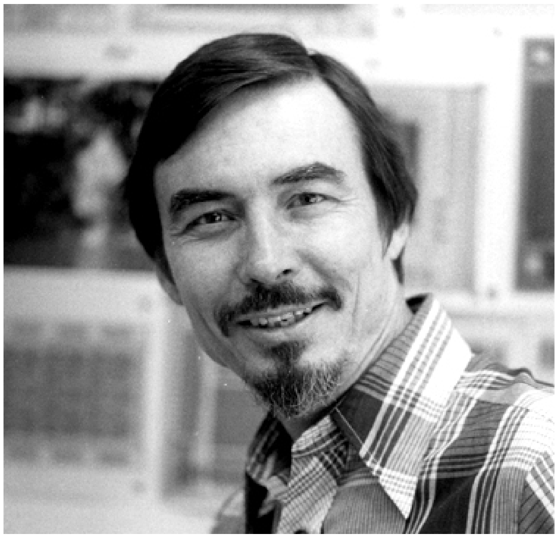

# CARVER A. MEAD
**(1934– )**

**INTERVIEWED BY**
**SHIRLEY K. COHEN**

**July 17, 1996**

**ARCHIVES**
**CALIFORNIA INSTITUTE OF TECHNOLOGY**
**Pasadena, California**

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## Subject area

Engineering, electrical engineering, computer science

## Abstract

An interview in July 1996 with Carver Andress Mead, Gordon and Betty Moore Professor of Engineering and Applied Science (as of 1999, Moore Professor emeritus). Dr. Mead received his undergraduate and graduate education at Caltech (BS, 1956; MS, 1957; PhD, 1960). He joined the Caltech faculty in 1958, becoming a full professor in 1967.

In this interview, he recalls growing up in the mountains east of Fresno, father's work for the Southern California Edison Company; early education in a one-room schoolhouse, then high school in Fresno. Early interest in electronics. Enters Caltech in 1952. Freshman courses with Linus Pauling, Richard Feynman, Frederic Bohnenblust; junior year focuses on electrical engineering. Stays on for a master's degree with the encouragement of Hardy C. Martel. PhD student with R. David Middlebrook and Robert V. Langmuir. Work on electron tunneling; grants from the Office of Naval Research and General Electric.

Helps establish applied physics in the 1960s with Amnon Yariv and Charles Wilts. Discusses his friendship with Gordon Moore and work on design of semiconductors. Discusses the establishment of a computer science department at Caltech in the mid-1970s and the arrival of Ivan Sutherland: the

Silicon Structures Project. Departure of Sutherland in 1978 and decline of computer science under Pres. Marvin L. (Murph) Goldberger.

MOSIS [Metal Oxide Semiconductor Implementation Service] program. Teaching at Bell Labs, 1980; startup of fabless semiconductor companies. Discussion of Caltech's attitudes toward investment in small technology companies and licensing arrangements. His own consulting for Silicon Valley companies. MESFET [Metal Semiconductor Field Effect Transistor]. Formation of CNS [Computation and Neural Systems] program at Caltech with John Hopfield, early 1980s. Caltech's Center for Neuromorphic Systems Engineering; help from National Science Foundation; involvement of Christof Koch, Demetri Psaltis, Rodney M. Goodman, Pietro Perona, and Yaser Abu-Mostafa.

The interview concludes with a discussion of his interest in the freshman and sophomore physics courses and his advocacy of greater flexibility in the curriculum.

## Administrative information

**Access**

The interview is unrestricted.

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**Preferred citation**

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Carver Mead works out integrated circuit design with graduate students in his microelectronics class, described in 1972 as a “course in the physics, design, production, and use of semiconductor devices” (EE 281). Photo courtesy of *Engineering & Science*.

CALIFORNIA INSTITUTE OF TECHNOLOGY ARCHIVES

ORAL HISTORY PROJECT

# INTERVIEW WITH CARVER A. MEAD

BY SHIRLEY K. COHEN

PASADENA, CALIFORNIA

Caltech Archives, 2000

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# ORAL HISTORY PROJECT

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**Pasadena, California**

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**Begin Tape 1, Side 1**

COHEN: Would you give us a little bit about the family background and your education? What brought you to where you are?

MEAD: I didn't know you wanted to go that far back. I was raised in a place called Big Creek, which is halfway between Yosemite and Kings Canyon National Parks, up in the mountains. And the reason there's a place there is that there was a company called the Southern California Edison Company, which actually has a lot of connection with the old Caltech, because it was the first company to do real hydroelectric development up in the Sierra Nevada Mountains and then ship the power down to Los Angeles. They had to do longer high-voltage transmission lines than had been done before, and the insulator technology and the transformer technology were worked on right here in the High Voltage Research Lab at Caltech. So there was actually a connection going way back there that I didn't know about at the time.

COHEN: I gather your father was an engineer with this company?

MEAD: Well, he was a guy who ran one of the local power stations. He didn't actually have an engineering background; he was kind of self-taught. But of course I got exposed to all this wonderful electrical machinery, which I just loved.

COHEN: Now, I see you were born in Bakersfield. But I suppose that's where one had to go?

MEAD: Yes. That's where you had to go, because there were no hospitals up in the mountains. In fact, between zero and two years old, I was on the Kern River project of the Edison Company. And then my parents moved to the Big Creek project, which was east of Fresno. I grew up there. It was a wonderful place to grow up, because you're back in the mountains but it also had all this technology, and I loved that from the first time I had any contact with it. My dad used to bring home stuff they'd throw out of the power plants, and I would build stuff. From the time I can remember, I was building electrical things.

COHEN: Was there a community there?

MEAD: There were about fifteen families that lived around each of the power plants. We had a school. There were twenty youngsters total in the school, for all eight grades, and it was shared by two power plants to get up to the twenty. One of the big deals, when I was in about third grade, was that we went from a one-teacher school to a two-teacher school. That was a big deal. They put a big divider down between the two rooms. But it was a neat way to get educated, because you could overhear what they were telling the other people. [Laughter]

I had a super teacher for sixth, seventh, and eighth grades—a guy who had taught all his life, and this was his last teaching assignment before he retired. He was married to a woman who became a very, very good friend. She was a full-blooded American Indian lady, and we used to go off and hunt and fish. They were wonderful people. He noticed that I was interested in mathematics and stuff, and he said, "Let me tell you something interesting." And he showed me how to do trigonometry. I was just fascinated; I said, "You mean I can tell how high a tree is without climbing it?" That was my first exposure to being able to do something that just couldn't be done any other way—and it was great! My interest in mathematics started then; that was in sixth grade.

Then I guess it was seventh grade when a guy moved into the camp—these places where we had our homes were called camps—who was a ham radio operator. This was just at the tail end of World War II. I was just fascinated, so I used to go over there a couple times a week and he'd give me little tutorials.

And then all of this wonderful electronics came on the surplus market. I'd save up the little money I made from trapping furs and doing the little things I could do back in the woods, and go down and buy a huge amount of electronics for a dollar back then. Then I could take it all apart and use the parts to make things.

When I was fourteen, I moved to Fresno—there was no high school up there in the mountains—and lived with my grandmother and went to the high school there.

COHEN: That must have been an easy jump for you in some sense academically, but probably not socially.

MEAD: Oh, it was horrible. All the kids there had grown up in the same school and everything, and it was the rich end of town, and I was this little backwoods boy. So socially it was just awful. I hung out with the college crowd, because the college crowd had a group that was interested in electronics and radio, so at least I had people I could talk to, and I knew about as much as they did about this stuff. But it wasn't great socially.

So then, when I was a junior in high school, I went to the trouble of not only getting an amateur radio license but I studied all the stuff and got a commercial license too. And that allowed me to do things like work on two-way mobile radios. So I got really good jobs in Fresno. I got a job at one of the local radio stations as an engineer there, and it paid really well.

