Oral History: Interviews with David A. Hodges

Interviews conducted by Christophe Lécuyer
between June 2008 and June 2010
Transcript edited by David Hodges

2004

Table of Contents

Youth and undergraduate education, 1937-60

CL: We could perhaps start with your background and education.

DH: I was born in New Jersey in 1937. My father was an electrical engineer, a Cornell graduate. He worked as a patent attorney, and he moonlighted writing books on electronics for correspondence courses, radio and vacuum tubes and so on. He was a hobbyist, had a shop which he introduced me to when I was about seven years old. I was building radios before I was ten, I guess. That's how I got interested in engineering and physical sciences. School was easy for me, mostly boring. I went to high school in Westchester County, north of New York City. My parents were Cornell graduates and it was quite natural for me to apply to Cornell and quite natural to study engineering.

CL: If we go back to your family, was your father working for GE or was he…

DH: He graduated in the depression and was unemployed for many months and finally found work as an assistant to a patent attorney in private practice and basically got apprenticed into that sort of work. He never studied law. In those days you could be a patent attorney without a law degree, but nowadays you would be a patent agent without a law degree. But he had enough experience so that he did work for GE before WWII. He may have had other jobs in between, but he worked for GE in Schenectady for several years and then he moved about 1943 to a company called Airborne Instruments on Long Island that was doing military work. He was deferred from the draft for doing this war-related work. His career was more like today's pattern, he changed jobs every three to four years. We were in Indiana for three years, before WWII. It was a company called Capehart in Indiana which became Capehart-Farnsworth. I read the Farnsworth biography; quite nice. And then GE. It had happened that one of the bright young engineers at GE, when my dad was a patent attorney, was John Whinnery who became the founding father of the modern Department [of electrical engineering] here in Berkeley. They didn't keep in touch and we only discovered that many years later.

About 1947, my dad moved from Long Island, from Airborne, to Stromberg-Carlson in Rochester, New York. He was doing patent work and then in 1950 moved to IBM in New York City. We lived in the northern suburbs, he took the train. He was with IBM until about 1962 and then they wanted to move him, transfer him, and he didn't want to move so he left IBM and then he finished his career working for Western Electric in NYC, all the same type of work in general. For Western he was doing foreign patent filings which I think was a pretty dull job. He wasn't fluent in other languages so he had to have assistants with language skills, but that was the work he had to do.

My parents were really supportive of their children. All three of us earned PhD's in one field or another. One sister still is Professor at New York University in educational sociology and my other sister is a PhD clinical psychologist in Minnesota. To wrap up the family part, my wife and I met here in Berkeley during my graduate study, and her graduate study in history; and we were married in 1965; and we have two adult children now, both married, and so far, no grandchildren. So should we talk about electrical engineering at Cornell?

CL: Absolutely.

DH: In Cornell, electrical engineering at that time was a five-year program, which I have always been grateful for and that meant more non-technical [courses]. So I had a broad range of liberal arts, Cornell at that time was considered to be one of the good places, but it was definitely an old fashioned electrical engineering department in the sense that handbooks were still a prominent part of the undergraduate study. So there was a lot of empirical aspect to the study, and the science-based approach to electrical engineering was only starting to come in. But the classes were not large and you got a lot of attention from the faculty, and the graduate program was not large. We had a lot of laboratory work, hands-on work, and they had modern instruments for microwave measurements and so on. In my last year the faculty decided that there needed to be a course on transistors, this was in '59, so one of the professors (a number of them only had masters degrees) - one of them went off on industrial leave to Hughes and came back with a bag full of early transistors and some idea of how they worked. He taught a fifth year class of about, I don't know, a dozen students. We had a lot of fun. We had a very good time. It was an early textbook that was remarkably good, by a man named David Dewitt who was from IBM, I think, about transistors and transistor circuits. We used that book.

Graduate study at Berkeley, 1960-65

I had been in California once in the Summer of '58. I decided I would apply to graduate school in California and I applied to Berkeley, and Stanford; and I also applied to MIT. I wasn't at the top of my class, I was about the 80th percentile and I only got into Berkeley. I didn't get into Stanford or MIT. It turned out to be just fine. A very, very important part of my professional and personal development was working with Donald Pederson; and the integrated circuits program was just at the beginning at that time. Basically Don, and Tom Everhart, and Paul Morton were associate professors I think. Paul Morton was quite a few years older. Morton was whatever we called computer engineering in those days. I had for years, I don't have them anymore, some models that he and his students built, for instance a ferrite core memory. It was all on a macro scale, and so each plane, I think, was about 8X8, 64 bits. They all decided, the three of them - and this was not accepted wisdom at that time - that integrated circuits would be important; it wouldn't just be for hearing aids and miniature stuff, it would be broader than that.

CL: It was really early, because the integrated circuit had just been developed at Fairchild.

DH: Yeah. In 1960 when I started, the first Fairchild micrologic circuits were coming out, and at that time the military wanted them for compactness and low power. It was in a very early stage and then against all advice they said "We should build a facility here so we can actually fabricate experimental circuits." Tom and Don were both the kind of personalities that wanted to do something different and didn't accept the idea that this was impractical or impossible in a university. There were a lot of different arms of the defense department that were funding university research and we didn't have any classified work or any requirements of that sort, they just wanted to broaden the pool of knowledge and the number of graduates, so we got a $300,000 grant. I don't know if you have seen the pictures on the fourth floor corridor - we can walk past them - of the first lab. I am in one of those pictures. (link to pictures appended.) So, with that started, we built our first circuits, I think, in '61 or '62 in that lab. Don and Tom and Paul agreed that in order to get help from industry, it would probably facilitate things if they were not identified as a consultant with any one particular company. So that they said no, we are not going to do that. We did get very good help, from a lot of companies. Gordon Moore was with Fairchild then and we got a lot of help from them.

CL: What would they do?

HD: Well, I mean, they would just sort of tutor us a little bit, introduce us to their people, invite us to visit their facility, talk about how things are done. There wasn't any Google or anything then - give us pointers to the literature where the background information was; and generally be supportive in the sense that if anybody asked, or you had to get a reference letter or a supporting letter for a grant application or something, they would help. I wish I could remember all the names, but there were half a dozen or so people at HP and Fairchild, I'm not going to remember them all, but a number of people mentioned in your book. They would come in, we would invite them, they would come over and speak about their work. Very helpful. Your book really passes by the Berkeley site completely, but when Stanford started, they came over and basically copied everything we had done. There were a couple things. How could you make the masks? The machines were way out of our price range, so we had an old machinist here and we said this is what we want to do; we want to make repeated images of the same pattern, so he built us a little fixture which was something that looked like a T-square with two T-bars and we could, manually, move things along by hand and get the positioning right with this T-bar thing; very clever. Stanford copied that exactly.

CL: Was this in ’66?

DH: Let me see. No, we were doing all that in ’61 and ’62. They [Stanford] were later, several years later. There was device work at Stanford before that time and I think [Jim] Gibbons was one of the pioneers in that work, but they hadn’t really done anything about integrated circuits, so we were able to do that and get papers accepted at the major conferences and get a little respect from the industry and then in time the graduates were in very big demand. And so we had a lot of bachelors and master’s graduates who became leading players all over the country, and even in those days some of them outside the US. So, it was a very exciting time for us. I always felt we were so lucky to be there at the time we could come along and skim a whole lot of cream off the top of the few things that could be done with limited resources and still would cause people to take notice. That’s much harder to do when a technology matures.

CL: You were the first ones.

DH: Right.

CL: By what name was that lab known at the beginning?

DH: As I recall, it was named “Integrated Circuits Laboratory”, to distinguish it from existing solid state electronics labs. Also, I think that was part of the title on the first research grant. Later, when the capability for making very small things became widely used, the name evolved to “Microfabrication Laboratory,” or Microlab.

CL: Would you want to talk about the principals, Don Pederson and Paul Morton and some of the other people involved?

DH: Sure. I don’t know Paul’s history, but he was a very smart man who had been very early getting into computers. This department started in electric power really and Wendell Cory was a power guy. Did I send you that thing a student wrote, a very nice historical piece about the department? He took a history of technology course here on campus and did a term paper; it’s a beautiful piece of work, about 90 pages. (link appended.) He went out and found a lot of old photographs and graphics. The first goal of the department, if you talk about “research,” was on power transmission from the Sierras. There was lots of hydroelectric power available and unfortunately this guy Cory was a mechanical engineer. AC, alternating current, mystified him. So he thought it should be

DC, but direct current is really not the right way to go for a distance. So, they never really established a position of prominence or leadership. Right after WWII, I think the first really prominent program was in microwave tubes, where they had several faculty including John Whinnery, and Ned Birdsall, and a couple others that were from Hughes or companies that are in your book. They worked with those companies and joined the faculty. So, they had a shop here and they could make traveling wave tubes and magnetrons and everything else to the specifications for the research.

CL: So the same people in the shop started making equipment for semiconductors.

DH: The machinist was a remarkable guy, he never had any college degrees. His name was George Becker, and when he retired from the university he set up a little independent machine shop. "Machinist" is inadequate; he was much more than a machinist. We had glass blowers and some people who could work quartz for scientific application. We had a very well staffed shop and in those days it was part of the core budget for the University. It was budgeted right alongside the faculty and everything else, but that's no longer true. We do have some of those services, but they have to do everything on a recharge basis.

It's always been that the faculty salary for nine months was covered by the core budget, but the summer salary [was not]. So, it was customary, by the time I came, for the faculty to apply for grants, get grants, pay themselves summer salaries from the grants as well as paying the students and other expenses. So, Paul [Morton] was early in computing and we didn't have any computer science, of course, in those days; and Paul was definitely a hardware-oriented person and, as I said, built things. Some of his students went on and became leading figures.

Everhart was a pioneer, he studied in Cambridge [UK] on electron beam lithography. Then I believe he worked at Westinghouse in Pittsburgh for a while, where they were at that time a leader. Everybody was saying well we're going to make, instead of this photographic masking, we're going to be making circuits with electron beams. They're still talking about it today, but now they're talking about to get the throughput rate, you have to have a lot of beams all at once. So they are getting away from the big evacuated column and the electron optics and so on down to the point of making micro emitters, like little arrays of points that are just held a micron or less from a surface and selectively energized to make the patterns, but this is still very problematic. So, research labs do use electron beams to make sample devices in very small dimensions, but it never has been a production method. Everhart was a leader in that field and he was very much a pioneer in a lot of things, but quickly found his career as an administrator. He was department chair here, then he was Dean at Cornell, then he was Provost at Illinois, and then I think he went directly from Illinois to President of Caltech. He preceded David Baltimore at Caltech. Everhart is about 75 and he is retired in Santa Barbara.

CL: If we talk about beams, there was also a big push in the early ‘60s to use ion implantation.

DH: Yes. We had absolutely zero capability, ourselves, except that we did get the money to buy a machine [ion implanter], but that was later. That was after I joined the faculty. We got a very good deal from one of the suppliers and we had that ion implanter here, and we used it for about ten years and did some very interesting stuff, but it was very maintenance intensive and implantation became readily available as a service. So, for at least 20 years now, we take the wafer down to the service. Aside from making a lot of things better, ion implantation made it possible to do things that were almost impossible to do some other way. For instance it made high quality CMOS possible. With just the native silicon material and the diffusion processes, it was very hard to get the right combination of dopants and electrical properties, but the implantation solved that. But there were a lot of other things too like the fact it geometrically aligns things, which is helpful in improving the devices.

CL: What about Ernest Kuh and Robert Pepper?

DH: Ernest Kuh, who was really a circuit theory guy, became involved in integrated circuits only some years later and mostly from the standpoint of the design aids, the computer design aids for circuits. Robert Pepper was one of Don Pederson’s first doctoral students. He joined the faculty when he finished his PhD and I think that was just as I started in ’60 or might have been ’61. Pepper was very important when we were setting up the first lab. Despite their intellectual leadership over this enterprise, Pederson and Morton were never, never seen in the lab, and Everhart only with the electron beam. That was ok because we had eager graduate students and in those days, Bell Labs was publishing everything, and we had friends from industry that came. Pepper was a really energetic guy, and as an assistant professor spent a lot of time in the lab. So we didn’t complain. We would tease Pederson a little bit, that he had never processed a wafer himself. It didn’t bother him.

CL: What about Pederson himself?

DH: Well I sent you a biographical piece. Pederson was the godfather, but in a very friendly way. He was always accessible to talk about your problems. He would figure out how to proceed, where to get the help you needed or whatever it was, and he was very generous in sharing whatever he had in terms of resources. When I joined the faculty, he said: “here are the grants I have got, basically you are my partner in all of this” - not in a formal sense, but I could draw upon those, those resources subject to the limit we jointly had to put on students and so on.

When I arrived as a graduate student in 1960, Pederson was the Director of the Electronics Research Laboratory. The organizational structures have changed somewhat with time. At that time, that was one of what was we called, I don’t know if we have this term any longer, it was called an organized research unit, ORU. The ORUs in those days had both the administrative responsibility once the grants were in place, but also, at least in this case and with, say, the Space Sciences Laboratory and a few others, the leadership of the ORU was responsible for identifying prospective sources of funding and for relationships with agencies and with industry. Pederson was wearing that hat at the time, so he was actually a really busy guy, not only responsible for the integrated circuits, but for a number of other areas. He was a high output guy, so we didn’t feel short changed.

The integrated circuits laboratory was within the Electronics Research Laboratory and then after, a few years along the way, I don’t remember just when it was, Diogenes Angelakos became the director of the ERL and Don didn’t have that job anymore. Don took off a couple times for overseas because his first wife taught in schools and she got a position in Australia, I’m trying to remember which way the teacher flow went, but there was an extreme shortage on one side of the ocean and a surplus on the other side. I can’t remember. It was either American teachers going to Australia or the other way around, but he went with her to Australia as part of that. I think he went twice. Bob Meyer, who was a colleague, came from Australia based on the contacts made then. Bob came as a faculty member and then Richard Newton and Ian Young and I think one more came as grad students. When Don was away, the first time at least, Bob Pepper was the supervisor for the students. It was a very tight group who would often go to lunch together and socialize together. Play darts, drink together. Not Don [Pederson], but Pepper and the doctoral students were a very tight group.

CL: Could you discuss the ways in which Pederson, Everhart, and Morton set up the lab and the early decisions that they took?

DH: One of the principles that Everhart, and Pederson, and Morton agreed upon was going to be one lab and they would all share as equal partners and if others wanted to join, they could be accepted as equal partners too - which was definitely in contrast to the pattern of most experimental academic programs, where it was Professor X’s lab, right, and only X and his students, and everything was under his control. So, we had another leading guy in solid state electronics at that time, Shyh Wang who had come from Harvard. Wang did not oppose to this model, he did not quarrel with this model, but after three or four years he saw the advantages that he came in. We pretty well followed that policy and it is a hugely valuable policy. When you are recruiting a new faculty member, the competition might offer 1,000 sq foot of lab space and a million dollars and we offer 10,000 sq foot lab space, fully equipped with operating equipment and technicians, and everything; and you are an equal partner. But the key to making that work, and the reason a lot of people oppose it, is that when you don't have enough of the right lab discipline, one person can screw up everything. Everything can be put out of order and damaged.

Don recognized that at the beginning and I think Everhart did. People with the right skills weren't easily available, but Pederson had a wonderful eye for people at all levels. We had a woman in the office. She was just a secretary, but a lot of hands on skills. "Dorothy, we are going to train you into this, you know, this is your new career." Well, she was excited, she was very happy about it, and she never had the scientific fundamentals, but she was very good at establishing the culture and maintaining the discipline. She was in her forties I guess and the students were all in their twenties, thirties, and she was sort of the iron mama. For a period of time we had a professional, a chemist, a woman with I think a master's in chemistry, who technically knew a lot more, but absolutely did not see the culture aspect of it, the discipline aspect. So, that was one of the keys to the success of the multi-user lab.

CL: Was the chemist the lab manager?

DH: She was the lab manager. I don't remember all the intervening history, but since about 1980 we have had a woman, Katalin Voros, who is has been the lab manager. She came out of Hungary at the same time as Andy Grove, they're acquainted, and she earned an engineering degree in New Jersey and worked for RCA in the semiconductor business; and then she married Charles Tobias, who was a chemical engineering professor 15 years her senior here at Berkeley. She moved to Berkeley, she took a master's degree working with Bill Oldham, one of our colleagues here, and we hired her. You couldn't have a better person for this job. She is both up to the master's level in terms of understanding [the technology] in depth, but also a superb manager, just a superb manager. We are going to be moving into the new building in January 2010 and I think she will stay to the end of next year. She has trained her successor who is a doctoral chemical engineer here named Bill Flounders who has been here four or five years now. He is definitely the right guy, so the culture survives.

CL: Stanford had a similar set-up.

DH: They had Jacques [Beaudoin].

CL: I know him.

DH: I'm sure you do. Is he still around there?

CL: I think he retired 3-4 years ago

DH: I remember him very well.

CL: How did Pederson, Morton, and Everhart secure the lab space?

DH: The lab space always was an issue, and Pederson and Morton and Everhart had to fight a little bit to get the space. I don't know how hard that really was. It was quite a bit of space at the time. I think it was about 5,000 sq feet when we started. There had to be some minor renovations done, but you know a lot of stuff was never done the way it really should be for that sort of work. Over the years for instance, before anybody paid any attention to these things, we would put chemical reagents down the drain, and the pipes would get eaten through, so it was really on a shoe string for about 20 years. Then, in the 1980s, after a very long fight, we got state money to expand it a lot, and what you see right now down there. It was about $2.5 million in state money. We may have inflated it a little bit, but we claimed we got $18 million in private support mostly in terms of equipment and computers, but some cash too.

CL: Did student unrest in the 1960s and early 1970s impact the microelectronics research program at UC Berkeley? Did it lead the faculty to move away from military funding to other types of funding such as industrial funding? Did it lead to a reorientation of the research program?

DH: The student unrest in the 60s had very little impact on engineering students and faculty. I recall only one incident when a professor funded by a DoD agency felt threatened. No one moved away from DoD funding. There was no particular impact on the integrated circuits activity. The engagement with industry grew based on faculty contacts, especially with our graduates who became early leaders in the microelectronics industry.

CL: How did the faculty keep the microelectronics capability up to date as the technology advanced?

DH: In the early 1980s we expanded and upgraded the microelectronics facility on the fourth floor. We had some state funds for that. Subsequently the fifth floor was constructed with a focus on the EDA activity, largely from gift funds. None of the microfabrication is on the fifth floor. I'm fuzzy on that history. We were pretty continuously either in a planning, development stage, or in the execution stage. The first lab was in '61 and then in the early '80's, that was 20 years, and then I guess the new Marvell lab is counting as the third one, the third lab. We've been planning this thing for a decade. That's the way it works in a mature institution. When they built the new engineering building in Davis, they were building it from scratch, and they said well, we want to do microelectronics; we've got to build a lab. So basically, the state funded much more than they ever funded here in terms of a purpose-built building, properly designed for this work. But they [Davis] didn't have the leadership to make it run. So they have basically under-utilized facilities out there.

CL: I talked to Robert Bower about this.

DH: A lot of people thought that Robert Bower was going to come in and be the leader and sort of put it all together, but he didn't have the right chemistry. You've got to work with what's available. The physical resources were great. The human resources were variable, but you have to work with what you've got. I think Bower spent about 25 or 30 years at Hughes. A lot of people think they are going to retire in the university, but I haven't seen anybody succeed in that. You know, if they retire in the university they don't have any impact.

CL: How did the group here equip the laboratory?

DH: We built equipment, we bought equipment, we got gifts of second hand equipment, we bought second hand equipment, we always tried to have a priority list of what do we need most. Then industry got to be very helpful. There has always been a market for used equipment and there's a market price that can be found and so they can donate it and take the tax deduction and it actually usually works out almost the same to them as if they tried to market it through a second hand channel. So, we have never had a really tough time getting what we needed, when we needed it.

CL: In 1962, Don Pederson announced that his group had produced the first working integrated circuit at a conference of the IRE.

DH: Yeah, 1962 the first working circuit, the first integrated circuit. We should get out and look at the historical wall. I think Gary Hachtel made the first working circuit and I think it was some kind of an oscillator. It was with bipolar technology. They gave the paper at a conference. So, at that time it was just: could you make all the elements necessary for a functional unit like an oscillator on the chip and not have to have anything else besides the power source?

CL: That was really early. Industry went to analog circuits later than.

DH: Our assessment always was that there are a lot more degrees of freedom; there are a lot more parameters that must be considered in an analog circuit. Digital was being very heavily pursued by industry and so I think a much larger fraction of our recognized output had an analog flavor. I had worked on semiconductor memory at Bell Labs before I joined the faculty and I was fairly familiar with what was going on in the industry. I had one memory project early and one high speed logic project. I had a lot of trouble getting the logic work funded. They said, “well the industry is doing all that,” and the memory project, we were trying to do a multi-level memory which is actually the way a lot of the flash memory is made today; you store more than one bit in a single transistor cell and we built something, but it was a different approach. It was a junction field effect transistor, where you had an isolated junction that stored the charge and could store several different charge levels. It was pretty complicated. The student succeeded, made a working chip, gave a paper.

Everybody was saying: “the world is going to go digital and so analog is passé and why are you wasting your time on analog?” But we said: “people are analog and the real world is analog and there probably is always going to be some need for analog.” Wee ought to have a bibliography of the work out of here. What they did at the Solid State Circuits Conference three or four years ago when it was their 50th anniversary, they had a list of fifty year highlights. We have five out of the thirty or forty items there, which were mostly our early analog stuff. Bob Meyer was RF, radio frequency, well that’s always analog, and Paul Gray, who came in, had worked on operational amplifiers at Fairchild. So there was a lot of analog background. I was the only guy with professional experience on digital. So I taught some of the digital courses. I always taught the digital courses and I taught the digital computer engineering courses too at the beginning. So I remember teaching about ferrite core memory because you still had to know something about those.

CL: If we move to circuits, Bob Pepper was working on integrated circuits. Was not he?

