clarifying.

Irma: Yeah! And working with the seaweed. And that was when we began to have a lot of Japanese students because, of course, they were very much interested in seaweed culture. And, then in the later years some of our students were interested in the practical aspects of aquaculture and when they left they went into, not fish farming, but the study of aquaculture and various aspects of aquaculture. One went out to California growing lobsters.

Pat: Can you remember the names of some of these people?

Irma: Douglas Conklin who was a graduate student of ours and he came here with us to New Haven and he has joined the group at Davis Marine Station and has been working with lobsters. And, another graduate student was **Louis Debrano**, he is now in Mississippi working with crayfish culture and they've got lots of big ponds where they cultivate the crayfish on artificial diets. Well that's what I can think of at the moment, but...

Pat: Can you recall any famous people visitors that came to the Labs that might lead to a story of interest or amusement?

Irma: Yes there were. I do not know about a story of amusement, but I do remember we had some of the old…. **Gilbert Smith,** as anyone who is familiar with the algae will remember **Gilbert Smith** as a delightful old gentleman who came in the first few years when I was there, but I do not have any particular stories to tell about him.

Pat: Were there a lot of visitors that went through the Lab?

Irma: There were a lot of visitors.

Pat: They would just drop in for a day or a few hours or something like that.

Irma: They would not. They did not come to work in the laboratories. And, I cannot think about anything. Generally visitors of short duration.

Pat: Agnes, how would you respond to that same question?

Agnes: Visitors? I remember many of them who again who's interests were apart from linguists and phoneticians like Dr. **Lilly** and with his...

Irma: Dolphins?

Agnes: Dolphins. Of course. He was working on a vocabulary to communicate with dolphins and he was positive that they communicated with him. I am sure you know that also. Dr. **Haskins** one day brought a marvelous elderly gentleman to us and I fail to remember his name, and he was a zoologist and he was working up a vocabulary for a chimpanzee that he had.

Pat: He felt he could understand chimpanzees.

Agnes: He was a dear little man, but…

Elaine: Those are **both** things that people are still working on?

Agnes: Oh! Yes. There is no question about it. And **Yerkes Laboratories,** remember, with their favorite chimps. In the early days, there was great communication between **Haskins Laboratories** and what they were doing. And the psychologist, **Lou Gerstman,** was fascinated and so was **Kathy.** We all were and there would be visitors from that laboratory and I don't know if you want to waste your tape on a little anecdote. I will not mention the visitor's name. He came from abroad and very excited to attend the tea that day and as many people as we could gather (these were not the usual daily visitors, but those who came in a few times a week) arrived that day to hear this man hold forth. It seems that he had made these speech simulators and what he had done was take thin pieces of what might have been a mixture of plastic and rubber. I do not know, but they were in the shape of an old fashioned key if you can imagine it, a straight length and then bulging out at the final tip and he might have two layers together or three. But what made it difficult for us all was when he came to his demonstration he had to put the long end in his mouth and by blowing through it he would simulate vowel sounds. And indeed, you know, he had a professional background. But although everyone was trying to be serious throughout this demonstration, it was almost impossible to maintain your composure because the sounds alone and then this peculiar device vibrating at the far end. And at that time, Dr. **Liberman** had a graduate student **Belver Griffith,** you may remember.

Pat: Yes I remember seeing the name on a paper.

Agnes: Man! He was a very tall and slender and very serious young man. He wore glasses and the glasses kept going further and further down his nose and his eyes opening wider all the time, you know, during his demonstration. Also, at that meeting we had André Malecot who was working at the Laboratory and he saved us all because at the end of the demonstration, he immediately went into story (like a shaggy dog story) about a man who went to an Inn and the electric light went out. The manager brought him up a candle, you know, and he lit the candle, but then when he was going to bed he could not blow out the candle because he had a speech defect. So, he called down to the manager. Well, it goes on and on and on and everyone who came up to blow out the candle had a different kind of speech defect. None was able to blow out the candle. So, it was so uproarious, the way he told the story, that it did immediately release all of us and no one ever laughed so hard at a story, as we did at that one. It was very funny and if only Jane (Gaitenby) were here. The following day I had a great deal of work to be finished in a certain time and I was a bit harried and Jane knew it. She and André Malecot disappeared for a little bit, and you know, what a T-square looks like. Well, that would have been a better demonstration of what these little plastic things looked like only shorter. All of a sudden the two of them appeared at the doorway that came near my desk and **Jane** was playing the T-square and André was making the sound effects and all seriousness went with the wind. But, there was always great interest and great excitement because each day there was a new discovery particularly when other visitors came from other universities and from abroad, to use this new tool and everyone just advanced it a little further and then with … The Playback just spoke in monotone and the objective was to get another device that would incorporate pitch and intonation. And you have heard about octopus, that was André Malecot's baby. And, he was the only one who was really fascinated with Octopus.

Pat: He worked with that a lot, did he?

Agnes: Yes. There were eight different events hence the name and if you turned the dials to each event according to his instructions of course (he had tested it out many times) you could reproduce speech with it too. And you could make changes in the speech pattern.

Pat: If you knew what the knobs did?

Agnes: If you had all the formant information that you needed – the consonant and vowel information. For instance, the word "gas" you could change to guess, you are changing the vowel of course and the stress on the "S". But that could all be done just by turning these dials, however, he was the only one who could and it died a slow death. But then later on when the vocoder was built and you've all heard the sentence "Alexander's an intelligent conversationalist". That was the first.

Pat: So all the familiar phrases were given new life then?

Agnes: With stress and intonation, yes, that's right.

Pat: Were you there at the time that the Charles Collingswood broadcast was done?

Agnes: Yes.

Pat: Is there anything that you remember about that?

Agnes: Do you remember that, Irma? When the laboratory was wired for sound – the Adventure film?

Irma: Is that the one that was played at the 50th anniversary party?

Agnes: Yes.

Irma: Well I had not seen him before that, but…

Agnes: There were cables running from 43rd Street up to the windows on the second floor. You have seen the film I am sure and Dr. Cooper...

Pat: Yes, it was all done live.

Agnes: …looking very serious behind the Playback because he had to hold something in place to maneuver. And in mid stream there were complications because the sound actually was going between our laboratory and their place up on Second Avenue or First Avenue in the 60s.

Pat: That's the CBS studios?

Agnes: Yes. We went out there to see the cameramen and that was fascinating too of course. But, Pierre again stole the show…. Just by removing his glasses, you know, he'd been acting for 50 years, all of his life, and he gave it that little touch. So that was a wonderful time.

Irma: Well, I don't think I was there, I do not remember anything of that….

Agnes: Really?

Irma: …excitement.

Pat: Because they cleared the decks for that?

Agnes: Yes, nothing else could happen until that was finished. And we repainted many of the original tapes just for that demonstration. Again, when you look at it now and see the sophisticated copies of the vocal tract in operation and the pictures that they take today with fiberoptics, they are so perfect. In those days, while you were able to take a picture of the glottis and the velum and everything you know in motion as certain sounds were being made, I remember Arthur, I guess when they went up to Rochester to take most of those.

Pat: You are thinking about the x-rays now?

Agnes: Yes that's right. And, I can still hear the clap together of those two sticks we tap when someone is suppose to say a vowel, an /a/, /e/, /i/, /o/, or /u/.

Pat: Yes. That was because the sound had to be separately synchronized with the film at a later point wasn't it?

Agnes: That's right, yes. Even the recording equipment was nothing when compared to what you have today. And, so you really did feel that you were in on the very beginning of things, because.

Pat: Trying things that had never been done before.

Agnes: The Playback itself, a good portion of it was held together with masking tape, you know?

Irma: I think that is one of the things that made Haskins exciting, isn't that right?

Agnes: That's right. And that's why the young people were so enthusiastic.

Irma: They were exploring new areas, which hadn't been really touched. I think that was true in biology as well because, although it was a much simpler sort of work that we were doing,…

Pat: You felt you did…

Irma: …we had like the first… Before Luigi and Seymour started working on marine organisms there was only one bacteria free marine algae in existence. And everybody used it for all kinds of experiments in physiology and biochemistry, and it wasn't a typical organism at all. So that when we started to develop a collection of a variety of organisms, it was really breaking new ground to be able to get organisms from different locations fresh water, marine, and I think, that was something that was quite exciting.

