Carver Mead Oral History

Interviewed by:
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Doug Fairbairn: I'm Douglas Fairbairn. Today I'm interviewing Dr. Carver Mead, the Gordon and Betty Moore Professor of Engineering and Applied Science Emeritus at California Institute of Technology or Caltech in Pasadena, California. I've actually known Carver for over 30 years. We originally met in connection with his work in VLSI design and design methodology, and we're using this opportunity to look at some of the roots of that, some of Carver's background and in particular the roots of his work in VLSI design and VLSI design methodology and how that important work came to such an important point. In order to set a context for that, we're going to delve in to Carver's early years and growing up in southern California and so forth, but we will not go in to nearly the detail that some others have. I want to point out to the viewers of this tape that there has been an extensive interview of Carver's background and career in most of the important areas that was done by the Chemical Heritage Foundation in 2004 and 2005, and the transcripts of that very extensive set of interviews that spanned three days are available from them. So we are going to focus in this particular interview on a summary of his background and how it eventually led to his work in VLSI design and VLSI design methodologies and something about the impact that has had subsequently. Welcome, Carver. Delighted to have this chance to be able to catch up on some of the details that I have not previously been aware of having known you for all this time but never having dealt in some of the past. I had reviewed this interview with you done with the Chemical Heritage Foundation and found that there are some important points in terms of your childhood and growing up and how you got to Caltech that I thought were interesting and relevant in the context of the contributions you've made subsequently. I'd like to delve briefly back in to those early days and have you describe what it was like growing up. I understand you grew up in the foothills of the Sierra, and why don't you take us back to those days and briefly recount what life was like at that time.

Carver Mead: I was raised in the central Sierra, a little town called Big Creek. It was about 5000 feet elevation. It's located about 30 or 40 miles east of Fresno, and the reason that town was there is there is a string of hydroelectric power plants that were built in 1911, 1912 era which at the beginning of World War II generated about over 70% of the electricity for Los Angeles. So it was at the time a really impressive piece of technology and I was always fascinated by the power plants. It was also a great educational experience. I went to a school that had 20 students in it, one teacher for eight grades. There were-- We had a big class; there were four of us in my graduating class. Two of the members of that school are still close friends of mine, which is quite a nice thing. Both of them went in to technology, one a mechanical engineer, the other one did electronics like I did. It was a great place to grow up. We were ten or 15 miles behind locked gates so there was no public access to the area where we were, and the landscape was pretty much like it was before people had settled there.

I made my spare cash by trapping furs and sending them off to Sears, Roebuck, and I remember getting a dollar apiece for furs. That was a lot of money in those days. It was the only way a kid could make any money back in the woods, and I was always just tremendously fascinated by the power plant. I would beg my dad to give me a tour of the power plant, and finally he'd give in and take me through starting at the bottom where the turbines were and ending up at the top where the high-voltage lines went out. And I never got tired of that and every year he'd do that and I'd learn more. He thought of course that he'd shown it all to me, but of course I was a year older so I could grasp it better. That was a fabulous experience to be so close to such high technology growing up.

Fairbairn: You went through eighth grade behind these locked gates in a very isolated community up in the Sierras and you were introduced to technology through the hydroelectric power plants where your father worked. What was the next step in your educational process and how did that eventually lead to your going to Caltech?

Mead: When I was in seventh grade, a family moved in to the camp where we were. They called the group of houses around the power plant a camp because when they were building a power plant, there were just tents sitting on the side of the hill and later became real houses but it was still called a camp. A family moved in to our camp and one of the members of the family was a girl my age that I liked, and so I used to go over there for a bit and her dad was a ham radio operator. And he took a liking to me and started teaching me about electronics in the form of ham radio, and that got me really excited and by the time I was in eighth grade it was just the period where World War II was ending and a lot of surplus electronics was appearing on the market at just dirt-cheap prices. So even with the little bit of money that a kid could earn I could buy a lot of electronics, and that really got me going on my career as an electronics person. Once I graduated from eighth grade I had a choice. There was a high school that was about halfway between where we lived and the town of Fresno. And there was a high school in Fresno that was much larger and better equipped and had more wide-ranging courses and so forth.

So my grandmother lived in Fresno not far from that high school so I moved away from home at age 14 and lived with my grandma and went to high school in Fresno. It turned out to be a really good experience. I took the trouble to get my ham radio license and in the process I studied up and took the tests for the commercial license which it turned out weren't that much harder than the ham radio license. That qualified me to work on the two-way communication gear and also to work at the radio station locally. So I started out when I was a freshman in high school. I went down to the local two-way repair shop that repaired police radios and radios for fire trucks and construction companies and things. I told them I would work for them for free if they would teach me how to do the stuff. So I worked for about a year and a half there without getting paid, but I learned a lot and finally he started paying me a little bit and that helped. And then a little later I got a job at the radio station and I was going to high school meanwhile and there were a couple of people in the high school that were really interesting. There was a teacher by the name of Hewlett who taught ham radio class and he was a neat guy, and had a biology teacher who was really good, and Mrs. Stubblefield who taught us geometry, and that was the first time I knew what a proof was and I still remember being just blown away that you could actually have a world that was simple enough that you could actually prove what was true. That was one of those great epiphanies that happened in high school. There was a guy that worked with me at the radio station. He and his wife never had any kids and they took a liking to me and I'd go over there for dinner once in a while, and he asked me what I was going to do when I got done with high school. I said, "Well, my dad wants me to go to Fresno State and become a dentist." Well, I really wasn't much into being a dentist. He said, "Well, how do you at mathematics?" I said, "Well, I do well." He said, "Well, you should go down to Caltech and get a degree in electrical engineering." Well, I'd never heard of Caltech and I didn't know there were degrees in electrical engineering but I applied there and I applied at Stanford. I got admitted both places, and my folks were good enough to take me to both places and visit. And I finally decided to go to Caltech and have been there ever since.

Fairbairn: As soon as you were going to Fresno State did you always have other things in mind whether it was Caltech or somewhere else? It seemed like you had led a pretty independent childhood. What was your vision of what you were going to do after high school?

Mead: As I neared graduating from high school, I knew I wanted to do electronics. I didn't know what that meant but I knew I wanted to do it and I wanted to get an education that would allow me to understand it better and to do leading-edge stuff, whatever that meant. So that was my quest and that was what I talked to everybody about that I could meet. There was a turning point in my high school career. I had some very good friends in the ham radio club who were about four years ahead of me. They were majors in physics at the local high school. No. They were majors in physics at Fresno State College and every once in a while they'd have a field trip, and one of the field trips they asked me if I'd like to come with them. It was a field trip to Silicon Valley, which wasn't called Silicon Valley back then, and we visited two companies. We visited Hewlett-Packard and got a tour from Barney Oliver. It was when they were making the model 200 audio oscillators. And we visited EIMAC and watched them make the great, big vacuum tubes. And that trip was just fantastic for me. It really convinced me that there was a career in electronics and whatever I had to do to get there I wanted to do it.

