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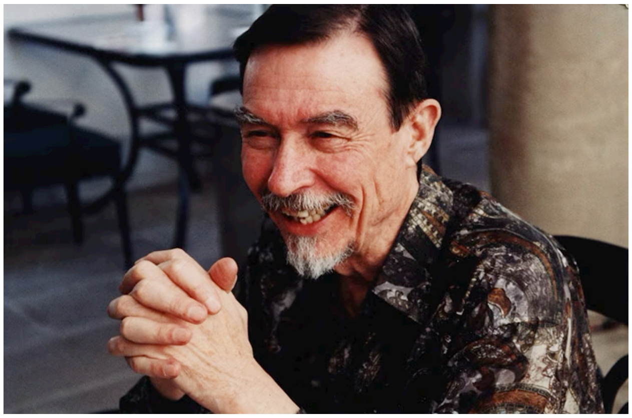

Carver Mead

Gordon and Betty Moore Professor of Engineering & Applied Science, Emeritus

By David Zierler, Director of the Caltech Heritage Project

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David Zierler: OK, this is David Zierler, oral historian for the American Institute of Physics. It is June 29th, 2020. It is my great pleasure and honor to be here with Professor Carver Mead. Carver, thank you so much for being with me today.

Carver Mead: Good to be here, David.

Zierler: OK, so to start, would you please tell me your title and institutional affiliation?

Mead: I'm the Gordon and Betty Moore Professor Emeritus at California Institute of Technology.

Zierler: OK. And when did you go Emeritus?

Mead: I think it was '97, but I could be off a year because I took a year off—I had never had a sabbatical in my 40 years teaching at Caltech. So before I retired I told our division chair, "Look, I need to think about this. What do I want to do next? And I've never had a sabbatical before." So I took that year to work on my little green book, and by the time I was getting close to finished with that I could see that I would be better if I were Emeritus.

Zierler: But of course, Emeritus did not mean that you cut ties with Caltech. You've been hard at work ever since.

Mead: Yes, I always tell people that, when you're [laugh] Emeritus faculty you have all the privileges and none of the responsibilities.

Zierler: [laugh]

Mead: But you're a regular faculty, it's the other way around [laugh].

Zierler: Right [laugh]. So life is pretty good [laugh]?

Mead: Life is good and I still have a program there. I even have a grant there that is really to support my colleague—but it's for the little nitty gritty overhead stuff.

Zierler: Right.

Mead: I always tell people that what I do now is I work on what I want to work on, and what I think is important, instead of what somebody else thinks is important.

Zierler: That's right.

Mead: And so, actually it's been the most productive time in my life.

Zierler: That's wonderful. That's wonderful. Well—

Mead: Continues to be.

Zierler: —Carver, let's take it all the way back to the beginning. Let's start with your parents. Tell me a little bit about your parents starting with where they are from. Where's your mom and dad from?

Mead: Well, they're both Californians and so, I'm a fifth generation Californian. And my dad ended up in Redlands, California. His father had been a drifter and drifted from Missouri, I think, across the country. He had a little job here, a little job there, dragging the family along. My dad went to more different schools than he was years old.

Zierler: [laugh]

Mead: And they ended up in Redlands and what my grandfather did was to—well, there were a lot of orchards in Redlands, citrus primarily. And they were owned by people from St. Louis and Chicago and Points East as a place to get away during the winter. So there were all these wealthy families that had a place to come and enjoy the winter in California and the orchard was what made it a business trip. But, of course, the orchards needed to be tended, so my grandfather had this little business of tending people's orchards and making them work, and he got a nice living out of that. So my dad grew up with a plow and some mules—-tilling the fields and doing that kind of stuff. And he actually got through high school. He was born in '04 and so, in those days, not everybody even got through high school. So he got through high school and then got a job working at the local drug company doing deliveries, because in those days drug stores delivered stuff to you just like they're starting to do now [laugh].

Zierler: Right [laugh].

Mead: And so, he was a delivery boy and then he read in the paper that there was this power company up northeast of Redlands in a little area where a stream called Mill Creek came out of the mountains there, which gives you a hint that it had been used for water power before. And they had built an electrical power plant there. One of the historic plants in the United States. Actually, it was the first three-phase plant in full commercial operation, so it was making electrical history. But they needed operators. This brand-new business was growing. Every time they had electrical power; people wanted it! The big customer was an ice plant in Redlands that made ice cheaper than you could make it in Los Angeles, because ice is all about refrigeration, which is about power. And they could use the local electrical power and make ice and then put it in to cool the railroad cars and ship oranges back East, where the market for fresh fruit was veracious in the wintertime. And so there were these opportunities. My dad went and somehow get a hold of some books and studied up on electrical stuff just on his own and got to where he could kind of grok it a little bit. And he went and interviewed, and he got a job as an operator in that new power plant. And so, that was how he got started in the electrical power business which was leading edge stuff then.

Zierler: Right.

Mead: The first power plant on that stream there was built in 1893. The power plant he worked in was built in 1896 and then was re-powered in 1904. That was the growth era in electrical power, local to Southern California. And so, he got the job and kept learning about hydroelectric machinery, and there was a better job that opened up in the same company, (Southern California Edison). They had a plant up on the Kern River which is east of Bakersfield, California quite a bit further north. So, he went up there and took that job and met a girl at one of the local dances. And so, that's where I came from.

Zierler: [laugh]

Mead: [laugh] And then when I was two years old he got a better offer at a place called Big Creek, which is halfway between Yosemite and Kings Canyon in Sierra Country east of the town of Fresno. And so, that's what I remember. I have only the vaguest memory of the Kern River. I was two years old when we moved away. I have, of course, vivid memories of the Big Creek Plant. And in those days when a plant was back in the mountains like that—we were two-and-a-half-hour drive to get to Fresno on these very crooked mountain roads.

Zierler: Right.

Mead: So we went to town once a month and bought the necessary things to get us through the month. Locally, the company had a commissary that would deliver once or twice a week to the plants. There were, I want to say, five power plants at various elevations down the drop of the water coming out of the high country, and each plant had the power plant itself and then they had a group of what they called cottages. Each plant had 14 of them—-that's what it takes if you have two people on shift, a station-chief and an assistant chief, and utility man and they're families. And then halfway between our plant, with its little group of cottages, and the next one upstream was our school. It was a one room schoolhouse, that had about 15 or maybe 20 total students it in it for eight grades. So there were a couple of students per class on average. It was a great experience because we got to listen in on what the kids in higher grades were learning, and that was much more interesting than the stuff that we were learning in first and second grade. And then, if you got stuck, one of the older kids would help you out, and it was like a bigger family. I was an only child, so I loved going to school because there was a social life there.

Zierler: Right—

Mead: Understand?

Zierler: —right.

Mead: So that's something people in education don't understand. The educational power of a social group like that where it was all ages and enough structure around it so the learning propagates up and down because the older ones get the experience of teaching younger ones and the younger ones get the experience of learning not just from a teacher but from the older kids. You know, you always respect the bigger kids. And so, it's a wonderful social environment for learning especially, in K-12. I think it's understood by people that do home schooling, because a lot of them do it communally, and that's a very effective way to have the kids exposed to other people and learn from different people in different ways.

Zierler: Right.

Mead: I think, from my own experience growing up in this little one room school house, the fact that there's more of that nowadays is very healthy. Today I would be put in a special class because I had ADD, and I was always listening in on what the older kids were learning. But it was a wonderful way to not constrict a person's learning, but allow them to tune into things that they found interesting. And so, I think it's much richer than a one classroom per grade arrangement.

Zierler: Carver—

Mead: Take it—

Zierler: —I want to ask with different opportunities and circumstances, do you think your father would have pursued an advanced degree in electricity?

Mead: They didn't know what that was.

Zierler: Uh-huh.

Mead: I mean the advanced degree part. He knew what electricity was.

Zierler: Yes.

Mead: But he didn't know about advanced degrees. I went all eight years in this one room schoolhouse. About halfway through, when I went in to the 6th grade, they put a big divider down the middle of the room and it became a two room schoolhouse, and we had two teachers. That was also a good experience for me because the teacher I got for the more advanced grades was a very wise man who was nearing retirement, and he was teaching because it seemed like a neat thing to do. And he became like an older relative to me, like a grandfather or something. And, for instance, one day I was working away and he said, "Here, you might be interested in this." And it was a little book on trigonometry (I was probably 6th grade) and I was fascinated by that. That wouldn't have happened with just any teacher, so I was blessed with it. And his wife was a full-blooded American Indian, and she loved to go out in the woods and so, on weekends we would go off in the woods and go fishing together and exploring, because she just loved to get out. And I was somebody she could go around in the woods with, so [laugh] those are personal things that wouldn't happen with a big school…

But then, when I graduated from the 8th grade, the high school we had was halfway down to Fresno. And so, it was like an hour away by bus, and it was a rudimentary high school. And my father quite properly saw that there were better options for my education. My grandmother lived in Fresno and she was a really neat lady, very smart lady, and her husband was getting older and her kids had moved away. My mother, of course, was her daughter, and her son had gone off to Europe during the war. And then he moved back to Fresno. But her husband was failing, so she agreed to take me in as a boarder in her place. It was a short bike ride to the local high school which had a lot more to offer. And so, I was one of very few kids from up in the woods that moved away to go to high school, which was great because her husband was not able to help very much. I ended up being the man around the house and I would fix things when they went wrong. And do stuff around the yard and do other things that normally he would be doing. I got to be very close with my grandmother. And I had the freedom, not only to go to this high school where they had—for example, by then I was very much into radio. We can talk more about that sometime. But it fascinated me, and there was a teacher in that high school that had a room where he taught electronics. What we call electronics now. It was radio then. And that would never have happened in the little rudimentary high school.

Also there were the radio repair shops around Fresno where I got part time jobs and there were several radio stations. I studied up while I was in high school, studied up on everything I could get my hands on. Got my Ham Radio License and then I realized that these licenses weren't hard to get good grades on [laugh] and so, I studied upon the commercial license. There were two grades of commercial licenses. The first one allowed you to work on two-way radios, so that got me jobs at the little shops that maintained the two-way radios. And then the next one made you qualified to work as a Transmitter Engineer in a radio station. So, for my last two years of high school, I got a part-time job during the year. I was working the odd shifts on weekends. Nobody wanted to work on Sunday and all that. And then during the summers I worked vacation relief. So I had as much work as I needed and I was really well paid [laugh]. So those were just opportunities that never would have come up back in the woods. So my dad really had wonderful judgement about where I should go to High School. But he thought I should go to college there at the local state college and become a dentist, because our dentist was the highest paid guy he knew!

Zierler: [laugh]

Mead: He thought I should be one of those too. So he didn't have a clue about what advanced degrees were. He knew I should go to college. They always said that, "You got to go to college." But they didn't know what that meant really.

Zierler: Carver, did you—was your sense that your interest in radios and electricity and things like that—did you —was your sense that that specifically came from your father or do you feel like these interests you more or less developed on your own?

Mead: Oh, no. He was great about that.

Zierler: Mm-hmm.

Mead: And every year he would take me down to the power plant. We'd go down and start at the bottom floor at the turbines and then work our way up to where the high-tension lines went out onto the grid. And he'd explain to me all the stuff. And, of course, to him he did the same thing every year, but to me I was a year older and I had a whole bunch more questions. And I learned a bunch more every year. And so, I would hound him to take me to the power plant and in those days that was leading edge. All of the electronic stuff that happened during the war at the Rad Lab and places like that—

Zierler: Mm-hmm.

Mead: —hadn't rippled through into a place like Fresno. And so, the power plant was leading edge and what little radio I could glean from—we had one guy who really got me into it. He moved into camp when I was just going into 7th grade. And his daughter was in the same class with me, so, of course, it was nice to go over to their place. And there he had this ham radio set up and he had two daughters, so I think he kind of wished he'd had a son that was interested in radio, so of took me in. I had been building crystal sets and stuff, but back in the mountains the crystal sets don't work really. So I'd have to take them to town and then see if they'd work when we visited my grandmother, and it wasn't a way to de-bug a system very well. But this guy really helped me get started and that was just as the World War II surplus started being available. And there was a place in Fresno that had lots of surplus stuff. So I could, you know, before I could get jobs doing electronic stuff I was cutting people's lawns and helping out with odd jobs and that kind of thing, and for a day's work I could buy an amazing pile of electronics [laugh]—

Zierler: [laugh]

Mead: —on the surplus. So yes, my dad was the one that got me started in electricity and then it was this fellow that moved into camp who got me started in electronics. Not because I hadn't built little radios and stuff, but I hadn't met anybody around that I could really ask questions and figure things out. So by the time I got to high school I had already built things and made them work and so, that was a two-step process. My dad was very, very good at teaching me about electricity and how all that worked and then this other fellow, Mike Blaine, was the guy whose daughter I liked and who, you know, was always pleased to have me around. I think both he and his wife thought maybe the girl and I would get together, but then when I moved to Fresno that wasn't what happened. But otherwise I might have been their son-in-law. So yes, he was an essential part of my early feeling about electricity. It was never mysterious. You know, a lot of people think electricity's mysterious. It's not mysterious. It just works and it works like this. That's what happens when you start out from two years old being told about the power plant and how it works.

Zierler: Carver—

Mead: So—

Zierler: —how were you as a student? Were—did you get good grades in math and science in high school?

Mead: Yes, I did, but largely because we had really good teachers.

Zierler: Mm-hmm.

Mead: There was a little lady that taught us Algebra and she was just dynamite. She just really knew it! The other kids didn't like her because she was very strict and very demanding. But I loved her because she really knew what she was talking about. And I had a wonderful guy who taught chemistry and physics. He was actually the tennis coach, but a great guy. And these people were always around. I could go over to his house if I had something special, I want to talk about. I was welcomed there and that's very unusual for a public high school. And we had this fellow who taught the electronics stuff and of course, I loved that. But I found the math very interesting. Not because of the math, but because it represented physical things. So the teachers I had could say now, this technique is useful when you have a problem like this and it was a physical problem. And so, I could see the mathematics being useful as a representation of physical reality. How would you figure out how much power it took to do this or how much—how would you figure what's the optimal diameter for a water tank to use the least steel to make it out of and for a certain volume of water? And that was what grabbed me about the mathematics. It wasn't as a subject in itself. It was as a technique for being able to figure out things about the real world. because I could see that the radio stuff—you had to know how to analyze it and I wanted to know how to analyze it and math was a useful thing. But I never was one of these math whizzes which just did it because it was a game. To me it was never a game. It was a tool, so I've never been popular with math teachers.

Zierler: [laugh] Carver, tell me a little bit about your decision to go to Caltech. What other schools besides Caltech did you apply to?

Mead: Well, that's interesting. So by the time I was thinking about colleges my dad wanted me to go to Fresno State and be a dentist. But by then I was working at the radio station and I got to know some of the other Engineers there. You know, I would take their shift or I would come on shift right after them and they'd stay around and talk or, you know, I just got to know them. And there was one couple Randy Kyle and his wife who didn't have any kids, so they sort of took a liking to me and we'd have dinner together once in a while, and one day we were talking and he said, "Well, where are you going to college?" And I said, "Well, my dad wants me to go to Fresno State." He said, "How do you do on math?" And I said, "I do well." And then he said, "Well, what you should do is you should go down to Caltech and major in Electrical Engineering." Well, A: I'd never heard of Caltech—

Zierler: [laugh]

Mead: —and B: I didn't know that electrical engineering was a field. Never heard of it. We had one guy at the high school that was sort of a counselor, who would help you with getting ready for college, and he knew about Caltech. So he got me the place to write to get an application and a catalog. And then I knew about Stanford because a lot of the electronic stuff I had read was by Stanford professors—-they were very early on the West Coast –

Zierler: Right.

Mead: and they did a lot of remarkable stuff. So I had heard about those two schools. I didn't want to go to the east coast because it was hard enough moving from the back woods to a big town like Fresno—

Zierler: Sure.

Mead: —but to move to a place in an environment which I had no familiarity would have been a big thing, to say nothing of the expense and all that. So my folks were real good about it. I applied to both schools and Caltech was real interesting: In those days they sent a full faculty member to interview every student in the United States that had applied and passed some minimum criterion. And we had a guy by the name of Jahns, who was a geology professor and he came up. And there were two of us that had applied that year and he interviewed us both and said, "Well, you both look like admissions to me, so I'll make a favorable recommendation. It's not a done deal, but I'll recommend that you be admitted." Which was really nice that he'd just come out and say that [laugh].

Zierler: [laugh]

Mead: Because, of course, you're nervous. So I got admitted both places and my folks were willing to drive me up to the Bay Area and spend a day at Stanford and then drive me down to Pasadena (we were just halfway between). So we went to Stanford and went to their Undergraduate Office, I forget what they called it, and asked them if they had any tours or anything and they said, "No, but here's a map of the campus." So we walked around. Really nice campus.

Zierler: Yes.

Mead: But I didn't get to see anybody, didn't get to talk to anybody, and all I learned was it's a really beautiful campus. And then we drove down to Pasadena and went to the Undergraduate Office and there was a lady there and we told her who we were. She said, "Oh, you're a new admit. Well, we do have tours and they're given by the Y[MCA]. I don't have any scheduled today but let me see if I can round somebody up." And she called around a little and then said, "There will be somebody here in an hour if you can come back." Well, Yes. So we went and got lunch I think and came back and this guy took us around and showed up various places. One of the interests I had as a high school—I had built my own six-inch telescope and made it work—because I'd always been interested in astronomy and there was—in addition to the local ham radio club there was an astronomy club. I would hang out with the guys and they let me look through their telescopes. So I wanted one of my own, so I built one. I couldn't afford to buy one, but I built one. Wasn't as good as theirs, but it worked. I ground the mirror and everything. I learned a lot with that. And so, when this guy was showing us around he said, "This is the astronomy building." And I said, "Well, I've read that on top of this building there's a one-tenth scale model of the Palomar Telescope." And he looked very surprised that I knew that. And I said, "Is there a chance we could see that?" And he said, "Oh, well let's go ask." So we walked in and at the back of the first floor there was an office that said Astronomy, and we went in there. There was a lady there, and our guide said, "These people are with a new admit and would like to see the telescope." And she said, "Oh, here's the key. It's up on the roof level.

Zierler: [laugh] Help yourself.

Mead: So then we went up there and here was this beautiful thing and it was a scale model of the real thing which is—you know, that was—I think for 40 years it was the biggest telescope in the world—and this was the model that they debugged all the design and everything on. It still worked and this guy had never seen it before. So I started explaining to him how it worked and this must be this and it works like this and so forth. And he was getting a kick out of this too because he had had a chance to see it. So we must have spent half an hour up there and it was really neat. And—neat not just because of the thing, but because these people were taking me seriously.

Zierler: Yes, Yes.

Mead: I mean I was being treated as a member of their community.

Zierler: And you felt that at the time. Not just looking back?

Mead: Felt that at the time, Yes. I mean it was palpable from the lady who called over to the Y, from the lady that gave us the key. They all treated me as if I was a member of that community. So we went down and he went in to give the key back and I just went in with him. And I looked at the lady and I said, "Is there any chance if we drove down to Palomar that we could see the big one?" And she looked at me and she said, "Well, they don't give tours, but, you know, if you wanted to take a chance, you could go down there just about sunset and there'd probably be somebody inside that was setting up for the nights observation and if you banged on the door, they may or may not answer it, but you could take the chance."

Zierler: It was one of those moments that really changed everything for you?

Mead: Yes, so we did that, and we got there just as the sun was setting. I went up to the door and banged on the door and we waited. And there was no sign there was anybody in there —but we'd driven a long way and so, I went up and banged on the door again. And nobody answered and after about five minutes I thought I'll try one more time. So I went up and banged on the door even harder and then we waited a few minutes and were just about to leave and the door opened. And there was this big guy, big bright blue eyes, and he was—looked at me and he said, "Yes?" In a sort of "what the hell are you doing?" And I told him I had just been admitted to Caltech and got a chance to see the 20-inch scale model and I just wondered if we could just look at the big thing. And he looked at me a long time, and then he said, "Sure, come in."

Zierler: Mm-hmm.

Mead: And that was Hubble's Research Assistant Alan Sandage.

Zierler: Okay.

Mead: We became friends after I got on the faculty. And he showed us the instrument and how it worked and wished me the best. And that's why I went to Caltech.

Zierler: [laugh]

Mead: Because I was treated like a person and not some lower form of life, and that worked all the way through my undergraduate time there. Some of the classes weren't that great and some of the teachers weren't that great, but I had Linus Pauling for freshman chemistry—-

Zierler: Wow.

Mead: —and he taught me quantum mechanics because he had just written his book: *The Nature of the Chemical Bond*.

Zierler: Uh-huh.

Mead: And It doesn't get better than that!

Zierler: Right [laugh.]

Mead: Because you get the real feeling for the subject. You know, it wasn't just some math that was being spewed out. There were just enough good teachers—I had Bonenbhlust for freshman math—-he was one of these people who taught math with examples from the physical world. So I could do well in it, but the next year I had a pure math type and I barely got through that class because there was no connection with anything physical.

Zierler: Too abstract?

Mead: Yes, Yes. That was before there was a notion of applied math.

Zierler: Mm-hmm.

Mead: So the distinction hadn't dawned on people yet for some reason [laugh]. I had a hard time with those classes because they didn't connect with anything physical. But for instance, I think I was a sophomore and I got a job at the local—there was a local company called Consolidated Electrodynamics. And it was an outgrowth of Herb Hoover Jr's company that he had there in Pasadena for doing electronics for things like oil expiration where they were getting signals coming back from an array of geo-sensors and wanted to record them. They'd either put an explosive in the ground or they would have a big truck that had a big vibrator on it that would do a chirp. They made a recorder that would record simultaneous signals from 100 geo-sensors—-in those days there was no digital stuff. The signals were recorded on a 12-inch moving piece of film by light beams bouncing off little galvanometers, each with a mirror on it.

Zierler: Mm-hmm.

Mead: They would write all these light beams on this piece of film and so, you'd have 100 light beams all crossing over each other. Somebody had to unwind all this.

Zierler: [laugh]

Mead: By today's standard it was just laughable. But they figured stuff out, and I got a job with them as a technician. I guess I did well enough that I got to be sort of an engineering helper at the transducer division. And the transducers were these little galvanometers—-it was the first time that I had to learn about electro-mechanical coupled system. And so I studied up on that. They had to have a magnet for the little coil in the galvanometer—-the magnet, that give it the bias field so the current in the coil would make a torque on it. By that time, I was a junior, and I had just taken the junior lab, which was very out of date. So I volunteered. I went to the Caltech EE people and said, "If I get these guys to donate a galvanometer and a magnet to put it in, I will make an electro-mechanical system lab."

Zierler: Mm-hmm.

Mead: So it turned out they must have liked me well enough because Consolidated agreed to donate one of these to Caltech, and EE paid me for a summer to build the thing. I made a box with a ground glass scale on one end and the magnet block with one galvanometer in it, so the light beam from the galvanometer mirror would be visible on the ground-glass scale. And I used an ordinary audio oscillator to drive the galvanometer coil. The damping depended on the impedance of the source that was driving the coil, and by changing that you could get it to go above and below critical damping and you could figure all that stuff out. And I wrote up a nice little lab for the lab class and they paid me for the summer for that. So it was a continuation of being treated like a person instead of a number on something.

Zierler: Carver, did you find it hard to settle on one major? Did you want to just be in all of the departments in a sense?

Mead: I had liked physics until I went to their freshman physics class and their sophomore physics class and then the junior physics class was the first year they did quantum mechanics the way it's done now which is, you now, you have to learn all this matrix math, Most of which has very little to do with anything physical.

Zierler: Mm-hmm.

Mead: And can only calculate probabilities and none of the intermediate stuff has any physical significance. So I got lost just like I did in sophomore math—-there was no physical connection between the math and the reality that you're trying to analyze. And so, I struggled like hell in that class and I said, "I loved physics, but not the way it's being done." Here I was, a kid that had made a meaningful contribution to a lab that I understood! And they were trying to turn it into something that wasn't physics. So I had another really bad experience. I had been introduced to quantum mechanics by Linus Pauling. I knew what it was about, and this class had none, none, zero! So if that's what they do in physics, I want none of it!

Zierler: Mm-hmm.

Mead: I had had a job for two years as technician in physics. We had the synchrotron on campus, and they needed a RF transmitter basically. They would FM it to follow the beam as it sped up. In the synchrotron, you know, you squeeze it down and it goes faster and so, you have to get the RF that drives the plates in the accelerator to track the rate that the beams going around. And so I built a little transmitter that did that for them and—but the particles didn't mean anything to me and in EE they were doing good stuff. I mean I could make up experiments that understood, like the coupled mechanical-electrical systems, and that's real stuff. Not so trivial to understand. So when I declared a major I just declared EE because these physicists were nice guys and I made some really good friends over there, but the way they did things wasn't the way I did things, so OK, OK. And what I didn't realize is that I wanted applied physics, not what they call "pure" physics.

Zierler: —Did Caltech not have an applied physics program at that point?

Mead: No, Amnon Yariv and I started one in 1964.

Zierler: Mm-hmm.

Mead: After I was on the faculty we both were annoyed with the Physics Department. Well, Murray Gell-Mann used to say, "Oh, that's squalid state physics." [laugh]

Zierler: Right [laugh].

Mead: He was a very dominant kind of guy and he also didn't believe that anybody else's viewpoint was meaningful except his. And so he wouldn't allow any such thing in the Physics Department. But meanwhile, I knew I couldn't do it by myself and when Amnon got here we started working together and he said, "Hey—this sucks. We've got to have a way to give degrees to people doing applied physics, not just this particle stuff which is a tiny fraction of the field."

Zierler: Right.

Mead: —in the—what we would think of as applied physics now.

Zierler: So you really went to EE because there wasn't an applied physics program. That's what—where you wanted to do your applied physics was in Double E?

Mead: Well, it was—yes, yes. And I was lucky that we had two faculty—it was a tiny little department. I think it only had six faculty total.

Zierler: [laugh]

Mead: And we had two guys that knew that the physics of the electrical stuff was important. Like, one of them was Joe Langmuir who was the nephew of Irving Langmuir and a really neat guy. And so, he—when he taught E&M, he taught what it was that made an inductor work the way it did. What did you do physically to make a thing that would have this property? And what the physics of what was inside there? So start with Maxwell's equations and figure out an inductor works. And there was another guy Charlie Wilts who did the same thing with electro-mechanical systems. That's where I learned it. So we had two guys in a department of six who actually could make the connection across from some more abstract representation of the physics to real things that had real electrical properties that used electrical circuits. It's a way of thinking that they don't have in physics. And I was just lucky that there were two guys that actually had that way of thinking. So I ended up doing my thesis when transistors were just new and I was fascinated by these things because I'd been working with vacuum tubes all my life.

Zierler: Right.

Mead: And transistors were interesting! We even had a guy on the faculty that taught about the physics of vacuum tubes. The first level of that is really neat because you can learn about temperature limited and space charge limited current and that's really neat. But then they wanted us to learn the physics of a round vacuum tube which had grid wires that were vertical and get some transform to put them in some coordinate system that you could analyze. Not Interesting! By then I had surplus World War II vacuum tubes that had parallel plates, so they were much more interesting than these things with, you know, glass and stuff. And they also had one-dimensional current flow. They were just like the idealization.

Zierler: Mm-hmm.

Mead: So why should I [laugh] go through all this mathematical rubbish when it isn't the ideal thing anyway?

Zierler: Right.

Mead: So I had trouble with those guys because they're not thinking about it right. Just get the right vacuum tubes so it is the ideal thing and then you can tell if it really works or not. So anyway—so I ended up in this in-between stuff. Dave Middlebrook had been at Stanford, learned transistor physics from the guys there. I think John Moll had moved out from Bell Labs to Stanford. If I'm right, I think he was the first Bell guy from that transistor group to get on the west coast. And so they were ahead of Caltech in the whole transistor thing. So Caltech hired Dave Middlebrook who, I think, was one of their first graduates there at Stanford to do this kind of stuff. Dave had written this little book called the *Introduction of Junction Transistor Theory*. He had a very simple explanation of the physics in one chapter there, but I found him to be really interesting and a neat guy. Good teacher. So I signed up with him.

Zierler: Take your time.

Mead: So I'll be right back.

Zierler: All right.

Mead: Hi, David.

Zierler: Hello.

Mead: So anyway, I was talking about Dave Middlebrook.

Zierler: Right.

Mead: And he was our transistor guy and he had done enough physics to have one chapter in his book on it, but he was really a circuits guy. And the circuits were interesting, but I got more and more into the fact that the transistors we could get weren't very good and they had a lot of limitations. The first year I was working with Dave we had some guys come up from a little company down by the Los Angeles Airport called Pacific Semiconductors, Inc. which of course had a Greek Psi for a logo [laugh].

Zierler: [laugh] Right.

Mead: And nice enough guys. Of course, I had been reading voraciously all of Shockley's stuff and of the stuff out of Bell Labs because you just had to. They had published a bunch of stuff. In those days the literature was funny because we had people like John Moll and a guy by the name if Ebers at Stanford who had written his authoritative analysis of a transistor, so they wanted to make a little model of a transistor. And it turned out that they had made this small signal model of a transistor which was fine, but I knew that most transistors, even then, were not used for small signals [laugh]. The linear approximations were never very good – transistors had in them these junctions that were enormously more non-linear than vacuum tubes were. So you had to think about them differently even to use them for small signals. And if you're going to use them for large signals they had even bigger issues that you did not have with vacuum tubes. So they were a new thing and all of the vacuum tube stuff was only partly relevant and I knew this because I was voraciously reading all the stuff I could, because that was what I wanted to do somehow was transistors. And yes, circuits, but the circuit got limited by what the transistor was doing in plenty ways that I didn't understand. So these guys showed up from Pacific Semiconductors and we started talking and it turned out I had read a bunch of stuff that Dave had not read. So by the time we had lunch they offered me a consulting job as a first year grad student [laugh].

Zierler: [laugh]

Mead: Well, that was great because I already had two kids and I had to support them.

Zierler: Right.

Mead: And also I can learn something.

Zierler: Mm-hmm.

Mead: So it turned out I was just lucky, like I had been at other times—-things work for me when I can relate to people in a creative way.

Zierler: Mm-hmm.

Mead: And there was a guy there by the name of Jim Buie, and he was down to Earth—I think he had a Bachelor's Degree from somewhere. And he did experiments and he invented things and I loved to talk to him because he was very smart, and he had a physical way of thinking which I could relate to. So we got to be close collaborators—when I would go down there I would spend half of my time with their materials and fabrication people. They were making one -inch wafers of silicon and making junction transistors that would work at high powers and high frequencies. And they had the only ones that really worked, so, they could sell them to the military. They were right down by LA airport with a lot of other World War II defense people. And a lot of those were heavy in electronics, so—Pacific Semiconductors later on got bought up by TRW. And now it's a division of Northrop Grumman.

Zierler: Okay.

Mead: Northrop Grumman Space Technology it's called now. And they make all kinds of III-VI high frequency semiconductors. But back then they were leading edge on the high frequency Silicon transistor stuff. When I went consulting there I would go in and see Jim Buie because he always had interesting things—I learned a lot from Jim and I think he liked somebody to ask him questions and try to understand what he was saying because I don't think many people there understood what he was doing. And so, one day he said, "Hey Carver, look at this." I looked through the microscope and there was a transistor-like thing that had five emitters on it and I said, "Jim, what the heck is that?" He said, "That's a way of doing logic which you have more than one emitter, and any one of them will make current go out the other side." He had invented TTL, which was, for many years, the dominant logic family.

Zierler: Mm-hmm.

Mead: And that was the first notion I had of how you use physics to make logic. The guy that invented TTL taught me how transistors make logic [laugh]. I mean, it doesn't get better than that!

Zierler: That's right.

Mead: So I was learning from the guys who were doing it. And in those days the thing that always annoyed me about the EEs is that, except for a few of them, they would teach all this linear small signal stuff and nobody ever talked about what happens when it's nonlinear. Well, already a whole lot of circuits worked in switching mode, like switching power supplies and digital logic. And even then—this was in 1960-ish—that was a thing that was happening. I could see that this small-signal linear stuff was a tiny, tiny fraction of electronics. After all, how many audio amps you need to make? [laugh] And I had been very annoyed by this Ebers, Moll paper which had all these complicated modeling equations. Then a paper appeared, I think it was around 1960 or '61 by John Linvill, who was a prof up at Stanford—- I think he was a second-generation faculty there—roughly same generation as Middlebrook. And he had written this beautiful paper where he had the most beautiful insight. He said, "Look, inside the transistor you have minority carriers and they work by drift and diffusion and those are linear in the density of minority carriers. And then you have the outside of the transistor where you have circuit elements which are linear. The only problem is you have these junctions in between which are exponential, so why don't we just say that?" So he built this model which made a lumped approximation for the diffusion of minority carriers in the piece of Silicon or Geranium or whatever in the base region. Then there were two boxes which had the exponentials that converted the emitter-base voltage into the carrier density at the emitter and the collector-base voltage into the carrier density at the collector. And so, it was a two step process. Well, it was beautiful because it was straight physics, straight from the device. No bullshit about linearizing an exponential which is problematic, and I just fell in love with that. It was great because I can solve switching problems and there were no papers.

Zierler: [laugh]

Mead: One of the things that happened with either a diode or a transistor is you would forward bias a junction to turn the device on, and that would flood the base region with minority carriers, which made the base region a good conductor and you could switch a lot of current. And then you would reverse-bias the junction and it would still be a good conductor because of all the minority carriers that were stored there—-the density would get smaller, and smaller, and all of a sudden WHAM! It would turn off. And that was because minority carriers were in there, and until you got them out of there they were still carrying current. So I made a little model for that physics, and I measured power transistors that showed this minority carrier storage effect, and it all fit with John Linvill's lumped model, so I could actually get real equations that gave me the non-linear behavior of the transistor in switching mode. I figured this all out and John Linvill hadn't done it. We later became good friends. Wonderful guy! I was very pleased because I had figured this out and nobody else had figured it out. It was simple. So I wrote my thesis on this phenomenon, that was half physics, half electronics. What do you call it? Well, it later became device physics which became a field of its own. But in those days it was just trying to figure out how things worked and it was half electronics and half physics. So in terms of who I am and where I came from, that's the trajectory. You can probably see a theme running through that.

Zierler: Absolutely [laugh].

Mead: [laugh] I think if it's OK with you, we can stop there.

Zierler: Okay.

Mead: And do another one soon?

Zierler: Absolutely, Yes.

[End of recording]

DAVID Zierler: OK. This is David Zierler, oral historian for the American Institute of Physics. It is July 5, 2020, and it's my great pleasure to be back with Professor Carver Mead of Caltech. Carver, thank you so much for joining me again.

CARVER Mead: Good to see you, David.

Zierler: OK, so let's pick up on where we were from last week. We talked about the defense of your thesis, and the origins of what would become known as device physics. So, my first question is, was that term bandied about at the time of your defense, or this would only come about later as more people got involved in combining EE and physics?

Mead: That's a good question. It's funny. I don't remember when the term came into use. I just don't remember. I don't think it was in common use when I was doing my thesis. I looked at the minority carrier storage problem with minority carrier devices. That problem is why they're not used anymore, or used very little.

And I just looked at it as a problem that needed to be solved. Whether you thought of it as a circuit problem or a device problem, it was both, and it needed to be understood. And so I just did that. And it isn't obvious that that term was used at the time. It's an excellent question. I just haven't thought of it before.

Zierler: Maybe we can get at it this way. What—did you think of your thesis in your own sort of scholarly identity as multidisciplinary? Did you see your work as, you know, necessarily combining two distinct fields?

Mead: Yes, it was—here was a thing, an emerging class of electron devices that were obviously going to take over electronics and—

Zierler: And why obvious, Carver? Why was that obvious?

Mead: Just power. It was so clear that if you were going to build anything at scale, it was already known that the big computers were limited by the power to run the vacuum tubes. So it was just obvious to everyone that if you had a low-power, high-performance solid-state device, that would be the way electronics was done. Of course the devices we had in those days were pathetic, but they even then took over applications that they could get to with the performance they had.

So the performance of the device were limiting what was obviously the next wave of electronics. And that was obvious. It was certainly obvious to me, and I think obvious to everyone. Although I remember the [laugh] long story about my involvement with people at General Electric Company down in Owensboro, Kentucky, where they had a major facility. And they had developed a marvelous little vacuum tube that was about a quarter inch in diameter, and you could stack up in these quarter inch little tubes. The technology was called TIMM (Thermionic Integrated MicroModule. And it was a fantastically innovative technology.

And what they did is—they would stack them up—they had little cylinders of aluminum oxide ceramic that were metalized on the two ends. And then they had a bunch of prefabricated little discs that they could put in between neighboring cylinders and stack up and make a sort of a log, a long cylinder out of it. And these little discs, they had one for anode, one for cathode, and one for grid. And they would stack them up. Each disk was round, and had a little tab that stuck out to the side. And they had I think 12 positions that it could stick out.

And so they would make a stack, and the entire assembly was going to be like 10 centimeters long. And they would stack up these things. If they weren't connected, they'd leave a little space in between. But you could make things that had common elements. And if you wanted to connect elements that were not common, you made the tabs in the same orientation, and then they had a wire would connect all the disks whose tabs were in the same orientation. Then they put the whole thing in a vacuum oven, and run it up to a temperature where the metal that was on the ends of the little ceramic tube sections would melt and it would braze into the little discs and make a single unit out of the "log", and the whole thing then became an assembly of vacuum tubes. They could make diodes and triodes and connect them together this way. And then they would make a bunch of those logs, and stack them up, and then they had wires that ran to interconnect them from the ends. So it was an integrated circuit made of vacuum tubes. And they put it in a little thermal insulating box, and they had a little heater in there, and they'd heat it up to the point where the cathode started to emit electrons. And then current would start to flow in the circuits, and then the dissipation of the thing kept it at the right temperature.

It's an extraordinarily ingenious use of vacuum electronics. The little discs were made of I think titanium. But titanium if you heat it up in a vacuum like they did when they processed the thing, it was really hot because they'd melt the metals—not the titanium but the braze that they used to hold it all together. That also sealed it, of course.

And at those temperatures, you get all the dissolved gases out of the titanium so then when it cools down, it's a wonderful getter. And at the temperature they'd run it, it was a good getter, so that the vacuum became extremely good inside. Fantastic technology! And I remember one of the GE guys telling me, he said, "You know, these solid-state guys, they just don't know what they're up against, because we've got them going away." And it was true compared with individually packaged transistors. They were never going to get close to this vacuum technology. They were thinking individually packaged electron devices because that's all there had ever been. So if it weren't for the integrated circuit, we'd be making our computers out of thermionic integrated micromodules. [laugh]

Zierler: [laugh]

Mead: And they'd be working really well, and they would be orders of magnitude—probably two orders of magnitude better than what you could do with plug-in vacuum tubes of the day. So, they were right. If the integrated circuit had not been invented, that's the electronics we'd be working with.