Pat: Did you find yourself in the situation of being expected (or asked) to be a sort of supplier of these organisms to other researchers?

Irma: Eventually, yes.

Pat: When did that come about?

Irma: Well, it grew up gradually, but then in later years a culture collection was developed at the University of Indiana and that was a help because it was a place where we could deposit cultures. And another one was developed in England at Cambridge and so we could send cultures to them and they would take care of them. But, in the early days there was not any such thing – except, perhaps, in Germany. One of our most famous visitors I guess was professor Ernst Pringsheim[96] from Germany who was the real pioneer in culturing algae and he visited us on a number of occasions. He was a real Herr Professor.

Pat: Everyone clicked their heels in his…

Irma: And although he had lived through the war in England, in Cambridge, he never really liked any other country but Germany. And he went back as soon as World War II was over. So, it's really strange. But, he was an innovator and a green thumb and he did do some very interesting culture work and I think he was inspiration for great many of the other people who worked in the same field.

Pat: At what point, did **Luigi** become involved… I think he founded a journal, didn't he, in the field?

Irma: Oh yes! The Journal of Phycology. Since we are this year in I think its volume 24… He was the editor for the first ten years. And, he really worked very hard to get started. There were many people who were not too enthusiastic about it and it started out as a rather small journal, but every year it has grown. And he did not want to be the editor at the beginning, but since his first choice was already doing a great deal of other editorial work, he finally decided that he would have to do it himself. It was very frustrating at the beginning. He had a lot of trouble with their printers at the beginning. One of the printers folded up while all the manuscripts were in the printer's hands and he had to get somebody to go down there and find him and get him to open up the printing establishment to get back the manuscripts and start all over again with a new printer. And that was quite a horrible experience. But, afterwards following this it has been thriving ever since. Luigi had it for ten years, and no other editor has stayed on that long. I think the following editors have stayed about three or four years a piece. But, it is going very well.

Pat: So it began around 1964 would that be right?

Irma: Let's see, Oh! I would say 24 years ago, when would that be…

Pat: Yes, that's 1964. Yeah!

Irma: I cannot remember exactly, but I think that is about probably that time. It might have been a few years earlier. I might be wrong about that 24 years.

[96] Ernst Georg Pringsheim. Author of "Pure Cultures of Algae: Their Preparation and Maintenance" Hafner, New York**:** 1964

Pat: So, prior to that point then there was not a recognized journal that would in fact accept the kind of research that you were doing.

Irma: No, they were scattered about various microbiological journals or botanical journals and there was nothing specifically at least in this country for algae and the society of phycologists was very small. I think there were about 300 members. And, now, I think there are about, I'm guessing, but I think about 1000. So, I think that it is really growing both in the Journal and in the Society. There was I think a journal in Sweden, but generally we published in the Journal of Botany or the Journal of Microbiology or Experimental Biology, some of them were more scattered. I think it was an accomplishment, because we managed to have one that was more or less oriented toward the algae. Now there are several others, but…

Pat: I suspect you were involved in some of the editorial work in the early days too?

Irma: Not very much only the most manual part of that work. We had a girl who did the copy editing and who was a professional. She would come and take the manuscripts home with her, bring them back. But no, I didn't really do any real editing. But there was a lot of work and it took lot of our time just receiving and sending out all the letters and stuff needed to review it. But it was interesting to see what came in as we were… Well, I think that they improved, I think that manuscripts improved as the years went by too as well as the size of the journal and we had more variety and we had more choice, we could be more selective.

Pat: Did anyone attempt to start a journal in the speech field from the Lab?

Agnes: From the Lab? No because they were already established.

Pat: Everybody was content to say…

Agnes: The Acoustical Society and the journals of Romance languages.

Irma: Our first part was mostly with fresh water organisms, then we migrated to the marine field and in the last years were mostly devoted to the marine organisms. I think that came about partly because we had so many Japanese students. We had about eight altogether Japanese regular students who came over to work with us and some of them became very important in Japan. Professor Tatewaki is now the director of the laboratory in Hokkaido. He's in Hokkaido and the others are just scattered around the country, but he's the one I know most about. And I think they really felt that they had gained a lot by their stay.

Pat: I'm curious, at the point when the Lab had to leave New York, was this a sad time or…?

Irma: It was, I think, for me it was, yes, I'd been a **New Yorker**, for most of my life, although I was perfectly willing to go. I knew that it would not be the same because we were dividing up into three parts. Instead of all being together in one place.

Agnes: It was not only sad and it was disconcerting and we weren't very sure that the right decision had been made until we became established again.

Pat: You felt that the Lab was insecure at that point having divided into three parts?

Irma: I didn't have that feeling. It was quite exciting to come to **Yale**, I thought that it was interesting to be in the Biology Department here, but we were divorced from all the others. Seymour had stayed in New York. Dr. Cooper's group were over on Crown Street and so...

Elaine: It did leave you kind of isolated.

Irma: Yes, yes, very much.

Irma: Of course, we did have a lovely laboratory. We had physically a much better laboratory.

Pat: At Yale.

Irma: We had colleagues in the biology department some of whom were interested in what we were doing, not too many, but a few and… but we missed Dr. Hutner's group because they were closer to what we were doing.

Elaine: They never worked with you in New York, they were always in separate locations?

Irma: No, they were just one flight up the stairs, different rooms, I mean, no, no.

Elaine: They stayed there, they didn't move at all.

Irma: No, they went to Pace University, but they stayed in New York City. We all had to evacuate. But I am glad that we came to New Haven.

Agnes: Well I am too. After a while everyone felt that way (glad that we stayed to live in …) And certainly the graduate students that we had were just grand.

Irma: Well, we didn't have many, but those that we did have were excellent.

Agnes: They too were so excited about everything that seemed to be new to them and they contributed a great deal.

Elaine: Did you still get Japanese students once you moved to New Haven?

Irma: No, we had visits of several months from the old ones and we did not get any new ones. We had the Yale students.

Elaine: Yale students yes.

Irma: Yes. And they were fine, we didn't have very many. It was just Dr. Provasoli alone. It wasn't a big section of the biology department. The biology department's main thrust was in other directions. We had plenty of space but not too much to… We could not attract too many students.

They were going into genetics and molecular biology. Ours was a very old-fashioned kind of work in a certain sense. It was studying the organism, which was very unfashionable at the time. You got to study one part not the whole beast. If you were in molecular biology, you are not studying in style. But they still need the organisms to do all that work. They have to have somebody identify them, grow them and keep them.

Elaine: If your work can be applied to growing things like prawns and lobsters, it's a very practical kind of thing.

Irma: We need them a lot. Microbiologists need them too. That is their starting material. Every time they think they won't need us, but they still do.

Pat: Have you been consulted at any time by the folks at Boothbay Harbor?

Agnes**:** Well over the last few years that we had grant here at Yale we actually had a grant with them, we worked with them. With Professor Yard up there and we were collecting some of the organisms from the open ocean which had never been cultivated and there are great great many of those and so we got about 150 different strains, which we isolated and those are all up there now and they are continuing to work with them. These are all, what you call, now a days call ultra plankton, they are less than 10 microns in size. In the old days most of them would go through the collecting nets and they were hardly ever collected**.** But it has become obvious that they are large proportion of the biomass of the ocean although they are so small and that they were never really bothered with in old days. So now there is much more attention paid to them and they are beginning to be studied individually instead of… Well, the oceanographers are also studying them, but the nutritionists are getting interested in them too.

Pat: Luigi would occasionally spent time at the marine biological lab on Cape Cod. Did you join him there at any time?