Fairbairn: You chose Caltech. You went to Pasadena and your father presumably wasn't making a ton of money. How did you afford the education going to Caltech or was that a problem? Tell me how that came about and what your early days at Caltech were like.

Mead: We were a poor family and my dad didn't have a lot of money but he had saved up for my college education. Now it wasn't enough to pay for a Caltech education but it was a start. So went down there and I got through the first year okay, and at the beginning of the second year our dean whose name was Foster Strong called me in to his office and said, "You seem to be doing okay, and from the look of your family income it looks like you could use some financial help. We have a scholarship for you," and that was really nice. That helped a lot. Then when I got to be a junior I started helping out teaching courses. I was a teaching assistant in one of the courses and by the time I was a senior I was already inventing lab exercises and lab setups for one of the electronics courses, and that was when I first got the idea that I liked teaching. So I've actually been teaching since I was an undergrad. And when I got in to grad school, which was sort of a fluke because I hadn't been a stellar student, but I did get in and I started teaching right away when I was a first-year grad student and loved it. And I learned a lot more teaching than I did from courses so that was when I - it finally dawned on me that the way you really learn something is to teach it to other people.

Fairbairn: Were there a couple of professors in your undergraduate education who had a significant impact on your later career or at least on your college experience?

Mead: I was fantastically lucky when I got to Caltech. My first year chemistry course was taught by Linus Pauling. Linus was a very colorful character as most people know, but he was also one of the best teachers I'd ever known. There was a guy by the name of Bohnenblust who taught the freshman mathematics and he was a wonderful teacher, had a fantastic freshman year. The sophomore year was terrible. I had a horrible time in sophomore year and the teachers weren't good and the subject matter wasn't of interest to me and I had a really bad time. By junior year I was getting in to stuff that I really liked and so I was doing better. Junior year I had a course in mathematical physics from Dick Feynman and that was just great. He's sort of the epitome of what a good teacher is, well known for that. We later on became collaborators once I got to be a faculty member and worked together actually to very close to the end of his life.

Fairbairn: You mentioned that your entry into graduate school was somewhat of a fluke. Tell me a little bit about that and how that came about.

Mead: Well, I hadn't really thought of going to grad school. It hadn't been on my radar. I thought I'd get my bachelor's degree and go to work for some electronics company and there were job opportunities.

This was after the war and electronics was booming and so there were opportunities, and it was almost an afterthought when I was talking to one of the faculty members, a guy by the name of Hardy Martel who taught the circuits course who was a good guy. And he was explaining to me that I could do more exciting stuff if I got a master's degree than if I quit after a bachelor's degree. So I applied and I got in and I got started on my master's degree. And at one point I was talking to I think it was Hardy Martel again and asked him about this PhD thing. And he explained to me that probably if I was interested in doing this stuff that it wouldn't be a bad thing to do so I applied. I guess I hadn't done well enough that they felt they could admit me but there must have been a certain amount of dissention because they gave me a special little oral exam, and I guess I did well enough on it that they let me in so that's how that happened.

Fairbairn: You were interested in electrical engineering. You were interested in physics. You subsequently spent your career mainly focused around semiconductors in one form or another. You have had forays in to very different fields as well but tell me about how you gravitated toward semiconductors and what some of the early influences were in getting you focused in that area.

Mead: When I was doing ham radio all of the electronics was done with vacuum tubes and they were interesting but sort of big and clunky and clumsy, and I was still in high school when the transistor was invented. And of course it was in all the magazines that I was reading and was fascinating but I didn't understand them; I didn't know what they did. In those days they weren't very good so they weren't really competitive with vacuum tubes performance-wise. But when I got to Caltech, none of the courses were about transistors until I think it was my senior year. We had a guy by the name of Dave Middlebrook who joined the faculty and he taught the transistor course, and it started with transistor physics and ended up with some circuit stuff and I just loved it. It was fantastic! The physics and the things you could do circuit-wise and the fact that those two things weren't really separate was just phenomenal to me, and that was when I decided that was what I was going to do.

Fairbairn: How did that progress? You had this course. Were there other people at Caltech? I think you had said earlier in our previous conversation that Caltech was not on the forefront of technology at that time with relating to this so how did you manage to keep that going in the context of the school where you were?

Mead: Caltech was really a backwater in electrical engineering in those days. It had been closed down during World War II. All the faculty had gone back to the Rad Lab at MIT to work on the war effort so it was just getting back on its feet when I went there and Middlebrook was the first example of someone who was at the leading edge. I was so in love with the transistor technology that I read everything I could about it, and one of the papers that Middlebrook had come across was a paper by John Linville here at Stanford who later became a good friend. It was a thing about how to make a lumped model very much like a circuit model of the flow of minority carriers in a transistor. It was a fantastic simplification of how to think about the physics inside the transistor. Instead of solving these horrible nonlinear equations you had a linear system with two nonlinear elements at the two junctions and it was that partitioning that made it possible to reason about what was going on in very, very clever ways. I discovered that when I was a first-year graduate student and I decided- once I decided to stay for a PhD, I decided I was going to use that model to figure out how transistors worked when you used them as switches because all the books talked about using transistors for linear circuits But a lot of the interesting stuff was using transistors as switches. And there were all these issues of minority carrier storage that limited the speed at which you could do switching and all kinds of complications, and I was just determined I'd get to the bottom of that.

So I adopted John Linville's lumped circuit models and figured out how these transistors worked in- when you used them as switches. In retrospect it's a pretty simplistic thing to do but nobody had done it and nobody at Caltech really understood it by the time I was done. Linville later on became a friend and he liked the work very much, but that was really the first time I had an original idea and carried it through and got it published.

Fairbairn: Was this directly related with your PhD thesis or what did you eventually do your PhD thesis on?

Mead: That work on transistor switching using the lumped models was my PhD thesis.

Fairbairn: You then had this other transition from being a graduate student to being on the faculty and that also seems to have happened sort of seamlessly. As you actually went through graduate school, you were teaching. Is that correct?

