

MSK oral history: At Haskins Labs, 300 George St

CAF: All right. This is February 19, 2013. Present are Carol Fowler, Donald Shankweiler and Michael Studdert-Kennedy.

So you have the list of questions. Shall we start at the beginning about how you got to Haskins in the first place.

MSK: Yes, well. Shall I go into a personal detail?

DPS: Sure.

CAF: Absolutely, Yeah.

MSK: Well in the beginning way before this...I should say that I regard my arriving at Haskins as the luckiest event of my life. And it completely shaped it. But at any rate. I originally went into psychology because I thought I was going to become an analyst. I'd read a lot of Freud, and that's what I thought I was going to do. And so I enrolled in the clinical program at Columbia in 1957 when I was 30. I had wasted my 20s in various wasteful activities in Rome and elsewhere. They would give me an assistantship in the experimental department for my first year but they wouldn't for my clinical. So the idea was that I'd spend the first year in experimental and then switch to clinical. However, after taking a course in abnormal psychology and a number of experimental courses I realized that I couldn't conceivably become a clinical psychologist because I couldn't believe a word they said half the time. And so I became an experimentalist and it was a heavily behaviorist department, and I did rats and pigeons. And then one summer by a great fluke, I needed a summer job and Bill McGill who was later the advisor of Dom Massaro incidently, asked me to be his assistant. He was an acoustician and a mathematical psychologist, and he was doing very interesting, rather involved work on reaction time in which he was collecting reaction times and getting distributions. And these formed things galled gamma distributions, which were actually a convolution of several normal distributions. And he thought that he could....One of the parameters of the gamma distribution is the number of normal distributions that had been combined. And so he thought that he could work out from the reaction time the number of normal distributions produced by waiting time at synapses. So that he could count the number of synapses that had been passed between input and output.

CAF: Wow!

----- (Richard Crane came in to help DPS get on network)

MSK:.....mathematically inclined and statistically inclined. And this was a heavily Skinnerian department. At any rate so the long and the short of it is that I ended up doing...I had intended to do a behaviorist thesis of some kind on secondary reinforcement I think or something. And...but the pain of learning what I had to learn to keep up with Bill McGill was so great that I thought that I can't possibly waste all that agony. I'm just going to do a thesis in acoustics, and so I did.

CAF: What? Wait a second. So he was using sound as....So Bill McGill was using sound as a way of collecting reaction times?

MSK: Yes, he was an acoustician. And so I did as a thesis an experiment....

This is all somewhat relevant to how they picked me up at Haskins. A thesis on very brief bursts of sound. It was a temporal integration study, using 20, 40, 80, 160, 320

millisecond snippets of sound formed by square waves or exponentially declining waves. And the big idea essentially was to see whether reaction time...no not reaction time, whether threshold response to these various tones that I was using varied as a function of their spectrum. In other words, was it the case that, when you reduced a sound and so made it less hearable, it actually didn't get as much less hearable as you would expect, because there'd been a scatter of frequencies into where the reduction...into other areas to which you might be more sensitive. So the long and the short of it was, does the ear do a frequency analysis of 20 ms bursts of sound or does it not? Is it integrated completely and unanalyzed. And it turned out that it was.

CAF: Was doing a frequency analysis.

MSK: Yes. So you can...the ear was sensitive to the scatter of frequencies that came about. Anyway so that was what I did. And I think that that, quite unreasonably, persuaded Al and Frank that I knew some acoustics. And it *was* rather elaborate, and I'd have to do a fourier analysis by hand, and, I don't know, a whole host of ghastly things that I hoped never to do again and never have.

3:07 second file

But at any rate, now why was I...how did I come to be here in any case. Well, I had spent my first year after graduate school, I taught at Bowdoin for a year. And I was married then, but my wife died soon after child birth. She committed suicide actually, during a post partum depression. And so I found myself at the end of my first year with no wife and a six month old child. So I came down to New York and after various difficulties got a job teaching at Barnard, where amusingly the offering salary in 1961 for an assistant professor was \$6000, which I bargained them up to \$7000. But that still was...

CAF: Angering Tom Tighe if I remember

MSK: Yes, infuriating Tom Tighe. Right exactly. Who was there and who I think had only been paid \$7000 although he'd been there two or three years and so when he heard this he was infuriated. Quite reasonably.

DPS: So that was '61

MSK: That was '61. But that still didn't give me really enough money because I needed a full time nursemaid. And so I went and talked to Bill McGill. And he said: Well, I don't know, but you might be able to find something at this odd place downtown on 43rd St. Haskins. Which he knew about because he and Kathy Harris and been colleagues at Harvard. Bill McGill had come to Columbia from Lincoln Labs at MIT as a matter of fact. And so he sent me off to see Kathy. And so I came and I saw Kathy and I saw Al and I saw Frank. And I left them with this appalling thesis of mine called "The effect of waveform on the probability of hearing a short tone near threshold." And they offered me a job! So I came into the Lab, and I frankly hadn't the beginnings of an idea what they were doing when they said: We're studying speech perception. My first thought was: Well you just hear it, don't you? I mean it seemed absolutely absurd that you would be studying speech perception. So the first thing I did... In answer to a question which we'll come to again probably, but: It was absolutely wonderful and amazing the utter freedom. Nobody ever said: Go and do this. Ever.

CAF: Now was Caryl Haskins...He was not present any more then?

MSK: No. He wasn't.

CAF: Because he says in a few places, his ideal for the scientist is the undirected scientist. I think, you know, you bring in really good people and then just let them do what they think they ought to do. And I feel like that's the way Haskins was run for a long time.

MSK: It is. It's exactly what was done. Of course, what that presumes is that there is going to be a community, because obviously each person isn't going to work on their own. They're going to talk with other people. But that's exactly what was done. And so nobody every told me to do anything. They just said: Here's the situation and gave me some papers to read. And so I spent...I was teaching full time at Barnard. And so I actually spent two days a week at the Lab. A whole day whatever it was, I forget, and two half days, I came in the afternoon.

DPS: I remember you came ...were there part time when I came in '65.

MSK: Right. You came in '65. Right. When I first came, the Lab was...you (DPS) remember, you (CAF) don't know probably, had the top three floors of a sort of warehouse building, which had a tie factory down below it. The third floor was speech, the fourth floor was biology, and the fifth floor was speech again.

DPS: And the machine shop

MSK: The machine room Right. And I was on the 5th floor with a fellow called Jarvis Bastian who was the co-editor with Sol Saporta of *Psycholinguistics*. But was delinquent, and so it came out eventually under Sol Saporta's name. Jarvis Bastian had the peculiar habit of leaving punctually everyday at 5:00.

8:51

Whatever was going on, he arrived at 9 o'clock in the morning, and he left at 5 'clock in the afternoon.

CAF: Now, he's the one that left and went to California and studied dolphins or whales or something like that.

MSK: Yes, yes he did. And many years later I saw him in California when I went to give a talk, I forget it must have been at Davis, I think. But my talk began at 4 o'clock. And at 5 o'clock, Jarvis got up and left.

CAF: He left! What a stinker!

DPS: But he did some stuff...

MSK: He'd done some stuff with [speech]...

DPS: some experiments with slit-split that Al was very upset that never got published. He did it with Kathy.

MSK: Oh. I'm not surprised.

DPS: Were you involved with that?

MSK: No. I was not. However, slit-split was all the rage when I arrived. And one of the things that I spent an enormous amount of time on during my first year was trying to construct a machine that would enable you to do slit-split by the method of adjustment. So that you could stop and start the tape recorder and adjust the interval...

CAF: Whoa! Do you mean the period of silence?

MSK: Yes, introducing a period of silence. Well, the thing was that my impression of psychology at that point was that it's really a matter of physics. You have to learn how to make all this machinery. And put that between the person and...

CAF: Now would that be your behaviorist training?

Isn't that kind of [...]

MSK: Yes, everyone built their own damn stuff, and I did for my thesis. I built my own sound room even. Absurd as it seems. Yes, so what I really did then initially was read all the papers. I just read everything that had been published, I guess, by the Lab. And I worked on slit-...this device for slit-split which eventually got made but never got used. Arthur Abramson used to call it the feasibility machine, because the question was: Could it be done? I showed clearly it could be done, and then we didn't use it. And that was very wasteful, of course. I[t] couldn't possibly go on these days I don't think. But in the meantime, the other things I got involved with were categorical perception, and the only thing I had to add there was, I guess, was reaction time. And I did a couple of studies of reaction time along a ba da ga continuum, I think. And maybe a vowel continuum, I'm not sure.

CAF: In discrimination as well as identification?

MSK: Both.

CAF: So I'm asking that because Pisoni eventually showed that you...in terms of discrimination, even if you would say that two things were the same, if you called them the same and measured reaction time, you were slower if they were physically different than if they weren't.

MSK: Yes

CAF: Did you get that finding before David?

MSK: No. I didn't. No, I'm sure those data were there, but I probably did not see them correctly.

CAF: Were you looking at distributions a la McGill?

MSK: Was I looking at what?

CAF: Distributions? Why were you looking at reaction times?

MSK: Yes, I was. The main thing that I remember is that the reaction times came out as you'd expect shorter within categories, and longer between.[CAF: I think he means: short RTs to identify a segment if it is within the category; long if it is near the boundary] And vice versa for the discrimination function. But I don't think that I did find that David Pisoni finding.

CAF: Shucks.

MSK: However,... But those papers got reported in the Acoustical Society. They never came out...I never wrote them up as papers. They are as abstracts.