COHEN: While you were in high school?

MEAD: Yes, because people had vacations. When I was off in the summer, I could work essentially full time, and I could work one day a week during the year. That was just the perfect kind of a job for someone in school. It saved them from hiring another engineer. It was a good job. It wasn't much technology, but it was in and around these wonderful things. But I didn't get to take them apart or anything.

One of the guys there knew about Caltech. He said, "How do you do in mathematics?" I said, "I do fine." And he said, "Well, you should apply to Caltech."

COHEN: It hadn't occurred to you before this?

MEAD: No. My dad wanted me to go to the local state college and become a dentist. That was his aspiration for me. That's all he knew. And what did I know?

So I applied to Stanford and Caltech, and I got admitted to both places. A guy by the name of [Richard H.] Jahns [professor of geology, 1946-1960] interviewed me. It used to be that every applicant was interviewed by a faculty member from the Freshman Admissions committee. That was a good system. And this guy was good. He was a geologist and was very helpful. There were two of us admitted from my high school. The other guy's name was Phil Conley. We both came here, and he became, I think, a mechanical engineer. I read something about him in the blurb you get from the thirty-year reunion or whatever, and he seems to have done well.

COHEN: Why did you choose Caltech as opposed to Stanford?

MEAD: Well, my folks were very good. They took me to both places. Stanford is a beautiful campus, but we couldn't get in to see any of the labs and we couldn't do anything. And we came here, and there was a YMCA tour. There was a young guy by the name of Perry Vartanian who later started a company in Silicon Valley. He ran the Y tour, and he took us all around and showed us all the labs and everything. It was just much more of what I wanted than that giant place. It was a small situation, and it was all technology. And that's what I wanted, that's what I was here for. I didn't need any of that other stuff. The first day I was here, I loved it, and I've never changed my mind. It's a fabulous place. If you want to do technology, this is the place to do it.

That was 1952. I came here in September. I came out of the student houses, and I looked down the Olive Walk at Throop Hall, and I couldn't see the dome on Throop Hall, the smog was so bad. That's when they still had the backyard burning and they hadn't cleaned up the sulfur. It's gotten better every year since then. But that was a bit of a shock.

COHEN: Were you in the electrical engineering department to start with?

MEAD: Yes. But I was lucky. I had Linus Pauling for freshman chemistry. And I had a guy by the name of Frederic Bohnenblust for freshman mathematics—a wonderful teacher. He was a fabulous teacher. Linus was unbelievable. I had Dick [Richard P.] Feynman for mathematical physics. I used to go around to all his seminars when I was an undergrad, even. I couldn't understand any of the mathematics at all, but every once in a while he'd stop and he'd say, "What this really means is...," and I could understand that.

COHEN: Did you have any problems? Because obviously your background is not going to be what—

MEAD: Oh, I had a terrible time. It was very hard for me. I worked very hard. I wasn't as quick as the other students, because I kept trying to really understand. I didn't want to just figure out how to [do things], because I didn't come here for that. I came here because I really wanted to understand this stuff, and that's much harder than just learning how to do things.

I remember one of the most painful times. My roommate in freshman year came to me the second quarter and said, "I don't understand this chemistry stuff. Please explain it to me." So I spent about three hours. And we took the exam the next day, and he got an A and I got a B. [Laughter] I was a good teacher even then. [Laughter] I was never good at the examsmanship—how to get grades. But that was important, because otherwise you didn't get through. So I kind of struggled through undergraduate school.

COHEN: But you never felt discouraged enough to say, "I'm going to leave here," or anything like that?

MEAD: No, but sophomore year was really a down time, because I didn't have really good teachers. I'd had them in freshman year. And then I didn't have the interesting subjects, because that would start in junior year. So I had a bunch of stuff I wasn't very interested in, and the teachers weren't so good that they could make it interesting. So I ended up not being very happy; it was not a happy time.

But then junior year things got much better, because we started doing the electronics stuff, and by senior year I was doing well. But I had a very checkered record, which was not good enough to get me into grad school. I had never thought of going to grad school. Until I came here, I didn't know there *was* a grad school. I'd heard people say, "Dr. This" or "Dr. That," but I thought that was medical stuff. The whole idea of PhDs and research was all new [to me] when I got here. And I had jobs in the labs. So by senior year, somebody had said something about grad school. It was Melvin D. Brockie, who used to teach an economics course. He talked about the economics of degrees, and he said that master's degrees pay off over bachelor's degrees.

So then I went to see Hardy Cross Martel [professor of electrical engineering, emeritus], who had taught some of the electronics courses. I said to Hardy, “What’s this grad school stuff about?” And he said, “Well, look Carver, you’ve gotten through undergrad school all right. And the competition’s about the same in grad school. So you’re not going to do any worse.”

COHEN: So he said, “Why don’t you try?”

MEAD: Yes. So I took a master’s degree, and that was all fine. I did really well, because I got into stuff I really liked.

COHEN: Who was your professor there?

MEAD: Well, I took courses and got to know the guys, and I did really, really well. And then I got to thinking, and I went back to talk to Hardy: “Well, what about this PhD stuff?” And he said the same thing, “You know, you’ve been doing well.” So I applied, and they didn’t know what to do with me, because I had this very checkered record. But on the other hand, I had done super well during this one graduate year. And it was sort of like, “What do we do with this guy? He doesn’t fit any of the standard patterns.” So they invented this thing we now call a mini-oral, which is what happens at the end of the master’s year. I was the very first one. They invented it just to try to get rid of me. [Laughter] Because I obviously didn’t fit. And we still do it in EE [electrical engineering]. We have mini-orals every year for the students who want to go from a master’s into a PhD program. It turns out it’s a good idea, because you find out a lot. And with EE, it’s always been an important part of the program, because a lot of people who are going to work in industry will get a master’s, and it’s a perfect preparation for going off and doing stuff. It’s a very honorable thing. In fact, everybody has to go through it, at some point.

COHEN: So then you were a doctoral candidate?

MEAD: Well, then I took this mini-oral. I did really well, because that was the first time that all this energy I’d put into really understanding instead of just being able to pass the exams, that was the first time it showed—at this mini-oral. They were all surprised that I had done so well. So I was very pleased that all that energy and time I'd put in, that never showed in the grades, finally showed up. I actually did understand a lot. So, yes, I was admitted to the PhD program.

COHEN: Who was your professor at this point?

MEAD: It was halfway between [R. David] Middlebrook [professor of electrical engineering, emeritus] and [Robert V.] Langmuir [professor of electrical engineering, d. 1993]—Middlebrook for the semiconductor stuff and Langmuir for the more traditional. And I made up my own thesis topic. Middlebrook had taken a sabbatical while I was a grad student. I taught his course, which was a graduate course in transistor electronics. And it was what I wanted to do and what I loved. And then I had taken on one of the labs. I think it was called the EE7 lab when it was a senior lab. I taught that and made up a bunch of experiments. I loved teaching, and I learned a tremendous amount doing it. That's when I figured out that you learn much more teaching something than you do just learning it. So I was very lucky; in fact, I've always forced my students to teach, because I had this experience. You don't understand it unless you teach it, and then you really understand it.

As it turned out, then, Middlebrook had gone off into the circuit side of things, and I was really interested in the device physics stuff, and they didn't have anybody here doing that. So they asked me if I wanted to stay.

COHEN: This was when you got the PhD [1960]?

MEAD: Yes. I had gone my own way, and it wasn't like I was in the shadow of anybody; it was an area that nobody here was doing. So I started doing that, and I worked on tunneling. That was just when Leo Esaki in Japan had invented—he soon after got the Nobel Prize for this—tunneling [in semiconductors and superconductors]. And I was fascinated by that. So I made some of the devices and got started looking at them. One thing led to the other, and I did a bunch of electron tunneling in thin films and in various kinds of devices. I started teaching this freshman introduction to electronics course—the first thing I taught. Then a few years later I started a course on device physics, and that was fun.