Irma: No, I didn't join him. But I went there one year when he was not there. I think it was a comfortable place to work in summer time and place to collect and there was some lab space available and it was a good place to meet colleagues who were coming and going over time as well as a good place to attend lectures and seminars and so forth. There was always something going on, but I went one summer and I enjoyed it and collected a few interesting things. I think I made the most of it. I was fortunate that I was given some space in the oceanographic, a situ that just happened to be free as they usually don't have any and I had a lab all to myself and went out collecting. I did manage to isolate a number of very interesting little beasts that summer. And, it is a good place to work because everybody is working hard there and yet you are free to come and go whenever you want. And, there everything is close together and there is a wonderful library there and it is a good place to spend the summer. I never took any other courses, but I think Luigi did that when he first came here. The first year he went, he took the course in invertebrate biology – invertebrate zoology. It is quite a famous course, it was a famous course, and I think it gave him a chance to meet people here and to see what the American teaching was like.

Pat: Luigi taught in the biology department as well I assume..

Irma: At Yale?

Pat: Yes. He taught a course there?

Irma: Well, yes. He did not want to become a regular professor. So, he agreed to become an adjunct professor. And, so he did not have to be involved in the committee meetings in the department. I helped out the labs a lot on the side. I wasn't officially part of the teaching, but I did help a lot in the labs. And, he had generally courses with several other members of the department – combined courses. And, I think they went very well. bu

Pat: Well, Agnes, I wonder going back now to the early 1950s, those were times when people like Katherine Harris and Arthur Abramson and so on joined the labs. Do you…?

Agnes: Well not, Arthur…

Pat: Arthur came later I suppose.

Agnes: Was a graduate student. But Kathy Harris and Lou Gerstman and I, we all started on the same day at Haskins Laboratory.

Pat: Really?

Agnes: Yes. The same day.

Pat: How did that happen?

Agnes: I haven't any idea how that happened, but… They of course, **Kathy** and **Lou Gerstmann** were doing the research. I was just the painter. Well, you know you have heard the story often enough that I had responded to an ad in the newspaper, The Sunday Times that said a laboratory technician with an art background, not odd, but art and that sounded very odd and unusual and that was… I was interviewed by **Dr. Cooper,** from day one from the interview the enthusiasm of every one was just so contagious. And, you did not understand what it was all about. It took usually months for any new person at the laboratory to have a clear picture of what was going on, but you knew that things were going to happen just by what was explained to you then. And after hours I did the drawings for Arthur's thesis and it was after that that he came to the laboratory. Now, that I think was probably, when was it? **1956**, **1955** may be…

Elaine: So, it was a kind of through you that he came…

Agnes**:** Oh, no. Dr. Cooper evidently knew Arthur at Columbia University. And you know how serious Arthur is about his work. It's part of his enjoyment of life. And his sense of humor when it he is working. Every bit of it is a serious matter. His thesis was on Thai and he had made beautiful spectrograms. The best spectrograms I think I have ever seen are in his paper at that time and it was just my task to mount them and get them all ready for printing ultimately and then Leigh Lisker came a few years after that. And, so, indeed, it was small and then later on Michael Studdert-Kennedy and the man from Texas, Peter MacNeilage.

Pat: Peter MacNeilage.

Agnes: And they were both teaching up at Radcliff. The little girls were swooning. Not all the time.

Elaine: And so Michael has been involved with the Labs for a long time.

Agnes: For a long time, yes. Not in the very early years, though that must have been in the 60s.

Pat: A person who has not been spoken of very much before is Paul Zahl. What do you remember of him?

Irma: He was the person who interviewed me when I first came to the job. But that was just a technicality, I think, because I was really working for Dr. Hutner at that moment. He came and went very frequently and his work was a little apart from ours.

Agnes: Initially, wasn't there some cancer research done?

Irma: Well, yes. But I did not have anything to do with it and I do not really know much about it.

Agnes: And, he went with the National Geographic.

Pat: Yes, I have a newspaper article, which describes some travels he made in South America.

Agnes: Oh, yes, in the rain forest

Pat: To collect some spiders called "four bites" or something?

Irma: I know that very early I once heard – I'm sure that Dr. Cooper or Dr. Haskins knows more about it – but he made a journey up…

Agnes: The Amazon?

Irma: No. Not the Amazon, but somewhere in those parts and wrote a book about it that was published.

Pat: Somewhere on the border between Columbia and Venezuala. The lost world, isn't that what they call it?

Agnes: For many years, Dr. Cooper had the pictures in his room, remember those that were taken in…

Irma: A doctor's office.

Agnes: In the rain forest.

Irma: Men with spears. Men from South America. But I am afraid I did not have too much to do with his work.

Pat: So, you weren't breeding spiders or nobody was breeding these creatures in the Lab.

Agnes: I would not have spent all those years at Haskins Laboratory if I'd thought that they were breeding spiders anywhere on the premises.

Irma: I think Dr. Zahl did some marine work with Dr. John McLaughlan who was one of our early students – Luigi's first student as a matter of fact. And they later worked on some symbiotic organisms, but that was not really part of the Haskins work I think. John went later to Florida and is still there.

Irma: Agnes, do you know anymore about Dr. Zahl's work?

Agnes: Not really. I only knew that he and Andrew Novack worked together didn't they. Or Andy worked for Paul Zahl on the guppies, changing the color.

Irma: A part of that was with Dr. **Haskins**.

Agnes: Was that with Dr. Haskins?

Irma: Because I had the guppies for a short time when I first came to the Lab, and they were Dr. Haskins' guppies. I didn't really…

Agnes: Of course, we all remember Dr. Haskins**,** you know, it was a short walk from Grand Central to Haskins Laboratories and very often you would see Dr. Haskins striding up with the milk pails for sea samples.

Irma: Fish.

Agnes: You know, the old milk pails.

Pat: Yes.

Irma: And they would be filled with fish….

Pat: Where did they come from?

Irma: South America. There were two parallel streams that he was studying in Trinidad. That's where they were. He was studying genetics of these two races that were growing in parallel streams in Trinidad.

Pat: Oh! I see.

Irma: And color, adaptations, and changes.

Pat: Where did he keep these specimens then?

Irma: In the Lab.

Pat: He kept them in the Lab.

Irma: Well, I also understand he kept some at home.

Agnes: Yes, the deadly ants and the butterflies.

Irma: But, he did keep good many in the Lab and I kept them for a little while, but that was not really what I wanted to do and Andy took over the guppies. I stuck to the microbes.

Pat: Yes, it's just occured to me. I am not sure now whether they weren't ants rather than spiders.

Irma: Ants I expect.

Agnes: Carnivorous ants.

Pat: They were poisonous I know.

Irma: Many of them are from Australia. They used to go periodically to Australia to collect the ants.

END OF REEL 8 PART 1

Irma: And that is why really…, When I arrived Dr. Haskins was in Australia and there were a few notes, but I really didn't know what I was supposed to do with all these fish.

Agnes: Was it in Australia? The Haskins' would always be taking off on these long trips and I remember that it was at one tea, I think after they returned and they told us that the people they were staying with had a television and that there was a battle going on between natives not too far from this residence, but that once a week this man would put the television so that the natives would come up very quietly on both sides and watch the television. I don't think they could hear anything and then after the show or whatever it was is was over, they would all disappear quietly and then resume their battling the following day. All these wonderful tales that they would tell we would hear every once in a while about their travels.

Irma: They traveled, and Dr. Zahl traveled, but Seymour never traveled anywhere.

Agnes: Well. That's true.

Irma: At least in his earlier days he was against it.

Pat: Was it a matter of principle or…?

Irma: I think it was in the beginning, I don't know. It didn't last forever because he does go places nowadays and…

Agnes: He probably did not want to be away from his work.

Irma: He didn't want to be away from his work for any time at all.

Elaine : Perhaps at that time they didn't have scientific meetings that had to take place in some exotic part of the world so that everybody would go.

Agnes: That is true.

Irma: They didn't tend to. They didn't have to. It was just a nice exciting place to work and it was...

Agnes: Remember? Dr. Haskins had…, I certainly don't know the name of the butterfly, but they were sent to him in a glass, there was just a glass on this portion of it, and you saw the cocoons on branches and this was in Mrs. Haskins' office for sometime and Andy would come down and check them everyday and finally he alerted us on the day that the chrysalis was going to open and it was just amazing. This was a butterfly that would just live for 24 hours, it would spread its beautiful wings and mate and then that was it.