But it was because of that that...Oh, let's turn to something else: Looking at this [CAF: list of questions]...How was my work funded in the early years because, yes, I became very interested in the whole reading machine issue, and particularly in the difficulty of finding a code that would substitute for speech. And I fundamentally didn't believe that. And so one of the things that I did...

DPS: You didn't believe that there...

MSK: I thought that it must be possible to have a code.

CAF: To come up with an acoustic alphabet

MSK: Yes, if you did it properly. I was utterly naïve, and I don't know why AI was not more severe with me, because he had already gone through Wuhzi and all these odd

mixtures of speech and nonspeech that...And they hadn't worked. But again it 's part of the policy of humoring people. And saying, well if he wants to do it, let him do it.

DPS: I got the impression that Al wanted to believe that too. That he...

CAF: Certainly at first.

15:03

MSK: Yes, I think he did. 'Cause Al of course had sound behaviorist training.

DPS: Yeah, he wanted to believe that. Three years didn't they work with that hypothesis?

MSK: Oh yes, they did.. And they had...The reason that I'm surprised that he didn't put up more of an objection was simply that he had had those three years and had gotten to a point where ...They were using this code Wuhzi which was made up of pieces of speech.

CAF: Was it? I thought it was spelling... coming up with fluent speech that just wasn't English. It wasn't that? It was concatenating speech?

MSK: Yes, it was concatenating things. I think.

CAF: I don't know. I never saw it described.

[In Cooper, Gaitenby, Nye '84:" Wuhzi) was devised. It was based on a transliteration of written English which preserved the phonetic patterns of words and so rnade the new language pronounceable."

Lieberman's intro to his 96 book: Thus, vowels were converted into vowels, stops into stops, fricatives into fricatives, etc., so the syllable structure remained unchanged, hence easy for our human reading machine to pronounce. We called this new language 'Wuhzi', in honor of one of the transposed words.]

MSK: Yes. But it hadn't been successful even though it was incorporating some speech spectra into it.

DPS: Did they invert it, have it go backwards or something? Wasn't that part of the trick...

MSK: Maybe yes, yes, to retain the speech like quality. Yes

And of course there was speed spelling; I mean you know trying to read out the spelling immensely rapidly.

CAF: Yes, apparently that was really obnoxious. According to Frank. Awful to listen to.

MSK: So my little code was one....George Miller of course was around in those days, "The Magical number 7" had come out. So multidimensionality as the solution was always there. And so what I had were 8 sounds which were either I think 8 different frequencies, or 4 frequencies and two durations, or two frequencies, two durations and two amplitudes. And people did learn the multidimensional ones more easily. But it took them a long time. And then they certainly couldn't follow them, of course, when they were given a string of them.

DPS: I thought that one of the conclusions of that early work was that these multidimensional codes did not perform reliably better than Morse [Code].

MSK: Yes. Yes.

DPS: And Morse is the crudest code you could imagine. I mean...

MSK: Yes. Right. That's definitely the case as far as I know. There was nothing ever faster than Morse. So that was another fruitless step that I followed during my first year. Building the feasibility machine for split-slit, the RT studies for categorical

perception and this nonspeech stuff. By which point I had become a convert as it were. I mean I now believed that speech was definitely odd in some way. And I still believe that. And I believe it fundamentally for that very reason that you can't substitute anything for it. It's amazing to me that not so many years ago, shortly before he died, I had a long conversation with Dennis Klatt about this. And Dennis was absolutely convinced that if you worked hard enough you could find a code that would substitute.

CAF: Wow!

MSK: I was amazed!

CAF: Me too. Really amazed.

MSK: Anyway so, I think that that ...that work was funded in my early years. That's right.. ..I never knew! But as far as I thought, it was funded by the Veterans Administration.

19:20

DPS: Originally the speech..the reading machine was funded by money from a Committee on...

MSK: Carnegie.

CAF: No. Committee on Sensory Disorders, not disorders. .CSD anyway [Devices]

DPS: Sensory aids... It was a committee of the National defense research, of that World War II arm of the National Science Foundation.

MSK: Under Vannevar Bush that was.

DPS: Yeah. And so the Labs were somehow fingered for making guidance devices and the reading machine by this committee that includedKarl Lashley was one of its members.

MSK: How interesting. I didn't know that.

DPS: I don't know exactly what the connection was. Which of Caryl Haskins' friends of ...pushed the...

MSK: Right. But the Carnegie Foundation gave money, didn't it?

DPS: A little bit later. Yeah. A little bit later. So all of those two sources came before the VA contract.

MSK: I see. Well, the fact is that Frank ran a very paternalistic lab. And I hadn't the faintest idea. It was never mentioned. Nobody discussed the grants.

DPS: Yeah. I remember being brought in when Hogan and maybe Ignatius came up from NSA to...and we'd just sit around a table and chat, mainly. That was a site visit.

MSK: How interesting. Yes.

DPS: But I never thought at all about how the Lab was supported until we worked on the first A40, which happened not too many months after I arrived.

MSK: '65, Yes.

DPS: That would be at the end of '65. You must have worked on that too.

MSK: Yes. Yes.

CAF: So do you remember that, both of you, writing the first A40?

DPS: Yeah. I remember that. Because that was a big deal.

MSK: Yes, I do dimly remember it. But I don't remember what... I'm sure I that I must have put in some stuff on categorical perception.

CAF: I wonder if we have that [copy of first A40]

MSK: But had we started laterality, you and I?

DPS: We had. I think...Or, I think so.

MSK: In time for that we had...So that puts...Right.

DPS: I've long ago thrown away my copy of that, so I don't know what the proposal looked like. I would be interesting to see it.

CAF: Yeah. There's something that I want to know about that time period, and Al is not around. In 1957, he wrote what I would say was the first presentation of his motor theory. And it was very much from a behaviorist perspective of associating articulations with acoustic products and acquired distinctiveness and similarity and stuff. And then in 1967, he wrote that beautiful [Perception of the speech code], you guys, wrote that beautiful '67 paper that was utterly different theoretically. It had become biological. And it seems to me that it has to be the case that Chomsky's arriving on the scene and George Miller becoming a cognitive scientist type had to have an impact on the thinking that changed between '57 and '67. But I can't think of anywhere where Al writes about the revolution in his thinking about speech. So do you guys remember what went on at the Laboratories in that time?

MSK: Well, I can say a couple of things about that. The first is that I had become aware of Chomsky while I was still a student at Columbia. Because I was in this heavily behaviorist department in psychology. And one...In August, 1959, 23:36

I was walking across campus with Bill McGill, who as I said was sort of ...something of an outcast in the behaviorist department. And he said: "You know there's a paper reviewing Skinner's *Verbal Behavior* in the March issue of *Language*, a fellow called Chomsky. You might be interested by it." Well, I happened to have given a paper that spring on *Verbal Behavior*. I'd read the book and what I essentially said in my paper was: It's brilliant. I don't know why he did it. It's obvious that it had to be that way. I mean he just supplied simple behaviorist principles. And they work obviously.

CAF: Bad job.

MSK: So off I go and find March 1959 *Language*. And there's that damn review. And that utterly altered my life. Because it now absolved me from the guilt of having abandoned behaviorism for acoustics and all that stuff. And it made me into a clear anti behaviorist. But the interesting thing is that, now jumping around a bit, when Harlan Lane's first papers came out. Before his critique, I think there were two or three earlier papers, Cross and Lane or Cross, Lane and Shepard.

CAF: OK

MSK: Which put forward an anti motor theory account and a behaviorist account. And I immediately began to write a critique of their papers. And...which was never published. And I talked about it with Al. And I think it was '63 that Al and Frank were in California at the Behavioral Studies place [Center for Advanced Study, Stanford]...I'm just going to look up and see the date of the Cross, Lane, and Shepard. Oh no, it says it's '65. I thought it was earlier.

DPS: The research might have been done earlier.

MSK: Yes, maybe there was a .maybe there was a...No. I was going to say maybe there was an earlier...a paper given at the Acoustical Society or something like that. I forget. Because it was certainly before its critique came out. But at any rate...And Al and Frank were in California. And I remember writing, having exchanged several letters with Al. And one of the things I remember saying in it. Giving in the draft that

I sent of this comment on Harlan was that it just depends on whether you accept Chomsky or Skinner. And the odd thing is that I don't think that Al *had* read about Chomsky then. That was '63. But I think that that's when he did. When he was out in California. And I think that did alter his view.

CAF: OK. I kind of have a sense of Al as not a very scholarly guy. But he really was an...

MSK: Not a very...?

CAF: Scholarly, I mean he was an original thinker. And he doesn't owe much of his intellectual thought processes or development to anybody.

MSK: Yes. I think that's true. He hated to read.

CAF: Yeah, I think so.

DPS: He often said that he didn't learn anything in graduate school that was of any use to him in his later career. I mean he often said this.

MSK: Yes, yes, right.

CAF: But this must...but reading Chomsky must have had an impact on him then.

MSK: Yes. I think it was that year that he was in California probably, with Frank. And I think it's true that...When I look...I just re-read Perception of the speech which hadn't done for about forty years. I was astonished as a matter of fact that it's still not unreasonable.

CAF: It's a beautiful paper, in my opinion. One of the best papers ever written.

DPS: I would give it to people to read. I think it's held up pretty well.

CAF: So can...do you guys remember what you each contributed vs Al vs Frank?

DPS: No. No. I don't

MSK: Not really. There are several things I can say there. One is, for sure, the whole drive of the paper was Al's.