COHEN: Now, as far as setting up your lab, where did the support for that come? Was that departmentally, or did you get yourself a grant?

MEAD: Oh, that was very interesting. I just sort of begged. At that time, they had a little tiny fund for any faculty member; I think mine was $3,500 a year. So you could buy little things. And then you could often get matching grants. The Hewlett-Packard people donated a little money. So I was able to start putting a little lab together.

One day I was sitting there in my office, and this guy waltzed in. And he said, "Hi, I'm Arnold Shostak from the ONR [Office of Naval Research]." He was a wonderful guy. "What are you doing?" And I said, "Oh, I'm doing electron tunneling, and it's this and it's that." And he said, "That's really neat. You've got some results?" I said, "Oh, yes, I'm just starting to get this and this and this." And he then said, "That's really great. Would you like a grant?" And I said, "Yes, how would I go about that?" And he said, "Well, write me a proposal." And I said, "What's that like?" And he said, "Well, write a couple of pages and say what you want to do, and then attach a budget." And I said, "How do you do a budget?" And he said, "Talk to somebody in your grant office. He'll know how to do that."

OK. So I sent in a proposal, about four pages, and they sent me a little grant. I think it was $30,000. I could hire a technician and get some equipment and support a grad student, and that was good. So I got this little lab set up. And then the General Electric Company gave me a little $10,000 grant to get equipment, so I got some gear together.

COHEN: The sixties were good years.

MEAD: They were very good years, because you didn't have to fight. That was before the Societies for the Absorption of Federal Funds were taking all the funds away. So individual investigators were really sort of hand-picked by the agencies, and the guys in the agencies knew when they had a good thing and they would support it. You didn't try to escalate your budget or anything. We were all ready to do the work, and we got a huge amount done for the money. We obviously weren't doing it for the money, because there wasn't enough money in it to do that. There was very little bureaucratic overhead. There wasn't this fierce fighting for the federal funds that later grew up.

COHEN: ONR was very good. They did radio astronomy here, you know.

MEAD: Oh, yes. That's how I got to know them. I had worked in the physics labs as an undergrad. I worked with Carl Anderson [professor of physics, d. 1991] and Bud [Eugene W.] Cowan [professor of physics, emeritus] and those guys during the summers, part time, to get myself through school. So when I was doing this applied physics, all the stuff got published in the *Journal of Applied Physics*. The physics people here were very hostile to solid-state things, except for Dick Feynman. He always liked it. He loved the tunneling stuff, and he would always come to my talks and be very encouraging. But everybody else was off chasing particle physics; they thought that was real physics and the rest of this was just dirty.

It was in the sixties sometime that we started this thing called applied physics. And that was myself, Amnon Yariv [Martin and Eileen Summerfield Professor of Applied Physics and Electrical Engineering], and Charlie [Charles H.] Wilts [professor of electrical engineering, d. 1991], I think. There were a few people doing physics, but the physics department here wasn't at all interested. So we started this discipline, and it's still going and doing well. Actually, it helped a lot, because when the physics department got less bigoted and started hiring people like Axel Scherer [Bernard Neches Professor of Electrical Engineering, Applied Physics, and Physics], then there was a way that people could work together. So it actually has made it easier, because we could keep the things alive until the physics department came to their senses.

COHEN: And particle physics has gone other places, too.

MEAD: Yes. It's also been one of those things where, let's face it, there hasn't been a huge amount of yield for all that money. Whereas if you look at the things that have happened and that are exciting in physics, they've been the scanning tunneling microscope and the high-Tc superconductor and the quantum Hall effect, and those are all small lab things. And those are all things that would have been applied physics here. Gradually, the physicists have realized that's where physics is going.

COHEN: Now, during all this period, I can't believe that other places weren't tempting you to come?

MEAD: Well, yes, I had feelers, of course. But this is a really good place to get work done.

COHEN: It never occurred to you to leave here?

MEAD: Well, I thought about it. Actually, that was later. And I'll tell you the story, because it was one of the low points in the Caltech history.

I'd been doing this device physics—the tunneling and metal semiconductor barriers. Now, Gordon Moore had been a good friend, and since '61, I guess, I had been going up every week to consult with Gordon Moore when he was at Fairchild [Semiconductor]. So it's been a very long-standing friendship, and he's been just a great friend.

COHEN: Well, here you are sitting in his building [Gordon and Betty Moore Laboratory of Engineering]. [Laughter]

MEAD: Yes, exactly. But [the friendship] started a very long time ago.

COHEN: He was a student here, too.

MEAD: That was before, but yes, he got his PhD here. He had already, by then [1954], been at Shockley [Semiconductor] Labs, and then he and Bob Noyce and those guys spun off and made Fairchild [in 1957]. And I got to know him probably about a year after Fairchild was started. He waltzed into my office and said, "Hi, I'm Gordon Moore. What are you doing?" [Laughter] And it was great. We've been good friends ever since. He was the one who told me, "You should figure out how small you can make transistors," and so I worked on this. It was the first time I worked out the scaling of things, and it was at Gordon's suggestion. It was very surprising, because it was one of those things where everything got better. And that wasn't the standard lore, so I published a couple of papers that basically said that, and gave a couple of talks. And then other people, particularly the IBM people, did their version of it, and then it became clear that this was indeed true. Until then, people hadn't really seen that as a big direction. And the Japanese took it seriously, which helped things along. I remember looking at that one day and saying, "You know, if that's true, the problem's not going to be making the devices; the problem's going to be *designing* the devices—how the heck do we do that?"

So I went off and started working on that problem. I've changed fields every thirteen years since I've been at Caltech, and that was the first time I changed and decided this was so exciting I had to go off and figure this out. And it ended up being the next thing I did: to figure out how you do automated design of these big, complex microchips. How do you do all that, and how do you think about it?

One of the things I did differently is that I said, "No way do I want to create a fabrication area to make these things, so I'll make a deal with somebody who has one." All the other schools said, "Oh, we're going to do integrated circuits. We need an integrated circuits lab." And they'd go put up one of these big labs to fabricate. Well, by that time, you were spending all your time fixing vacuum pumps and that sort of thing and you didn't have any time to think about how you design the things. And that actually ended up in a new business model for the semiconductor industry, which has come to be called the ASIC, for Applications Specific Integrated Circuits.

COHEN: Wait, let me understand this. You do the business of just designing, and you go to a company to make it?

MEAD: Yes, you have a nice clean interface with someone who does the actual fabrication. At that time, nobody did it that way; and there was a huge, huge resistance to the thought that there could be a new way. You know, people are always threatened by what's new. As it turns out now, in today's world, about half the semiconductor things are done the old way, by companies like Intel who do everything. And about half are done by people who design and get other people to fabricate. So this was a new business model for the industry, and, as I say, it's come to be used about half the time.

COHEN: So then, did the industry support the research at the university? What was the give-and-take of that?