Pat: Was it a big butterfly or…? So big?

Agnes: But it was fascinating, I never forgot that. To see that gradual unfurling you know because it was a cone shaped little cocoon.

Pat: Well, we could pause a moment here.

Pat: All right, we are back on the air.

Irma: Shall I start?

Pat: Irma, Yes you start.

Irma: Well, I got interested as an undergraduate in biology and….

Pat: That was where?

Irma: At Vassar and, well I was interested in sociology and microbiology and so, I became a major in public health. But then when I got to the point of entering graduate school they said that the only way for a woman to get into public health, at least in those days, was to go to a medical school. And I applied to a few schools and I did not get into any of them – the ones that I wanted to go to. So, I decided I would go into microbiology and I took **Cornell** as a masters student in microbiology and that was not particularly medical, but the head of our department, which was in the school of agriculture, was a specialist in Streptococci. So, when I left it was in the middle of the depression, so there weren't many jobs going, but I was fortunate enough to find a job in a small home for children who had rheumatic fever. It had quite a good research lab dealing with relationship between rheumatic fever and streptococci. And I stayed there four years until the war broke it up, there was nobody left to do anything. When I left, there wasn't even a doctor in the place. And so, I went to the Rockefeller Institute to work again with streptococci with Robeck Lansfield who was the dean of workers in that field. And we were working for the OSRD at the time – the office of scientific research and development – while we were preparing antiserum for diagnostic work in the army camps. And Squib was supposed to take it over when they learned how to do it, but they never did learn how to do it until the war was over and then the job was finished because it was a part-time job. And so I went to the Sloan-Kettering across the street and worked on antibiotics, a search for antibiotics, for chemotherapy and cancer research for two years with Albert Schatz who was Dr. Waksman's[97] assistant in the development of streptomycin and he was the one who introduced me to Haskins Laboratories.

Pat: Oh! Yes.

Irma: He said, Oh! Come down to see these people and then Seymour said he knew of a job in Amherst, which might interest me. So, we went down one day in a pouring rain and we got soaked when we got there. And Seymour was working with some bacteria at the time. But I had worked at Sloan-Kettering mostly with fungi and molds. It was really a screening program. We would prepare cultures and then they would be tested in animal tests to see if they were promising anticancer drugs. Of course they weren't, but it was something had to be done. And while I was at the Rockefeller I had some other possibilities. I could have kept on working in the same field with streptococci, but would have meant leaving New York. At that time I wasn't so sure I wanted to leave New York and then I could have worked with Rene Dubos[98] whom you may have heard of because he has become quite a public figure and he worked on tuberculosis and I wasn't sure and I have decided that I would rather go to another institution. Unless you were a PhD you really didn't have much chance to get anywhere in Rockefeller in those days. It was not a university in those days, it was an institute. And so I thought that Haskins sounded more adventurous, I guess. And that it might offer some more variety, although I didn't know anything about algae, I could see what Seymour was doing with this. It was technically very simple for me to get into that field. It uses the same sort of procedures and, well, that's really it.

Pat: That is how it happened. Good.

Irma: I never regreted it. I didn't expect to stay **40** years I must say… **39** actually. I was in my 40th year.

Agnes: Well, Agnes does not have any comparable academic background like that. She went to the Pratt Institute and that was an art and engineering school well known for its engineering school and its art school and I was going to be the next great American fashion designer. And I too got

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[97] Selman A. Waksman, born 1888, died 1973. Nobel laureate on Physiology and Medicine in 1952.
[98] Rene J. Dubos, born 1901, died 1982.. Author of the book "The White Plague: Tuberculosis, Man and Society".

out of school in the height of the depression and had to have a job because of things that happened in the family. And after a series of rather small and disappointing jobs, I became connected as the result of taking a course in pattern making. My teacher thought that I was wasting my time being a student and that I should be a teacher at the fashion institute in New York, but I again felt I didn't have the proper background and, when I mentioned the Singer Sewing Machine company, he made an appointment for me with the educational department head and I became a member of the educational department. In the beginning, in the Singer building, I sold machines in the shop downstairs and gave classes in sewing, dressmaking, and tailoring upstairs. And then they asked me to open up sewing rooms as they opened shops throughout Manhattan and when I had opened a sewing room up on East 86th Street I had a cross section of students in those classes from Park Avenue to Yorkville and some of the students who came from the Park Avenue and Gracey Square area loved the sewing courses and the people that they met and at that time I met a Mrs. McGill whose husband was a microbiologist and immunologist teaching at what was then called Holborn Hall in Brooklyn Heights, the Long Island College. And, he also had the Strain study for the influenza virus and the poliovirus. He worked with Jonas P. Salk[99]. So, he had the strain study center at Holborn Hall and his wife told me one day that Dr. McGill's secretary had to go to Texas with her husband, (he had received a position out there) and would I please speak to my students (as she knew they were business women), perhaps one of them would like to be a secretary to Dr. McGill and, knowing the man, I kiddingly said, "you know, I will tell them that I wish I could." And an hour later she called me up and said, "Agnes, were you serious" and I said, "of course not" because I'd never had a typing nor a shorthand lesson in my life. And, she said, that isn't the most important thing. He needs someone who can meet people (he can always get a stenographer) and run an inventory of all of the equipment needed for the Strain study center and for the classes. Well, in some ways it was a big mistake for me to go into something because Dr. McGill said, "Oh! any idiot can type." And, until this day I remember that remark painfully. I started, and of course it was not a question of not being able to type. Classes were beginning and I had to cut stencils and I was working on an electric typewriter staying late at night. And of course I had to go all the way down to Brooklyn, you know, this was way down in Brooklyn – Smith Street. So that it took me an hour and a half to travel each way plus a full day of work and the extra hours trying to teach myself to type. It was just at that time that the state decided to take over and the Long Island College Hospital eventually became the Down State Medical Center and it is now in a different area entirely. But, anyway, notice came through from the business office that anyone who had not been there for particular length of time would have to take a test to be rated. So, Dr. McGill said go, go and take the test. I'd been talking of not taking it at that point. So I went and they shot off the gun or whatever you did, and of course they were all manual typewriters everywhere and everyone was typing away. I could not even type my own name. Until this day I have had a mental block against typewriters. How many times did Dr. Cooper ask me during the course of thirty years, "Agnes, do you type?"

Elaine : I can understand. I have a mental block about typing too.

Agnes: And here was this wonderful man and I felt like cringing every time I heard his footsteps in the hall. And I thought that he certainly does not need this and things were going from bad to worse with me. And when he went to a convention in Geneva the last notice came through from the business office that it was now or else, you know. So, when Dr. McGill returned a few days

[99] Jonas Salk. Born 1914, died 1995. Developer of a successful vaccine against poliomyelitis.

later, I told him that I thought it would be better if I looked for another position and he agreed that it would be better for both of us. But he said take your time and be sure that you are happy with what ever you select. And I had gone to many places and I was honest with people, but remember that this was my first taste of working in an academic environment and I loved it. I certainly didn't ever want to go back to 7th Avenue, and the dress trade or Singer or anything of that sort and so just the weekend before I answered the ad for Haskins, I had been interviewed by a nurse up at New York Hospital and she said that she was starting a whole new program for graduate nurses. It had never been done before and when I told her my shortcomings, she said, "We will both be learning together", but that Sunday I looked through the New York Times once more and found a little squib that I told you about and went in and I was interviewed by first Anne Gallagher and then Dr. Cooper and I just felt that if I couldn't work at Haskins nothing else would equal the feeling that I had. But Dr. Cooper said he had many other people to interview and he would let me know. The next day he called and said that if I was interested, they would like to have me and that was how I started at Haskins.