Mead: I started teaching when I was an undergrad already and I was always a teaching assistant when I was in grad school. I started out being a teaching assistant in one of the courses. I don't remember which one. I think it was a senior lab course in electronics, and then the second year I actually- they tapped me to actually teach the introductory course on electronics because nobody had taught that course with transistors. It had always been taught with vacuum tubes and they knew I knew something about transistors and liked them, so the year after I had taken Middlebrook's course I was teaching the introductory electronics course using transistors, which hadn't been done before. That was a lot of fun. I had-- I worked my tail off but it was just wonderful, and then in my junior year, our third year of grad school, Middlebrook decided he'd go on leave and he had another book he wanted to work on so they asked me if I would teach the graduate course in transistors, and that was really, really tough. I remember I was trying to do it Middlebrook's way and Middlebrook was a British fellow and he was a wonderful pedagogist. He could just do a class just so beautifully, and I was trying to be Middlebrook and I was falling all over myself and just failing miserably. I remember after class one day I went up in the mountains and took a very long walk and I was standing up on top of Mount Islip that looks out over the Los Angeles Basin. And I finally said to myself, "You know, you're never going to make it trying to be Middlebrook. If you're ever going to do anything, you're going to do it by being Carver." So the next class Monday morning I went in and I taught it my way, which was lots of intuition, lots of experimental stuff, mathematics only when it actually conveyed something that was important, and it started going better. And by the end of the class the students were actually liking it and I got through it. That was actually the toughest time in my whole career was getting through that class.

Fairbairn: You were well on your way. You got your PhD in '58, '59?

Mead: ‘59.

Fairbairn: At the same time you had started to do some consulting, and I think you had started to work with outside companies as well and I think that thread is a very important part of your career and so forth. I'd like to get more into that and how that happened and—

Mead: I was very lucky that I think I was a third-year graduate student, maybe second-year, when some people came over and introduced themselves. They were from what became TRW Semiconductor. At the time it was called Pacific Semiconductors, Inc., and they used a little psi symbol for their trademark, and they invited us to come over. So Middlebrook and I went over one time and visited them. And then they asked me if I'd like to come and consult for them, and I was very pleased about that so I spent quite a lot of time doing my one-day-a-week consulting over at Pacific Semiconductors. That was a really important period because that was the time there was a guy there by the name of Jim Buie, B-u-i-e. And he was the guy that invented what we now call TTL and he was in the middle of just starting that when I knew him, and so I was able to live through the invention of a whole new logic form with the inventor, a wonderful guy, Jim, and very creative, very insightful. He didn't have a PhD, didn't even have a master's degree, but he was one of the deepest thinkers I have worked with. That was a great thing to happen when I was just a starting graduate student, and I worked with them for a while as a consultant and then I think it was 1960 or '61.

I had just become a young faculty member and I was sitting in my office in the basement working away and this guy waltzed in to my office and said, "Hi. I'm Gordon Moore from Fairchild." And I'd heard of Fairchild but I'd never heard of Gordon Moore and I shook his hand and said I was glad to meet him. He had one of these old-fashioned briefcases that opened up like a clam shell at the top. And he looked at me and he said, "I hear you're teaching the lab course in transistor circuits," and I said, "Yes, I am," and he said, "Would you like some transistors?" And I said, "Yes, I would," so he reached in, opened up this clam-shell briefcase, and he pulled out a sock and old undershirt and I was looking a little surprised. And he looked at me with that little grin that Gordon gets and he said, "I travel light." And then he finally pulled out this manila envelope- full-size manila envelope bulging, and he said, "These are 2N697s" and then he reached in and he pulled out another one just as bulging. "These are 2N706s." Oh, my God. I never had seen so many transistors. I didn't know there were that many transistors in the world. So I was in a position now where when a student did a project I could give them the transistors and they could take the project home with the transistors. And these were transistors that would have been 20 or $30 apiece and we were working with these crummy little transistors that sold for a dollar in the stock room and they were really awful. So here we had world-class leading transistors that we could work with in the course and the students could take them home when they were done, and that was just a wonderful thing. So that was my introduction to Gordon Moore.

Fairbairn: That was 19—

Mead: That was 1960 or '61 and I'm not clear exactly—

Fairbairn: They had founded Fairchild in '58, '59. This was the group of eight that left Shockley and founded Fairchild, and so they were obviously up and running and manufacturing silicon and they had some transistors—

Mead: Gordon was one of the founders of Fairchild Semiconductor and they were manufacturing at the time. They invited me to come visit them. They were in a little two-story structure. I remember it as being back up by where the old original Hewlett-Packard had been, back in that funny little industrial area where Circuit City is now or—

Fairbairn: In Palo Alto or—

Mead: --or the big Fry's is there in Palo Alto. I think it's back in that area, and it was a small building and I remember going up there and they wanted me to come up and give a seminar and I did and talked about the stuff I was doing with tunneling and that. And Bob Noyce was there and Vic Grinich and Tom Sah and Gordon Moore and I think Gene Kleiner was there, and I gave a little seminar and we all sat around one table and talked about it afterwards. And they took me out to dinner that evening and asked me if I'd like to become a consultant for Fairchild so that was the beginning of my once-a-week commute to Silicon Valley which has lasted all these years.

END OF TAPE 1 / BEGINNING OF TAPE 2

Fairbairn: Please take a moment or two to reflect on the engineering aspect of your career.

Mead: It was interesting when I was still in grade school that I would trap furs to make money and then I'd go down to the surplus store and spend it on war surplus electronics that was, at that time, the leading edge technology in the field, and it was amazing. I'd get about a dollar for a fur and for a dollar, I could buy an entire IFF receiver, which had probably 10 or 15 vacuum tubes in it and lots and lots of other components. And I could tear that apart and have a stockpile of things that I could use to actually build things that I wanted.

Fairbairn: So you were always a hands-on type of guy. You were always building real stuff – interested in how things worked.

Mead: Since my earliest years, I've learned by doing things with my own hands. I've never been able to do things in the abstract. Different people work differently and that's the way I've always worked. I've never been able to learn in principle. I always had to learn in practice. So I've always been experimentally inclined. I've always taught my courses with laboratories, not just lecture courses. I've never been able to do that. I've always had the students learning by building stuff and measuring it and figuring it out, and I still do that with my teaching.

Fairbairn: How did you get introduced to tunneling and what role did it play in your later understanding of the future scalability of electronics?

Mead: I was either a last year grad student or a first year faculty so it would've been around '58 or '59. A guy by the name of Leo Esaki came from Japan and gave a seminar. He was on a seminar trek through the United States. He talked about this tunnel diode he had invented. It was an interesting device that had a negative resistance characteristic so it could be used as an oscillator or whatever, and very high frequency performance characteristics so it was interesting. It worked by electron tunneling through the forbidden gap of the semiconductor from the P side to the N side of the junction. It was absolutely fascinating to me this quantum phenomenon, electron tunneling, and the fact that it showed itself in such a tangible and useful way. That really got my attention and I determined to work on that phenomenon. So I did a lot of work on tunneling over the next 10 years. In fact, that was pretty much what I did for 10 years. Tunneling through very thin layers of insulators of various sorts and tunneling through junctions between metals and semiconductors, lots of different context for that.