Zierler: Carver, I wonder if you can talk a little bit about—you know, the technological limitations are very clear, you know, in terms of what you're discussing in the late 1950s and early 1960s. What were some of the theoretical limitations that you were coming up in this new interdisciplinary field that you were quickly rising in?

Mead: Oh, that's a good, good question. Of course, for me, it seemed as though the minority carriers were the problem, and my work for the thesis had been on the problems they caused and what the application issues were that came from those device problems. And so it was just clear that they would never be an ultimate solid-state technology. And so here I was having just written a thesis on why these things are a problem. [laugh]

Zierler: [laugh] Right.

Mead: —and how you analyze it, and how you quantify it, and so forth. And that same year I think, which would've been 1959.

Zierler: You defend in 1960, I believe.

Mead: '59, actually.

Zierler: '59, OK.

Mead: But I missed the parade—

Zierler: [laugh]

Mead: —I nominally got my degree in 1960 but I defended in '59. So early, early '60s, they wanted somebody at Caltech that did what we now call device physics. And it was clear that Middlebrook did transistor circuits, and he had a passing acquaintance with device physics but that's not what he really did.

And here was this kid that had certainly gone off on his own, and done this thesis on device physics, and those people are in short supply. So they asked me to stay on in the faculty. I had been teaching a course as an instructor for a couple of years anyway, so I was a likely suspect. They don't like to hire their own graduates. But I guess you do what you have to do, right?

Zierler: [laugh]

Mead: But it was probably '60 or '61. I could pin it down for you, and I'll tell you how. We had a visitor to give a seminar, and his name was Leo Esaki. And he had invented this thing which we called an Esaki diode, but he called it a tunnel diode. And it used electron tunneling to make a negative resistance device that was very, very fast because there were no minority carriers.

So I went to his seminar, and it blew me away. OK, here's a way to make electron devices that don't have minority carriers, so that then became a quest for me. Yes, you need to make devices that don't have minority carriers because they're the root of all evil in terms of getting high performance in solid-state devices.

Zierler: And how do you define "high performance"?

Mead: Fast, and low power, you know, you can define it several ways. We'll get to that. But that seminar changed my life. And they tried to hire Leo Esaki. He was basically hunting for a job in the US, and they made him a faculty offer, and so did quite a number of other people. But he ended up at IBM, and got a Nobel Prize for that tunnel diode—-well-deserved.

And later, we became friends, but weren't close collaborators or anything, but I would see him at meetings and stuff. And I'd give a talk once in a while at IBM—he was a smart guy. So I fell in love with the idea of electron tunneling because you didn't need the other kind of carrier, which was what limited everything about a minority carrier device. The Bell guys actually started out to make a majority carrier device, but I didn't know that. And I'll get to that part of the story later because it's a fun story.

So I was determined to—instead of a negative resistance two-terminal device—-to make a three-terminal device with electron tunneling as the mechanism. That's a good thing to do. And so I started making thin films—-just a metal substrate, evaporate a thin oxide on it, and then I put little dots of some other metal on it, and then tunnel electrons from the metal substrate, through the thin oxide, and into one of the metal dots, because I wanted to understand this tunneling stuff. And, you know, the first experiments were crude. But within a couple of years, I was able to get a lot of information and a lot of understanding about the tunneling.

You could make the thicknesses different (I used the capacitance to tell how thick the oxides were), and that made the tunneling probabilities different, and from that I could work out what the propagation vector was. The propagation vector of the electron gets to be imaginary in the insulator. And I could figure out what that imaginary part of the propagation vector of the electron was. So I was up to my ears in the quantum nature of electrons, and how they behaved in these forbidden gaps where they're not supposed to be. So, anyway, within a year or so, I had become good at the measurements that it takes to figure out what the electrons are doing as they tunneled through various insulators. And I started writing papers about that and giving papers at the conferences. By then, there was an IEEE workshop on electron devices. In those days, you could gather together all the people that were doing leading-edge work, and put them in one room, and there might be 30 of them. And so it was a great place to get to know everybody that was doing related stuff, and hear what they were doing, and have arguments over dinner, and it was just marvelous. What had happened was when I did my thesis, we had connections—by "we" I mean the Caltech EE department—had connections at GE Research Lab and RCA Research Lab, which were both major leading-edge research labs in those days. This would've been early '60s. Nobody knows RCA anymore. But they were a leading supplier of transistors in the early days.

Zierler: And did RCA have a culture of basic science like a Bell Labs would have?

Mead: Absolutely, it was a research lab that was set up—there's a story that I'm only going to remember part of. But as radio became a big market, then a group of companies pooled their radio intellectual property into a particular company, and they called it Radio Corporation of America, and that became the place where they made the vacuum tubes, they made the radios, it was the big leading place. And they had this research lab where they had developed Television, which involved lots of really good technology.

Zierler: So if this was your area, this is where you would go even over an IBM or a Westinghouse or a Bell Labs?

Mead: IBM had not yet gotten very far in this business. Bell Labs of course had invented the transistor, and they always pretended to be the only ones that knew anything. And that earned them lots of enemies because everybody else was doing a bunch of stuff that rapidly outran what the Bell guys were doing. And the Bell guys didn't like to admit that. And there were a lot of humorous incidents that I can tell you about. But it meant the rest of us kind of had a common enemy. [laugh]

Zierler: [laugh]

Mead: And so we made lots of friends over, you know, "we're going to get those guys!" [laugh]

Zierler: Right. [laugh]

Mead: And there was a great competitive environment. I think that helped. Just gets the juices flowing, you know. I got invited to RCA to give a talk because they had heard that I was doing interesting work. And there was a guy there by the name Al Rose who had been very active inventing the picture tubes for television. RCA was the place way ahead in television. And Al had been a key guy at the device end of it for picture tubes. There were Iconoscopes and all kind of "…oscopes" that were used as picture tubes. But the way they worked is the image that was shined on the face of the picture tube fell on phosphor that would then release electrons. And the early ones worked just like that.

But it turns out if you get one electron per photon, you need an awful lot of light to make a picture. So the RCA guys worked very hard to put gain in the phosphor, and that's a physics problem. And they were able to do that to get gain of I think thousands in the phosphor itself. So you get 1,000 electrons out for every photon in. Well, that's a major technology, and Al was one of the guys that had contributed a lot to it. There's a similarity there—-every pixel's an electron device that has a gain of 1,000, right? That's a lot of physics in there.

So, of course, he became the guy that related to the device physics of these new amplifying transistor-like things. And he heard that I was doing stuff, and so he invited me to come to RCA and give a talk, and then took me out to dinner, and he sort of took a liking to me. And he was the one person that saw something in this young whippersnapper that didn't have a first clue of any of the stuff that East Coast people were experts on. I had never had a course on any of it—Caltech didn't have a course in solid-state physics! I had never heard of it. And our physicists acted like they had never heard of it.

So, but anyway, Al sort of took me under his wing, and got me invited to these little meetings, and invited to give talks there. And so that was the way I became a member of the active people in the field. And without Al Rose, I would've been more like an outsider looking in. It was bad enough the way it was, but that would have been much worse!

Zierler: Carver, what do you think Al saw in you that you might not have even seen in yourself at that time?

Mead: I suspect it was the fact that I found my own way of thinking about things instead of remembering what I read in a book someplace. I read a lot of stuff but, you know, it's different than taking a class, and memorizing stuff, and all that. And I think he probably had done that himself, and appreciated it. And he was a wonderful mentor. In just the little bit of time that I was able to spend with him, we developed a real bond.

Zierler: What did you learn from him?

Mead: All kinds of stuff. I remember one time I was trying to calculate something about this transistor, and Al said to me, "Well, Carver, the current is the charge in transit divided by the transit time, just that." Duh—of course it is. But Al had a way of seeing the essence of the thing. I'll never forget the time he said that. Yes, you could just eliminate the people writing these god-awful equations to try to represent what was happening in a transistor. And here's one equation, Click! It just captures the whole thing. And then you have to figure out what those two numbers are, but that's it in a nutshell.

And so it was a lot of his way of thinking, and just those little nuggets that I would treasure. When he gave a talk, he would always drop one or two of those clicks. So, he was a wonderful, wonderful coach, even though I didn't spend much time with him. Years later, we got him to come spend a sabbatical at Caltech—many years later—and I was able to thank him for really being my coach.

Zierler: Carver, when did—

Mead: And that's really—Yes.

Zierler: When did you first connect with Gordon Moore?

Mead: Oh, interesting. It was 19…

Zierler: Before you defended, like '58, '59, around then?

Mead: I think just after. I think it was '59. Because I remember I was teaching an undergraduate class and lab, the first one at Caltech that had ever used transistors. They had a circuits class they taught to, I think, juniors that talked about how you made circuits out of devices. And it had always been done with vacuum tubes, and they said, "Hey, you're into transistors…do you want to take a run at it with transistors?" And I said, "Sure, I'll do that."

Gordon was a Caltech grad—He graduated four years before I did, and was one of the founders of Fairchild Semiconductor. And so, of course, it was his job to come back to Caltech and recruit. So he would go around and visit faculty people and ask them if they had any really good students. And somebody said, "Well, go see Carver because he's teaching this transistor course." And of course, Fairchild was a transistor company, so it made sense.

So, Gordon waltzed into my office—I was working away on something—and, unannounced, and said, "Hi, I'm Gordon Moore from Fairchild." And I of course knew about Fairchild because they were obviously a leading-edge place by then. But I'd never heard of Gordon Moore. And I said, "Oh, hi, how are you?" —you know—I'm delighted to have a connection there. And he said, "I understand you're teaching a lab with transistors." I said, "Yes."

And in those days, the transistor we had was a CK722 made by Raytheon, and it cost $1, and this was in 1959. And they were very fragile, and their upper frequency was only about a megahertz. And I had read about the Fairchild transistors, but they were tens of dollars, and no way I could afford one. And the students would often burn them out anyway, and so you needed more than one. So, I said, "Yes, I'm teaching a class." He said, "Would you like some transistors for the lab?"

Zierler: [laugh]

Mead: And I said, "Yes." [laugh]

Zierler: Yes, please. [laugh]

Mead: And so he turned around, and he had one of these—I don't know if you've ever seen one. The old—there was a style of briefcase in the old days that's sort of like a clamshell on the top, and it opened up like this. He had one of those. And he opened it up, and he reached in, and he had a sock, and then there was another sock. And [laugh] I was looking at this, and then he turned around with two socks in his hand, and he said, "I travel light." [laugh]

Zierler: [laugh]

Mead: A wonderful man. And that was my first meeting with Gordon Moore. And then he reached in and he pulled out a manila envelope, you know, 8½ by 11 manila envelope and it was bulging. And he said, "Here, these are 2N697s." (the 697 was their first product—-it was a core-driver for magnetic-core memories—-it was not a device you'd really want to use for logic or anything because it was for higher currents).

I'd never seen so many transistors! But then—he reached in and he pulled out another Manila envelope bulging just as well, and he said, "These are 2N706s." The 706 was their first logic transistor, and it was beautiful! And of course I knew about these devices, I just couldn't afford one. And so that was our first meeting.

And then he got to asking me what I was doing research-wise, and I told him about some of the tunneling stuff, and some of the work I'd done in the thesis. And he said, "Would you like to come up and give a talk about that?" "Sure." [laugh] So, two weeks later, I flew up there and visited. My freshman roommate at Caltech (Pete Lauritsen) had gone to work there, and so I visited with him, and he showed me what was going on what he was working on. And then I got to meet other people.

And then Gordon and—I think it was maybe Bob Noyce, but I'm not absolutely sure. But they took me out to dinner, and we had a great discussion of which I don't remember the details. But at the end of it, they said, "Would you like to consult for us?" Well, I'd just gone to heaven, because they were the place doing the leading-edge work in device physics, and I could work with them. And I knew that was my big break because I had been consulting—I think I told you—with Pacific Semiconductors, and that was a fantastic experience. And these guys were the next step beyond that.

And so I had—by stumbling into it—had been able to collaborate with the leading-edge people in the field. And what's your chances—that a kid that had no background in any of this would get that chance. It was wonderful! So I began my every week commute up to Silicon Valley in early '60s, and I don't remember if '61 or so, but it was in there.

Zierler: Was it called Silicon Valley even back then, or you're sort of putting this name, you know, in retrospect?

Mead: Oh, no, much later, much later. These companies were invisible at the time—I mean, there were big companies there like Hewlett-Packard who made instruments, and Eitel-McCullough, and Varian, who did big vacuum tubes, and these were the big electronics companies. And little Fairchild was, you know, just a little start-up, and nobody was paying much attention to them.

Zierler: Carver, can you describe a little, so that we understand the historical context, the aura and the exoticism of the transistor? Why exactly is it so special, and what are the major problems that it seeks to resolve?

Mead: The transistor was a holy grail because of the size and the heat that vacuum tubes took to make any sizeable electronics. The radios of the day were six-tube or eight-tube radios. And people worked hard to get two tubes inside of one so that they could call it a five-tube radio, and the tubes gave off a lot of heat. And that was just an AM receiver!

The first application for a transistor that really took off was the hearing aid. I remember my grandfather had a hearing aid that had a vacuum tube in it. And he carried around this big thing on his belt that had great big batteries in it, and this box that had a single little vacuum tube in it, and then little wires that went up to his ears. And it was just an audio amplifier. So that was the first thing to go to transistors because they used much less power. And to make an audio amplifier doesn't require high performance.

But I think everybody got it that we could make them faster. We just had to figure it out. And so everybody knew it was going to be the way you built computers in particular. So, in a way, it gave us all a common vision. We all had different ideas about how to get there, of course. And that made it fun because you could argue over drinks would this be a best way or would that be the best way? But we all knew in our gut that it was going to be the way electronics went.

Zierler: Carver, did you ever think about entering industry full-time, and leaving Caltech?

Mead: Yes. But once I started consulting with the Fairchild people, we got this dynamic going where they had lots of people working on projects, but they had to get product out. And a product had to work, and it had to be reliable. And that was what they did. They had to do that because it was life or death for that little company. And so they had lots of physics problems that they couldn't spend time on.

So I'd go up there on Thursday night, and stay over, and then end up early morning at Fairchild Research Lab. And Gordon was an early guy too. So, about seven o'clock, we'd both be there, and we'd sit and talk for an hour, and then other people would start showing up, and I'd go off and spend time with the people doing the stuff that I thought I could help with. And then at the end of the day, I'd come back, and Gordon and I would talk through what I found out and what I did and all that sort of thing. And then probably once a month, we'd go off and get Betty and go off to dinner together, and just get to know each other better.

And the people I'd work with, they'd have these issues or sometimes there were problems, sometimes they were opportunities. But I looked at them as research opportunities. So, there'd be some question come up, and so I'd say, "Hey, give me some samples, and I'll go work on that." Well, for them, it was like having another person that could work on stuff that they didn't have to tend or spend money on or anything.

And for me, it was a source of leading-edge structures—semiconductor structures—and that I could measure and figure out. And then it became clear that being at Caltech was a heck of a lot better than being in industry because I didn't have to just get the thing shipped, I could go understand it, and yet I had access to the technology. So that started a run of quite a number of years of just this synergistic working together, and that was marvelous.

Zierler: So consulting allowed you to—it was really the best of both worlds. You had access to the technology, but you could remain in an academic environment.

Mead: Exactly, and go at it my own way, and it didn't have to be solving this particular production problem.

Zierler: Why not make your commuting a little easier, and set up shop at Berkeley or Stanford?

Mead: Well, I did end up chatting with the Stanford guys at one point, but nothing ever came of it—I'm not sure why not. But I was going strong, and it would've taken a lot to make it worthwhile to move.

Zierler: And, Carver, what is your home department at Caltech after you defend, and you're part of the faculty? Do you have joint appointments?

Mead: No, it was always EE. We had Murray Gell-Mann in physics, and he called what I did "squalid-state physics." Dick Feynman always liked the stuff I was doing, and we collaborated a little a few times, and ended up doing a course together in 1981, but that was much later. They were basically hostile to device physics—especially since it was so applied. I mean, there were these devices, and there was physics in them but it wasn't *their* kind of physics!

Zierler: Would Gell-Mann ever come around? Would he ever regret calling it squalid-state physics?

Mead: No, he never did. But Steve Wolfram did, who was Gell-Mann's star student. Much later when I was doing the neuromorphic stuff, he would come to my group meeting once in a while, and we'd have dinner—-it was an amiable relationship. But then, it just wasn't way they thought—except for Dick.

And he and I could relate because he had a way of conceptualizing things that was really good. And I had followed him around when I was student. And whenever he'd give a seminar, I'd show up and listen—I couldn't understand the math at all, but the way he described the physics I found really inspiring. So I would always go to his talks, no matter what they were on, because he had that way of conceptualizing things.

Zierler: Carver, I wonder if you can talk a little bit about how your consulting shaped your research agenda? In other words, would you take your cues from what was going on in industry, and then use that—these developments as a starting point for the kinds of things that you wanted to research on campus, or would you start with ideas on campus, and then take these ideas to your friends in industry, and see how they might be applied? How did that process usually work?

Mead: It was inextricably interwoven with what was going on in industry—not just at Fairchild, but I knew these other people at RCA and later on at Ford Research Labs. And some guys in the Netherlands—Philips Eindhoven had a research lab, and I had some good friends there, and we collaborated on stuff. And so, fortunately, thanks to Al Rose—and my introductions to people around the world at these meetings, I was always getting in discussions with these other people, and there are cross-links there all the time.

And the closest collaborations were at Fairchild but I also consulted for Texas Instruments at the same time I was consulting with Fairchild. And I just told them, "Look, don't ask me about the other company. You wouldn't want me telling them about what you're doing, so I don't carry stories." And so I did that for quite a number of years. And I got samples from people at TI and at other places. So it became a way of life of collaborating with people, and doing joint projects—-we could each bring something important.

So—-how the ideas started—any of these things, by the time it became a research project that was well-defined, it had been through an awful lot of just thinking about "What if we did this? Could we do that?" You know, that sort of thing. And, "OK, you have this kind of material. Let me measure it in this way, and then we'll see what happens", and then we write a joint paper. And that was a marvelous way of working.

I've always enjoyed collaborating with other people, and it's become sort of a way of life with me. I enjoy the discussions and the fact that somebody has a different set of ideas, and the camaraderie that comes from working together on a hard problem. And so, in a milieu like that, it's really hard to remember who had what idea. And the most important collaboration that I had—let me take a break.

Zierler: Sure thing, take your time.

Mead: And then we'll talk about my first important collaboration, OK?

Zierler: OK.

[pause]

Mead: So, this actually goes back to the tunneling story, which we were in the middle of, and then went onto other stuff. I was making these in films and understanding the tunneling, but I had an ulterior motive, and that was to make it a device that worked by electron tunneling. And the idea was that if you were lucky in the tunneling process—the tunneling process is inherently lossless. But of course there can be stuff in the forbidden region of whatever you're tunneling through that disrupts that. But suppose you're lucky—I mean, these are very thin films. They were, in those days, 40 or 50 angstroms. And I would put a high electric field on them, and electrons would tunnel through them. And when the electrons came out the other side, if you were lucky, they had a lot of energy—-a few electron volts. And that would be enough that, if the electrode—the second electrode, the one that has that lower energy, so they're tunneling into it—if it was thin, they could go all the way through it, and come out the other side. So that would be a controlled source of electrons. Well, that's what a transistor is, a controlled source of electrons, and you pick them up somehow.

The first way I tried to pick them up is I made the little devices very thin, and then I put them in a vacuum, and put a positive piece of metal near the surface and measured to see if any electrons came out. And, lo and behold, some electrons came out—not a lot. It was like a 10 to minus 4 of the electrons that went in. And it depended on the thickness of the metal how many came through. So, I was getting information about how electrons lost energy, a high-energy electron (like a few eV over its Fermi level in the metal) how far they went before they scattered because the fraction that would come out went down exponentially with the thickness of the metal. Well, that was interesting!

There weren't as many as I thought, but they were nicely measurable. So I had done these measurements. Also, instead of putting the electrons out into a vacuum, I put another insulator, and then another metal, so it was a triode, and I could get electrons into the third metal as well. I did both experiments, and worked up what was happening in the metal. And the physicists, of course, had data on how far electrons went in metals because they had their electron beams. And they showed me these plots that the lower the energy, the bigger the attenuation. So by the time you got down to a few eV, they weren't going to go anywhere at all.

And they were happy to make clear to me that I was in a dead-end street. But actually, what I was seeing was a well-defined range of electrons that was around 10 angstroms. It turned out that what happened was as the energy electrons got lower and lower, they scattered worse. But then when they got down to a few eV, there weren't that many states for them to scatter into, so the range went back up again, and nobody had measured that. So, it was one of these meetings out on the East Coast someplace, and I gave a talk about this. And it turned out there was another guy there from Bell, a guy by the name of Bill Spitzer, who had done a related experiment by shining a light on a metal. By knowing how far the light went into the metal, he could figure out where the electrons were coming from. And he was seeing that they were coming from pretty deep in the metal by photoemission.

And our numbers were comparable, so we got together afterwards, and we were both very excited because this was a new finding in the world of physics, and nobody believed it. And he had the same experience I had, that people told him he was nuts. And so we had dinner that night, and we had a great talk about all this. And he said, "Hey, next time you're on the East Coast, come by, and I'll show you the lab." And I said, "Great. And, if you're going to get down our way, come back Caltech, and we'll host you, and you can give a talk."

And, you know, we were going to do this. So, next time I was going on a trip to the East Coast, I wrote to Bill at Bell, and said, "I'm going to be on the East Coast on such and such a day. I'd love to come visit you." And in those days, we wrote letters to people. It's inconceivable today, but that's what we did. Well, I didn't hear back from him, but he had said "I'll be around."

So I went to see RCA and then I showed up at Bell, and told the lady at the desk that I was there to see Bill Spitzer. And she was taking an inordinately long time to find Spitzer. And finally I came back to the desk, and said, "Is there a problem?" She said, "Well, he doesn't work here anymore."

Wow! So then I said, "Well, do you have a contact where I can reach him?" And she said, "Well, let me let you talk to personnel."—what we now call HR they called it personnel back then. So she put me on with the lady in personnel. And she said, "Well, I have an address and a phone number, and he's at Bell and Howell Research Center in Pasadena, California, and here's his phone number." *He's in Pasadena!*

Zierler: [laugh]

Mead: So, I thought, "That's great. We'll be able to work together." I imagined that, if he was going to move, he probably got real busy, and couldn't remember to tell everybody. So, when I got back to the lab, I called him over at Bell and Howell Research Center in Pasadena. It was right on my way from home to Caltech.

And I said, "Hi, Bill, this is Carver." "Oh, Carver, oh, I forgot to tell you I was moving out here." And I said, "I bet you don't have lab yet." And he said, "You're right." I said, "Well, until you get one set up, how'd you like to come over, and we'll work together over here?" And he said, "Oh, that would be wonderful."

Zierler: [laugh]

Mead: So he came over the next morning, and we had breakfast together. In those days, there weren't many journals—there was the *Physical Review*. I don't think there was a *Physical Review Letters* yet. It came shortly in that time period. And there was a *Journal of Applied Physics*. And so basically, every month when you got your mail, you'd look at the table of contents of those two journals, and if there was anything new, it'd be there. And, of course, the *Journal of Applied Physics* was getting loaded up with device physics papers, because that was the place where things were happening.

Zierler: Not squalid-state anymore, right?

Mead: Not—the Caltech physics department hadn't figured it out, but everybody else had.

Zierler: [laugh]

Mead: So, there was a paper in the latest issue by Bube and Williams who were at RCA, and I knew them of course because I had been by there several times. And they had made Schottky barriers by putting Copper on cadmium sulfide. It was a II-VI semiconductor, and it was in the class of materials that RCA knew a lot about because of the phosphor work I told you about. So, they could make crystals of them, and they could do all kinds of measurements. So it was natural that they would look at that class of materials, not just the group 4 materials like germanium and silicon. And so they had shined light on a Schottky barrier on a metal—a little metal dot on the surface of cadmium sulfide.

And, of course, when you shine a light on it, it excites electrons in the metal, and some of them go across the top of the energy barrier and get into the cadmium sulfide. The number that can get over the barrier from the metal changes with the photon energy, (the wavelength of the light), so you can figure out how high the barrier is. They had worked the thing out, and had all the theory and everything, and it was a really beautiful paper. And it had just come out the day before. So when Bill showed up for breakfast, I said, "Did you see the Bube and Williams' paper in *JAP*?" And he said, "Yes."

And I said, "I thought it was a really beautiful paper." And he said, "Well…" I said, "What's wrong? You know something I…?" He said, "Well, I stopped by RCA just before I moved, and did you notice that they used copper as the metal? And did you notice they deposited it out of a chemical solution instead of vacuum-evaporating it?"

I said, "Yes, I thought it was a little odd." He said, "I asked them why they did that? And they said that if they vacuum-evaporated it, they didn't get the photoresponse. And they never said that in the paper." I said, "Oh, my goodness, that's fraud." I said, "Let's go get them." So, we bought—I may have had some cadmium sulfide, but, if not, you could buy it from Eagle-Picher, which was a company that was set up during the war to make semiconductors for the Rad Lab.

And I don't know if it was specific for that, but they continued after the war supplying crystals of all kinds to people that wanted them, and they were very good to deal with. So either I had some or we ordered it, and we got it right away. At very short wavelengths, you can excite electron-hole pairs in the material across the band gap, and so you get a lot of photoresponse. And then as you go to longer wavelengths, you don't have enough photon energy to excite across the band gap. And then what's left are the electrons in the metal—-the only place you can get current is exciting electrons in the metal across the barrier into the semiconductor.

So what it looks like when you plot how much photoresponse you get as a function of the photon energy, you get a big response up at high photon energies, and then at the band energy you get this steep drop—for materials, the II-VI materials, it's very, very sharp, as good as the resolution of your monochromator. And it drops several orders of magnitude down. And then out to longer wavelengths, you have this response from the metal. And Bill and I had both done our own versions of this experiment. We knew it inside out and backwards.

That's how you determined what the barrier heights were. And we had both done it, so I was all set up for it. So we did it, and we noticed that the drop—they published their curves, so we could tell exactly what was going—the drop they had only went down about two orders of magnitude. And then they started having this—what they thought was a photo-response from the metal. Well, when we did it, it dropped by four orders of magnitude before you got any. Well, four orders of magnitude down, now you've got scattered light, and you're not sure.

And we got something, and I said, "Bill, we've got to get higher intensity," and he said, "Oh, somebody makes a 10:1 off-axis focusing ellipsoid mirror." So this is what you get when you work with a guy who's done it for years. They know every place that has every optical thing, and he knew this one. Well, a factor of 10 and spot size is a factor of 100 in intensity, and it's actually better than that because the light from the monochromator is diverging. So this thing would probably give us three orders of magnitude advantage—so I called them up and asked them to rush it. And within a week, we had that mirror. And I had a machinist working with me at the time, who built up a nice little rig to put the off-axis ellipsoidal mirror in front of the exit slit of my monochromator. And so within two weeks, we had this thing running, and then we had three orders of magnitude more light. And you go down four orders of magnitude, and there's the photoresponse, and it's two orders of magnitude below where the RCA guys were.

So they had never seen a response from the metal, and what they were seeing was the response from goop on the surface due to their chemical deposition. So that was my introduction to (a) the joy of working with a Bell guy who knew all the ropes, and (b) the treachery of believing people, even good people from a good place, that might just omit telling you of a key thing that makes their results meaningless. It wasn't just they were off a little bit. They're results were meaningless! I'd never even imagined that people in that situation would do that. Of course, I've experienced it many times since. So that started a wonderful collaboration—and once again, the common enemy thing gives you more juice. So we did a series of maybe five years of Spitzer and Mead, and

Mead and Spitzer papers where we just take turns of who was first author, and a just wonderful partnership. And that was the other lucky break I had was having—so, you see, my life has been a series of lucky breaks, yes, without which there wouldn't be much to talk about. [laugh].

Zierler: [laugh] Carver, I want to ask you, at some point probably pretty early on, Caltech gets wind of the fact that you are now collaborating in areas that are soon to be tremendously economically successful. In other words, there's a lot of commercial value to many of the projects that you're working on. So I'm curious, in what ways did Caltech look to protect your intellectual property, its intellectual property? How—what was Caltech's approach to the kinds of things that you were working on collaborating in industry that would soon be of enormous commercial value in the market?

Mead: That's a good question. They were pretty clueless. I knew about patents. I think because I had—as a kid, I used to admire people like Ben Franklin and George Westinghouse and, you know, Tom Edison, and read about them. And patents loomed large in the history of early ideas. And so I had always thought of if I do something important, I should patent it.

So I did patent a number of things, including this triode made with tunneling, which of course never turned into a major device, and quite a number of other things. And they were cooperative. I had come to know a good patent attorney because of the Pacific Semiconductor guys—-they had a good patent attorney. I had talked to one of their people, and they said, "Oh, you should work with Marty Horn, he's a really good patent attorney."

So I told the Caltech guys, "Hey, I want to patent this thing, and Marty Horn is the guy that does a good job." So they called up Marty Horn and said, "Hey, we'll pay for it," and I just did it, you know, and they were cooperative. I don't remember having any tussle over it. But I don't remember them being particularly interested—not like anybody got excited or anything. It was like, "Oh, Yes, it's a good idea, so we'll patent it."

Zierler: And at what point did you realize—

Mead: And none of them ever turned into anything.

Zierler: [laugh] At what point did you realize that, you know, a company like Intel was going to corner the market, and make billions and billions of dollars in profits? At what point did it dawn on you that there were enormous, enormous profits to be earned in these industries?

Mead: Well, I knew enough. I had always been interested in business. And I knew enough to know that Fairchild was going to make a lot. They were a successful company. And then of course, I was badge number 5 at Intel because I had been consulting for Gordon. And he just said, "Hey, we're going to do this new thing, and would you like to continue consulting?"

And I went, "Oh, Yes, of course." And so I learned a lot from being part of the Intel start-up. And Bob Noyce—I had just been on a "hi, Bob" basis with him, but when we started Intel, I had a lot of dinners with Bob. And at one point when we were both single, we used to double-date—so we got to know each other really well. And Bob was a very acute businessman, and I learned a lot from him, probably more than from anybody else.

And, yes, it was obvious there was a lot of money to be made, but also there were a lot of people after the same bucks, and the competition was fierce in the early days of semi. And now it's a highly saturated market, so it's not in the exponential phase like it was back then. But it became pretty obvious that if you hit upon the right thing, and if you could keep ahead of other people who would have more resources, you could make a company that was successful. And that's of course a thing that I've helped students do with starting start-ups.

That's the mechanism by which I participated in these opportunities. And of course, I think I've been involved in about between 25 and 30, depending on how you count. And there have only been a few that have made it to being public companies, which is what you expect. I got to know—one of the great things about the Intel start-up was they were just starting it, and so they included me in the process—not because I could add anything business-wise, but because I would ask questions. And I think they just liked to have another pair of eyes that thought differently.

But, anyway, they would take me along on meetings. And I got to meet Art Rock. And Art Rock was the number one venture capitalist in the western US. And he funded Intel, and I learned a lot from Art and from being part of the process, just watching, and being included in the process.

You may be lucky if you hit 10%. They're all wonderful opportunities or you wouldn't get involved. But then what really works out is maybe 10% if you're lucky. I think mostly they say 5%. And then there's probably another 10% that get acquired, and you come out OK but not great. And then the others—you either lose all your money or you about break even or whatever. So, that's been a great learning experience, and I know a lot of things not to do now, a lot more than I did back then.

Zierler: [laugh]

Mead: But not enough to not have a new problem that I didn't see coming.

Zierler: Carver, I'm curious. By the mid-1960s, you're involved in a lot of research that would become foundational to wireless electronics and to telecommunication systems generally. And so I'd like to know – was your goal to develop research that would be useful for wireless and telecommunication systems, or was the research sort of just basic and then this is naturally where it led?

Mead: Oh, that's an absolutely great question. I'm drawn to things that work. And it can be a basic science thing, or it can be a commercial opportunity which is technology-based, or it can be somewhere in the middle. And what draws me is the ability to figure something out that's central to whatever the thing is. And it's always better if there's use for it.

But most things don't work out—well, it was like this transistor thing I did, you know, the tunnel transistor. It didn't go anywhere. I learned a lot. But it led to another level of understanding, and led to me getting involved with Bill Spitzer, and then that led to the study of surface states on semiconductors because we were looking at barrier heights of Schottky barriers.

And Bill of course had known John Bardeen, who had figured out that it was surface states that were why the Bell couldn't make the original kind of transistor that they set out to make. And so he knew about that. So we published a whole set of papers on surface states. Well, it turns out that by understanding the surface states and how they work—where they were in the band gap, I figured out why Shockley's original Schottky barrier field-effect transistor had not worked.

And so I built one over the Thanksgiving holiday, I think it was '65. And it was the first Schottky barrier gate field-effect transistor *that actually worked*! It is a pure majority carrier device, and it has a metal gate so it doesn't have a problem with the conductivity of the gate fighting against the transfer efficiency—because there's always that fight with a minority carrier device. *It is a beautiful device!*

It turned out Bell had tried very hard—I didn't know this at the time—and they had failed to be able to make one to work because of the energy of the surface states on Germanium. They knew about surface states. John Bardeen had explained it to them. But the location on germanium was too close to the valance band, so it would inject holes instead of being a barrier for a field-effect device. And I used Gallium Arsenide because it's surface states are located close to the middle of the gap, and it had a wider band gap. Also had a lot higher mobility, so it made beautiful devices.

And those are devices that are still used—they're fantastic for RF transmitters. An RF transmitter uses a Class-C amplifier because it's very efficient—when you have to make power like you do in a final amplifier of a transmitter, you want it to be very efficient. So, you have a tuned circuit in which the transmitted wave is resonating in the tuned circuit. So at the very bottom of the cycle, when the voltage on the tuned circuit is very close to zero, that's when you want to put the charge through the device, and you want to put a lot of it right there. Well, if you try to do that with a minority carrier device, you forward-bias the emitter-base junction, and flood the base region all those minority carriers, and they just ruin the efficiency. And if you use a junction field-effect device, which is one that Bell Labs made—it wasn't the first one but it was a device they had come up with, then you had the same problem.

You forward-bias the gate—-but with a Schottky barrier, it isn't a semiconductor. It doesn't inject minority carriers when it's forward biased. So you can go right up into forward bias with it, and never get any minority carriers in there to ruin the efficiency of the device. Of course, I had been a radio amateur, so I had built a lot of transmitters, and I knew exactly what you had to do to make them really efficient. And this device did that.

So, you know, it was all over the map. There was never a single thing that was like, "Oh, I got to make this device, and solve all the problems." It was like, "Oh, I now know enough to make that." And that device was the one that Lilienfeld had invented originally, in 1926. I gave a talk like maybe a year ago at Caltech about the history of semiconductors. And I told Lilienfeld's story because I was so pleased to be the first one that had ever made Lilienfeld's device work.

And he was an amazing inventor, and scientist as well. But, for me, it never had to be one way or the other. It was kind of like: whatever is left to be figured out, go figure it out if you can. And of course, there are many more things I tried to figure out that I never have because it's just the way it works. And you don't know till you've figured it out if you're going to be able to or not.

Zierler: Were you envisioning as early as the mid-1960s that people would be able to talk on cellular telephones, unwired?

Mead: No. Gordon and I—of course, it was '65 when he put his Moore's Law paper out, and then of course I started working on transistor scaling. And we both wrote about what you're going to be able to do as you make faster and lower power and more complex integrated circuits.

And we both saw a lot of things that would be made better by doing that. But the cellular network, it wasn't that you couldn't imagine a—I mean, I built transmitters. I built receivers. I knew about that. But you must remember that was only a few years after the Consent Decree—

Zierler: Right. [laugh] Right.

Mead: —where IBM could do computers, and Bell could do communications, but they must not go into each other's field. Well, in that environment, there's no hope. The problem isn't technical. The problem is this horrible political stuff!

Zierler: Bifurcation.

Mead: Yes, Yes, the bureaucratic nightmare of ultimate proportion, right? Consent Decree!

Zierler: And did you get at the time that this division was harmful for technological advance?

Mead: Oh, yes. Yes. In fact, I have a slide about that. When I was evangelizing Moore's Law, I would go round and give talks about Moore's Law, and how it's going to change the world. And Gordon did too. We sort of took turns with opportunities to do that. I would do it from the point of view of you could scale these devices, and you could scale them much further than anybody thought.

And Gordon was doing it from the point of view of, "Hey, we're doing this now, and we're going to keep doing it, and the economics of it are unbelievable. Let me show you." So, we kind of came at it from two different angles. And it was good that we were able to. But I talked about—and I think both of us mentioned—-telephones being a very important application area of communications.

But I know myself, I didn't think that government was capable of unwinding the mess they had made. But I have a slide that shows two big arrows, one was called Computing, and the other was called Communications, and they were headed for the same place. And then there was—this next slide I showed was an atomic explosion. And then I said, "Now, what'll the fallout be from this explosion that is going to happen when you can't keep those two things separate?" So I did see that part, and gave many talks about it.

I always got a good laugh at the—because it depicted the size of the transformation that was coming. And then I had a slide that showed all the applications that would fall out of that. And of course phones were I think one of the things there, but there was all kinds of other stuff too. But the fact that it would become possible to have the kind of RF spectrum usage we have today, when we had a Consent Decree, it never occurred to me that government could allow a thing like that to happen. I just couldn't see it.

Zierler: Carver, one thing that the government was getting good at in the 1960s was shooting stuff into space, right, leading up to the—you know—the moon landing in 1969. I'm curious when you thought about the possibility of satellites orbiting the planet as a useful point of contact for global communication systems?

Mead: Oh, that was interesting. We had a group of—well, there's a longer story. But, anyway, two of my Caltech colleagues in the humanities department were people that had an interest in the economics of communication systems. We had lunch once in a while, and one of them said at lunch one day, as I remember it, that the efficient way to do communications is not wires, but is through satellites. And I hadn't thought about it before. And so he said that. I had not had that thought. But I couldn't see a way to get a hook into it. I couldn't see anything I could offer to that effort. You know, it seemed to me that was big companies and huge investments and government. And I just didn't see how I could make any contribution to that.

Zierler: Gordon Moore has credited you with coming up with the phrase "Moore's Law". Do you agree with that? Is that sort of settled history?

Mead: Well, I can tell you my memory of how it happened. And this is my memory, and it could be mixed up. But I remember when we started doing the VLSI stuff, and hyping the Moore's law stuff, and all of that, I spent basically full-time trying to get people to believe that circuits would scale that far, and that they actually scaled down OK—they wouldn't get too hot that they'd melt because you could lower the power-supply. And it was exhausting because people just wouldn't believe that there was a possibility that this thing could keep going.

And so we worked out the smallest transistor that was built exactly like we were building them then, and it would still work. And it turned out to be a .15 micron device, and commercial transistors at the time were 12 microns, so we had a long way to go. And, in the process of that, of course, I got interviewed by a lot of people from the press, and mostly honest, good people that tried to make things accessible to ordinary folk. And so I enjoyed discussions with those people. And some of them I got to know.