DH: Bob worked on tunnel diode circuits for his dissertation, which turned out to be kind of a dead end. He did a better job at characterizing tunnel diodes. But two terminal base band circuits never work. In RF or tuned systems for microwaves, two terminals can be used, a circulator or something, but in a base band circuit, it’s terribly hard to keep the input and the output separated if there are only two terminals on the device. So, you could build demonstrations, but you couldn’t build practical stuff.

CL: Could you discuss your MS project on exponentially tapered distributed RC networks with filter applications?

DH: My master's project was really influenced by Ernest Kuh. We were looking for topics, and we all thought that we wanted to be able to realize almost any function; and filters were a goal. The traditional lumped parameter filter was a RLC. Well, we couldn't make useful inductance in those days, so we had to do it with RC. A distributed RC was the naturally occurring kind of a structure in an integrated form, as opposed to trying to lump it. If you have discrete components, lumping makes sense. So, if you taper the RC network then you can get a somewhat better relationship of phase and gain. That's another degree of freedom in shaping a frequency response. So you can get a sharper cutoff filter or you can make a better band pass filter and so on. A lot of that was not experimental, it was computing. I used the Bendix G15 computer which is the same one that now is in the Smithsonian. In the Smithsonian they identify the G15 as the first minicomputer, 15,000 vacuum tubes. It says on it, "donated by Harry Husky." He was a faculty member here, I never knew him, but he came from Bendix and he was part of the team that developed that computer, and he brought one to Berkeley. It was always here, but apparently he retained title to it and later gave it to the Smithsonian. So, this was programmed in binary, happily would deal with decimal data, and with the aid of a paper tape subroutine it would do transcendental scientific calculations; and the output was an IBM electric typewriter.

CL: You got to wrestle with this thing for quite a while.

DH: Right, so I had studied old-fashioned constant-k and m-derived filters at Cornell, and then I came to Berkeley; and oh my god there is this whole world of approximation and realization. You approximate the function that you want and then you synthesize to implement it, directly from fundamental analysis. It's not that hard, and I'm amazed it didn't occur sooner but Guillemin at MIT pioneered this and Ernie [Kuh] taught it. He was a superb teacher. I once wrote, that he was the best classroom teacher I ever had, he was so well organized and so clear. English is not his native language. His English is not perfect, but that was no impediment, he was absolutely, totally clear. So, that was fine. That was in '60, '61. The lab was just getting started. We really weren't equipped to do experiments and my MS project was done with a visiting professor, not with Don, but Don was overloaded so he said why "don't you work with Roy Boothroyd" who was from the Imperial College I believe - a very, very fine man whom I have kept in touch with and saw a few years ago. A classical British electrical engineer, very sound, modest, very nice man who is retired now in Canada.

CL: Tell me about your PhD project.

DH: At that time, the synthesis of analog circuits, particularly filters, had advanced quite rapidly. By synthesis, I mean you start with a specification of what you want and you have a systematic step by step process that will produce a circuit that gives you the desired response. So my advisor, Don Pederson, said: “let’s see if we can do some of that with some digital circuits and in fact with sequential digital circuits” – ones that have a history that they act through. We were just starting, touching the edge of what later became a much bigger project with more people. I had some degrees of freedom there. We decided we could use different kinds of transistors, specifically bipolar transistors and unipolar transistors. This was before MOSFETs. I went through a catalog of all the possibilities of connecting these and showed that we could synthesize circuits, some kinds of monostable and bistable circuits. Then Don’s view was and I agreed with that and in retrospect it was very important – “now we have got to make one.” We had the first lab here just starting. So I designed a couple circuits to be made in an integrated form and learned how to do the processing in the lab. Many, many hours. Made the chips. It was a very good experience.

CL: Were you one of the first students to use the lab?

DH: Mine was the second circuit. Gary Hachtel had made the first circuit. And then there was a whole stream. The circuits are all on the wall [in Cory Hall]. Have you been to look at the wall?

CL: I don’t think I did. We should do that.

DH: I will append a link [to the interview transcript] with all the pictures.

CL: Please discuss your fellow students and their subsequent careers.

DH: Gary Hachtel worked at IBM for 10-15 years and then he went to the University of Colorado and was Professor there until he retired a few years ago. Art Broderson went straight to the University of Florida, I think, when he finished and then later moved to Vanderbilt – an academic. Some guys finished with a master’s degrees and they went to industry. George Haines went with Pepper to Sprague and Al Thiele went to Southern California - all over the place - but we have this big conference, the Solid State Circuits Conference which is every February, not so much now, but for a good many years, we were all there every year and we got together. Bill Howard whom I did not overlap with, but came in just after I did, became a Vice President of Motorola. Noel Mc Donald was Everhart’s student, but he is a good friend for many years, and he worked for Rockwell for a while and then he founded a company called Physical Electronics Industries, Inc.

which did an instrument called a scanning Auger spectrometer, and the advantages of this particular instrument is that you can analyze the chemical composition of a very small area of material. If you have a little contact window for integrated circuits and you are worried about contamination, you scan it with a beam and then you can analyze. It has a high sensitivity to very low concentrations. That company was acquired by Perkin Elmer and the founders walked out with quite a few million dollars. Noel worked for Perkin Elmer for a while and he became professor at Cornell and then he spent a period of time at DARPA. He was one of the program managers at DARPA and after that he said he promised his wife that after that they could move to California. He is professor at Santa Barbara now. He is an extremely creative inventor and he started about three companies down there. Let's see, whom am I forgetting? There are a lot more.

Some of the students I remember from the 1970s are Jim McCreary, who always worked in Silicon Valley I think, in a succession of smaller companies, kept getting pushed into management jobs but …. He was a little older because he was a military veteran before he came.

Ricardo Suarez was one of our early students on the A to D. Ricardo originally is Venezuelan; he married an American. He worked for Intel for many years and was our Intel liaison. You know I am moving up through the years now. Ian Young is an Intel fellow. The original switched capacitor filters came in two sorts. The classical approach to making filters was that you make a basic element called a second order section which has two mathematical poles available and you can adjust them to be anything you want, and that's the one that Ian worked on. But the more successful version is basically you make a single pole elementary function with integrators. So we had parallel projects about the same time, each one learning a lot from the other. David Allstot was working on the integrator filters and he is a professor at the University of Washington now. There are a lot more; we didn't have a lot of women in this group, I'm not sure why.

A year in Denmark, then Bell Labs; 1965-70

CL: Would you want to talk about your postdoc in Denmark?

DH: My wife and I were married in 1965, just a few months before I finished my doctorate and we decided that it would be nice to spend a year abroad. These were the days when jobs were not so hard to find and I figured that I could postpone a career position for a year. Through a very fine man who had been a visiting professor here, a professor at Technical University of Denmark, I had what amounts to a postdoc position there, in Denmark. I did a little bit of teaching and I helped them plan a facility for making chips similar to what we had here. That was a planning project. During the whole year, we got a lot of it planned. But they never got it built for a couple years after that. But they did finally build that. I extended my experience with computing there. I started computing here [at Berkeley] with the computer center. I think it was an IBM 709 and then a minicomputer, a Burroughs G15 minicomputer. In Denmark, they had a GIER computer which was a Danish design – a minicomputer- and Algol. I worked with Fortran here. I worked with Algol [in Denmark] and basically did some computing to simulate the performance of circuits. So I became comfortable with using computers for these purposes. You know, this was 1965 and computers were just coming to be important. It was a lovely year. We had a chance to travel all over Scandinavia and down to Greece and Paris. We enjoyed that a lot and came back with negative net worth (laughs).

I started at the Bell Laboratories in fall 1966. We rented a house. My wife was a high school teacher near Bell Labs in New Jersey. Bell, you know, was the icon in those days. I had interviewed at Bell, and IBM, and RCA, and several others. I had serious offers from IBM and Bell and I initially decided to take the offer at IBM research in Yorktown Heights. That would have been working on semiconductor lasers. Then midway in the year in Denmark, Tudor Finch from Bell Labs, who turned out to be my first boss up there, called up and said: “Dave, I have a terrific opportunity for you. I wish you could reconsider your decision to go to IBM.” And he said: “I would like you to join our project on semiconductor memory.” This was a brand new concept. So I thought a little bit. Then they upped their offer to match IBM’s offer. So I thought about it for a few days and decided I would go to Bell. They had already built a first generation bipolar semiconductor memory chip, a 16-bit chip. My project was to see if we could make it out of MOS, which was already seen as the low cost alternative coming along. So we ended up building a 8,000 bit memory module, with 24-bit chips. The assembly was a terrible problem.

CL: Was it MOS?

DH: MOS chips, PMOS. Not CMOS. The memory array was made out of PMOS. But in those days the minimum features were 5 microns or more. They were very slow. So to get a respectable speed, which at that time would be a microsecond, we had bipolar transistors for the circuits around the edge to read and write. It turned out to be a nightmare of, you know, thousands of chips with all these little discrete bipolar transistors. We had a technician, a young man who was just amazing, who could stitch the wires on. It was all wire stitch bonded. But it was enough to convince management that this indeed would be possible. They had a very big need for this. They had an early generation electronic private branch exchange (PBX) that needed this 8,000 bit memory. They were working with ferrite core memory. It cost 50 cents a bit for the completed module. It was $4,000 for this 8,000 bit memory. They said: "can you make it cheaper with semiconductors?" We envisioned that we could indeed. The feasibility demonstration convinced them to begin a serious development effort which was started about 1967 at Allentown - Western Electric at that time. It took them quite a while because they were hung up with beam lead technology, an interconnect to replace the wires. IBM had a ball technology, called SLT. And Bell's answer was beam leads. Beam leads had very exotic metallurgy - multilayer and finishing with gold - and it never, never proved to be manufacturable at least with MOS. So by the time they got all through replacing that, it was 1970. They did manufacture a 64-bit chip designed to go into this 8,000 bit module. That was just the same year that Intel announced its 1024 bit MOS DRAM, dynamic RAM. Ours was a static RAM. I did not take part in the Allentown work. It was another crew there. So that was what they always did. They handed it off to another team. Of course it had to be redone from scratch for proper ownership.

CL: What other projects did you work on at the Bell Labs?

DH: I became quite interested in the whole evolution toward digital switching and could see that electromechanical switching was going to be replaced with electronic switching. So I worked on some designs for the switch matrix. But also I became interested in A to D conversion for voice. It's 69. I was offered a promotion to department head in research at Bell Labs at Holmdel. Some years earlier, the research people at Bell had decided that silicon was not research anymore. It was development. So my initial job was in development. But then Bell began to see that silicon was actually going to be much more important than they thought at first. Maybe research ought to have some involvement. They appointed me the head of the department, which was called the System Element Research Department at Holmdel. I was only in that job for a year. What they did is that they gave me a group of mostly rather senior people who in the tradition of research were each working on some little project and interesting on a small scale but not so attractive maybe as part of a bigger plan. My boss said: "Why don't you hire some young guys?" So I recruited two young Berkeley graduates who had also worked with Don Pederson and they said: "well, we really have only one slot this year. But you can make two offers because you will probably only get one." I made two offers and got them both. They both came. One had already come and the other one had not come yet when I got the offer from Berkeley to join the faculty. So I went off and came back to Berkeley. These two guys I still know very well and I am very friendly with them. One is Bruce Wooley at Stanford and the other one is Gary Baldwin who has been involved at CITRIS here as a staff member. But I had learned quite a lot about the telephone business. That had been a sleepy old business for forty years, right? The dial phone was the same one you had in 1930. You could begin to see that everything was going to become electronic. When I got here to Berkeley on the faculty, I thought that it would be good to work on some of the pieces that were going to be needed to make this a reality. One was strictly digital components - logic gates and memories and so on. I soon decided that the semiconductor memory work was going to be very capital intensive and that it would be very hard to do publishable work on memories at a university because the technology was advancing very fast in industry. I really had only one doctoral student who did work on memory. But I worked on the A to D and D to A conversion and then we worked on the filters. In all of this work Paul Gray and Bob Brodersen were very important colleagues.

A move to teaching and research at Berkeley, 1970

CL: Could we talk more about the offer that you received from Berkeley?

DH: I always had in my mind that I would like to make my main career as a professor and Don [Pederson] said to me – I can't emphasize enough the influence that Don Pederson had on my whole education and career – he said: "Well, that would be great; but you would probably benefit from some experience in industry." He had done that himself. He had finished at Stanford and gone to work for Tudor Finch at Bell Labs, the same guy that I ended up working for. Tudor had no PhD and did not understand a lot of the modern stuff, but he was a very good first supervisor. I learned a lot from Tudor. He would say: "You might be right, but you could be dead-right." In other words, think about how hard you press your viewpoint.

When I got the offer from Berkeley, in those days, they did not have the same mandate for faculty search than came later. The department would recruit with a rifle. They would identify a candidate they would like to have. Don was my advocate here and he said: "Well, this is a growing field and we need somebody else." He had already lost two assistant professors who resigned. Both of them went to industry. They gave me an offer. I had been in Denmark and was five years out post-PhD. I said: "Well, you know, I hate to leave this good job at Bell without a tenured position." So I was offered a tenured associate professorship in 1970 and had a salary which was about three quarters of what I was making at Bell Labs – you know, nine months salary. It was actually on par if I had any summer salary. Don said, as it was his practice: "If you come, Dave, you are an equal partner. I have got some grants. I will share them with you. We will work together. But on the other hand, feel free to go and work with others as well." He could not have been a better mentor for a young faculty member. We still had the original lab which had been built in 1962. There was still a lot of student work going on in there. My students in general built things in the lab. I think all of us profited from having the experience both on the design side and on the processing side.

CL: How different was the integrated circuit group in 1970 compared to the one you had known as a student?

DH: Well, a lot of things had happened. Three young faculty members had resigned: Bob Pepper, Graham Rigby, and Bill Howard. They were all graduates of this program. All had worked for Pederson. All stayed on after the doctorate, stayed on immediately. Don and all of us came to see that it is terribly hard for a PhD student to just continue on in the same department without an independent period somewhere else. Those three had all gotten restless. None of them was denied tenure or anything. They just got restless. They took other jobs. So Pederson was still here, right. Paul Morton, I think, had retired. Tom Everhart was I think Chairman of the department, somewhere during that time. I knew a lot of the faculty people. I liked teaching and I was not sure how good I would be at teaching. But I became pretty good, I thought. I got good ratings from the students at least. You know, it was not all that much different, except that I had the huge advantage of this one year in Europe and four years at Bell Labs, which gave me another complete perspective on technology and research life. We were all on a growth path. I had the advantage on drawing on an on-going group with an on-going experimental facility. I did not have to build it and I knew how it worked. I think there could not have been more favorable circumstances for me.

CL: Which courses did you teach in the first half of the 1970s?

DH: I was really the only one teaching digital circuits courses for quite a while – I and Bill Jackson. Jackson was a lecturer who was half time, maybe he was 40%. He was a staff member at Lawrence Berkeley Lab. A very good circuits guy. Not a researcher really. A design-oriented man. Very British. British born, then time in Canada, and then he came here. A good teacher. Bill and I taught the digital circuits courses and then we ended up writing a textbook together. I also taught one or two of the computer engineering courses, particularly ones relating to memory and input-output. This was before computer science was really formally established. We were EE. We were not EECS at the time. But we did have this kind of courses that Paul Morton had originated. I taught both undergraduate and graduate level courses.

CL: Was Andy Grove teaching at Berkeley then?

DH: I don't think he was teaching in 1970, when I got here. But like 72, 73. For three years, I think, he came as a lecturer. He had written a very good book that is still referenced and he taught that course. Very, very fine teacher. Students loved him and he was very authoritative. He had the experience. He was at Intel by then. Around the third year that he was doing that, I became vice-chairman of the department for instruction, which means figure out who was going to teach all the classes. Andy was here one day and I remember we went to lunch together and I said: “Andy. I hope that you will be able to do this again next year.” He said: “Gordon [Moore], Gordon said to me the other day, he said, maybe you better stick here at Intel,” something like that. A few years later, Andy became the CEO [at Intel]. So we lost him. But we had good people teaching these classes, Richard Muller, and several others who teach these classes.

Research on memory and digital circuits; SPICE: 1963-75

CL: Could you discuss your work on memories at Berkeley?

DH: I did a little bit of work on memories and memory cells at Berkeley. But it was very hard to get grant money for that. As I said, it was hard to do competitive work within the constraints of our lab and our capability.

CL: Did you consult for memory companies?

DH: Well, I did quite a few times. I taught a short course on semiconductor memories through UC extension. Usually, I got a big crowd, because a lot of people saw this was going to be important. For the people who had been doing magnetic memories, it was all new. Sometimes I taught it with Richard Muller who was more the device side of it. I was the circuit and system side. We probably taught that six or eight times in a span of three years. Once or twice in Europe and we taught it, I think, once in the East and several times here. I got calls about consulting based on that. But not from the companies designing the chips. I don’t know why. I got them from companies who were trying to use them and who wanted to understand their limitations and so on. One of them was TRW Vidar. I think I spoke about that earlier.

CL: Yes, we talked about it. This helped you to get access to know-how and knowledge in communications.

DH: Yes. I also taught the memory class once for Lockheed, which was then in the business of making magnetic memories. Actually, I had a consulting job with Signetics for several years, for maybe two years. They wanted to take a whole wafer full of memory chips and figure out how to wire them. There was a name for doing this, wafer-scale integration. You know, figure out how to wire up the good ones, without cutting them apart. That did not come to anything really.

CL: You also did some work on computer aided design.

DH: I did present a CAD paper with Don Pederson. That was sort of a user's perspective, a circuit designer's perspective on computer aided design. I can't claim to ever have made much of a contribution. At Bell Labs, I had done one project modeling the MOS transistor for use in computer simulation. But that was about the only contribution I ever made. That work was with Harold Shichman at Bell. Don was overseeing much of the circuit simulation work at Berkeley. But Ron Rohrer was also very important. Ron was the big contributor of fundamental techniques and analytical techniques that could be applied. I don't think Don ever wrote a computer program himself. But he had the big picture. Ernie Kuh became part of this and the whole sequence of different programs and the whole method of doing it using students as the guinea pigs came to be - get the students to help improve the programs. Don would say to the grad students: "You've got to be on call seven by twenty four, when the students have trouble with this program. You have to help them." It was not literally true, but there was an awful lot of interaction between the grad students and the student users.

CL: The grad students were writing the code ahead of the users.

DH: Yes. The students generally liked this and it was very complementary to their, you know … here you are doing design and the philosophy of – "We will do a simple back of the envelope design," as Don would say. "Get a first order idea of how this work but if you want a more refined solution, we will try to refine it by hand calculations and with the computer." It was always important to do the raw calculations first, just to make sure that the orders of magnitude are right and so on.

CL: This would be for class projects?

DH: Undergraduate classes. You know, undergraduate weekly assignments.

CL: Were they using the computer tools to design the circuits?

DH: Yes. You know, it was hard because it was punch cards in those days. They had to punch their cards. We had card punchers all over the place. They had to take them over to the computer center. I think we had a remote entry station. You drop the cards off and you come back in an hour or two with your output.

CL: Did every EE major at Berkeley learn to know how to do this?

DH: Yes. This, I think, greatly affected the industry. They would go to work in industry. If they did not have SPICE available in industry, [they would say:] “I can’t do my job without SPICE.” When we did the new lab, by 1980 or so, the students were all tuned into this. Eventually, when we built this fifth floor that we are on right now, Digital Equipment gave us a large grant and they said: “SPICE sold a thousand VAXes” – a thousand VAXes! You see, the VAX was the first affordable computer capable of running SPICE. Before that, we had all done it on a CDC or an IBM mainframe. But you could buy a VAX for a hundred to two hundred thousand dollars. DEC eventually had their own version of SPICE with enhancements or adaptations.

CL: Was SPICE first developed for analog circuits?

DH: It was not really first developed for analog circuits. It was general, because it was really mostly in the beginning time domain. So you could provide a sine wave in an analog circuit in the time domain. You could simulate that. But you could simulate an arbitrary waveform. So you could do digital just as well. Certain features of SPICE were particularly for analog, such as noise and sensitivity analysis. That turned out to be very valuable to people. It was very hard to do that in any simple hand calculation. The other thing that had to be done is that SPICE had to be able to handle large circuits. So by the time it got to be SPICE, it had what’s called sparse matrix techniques. When you have a circuit that has a hundred nodes, you can’t have a hundred by hundred matrix. You can’t invert that matrix every time step. It is just computationally too hard – too much computation. But if you have a matrix representing the circuit with all its connections, it is nearly a diagonal. There are an awful lot of off-diagonal elements that are not present. So you need computational techniques to take advantage of that and don’t do the computation for all the zero elements. So Ron Rohrer brought these capabilities into SPICE and that made it possible to run a hundred node circuit on a VAX. The first version of SPICE, I believe, had the sparse matrix techniques.

CL: Was SPICE developed when you were at the Bell Labs?

DH: Yes, SPICE was ’71. SPICE went on through continuing evolution and we used it in the circuits courses. That was starting before I came back. Don started it in the late 60s. Don advocated a very open approach to this development. First of all, he was influenced by how important to the improvement of the program it was to have all these student users. But the student users were not going to be the most challenging users. Also he wanted to have people in industry. Don had former students and other friends, particularly at HP and Tektronix, the instrument makers. There were some sensitivities about proprietary information in those days, but in the case of these instrument makers, they said: “Well, we need to be able to design and simulate circuits for use in our instruments. But we will never provide merchant catalog circuits to the industry at large. So we think that we don't have any sensitivity about sharing what we have learned about circuit simulation" - even when it was HP and Tektronix which were rivals in some places. Of course, several of the employees there, the key people, were graduates of this program. They were all good friends. So Don said: "We will give you this. You can't ever sell it. We give you this program and we would like to know about any improvements you are able to make and we would be delighted if you could give them back to us." And they did quite a lot of this. So it was a two way street. The program went through versions SPICE 2 and SPICE 3 and 3.1 and 3.2, continually evolving it and with contributions from many contributors. The proprietary competitors to SPICE, people who were trying to make a business out of this, did not have the same avenue to improvements and they were not improving. SPICE quickly became the best. Other parts of Don's point of view were: This is almost entirely funded by public money, State of California and NSF and several other government agencies. It is a little hard to see how we could try to make this totally a closed profitable venture. I think that time has proven that he was absolutely right. Somebody should do the study of who deserves the recognition for first important application program that was open source, but Don Pederson might be the one.