CAF: Yeah.

DPS: Yeah. No doubt.

MSK: But it's also the case though that, it was a....when Harlan's critique came out, Al was very rattled, and it became my task, because I'd already engaged it, to reply to this. And so I began to draft replies of one kind or another. But it then became obvious as we talked about this that this was an utterly negative and useless thing to be doing. To showing that he was wrong about categorical perception. What had to be done was something much more extensive to show that speech was indeed special and odd in the ways it was. And so that...it was...Perception of the speech code was really the positive response to Lane's critique. It was to show that...and Lane get's mentioned at the very end of the paper and dismissed quite peremptorily. So that was why there was that delay.

CAF: But then there's another delay. Right? Didn't you reply to Lane in a paper that came out in Psych Review in 1970?

MSK: Yes. Yes. That was the...an actual reply to Lane. And that, well that was just.... There had been a break in my period at the Lab, because I left Barnard in '65 and went to teach at... in Puerto Rico for a year. But I didn't stay there very long. I stayed there a year. And then I came back to the U.. And I...But during that year, which was '65 to '66. Is that right? No, it was '66 to '67. During that year, I was out of Haskins. Although I was winding up things. And so the paper that we were working...when we were working on it, it was before I'd left, I guess. Because, in due course, I came

back when I moved from Puerto Rico to the University of Pennsylvania and used to come in from Philadelphia.

CAF: With Leigh [Lisker]?

MSK: With Leigh. Right. And I remember you [DPS] and I working on the paper together or going over things in it.

DPS: Yeah. There was some reference to laterality in that paper.

MSK: Yes.

CAF: Yes, there is, that's right.

DPS: And some to reading too.

CAF: Really? I completely don't remember that.

DPS: In the '67 paper, yeah.

MSK: Well, is the reading work really mentioned there?

32:26

DPS: Well, I think the ..

MSK Claim that it's easy to read but hard...hard to read but easy to

DPS: Easy to hear and hard to read. Yeah that point was already in the '67 paper.

MSK: Yes, that was made yes. But you must have undoubtedly played...put the stuff in about laterality, or we did it together probably. But I'm sure it was you, because it was you who introduced laterality to the lab.

DPS: That 's one of the, I think, one of the reasons that Al got... maybe got interested in the biological basis of language, because he was interested in this laterality stuff.

MSK: Yes, I think that's true.

DPS: And I was also going to follow up when we talking about Chomsky a minute ago. When was Mark Liberman studying at MIT with Chomsky?

CAF: In the '70s, mid 70s.

DPS: That was later?

CAF: His degree was 1975.

DPS: Oh so that's not an explanation.

MSK: Oh. No.

CAF: Oh no. Mark was a twinkle in his father's eye then. Well in 1967, he was probably in his teens or....

DPS: Wait a minute. He was...That was his SD...

MSK: Oh '68 was when he was a Maoist at Harvard.

DPS: Yeah Students for democratic

CAF: Excuse me?

DPS: SDA: Students for Democratic Action [SDS?]

CAF: But wait a minute...Oh Mark was?

DPS: Mark got tossed out of Harvard.

MSK: Without a degree.

CAF: No kidding! I didn't know that.

MSK: Yes and not only was he tossed out but he then opted to go into the army and to become a helicopter repairman.

DPS: That's right.

CAF: My gosh! How interesting!

MSK: He was in Dien Bien Phu

DPS: Al was terrified that he was going...people in his army unit were going to discover his...

CAF: His leanings

DPS: his student activities

MSK: And they did!

DPS: Did they?

MSK: And they sent ...he was sent back

DPS: He was sent back eventually

CAF: Well, that's a good thing.

MSK: Because... particularly because there was a lot of fragging of officers going on. That is, of soldiers blowing up their own officers on our side.

And he was in a unit where that had been happening. And so...So I heard. I don't know where or how I heard that. Probably from Al, I guess, I don't know.

DPS: Al was very worried, I know, when Mark was in Vietnam.

MSK: Right

CAF: Wow. So I did not know that about Mark's history.

But anyway, Mark was not a reason why he [Al] became a Chomskian type.

DPS: Right, OK

MSK: I should say though as far as what we contributed to that paper I remember it as more than anything else it was editorial, it was going over all this prose and checking on references and so on.

DPS: Yeah. Al didn't have the patience for that sort of thing.

CAF: Right, right.

MSK: Right, and making sure that we were referring to the right paper at the right time and so on and so on. But also the biological aspect such as it is, was something that I would... spent a lot of time with Al on because I had originally gone into psychology because I thought of it as a branch of biology .I thought of Freud as fundamentally the biological psychologist.

CAF: Yep. Sure.

MSK: And then when I found Skinner, and Skinner' was obviously, his whole system was modeled on Darwinian theory: with extinction, reinforcement and all that stuff.

CAF: I never thought of Skinner that way.

MSK: They are sort of utterly parallel. And so I absolutely fell for it as being biological. And so the laterality thing when it came along was the first time I saw a glimmer of light, when Don appeared with this. Because it seemed to me: There's going to be a way by which we can get to the biology of this. And that was ...Given the sort of mire of psychophysics in which I was currently thriving, it seemed a rather distant undertaking.

CAF: Right, right.

MSK: One thing that I do remember is that the remark accounting for the speed of speech as due to the distribution of the effort across several articulators such that you could have only one acting at a given time, etc. was Frank.

CAF: Really!

MSK: I remember Frank laying that out very clearly at one ...one day. Incidentally, a thing here that we haven't mentioned which is important because...Don was on the third floor sharing an office with Al, weren't you?

DPS: Yeah. And John Borst.

MSK: And John Borst. Was he there in that office too? My god! There was a cantankerous old bugger.

DPS: He was. He was the only, I would say, negative feature of my early time at Haskins.

MSK: He had absolute scorn for anybody...for psychologists. And he was always asking you telling questions about engineering aspects of speech synthesis that he knew you wouldn't be able to reply to.

CAF: So was he an engineer?

MSK: Yes. And he was crucial in building the first...

DPS: The Pattern Playback

MSK: The Pattern Playback. Right

38:57

DPS When I got there, I couldn't discover that he that he was doing anything. I mean if he was, I didn't ...

MSK: I'm not sure that I knew that though. Although he did do stuff on...with Alexander upstairs. On the vocoder. But what I was going to say was that: There we were on the fifth floor, I was, with Jarvis and later Peter MacNeilage. And Kathy Harris. And the whole EMG operation was up there on the fifth floor. And Leigh and Arthur and Don and Al and Frank and Caryl if ever he came in and who else, were on the third...a few others, were on the third floor. So the way we all met was that we had tea every day at 4 o'clock. And that was an absolutely crucial aspect of making this community. Because that's how I got to know the people who came to visit the Lab.

DPS: But that custom ended after I got there.

MSK: Oh, did it?

DPS: And I don't know why. But it did. Yeah.

MSK. Oh. Oh. That *is* peculiar, because it seems to me that it was absolutely crucial. That's how I met Sven [Ohman] and Bjorn [Lindblom] and Kirsten [Hadding-Koch]..

..

Right, because the kind of bullpen arrangement of the 270 Crown, which was so facilitative of interaction, wasn't present in the old Haskins.

CAF: Yeah. That's interesting. I think that even just having people on two different floors can be an enormous deterrent to communications.

MSK: Yes, yes.

CAF: So when we designed the Labs here [300 George], we intentionally kept the arrangement of... bullpen arrangement.

MSK: Frank also had a highly, not only a paternalistic view: He ran the Lab. He also had a class view of the Lab. He thought that if you had a PhD you were a different kind of person. And those people should be treated differently than people who didn't have PhDs. So the workshop people who were, of course, absolutely crucial...like Bill Winter, and Dave Zeichner, and Dave Speaker and there were a bunch of them

CAF: How about Jane Gaitenby. She didn't have a PhD.

MSK: No. She didn't have a PhD. But she was an honorary PhD I guess in Frank's estimation. She was... But he clearly had this kind of...It's not snobbism exactly, but this class distinction. And, when I became President briefly, in the first year there was an issue that came up, which was to do with TIAA-Cref and whether you could..TIAA Cref now made it available to people to take their own...the money they had accumulated and use it themselves in some way or other. But it had to be authorized by the institution that gave the TIAA Cref money. And it was never authorized at City University. And Frank was happy to authorize it for the PhD people, but not for...

CAF: Not for the stupid people.

DPS: Did those people...Did the workshop people come to tea?

Bill Winter and Zeichner and the others?

MSK: No they didn't.

43:25

DPS: OK.

CAF: Hah. That really surprises me.

DPS: Even at the Cambridge psychology lab, the workshop people came to tea.

MSK: They did.

DPS: Well, yeah, I was a post doc there before I came to Haskins.

MSK: So you had the familiar experience of this teatime event.

DPS: Yeah I did, but not at Haskins.

MSK: Right. That's strange. Yeah.

CAF: So another question I had was about Peter MacNeilage. How did he come to Haskins? And I'm also curious about his leaving. Whether it had to do with the results of the EMG research.

MSK: Yeah, it definitely did. Yeah. Peter, I don't know how he came. He just blew in from Montreal.

CAF: He was a Hebb student, somebody said.

DPS: Yeah, we was.

MSK: He was a what?

CAF: Donald Hebb?

DPS: D. O. Hebb was his advisor at McGill?