One of the guys I got to know was Larry Waller at *Electronics* magazine. And I'm not sure that I—because I talked to him so many times, I'm not sure that I'm remembering the discussion that led to Moore's Law being him. But it was one of those guys I was talking to about this thing about the exponential increase, and what was behind it and all that. And I used to call it Gordon Moore's plot.

And out of that discussion, it somehow became a law. And I don't remember if I said it first or he said it first or—and I may not even have the right reporter. But, anyway, it was out of that discussion that it came. And so I had a hand in it. At least, that was the first time I had ever said those words. So Gordon likes to credit me with that. I'm sure that it was out of that story that "Moore's Plot" became "Moore's Law."

Zierler: And in this difficulty in terms of trying to convince skeptics, right, is the science that transistors would become better and cooler and cheaper as they got smaller, is that science counterintuitive, or is there something that you simply understood that others did not?

Mead: That's funny. I had been having a hard time. People just *knew* that something would get you, and if you hadn't thought of it, that was your problem. But they *knew* there was going to be something that would get you, and Nature just wasn't going to let you do that. I mean, it was universal. Nothing has ever gone more than an order of magnitude, so this would sure as hell not!

It was 1968 that there was one of these device research conferences that I told you about. They weren't called "device physics" – they were just called "device research conferences." And this was an IEEE workshop. It was that group that had this thing every year. And in 1968, they had it at the Lake of the Ozarks. I was scheduled to give this talk, it was the afternoon on the first day, and it was Lake of the Ozarks down in Texas. And I remember walking up the trail that looked down over the lake, and I got up there, and I said, "Look, I've got to have a way of making it so clear you can't miss it."

So I sat down, and I remembered writing down on a piece of paper I had with me the thing that Al Rose had said, that current is the charge in transit over the transit time. So I wrote that one down, and then I—there was a little MOS device, and, you know, just worked out that it's just a capacitor. And if you just forget about all the details, and don't worry about the 10% here and 5% there, you just write down the physics, it's three lines.

And so I went back to my room, and made up a foil—in those days, you did foils with, you know, colored pens—and gave a talk that afternoon about the scaling, and how that's all there was to it. And it's *astounding!* It turns out that the computation you get per unit energy goes like the *cube* of the scaling. So obviously *it can't be, but it is.* So I gave the talk that afternoon. And of course, everybody was all over me. But it was the right way of thinking about it that I got from Al Rose years before.

And so when I was rummaging through some old slides a year or two ago, I found the original slide. That was really neat because that was what turned the tide. There was a guy there from IBM named Bob Dennard. He's the guy that invented the dynamic RAM. Neat guy. Bob was there, and he had a bunch of questions, but he wasn't nasty like the other guys. He was thinking about it. And Bob was a very thoughtful guy—not just arguing for the sake of arguing. And so I was pleased about that.

So the next year, there was the same meeting. It wasn't at Lake of the Ozarks. It was someplace else. But then he gave a talk, and he had gone through very carefully, and had his own version that came up with the same basic conclusions. And I was so jazzed because I finally had somebody from industry that had worked it out and came to the same conclusion. It was the first time there was a crack in the armor.

And then it went. But up to that time, it had been a brick wall. So that was a great experience. Before that, it was just a knee-jerk reaction: "Yes, sonny, you can do your calculations, but something's going to get you. I know it, I know it. I've been there before" They were all 100% sure there was no way. And within a couple of years, everybody was yelling and screaming about Moore's law.

Zierler: [laugh] Carver, I think we're coming up on our two-hour mark. So I think for my last question for today, if that's OK with you, if that's—

Mead: Perfect, perfect.

Zierler: Great, great. So I'd like to ask at what point when you're making predictions about ultimately, you know, where Moore's law is headed at the submicron level, at what point in the chronology do you feel like your prediction has been fully realized in terms of microcomputing?

Mead: Well, the question we asked wasn't the ultimate limit, no matter what you did. It was if you did the same thing we were doing, with the same materials and everything, where could you get? And that's where we got the 0.15 microns. We published that prediction in 1971, and commercial technology got there about 20 years later. I remember when it happened, I was thrilled. I think the first place I saw it was from Japan, but I'm not sure now. But I remember seeing somebody had a 0.15 micron process.

About that time, it became clear that people needed to not only just shrink what they knew, but they also needed to use materials in a smarter way. And now, of course, you have to do more than just scaling the stuff you have already. You have to put them on insulating substrates or use high dielectric constant oxides. And put the channels on edge so that they can wrap the gate all around them or, all kinds of stuff they're doing now—-it's gotten very sophisticated.

But I wasn't counting any of that, so, because that's a different question to say is there still room when you don't just make them the way that they were already making them—is there still—well, of course there is. Yes, there's no limit to how people can tweak stuff. But yes, it was around 1990 when somebody came out with a 0.15 micron process, and—Yes. It's been good, David.

Zierler: Absolutely, so let me cut it right here.

[End of recording]

DAVID Zierler: This is David Zierler, Oral Historian for the American Institute of Physics. It is July 19th, 2020. I am so happy to be back with Professor Carver Mead for round three of our discussion. Carver, to pick up on where we left of last time, we were talking about the measurement of 0.15 microns and the significance of this, and why you were so happy with this development. And the fact that you saw that this was taking place, if memory serves, in Japan. So, picking up there, can you talk a little bit more broadly about the significance of this development and what it meant for the future?

Mead: Well, let me just tell you what I saw at the time.

Zierler: Sure.

Mead: I told you the story about being down in that workshop down in Lake of the Ozarks where I had the epiphany that, basically, the price performance of VLSI technology got better as the cube of the scaling factor. Of course, that was *way* too good to be true. And, so, I worked it out there. And I told you that Bob Dennard got onto it and came up with roughly the same conclusions the next year. I remember it was on the way home from that trip in 1968 that I was thinking "That's going to happen. It's just way too powerful to not happen. Because there's just too much at stake. Somebody will figure it out because it's just too big a thing. So, it's going to happen, and I should work on what is preventing it from happening" Well, it turned out that just about then, I think it was '69, when Bob Noyce and Gordon Moore, who had become good friends because I was consulting with them at Fairchild, left and founded Intel. And they asked me if I'd like to come along as a consultant. So, I became Badge No. 5 at Intel.

Zierler: [laughs]

Mead: Bob, Gordon and Art Rock, who became Chairman of the Board and who was the first investor in Intel. Art had started the first real venture firm in California. So, he was—by this time he was a real veteran. He knew Bob and Gordon since Fairchild. So, he was the first investor. And then there was Jean Jones who was admin for Gordon at Fairchild, and she came along.

Zierler: I want to ask in what ways did you perceive, at the time, that Intel was a breakaway from Fairchild that was going to do the same thing but better? And in what ways was Intel aiming for something entirely different?

Mead: Well, that's an excellent question. What had happened at Fairchild—it's actually in that little book by David Brock. What had happened is they were working like crazy just to get transistors to where they were reliable. They had these wonderful high-performance transistors that I told you a little bit about. But they weren't able to make them reliably. And, so, they had these incredibly clever people who were working on the yield and reliability problems. And, you know, it was do or die. They had broken away from Shockley and had gotten a little bit of money—and they *had to make them work*. So, they finally got the fabrication process under control, but in the process, they had to invent a way to keep the surfaces from messing up the transistor. And, because they were making what were called mesa etched transistors—you know the technology?

Zierler: Uh-huh.

Mead: Where they'd make the transistor they would diffuse in impurities, first for the base region, and then for the emitter region. Then they would mask off the region where the transistor was, and etch the area around it down to the substrate. So that then the transistor was sitting out by itself. But then the place where they'd etched was a real problem. It was just a surface hanging out there and the edges of the junctions came out to the surface. So, any little bit of charge that built up there could make the transistor's current drift like crazy. So, Jean Hoerni, who was there at the time said, "Oh, I know how to do that. We'll make a transistor that doesn't have to be etched that way." You just diffuse everything—-collector region, base region, and emitter region—-into the surface and the surface ends up with an oxide on it that keeps it from getting messed up. It was a wonderful invention. And that stabilized their transistors right away. But then Bob Noyce looked at that and said, "Hey! You're making all these transistors on the same piece of silicon and they're isolated from each other. And we have a metal layer that we have put there already to go to the bonding pads. Why don't we just use that metal layer to hook up the transistors and we'd have an integrated circuit." And that's a real integrated circuit. The one that Kilby had done, he had to hook up the transistors with bonding wires. So, it wasn't really integrated. But this one would be really integrated. So, that was Noyce's insight. And, so, they started this little group that had been doing this stuff. Started making little integrated circuits under Bob's tutelage. Bob was really good at encouraging people with new ideas. And so was Gordon. And they had this little part of Gordon's lab that was off doing these things. And they started making integrated circuits. And, of course, I was there watching this. And it became amazing what this group was doing with these things, and they were working. And they were getting more and more complicated each year. And, you'd think that Fairchild would've just run with it because they were all tooled up and everything. But it turned out that the part of the company that made discrete transistors and diodes saw it as a threat. And they said, "Hey, you've invested a bunch of money setting up to sell these things and there's no market." Of course, there was no market because there were none of them.

Zierler: [Laughs].

Mead: They had a knock-down, drag-out fight inside of Fairchild. And eventually the old discrete-transistor guys, won! And they shut down the whole integrated circuit effort. So, Bob and Gordon said, "Hey, that's ridiculous. That's where the big thing's going to be—-not in the discretes. It's going to be in those integrated circuits." So, that's why they started Intel. They *had to* because they couldn't do it at Fairchild. They didn't want to leave. They just wanted to get it done. That, by the way, has happened to a lot of businesses, historically. That same story. So, it's not a unique. Anyway, because I'd been consulting with them, I was very close to all this and got invited to be part of the process, which was a wonderful experience. But I watched the way they were making the patterns for their integrated circuits, and they were using old printed circuit technology. Do you know about Rubylith?

Zierler: No.

Mead: Have you ever worked with Rubylith? What it is, it's a piece of mylar with a very thin layer of red coating on it. And, if you want to make the pattern of something you draw it with pencil and paper, and you put it down. And then on top of it you put this Rubylith. And you can see through the red and so you trace the lines that you want to have be part of a single layer in the process. You trace the edges with an X-ACTO knife, and then you peel out the part in the middle and you're left with a pattern that's clear,—the lines you want are clear and the rest of the area is red. And you put it up on a light-box and you use blue-sensitive film and you take a picture of it. And then you have what was then used for standard artwork. You know, signs and posters and all that stuff. In those days, a lot of that was done by hand. And for printed circuit boards. And people made those patterns using Rubylith. So, it was part of the lithography process for other reasons. And, so, what Intel was doing was drawing a pattern on a piece of mylar with grid lines on it, and the pattern had to be on the grid. And the different layers had little different styles or different colors on the mylar so you could tell which was which. And then they'd put Rubylith down on top of the pattern that had been drawn carefully by hand, which embodied the circuit they wanted—the transistors and the wires connecting them. I think there were five layers in the first Intel process. There was diffusion and poly and contact cut and metal and bonding-pads because, after the wafer processing got done—the very last thing they did was they coated the whole chip with phosphorus glass. It's a thick insulating layer that is also kind of a getter that keeps things clean. And they had to cut holes in that where the pads were that they were going to bond out. So, there were five masks—-simple. So, what they did is they would get a "layout person" who could go from a circuit to a pattern, which was then, and still is today, an incredible piece of conceptual mapping. And some people are just good at it. And, then, they had this drawing, and they would put Rubylith on it, and they would trace with an X-ACTO knife—it turned out it was fancy X-ACTO knife. It was a thing called a Coordinograph; I don't know if you've ever seen one? But you could set X and Y and that would put the knife at a certain position. And, then, if you locked one of them, you could move the knife in the other direction—you know, say you locked horizontal you could move the knife vertically. And if you locked the vertical you could move the knife horizontally and they would cut the ruby that way. And then by hand, go in with tweezers and peel out the little strips of Ruby left between the cuts. I watched all this. They had a whole team of people. It was the biggest part of the company, doing this process of pattern-generation—-*doing it by hand* onto the mylar and then doing the ruby cutting. The biggest source of errors were the contact cuts. You couldn't see very well to know where the little square ruby cut was that you had to peel out, and they would have contacts that weren't peeled properly because it was all done by hand. And I thought, "There's no way! I can't afford that." I couldn't afford a Coordinograph and I certainly couldn't afford 20 people to do all that. But I just felt down in my soul I had to do this. I just *had to*. It was too important and that was where everything was going. I just completely dropped my whole solid-state physics past. Not in my head, but in terms of where the effort went. But I knew I couldn't do this thing the way they were doing it. And, so it turned out I had consulted with some people in the defense area. And the defense contractors were using a thing called a Gerber plotter for making circuit boards because they had resources—-it's DoD stuff. They always push the limits, and they were pushing circuit boards to finer lines and much higher density, because they had to go in defense systems. They're going to fly them, they're going to put them in spacecraft, so they were pushing the limits really hard. And, when I looked at the complexity of one of those circuit boards, it was actually greater than the complexity of the initial Intel chips. I said, "I can make that pattern. I just have to learn how to program a Gerber plotter." The Gerber plotter had a wheel that had 24 apertures on it. And then it would shine light through a given aperture and it could move on a path which you could program. It could also take a shape and it could flash it. There was a command that would select any one of the stations on the wheel. Then you could take that shape and move it along a path, and it would make a wire. And you could make the pads and, the contacts between layers, by flashing little shapes. And it only took—-I think I had seven or eight shapes. It turned out that the big DoD contractors had these Gerber plotters, but I couldn't get access to those. But then somebody told me, and I forget who it was, that there were job-shops where these guys had Gerber plotters and they would just take your tape and run it for you. We were in Pasadena, and El Monte is not very far away, maybe a 20-minute drive. And it turned out there was a guy in El Monte who had a little shop with a Gerber plotter. And he was happy to see me, and so, I went and learned Gerber code, which is not that difficult. And I started programming things in Gerber code. And then I would go to our computing center and submit a deck of cards that had that Gerber code on it, and they would put it on a magnetic tape. And then I would take the tape down to this guy. And he would make little test run for me. And one day he said, "How are you making these?" I said, "Well, I just, you know—programming it." And he said, "In Gerber code?" And I said, "Well, Yes." And he said, "Oh, that must be tedious." I said, "Yes." And he said, "Why don't you use—there's this little program called PAL. Precision Artwork Language. It's made by this company—" And he gave me the contact. So, I called them up and told them I wanted to get a copy of their program that would run on our IBM 360 that we had down in the computing center. They said, "Well, normally it's $10,000, but since you're an academic you can have it for $3.5-thousand dollars." Well, that was a lot of money for me and I didn't have any support for this. So, I bought this thing out of my own pocket.

Zierler: Oh, wow!

Mead: And got it on the machine in the computing center we had. You've probably seen the old computing centers?

Zierler: Sure.

Mead: You've been around long enough to endure one of those.

Zierler: Yes.

Mead: And we had a plotter there. It was only a one-color plotter but—so, I took this—the Artwork Language was great because it allowed me to define symbols and then place the symbols. So, if I was making a memory, I could define a memory cell as a symbol, and then I step-and-repeat it. And, I could draw symbols in different orientations and draw them in different positions. So, it was a very handy thing. So, boy, I was in seven-league boots! I started writing these programs to generate artwork. All of a sudden, I knew that I could do this. I could see that there was a whole 'nother level at which I could go at the thing. And I could do it myself. And, it was a *huge epiphany*. And I thought "I must have something I want to design." So, in the process of just learning about this stuff, I invented a thing that later we called a Programmable Logic Array. And it turned out there were three of us that had independently invented the thing. There was me, and there was a guy down in Texas, and there was an HP guy. And we all had the same idea. A very simple idea but powerful. And that is, you have two planes. On the left-hand plane you had inputs running vertically and you could make a nor combination of them on wires that ran horizontally into another plane, and the other plane would make any nor combination of these inputs on the wires that ran vertically in the second plane. You can either do nor-nor or you can call it and-or with upside-down inputs. That was just too good to be true, because then making a logic circuit became writing code for which transistors in which array got contacts. So, by programming one mask I could get a wide variety of behaviors. And, then, if I took some of the outputs from the right-hand plane as inputs to storage, just flip-flops that would set or reset, and then run those back to the input, that became a realization of a general finite state machine. Well, I had never had a course in computer science. I didn't know what a finite state machine was, but when I finally found out about them from computer scientists, it turned out I had already been making and programing them for a couple of years. They thought of them as a way to prove theorems, and I thought of them as a way to get work done.

Zierler: [Laughs]

Mead: There was a lot going on here—-the overall system architecture, then the circuit realization of each of the system building blacks, then the layout of the wires and transistors that would create those circuits, then the program that generated the Gerber code to make the original patterns of the 5 layers, then getting the 5 Gerber plots for those layers, then making Color-Key color transparent check plots of the 5 layers, overlaying those in registration so I could see if everything had come out as intended, then taking those 100X Gerber originals to a mask supplier to get the final-scale masks made, then taking those to the Intel fabrication plant to get the physical chip fabricated. I had all these steps firmly in my mind, and it was clear to me that, if done correctly, that series of steps would create a working chip. But *there were a zillion things that could go wrong!* I would never be sure until I *actually made one and got it to work!* So, I decided I was going to make a thing. And the thing I chose to make was a 12-hour clock. Because it has just enough complexity. And I would drive the display directly from one chip. So, I'd have the whole thing on one chip. And the displays in those days, they were Nixie tubes. You've probably seen Nixie tubes.

Zierler: Yes.

Mead: But they had just started making vacuum fluorescent tubes—-each tube had a 7-bar display in it, and they worked on low voltages. They had a little filament in them that glowed just enough to get electrons off it, but wasn't hot enough that you could see it glow. And, then, behind it there were seven segments. Each one had a fluorescent material on it, and it came out to a wire that ran horizontally to the same segment of all the other tubes. And, between the filament and the bars was a very fine grid, that would let the electrons through if it was positive, and not if it was zero voltage. The grid could be used to select which digit tube was lit at any given time. So, you could select, let's say, the top bar in a fluorescent display, and then all of the digits that had that bar lit you could turn on and leave that for a little while. And, then, you could do it with the next bar and the next bar. And, so, in seven cycles you had gone through lighting the six digits of the 12 hour display. And, so, if you did that fast enough it appeared nice and steady. And these things would run on 15 volts which was the maximum voltage that the Intel process worked at, so I knew I could just hook it up to the 6 little vacuum fluorescent display tubes to the chip. And, so, this then became a real goal for me. If I could make that work, I'd made a general-purpose logic system with storage because you had to store all the digits that were there and then update them. So, except for the size of the memory, that was a general-purpose computing machine. All a general-purpose computing machine is, is a finite-state machine with a way to access memory. And, so, I had that. It was just a very simple one-chip version. And it would do this real thing so I could tell if I succeeded. So, I was running these plots in our computing center, which, then (1970) had a little place in the back where you could put your stuff and work on things. So, I had these plots out lookin for things I hadn't programmed right. And then, when I would get a whole plot that I hadn't messed up, and I could tell from the check plot, then I would go down to this guy in El Monte and get him to run the five layers for me. You made a separate Gerber of each one. And, then, there was a mask house that I got introduced to by—it turned out I had two former students at Intel. One was Gerry Parker, and the other one was Ted Jenkins; both great guys. Gerry became a vice-president at Intel and is now a distinguished alumnus of Caltech. And Ted became another vice-president, and he's now on the Caltech Board. So, they were, at the time, very willing to help out with this project. And they had access to wafer fabrication, and they would very often run engineering runs. They said if I could get a set of masks, they would run them for me as an engineering run. So, the only thing proprietary that I had to learn from them was what their alignment marks looked like, because, of course, you had to have alignment marks. But that was the only proprietary thing. And I had to figure out the scale. You know, what their minimum line width was but that turned out to be a thing you could see on any chip that they made. So, it wasn't secret so they could tell me and it wasn't proprietary. And I said, "Where do I get masks made?" And they said, "There's a cooperative guy over at—wasn't Micro Mask. Micro Mask was the big one, and they didn't want to hear from a little guy. But there was another mask house that would take 100X final size originals and reduce them down and step an array of them on the mask. In those days, the wafers were 2", so the masks were 2" x 2". The circuit I had was a tenth of an inch across, so there were a couple of hundred of them on a wafer. And 2" masks weren't that expensive. So, I would get the Gerber guy to make me a set of Gerber 100X originals. And, then, it turned out that you really wanted to check the Gerbers because they were a lot cheaper than a fab run. So, the check plot was a lot cheaper than the Gerber. And the Gerber was a lot cheaper than a fab run, so I checked. Well, how you check these Gerbers? They're black lines on clear or clear lines on black, and you had to get that right. So, Ted or Gerry would tell me which way it was, and I would put that in the spec for the mask house. Then I'd take the masks over to—it was Jerry in the beginning that would run them because he was in charge of reliability. He had to have test vehicles for reliability, so he was running those all the time to get reliability information on the fab. So, one more run, nobody noticed. In the middle of this, I went in to see Bob and Gordon, and I said, "I just want you guys to know I'm sneaking runs through your fab. And Bob looked at Gordon, and he looked back at me and he said, "You know, Carver, I don't think I want to know about that." [Laughs]

Zierler: [Laughs]

Mead: I said, "Thank you very much." And I never said another word.

Zierler: Right.

Mead: I was going to tell you about check plots. How do you check the Gerber? Because you really want to see the relationship of all the layers. Well, it turned out that the lithography industry had that problem for a long time because they did multi-color prints. So, since you have 5 black-and-white images of the 5 layers in the process, how do you check those originals? It's a real problem. Well, they had this thing called ColorKey. You could just put one layer of your original on one of these color keys and stick it in a UV box, and expose it for 5 minutes, and take it out, and the pattern was there on the color key. Didn't have to develop it or anything. It was great. So, I made a plywood box and mounted some UV tubes in it. And when I'd get Gerbers back I'd expose each one on the right ColorKey. Then I would line all 5 of them up using the alignment marks and that would give me a multi-color transparency that I could stick on a light table—I forget where I got access to a light table. I didn't have room for one in the lab and they were expensive. But somebody had one that let me use it. I don't remember who it was. I would put the ColorKey stack on a light table and I could see the relationship of each layer to all the others. That allowed me to check everything. If there was an error, I could change the code and run another Gerber. Usually it was only one layer that was bad and I would run another one and then make a color key and make sure I was alright. When I had satisfied myself, I went to the mask shop and had them make a set of masks for me. I took those up to Gerry and I said, "Jerry, this is my first chip. Will you run it for me?" And he said, "Sure, Carver." And, so, he did and a couple weeks later the finished wafers were ready (it was much faster in those days because there were only five layers). So, I went up on my Friday consulting up in Silicon Valley and went by to see Gerry, who was, by then, in charge of reliability for Intel. Aside: [Gerry had been my T.A. in the VLSI Design class. One day, the girl I was dating brought her sister along up to my house. And Gerry was there because we were talking about how to grade a particular exam or homework problem. Gerry and the sister (Carol) got along really well and they've been married ever since. On my weekly trip to Silicon Valley I would usually take Gerry and Carol out to dinner. Nowadays I see them quite often at events.] So, back to the story. The wafers came back in a box that had, I think, 10 wafers in it. It was the minimum run they would do. Gerry had an assistant that could do anything. There were only a few such assistants in all of Intel. They were always ladies, and they were always incredibly clever in all kinds of ways, and could do just about anything in the lab. And, so, all you had to do was explain to her what you needed done and she'd just get it done. Sometimes she'd ask somebody to do something, but she just knew how to get anything done. And it was amazing. It may still be true; I don't know. Anyway, he had one of these ladies that was his right-hand helper kind of person, and she would go and scribe them, and break them. Which is amazing because in those days the way we separated chips is just to use the diamond scribe vertically and diamond scribe horizontally, and then put them face down on a piece of the right stuff, and take a little roller and go "RRRRRRTTT" [makes the sound the roller made]; that would break them vertically, and "RRRRRRTTT" [makes the sound the roller made]; and it would break them horizontally. And very few were fractured at an angle. Most of them broke right on the lines. And when I tried to do it, I broke a lot of them. [Laughs] But she could do it—just "RRRRRRTTT" [makes the sound the roller made]; done, you know.

Zierler: Yes.

Mead: So, then I had a few wafers "diced" into their individual chips. They needed to be put in "packages" which had the little pins on them that connect to the circuit board. Then the "pads" on the chip needed to be connected "bonded" to the pins on the package. I had also managed to get a bonder because, when you get one of these chips, you have to bond it into a package so you can test it. So, I had acquired a bonder and learned how to use it. I forget how I got it, because they were expensive. But I managed to get one—I think I got a used one from somebody, or—I just don't remember. But it was a big expense for me because this was all off the record. I brought the chips back from my weekend at Silicon Valley and I remember, I think I came home early. I think I came home Friday night because I wanted to spend the weekend getting this chip working. And I had set up a little protoboard that I could plug the chip in, and I had the driver circuits and the power supply and everything all set up and all checked out so all I had to do was to package and bond the chips and then plug them in. So I spent Saturday packaging and bonding the chips—-it was just one wire at a time, and I wasn't very good at it. But I could do it. So, I did, I think, five chips. They should all work, and I really wanted to test them *right then*. And then I said, "But I'm not going to do it." I was tired by then; I'm not going to do it while I'm tired. I've always been an early person. So, I came in about 5:00 the next morning with much trepidation, and plugged in the chip. I had put a lot of money, and my hopes for the future, into this thing, and if it didn't work it was going to be a big downer. So, I plugged in the chip and turned on the power supply, and no life. So, I got the scope and started looking at the signals: The outputs were the horizontal (bar) lines and the vertical (digit select) lines for the display.

Zierler: Uh-huh.

Mead: Well, all the horizontal lines were low and all the vertical lines were high. I think that's right. And I thought, "Eww. That doesn't sound like just a single-point failure." So, I went through all five chips and they all did the same thing.

Zierler: Uh-oh.

Mead: Oh, boy. And, of course, my spirits were getting lower and lower and lower.

Zierler: Right.

Mead: And, so, I said, "Well, before you jump out of the window or anything—[Laughs] I knew that if I get low on blood sugar, I get low on energy, and I thought, okay, I know that about myself—and I hadn't had a good breakfast. So, I should go and get something to eat. So, this was Sunday. Well, I could go down to—I forget which restaurant. So, I got up from my bench—I had a bench in my office where I did my own stuff—not out with the students, where their benches were, you know. So, I got up and opened the door to my office and turned off the lights, and just before I went out there was a pattern on the scope. So, I turned the lights back on; the pattern went away. I said, "Oh, you idiot."

Zierler: [Laughs]

Mead: So, I fished around in my pocket and found a penny. Put it on top of the chip. And I got a pattern on the 'scope—*the chip was working*.

Zierler: Just like that.

Mead: Just like that. So, I thought, "Ahh! Now I can enjoy lunch." So, I had a good lunch and then started checking it out and got it hooked up to one of these tubes. And it counted and did all the funny cases, like, when the hours go up to 12:00. And then they go to 1:00 not to 0.

Zierler: Uh-huh.

Mead: So, standard counters don't work. And there's all these little cases with a 12-hour clock. It counts 60 seconds, 60 minutes, but only 12 hours. All those tricky cases were in the code. And it was all right. And it worked. And, so, that was one of THE great days. It may have been the greatest day in my entire life! Right up there with the day I got the first MESFET working!

Zierler: Wow! Carver, I want to ask, you know, besides the immediate pleasures of recognizing that this was viable, what did you recognize, at the time, of the longer-term consequences of this development?

Mead: Oh—Gordon had already—back in '65—-made a good case of what the potential was, and I was completely in sync with him. For me, it had an even more personal significance, because to me it meant that these damned huge computers were *not the way it was going to be*.

Zierler: Yes.

Mead: We were each going to have information technology *of our own*.

Zierler: Right.

Mead: Gordon wasn't on that kick. He had a perfectly good computing center and he didn't have an urgent-felt need like I did. I wanted to do all my experiments that way. And it was very clear to me that I wasn't the only one that wanted information technology of my own.

Zierler: Uh-huh.

Mead: So, when I would evangelize this stuff—Gordon and I were both evangelizing in our own way. I would get chances to give talks quite a lot because it was exciting to people that there was this technology with so much potential. I would talk about how we would each have our own personal information technology. That everything we do would be done that way. And Gordon had quite a bit of that in his message as well. I inherited that from Gordon already, but to me it was much more personal. It wasn't just that was going to be the way the economy went. It was the way my life could change by having this technology. I could design it and have it do the things I wanted to do!

Zierler: Such as what, Carver? What were things that you wanted to do that you recognized even at the time?

Mead: Well, the big one was just to run experiments by computer. The way all my solid-state physics worked, I would take the data readings and write them down by hand. And, then, I would plot them by hand on whatever kind of graph paper was appropriate. And I could get graph paper—-you know what that looks like. You could get them with either linear or log scales, with as many decades as you wanted. And I had a whole stock of those, and I would plot whatever I had in the way that made sense. And if I had to do, for example—if I wanted to get a barrier height from a capacitance voltage curve, I would plot one over C-squared vertically and V horizontally. Well, the V you read off a volt meter, but the one over C-squared you have to take the capacitance and do one over square of it. And to do that you use a slide rule. So, all of my stuff was, you know, the next column was done with a slide rule and then I'd plot those by hand. And then if it was something I wanted to publish, I'd taken those plots over to graphic arts and they would trace them with pen and ink so that I got a good plot to publish. But all the original stuff was done that way—-graph paper and pencil and slide rule. Then, when we finally had access that wasn't too bad to the computing center, I would type the values into punch cards. Then take them to the computing center and they would do the plot for me. And it was just easier. So I wanted that in the lab. I didn't want to have to go to a computing center for that. It was an urgent-felt need. And then, one of my students at the time, when he graduated, wanted to start this little company to do what now we call a word processor.

Zierler: Uh-huh.

Mead: And this was before microcomputers. Minicomputers were expensive, so, you didn't just buy a minicomputer and program it. So I helped him start the company. And we ended up with one of the very first word processors that became a commercial thing. That was a thing that I knew really ought to be in microchips, not in big circuit boards. So, there are all these kinds of things that I could see would change my life. And if there were things that would change my life—like one of the examples I used in my talks was—there are these people that look through microscopes and look at your chromosomes.

Zierler: Uh-huh.

Mead: And the trick was you find the two chromosomes that go together and you pair them up. And then what you want is a picture of the chromosomes in a certain order. Well, that's not the way they come on the microscope slide. They come in any order, and not together. And then some person has to go through and get the individual pictures and pair them up in the right order. And it's tedious. Well, what you need is a microscope that recognizes chromosomes and gets pictures of them and orients them right and pairs them up for you. Just a smart microscope. Obviously, today, that's old hat, but in the day that was one of the tedious things that they did in that business. So, every time I learned about something, I could see how it would be a lot better if it was done digitally. Anyway, it was obvious to me—partly because of Gordon—-large part. Because he had spelled out a lot of things that made it compelling. And, of course, I had my own wish list.

And in that was a thing that Gordon didn't like so much, and that was that it wasn't that hard to design these things if you used this approach that I had developed. And by then—right after I got my first chip working—the students had been coming around and looking at me, looking at these plots, and wondering what the heck I was doing. And there was one student, Dick Pashley, who ended up later going to Intel. But while he was a grad student, he wasn't in my group because by then my interest had shifted to this integrated circuit design stuff, so I had been graduating my existing grad students and not taking any new ones. And it was clear to me by then, once the chip worked, it was clear: I'm going to do this. I *have to do it!* This is the next thing I'm going to put my life into.

Zierler: You mean with or without Gordon's support?

Mead: Gordon and I have always had different takes on whatever it was. And that's been one of the wonderful things about our collaboration down through the years is we have quite different ways of looking at the world. But we have a lot respect for each other. And he has been quite tolerant of my excesses [laughs]. I think because, in the end, we were on the same overall page. Even though our position in life was very different. He had a company that had to grow and be profitable. I had an academic position where my view was: I'm training the next generation of people to be on the leading edge. And whatever that leading edge is, I've got to expose them to it.

Zierler: Right.

Mead: So, Dick Pashley came around and he said, "Carver, why don't you teach a class on what you're doing. Whatever that is." And, I forget if it was just before or just after I got the first chip to work, but of course I wasn't going to teach that class if the chip didn't work. Because I had to believe that if you did this it would work. I wasn't going to teach the students something that didn't work. [Laughs] But anyway, once that worked, I told Pashley, "If you can get 15 students to sign up for it, I'll teach the class." Well, it turned out he ended up with eight and he said, "Would you teach it anyway?" And I said, "Sure." So, we did the first class. We each designed our own little circuit with pads and everything. And then, they weren't real big, so there were room for quite a number of them on the size chip that I could get fabbed [fabricated] at Intel. So I went through my process with the student's designs: The first thing I had to do was take their designs and put them down as a little array on the single chip. So, I had to learn how to do that. It was just programming like before, but you had to be careful you didn't run out of memory because they were now whole projects, not just memory cells or something. But that worked out okay. And, then, I had to teach them how to bond. The bonder I had was a thermo-compression bonder—the bond-wire goes through the hollow tip of the bonder, and the way you cut it off is with a hydrogen flame and it melts the gold wire so it has a little ball on the end. The bonder heats the tip, and then it pushes that hot ball down on the aluminum pad of the chip and holds it for just long enough to where it makes a funny intermetallic bond with the aluminum. And, then, the wire is still threaded through the tip. So, then you take the tip over to the bonding pad on the package and you push it down. Now it's not in a ball, it's just a wire coming out and you push it with the edge of the tip and it bonds. And then it comes up, and then it automatically melts the wire loose and leaves a ball to do the next one. And if you don't do everything just right when you do the ball, and it doesn't stick to the chip, then you lift the tip up and the cold ball is stuck inside the tip. And the tips are expensive. It took me quite a while to get good at bonding so I didn't plug the tips. And there was a little tool that you could put the tip in and push the ball out, and it worked some of the time. But if you'd really plugged it then that wouldn't work either. And I didn't have infinite money. In fact, I had no budget for this except they gave me an account on the computer that I could use for the class. But there was no budget for any of the rest of it. It was all stuff I had to pay out of my own pocket. But that was okay.

Zierler: Carver, I want to ask on that, what was your sense, was Caltech specifically not interested in supporting your research. Or, were you doing things that were so far afield that you wouldn't have expected them to support this kind of work?

Mead: Well—in those days, they had a little money for each faculty each year. It was sort of, as I remember, it was about $5,000. And you could use that for whatever you were doing. And, of course, they provided us space. But if you needed more than that, you had to go rustle it up. I would get little dribs and drabs from the industry, and the ONR had come quite early. And, I'll tell you the ONR story; it's part of this sometime, if you want because it was quite an interesting story. Maybe now's not the time, but if you—

Zierler: Okay.

Mead: It's from earlier. It's from 1960; probably '60.

Zierler: Well, you know what, Carver, it's on your mind now. Let's hear it. This is good.

Mead: Okay. So, this is in 1960, I was just a wet-behind-the-ears PhD. New assistant professor. I told you the story how I met Gordon Moore.

Zierler: Yes.

Mead: It was a similar thing. This guy waltzed into my office, unannounced. And he said, "Hi. I'm Arnold Shostak from the Office of Naval Research. What are you doing?" Well, it turned out Shostak was a very smart guy. And he had liking for Caltech somehow. And in those days at the ONR—there was a lot of personal freedom that the officers there had to find the best people. So, they didn't get this, you know, equal everything thing that people in that position get today. They were really in search of the most original ideas and the highest energy people and that sort of thing. And, so, this guy would hear about something somebody was doing and come around and find out about it. And he did that every year. So, I told him what I was doing. It was the tunneling stuff at the time.

And he said, "Oh, that's interesting. How would you like a contract?" Well, I knew about the ONR because some of the other faculty had ONR contracts—in fact, they had supported some of the things I worked on when I was a student—I was a technician in a lot of the various labs. And, so, I knew about that. But I didn't know anything about what a contract was or how it worked. So I said, "Well, how do I do that?" And he said, "Well, you just write a little description of what you want to do, and then attach a budget. And make the budget come out to be —" I think the first one was $15,000. Well, to me that was a lot of money. And I had said, "Well, how do I do the budget?" And he said, "Oh, go talk to your contracts guy. He knows how to do that for us." So, I wrote a little thing about the what the next things were that I wanted to do. And I took them over to the contracts guy, and I said, "This is what the ONR guy said to me." And the guy said, "Oh, we can do that." So, he did a little a budget for me. And I got this little grant from ONR. It was all very homespun. Anyway, that's the other story about people waltzing into your office.

Anyway, where were we? We were at—oh, Dick Pashley and the course. Well, you know, it turned out, I think, everybody's little project worked. I had told them, a little bit tongue-in-cheek, that if their project worked, they'd pass the course. So, they all tried really hard. I think some of them didn't work quite as well as they thought. But they made them all work. Which meant they had to bond them, and then put them on the bench, and hook them up, and figure out how to test them. And then demonstrate to me, by looking at the scope, whether they were working or not. And that became my style of teaching—The courses I'd been teaching always had a lab component, because that's where people *really learn*. And I've always learned much more in the lab than I do out of anything textbook-like. And I always felt like students would as well. Of course, there are two kinds of students. There are students that just love that. And that was why they came to Caltech.

Zierler: Right.

Mead: And then there are students that really just want to pass exams and get grades. So, I always had double-hump distributions in my classes. They were the two kinds of students. And the best students in my classes were very often opposite from those that would excel in their other classes. But—in terms of support from Caltech as an institution. They were very good to let me to teach the courses I wanted to teach. And I think the reason was, it started out that they knew they were going to be needing a transistor course. And Dave Middlebrook went on sabbatical and I taught the course for him. I think I told you about that. And, so, they had somebody in this hot new field and those people were real scarce. So, when I said wanted to teach this new course, they would generally approve it, and I would get to teach it however I wanted to. I consider that the biggest kind of support you can get. Because then the connection with the students is where you really get things going, not just for yourself but for the next generation. And, so, that was why I taught. I didn't teach just because I had to; I taught because that was what I was there for.

Zierler: Right.