CL: How was this received at Berkeley and in the UC system?

DH: There was a man responsible for the licensing and patenting. At that time, it was only OP. It was Roger Ditzel. He came out of the chemical industry, I think. He kept saying for a long time: "You ought to be doing more to protect your IP rights here." So we did put on a copyright notice and then we said: "You may use this for internal purposes but not for sale," and a few other disclaimers. The distribution was made by of magnetic tape. These policies did not make any problem. But Roger kept saying: "You should have some way of getting revenue out of this." Finally though, he sort of threw in the towel, when we received $18 million to build this floor, most of it from companies that had profited from our [CAD program]. He conceded: "Well, you would have never gotten that much in royalty income if you protected it and tried to license it." And then we said: "It would have never been as good a program if we had tried to do that." The UC policy, as I read it right now, is it's the faculty member that has the power of decision about whether something should be protected. Is that your understanding?

CL: It is my understanding.

DH: Don said: "This is the position I advocate for faculty."

CL: If we go back to SPICE, was SPICE influenced by programs built in industry, by companies such as Intel and IBM?

DH: Each big company had its own circuit simulation program for a while and many of them had roots with ECAP, which I think was an IBM program. But I don’t think ECAP had the sparse matrix. Nor did it have the adjoint formulation that gives you the noise and sensitivity analysis. Of course they came to have it, because they saw the value. So these larger companies were trying to maintain their own programs and keep them proprietary, only for internal use. It only took about five years to replace them. There was no way they could keep up with our improvements.

So what happened was that ….. It was first called Solomon Design Automation. Jim Solomon was a student of Don’s, a master’s graduate. He said: “Well, isn’t there a need for a commercial version of SPICE and other things? And we would provide extra service to help people and we would provide extra features. So then we can charge more.” That was alright. Some of the faculty became consultants to him. I don’t think that Don ever did. But Richard Newton was and Alberto Sangiovanni-Vincentelli was a consultant as well. They made a commercial version available. Others developed and sold variants called P-SPICE and Meta-SPICE. These were all independent companies. They were not IBM or Bell. They were independent companies and basically they sold based upon better documentation or better service. They could not charge tons of money for this. But they made a business and some of these guys gave money back when we built the Pederson Center over here. One of them was called the P-SPICE. It was done by an independent guy named Wolfram Blume. Now, it is owned by Cadence. Cadence has P-SPICE and they have a version for student use that is free, but with improved documentation. Today, I am sure that some people use the Berkeley SPICE. But there are many more other versions available that are free or low cost.

Then they [Richard Newton and Alberto Sangiovanni-Vincentelli] went on to other things. I said when I talked about my dissertation that this was the first baby step toward synthesis of digital circuits. But synthesis in the 80s and 90s became a big thing for Cadence and Synopsys. Richard and Alberto contributed to these developments. Now synthesis is a very valuable tool. You can take a field-programmable gate array and synthesize a large digital function – program it in the field to quickly realize a complex digital function and have a pretty high assurance that this is going to work the first time. Synthesis became important, but an awful lot had to happen after my dissertation. It was not even at the root of it.

Research on mixed-signal MOS circuits; move to CMOS: 1974-85

CL: Could discuss your work on mixed-signal circuits?

DH: In the early 70s I came to focus primarily on analog and mixed-signal MOS. Paul Gray had joined, I think, in 1971 with a very strong analog background that I did not have. We worked together. He and I talked about this recently, because we plan to write an article about it for the *Solid State Circuits Magazine*. We were aware of the things that we could not do here because of the scale required and some of the limitations.

We could see that MOS was going to be of growing importance. We decided to focus on MOS and we could see that there would be a need to have both digital and analog on the same chip, that this would be important to economics. So we said: "Let's see what we can do with MOS." By then, the new thing was the charge-coupled device, invented at Bell Labs. Texas Instruments had, I think, a DoD contract to do signal processing, analog signal processing in charge coupled devices. They quickly ran into the limitations of charge-coupled devices. You could just make one kind of filter for instance, a transversal filter, and you had no way to do a multiplier. So we worked on the multiplier. It was easy to make a multiplier with an array of capacitors. That could also be called a digital to analog converter. Then once you had a digital to analog converter, you put it in a loop with a comparator and you have an analog to digital converter. We had a student project for each one of these things. They all built chips.

But our work never really jelled until we focused on one important application. That was the voice interface to the telephone network, which requires a particular form of pulse code modulation. It is a non-linear, piecewise linear approximation to a logarithmic coding - very well supported by fundamental analysis about how to make most efficient use of bits. We said: "Let's try to see if we can do that and make the pulse code modulation A to D converters to meet the spec for the telephone application."

The necessary specification information was readily available. It is a tight specification. We were able to meet the requirements thanks to the excellent matching of MOS capacitors. Doctoral students Yannis Tsividis and James McCreary were important contributors. We published the results and it showed the accuracy of the gain tracking to fall within the specifications. We were pretty excited about it. We had contacts at Bell and at other places. This was in 1975. We invited people that we knew were interested in this subject to a special session the day before the 1976 Solid State Circuits Conference in San Francisco. These were from the East Coast and several from Bell and several other places. We stood up and showed them what we had been able to do.

CL: Did you have the chip there?

DH: Yes. It was kind of a kluge. We had a working MOS chip with the precise analog functions that was the key. Separately, there was all the digital successive approximation logic around it to make it perform. But it was persuasive overall. Nobody had ever built this function in MOS before. They could clearly see the value of having the digital and analog together. They could see that it could meet the spec. We pointed out that they had all been making their A to Ds with resistors. Making accurately matched resistors in integrated form is hard and the capacitors were natural, because they are area-related. The first ones we made were round. You want to maximize the ratio of area to perimeter. It turned out to be too much trouble. We made them into little rectangles. But still it was easy to meet the specs within the necessary one tenth of one percent.

CL: You also did work on filters.

DH: We had the A to D and the next step was the filters. Gray and Brodersen and their students had the switched-capacitor filters in 1978. Digital filters could perform these filtering functions. But two things would have to be true. First of all, you would have to do your A to D at a much higher sampling rate if you were going to do the filtering after conversion. The second thing was that we were still in the era of two or three microns minimum dimensions and the digital filters were just too big - too much chip area. So the analog filters for a period of about five years were the most effective way to do the filtering. Today it's all done digitally.

CL: Did you patent this work?

DH: We went against the stream and we did file a patent application on the A to D converter. It was a lot of work to file the application and work with the attorneys. Then more in the interest of good relations than absolute necessity, both Bell and Nortel took licenses. We actually ended up getting some revenues. You know, it was contrary to the philosophy that I later subscribed to very strongly that it was not worth the effort to file patents. Indeed, people who wanted to find ways to design around those patents could do so. That was the last significant patent we did in that area. For the filters, there was enough prior art to make patenting difficult.

CL: If we go back to the early days of mixed signal circuits at Berkeley, were there people working on similar problems in industry? Or was it a Berkeley specialty?

DH: At first, there were a couple of people who had come up with a few ideas and published them – somebody at TRW. But they never did a lot of work on it. I think that I mentioned earlier that several years later one of the students found a publication by James

Clerk Maxwell from the 1880s or 90s that basically anticipated the filters. But he did not have the technology to do it.

Other people were not working on this, but then Bell started working on it and Nortel worked on it and another company that I consulted for called Silicon Systems down in Southern California worked on it. You remember in the 1980s when if you wanted to use a non-Bell long distance provider, you had to touch-tone the code and it would switch you over to the competitor network. So there was for a brief period of time a big market for DTMF receivers, dual tone multi-frequency - that means touch tones. The third party providers of long distance had to have a box to put somewhere that would capture these tones and use them to control their network. Silicon Systems was a small company that had already a few products and the president decided this would be a good product. They hired one of our students and I consulted with them. They had another Berkeley guy who had predated this and they had a very good engineering team. They made a chip in a cheap metal gate CMOS process, a hundred mil chip, a small chip. They could get ten dollars for this chip, because the alternative was a box that cost a hundred dollars. So they made tens of thousands, if not hundreds of thousands of these chips.

Then another small company (the big guys were slow to pick it up), another small company, Siliconix, hired one of our students. They designed a telephone coder and decoder, two separate chips. I don't remember why they decided to separate it. But that's OK. They designed the two chips, but they had no way to market this thing. Who were going to be the customers for this pulse code modulation? Well, it was going to be the people who built telephone exchange equipment and there was Western Electric, and there was Nortel and there was Vidar and a few others. It never amounted to any piece of business for Siliconix. But then finally the big guys started to get more interested. National Semiconductor developed a PCM coder-decoder with filters. Paul Gray went out for two years with a leave with Intel. Ted Hoff headed the group. Intel built a PCM codec filter and sold it for a while. But then people started to find lots of other applications for this and the applications multiplied. Actually, it was not hard for designers to learn how to do this. The little guys got on board, and then slowly the bigger guys.

CL: Did Federal agencies finance your work on mixed signal circuits?

DH: For a while, we got good support from one program officer at the Air Force Wright Patterson air base. I think we had two three-year contracts.

CL: Why was the Air Force interested?

DH: You know, I never quite understood the independent agencies, the Army, the Air Force, the Navy. They all had their research offices in those days. Mostly they are gone now I think. As far as I can tell, it was the whim of the program officer: “That looked cute; it might be of some help to the Air Force someday.” But then finally our sponsor called up and said: “Well, we can’t support that anymore,” even though results had been good. All these good results had been coming out, right? We also had some NSF money. We also got started with industry money. I think Bell Labs gave us a little bit of a grant and eventually Nortel did and National Semiconductor did. So that was when we started to get projects focused. This predated MICRO. When MICRO came along, we were all ready for it. We would put these together with MICRO. I think a lot of the value of MICRO is actually to encourage the development of the interaction with industry partners and learn from them – not just take their money, but actually learn from them.

I talked a lot about Paul Gray and Bob Brodersen. We really had a wonderful group with our students. We were all learning a lot very fast. Paul came in I think in ‘71 and I think Brodersen in like ‘73. Bob Brodersen had been at TI working on CCD. So it was very easy really to get moving on this.

CL: Could you talk about the development of a CMOS process at Berkeley?

DH: We went from the NMOS to the CMOS and we developed our own CMOS process. This was in the early days of ion implantation, which is a doping technique that allows you to do well-controlled doping at low levels.

CL: Was this in the late 1970s?

DH: Yes. Bill Black and I first published that in 1976. That was before Intel was doing any serious CMOS work, but Neil Berglund, then at Intel, became interested. Intel had a low power CMOS process and used it in Intel wrist watches for a while, but that CMOS process was very slow. Each period, Intel seemed to have one product that made them more money than any other product. Along that time in the late 1970s, they had a 4,096 bit NMOS memory chip that was as fast as bipolar. So it was like 55 nanosecond 4 kilobit static NMOS memory that burned about two watts. It was really hot, so hot that they had to put it in an expensive ceramic package. Hitachi looked at the sales volume of that product and said: “Well, we could do that in CMOS.” They made a 55 ns CMOS version that used one fifth of the power. The chip was bigger, but it used one fifth of the power and you could put it in a cheap plastic package, so that they could undercut Intel on the price. That got Intel all shaken up. They had to have serious CMOS. The CMOS they had for watches did not perform. 55-nanosecond was completely out of reach.

We had done the CMOS by starting with an NMOS process, which was at that time a lot faster than PMOS. We did an implant to provide the well in which the PMOS device could be made. That turned out to have some useful simplifications overall. So I was a consultant in 1980 to Intel in Portland. They said: “We can take our existing NMOS process and add this well and the extra steps for the PMOS and then we will have a high speed CMOS process.” I would fly up to Portland about every month or two for a couple years and worked there with Neil Berglund, who was an old acquaintance from Bell Labs. They did develop a high performance CMOS process of course. Intel evolved very far beyond that early stage, but that really helped get the CMOS running at full speed, where previously people had said: “Well, CMOS has got to be much more expensive than NMOS. We can’t afford to do it.” It turned out really that the power side of it forced it.

CL: Intel was late with CMOS.

DH: Yes. They could make NMOS more cheaply with all the speed requirements. But chips got more complicated. Power consumption increased. It got to be impossibly too much power. So they had to go to CMOS. They were a little late. When you do your examination of Intel’s resurgence, I can probably find some dates on that, because that was the first high speed CMOS. That was done in Portland in Neil’s group. Andy Grove was senior VP at that time. Neil told me that Andy said: I bet that you can’t get this working by such and such a date. I will bet you a case of champagne. Andy was known for making these bets, and most people lost. But Neil won the bet. He made the deadline.

CL: Was it the time when Intel started ion implantation?

DH: They certainly started using implantation well before that. But the first reasons for doing implant were different. The first use, I think, was to make self-aligned MOS structures, where you put down a gate and then you implant to create the source and drain, with the gate masking the channel. That was the earliest use.

CL: It was also used to gain a speed advantage.

DH: Yes, with the depletion load NMOS. Using implantation, one makes a transistor which was normally ON and use it in place of a resistor. It was much smaller than a resistor. So it was a whole sequence of things. And indeed ion implantation was used for making lightly doped regions such as the well for the alternate device in CMOS. Some put the NMOS device in the well and some put the PMOS device in the well. All of that was in a state of flux.

CL: How do you operate the microelectronics facility here today?

DH: In the microelectronics laboratory in Cory Hall, we keep running a full process flow, that is all the steps necessary to make a complete circuit. We have a technician whose job it is to do that. This is basically a way to keep everything in tune. If you don't exercise it, it doesn't automatically stay where it should. So occasionally, some circuit would be put in there, some circuit of research interest, but mostly the circuits of interest are sent out to whoever happens to be the most accessible and appropriate. So we have had circuits made by ST Micro[electronics] and by the DARPA/NSF MOSIS, and IBM has made circuits, and National Semiconductor has made circuits. Sometimes there will be a specific technology or a company that will have a particular interest in a project. So today the circuits are mostly going out. But the big thing now in our lab is microelectromechanical systems (MEMS), sensors, actuators and, it's just one innovation after another; and you absolutely need this kind of facility for that purpose, and now they're getting into bio-MEMS. It's usually in the form of some sort of instrument or an aide for the research that they're doing, or it might be something that executes a practical operation too.

Recently, we have 300 or 400 graduate students in a year doing something in this lab, and the numbers keep growing as researchers get the idea of what can be done now that you are able to make very small things with very a wide range of materials. It's a wonderful stimulus to the imagination and I've always felt that the merit of having the shared facility, maybe the biggest advantage, is that students are learning so much from each other. They're coming from physical and materials science, chemical engineering, electrical engineering, mechanical engineering. They're all learning like crazy and they're teaching the faculty and that's the way the faculty learns. The students come say, "I was working with Joe on this and look what we thought up." Whereas in Professor X's lab, you don't get that. You just don't get that kind of thing, so it's very pleasing.

Relations with other universities, industry; founding of MICRO: 1980

CL: Could you discuss the lab's relations with semiconductor firms?

DH: The relations with firms always tended to be stronger when we had alumni that had gone there, but not necessarily, not always. Sometimes there were others that were very good; and it changes with time. In the first half of the '60s, well Fairchild of course was very helpful; TI being at a distance; Sprague was very close because Bob Pepper went there, but they didn't know anything; they were new to it. We always felt we had really good access to Bell, and Westinghouse was very big in those days and Westinghouse had contracts from the same agencies that we did and so we had a lot of interaction with them; and Motorola - there were some very good personal contacts. I don't remember Pacific Semiconductor. I remember the name, but…

CL: They were based in Southern California; they were making power devices.

DH: Right. I don't remember our engagement with them, but it might well have occurred. Signetics we had a lot of interaction with because basically, some of the senior management there believed that interaction with universities was valuable. I was a consultant with them for a while. They were owned by Corning at that time. I think Corning is one of those companies that has always had good relations with the universities and I think that they probably encouraged Signetics. The guy at Signetics - PhDs weren't all that common in their management - but David Kleitman at Signetics was a PhD physicist, so that was the kind of factor that would enable it. Definitely with Hewlett Packard and Tektronix. One factor was more favorable with them than with the merchant semiconductor guys, and that is that neither Hewlett Packard nor Tektronix saw their business as making chips. They made instruments and the chips were only a part of it, so they were a little more open about their chip technology.

CL: Could you discuss the relations of the Berkeley group with the solid state electronics group at Stanford?

DH: We always knew [John] Linvill well. Linvill and Don Pederson and Gordon Moore were three of the really critical folks in forming the IEEE Solid State Circuits Council and conference. They were really the West Coast part. The conference was for years based in Philadelphia and was dominated by Bell and Philco and RCA - that group. But there were always these guys, Linvill, and Pederson, and Gordon Moore. They would show up at the meetings, although they were in Philadelphia. So that was a natural affinity group and what that meant was that we had a lot of interactions. [tape stops]

DH: You asked if I had participated in the establishment of the MICRO program. Yes, I did. Could you remind me of the date?

CL: It was enacted in December 1980.

DH: While we were working on it, the Bayh-Dole Act passed. Earlier in 1980, Jerry Brown was governor, and Jerry Brown said to the university, "I want some proposals for innovative programs that would encourage the university to work with industry," and so Chancellor Heyman, in '80, appointed me to a committee with Roger Ditzel who was the licensing officer for the Berkeley campus…actually he was U.C.O.P. I think. (I'm not sure when the campus got its own licensing officer.) The Committee included several other faculty people; I don't remember all their names, and we brainstormed this for a couple of meetings and we came up with the idea of a program essentially the way MICRO turned out.

The biggest difference from what we proposed and what was finally approved and funded, was we had proposed that it should be open to participation by all doctoral granting institutions in California, not only U.C., but when approved it was limited to U.C. And we had proposed $10 million per year and it came back as $4 million. Other than that, basically, the proposed mode of operation we suggested was adopted. So, it became an immediate success because up till that time, faculty members, some of whom had worked in industry and others hadn't, but the main source of research funds was government agencies, NSF, DARPA, and other agencies. And faculty were all attuned towards writing proposal to those agencies and the whole process we know so well. They weren't at all motivated or stimulated to make contacts with industry. So this program came along, and it never had very large amounts of money involved, but people sort of saw it as what you would hope the government money would be used for, which is slightly crazy new ideas that weren't quite ready for full support, and especially when there was a focus on an application problem. So some of the NSF reviewers would say, oh no, this is too applications focused, for our support this should be more fundamental. But the industry folks loved it.

CL: Was there an overhead waiver?

DH: Yes there was. That was part of the original proposal. The university had really no experience with this sort of program. The budget was tight then as it always is you know and everyone said "Why do we need to have a new program, won't the governor just give us more money for the existing programs?" But no, the rule was no new money for old programs. If you want real new money, you have to have a real new program. So MICRO passed muster and the response was very good. Industry loved it and we could point to the money that was coming in from the matching, from the industry. A lot of good relationships developed and it gave us access to industry people that were valuable to us in other ways in serving on advisory committees and things of this sort. Faculty never believed that their work should be wholly applications focused, but what this proved to be was exactly what I had hoped and that was that as you go further with a crazy new idea and you see "Oh, there's all kinds of interesting threads we could pursue here, this one would be good for the NSF, the more fundamental part of it." It became the basis for what were later some of the large projects. Of course the SPICE simulation program and all that predated MICRO, but the RISC and RAID projects got started with that. And they started very small and they were a charm of an idea and after a while they got very large grants, which you couldn't have gotten starting with zero. You had to have preparatory work. But the huge defect in all of this, which has led to the challenges in recent years, is that no systematic method of evaluation was built into the whole process. So UC Discovery was designed to remedy that problem, with a sledgehammer. Therefore, you had some talented people who declined to participate in UC Discovery because the rules were so onerous. Our point is that you are never going to be able to, you know…what was her name who was head of that for a while?

CL: Susanne Huttner

DH: Susanne Huttner. You know, she wanted to see revenues from products developed from UC. She wanted very specific economic outcomes and we said look, "the industry in California is the strongest in the world, and many graduates of this program are working in that industry and they wouldn't be such strong contributors if they hadn't had this opportunity in their student years." But that didn't sell; it didn't sell. You know, it's hard to quantify that year by year. Are you doing better this year than last year or worse? You don't know until you are 10 years down the road. So I don't know whether, now UC Discovery has been in place for 10 years I suppose.

CL: 12. About 12… UC Discovery, like MICRO, is now in jeopardy.

DH: It's a very sad outcome. We hadn't anticipated this kind of situation very well in the beginning. The other thing we were quite firm about was that intellectual property was not planned to be an important part of the outcome. And that was really drawing on our experience with SPICE, which predated MICRO, and we talked earlier about this and about how Don Pederson said "No, we are not going to try to make a lot of money by patenting or copyrighting. If we do that we will lose the cooperation of our partners." And I still believe that is absolutely correct, but many university administrations around the world are putting heavy priority on protecting intellectual property. You and I talked before also about how it is a different story for biotech and pharmaceuticals because of the necessity of patent protection to support years of development and testing. In electronics and information technology, the time to market and market life of new products are so short that patents have significant value only for cross-licensing purposes among manufacturers.

CL: One more question regarding MICRO. Were the other UC campuses involved in the formation of the program?

DH: Yes. Representatives from the larger campuses participated in the planning, and all were eligible to participate; so all the UC campus participated. In another slightly unfortunate outcome is that on some of the other campuses the faculty relied too heavily on MICRO, and they are in greater jeopardy than we are. Frankly I think that some of these faculties just don't have the ambition and the commitment that a lot of the people here do have to work very hard to build a large and successful program. There are a lot of faculty that are happy you know "If I just had $100,000 a year and worked with a couple of students, I'd have a wonderful life." That is not the way the really successful programs are created.