MEAD: Well, that was actually quite a significant struggle. What happened was I got the stuff to where it was working, and people got excited about it. That was just about the time when Bob Cannon became chairman of E&AS [Engineering and Applied Science Division, 1975-1979]. He had these retreats where he would take a dozen faculty. He'd pick the influential faculty in various parts of the division and take them down to Fallbrook, to the Capra Ranch, and we'd all stay there for a weekend and have discussions. One of the discussions was whether we should be doing computer science here at Caltech. There had been an early start in computing here, before my time, and it had ended up with the LGP-30, which was designed here and built by Librascope [in Glendale]. And then it had all gone away; there was no vestige left. So they were thinking of what to do about this. I had been doing this computation on silicon stuff, and we were the only school that was doing it. So finally there was a committee and this urge to talk to everybody. It ended up that they hired Ivan Sutherland to come here, and he and I started a computer science department here. Ivan was a promoter. He had been at ARPA [Advanced Research Projects Agency]; he knew the Washington circles and he knew the industry circles, and he was a very good promoter—and still is. He saw this was a thing worth promoting. So he came, and we started computer science, and then he got the idea that we should have this thing called the Silicon Structures Project—or SSP, as it became known—and we got an industry consortium to fund it. To become a member, you had to pay $100,000 and send a full-time person. They typically came for a one-year stint, so it looked like a sabbatical, and that was attractive. The first companies were Hewlett-Packard and Xerox and Digital Equipment Corporation and Burroughs and IBM. It was a good lineup. Then later we expanded to include people like Univac and Honeywell. We had basically all the major players in those days; it was a good group, and they sent these guys here; they would come for a year. It was really an interesting experience trying to keep an academic program going with that many industry guys, because of course they were used to a project-oriented, very disciplined environment rather than open-ended research. It was very hard to get the students to focus on degrees rather than on doing projects. There was a whole bunch of stuff that looked more like development than like research. It was a very hard time. I got very discouraged.

COHEN: How many of these people would you have here at one time?

MEAD: Well, one for every company. So it was maybe twelve people—many more than there were faculty, and almost as many as there were students. There were maybe thirty students, and twelve of these industry guys, and five faculty.

COHEN: I imagine there was criticism from the institute. Were you in charge of this program?

MEAD: Well, Ivan and I were doing it, and we had a guy to help run it, but he wasn't really a Caltech guy either. Ivan wasn't a Caltech guy. So I was the only one who knew how Caltech worked, and this thing was not working the way Caltech worked. It was more like development. The students were doing things that looked more like development projects than thesis projects. And it was very, very hard. I got all stressed out about it. We were managing, but we were in way over our heads. We had far more to do than we could actually do. There weren't enough faculty, and that sort of thing.

Then in the middle of that, Ivan left and Murph [Marvin L.] Goldberger took over the place [as president of Caltech, beginning in 1978—ed.]. And Murph didn't much like this computer science stuff. Then Jack [John D.] Roberts [Institute Professor of Chemistry, emeritus] became provost, and Jack Roberts didn't like anything interdisciplinary. Then Roy Gould [Ramo Professor of Engineering, emeritus] became [E&AS] division chairman, and *he* didn't much like this computer science stuff. So I had three guys against me.

Eventually I ended up—I was up north at a friend's house, and I was very despondent. I had been out swimming in the pool, and she had a sliding glass door between the pool area and the house. It always stayed open in the summer, but unbeknownst to me she had closed it. I ended up coming into the house and going through the glass door. A huge shard of glass came down on the whole side of my face, blood spurting everywhere. So here I was at midnight in the Stanford emergency room. Fortunately it all came out OK and there was nothing permanent, but it could have been just horrible.

That sort of gave me a moment to think, and I realized, "I'm trying to do too much. This isn't working." So I just sort of backed off and said, "I've got to think about what I'm going to do next."

COHEN: Did that whole program die then?

MEAD: It just sort of wound down. It was in the process of winding down anyway, because we'd done what we could do with these companies. It was very, very interesting what happened there. We had basically done what's now called silicon synthesis. It was called silicon compilation at the time. You know, every generation has to invent a new word for what's happening. And it was just obvious that that was the way things were going to get designed, and not by drawing every transistor on a sheet of Mylar, as they had been doing in the industry. We had basically put in place an awful lot of the technology and showed how you could do all that.

COHEN: This all was coming from Caltech?

MEAD: Yes, a little tiny group. Then other people got onto it, and we got a whole bunch of universities interested in this. And we got ARPA to put the MOSIS [Metal Oxide Semiconductor Implementation Service] program together, which is a program where you can send design files over the network, to MOSIS, which is physically over at ISI [Information Sciences Institute] in Marina Del Rey. And they group them all up and then send them off to a silicon house that fabricates them and cuts them up and sends them back to the clients. That was the first time that had been done, and we got all that to happen. It's still running, and it's become a model for every other country in the world. [Tape ends]

**Begin Tape 1, Side 2**

MEAD: So you end up being able to share the cost. Let's say it costs $50,000 to fabricate a batch of wafers. Well, if you share that with a hundred projects, that's not bad, but if you have to do it for one project, it's hopeless. That's what happened with all that, and, as I say, that became a model.

Then, the guys who came here got infected by this idea stream, and they would go back to their companies and try to get something to happen there. It was 1979 when that really took off. I taught a summer course for teachers up at the University of Washington. People basically took their vacation and came to this summer course to learn how to do this, and then they all went back and started teaching. So courses started appearing in all these universities, and the kids could actually get their projects fabricated and test them.

COHEN: I read the talk you gave about that—how kids left the course with something concrete.

MEAD: Nobody could ever tell them that they didn't know how to do silicon, because they had done it. So instead of having a few hundred engineers who had been able to do that, we were creating thousands of kids every year who had had that experience. And nobody was going to tell them they couldn't do it; they'd done it. So it's changed really radically, the whole way the industry works; and that's been wonderful.

After that course, the next summer I went and taught a course at Bell Laboratories. Because one of the things that was true—it's less true now, but at the time it was true—was that if Bell wasn't doing it, it wasn't OK. If you could get it going at Bell, then there would be sessions in all the conferences.

COHEN: It conferred legitimacy?

MEAD: Exactly. Because Bell was so dominant in controlling the IEEE [Institute of Electrical and Electronics Engineers] meetings that if they weren't doing it, it wasn't anything. It's not so true anymore, although they're still very influential, but at the time they had sort of a hammerlock on the IEEE. Bell asked me if I wanted to do it, and I said yes, because that was waiting to happen. And then with the universities all doing it, and Bell doing it—well, the net result of all that was very funny. Looking back on it, it's hilarious.

These guys would go back to their companies and they would be all energized and would want to do this stuff. And gradually they'd find out that the answer was, "That's not the way we do it here." So essentially, all of those guys would go off and start their own thing. Not because they hadn't tried to get it going inside the big company; it was just that the big company wouldn't do it, and they'd get frustrated.

So the Bell guys started Silicon Design Labs. And some guys from here started Silicon Compilers, which I helped get going. And there were some people, out of the course in Washington, who started a thing called Seattle Silicon, which is Cascade Semiconductor Design right now. And there must have been twenty startup groups that came about because people in the big companies just couldn't do it. It was too new, too different.

COHEN: So these were really energetic people who did this. They all had good jobs, after all.

MEAD: Oh, yes. They just got so frustrated that they had to go do something; and that started a whole industry of people doing things this new way. There were a bunch of people who did these things, which are now called fabless semiconductor companies. They have a line of semiconductor products but they don't do the wafer fabrication. They outsource that—that's the new term.

COHEN: So these companies are just design companies?

MEAD: Well, design and application and all of the things around that. The only thing they don't do is print the wafers. And that was a big success, because now a substantial fraction of the industry works that way.

But in response to your question about how you get paid for this, we didn't get paid very well. We got the money from the SSP companies, while they were here. We have since gotten quite a number of people who've come back and donated to the institute. This building is a good example of Gordon Moore's contribution. They're very long-range payoffs; they're not very direct, and they usually come as a big stock gift from somebody. We're seeing those in people like Phil [Philip M.] Neches and Louise Kirkbride, and those people who are now trustees who were students of mine during this period.

COHEN: So they have this loyalty, and they know from where they got their—

MEAD: Oh, yes. And they've always stayed in touch. It's kind of neat to see your own students on the Board of Trustees. But it's very long-term and very indirect. The big ones tend to be longer term, because it takes a while to grow a company and not everyone who starts a company is successful.