During the war years, I had worked for Fairchild Instruments on aerial cameras and that was a place that was fairly close to where I lived – Kew Gardens. Their plant was in Richmond Hill quite adjacent to it and I had to take courses. I first took tests for the navy and I was always mechanically inclined, so I passed the navy test with a very high mark **98.6** or something like that and they wanted me. And then I had taken the test on the same week for Fairchild and they wanted me. Well, because of the proximity to my home, I accepted that and that was an education in itself because I worked in a very special department, which originally had been a repair department for aerial cameras. But these men were t all, you know, precision workers, and they were so intrigued to have a woman. Later on everything disintegrated as we all know in these war plants, but initially they taught me how to work at little bench lathes and I would make the shutter diaphragm for aerial cameras and that was the same sort of diaphragm that you have in a regular camera, but it would pinpoint with light the various instruments on the panel board in front of the pilot and those little shutters… the men taught me how to make the hammer head, which is no bigger than this and you could just hit that little brad or peg one blow and it had to be absolutely just one, and precise, before it was inspected then the shutter leaves, it was the stop I guess.

Pat: Oh! I see.

Agnes: Well I've had a very varied and interesting background, but I told Dr. Cooper during the interview you know that it was very varied and I was not sure that it was suitable for his work, but as you could see it, it worked out some how.

Pat: Well, thank you both, I think you've done enormously well you know.

Irma: We have done what you want.

Pat: You were so reticent about…..

Irma: I was really very uncertain about what you did want and I ___.

Agnes: You certainly have given.

END OF REEL 8 PART 2

This recording is being made on Tuesday, February 5, 1990. Raymond Huey[100] is the interviewee. I am interviewing that person. The purpose of the interview is to talk about the early days of the Lab. I suppose, in particular, these would be the late 1940s, would they?

Ray: Yeah. 1947, is when I started to work, in January of '47. At that time Dr. Zahl was secretary of the Lab, Dr. Cooper was the treasurer, and Dr. Haskins was the president, Mrs. Gallagher, Anne Gallagher was the office manager and the laboratory had two subsidiary corporations, one was wholly owned and was called the **National Photocolor Corporation**and they manufactured color cameras and pellicles (a part of the cameras), and the other corporation was partly owned by the Laboratories and partly owned by a man named **Nicholas Langhan.** He was an expert in color printing. The name of that corporation was **National Photocolor Carbros Incorporated**. I believe the Lab owned 60% and Mr. **Langhan** 40% – at least and the Lab had the controlling interest. At the time, I started to work for the Lab there were of course three complete sets of books, one for each corporation and the main work in the Laboratories at that time was attempting to build a guiding device for the blind and the money was furnished by the Committee on Sensory Devices, which I believe was set up by the Office of Scientific Research and Development after World War II, mainly for the benefit of veterans. We had blind subjects at that time which we were testing with these devices and we had one young man who was blind from birth and the device looked something like a flash light and I guess worked something like radar or sonar and they would set up an obstacle course and the subjects would try to go through the course without bumping into anything.

Pat: Where was this?

Ray: In the Laboratory.

Pat: Was it on 43rd Street?

Ray: On East 43rd street. I think the obstacle courses were on the fifth floor at that time which was not used for much of anything else. It was mostly vacant except for this and this. One fellow, whom I mentioned, who was blind from birth could do almost as well without the device as he could with it. He seemed to have his own built-in sonar system. It was very interesting to see how these blind people could do that. There was quite a number of people, I cannot recall how many were working on this one project and I am sure it was at least two-thirds of the total Lab expenditure at that time. Dr. Hutner was there and Dr. Zahl had his laboratory, they were biologists and even at that time Dr. Haskins used to do some research too at the Laboratory. They were all biologists. Dr. Cooper was a physicist running the big project that I have been talking about with the blind, although all the people took an interest in it to a degree. Then in the middle of 1948, the project was cut tremendously and we lost the contract. We had a six-month period to terminate it. So it meant laying off about two-thirds of the people that worked at the Lab and we went down considerably at that time. But later on then when the NIH and NSF started giving out grants, we worked our way back up. And then we also got a contract from the Army Signal Corp, for different work. Some of it was secret work and I cannot tell you except that it had to do with cryptology I

[100] Raymond C. Huey worked at the Laboratories up to his retirement in 1982. He died in 199*.

believe. Other than that, I don't know too much about that end of the work and all a lot was classified and we had to have our own private post office box, and only certain people were entitled to get mail from that box and I was not one of them. Well anyway, the Laboratory of course from then on gradually improved as we got more and more grants. Dr. Zahl left to work for the National Geographic. Dr. Haskins gradually phased out and became president of the Carnegie Institution of Washington and spent almost all his time down there and Dr. Cooper then became president. Mrs. Gallagher became treasurer and I think when Dr. Zahl left, Luigi Provasoli became the secretary. And that was the way it was still organized until we moved to New Haven. Then I became treasurer and Mrs. Gallagher retired – except that she did go down to Pace and work with Dr. Hurtner and help with the office work down there. Now I will get back to the subsidiary corporations. Both the corporations were managed by a man named **Ralph Wareham** and his job was only part-time since he was also the secretary of the American Society for Quality Control. I believe he was the secretary. He was an officer at least and he did a lot of work for that organization and he used to have to make trips but he managed the two subsidiaries. The Color Camera Corporation manufactured these cameras which were used mostly by commercial photographers for advertising purposes. Do you want me to describe how the camera works as best as I can? Well, the camera had one lens but instead of having one plate or film, so to speak, it had three plates. One directly, I think, behind the lens, and one off to each side. When the light went through the lens it also went through what was called a beam splitter or a pellicle which was a very thin membrane made from some sort of plastic material, sort of like Saran Wrap (I guess that is the nearest way to describe it) and the thing that made this camera better was the thinness of this pellicle because other cameras had used glass and the glass was thicker and distorted the rays of the light more. The pellicle split the beam without any great distortion. The first pellicle was at an angle so it would reflect part of the light, approximately one-third of the light would go on to the first plate and through a filter. I don't remember in which order the filters came but they were red, blue and I guess yellow were the primary colors and the first beam splitter would send a fraction of the light to the red filter. I am not sure of the order, but let us say the red filter, then the remaining two-thirds of the light would get through that pellicle and hit the next pellicle which would reflect half the remaining light through the second filter and plate, which could be yellow or blue and then the remaining light went on to the third plate. So the light was broken down into its primary colors by means of the pellicles and the filters. Then when the plates were developed, the coating, so to speak, indicated which color was meant to be which and then prints could be made from those in color and the commercial prints that were made at that time were called Carbro prints. And the Carbro prints were made by the **Carbro Corporation**. To get back to the **Camera Corporation** what caused the demise of that was that the Kodak Company came out with Kodachrome and Ektacolor film and that must have been in, well I guess the first came out in, 1948 or '49, around that time, because that's was when the demand for color cameras just became nothing. These color cameras sold for around $1500 and no one was going to buy a $1500 color camera when they could get another camera that would take prints on colored film and they could use those instead. So, the color camera business just went to pot, so to speak. But there was still a need for pellicles because they were being used for other purposes as well as in color cameras[101] I think they were used in bomb sights and I know we used to sell them to, let me think, some company used to buy quite a lot of them and they were used I'm pretty sure in bomb sights or some such use in military machines of different natures. And we had a man named John Ogden who made these pellicles and he was very good at it. Nobody liked to do that but somehow John

[101] National Photocolor Corp. , operating out of Mamaroneck, NY, was still supplying pellicles in January 2005.