It was just fascinating the gyrations I had to go through to figure out for myself what it really all meant and what it was trying to tell me. The theories of the day were just mazes of chicken tracks. It was just impenetrable stuff and it was a lot more heat than light. So I had to figure it out my own way and it was great because I did that by doing a lot of experiments and actually figuring out-- The thing about tunneling that's neat is an electron has a wave function. It's a wave that propagates along and when it comes to some kind of barrier -- in this case, it was the forbidden gap of the semiconductor -- then it goes from being a wave to having an imaginary wave vector. That's just another way of saying it damps out exponentially with distance through the semiconductor and then it emerges on the other side. So the current that goes through is a very small fraction of a current that could go through if there weren't a barrier there. That amount that gets through goes down exponentially with the width of the barrier, so if you make the barrier wider, the current goes down exponentially. That's a fascinating thing. You can measure that. You can make barriers of different widths and you can measure the current, which I did and worked it all out. It's amazing how much you can learn about what goes on in a classically forbidden region of an insulator or a semiconductor by doing these exponential kind of experiments. So that was what I was in the middle of.

Fairbairn: You spent years investigating that phenomenon.

Mead: The study of tunneling occupied 10 or 15 years of my life and that was coupled with work I was doing with Bill Spitzer on the nature of the barriers between metals and semiconductors. Those two studies are not separable because most of the tunneling you do is from a metal into a semiconductor; at least most of it that we did was like that. So you have to know about the barrier between the metal and the semiconductor to do it. So that was work I did with Bill Spitzer and Bill was my first real close collaborator and that was a collaboration that lasted a long time and a friendship that became very deep and has lasted all these years. That was a big part of the way I ended up viewing how the electrons behaved, came from that tunneling experience.

Fairbairn: This must've been throughout the '60s you were working on this.

Mead: That's right.

Fairbairn: Let's go back to Gordon Moore, who was a graduate of Caltech himself, right?

Mead: Gordon graduated I think five years before I did and he was a chemist. There's a lot of Gordon Moore stories. I'm sure you have interviews with Gordon Moore. There was also a wonderful program that I did interviewing Gordon Moore here at the museum which was great fun. I like Gordon a lot. He's been a friend all these years. We worked together all these years. It's been pretty remarkable.

Fairbairn: He was looking for new recruits after establishing Intel.

Mead: In the ‘60s, there was a very congenial relationship between the universities and industry. It was before this big bifurcation took place that happened in the ‘70s. So it was very common for people from industrial labs to come to the university. They would often give talks. They’d go around and visit faculty. They’d get to know the students. They would help out with the labs like Gordon did. We had that kind of relationship with a lot of companies. General Electric was one. A guy by the name of Bob Hall used to come out and give seminar every year. Bob was the guy that invented the semiconductor laser. So we were exposed to people from these-- Bell Labs people would come and we got to meet these people and they’d give talks and we’d go there and give talks. It was a much more friendly relation than it became in the ‘70s and ’80s.

Fairbairn: Your consulting work with Fairchild continued until several of them left to form Intel?

Mead: I was working with the Fairchild people off and on through the entire period until I think it was ’68 maybe, when Bob and Gordon left Fairchild. They hadn’t decided to form Intel yet but they had a falling out with the mother organization at Fairchild. I never did learn all of the details of that. I wasn’t very interested in why, but they left at the same time. Gordon and I had a lot of discussion during that period about what they might be doing. So when they started Intel, I was there and was consulting with them just informally as it started up. That was a really neat experience to watch the genesis of a semiconductor company from scratch. I had never been through that process before and I got to know Art Rock and watch the whole venture capital process work. That was all a really nice experience.

Fairbairn: Tell me about how the work with Bob Noyce and the integrated circuit became visible to you and when you started thinking about not just transistors, but integrated circuits.

Mead: It was very interesting the way we thought about things in that period. I was all caught up in the physics of the transistors and making better transistors. In ’65 when Gordon was inventing his law, I was off inventing the Schottky barrier gate field effect transistor, which became the MESFET and is now called the HEMT. I wasn’t really focusing on how to interconnect transistors to make a circuit. I was deep into the physics of how the semiconductor worked and the tunneling and that sort of thing. I only vaguely became aware of the integrated circuit evolution through just being there at Fairchild and interacting with Gordon Moore. It was really in the ‘60s somewhere, I would guess ’67 or so, when I stopped into Gordon’s office. Gordon was a morning person and I was always a morning person so I would get to my consulting day at Fairchild at 7:30 or something in the morning and Gordon would be there. So we’d have an hour before other people started showing up when we could talk. Then I’d go out and spend the day in the lab and check in with all the stuff that was going on and then I’d come back at 5:00 or 5:30, and Gordon and I would sit and have another hour together sort of decompressing all that. So the consulting day in Silicon Valley at Fairchild started out with time with Gordon Moore and ended up with time with Gordon Moore. That was some of the most precious times I remember because both of us would get our thinking about whatever it was straight by talking with each other.

It was in that period when I met Andy Grove. He was still a grad student over in Berkeley and he was teaching a course, both at Berkeley and at Fairchild R&D lab. In those days, the R&D lab ran a course for people who were coming in, to teach them what a transistor was, how the physics worked, how you processed it, how you actually made a transistor. Everybody got to go and make their own mask and make their own transistors and it was a great little course. So Andy and I would compare notes. Andy was writing his book and he was very critical of the way I was teaching the course down at Caltech because it wasn't as advanced as what he was doing. That was true and so I thought "Great, I'll use your notes." So Andy made a preprint of his book, the first book, The Physics and Technology of Semiconductor Devices I think it's called, and he provided me with enough for my whole class. So that year, the class got Andy's version, which was better than what I was teaching and we used to have great discussions. I've been told that in Hungarian, the word for "discussion" and the word for "argument" are the same word. Andy had an admin once he got fulltime at Fairchild R&D. He had an admin by the name of Adrian Giuliano and she was a very spunky lady. One time, after I'd had my weekly discussion with Andy, she said "I can always tell when you guys have a good discussion by the number of four letter words that come out from under the door." So that was a fun time, not only starting the day with Gordon but then having the weekly tussle with Andy, it was just great.

Fairbairn: Tell me about Moore's Law and the scalability of integrated circuits and how that linked in with your work later on.