During that period, the institute would not accept stock. I used to talk to Dave [David W.] Morrisroe [Caltech vice president for financial affairs and treasurer, d. 2002] about it. I said, "Hey, I'm involved with this company that's starting up." And he'd say, "Carver, you go ahead. And when they go public, give us some stock." [Laughter] At the time there was no mechanism; now we have a good mechanism for doing this. And we've got Larry Gilbert [senior director of technology transfer] , who, as it turns out, is very good and is sensitive to making deals with small companies and taking stock for licenses and doing that sort of thing. And once again, these will be longer-term payoffs, but at least we'll be getting it on the way in. We don't have to wait for somebody to donate it, we actually get it going in, and that makes much, much more sense. To me, that's a model for how we ought to be doing business.

COHEN: When did that start?

MEAD: Only a couple of years ago. As long as we had people like Murph running the institute, there was no hope whatsoever of doing anything sensible. It was just not possible.

COHEN: Was this because he felt there had to be a barrier between industry and the university?

MEAD: No, it's because he didn't understand the process. And the trouble with Murph was that not only did he not understand the process but he wanted to believe he knew everything. So there was no win, because you couldn't teach him anything, since he already knew everything. But he didn't know anything about this. Therefore it couldn't exist. It was just that simple—it can't exist if I don't know about it, and I don't know about it, so it can't exist. There you are. So it was just not possible.

When Tom [Thomas E. Everhart, Caltech president 1987-1997] came, Tom was from Silicon Valley, he knew about this stuff, he understood. We actually had a dinner up in Silicon Valley with all these Caltech alums who had started companies—and I know most of them, because I stay in touch with them—we had this great dinner for them. Probably two-thirds of them were Caltech alums. It was a fabulous evening. Everybody wants to help, everyone wants to be involved and contribute. It's then become possible for us to deal with companies. And it is the right way to deal with a small company. Larry's got the right model. You take some stock in a small company, and you give them a license to the technology, and you be helpful. And when they succeed—they won't all succeed, but some will—you have a big return.

COHEN: But you have to be careful. Because the people who would argue against this would say, "Are they telling you what to do?"

MEAD: You don't do any of that. It doesn't happen that way. Now, it *can*: I mean, there are things where—Stanford has gone too far the other way. There are all these professors running little companies out of their offices, and the students are never sure if they're working on a company project or a research project. You don't want to do that.

COHEN: So that doesn't happen here?

MEAD: Well, I won't say it doesn't happen here, but you know, there are some very simple things you can do. I always make sure there are no secret projects in the lab. Anything anybody does, they have to be able to talk to the whole group about it. It's got to be up front. So that immediately means you can't do any contract development for companies or any of that sort of thing, because if you do, they don't want [you] to talk about it at all. It just settles a whole set of issues. Students aren't saying, "Gee, who can I talk to about this?" They're just not in that mode. This is Caltech, this is a research institution.

COHEN: That's comparable to the classified research that went on.

MEAD: Exactly. It's exactly the same issue. Except now it's not the government that's doing the leading-edge stuff, it's the companies. We've done deals here at Caltech where if someone sponsors some research, they get the first cut at licensing. I think that's OK. But it's research. It's led by Caltech people; it's conceived by Caltech people. It's done as research. And if something comes out of it, the people who pay for it should have the first cut. I don't see anything wrong with that. But what you don't want is having things the other way, the company saying, "I have this problem I want you to work on." Because by the time a company has identified a problem, it's way too late to be a research problem. I've gotten most of my research issues, down through the years, from my interaction with Silicon Valley, but not because they told me to work on it. It was because I was working with them and I could figure out, "Gee, that's an interesting fundamental thing and they don't have time to look at it." So I would go off and look at it, and then I'd go back to Gordon and say, "Hey, I did this and this and this." "Oh, that's interesting." So there was always a good mutual back-scratch.

But it is true that if you insist on that kind of autonomy for the research program, you won't get paid for it as well as if you're willing to be in the prostitution mode, where you do what they say and you get paid for it. I don't think that's good for Caltech. I don't think it's a good model. Essentially, every other university has done that, but I don't think we should do it.

COHEN: Of course, people, particularly in engineering, think they should be consultants—that's positive.

MEAD: Oh, that's a very good thing. I think probably ninety percent of my research topics have come out of consulting with Silicon Valley companies, where I would be working with their people and I'd see something come up that led to a very fundamental thing and there was no way they could chase it. So I would either collaborate with them or I'd go off and look at it.

COHEN: So that interface is really very positive.

MEAD: Oh, it's very positive, extremely positive. And the interaction of our faculty with the small companies is a very positive thing.

COHEN: You really have a leaning toward small companies?

MEAD: Well, they're the only ones that can respond quickly when there's new stuff. I will work with every company that's interested; it's just that most big companies can't get it together to move. To give you an example, I came up with this thing called the MESFET [Metal Semiconductor Field Effect Transistor]. There's a picture [of one] down in the lobby. It was obvious to me this was going to be a dynamite high-frequency transistor. It turns out to be the transistor that's used in all the satellite communications. So I took it to Gordon Moore, and I said, "Gordon, this is a really neat thing." And he said, "Yes, Carver, that's a really neat thing." But it turns out it was a gallium-arsenide device and Fairchild was doing silicon. So to humor me, he put one guy on it, and they got a government contract. Meanwhile, the Japanese saw it, and they came and talked to me. And within a year, they had them on the market.

COHEN: Missed opportunities.

MEAD: Yes, exactly. And Fairchild wasn't that huge at the time; it was just that they were going in a different direction. There was nobody there that was in that flow. Whereas that was an opportunity where, if I'd known what I know now, I would have said, "Hey, this is an opportunity for a small company to do something really remarkable." And it probably would have been.

COHEN: Let's talk about the formation of this whole CNS [Computation and Neural Systems] program. How did that come about?

MEAD: Well, that actually came about because I'd had the accident with the glass door. That was at the very end of the computer science thing, and I just couldn't carry it anymore. I was burned out. This would have been about '80, somewhere in there.

COHEN: Was John Hopfield [Roscoe G. Dickinson Professor of Chemistry and Biology, emeritus] here already?

MEAD: Just [Hopfield came to Caltech in 1980—ed.]. I started hanging out with John, and we got to talking. I'd known him from the old solid-state physics days. We both used to work on cadmium sulfide. He had been at Princeton at the time. In fact, one of my former students, Tom McGill, had been a postdoc with John.

So when he came here, we started chatting, and he told me about this neural stuff. Well, I had had an itch for a long time to do neural stuff. I'd spent some time with Max Delbrück [professor of biology 1947-1977, d. 1981], looking at nerves. We'd done some lipid bilayer membrane research in the late sixties; we had a little group that we shared doing that work. So I had a sense that the computation done on the brain and the computation we did in silicon, there was something there. I'd always been fascinated by it, and especially since I'd worked on the nerve membrane research, I knew something about it, and I knew that the physics wasn't really all that different, if you looked at it in a certain way. So when John started talking about this, I got very excited and said, "Oh, we've got to do that!"

We taught a course together, which I think was called Physics of Computation. He taught about the work he'd been doing, and I taught about the work I'd been doing, and the students were just bewildered, because they didn't see any connection at all between these things. A year later, we got Dick Feynman into the thing. So there were three years, in the last two of which Dick was involved, although there was a period there when he was sick and fighting with his tumor, so he couldn't be there all the time. But it was still a really neat time. There was a bunch of ferment in this. We were kind of arguing with each other and trying to figure out how we thought about this stuff. And that turned into three different things: Hopfield's course on neural networks, my course on neuromorphic analog circuits, and Dick's course on physics and computation. It was clear to both John and me that this thing was all about getting the neuroscientists and people doing the computer models and people into circuits all talking to each other, because otherwise it wasn't going to happen.