MSK: Yeah. Yes, He was. Yes. And Peter appeared, and he was...went to work with Kathy immediately. And that was the beginning of the EMG project, I think. And that was first time a computer was introduced into Laboratory, I think. It was a computer that was used in some way for ...analyzing the data.

CAF: collecting the data maybe, yeah.

MSK: But, Peter was also ...He was very strictly empirical, and he was very interested in speech errors, even then. And he...and I, because I was teaching at Barnard, then... so he came and was took on teaching one course at Barnard. So he taught the, I forget, physiological psychology probably course at Barnard. And he would have his students, many if not all of whom were touch typists, I mean typed and didn't... properly.

DPS: Well, did you provide an entry for him at Barnard?

MSK: Yeah, I think I suggested to Ed Manenta {?} or what's his name Utes (?), my awful chairman up there.

DPS: Who was he? Your chairman?

MSK: A man called Utes.

DPS: Oh yeah, I've heard of him.

MSK: Absolutely horrible man who read detective stories all day and had fierce animosity to anybody who did any research, because he didn't do any. Though his thesis had been mentioned in a footnote of the *Behavior of Organisms* and so that was his claim to fame. And anyway...

CAF: So you were saying Peter had his students...

MSK: Peter had this great interest in speech errors. And the fact that typing errors were different.

CAF: Did he write about that?

MSK: Yes he did.

CAF: Sounds familiar

MSK: Several times actually, and one of the things that he did...When he collected data was to have his students who did automatic touch typing, never to correct errors, to leave them in there and then they could type on and redo it correctly. So he got a sizable collection of errors of that kind. And then he also used to have people, I don't quite know why that worked, but he had a typewriter built there at the Lab, which had an endless roll of paper. And as you typed, the thing rolled up each time, up instead of going across,

CAF: Oh my god! How awful.

MSK: And you touch typed this. I what it gave you was timing. It told you how long had elapsed between the strokes.

CAF: Good for him! Wow. Ahead of his time.

MSK: So it was very elegant, nice work. But the long and short of it is that he and..he worked with DeClerk

CAF: Yeah. MacNeilage and DeClerk is a '69 paper.

MSK: He and Tom Rootes used to work together, I think. But it became clear that they were not finding invariance [in the EMG speech work] that had been hoped for. And I don't... I know that that was a source of fierce argument and friction between Peter and Al and Peter and Kathy.

CAF: Peter and Kathy? I think of Kathy as completely nontheoretical and wouldn't care

MSK: Kathy...

DPS: Kathy, we talked to; she denies that, I think, that she was a fan of the invariance hypothesis..

CAF: Yeah., But I think she also said that she didn't work with Peter, which I thought I knew wasn't right.

DPS: Yeah, She said that she did not work with Peter. And, so...But he came to work with her. So...

MSK: Well, he did come to work with her.

CAF: I think her memory was just failing.

DPS: Yeah, I'm sure that's true[that Peter came to work with Kathy] but they must have parted company fairly early, but, or maybe Kathy is just forg

MSK: Maybe

CAF: But Al was reluctant to...I know in the '67 paper, if you read about that research, there is some acknowledgement that there is context sensitivity, but there's still the hope that there's still going to be some core of invariance.
MSK: Yes, That's because Ladefoged's crew...Vicki Fromkin had done some work in which she had not found invariance for labials.

CAF: Yep. So you think that Al was reluctant to give up the idea that there was invariance. And Peter realized that there wasn't.
MSK: I don't think that...I think it was Peter who got out. I mean I don't think he was thrown out.

DPS: No, no he wasn't ...I'm sure he wasn't.

MSK: No. No no. It's simply though that he became uncomfortable. And two things there are that... I remember once...in some review paper or other I refer to Peter's 1970 paper, and Kathy reproved me.

CAF: For even referring to it?

MSK: Yes, she said: I don't think you needed to have quoted ...It was very mild. Something like: I don't think you really needed to have quoted him like that, you know.

50:41

CAF: It's interesting, 'cause she said at some point when we were talking about this research on EMG and invariance with her: "We got zapped" she said. And she meant by Peter and she meant in that paper. At least that's what she said she meant. So I looked at the paper just after that, so about three weeks ago. And he was just very straightforward. He wasn't being mean-spirited or angry or anything. He was just reporting the context sensitivity that they had found. But she may remember it as him sort of attacking [...]

MSK: Right. Well feelings ran higher on the invariance side than they did on the other I think.

DPS: What's the other side? [Peter: context-sensitivity in motor commands]

MSK: So I don't think that Peter felt...He felt uncomfortable. Peter's a very mild mannered--or was then--man.

CAF: He was just reporting the data the way he saw it, and Al wasn't happy.

MSK: Right.

CAF: And maybe Kathy wasn't happy.

MSK: Right. And so Peter decided to go. And he got a job at...

DPS: First at Berkeley.

MSK: At Berkeley.

DPS: And then at Texas.

MSK: Then Texas, right. He is a sworn enemy of Bill Wang at Berkeley, because Wang didn't honor Peter's research for whatever reason, I don't know.

DPS: He was hostile at Berkeley or at least wasn't able to stay.

MSK: He wasn't given tenure.

DPS: Yeah. Yeah.

CAF: Peter MacNeilage was not given tenure! Would he have been in the Linguistics Department there? Is that how he would have talked to Wang?

MSK: That's possible.

DPS: I thought he was in the Speech and Hearing Department

MSK: He was what?

DPS: In the Speech and Hearing Department

CAF: Speech and Hearing

MSK: Perhaps he was. I just don't know. I wasn't...Look, but Peter's departure from the Lab...Although I'd been quite a buddy of his, 'cause we shared an office, must have coincided with my absence, when I went off to Puerto Rico, and I think maybe...I think...I know he was still there when I left, but I think that he perhaps had gone by the time I came back from [to?] University of Pennsylvania.

DPS: I think I overlapped with him for less than a couple of years, but I can't remember exactly when he left.

MSK: Many years later when Peter came to the meeting in honor of Al, he spoke very kindly and respectfully of Al, saying that he had come to recognize that Al had certain qualities of imagination and so on, and so he sort of vaguely forgave the...But nonetheless that animosity to the Lab contaminated the whole Texas operation...

54:03

CAF: Did it?

DPS: Really? You think that's the...

MSK: Diehl.

DPS: Yeah. Diehl and

MSK: and Bjorn [Lindblom]

CAF: So I'm curious about Bjorn. I mean he strikes me as an extremely gentle-hearted, kind person, and...Did he share the animosity toward the Laboratories of Peter and Randy and Sussman and those?

MSK: No. He didn't

CAF: That's what I thought

MSK: Though I have to say though that, as you all know, we three were collaborating on a book and we had a falling out. And the falling out came about when I was down in Texas with them, and I was arguing for a gestural phonology. And they both got angry.

CAF: Huh. They got angry.

MSK: And it was I who then said: We can't do this anymore. Because I've never been angry with anybody, ever ...Well I've often been angry with people, but never been angry with people over a scientific matter. And they were...And later I had a long email exchange with Peter in which he was appallingly vituperative of me.

CAF: Of you!

MSK: And my...And the whole...

CAF: Because you stuck it out with Haskins Labs?

MSK: No. Because I believed this nonsense about gestures and it was so obvious, and I don't know. It was very, very unpleasant, and I haven't talked with him since. And that was two or three years ago.

CAF: Wow. So that would have been...

MSK: That was quite late. That was...That squabbling with Peter and I was in 2005, 2006.

CAF: OK. OK.

MSK: We were on perfectly good terms up until then.

DPS: Well, wait a minute. Hang on. I thought the falling out came when you all were at the Center [for Advanced Study, Stanford] together

CAF: At Stanford

DPS: At Stanford

MSK: No, we survived that year very well together.

DPS: OK.

MSK: It was following that. When we didn't finish the book. We each wrote...We had three chapters to write, and we each wrote one. And so there were two to go. And...

CAF: You realized it wasn't going to be coherent document, it wouldn't be integratable.

MSK: Right. Right. And that was when that became clear when we began to meet in different places at various times, and that's when the falling out came down after that.

56:58

CAF: But still earlier than 2005 probably.

MSK: Oh yes. Oh yes.

DPS: Well would it have been in the middle 90s or earlier. No in the 80s even?

CAF: So Articulatory Phonology was developed probably starting about '83, '84 or '85 something like that. I mean the first big paper was published in '85 or '86. So probably around...

MSK: Right. And I wrote a paper while I was at the Center in '85-6 in which I referred to Articulatory Phonology, and at that point, Peter and Bjorn accepted this as a plausible line of...But then as things advanced, that ceased to be, for whatever reason. But Bjorn is, indeed, a very gentle and unangry person. He would not... though he has particular people that he dislikes.

DPS: He spoke very, very well of you [MSK].

MSK: Of who?

DPS: Of *you* in Stockholm when I was there in that sabbatic visit... visitor. I needed a place. Basically Ruth had a..

MSK: When was that?

DPS: That was '98. Ruth had a visiting professorship.

MSK: Oh yes, I remember that. Right.

DPS: And I had to do something. I wrote to Bjorn, because he was the only person in Stockholm that I knew, and he was very gracious to me. But he was extremely...

MSK: Well we were very good buddies.

DPS: All of his comments were very friendly and respectful of you.

MSK: Well, we got on very well, and continue to. Although we broke up over the book, of course, it really was Peter and me...

DPS: It was clear that he was not a fan of the motor theory

MSK: Oh yeah

CAF: No

DPS: and that he was pushing this vocal tract favored location notion that he had.

MSK: That he and Rene Carre...