CL: Are you thinking of UC Davis?

DH: Yeah, I'm fond of those guys. They're wonderful teachers.

CL: I talked to people at UCLA and they told me that MICRO grants enabled them to start their research program on broad band circuits and led ultimately to the formation of Broadcom.

DH: Yes. We could point to others up here. The biggest one is Marvell. Those guys, the two brothers that founded Marvell, were both graduates here and I'm almost certain both of them worked on a MICRO project. I mean, this was some years ago; they were students, I think, in the late 80's or early 90's. But there are a number of others and there's a website now for this department of all the companies started by faculty members and students. Have you seen that website?

CL: No, I haven't.

DH: I will send you the link. It's a very long list and it's got the particulars of who actually worked on it. It's the kind of thing that U.C. should have been collecting.

CL: Let's go back to Marvell. The people who started Marvell were working on basic disk drive data recovery circuits here.

DH: They were, the two Sutardja brothers. They worked on that sort of thing here. I think they worked briefly for Intel or some other company before they started Marvell. They're from a rich family in Indonesia. Pop is a successful businessman in Jakarta. The company has been very successful, but it's also had its share of controversy. Paul Gray, with whom I share this office, was a member of that board of directors until six months ago, and had to deal a lot, you know they had stock back-dating and other kinds of things that were not right, though nothing criminal.

CL: Could we go back to the formation of MICRO?

DH: Yes.

CL: Were other U.C. campuses involved in the formation of MICRO?

DH: Yes, Yes. But probably not every campus, but there were about 6 or 8 people. Roger was the licensing manager for O.P. and I think there was another staff person of some sort and then there were faculty from several campuses. Probably Patrick Mantey from Santa Cruz might have been there. I don't remember.

CL: Was Herbert Kroemer in there too?

DH: I knew Herb in those days, and I don't remember that he was one of those. Herb, I don't think Herb was ever much of a guy for committee work.

CL: Okay.

DH: Nice guy and his students love him. You can probably, you know, if you needed to, you can probably find a record of who was there because we did do a formal report probably to the Office of the President. And then the governor came through and put it in the UC budget. It still is I think. MICRO is still its own budget line right?

CL: It's not a line anymore in the U.C. budget, which creates many other problems.

DH: Yeah. My colleagues have been telling me that they've been getting calls. They suspended the review and selection process until we have a state budget. Well, in my opinion it's the model for what a program should be and other states, some of them have done similar things. I think the one in Pennsylvania, the Ben Franklin program, I think is similar. But the other states spent a lot more money and built physical facilities, like North Carolina and Massachusetts did that. And I don't know of an example of one of those that's turned out to be successful in the long run. If you don't have a top flight faculty, pretty heavily committed to it; in needs to have its own staff. The only way the national lab succeeds is when they have the very top faculty as investigators as we have in LBL; a lesson that people have a hard time appreciating.

In the beginning the attitude was well, $4 million and the total funded research is system-wide four billion dollars or something, I don't know. It's in the noise. Certainly at the beginning, whenever we were trying to negotiate support from industry and talk about overhead they just went ballistic. So we broke that barrier, basically by starting, by asking for student support. So somebody gives enough to support a student for a year and that's the way Don Pederson started it. He said, "Look, give me enough to support a student for a year and we'll work on that and give you a report, and if it looks promising, we hope you would continue your support." There's no overhead on grants for student support. You know, we can still, the MICRO money is small enough so that we could actually still probably make that argument most of the time.

CL: Faculty members at UCLA were using MICRO to support students and funded the rest of their programs with Federal grants.

DH: Yes.

Research on communication circuits, 1976-86

CL: Could we go back to your research on communication integrated circuits?

DH: Sure. I had become interested in digital voice encoding at Bell (in the late 1960s) because I learned how they made the encoders originally for the first pulse code modulation trunking system. They were very expensive, and because they were so expensive and there was a lot of individual trimming and adjustment to meet the specs, they had to build multichannel encoders. I think they typically would encode up to twelve channels simultaneously; twelve conversations through the same electronic hardware. The cost per channel was pretty high and there came to be more and more demand for features on an individual basis. Each customer wants these features and another customer wants other features, and this single twelve channel thing didn't provide for easy individual services. So they really wanted a per-channel encoder, but it had to be cheap, and the idea was, can you make such a thing in a single chip because then it should eventually become cheap. So when I started working on this at Bell, and I talked to the people that were responsible for these encoders, I said "Please tutor me on the details of these technical requirements and I'll try to find a way to do it." And they said "No, no, no. Don't worry about that, we're the system engineers. You just give us 0.1 percent precision resistors on the chip and we'll design the encoder." Stop me if I'm repeating what you heard earlier.

CL: We talked a little about it earlier, but not much.

DH: So, in fact, on an integrated…they were accustomed to making the resistors on a piece of ceramic or a piece of printed wiring board using a laser and individually adjusting it to get the 0.1 percent precision. This is why they were expensive and they had to be shared over twelve channels. So, then there was starting to become capability of trimming things on the chip too, but in any case, that's an expensive process and it has to be done separately on every chip and you have instruments connected to the chip while you're doing this work to make sure it's okay. So you really wanted a scheme that didn't require trimming, but resistors, fundamentally when you make them on a chip, they're long and skinny. So the little fluctuations that you can't control are killers. But capacitors can be made, we started, the first ones we made were round. If you look at the pictures I sent you, the link to the pictures, there's one with round, two round things. The idea was that if the perimeter to area ratio is minimized, you have the least problem. Well, it proved to be overkill; it was okay to build squares. You just didn't want to build long, skinny rectangles. Circles were tough in those days; making the pattern was much tougher than straight lines. So we got to the scheme, which is also one of the pictures where you have a sort of a little checkerboard of a thousand little identical rectangles connected in groups of 1, 2, 4, 8, 16 …. So this provided a binary ratioed array of capacitors, which could be connected circuit wise to make an encoder. We made the first ones here that anybody ever made, and it proved that you could get adequate accuracy for the purpose, and figured out an efficient way to use them, and we came up with basically the feasibility demonstration that a single chip, pulse-code modulation, meaning the full Bell systems specs, could be made in a standard MOS semiconductor process. We had sort of the unveiling of this, I think, one day in the late 70's I think. We had the guys from Bell labs and other companies here, and you know, they were trying to shoot holes in it, tell us why it wouldn't work, but nobody could. So they went away and they made these things. In fact, we got a patent on that, and there was some revenue to the university. In total, I think about three quarters of a million dollars total revenue. And the inventors got shares of this. It turned out that that method of doing it lasted for about two generations of integrated circuit technology.

Things kept getting smaller and you could continue to do that, no problem, but it became feasible to do what's now universal, which is delta sigma encoding. Well this is an encoder that operates at a very high speed and it is basically, and you see this in all the audio equipment advertisements, a one bit encoder. But it goes very fast; so if you have a sound wave that could be plotted like this, you track it like this, very fast. The speed wasn't possible earlier so we had to do it by just taking the Nyquist rate, sampling at the minimum rate that would guarantee you the full bandwidth. The Nyquist rate is just twice per cycle so you had to sample 8,000 times per second for a 4 kilohertz signal. You had to sample twice in a cycle. Speed was not an issue, but if you had to sample a thousand times in a cycle (as required for Delta-sigma encoding), you needed the smaller transistors that would operate faster. So now they don't make Nyquist-rate encoders anymore. In our case, we had to use a good filter before the encoder because if you don't limit the band of the signal before you sample it, you'll get a lot of extraneous, very bad distortion.

CL: So the filter was part of the chip right?

DH: That was the next project for us. So we had the encoder first. The most advanced prior work on the filter in integrated form was using the charge coupled device filter. We didn't do that work here, but you can see pictures of those in the literature where the actual pattern on the chip has sort of a sine wave shape to it. There's a bunch of little rectangles that are split at different points. They're all the same length and they're split in a pattern, but the charge coupled device never became a commercial feasibility; there were too many secondary problems that kept it from meeting the specs. So after we did the switched capacitor filter, and we were including the codec (coder decoder) work, we had to have the decoder too. The decoder is easier. We started working with filter and we had several trials on the filter. One my students and I did the first one, which was an attempt to replicate a filter structure that was common in the days that you had do the trimming. So it required precision ratios, but we did that with capacitors. Honestly I have forgotten the details of why we went over to the integrator based filters; it's a different configuration. All of those things were part of the stream of publications that came out of this place. So we talked about the filters and then, by early 80's, there were commercial single chip codec filter chips manufactured by Nortel. Northern Telecom was the first and they took a license. Bell did one; they took a license.

CL: So your patent covered both filters and…

DH: No, the patent only covered the A to D. We didn't patent the filters and we thought later we should have, but then somebody went back and found out that James Clerk Maxwell in the proceedings of the Royal Society in like 1890, had made a publication that really anticipated the integrator form of the filter, which had proven to be the most effective. Maxwell lacked two things that were unavailable at that time. An amplifier that required no current at the input, that is, an infinite input impedance amplifier, and a zero-offset switch. That means a switch that closes, is purely resistive, so that if you plot V and I, it is a straight line through zero.

The MOS transistor provided these necessary characteristics. In one device, you had the infinite input impedance and the zero offset; two things that a vacuum tube can't do, bipolar transistor can't do; the MOS transistor is perfect. So, I think we probably could've submitted a patent application on that and the examiners never would have found Maxwell's work. There was another anticipatory thing by T.R.W. a few years earlier, and I can't remember to what extent we were aware of that. In any case, we didn't patent the filter. The codec was patented and some other people built codecs. Intel's codec actually used a different structure than we patented. Hitachi claimed they did something different. .National Semiconductor seemed to believe that the university was just basically incapable of bringing them to the mat on a highly technical thing like this; it just couldn't do it. And besides that, both of those companies had made important gifts to the university so, what are you going to do?

So, that was one step that led to complete replacement of electromechanical switching systems with digital switching systems. Electromechanical allowed analog signals to pass through and there are still in small towns, crossbar switches, and ferreed switches, and so on. But, all new installations were digital and that revolution came on extremely [fast], much faster than anybody thought. When I worked at Bell, they said "We're proud of the fact these electromechanical switching systems are good for forty years. They won't wear out." There was no problem making them; they were beautifully engineered. But they had to change to digital because they were too big; the old ones were too big. In New York City and other major cities, many cables come into one building, and they just needed more capacity. They needed more telephone lines and you could not put in anymore of those switches. So the new digital switches had to be designed so that the cabinets were small enough to fit in the aisles between the old and they had to be able to switch over in one night. They had to have people in the aisles moving the wires from the old system to the new system. Then they took out the old system of course and then they had room for two, three times more switching. What else can you do in a place like New York City? It's so expensive. You can't put it in a different site. You know, acquiring the immediately adjacent site in many cases was just out of the question. So, the digital switching came on very fast and Bell was not ready for it. Nortel had been a small company, it originated from Bell Labs, but they were early and they made a commitment to digital switching. They had a family of systems and now most of our telephone traffic in California goes through Nortel switches, the DMS-100 switches just right down here on Bancroft. It's a big DMS-100 installation. It's been there for twenty-five years; still runs fine as far as I know.

CL: Would Nortel manufacture systems and sell them to Bell Labs?

DH: Nortel sold them to the Bell operating companies. So they were more like Western Electric, selling to the Bell operating companies. But Nortel in Canada operated as an integrated enterprise, the way AT&T did way back, and they are still in that business, but they've made some bad bets and Nortel has had huge difficulties. But, you know, all of the telephone switching now is really digital; there's no more analog.

CL: It's interesting because the Bell system didn't really adopt bipolar transistors in the 50's because of the electromechanical systems that we were talking about.

DH: So they invented the transistor…

CL: But they never used it.

DH: Well, they used it in some of their analog equipment, in the analog transmission equipment. So, they had transistorized channel banks, you know the things that… And of course, the pulse-code modulation system. It was first installed in 1961. It was called T-carrier. T-carrier came in at first, you know, no integrated circuits; it was all discrete transistors. The twelve channel codec was all part of that.

CL: How do you explain that the Bell system was so slow in adopting transistors and digital techniques?

DH: Bell was convinced that theirs was a slowly changing business. So it was worthwhile to design a telephone set or a switching system to last forty years because that would minimize the depreciation cost, and they didn't visualize, for instance, the rapid growth in use of modems and fax. In fax, they were way behind on fax because they just didn't believe there was a future to fax. And they thought 2400 baud would be plenty for fax. They just didn't believe in change. That's my analysis. You know, they said it was wonderful when they replaced the vacuum tubes with the transistors in the station equipment, not the switching initially, but in the transmission equipment, and most of all, for the undersea cables. So, again, the focus on forty year life was with them all along there because of the cable; it's hard to make a replacement. But, the focus on the extreme reliability prevented them thinking about it, but also the other one, the extreme low cost, which might be exactly the right approach for that thing…

There are many examples of large organizations that don't change fast enough, and that's where Nortel had the advantage. They said, "We're going to make a bet on this. We think it's the wave of the future and we're going to be there first." And then, I worked as a consultant with TRW/VIDAR and they were just a very small operation so they went after the private branch exchange for small offices using microprocessor control. This was right after the first microprocessor chips. Basically it was a company of only a couple hundred people. I got acquainted with what would be called today the chief technical officer. He said "We're going to do this digital PBX and it requires memory and we want to use electronic memory. The only electronic memory when they started this project was Intel's very first product, which was a 1024 bit chip register. So, I met them when they asked me to assess the reliability of this brand new technology. What should they do to avoid the embarrassment of field failures in their product? I worked with them on that, and then, so I told you about how I found the Bell guys not responsive when I said "Tell me how this PCM codec works." VIDAR had a guy, named Bob Scarlett, who was a Stanford PhD and he knew this stuff front and back. So Jack MacDonald, who was the CTO, said “Well sure, we’ll tutor you. We’ll even pay you consulting fees while we were tutoring.” Paul Gray came along for some of that too. So we finally understood the critical aspects of the specifications. It’s a very sophisticated concept, which is all due to Bell. It’s a non-linear thing, but it’s piecewise non-linear with binary steps, which is perfect for digital implementation. So, they tutored us, and then we came back to campus, and that’s when we worked along and we started with simple A to D converters with the two circles. Then we, well one with the 1024 little separate capacitors was initially a linear A to D, but then after we understood the codec problem, we were able to cut a large one of those arrays into two smaller arrays to do the non-linear encoding. So a lot of credit is due to Jack McDonald and TRW/VIDAR, which was eventually acquired, I think by Continental Telecom, which was then acquired by GTE, which was in the Northeast of the U.S. and was a competitor with AT&T. GTE was eventually acquired by Bell Atlantic I believe. Jack followed along with some of that and was an officer at Continental Telecom in New York. I still see him once every five to ten years. So that was a terrifically exciting period for us, and we were all energized. A lot of students were involved and we learned a lot, and we got a lot of recognition for it. We won one of the IEEE awards. It was really the high point. So that is a long story, which isn’t directly tied to one of these questions.

CL: You designed a telephone crosspoint array in collaboration with engineers at Signetics. What were the circumstances that led to the design of this circuit?

DH: The cross point was a diversion. So if you wanted to switch analog signals through a network, and there still are reasons that you might want to do this today would be for video perhaps, but today even video is pretty much on digital. But in ’76, this was interesting. If you wanted to build a small switching array instead of metal contact closures, you would have a cross point switch, transistors, and I had that, actually that student project in the bottom drawer till about a year ago when I met that student for lunch one day and I gave it to him (chuckles). He made a chip and tested and we got a paper, but the world was not going in that way.

Reflections on patenting activity at UC

CL: Could you discuss the patenting activities of the analog/mixed signal circuit group at Berkeley?

DH: Basically, the weighted capacitor A to D converter patent was the key patent and that produced some royalty revenues to the university. Philosophically, I was even at that time a little bit unsure about which principles I should apply in that I was excited by the idea of getting a patent on it, but we had not had any direct support for that work from people that we went to for license, on a license basis. Nobody believed us. When we started on this, nobody believed us, and we had an NSF project that supported that, and that predates the MICRO. So we did patent it, but we published it all. We never had a big conflict. AT&T would not agree to a per unit royalty; they wanted a flat license. So Roger Ditzel negotiated that, and it turned out that he negotiated that with a man who had been my father's boss at Western Electric. Coincidence. Younger man than my father. But it was very time consuming for us, that whole business of getting the patent, getting the application completed, all of the to and fro with the attorneys. I think that probably earned back its costs, but we had several other patents that never brought in a nickel, and we just basically stopped paying attention to patents.

Some of our MEMS people still pursue patents, but they have gotten some licensing agreements with folks and gotten small payments and so on, but I don't think there's anything big. The big winner out of their work is the accelerometer, which is very common now for auto airbag deployment. Analog Devices is the biggest maker, and Analog had been a continuous sponsor of that work. Nevertheless, the university went to Analog with a patent and Analog said "We'll show you what we're doing."

There are two key aspects to the MEMS accelerometer, the fabrication process and the mechanical structure. Roger Ditzel was a chemical engineer and Roger said process patents are really tough to write and defend because the process is complicated and there are a lot of little steps, you could change things. So he said patent the structure, the mechanical structure; that's the way the patent came out. Well Analog had a different mechanical structure and, in fact, after our guys analyzed it, they said "Well, theirs is better." Theirs is a better way. So we avoided the painful problem of having to press a major supporter for royalty payments.

CL: A friend of mine is looking at MEMS research programs at different universities, and he discovered that Berkeley has very few patents compared to Stanford, even compared to even the University of Tokyo.

DH: Well, so the MEMS group is large, and there is not one view among the faculty on this matter. And the way our rules read, basically the faculty member individually makes the decision whether to pursue patenting. So there are a few faculty people who are vigorous at pursuing it and others who take my attitude. When a young guy asks me, I tell him what my view is, and I point out that the payoff from these things is pretty hard to find. I'd be interested if your friend finds whether other institutions are realizing enough revenue to pay the costs.

CL: That's a good question.

DH: The structures are also easy to modify. Even if they had no new idea, I think that the Berkeley MEMS patent for the accelerometer structure, somebody could go in there and change something, and not thereby infringe without materially affecting. There's no unique way to do something like this. So it's a very hard thing to write good patents about MEMS.

I think we at universities should go out and do what we do best, and talk with anybody who will talk with us and learn from them. You know, if they're thinking you're going to go off and patent it, they're not going to talk to you, but if you tell them what you're doing and they're confident that you're not going to steal their stuff and write a patent, they'll tell you more about their stuff at least. And you learn from it. So when we have these meetings, there is a huge learning experience. We can have a forum here, and the MEMS forum is still largely in that model where everybody comes; most of the presentations are by the faculty and students; anybody can ask questions. Usually there are a few presentations by industry people, and they have rules that you have to be a paying member to sit in this meeting. But you don't thereby get any intellectual property rights, and there's a lot of fruitful exchange. It's a forum that can exist at the university and there's no other place that that forum can exist. It can't exist at any one of the companies. The format for major conferences is in general, you know, somebody's up there for twenty minutes, makes a talk, 4-5 questions, and sits down. It's not really an extended discussion. So, when people from other countries ask me, I say you got to try to create this kind of situation if you want your universities to really be working with industry.

Involvement in IEEE conferences and publications, 1972-82

CL: Could you discuss the circumstances that led you to become the chair of the ISSCC in 1978?

DH: The ISSCC (International Solid State Circuits Conference) had been established by a group, a leadership group, in Philadelphia at the time that RCA, and Philco, and Bell Labs, and they were very close by there, and there were other companies. Then there was IBM, mostly in New York State. The early semiconductor industry, a lot of it was there in the Northeast. But by 1978, that wasn't true anymore. The people there were very resistant. This was under the umbrella of IEEE, but you know, the Philadelphia people had a real strong sense of ownership. These are volunteer leaders; they're not paid people. They wanted to keep it there; so we had a battle that started in about 1973. And in ’78, finally, was the first meeting in San Francisco. I had been leading this battle really, and they said, “Well you’re going to have to be the chair.” I knew that; I was ready for that. It was at the SF Hilton. They had fifty percent more people than they’d ever had since the boom year of 1960, the tunnel diode year. They had 3,000 people for the tunnel diode, and I think when we had it over here, the first time, in San Francisco, we had, I think actually it was about 3,200 people, I don’t remember, up in that range. In the immediately preceding years, it had never been more than about 2,400.

There was a lot more going on in Japan than there was in Europe; so a whole lot of Japanese came, and they all spoke English, or they tried. We didn’t have any translation. It was a huge success. So, the lead time is several years; so we alternated for about 4 years between east and west, the last time in the east we were in the New York Hilton and it was a mess. So I think starting about ’80 or ’82, ISSCC was out here. Starting probably in ’82, we have been out here every year since. Some conferences move around in different cities, but it’s always much more tightly organized if there is continuity in one place.

The way that area of activity proliferated is that now there are sister conferences at about 3 month intervals in Europe, and in Japan, and in Asia now. It has taken years, but they are philosophically all very similar, and there’s international participation in all of them. And I think that we had really created a wonderful professional culture. I’m really proud of what the non-Japanese Asian group has done with the leadership of one of our grads, who’s a Taiwan native, and he led it, and the first one was in Taiwan, then they had one in Korea. This year it’s in Japan; last year it was in China, and it’s going to rotate around those four. And they have built up their leadership cadre of volunteers, and there are no national jealousies or anything. Europe is quite similar. It rotates around countries. France, Germany, Italy, and Britain, but they’re staggered around the year. So the ISSCC is in February, and the Asia one is now in the first week of November, and well they’re all, you know, through the calendar so that they’re not in direct conflict. I’m still heavily involved in IEEE and I think it’s absolutely the model that should be looked at by people who are committed to internationalization. I’m still battling at IEEE because the IEEE, they have a big career staff, about 900 people, and it’s a four hundred million dollar a year business. Some want to retain full financial control for all these around the world. That means they have to conform, for instance, to U.S. accounting standards, and conflict of interest stuff that is unfamiliar and difficult for them. I say, help the indigenous folks develop themselves, provide their own financial [resources], make their own rules. You know, IEEE is paying some accounting firm like $300,000 a year to ensure that overseas conferences conform with US accounting rules as well as local rules. They have to collect value added tax on the conference registration fees, and the rules are different in every country. It’s a huge mess I think. Look, the Europeans can pay for it, and so they have to have a financial sponsor in each country which might be a local society, or a government agency, or a university, or whoever. Play by your rules, but we will be technical co-sponsors, which means, in every case the program committees are international, the quality standards are international, and there’s no argument about that. This guy pays the bills; usually there’s a surplus. Take turns. I’m still working on that though, and I think eventually I’ll win. Things of this sort are probably not going to be in my IEEE article.