Mead: The origin of Gordon's work on the number of transistors versus time has been well documented. I wasn't very much into that in the earliest times because I was so deep into the physics. But one time in my early morning discussion with Gordon, I remember him asking me, I think it was around 1967, and he said "You've been working on electron tunneling," and I said "Yeah." He said "That's something that happens when the dimensions get really small, isn't it?" I said "Yeah." He said "Would that limit how small a transistor you could make?" I said "It certainly will." And he said "How small is that?" and he absolutely stumped me. I remember it like it was yesterday. I said "I'm going to have to go work it out, Gordon." I spent the next several years working on that problem. It turned out not as simple as I had imagined. But the first realization that came to me, I was preparing to give a talk-- there was an IEEE workshop in Lake of the Ozarks in 1968 and I'd been asked to give one of the talks. I had decided I would talk about this question that Gordon had asked and the more I worked on it, I couldn't believe my results, because the more I worked on it, there was only one way that you could scale. You had to scale the thing laterally, you had to scale it vertically and you had to scale the voltage down to keep the electric fields about constant. And if you did that, everything got better. The transistors ran faster. They ran cooler. They took less power. Everything got better. Not only did the individual transistor take less power, but the power per unit area stayed constant while everything was getting more complex and much faster. The more I worked on it, the more I thought this is way too good to be true. This is a violation of Murphy's Law that just won't quit. So I gave the talk at the Lake of the Ozarks meeting and everybody was all over me. This couldn't possibly be right. Everybody knew that things were-- the end of the earth was near. It was very interesting then. I went from then into this basically a crusade to convince people that you could scale devices to smaller dimensions and I would always show a picture of Gordon Moore's plot and say this is going to keep going up to where you have millions of devices because you can make these things a very small fraction of a micron and dimension the number you came up with. I had a grad student at the time by the name of Bruce Honeisen [ph?] who did his thesis on this subject. We could see our way very clear to getting to 0.15 micron. Then after that, it got murky because the tunneling was starting already then and we didn't know exactly where it would go from there. That turned out to be about right. The .13 micron technology was just about the time when all of the substrates currents started going up, the gate currents started limiting, and that was a time when we started to see just the effects that we had talked about 30 years earlier.

Fairbairn: When Gordon came up with this law, he didn't have any idea about the scaling?

Mead: Gordon thought of Moore's Law as an economic thing, that as you made more complex devices, you were getting more value and that would drive the cost down. And he's, of course, been proven right over a vast span of time and scaling. But he hadn't worked on the scaling part and there was a lot of resistance to what Gordon was saying that came from people's disbelief that you can make devices ever smaller. So in a way, Gordon and I were working on the complementary parts of the problem. He was working on convincing people that the economics drove you to ever more complex circuits, and I was working on convincing people that the physics allowed you to make devices that were ever smaller, which would allow Gordon's thing to keep going.

Fairbairn: What were the roadblocks that other people saw that were going to limit the scaling?

Mead: The literature of the day was full of arguments about why a device would fail when you made it smaller. The first one I remember said that if you got less than about 10 microns, the cosmic rays would get you. Then there was the IBM argument that if you made things smaller, the power density would go up until the chip would melt. And there were a whole bunch more like that. I don't remember them all but it was widespread belief that you just couldn't go much further. So that was an impediment to people putting real time and energy into better fabrication techniques in equipment, which were necessary to get the finer line widths and that has, of course, continued to this day. The turning point really was the SIA roadmap and I forget what year that was, but that was the first time that scaling was explicitly center stage for the whole industry. After that, we didn't have to worry about it.

Fairbairn: At some point, you seemed to have changed your focus from tunneling to starting to think about how would people design with millions of transistors on a chip.

Mead: My personal transition from device physics to how do you design these complex things came when I realized how the scaling worked. I was trying to imagine designing something that had a million moving parts and it was just incomprehensible how you would do that. That was a period where Intel was designing things by drawing them on Mylar and then they would put a Rubylith down on top of the Mylar and carefully arrange it, and take a thing called a coordinograph, which allowed you to draw very good straight lines at very precise locations, and they would trace over the lines that had been drawn on the Mylar one layer with a razor blade. And then they would peel out the red layer on the Rubylith, leaving white area where the trace was supposed to be. So that was the way design and masks were done at Intel in the beginning, and everywhere. I used to go visit there and I can't imagine getting a million transistor thing to work by peeling out all those little pieces of Rubylith.

The biggest problem with mask sets was that somebody wouldn't get one of the little pieces that had been cut out -- you had to do that with tweezers -- and for the contact layer in particular. They're just little squares you had to pick out and you'd miss one or two. And then something would be floating and you'd have to go find it. And sure enough, there wasn't a contact where there should be and you had to go back and pick it out of the Rubylith and then make a new mask and then make a new run. That was the major source of errors in the beginning. I knew I could never get that right. So that was an interesting period. It was in the late '60s, '69 maybe and I was teaching a logic course for freshmen. I had one freshman, a guy by the name of Steve Collie, a just extremely bright, delightful young fellow, and I used to talk to him. He came around and wanted to know if there was a project and I said "Boy, have I got a project for you." At that time, I had this idea that you could do logic not by doing logic gates and people had routing programs and it was just a nightmare. So I had invented this thing that had all of the -and functions on one side and all the -or functions on the other side, a thing that we later called the PLA, Programmable Logic Array. I had come up with this thing and it seemed to me this was just a really good thing. It turned out that two other people had come up with the thing simultaneously. We all didn't know about the other. It was quite interesting. It was a guy down at Mostek, I don't remember his name, and there was a guy at Hewlett Packard in Colorado I think who had come up with the same thing, and myself at about the same time. I remember Texas Instruments actually marketed one for a while, that didn't go anywhere to speak of. But I got together with this student and I said "Look, what we want to do is we want to simulate what would happen with this thing." So I taught him how the thing worked and he made a FORTRAN program where we'd plug in a card deck that had the contacts that were in the -and plane and the contacts that were in the -or plane. And it would do the logic and would go through what the state was at every clock cycle. It was wonderful because you could debug your PLA code that way. Then I found an outfit that had a little program that was called PAL, Precision Artwork Language, and it was just a macro assembler. I think you've seen it, Doug.

Fairbairn: I used it, I think.

Mead: And it wasn't done quite right but it was what I could get my hands on. What it did is it allowed me to write a program that would generate artwork for a Gerber plotter. The Gerber plotters were in use to make circuit boards so they would make very complex patterns and the patterns had all the right things. They had wires and they had flashes, round circles, and they had contacts which were the through holes of the circuit board. A pattern on a circuit board didn't look very different than a pattern on an integrated circuit. So if I could program a Gerber plotter, I could get artwork to happen without having to peel Rubylith and without having all those errors because once I got the program right and I could figure out if the program was right by this simulation that Steve Collie had done. So that was the first silicon compiler, was this little complier for making PLA state machines which would generate the simulation and then generate the artwork from the same input. It was '70 or '71 and the reason I know that is that I had a set of Gerber plots that I took to Germany with me in '71 for the summer to work on it while I was over there as a visitor. I came back and I was able to debug my program and actually get my first chip to work in '71. So then Dick Pashley who later on ended up at Intel came around and started bugging me to teach a course on -- I forget how he thought of it but it ended up being a first VLSI course, which was fall of '71 and we had-- I think I gave you a picture of the class.

Fairbairn: The museum has this material and the picture of the class.