did not mind it. I remember we had a man named Randy Novack who worked part-time in the Lab and he used to make some pellicles, but he hated to make pellicles, so John Ogden took it over 100%. But John began having personal problems and he began not showing up for work and he would be out for long periods of time. So Dr. Cooper decided to sell the Corporation and he found a man whom he commuted with on the train from Westport who was interested in it. So, he bought out the National Photocolor Corporation. He continued to make pellicles and we got a royalty I guess should say from him for sometime until I don't know whether there is anymore demand or whether pellicles are still made at this time or not. I have no idea what finally happened in that respect and since that happened of course, Ralph Wareham left I don't know whether he went to work full-time for quality control or… At least he left. When I first started there Nicholas Langham was the expert on making the Carbro prints and we had one man helping him named Al Beerline. There were just two of them and they were making a little money, but not a great deal. And then we got a new customer named Charles Kerlee who was quite an expert photographer and had some very good accounts, one of them was Cadillac. And he began buying all his prints from Carbro because he liked the quality and the company was making good profits because his was a very good account. So, then they put on some extra help in Carbros. They hired a young man named **Jerald Wind** and he worked for Carbros and then there was a woman who was taking the course in Carbro Printing and wanted to learn more about it and she came in and just worked for nothing, just to learn, her name was Ruth Davis. I don't believe Ruth evver went on the pay role and then there was a man named, I forget his first name, his last name was Hancock and he came in and sold a bill of goods, so to speak, about making Carbro prints of art work. And thought that it should be a good thing. So they started making prints of great works of art and it turned out there just wasn't a market for it. That part of the work turned out to be not profitable at all, in fact a losing proposition. And so Mr. Hancock had to go away. Then Kerlee got so big and didn't have enough room in his old studio, so he rented a larger space and then had more room than he needed. So he sublet some space and he got a tenant named Peterson, I believe, who was also making Carbro prints. So he told Mr. Langham that he had to give his business to his tenant. So, he lost the Kerlee account. So then Carbros just wasn't doing well at all and it was finally decided to close it down because it was losing money. So, **Nick Langham** and **Gerald Wind** decided to go into business for themselves and they took Ruth Davis with them and they set up their own laboratory called the **Carbro Prints Incorporated** and I don't know whether you are interested in history of that company or not.

Pat: Well, did it survive very long?.

Ray: Well, yes. Let me get back to the Camera Corporation and John Ogden before I go into the other. **John** had a lot of personal tragedies, sorry I am repeating myself.

Pat: Doesn't matter though.

Ray: Anyway, as I said before, Dr. Cooper met this man and he was interested and bought it out. So that closed National Photocolor and then we lost the Kerlee account and we went on for a short time after that, but not too long, and by that time the Labs had this large contract with the Signal Corp through the National Security Agency and began getting HIH grants and NSF grants and has comeback to where it is now. In 1948, when we lost the big contract, in the middle of the year, there just wasn't enough work for me to do. I was pretty much bored and I used to do crossword puzzles just to keep busy. So I went to talk to Mrs. Gallagher and Dr. Cooper and decided to see if I could find another job and work at the Lab part-time. So, they agreed to that. So, I found another job in Brooklyn, I was living in Brooklyn at that time. And I came in the Lab on Saturdays and Mrs. Gallagher, with some help from one of the other girls, kept the daily books, the cash books, the cash receipts, and cash disbursements book, paid the bills, and did all that work. I came in and kept the general ledger and did the monthly orders for them that they didn't know how to handle. That went on for several years, and it was in 1958 – no it was before 1958 – sometime around in the middle 50's after **Nick Langham** and **Gerald Wind** had this company of their own. They had had a CPA doing their work and it was too expensive. So they asked me if I could come in and do some of their office work, keep some of the books and so forth. So I started to work on a part-time basis for them and I was working on a part time basis for the Laboratory and I also had a full-time job. In 1958, it got to where I couldn't handle all the work, because both the Lab and **Langham** were getting busier and I quit my regular job. I worked then three days at the Laboratory and two days at Carbro Prints and sometimes I would have to go in even on Saturdays to work at one or both places and I was kept quite busy as it was.

Pat: I am sorry. At this point, Carbro Prints was totally separate from the Labs?

Ray: Yes. They had their own corporation. **Gerald Wind** and **Nicholas Langham** owned it 50**:**50. Ruth Davis was an employee. Well, they hung on by a mere thread for one or two years. They just barely could make ends meet and then they got more accounts and started to make some money and then the Carbro print process became outdated. Eastman Kodak developed what they called the dye transfer process for making color prints. The reason they did that was that they furnished the paper and the dyes to do it with. For Carbro prints**,** the dye material was furnished by McGraw Color Graph. I don't remember whether they did buy their paper from Kodak. Well at least Kodak developed this dye transfer process because it was a boon in their trade and it proved to be, I guess better than the Carbro process. But nobody at Carbro Prints knew how to make dye transfers. So**,** they were kind of stuck and they were concerned about it and finally they hired a man who knew dye transfer techniques and he taught them how to do it. So they quit doing Carbros and started doing dye transfers and then the name Carbro Prints Inc. didn't fit anymore. So they changed their name to **Langham and Wind Color Laboratories, Inc**. and they did very well after they really got into the dye transfer process. They had several good accounts with advertising agencies on Madison Avenue and with some art directors who did work for the advertising agencies and bought their prints from **Langham and Wind.** They made good money, so they were able to rent a much nicer bigger place on Madison Avenue, 420 Madison Avenue and they stayed there and a building down on the corner of 39th and Madison was for sale. They bought that building. They rented out the first floor, I think there was a drug store, but that later closed up and a restaurant opened. And in the back of the building the entrance to the elevator was actually on the 39th Street. As you went into the entrance, the elevator was straight ahead and on the right, was a little new stand where they sold newspapers, candy, and cigars and things like that. So they had tenants, they used the second and third floors and they rented out the fourth floor too. And things went very well with them for quite sometime, they opened an office in London and had a good business going in London. They sold a number of prints in **France** just through an agent there. Well, Mr. **Langham** was getting on in years and he was the salesman. He said, go out and bring in the business. Gerald Wind couldn't. He was the inside man. Unfortunately, he didn't have the best personality and disposition and was not suited, so to speak, to be a good salesman.

Mr. **Langham** became older and began to, I guess, develop Alzheimer's**.** In those days, we called it senility and he wasn't able to deal with much business and the business started to going down hill. At about that time, Haskins Laboratories was moving to Connecticut. So, it was up to me to make a decision. I had to either drop my work at **Langham and Wind** or drop my work at the Lab. I couldn't very well do both with them being such a distance apart. So Dr. Cooper talked to me. Mrs. Gallagher was approaching retirement age and they decided that the best thing was that she should retire except that she could work on at the Pace Lab. There was some work down there for another year or two until I guess she became 65 and could draw on her social security and pension. Then I became the treasurer and then started working full-time in Connecticut.

Pat: You continued on full-time until was it **1982**?

Ray: 1982, supposedly. But then I've been working part-time ever since. I don't think that's going to be necessary much longer, the way I have found Betty handling the books now. She's got things pretty well under control. There are some interesting side events. I don't know if you are interested in hearing all this or not, but I also had some other little accounts that I kept and some tax clients. Our accounting firm when I first started to work at the Lab was George W. Morren, Jr. He owned the whole firm himself as an individual proprietor. And he had a man named, Eugene Greene and a man named Degge[102]**.** I don't member his first name. And a man named Paul. He had 5 or 6 employees and they used to come in and audit our books at the end of each year. I remember the first year when I went to work there, they were auditing at that time in January. They had 2 or 3 auditors in there and they made all kinds of corrections because they just didn't have a bookkeeper who was competent and Mr. Greene was the main… (I never did meet George Morren, he was pretty well along in years) and Mr. Greene was the man in-charge of it**.** He was the one who interviewed me for the job. I answered an ad in the paper and it turned out to be his ad he was interviewing me for the job at the Lab and set me up to be interviewed there. And Dr. Zahl was the man who actually hired me with the consent of Mrs. Gallagher and Dr. Cooper of course, and Dr. Haskins too. Then, in later years and I can remember Mr. **Palm** used to be the main auditor, although **Mr. Greene** would come in and get things started. And at that time, they seemed to do a more thorough audit. They did things that the auditors haven't done in later years. I don't know, in the later years they have seemed to think that we know what we are doing. don't dig too much into detail and they know that we are audited too by the government. In the early days, we used to have auditors for each government agency. The Army sent in the auditor for the Signal Corps contract. The NSF send in their own auditor and the NIH send in their own auditor and I think we once had an Air Corps contract. Then the government decided that instead of having individual auditors for each agency to put us under auditors of the agency that we did most of our work for, which happened to be in the Army at that time. So then the Army and Navy and all the others became one agency, the Defense Contract Agency. So we were audited by the Defense Contract Agency up until this time when we will be shifting over to our own auditing firm, Myer Greene and Degge who will have to do our audit since the government no longer be coming in. But anyway, to get back to the auditing firm of Myer Greene and Degge, they had a client called Foresman Wolans. Mr. Foresman had died and Mrs. Forseman was a widow and Will Hyde, who was with the accounting firm, was her accountant. She and a woman named Mrs. Armstrong opened up the little sweater shop on the corner of Madison Avenue and, I believe, it was 59th Street.