Mead: The research and the teaching were *not* separate—-They were part of the same learning dynamic. And this new course was the same way. And, so, that worked. The students rapidly became much better at designing these things than I was. Because they're just very quick and they have their own ideas about what they want to do. So, we ended up having projects that were more and more ambitious each year, and we would get them fabbed [fabricated] and that—by then, it was around 1974, so it would've been the class had been taught for, like, four years. And our division chair, which is what we call our deans at Caltech, came to me—we had a new division chair that year. His name was Bob Cannon. Great guy. The guy before him had been a great guy, too. Cannon said, "Don't you think we need to teach computer science at Caltech?" Well, of course, I had been sort of drug into it because you can't make these chips without writing some pretty hefty programs. And, then, you needed better design tools, so you ended up having to build your own design tools. The next thing past the rudimentary little PAL program I was using was an honest design program—oh, I can tell you that story sometime; that happened a bit later, but we started doing our own design tools. And, so, I had been dragged, kicking and screaming, into computer science. I had to learn about it. Never had any exposure to it. I shouldn't say no exposure, but we didn't have a program at Caltech. Don Knuth, who, you know—fantastic computer science guy. Top of the list—had been a post-doc at Caltech. And they wouldn't keep him on as a faculty. He'd already written, by hand, his famous book on the analysis of algorithms, and nobody knew what to make of it. The whole place was that clueless. Don has since become a good friend since he's been at Stanford. Wonderful guy. But that's how clueless the whole place was. And most Caltech people, I think, thought of it as, sort of, a technician-level thing. You learn how to program. Well, everybody has to learn how to program, but that's not an academic discipline. That's kind of a technician-level thing. So, anyway, of course, by 1974 it was becoming pretty obvious that if you didn't have computer science you were not going to be a technical place.

Zierler: Yes, right.

Mead: And we didn't have one. And so, what I was doing was about as close as we had, except there were some people doing numerical computations—and they were in applied math. Which is another story— So, of course, I said, "Well, of course we should. But if we do, we should include this technology because that's going to be the vehicle by which computer science goes out into the world. So, if you don't have that represented as part of computer science, it doesn't make any sense." Well, of course, our dean was talking to people all around in various places—-he had been Assistant Secretary of Transportation before he came to Caltech. He'd been at Stanford. Then they got him to come back and take a four-year stint in the transportation department. And then he got recruited at Caltech to come and run our engineering division. So, he had a very different view than anyone at Caltech, coming from Stanford and through government office. He had a very different and broader view than a typical academic. So, he had nosed around in his contacts about who were interesting people to talk to. And one of the people he found was Ivan Sutherland, who had been at DARPA for five years. I guess you get a five-year stint at DARPA. And Ivan ended up running IPTO, their information processing technology office at DARPA—well, it's gone back and forth ARPA and DARPA. I forget what it was then. So, he knew the ropes, not only there, but ARPA or DARPA was funding a lot of the leading-edge computer science in the whole U.S. And he had been the one who was watching over that. So, he knew everybody in computer science. And, so, it turned out when he retired from DARPA he got a job at Rand, which is out by Marina del Rey, west of L.A. And it's nice out there. It's near the ocean. So, he got a place out there a block from the beach and worked at Rand doing whatever he felt like. And, basically, looking for what he wanted to do next. And, so, somehow Bob Cannon got ahold of him and invited him over to Caltech, and got him to talk to the people who thought computer science might be a good idea. And I was one of those. He arranged for me to be the last guy that Ivan saw that day, so, of course, we went off to dinner. And we hit it off right away. Ivan's a great high-energy guy. Is very open to new ways of thinking. In fact, to the point where he's on the verge of people thinking maybe he's crazy sometimes. Just like I am.

Zierler: [Laughs]

Mead: And we both had our own style but we were willing to be on the edge, and maybe over the edge once in a while. Because how do you find out where the edge is if you don't push it?

Zierler: And, Carver, what did you come away with after that dinner in terms of your shared views on where this was going?

Mead: Well, mostly, I was selling Ivan on the fact that this VLSI technology was going to completely revolutionize computer science. Because it simply wasn't represented in computer science, and there's a whole bunch fundamental computer science issues. Like, for example, the architecture of an information system is very different if you do it on a chip than it is if you just program it in a program and take the computer as a thing you can't change. It's a very different way to look at the world. If the way you're looking at the world is to develop the hardware that does the computing that you need done—and, of course, general-purpose stored program will be part of that always. But there'll be all these other things which you do all the time and you don't want to tie up cycles of your big computer to do them. So, I spent the evening selling him on this. Well, it turned out that he bought it. And, so, then Bob Cannon managed to get him an offer, and the two of us started the Computer Science Department. That was '75. The VLSI stuff was the real reason Ivan came to Caltech. Because he knew what was going on in all the rest of the computer science research community. And it just wasn't there. So, this was a chance to be on the leading edge again, and he liked that. He had pioneered computer graphics as a grad student. And, then, gone off with Dave Evans in Salt Lake City and started Evans & Sutherland, a company to do real-time computer graphics. Back when nobody did that. So, he had been on the leading edge, and he knew what that was, and he loved it. And, now, he could see this was another chance to do that. That was just great because we got a running start. He started designing these projects. Then he saw what I was doing to try to batch up the projects and get them ready for the mask house. And he said, "Hey. You're working too hard—- We should be able to get somebody to do that." I said, "Yes." Because I knew a lot of people in Silicon Valley, and I had given these talks. I'd been talking to people about becoming a silicon foundry. By the way, let me tell you where that term came from.

Zierler: Uh-huh.

Mead: You hear it all the time nowadays.

Zierler: Right.

Mead: People think I invented it. I did not invent it. I was talking to Gordon. They were just setting up a second fab. The original fab had been in Mountain View, which is just south of Palo Alto. Do you know that area?

Zierler: Sure.

Mead: But they needed a second fab because they had the 1103 which was their first dynamic memory. A 1,000 bit, maybe? Which was a lot for that time. And they needed a place to ramp up production because that was going to be a really hot part. So, they built a new fab in Livermore. You know, East Bay. Ted Jenkins became the fab engineer, you know, in charge of fab engineering for Livermore. No, I think he was in charge of the fab. But, whatever, he basically became the technical guy in Livermore. The brand new, state-of-the-art fab, and was set up to do 1103s. And that's how Ted became the place I got our projects fabbed [fabricated], was at Livermore. That might have been in '72 or thereabouts. And I remember having dinner with Gordon one night, talking to him about Livermore. And he said, "Oh, Yes, Carver, that's just going to be a foundry for stamping out 1103s." So, Gordon invented the term "silicon foundry." That's a place you just stamp out these parts.

Zierler: That's it.

Mead: [Laughs] And, so, I thought it was a good term so I started using it, but Gordon came up with it. And I tried to get people interested. A friend of mind said it the best. He said, "Carver, you're doing a Protestant rite in a Catholic church." [Laughs]

Zierler: [Laughs]

Mead: The guys that had fabs did not want to be a service. They wanted to own the designs. And I kept saying, "Look, when you go get a book printed, you don't buy a printing press."

Zierler: Right.

Mead: It makes no sense. There's a very clean interface between the pattern and the fab. It's called a mask. And there's a rather compact set of rules that if you obey them, you won't violate any physics in the process. And that's the point of minimum information that has to pass from one side to the other. From the design people, more and more of which was getting done by computer because of what we were doing, and by then other people were starting to do it a little bit. And then the fab people, who live in a whole different world. They're not doing information technology; they're doing the most exacting kind of chemistry. It couldn't be two different worlds.

Zierler: Right.

Mead: So, passing a mask across that boundary was a fantastic interface. You don't actually have to pass the physical mask, you just pass the pattern. And that pattern is what you design and that's what they fab. And they look at it from two completely different points of view. I was trying to sell this to people that had fabs, and it went all the way from, "Yes, that's interesting but we want to own the design—," to, "You're threatening our business, buddy." And, unfortunately, Andy Grove came up with the idea that I was threatening their business. And I tried to explain to them, "Look, the more people do custom chips, the bigger the market for standard chips because that's the way markets work. There's nobody going to have a thing, except a hearing aid or something, that doesn't have a general-purpose machine with it. So, all it does is enlarge the market for everybody."

Zierler: Carver, you understood this intuitively, or you had economists who were advising you on this kind of thing?

Mead: Well, no, I had just watched Silicon Valley.

Zierler: Yes.

Mead: I mean, I'd been consulting there for years. I watched what happens.

Zierler: Yes.

Mead: And it's not a zero-sum game. It just isn't.

Zierler: Right. Right.

Mead: You know, once in a while it is, like when Fairchild—well, even that one, they had their chance.

Zierler: Sure.

Mead: And just didn't do it. That wasn't Bob's and Gordon's fault. They had tried like heck to get it done at Fairchild. But Andy's a combative guy. Andy and I had been working together for years before—while he was still at Berkeley and he was teaching there and I was teaching at Caltech. And then we would both consult on the same day so we could work together at a Fairchild lab. So, I got to know Andy really, really well when he was still a student. Andy's Hungarian and he's a wonderful—I was told that in Hungarian, the word for discussion and the word for argument are the same word.

Zierler: [Laughs]

Mead: Andy loves a good argument. And it's great to argue with him because he's so smart and so quick. Partly what I would get, my kick for the Friday I was up there, was my argument with Andy. He had an admin whose name was Adrienne Giuliano and she was a spirited lady, because you had to be if you were going to work for Andy. She told me one time, she said, "You know, I can always tell how good a meeting you've had by the four-letter words that are coming out under the door." [Laughs] And it was true. Discussions with Andy were arguments, and they were always good. But that one, I didn't convince him. But meanwhile, Ivan was onboard and he saw me trying to organize all this to get masks made, and he said, "There's have to be a better way to do this." So, it turns out that there was this little thing called ISI over in Marina del Rey. Information Sciences Institute, maybe? And it was a little think tank that had the same relation to USC that JPL had with Caltech. Caltech administratively watches over JPL. And USC administratively watched over ISI. But actually it was a DARPA presence on the West Coast. Ivan had a former student there by the name of Danny Cohen who is super bright, super quick, and he really liked the idea. So, he just started working with Ivan to automate this process of getting the projects batched up and getting them put on the same chip. And getting the thing fabbed. And then getting them diced up to where they were separate things. And then getting them packaged and bonded. And then getting them back to the designers. And, that was Ivan's idea. He worked hard to convince DARPA to fund that thing, and eventually succeeded. And they set up this thing called MOSIS, which, of course, that was Danny Cohen's name—MOSIS, MOS Implementation System. And they got going but we had actually done a fair bit of it just informally with Danny. Because Danny came over and he and Ivan designed things together, and, so, we were getting the pieces put together. But MOSIS was Ivan's idea of a middle person that could take designs from universities to start with, but anybody who creates the pattern, and then interface it to a fab. Because, for the same reason that people are different, that languages are different, and that interface was an important thing. And that was—just to jump ahead a little bit while we're on that. That was the link after Lynn Conway taught the course, in I think '81, back at MIT. Then she came out and went to DARPA, and actually took over all that and drove it forward. Because it really needed someone there that was super knowledgeable. And, of course, by then Lynn was super knowledgeable so she drove that thing forward in a big way. And, so, that's that thread. But there's a few years in between when this was all happening in the time. So, I might as well tell you about that.

Zierler: Please.

Mead: Ivan and I were doing this department which had really two parts. VLSI Design was the big part of it. And, then, we had one faculty; a super bright guy by the name of Jim Kajiya, who had done fundamental work at the University of Utah. Because in those days they had the Dave Evans lab which did all the leading-edge graphics. So, Jim was a super bright graphics guy that Ivan convinced, to come out and be an assistant professor. And he started building up a graphics group which is another thing that was still leading edge and was for quite a few years after. May still be, actually. There's still a lot going on there. So that was what our little department was. And we had a guy that had been around. A guy by the name of Fred Thompson who did natural language understanding. That was our little operation. And, of course, Ivan was always inviting people to come out and spend a quarter with us and teach a course. Or, give some seminars. Or, whatever. And that was the way we got to know all the neat people in computer science. Which was phenomenal. It turns out, one of the big shots in computer science in those days was Edsger Dijkstra who was in Eindhoven. He was one of the couple of structured programming gurus. You know, if you just write random code it doesn't scale and he saw that early on and invented ways to structure code so that it was modular and that sort of thing. It was early on, and we had to do that for the chips. So, in our own funny way we were right there because we had to design our chips so they were modules inside of modules. And the interfaces had to be agreed on and all that. And, so, it was a perfect environment to introduce structured programming as a metaphor for how we did our software. And, then, Ivan, he was marvelous at this. He got Edsger to send over his post docs. So, we got two post docs for a couple years from Edsger. One was Martin Rem, who unfortunately isn't with us anymore. A fantastic guy. And he and I together started the whole business of: What does it mean to optimize a VLSI design? In the sense that how to do you optimize a program? Well, this is: How do you optimize a VLSI design? What are the cost metrics? What do you do? How do you assign weights to what's important? And, you know, all the stuff you do. And that got that whole way of thinking started. And then the next guy that came over was Alain Martin who just went Emeritus a year or two ago. He stayed at Caltech and became a faculty there. He's the guy that pioneered, I think, the longest run at self-timed logic that didn't rely on a global clock to make it work. And that's becoming more and more important, of course, as we've gone to bigger and bigger, more complex systems. Ivan was very much into that. He made some of the very original self-timed circuits, and found out how hard it is to get them right. And, so, he was very keen to get someone. And that turned out to be a very far-sighted move because we had a presence at the leading edge for that design practice. Which was way before its time actually. But what a wonderful direction. And that went quite far under Alain Martin, who I still see once in a while on campus. Ivan knew lots of people. And, for example, he had just retired from his Evans & Sutherland company, so he introduced me to Dave Evans. So, I gave the pitch to Dave Evans. And Dave said, "Yes, we could really use that." And, so, I gave a little one-week course at Caltech to about six or seven people from Evans & Sutherland on how to do this chip-design stuff. Just because they were leading edge in graphics, and it was very obvious to me that graphics was a place that could really use a lot of custom chips. That has turned out to be, of course, major. Nvidia really has made a huge living doing that after a number of other people tried. Typically, something would happen when Ivan introduced us to somebody. Like, I would get invited to come back and give a talk at CMU or MIT or Stanford. In fact, there were two guys, Newkirk and Mathews, who were grad students at Stanford. And they heard about me, and one time when I was up in the Bay Area, I had dinner with these two guys. And basically convinced them that this was a good thing. And they said, "Well, would you help us along if we started a course at Stanford." And, so, they did. They taught the first VLSI course at Stanford. And that was, I forget what year. It was in the, somewhere in the '70s. '77 or 8 or somewhere in there. So, like Ivan said—Ivan really could see we needed help. MOSIS was the first thing that we needed—-somebody to interface with the fab. And, then, it was their problem to deal with the fab. The system people didn't have to dicker with the fab about what it's going to cost to get the wafers run. That doesn't work. And one of the people that I got introduced to was Ivan Sutherland's brother, Bert Sutherland. Bert was at Xerox PARC. And in those days, I don't know if you remember those days at all, but Xerox PARC was right up there with CMU as one of the top computer science places. And in many ways were ahead, because they had enough resources to build their own hardware. So, they had built this thing called the Alto which was an amazing personal computer for the day. And everybody had one on their desk and could get work done. And it was the environment I had imagined. So, Ivan invited his brother and someone else from Xerox PARC, I don't remember who, to visit us. So, I showed Bert and his colleague around. Showed him the stuff we were doing. And by then some of the chips were getting to be respectable. And I had a couple of grad students who were doing thesis on chips that did interesting things that people could see were interesting. And, so, Bert said, "Why don't you come to Xerox PARC and give us a seminar?" Well, they were the top of the heap—in terms of computer science they were the number one spot. Of course, I said yes. But I was also a little intimidated because this is THE number one spot. I wanted to put my best foot forward with these guys. By then, I was not wearing a tie, ever. But I wore a coat and tie for this one. Well, they took me to the room where I was going to give the talk. And it was what they called their Beanbag Room. And it had no chairs, and a bunch of beanbags on the floor. And everybody made up their own comfortable spot when there was a seminar. And here I was with a coat and tie. [Laughs] The heck with that. I took my coat off. Got rid of the tie. Gave my seminar. And after the seminar, one of the people came up and it was Lynn Conway. And Bert had told her that he thought maybe the two of us would have some common interests. I didn't know Bert had done that, but, he did. And, so, we went off to lunch. They had their own lunchroom. So, we went off to lunch and then I spent a fair bit of the afternoon talking to Lynn. She was right on it. So, then they wanted me to consult with them. And, so, Bert signed me up as a consultant after this seminar, in spite of my tie. [Laughs] So, next time I came back, I, of course, spent time talking to Lynn again. And she was even more up on it by then. At the end, I think it was at the end of that second meeting, although it may have been a third meeting, I don't remember. She said, "You know, Carver, you really should write a book about this." Well, you know, I'm a guy that does collaborations.

Zierler: Right.

Mead: And that works better for me. So, I said to her, "Well, if you really think so. There's a way to get it done."

Zierler: Co-author it.

Mead: Yes, exactly! And she said, "Well, let me think about that." So, then the next week when I came back, here was Lynn, with a stack of books about 2 feet high that she had acquired that might have anything to do with what I had been teaching. And, she said, "I've been through these books. There's nothing like what you're doing in any of them. I'll be your co-author."

Zierler: Wow.

Mead: And that was the beginning of that process.

Zierler: You must have been—Carver, you must have been so impressed by that?

Mead: I was. I was impressed by her directness. And the fact that she could figure it out.

Zierler: And her mastery of the literature in such a short amount of time.

Mead: Yes. Yes. Yes. And that she really cared and she was a quick study.

Zierler: Right.

Mead: So that clinched the deal for me. So, and of course, I had been teaching the course already for 6 or 7 years, and had it pretty well figured out. It was mostly getting that down on paper. And, so, we got into this rhythm, which was really neat. I would go up to the ranch. I had this ranch in Oregon which was right on a river there. My son now has it, and the grandson is actually doing the work. I had bought it because my son and I were single parenting and we needed a place where we could just go and get away from everybody, and be together, and do stuff. That's the way we've always related, since he's been a little tiny kid was, we'd go do projects together. And that's just a wonderful way we've always related, and so I figured we could do this ranch thing together and it would be hard work but very different from academic stuff. And it's in a beautiful environment with fresh air, and hard work, and trees. We bought a filbert orchard, which—you know filberts?

Zierler: Yes.

Mead: Most people call them hazelnuts now. That's a long story, too. It turns out, you can just barely do it if you take all your holidays and all summer, you can do it with two people. During the summer it got to where he was doing most of it. And I could go there and do some thinking because there was a beautiful place right on the river. Big trees and just really lovely. So, I would go up to the ranch because computer science by then was going in funny directions and was complicated with people stuff. Which, you know, places get like that.

Zierler: Sure.

Mead: That's not my thing. I don't do well when people aren't getting along. So, I needed to get away just for sanity. So, I would go up and I would spend Friday. By then I was consulting with Xerox PARC, not Intel anymore. I'd stop and do my consulting day there. And then I'd go up on from there up to the ranch which is just east of Eugene. And there's a direct flight from San Jose to Eugene so it was an easy thing. I'd go up to the ranch and get a good night's sleep. And then go out on the deck overlooking the river. And the first day or two, I would just draw the figures that were going to be in that chapter. Not finished figures, just sketches. It's kind of like a storyboard that you would do if you were making a movie. I would do the figures, because I think in pictures. So, for every idea there had to be a picture. So, I would do that. And, after I felt good about the storyboard, I would sit down with a little recording device and I would dictate what I had to say about each figure. And, then, on my way south would be the next Friday. I would go into Xerox and give the tape to Lynn who had a person there that would transcribe it. And, meanwhile, there was a draft from the last one. And we would work on that together. And, so, that's how we worked together. Extremely effective. It was just a very, very productive time. So, that's how we got the book done. There were rough spots at the end because Xerox changed their system, and in the process, they changed their font assignments. So, a whole bunch of symbols didn't come out right when we printed them. They had a wonderful printer down in Pasadena. Xerox had a lab in Pasadena that did government work. And one of the things they had developed was a very high-resolution printer that took the same code as the one they had up at Xerox PARC, but was much higher resolution. And Bob Sproull, who was also at Xerox PARC—he was a graphics guy that Ivan knew real well from Salt Lake City. Super good guy. He was writing this book on graphics. And it was the same time we were doing the VLSI book. I would very often take he and his wife out to dinner after a day's work there at the lab. And, so, he became a good friend. I haven't seen him for years, but I got to know him and his wife well. I've always worked better when I have collaborations like that. So, the thing with Lynn was really good. But we ended up having to decide whether we were going to do what we had to do to get it printed on this high-res printer down in Pasadena, which is what Bob Sproull chose to do. Or, were we going to have a publisher typeset it? And, it turned out Lynn took over that part and decided that we would have the publisher typeset it. Which turned out, in retrospect, to be a bad mistake. Because Bob Sproull got his done a year before we did. And that year was hell. It was trying to go from a streamlined system to an archaic system. And we, both of us, suffered a lot in that. But you never know 'til you do it.

Zierler: I said, "Right."

Mead: Oh, Yes. Yes. Okay. Good. So, anyway, that's how that happened. That was a very productive time.

Zierler: Well, Carver, we are—we're at the two-hour mark so I thought I'd ask one last question for today's session, if that works for you?

Mead: Good time to break.

Zierler: Okay, so then let's—we'll end with your collaboration with Lynn. When the book came out, what do you feel—what was your primary objective with writing this book? And where did you see it taking the field next? You know, it was out there, people were reading it. What do you feel was your primary objective with the book? And, in what ways did you think it was going to move the field forward?"

Mead: Well, I saw it as a way to make it possible for the other universities to teach the course. And, in that way, it was a big success. And, basically, everybody pulled in that direction and made it successful. So, I feel like that was a big success. And, I was very—I was totally exhausted because I had been, this whole time, I had been out promoting the whole thing. Which is a part that Lynn didn't get into 'til later.

Zierler: Right.

Mead: And, then, she did it from a very effective position at DARPA. So that was a really excellent thing.

Zierler: Right.

Mead: And, so, I was spending—it's a lotta work.

Zierler: Sure.

Mead: Going around giving these inspirational talks about where the future's going. So, I was exhausted, and I didn't have any more energy. We had gotten it over the top. It was rolling now. MOSIS was running. I didn't have to teach the course anymore. I didn't have to do any of that. I just wanted outta there!

Zierler: And there was momentum on its own, at that point. Absent your cheerleading.

Mead: Oh, Yes. It wasn't just Lynn, but there was all the other stuff. The MOSIS stuff. And several universities—in 1980, before the book was really out. Lynn had gotten a batch of pre-prints run off at Xerox of the first five chapters. It made a nice little book. We could teach out of it. I did a summer course at the University of Washington, right here, where they got Boeing to fund it. The University of Washington computer science people—they're a first-rate computer science place. They wanted this as part of their offering. They agreed to get funding for this summer course for teachers. So, if a person from a university wanted to teach this course they would come and spend the summer learning this stuff, and getting a chip designed on their own. And, then, they could go back and teach it to their students. We had one guy there, Lance Glasser, who left the course a week early because he had to start teaching it at MIT. And, so, he went straight back and did it on his own.

Zierler: Yes.

Mead: A gutsy guy. He later became a DARPA guy, too. That was an exhausting thing. It was all summer long. So, my summer was gone, and I was outta there. I had no more energy for it. So that was where I took some time off. I had thought about what I wanted to do next, and that was the beginning of using the brain as an inspiration for computers [and] computation.

Zierler: Right.

Mead: That was the thing, I spent the following 15 years, on. But it was an abrupt cut for me. It was just, like, I can't do this anymore. I can't.

Zierler: Right.

Mead: I couldn't do one more course. I couldn't do one more talk. I couldn't do any of it—-squeeze blood out of a turnip.

Zierler: [Laughs]

Mead: So, anyway, that's where we are.

Zierler: That's a perfect cutting off point. And we'll pick up from there next time.

Mead: Good to see you.

[End of recording]

DAVID Zierler: Okay. This is David Zierler, oral historian for the American Institute of Physics. It is July 26th, 2020. I am so happy to be back with Professor Carver Mead. Carver, thank you so much for joining me again.

CARVER Mead: Great to be here, David.

Zierler: Alright. So, let's pick up on where we left off last week where you were talking about that formative summer where you were spent. You wanted to move on to something new. And that this you saw as the opportunity to start on what would become a research endeavor for the next 15 years. Which is, you started to think of the human mind as a source of inspiration for where it might take electronics and physics and microprocessors.

Mead: Yep.

Zierler: So, the first questions there is, was it a particular event or was it an evolution in your thinking or in the field that the human mind would be a source of inspiration for where your future research in electronics might take you?

Mead: Well. For [pause] a little over 10 years, probably 12…Yes, since 1968, I told you that was when I had a huge change in what I thought was important because it was no longer the physics that was going to limit. It was our ability to design complicated things and get them to work. And that's a whole different challenge from making an individual transistor work better. Which is what I had been doing, roughly speaking. So, I didn't know anything about computer science or complexity theory or any of that stuff. But it was real clear that computers were in one of these stuck places. They knew how to build big computers on von Neuman style stored program, step at a time, sequential computer. And, actually, at Caltech we even had, for quite a number of years, a big analog computer. And that was just after World War II. And what it was for was analyzing these big aircraft structures. For example, you could have a wing of an airplane. And you wanted to know what kind of modes it would have—-normal modes of how it would bend. Because those things determine if you have a high-Q mode someplace, it can get excited while you're flying and the wing could come off. And that's just not fun at all. So, at that time there was no digital computer that was anywhere near being able to model complicated structures like that. But you can make an analog model of a mechanical system. There's a book by Gardner and Barnes that tells you exactly how to do that. I always got all my students to read that book and understand it because the connection between mechanical things and electrical things is not going to go away. [laugh] And you'd better know how to do that. That book is still, to this day, I think the best one to understand at a fundamental level, how do you take a system that's half mechanical and half electrical and analyze it? And figure out how to make it work? So, I knew something about that. But also, I had worked with one in one of my undergraduate classes. We actually built a little model. We had one of the analog computers. They had operational amplifiers, inductors, capacitors and resistors, and you could make a mechanical model with it. It was great fun to do that. So, that's one thing that had left an impression on me. Most places didn't even know you could do analog computing, but a few places did and we just happened to be one of the places. Which, I was just lucky. [laugh]

Zierler: Carver, when you say "we" who were the chief partners with you on this?

Mead: Well, we…I was a student then. And the guy who was really good at it was Charlie Wilts. And Wilts was a wonderful mentor and teacher and a guy that really got in and understood things all the way to the bottom. He became a close, personal friend after I got on the faculty. We used to go hiking together on weekends and so forth. But, he was a guy who had actually made an analog computer work and just understood it all the way to the bottom. So, that was one of the high points of being at Caltech. So, Yes. I was, as a student, exposed to this idea which wasn't obvious to most people. And that had quite a lot to do with my viewpoint. Because it was always there as a possibility. Then when I started doing the VLSI stuff. When you actually design the chips, which I was doing all day every day…doing it and teaching students how to do it. And writing about it and all that. It became very clear that, as we were able to build more and more complex chips, the models we had for what computation was were extraordinarily limited. A stored program computer dominated everybody's thinking. So, when you would say computing, that's the only thing people could think of. When we started computer science, I think I told you about that. There was a guy from Edsger Dijkstra's place that came to do a postdoc with us. And his name was Martin Rem. I think I mentioned it to you. And he was an extraordinarily smart guy. Very mathematical and he understood the computer science thing very, very well. Because Dijkstra understood it very well. And Martin was also a wonderful human being. So, he and his wife moved over to Pasadena for a couple of years. And we became very close collaborators. And I kept talking to him about, "We don't have a way of figuring out what optimum means for VLSI. This is going to be the way every computing thing is built—-out of this stuff that we're teaching the students to do. And we don't know what it means to have a good design." This became a focus for me when I wasn't designing the chips or teaching the students to design the chips—-over dinner with Martin. And at odd moments we'd just talk about, "What does it mean?" What's a measure of cost? Well, the area of the chip is obviously a cost. The power that's required is a cost. And the time that it takes to do something is a cost. So, there's this kind of three-dimensional cost metric that you could use to put a number, at least a relative number, on various designs. And that was essentially independent of the particular technology because it was a relative thing. And then you could put your own cost weighting later—-how do you trade off time and power? How do you trade off time and area? That sort of thing. And that became a way I started teaching the students. How to think about what are we doing here? And you can define an operation somehow. Either a simple logic operation or an arithmetic operation or whatever. And then you could look at different ways of doing it and get an idea of how efficient it was in this three-dimensional cost space.

Zierler: And Carver, when you're asking such an existential question… [pause]

Mead: [sneeze] Sorry.

Zierler: When you're asking such an existential question as what are we doing here? Is that…are you approaching that more from a basic science perspective or from a societal application in where this technology is ultimately headed in its broad application perspective?

Mead: Well, to me they weren't separable. This was an important thing. Every day we were faced with, for example, if you make a serial processor that does things serially, or do you make it parallel so that it does all things in parallel? They have different costs and different performances. And, so, if you ask a question, what's the time, energy, and area it takes to do this thing? Then those two design styles were very different. So, we built a lot of things that would do what we call pipelining. Instead of having a big parallel multiplier, for example, you could make what was called a pipeline multiplier where it took as many clocks as there were bits in the word. But it was order n size thing instead of an order n-squared size area. Well, that's a big difference if you have a long word. So then if you ask the question, well, but how long does it take to do an operation? It takes a lot longer to do it in serial. Well, be careful. It isn't how long it takes to do one. It's how many you can do per second. And if you do things serially you can often have a new result every bit, even though it takes a word time to do the whole thing. And that depends on how you design it and so forth. So, this became a very real question every time we went to design a chip. And some of the first chips we did, because the area was very hard to come by in the early days, we did a pipeline multiplier that a couple of the students…there's a long story there but anyway, they came up with this thing and it was a really neat thing. So, this wasn't an academic question. But it became an academic question because you wanted to know. And if there was a principled way of characterizing designs by their cost metric, that made the discipline more sensible. It's something that you could discuss. So, it was important in a practical sense but it also was a nice academic question. Dana Scott, back at CMU, was a complexity theory guy. And he got interested in this stuff that Martin Rem and I were doing. And he went off and did some really beautiful work on the complexity analysis of VLSI circuits. Really nice. So that was a bridge to a discipline in computer science that had been wrung out pretty well by then. And this was a new playpen for those guys. So that was a lot of fun and it gave me a new kind of person to talk to. So, CMU invited me to come back and give a talk and Dana was the host…so we spent a lot of time at dinners together and all that. And it was just a very nice way to broaden the way to think about things. I'd never done any complexity theory, and they thought totally differently than I had ever thought. So that was great. I was learning something instead of just grinding. But also, it was very useful because people had become, even then, very stuck in the way they thought about designing digital stuff. Largely because they did not have a way to evaluate how efficient it was. And this was a way you could do it. So, Martin and I wrote up some papers about that. We couldn't separate. Some things just had to become an academic thing because you wanted to know the answer from a fundamental point of view, and that was a way to do it. But then you wanted to go out and practice it by making real chips and seeing how well they do and seeing if you missed something. It was a thrilling time, that way. What we learned was that the finer grain you made the computation, the more effective it got by these metrics. And that was really interesting because it was exactly the opposite direction that big computers were going. The architectures were big things and pretty much the same in terms of computers today. The big microprocessors were just like the big behemoth computers of the '60s. They're just made on smaller stuff that runs faster, but in terms of the ideas, they were just like we talked about with physics. If you get a set of ideas that work, then they get taught and written about and they get built into a system and that's what kids learn about in school. And that's what, if you write papers about that, they get published and if you try to do it about something else, they won't get published. And if you do that your students will get faculty jobs and if you do something else, they won't. And so, there's a huge inertia that goes with a thing that's been thought through and been built into a discipline and has an industrial base behind it. And jobs get assigned, certain disciplines. So, like, I think I mentioned to you when we started this, there was an architect person and there was a system design person that took an architecture and made it into things that would do this and that and hooked them up just logically. You know, multipliers and registers and stuff like that. And then, there were the logic designers that made those out of AND gates and OR gates. And then there was the circuit designer that took those AND gates and OR gates and made circuit diagrams of them out of transistors. And then there was a layout guy who took those circuit diagrams and turned them into patterns that you could fabricate—-and that's a huge stack of stuff! And if they all thought the way they always had when they were building stuff out of vacuum tubes, it's going to have the same structure and the same organization and the same limitations—-it just is. And there's nothing you can do about it. So, that's when we conceptualize the idea of the tall, thin person that stood on the layout at the bottom but had their head up in architecture. And so, could think across those levels without getting stuck in one of them. This was life developing VLSI…as a discipline. And getting people to think in a little bit different way. It was still digital and it was still logical and it was still on the same kind of transistors. None of that had changed. But just thinking about how you picked an architecture and how you went from there to the transistors was really very different from what people were doing in industry.

Zierler: Right.

Mead: And the pushback was just amazing. Because, "No, you don't do it that way." [laugh] "Well, yes we do and it works." And if it hadn't been for the students going out and every once in a while one would get a job where the boss could see that, "This seems to be a bright kid and it'd be really great to have a chip that did this thing, and why not give them a chance? Because they said they made one and they showed me that it's working. So, if they have done it before, maybe they can make it work. And that would be neat." And so every once in a while a student would get a chance like that. And they'd do a chip and it would work and it would revolutionize the way that particular place in that particular company would do things. And then the word would get around. So, there were just enough bright spots of success out there that people couldn't say that it doesn't work. And they couldn't say, "Oh, well. You can do that in academia, but it doesn't work in industry." But, you know, just go down to Hewlett-Packard and look at Dr. blah-blah-blah and, "Oooh. Hewlett-Packard. Oh, well they know what they're doing." And so then get on the phone and the word got around. So, that's really what broke loose the acceptance of thinking about it in a new way. By the time the book came out we had already maybe a dozen students who'd had successes in industry doing this or that little project that ended up as a chip. And those were things that people couldn't deny. Just talk to this witness. But, in the process, it keeps bothering you. There's this sequential program mindset: Alan Turing thought of the sequential execution of a program as how a mathematician proves theorems. That was his model. And it's a wonderful model because it has a lot in common with what we call computer science. That there's well defined steps and each one has a well-defined output. It becomes the model for what you use to decide the next step. So that looks just like a stored program, right? It's an algorithm. But, you look at the medium of a million transistors on a piece of silicon and here they are all lockstep going, "Ca-chug. Ca-chug." And most of them weren't doing anything useful, most of the time. And it just wasn't sensible…so that's where we got into these pipeline things and distributing the memory in with the computation, so that you didn't have to go clear across the chip to get one word of memory. And then shovel it someplace where there was an arithmetic unit and go, "Ca-chug." And then take it and go clear across the chip to put it back in the memory. And it was crude, rude, and lewd. It was just a terrible waste of this beautiful medium. So, it just irritated me. We did some very unique and creative designs that used the transistors better. And we had this sort of cost-metric way of thinking that would guide us in that. But still, it was, "Ca-chug. Ca-chug." And I had this background where I had a class in analog computing one time. And the way analog computing works is it just…the data…they're just signals. And they do what signals do. And if you have an oscilloscope you can peak at them. But it's a real time thing. There's no, "Ca-chunka." You want to wiggle the wing to see if it's going to resonate or not. You can do that. But that's a real-time thing. It's a very different way of thinking. So, it turned out there were neurobiologists at Caltech who were very broad thinking people. The one very remarkable one was John Allman. And he wrote this wonderful book called *Evolving Brains*: A *Scientific American* book. Which is basically a lifetime's worth of wisdom at *Scientific American* level.

Zierler: Yes.

Mead: Most remarkable. John's one of the more deep thinkers that I've ever met and became a very close friend.

Zierler: Did you work with him substantively, Carver?

Mead: Oh Yes. Oh Yes. But it started out that I just I got to know him, and we'd have dinner and talk about things. So it was like an itch that I hadn't been able to scratch. And I began to feel like maybe there was a way of scratching it. So, when I got done with all this stuff I told you about—-I got through the teacher's course and then I could take a much broader view. It turned out that John Hopfield was a solid-state physics guy that I had known back in that part of my life, and he was branching out in biology. We were able to get him to come to Caltech as a professor of, I think, maybe chemistry. But it was really biology…I don't remember the details, but anyway, he came to Caltech. And because we had both done solid-state physics…I had only met him a couple times, but we became friends and had lunch a lot, and talk about this whole thing. Because he was interested in complex systems. The particular problem that he had spent a lot of time on was called spin glass problem. This was the problem that Anderson, who had done some remarkable things, had broken his pick on. Wasn't able to make progress. And John Hopfield had cracked it in one particular way. And he thought of it as a thing that might be useful in computing because he could show that if you had the spin glass and had a certain boundary condition on it, that it would settle into a stable place. So, it was like it computed something from boundary conditions. And it was just a time when neural networks were starting to be talked about by a few people. So, John was thinking about that as a way of doing computation. And I was trying to find a way to use transistors better. And then one day we decided we'd try to get Dick Feynman to come to lunch with us just to talk about this. So, we had a nice, long lunch where we were just talking about, "Look, these are all physical systems that do something you might imagine as computation. And they're all very, very different from stored program computers." And then of course, Dick Feynman was interested in, "Well, maybe the wave functions of quantum things could do that," sort of thing. It was kind of a quantum version of John Hopfield's thing. Which was kind of quantum too, actually. So, the three of us started talking about this and decided, "Hey, let's teach a course. And we'll take turns giving lectures about what we're thinking. And we'll see if anything comes of it." Because there's nothing like teaching a course to stretch you to…especially when you have colleagues like that!

Zierler: Yes.

Mead: —who would sit in on each other's lectures...and it was just a super neat experience.

Zierler: Now, would you frequently bounce ideas off of Feynman or did this rise to the level where you really wanted his input on this particular project?

Mead: Oh no. We'd go off to lunch and argue about stuff. And the three of us sometimes… and sometimes just two of us. And I'd been collaborating with Dick since my first year on the faculty. It was probably 1960…it was before he did his freshman class. So, it would have been 1960 or so. I had a grad student when I was a first-year assistant professor, and we were doing this thing about electron transport…I think it might have been Barium Titanite. But it was this highly polarizable material and it was an interesting problem…I guess we'd call them polarons now. I shared the grad student (Karvel Thornber) with Dick. I had known him, of course, as one of my teachers when I was a student. But now that I was on the faculty, and I was doing the electron tunneling, and he really liked that! And so, we would chat about that. So, I had known him for a long time. And first as a student in his classes and second as a colleague sharing students. So, no, we had had that kind of chat for a long time. And Hopfield, I think had known him…and I don't remember how…but it ended up being a great group because we had a certain commonality of stuff we knew about. But otherwise, very different viewpoints. So that makes it just a fabulous thing because you have enough common ground that you can understand what each other are saying. There's some of the biology stuff that, the language is so different, it's really hard to get a grip on. It turned out that we had this class and there were students all the way from, like freshman and sophomores, up through advanced grad students and a few faculty members, that came to this thing. And it was just a motley crew—-every possible background and everything. And that made it super neat because we got questions that were very different and of course, it was baffling. Because here were these three people who didn't have a clear idea where we were headed, but would come up with interesting things. So, you never knew what a lecture was going to be about. And what it was got us all thinking really hard about the whole question of "how does physics in general do computation?" And in particular, how do these wet biology things do computation? Because they obviously do. But, you know, how in the world? It's true to this day that the common housefly with a hundred-thousand neurons can do things that our most advanced autonomous vehicles can't do. Well, that's sobering! We've gone…factors of what? Ten to the 15 or 10 to the 18 or something in capability of doing computing on silicon. And we still can't do what a housefly can do. Even if we put a kilowatt into it, we can't do that. So, on the milliwatt that they operate on, we're not even close. After 15 orders of magnitude of evolution in Moore's law!