About that time, Robbie [Rochus E.] Vogt [Avery Distinguished Service Professor and professor of physics, emeritus] had become provost [1983]. And Robbie was Robbie. He was very into making an impact. And you know, wherever he goes, there's certainly an impact. [Laughter]

Murph had known John for a long time, so instead of being a problem this time, Murph was actually a help—which is the first time I'd ever seen him be helpful in anything. But I was happy to take it. [Laughter] So that was a nice time. Actually it was the first time I'd ever seen a Caltech administration be helpful. I mean, usually, before Murph, Harold Brown [Caltech president 1969-1977] had been helpful, but his way of being helpful was basically to kind of set you on the right path and let you go. And it was great, it was a nice way of being helpful, but it was very different from having somebody say, "Hey, wouldn't you like to start a new program?" It's like, Wow, this is a new experience! [Laughter] So we did. We got the biologists involved, and we started CNS.

COHEN: Everybody was helpful there?

MEAD: Yes, everybody at every level in every direction. And it was painless. And it's been a very, very nice program.

COHEN: How much are you involved with it?

MEAD: Well, I did that up until this last year, and now it's time for me to change fields again. So I'm on sabbatical this year, and thinking about what to do next. I'm just generally being helpful, because I'm still interested in the topic.

What happens is, I realize where my thirteen-year cycle comes from. It turns out that the first few years you spend time finding out things that other people already know. But you need to get it in your own frame of reference so you can think about it your own way; because if you can't think about it your own way, you'll never make a contribution to it. So that's two or three years there. And then you finally start getting some ideas and trying them out, and most of them don't work.  But then, after a while, you find something that will start to yield, and you follow it, and only then can you get students involved in it.  So you're probably three or four years into it before you can even get students involved.  Then you get the students going, and the first couple of batches of students don't believe you can do anything, because they can't find any theses on the library shelf that did that.  So you get only the most adventurous ones.  Then gradually you start getting some hits, and everybody gets excited.  And then it starts being successful.  The other universities start doing it.   You get the book written about it.

And what happens is you get the really, really good students doing really good work. Like right now I have six or seven students who are just absolutely fabulous, and they're all doing this research.  They're all going to go off and get faculty positions and do this stuff, and do it very, very well.  Well, I shouldn't be doing that anymore—I mean, it's their field now, it's not my field anymore.  I want them to make it their own and I want them to be successful.  My success has been that there are those young people who are good at it, and I shouldn't be competing with them.  I gave it to them, and then I gave it all away.  [Laughter]  So now I've got to go and do something else—there's nothing I can do anymore.

Besides, you get a little burned out after a while, because, for me, if I'm not in the lab, I'm not happy.  And when you get it going like this, and it's going out in the world, and the other universities are doing it, then you're spending all your time talking to people and keeping these kids funded, you're not in the lab anymore.  And who wants to live like that?  So after a couple of years of that, I'm not going to do this anymore.

COHEN:  What about this big project—the Center for Neuromorphic Systems Engineering?

MEAD:  Well, actually that's a real good thing.  Let me tell you how that came about.  I'm generally against these big center things, but this one was a little different, because we had a group of faculty who were in CNS and we were all working together.  We shared grad students and we'd get together all the time.  We'd have seminars and we'd go to dinner together.  So a couple of the young faculty came around and said, "Hey, why don't we get an NSF center?"

"No, you don't want to do that.  [Laughter]  Let me tell you all the bad things about NSF centers."

They said, "Yes, we understand that.  But look, we're working together already."

And I said, "Yes, that's true."

And that's different, because most NSF centers are shotgun marriages. You've got to gather up enough people to make it look like a center, but really they're warring camps—and we didn't have that, we already had a very successful academic program. We had these cross-disciplinary collaborations that were working already. We had a new art form that was developed quite far along, and people started applying it to real things. So I thought, "Well, you know, I don't much like centers, but if you promise me I don't have to administer anything, I'd certainly be helpful." Because any time a group of people want to work together, somebody has to be in charge.

So then they started talking about technology transfer. And when Fred [Frederick] Betz was out here last time—he's the NSF guy who watches over us, and he's actually a good guy, even though he can be a bit abrasive on occasion—he said, "You haven't got your technology transfer model down." And he was right. What was really going on was that everybody knew that the people we could get to do this stuff were the small companies, but everybody assumed that the NSF wanted big companies to put in some money every year. And that would be fine, provided the big companies don't say, "We'll put in our money, but then you've got to do this, this, and this," because that's not what a research program is about.

So in the end what happened was, we had a long talk with Fred, and I basically said, "Look, Fred, this is what works. I've been there before. Let me tell you, this is not something that's going to happen the usual way."

He would give examples like, "There's a center for magnetic recording back at CMU [Carnegie Mellon University]." It turns out to be one of our old Caltech students who runs that.

"But magnetic recording has a whole industry. It's already known what it is. It's already known that there's a next generation. You're improving a known thing. Neuromorphic systems don't exist right now, except in the lab. They have huge potential. But it's a longer-term thing, it's not short term. We're just now starting to see little examples, and there'll be a lot more. But it's taken since 1980 to get this far."

The neural networks are now working in a lot of computer programs, but there haven't been versions that work in real time yet. So they've taken a long time to come along—largely because we didn't have the silicon technology that would support them. We're just getting that now. It's a whole new thing. It's as if you had to start computer technology from scratch, and you didn't know any of it, and you had to invent it all. That came over a very long period of time, if you really look at it. But it was driven by always having some application that was working. We're just now starting to get some application. So actually, for the fifteen years or so that we've been doing it, it's come a very, very long way. I mean, right now, you can really see how to do a lot of this, and there are working examples in the lab which have been remarkable properties.

COHEN: Does this mean that you're not going to be interested in this for very much longer?

MEAD: Oh, no, I'm very interested in it. It's just the young people who are carrying it. They're going to be the mechanism. It still has to go through a lot of evolution to get to where it's going, to be something that can be picked up by a company.

Meanwhile, there are little companies that find a way to do a part of it and then go off and start doing it. And fortunately, we have Larry Gilbert, who knows how to make deals with little companies and take some stock. So I think the way this center is going to get funded for the future is to have stock in all the little companies that get spun out of this technology, and it will have a few big companies as part of the thing.

COHEN: So what does the NSF do for you? They get you started?

MEAD: Yes. They basically put in some money for a period of five or seven years. And it's actually very appropriate, because right now we have enough technology that we can show something. There's still a lot of work to be done, but that's something that companies can kind of see. But in the last ten years, it would have been hopeless. I never would have been able to get started, except that it turned out that there was an event that happened around 1980. The System Development Corporation, which was a software house, got sold to Burroughs. But SDC was a nonprofit company. So what do you do with a bunch of money when you sell a nonprofit company? There aren't stockholders.

Well, they went to court, and the court said, "You should make up a foundation for the support of basic research, and then you have some trustees, and you use the money for that."

It turned out that one of the trustees was Arnold Beckman [chairman, Caltech Board of Trustees, emeritus, d. 2004]. And Arnold said, "Yes, I know just the person you ought to talk to." So he had them contact me, and I talked to them. They supported our stuff for about six or seven years, when no government agency would touch it because it was way too early and too speculative and too controversial. Actually, it's just now getting to where it's controversial, but at least there's enough stuff that people can't say, "Oh, well, it doesn't work."

COHEN: So who's working on this program? You. And of course, John is leaving.

MEAD: Oh, John will stay associated with the center. And then there's Christof Koch [Lois and Victor Troendle Professor of Cognitive and Behavioral Biology and professor of computation and neural systems], and Demetri Psaltis [Thomas G. Myers Professor of Electrical Engineering] , and Rod [Rodney M.] Goodman [professor of electrical engineering, 1985-2001] and Pietro Perona [professor of electrical engineering] and Yaser Abu-Mostafa [professor of electrical engineering and computer science]—a good group, really topnotch.

COHEN: But all these people are still doing their thing also?