[102] The firm of Myer Greene and Degge have been the Laboratories auditors for more than 30 years.

Mrs**.** Forseman of course asked we Will Hyde if he would do their accounting and keep their books, and do their tax work. And they (M,G&D) didn't really want to bother with it, so they recommended me to Mrs. Armstrong. She really ran the business. I don't believe I ever met Mrs. Forseman. If I did it was only on one or two occasions, but anyway Mrs. Armstrong and I had a meeting and I decided I would try it on a six month basis, and did it for a very small fee. And then, after the six months, I told her I couldn't continue on that basis, so she said "then charge whatever you have to" so, from then on, I charged according to the time I put in. Mrs. Forseman was in it mainly. They were both in it just to have something to do and Mrs. Forseman didn't need the money, well neither of them really needed the money, but Mrs. Forseman got tired of it. It was just a hobby and she no longer wanted to bother with it anymore. So she quit coming in and they had another woman help me. They weren't really making any money. It was something for them to do, you know, keep them busy. And in the meantime, there was a businessman who had worked for one of the big department stores who had lost his job and he found out about a department store in New Rochelle that was for sale and he talked these two women into going with him and buying that. So that ended the sweater shop. In the meantime, **Nicholas Langham**, I would like to tell you a bit about him because he was quite a remarkable individual in many respects. He was a white Russian. He was born and raised in Russia and he was, I think, a Major in White Russian Army and when the revolution started, of course, he was on the black list to be eliminated. But he escaped from Russia and he got into France and that is where he learned the Carbro print trade, in **France**. He came over to **United States** on a visitor's visa and never went back and he married. He was quite a tennis player. He loved to play tennis and he met a woman on the tennis courts named **Grace,** I don't remember her last name. But they got married, and she died sometime in, well it must have been around 1970 – before I came to Connecticut. Anyway she died and it was after that he started going downhill pretty fast. He had a lot of, I guess, dangers and thrilling experiences getting out of Russia and getting into France and finally to the United States. He had a friend named, Lev Sukachev and Lev Sukachev was also a white Russian and he was also married. He was also in the army and he got out of Russia into Albania and the king of Albania at that time was King Zognn **(**and I have seen that spelt Zogu). Anyway, he became somehow well acquainted with King Zognn, I believe, became good friends. And King Zognn was a good friend of King Farooq of Egypt. So Mr. Sukachev also got to know King Farooq of Egypt. Well, when World War II started, the Italians conquered Albania and captured all of their army including Lev Sukachev. So they took him and sent him off to the generals' school and put him in their own army as a general and he fought under Romel in North Africa and when, Patton and Bradley**,** I guess, defeated Romel in North Africa, the Italians surrendered in droves at that time and Lev Sukachev was one of them. He was taken as prisoner and stayed as a prisoner of war until the War was over**,** then he came to United States. He married a Polish woman, who spoke English pretty well, but he only spoke some English, and had a hard time with it. You could carry out a conversation with him, but he didn't have the best command of the English language. Anyway he invented a scan and he sold it to, I believe, it was Westinghouse for quite a large sum of money. So, he started up his own little laboratory and he was working with metal coating. He'd coat flowers and leaves with metal and other things like that. He formed his own company and so he needed a bookkeeper, and Nick Langhan told him about me so I went up there and talked to him and his wife and started keeping their books and doing their taxes. And things went well with him until he had a stroke which left his total right side paralyzed. He was not able to carry on much after that. But he still did a little work and then they decided to move down to New Jersey near a town called Jackson, which happened to be the area where a lot of white Russians had settled and

I used to have to go down there at least once or twice just mainly to do their taxes because they weren't really doing any business anymore. As long as they kept the corporation, they had to file tax returns. It was interesting to hear all about their lives. So one thing just led to another. It's funny the things that can happen to you during your lifetime, one situation develops into something else.

Pat: Do you recall when the companies were acquired, Carbro and the other?

Ray: I cannot tell you what year. The Lab, I think….

Pat: And were they making any money at the time you first made contact with the company?

Ray: Well, National Photocolor Corporation was making some money at that time. They were selling some cameras and pellicles, Carbro was just about breaking even, may be making a little, but not much. Then when Kodak color film came out and the color camera business died, they had to lay off everyone except, as I mentioned, John Ogden and myself keeping the books, just part-time for them. They did hire at the time when they were making money. They hired a couple technicians to try to develop new materials. I remember, one was working on plastics trying to develop a plastic filter that would attract (through static electricity, I guess) would attract dust and be used in air conditioners and such. But they were never too successful with that. There was another project that was being worked on and I cannot remember what it was, but it did not turn out that they were able to develop anything else to make a profit on. And I cannot remember just what date the company was sold. It was sometime in the 50s. Then, I talked about this sweater shop, Mrs. Armstrong was the active partner and she then gave me her personal income taxes to do and after they closed the sweater shop and I still did her taxes for several years. She lived in an apartment in Manhattan and she decided to buy a Co-op apartment in Sutton Place south, I believe, and she bought this apartment, but each year she had to pay, (most of her income was from investments) and she had to pay Federal and New York State and City taxes. She owned a house in Massachusetts on Nantucket Island and she used to go up there and spend her summers. She had a daughter and a son. Her son was not living with her at the time. He was on his own. Her daughter when I first knew her lived with her mother and they would go to Nantucket every summer. She got tired paying so much tax and so she decided to sell her place in New York and then live in Nantucket the year round. But then she got involved with Massachusetts State tax but they weren't as high as the New York City and State combined. And I still did her taxes until the time of her death, which was, I think, two years ago. And I used to get some interesting letters. I got another side account through her too. She became the treasurer of the Nantucket Garden Club, so she used to send me their books to audit. And so I audited the Nantucket Garden Club books and prepared their (IRS) nine-nineties for two or three years until some other woman became treasurer and that was the end of that. She used to write letters to me about how could she save taxes. Well, I couldn't see anyway she could save taxes unless she wanted to buy tax free bonds and stocks. But then she would be getting lower returns and it was questionable whether she could really save any money. She would pay less tax, but she would not have as much income. But, I remember she used to sometimes…, Oh yes! she had some mortgages in New Jersey and she was having a lot of trouble collecting on those. The man was a sort of a dead beat. So she used to have to get lawyers to sue him. She would write me a letter and explain what was happening. She would say I got a bill from the lawyer, X dollars, I don't remember the amount. Highway robbery she would say. She was a nice woman and she had quite a nice sense of humor. I did her returns many times. She died and that of course ended that. I don't know, now do you have any questions?

Pat: I suppose that I would like to go back for a moment to the beginning in the late 40s. I wonder whether you can perhaps provide any thumbnail sketches of some of the characters that you have known that have figured in the Lab's history, then and later on?