DPS: Yeah.

MSK: Yes

DPS: And he was getting to be buddies with Kluender at this point

MSK: With who?

CAF: Keith Kluender

MSK: Keith Kluender. Right. And Diehl. Of course, he's always had a very acoustic direction.

Well since we're on Bjorn and you asked how did we all come ...Bjorn and Sven [Ohman] I think probably it was '63 came to spend time at the Labs. But I'm not to sure how it worked out, but I think they'd had six months or more at MIT with Ken [Stevens]. Then they came down here for an additional span of time. And I think that then Bjorn came and spent some later time here in '64-5 maybe.

DPS: Who would have sent them to Haskins?

MSK: Oh Gunnar [Fant]. And Ken. I mean Al was already going...I mean, he was already collaborating with Ken, and he and Ken went to Gunnar Fant's lab.

DPS: Yeah. I think that Ken was mentioned very early after I arrived. In my earliest talks with Al, after I arrived, I think he mentioned Ken.

MSK: Yes, because he had a sort of analysis of synthesis theory that was appealing to...

CAF: Right, with Halle, right? Morris Halle's name was on it, right?

MSK: Yes. Right.. And so they came down and it's really through the teatime meetings that I got to know those two [Bjorn, Sven]. Yes, I should say that, although I clearly had become a partisan of Al's beliefs, I also was skeptical of them in some degree. And in fact, I was particularly over vowels, because it always seemed to me absurd to separate them off and behave as though you could somehow have one half of the brain that did the vowels...

DPS: Well I had some heresies there too that offended Al. I mean...

MSK: I'm sure.

CAF: Well, how did he reconcile that? It always seems an odd thing that...

DPS: I went off and did work with Pinky Strange and Bob Verbrugge [on vowels]

MSK: Yes. Of course. Yes

CAF: But in Al's mind, how did that fit together?

MSK: Well, it never did! I mean it seems to me, Don's laterality, coming to the Lab with that technique, I remember being thrilled thinking: Ah! Now we're going to show Al's wrong, because the vowels are obviously going to be left hemisphere just like the consonants. So imagine my chagrin.

DPS: Yes. Right, exactly. But then was it Chris Darwin or somebody who showed that if you shortened the vowels enough

CAF: And made them similar enough

DPS: You get more right ear advantage

MSK: Yes, right. Exactly. But still that was..And so that was another reason though that I was drawn to Bjorn, because he was interested in VCV syllables, I mean syllables where... Did I say VCV, I don't mean that

CAF: like wiwa, wuwa

MSK: He was interested in CVC syllables. Wiwa , wuwa. Those. And so that...So ...and I was interested by that because of course that's movement. So vowels as movement was always something that I liked the idea of. So that's how I came to do that study with Bjorn, to collect some data at Barnard to go with his Swedish data. And that's how I fell in with Bjorn.

CAF: So I haven't read that paper [Lindblom and Studdert-Kennedy On the role of formant transitions in vowel recognition. JASA, 1967] in a really long time. I remember really liking it, but what was the conclusion? Was it that you didn't... the vowel didn't have to reach its target to be perceived?

MSK: Yes, right. That its target shifted. I mean that the acceptable vowel position shifted as a function of the speed of the transition... the length of the transition and the speed of it.

DPS: Well that influenced work that I did with Pinky on vowels.

CAF: It must have.

DPS. Very much. We did this stuff that I started with her but that she went much further with it with Jim [Jenkins] with ...

CAF: Silent center vowels

DPS: Silent center vowels

MSK: Right.

DPS: We together conceived of this research. It was directly following from your stuff.

MSK: How did Al ever take that really?

DPS: He didn't take to it gladly, I think.

MSK: No.

CAF: But why wouldn't he have? What about it would have not appealed to him? Was it that vowels weren't worth studying, or...

DPS: Yeah. That vowels were just fundamentally different kinds of beast. He even would occasionally said very extreme things like they were essentially nonspeech.

MSK: Yes. Yes.

CAF: I just don't know how he could think about speech perception. I mean vowels just are interleaved. How could you possibly treat one as speech and one as nonspeech?

DPS: Yeah. He thought that the consonants were the information bear...more information bearing parts of the signal

CAF: Sure.

DPS: And of course there's some support for that.

1:06:25

CAF: And stop consonants were the best kind of consonants of all. Most encoded.

MSK: Right. But Al never really...I mean...He never really put his ideas together. The nearest he came to it of course was in the big book [1996]. But that was really annotating the work that he'd already done and not putting himself out on a limb with a solid position on every issue.

CAF: Well there never was a paper after Perception of the speech code that was as global in its topic, was there...by Al, I mean.

MSK: No, no.

CAF: I think that was really the...There was the paper with Ignatius in '85, but I think....

DPS: But you [MSK] wrote a couple of very comprehensive reviews,

MSK: Yes

DPS: an early one and a later one.

MSK: I did one...the first one..This was...You see it's obvious I'm not really an experimenter and I'm really a charlatan in amongst you scientists.

DPS: Come on.

MSK: Because I've never... No, it's true...Because I've really lived off other people's work for the most part. I have not done...I'm not good at working out and thinking up experiments. And I've just..

CAF: Would you say that Ignatius was the same way and people like that are pretty valuable?

MSK: I guess Ignatius was a bit that way also. Right.

CAF: I call people like that "dreamers;" I like dreamers.

MSK: Right, Well, but so why was I saying that..

CAF: So you wrote this review set that Donald...

MSK: That was all part of the habit I'd gotten into when I first came to the Lab of reading every damned thing, because I really couldn't understand it. And so like most [??] They were really self-educational when I wrote them.

DPS: They were very impressive.

MSK: It was Arthur [Abramson] who first invited me to do the one for his book. He was editing, sub-editing that book with...who was the great man from Indiana who...Modern Trends in Linguistics...

CAF: Sebeok?

MSK: Sebeok. Right. And Arthur was the editor of one volume of that, I think the Phonetics and Phonology volume. And he asked me to ...And he simply asked all the Haskins people. Kathy had a chapter; Leigh had a chapter.

CAF: So when would that have been about?

MSK: That was..I think I must have written that...It's one of those annoying things. It came out in '74, but it had actually been finished in '70, I think. So I think I probably began it in '68-69 something like that. I'm not sure, I think, probably about then.

And then the second one was for Current Trends in Linguistics. Is that correct?

DPS: Yeah.

MSK: That was in 1976. And then I did a third one which was for...It was for a paper for the International Phonetics Congress in '79, which I did because...Oh that's interesting, because I'd fallen in with Eli Fischer-Jorgenson. And I'd fallen in with her because there was a Linguistics Society of America summer school in Oswego, which is...must have been in '78, I think. And there she was, and I was giving a course, and she came to my course, and then asked me to give this paper for the Congress which she was organizing in Copenhagen. But the reason why that really comes to mind is that's when I fell in with Robin Battison. Something that was important for me, but I don't know that it was for anybody else very much, was sign language.

CAF: Oh yeah.

1:11:04

MSK: Which came about because there was a conference called: The role of speech in language, which a book came out on edited by Jim Cutting and...

CAF: Kavanagh.

MSK: and who?

CAF: Jim Kavanagh?

MSK: and Jim Kavanagh. Right. It was to have been Ignatius [Mattingly] and Kavanagh, but for some reason, Ignatius dropped out and Jim Cutting took it on. And at that conference, I fell in with Ursula Bellugi. And I was extremely rattled by sign language. I thought: this is really outrageous. We can't have this. Speech is what language is about and this... I really struggled for quite a time with the...before I accepted what had to be accepted.

CAF: Right.

MSK: That sign language is a full language of some form. But that did alter my view of speech and language considerably. I mean, made it...

DPS: You wrote this wonderfully detailed review of their book.

MSK: Oh, I did. Right That was *Signs of language*, the book with Ed [Klima]. Right. But there's still something that, I think, there's something odd about sign language, and it's that it can't be written down.

CAF: I know. That *is* odd about it. Very odd about it. So did that trigger also your interest in evolution of language?

MSK: No.

CAF: Because I could see saying; How can it be possible if we evolved to speak. Definitely, we didn't just evolve language; we evolved to speak language. How could sign possibly work?

MSK: Yes, well it was *part* of it. But actually I'd always been interested in evolution, because it was one of the batons with which I beat my way out of the Church of England when I was 15 or 16.

CAF: Hmm. How did that work? How did that work that you used evolution of language as a baton to beat your way out of the Church of England.

MSK: No, no, it was evolution.

CAF: Evolution itself.

MSK. Evolution.

CAF: Oh! Now I get it.

MSK: Yeah. I used to read books in a wonderful series called, *The thinker's library*, which had a wonderful author named Joseph McCabe, who was wonderfully sacrilegious and wrote appalling diatribes about the wickedness of the Bible. And so on.

CAF: Wickedness!

MSK: Well, all the irreligious acts that are committed there and are approved. And so on and so on. Which is wonderful grist for a [...] year old. And in that series, I came across Haeckel, *Phylogeny and ontogeny*. And anyway. So I had always been very interested in it. But then I was deeply affected by going to, for this year, at Bielefeld which was '77 to '78. Which...Because there I discovered that I didn't understand a thing about evolution. As a matter of fact that all my notions about it were wrong-headed in one way or another. Because I lived for a year with these biologists who lived and breathed evolution.

1:15:07

And that was an enormously exciting event. And that is really what pushed me into evolution of language. Because I was the only language person there. And I was invited to it by Gilbert Gottlieb who was very interested in categorical perception.