MEAD: Well, it's part of what they're doing. Basically, all of those people are working on aspects of this neuromorphic way of looking at systems. And it's really a remarkable concentration of talent with quite a good shared vision. That's really an amazing thing; I mean, at Caltech usually everybody goes their own way. We have no mechanism for corralling people at Caltech. Thank God, we don't have that mechanism. That's why I'm still here.

COHEN: Of course. And most other people.

MEAD: Exactly. Because we're all a bunch of criminals and misfits. But this happened spontaneously, you see. These are people who respect each other and who use each other's work. and that's a really nice thing.

COHEN: So you're a happy family at the moment?

MEAD: Oh, yes. It's a good group.

COHEN: It's only about a year old.

MEAD: Well, the center, yes, but you see, it had actually been happening already, and this just brought it into a little more focus. We do have to get our act together on our technology transfer. We need to be very clear about how we go about dealing with small companies, how we deal with large companies, making sure we don't step on anybody's toes. But basically I think we all know what's going to happen. It's just that it takes a while to get it to where you can say it. Because you do what actually works. It's not what somebody said because they were trying to prove something. In the end, you do what actually works.

COHEN: You say you're on sabbatical this year. So that means you're going to think about—

MEAD: Well, right now I'm off thinking about how we ought to teach introductory freshman physics.

COHEN: Are you interested in this core curriculum change that's going on?

MEAD: No. You see, those things always happen, assuming there is no change in the material. But ever since Dick [Feynman] did the freshman and sophomore [physics] lectures, I was very jazzed about that. I've done freshman courses all the way through my career, because I liked being able to talk to the kids before they were brainwashed. There's lots of life and energy there. And you can really get people excited about stuff when you get them that early. I had sat in on Dick's course, because I had thought that what he was going to do was to really find a unified way to teach that. And he got about halfway there. But you know, I was really interested in the sophomore year, because that was all about electricity and magnetism and quantum mechanics and how they go together—which is the material I work with. Unfortunately, that was the year Dick was doing the lectures on gravitation; he was doing the Hughes lectures. He was involved in a California school curriculum study, and he was doing the sophomore lectures. It was enough to kill anybody. I mean, any one of those projects is enough. So the poor guy just didn't have the time to do it all the way out. So I've always wanted to go back and do the rest of what needs to be done. I'm taking some time to see if I can get there.

COHEN: You're going to knock heads with the biologists who want that time?

MEAD: Oh, I'm not even worried about the embodiment of it. I mean, I don't care what course it goes into. I want to get the material right. I mean, if you don't have the material right, there's no point in talking about where it's going to show up. You don't have anything to do.

COHEN: So you're not going off anywhere?

MEAD: Well, I'd spend more time in my cabin up in the redwoods. It's a nice place to do writing. I tend to be less cornerable than normal. You know, if people know where to get you, then you don't have any time to think, and it's all about thinking.

COHEN: But you're in no way going to give up on this center?

MEAD: Oh, no. It's just that I'm thinking about these other things. I'll probably be more like a senior statesman in the center, rather than the guy doing all the work.

COHEN: And John's going to be a senior statesman from afar.

MEAD: Well, that's basically what we've been doing the last year or so. And that's as it should be. They're the up-and-coming generation.

COHEN: I could ask you now about what influence people here at Caltech have had on you. But I think you've answered that as we've gone along.

MEAD: Yes. Well, the biggest one was Dick Feynman. He was really the reason I could imagine being in science, because he was the most central inspiration. But there are other people who were really important. Like Charlie Wilts who was in electrical engineering. He was a very deep-thinking individual, and he was very important for me. And of course the freshman guys—Pauling and Bohnenblust. Barclay Kamb [Barbara and Stanley R. Rawn, Jr. Professor of Geology and Geophysics, emeritus] was a grad student when I was an undergrad, and he was the TA [teaching assistant] in the freshman geology course. But he was a physicist doing geology. I remember one exam that had questions on it which were really, really neat. And I struggled with it. I remember he wrote on my exam, "Well, you dropped the mathematical ball, but at least you made a run with it. And that's why you get so much credit for the wrong answer." [Laughter] What had happened was I was the only one in the class who had figured out what the question was about. Instead of just trying to zip off something, I'd actually thought about it. Everybody else was much better at taking exams, so they assumed it was some little trivial thing they could just zip off. He was great; he was terrific. Actually, Bob [Robert P.] Sharp [Sharp Professor of Geology, emeritus, d. 2004] taught the lectures and Barclay was a TA. It's very hard to do better than this.

I was very, very lucky. I got to know Tommy Lauritsen [professor of physics, d. 1973] really well. He did our modern physics course, and it was very interesting to hear a lot of the old stories, because of course, Charlie [Charles C. Lauritsen, professor of physics & director of Kellogg Radiation Laboratory, d. 1968] had been around when all this original research was happening. So Tommy knew all the guys.

COHEN: Well, Caltech has a lot to offer.

MEAD: Oh, yes. But it's spotty. You see, we're not a monolithic institution, so you don't get a highly organized education here. But what you get here is much, much more valuable than that. You get real insight into things that most people take for granted or don't think about very much or just parrot what somebody else once said.

COHEN: So maybe you would feel that there shouldn't be too strenuous a core curriculum. You know, just get the people who can really inspire and let them do what they want.

MEAD: Well, it's always dangerous to have anything too mandated. The material I'm doing now has been frozen since the 1850s, roughly. It's [James Clerk] Maxwell's stuff. Well, it's actually not a very good way to present this material. And it's also true that we introduce our freshman physics with mechanics. I have a wonderful quote on my bulletin board from Ernst Mach, out of a book called *The Science of Mechanics*. At the end, he has a chapter on the relationship of mechanics to the rest of physics, and basically he says, "Well, there's some historical value to teaching these materials in the order in which they were discovered. But basically, there's no reason why that should be the order in which the understanding comes." And in fact, he doesn't believe that mechanics is a very good starting point for physics. I don't either. I think it's a lousy starting point for physics. It's a result of a very messy, incoherent interaction of quantum mechanical things, so it doesn't give you a very good window on the basic laws. I would rather start from the fundamental things and then have mechanics develop as an aggregate of big, incoherent objects, rather than as something you start with and then you try to make the little things look like that. Because they don't; they don't act like that. Well, you're not able to do that, if you get too rigid about the way you organize the curriculum.

**Begin Tape 2, Side 1**

MEAD: The great thing here at Caltech is that we really value doing things from fundamentals.

When you come right down to it, most places just sort of hack through stuff. But you can sit down at any table in the Athenaeum [Caltech faculty club] over lunch and have a discussion with someone and you find out what the real fundamental things are in that particular field. And that, to me, is what sets this place apart from anywhere else. You can get as good an "education" at Berkeley or Stanford or MIT or anyplace as you can here—probably better, if the truth were known. But if what you want is to understand really, really, all the way to the fundamentals, there isn't any place like this. That's what we ought to be doing, not emulating the way they teach physics at Berkeley or at Harvard. So what I'd like to do is to get to where we have a more fundamental way of introducing those things to our students and not just treat it in this pro-forma way. Fortunately, Caltech is flexible enough that if you come up with something, you can find a way to do something about it. It hasn't become so rigid that it's hopeless now.

COHEN: So what do you think, then, when people complain that students are not rounded here, that they don't get enough humanities courses? How do you feel about that?

MEAD: Well, I don't think people come here to do that. I think they come here to do science. If you want to do that, you can go to Stanford.

COHEN: But don't you think a person has to be balanced a bit?

MEAD: Yes, but I think people find a way to do that. Each person's way of doing that is different, and requiring them to do something isn't going to help. All it's going to do is make it worse. I was required to take a bunch of courses I didn't like, and all it did was make me angry, because it was taking time and energy that I could have spent on something useful. And I didn't think that was the slightest bit helpful. Having the flexibility to do things you want to do is very important. But being told that what you want to do is this, is not at all useful. I mean, why should it be that, instead of something else? That's something the person can decide much better than some committee someplace.