Zierler: But, Carver, how does 15 orders of magnitude at evolution in Moore's Law compare to millions of years of evolution in nature?

Mead: Great question, isn't it? And that's basically what we're faced with. This process that produced higher animals that all it cared about was, "Can I do things better than the guy that wants to eat me?" So, the creature that does it best eats the one that doesn't do it so well and…so steps of evolution have to pay off pretty much incrementally. I'm sure there are times when you get big random steps that just happen to work, but it isn't very often. And of course, now we know that a huge amount of evolution happens when some part of the biosphere figures out some little thing. And then makes a symbiotic relationship with some other life form and one thing leads to another and before long, becomes part of the DNA. And it's thought that that's where our mitochondria came from. That's where the photoreceptors in our eye came from, was that process of having a smaller version working. And then it becomes so useful that it gets incorporated into the DNA of a bigger thing. And of course, we see it in the industrial world. Startup companies are where the new stuff happens and then if they get successful, they get gobbled up by bigger companies or they grow into a bigger company. And both things happen. And so, these innovations get incorporated. So, we see it, like I say, in the free enterprise world there's a great example of evolution happening in front of our very eyes on a very fast timescale by biological standards. And then there are these amazing results of the biological process. So, this was all stuff that got talked about in this class. And the students were baffled and excited and we were baffled and excited. [laugh] And motivated to make this stuff move forward in our own ways. You know what you do, you leverage what you know to do the next step. So, after three years of this class…Dick missed the second year. Most of it. Because he was fighting his bout of cancer. But he was there for the third year and the whole thing was just an amazing experience. I mean it was just a whirlwind! And then at the end, Dick wanted to go off and do quantum computing and Hopfield, by then, had worked out his spin glass thing to where he had a pretty good grip on what became known as neural networks. It was just becoming neural networks at the time. And he went off on that direction. And of course, I was determined to use transistors to make some of this stuff. And there was one student in this class, a lady by the name of Misha Mahowald, who had taken a biology class as a freshman. She was an incredibly bright, creative person. She had fallen in love with the retina of the eye because the retina is a little outpost of the nervous system. It's actually part of the brain that is posted out there so it can take signals from the real world and filter them so that the signals mean a lot more. Because you can't afford a lot of wires coming back from the eye or else the optic nerve would be too thick, and you couldn't move the eye. So, it's a fantastic thing to study because you could imagine figuring out what it's doing, because you have access to the optical nerve so you can see what the cells and the retina do when you put patterns on the retina and that sort of thing. And there were biologists at Caltech that were doing that stuff. So she took this class and came up one day and she said, "I want to see if I can build a retina." And that was the first student that had a vision that was probably simple enough to make at least some approximation to and make it actually work with real images on a real piece of silicon, and yet was significant enough that it could provide a step towards the next thing which would try to understand the images better. So, she joined our group as, I think, a sophomore. That's a great thing about Caltech—-if a student is motivated enough, they can find a way through and they get involved with a research lab as undergrads. And it changes their life because they're on the leading edge while learning the rudimentary stuff. And it really motivates them to learn the rudiments because they can see why they could be important.

So, she picked up how to design chips, partly from the class and partly from just being around. She hung around the lab and I just treated her like another grad student. Gave her a place in one of the offices, and bench. And then the grad students taught her how to use the instruments and how to do layouts and that stuff. And so, she just became one of the group. And by 1983, I think, she had a silicon retina that could do very rudimentary things and by '84 she had a credible one that actually could put out signals that looked a lot like some of the signals in the optic nerve. It was sensitive to motion in various directions and edges and that sort of thing. So, that was the first substantive step that we took to demonstrate that there was a thing to be done there. And she went on…by then the rest of the group started doing it. And so, there were a bunch of projects. We built cochleas to listen to sound and we built things that would process signals and tell you which one was the strongest signal out of a bunch of signals. And there were a bunch of these computing primitives that were very different from digital primitives, but were useful in doing our version of the silicon neural network things. And by, I think it was '88 or so, there were enough projects that I told the students, "We're going to do a book, and each chapter is going to be one of your projects. So, if you have a project that works and is credible, it goes in. And if not, it's not going to go in." That was a motivator for a bunch of students to get their projects working, and it was a way of sorting out how things were. I wrote some description of the way I was thinking about all these related things. And that was a good discipline for all of us. The project she was working on didn't get in because we put that book out in I think '88, but she was working by then on a thesis. She had graduated, become a grad student, and done all that. I think it was around 1990, her thesis had become the design and realization of a system that took simultaneous images from two silicon retinas and matched them up and made a map of distance of objects in the image from the two retinas…so it was solving, in real time, the stereo matching problem. This amazingly complex problem had been worked on by Marr and Poggio, two MIT guys. I got to know Tommy Poggio pretty well. He was a great guy and so we used to hang out at meetings together. He had worked on it as a computing algorithm. And had found out it was a hard problem. So, Misha decided that she wanted to solve that problem. And in this real time way, which is just a completely different medium than anyone had thought about doing the problem in. And you can imagine having a collective analog circuit that the best match would be the one that gave the depth in any particular place. And so, it was a complex thing and it was all built in analog transistors. And the thing worked. And it was astounding! This was around 1990 or '91 or something like that. And she got the big award for the thesis who showed "the greatest promise for changing the direction of human thought."

Zierler: Wow. [laugh]

Mead: That was—

Zierler: Carver, when you say it worked…what does that mean exactly?

Mead: Well, you could put things out in there in front of the thing and it made a map of distance that corresponded where they were. It was astounding!

Zierler: Yes.

Mead: Just think about what that takes. Nowadays with a huge digital computer you can do things like that. And people are using these neural networks now to do stuff like that. But this was in 1990 with analog stuff. So, it was a whole new paradigm and a difficult problem. And she pulled it off. And it just blew everybody away! To this day I don't think there's been anything that significant done in that medium. You can do a lot more because the transistors are smaller and all that. But I don't think, as a system concept, there's been anything better than that as a demonstration of the art form that we were thinking about. Unfortunately, Misha isn't with us anymore, but it was an amazing time. The group that was there during that period was an amazing group of people. And Misha ended up being the star, but all kinds of good things were going on. The cochleas were doing interesting stuff. And then there were these various examples of systems that did other things that were all very interesting and ended up in theses…and these people went off and started groups all over the world that are still going gangbusters. The journal *Nature Electronics*, you probably know it, wanted me to write a little blurb about that time and it just came out. I think last weekend. It is a nice little thing. It has a picture of Misha testing her stereo matching chip. And I talked a little bit about what the future might look like there.

Zierler: And what did you see from that vantage point? And what the future might look like?

Mead: Well, meanwhile what's happened is that I have colleagues like Yasser Abu-Mostafa and Terry Sejnowski who have become leading people in what are now called deep learning networks. And so, they went barreling ahead, figuring out what you could do with Moore's law—-if you had a big enough computer and enough of a data base that was significant stuff in it that wasn't obvious how to find meaning in it—-that you could use a neural network architecture as hacked up in a digital computer and make remarkable progress. And so, the industry being what it is, has made available a huge amount of computing for basically free, and the result has been people training up these networks to do absolutely amazing things. And the only problem with it is it takes a computing center—-gigawatts of energy and a river to cool it —-in order to train these things. And so, it's clear, and it was clear to us back then, that the nervous system has a very particular way that it organizes its information. And that building a network that could be trained in this analog way would be vastly more efficient by many orders of magnitude. But, of course, it requires very special analog learning hardware—-we even had a start on trying to make it. I built some chips that would do part of the learning and would look at inputs and discern patterns and inputs, but it was rudimentary. But it's clear that the way the brain of a fly works—-it takes a milliwatt, roughly, to run. And can do a lot of this stuff in real time, relatively effortlessly. And these big systems take gigawatts and lots of time to train. And so, there's a huge opportunity there. And as far as I know, the only guy that's seriously looking at that in this way is Kwabena Boahen at Stanford. You probably run into his stuff.

Zierler: Sure. Sure.

Mead: Great guy. Very knowledgeable. Very perceptive—-he gets the potential of this thing. And I've been amazed at the tenacity he has to take on a thing that big and just keep at it. And he's doing that. And then there are a bunch of other groups around the world that are doing pieces of it. Like there's this group in Switzerland that have kept at the silicon retina business. And they finally convinced the industry that it's the best thing if you want information fast and low power. It was really things like self-driving vehicles that created the need for that. See, what happens in the retina is that instead of scanning out an image, which takes a 60th of a second or so, and then scanning out the next image, and then trying to match them out and see if something changed, that's not anywhere near fast enough to respond in real time. Especially if they're a number of things happening, you know? And doing it on the image plane, like the retina does, is vastly more effective. So, people finally, after these two former students went over and they now have faculty positions at the university there in Zurich. And they just stayed at it and they finally got the industry interested and now you can actually buy from some of the biggies these things—I think they call them "active vision sensors." So, adoption is a lot slower than it was with the VLSI stuff. Because it's a much bigger step, conceptually. And there had to be a need for it, which the real time mobile platforms provided…you really want it to be low power. You really want it to be fast. And you also want big dynamic range. And that's easy if you do it on the image plane, but it's not so easy if you try to do it later. So, they've been able to do all that. And now it's stuff that you can buy off the shelf from people like Samsung. So, interesting.

Zierler: Carver, where does the term neuromorphic come from? Where did you come up with that term?

Mead: That's a great question. [pause] I don't know the answer to that. But, the first paper I wrote, that I remember using the term, was in 1990. I wrote a review paper which was backward looking for *Proceedings of the IEEE*. It was kind of a snapshot of how far we'd progressed in 1990. And how we were thinking about things. I used that term in the title. But I'm sure we were kicking it around before then. I mean, it's an obvious term. But, you know that was a long time ago. So exactly when it hatched as a term the sands of time have probably covered. There may be a leftover. It might even be in that book, but I'm not sure. The name of the book was *Analog VLSI and Neural Systems*, so obviously it didn't have anything neuromorphic in the title. But there may have been references in there. I don't know.

Zierler: When did you start working with Arnold Beckman?

Mead: Oh…Arnold [pause]…that's a great question. Arnold was chairman of our board of trustees way back when. I think it was in the late ‘60s or late ‘70s. But I'm fuzzy about that. And somehow I got to know Arnold [pause]. Well, I remember one time very specifically. I had this idea for a chip that would make this self-contained temperature sensor that you could hook up to a temperature probe. And it would read out the temperature on digits. It was an extension of the first chip that I ever did, which I think I told you about. There's a little movie on the website about my first chip. So, of course, I got to thinking about what would be neat things to do with it and one of them was a self-contained temperature meter. Which would just be really neat. And I thought, well, one of the applications for that would be an industrial temperature meter. I'm thinking about what other things are like that. I thought of Beckman's pH meter because Beckman, you know, he was…Beckman instruments, and his first big product was the pH meter. And this isn't so different. Maybe his company would be interested in doing something like that. So, I scheduled a meeting with him. And he was very cordial and listened to what I had to say and I'd had this experience with the little clock I had built on a single chip at the time. And he said, "Well, that's an interesting idea. Why don't I introduce you to some people in the company?" So, he introduced me to one of his people and I showed him stuff about the clock and told him how that would make a really neat temperature reader. And they went off and thought about it and came back and said, "I don't think we can take this on." So, it didn't work out, but somehow I think Arnold saw some spark there. That worked into an ongoing relationship. I would have lunch with Arnold once in a while. He just liked, I think, the energy. And then, if there was a funding opportunity, he would always put in a good word for me. So, I would get these grants from places that I wasn't sure where they came from, but they'd materialize out of thin air. They were sometimes industrial places that would say, "Hey, we're interested in your stuff and would you like a little grant?" That's *really* nice. Well, where'd that come from? Well, it's probably Arnold who said something good about me.

Mead: I'll be right back.

Zierler: Okay.

Mead: Hi David.

Zierler: Hello. So, Carver I want to ask with your work with Beckman, generally. In what ways did venture capital prove to be a more nimble or exciting source of potential funding for your research rather than the more standard grant application process with an NSF or a DOE?

Mead: That's a direct segue from the Arnold Beckman thing.

Zierler: Good.

Mead: It turned out that when we started the neuromorphic stuff, we had a little bit of funding left from the ONR. It wasn't for that kind of stuff, although they were open to that being a thing, but at a level that wasn't going to support my research group. So, somewhere in there, and I think it was very early in the neuromorphic era—-it would have been around the early ‘80s that the System Development Corporation, which was the big computing company down in…there's an area of Los Angeles that's heavy on military oriented electronic stuff. And has a bunch of companies like Hughes and Northrop Grumman and TRW and people that basically do stuff for the DOD. And System Development Corp was a nonprofit company that was basically there to support the DOD. I think that's what it was. I think they were mostly a software outfit. I'm fuzzy on this because I never knew them. But I'd heard of them because I was around. I told you some of those stories. And at some point, there was somebody, one of the DOD companies, decided they wanted to buy this System Development Corp because they had technology that the other company could use. And I don't remember which company it was. But they made an offer for a certain amount of money for this company to become part of their private company. Well, what do you do when you're a nonprofit that gets bought for a handsome sum of money by a for-profit company? What do you do with the money? It's a nontrivial issue because you're a nonprofit company. What do you do? So, it turned out the way that got resolved, and I have no idea what the machinery was that resolved it, was that they set up a foundation: System Development Foundation, that had the money. And that foundation could use it to support research. And that was the way they solved this issue of this pot of money that couldn't be given to shareholders cause there weren't any. [laugh] And of course, when they made this System Development Foundation, it had to have a board of directors. So, they picked people who were knowledgeable and had wisdom and so forth. And Arnold was one of the people. And he was aware of the stuff I was doing and I loved having lunch with him and hearing stories. Arnold was the first guy to start a tech company in Germany after the war. And basically, he got them started on the path to being a free enterprise powerhouse. And he is deeply loved by the German people because it was not an easy time for them. So, he just made sure that my group was one of the things they supported. And I'm sure just very gently hinted that they should talk to me. And I got to be not only supported by them but also kind of an adviser to them about stuff. And so, they had a whole program for the support of neural networks, generally. And I was the neuromorphic one of those. And so that actually kept me going…there was no way I could've kept this research group going if it hadn't been supported that way. Because the ONR and agencies like that were getting to where the size of their grants were shrinking because they had to satisfy more and more of the "well, we're all equal" kind of things. And so, this System Development Foundation didn't look at it that way. They looked at it like, we want to find the outstanding innovation that's going on and fund that. And as part of that, I got to be part of the neural network group in a larger way. This was very early on. So, I got to know all the people that were doing back propagation and all that stuff. That was '85. So, by then I was there when that was happening and I was giving talks at the same meetings that those guys were giving talks. So, I was very aware of all the stuff that was going on with the guys that were making network simulators on computers. And, of course, we were doing a very different thing, but there was enough overlap that there was appreciation both ways. And it was great being part of that because, well, you know what it's turned into today.

Zierler: Right.

Mead: And being there as part of it when it happened was just a very thrilling thing. So, that was all due to Arnold. The fact that the System Development Foundation would fund me through that period. And, I'm fuzzy now, but that went into the ‘90s and so I was able to keep this fantastic group going without having to get 10 little grants from the agencies and squabble over this and write all these things. And I was getting toward the end of my attention span—-I was beginning to feel about being a full-time faculty, a little like I had felt about being in the VLSI business 15 years before. I had done that a long time. And I was spending an awful lot of time just tending the problems of students and putting up with bureaucratic nightmares that do come up in every organization. And then, when we did have some support from some of these agencies, they would end up taking away more time than it was worth in terms of the money I was getting. And I'd just had it with that. And I wanted to have some time to think. And not just from this group—-the best group I ever had. And so, it was around '95, I think. Or '94. Somewhere there. I think the money was running out. And I remember…we had group meeting every Monday night and we'd bring in dinner and have talks from the students and sometimes a visitor. It was a great little event that would happen every week and I remember one of them I said, "You are welcome to stay here as a student as long as you want. But, I have funding for one more year." And everybody gasped…but they all finished very well. They had all been writing papers that were excellent. They were all at a position to write a thesis even then. It was just such an exciting time that they didn't want to leave. I didn't really either, but it was just too much. It was just like I felt in 1980. I was like, "I have to go off." And so, that was the way…and maybe we should stop with that.

Zierler: That sounds great. Okay.

[End of recording]

DAVID Zierler: Okay, this is David Zierler, oral historian for the American Institute of Physics. It is August 2nd, 2020. I'm so happy to be back here with Professor Carver Mead. Carver, thank you so much for joining me again for round five of our talk.

CARVER Mead: It's great to be here, David.

Zierler: Okay, so to pick up on where we were from last time, it was a rather dramatic scene that you had set where you had told your students, you had gathered them in the room and you told them you believe in them, you care about them, you want to see their success. But unfortunately, funding runs out in a year. What's the reaction that you get from there? And what do you see as the opportunity to salvage this operation?

Mead: It wasn't really a salvage thing. The students had all gotten to the point where they had completed numerous papers and had them published. There wasn't a one of them that didn't have enough stuff for a thesis. But it was so much fun and they were having such a great time. It became a social group. It became a mutual help group. It was just a wonderful experience for all of us, personally. It was the greatest family I ever had. There were 15 students who were all like my siblings. And they were just wonderful to work with. We all had figured out how to work together. There was a student there by the name of Telle Whitney who later became the CEO of the Anita Borg Institute for Women and Technology. You've probably heard of her. She was in the group. I delegated to her, kind of keeping the group running. Like if we had a visitor, she would figure out how to get the visitor to visit with each of the students. Everyone was contributing to making the group work as well as contributing technically. It was a fantastic time. Incredibly talented students. All past the friction part and working as a group, like a family. It was just an incredible time. Nobody wanted to leave. [laugh] Meanwhile, I was getting interested in getting back to the physics. I had kind of done what I could do in the biological approach to computing using silicon as a substrate for building models of nervous systems.

Zierler: Now Carver, is this to say, were you approaching both theoretical and experimental limits at this point?

Mead: The theorists had gone off and were working with the people doing simulations. And they have lots of theorems and all that stuff. But they weren't paying attention to the problems we were paying attention to, which was what are the limits to really making these things with physical material? Yes, there was a little theory, but not a lot. There was a lot of trying to figure out what the biology meant. And then how could we incorporate our insights there into how to make these analog models of neural systems work better. Yes, there were a few people doing theory. That's right. That was in '94 or '95, so the timing was a little off. But, that's right, there were some. And some of our people actually delved into some of that. I didn't believe any of the theory enough to put time in it. But, it was a glorious time. I mean, it was a fantastic group. Fantastic. We had funding from the System Development Foundation, as I told you. So, I wasn't fighting with the agencies all the time. You know, clawing at them for money and the sort of thing that drives you nuts when you have a group in an academic place. So, that was just a marvelous time. There was no downside to it. These people were all going to go out and get their own groups in different universities and get into industry and do great stuff. But it was one of those things that…instead of just keep doing what they're doing, it gave them a view of, "Hey. Now I have to figure out my next step in life." And, of course, they all talked about that among themselves. They're a worldwide group that has hung together. Shih-Chii Liu is one of the students. She has her own institute now in ETH Zürich. She's kind of kept the group together worldwide. They edit books and everybody contributes just like when we wrote the *Analog VLSI* book and they have this Telluride conference which is coming up on the 3rd, which is a day or two from now, maybe? Yes, tomorrow, I guess. And that's a workshop that was started clear back then by…I think Terry Sejnowski from the Salk Institute who had spent time with John Allman at Caltech. He was a bridge to the Salk, and he's an editor of *Neural Computation,* which became the leading journal in the field. He was at Caltech a fair bit and in touch with all of us a lot. He will be there tomorrow, virtually, we're all going to be virtually there at the Telluride workshop, which became the workshop every year…they'd take over a space there and they worked together on projects. Hardware and software projects and combinations of them. And they still have that. They're going to have virtual groups working on integrating their stuff. So, it's going to be very interesting to see it happen now. It was exciting having it in Telluride because they have the best Fourth of July fireworks in the world.

Zierler: [laugh]

Mead: This huge field in the middle of the valley. Beautiful valley there, Telluride. And we would all go out there about 10 o'clock at night when it got dark. And we would lie on our backs and they shoot the fireworks up, so they went off right over our heads. [laugh] So, that became a way that people who had been in these groups have stayed part of the community. And Shih-Chii has really been the leading person that's brought all that together and keeps it cooperative. So that was kind of the spirit of the group. And the connection with Terry was extremely important because he knows everyone in the field, being the editor of the foremost journal in the field. And a fantastic guy. He's one of these people that has the property that every person…he knows exactly the most positive thing about them. No matter what anybody else says, he'll say, "Oh yes. That's the person that does X." And it's always a fantastic, positive thing. [laugh] So, what a great asset for a field to have someone that thinks about everyone in terms of the pinnacle of their contribution. It just is great. So, we were fortunate to have not just people on campus. But there was this thing called the Helmholtz Club. And it was started by Terry and…[pause] Francis Crick and [pause] I'm not going to get it. One other guy from…it was either Salk or UC San Diego who was a more medically oriented guy. They had this thing where they said Helmholtz was a guy who delved in biology, he delved in electronics, he delved into physics. He was an all-round polymath and he thought about things, not just narrowly like in one field, but across fields. And so, what we had was a very specifically, cross disciplinary group that would meet every month at UC Irvine, which was chosen specifically because it was halfway between the groups down in San Diego and the groups in the Los Angeles area. So, we had people from both ends and some people from UC Irvine that all got together. It started at lunch and everybody got reacquainted and then we would have a talk for a couple hours. And then a break. And lots of discussion. And then another talk for another two hours. Then we'd all go off to dinner and argue about it until all hours. And it was a fantastic way…each of us who are members could bring one guest. So, we'd bring the person in our group, typically, who was the closest to the topic of that particular meeting. It was just amazing cross fertilization across fields that you'd never think could speak the same language. So, this was the milieu, why nobody wanted to leave. Because it became a way of life which was already integrated with people all over the world. The Helmholtz Club—-they would invite somebody that they knew did really, super neat, typically provocative kind of work. Then they'd have a local person that gave the second talk, which did something very closely related. And so, you had two points of view on a topic that was typically quite appropriate for cross disciplinary discussion. It was a marvelous, marvelous time. But, they would go out, they would continue to be part of that community. That community is still vibrant. I'm looking forward to the thing tomorrow. They have a prize for the best piece of neuromorphic work that was done in the past year. And it's named after Misha Mahowald, the lady I told you about that did the binary vision thesis. So, of course, I'll be there with them tomorrow. Virtually.

Zierler: Carver, it might be a difficult or painful topic to talk about, and if you don't want to I'll certainly understand. But, I'm curious, did you know if Misha was not well when you were advising her?

Mead: Misha was a very complicated person. She was one of these savants who could see things way beyond her age.

Zierler: Yes.

Mead: And she could see about people. She could see about technical things. And she could just grok it. And from the first time I met her, you could just see that her way of grasping things was way beyond her level of education. That, of course, made it difficult for her just because she could see things that she assumed everybody could see. And most people couldn't see at all.

Zierler: Yes.

Mead: And so, it was difficult for her. She started being a member of the group I think when she was a sophomore. And she was way more perceptive about a lot of things than the most advanced people around. She hung out with John Allman's group and other groups in biology, as well as in our group. It was quite astounding…the things she would come up with. But, it was hard on her. She also dabbled in drugs and insisted that it was liberating for the mind and all of that. I had lost a student to drugs earlier in my career. [pause] It's wrenching. Because if you try to convince them that the drugs aren't good for them, they just stop talking about it. And if you just listen, you wish there was something you could do to intervene somehow. It was very difficult for me to know what to do. In both of those cases. So, I chose to basically just listen. And not to try to convince them of what choices to make. It may have been the wrong…that may have been the wrong thing. And Misha had a very complicated relationship with the drug thing and how that affected personal relationships and so it was complicated and…John Allman also knew a lot about what was going on. I would talk with him because neither one of us knew what to do because…you just don't. So, she also was involved with groups that had various beliefs of various sorts. And we think that there were drugs involved in those too. It was difficult for her. But also, the drugs were there and they were always part of the picture. So, she got the big prize for thesis work and she got a nice faculty position in Great Britain with Rodney Douglas's group there. And it was Rodney and a biologist, whose name was Kevin…I'm not going to be able to come up with right now. So, she joined that group and it was a group that very much appreciated what she was doing. It was during that time that BBC made a wonderful movie about Misha. I don't know if you've seen that.

Zierler: No.

Mead: But it was documentary. And it was done in a wonderful British style. And we thought, you know, that when she got settled into a faculty position and was doing well and had a group around her that was appreciative of what she was doing, that maybe things would get better. And it seemed like maybe there were…I was of course in Pasadena, not in Britain, so I only had contact with her intermittently. But, she was in touch with people that were former members of the group. So, I would hear once in a while what was going on. And the group there in Oxford was…I got the feeling that they were…well, it wasn't a traditional thing they were doing. And I got the feeling that that wasn't particularly what Oxford wanted. And, I got to talk to Rodney once in a while. Rodney's a great guy. He's now retired from Zurich. Well, let me get to there. The whole group got recruited to start a kind of activity, the neuromorphic thing, at ETH Zürich. And of course, what comes with that is basically assured funding, which is a huge thing. They were struggling with the finances there in Oxford. So, they decided the whole group would move to Zürich. And Misha called me and wanted to talk about it and I told her I thought it was a wonderful opportunity because they would have assured funding and it's an excellent place. And the group would thrive. What I didn't anticipate, of course, was the stress associated with a foreign culture. So, they moved over there. And it seemed like they were doing well. I think it was '95. No, maybe '96, that my wife and I went over to visit Misha. They had invited me to come and give a talk at their seminar and visit with the people. So, Barbara and I went over and we hung out with the group there and it seemed like they were going great. Misha was having frustration with the fact that this address event signaling that had grown up in the group…just before the group went to the four winds. We haven't talked about it, but there's a real problem when you make things like retina chips and then you want to hook them together to make a binocular vision chip. How do you hook them together? The visual system uses about a million nerve fibers to do that. The optic nerve. We can't have a million wires. No way to do that. So, there has to be some way of transmitting that much information. The wires are much slower in a visual system, but there's a lot of them.

So, that means that there needs to be an electronic equivalent of something like an optic nerve. And that became the urgent felt need. A group of people all got together and there were a number of people who made really seminal investments in getting that going. It was a not trivial problem at all. Because it's an event driven system of signaling. And that means it's an inherently self-timed system. There's no global clock or anything. And that's a difficult electronic thing. And it involves nerve pulses which are when you don't have as many wires as the nervous system does, you need to encode the information somehow. So, each nerve pulse becomes a digital string that tells about the source, where in the visual field, and what happened. So, that a limited number of wires could carry all the information. The time of the event is real time. So, it had the properties of the nervous system, of using time as its own representation. It isn't an address in the memory. Time is real time—-marching on. And so, just from a circuit design point of view, this was a difficult problem. And there was a group at Caltech that had been doing self-timed digital systems. Alain Martin at Caltech was one of the leading people in the world that was trying to make that kind of system, and define a discipline of how you design that sort of thing and make it work. Which is a totally non trivial thing. People still have trouble with it. Well, Misha had not been part of that whole world. And so, when she got to Zürich what was required if you wanted your stuff to work with other people's stuff, was that you were able to make it work with this address event thing. And when my wife and I got there, Misha was struggling terribly with that…and I think she had some expectation that I might be able to help. And I spent a day with the people…she had a couple of really good students working on it. It was more than I could grok without spending at least a month to try to catch up with what they were doing and what the system was they were using and all that. So, I was unable to figure out what the bugs were that they were chasing. And I think that was a disappointment. Well, I know it was a disappointment for Misha.

Zierler: Carver, I wonder as her adviser…many people…if you were to say to a young, promising student, "You're going to accomplish great things in your career." For people who aren't struggling, that's inspirational. But, perhaps, for somebody like Misha, that would've been a burden. It would've been too much to bear.

Mead: I don't think that was what was hard for her…I think she had…her vision was already beyond what I was seeing in many cases. So, I don't think that was what was difficult… For example, when she chose to do the binary stereopsis problem, I thought it was going to be a hugely difficult problem. And I didn't try to dissuade her from it because I couldn't see how to do it. And it turned out the solution she came up with was incredibly brilliant. It was something I had not come close to. And as far as I know, nobody's come close to anything like that. So, I think her vision was already past where I was. I was just trying to be supportive. But, I'm sure I made a lot of mistakes because I couldn't tell what was going on in her head. I think the same thing that gave her this incredible vision of technical things…also she could see things in the world and things in the group and things in the field that none of the rest of us could see. So, I don't think that was one of the worst blunders I did. Sure, there were lots of blunders because…Yes, being human is a terrible limitation.

Zierler: [laugh]

Mead: Anyway. So the last time I saw Misha, she had come to the U.S. to go to one of these personal development workshop kind of things. And it was up in the Bay area. So, it turned out, she came by Caltech first. And got to see everybody and then I flew with her up from Caltech to San Jose. And she was going to go from there to this workshop. And she told me a little bit about it. And it was [pause] well, it was one of these self-help, self-development things. And she seemed very up for it. She seemed to feel it was going to expand her horizons and she seemed pretty up. So, I felt like, maybe this is a good thing for her. So, the next thing that happened, [pause] by then my wife and I had had a log cabin up in Los Gatos Hills, and then we built a house over in Woodside…up looking out over the Stanford campus. That we loved. We had either just moved there or we were just about to. I'm not getting the timing very well. But, I think by then we had moved to Woodside. And so, Barb came and picked me up at the airport and said hi to Misha. She went on her way. And the next time I heard I got a phone call from Rodney. And he said, "Misha is psychotic in a mental institute in Marin county." You go north of San Francisco, across the Golden Gate Bridge and up. It's one of those towns up in there. There was this mental hospital and Misha was psychotic, in the mental hospital. Something had happened. Rodney said that Tobi Delbruck was headed up to the hospital—-he was a close friend of Misha 'til she died, actually. He was going to come and watch over her. Meanwhile, I had a class I had to get back for. But, I went up there and spent a few hours with Misha and while I was there the doctor came in. So, after he had spent time checking her out, he came out and I had a talk with him. And he thought that probably there were drugs involved in her situation. But he couldn't tell anything more specific than that. So, that's the last time I saw her. And she went back to Switzerland, and that would've been in the fall of, I guess '96. And she went back to Zürich. And invited her friend, David Feinstein, who had been a longtime friend. In fact, David was the one who got her involved in the group. They were close friends and he got her involved in the group, clear back in '82, I think. And so, David was there and Misha went out and put herself under a moving train.

Zierler: Carver, what kind of resources did Caltech offer faculty members who were dealing with students who had mental health issues? Did you feel like there were places that you could turn?

Mead: No. I didn't feel like there was any such thing. I think they had a psychiatrist or something. But this wasn't that kind of issue. It was a much deeper and broader issue. I think Misha was searching. She had a connection with the religion there at Caltech. There was a religious person. Official religious person of that belief that was there. And she had access to him. And spent a lot of time with him, I know. So, I think to the end of her life she was searching for what…how that whole thing could come together for her. And I think it never did. I think that workshop was part of that search. The contact with the religious representative there on campus was part of that search. The discussions with me and with John Allman and with many other people were part of that search. And I think she was never able to find a place in there which worked for her. And that's a space which is hard for many people. And it was especially hard for her because she could see so much further than others. And, I don't think a psychiatrist would have helped. I think what she really needed was someone that could see much further to, sort of, and I don't think any of us were able to see as far as she could.

Zierler: Now, your subsequent work on an adaptive silicon retina, this of course built on what she accomplished. Her research.

Mead: The two of us together did the first one. And I think it was the first chip she had ever done of any kind. And she continued to do them. And other students started doing them. So, all of us were doing versions of that and trying to add functionality. Shih-Chii ended up doing some wonderful work and continues to do wonderful work on early vision. So, Misha just started basically the whole group on that path and all of us, basically, were adding our understanding to what she'd been able to accomplish. So Yes, she started the whole thing and was active in it as long as she was there. And she was a good coach for the other students. She would share with them her insights and was an extraordinarily important visionary in the group. Yes, That still goes on. Tobi Delbruck just sent me, in the last week or two, a review article he had written about…silicon retinas are now called active vision sensors, and they're being now manufactured by leading companies and all that. And he wrote a wonderful review article that he just sent me. If you want to look at it I'll send it to you.

Zierler: Sure.

Mead: And he's the one that started the Misha Mahowald prize that's being awarded tomorrow.

Zierler: Oh, wow. Oh, that's so special.

Mead: He's married to Shih-Chii Liu. And they're both professors there at ETH in Zürich. They met when they were in my group, so I'm very pleased with them.

Zierler: [laugh] Now, Carver, was your research with Misha, did that inform what would later come to be the Foveon corporation?

Mead: Um. Actually, Synaptics started about '85. And yes. The silicon retina work and the silicon cochlea work were part of what the technology that we set out to try to find a market for. Misha had this silicon retina and she didn't have any way of displaying what it was seeing. So, there was a young fellow who was an undergrad, by the name of Tim Allen. And he wanted to do something in the group, because he liked what was going on. So, I said, "Help Misha get a display for her retina." So, he designed a little interface for Misha's chip to get it to display on an ordinary TV. And so, when he graduated, he went to work at Synaptics, and several of the other people in the group went to Synaptics. As a company, we looked at all possible things that we might do…that there would be a market for now instead of in 20 years. Actually, that was 35 years ago. And just now companies are doing silicon retinas. So that market hadn't developed…there wasn't anything even close back then. So, it turned out that Tim Allen came up with this way of doing a touch sensor, where you could touch the surface and it would figure out where you finger was. And he had a very clever design for that. So that became the first Synaptics product. And was the central product up ‘til maybe 10 years ago. So that's what happened there. Yes, it was all about is there something about this analog VLSI technology with how you interface computers with natural things. And it ended up that the first natural thing we could interface was touch. None of us knew that when we started the company. But that became a big success. And they're still doing well. That wasn't Foveon. That was Synaptics. That was well before Foveon.

Zierler: Foveon might have been later. It might have been 1999.

Mead: Yes, it was. And it was a spinout of Synaptics. Because by then, Tobi Delbruck, for his thesis had figured out how to use the optical absorption of Silicon as a function of wavelength. Silicon has an indirect bandgap, so optical absorption is a very smooth function of wavelength…it increases as the photon energy gets bigger because there are more states that you can excite from as you have higher energy photons. It's just a state-space thing. And because silicon has this indirect gap, which makes the absorption rise very slowly with photon energy. The reddest light is absorbed furthest into the material, and then it absorbs green closer to the surface, and finally it absorbs the blue stuff very close to the surface. And Tobi said, "Well, look. Suppose we just make junctions that are at different depths in the silicon, they will have different response to wavelengths and we could use that to make a color sensor." So, actually, he did a thesis on that. And then he came to work up at Synaptics. And sort of pushed this idea. We were already doing a sort of a visual sensor thing—-fiddling with the retina stuff. Just look. This is an important thing that 3-color optic sensor. And Tobi came up with that thing and Dick Lyon who joined Foveon when we started the company. For many years he spent a day a week down at Caltech with the group. He's at Google now—incredibly smart guy.

Zierler: Where did you meet him? Where did you meet Dick?

Mead: Oh, he was a freshman at Caltech. I was teaching a freshman course in digital design at the time. Clear back in the digital era. And Dick came and took it as a freshman. And then he came around and he wanted to hang out with the group. I always had a way of making deals with the undergrads. You know, did they want units? Did they want to be paid? What did they need? Want lab credits? Because there was, sort of a catch-all lab course: "projects in electrical engineering." A lot of them wanted that. So, whatever you want, we'll either pay you or we'll give you units for a course. Whatever works for your program. He was one of those that came in as a freshman and he was smarter than most of the grad students. So he started working with the grad students and was there when we first started the VLSI stuff. And he helped with that. As he was going through school, he was acting like a grad student ever since he was a freshman. Just an incredible guy. And he's always been close to the groups that I had there and always…acted like a kind of visiting faculty member. And he has been just an amazing contributor to all of the things that happened there. I thought maybe these curves of how many electrons you get for a photon of whatever wavelength. They had very broad peaks in them—-there were three of them because we had three junctions. We just had diffusions that were there in the standard process they were using. I thought they were too broad to be good color sensors, but Dick looked at them and he said, "Well, you know. They're not that different from the color sensors in the retina. So, we can make that work." So, when we decided to spin Foveon out, we convinced Dick to come work with us. He may have been at Apple at the time. I forget. But anyway, he came and became our chief scientist. And just figured a lot of stuff out. Just made things work. And really good with people also. Synaptics had some kind of deal with National Semiconductor and I don't remember what it was. But, there was a National Semiconductor guy on our board. So, the deal was that National would provide the fab for this new company, Foveon. And they would provide a couple of key people. And the key person was the fab guy. And his name was Dick Merrill. And he was an incredibly insightful fab guy. Just amazing. The combination of Dick Merrill and Dick Lyon was just incredible. And they really made this three-color sensor work. First silicon. And nobody could believe it because there were no color filters on it. There was just the silicon. And it made these beautiful color pictures. And of course, there's a long road from there to a production sensor. And there's a long story there if you want to hear it sometime. But, it was another great technical group. It was as exciting as the one I had left down at Caltech. And ended up with the best color images that had ever been captured by the human race. I don't know if that's still true, but it was true then.

Zierler: Carver, how do you measure such a thing? How do you know just how good they are?

Mead: Oh. I had a very particular measure. There are people who have what we call a golden ear, that can hear in sound things that none of the rest of us can hear. There are photographers that have a golden eye. That can look at an image and zoom in on it and work with it and tell you all the things that are wrong with it. And the whole Foveon history takes a long time to tell, but we had as sort of, consultants to the company, a few of those photographers. I remember the day. It was our second 3-color design, that had been tuned up a fair bit. And we had a studio, of course. And we're taking pictures. So, we let the guy take some pictures of a model. And he was working with them on the computer, just the way he works with images. And he said, "I have never seen an image that was this good. Not from film. Not from any of the color filters." He said, "This is smooth. It's smooth in a way that no other image I've ever seen was." And so, we got this group of these people who could really see the quality in an image and they fell in love with it. And as far as I know there's still this group that won't take images with anything else. Tragically, Dick died…but his widow has gone on to provide just the most amazing images. She puts on exhibits of these amazing color images. See, the problem with the standard color filter image is that they typically have color filters on the image. They start with black and white pixels. And then they take groups of four and they put two greens, a blue, and a red. Like that. Called a Bayer pattern. And they just repeat that over the whole image. Well, that means that the position and the color are inextricably confused. In the raw data you get. And they do all kinds of things. They put a fuzzy filter in front of it. They try to process the thing to smooth it out. But you can't really get rid of it. So what Dick Lyon did was, when we'd go to one of the trade shows…he made a bunch of t-shirts which had a big spiral on them. Which had all the different color combinations with sharp edges between them. And of course, all of the other imagers would just alias the heck out of those, and at all angles so that you couldn't fake it. All the companies at the trade shows would want to take your picture to show off their cameras. So they would take your picture and here are all these alias artifacts. [laugh] And finally, they got to where they wouldn't take pictures of our people in those t-shirts because they couldn't make them look good. Foveon is now a part of Sigma. Sigma was the first real customer we had and it was at…you know there's this huge trade show called Photokina. I don't know if you've heard of it.