Mead: I think there were maybe six students in that class, or eight. They each did a project. They all did shift registers of different forms. They had to design their own and all coded them up. We got them all to go on one die so that was the first multi-project thing. It was a pathetic little thing but it worked. We used to sneak them through the Intel fab. Ted Jenkins, who had been a Caltech student and had gone to work for Intel was in charge of the Livermore fab. So I used to go and take Ted and his people out to dinner over there and then leave the mask set with them. They would run it through the line over there as an engineering run and about a week or two later, I'd go back and take them out to dinner again and pick up my wafers. That was a really good time.

Fairbairn: You actually built integrated circuits on a commercial line to prove your concepts had actual validity.

Mead: It was very important for me personally to actually realize working chips. I actually didn't believe deep down that the whole thing would work until I had made my first chip work and that was actually a fun story. I had made my first design of this PLA state machine and it was programmed up to be a digital clock, so it was something that would be fun. It would actually drive the little seven segment fluorescent tube display because at that time, the Intel process was a 20-some odd volt process which was just about right to run those little tubes. It was plus-12, minus-5, so that would've been 17 volts or something. So it was a great way to run those little tubes. So I'd program this thing up and run it through the Intel line and took it back. I had a bonder by then, so I went in for the weekend and bonded up my chips. I bonded up I think six of them and had a setup on my desk. I had a bench in my office so I had a little setup there, all set up to test it. So I plugged the thing in and all the rule lines were high and all the column lines were low. Ooh, that was a big disappointment. So I thought it must've been a bad chip. I went through all six of them and they all acted exactly the same way. I had spent the whole day bonding. I never got super good at bonding so it was laborious for me to bond all that stuff, then going through all these disappointments one after another. I was feeling really low. This was a big thing for me because I'd sort of committed the next part of my career to doing this and here it didn't work. So I thought "Carver, you've learned that when you're feeling this way, you should get something to eat," because I hadn't eaten anything all day and I was low on blood sugar and I was feeling really bad. So I thought I'll go get something to eat and then I'll decide what to do. So I went out the door of my office, flipped off the light and just as the door was closing, out of the corner of my eye, I caught a trace on a scope. So I went back in, turned the light on and the trace went away. Oops. Dynamic logic. Should've thought of that. So I took a penny out of my pocket, put it on top of the chip, and it worked fine. So I had a much better dinner that night, much better dinner that night. First chip worked fine.

END OF TAPE 2 / BEGINNING OF TAPE 3

Fairbairn: This is 1971. Were you completely focused on the issues relating to design, as opposed to what you'd spent the previous 10 or 15 years on, in terms of device physics, or were you still bridging those technology domains?

Mead: It's my memory that when I was in Germany, I actually made the decision that I was going to go spend full time doing this VLSI stuff. So, from then on, especially after the chip worked, and then Pashley talked me into teaching the class, and from then on, basically, I was full-bore doing the VLSI stuff. And then Ivan came and visited us in, I think, '75.

Fairbairn: This is Ivan Sutherland?

Mead: Yes. And he really liked the technology, and so Caltech hired him to work with me to start a computer science department.

Fairbairn: Caltech didn't have any computer science technology classes, professors, whatever, at that time, going back to '71?

Mead: We had some early history of a guy who had built the first minicomputer, which ended up commercially as the LGP-30, built by Librascope. There's one out here in the Museum. And it was a brilliant architecture, but he'd left and nobody really followed on. The big effort at Caltech at that time had been the analog computer, which was used for flutter analysis for airframes, and there was a lot of connection with a Southern California airframe manufacturers. So that was their big effort. They had *a* digital computer. There was very little real work. There was a guy that was working on natural languages. There was a little bit, but not really much computer science. And our dean at the time, a guy by the name of Bob Cannon, from Stanford, had decided that this was something that couldn't continue, so he initiated a process of trying to figure out if we should do computer science, and if so, how. And we had a lot of really quite illustrious people. We had Al Newell and Gordon Bell and Marvin Minsky and Al Perlis, and just a lot of really neat people come by and talk to us about their views on all that. And one of the people that came by was Ivan, and Ivan really got jazzed about the integrated circuit stuff. Ivan really likes new, jazzy things, and this was a real new, jazzy thing, and he got very excited about it and decided he'd join us, and the two of us started computer science, and we actually started in '75, but we didn't admit anybody, I think, until '76. I think that's the way it worked, if I remember right. And then we didn't have a faculty, of course, so we had to talk people into coming down and offering courses. So we got Bob Sproull and Alan Kay, and you, Doug Fairbairn, and any number of people to come help us out with that, which ended up being a great experience, because we got to know a lot of people with a lot of different backgrounds and viewpoints and that sort of thing. So that was a very exciting but very exhausting period, too.

Fairbairn: This was a fortuitous linkage. Ivan Sutherland joined Caltech in '75, you say, and his brother Bert Sutherland was a laboratory manager at Xerox Palo Alto Research Center [Xerox PARC] at about the same time. The people you referred to-- Alan Kay, Bob Sproull, and so forth-- were some of the lead researchers at Xerox PARC, and sort of started a collaboration between the two organizations. In the spring of '76, you came up to Xerox and gave a three-day version of your one-semester, one-quarter course in VLSI design, and got a number of people, myself and Lynn Conway in particular, excited about this wonderful new world of doing integrated circuit design. Is that your recollection of how those things came to be?

Mead: It was very early in the formation of the computer science group at Caltech that Ivan started bringing people by that he knew, and the two most memorable groups that I remember, the first one was Dave Evans, with whom Ivan had started Evans & Sutherland in Salt Lake City. And Dave came by at Ivan's invitation and spent a day with us, and I spent a lot of time trying to convince him that doing a graphics processing pipeline in VLSI would be a really smart thing to do. Well, he was convinced enough that he got a lot of his top people, like half a dozen of them, to come out, and I gave them a little one-day course or two-day course-- I don't remember which right now-- at Caltech, on how you do this stuff, and also put in a lot of plugs for why graphics would be a very, very good thing to do in VLSI because it's highly pipelinable. And that's what you could do with VLSI better than you could do any other way, is just to pipeline stuff through, and we had a lot of examples by then of doing that. I really thought they would go off and do that, but they didn't. They left it for Jim Clark to do, which I guess, as you pointed out earlier, is why we're in this building.

Fairbairn: Right. Jim Clark took the technology and created chips that became the basis for Silicon Graphics, and Silicon Graphics became the creator of this building in which we currently sit doing this interview, and their later fall and demise provided an opportunity for the Computer History Museum to occupy the space which they had created. So there is an interesting circle to be completed there. I think we've sidestepped one important piece here, and my observation was that there'd been some collaboration and research done among yourselves-- Ivan and others-- trying to put more substance on this VLSI design problem, which you had started to internalize back in the late sixties. I remember seeing a paper that you and Ivan and Tom Everhart had published, having to do with some of the challenges of VLSI design, and I think Ivan was focused on wiring as the major challenge. What's your recollection of that piece of the puzzle?