And since I seemed to have heretical views about categorical perception when we were chatting at some conference or other [where] we met. He liked that whole idea. I actually...On categorical perception---I don't know how you view it all now---I regard it as a gigantic error.

CAF: Me too. A big red herring. The only good thing about it is that it got psychologists interested in speech.

MSK: Yes. Good! I entirely agree. And I think David Pisoni was a very important figure in that.

CAF: Yes.

MSK: And... One thing though just to say though is that the thing which is right about categorical perception is the discreteness: it's the fact that you are putting it into a discrete category. And that does turn out to be absolutely crucial. It's just that the categories were not the right categories.

CAF: Right.

MSK: But David I met because...It must have been '68 because it was the year I went to teach at the Attenborough School at University of Pennsylvania, and that year the Acoustical Society met in Philadelphia. And David pursued me, and I found him rather irritating.

CAF: I continue to.

DPS: I do too.

MSK: He's a very...whatever it is...he's not exactly aggressive...But he's..

CAF: Persistent

DPS: I would say he is.

MSK: Alright. OK Maybe he is. Yes. Maybe that's what he is: aggressive. Right. But he got on my nerves for a bit. But after a bit I began to think he was talking sense. I mean, he was always coming over to me and bending my ear and going on. So in due course, I agreed to be the outside advisor on his thesis that he was doing with Irv Pollack at Michigan. And that's how I fell in with him and how he subsequently came to the Lab for year or something.

CAF: Now did he collect his thesis data here at Haskins?...or did he just make the... maybe he just made the stimuli here. And went back to Michigan.

MSK: Made the stimuli here. He collected the data out there. Right In Michigan

CAF: But...And it was reproduced as a [Haskins Laboratories]Status Report, his dissertation, I think, 1971.

MSK: Yes.

CAF: And it was beautiful research.

DPS: We did a piece of research with him. It was quite intensive as I remember. A dichotic feature exchange study with David.

MSK; Oh yes.

DPS: Do you recall that?

MSK: Yes, yes. Yes, I do.

DPS: It was published in *Cognitive Psychology*. That was quite a lot of work. I mean he did the experimental work, but we all worked hard on it.

MSK: Yes.

CAF: So did Terry Halwes ever...His dissertation was also on dichotic listening and features. Did he collaborate with you guys?

DPS: No. No, not...

MSK: No

DPS: No, no. He did his work completely at Minnesota, then came to Haskins. I never was able to work with him.

CAF: Crazy guy.

MSK: Well David, though, has been a very important figure in sort of bringing speech into cognitive psychology. Which really, Al [Lieberman] launched, but Peter always...not Peter, David always said, and I think with some justice, that Haskins finds things and it doesn't always follow them up analytically enough. And Al used to say also that, of course, if Haskins finds anything, other people will find it too. It's there. It's just that the interpretation may differ. But nobody has ever shown...found that one of the phenomena that the Lab reports is not a real phenomenon.

CAF: Oh. I see.

DPS: Well, David got interested in natural speech and more rapid speech and things like that.

MSK: Yes. Yes, and diversity of speech. Yes.

CAF: Talker differences.

MSK: Very much like Remez who's also gone off into ..

CAF: Yeah. Individual differences. Not individual differences, talker differences.

DPS: But of course Remez had a period with Pisoni.

MSK: Yes, he spent a year out there. Or two years maybe

CAF: They are soul mates in some ways, I think. Surprising friendship. Maybe It's not.

CAF: One question...This is really changing the subject..But you mentioned that Frank [Cooper] was paternalistic, and other people have said he wrote all the grants. This is completely not my idea about Haskins' presidents. So one question I had in my mind is: Did you notice like when Frank stepped down and Al stepped up, was there a change in the way the Labs was run? There must have been. I mean, because Al was in the...I would say started the tradition of you and me of not being administrators. Not ...just kind of...I see it as just letting people do their work and not getting in their way,,,But maybe Frank...

MSK: Yes, I don't think that Frank got in people's way.

CAF: No. He sort of got things out of their way is what it sounds like. I mean he wrote the grants.

MSK: Yes, yes. He was not...He didn't engage himself with people the way Al did, of course. He didn't go around talking with people.

CAF: What's your discovery? [Al Lieberman was famous for asking people what they had recently discovered]

MSK: Right.

DPS: I think it's fair to say that Frank wrote the grants only up until the A40. Once the A40 got going, it was very much a shared enterprise. Right?

MSK: Right. Absolutely. But Al continued the tradition of leaving people alone. Very much.

DPS: Except at grant writing time. And then he really put the screws on us.

MSK: Oh Yes, yes. That's true.

CAF: But one thing that I think about Al I really give him credit for. I mean he was extremely committed to his theoretical point of view. But it was OK for me to disagree with him. I mean he told me I was a lunatic at one point. But he didn't stop me from doing my work. He didn't stop Cathe [Browman] and Louis [Goldstein] although I think he never really sort of figured out articulatory phonology.

MSK: Yes, I never understood why he wasn't thrilled by that work.

CAF: I know. Well, he sort of adopted it his Liberman and Mattingly '85 paper, except you see when they write about it that they didn't really understand what gestures were. So it's just more of my " Al wasn't stopping" ...

MSK: Right. It is very peculiar because the term gesture instead of feature is used in The Perception of the Speech Code.

CAF: Is it?

MSK: I mean they say that the gestures are the equivalents of features.

CAF: Right.

MSK: And I agree with you. He never really apprehended that. And I don't know why not.

CAF: I just think it was his lack of scholarship.

MSK: It was a lack of scholarship as you say.

CAF: But still you know, he didn't stop Kathy [Harris]. I mean maybe that's the best example. He had no...As far as I can tell, he had no interest whatsoever in speech production. Once the EMG project didn't give the results that he had hoped, he had no interest in that at all. But he never stopped her and other people from doing that line of research at the Lab even though he couldn't have cared less about it. Which I think is really a good characteristic of a president.

MSK: Yes. No, I agree. Although I have to say it seems to me that the EMG research is also another error.

CAF: Oh true, but you couldn't have known it back then, could you? I mean I thought it was fairly smart. And it was either Kathy or Frank or both together who thought of importing EMG to that kind of work. I guess some people had been putting things in the larynx...electrodes in the larynx and stuff.

MSK: Yes, it was. But an interesting thing, though, is that I remember that, when I was first at the Lab, there were available--and Arthur was busy classifying them and organizing them—X ray photographs of people talking. And it's ironical that it's the X rays that turned out to be valuable and to be used for gestural phonology, and the EMG ran into a dead end.

CAF: Yeah but still I think that, back then, you couldn't have known that it was going to be a dead end. It was a smart move.

MSK: Yes. I think that's true.

1:25:04

CAF: And that's just another thing that Kathy's memory has failed her about. I mean she remembered some of it.

DPS: Yeah

CAF: She remembered the contact with the Japanese doctors and bringing them on board.

DPS: Which she took the initiative in apparently, Kathy...in getting the Japanese.

MSK: Oh, very much. Oh yes. She was very active and was absolutely crucial in... I think she, as a matter of fact, was glad to get away from Al and to get with Frank. Because Al never really liked Kathy very much.

CAF: I was looking for her to say something like that. Because I felt she was maybe the only female senior scientist there. Not even senior, but she was the only female scientist there, and I wanted her to say that Al just didn't...

DPS: Except for Jane [Gaitenby]

CAF: If Jane Gaitenby counted, yeah.

MSK: She wasn't a senior scientist

DPS: She wasn't a *senior* scientist, but she was an investigator.

MSK: Right.

DPS: I guess in a pretty subsidiary role.

MSK: Yes, I remember Freddie Bell-Berti saying to me once, when she'd been complaining about Al. It's all very well for you. You're a man and you're English.

CAF: Right. That's a help.

MSK: Yes, well. Kathy was...And I think that Kathy was deeply invested in the whole EMG project, and I think probably... because as I say I remember her being at some kind of loggerheads with Peter.

CAF: Yeah, that's interesting.

MSK: And it's funny that she doesn't remember it. Although... she's also...Once fairly recently when we were traveling back together after one of the [Haskins] board meetings, I apologized to her for something which I had done and had done in complete blindness. It was an error, and it was...but it was a self-centered error. Which was: I had not invited Kathy to give a paper at Don's... at Al's meeting.

CAF: Oh!

MSK: And it was flagrantly obvious that she should have been. And I cannot understand how....Except of course Ignatius didn't like her either. But why didn't I, who after all owed my position here to Kathy and had never been on bad terms with her. Why didn't I come through and say that? Whereupon when I said to her and I said, now, I finally years later apologized for this. And she said: Oh I never noticed it.

CAF: Didn't even notice it.

MSK: I don't believe it though, I think that...because her students noticed it: Freddie and Larry Raphael.

CAF: Freddie has definitely been a great supporter of Kathy.

MSK: Yeah.

CAF: And Kathy...I think that one of Kathy's greatest contributions was being a mentor to students. I mean...Freddie, probably the greatest of them. Just wonderful research. But a lot of especially young women. Larry was in there too, but especially young women...helping them learn that difficult work and supporting them.

MSK: But did you...you never felt or did you, any...antifeminine feelings, did you?

CAF: No. I didn't. But I wondered if Kathy had because she came so much earlier and was kind of alone. You know, by the time I was there, there were lots of women.

MSK: Well I think that... I think in the early days, he [Al Liberman] sort of accepted Isabelle as a sub figure.