Zierler: No.

Mead: It's the largest show associated with photography and imaging and printing in the world. And it's at a thing called the Koeln-messe. Messe is…you know German? Okay, it's a place you have big meetings. Convention center is as close as we get. It's the biggest one in the world and it's where everybody goes to see the latest stuff. And so, everybody's out showing their best stuff there. And it's the most exhausting two weeks I ever had in my entire life. My wife Barbara came with me.

Mead: Our marketing guy had set me up with the founder and president of Sigma. And I don't know if you know Sigma.

Zierler: No.

Mead: Sigma has for many years been the leading provider of third-party lenses for single-lens reflex cameras. They have a series that fits the Canon camera line and a series that fits the Nikon camera line. And I had had one of the Sigma lenses on my Nikon camera because they made better zoom lenses than you could get from Nikon. And a fair bit cheaper. So, I was in love with their zoom lenses because they had bigger zoom and focus stayed good over a wider zoom. So, if you're going to carry one lens, that's the way you do it. And they had very early on taken a license to the ultrasonic focus technology. You know how the ultrasonic drives work on telephoto lenses? He had taken a license to that, so that Sigma lenses had this silent and very precise way of self-focusing. And so, they had really perfected the lens art. Their lenses were wonderful. I mean, just fantastic. So, I knew that. And our marketing guy had found out that they were going to announce a film camera with their own brand. They just made these third party lenses prior to that. But the year we were at Photokina, they were going to announce these cameras. And I said, this is made in heaven because these guys aren't big enough to have their own fab. So, we could make a digital imaging system that would fit in their new camera body. And it would be a win-win deal for both companies. So, their founder and president was Mr. Yamaki and my marketing guy had made a half hour appointment with Mr. Yamaki. And I went in to see Mr. Yamaki, a very quiet, Japanese fellow who shook hands. He showed me their camera. It was beautiful. Just like their lenses. I mean the finish on the Japanese camera…you know, it's just astounding. And then I showed him some of our images and I said, "We can do the digital imaging part for your camera." And I could see the wheels turning. We talked for three hours. He became a close, personal friend. He's my age, Japanese, lived through the war on the other side of the war. He came to our home. We could talk about World War II. He was one of the most visionary people I've ever met. When he talked about World War II, he talked about the whole picture of the globe. And what people were doing and the mistakes they made, as if he was above the whole thing and just looking down on it. And wasn't part of it. Extraordinarily rare. Amazing person. So, we figured it would take a year for us to get our imager working in their camera. We called our technology X3, for the fact that it had the three sensors, one behind the other, instead of spaced out on the image plane. And just different depths. So, we had an X3 system working really quite well already. We could demonstrate the images, but that's different from having it work in a camera in real time and all that. So, we had one. We developed it. And he developed a camera. And by a year it wasn't close on either side. The guys at Foveon were frustrated as heck trying to work with a stodgy old company in Japan, which had totally different culture. Never worked with a Silicon Valley startup. And we had never worked with any company with real products. Different members of our group had all worked with companies that shipped stuff. But that's different from a camera. [laugh] We'd all used cameras, of course, but we had never developed one! And so, Mr. Yamaki and I would talk about it. And his people were getting frustrated as well…they wanted to cancel the thing. They said it's never going to work. Our guys were saying, "We can't get anything out of these guys. They won't do this. They won't do that." So, I talked to Mr. Yamaki and I'd say, "We can get this done. If you just make sure that step by step, your guys are willing to understand that we're a startup and our guys act differently. So, not to take it personally." Then he said, "Yes. I have the same problem, but it's cause my guys are used to doing a thing that we've done before, better. And we haven't done this before and they're very reluctant to take a step." Japanese wanted to know exactly what the result's going to be. And we don't know. We just take the step and figure it out. So, it was alien to both cultures. What the other one was doing.

Zierler: Was the partnership worth it in the end?

Mead: Well, I'll get there. So, I would sit with my guys and I would talk to them about the Japanese culture. And how those people think, what they're used to doing. And how big a step it is for them. And how we have to go the extra mile and just make sure that we do a lot of the stuff that we might imagine they would do. And he was doing the same thing with his people. And we were both right at the edge of mutiny for about a year. But by the end of the second year we had a product out. It was called the SD9. And it was a camera. It worked. It had what now looks like a lot of defects. But it still made beautiful images. And he called me and he said, "Carver, we have crossed the digital divide." He could see. That's where photography was going. And if they didn't go there, they would be in a dwindling market after that. So, that led to a series of more and more capable cameras and tighter and tighter integration of the sensor in with the function of the camera. And just before this COVID time, one of the Foveon guys I was the closest to sent me an email and said that the whole operation had been moved to Japan. So, it's now part of Sigma, and it lives on. And it was a fantastic thing. And Mr. Yamaki lived to see it. And the last I heard from Mr. Yamaki, I received an SD1 with a fantastic lens on it. And then I heard that Mr. Yamaki had died. So that was his going away present to me. [sigh]

Zierler: So, that answers the question. Of course this partnership was worth it.

Mead: Fantastic. [choking up]

Zierler: And each side adapted to work with the other side, it sounds like.

Mead: Not work well. They got through it.

Zierler: Yes.

Mead: And then it became more and more…it took 10 years to get to where they were smoothly working together. But, we crossed the digital divide.

Zierler: What have you learned about the Japanese way of approaching technology and work culture?

Mead: Well, just what I said. My brother in law, Wayne, Barb's older brother, was the guy that built the…there's a big theme park down in Florida, what is it?

Zierler: Disney World?

Mead: No, the other one.

Zierler: Uhh…Universal Studios?

Mead: I think it was that one. Anyway, the biggest theme park in the biggest city in northern Japan. He personally oversaw and ran the building of that theme park!

Zierler: [laugh]

Mead: He built it in that city with Japanese people. And I don't think there were very many people who could've done that. He's one of these people who just works it. Amazing guy. We've become close. We're like brothers. [laugh] Even though we're brothers-in-law. And he said it the best, I know. He said, "The Japanese want to do everything up front so they know what the outcome's going to be and then they can do it." And with that kind of a project or with our kind of project, you don't know every step. You have to figure it out as you go. And they're excruciatingly uncomfortable with that. So, they're fantastic at doing things where you can do one step at a time and you can see ahead of time how much better it's going to be. And that's why they're so good at making microchips where the process just gets better and just gets better and just gets better. If it's a smoothly evolutionary thing; they're probably best in the world doing it. But when it's got uncertainties and you have to work it as you go, it's just beyond their culture. Excruciatingly. They can't even comprehend doing that.

Zierler: Yes. And that's where some swashbuckling Americans can come in and help.

Mead: Well, it means that as an American working with them, you have to show them how it's going to work. And assure them that it will be okay. And after a while they begin to sort of trust you. Even though they can't see it, you've sort of explained it and you've sort of delivered on part of it and they can see how by making it a little bit better that it can get there and so then they can pitch in. And it feels from the other side as if they're just being totally uncooperative. It's sort of like they're purposefully trying to stop the project. And it isn't that, it's that they can't imagine it. So, you have to help them imagine it. And then, of course, our guys say, hey, charge in and if there's a problem we'll fix it. [laugh] So, they're so alien. And you can understand if you just take the other viewpoint, you can understand how the other side feels just totally nuts. [laugh] And so, anyway, Wayne was able to get the project built and we were able to get the ST9 built. And each one after that was easier and much better. But…wow. What an experience. Meanwhile, I ended up with a very close friend from that other culture.

Zierler: Yes.

Mead: And I treasure him. Anyway, I think we're about there, aren't we? I think I'd better stop. This is a good place to stop.

Zierler: Okay.

Mead: You okay with that?

Zierler: Same time next week? I'm good with that.

Mead: Okay. We'll do it.

Zierler: Alright, Carver. I'll see you then. Thanks so much.

[End of recording]

DAVID Zierler: This is David Zierler, oral historian for the American Institute of Physics. It is August 9th, 2020. I'm so happy to be back with Professor Carver Mead. Carver, thank you so much for joining me today.

CARVER Mead: Good to be here, David.

Zierler: Now, we're going to be a little off narrative, but in order to take the story forward from neuromorphics, tell me about your experience with Linus Pauling as a freshman.

Mead: Pauling was I think the best teacher I ever had. And he taught chemistry. He had been with Bohr and Heisenberg and all those guys as a grad student, so he was right in the thick of it when all that quantum stuff was going down. And what he took away from that was a deep intuitive understanding of quantum things. So he started chemistry gently in the first quarter, but second quarter he jumped right into the quantum nature of the chemical bond, and it was just thrilling. I mean, this is why I went to Caltech was to get stuff like this.

Zierler: Right.

Mead: And so I spent far more time than I should've trying to understand that part. I've always had trouble with the academic way of doing things.

Zierler: Uh-huh. [laugh]

Mead: So I remember the night before the final exam in Pauling's class, my roommate came to me and he said, "Carver, I don't understand this quantum stuff at all. You have to help me." So I spent later into the night than I wanted to going through the whole quantum stuff with this roommate. So next day we had the exam and he got an A and I got a B.

Zierler: [laugh] That's not fair!

Mead: [laugh] Well, it's the way the system works. It's just the way the system worked. I'm not a good test taker, never have been.

Zierler: But, apparently, you're a very good teacher.

Mead: Well, it worked that way that time. But it was just a fantastic experience. And Pauling was an interesting guy. I could walk over to his office any time of the day and his door was open and I would poke my head in, and he'd say, "Come on in." And he would talk to me about whatever I wanted to talk about. Astounding! And I don't think very many of the freshmen took advantage of that, but, because I was really interested in the quantum stuff, I got way too carried away with it. But that gave me an intuitive sense of how quantum things worked from a guy who had been there and grew his chemical discipline out of his experience in Copenhagen.

Zierler: And, Carver, do you remember what course Linus was teaching at that point? I guess this was not a biology course.

Mead: No, it was Chem I.

Zierler: Uh-huh.

Mead: Chemistry I. Everybody had to take it. And I loved it. Most of the freshmen didn't care because they weren't chemists. But for me that was the high point of the whole year. So I was looking forward to the physics course that did quantum stuff, and it turned out Tommy Lauritsen taught it—we had two Lauritsens, a father and a son. Charlie Lauritsen was the father and Tommy was the son, and they had both been over with the Copenhagen clan when all that was happening, too, so they must've known Linus from then. But I didn't know any of this, of course, because in those days you couldn't look stuff up. It was really hard to find out any of this.

Zierler: Right.

Mead: So it turned out I had Tommy Lauritsen for the introductory course in quantum stuff from the physics department. Well, the earlier version of the physics course—I think it was sophomore physics; I forget—but it had been taught out of a book by Richtmyer and Kennard…an old, old classic introductory physics course. And then, Tommy Lauritsen had added a section onto the end, and so it became Richtmyer, Kennard, and Lauritsen. And so we learned it out of his book, and it was a good book. But then, when we got to the quantum stuff, it turned out Bob Leighton had just written what turned out to be an extremely influential book on introduction to quantum mechanics, and they decided they were going to use a preprint of that book for our textbook. Well, what it meant was that Bob Leighton had fallen in love with the Heisenberg matrix approach to quantum mechanics and we weren't going to get any physics, we were going to get a whole course in how to do matrix calculations the way Heisenberg did. So there was no physics in it. It was all just matrix math. And it drove me nuts because I knew there had to be some physics there someplace, but you couldn't find it in all of the chicken tracks. So I would go to Tommy and say, "I'm not getting this." They never told us about the Schrodinger wave mechanics and the fact that you could visualize wave functions and visualize how they worked. And then how there had been this fight between Heisenberg and Schrödinger, and Heisenberg had won, and so everybody was doing his matrix stuff now. But they didn't tell us any of that. So I found some stuff about wave mechanics and I had to go read that and go to Tommy and say, "Now, Tommy, what's wrong with this?" So I talked about if you had a wave function and it makes two states, then a wave function that was a mixture of those two states would move back and forth and that was a little electrical current and that would couple to an electromagnetic field. And I was an electrical engineer; I knew about those. So I remember when I went to Tommy with this story —which, by the way, I hadn't found in any book. Just by stuff I could get hold of I'd kind of figured out how it must work that way. Tommy smoked a pipe, so—

Zierler: Was he an old-school guy? Was he like from a different era?

Mead: Yes, sort of. And the new stuff was all kind of alien to him, but he had been told to teach it that way, and I think there were times when he would let on that maybe that's not the way he thought about it. So I told him that story about how I was thinking about it—"What's wrong with that, Tommy?" So he sat there puffing his pipe looking at the ceiling. And now I know. I mean, he'd been in the wars and he knew that there were these huge arguments and Schrödinger had gotten shot down by Bohr and Heisenberg. But I didn't know any of this. So I just couldn't understand what's going on here; this isn't physics. And he said, "Carver, I think what you should do is go look in Herzberg's little book on atomic structure and atomic spectra. OK?" So it turns out it was a paperback book even then. So I went and found one and, sure enough, I found a section where it had exactly what I'd been thinking. This is how atomic transition works. Why didn't they just teach us that and then it might've been worthwhile learning some matrix mechanics?

Zierler: [laugh]

Mead: But without that, there's no physics. So I was bitterly disappointed in that physics course. Tommy saved my life by talking to me on the side. And I realized the physicists had lost it. They basically had lost track of any intuitive connection of the way quantum systems work and they were doing this math because it worked. But when you talked to somebody—I would talk to anybody I could talk to—"There's this transition in the atom going from one state to the other that radiates energy;" "Oh, you can't ask about that." I said, "What do you mean you can't ask? I just did." "No. You can't ask that question." And then I began to realize that they wouldn't say to you that this matrix theory doesn't address that question. They wouldn't say it. They would say, "You're not allowed to ask that question." Well, no. It's not a problem with what I can ask; it's a problem with what you can answer.

Zierler: Mm-hmm, right.

Mead: But they weren't honest about it.

Zierler: Uh-huh.

Mead: So I gave up on the physicists.

Zierler: What do you think that was about, Carver? Why not be honest about it?

Mead: It's still true. Still true.

Zierler: What are the big takeaways from that observation?

Mead: Well, we've just written a paper about it. [laugh] It's just up on *ArXiv2006.11365* as of Friday.

Zierler: Is it about arrogance? Is it about intellectual blind spots? What do you see?

Mead: Well, now that I know more of the history, I realize what happened; that Schrödinger was on this track of trying to really understand what went on during a transition. And Heisenberg had said, hey, none of that's real because you can't measure it. All you can measure is the statistics of the outcomes, so we're going to work with measurables because those are real, and the other things are all just in somebody's imagination. So they're not real, so we won't deal with them. Well, it turned out that Schrödinger and Einstein didn't think that was a legitimate position and they pushed pretty hard on those guys.

Zierler: Right.

Mead: And there were big arguments. And it's unfortunate that the arguments aren't well documented. The only ones that people hear about is the Solvay conference where Einstein thought there was something wrong with the uncertainty principle and he thought he had nailed Bohr, and they had this big argument. And finally, Bohr won, and that's the only story that ever gets told.

Zierler: Right.

Mead: Well, turns out that the uncertainty principle is just a property of waves. It has nothing to do with quantum things, per se.

Zierler: Mm-hmm.

Mead: If you have things that are waves, like electrons and electromagnetic things, they have this property and it's just the Fourier transform. And there's nothing about it that's anything to do with probability per se. And Einstein had sensed that, but he picked the wrong thing to argue about. So the fact that he lost the argument was really that he wasn't comfortable really thinking of the electron as a wave. Schrödinger was, but he wasn't there for the argument, so Einstein lost [laugh]

Zierler: [laugh] What was he there for?

Mead: Well, for some reason, he wasn't the one that chose to have the debate with Bohr. Maybe he'd been beat up by Bohr before, because Bohr was a fantastic debater. He was a very forceful guy. So it turned out it was an ongoing argument until many years later. And there were parts of it that Einstein never did get. And it turns out that the way waves really work is not intuitive for most people. I was lucky because I got started with electricity when I was a kid and I got into ham radio when I was still in sixth grade, and so waves were a thing that were a natural part of my life for a very long time. And alternating currents and phase and all that stuff was an intuitive thing. Anyway, Einstein, what he was really upset about—and he says this—there's a place later on, it's in my little green book, there's a reference where it was some event. I think it may have been one of his birthdays or something, but Bohr and Heisenberg and some others took the opportunity to roast him for being old fashioned and not accepting their probabilistic quantum mechanics. And his position was not an argument about calculating probabilities—that's a very fine thing to do, but it can't be everything there is. There has to be a way to understand the individual event. And he says that in his—the quote is in my little green book. And that always rang true with me that, yes, it's fine to calculate probabilities and yes, they're what you measure in most experiments, but there's got to be a way to understand how it happens.

Zierler: Mm-hmm.

Mead: And that's what Einstein was about. He says it can't be just this instantaneous thing.

Zierler: And you identified with this intuitively, this approach?

Mead: Oh, absolutely. Well, it has to be.

Zierler: Well, you say it has to, but not everybody thinks like that, as you're demonstrating.

Mead: Well, yes, but take their own uncertainty principle; if it was instantaneous it would have infinite bandwidth. You don't have an infinite bandwidth in a light signal. There's a line width. It isn't infinite, so it can't be instantaneous. It's built into their own system, but they've got this contradiction that they live with because they don't want to admit that it isn't everything. Because what Einstein kept saying is it can't be the complete theory.

Zierler: Right.

Mead: There has to be more to it than that. He wasn't saying it wasn't a good theory for what it did. He said that many times; very useful, but there has to be more underneath it. Well, I think Bohr and Heisenberg were feeling very embattled by having Schrödinger and Einstein beating on them about this because they were very proud, rightly, of the theory they'd built. Of course, nobody told me this, that I'd stumbled into a beehive. [laugh] And there was all this history of argument that nobody was talking about. And it wasn't any Joe Shmoe, this was Einstein.

Zierler: Right, right. [laugh]

Mead: He had started the whole thing. And he figured out a lot of the important things about it. That's what he got his Nobel Prize for.

Zierler: Carver, when you first start encountering these debates that obviously far-preceded your time, did the debates feel as fresh and unresolved in your time as looking back on them during Einstein's time?

Mead: Well, they felt to me—as a probably sophomore at Caltech, they felt to me like this can't be the way science worked.

Zierler: Yes.

Mead: I didn't come here to be told I couldn't ask that question.

Zierler: Right.

Mead: You could say there's a big history about this and you could say almost anything to me, but don't tell me I can't ask that question.

Zierler: So who among the faculty most encouraged you that at Caltech you could ask that question?

Mead: Well, Tommy came the closest with his little, "Go read about it in Herzberg's little book." I think he was searching for a way to get me in touch with that line of thought that Schrödinger had very strongly in the beginning, and then he got into waffling as he got beat up by the guys with the matrices. But I think Tommy didn't want to go into it for reasons of his own. I don't know.

Zierler: Mm-hmm.

Mead: I never got encouraged, and I didn't know that Linus had been there.

Zierler: Right.

Mead: I think Linus would've told me, but I didn't realize he'd been there at the time. I didn't even know these debates had happened. I just thought, there's something just not right about this. And that was just not what I came here for—I came there to get to the bottom of things and here was something that was hanging. And there were legitimate questions. I didn't even know Einstein had asked the same question and got the same answer. [laugh]

Zierler: [laugh] That's a good day when you come to that conclusion, that you're on the same page with Einstein.

Mead: [laugh] If they'd have just told me that I'd have felt better.

Zierler: Right.

Mead: But that always stuck in my craw—-this is not physics, this is something else going on. It felt like politics or religion or something but not physics. So I had always wanted to get to the bottom of that. So, as the time was coming to where I was going to "retire"—well, it turned out that a former grad student of mine had become a Caltech professor through a long story that you don't want to hear about, and he had this lady grad student, Arati Prabhakar—- I had been on her thesis committee so I had come to know her; a very talented lady who decided to go into the government side of things. She had come and talked to me about it—-this is a different kind of career and what do you think of it? And so what I told her was, "Well, that's not a thing that I've been at all suited for. It just wouldn't work for me, but if you have a calling to do it, you feel drawn to it, the worst thing that could happen is you learn something about it and don't like it and go do some technical stuff. But, if you like it, you might have a good career there." So I didn't discourage her. I let her know that I wasn't the person to have an opinion. So she went back to DC, and she started as some kind of a postdoc in government agencies someplace. You probably know what they call them.

Zierler: Yes.

Mead: And she did one of those and liked it and did well. And then she ended up in DARPA. It was, I think ARPA then, not DARPA, but whatever. It's been back and forth. And she really liked that and did super well at it. And, in fact, she was the one that was overseeing it from the government side when they made SEMATECH, which was a project to standardize fab equipment so that it would inter-operate, so you could have a wafer that went into one supplier's machine and came out and went into another supplier's machine on the same line—they have little tracks the wafers run on. And if you standardized all those interfaces then you could make a fab that worked with multiple people's fab equipment, which you had to do because nobody made it all. And she was the DARPA person that oversaw that effort. Bob Noyce went and became in charge of SEMATECH, and then Arati, this lady, was on the government side funding it out of DARPA and watching over it, and added a bunch of technical value because she could see the overall picture really well. And both Arati and Bob Noyce were very good at seeing how things could work together. So she ended up having a responsibility that was way above what most program managers at DARPA would get. Just happened to be at the right place at the right time and did a just stellar job of shepherding all that from the government side. And, of course, I would see her once in a while when she came through town, she'd come to Caltech, and it was just great to see how she made this thing into not just a career but a trajectory that really mattered. So we got to be better friends as she was doing this because I was on the technical side of all that and doing the Moore's law thing. And she was doing the thing that was making all that possible and working with Bob Noyce, who was a good friend of mine. So we got to where we would have dinner any time she came in town. And the DARPA things only last five years. That's just the way they work there. You come, you got five years to get something done. And she happened to land there at just the right time—it took about five years to get the SEMATECH thing going really well and get the fab equipment standardized and on the right track. This, of course, made her well known in Washington circles.

Zierler: Right.

Mead: So, sometime later, and I don't remember how much later, but sometime later she got put in charge of NIST, the National Bureau of Standards, which is a big job. It's one of the top technical jobs in the country.

Zierler: Right, right.

Mead: So, when she took over NIST, she wanted me to come back and give a little talk about how the Moore's law thing had gone and just what that was all about. And so I did that. And then, as a quid for their pro quo, she said, "Have a nice tour of NIST," which has lots of good stuff. And one of the things they had there was they had just figured out that the quantum Hall effect was going to be the next way to define the fundamental constants. And that was a huge change in the way people thought—there weren't just little blocks of platinum or anything anymore. Here was this thing—so, of course, they had to get good at the Quantum Hall effect. And she said, "You're going to really like this, Carver." So I went and spent most of a day with the quantum Hall guys. And I said, OK, this is what I've been waiting for! The one quantum thing that had made sense to me before that was the quantization of magnetic flux in a little ring of superconductor—-the quantized flux experiments. And then, there was one that was done by a colleague of mine at JPL, which I've told you about. He did the first London experiment—-you take a superconducting ring and you make sure it doesn't have any flux in it when it's still. And then you spin it and it makes a flux. And London had predicted that, too. He had predicted both those things. And the only thing people ever tell you about London is he got off by a factor of 2 with the quantized flux. Not that he invented the whole idea and figured out the physics of it, but that he got off by a factor of 2, due to the fact that there are pairs, not single electrons, in the superconductor is the standard way people look at it. So I knew about those, but then this quantum Hall thing was just fantastic. And I forget what the date of this was but I could see that I wasn't going to take on any more students and I could get the ones I had graduated, and I wanted something that would be next for me. So I asked the guy if he had one of these things that I could take back and measure for myself. And they were very—they don't do that. But I'm sure Arati said it was OK, so they put one in a little can [laugh] with a little label on it that said "NIST" and sent it to me. Well, it turned out a colleague of mine, Michael Roukes, had some storage dewars for liquid helium that, instead of a 5/8-inch or 3/4-inch neck, they had a 2-inch neck. The dewar as it's built has about a 2-1/4-inch or so neck, but then they put another pipe down it so you can't get anything bigger in it. So he had had them leave that out so you could actually get something up to about 2 inches in diameter. So I got hold of American Magnetics (or some such name), and I had them make me a little superconducting magnet that was just 2 inches in diameter, and had I think a 1/2-inch bore in it so I could put little quantum Hall things in it and measure them. And I had an old HP power supply and so I made this little jury-rigged thing in my office where I could get one of these storage dewars and I could lower the magnet down in it and it would last for a long time because it was in a storage dewar. I didn't have to transfer helium or any of that. So I started measuring the Hall effect in these things, and the plots steps in them—-I couldn't get below 4 Kelvin, but you get pretty good steps in a good quantum Hall sample at 4 Kelvin. You couldn't see the odd ones, but that was a way for me to get back in touch with physics. So I didn't do anything remarkable with the quantum Hall effect, but we did have von Klitzing come by and visit us, and I had a nice visit with him. And I was doing some work with what ended up being almost like quantum point contacts. They were just real thin semiconducting things and, every once in a while, one of them would have nice little quantized steps. So I was getting back into physics—this is real quantum stuff now. None of the stuff about, you can't ask that question. [laugh] You can ask whatever question you can experiment with. And this is getting a lot closer than you ever got with statistics.

Zierler: Sure.

Mead: So I was in the middle of that as the group was gradually getting ready to graduate. And I had two grad students who were interested because they'd come into my office and I was sitting with this setup taking data. And so I started having a little meeting once a week with them and talking about ways of thinking about quantum things that weren't just grinding matrices. And one of the things, of course—well, I hadn't followed physics for quite a while, but I would hear about it. I'd go to their seminars. And I knew there was this Bell's inequality stuff that I didn't understand at all. But I knew this guy, Clauser, had done an experiment on Bell's inequality and everybody was all excited about that. And it was all about statistics. That was the part of the quantum stuff I didn't like. I didn't ever understand it, but I knew that it happened. And then I also knew that there had been a cleaner experiment done—Clauser's stuff was in '72 and '73, and Alain Aspect hadn't done his thing until '82; it was 10 years later. And, of course, by then the whole technology was better. And so he did have the cleaner experiment, and everybody got excited. And then, people were getting excited about the quantum cryptography. I had a friend from IBM, Charlie Bennett, a well-known guy. And he had come out in '81 or '82 to visit us because Feynman was interested in if you could do logic reversibly, and Bennett had a big thing going about reversible logic. And there were a bunch of people trying to do reversible logic with transistors, and I had shown that the energy it takes to do a reversible thing with transistors is always larger than if you just did it with an inverter. And this did not make me popular with Charlie and Dick Feynman because they wanted to do it some other way. And that's one of the things that got Dick going, thinking about, well, if you did it quantum mechanically, you could do this and that. So we invited Charlie out and we had a big argument about reversible computing, but I got to know the guy, a nice enough guy. And so then I kept hearing about all this stuff, but I had never gone back and looked at it. But it turned out that, by then, the thing had morphed into quantum entanglement experiments. And those had this nonlocal element in them which just came out of the math, this Heisenberg math, but there was no physics.

Zierler: Right.

Mead: And I said, "Look, this is the same problem." Yes, you get this answer and you say quantum mechanics predicts magic. No, it doesn't predict magic; it just shows us that there's something we don't understand. So what we need to do is instead of saying, oh, it's mystical, we need to go figure it out.

Zierler: Carver, what are the bigger philosophical underpinnings behind your resistance to magic or mysticism? What's your approach on that?

Mead: Well, that's a good question.

Zierler: Is the basis that all physical phenomena can be understood, we just lack the tools or brainpower to do it at a certain point of time?

Mead: It's interesting, isn't it? Because that in itself is a religious belief.

Zierler: [laugh]

Mead: I mean, Einstein said so.

Zierler: Yes. Right.

Mead: And it's always been what I wanted to do, is to figure things out. And, somehow, I felt that—not that you would figure everything out, but that there was a path—-that there was always something new you could figure out. And, somehow, I just, as a kid—maybe it was because I read the stories about people that had done certain things and figured them out when they were hard to figure out. But, somehow, it's just been in my belief system. And it wasn't a belief that I could do it, but just a belief that there had to be a way. It may take a long time, it may take way past my lifetime, but there had to be a way. It can't be this, "It's just this and just believe it because it's an equation." That can't be the way it is. So, yes, you're right, it's just in my belief system. Einstein had the same one. I didn't know that until much later.

Zierler: Carver, has there been any part of your research over the course of your career that has made you think otherwise?

Mead: No. There have been a lot of things I couldn't figure out—and there have been things that felt to me like they were an OK question and later I realized that I wasn't asking the right question. And that's happened a lot. In fact, most of the good things I've done are because I was asking the wrong question and I kept at it until I realized it was the wrong question and figured out what the right question was. And then that would turn into a good thing. So there was never the belief that every question I could think of was something that could be figured out. Maybe just that isn't a question that makes sense given the way nature works.

Zierler: Right.

Mead: And that's OK. But the fact that there's no way forward, there's no deeper understanding, that never made sense to Einstein. And I wish I had known his belief system earlier in life because it would've given me a lot of better feeling—I went through some pretty discouraging times with that. But, anyway, the whole entanglement thing had come to my attention because I'd developed it, by then, to where it was about one interaction that would go this way or that way. And the two were locked together so if it forced one electron to go one way the other would go the other way. Well, that's not about statistics anymore, that's about the individual interaction. That's the thing which I wanted to know about as a sophomore and was told I couldn't ask about it, and here it is. And it turned out that they were calculating statistics about it, but it wasn't about statistics—they weren't looking at it as an individual interaction, but whenever they described the mysticism of it, they would describe it that way. So I was thinking about this in my odd spare moments and talking to the two students when we had this little once-a-week meeting. We'd get together and go to dinner and talk about physics. And I said, look, this nonlocal thing that's going on, what it means is that when you have an interaction between two atoms, say, that it's a bidirectional thing, and it's bidirectional in spacetime.

Zierler: Mm-hmm.

Mead: So it's on a light cone, but it's a very special thing. It's an interaction between those things which has both forward and backward elements in time. And it has to be because otherwise you couldn't have this nonlocality in the way it works. Which, of course, by then everybody would say, oh, it's magic because you can go backwards in time, but you can't send messages that way, and blah, blah, blah, blah.

Zierler: Carver, I want to ask: Going back to your perspective on these discussions between Bohr and Einstein, over the course of your career, in what ways has the development of technology broadly conceived added a dimension to the fundamental disagreements that Bohr and Einstein had that may have changed the terms of their debate but were simply unavailable to them at the time?

Mead: Oh, I have the whole list in my little green book.

Zierler: Oh, perfect.

Mead: It starts with that. Huge amount of technology that, in one way or another, bore on this issue. I remember both Dick Feynman and I were in the seminar where Bill Fairbank—the quantized flux guy.

Zierler: Fairbank.

Mead: Bill Fairbank. He came and gave a seminar on the quantized flux thing and Dick and I were both there. Of course, I had interacted with Dick quite a lot when I was doing the quantum tunneling stuff, which was very early in my career. We haven't talked about it, but I spent 10 years doing that.

Zierler: Yes. Let's talk about tunneling now. How did you first get involved in that?

Mead: Oh, Yes. OK. Good. That's a part I left out. Thank you, David.

Zierler: Right.

Mead: Well, I had done this thesis on how minority carrier transistors had this problem with minority carrier storage, and I had figured that out and how to analyze it and then did experiments on it. And that led me in that sort of the physics of semiconductors. We talked about that.

Zierler: Mm-hmm.

Mead: And so I was getting more and more into that and working with the guys down at Pacific Semiconductors. And one day, the EE seminar was going to be by this guy by the name of Leo Esaki from Japan. And it was about some kind of semiconductor device that had a negative resistance (this would've been early '60s, maybe '60. I think I had just become a member of the faculty, but within months of that). So I went to this seminar and he described this junction that, when you dope it high enough that the junction is thin enough where it bends from P type to N type, there's a place where the electric field is very high. But there's no voltage across the device, it's just a very high field due to the built-in potential of the junction. OK. I knew about those. I'd just done a thesis on all that. And the junction was thin enough that electrons could tunnel. Well, I knew about tunneling because that was one of the things that I had heard about in class—they hadn't used good examples in class—-they used a decay of a radioactive atom as an example of tunneling. Well, I knew that tunneling could be useful because that's an electron going through a forbidden region. I could see that. So I had known about it. I picked it up as a sophomore. One of the few things I got out of that class. So I was jazzed. This was great. This was real quantum physics in my world. And so what did I do? I sat down in my little lab and started making these things and seeing if I could get his kind of results. And I never got one that was as good as his, but I got it to work. And I could see how, if you made the doping higher, it made the junction thinner. And then the current levels should go exponentially with the doping, and that gave you how the wave function died out in the forbidden gap. So that was all intuitive to me, just the obvious way you'd think about it. So I did some of that just to catch up with Esaki. And I thought, you know, if you could do it in a PN junction, I could just make a thin insulating film and tunnel through that, and that's a lot easier than messing with alloy junctions and stuff. So, within a week, I had figured out how to anodize aluminum to get really nice, thin – controllably thin—insulating layers. I was making metal-insulating-metal junctions that I could see electron tunneling over five orders of magnitude in current and make them at different thicknesses which I can measure with the capacitance. And you get different slopes for that log-current *vs* voltage curve. And you know the thickness from the capacitance—so you can figure this all out. And this is great stuff! I mean, I should be doing this! Well, I was doing this, so I started on that track. And I've told you about getting together with Bill Spitzer, and that was after this tunneling stuff. But I was still doing the tunneling stuff—because if you do tunneling through an insulator, the insulator has a forbidden gap and it has a metal on each side, and there's a barrier between the Fermi level of the metal and the conduction band of the insulator, or there wouldn't be a forbidden gap there. And I could figure out what the barriers were by shining light on the sample and seeing the photon energy where I started exciting electrons over the barrier—the photoelectric effect, basically. You can tell where the barrier is just by plotting photocurrent versus photon energy, and if you do it the right way it has a nice intercept and that's the barrier energy. And so I could figure all that out. And I had a little grant and I got myself a little monochromator and some capacitance measuring stuff. And so I was doing this stuff when I met Spitzer, and I told you about my foray into trying to make an amplifying device out of it, which turned out to be a fun experiment but not a good device. I was measuring the heights of the barriers because you have to know them to figure out what the imaginary part of the momentum was that damped the wave function. And so I was figuring all that stuff. I mean, it's freshman stuff.

Zierler: Yes.

Mead: This is intuitive physics. This is the thing you can just go measure what the wave function's doing. Of course, that's what I came to Caltech for. [laugh]

Zierler: [laugh]

Mead: And Dick Feynman always liked the tunneling stuff because it's hands-on quantum mechanics.

Zierler: Right.

Mead: So, Yes. I'm going to need to take a break, David.

Zierler: OK. Take your time. [pause]

Mead: OK.

Zierler: Carver, I think this would be a good place to ask—it's a very broad question but it's rooted specifically in time. We've been talking about it tangentially. When did you come up with the idea of encapsulating all these ideas in the book, *Collective Electrodynamics*? How drawn out a process was that, or was it sort of a—you woke up one day and you said, I have to get this all down into a book?

Mead: Actually, it was this little meeting with the two students that I was having while we were still doing neuromorphic stuff that got me going again, because they were both super sharp and interested, and had very different ways of thinking than I did. So it was a really nice—I remember one time I had just come up with this notion of the bidirectional spacetime connection thing. And the next day I got an email from one of the students and the title of the email is, "You've been scooped." And they had found this article by this guy, John Cramer, at the University of Washington who had this thing he called a transactional interpretation of quantum mechanics, and he'd written that back in 1980. So that was like more than 10 years before. He had documented it more and more as time went on and written several papers about it. And so one of my students had found one of these, and he expected me to be upset. Of course, I was very jazzed because it meant I wasn't the only crazy guy in town. [laugh] So it turns out that, as you know, I'm now in the throes of getting this paper out coauthored with John Cramer. So that has become a really good collaboration, but it took a long time. Finding John's stuff, which made sense as far as it went, but it was very specifically within the context of the statistical quantum mechanics, which is fine but that doesn't answer Einstein's question: "How does the individual connection work?"

Zierler: Did you have, from the beginning of conceiving this book, that you would have this tremendous ambition to unify electromagnetic phenomena and quantum nature of matter? Was that sort of baked in from the beginning?

Mead: See, where I came from, they weren't different. They weren't different. Electromagnetism was just one electron talking to another electron. And they're not different, they're just part of the same thing. You can't have a thing about electrons without having the thing that's about how they interact. And so, if you're going to talk about electromagnetism, you need to go back and figure out from the quantum point of view what it is, not just take Maxwell's equations and say this is a God-given thing and now I can't think about it anymore. That's just another thing about it you can't ask, right? So, no—it's about electrons talking to other electrons. And so to me that was all part of what it is. And then, when this entanglement stuff came up, that's all about how electrons talk to other electrons. That's what that is. They are connected somehow and we're observing it and treating it like it was magic. So it was more of that thing about this is one picture of how electrons talk to other electrons. And that's what electrical engineering is; it's what physics is. They aren't different. And so that was what it was all about.

Zierler: But Carver, when you say they're not different, you present it as being obvious to you but clearly that's not obvious to everybody, otherwise you wouldn't need to write this book. So what are the—

Mead: And people who might've read the book—[laugh]

Zierler: [laugh]

Mead: —and it's not obvious to them.

Zierler: But I guess the question is: Who is the audience that needs to understand this? Because it's not just undergraduates because undergraduates would've learned this over many generations if it was so apparent to their professors.

Mead: Yes. So the obfuscation of knowledge is not limited to quantum mechanics. It's true in electromagnetism. After I had just published the little green book, I got invited by the alumni—there are a lot of Caltech alumni in the San Francisco Bay area because it used to be there was a lot of technology there. Now it's mostly software. And so we had a bunch of Caltech grads that lived in the area and they had an alumni group there. There were about 50 people, I think, in it. And they wanted me to come up and give a talk. So, well, sure, I'll talk about this. So, for some reason, somebody felt like somebody should go up from Caltech and introduce me, as if I needed an introduction to that group. Probably half of them were my former students.

Zierler: [laugh]

Mead: So it turned out they sent Steve Koonin. So Steve and I think very differently about things. And so he stood up to introduce me and he said, "And Carver's going to talk about electromagnetism, which all of the people I know think is a dead field."

Zierler: [laugh] What an introduction!