Mead: Well, it was very clear very early on that the wiring was the problem in a VLSI design, and you sort of hid the transistors under the wire. I mean, that's what the PLA did. It was all wire, and you just put transistors in where you wanted them, at the intersections. So that was the key to making efficient VLSI designs, was to get the wiring such that it didn't become a rat's nest, and that it was also the thing that cost you all the performance, is charging up the wires and discharging them-- still, to this day, is the problem.

Fairbairn: Still?

Mead: Yeah.

Fairbairn: Physics remains completely intact.

Mead: Physics hasn't changed. Yes. It has not gone away. So that was a thing that was very clear from the earliest days. During that period, nobody wanted to hear about this stuff, so when Ivan came, he was the first entrée we had to any public awareness of all this stuff that was going on. I mean, I hadn't published anything. There wasn't any place to publish it. You know, it wasn't that sort of thing. So there were a couple of early papers. Ivan took the lead in generating public awareness of this sort of way of thinking about things, and that was absolutely instrumental in getting people thinking about it, getting some receptivity out there to the fact that there was a thing to be done here that hadn't really just all been done by industry. Like, the standard lore is, "Oh, it's all done by industry, so you don't do that." And it was clear it hadn't been, and wouldn't be, unless we got the foundation laid for that. So that was a big boost that Ivan gave all of us, was his own interest and enthusiasm and energy and smarts, in practical ways, too. I mean, he'd done graphics, so he knew, for example, how to do the right transformations and things to make the layout work out right, which this little PAL program that I mentioned didn't get right. So that was a huge contribution to just get us into the modern world, and the introductions I mentioned to people, his brother Burt being one, that he brought down and we had a day with, and then Burt invited me to come to Xerox PARC, which is where I met you, Doug, and Lynn Conway, and then lots of other people-- Bob Sproull-- a lot of people that were instrumental in getting a lot of this to happen.

Fairbairn: So this was a critical period in terms of taking some of these ideas which you had realized and seen the physical limitations, had started to realize the design and design tool limitations, and sort of enabled an explosion of activity built around the ideas which you had been working on and teaching courses around during the first half of the seventies. What's your memory of your role in how that collaboration evolved, and where did you take it after that?

Mead: Well, once Ivan was driving the whole thing, a lot of things started to happen. He got a connection with not only PARC, but with DEC. He got Gordon Bell all interested in what was going on. He actually went to Intel and got them more actively involved in the collaboration with Caltech, and it wasn't just a friendship between me and Gordon anymore. It was a more formal relationship. He got a number of other companies, Xerox being one. But for me, personally, the connection at PARC was by far the most important. The little course you mentioned was well received at PARC, at least by you and Lynn and a few other people, Bob Sproull being one, and that was, at the time, an extremely influential institution. It was looked at as sort of the leading edge of where computer science was headed, and where computer technology was headed. And so having the VLSI stuff embraced by Xerox PARC was a big thing because it leant some credibility from the computer science community, which is by and large standing back from all this up to that point. So that was extremely important. And then there were the practical things that happened: you and Bob and Alan Kay coming down and teaching courses. And then, of course, out of that sprang my friendship with Lynn Conway, and then that turned into a resolve to coauthor a book on the subject at her-- well, she wanted me to write a book, and I told her that it would work a lot better if we coauthored it, so that turned into, then, a collaborative partnership in getting that book written, which it wouldn't have happened if Lynn hadn't, you know, had that impetus. So it really was the turning point in getting this thing out of the closet and out into the world at large.

Fairbairn: This book was coauthored by yourself and Lynn, with lots of contribution from others, as well, and it became a mechanism by which these ideas could be spread to a much wider audience around the world, and with surprising speed. When people talk about the Mead-Conway VLSI design methodology, different people take away different meanings from that phrase. What were the critical things about that "methodology," or the things that happened at the time? What were the most important elements-- things that were different, or that caused so much change to happen, or that was unique?

Mead: That period was one where... earlier times, the integrated circuits were created by "wizards." They were people who had extremely deep knowledge of the process and of the transistor physics, and it was a very mysterious process by which an integrated circuit was created. And I think the book and the methodology that came with it really demystified a lot of that. Just how a two-dimensional pattern created a circuit was made very clear, but that wasn't an obvious thing to people back then. The whole notion of, "Where did circuit performance come from?" The little tau model was a simplification, but without losing the essence of where the time went in an integrated circuit, where they're making performance. We had a simple set of design rules. Turns out design rules in those days were proprietary. Nobody would let you see their design rules. So although they were all pretty much the same, nobody would let you see them, so nobody knew-- what would you draw if you were going to draw an integrated circuit? I mean, you have to have a set of design rules before you could do that. So there was just a lot of demystification that happened as a part of getting that thing down. And I think another important thing that happened about the same time is that it was Ivan's inspiration, really, that we should start a silicon foundry to be available to the universities that were teaching these courses, and he was the one that convinced ARPA to sponsor the thing which later became called MOSIS. M-O-S Integrated Circuit-- no. M-O-S Implementation System. MOSIS, yeah. And it was done out at ISI, which was a sort of a housekeeping organization for ARPA, in a way. And as far as I know, they're still doing it.

Fairbairn: I believe that's true.

Mead: It made available to all the major universities at the time who were teaching courses-- it made fab available, so all the students could get the experience of actually designing a circuit, getting it fabbed, measuring it in the lab, and that was a huge thing. Just having fab available-- I had to kind of sneak it around through people's fabs, and sometimes they would do it, and sometimes they wouldn't, and it was very difficult. And having that be a very clean interface which was supported and acknowledged, that made a huge difference. And then soon after that, NSF kicked in some money to make it available to all universities, not just the ones that were DARPA supported, and that made a huge difference. So I think those were a little later, but they're part of that picture, as well. The... <pause> At some point, I remember-- I forget if you, Doug, were part of that discussion early on or not, but I remember Bob Sproull and Ivan and I trying to get the GDSII format that we could use as an intermediate form for our work, because we needed something, some kind of output. We didn't going to use Gerber plots anymore. By then, there were better pattern generators for making masks. And nobody would-- we tried to get Calma to give us the-- they wouldn't. That was proprietary. So there was this effort in creating the Caltech Intermediate Form-- CIF that we called it-- and I know you were involved in that somehow. And that is something we never should have had to do, but we had to do it because nobody would let go of their proprietary thing. Well, once we did it, then of course Calma decided that GDSII wasn't so proprietary afterwards, and now everybody uses it. But it was all of this stuff we had to do just to get it broken loose. And the whole notion that Lynn came up with of the lambda unit for design rules was a very important thing, because it had the notion of scaling built into it. It wasn't a fixed dimension. It was a unit that you multiplied everything by whatever the scale was to get it. That was a very nice insight to--

Fairbairn: Yes, you're right. That really encapsulated scaling in the whole process.