CL: In 1980, you edited a book on analog MOS integrated circuits in collaboration with Paul Gray and Robert Brodersen. Could you discuss this book project?

DH: The edited reprint books are, I think, a terrific format, but publishing people hate reprint books, you know, collections. The value of these books, and that was exactly the value, I had done an earlier book on memory, an edited book. Did we talk about that?

CL: I’m not sure.

DH: Soon after I got to Berkeley, it was about ’72. It was an edited book on semiconductor memory.

I wrote narrative pieces stitching it together, relating the articles to one another, and pointing to other references perhaps. So I wrote maybe 5% of the whole volume of the book and all the rest was collected stuff, and it was a big success. IEEE had a program of those books, and they were invaluable to engineers. Boss says “We got to get started on this.” So they would buy this book. I was considered to be one of the pioneers in that field so to have somebody of that sort make the selection of the material to bring a new person along was valuable. Everybody looks back and says it was valuable. But in the publishing world, it’s the dog. I mean, you don’t make your reputation with edited volumes; so somehow, the staff, and a few volunteers, that were badly misguided, persuaded them they should move over to fully authored original books. That was back about fifteen years.

The analog MOS was exactly like the first one except Paul, Bob and I worked together on it. Then there was a second edition of that later which I did not participate in. There was another author, a third author. They never made anybody a lot of money, but that is not IEEE’s goal. It's to provide materials for practicing engineers in the form that they can use. Of course this predates the electronic library, so now we are trying to get it going again, still staff resistance, but totally electronic. What you get is basically a bunch of links and still have the selections made by an expert, the material stitching it together written by an expert. And so that staff says, how are we going to make any money on this product, and I said well you're not going to make any money on this product. It’s very hard and of course all of your digital library subscribers are going to say well we should get this as part of our $110,000 a year subscription. I said yes, but this is going to meet a need and its going to help to keep the IEEE in the forefront. We're still fighting rounds here.

Writing a teaching textbook: 1983, 1988, 2003

So then the textbook, *Analysis and Design of Digital Integrated Circuits*. That had been the pattern at Berkeley. The faculty would write books, particularly in fields that were widely being taught. There were really only some old books at that time. My colleagues Gray and Meyer had written the analog book earlier and that was a huge success. It's still in print, 4th edition and in five or six languages. So Gray and Meyer, both professors, and I worked with Bill Jackson who was a lecturer, very knowledgeable guy, but we were a very good team bringing different skills to the party. Bill did the bipolar circuits part. However, ours was the first digital book that had a lot of MOS circuits in it. I did the MOS stuff. We used it four or five times in class over a period of two to three years during its development which I believe is very valuable in making sure that it is clear for students. We wouldn't sign a contract with a publisher till we done a lot of that, and then finally we did sign with McGraw Hill. Then we did a second edition in '88, and then I was tied up in the dean's office, Bill retired, and they kept on saying we would really like a new edition. We had a guy that actually signed a contract, one of our former students and he didn't perform, so we finally came out with the third edition in 2003 with a different guy as the third author who did a wonderful job – Resve Saleh, one of Rich Newton's former students - and we were too late. So, number one, my colleague Jan Rabaey, had done a book, which I still think is inferior as a teaching book and other people have said this to me, but he had Prentice-Hall which did a better marketing job than McGraw-Hill. We had Asian editions. These are authorized Asian editions, they're paper, smaller format, cheap, and kids in dorm rooms in the USA buy a pallet full of them and have them shipped from Amazon in Hong Kong or something and distribute them. So you see a lot of these Asian paper versions which are not supposed to be imported, but there is no border control. So the sales have been, you know, we get a dime of royalty on them, the paper copies. There are two Chinese translations. I think one is Hong Kong, and one is Beijing. So every professor should write a book, but I'm not sure what the future of books is, at least in the engineering context. There are so many ways to put together materials for student use. Our classes are very heavily on the web; lectures are on the web, problems are on the web, everything is on the web.

CL: Is there a tradition of textbooks at Berkeley?

DH: Oh yes. The most famous one is John Whinnery's *Fields and Waves in Modern Radio* which is in 5th edition now, 50 years old and that's classic material, that's Maxwell, but the later chapters are modern applications. There is a display case on the second floor along the Hearst Avenue side of books, recent books.

CL: Is there a Berkeley style of writing textbooks?

DH: I never thought about that until, while I was Dean, I used to meet with Jim Gibbons who was Dean of Stanford at that time and he asked me, how do you get your faculty writing these good books, he said, I wish my faculty would write more of these books. I said, I don't know, it's just the culture. You know, it's just what people do. I think it's less strong now than it was but it changes. Culture is just a good example of predecessors, so not only Whinnery, but Desoer and Kuh, and half a dozen others. Berkeley's national standing in 1950, - there weren't any charts in those days, but Berkeley wasn't there. It wasn't on the charts. Berkeley came up with a very wise succession of faculty hires, post WWII, and John Whinnery was really, together with the ports and harbors guy, O'Brien; there's a wonderful oral history of Mike O'Brien. He was a civil engineer, but he and Whinnery were sort of the visionary intellectual leaders that said that Berkeley can be a much, much stronger place, and the key is hiring the right people. They led the way in making that happen and set examples. Clarence Cory was a mechanical engineer and didn't believe in alternating current. He certainly didn't believe in electronics.

CL: Victor Grinich used to teach here. What did he do here? He was at Fairchild and then he moved here right?

DH: He and Pederson were good friends. I think they were students at Stanford together. So Vic was a lecturer like Jackson, and had a lot of practical experience, and he and Jackson actually wrote one of the early textbooks here. It was Grinich and Jackson, and it was used for a sophomore course for quite a long time. I met him maybe once, I never got to know Vic very well, but Don often spoke very highly of him. We still do that, we have people here that are not regular faculty people, you know, but very highly respected people. We now have Ivan Kaminow, a retired guy from Bell Labs, who is in optics and lasers and so on, and we have another guy, whose name I have forgotten, who was CTO for National Semiconductor for a while, a Berkeley graduate.

CL: So there is a tradition of bringing really good people from industry as lecturers?

DH: Yeah. If they have the credentials, they can be appointed to the faculty. It used to be that half of our faculty, our regular ladder faculty had serious full time industry experience either before or after the doctorate, before joining the faculty, and those who don't have it, we do encourage them after 2-3 years to take a leave and go out for a year or something. Work and hopefully find a firm working in an area related to their research. It pays off I think.

CL: Grinich did not join the faculty, because he was not a researcher. Am I right?

DH: Nobody ever saw him as a researcher. I think he made a lot of money on Fairchild or something. He didn't have to earn a big income, and he liked it. I think he liked students. Nobody stays for long if they don't like students, but if they like students they love it, they love it here. Working with that guy John Torous on that paper, I didn't work closely with him; I just talked with him half a dozen times, he was just a joy, on other things too I talked to him on other things. He was just such a delight to work with.

More on relations with industry; MOSIS

CL: Could you talk about the IC Lab's relations with industry in the 1970s?

DH: HP and Tektronix that did not make integrated circuits for commercial markets were more open than Fairchild and Motorola and so on. It always helps to have your own graduates working in those companies, and they did in substantial numbers, but we did get some help from Fairchild and I remember Gordon Moore and Andy Grove came and Grove taught the class here for three or four years based on his book.

CL: How did the lab acquire its processing expertise?

DH: The way it seems to work around here is we take what you would call a plain vanilla starting point and there are, you know, probably 100 different steps that you have to know how to do if you want to make an integrated circuit. Maybe you only use a subset of those, but there is a catalog of capabilities you have to have for etching and deposition of different materials, and for doping and for patterning. So after struggling at times we are now in a mode for about a dozen years now where we basically have a technician who runs a standard sample through all the steps every couple of months. I mean, it takes several weeks, but it's not pressed as a priority, the goal is to keep everything in tune. To keep everything running smoothly and there is plenty of room to put experiments and stuff on there. Then there tends to be a real big in-depth effort in a particular area, so all the MEMS is the best example of that where they have to have the etching in creating these suspended structures, so there will be a very intense effort by one or two or three grad students working on special processes for MEMS.

You do some of the standard steps and then you do some special steps, and then you do some standard ones. So then, if you come, and they have for the mechanical structures they basically come to a very standard vanilla electromechanical process from which you can depart for research purposes or use it as it is. We do a better job than a lot of such labs at documenting things because it's not just one professor, it's as we talked earlier in a year there may be 80 professors and 400 students. Professors are not in there with their gloves on, but 300 or 400 students of 60 or 80 professors are using that facility, and some know-how is lost, but it's a pretty capable facility. When there is one of these in depth aspects those folks are almost always in touch with peers in industry and learning from them, and there's always a lot of art you can learn if you are friendly with somebody even if you are under some non-disclosure agreement. So there is a threshold point that, for instance, Davis has never gotten above, and they have a very nice physical facility, but they really can't do much.

CL: Because they don't have the know-how?

DH: Well, it's chicken and eggs. They don't have the money too either. So I just saw the numbers the other day; it costs us about $2-3 million dollars a year to keep our lab running, most of which is basically recharges. Log in to the lab and go into the lab and the clock is running and it's like $40 an hour or something just to breathe, and then a dozen or fifteen of the high cost processes have additional billing on top of that, and these are all paid by grants.

CL: So, they charge to the grants.

DH: They're all charged to the grants; it used to be some from the department budget. I think there is only half of staff FTE now, funded from the department, and that person has to make sure the teaching lab works. So, you have to have the grants to do that, and so the rules are that there is a cap on the total billing for a year, so you know, it's sort of like Medicare. If you pay 20, I don't know what it is, $20,000 or $24,000, or something for one student per year, then no more billing. You know, it's all worked out; very empirical; just to make it feasible, and we're about to move into the new lab next door. So the move, the staff says, is going to cost $2 million.

CL: Could you discuss they ways in which the group at Berkeley incorporated the development of VLSI techniques by Carver Mead and Lynn Conway in their teaching and research programs?

DH: Mead of course is a solid state guy himself and Conway is more of a digital systems person. Their [collaboration] was more like professor and post-doc, although Lynn [Conway] has her own doctorate and she's now a Dean. The researchers in the computer world back in the 1970s were drawing block diagrams. They said "Well this is a processor, memory, or this is a register block, or this is an adder." And they would draw a block and they had no idea of how they should parametize it in modern technology being a single chip. How fast could this run? How much power will it take? How much will it cost? They couldn't answer those questions. So Mead and Conway was basically for those people, to give them in one course without knowing a lot of transistor physics, an understanding, part of the way toward being able to say "Well, it's going to cost this much," you know, "so many nano acres, and so much power, and it'll work this fast with this generation of technology," and then to allow them to build it. So that was the MOSIS. So Mead, a man of great prestige, talked DARPA into funding MOSIS.

CL: Oh, I didn't know that. Wow.

DH: Yea, so I think both DARPA and NSF were involved; NSF may have been involved for longer.

CL: So the goal was to allow computer scientists to…

DH: To experiment with chips.

CL: And then they would provide the solution to make the chips.

DH: Yes. MOSIS worked out the software that makes it possible to put like twenty or thirty different chips on one wafer. So that needed some special software and then the masks were a little more expensive, but not twenty times more expensive. So there are, you know, you can have a square millimeter or two square millimeters or four square millimeters or something, and you always waste space but you can compute the actual space of your thing. You're allowed to discount the picture frame. So you do all this and then MOSIS was the organization, which had an office down in Santa Monica, where they had some Caltech guys and some draftsmen or some programmers or something, a small staff. They would assemble these things and they'd have a calendar and they'd say "Okay, Project Chip is going to go out, such and such. Here's your deadline." You get your design in and then they had contracts with wafer fabricators. This actually predates TSMC and so on, but they were asking for that kind of service. And, you know, the attraction of that capability may have been one of the stimuli to TSMC to make a commercial business out of such a thing. TSMC will do the multi-project chips although most of their business is not that way. So all these computer classes…they started having these VLSI design classes for, basically, computer science students who didn't know any physics or anything, but they could design a chip. And in those days, what did they call their design rules? Lambda based. Everything was a multiple of lambda. Lambda was like half the smallest feature size; so nothing was smaller than two lambda. So it could be two lambda, three lambda, four lambda. And the reality is, for instance, if you make a metal contact, it's two lambda. The contact must overlap the edges of the hole below it.

So the software had to do little smart things like that the designer didn't have to think about. They had rules, and the rules said this kind of thing can only be made five lambda. So there was a whole set of design rules, which in those days were tractable; they were manageable. There weren't so many that you couldn't deal with it. And it was a huge success; very popular, very motivating to students. A lot of chips were designed. One of the rules was that you were supposed to test the chips that came back, but that's a whole project itself, and a fairly substantial project in many cases. So, the joke is that most of the chips were drawer tested or they were tested on tie pins. They didn't get tested.

CL: Did you use MOSIS?

DH: Yes, we used the MOSIS channel, but didn't stick to lambda based design rules. We took real design rules and tried to be as realistic as possible, and expected to have working chips, and we planned, and in fact did, test the chips. These were research projects that mostly went on for like two or three years. The ones that were drawer tested were typically the class project.

CL: What was the motive for fabricating these things for class projects?

DH: In many cases, you know, one semester wouldn't be enough; so it would have to be some kind of two semester experience. But I don't think that that's taught very much that way anymore. It is expensive; somebody has to pay for it and NSF, I don't know what the rules are now, initially paid all the bills. But now I think there are some charges going with it. David Patterson would negotiate outside the MOSIS channel, and for a while Hewlett-Packard Corvallis built some chips for us. I think the RISC 3 chips were built by Hewlett-Packard Corvallis, and they actually sent an engineer down here to work with us for months. Their chips worked. They did have one or two design errors. They had this laser tool that you can go in and cut metal selectively or add metal if you had forgotten something or made an error. We had to do that. That focused ion beam tool was very widely used.

CL*:* Could you discuss Berkeley's competition with the Center for Integrated Systems and the VLSI program at Stanford?

DH: We have to separate the stuff that goes on in the Center for Integrated Systems and the Mead and Conway stuff, which was much more in the province of computer science. Stanford, at first they were five years or so behind us, in doing the lab, and then they got up to be very competitive. Generally Stanford, their faculty…I don't know what it is nowadays but the faculty involvement in the actual fabrication of stuff was a fairly small number, and then they had more of these research professors, which Saraswat was for many years, but now he's regular faculty, and Ernie Wood I think was a post doc there doing a lot of this stuff. So, we don't really have research professors here. We talked about it. We made one offer; he turned us down. He is a guy who's very well known to us and very proven.

[Going back to the] Stanford program, you know, we have a lot of mutual respect, and I think ours is bigger and broader in terms of what you actually fabricate. Stanford still has a number of faculty who maintain their own separate labs, as we do, but I think more of their work is conducted in the separate labs because they have a lot of optoelectronic and quantum stuff that's more or less I think conducted outside of CIS, where as our opto guys here like, we have opto MEMS, which is all done in here. They have some opto MEMS too. So, we watch each other and the staff in the lab is very friendly and we will help each other out. The researchers in a given field will know each other well. So, I know Bruce Wooley very well.

So, we've had good relations with the Stanford guys, and frankly also with the MIT guys because we had one very big joint activity with them, with Dmitri Antoniadis, and Rafael Reif before he became Provost. Charlie Sodini and Harry Lee are Berkeley grads. But the distance makes it a little harder.

CL: The facilities of the IC Lab grew in the 1980s, did not they?

DH: In 1985 or 86, we moved into the lab we now occupy, which was basically an expansion of the old one. Now we're getting our new one, seventy-five million dollars just for that part. But we have three or four hundred users. In selling this we said "Hey, it's not just some electronics guys doing their stuff. It's people from physics, materials, and chemical engineering. Biological sciences are using it; they're building instruments or tools for their own work."

CL: Seventy-five million dollars for the clean room?

DH: For the building. For the portion of the building that's got the clean room in it because the standards for health and safety are honored. I mean, we have been in violation here for years, and they never shut us down, but they said a few years ago "If you don't have a plan, we are going to shut you down…you don't have a plan to improve this." It was essentially impossible to improve it within…the space it's in now was office and dry lab before we started.

CL: Where did money come from for the new lab?

DH: Well, this was special state appropriation with CITRIS. Then there's a lot of private money too. The Sutardjas pledged forty-five million I think. I think the budget here is only about ten million short; the whole thing is one hundred seventy or one hundred eighty million, the whole building. And I don't hear so many worries about that anymore either.

Participation in the SPUR project, 1985-1990

CL: I was thinking maybe the last topic we could talk about today is the RISC chip.

DH: Sure. Patterson came straight to the faculty from UCLA, but obviously a very innovative guy.

CL: Was he a UCLA PhD?

DH: UCLA PhD. There had been a number of earlier systems projects here, none of which particularly distinguished itself. There was a guy named Herb Baskin, who had a spin out company that was called Berkeley Computer. This was the dark ages; this is really before integrated circuits. So Patterson was really a computer scientist, but wanted to do things with modern technology, and we had a guy who came in from Bell Labs named Carlo Sequin, and Patterson and Sequin hit it off, and Carlo knew the new technology quite well, and had some background in computer engineering. So they teamed up and did RISC 1, the first RISC, and they had a couple of very talented students. So the inspiration for RISC (and I think they coined the name), was that Patterson went for a leave at Digital Equipment and they said "Do a feasibility study of a single chip VAX."

CL: Okay.

DH: Right. VAX had four hundred fourteen instructions. So Patterson worked at DEC in Massachusetts for six months. And he said "Well, you know, with the technology at that time, four hundred fourteen instructions directly executed in hardware, no." And the way IBM had done this is they microcoded it so that they could have four hundred fourteen instructions, but each one got broken down into a sequence of four or five, six, seven steps. And then the different IBM 360, the smallest one, everything was done like this, and the bigger the machine you got, the more parallel it got. So it got faster. So it was a compatible architecture, which was a huge success. But Patterson didn't want to do the micro code, so he got hold of some studies on the frequency of use of the four hundred fourteen instructions. He said “Well, a hundred instructions account for eighty-five percent of all the instructions executed. What if we just build a machine with only a hundred instructions and figure out, with software and the compiler, how to do the other things? And then we could put it on one chip.” So that’s what RISC 1 was. I think it had like seventy instructions or something.

CL: So they designed the chip and it was built by MOSIS.

DH: Yea. So there were successive generations, and I was not working with the first two generations, although I advised a couple of the students who were working on it. The third one was called, I think it was RISC 3, was SPUR, which was Symbolic Processing using RISC. Symbolic processing means LISP, that is a computer language that’s oriented towards A.I.. T.I. was manufacturing a LISP machine. There were very close ties from T.I. to M.I.T. LISP came out of M.I.T. T.I., which was basically a semiconductor company, built and sold a LISP machine like a Sun workstation. But the whole thing about LISP is when they built it originally they had to have…you know, our words are always like 16-bits, 32-bits, or 64-bits, but the initial implementation of LISP always required two or three extra bits. So it would be like 18 or 19 bits or something. Then they had special tasks to perform to manipulate them. So, T.I. sent us this guy named Doug Johnson, who was a master’s guy, but with a lot of experience; a very canny guy, very, very helpful. Doug had personal reasons he was happy to be out here, and helped us a lot. We gave Doug an office; he brought his own LISP machine from T.I., and we gave him the latest generation of Sun. So he’s a software guy basically, and he, in about three months, figured out how to do everything that needed to be done for LISP within the standard Sun architecture. T.I. didn’t want to make any of those machines very much longer. The point being, which Patterson always made very strongly, is that if you have a standard architecture, you could ride the path of improving semiconductor technology at the peak rate. You’re not waiting around to do a whole unique design after the new technology becomes available. And that proved to be absolutely true, but nevertheless we completed the project with the tagged architecture and made the chips. The chips worked, and they made this compiler, and they made all the operating system--everything. There were like fifty people. Ten professors and forty or fifty students. And from the pedagogical standpoint, it was a brilliant success. From the technical accomplishment perspective, it was a flop.

CL: Nobody really used it.

DH: No, nobody ever really used it, but everybody learned so much. So there was a tremendous group spirit and credit goes to Patterson who knows how to build this. We would go up twice a year up to Tahoe or someplace for a retreat and we'd invite half a dozen industry friends to help assess our progress and plan for next steps…

DARPA wanted the industry involvement; so we had guys from industry. At that time there were more computer companies. There were companies like Prime, and Digital Equipment; there was IBM, there was Burroughs, two or three others, and Sun I think. Sun came after RISC 1 but before RISC 3. So, there was tremendous group spirit, and these workshops were exactly what I said earlier they should be. Faculty and students did most of the talking; guests asked all the toughest questions they could think of; and there were lots of dialogue – very valuable. That was the only project of that magnitude; it was one of the very few large projects. After ten years, we had a reunion. We invited everybody that participated. Everybody but three people came - like fifty people came back for the reunion.

We had a very good time with them. Patterson is just a genius at doing that, and they're doing it again and again. They have the PAR lab now, which is parallel computing. The value to the students is enormous because they are exposed to the whole range of problems associated with the development of a complicated system. So they're very, very valuable programs. People can think of all kinds of objections to these. In the New York Times within the last week, a woman wrote a piece about, you know, it was more like David Kirp's title like "Buying the University" . (Shakespeare, Einstein, and the Bottom Line: The Marketing of Higher Education).