Mead: [laugh] Wonderful introduction, right? So when people think that, it means they aren't going to ask any questions about it. And so here's a card-carrying physics guy that not only doesn't believe there's anything there but is delighted to go out of his way to convince people that there isn't anything there. Well, quite frankly, it floored me that nobody had thought through—that's what I say in the beginning of the little green book—with all of these new experiments, somebody must think through electromagnetism in the light of those experiments. And it just floored me the lack of interest in that question.

Zierler: Right.

Mead: Just absolutely floored me. But, if everybody thinks it's a dead field, then it means nothing new left to be discovered, right?

Zierler: Right.

Mead: Just like Bohr said about quantum mechanics.

Zierler: Yes.

Mead: No, there's nothing there. Don't look. So, yes, it continues to floor me.

Zierler: How was the book received in terms of its acceptance in courses, in terms of from your peers? Were you satisfied with the impact that it made, mission accomplished or no?

Mead: Oh, basically, I would get one or two letters a year that said, "I just discovered your little green book and it's changed my life." But, other than that, dead silence.

Zierler: So it's only the true believers?

Mead: No. It's the people who are open to a different way of looking at things. I think everybody else is a true believer in the status quo.

Zierler: Uh-huh.

Mead: In our the new paper—we have a nice little introduction there which goes through the history of quantum mechanics. But we had a reviewer who knew all of the history of the Bell's inequality stuff, and he was incensed that we hadn't put that history in there, and gave us all the references. So I spent a month going through every paper—he had exactly the right ones. Ed Jaynes had been my hero in this whole thing because he had an approach very much like what's in the little green book, but he didn't have the bidirectional connection in time. But otherwise he could work all of the things in a very intuitive way. And I always thought he was a great guy. He had a lot to do with our current view of the subject—anybody who is doing any quantum optics really is steeped in Jaynes' stuff. But I hadn't known what had happened to it, until this reviewer guy, who had personally been through the wars and knew all of the arguments. So it was just a thing that I had left out of this paper. I didn't keep up with the arguments that were going on and who the people were, and who won the argument and any of that. But this guy had the story because he'd been there. So I went back, and I put in the whole story as I'd learned it by going back after reading this reviewer's comments and then going and reading all the papers and understanding each one. And then I went back and worked out, using basically the extension of the little green book stuff. I worked all the problems that were pivotal in their arguments, and so they're in the paper. So now that paper is 10 times more valuable because we had a reviewer that knew what the critical juncture had been, and when it turned this way and when it turned that way, and we were able to get it in the paper. So it's fantastic! I've learned more in the last two months than in the last 20 years. [laugh]

Zierler: [laugh] That's great.

Mead: Just fantastic.

Zierler: And Carver, in terms of learning so much, how much are some of the basic arguments you're presenting, both in the book and in the paper, how much are they empirically presented? In other words, even scientists can have debates about intellectual distinctions, intellectual differences, but once you have qualitative evidence or you have an empirically derived perspective, where's the debate? Where's the argument? Where's the immediate acceptance of this point of view?

Mead: Oh, it's very interesting, actually. I've learned so much, not just about the substance but about human nature from the reviewers of this paper.

Zierler: Yes, Yes.

Mead: One thing, because we do things the old Schrödinger wave-mechanics way, there aren't any probabilities. What you calculate is the wave function and the charge distribution of the wave function and how it moves with time, which gives the current. And so you just do that stuff which is all the stuff that I did when I was a sophomore and made up myself because I couldn't stand what they were doing. So any sophomore could do it. And that's what I've been trying to make clear, you know, this is a way of understanding. It isn't competitive with the statistical thing. I don't make any pretense of calculating statistical stuff. We're looking at a level below that which is what Einstein wanted. Well, what's underneath those things? Well, this is what's underneath those things. It's simple.

Zierler: Yes.

Mead: It's not a problem. And I would say there wasn't a single reviewer who was a card-carrying physicist who didn't, at some point, identify one of our things as a statistical thing instead of as a continuous thing—and they just couldn't help doing it. It's very interesting. And so I had to go in and put reminders: "now this is not statistics of a number of trials, this is a single smooth event that's happening during the transition." And it's not the probability that a transition will get started, which is what they calculate with their matrices. These are people who are actually quite interested in it and may or may not go further than that. And Ed Jaynes, in one his papers—I don't quote this in there but it's the paper where he gave up on his approach—he actually has a whole page about how difficult it is for people to communicate about this thing.

Zierler: Mm-hmm.

Mead: No matter which side people are on, if they're trying their darnedest to communicate, they just can't get past the differences in the way of thinking.

Zierler: So, Carver, you alluded to it, you've learned so much about human nature, what is it that you've learned about human nature over the course of these experiences? And how do you know—let me even expand on that—how do you also know that you're not just part of the problem, you're just representing your side of looking at it?

Mead: Oh, it's true. It's true. Of course, I'm part of the problem.

Zierler: [laugh]

Mead: And having especially that one reviewer who was so completely into the other way of thinking and completely knew all of the stories about the Jaynes thing and what did and what didn't work and all of that, I can get there. I mean, if I had been through that, I could be as convinced as he is if I'd have come through it a different way. So I guess the part I don't understand—I understand being firmly convinced about something, but that's very different from arguing about does this experiment agree or not agree, does it prove or disprove—well, you can never prove a theory, but does it disprove the theory? And those are substantive discussions, which is what I was getting from this one reviewer. The others—I think it was a genuine—I think it's a genuine difficulty of actually thinking about a thing in a fundamentally different way.

Zierler: Yes.

Mead: I think it's hard for all of us. It's the reason I have trouble with their math. And it's not that I couldn't buckle down and do it if I believe there was a point to it.

Zierler: Uh-huh, right.

Mead: But everybody can do that. I think I understand what's behind it and I think we should have that discussion—I don't see anything wrong with the answers they get, but where do they come from? It's just math.

Zierler: Carver, in what ways does academia reinforce these unfortunate divides? In other words, these are not just thought experiments that exist in bubbles, they're reinforced in tenure decisions, they're reinforced in departments, they're reinforced in peer reviews, right?

Mead: Yep.

Zierler: In what ways does academic culture—which ideally should just be a perfect free forum of ideas—in what ways does academia ironically reinforce and make these distinctions more rigid than they need to be or even should be?

Mead: Oh, that's, of course, a sociological question.

Zierler: But you already told me that you thought a lot about human nature as a result of this, which is why I'm asking.

Mead: Well, it's very clear that the people that told me that I couldn't ask that question were uncomfortable being faced with the question. And when faced with a question where they're uncomfortable, there are two things you could do, right? One, you could say, well, Einstein and Bohr had big arguments about that same question and it never got resolved.

Zierler: Right.

Mead: That I could've been very jazzed about. Someone that I talked to must have known that.

Zierler: Mm-hmm.

Mead: Like Tommy must've known it.

Zierler: Right.

Mead: In one of Schrödinger's very early papers, he lays out what we're doing, specifically; I put the quote in this paper. Tommy must've known that, so why did he put me off on Herzberg? Well, maybe he knew there was a baby version there that I could understand.

Zierler: Right.

Mead: It wasn't easy to understand Schrödinger's papers. He was a very good mathematician and he loved to play like a virtuoso, so it was never, look, let's do the simplest thing first. So I couldn't have understood Schrödinger's papers back then. But if somebody just showed me what he was doing—he says what he's doing—then I could've got that. Maybe Tommy never really understood the arguments.

Zierler: Mm-hmm.

Mead: I mean, it was messy. They didn't have all these nice experiments we have. It was almost as if they didn't know the history, but all they knew is there'd been a big argument and it ended up that people that didn't get good at Heisenberg's approach didn't get jobs.

Zierler: Uh-huh.

Mead: And so you did that. And maybe that had already happened by the time I was a sophomore. But looking back on it—that was in 1954, so that was already 30 years since the whole battles were going on. So I think already what you were talking about had happened—-that when you ask questions, the teacher says, "hey, don't go there", or "shut up and calculate."

Zierler: [laugh]

Mead: And Feynman says that in his red book, just don't go there. Nobody's ever got back from there.

Zierler: [laugh]

Mead: And Dick never hinted at it, even though he had obvious discomfort—I mean, he said it in his red books—he said that "nobody understands quantum mechanics," which is basically what's true.

Zierler: Even today?

Mead: Yes. It's just a way of calculating that gets good answers and it's magic. And don't ask because you won't get a job. [laugh] And, of course, having retired, I didn't need any job. So, yes, the entire academic system limits what we are allowed to think. The guy that invented random-dot stereograms—you know what they are?

Zierler: Yep.

Mead: He was Hungarian guy—-Bela Julez. He came and spent a winter with us one time when we were doing the neuromorphic stuff. And he gave classes and I went to his classes because he was a great guy. He says, "You know why people believe in receptive fields?" And one student answered this, you know, maybe so and maybe so. "So you know the real reason they believe in receptive fields?" "Uh, no." "It's because their advisor believed in receptive fields and he flunked out all the people that didn't."

Zierler: That's amazing. It's amazing!

Mead: You know, the Hungarians are blunt, right?

Zierler: Right.

Mead: Well, he was just being blunt. This is what really happens. And there's actually more truth in that than any of us would like to believe. And I had lots of students down through the years and I've seen students that I knew and worked with and all have that experience as part of their education, it's just you can't ask that question if you want to get a passing grade in this class. [laugh] And so you shut up and calculate, right? And that's about like Bela said—-it is simply a fact that once ideas get the upper hand somehow, that they tend to self-intensify. So what may have had questions surrounding it gets more and more ossified into better mathematics, slicker ways of doing things, theorems get proved, and a fortress gets built around it. And what actually is amazing to me that any new idea ever gets through at all—-ever! Really!

Zierler: Wow.

Mead: And typically, it's when there's an experiment that—like Tommy Lauritsen said; "Why do people believe in special relativity? Because of the Michelson-Morley experiment."

Zierler: Right.

Mead: Now, I've read a lot of the stuff about the Michelson-Morley experiment. It was a wonderful experiment. But it didn't answer all the questions about relativity and there are ways in which relativity is taught that I don't think are a very good way to teach it, but it's the way everybody is teaching it now because it's very streamlined and it's very easy to get the results and you don't have to ask any stupid questions. So that, to me, is a beautiful example of the phenomenon—fields tend to gravitate and make their own "potential well" by all of the work that gets done from that particular point of view, and everything that is related gets to be an overwhelming amount of "evidence" that the approach is working. So the more of that there is, the more people can say, oh, that can't be right because of the XYZ effect. What's the XYZ effect? You don't know what the XYZ effect is? Ugh, well, then you have no right to even ask me that question.

Zierler: Yes.

Mead: That's a typical thing. Then you go and read about the XYZ effect. It has nothing to do with the argument. It was just a way to get the upper hand in the argument at that moment.

Zierler: Sure.

Mead: Happens all the time. I've had that happen to me hundreds of times. Now, that's not the kind of people I'm talking about.

Zierler: Right.

Mead: I'm talking about the people who don't do that but still have a hard time. So the potential well is just very high, and so people need something to hang onto to get past that, climb that hill. It takes a lot of work. So, yes, academia and scientific fields on the whole reinforce the dominant of way of thinking about it and, in the process, they squeeze out any knowledge of the fundamental questions that are left behind. So that it's very hard for someone to even find out what they were. My Caltech colleague (Jamil Tahir-Kheli) and I spend time over dinners talking about all this stuff—-we're both very interested in the critical points, in the arguments between Einstein and Schrödinger and Bohr and Heisenberg and Born and Pauli when they were trying to wrestle this thing. And, of course, the World Wars didn't help. But this was between the two world wars, so there was room in there to get it wrestled, but it didn't happen. It got cut off too soon and I have still not found out how. And my colleague reads all kinds of original papers and history and stuff, and he hasn't found it—it's very unfortunate. There's one—-I think it's maybe Heisenberg's letters—-that we can't get because it's thought that the Allies, when they took over Germany, took all his stuff and put it in some secret place and we don't know where.

Zierler: Oh.

Mead: Yes. I think it's his letters from people. And, of course, in those days, most of the communication was done with these letters. Oh, in this paper with Cramer we've got a beautiful quote that my Caltech colleague found for me, a letter to Schrödinger from H. Lorentz. Well-known guy, right? And Schrödinger had written to Lorentz with this paper of his, asking very respectfully what he thought of it. And Lorentz wrote back, and it was a beautiful thing. He said, "Well, if I were to analyze just a problem that exists in XYZ coordinates, then I favor your approach because of its greater intuitive clarity. But if I have more dimensions than that, then I'm not able to make a picture in my mind anymore of the wave function as a wave and how it works, and then I have to go to the matrix form. So the choice was really hanging on the fact that Schrödinger's approach was limited to a one-electron problem, basically, the wave-mechanics stuff. And, of course, everybody went on from there to do all kinds of other stuff, but it really was when Heisenberg used his approach to get the energy levels of helium in 1927. And, because he could do that and Schrödinger could not, that won the field over to the matrix approach. The issue is—when you put the two electrons in the same wave function—now Pauli is there with his exclusion principle, and you can't do that with wave mechanics. So that was really what tipped it. He had a case that Schrödinger could not work. You got all the same answers for one-electron problems, but with this two-electron problem you can't get there with Schrödinger. And that was enough—that was the thing that tipped it. I'd never known that before. But it's important that people know that, and that there was this argument, and that, for single electron problems, Lorentz himself said would really prefer wave mechanics because of its *greater intuitive clarity*—and then he went on to say another thing. He said, and even for many electron problems, there's some truth in your approach because down deep they have to be the same kind of wave function. And even you can't calculate it with your stuff, it gives you a way of thinking. And I thought "This was Lorentz! I would never have imagined him thinking like that!"—but see, that was what Einstein did. They really wanted a way to conceptually think about it. And if you couldn't have one, you couldn't have it, but they sure wanted one. So it's interesting. We don't teach the students any of that—they don't need to know all the history, but they need to know the tipping points and what tipped them.

Zierler: Right.

Mead: Because it could be there was something in there—well, there's certainly something in there to be learned. That's a key place to look if you're going to try to make a next step of any kind is the place that broke the last one, right? So that's it. But, yes, it's self-reinforcing, and this isn't just about science. I mean, this is about schools of thought.

Zierler: Right. What you're really saying is that science is not immune from these things just like any academic discipline.

Mead: Right. Yes. It's its own religion, and people have beliefs, and those beliefs, at least in science, some do change when there's enough evidence—typically it's when there's a key experiment that people look at as something that they want to understand and it makes everything they do not fit quite. And then there'll be a few folks that say, "I have to understand that." So that's typically the way it happens. You know, there's this book, I'm sure you've read it—

Zierler: Mm-hmm.

Mead: *The Nature of Scientific Revolutions*, it's called.

Zierler: Oh, OK. Right, right.

Mead: You know the book. And it's too long for what it has to say because it had to be a book, not a short book, but I learned a lot from reading that book just how difficult it is to get over that hump.

Zierler: And it's important to note that reading Thomas Kuhn is not just important for historians, it's important for scientists, as well.

Mead: Yes, Yes. It's especially important for scientists—Yes.

Zierler: There you go. [laugh]

Mead: I wish he'd been a little more meat and a little less verbiage.

Zierler: Yes, Yes.

Mead: But if you don't let that get you down, it's a worthwhile thing to read.

Zierler: Right.

Mead: Yes. That's probably a good place to stop.

Zierler: I think so, too. Perfect.

[End of recording]

DAVID Zierler: OK, this is David Zierler, oral historian for the American Institute of Physics. It is August 16th, 2020. I'm so happy to be back with Professor Carver Mead. Carver, thank you so much for joining me again.

CARVER Mead: Great to be here, David.

Zierler: OK. So today, I'd like to spend an extended amount of time on a major project that you've been involved with for the last major portion of your career and that of course is G4V. And so just to introduce our broad audience of researchers who are going to be accessing this for generations to come, tell us a little bit about first of all what G4V is and where it comes from conceptually.

Mead: Well, the way I got into it, of course I'm an electrical engineer, so I tend to think of things like gravitational attraction as an analogy to electromagnetic interaction. I can't help it. You drag your background with you whenever you come into anything. So a number of years ago, I'm not going to remember the year, but you can find it easily enough, it turned out that Stanford University had a contract, which went for 40 years to develop this satellite which was going to measure the induced rotation in a superconducting sphere due to the rotation of the earth that would induce in a free-floating sphere a rotation due to gravitational interaction.

It was a big project called Gravity Probe B. It had gone for 40 years before it flew. It was Bill Fairbank who got the thing started. And a close friend of mine by the name of Bob Cannon was the guy that had suggested that the only way to do it was in space. And he was at Stanford at the time. And Cannon was the reason I knew about it and it's not something I would normally know about. And then once I learned a little bit about it, it turned out my good friend Kip Thorne was also heavily involved in the project and had sort of master-minded what the numbers might be for what the resulting rotation might be like.

And it's a very interesting problem. And of course, for an electrical engineer, the immediate thought is, "Well, all we know about gravitation is that it's opposite from electromagnetism, that like things attract and we don't know if unlike things attract or not, we don't have—that we know of—any binary system where half of it is matter and half is anti-matter. That would be a gravitational analogy to the kind of binaries we have in electromagnetism." So the whole thing's very interesting just from the point of view in what way might it be like electromagnetism? And that's a discussion that has gone on since there was any knowledge of both forces—that analogy was known since the beginning.

Zierler: Now Carver, how have the questions changed in recent times? Given the fact that this has gone back so long, what developments—both in the distant past and in the recent past—caused those questions or analogies to change?

Mead: Well, I can't give you the whole history because I don't know it. I haven't been in that business except there was this thing came up: the Stanford Gravitational Induce Rotation experiment. Because my friends at Stanford were doing it, I got invited to the launch of the satellite and I hadn't really paid attention to it before then. So the launch was down at is it Point Mugu? Halfway down the California coast.

Zierler: I think so.

Mead: Barbara came with me, and we stayed over and there's a little town called Solvang, which is a tourist trap in that neck of the woods. And we stayed over there or close to there. And they had talks—-it was a nice event and I learned a lot about the thing. So after the launch, which went very successfully, my wife Barbara and I were driving home from there and I kept saying, "You know, that sounds to me just like a magnetic interaction."

And so I got to thinking about it and the only thing we really knew about gravitation was a static interaction. But, of course, by that time, I had worked a lot on the four vector way of looking at electromagnetism. So there was a scalar part, which was the zero part of a four vector, and then there was the three components of a spatial part, and those are the parts that are involved in magnetic interaction of electromagnetic things. The scalar one is the part that interacts when you have two charges, opposite charges that attract each other.

So I thought, "I wonder if this interaction they're looking for with the Stanford experiment is a vector interaction by analogy with four vector electromagnetism?" Which I'd just written the little green book about. And so I thought, "Well, I can work this out." So I spent some time thinking about it and I worked it out and I got the same answer they got. So I thought "This interesting, because they're saying that the only way you can get this is Einstein's general relativity, and it looks to me like it's just a plain vector interaction, just like magnetic interaction in E&M."

So that was curious. It always bothers me when people use a complicated way to get something that's basically simple. And this looked, to me, like another thing that always irritates me. If it's that simple, why doesn't it get explained that way? And so I wrote down my little finding and I thought, "Well, you know if that's true, I wonder if it's true of other things that we know about gravitation?" And of course, I knew freshman level stuff about gravitation. I never thought about it.

And so I started reading up on what we knew. We knew that there was this precession of the perihelion of mercury—so I tried to think about how you could work that one out, and it ended up being much more complicated than the Stanford experiment. The Stanford experiment was very clean and it has been underrated as a contribution to our basic understanding. The reason it's been underrated is that, the minute they got an answer, everybody said, "Oh, we knew that because of general relativity." Well, as my friend Alan Weinstein says, "You don't know it unless you've measured it!" And that was certainly my view of the case.

So I think that GPB experiment that Stanford people drove to completion was a historic experiment in our understanding of gravitation has been grossly underrated because it gave the same result that general relativity did. That was a major finding that everyone just said, "Poo poo." That's not right. It isn't right when something that fundamental is established by experiment. But of course I had to wait for, what was it, four years for the thing to fly around long enough to—or maybe it was two years—I forget. Anyway, it took a long time. I think it was four years before they had it worked out because there was some questions about the data that had to be fussed with and all that.

So to me, that was a wonderful finding because it meant there was a simple way of looking at this wonderful experiment that showed it was the vector part of the interaction, which had not been measured previously.

Zierler: And the significance of that is what? I mean, besides it being a new measurement, what are the broader implications of this achievement?

Mead: Well, you know, gravitation is a very fundamental part of the way the universe works, and if we have clean, simple ways of looking at it that we can teach our kids without burying it under four years of fancy mathematics, that's something that should be cherished. We can have simple ways of seeing things that everyone can understand. Everyone experiences gravitation, that's the scalar part, and here's this very, very clean experiment of the vector part. And here was a way of thinking that made it obvious. You know Feynman has my favorite quote of anyone in the world. He said, "The real glory of science is that we can find a way of thinking such that the law is evident."

And that was one of the things that I always appreciated about Dick, both as a teacher when I was a student and as a colleague once I was on the faculty and we started working together all those years. He had this drive to see things cleanly and clearly and simply. And it's something that I've always treasured. It's built into the way I think and the relationship with Dick had a lot to do with that because he was the first person I had ever encountered that knew how to do that, just in his bones. It wasn't something he did part-time. This is what he lived for.

Zierler: But you never worked on gravitational concepts or questions with him?

Mead: No. And I don't think he was particularly interested in that—and of course I was emeritus by then, so I wasn't on campus that much, so I would only see him rarely. So I did talk with Kip about it. And Kip—he's deeply, deeply into general relativity, but he's also a true gentleman, and he encouraged me to think about this in a different way and acknowledged that there might be some value in having a simple way of thinking about it. Kip's a great gentleman, he really is. He also—just as an aside—has been the one Caltech faculty who has supported the Feynman lectures on physics and we have a young fellow—not so young anymore—who has brought those to a high level of finish, he and his collaborators. Name is Michael Gottlieb and he just devoted his whole life to that endeavor. And Kip has been the guy from the physics department side that has supported that. And I'm the guy from the other side that has supported that. So Kip is very generous with support of people doing things that he's not directly involved in, but can see the value in. So it's just great to have colleagues like that, and that's an aside.

But then I went ahead and worked out things like the bending of light by gravitation, which is a non-obvious thing and the way people explain it is warping of space time. Well, in electromagnetism, you don't warp space time to send a radio wave, so is there another way of looking at it? And it turned out that you can do a vector analysis that gets the same answer that they get with warping space time. And I thought, "Well, that was interesting".

So that was all going along and by then, LIGO was up and going and starting to get data—but there were no signals yet. I got involved with the LIGO guys about two years before there were any signals detected, which is a great time to come in and got to know Alan Weinstein and Max Isi who's now back at MIT, incredibly neat people. You hear a lot from Alan, of course, because he's the main representative of the information coming out of the LIGO collaboration.

So I started having lunch with these guys when I'd go down to my trip to Caltech and we ended up doing a couple of collaborations on the question of "would there be a vector component in a gravitational wave signal? So I worked that out. And it turned out that I made a factor of four error because of an error that was in a paper that I referenced without redoing a derivation myself [laughs] important lesson.

It was a factor of four error which would have been a factor of 16 error in the energy of a detected wave. And that was just a blunder. I didn't catch it. When I had published the thing, a fellow whose name I can't remember right now got back to me and said, "Hey, there's a factor of four there." And I went and checked it and sure enough he was right. And that was really great when somebody does that.

Back then, we thought we were going to see signals from pulsars. We thought maybe there were big enough lumps on a pulsar, and they're going around really fast, and maybe we could detect something. So we spent a couple years doing that. And you have to integrate over a year because otherwise, you can't tell the difference between a solar year and astronomical year. And when you're looking for a really, really small effect, that's the signal that you would see is you would see that one day slip. And so we were all working that out and we never did see anything from any of the pulsars that were likely candidates.

Zierler: And Carver, how long would you have to look to be confident that there was nothing to see?

Mead: Well, the LIGO guys are incredible. I had some friends, in particular Dick Lyon who I mentioned before, who was part of the Foveon group, and he fantastic signal guy, and he put me on to the Lomb-Scargle algorithm for looking for periodicity and signals that are not regularly sampled, because LIGO doesn't run all the time. It has times when it's down. And so you have a dataset which is not complete. And so you can't handle that with standard Fourier techniques. So it turns out there's this Lomb-Scargle periodogram which is perfectly happy to have left out data. I actually ran my whole analysis on the data and they ran theirs and we compared and Max Isi and I both found bugs in the other guy's code and in our own code and that was a thrilling time, just to get to the bottom of exactly the whole thing. And we didn't find anything.

And then of course what was that September of '17 or August, I forget when the first one was, but we got a detection from LIGO. And of course then all hands on deck. See if we can understand that one. And so I looked at the data and they looked at the data. Of course, they have a highly sophisticated way of processing data due to some incredibly smart people—-the LIGO group has off-scale good people. But I was tracking it and looking at it my own way and I couldn't rule out that it would have been a vector interaction. And they thought they could, and they were pretty sure, actually, but I thought, "I'll suspend judgment for a while."

And then we got another detection and that one was, once again, it was a thing that I think for those two, we only had two detectors up. And then I think it was August '17 or something that we had the biggest thing that ever happened in that business, which was the double neutron star merger. And it was fantastic. We got signals in everything. We got gamma rays and we got radio and we got optical and so we knew where it was and what was amazing is that gravitational wave came within one and a half seconds of the optical wave.

So for the very first time, we knew the speed of gravitational waves to one part in ten to the 15th with the very first experiment. And as Alan said, "You don't know it until you've measured it," and we've measured it. And for Alan and I, that was thrilling because we're experimentalists, and yes, theories are nice, but you don't know it 'til you've measured it.

Zierler: And Carver, on that point, is it really something that the theory is so rock solid that you are confirming something that's known except for the measurement? Or is this really advancing something that previously was not well understood?

Mead: Well that's a good question. That was the other reason I was interested in the Stanford experiment is that we had, of course, an actually less precise than people think—measurement of the value of G, the constant out in front of the 1/r potential of gravitation, but there were really no convincing measurements of the vector part. And the ratio of those two things gives you the speed of propagation of gravitational waves. So until the Stanford experiment, we had no solid evidence—of course everybody believed it would be the speed of light—but everybody believing it is a very poor guide for what it actually turns out to be.

So that was the other reason I was very charged about the Stanford experiment was that it really gave us the first measurement of the speed of gravitational waves—I forget the accuracy now—but it was—

Zierler: Remarkably accurate.

Mead: I think it was in the percent range. I'm fuzzy now because that's been a long time ago, but it was a first indication experimentally, in my mind, that was sensible that we had a vector part and that speed was within tolerance of the speed of light. So that was a wonderful—and that of course was well before we had any sightings with LIGO. But this thing pinned it down to a part in 10 to the 15. It was fantastic. So for the first time, we knew for a fact that this was a real thing.

Zierler: Right.

Mead: And for me that was a big thing. Once again, everybody said, "Oh, we knew that because of GR." Well, yes, GR predicted that would have been and it turned out that's the way it was, but as Alan says, "You do not know it until you've measured it." And so that was thrilling to be in the thick of it when that happened. But then we looked at the pattern—Max went through the careful analysis of the relative amplitude at the different receivers—and for that one, Virgo was up too, so we had three. Night and day difference between having three detectors and only two. And it was loud enough in all three to have a good signal-to-noise. It was made in heaven. It was a fantastic experience.

And so Max looked at it and said, "Carver, this can't be vector." And so first thing I did is do the magnitudes, and even with my error that I hadn't discovered yet, it was not going to be a vector, when you looked at the relative magnitudes, not even close. I mean, it was so clear, which was thrilling because it means you don't have to agonize over it anymore.

So then I started thinking about it and realized what had gone wrong in my thinking. And it was actually something I knew, but I didn't know enough about GR to know the corresponding thing there. And in a electromagnetic quadrupole, the magnitude of the signal contains the distance across the quadrupole, the scale of the quadrupole. If you make it bigger, it radiates louder in the quadrupole mode. Dipoles aren't like that. I mean, the dipole moment gets bigger, but it isn't one of those things that inherently has the dimension in it. And of course you end up with a one over R for a dipole.

So there's an extra one over R in the quadrupole radiation. That R being not one over R distance, the scale of the radiator over R. And when you get far away, that's a big factor—because it's the ratio of the antenna size to the distance away. So that's a huge penalty factor. And I knew that, but what I didn't know was that Gravitation had no analog to a dipole—I knew that because like things attract in gravitation, that you don't have a dipole. So I thought the first mode was going to be a quadrupole. Turns out, they have their own version of a dipole and it's the mode that, in their language, stretches space time. And that doesn't have the little R over big R factor in it. And I didn't know that. So it turns out that that's a huge factor. And it never occurred to me there would be a thing that radiated what they would call the strain until my colleague, Jamil, pointed out to me that what GR really says is it's a Del-squared kinda thing operating on this thing and that the mode that they saw was what they call the strain.

So that was a big ah-ha for me. This was all in retrospect after I had said it was going to be a quadrupole was that there's this lower order of mode that doesn't have the little r over big R in it, and so I learned something that nobody else did [laughs] because they all knew the other thing already. But they knew it in GR language, not in electrical engineering language. So I learned something I can now say in electrical engineering language.

Zierler: Carver, you learned something, but what did the field of electrical engineering learn as a result to the extent that what you learned is representative of where the field is?

Mead: I don't know a lot of people in electrical engineering that think of themselves as being able to make a comment about gravitation. It just isn't part of their world.

Zierler: But then why is it part of yours if you are an electrical engineer? Meaning the question, Carver is I understand that most, if not all electrical engineers, gravitation is not part of their world and yet your interest in this discovery is significant for the field because by virtue of you being interested in it, right? So I guess the question is what would make you unique in this regard among electrical engineers?

Mead: Well, I don't think I am unique, but I can tell you that people ask me quite often about the relationship between engineering and science and to me, there's a very broad, fuzzy line, which divides those things. And they're more alike for me, personally, than they are different because they're about how things work. And in order to build something that's useful, you have to understand the basic principles behind what it is you're going to build it out of. And it's not OK just to say, "OK, somebody said blah blah blah and now so you do that." No, that's not good engineering. Good engineering is you understand the medium you're working in all the way to the bottom.

And then you can make something out of that medium because there's ways it wants to work, and if you go with the way it wants to work, then it naturally will do the kind of things you want—you can coax it into contributing to your wishes for how something could work. And so, the figuring out how the medium really works down at the bottom is, for me, the essential part of doing good engineering, because, if you don't understand that, you don't have any business trying to make something out of it.

But what goes with that is anytime something comes up what works in an interesting way, it's interesting and not because you want to make something out of it, it's because it's interesting just to understand if there's a simple and clean way of understanding how it works, then it fits into a picture you have of the way the universe operates. And if it's fuzzy because of somebody's theory someplace that's complicated and covered over with lots of fuzz, then it means you don't know enough about it to have an opinion—So, OK. You don't go there, but there's always a kind of a funny feeling that there's a neat thing to be understood there and I don't understand it. It's like an itch you can't scratch, you know? And this was one of those that I'd always wondered about.

So I went on to write a bunch of these things up that I did. I figured out the vector thing for the Stanford experiment and the bending of light and the procession of the perihelion of Mercury and all those, and then people started getting, along about that time—they got good at plotting the magnitude of what they call the number one supernovae—-1A supernovae I think, as a function of how far away they were. And that was to inform our notion of cosmology.

And of course there were arm wrestles about whose data's right and all that, and that was all very interesting, but I thought, "Well, you know, that's an interesting thing. I've been thinking about gravitation. That's certainly the big principle upon which the macroscopic view of the universe works. Maybe there's a simple way of looking at that." So it turns out I sat down and worked out a scalar cosmology. Turns out you can do it as a pure scalar problem with a minimum of assumptions and get within less than a factor of two of the kind of numbers they get with GR.

Well that's a neat thing. So I think out of the whole thing, that was the neatest thing, to have a cosmology I could explain to any high school student because I understand how it works. And I never published that, I should put it on archive because it's a really neat thing. The gravitational wave way of thinking is different: If I just say yes, "there's this strain that can also be propagated", then I can get that too. But that doesn't fall out of any physics that I know.

Zierler: Carver, I just want to note how deeply touched you are by the educational value of this endeavor. You've expressed this several times so far. It seems like it's really important to you.

Mead: Well, to me, if I can't explain it to freshmen, it means I don't understand it. And if I need more than freshman math, that means I just don't understand it well enough. And Dick was the same way, he really believed that if he understood something, he could explain it—and he did explain a lot of stuff to freshmen—in his stuff that ended up in the Feynman lectures. Those classes, I was collaborating with Dick on some tunneling stuff at the time, so I sat in on a number of his freshman classes and that inspired me to do my own set of freshman classes for engineering stuff. How semiconductors work, and how logic works, and that sort of stuff. And I found freshmen to be just delightful to teach because they're so bright-eyed and bushy-tailed and they haven't become bogged down with the, "Oh, this is really hard and I can't be excited anymore, I just have to grind through it," you know, which happens after about a year.

And that's an important part of education too—-the grind part to get to the end, but it sure is nice to have the bright-eyed bushy tailed freshmen that are just all excited about things. So I had more fun teaching my freshman classes than any of the others, just because of that.

Zierler: Carver, where is the Shapiro Delay in all of this?

Mead: Oh, that's right. I had some very interesting discussions with Shapiro.

Zierler: He's an amazing person.

Mead: He's a great guy and he should have got a Nobel Prize a long time ago.

Zierler: Absolutely. No question.

Mead: And it's absolutely criminal that he has not, because there have been Nobel Prizes given in the neighborhood that he certainly should have been a part of—I have no idea what's going on there. Don't ask me about politics because I don't understand it. But that's wonderful—what he did—see, in G4V, the speed of light depends on the gravitational potential—-and that's a poor man's way of not having to warp space time. If you insist that the speed of light is constant, then you have to warp space around because the speed of light isn't constant. And well, you know, the bending of a light beam is an indirect indication that the speed is slower near the gravitating body than it is a little further away and it acts kind of like a lens and it bends light. But that's an indirect indication, but when you have a light beam or a radar signal that goes next to a gravitating body and you can see the delay change in just the way you would expect the speed of light change with gravitational potential, thrilling. Absolutely thrilling.

So I worked that out of course and so I corresponded with Shapiro. Very generous man, very modest, and he recalls some of the things and questioned me a little bit and it was one of the great interactions that I had when I was working on G4V. Huge contribution, once again, it's a simple thing that's one full step closer to the underlying reality. See, that's what the Stanford thing was, but the precession of perihelion was a very indirect thing. You have to go around and around. But the earth turning here, turning the little suspended superconducting sphere by the vector interaction and extraordinarily direct measurement of the vector coupling.

So to me, that's why it was thrilling. It was a step more direct than these other things. And Shapiro's thing is the same thing. It's a whole step more direct at what the underlying physics is, and I expressed it as the speed of light being dependent on the gravitational potential. That's not the way people in gravitation look at it, but it works for a lot of things and it certainly works for the Shapiro Delay. And for me, it has become part of a way of thinking where a lot of things are evident. It's never going to get you all of the answers, that's clear because of the double neutron star and the gravitational wave stuff, but that's the first place that it hasn't worked for me.

So it means there's a lot of what's going on in the universe that can be thought about in a simpler way and this little cosmology is pretty interesting because it just doesn't depend on the presence of dark matter or dark energy and it doesn't require any of that, which is interesting.

Zierler: Is that to say that it leap frogs the gap in understanding that we currently are living through with regard to dark energy and dark matter?

Mead: It could be—I believe—of course now I've been proven wrong big time at least once. And actually, if you look back, I went after more things that didn't work than things that did work. And that's just a big one that I went after and it just didn't work out, but a lot of things did work out and the cosmology was one that I found very instructive because it's a way of thinking that is one step closer to not having a lot of assumptions. The way they construct cosmology—-it's just built into it, has all of these assumptions you have to have, which require that you have dark energy and dark matter and so on. I think that dark matter is with us to stay. I don't think dark energy is with us to stay. I think it's just that we're thinking about it wrong, but I could be completely wrong. I've been completely wrong many times and perfectly willing to admit that if it turns out that way.

Zierler: Carver, another big question mark of course is the inability to incorporate gravity into other fundamental theories is what some people think is keeping us from a truly grand unified theory. Do you see this research as getting us closer to that sort of all-encompassing theory or is that sort of beyond how you think?

Mead: Well of course—OK. Let me tell you what I really think.

Zierler: Please.

Mead: I think that—I actually said it in my little green book—I think that this period since somewhere in the 1920s got off on mathematizing physics beyond what made sense—it ran ahead of any experimental guidance. And I think there's a huge number of symptoms of people that fell in love with certain mathematical constructions that ended up then being taken as reality rather than as one way of trying to systematize reality. And it's turned out that general relativity has held up much better, in my view, than the other parts of physics have.

I think the quantum stuff, as I think I've said to you and you've certainly read it, has been on what I would call it a wild goose chase for the simple reason that there's an assumption that the radiation field is quantized, which I think I've said this to you, gives us an energy density in space which is 120 orders of magnitude too big for any cosmology. And of course, that's why I had to do a cosmology just to say "Is there something specific about a cosmology that could hide 120 orders of magnitude?" and the answer is no, there's nothing that could hide more than about one order of magnitude because I've got a zero order of cosmology—-it can't get any simpler than that—-and I've compared it to the data that's available and I get within factors of two or so. I fit the curves of the brightness of the pulsars versus their distance and I actually use them to get a parameter.

And then, using that parameter, I get for the energy density per unit volume in the universe a value that's within a factor of two or three of what the big fancy theories get. So there's no orders of magnitude to be had because I'm doing it with grade school stuff. So if you have to have something that's got 120 orders of magnitude that aren't there, you're just doing something wrong. It's a non-starter.

Now, that doesn't mean that those calculations that people are doing can't get good answers. We learned a long time ago that you can find ways of calculating that down underneath somehow reflect the nature of reality, but not for the reasons that you wrote the equation. And I think that's what's gone wrong with the whole quantization of the modes of space time. And until we get that straightened out, I mean, they've thrown away gravitation by 120 orders of magnitude, how do you expect it to fit? [laughs] You can't even ask that question when you have the 120 orders of magnitude hanging over you.

Zierler: Do you see this problem originating in Niels Bohr's criticism of Einstein? Does it go back to that foundational of a level or is this after Niels Bohr?

Mead: That particular one started with a 1932 paper of Fermi's. Now, I'm a big admirer of Fermi. I mean, he was a renaissance man, he was a great experimentalist, a great theorist, you know, he's one of my heroes in history. And he just postulated that as a way to get answers. I think if Fermi were here and you told him, "Hey, this thing—look at what happens with the energy in space time." He'd think about it about a minute and he'd say, "Well, that means that's not what's there." And I think that's what's true. That's not what's there, but it doesn't mean you can't make a way of computing out of it.