CAF: Oh really! I didn't think you were going to say that.

MSK: Oh. No, and then... well, he seemed to be happy...I don't know... happy is not the right word. He never made anything of the fact that there was he in the Psychology Department and she was...

CAF: And she was in Education

MSK: And she was in speech and hearing and so on. And that 's something I 'm interested by, because it seems to me it was through your [DPS's] taking up with Isabelle that somehow the reading work began and without that it wouldn't have begun.

1:29:40

DPS: No, I think that that's true, and I think that Isabelle wouldn't have got her...She got a foot in the door through that work. And Alvin I think was first resisted; I mean he was very...

MSK: Resistant

DPS: He was resistant. He was very worried about nep...nepotism issues.

MSK: Right.

CAF: Which is why Isabelle was in the Education Department, right? [UConn's nepotism policy at the time]

DPS: Well, that might not be quite right. I mean...

CAF: Well, isn't it true that you couldn't have, at that time,

DPS: Yeah, yeah.

CAF: You couldn't have two married people in the same department

Later, you could with the Wilson's [Martha and William]

DPS: It was a problem for the Wilson's , right. Yeah, I suppose that's true, but then Isabelle got this rather fancy post doctoral fellowship to go around and visit all the places where reading research was done.

MSK: When was that?

DPS: That was just before she took the position at...

CAF: At UConn?

DPS: At UConn. And that was a kind of segue into that position. Because she was...

MSK: In what year was that though?

DPS: That would have been...That was just after I came. Or about the time that I came, which would have been ...

CAF: Mid sixties.

DPS: Yeah. Mid sixties.

MSK: Right. So when was your first paper with Isabelle?

DPS: Well, we weren't very quick in getting it out.

MSK: '69, maybe?

DPS: Yeah, or '70. Or maybe...

[The first Shankweiler/Liberman paper in the Haskins publication data base is Misreading: A search for causes, 1972. Can that be the first?]

CAF: Maybe 1970, I think around then. So I didn't realize that. I sort of had an idea that people were sitting around the Lab and talking about: Why, why is reading's so successful if we're biologically adapted to the spoken language and not to taking... language in by eye. Why does reading work so well? I didn't assume that Al wasn't involved in that, or Ignatius for that matter. So was it really you and Isabelle first?

DPS: Yes, it was I think. We were ...I think that Alvin was interested in me because I had a background ...some background in reading and he thought that I would be a buddy for Isabelle. And we did hit it off and started collaborating on reading errors and comparing reading errors and speech errors. That was one of the first things that we did.

MSK: Yes. Right, right. Which you [CAF] did that.

CAF: I did spelling errors, I think. [1979]

DPS: And reading errors too. [1977]

CAF: Did I do reading errors too? Maybe.

DPS: A much more sophisticated analysis of reading errors than anyone had done before.

CAF: Maybe it was reading errors. I think it was reading, you are right. Boy, it was a long time ago.

DPS: We published it in Language and Speech.

MSK: But that issue of how come we can read seems to me to be *the* most amazing thing in the whole shebang.

CAF: It does to me too, especially because, if you ever try to teach phonetics to undergraduates, they always think you are talking about letters. It's not just that we can read well. But we...It takes over. It really takes over the way we think about language.

MSK: Well, it's interesting that in early philosophical stuff, which I haven't read very much of, but which I came across by, through Chomsky. So I need to go and read it in fact. I mean in one of his papers. In the Port Royal...the grammarians of the 17th century, they talked about letters and sounds as the same thing. They...it was just ..It just hadn't occurred to anybody that there was a sound and there was a written form--there was a sound form and a written form. They were the same thing.

CAF: Same thing. Wow.

MSK: And that... I'm just trying to remember when that phrase came to me.

But...Harlan Lane is relevant here, because Harlan was such a behaviorist. And I knew Harlan personally because I was in graduate school with him. He was...My first year at Columbia was his last year as an undergraduate at Columbia in which he was being permitted to make that also his first year as a graduate student. And he got an MA in psychology out of it there that year. Though he distinguished himself by refusing to go to physiological psychology on the grounds that it wasn't part of psychology, and...though it was a required course for the master's degree. But what they did was to give him the degree and tell him they wouldn't...he couldn't take a PhD, and he damn well better go to Harvard if he wanted a PhD, and so they sent him to Harvard. But one of the things he would say was that you learn the sounds of speech by learning what to call them. By associating...

CAF: The sounds with letters?

MSK: So that it's as though you learn the names of the sounds by some kind of reinforcement procedure. Whereas the fact of the matter is that, if you ask...The speech sounds name themselves. That's the phrase that I'd come up with at some point.

CAF: Yes. I remember.

MSK: Which is the mystery. It is that: there they are; they name themselves. And there's no other way of saying what a /b/ is than they...

CAF: Yup.

MSK: But...And that is why I recently in my last sort of bout got so exercised over the particulate principle. Which, when Al first read about it, said: Hasn't anybody written this down before? And the funny thing is, they hadn't. But Bill Abler, although he is a complete nut in his paper..in his papers, is the first person actually to make it perfectly clear that you can't have a ...Only yesterday I read in some book review, London review of books or something: Somebody writing and saying: It seems obvious that once we've got language that you're bound to get reading and writing. And then they went on to say whatever they were going to say. But it doesn't seem obvious at all. It seems to me absolutely flabbergasting that we got reading and writing. And that it's *the* peculiarity of speech that's made that possible.

CAF: Right. So, when I try to relate the particulate principle to duality of patterning, it seems to me the thing that's distinctive about the particulate principle, or one thing, is the idea that you put the sounds together and they don't blend. Right? That they maintain their distinctness as opposed to the ingredients of a cake or something like that; where you put them together and they lose their identity.

MSK: Yes.

CAF: And I don't think that was in the notion of duality of patterning. But the particulateness otherwise is in that principle [duality of patterning], isn't it?

MSK: Yes. That's true. However, what the particulate principle does is to generalize that.

20:34 from end

CAF: Right. That's the other thing.

MSK: And Hockett said: This is peculiar to language in effect.

CAF: And it's not

MSK: Whereas Abler is saying it's all over the place

DPS: It's in chemistry and genetics

CAF: In DNA and chemistry

MSK: Right. Right. So it was only one way of doing it . And that's remarkable because what that also seems to me to also say is that, since you can do it with your hands [in sign] and your mouth...There's absolutely nothing else in the animal kingdom that anybody could do it with. You couldn't... Unless you did something to train them up so that they evolve to have a differentiated system of many parts and all the rest of it.

CAF: Right. So did we get off track though. We were talking about reading and then you brought up Harlan Lane and said he is relevant here.

MSK: Oh. He is relevant, because Harlan is the archetypal behaviorist account of reading. And saying that you learn the names of letters by association.

CAF: So did he...IS he still a behaviorist today? I'm having a hard time relating that to

..

MSK: I don't know...I think he's probably...I don't think he's done any work that's clearly behaviorist. He's done entirely sign language since he got into that.

CAF: Right, and wild children.

DPS: Yes, wild children. It's amazing that he could write such a massive tome on the wild boy, that there would be that much to say.

1:39:30

MSK: Oh yes, yes.

CAF: All right, so what haven't we covered, Donald?

DPS: I know what I wanted to ask you.

MSK: Oh, one other person that we didn't really mention was Kirsten, Kirsten Hadding-Koch or Hadding as she became when she dropped her husband. And again, I met her at teatime. And she was there at the Lab and I think she was...I think it was Frank who had invited her. And the interesting thing...I was of use to her because one of the few things I had learned how to do was psychophysical experiments: methods of limits, and constant stimuli and all the rest of it. And it was applying that, psychophysics, to intonation and seeing whether you got the same patterns for speech as you did for nonspeech patterns of a certain kind. And that was also part of the movement that is that I...One thing I was always fond of was, though not having any idea how to deal with it, was that there was movement and that was why speech worked. And it was obvious that it didn't work by being no movement, because that was exactly what didn't work. So I was interested by that whole intonation business. And then we had this very interesting result with "November," I guess it began, or "For Jane" or whatever, that if the pitch was high enough you needed less of a rise at the end [to hear it as a question].

CAF: Yes, and Phil Lieberman made a lot of that in his book [*Intonation, perception and language*], and he was a motor theorist at the time. Right?

MSK: Yes. Right. Phil made a lot of it, and Kirsten said to me: Phil's so clever. Look what he did! Why don't you do something interesting with our data?

CAF: So what I remember: You mentioned a paper a while back today that you wrote in '68 or '69 and it came out in '74. But I think it's your paper with her that you wrote in '65 and it came out in '63?

MSK: Yes, that's true. In *Studia Linguistica*, I think which was so behind. Right.

CAF: So we haven't talked about Phil Lieberman. How long was he at the Laboratories? He was at the Labs when I came in '71.

MSK: He was here for a year or two, when he was...? The whole time...How long was he...

DPS: A little bit more than that. I mean I think he came just after... Yeah, well, he was appointed to the linguistics department in the earliest days of that department at UConn. I mean it was Ignatius came by himself the first year and then Arthur, and then Phil.

MSK: They were forming the Linguistics department, weren't they?

DPS: So they came in...I think that Phil came in '68 or '69. To UConn and he became associated with Haskins at the same time. Whatever year that was. That's what I think happened. He was not in New York. He was not in the Lab in New York.

1:43:08

MSK: He was there, but he was not a regular person, right. I mean I'd seen him.

DPS: Yeah, no, I met him I'm sure. Yeah, he had...His main job was at that Air Force Cambridge laboratory in Massachusetts.