Did you see this?

CL: No, I have not seen it.

DH: I'll find that link and send it to you; it's a short piece. But it's the same old stuff. There was no problem here. First of all, there was never any protected intellectual property about this. There was never any dispute about it. Some of those guys went out there and took some of our ideas, and adapted them, and used them. Mostly, they just departed with a broader understanding of options, alternatives, and opportunities than they might have had.

I advised two of the doctoral students that were heavily involved in the SPUR chip designs. One of them went out and founded Silicon Image, which is one of our smaller, but successful companies. The other guy is a professor at Seoul National University now. So that was another high point; that was a wonderful project. You know, a lot of computer scientists at that time didn't actually want to get talking to technologists, but Patterson said, "Hey, I need your help." Because he was aware of what we had done in pioneering this forum with the weighted capacitor codec, with the industry people, Dave said, “Help me do that with this project.” And I did. I was terribly pleased last year at some faculty meeting when Dave Patterson gave me credit for teaching him how to do that. He’s a really generous colleague.

Research on semiconductor manufacturing, 1988-2000

CL: In the mid-1980s, you reoriented your research toward semiconductor manufacturing. Why such a shift?

DH: We had a very good run with the mixed signal circuits and so on. By the mid-80s, there was this huge threat from the Japanese and it was generally agreed that the Japanese had really done a much better job at high quality, high efficiency manufacturing, and there really wasn’t any manufacturing research in US universities. There hadn’t been any funding for semiconductor manufacturing research. So I felt that, you know, it’s good to change your direction sometimes, and this was an interesting area, and I decided to work on it some, with some encouragement from friends of industry, and we got a little bit of money from NSF and we got some industry money with, actually I think we actually got MICRO money, and we had some projects which had a very academic flavor to them.

One major project was trying to standardize the control interface for manufacturing equipment. You get into any car and pretty much the controls are all the same, but every piece of equipment was dramatically different. So we worked on that, and we worked on improving process control, plasma etching processes, and had a couple students do dissertation work. Graduating students in general didn’t get work exactly along those lines because the US industry manufacturing was run by people with bachelor’s degrees and it was pretty much empirical, and they said, what am I going to do with a PhD?

Among students from that activity, only one of them failed to find a job and that‘s because he was a foreign national; he is Australian. He went to Germany and he’s done very well, he is still in Germany. The big project we had was with the Sloan Foundation, which was also upset about the decline of American manufacturing. Sloan started a program that is now called Industry Studies, and so they called and said, “would you like to give us a proposal for a study of the semiconductor industry?” “No prescribed format, you decide how it ought to be done,” and they said “we’ll give you a three year initial grant and it could be renewed up to twice.” Three, three and three, and so what we decided to do was to try to better understand what the real reasons for the productivity differences and the quality differences were.

We developed a study we called, “Benchmarking Semiconductor Manufacturing”, and I owe a great amount to my colleague in industrial engineering, Rob Leachman. Just about the time we started this, I became Dean, so I didn’t have a whole lot of time, but I knew the technology and I knew the industry, and I had the contacts to get us some access. Rob was the guy who figured out the right questions to ask, but one thing that I pushed for, and we did do, we decided at the beginning, “we don’t know how to do this, we are going to do a pilot study, to try to figure out how this should be done, so it’s really a preliminary sort of thing” and then we leaned upon some good friends in the industry to make time for us, to work with us. The first one was HP, which was producing and manufacturing wafers in Corvallis already and we got NEC in Japan, and we got Intel, one of the Intel plants. We learned a lot and we modified the plans quite a bit, based on those first three.

CL: So the idea was to just go to the plant and see how they were organized?

DH: Yes, we broke it down, we were already starting to look at the productivity of individual processes and steps, such as the deposition steps or the lithography steps, and we decided we had to get people to give us a lot of objective data in the beginning, that is, how many machines do you have working, what kind are they, and what is your record of productivity with those machines over a period of time. So that the main study, shaped by the pilot study, had this hundred page questionnaire which almost drove people crazy, but it turned out to be data that really almost everybody had, so they had the data and the management decided they wanted to cooperate, they could tell some staff to complete the survey. The other thing of course was the question of proprietary information and concern about their proprietary rights. Companies in general had been pretty secretive and there had been a previous study by Rebecca Henderson. Do you know the name?

CL: Yes, I know her name.

DH: She had taken 5 or 6 years because she had a baby in the middle of this, or something, and she was a business professor who didn’t know the technology, didn’t know the industry very well, so she asked a lot of questions which people thought were really too naïve. They were afraid we were another round of that. So we were able to get an audience, at lunch time, with the board of directors of the Semiconductor Industry Association which was then chaired by Gordon Moore. I knew Gordon for a long time and he said “sure, c’mon.” So we went and we gave about a 10-15 minute presentation about what we wanted to do and asked for their endorsement and support. So, you know, Gordon controlled that group and Gordon basically said, “well, colleagues, I think we ought to support this thing.” Well, OK that didn’t obligate any one of them, but the fact that we got HP, NEC, and Intel as the first partners was influential, so it went very well. I only went on about a quarter of the site visits. This extended over several years and several generations of technology. Rob [Leachman] really ran all that, and we had another participant you may know, Clair Brown, from economics, she's a labor economist, who added a really good dimension, because she focused on the labor practices which are quite different in Japan, Korea, the United States. European participation was pretty limited. ST cooperated. Phillips and Infineon in my memory were not cooperative. We had one other European I think, but we got basically all the big players in Japan, and Samsung in Korea.

CL: How did you convince the Japanese companies to participate in the study?

DH: Well, the nearest equivalent to Gordon Moore in Japan is Hajime Sasaki, who at that time was head of all NEC Semiconductor operations and he was another old friend of mine and he was definitely the most westward looking of the people in place at Japan at that time. Very fluent English, spent time, a lot of time abroad, and the company itself, NEC, has always been one of the more internationally minded. I don't know what he said in Japan, but basically we didn't get 100% of what we asked for, but we got four or five of the main Japanese companies, and Korea came later. The only one we didn't get in the United States was Micron; they never would play; they never would participate. But we got Intel, TI, and Motorola, and IBM.

Rob developed a very systematic way of dealing with all this data, and talking to them when they said, "why should we give you our proprietary data", we said "we're going to give you back a chart from each one of your major processes showing performance, or whatever parameters chosen, productivity, yield, or reliability over a period of time;" we want three year data. We wanted to do factories that had been operating for at least three years, so they would have established a pattern. We plotted comparable data from all participants, but maintained their anonymity. That was a very strong point because people had not had access to comparative data, and they would say well, we are 20% better this year than last year, but everybody was improving.

At the beginning we thought, and most people thought, that the differentiator would be the rate of improvement. That some would improve slowly and some would improve much more rapidly, but the reality was that when we got done with it, the rate of improvement in general was pretty much all the same. The starting point was different, that is, how thoroughly had you developed the technology before you started production. You might have developed it in a separate lab, as some companies do, or you might have developed it right on the manufacturing line. Today the latter is pretty much the norm, but even if those factors are the same, some people are just much more disciplined and thorough about debugging things and documenting and characterizing their capability before they commit to production, but on the other hand you don't want to delay too long because, you know, one of the economic studies we did is, what's the price of delay? So, you know, if you're six months late, but 10% better it's a loser.

It was a very fascinating, very fascinating study, and, you know, really a terrific experience. The students did most of the work, Rob set up the framework. I thought this was going to be a challenging project for electrical engineering graduate students. Well, it wasn't because the problems weren't electrical engineering problems, they were mostly industrial engineering problems, or mechanical engineering problems. So it was more IE students than any other, but we had business students, and we had economics, human resources students, and we would typically have a team of two or three faculty. We had a couple non-faculty people with a lot of industry experience that participated in some of those visits, and then we had 4 or 5 students.

Another thing we did was that we made a decision that we would keep that whole group together, during all the meetings, we asked for meetings in the plant with the different players, with the engineering staff, with the operators, and with the management, and maybe a couple specific groups like lithography, and we had our whole team, six or seven people, present in all of that. It was really pretty interesting how, you know the blind man and the elephant, what they hear is affected by where they come from, but typically the evening after visits our team had dinner together and talk it all through, and come to some convergent view of what they really see. Most of it was not walking around factories; most of it was sitting in windowless conference rooms interviewing people who ran the factories. We'd always have like one hour to go through the factory; in total, typically spent a day and a half or so. So, it was a fascinating study.

CL: What were the results?

DH: The results were that, well that part about the starting point is most important. We thought some particular set of labor practices would prove to be the differentiator and it's a little hard to make that case, but two extreme cases were the Japanese, where it was lifetime employment, and at that time, which was in the 90's, still almost 100% male because the women couldn't do shift work. In Korea the labor force, the direct labor was all young women who worked for 3-5 years before marrying and leaving the labor force. In Korea the training was like the army, total discipline, do exactly what we tell you to do, don't even think about doing anything different. In Japan, after you mastered your process, be thinking about it all the time, be observing it, and be trying to think about how to improve it. Suggestion boxes and rewards for that. Two extremes, and then the United States and Taiwan, we had the major Taiwan players too. Taiwan was really driven by Morris Chang and the others that went back. It's American, which is a mishmash, it’s not any single consistent policy. The closest thing to a consistent policy in the US was at Texas Instruments where there were an awful lot of TI people that were career people with TI. They hadn’t jumped around a lot, but there weren’t a lot of other companies there in Texas. At TI, we got their whole record of twenty five years of personnel in, I think, their semiconductor manufacturing operations. Their performance evaluations, and their pay, and their training. Clair Brown just had a ball with that - fascinating stuff.

The bottom line message was that very strong discipline in developing the process was key; that there is definitely a pay-off in the long run for real-time monitoring; and when you can, close the control loops, which is getting to be more common now. I mean it used to be a completely open loop in the sense you put the wafer though 175 different steps and after the end you measured it in and oops something is wrong. So, you can’t afford that; you have to be constantly monitoring individual process steps. So, those are some of the best practices.

CL: How did the Americans compare with the Japanese and the Europeans?

DH: The productivity performance was best in Asia and it differed by firms. Everybody liked these comparative charts and as I thought about it a couple years ago, what was the outcome? The outcome was that a whole lot of inefficient people got out of business, and the older firms that are left in the United States are IBM, Texas Instruments, Intel, and Micron. We never did Micron. There was a philosophical difference in the firms. IBM spent a lot more on quality control, because they said, well you know, it’s going into this computer that is going to be out there twenty years. Some of them spent a lot less. Their product goes into a cell phone, and if it fails after five years, what does it matter? IBM, had the highest cost, by our estimate. We never asked about costs; we never asked that, we just did the raw data. How many machines? How many people? How many wafers? So that was a very good strategic position. The bad guys got out of the business. Intel had already in the 80’s done a lot on quality control. I think Craig Barrett deserves a huge amount of credit. He was a professor of material sciences in Stanford, you probably talked to him.

CL: I know him, but never met him.

DH: He came into Intel and he thought, everybody thought that he would stay a year or two at Intel and then go back to Stanford, but they assigned him to work on improving manufacturing. He really did a wonderful job. I’ve asked Intel people how much of a credit he deserves, and they said, “well a lot of people did a lot of things, but Craig deserves a lot of credit.” He was a PhD guy in charge of manufacturing who was naturally inclined to be whenever possible more scientific, more analytical than just do a five way split where you tweak the process five different ways and see what happens.

CL: He's the one who pushed for having plants built on the same model right?

DH: That's not so much Barrett as it was Gerry Parker, another PhD, a Caltech PhD. At least Gerry Parker was the one that talked the most about it. He was the first one I heard talking about copy exactly. Well, of course the equipment industry hates that because it doesn't give them a second chance. If Intel makes the decision for brand x, brand y is never going to sell to Intel. Not for a long time, and also [another important rule was] to reuse 70%. I think the (Intel) rule is that we are going to reuse 70% of our equipment in the new generation of process and that means we are only going to go as far as we can subject to that constraint.

It turned, out of course, that the process generations actually went faster than the roadmap ever said, and that constraint didn't prevent the generations from moving faster, more rapidly, and yet it was still terribly expensive, and the complaint was outside of Intel that even reusing 70%, the cost of keeping up the pace with the new generations, with the purchase of 30% new equipment every two years, was going to bankrupt us. The less efficient producers basically couldn't make any money, so it would be hard to justify new investments and they disappeared. So, a lot of those old facilities have been put back to work for other purposes like solar cells and so on, and it's not that the investment is lost, but basically the investment has to be written down pretty close to zero. I heard a wonderful talk by T.J. Rogers about buying up obsolete equipment from semiconductor makers for 5 or 10 cents on the dollar and making it highly productive in building solar cells - a good form of recycling.

Sloan had [a study] on the steel industry. They had one on the auto industry, the first one was the auto industry. They weren't the initial sponsor. *The Machine that Changed the World*, 300,000 copies of that book sold. That really inspired Sloan to look at other industries, and most of these studies were led primarily by business and economics faculty. Engineering faculty were less involved, but all those studies were quite interesting, and most of them resulted in a book on the industry. We put out about 50 or 55 substantial reports which we distributed and we just charged the cost of reproduction and distribution. Sloan kept saying to us "when are you going to do the book?" and both Rob and I felt that the people who were really interested in this, that is the people in the industry, have full access to all of the information and someone isn't going to sit in an armchair and read that book if they're a member of the industry. If you're not a member of the industry, a lot of the details of what goes on are beyond access to the layman. The general things like different policies for direct labor and so on, other people have written about that, so we never did a book.

Sloan required that we get industry matching money and we did, starting about year three. I think Sloan was giving us like $300k and we'd get another $200k from companies or industry organizations. Most of the expense was international travel, that was expensive, but as we got towards the end of the project, actually the parameters that we were evaluating became known in the industry as the Berkeley Metrics. The Semiconductor Industry Association and the equipment group sort of put their endorsement on it and said "we encourage you to use these metrics." They tried to collect the data and they'd get spotty response, so this capability of continuously watching your performance relative to others is less available now, but the industry is more mature.

By about 2002, Rob and I sort of lost interest. We'd figured we've done what we could and we had a very good time with it, and we were willing to let it go, and Sloan was disappointed. They visualized these things going on indefinitely, so the auto study still goes on, and there's an airline industry study, a banking study. They convene annual meetings with all the scholars that are involved in industry studies. I just missed the last one in May, but I did attend the previous ones and I'm not that fond of going to a meeting where there is a bunch of business school professors that don't really understand the nuts and bolts of the industry. They like to talk about business models and things like that. They [Sloan] were hoping that there would be some larger synthesis that would come out of this - that all these industry studies taken together would lead to some great insights about how to be more competitive, but I don't think that has been the result.

There were other offshoots from this and my colleague Costas Spanos (who has now been just appointed Vice Chair of the department) has a much more focused research program that has been very successful on improving process control as the dimensions keep getting smaller. What they are worried about is dimensional control. He has done a lot of very interesting work, and three of his students founded a company that was doing what they call in-line metrology. After your wafers come out of an etching process for instance, they use an ellipsometer, a standard instrument, and they scan the wafer and check the line edge profiles. They're trying to make things that look like this, and they can basically image them and get feedback right away in real time if something's gone wrong in processes and control. That company started with three students and seven PCs; they wrote the software. Costas always worked closely with industry. Actually one of the leaders in adoption of new process control stuff is AMD. Their idea is that we only have one state of the art fab it's got to be very efficient.

CL: Of course

DH: So this is now 2001. AMD came around and said, “well we’d like to buy four copies of your software.” These students were immigrants. There was one student from India, one from China. They didn’t know, so they chewed on it for a while, and they said well, $25,000. So AMD wrote them a check for $100,000 and got their four copies. They were prepared to deliver four copies for $25,000. A year or two later there was a bidding war, this was in ’01 now, the bubble had already started to leak, but all the equipment makers wanted to add this capability and there was a bidding war for this little company. Those three students sold the company for $120 million to TEL. You’ve heard that story?

CL: I’ve heard that story.

DH: The second company was actually sold within the last year I think to somebody like KLA. The second one was founded by Spanos. (The first one was founded by his students.) Spanos built a special wafer, starting with a standard wafer with something like 44 little temperature sensors all over the area and then a circuit on the wafer that transmits this information out. I It has a battery on the wafer and it telemeters the data out up to about 300° C, I think, maybe 250° C, but there’s one critical step for dimensional control which is the heat treatments with the photo-resist after you coat the wafer with the photo-resist, but before you expose it or maybe after you’ve exposed it and developed it, there are two steps. It turns out that 1/10 of a degree variation in the temperature across the wafer during one of these steps is enough to exceed the allowable tolerance. …I mean I don’t know exactly what the numbers are, but it’s very, very sensitive. So they’re selling these wafers, I think they sell them for about $10,000 and they’re good for about 50 rounds, and they make these in a little place out in Concord. It’s not even a lab. I don’t know how they do it. They’ve sold millions of dollars worth of these, of these wafers, of these sensors. So, that’s the kind of things Spanos does, the techniques for improving process control, and he has a colleague in Mechanical Engineering named Kameshwar Poolla; it’s a very good program. When Sloan asks, I say that we still have a program, but it’s focused on these other points. What we found, the industry support for the big study, they weren’t willing to continue it. They said, look you know, the Berkeley Metrics are great we’re using them. Now we have a specific problem. We’d like you to work on this problem, and Spanos got some of his problems that way. But it was very focused. Sloan really wanted to have the mix of engineering, business, economics. These are not problems that require that mix. You can’t get dissertations for all of those students. So, it’s bound to change and I told them you know, you can’t expect anything to go on forever. You have to look at the natural life cycle of certain kinds of studies.

CL: What was the connection between your program and the Spanos program?

DH: There was never any Sloan money for Spanos. The only connection was sort of intellectual. In the benchmarking study, we had seen the need for better controls, and this is sort of an inspiration for those guys. It's a nice way to have things work.

CL: Andy Neureuther was involved too.

DH: Neureuther, exactly. I should have mentioned that, because that predates Spanos and his focus on the lithography. Andy Neureuther and Bill Oldham never had Sloan funding, but they had SRC funding for years and years, and Spanos has those also, but he also draws from the UC Discovery program has had several big grants from them. They have to reshape it every four years or something. Do you still have those rules?

Founding of the *IEEE Transactions on Semiconductor Manufacturing,* 1987-90

CL: Yes. Could you discuss the ways in which you founded the *IEEE Transactions on Semiconductor Manufacturing*?

DH: There was an old guy from General Motors and we were out for a walk; the semiconductor manufacturing group had their annual retreat, and we were up on the Snake River in Wyoming. We were out for a walk and he says we need to have a serious kind of publication for manufacturing, and you know, they really twisted my arm to do this. I had been an editor of the *IEEE Journal of Solid-State Circuits*, and so I said ok. It's always a little slow to get something started, and the industry cooperation wasn't there. Well many of the US manufacturing guys were bachelor's degree guys, so if you look at that publication even today, there are not many contributions from US industry.

CL: I've seen copies of it.

DH: Recently the authorship is more than 50% outside the US, and the authorship within the US is mostly academic. So, you know, we still haven't transcended that. We also started a conference. I was one of about a dozen that started what is now known as the International Symposium on Semiconductor Manufacturing, ISSM, and it alternates between the US and Japan. It's going to be in Japan in October, and it was in October here. In the odd years it's here, and the even years it's in Japan. But the attendance is always bigger in Japan, and it's really struggling here; it gets about three hundred people. You can't make a break even budget.

CL: Who comes to the US conference?

DH: You know a lot come from the equipment industry, and the equipment industry does hire PhDs now in chemical engineering, or mechanical engineering, or industrial. So they're the strongest sponsors. IBM has always boycotted it because they support one that's in Boston, and they've supported it for years. They say, well its closer to IBM location, but the Boston one has basically devolved into just a tradeshow in the sense that the speakers are people promoting their products as opposed to people who have done independent studies. The standard is not high. The ISSM standard has been pretty high, but there are just not enough players in the US. Intel always sends a bunch of people. Last time, four or five people came from TI, and one or two from Micron, and some from KLA-Tencor, and Applied Materials quite a lot. So, there is not a lot of activity in the US. Taiwan guys always show up in numbers; most of them were educated here.

CL: Many at UC Berkeley right?

DH: Many of them, yeah.

Service as Department Chair and Dean, 1989-96

CL: You became an administrator in the late 1980s.

DH: I was administrator at UC Berkeley starting in '89 as Chairman of the department. I don't have a lot of memories; it was a one year thing. Hiring was always a big piece of the job for the chairman - recruiting new faculty people. That was just before they had the VERIP. When was the first Voluntary Early Retirement Program? So we were just, let's see, Soda Hall had been committed and I think the construction had not started, or maybe was just started, so computer science was all in Evans Hall, and as long as they were there that was a problem.

CL: Because of bad interactions between the faculty?

DH: Well, yeah and we were working continuously to try to get what's now been accomplished, it's only 15 years later. The chair has also a lot of work on the review and coaching of the junior faculty, and I think we've had some very good people that came along during that time. In 1990, I became Dean. Karl Pister had been Dean for two five year terms, and campus policy is that's pretty much the limit, at least for a large unit. Karl would have liked to continue as Dean, but then he became Chancellor of Santa Cruz. There was an internal search. There may have been candidates from other campuses. I had thought at some point I'd like to be Dean and maybe that would be my final career step. So, there were interviews and so on, and let's see Jud[son] King who was provost asked me, but that was just about the time Chang-Lin Tien became Chancellor. So I know I talked with him too. He was an inspiring guy. King was my boss for the first three years I think, while I was Dean and of course King was an engineer, and a very cautious guy, not any innovator. Actually I always felt that Carol Christ was a much better provost than Jud was, even though she was English and he was engineering, because Carol said, "you know, I understand you have problems, you can come to me with your problems, but bring me some ideas for solutions." You go to Carol and say, "alright here is my problem and here are some alternatives" and she, you know, right away or at most the next day she'd say "well I can help you with one or two, but there's nothing I can do about three." Whereas Jud would say, "yeah hmm that sounds like a problem," and then you never get any other feedback. Carol is very successful at Smith, I understand. She's president of Smith College.