Mead: Just in the way you did it. And that was a very nice insight. So there are a bunch of things like that that were all by way of demystification and simplification, without losing the essence of the whole technology.

Fairbairn: And the result of that was an involvement in the challenges of design and tools by a much, much wider audience than would otherwise have been brought to bear on the problem.

Mead: Yes. Basically, the entire computer science community participated one way or another in VLSI, all the way from the complexity theory that Dick Karp did a wonderful set of things there. We did a lot of work on energy, and how you minimized energy for per-unit computation. There was a huge amount of work on design tools and design methodology that ended up with any number of companies springing up to do various things there, and that sort of took on a life of its own. I guess it's mostly been gobbled up by the big ones now, but at the time, there was a huge amount of innovation in different approaches to that whole set of problems, I think all of which, or many of which, made lasting contributions to what's done today. And that would never have happened if the thing hadn't gotten demystified and simplified to the point where you could work on the essence of it rather than all the details.

Fairbairn: So you collaborated with Lynn on this book. There was an explosion of activity. Then what happened to Carver? You continued teaching courses, but my perception is that that was no longer an area that you put your primary focus on.

Mead: No, no, I think that's right. Yeah. Yeah. As the greater community took on the whole VLSI design methodology and tools and architectures and all that, my own focus was on, "Well, what are you going to build?" And it was very clear from the work we had done on energy consumption that you always win, energy-wise, by having more small things running in parallel than in having one big thing, because with the big thing you're just pumping the wires up and down all the time. Like with a memory, you pump all the wires in X and all the wires in Y, and you get out one bit. I mean, it's the silliest thing in the world. But at the time, that's where everybody was, and, by the way, it didn't change till very recently. But it seemed to me it was just very obvious that in order to make the maximum use of this technology, we had to have a lot of small things running in parallel. And in order to do that, we had to figure out how you'd get a whole bunch of things working in parallel. And there were special-purpose algorithms, like some guys came up with a thing called "systolic arrays." That was a really neat-- I'm trying to think of their names-- really neat thing.

Fairbairn: I remember the technology.

Mead: It was basically a two-dimensional pipeline thing, which was really nice. So the people were being very creative about all this stuff, and I was struggling with how you would do a more general-purpose organization of a lot of parallel things. And we tried trees of interconnect, which had certain amount of promise, because it was a "log n" thing instead of an "n" thing, and that was helpful, but we could never get really anybody interested in all that. And the more I thought about it, the more I thought that the only existence proof we have for a thing that works with huge numbers of things in parallel is the brains of animals. So at that point, John Hopfield had been working on neural networks, and he came to Caltech, and Dick Feynman had been thinking about computational things, and the three of us decided to start a course about physics and computation in biology, and parallelism, and that sort of stuff, which is not very well specified, but it wasn't well specified. So we took turns lecturing to this poor group of students who had absolutely no idea what the topic was, even. And they got three very different viewpoints on what computation was, and where it was headed, and how it could be better, and that was basically the thing that started me off in a new direction, which was thinking about how computation gets done in brains, and if there's something we can do to learn from that and make computational structures that are massively, massively parallel in a way that's very different from standard digital computers. So that's when I took off in that direction, and basically haven't done much digital design since then.

Fairbairn: You've had this career spanning a huge range, from quantum mechanics to VLSI design to neural networks and brains and biology, and I know you've done further work in imaging and so forth. I know you're still pushing ahead, but what are the things that you feel best about, in terms of the contribution you've made?

Mead: Well, I think in terms of what impact it's had on the world, the scaling stuff is by far the most important. In terms of a fundamental contribution, the collective electrodynamics stuff, the quantum foundations of electromagnetism, is much more deep and lasting than any of the rest of that, and I'm still working on that.

Fairbairn: And that's work that continues?

Mead: Absolutely. I am working harder than ever on that topic as we speak. Yeah.

Fairbairn: That's your biggest personal challenge and personal interest at this point, and over a long period of time?

Mead: Yes. Yes. I've been doing the electrodynamics stuff since 1960. It was a background thing most of that time, but it's been worked on that whole period.

Fairbairn: Interesting.

Mead: Finally got published in 1999 as a little monograph, and we're right now working on trying to make that fleshed out so it could be a real course for introductory students.

Fairbairn: And still with an experimental basis?

Mead: Oh, absolutely. I don't know how to teach a course without a lab that goes with it. I couldn't imagine learning the stuff myself without doing real experiments, so any course I have anything to do with, it's going to have real hands-on experiments. You bet.

Fairbairn: Is there any area, in terms of this VLSI design work, that you think we haven't delved into, or some important contributions, origins, or linkages that we might not have touched upon, in terms of getting a complete understanding of that topic, which was the goal that I set out at the beginning of the interview?

Mead: One of the important themes that go through here is the extent to which students make some of the most important contributions. And along that line, I mentioned Steve Collie, but I haven't mentioned Dave Johannsen, and Dave was probably the most pivotal figure in the whole development of silicon compilation, and did a vast amount of the work there, and had a huge number of the insights, and was just an amazing figure in that era in developing that technology.

Fairbairn: He was the lead on the design of what you called the OM, or "Our Machine", and how it was used as a touchstone of your work in VLSI design.

Mead: Well, when Ivan came and we got the department started, he thought we should have a department project. That's kind of foreign to the way Caltech works, but it served to bring everybody together and get everybody thinking on the same page, and he called it OM, "Our Machine." And the idea was to make a chip set, and to make the software that ran on it, the operating system, everything, so that people can see where all this came from and could see how the architecture interacted with the operating system and all that. And of course we never got finished with that project. I mean, it was vastly larger than anything you could do with a few students, but it was a great way to galvanize everybody's interest. And yeah, Dave was the principal guy on the design of the silicon part, and many of the other people chipped in in all kinds of ways that were-- well, it was just wonderful. I mean, it was a great period.

Fairbairn: Does the VLSI course and VLSI design work continue at Caltech in 2009?

Mead: I don't know what Caltech's doing about courses in that area right now. I haven't looked.

Fairbairn: Just to wrap things up, your current interests are in pursuing the work that you just talked about, in terms of electrodynamics and so forth, and that's where your major research work is being done right now. Is that correct?

Mead: That's right. That's where all my energy's going.

Fairbairn: Well, I very much appreciate the time. It's been insightful for me, even though I lived through a good part of that. There's a number of pieces which I wasn't familiar with, and I think the linkages back to your early days was especially important, in terms of people's understanding of how these inventions, insights, creations come to pass, so I really appreciate the time that you've given to us today. Thank you.

Mead: It's been great fun, Doug.

Fairbairn: Thanks.

END OF INTERVIEW