MSK: Yes. That's right

CAF: And was it coming to Haskins in New Haven that got him hooked up with Edwin or Edward Crelin [Edmund Crelin]?

MSK: Yes, I guess so.

CAF: Vocal tract guy. So, I have...I always think of Phil as being kind of a maverick, as a guy who really couldn't collaborate with other people. Is that the way he would have been here?

DPS: Don't think he had any collaborations with any of us, did he?

MSK: No, I think, that 's right. I remember... Yes. My wife came up once to visit here and found herself locked with Phil Lieberman in a back room talking about things, and one of the things that Phil was talking about was how he planned to spend the Nobel prize money he expected to win.

CAF: Oh, gee. It didn't work out. Didn't work out.

MSK: I mean he was a very odd bird.

CAF: He is very odd.

DPS: But interesting. I mean...

CAF: True.

MSK: Very interesting.

DPS: One interest I shared with him was on vocal tract normalization, and he had some interesting ideas on that that I think were wrong, but interestingly wrong.

MSK: Yes.

CAF: So did he leave Haskins at the time he went to Brown or before that?

DPS: No, he left...Yes, I think all at the same time. He went to Brown because...

CAF: His wife didn't get tenure.

DPS: That's right, it was a nasty business.

CAF: Yeah. She did not get a faculty position at Brown.

DPS: No.

CAF: So, I think she just gave up academia.

DPS: That's right.

CAF: Not that it matters.

MSK: And now his son Daniel has really hit the big time.

CAF: He has. And he's also known as a barefoot runner. He runs barefoot..jogs barefoot.

MSK: Oh really?

CAF: Yeah, yeah. If you google him, you find the barefoot runner or jogger.

DPS: He's the origin of the born to run thesis about human anatomy. I think...Yeah, we could outrun any creature, not in brief bursts of speed, but over the long haul.

CAF: Ah. We can run marathons.

MSK: Yes, yes. He has a book on the head.

CAF: Does he?

MSK: It's a massive book. It's the evolution of the head.

CAF: Hah! See he was probably influenced by his father.

MSK: Yes, definitely. He carried on his father's...

CAF: I was doing a project for you guys when I was a graduate student. It was the relation between handedness and laterality [for speech]. And Daniel Lieberman was my subject...was one of my second-grade subjects. I thought he looked just like Phil.

MSK: Oh.

CAF: Just a little seven year old Phil.

DPS: So Chomsky ...Look can we get back to Chomsky before we quit? He was important in bringing you to the view of speech that you held.

MSK: Yes, well only because of his emphasis on the biology of language.

DPS: Right, but the irony is, of course, is that his curious views about evolution...

MSK: Yes.

DPS: And you tangled with him, did you not, late in the game somehow?

MSK: It wasn't much of a tangle, But yes, because he just doesn't...he just thinks it's biological, and it was some event.

CAF: A massive mutation or something gave you an LA...a language acquisition device.

MSK: Yes, and that this changed the wiring of the nervous system.

DPS: What do you call these sudden changes?

MSK: Mutations

DPS: There's a name for it. There's a kind of theory of how evolution could...I've forgotten what it's called. It doesn't matter.

MSK: Yes, I think it's ironical that of course ...well that Chomsky precipitated the biological interest in language, and...

CAF: Had such an unrealistic view.

MSK: Yes, and of course the child language stuff. That was something that eventually I came to see as much more important than I'd ever thought it was before. Was child development.

CAF: In which way? So we have...

MSK: Well, by...Because I thought that everyone agreed that we don't recapitulate evolution. But nonetheless there are certain routes to get from the simple to the complex. And some of those routes are necessary. So I think that one of the interesting things that hasn't yet been adequately studied is how children get from moving articulators individually to organizing them into phonetic segments. So I think that would be my model of the evolution of the system too.

CAF: Uh huh. Right, right. Yeah. OK I thought you were going to be talking about the kind of Chomskian view of acquisition of syntax of the Stephen Crain, Diane Lillo-Martin kind.

MSK: Well that...I don't know what to do with that. I don't really know that work well enough. But..

CAF: It's absolutely beautiful work that I think has to be completely wrong. But it...At least Stephen Crain's work on showing that kids don't make certain kind of mistakes. They behave as if they know certain very unbelievably complicated things right from the start. That I think has to be wrong, but it's beautifully done.

MSK: Why does it have to be wrong?

CAF: Well, just because I don't like the idea that you have a language acquisition device with things... knowledge inside of it.

MSK: Oh I see.

CAF: That seems sort of evolutionarily...Not that I know much about evolution, but that seems sort of impossible. So I have read *Rethinking Innateness* by Jeff Elman and cast of thousands in which they argue that innate knowledge is a pretty implausible kind of a thing, because of the way the brain grows in a fetus; these massive

[numbers of] neurons are generated and then they get cut back. And how do you keep the ones that have the knowledge in it.

MSK: Right. So it much be much more an utterly inevitable process of interaction between...

CAF: Something like that. You've got to figure out why kids don't make certain kinds of mistakes, like c-commanding. C-commanding; How can you possibly be born knowing about that?

1:50:57

So our last question was: What do you wish we had asked about that we didn't ask.

MSK: No you had another question. I don't know the answer to that. There is another one, though, What was my idea of what I should do as president. And the answer was, really, I didn't have any idea what to do from a research point of view, but I did have a clear determination that the whole system should be democratized. And I wanted to get rid of the board of directors as being a group of...Not difficult to get rid of them since they were all dying off anyway. And nobody knows anybody with that kind of money except Caryl Haskins. But still it was to... the idea was, I thought, was that it should be an academicized board, one where the people who were on the board knew something about the field or at least the academic circumstances of ...

DPS: We have moved in that direction, haven't we?

MSK: Yes. I think that that was a successful move.

DPS: Good.

CAF: So that's what you mean by "democratization"?

MSK: Yes.

CAF: Because I wouldn't have said that the board of Caryl Haskins' pals exerted any influence on what we did as a laboratory. I mean...They didn't try to tell us what to do. They didn't even seem all that eager to have input

MSK: No, they didn't. They were just a rubber stamp.

DPS: Yeah that was really what my impression was.

MSK: So. Right, it was to make the board a more active participant in the whole process. On the other hand, it's also the case that I've always been opposed to fund raising. I mean, that is, obviously, funds have got to be raised. But, it always seems to me that the amount that can be raised from ourselves and by the various approaches that have been taken is minimal compared to what's needed.

CAF: I think that that 's the way it's so far turned out. I mean I'm not involved any more, but it seems like we got one \$300,000 grant from someone that used to be on the board. I don't know what's come of that work. But otherwise, I don't think we've raised any huge amount of money. Is that right, Donald?

DPS: No.

MSK: Someone that used to be on the board from the old days?

CAF: No, not from the old days.

MSK: Oh but still that's wonderful then if there's a rich person on the board.

DPS: One significant contribution so far.

MSK: Yes.

CAF: But I think your fundamental idea has proven to be right, that we're not the kind of people that can get billionaires interested in what we do to the extent that...

MSK: Right and I always used to tell Al this when he would go on about dyslexia. Because the fact of the matter is that there's certainly a tangential interest in dyslexia, but nobody who's got a dyslexic child is going to go wild about Haskins' research.

CAF: Well, yeah. Although it seems like the present Laboratories is moving in the direction of both doing more reading research and doing more outreach to educators than in our time.

MSK: Well that seems like a good thing.

CAF: A practical thing. I don't know if it's a good thing.

MSK: A very practical thing. Right. But it's not your feeling that the field is drying up.

CAF: Yeah. At least I think it's true that at least NIH is very much more wedded to the idea that you've got to do research that has clear translational...into application so that it's hard to get funded for the kind of really just basic research that we did for so long. But I don't know much about it.

MSK: The lead article in the Times yesterday was about Obama's support of the study of the brain.

CAF: Uck. I didn't know that he was interested in that. But... I'll write a letter.

MSK: Isn't this what Phil Rubin's been taken to the White House to do?

CAF: I hope not. I don't know. I actually don't know what he's doing.

MSK: Oh. I understood that Phil had a position at the White House advising people on neuroscientific matters.

DPS: He is...Yeah. He is spending time there regularly. I think he told me. Yeah

MSK: He's no longer here, is he?

CAF: I guess he's technically, he's here. Yeah

DPS: Technically, he's on leave from Haskins. But he's at NSF and got this thing as you say at the White House in the science advisory group. I'm not sure.

MSK: Well this announces a decades long program with huge amounts of money to be put in to NIH and so on.

CAF: That sounds good.

MSK: Which will it sound to me like the kind of thing that Ken would be after.

CAF: How dreadful.

DPS: There was one thing that puzzled me in reading about Cary Haskins was that when he was director of the Carnegie Institution in Washington, they had a staff of 40 full time investigators. And this was back in the '60s, I guess '50s. and...

He urged his people not to accept federal funding for their research. Because...On the ground that it would limit their freedom to follow science where it led.

MSK: Oh.

DPS: I was very... very surprising and wonder if ..

MSK: I never heard that because...

DPS: ...if he disapproved of the way things had gone with the Labs.

MSK: He may have just bowed to necessity, because..

DPS: I mean this was quite a while ago. I mean he was President of the Carnegie Institution I think from '56 to '71, those years. But that would have been exactly the years when federal influence on research was burgeoning very fast.

MSK: Yes, yes.

CAF: Are we done for today?

MSK: Yes.