Managing the VERIP was a big problem; I think all three VERIPs occurred while I was sitting as Dean, and so it was a morale problem, and it was also a problem that actually over the three VERIPs we lost 30% of our faculty in a space of about four years.

CL: The goal was to ask people to leave?

DH: No, we did not ask people to retire. That was the last budget crisis and it was saved by the fact that the retirement fund investments had done so well that they had gone way beyond actuarial needs, so they could give people some kind of bonus for retiring. So they'd get them off the operating budget, right, and sweeten up their retirement. I forgot what it was. It was substantial. If you agree to retire this year, you will get this much extra in terms of pension, and it was like or 15-20%.

CL: Oh, that's substantial.

DH: So most people would sit down and say well, gee whiz, I'd have to work three or four more years to match that if I just go on. There was a lot of speculation that there might be more than one VERIP cycle, and there were. There were three cycles and the terms were slightly different each time. So, very few people under sixty retired, but there were a lot of people over sixty. So, some of those people had the firm belief that their departure would cause the ruin of Berkeley; that Berkeley would never be the same. It didn't turn out that way. Nobody is indispensible and this was most evident in civil engineering. The younger crowd has done a terrific job and has definitely kept the reputation of the department up.

CL: Did you bring more people in?

DH: Yes, we hired, I think, an amazing number of people. I think we hired about sixty people in my six years as Dean. When I started I believe there were maybe two women on the college faculty. Of the sixty we hired, I think about ten were women. The candidate pool was not that large.

The Hearst Mining building, that was a big job for me. It was already a historic landmark and couldn't be demolished. The walls are unreinforced masonry and so the structural people said well we're going to get inside that building and strip the interior out of it completely, and put up a new steel framework all the way around the interior so it won't collapse. The historic folks were upset; I mean that would have destroyed that lobby. You've been in the lobby?

CL: Yes, it is a very nice lobby. It's beautiful.

DH: Very nice. So what I spent a fair amount of time was fighting for the ultimate solution which was seismic isolation. So, they got underneath and they put, I don't know, 120 or something great big steel and rubber isolators, and then around the perimeter, you can't see it, but there is a two foot wide, you'd call it a moat, there's no water in it; there is, you know, cosmetic cover over it. So, what happens in an earthquake is the earth moves rapidly and the isolators dampen the building, following slowly behind. So it made it unnecessary to build the steel framework and that of course when they started working on it, they found a lot of asbestos and the costs kept going up, so it cost $80 million for that job. I had to go down to the Office of the President. We had one of the world's leaders on the faculty here [on seismic isolation] and they'd already built things in LA. They built a hospital down there; it was a new construction, but eventually the Oakland City Hall was done that way. So it's now become an established technique, and it's not more costly than a rigid frame.

CL: But you had to raise private dollars to pay for it right?

DH: Yes. I forgot how much we had to raise, but we didn't have to raise it all. We had to raise a good part of it. We were also raising money for endowed chairs. When I became Dean we had two or three endowed chairs in the college, and during my term - that was the focus of that capital campaign - engineering picked up about forty in six years during that campaign. It became a very fashionable thing to do. The new model now for endowed chairs was the Hewlett gift, which is a better model in the sense that some of the money goes into an endowment for institutional use, as opposed to just for the pocket of the professor.

Karl [Pister] had built up a development office in the College during the 80s. The head of the alumni group and Karl were terribly afraid that the new Dean would dismantle this, because it hadn't yet really born a lot of fruit. As I said, we only had two or three Chairs. We had done the Soda Hall. The major gift for Soda Hall was already in place when I started. There was a lot more that had to be done, but we had a development staff. Marily Howekamp was the head of it. It had real capability, and they did everything very professionally, so I had many, many visits with these prospective donors for the endowed chairs particularly, but also for Soda Hall. I went to Japan with Chang-Lin [Tien] just after I was appointed, before the bubble had really burst in Japan, and we got a lot of Japanese money for that building. I went to Japan at least once a year and I think in six years, probably, I went ten times. So, that was the kind of thing that Deans do. I would go to the meetings with Rob [Leachman] and the students and talk about the semiconductor benchmarking study, and I could usually help with some insights about what they were seeing, but I probably didn't spend more than two or three hours a week on it.

Evolution of solid state and EDA programs in the 1990s

CL: Could you discuss the evolution of solid state electronics and EDA programs at Berkeley during your tenure as dean?

DH: The MEMS stuff was actually just getting started I think about that time and has become a really big program, very distinctive. Several of the solid state faculty lead that program. There are MEMS programs at other institutions, but ours is probably the biggest and probably the best. EDA was continuing. Newton and Sangiovanni-Vincentelli and Brayton were a very strong team with a lot of industry contacts and always able to identify the emerging opportunities first and pick the part that the university could do and do it. That was a big, big program and Don [Pederson] still played the godfather role for these younger faculty. Just trying to think about solid state, so the amount of work on optics has gone up and down and other places have probably had stronger programs in optoelectronics than we do.

CL: Like UCLA?

DH: Stanford and MIT, but you know there was an awful lot of fiber installed around the world, and a lot of it is still dark, and this has really dampened down the funding for that. People say, well what's needed, you know, it's all done. So, it's also just the turnover of the faculty. Our Kam Lau got a disabling brain tumor and Connie [Chang-Hasnain] has moved as our lead player. Connie Chang-Hasnain, who was a student of Whinnery's, taught at Stanford before she moved over here. Connie tends to work as an individual.

She doesn’t build a program with a team. Ming Wu is an excellent guy that we recruited from UCLA, another Berkeley guy who had been somewhere else and brought back, but he is mostly focused on opto-MEMS, things with optics in MEMS.

CL: The EDA faculty turned to entrepreneurship in the mid-1980s and 1990s. How was it viewed and managed in the department and in the school of engineering?

DH: Well, very valid question. Don [Pederson] was very thoughtful about these things and had established a pattern of his own and a culture that didn’t have a whole lot of trouble staying within the boundaries that UC expects. So people, many of the faculty would be involved, a day a week, with consulting work. The department in general would approve leaves - full time leaves for up to a year at a time, renewable once or in a couple of cases for a total of three years. We didn’t pursue a lot of intellectual property protection because philosophically we didn’t think that was a good way to spend your time; it wasn’t likely to produce a lot of revenue. So, it’s a lot more fun to be free to get up and tell your story, spill your ideas and see what feedback you can get, and that was the model people used even when they were working as a consultant. So they have to use good judgment and not say things that would upset anybody. So I think those people [the EDA group] didn’t take a two year leave during that time. So we haven’t had any trouble in the EDA area.

We had a recent flap with one of our MEMS guys who ended up resigning and going to Stanford - very determined guy, Roger Howe. This is after my Dean term. Roger felt that he couldn’t live with the requirement that the UC imposes, that any patent disclosures that you may be involved in as a consultant must be disclosed to UC, and UC is entitled to a claim on those, unilaterally entitled to make a claim. So it’s not a negotiation; it’s a unilateral determination, and he did not feel that he could live with that. The university auditor who investigated all of that from OP, from Oakland - Fitzgerald or something. He questioned about 30 or 40 faculty members on these matters, and a report was given to the Regents and then the Provosts were asked to review this report, and Paul Gray worked on that, and I don’t remember the details. So they wrote some new verbiage for the academic personnel manual, and it went to the regents and it was approved, and it went into the manual and nothing’s changed, but it was sort of laid to rest for a while.

MEMS an area in which patents may be valuable sometimes. The industrial guys are still very interested in having the patents because they want to have something to negotiate with when they get sued. They need to have some patents, even if as a stand-alone thing they’re not valuable or they wouldn’t produce revenue. They just have to have something. So you could understand why they would want to have them, and the best way for a faculty member to avoid this kind of problem would be to be working on something different at the university during that period of time and maybe what you did before is what you are consulting on, but that’s hard; it’s hard to do. I want to have a conversation with Roger Howe and see if the reality of his daily life is any different at Stanford. I doubt it. So I did talk about the MEMS research here.

Corporate Directorships, 1995-2007

CL: Could you discuss your activities as Director of Mentor Graphics?

DH: Mentor was the oldest stand-alone firm in EDA. Newton and Sangiovanni were tied up with the founding of both Cadence and Synopsis. Mentor had a change in management. The founder had been there for a long time, he’d been kicked out and a guy was brought in from Texas Instruments whom I’d known for a long time, Wally [Rhines]. He is a perfect CEO in the sense of all the outside dealings, he is very personable, everybody likes him and…

CL: Wally is the one from…

DH: Wally is the one from TI, yeah. I’ve forgotten the name of the founder. So that was about a year before I started, he’d come in, a year before I joined the Board.

CL: Could you discuss your involvement in firms started by your students?

DH: The most important directorship I held was with Silicon Image, where David Lee, the founder, was my student and during its first year he came right here in this room, I had just finished as Dean as I recall and he said, “would you join my board?” I had a high regard for David and he is the kind of person that’s good for that role. Even when he was a student, I characterized him as always having his eye on the ball. That is the most important thing right? He was very good at that, just what you need and he’s also very good. He recruited good people for the company and for two or three years I don’t think they ever had a separation. Nobody resigned and nobody was fired. He just got people and they loved working there, they worked hard, and he was the perfect guy for that stage in business. It was a lot of fun at that point, and he also got Andy Rappaport on the board as an investor, a venture capitalist. Andy is a terrific Director, just really good, again eye on the ball, anticipating problems before they are serious, terrific. So that was fun and then so this was ’96, ’97. The company had products and had a little bit of sales, but no profits, nothing but losses, but you know it was the heart of the market run up and Andy said, his sixth sense was always excellent, he said “we should go public.” So I think it was October ’97.

CL: The company was two years old by then?

DH: Two or three years old, yeah, and the only reason it had any plausibility at all was that it got a big contract from Compaq, and Compaq for one year put Silicon Image graphics interface, digital interface, called DVI on every Presario computer, and probably without that the company wouldn't have survived. Compaq had thought that 20% of customers would buy a flat display, but they were still much too expensive and only about 3% bought the flat screens so they took the DVI interface off, but that got the company going. I was on that board for nine or almost ten years I think.

CL: Were Silicon Image products manufactured in Taiwan?

DH: Almost entirely Taiwan Semiconductor, and they worked well together. It was a perfect situation for a young company. Another founder who was never in residence for long now is a professor at Seoul National University. He was a student here, student of mine, and there were a couple other guys that were my students who were there, and so all the things that might have happened did happen.

But the most dramatic problems arose after David made some management mistakes; they weren't criminal mistakes or illegal mistakes, they were judgment mistakes. The most egregious was that he had an attorney, the in-house attorney, that David and the Board agreed was not good enough and he had to be replaced, there had to be a better one and David told this guy "you're not good enough and as soon as we can find a replacement you're out," I mean I don't think he used those words but it went on for a year. The guy made up a complaint charging all kinds of stuff which had hardly a shred of evidence to support it, but it was enough to put on the brakes, and asserting certain things that would have been crimes.

SEC and NASDAQ demanded that we have an independent investigation, and they did all the forensic stuff, and it cost $2 million and endless interviews and telephone calls, and they came to the end of the line and they basically said well, "maybe there was some bad judgment here, but there is nothing more." But the mistakes unfortunately continued, and they weren't things that brought on any further employee lawsuits, but they were things that the board was upset about, properly so, and David got himself in the mindset that the board didn't show him sufficient respect. He's been here since he was a teenager, but this is kind of an Asian reaction. I said David, I took him out to lunch twice, and said "David you have a big problem with this Board, and you may think that they don't show you respect, but the only way you get respect here is by your performance, and they feel you are falling short." Then I told him what I thought was wrong, and the Chairman of the Board had asked him for a meeting to discuss these issues, and David had basically refused to meet with the Chairman of his Board of Directors, privately, to discuss the matters of dispute.

CL: How much did he own of the company? What was his share?

DH: Less than 10%. So, the board put on pressure, and David agreed; he wanted to sort of move up to be Chairman of the Board and have a CEO, so we recruited a new CEO, a guy whose resume and record was entirely appropriate to the job, industry experience with another company as COO, and the guy came in and he quit after three months, because he said David is still completely in control. He gave us the evidence; there was plenty of evidence. So the guy quit. I think we'll just stop because there were more incidents. So finally it ended up that there was a confrontation. The board demanded that David truly step aside, and he said no, and so there were five independent directors, four of them resigned on a Sunday night. Four independent directors resigned, I was the last one. David finally recognized the seriousness of the problem and resigned. I didn't resign. I was the only one that had any stake in the company at all, the other Directors had options that were worthless. So, you know, we were in violation of public company registration. Monday morning I went down there and the attorney from Fenwick and West, very good calm guy, said this is how we work our way through this to get out of it. I may have gotten some of the sequence a little mixed up there, but it was a nightmare for me. So we had a guy, an internal guy, who had been passed over for CEO, well we appointed him, and we had to recruit some new Directors. NASDAQ gave us 60 days or something, and we did it. We recruited enough new Directors.

A few months ago, David called and said "my wife and I would like to come out and take you out to dinner at Chez Panisse." So we had dinner at Chez Panisse three months ago and then last week we had dinner at their home with several other friends of theirs, and so we're reconciled. We have not talked about those circumstances; I'm not going to bring it up. I think he realizes now that he was the right guy for the first six or seven years, but as often happens, he wasn't the right guy anymore. So he is very entrepreneurial and he's gotten into two or three new things and he's got a lovely trophy home that he built with some of his stock proceeds, and charming family and I don't hold any grudge at all. I still have a lot of respect for him, but he doesn't see, he can't see it when he's suddenly gotten crosswise to the current.

CL: It's a tough job to be the CEO of a small company. It's hard not to make mistakes.

DH: The Marvell students, they were Gray's students. I knew those guys, I still know them, not well. ClariPhy, that's Oscar Agazzi and I haven't seen him in a long time. I'd love to see him. So were there any others in particular? Not any really big ones, a lot of little ones.

CL: How did Silicon Image do in the stock market?

DH: It has about $300 million of revenue, and it's profitable, and the stock is about $5-6 dollars a share. That's in fact the price at which it came public in 1999. It was up to $66 at one point. I was not permitted to sell my stock at that time, but eventually I made some money on it.

Evolution of engineering research at Berkeley, 2000 onwards

CL: How would you compare the evolution of the solid state program at Berkeley with the one at Stanford?

DH: Don [Pederson] got the integrated circuits program going and drove it and his close associates or former students are still major players. John Linvill was the nearest equivalent guy at Stanford. Berkeley's undergrad program is about three times the size of Stanford's. The doctoral programs are about the same size. Stanford's faculty is smaller. I think that Stanford has always had very good relations with industry and they built that Center for Integrated Systems, 15 or 20 years ago, and all we were able to do at that time was the lab downstairs. I think we've broadened our engagement beyond just integrated circuits to a lot of ancillary fields, more so than Stanford has - including biological scientists and material scientists, chemical engineers, mechanical engineers. I think we have a bigger program here, and it is a good thing because Stanford currently has an NSF sort of infrastructure operating grant for their program that we don't have. But we have 300-400 grad student users of our facilities and we also have some non-academic users which pay twice the rate, so ours is financially viable, wouldn't be with half as many users. At Berkeley, we have Ali Niknejad and we have Bora Nikolic, and we have Bernhard Boser, Jan Rabaey, and one or two more. We have more people, more activity [than at Stanford]. It's a little harder in this field to find the academic suitable topics for dissertations than I think it was then. So there is a certain evolution, but that means that these guys are evolving themselves.

CL: Why is it difficult to find a dissertation topic?

DH: It's a more mature field. When we started on this, MOS was only used for digital purposes, and there was just a whole lot of unplowed ground that we plowed pretty thoroughly. Now some people are trying to use carbon nanotubes, you know, making transistors out of carbon nanotubes. They don't work very well and there's just less unplowed ground. I would divide our field into the physics side and the systems side. On the physics side, [there is] brand new stuff but there are a lot of other players, properly so – chemists and physicists. So far there's no device candidate on the horizon to replace the transistor, and as I say what we can do with the transistor is pretty mature - not a lot of new opportunity. But on the systems side, I think there is a lot of opportunity and the problem there is managing complexity. These are getting very complicated, how do you manage that and how do you increase your rate of success at defining what ought to be done, because almost anything can be done. What's going to be worthwhile to do, good value? We are better prepared to do that than the physicists or the chemists, or the materials scientists. On the physics side, I think maybe they are the ones that are better prepared to go to what's beyond the transistor. So maybe, you know, it's cyclical; they come up with something really good and maybe we can put it to work. So circuits people have a little hard time accepting that idea. I mean that's my private comment on some of my younger colleagues. They'd like to go on doing what we did for the last twenty years and like to go on doing it for the next ten years, but I'm not sure the opportunity is all that good. I'm just cautious about it.

CL: What are your views on the role of Berkeley in the rise of Silicon Valley?

DH: I think Berkeley and Stanford were both very, very instrumental in the rise of Silicon Valley, but we can't get all the credit for the cultural things that Annalee Saxenian points out, contrasting it with the Route 128. I think Berkeley's and Stanford's engagement with the industry is a lot stronger than MIT's, a lot more extensive. We feel that it is really invaluable to have the worthy rival nearby, across the bay, and MIT doesn't have a nearby worthy rival.

CL: What does it do for just like Berkeley and Stanford to have the rival on the other side of the bay?

DH: We actually have friendly meetings quite often, informal or formal. There is a flux of people back and forth going to seminars. Stanford put on a show, invited all our grad students down there, not the whole department but one field or another. I think computer science people have been particularly good at this.

CL: What are your views on the IC program at UCLA.

DH: They have a couple of very, very strong individuals, and once again it's the thing that they aren't like Pederson or John Whinnery sort of trying to pull things together. One of their best guys Asad Abidi, who is a graduate of Berkeley and is a very, very strong contributor, won a couple of major awards and he's also been elected to the National Academy of Engineering. He's gone back to Pakistan.

CL: Yes, I sort of know him.

DH: Then there is the other guy Razavi. Isn't he UCLA?

CL: Yes, he's UCLA.

DH: Well apparently those two are both very highly respected for their own personal contributions, but they are not friendly.

CL: We could talk for a few more minutes about your reflections on solid state electronics research in an academic setting.

DH: I think engineering research in an academic setting really profits from having parallel and communicating efforts on fundamental understanding and on applications, and having those people continually engaged with one another, and sometimes the application sense in modern engineering, which is so complicated, rarely comes from somebody who's spent his whole career in academic settings. So, I think hiring faculty people that have some experience in industry helps a lot, and that almost everyone one of our integrated circuits people has to have that before they were hired, and those that didn't have it, the department encouraged to go out, take a leave and work in industry for a year or two, and that's paid really big dividends. So they have some sense themselves, but they also have some contacts with industry, so the proximity of industry and being able to talk to those folks, even if the faculty has a more fundamental inclination, often generates ideas that can be pursued here effectively.

I'm going to be on the external review for the electrical engineering at Illinois next month, and they sent advance questions, and the advance questions, the latter part of it was how can we stimulate more of this engagement with industry and with applications; terribly hard at Champaign Urbana. So that engagement, I think, is very important and I think that MICRO and the UC Discovery Programs have been the institutional mechanism to encourage that. I think that they're terrific programs, and absolutely something that every engineering institution ought to have on their list of valuable things they can do. With a small amount of money, I think UC has created engagements that wouldn't have existed otherwise, and I hope that understanding percolates through so they don't all get slaughtered.

CL: You were making this comment about mixing fundamentals with application, how would you define that method?

DH: Fundamentals really belong to the physicists, right, but a lot of our guys have physics backgrounds, and so for instance the lithography program is a perfect example, where they have a wonderful blend of fundamentals and applications. So, they had partners, they had a very important partner named Grant Willson, who is now at the University of Texas.

CL: He is the one behind chemical amplifiers

DH: Yes. Right and they learn a lot from the optics physics people here, but this technique now for taking radiation of 190 nanometer wavelength and defining a feature of 45 nanometers, that fundamental understanding is computational optics. Some of that came from Professor Avideh Zakhor who basically applied her communications systems theory to the process of transferring an image from a mask to the wafer with radiation four times the wavelength from the thing you're trying to build. When she was a young professor, Bill Oldham said come on, I'm going to fund you to work on this and she conceived of a way that this is now done, about ten years ago, and she stood up in front of the SRC group and told them what she thought she could do; nobody believed it. Nobody believed you could make something of 45 nanometers with 190 nanometer wavelength. It used to be that Bell Labs and IBM had research labs that had people do all those things. I think that's where Willson came from actually.

CL: Yes, he was at IBM.

DH: Watson, yes. They don't have those labs anymore, so last time I had a conversation with Craig Barrett, I said, what research is Intel doing? He says, Intel doesn't do research. In essence he said, we're working on the next product and also on the one after that, because that's how long it takes. So, we are relying on you guys at the universities for research.

CL: So if you go back to lithography, the fundamentals would be the work of this faculty member, and the application you put it to work.

DH: Yes, developing the tool enables you to apply that fundamental understanding too. Andy Neureuther came from an electromagnetics background so he really came from fundamental electromagnetic theory. The way that old lithography stuff was done was plain old geometric optics like you learn in high school, where you don't pay any attention to the wavelength of the radiation. So Andy and Bill Oldham ran a wonderful program, love to have some more like that.

CL: If you had a young junior faculty member joining UC Berkeley now, what would you tell him? What advice would you give him about how to interact with industry?

DH: Make sure you understand what people are doing right now in your chosen field and what are the limiting factors; if you don't understand it already, go find somebody and try to get them to tutor you. You may find a faculty member who can do it. You really ought to be understanding about how your PC works, and how your display works, and how your cell phone works. Jeff Bokor is teaching a class for freshman about how things work. Get interested in how it works, because the only way you are going to make a contribution is to understand how it works today and how it's deficient, or how it could be better. Nobody wants a solution to a non-existing problem.