COHEN: So you're not really overwhelmed by the idea of this core curriculum change?

MEAD: Well, my feeling is that when you change something, you ought to change it for a reason. If you stand way back and take the broad view—let's just do technology now, because it's sort of a special case. It's gone from being a little thing, like, we had music schools and we had technology schools. It was part of human culture, and the sciences were just one of those things that, if you were a university, you had to have a little of. That's the way it was. Now technology has come to be the major force in the economy, and that's a huge, huge change. And with that has come all of the problems we've talked about—the politicization of our funding.

Also what's come with that is this enormous explosion in the knowledge base in the technologies. You see, there are always two things that happen when you have a very rapid increase in knowledge. There's the sort of bifurcation phenomenon—you get a bunch of little subfields and disciplines and all of that stuff. But then there's the thing that comes behind that, and it's slower. And that is the assimilation of these things and their unification; so there's a whole set of things that come from unifying principles. If we didn't have that, there'd be no hope of ever having an education at all, because the knowledge base is doubling every year or two. In six years or so, there'd be absolutely no hope of getting anything.

It's interesting that a research institution really has two roles that are complementary. Well, there are three, really. The first one is obvious, but it's often overlooked. And that is that it's a flywheel in the knowledge base. In other words, fads come and go so fast that it's very easy to actually lose some knowledge before it surfaces in a useful way again.

A good example is right now. All the universities are trying to put back in place communications electronics—which is something I grew up with, doing radio and that kind of stuff. And then it all went out of fashion when the computer came in. But now we've got communications again, and all of a sudden people have decided, "Gee, what was that radio thing we used to do?" But all those guys have retired. Now we've got to hire somebody who knows something about radio.

Well, it's better if we just don't lose those people. Fortunately, we still have enough of the old guys around who did that and have been doing that all along. And they were kind of out of favor, and now they're back in favor. So there's this flywheel effect. It's a result of the tenure system that we have people who know things that aren't the hot rage right now but are very important. It's often seen as dead wood or something, but no, it's a flywheel. It's a way to preserve a knowledge base so you don't have to learn it from scratch all over again.

Then, of course, we are at the forefront of the research. We are the ones who are coming up with the new ideas, the new directions, who are fighting to get people to see that there really is something here. So we're at the leading edge—or the bleeding edge, as they say.

But then the other thing we do, which is sort of intermediate between those, is that because we teach, we're interested in the unification of knowledge. What I'm doing this year is a pure form of that. I'm not inventing anything, I'm just getting to where I can look at things in a way that makes things that were very complicated much simpler. So it doesn't take so much specialization and work.

If you look at what's happened down through the years, there's a remarkable amount that's happened—a lot of it right here—to make knowledge more compact and more unified. And that's as important as anything else we do. I mean, where else can I get a year to just go do that? That's pretty nice, and it's very important. The thing is, that's as speculative as anything else. Who knows if you're going to actually coalesce enough stuff to have it really be a better way, or you're just going to come up with the same thing everybody has. In which case, you'll say, "Well, I guess that's the way it is." But you won't know unless you try. But until you have that, there's no point railing about it. I had a discussion the other day with Bob [Robert J.] McEliece [Allen E. Puckett Professor and professor of electrical engineering], who was pretty vocal during the discussions on the core curriculum, how we weren't particularly happy about the way the physicists were teaching introductory stuff, particularly electricity and magnetism and those things. And the engineering people knew a fair bit more about how to introduce students to that. It's true, because they use it all the time. But then, when it came right down to it, I was teasing him, "You know, there came a time when you had to either put up or shut up, right?" And he said, "Yes. I finally decided to shut up." [Laughter] And I said, "Well, I think I'd like to put up. I'd like to find a way that we can actually deliver on that promise."

And until you do that, there's no point in arguing about it, because unless you have something to offer, don't offer it. So I don't want to talk to anybody about that until I'm ready to undertake exposing a group of freshmen or sophomores to it. I have one little student in the lab working with me, kind of going through the things, and in a year I'll know if I've got anything or not. And if not, it's still fine. I've learned a lot and gotten a lot of things straight. I'm sure something will come out of it; but I just don't know what yet.

COHEN: This is a pause for you, in some sense.

MEAD: Yes. And it's something I've wanted to do for a long time. The ball really started with Feynman's freshman course. It went a long way, but it just didn't quite—you could see him vacillate. It was very interesting, because I was really hoping he was going to do the things.

You see, I've always been interested in electricity and magnetism, because I grew up with it, and I did the ham radio. And I took all the courses and went to all the lectures and went to all the seminars. I got really, really good at that stuff. Then when I was a first-year faculty member, we had a British guy come by, by the name of Post. And of course, being British, he had this wonderful pedagogical sense that they have. The British learn how to put ideas together so they flow. I don't know where they get it, but they come with it built in, and it's rare for Americans to have that. So he stood up and said, "I want to tell you about this nice simple way to do electricity and magnetism." He talked about using the four-vector potential and the relativistic transformation, and all of electrodynamics falls out in one lecture—just simple, clear, beautiful. I was absolutely blown away. And I was so angry. Why did these guys drag me through this other stuff when you could just do this?

Feynman knew that. What he does in his lectures is, he goes along through the standard stuff, and then he says, "Now, let me show you this." And then he shows the neat way. And then he goes through the standard stuff for a while, and then he says, "Now let me show you this." And you could see where his heart was. It wasn't in doing the old stuff. But somehow—I don't know why—he felt he had to do it. So he dragged people through all the old stuff, and then these little vignettes of what it could be like if you did it the simple way.

But in working on it, I realized that in order to have a body of material, you have to go back and work through all the examples of how to calculate real things that people have gotten used to doing the other way. And they found all the little tricks that make it really simple. You have to do them in this other way. And it very often takes—you know, once you see it, it's easy. But the sense of how to do the little tricky things so it all is very simple—that hasn't been developed for this other way of looking at it yet. Until you have that, you don't have a course, you just have ideas. So somebody has to go back and actually work through all the problems and see how you do them the other way—and is it really simpler in the end, when you do all that? I'm far enough now that I can see I'm able to do that, and it probably *will* be simpler. But there's still a lot, because the other stuff is out of the 1850s.

So it's 150 years of evolution by really, really smart people doing all kinds of really clever things. [Arnold] Sommerfeld started this way of looking at electrodynamics. Well, actually, there's more history than that. [Wilhelm Eduard] Weber actually had electrodynamics that were much simpler. But [Hermann] Helmholtz didn't like it. I think there was a competitive thing. Helmholtz raised all kinds of hell and claimed that Weber's electrodynamics didn't conserve energy—which turned out not to be right. But by the time it was figured out that it wasn't right, Maxwell had already gone off and done his thing, and then everybody was doing it. So Weber never was able to prevail in this, or even be a contender in this. It's only recently, actually—the last few years—that people have gotten re-interested in what Weber did and the fact that, in a sense, we've been on a giant wild goose chase.

COHEN: So in some sense, you're going to be a historian of science before you're finished here.

MEAD: Oh, yes. Well, I spend a lot of time on the old stuff. It's very interesting how ideas get derailed because there's some very strong character like Helmholtz or [Niels] Bohr—Bohr was a good example of a guy who would do proof by intimidation. Perfectly good lines of inquiry would get derailed, just because Bohr didn't like them. It had nothing to do with whether they were right or not; it had to do with the fact that he didn't like them. So it's interesting to see where things went off the track.

COHEN: I can see you're really looking forward to this year. And then you're going to come right back here, because this is where you like to be.

MEAD: Well, I'm here most of the year. I just don't answer the phone. I'm rude to everyone. [Laughter]