Let me give you an example. So, is there energy in electromagnetic fields? That's the basic assumption, even before you quantize it. There's energy in electromagnetic fields. Well, let me give you a very nice example—this was one of the trick questions we always asked EE grad students on their orals just to see if they were awake or aware—so, you have a capacitor, the energy in it is one half CV squared. Now, you can look at that two ways. You can look at it as the plus and minus charges on the plates attract each other so to take an electron from one side and move it over to the other requires energy, and if you integrate that up to potential V, it ends up being one half CV squared. OK.

So the energy is the potential energy of the charges on the two sides. The other thing you can say is it's one half epsilon E squared. So you integrate that up and you get the same answer. So which is it? Well, that one's not so clear so let's do another one. If you have a resistor, there's a current going down through the resistor and there's a voltage drop across the resistor. So the current makes a B field that goes circles around the resistor. And the potential gradient makes a E field that goes along the resistor. So if you do E cross B (the Poynting vector), it's a set of lines that curve in and end up on the resistor. And if you integrate that up, you get the power that's dissipated in the resistor.

Now that one's a little easier to see. It's a perfectly good way of calculating the power dissipated there because of the energy in the space time, in the field, E and B. But you know, we know quite a lot about a resistor. We know about the current that flows inside and we know about the scattering of electrons, and we've built resistors out of lots of different materials and so I don't know anybody that would not say, "Well, down deep, it's the electrons getting scattered in the conductor there, and that's where the entropy is being generated, so that's where the power loss is. So that's really where the energy flow is. It's not coming out of space somewhere else and coming in."

So that's an example where you look at it two ways and you say, "Yes, you can calculate with that, but it's not the physics that's going on." It's a dead-end street. And yes, it's cute. It's really cute. But is it real? No! That's not the reality underneath. I don't want to stand up in front of a class of freshmen and tell them that's where energy comes out of space and that's this mysterious stuff that's out there and when you run current—it all comes in and gets the resistor hot. No. I don't want to stand up in front anybody and say that!

Zierler: Carver, is that to say that math can get so abstract that it approaches metaphysics?

Mead: Actually, I think it has. And I mean, you know, there's a very simple straight forward way of looking at the collapse of the wave function in certain very simple cases like a photon going from one atom to another and there are people that are saying, "Well, every time we make one of these decisions, there's another universe." Now, that's way past reality. And these are serious people. I mean, these are not idiots—they're smart people. And the question isn't that hard to answer, to freshmen. It's just that the value system we're operating in right now doesn't use that as a metric.

So to me, just like what Dick said, you can find a way of thinking such that the law is evident. And we need to get back to that, and we've strayed in the field very, very far from that. Fortunately, it's still working for Einstein. By the way, Einstein had all the right instincts about what was going wrong in the quantum world, but he never got the right examples. And so he got shot down every time he tried to do anything, and that was actually a tragedy for him personally because he got discredited as a influential figure in physics, which must have hurt him deeply as a person because he did have real insights and it's a shame they got lost. Fortunately, his GR didn't get lost and so that's a wonderful thing and I'm in awe of how far it's gone.

Zierler: So Carver on that note, looking ahead, what are the prospects for the ongoing testability of G4V?

Mead: Oh, I don't think it's viable in the form that I put it forth at all. I think it was discredited by the double neutron star, the merged neutron star. And so I don't see that as a viable path forward. What I would say is—up to the first two orders—-it's a very neat way of seeing what the Stanford experiment did, and what cosmology is about, and I don't see it as conflicting with GR in those regards. It's a way of saying GR is a complicated theory. It's very hard for freshmen to understand, so they don't.

But think about it this way, and then when you get to where you're good at GR, you'll see that this is a poor man's way of expressing a thing GR does much more elegantly, but it's a way that's close enough related that you won't feel like you're in foreign land. It's the next level up theory from this viewpoint—and I think freshmen would benefit from hearing these stories that I tell from these simple viewpoints because they could feel how the whole thing works without getting lost in the math.

And once you have a sense of how things work, you can go to a fancier theory from there. In fact, it probably would motivate a bunch of students to go wade through the fancier math to find out the next level of understanding, but I think we're so far from being able to communicate with high school students about physics that either they only just get Newtonian physics or, as so many high school students I talk to, particularly girls, and they say, "Oh, physics, no. No!" They had such a bad experience in their first course that they never want to see it again. And I see that a lot.

Zierler: Right. And it's upsetting to you?

Mead: It's very upsetting to me. It would be upsetting to Dick if he was here today. He did a lot to make things more obvious to people. And if you look at any of his lectures, they're just wonderful in the clarity of thought. So I think there's a huge amount to be done to resurrect being able to imagine and to picture and to have a intuitive feeling for physical phenomena that isn't covered up with math. And then certain people will go on and get good at the math, and that's great, but if you—

Zierler: Math is a barrier to too many people.

Mead: Oh Yes. Huge barrier. Especially when it looks disconnected from reality, which is particularly true of the quantum stuff.

Zierler: There's a strong democratic sensibility, Carver, that's running through your comments now, which is, you know, just to play devil's advocate, and not that I believe this, but one response is so what? So what if a bunch of high school students feel that the math is too difficult or it's too abstract and that serves to block them from the field? The field doesn't need people that are not on that level. And what you're saying is it most definitely does. It needs to be inclusive, it needs to attract a broad range of people that might have great opportunity to advance the field, but never would because of have difficult the math is. Is that fair to say?

Mead: I wouldn't say it that way. I was a big math fan when I was in high school, and—

Zierler: But it was the math of the real world. It wasn't the math of string theory that was a theory on top of a theory on top of a theory.

Mead: Yes. The way I look at it is there are people that think in symbols and there are people that think in pictures, and I've always been a person that thought in pictures. Einstein was like that. And to me, when I see an equation, it has to be about a picture that I have of real things. So when I look at an equation, I look at the symbols in the equation and I understand what that quantity is as a physical thing. So then the math is telling me a relationship between physical things. And if I can't do that, I lose interest in the math because to me, it isn't saying anything about physical reality.

And essentially all engineers work by imagining the reality they're working on and so just having math without that connection doesn't connect any of the neurons that are the way they function. And that certainly was my experience, that the math that I loved, when it spoke to me about connection with physical things, I had lost interest when it was talking about things that, to me, weren't physical.

And so a lot of those people ended up over in electrical engineering and they ended up as my students and it was almost universal that they had thought about maybe being a physicist and when they got to the first course in quantum mechanics, it was so abstruse that they said, "This isn't a place I can live. Let me go over where things are more real." And so, those are people that go off and do very remarkable things, and often with very leading-edge stuff. So it's not like they're stupid, it's like their brains are wired differently and we need those people because they're the ones that built the modern world.

Zierler: But perhaps they should not be setting the tone to the extent that this is how physics is being taught at the freshman level.

Mead: Yes. I had a great freshman math teacher whose name was Bohnenblust, and he was a wonderful mathematician and he taught freshman mathematics and it was one of the greatest courses I ever had and I learned so much. The next year, they had sophomore math, and it was taught by Tom Apostol, who is an extraordinarily well-known mathematician, but his idea of mathematics is, "This is what, if you're going to go and be a professional mathematician, this is the next thing you need to get good at." So his model was "I'm training graduate mathematicians and I want them to be able to get into a mathematics career."

I hated that course. I couldn't get to square one and whenever I'd ask him anything like, "How would I apply this to blah blah?" he would look down on me like I was a lower form of life and say, "This is Mathematics!" And so, if a student you're talking to is a person who views mathematics as a tool to express a language, to express and quantify physical things, that's a very different way of looking at mathematics than if you're going to be a grad student and go be a professor of mathematics some place. And they're both totally honorable directions, and one is not better or worse than the other, but what doesn't work is when you try to treat students of one kind like they were the other.

Like for example, I would have happily taught sophomore mathematics to engineers because there's all kinds of wonderful examples in electrical engineering of every aspect of sophomore mathematics, but I would be totally inept at trying to teach sophomore mathematics to mathematicians. Wouldn't make any sense. So I would look at it more that way. Mathematics is a fantastic subject and I've known enough mathematicians and heard enough about what they do to realize that they can express any behavior elegantly in mathematics, but that means there's no place in it for the constraints of the real world. There's no place for those symbols to represent something that has behaviors that you're trying to represent.

So that comes later, once you have an understanding of the reality and you can make very excellent and elegant mathematics and it really helps. So let me give you some examples; just plain old differential equations and the way we get solutions and stuff. That wasn't common knowledge, but once we started doing Newtonian type things, that became an essential part of what you had to do. And there were all kinds of fundamental questions, like, how do you actually handle infinitesimals? That was not understood for years later. Meanwhile, the engineers were going off doing it, and the physicists were off doing it.

Another good example is just linear circuits. Just the way you solve linear circuits with linear differential equations and there's a whole lore there and it became a language in how we taught electrical engineering. Heaviside invented what's now called the Laplace Transform, but the problem is that the mathematicians needed to explain to us engineers all of the lore behind all the theorems and where, mathematically, Laplace Transforms came from. And it turns out, you never use any of that in doing electrical engineering problems.

So the way [laughs] it's done is you use the mathematical form, but you don't go off and solve it with those theorems and stuff. You just don't do any of that. It's a way of thinking that you can visualize, you can plot things in a complex plane, you can do all that, but you don't ever go off and do all of those theorems and all that. It isn't that it isn't wonderful stuff, it *is* wonderful stuff. My friends that do computer graphics do incredibly fancy math because they have to get into three dimensional space and they have to be able to view it and you can't do it with three dimensional quantities because you get gimbal lock when you turn it to where one dimension locks up and then you can't figure out which way to go. So you have to use four dimensional space and they do that all the time.

My son teaches mechanical engineering, and they have to find ways to move the tool in a numerically controlled machine and they can't get gimbal lock. So they do four dimensional mathematics. So it depends on what you're doing, if you can see how it fits with what you want to do and what you want to understand, of course you go learn it. It's not the problem. The problem is does it fit with objective reality that you can imagine and picture and work with and create things out of. And that's the real issue.

Zierler: Carver, to bring the narrative up to the present, I'm reminded that we started talking on Sundays because you're so busy these days. So can you talk a little bit about what have been some of the most interesting projects you've worked on in recent years up to the present day?

Mead: OK. I'll take a little break and I'll be right back.

Zierler: Sure, OK.

[pause].

Mead: Well, I think I've already told you about the paper we just finished yesterday, so I don't think there's any point going over that.

Zierler: Right.

Mead: There's a project that—as you pointed out— I've always been interested in education. I actually started teaching classes at Caltech when I was still an undergrad. I contributed to a junior lab at Caltech the summer after I was a junior. I've always had to support myself, so I had a job at a company in Pasadena called Consolidated Electrodynamics Corp. (It was an outgrowth of a company called Consolidated Engineering which was Herb Hoover Jr. had started. I didn't know that there was a Herb Hoover Jr. and that he was an engineer.)

And I got this job for a summer and part-time during the school year with the transducer division of that company and they made, among other things—did I tell you this story about working at this company?

Zierler: Uh-nn. [no]

Mead: In the old days—are you sure I didn't tell you this story?

Zierler: Well, let me hear.

Mead: They made instruments for people who were doing oil exploration. I didn't tell you this?

Zierler: I don't think so.

Mead: OK. Let me tell you the story because it's a good story. And in those days, the way you did that is you went off on the desert someplace and you had a set of geophones, just transducers that would measure small earthquakes, and you would place them in a line somewhere that you were interested in and then you would inject a signal somehow. You could either set off an explosive or one of the things they had is they had a big truck, which had a vibrator in it, and they would go and they would find the place they wanted to explore, and they would jack the truck up so the whole weight of the truck was sitting on the vibrator and then they would vibrate the truck up and down with this huge mechanical vibrator kind of thing. And of course, that would radiate a sound wave down into the earth and that sound wave would reflect back and they would measure the reflected signals on these geophones.

Well, you would have maybe a hundred geophones out on one of these arrangements. This was back in the 1950s. We had vacuum tube technology from World War II, which was pretty up to date in the 1950s. It was only 10 years from when the stuff had been developed. But that's a lot of data and what would you record it on? In those days, magnetic tape was the most advanced recording technology we had. And there was essentially no digital technology outside of the great big experimental computers that people had. So it was early days.

So what they did is they had this machine that took 10 inch wide roll of film negative and they wrote on it with light beams and the way they did that is they would stack up a hundred—they had a big magnet block with little slots in it, and they would put little galvanometers in the slots, and the galvanometers were one tenth of an inch wide. They had inside them a long, skinny voice coil that would rotate in the magnetic field—the signal that you detected from a geophone was weak, but there were good amplifiers in those days, so you amplified each signal up and you could write on the film with a little mirror that was on this voice coil.

So they were, of course very thin, so that they could fit 100 of them in 10 inches of width—and they were suspended by one top wire and one bottom wire that held the voice coil just suspended—and of course you put the current down one wire and out the other wire, so there were your electrical connections. And so these galvanometers were clever little things. You had a hundred of those things and you could, in parallel, record all of the signals from all the geophones on the moving film negative. And of course they all had the full width, or at least a large fraction of the width, of the film available.

So you have this incredibly intricate thing, and then they had ways of trying to figure out what was coming back. I didn't get involved in the how do you decipher what's coming back, but I worked on the transducer part and we had just had a class for either sophomores or juniors, I forget, but on dynamical systems basically, and I thought—Yes it was a junior course—and I thought, "This would be really neat to have this electromagnetic system, which is, in all regards, like a loudspeaker but it's already instrumented." The trouble with a loudspeaker is they're really hard to instrument, but this thing is already instrumented and it's electromechanical system that had a resonance. You could damp the resonance by putting a resistor in series with the voltage source you're driving it with. They had to work all that in order to get these to have a nice, flat frequency response so they had to have worked that problem.

So I went to the Caltech EE people and said, "Hey, this is a great course, but couldn't it use this thing?" We'd get them to donate the magnet block and one of these galvanometers. And the magnet block had a little light in it. It was a thing they used to characterize their individual galvonometers when they'd build new ones. And they had an extra one of these magnet blocks around. And I talked them into donating that and two or three of the galvanometers, and so I worked that summer after hours on that project.

I got a ruler that was made out of frosted plastic, it was translucent, so if you had a light on the back side of it, you could see it, but you could see the scale on the thing too. And so then you could hook a ordinary audio oscillator into the galvanometer, and you could measure the frequency response by how wide the light stripe was for each frequency, so you'd just read it off. So I built this thing over a summer, got paid for it. And then I was the TA the next year in the class.

So I've always been interested in when you had a simple way of looking at what's an otherwise pretty complicated problem, because now it's analogous to a loud speaker or any of those things, but you had control of the damping, you had control of the frequency, and you could characterize the thing, then you could make a little model, which is a simple little model. In fact you had to make a model that included both the electrical and the mechanical parts, because it was a coupled electro-mechanical system. There was a very clever way of how you make a single model that represented both the electrical and the mechanical parts—because the degrees of freedom aren't separate.

So you can make a model that incorporates both the mechanical and the electrical degrees of freedom, and that's a very interesting process in itself, so you'll learn a lot doing that. So, this was a beautiful example that I got involved in as a junior of trying to teach—because I had been in the class with the other students and they didn't really understand what it was they were measuring and why it was a good idea, so they just treated it as, "Oh, I'll go through this and I'll get a grade" but they didn't get excited about it.

So that was my first experience in being able to actually teach a thing and make the physical thing that would embody the insights that you get by analyzing it, and that's what real science is about, it's what real engineering is about. You have your idea and you can make a model, which people call a theory, and then you can have a physical embodiment and you can see to what extent it actually acts like your model and that's all you will do the rest of your life.

And I got an incredible sense for how, when you explain it to people that way, they get jazzed because they can see this is real. This is what I came here to learn about. And I'll be doing this the rest of my life. And there are two classes of students. There were those that were just there to get a grade and they didn't care about any of this, and then there were the ones that said, "Yes, this is why I came here and now I'm finally able to do it." So that was a wonderful—well, you asked about how I got started on all this, but that was the first time I had the chance—I think I had the inkling there all along—-I told you the story about coaching my freshman roommate on Linus Pauling's course.

Zierler: Mmhmm.

Mead: So anyway, my son's a mechanical engineer, but he knows a lot of electrical stuff too and he's also a computer whiz. So it turns out, in today's world, if you're going to do mechanical engineering, you have to do all those three things well. So what he does is he teaches his students how to do those as one field, not as three separate fields. And so he builds experiments and gets them to make models and does all that stuff. I grew up back in the mountains where the technology was these power plants, which are amazing things. To this day, amazing things. We depend on them, all of us, for our life, and people have no idea what that means. They turn on the switch and the lights come on and they say, "What's the problem?"

Zierler: Well in California they're thinking about it because they're having brownouts right now.

Mead: Yes and that's because the people doing policy shut down the two big nuclear plants that they had for base load because they thought, "Oh, we can do that all with renewable energy." Well, now where is it when you need it?

Zierler: Yes.

Mead: It's not that this was any secret. It's that people just didn't understand, even at the grade school level. So my son and I decided we were going to build a model power plant so that people could understand how a power plant worked. We're now on version three. And just last summer, the two of us took it down to a museum on the lake that was a headwater for the power plants where I grew up. There's a big campground there run by the Edison company (that's the power company up there.) And the company donated some land to the museum right next to the campground. The people at the campground do water sports all day on the lake and then they come in and this is something interesting to do, is to go see the museum.

So it's a perfect place to have exhibits that are relevant to electric power. This lake is headwaters for one of the great power projects in the history of electric power, so it's a big part of the history of the area. So anyway, long story, but we were able to construct a little power plant that sits on a cart that'll go through a three foot door and it's about five feet long, and it has a tank in the bottom part of the thing where we put a submersible pump. So we have a source of pressurized water.

Well it turns out that pressurized water is what generates electricity up there in the mountains, and the water-wheel doesn't know where the pressurized water comes from, whether it comes out of a reservoir someplace way up high or whether it comes out of a pump—-the wheel doesn't know the difference. So you can run a little baby power plant with pressurized water that you make on the spot and it works just the same as if that water was out of a part of the power project. There's a dam and a big pipe that comes down a hill and that gives you pressurized water. So this is just another way to get it so that you can demonstrate it.

And then we have a little water wheel, it's about 6 inches in diameter. It's built exactly like the water wheels in the power plant that's about five miles from this museum. And we have a way of measuring the torque and a way of measuring the flow of the water and then we have a generator that generates real power and we can put that power back on the line, as if this were a real generator to generate power for the grid, because that's what generators do. They put their output on the grid and you have to know what that's about and how an individual power plant is related to a grid, which is connected to all of the generating plants in the United States, plus some in Canada—-on the same grid.

Just think about that as a technology problem. Well, it turned out that there were some incredibly smart people back in the 1800s that figured this out, Steinmetz being one of the foremost ones. There is a lot known. But it turns out it's simple if you understand just a few basic things. So, this little power plant, you can fire up the pump and get water onto the water wheel and you can see the water jet interacting with the little blades of the water wheel and you can slow it down to where a person can look close up and see how it's pushing on the thing and making it go around. You can't see that with a real power plant because it's all underneath and it's humming away and making a lot of noise, but you can't see how it works.

With our little one you can speed it up and you can start getting electricity out of the generator and you can measure that and then you can speed it up and you can see that phase of the waveform coming out of the generator and the waveform on the grid and you can see, as you speed it up, that the wave form from the generator comes closer and closer to that and you can get it to be stationary with respect to the grid and then you can hook them together. And you're now part of the grid. And then you can just give it more water and, all of a sudden, you start putting energy into the grid, like all the other generators that put energy into the grid. It's thrilling.

So last summer, we took one of these down there and the museum had an event where people would come and watch, and my son Nathan gave a talk about the history of how this is related. His grandfather was at one of the power plants that was right down the stream from where we were sitting. He went on to explain how this whole thing works and then he explained how you "operate" this little power plant. And that was the end of his talk. And then he said, "And if people would like to see this close up, come on up."

And who came up were the kids. And in about five minutes, we could teach each one of them how to run the little power plant. They were like six years old. Girls as many as boys. No gender preference at all. They were just as interested. They stepped right in, they weren't afraid to run it. The adults wouldn't touch it because they were afraid they'd break something. The kids went ahead and did it. It was thrilling.

Zierler: Sure. The kids are the real scientists.

Mead: It was thrilling.

Zierler: Yes.

Mead: So yes. There's probably 10 years of experimentation that the two of us did on the side. He'd been using earlier versions in his classes—he teaches a power conversion class, because if you're going to do electromechanical things, you have to know what motors are, you have to know what transducers are, you have to know all of that stuff. So in his power conversion class he used the first one of these. We had it on a 2 by 4 frame and it was a clunky thing. Then we did a better one. So he's been using the thing in his classes here in Washington for maybe close to 10 years.

And then the more recent one he used for a year to make sure we had the bugs out of it before we took it down to the museum and before the COVID thing—of course they can't let anybody inside of the museum now—but for the rest of that summer, they had people in there and the kids loved to run the power plant. And they get a feeling for what it does. And then they started having groups from downtown. This is of course up at 5,000 feet in the mountains, but they have groups of school kids come by and get the demos and learn to do this. It was just getting fired up and then we had the COVID thing. But at least we got the second half of last summer to use it. So that's been one of the thrilling projects, just to see that nobody will ever feel the same about electric power or about power generation or where it comes from and there'll be some of those kids that go on to do technical stuff because they had that experience when they were young.

Zierler: Carver, that's such a great segue for what I think will be the last part of our talk and that is some big questions that I will ask you to reflect over the course of your career. And so, so clearly in what you're saying now and what you've said through all of our many hours of time together, there's this duality that I want to ask you about. So on the one hand, your interests lend themselves so obviously to the world of industry. Right? Not the world of the ivory tower and theory and abstractions, but to real stuff, right?

Mead: Mmhmm.

Zierler: And so the question of course is what is it that has kept you in the academic environment all of these years? You're so well aware of, you know, through all of the innovations and technologies and inventions, your work with Gordon Moore, you know perfectly well what your contributions could have been had you entered industry right after finishing your doctoral work, right? And yet, at every fork in the road, you decided to stay in academia. And I want to know, what is the big overall explanation that you see that has kept you in the world of education and scholarship over the course of your career?

Mead: Yes. That's a fantastic question. Of course, it wasn't clear to me, coming out of school. I was going to go to work at one of these companies and do the engineering thing. But then I've shared with you some of my experiences with events that ended up being pivotal in my view of the world, like this thing with making a experiment for the junior course, which, at the time, it just felt like a thing that needed to be done and I could get paid for it and it needed to be done.

But then figuring out my thesis about the way transistors could work as switches and what limited them and the physics of that was something that I had no previous experience with. It's just that it was important to understand why they behaved the way they did. It was another one of those things that was just there to be done and it had to be done. I just had to do it. It wasn't like it was optional for me. I just felt that I had to understand that.

So I was getting started at being an independent researcher because I had actually figured stuff out that a lot of big names out there hadn't figured out, and it was simple. So before I had read these papers and they were long and complicated and mathematical and didn't seem to get anywhere, and that wasn't what I wanted to do. But this one, this was simple and it was clear that it was new. And so, that was just an experience I had doing my thesis that I'd never had before, but by the time I'd written it up then I got it published in this journal that doesn't exist anymore.

And then Gordon Moore came by and I told you that story. And he offered me a job as a consultant up there, "Come up and consult for us." Well, I knew what that was because I had been consulting for Pacific Semiconductors so I knew what consulting was. So I started doing that because it was a more exciting place. There was more leading-edge stuff happening there. And I told you how that worked, I would go in in the morning and talk to Gordon and then I'd go out and spend the day with the guys in the lab (They were almost all guys then. It's different now.)

And then I'd come back in the evening and we'd talk and sometimes go to dinner. But one of the things that I learned doing that was that these guys all were on deadline. It wasn't what it sounds like. It wasn't like somebody wrote down a deadline and if you didn't do it, you'd lose your job or something. No, it wasn't that kind of thing. It was just there was this urgency because they had products that had problems and they needed to figure out the problems so they could ship product so they had a company.

So there was this urgency that goes with startup companies, which I've learned to love because that urgency drives innovation. It's not because somebody's whipping you to do it faster, it's not that sort of thing. It's this urgency to make it real, to make it really work, to make it benefit the world, and by the way, I'll make some money on the stock if that happens.

So all of those motivations are in the same direction, and I got to feel that with this startup company. But I also would work with these people to solve their problems and I realized there's a physics question here. And these guys can't address it because they don't have time. They have to find a solution *now*. And if the physics will help them now, that's good, but if it's going to take a year, forget it.

So there were these questions that would come up in these state-of-the-art structures they were making that were fantastic research questions. So I'd say, "Well, let me have some samples and I'll take them down and we'll see what we can figure out." Well, those would often result in joint papers between the guys at the company and me and my grad students. And I realized this is a fantastic partnership because I don't have to invent the whole technology. Just the very fact that there's this leading edge technology happening in front of my eyes presents research problems that nobody else can get to because they didn't have the leading edge thing in their hands and I did.

So it ended up being this incredible win-win deal where I would take the thing down and work the science and sometimes it got there in time to help them and sometimes it just became, "OK, we've solved that, but it's really nice to understand what's behind it." In both cases, they were happy and I was elated because I had samples that nobody else could get. And they didn't have time to do the thing that I was good at doing and wanted to do anyway, which was understand all the way to the bottom how it worked.

So that ended up becoming a reality before I realized what had happened. And I became a completely different person—how to say it? My worldview evolved incredibly rapidly in that period to where I can say this now to you with clarity, but at the time, I was just learning it. But it changed my life. It became the opportunity to really understand this thing that's happening, and of course I appeared on the scene just when it was taking off. So I was lucky—-to know the people that were doing it and to be able to help out in various ways was thrilling. And I wouldn't have had that freedom if I had been at a company—-to choose how to contribute and in what way and for how long and all of that.

Zierler: Yes. And Carver, I can't help but point out that it's no accident that you used the word freedom here. Freedom to you is intellectual, it's not economic.

Mead: Yes. Exactly. Exactly. The only thing money does is it allows you a little more freedom.

Zierler: And that's not just a theoretical concept for you. You could have been a much, much wealthier person had you pursued a career in industry.

Mead: Yes. But I was able to squirrel enough way that I can, like right now, keep my colleague going through this time when there isn't any other funding.

Zierler: Carver, what do you see as some of the big interests or ideas or curiosities that unite all of the different projects that you've worked on over the course of your career? What are the big top line items where you can say, "Look at all of these different things that I've done, but they all share these things in common." What do you see as those commonalities over the course of your career?

Mead: Well, they're all interesting in and of themselves. It's not just because they'll make some money. They are deeply technically interesting. How they really work down at the bottom is interesting. And so, I've never done anything just because it might make money. If it wasn't interesting—I don't have energy for that. I know people for whom money is their main focus. I'm very happy when a company succeeds and I may have a little piece of it, that's very nice. But Foveon is a great example, I lost money on that company, but it was a fantastic technology.

It's still, to this day, a fantastic success in my own experience, even though I lost money on it. Because it was the creation of the most amazing imaging technology the world has ever seen. And that's thrilling. And the company was necessary to do that. It's not something I could have done in academia. And it's still a very real part of what's out there. The young Yamaki—I don't know if I told you, but Mr. Yamaki's son is now running that company and he's a very competent and smart and good-hearted person. I feel very good about it, even though I lost money on it.

Zierler: Carver, what concepts in physics, maybe even those that you learned as a freshman, since you're always interested in understanding physics in a way that you can convey it to freshmen, what concepts or theories in physics stay with you every day that are fundamental to the experiments that you've worked on, the way that you see the world, the way that you understand how the world works? What are those concepts in physics that you feel a special intellectual or even emotional affinity to, day in and day out.

Mead: That's a great question. [Pause] I think that for me, it's what Dick said, that "you can find a way of thinking so that the law is evident." That if there's a way to think about it so it all fits and there's a cohesiveness and wholeness about it somehow. And I think that's the only thing that, if I can't find that, I'm not the person to work on it. And there have been a lot of those, of course. And by the way, the G4V thing, I don't consider that a waste of time. I learned a huge amount and I think in fact there are truths to which that that way of thinking helps, even though it isn't a way to get the whole thing.

And so I don't feel like that was wasted at all, but it was the part that I could get hold of that way that worked. And the part that didn't work was the part I couldn't get and I just didn't get it and so I think that's right. I think that whenever I've been able to do that, it's worked. And otherwise, it just hasn't worked out. And there have been plenty of those, most of which there's no trace of, because it didn't work out. So you don't worry about those. Often you learn something from the ones that don't work out that lead to one that does. So it's interesting. Nobody's ever asked me that question.

Zierler: Carver, of course for you, the starting point is always the basic science and the intellectual curiosity and if anything real comes of it, that's great, but that's not the starting point for you. Having said that, you are unique among so many scientists in academia that you've had the pleasure over the course of your career to be at the center of so many innovations that have had real lasting and beneficial impact on society in so many areas. And so surveying all of those contributions that you've been a part of, are there any that are most satisfying to you or bring you the most pride because of the help that they have bestowed on society?

Mead: Well, of course it's wonderful when you can be a part of an evolution process that takes the society to another level. That's a thrilling thing. And I've been fortunate to be able to be, in one way or another, connected with that sort of thing. But there's another aspect to it I think we've flirted with, but maybe I haven't said it. The other thing about things I've gotten interested in is I have to be able to see how they're real. And it's just part of the way I think.

So the connection with reality to me has to be there for a thing to be even vaguely interesting. So, the fact that a solid state was a medium out of which you could make real electronic devices that did real things was, for me, the reason I was interested in it. In those days, people that did solid state physics were doing F centers in alkali halides and stuff like that, and I would never have gotten the slightest interest in that sort of thing.

And it was really the transistor and then the tunnel diode that made that field interesting to me. You could use it to make things that were real. And so, to me, that's always been there. That's the engineer in me and it's always been as alive as science. And of course, device physics became a whole field of its own. It used to be the "which came first, the horse or the cart." It used to be that people looked at device physics as the tail of the dog. But now the tail's wagging the dog and the fact that you can make real devices out of things now is front and center in everybody's view of the field, but it wasn't like that. And that was good because it meant that I had a chance.

I was just a kid from out of the woods, I hadn't had any chance with these people that had big labs set up to do alkali halides and fancy instruments and sophisticated analysis that required all this. I didn't have any of that. So no place for me in that business. Even if I'd been interested, but there was nothing there to be interested in. It was just one more thing that people were doing because it was there and you could do it. But the device stuff has always been super interesting because yes, it was real—-you could do real things with it. So that's the engineer in me.

Zierler: Carver, whether with your collaborators who are peers, your students, or even the delight that you see in exciting the imagination of school children, you have always championed women and girls in the field in promoting their work, in encouraging their interest, in doing what you could to give them every available opportunity that boys get more naturally. And so I want to ask, what motivations are there for you besides the obvious that science benefits the more people that are involved? What, besides that, do you see as some of the motivations you've had over the course of your career and even long before words like diversity or inclusivity were sort of a part of the lexicon in the sociology of science? What's been there to motivate you over the course of your career?

Mead: Well, it always bothered me the way girls were brought up. When I was a school kid, there were girls in equal number, roughly, in this little school up in the mountains. And the girls were given dolls and things like that for Christmas and the boys were given, you know, tanks and fire trucks and things like that. And of course, we kids all talk all the time and there were girls there that had no interest whatsoever in the things I was interested in, and there were girls there that were very interested in things I was interested in.

So I could talk to some of them about things that would come up. And the others weren't interested. So very early childhood, I noticed that they were quite different in terms of the way I could interact with them. Like I had a tinker toy set, a very extensive one, and there was one gal that lived in that camp with us and she would come over and the two of us worked together to build things and she really liked that. And there were others that had no interest whatsoever in that sort of thing. And this is grade school age.

I unfortunately didn't get a chance to raise my daughters to where the difference showed, although I think neither of them had interest—I know that the oldest one didn't have interest in this sort of thing. My son did, and, when my granddaughter came along, I was determined that we'd make sure she had opportunity to experience technical things. So I bought her Erector sets and Tinker Toys and she would come and play with me for a while with these things and then she'd go back to her little rainbow-bright horses. She was not interested. So she's one of the girls who are more stereotyped in that way, but I always felt like people treated them all that way and it was just wrong.

And my wife was very interested in boy things and never got the chance. Her mother treated her just like she was a canonical girl that was going to marry another Navy guy and that was going to be her career. She's gone off and done a bunch of neat things—she would have done wonderfully in technology if she ever had the chance. Very smart, very good at researching things, very good at building things, very good at figuring stuff out. And just never had the chance.

And it's always troubled me and it's always been really exciting to me when there was a girl that was actually really interested in technical things—I told you the story about admitting women to Caltech, didn't I? And the faculty meeting and all that. Yes. So, of course, I got to be the advisor of our first woman EE and that was Louise Kirkbride, a very accomplished lady. It's just so unusual to find girls that have that innate way of being interested in the things that I find interesting—it's unusual. And I think you have to give these people extra opportunity because they've been deprived, they all have, because of this stereotype.

So that's troubled me since I've been a kid. It just didn't make sense. It's so precious when it's there. Why would you ruin it by treating them like the ones that don't have it?

Zierler: Carver, do you consider yourself a spiritual person? And as a scientist, do you allow for the possibility that there might be things in the universe that can't be explained by physical phenomena and observation?

Mead: I have a very deep, spiritual life of my own. I don't have an opinion about whether the spiritual nature of the universe is such that it works through things that can't be understood in our sense. It's absolutely clear to me that there are many whole dimensions that we haven't even begun to understand. So one would certainly not need to have a belief that there are manifestations of some spiritual reality that have some kind of—I don't even know how to voice it.

Einstein had a very interesting response to that question when he said, "We not only believe that there is reality in the universe that's there for us to understand, we not only believe that, we put our life in it." And why would you do that? What is it about the universe that would give you the conviction to put your whole life into a thing that it isn't evident that it's there? And to me, that's sort of the nature of a spiritual thing. It's not just that you believe, it's that you put your life in it.

And for me—-it's not different—-is this also a thing which benefits humanity, the world, whatever? To me, the whole evolution process all the way from biological beginnings to the entrepreneurship and that ever-blossoming of human creativity and bringing with it understanding and the ability to create cures for diseases and ability for humans to communicate with each other across the globe and all those things, to me, there isn't a question about whether that's a good thing or not. Evolution is the spiritual reality manifest.

And what is it that's behind that, that continues to manifest these wonderful things that people are all afraid of when they happen? And then it turns out that that famine in the world didn't materialize because of it and all the prophecies of doom have not materialized because of this process that people are afraid of. And I don't have to ask that. To me it just manifests. It's obvious. The law is evident [laughs] Yes.

Zierler: Are you concerned at all that machine learning and artificial intelligence will get to a point that it threatens what makes humans human?

Mead: [Laughs] Yes. They said the same thing about using calculators, you know.

Zierler: So that's a no?

Mead: As if memorizing multiplication tables were higher learning! Now, learning how multiplication works is a fantastic thing. Memorizing multiplication tables as a way of implementing it is crazy. So the same response I had to you are there electromagnetic modes in the vacuum. If it gets you a way of calculating, that's fine, as long as you recognize that's what it is. But don't confuse it with reality. They're really the same question.

Zierler: Carver, it makes me a little sad to say it, but I think I've got my last question for you.

Mead: OK.

Zierler: Given that it's so great to hear you say and reflect over the course of your career, look at all of these prophecies of doom that have not turned out, right? And so for everyone's sake, I hope looking to the future, you can continue saying that for many years to come, right? That we do have the solutions at hand, even during this difficult time that we're currently living through, that at the end of the day, science and technology do offer the solutions that we need and that we will continue to need.

And so with that in mind, I'd like to ask, for my last question, for you personally and for the field of science that you've been a part of for so long, as you look to the future, what are the things that most excite you and give you the greatest cause of optimism, both for what you want to accomplish yourself and for what you see science accomplishing in the years and decades to come?

Mead: Well, to me, it's like I just said. The evolution process is the engine by which the world becomes a better place, we become more elevated as individuals and hopefully as societies. Anything that encourages and enables the freedom of that evolution process is on the evolutionary side—-what people think of as progress. Government controls have stamped out large parts of the creativity of many cultures, and yet somehow down there the individuals find a way to be creative anyway, in spite of all of the controls that get in the way.

I'm extremely excited about what has already happened and is happening. The part that hasn't been seen is much bigger than the part that has been seen, and the liberation of people in more backward cultures by access to the internet is a fantastic thing, that children can find a way to get on and figure stuff out. My colleague Yaser Abu-Mostafa, about 10 years ago now, put his machine learning course up as an online course that people can take, anyone around the globe. It's had millions of graduates from his course over the last 10 years, and they've gone off, many of them, to do amazing things. Many of them are from all parts of the globe.

It was the first virtual course I knew about. I'm sure there have been others, but he really pioneered a lot and continues to do so. And of course, that just kept going stronger than ever during the COVID thing because it was in motion 10 years ago, and so it's been refined. It's a fantastic thing. And that one example, a lot of people are just kind of coping with doing this virtual—he's done it for 10 years and it's been a fantastic enabler for people from everywhere. And that, I think, is just the beginning of what we haven't really seen yet in terms of the population of the most unlikely places becoming the key innovators of the future.

And the reason for hope is that the key innovators for the future have never been the majority. They've been the outliers. The kids that everybody thought were, you know, the ugly duckling phenomena. And they can be anywhere. So, to me, that enlarges the space by orders of magnitude. And you're looking for the needle in the haystack. So you won't see it until it emerges as a major new thing. And just enabling the freedom for young people to imagine being innovative.

I used to read about Benjamin Franklin and Thomas Edison and thought, "Oh, that's what I want to do. I want to figure stuff out and I want to build those things." They were my heroes. And just that, what's going on invisibly now, is fantastic. And we haven't begun to see the outworking of it. And things like that have potential—-and potential always gets used for good and evil. And if you just go back in history and look at what wins, it's the good stuff.

So the fact that we see lots of things going wrong, that's what's on the surface. What's underneath is all of the stuff that you haven't seen yet that's going to cure the diseases and connect people across the world even more closely. And we're just at the beginning of that.

Zierler: Well, Carver, on that note, it's thrilling to hear you say that because we need it right now in a big way. We need hope right now.

Mead: We've always needed it and it has always come through, otherwise I think by those calculations, 90 percent of the world would be starving right now.

Zierler: Yes, that's right. That's right. That's an important point. Let me just conclude then. First of all, I want on record to say it's been an absolute honor and a delight and a privilege to spend all of this time with you, and I want to thank you for so generously doing this because sharing all of your insights over the course of your career is just so important for the historical record, to hear your stories, your insights, your recollections in your own voice and it's been incredibly generous of you to do this and I really can't overstate how important this is going to be for researchers for generations to come to understand your perspective in such an unvarnished and spontaneous way and the stories that you've told have just been interesting and funny and emotional and you've really conveyed, more than anything else, that science is fundamentally a human endeavor.

And that's something that often gets lost in the equation and I hope, in our own small way, we can prove that it's all about the people at the end of the day. So Carver, thank you so much.

Mead: Oh, thank you, David. It's been a real joy.

[End]