Ray: Well, at that time when I started there, Dr. Hutner and his laboratory, Dr. Zahl he had his laboratory there and as I mentioned before, Dr. Haskins also did some research at that time. They were all (most all of them) I believe were on the fourth floor (may be some on the third floor) and Dr. Cooper's gang worked mostly on the fourth and fifth floors. I guess it was a year or so after I started there that Dr. Provasoli came into the picture, I don't know, somehow he met Dr. Haskins and we got acquainted and he was I think he was teaching at St. Francis College in Brooklyn. He was a marine biologist and I guess somehow Dr. Haskins got interested in his work and he started to work at the Lab and set up his own lab on the third floor. Well, Dr. Hutner had a woman named Irma Pintner working for him and when Luigi came, Irma went over to Luigi's lab and worked with him and each year they would get a man from Japan to work with him. Japan, as you know was very interested in marine biology and some of these fellows that would come over from Japan could not talk English. Irma Pintner finally learned to converse with them enough, you know, that they could work together. But, if I went in there and had a question, it was usually Irma who had to interpret for me or Mrs. Gallagher, or almost anyone else. But Luigi used to have, when one of the Japanese men would get ready to go back and the new one to come, he would have a little party and he would always have Saki**,** which I never liked. And then there was another man named John McLaughlin who was a Ph.D. and he had his own grants with…, At one time, he and Luigi had a grant together as co-PIs, but they didn't get along very well, so when that grant year ended they each got their own separate labs and went their own ways. I don't remember whether John McLaughlin was still at the Lab when we moved to New Haven, but anyway he didn't come to New Haven. He might have left the Lab before that, I am not sure. And then, Dr. Hutner had his crew. Each year he would, in the summer time, like to bring in a lot of high school kids that were brilliant kids and let them work in his lab and we always called them Hutner's chickens. It was hassle to keep track of the payroll as to who got paid and how much and so forth, just as it now is with the part-timers here. He had two dishwashers both colored women, one was Eleanor Bean (they were sisters) and the other was Marjorie Brown and they worked in his laboratory washing his glassware and keeping his equipment sterile. They worked with him until he moved to Pace and I think Marjorie Brown's health failed and forced her to quit, but Eleanor Bean worked quite a long time at Pace before she left. Well Hutner had a lot of people, he had Art Zalsky and Herman Baker, who both became Ph.D.s like Levendowski and, I don't know, several others whose names I just cannot recall at this time. Oh, what was his name? I wish I could remember his name. Stuart? After he left the Lab, he worked for Sloan-Kettering. I cannot recall his name now and I still keep in touch with him, and exchange ideas and things. And then he started teaching a course at Fordham University Dr. Hutner did. He had two students there that were, I guess, rather brilliant. One was Cy Bacchi and the other was Oakhall. What was her first name? Anyway, he hired them to work at the Lab and they finally became full-time, and of course Cy Bacchi still remains the main man down at the Pace Lab. The woman got married and her husband lived in, I think, Kentucky somewhere, so she is down there. She had a baby of course. She worked up until the time, she got married. Of course I have lost track now of who else they had. They had a lot of people go through Pace Lab just like they did in New York. Students, part-time college students, and I guess some high school students. And Dr. Hutner still works without pay down there. He draws no salary and his wife was a scientist also. She is a Ph.D. and was in charge of her own laboratory in Columbia University. She is a microbiologist and she is retired now and I don't know (she was doing lot of consultant work) and I don't know whether she still does or not. I did their personal income taxes for many years, but I gave that up. It got to where I dreaded that trip to New York and it was always a hassle to get all the information, so I did it up until last year. Last year they hired a new accountant. Some more of the characters were, well for Dr. Zahl he had this Andy Novack. Andy was from Bar Harbor, Maine and he was always talking about Bar Harbor and he developed serious cancer and died and we never knew whether there was anything in his life that caused it. Anyway he got cancer and died and he was never replaced because Dr. Zahl had gone anyway.

Pat: So Dr. Zahl was not there very long?

Ray: Well Dr. Zahl was there a few years after I was there. I cannot remember what year he left. He was there well for sometime. He had a couple of NIH grants I think – along with Seymour Hutner, Luigi, and McLaughlin. And then, of course, Dr. Cooper got the A-40. And then at one time he got a grant from the Carnegie Foundation of New York. I don't know, it was a sizable amount spread over three to four years. I think that was through the influence of Dr. Haskins. The Carnegie Foundation was somehow connected with the Carnegie Institution of Washington. And Dr. Hutner had Tom Borris as one of his main scientists and we had a blind man named Dr. Whitcher, I believe was his name. But he had to leave when we lost the CSD contract (the Committee on Sensory Devices contract), as did most of Dr. Cooper's staff. But he kept on Bill Winter, Dave Zeichner and three to four others. And when we moved to Connecticut, the only one… Oh! Richard Music was hired later on. And when we came to Connecticut, David Zeichner and Oh! Bill Scully was another one as a programmer – when we got the computer in then there were some programmers hired. George Scholes used to do programming and Bill Scully and we had two or three others. I can't remember their names any more. When we moved up here, lets see, George Scholes came, Bill Scully came, David Zeichner came. Bill Leonard did not come and Bill died shortly after that. He had a heart attack. He had bypass surgery and he was supposedly coming along alright, but he was driving his car, I think his wife was with him. He just died at the wheel and ran into the back end of a bus – just died like. Let's see, Dave Zeichner, of course you knew him quite well. He stayed on here sometime after I retired and he retired. Oh!, Eric Andreason who was the janitor, he came up here. Agnes McKeon who was the graphic artist, if that is appropriate name, whatever. And most of the scientific people came up here on a part-time basis as they always had done in New York, Fredi Berti, Kathy Harris, Alvin Liberman, of course – being closer to home it was handier for him. And from Pennsylvania, we had Leigh Lisker, and we had others, Nancy McGarr. They started in later, I guess, after we moved up here.

Pat: Alice had just joined hadn't she?

Ray: Alice came to us from an employment agency, not as an employee, but as a temporary worker (a Kelley girl) and she proved to be such a valuable worker that we hired her, and of course we then had to pay the employment agency an agency fee. And of course she didn't move to Connecticut, she commuted from New York for several years until after her mother, who lived in

Princeton, died. One reason she stayed in New York was because of her mother. She went to see her mother every weekend, I think, or almost every weekend. But after her mother died, then she moved to Connecticut. The bookkeeper that worked for us in New York, Rona Pollock was her name and she came up here and did some work after we moved. But she didn't want to move to Connecticut, so we had to hire a new bookkeeper. Getting back to New York again, I think, I named most of Dr. Cooper's staff who at least became (or who were not) laid off at the time of the big cut. John Borst worked until we moved to Connecticut and he retired. I guess he died recently, or is he still living? Do you know?

Pat: I don't know. I know that Paul Zahl died recently.

Ray: I know Paul Zahl died a couple of years ago, And I just found out the other day that Edith Cooper died.

Pat: Yes.

Ray: I didn't know that. Anyway I think John Borst died**.** Luigi had a woman who also washed his glassware and her name was Valentine Wilverstein and she is still living and Marjorie Brown I think is still living and she must be, oh, in her 90s by now and I don't know whether Eleanor Bean is still living or not, but she went off our health plan and went on the Pace Health Plan, so we lost contact with her. And of course and Irma in their lab decided to retire and close their lab in **Yale**. Luigi and his wife have since moved to Italy and Irma was living in North Haven, I don't know whether she is still living in North Haven or not.

Pat: She was when I last had contact with her.

Ray: Is there anyone else I should mention. Mrs. Gallagher incidentally who was a office manager when I started to work there and became treasurer later, she lived in New York close by and then she was divorced and she had one son named Charles and after he got through college, he was in the Marine Corps and then he got married and he lived in Montana a while and then he moved to Missouri, and as far as I know he is still living in or near St. Louis. She went to live with him, but first when she left she went to live with her sister in Ann Arbor. Her sister was a professor in Michigan University in Ann Arbor. And Anne Gallagher went to live with her and then I don't know whether her sister died or whether Anne just got to the point where she couldn't take care of herself. Anyway she went down and lived with her son until she died. She was a very nice woman to work with I think. I mean she never gave me hard time, she was not the bossy type at all. When I started to work for the Lab I had worked for a woman who was just the opposite. My first job in New York only lasted three months because of the characters as I call them. The woman and her brother ran the business and they never wanted anyone to know what they were doing and you couldn't keep books because you couldn't get information. Anyway that job did not last long and when Mr. Greene told me I would have a woman for a boss, I was little dubious about that. But he assured me that Mrs. Gallagher was not that type of person and it proved to be, of course, that she wasn't. And when we got more grants and I wasn't working full time at the Lab we had to hire another bookkeeper and then…. (Tape runs out).

END OF REEL 9