Also, if you haven't had experience in industry, take a leave and go out and get some. Don't just go to conferences attended by other academics. The whole field of integrated circuits was really helped along by the fact that the early players set up the Solid State Circuits Conference, which is our premier conference, and it was always a partnership of industry and academic people. Years ago the academic contributions used to be maybe only 20% and today they're more like 50%, but from all over the world, and that's a place of engagement. So if you have the opportunity to get into the leadership of some of these activities as you do through IEEE, or other professional societies such as ACM, make it a point to try to draw people in from different perspectives, industry, academic.

If you think you really have a hot idea, try to convene a meeting, try to invite those you most would like to question you about your idea to learn more, and try to get them to come here one at a time or all at once. When we were first doing the mixed signal stuff, we picked the day, the day before the Solid State Circuits Conference started. We invited our friends from around the country that we knew would be coming to the conference, industrial friends and said come over for a day, we'll give you lunch and we want to try some ideas on you and see what you say, and they did, they loved it. They loved coming and they asked a lot of good questions that led us to settle down on something, put more emphasis on some other things, and we did that for two or three years then. Then we didn't do it for a while and then we started hearing, "Why don't you do another meeting like you did?" They want it. It's getting harder to find the right guys in industry now, but they loved to come over and one of these guys explicitly said to me, "I wish I had the opportunity to work on this the way you are."

Your network starts with your fellow graduate students. If you went to a big place, you know a lot of people and you can call them up or send them an e-mail and you'll probably get an answer. Try to keep track of where these guys are. I don't think our young people have any trouble with such networking. Most of our young faculty have come out of other top five programs where this sort of thing is pretty common. Looks to me like Illinois is in the worst shape in that respect; very tough.

CL: Location is really important

DH: Carnegie Mellon feels that way even in Pittsburgh. They have tried to do something over here, at the former Ames NASA facility. They're not having much success. Cornell and Illinois were put where they were to get away from the evil influence of the city, which might have been the right thing 150 years ago, but today it's very tough to establish all the contacts you would like to have.

Well, Christophe, we've agreed to end it here. Thank you very much for all your good questions, and your time and attention to this project.

About the interviewer:

Christophe Lécuyer is a senior fellow at the Collegium de Lyon, the Institute of Advanced Studies affiliated with the Ecole Normale Supérieure de Lyon. Most of his research has been at the intersection of the history of science and technology, business history, and economic history. He is the author of *Making Silicon Valley: Innovation and the Growth of High Tech, 1930-1970* (MIT Press, 2006) and the co-author (with David Brock) of *Makers of the Microchip: A Documentary History of Fairchild Semiconductor* (MIT Press, 2010). Lécuyer taught the history of science and technology at Stanford University and the University of Virginia. He studied at the Ecole Normale Supérieure de Paris and obtained his doctorate at Stanford.

Links to reference materials (links verified: 3/14/2012)

Clarence Cory and A History of Early Electrical Engineering at UC Berkeley by John Torous
http://www.eecs.berkeley.edu/department/Clarence%20Cory%20History.pdf

History of the Microlab: http://microlab.berkeley.edu/history/MLHistory.html

Microlab history in pictures: http://microlab.berkeley.edu/history/Thefirstever.pdf

Record of EECS entrepreneurial activity: http://www.eecs.berkeley.edu/IPRO/entrepreneur.html

David A. Hodges UC Berkeley faculty page:
http://www.eecs.berkeley.edu/Faculty/Homepages/hodges.html

Dissertations supervised by David A. Hodges:
http://www.eecs.berkeley.edu/Pubs/Dissertations/Faculty/hodges.html

Editing completed: March 15, 2012
David A. Hodges

Publications, Conference Papers, and Patents of David A. Hodges

1. L. O. Hill, D. A. Hodges, D. O. Pederson, R. S. Pepper, “Synthesis of Electronic Bistable and Monostable Circuits,” Int’l Solid-State Circuits Conf., Philadelphia (Feb. 1963). *Digest of Technical Papers*, pp. 70-71. Outstanding Paper Award.

2. D. A. Hodges, “High-Performance Field-Effect Transistors Formed by Redistribution,” *Proc. IEEE,* 53, 1 (Jan. 1964) pp. 89-90.

3. D. A. Hodges, D. O. Pederson, R. S. Pepper, “A Simple Integrated Realization of a New Bistable Circuit,” Int’l Solid-State Circuits Conf., Philadelphia (Feb. 1964). *Digest of Technical Papers*, pp. 72-73.

4. D. A. Hodges, D. O. Pederson, R. S. Pepper, “Systematic Generation of Monostable and Counting Bistable Circuits,” *IEEE Trans. Circuit Theory,* CT-10, 4 (Dec. 1965), pp. 503-510. An early version of this paper was presented under the same title at the National Electronics Conference, Chicago (Oct. 1963). *Conference Proceedings*, pp. 261-281.

5. D. A. Hodges and H. Shichman, “Large-Signal Insulated-Gate Field-Effect Transistor Model for Computer Circuit Simulation,” Int’l Solid-State Circuits Conf., Philadelphia (Feb. 1968). *Digest of Technical Papers*, pp. 70-71, 166.

6. D. A. Hodges, “Main-Frame Semiconductor Memory,” IEEE Computer Group Conference, Los Angeles (June 1968). *Conference Digest,* pp. 33-35.

7. D. A. Hodges and D. O. Pederson, “Controlled-Conductance Modeling of Charge-Control Devices,” *IEEE Jour. of Solid-State Circuits*, SC-3, 2 (June 1968) pp. 199-200.

8. D. A. Hodges, “Large-Capacity Semiconductor Memory,” *Proc. IEEE*, 56, 7 (July 1968) pp. 1148-1162 (Invited paper).

9. J. H. Wuorinen and D. A. Hodges, “Component Technologies for Future Systems,” Int’l Federation for Information Processing Congress 68, Edinburgh (Aug. 1968). Full text appears in *Information Processing* 68, North-Holland Publishing Co., Amsterdam, 1969, pp. 787-795.

10. H. Shichman and D. A. Hodges, “Modeling and Simulation of Insulated-Gate Field-Effect Transistor Switching Circuits,” *IEEE Jour. of Solid-State Circuits,* SC-3, 3 (Sept. 1968) pp. 285-289.

11. D. A. Hodges, M. P. Lepselter, D. J. Lynes, R. W. MacDonald, A. U. MacRae, H. A. Waggener, “Low Power Bipolar Transistor Memory Cells,” Int’l Solid-State Circuits Conf., Philadelphia (Feb. 1969). Digest of Technical Papers, p. 194. Outstanding Paper Award. Full paper published in *IEEE Jour. of Solid-State Circuits,* SC-4, 5 (Oct. 1969) pp. 280-284.

12. D. J. Lynes and D. A. Hodges, “A Diode-Coupled Bipolar Transistor Memory Cell,” Int’l Solid-State Circuits Conf., Philadelphia (Feb. 1970). *Digest of Technical Papers,* pp. 44-45, 185.

13. D. A. Hodges, "Random-Access Semiconductor Memories," IEEE Computer Group Conf., Washington, D.C. (June 1970). *Conference Digest,* pp. 96-99.

14. D. A. Hodges, "Computer Memories," *IEEE Student Journal,* (Sept. 1970) pp. 15-20.

15. D. J. Lynes and D. A. Hodges, "A Memory Using Diode-Coupled Bipolar Transistor Cells," *IEEE Jour. of Solid-State Circuits,* SC-5, 5 (Oct. 1970) pp. 186-191.

16. D. A. Hodges, "Field Effect Transistor Memory Cell," U.S. Patent No. 3,540,007 (Nov. 10, 1970).

17. D. A. Hodges and M. P. Lepselter, "Schottky Barrier Diodes as Impedance Elements," U.S. Patent No. 3,585,412 (June 15, 1971)

18. D. A. Hodges, "Double-Clamped Schottky Diode-Transistor Logic Circuits," Proc. of Int'l Symposium on Low Power Digital Circuits, Wildbad, Germany (May 1972).

19. D. A. Hodges, *Semiconductor Memories,* A reprint volume with added new material, IEEE Press, N.Y. (1972), 286 pp.

20. D. A. Hodges, "Which Logic Technology for MSI and LSI Systems?" IEEE Computer Society Conf., (Sept. 1972). *Conference Digest,* pp. 135-137.

21. D. A. Hodges, "Alternative Component Technologies for Advanced Memory Systems," IEEE Computer Society Short Course, Santa Clara, CA (Dec. 1972). Published in *Computer Magazine,* (September 1973) pp. 35-37.

22. M. L. Leonhardt, D. A. Hodges, E. Marg, "Electronic Design of a Phosphene Visual Prosthesis," San Diego Biomedical Symposium (Feb. 1973). *Conference Proc., pp. 247-259.*

23. D. A. Hodges, "Components for Semiconductor Memories," IEEE Int'l Convention, New York (March 1973). INTERCON Technical Paper 2/1.

24. D. A. Hodges, "Double-Clamped Schottky Transistor Logic Gate Circuit," U.S. Patent No. 3,751,680 (August 7, 1973).

25. R. A. Heald and D. A. Hodges, "Design of Schottky-Barrier Diode-Clamped Transistor Layouts," IEEE *Jour. of Solid-State Circuits*, SC-8, 4 (Aug. 1973) pp. 269-275. A short version was represented at the Int'l Solid-State Circuits Conf., Philadelphia (Feb. 1973). *Digest of Technical Papers*, pp. 106-107.

26. R. E. Suarez, P. R. Gray, D. A. Hodges, "An All-MOS Charge-Redistribution A/D Conversion Technique," Int'l Solid-State Circuits Conf., Philadelphia (Feb. 1974). *Digest of Technical Papers*, pp. 194-195, 248.

27. D. A. Hodges and D. O. Pederson, "The Here and Now of Computer-Aided Circuit Design," Int'l Solid State Circuits Conf., Philadelphia (Feb. 1974). *Digest of Technical Papers,* pp. 38-39, 222.

28. D. A. Hodges, “The Future of Integrated Circuit Storage Technology,” IEEE Computer Society Conf., San Francisco (Feb. 1974). *Conference Digest*, pp. 291-292.

29. D. A. Hodges, “Integrated Circuits--New Capabilities for Information Handling,” Society for Information Display Int’l Symposium, San Diego (May 1974). *Symposium Digest*, p. 177.

30. D. A. Hodges, “A Review and Projection of Semiconductor Components for Digital Storage,” *Proc. IEEE*, 63, 8 (August 1975) pp. 1136-1147.

31. R. E. Suarez, P. R. Gray, D. A. Hodges, “All-MOS Charge-Redistribution Analog-to-Digital Conversion Techniques, Part II,” *IEEE Jour. of Solid-State Circuits,* SC-10, 6 (December 1975), pp. 379-385.

32. Y. P. Tsividis, P. R. Gray, D. A. Hodges, J. Chacko, Jr., “Companded Pulse-Code Modulation Voice Codec Using Monolithic Weighted Capacitor Arrays,” *IEEE Jour. of Solid-State Circuits,* SC-10, 6 (December 1975), pp. 497-499. (Correspondence)

33. Y. P. Tsividis, P. R. Gray, D. A. Hodges, J. Chacko, “An All-MOS Companded PCM Voice Encoder,” Int’l Solid-State Circuits Conf., Philadelphia (Feb. 1976). *Digest of Technical Papers,* pp. 24-25.

34. W. Y. Chu, D. A. Hodges, G. W. Dick, “High Voltage CMOS Decoder/Driver for Plasma Displays,” Int’l Solid-State Circuits Conf., Philadelphia (Feb. 1976). *Digest of Technical Papers,* pp. 212-213.

35. C. H. Moss, T. P. Cauge, D. A. Hodges, “Integrated Circuit DMOS Telephone Crosspoint Array,” Int’l Solid-State Circuits Conf., Philadelphia (Feb. 1976). *Digest of Technical Papers,* pp. 32-33, 226.

36. D. A. Hodges, “Trends in Computer Hardware Technology,” *Computer Design Magazine,* February 1976, pp. 77-85.

37. G. Smarandoiu, K. Fukahori, D. A. Hodges, P. R. Gray, “An All-MOS Analog-to-Digital Converter Using a Constant Slope Approach,” *IEEE Jour. of Solid-State Circuits,* SC-11, 3 (June 1976) pp. 408-410.

38. R. A. Heald and D. A. Hodges, “Multilevel Random Access Memory Using One Transistor per Cell,” *IEEE Jour. of Solid-State Circuits,* SC-11, 4 (Aug. 1976) pp. 519-528. Part of this work was presented at the Int’l Electron Devices Meeting, Washington (Dec. 1975).

39. Y. P. Tsividis, P. R. Gray, D. A. Hodges, “Non-CCD MOS Analog Circuits for Signal Processing,” Proc. *Midwest Symposium on Circuits and Systems,* Milwaukee, WI (Aug. 1976), pp. 427-429.

40. P. R. Gray, D. A. Hodges, Y. P. Tsividis, J. Chacko, Jr., “A Segmented μ-255 Law Voice Encoder Utilizing NMOS Technology,” *IEEE Jour. of Solid-State Circuits,* SC-11, 6 (Dec. 1976) pp. 740-747.

41. J. F. Albarran and D. A. Hodges, “A Charge-Transfer Multiplying Digital-to-Analog Converter,” IEEE *Jour. of Solid-State Circuits*, SC-11, 6 (Dec. 1976), pp. 772-779. An early version was presented at ISSCC, Philadelphia (Feb. 1976).

42. W. C. Black, Jr., R. H. McCharles, D. A. Hodges, “CMOS Process for High-Performance Analog LSI,” Int’l Electron Devices Meeting, Washington (Dec. 1976). *Tech. Digest*, pp. 331-334.

43. P. I. Suciu and D. A. Hodges, “Image Contour Extraction with Analog MOS Circuit Techniques,” IEEE *Jour. of Solid-State Circuits*, SC-12, 1 (Feb. 1977), pp. 65-72. An early version was presented at Soc. for Information Display Symposium, Washington (April 1975).

44. R. H. McCharles, V. A. Saletore, W. C. Black, Jr., D. A. Hodges, “An Algorithmic Analog-to-Digital Converter,” Int’l Solid-State Circuits Conf., Philadelphia (Feb. 1977). *Digest of Technical Papers, pp.* 96-97.

45. I. A. Young, D. A. Hodges, P. R. Gray, “Analog NMOS Sampled-Data Recursive Filter,” Int’l Solid-State Circuits Conf., Philadelphia (Feb. 1977). *Digest of Technical Papers*, pp. 156-157.

46. D. A. Hodges, “Microelectronic Memories,” *Scientific American*, 237, 3 (Sept. 1977), pp. 130-145.

47. D. A. Hodges, P. R. Gray, R. W. Brodersen, “Potential of MOS Technologies for Analog Integrated Circuits,” *IEEE Jour. of Solid-State Circuits,* SC-13, 3 (June 1978), pp. 285-294. This paper also was presented at the *European Solid-State Circuits Conf.*, Ulm, Germany (September 1977), receiving the award for the Outstanding Invited Paper.

48. R. H. McCharles and D. A. Hodges, “Charge Circuits for Analog LSI,” *IEEE Trans. on Circuits and Systems,* CAS-25, 7 (July 1978), pp. 490-497.

49. P. R. Gray and D. A. Hodges, “All-MOS Analog-Digital Conversion Techniques,” *IEEE Trans. on Circuits and Systems*, CAS-25, 7 (July 1978), pp. 482-489. A portion of this work was presented at the *IEEE Int’l Symposium on Circuits and Systems,* New York, May 1978.

50. G. Smarandoiu, D. A. Hodges, P. R. Gray, G. F. Landsburg, “CMOS Pulse-Code Modulation Codec,” *IEEE Jour. of Solid-State Circuits,* SC-13, 4 (August 1978), pp. 504-509.

51. R. W. Brodersen, P. R. Gray, D. A. Hodges, D. J. Allstot, G. M. Jacobs, “Switched-Capacitor Filters for Telecommunications Applications,” *1978 European Conf. on Circuit Theory and Design*, Lausanne, September 1978.

52. Daniel Senderowicz, D. A. Hodges, P. R. Gray, “High-Performance NMOS Operational Amplifier,” IEEE *Jour. of Solid-State Circuits*, SC-13, 6 (December 1978), pp. 760-766.

53. P. R. Gray, D. A. Hodges, J. L. McCreary, “Weighted Capacitor Analog/Digital Converting Apparatus and Method,” U.S. Patents No. 4,129,863 (Dec. 1978) and No. 4,200,863 (April 1980).

54. R. W. Brodersen, P. R. Gray, D. A. Hodges, “MOS Switched-Capacitor Filters,” *Proc. IEEE* 67, 1 (January 1979), pp. 60-74.

55. D. A. Hodges, P. R. Gray, R. W. Brodersen, "Enhancing MOS/LSI's Role in Analog Design," *IEEE Spectrum*, (February 1979), pp. 24-32.

56. Bahram Fotouhi and D. A. Hodges, "An NMOS 12b Monotonic 25μs A/D Converter," *ISSCC Digest of Tech. Papers,* (February 1979), pp. 186-187.

57. G. M. Jacobs, G. F. Landsburg, B. J. White, D. A. Hodges, "Touch-Tone Decoder Chip Mates Analog Filters with Digital Logic," *Electronics*, 52 (February 15, 1979), pp. 105-112.

58. D. A. Hodges and V. I. Johannes, "Introduction to Special Issue on Solid-State Circuits for Telecommunications," *IEEE Jour. Solid-State Circuits*, SC-14, (February 1979), pp. 3-6.

59. P. R. Gray, R. W. Brodersen, D. A. Hodges, "MOS Switched-Capacitor Filters - An Overview," *Proc. IEEE Int'l Symposium on Circuits and Systems*, Tokyo, (May 1979), pp. 905-908.

60. I. A. Young and D. A. Hodges, "MOS Switched-Capacitor Analog Sampled-Data Direct-Form Recursive Filters," *IEEE Jour. of Solid-State Circuits,* SC- 14, (Dec. 1979), pp. 1020-1033.

61. B. Fotouhi and D. A. Hodges, "High-Resolution A/D Conversion in MOS/LSI," *IEEE Jour. Solid-State Circuits,* SC-6, (December 1979), pp. 920-926.

62. W. C. Black, Jr., and D. A. Hodges, "Time Interleaved Converter Arrays," *ISSCC Digest of Tech. Papers,* (February 1980), pp. 14, 15, 254.

63. Bert Dotter, Alex de la Plaza, D. A. Hodges, D. G. Messerschmitt, "An Electronic Hybrid with Adaptive Balancing," *Int'l Conf. on Communications Conf. Record,* (June 1980), pp. 44.5.1-44.5.5

64. Berton E. Dotter, A. de la Plaza, D. A. Hodges, D. G. Messerschmitt, "Implementation of an Adaptive Balancing Hybrid," *IEEE Trans. on Communications*, COM-28, (August 1980) pp. 1408-1416. Prize Paper Award from IEEE Communications Society.

65. D. A. Hodges, R. W. Brodersen, P. R. Gray, "Limitations on Analog Circuits in MOSLSI," *IEEE Wescon,* (Sept. 1980), paper 26/3, 3 pp.

66. W. C. Black, Jr., and D. A. Hodges, "Time Interleaved Converter Arrays," *IEEE Jour. of Solid-State Circuits,* SC- 15, (Dec. 1980) pp. 1022-1029. This is an expanded version of item 62, above.

67. P. R. Gray, D. A. Hodges, R. W. Brodersen, *Analog MOS Integrated Circuits*, a reprint volume with added new material. IEEE Press, New York, (1980), 405 pp.

68. P. R. Gray, R. W. Brodersen, D. A. Hodges, T. C. Choi, R. Kaneshiro, K. C. Hsieh, "Some Practical Aspects of Switched Capacitor Filter Design," *Proc. IEEE Int'l Symposium on Circuits and Systems,* (April 1981), pp. 419-422.

69. D. A. Hodges, "Supply Voltage and Logic Levels for VLSI," 1981 Symposium on VLSI Technology, Hawaii, *Digest of Technical Papers*, (Sept. 1981), pp. 42-43.

70. Oscar Agazzi, D. A. Hodges, D. G. Messerschmitt, W. Lattin, “Echo Canceller for an 80 Kb Baseband Modem,” Int’l Solid-State Circuits Conf., *Digest of Technical Papers,* (Feb. 1982), pp. 144-145, 310.

71. Daniel Senderowicz, D. A. Hodges, P. R. Gray,, “An Integrated Vector-Locked Loop,” *Proc. IEEE Int’l Symposium on Circuits and Systems,* Rome (May 1982), pp. 1164-1167.

72. Oscar Agazzi, D. G. Messerschmitt, D. A. Hodges, “Nonlinear Echo Cancellation of Data Signals,” *Int’l Conf. on Communications Conf. Record,* (June 1982), pp. 7G6.1-7G6.5.

73. Oscar Agazzi, D. A. Hodges, D. G. Messerschmitt, M. P. Tippner, “An Integrated Realization of a Hybrid-mode Digital Subscriber Loop,” *Proc. of Int’l Symposium on Subscriber Loop Systems,* (Sept. 1982) pp. 201-205. Best Paper Award.

74. Oscar Agazzi, D. A. Hodges, D. G. Messerschmitt, “Large-Scale Integration of Hybrid-Method Digital Subscriber Loops,” *IEEE Trans. on Communications,* COM-30 (Sept. 1982), pp. 2095-2108. Expanded version of items 70 and 73, above.

75. Oscar Agazzi, D. G. Messerschmitt, D. A. Hodges, “Nonlinear Echo Cancellation of Data Signals,” *IEEE Trans. on Communications* COM-30 (Nov. 1982), pp. 2421-2433. Expanded version of item 72, above.

76. D. A. Hodges and H. G. Jackson, *Analysis and Design of Digital Integrated Circuits,* McGraw-Hill, N. Y. (1^{st} Ed. 1983, 2^{nd} Ed. 1988), 463 pages.

77. Vivian W-K. Shen and D. A. Hodges, “A 60 ns Glitch-Free NMOS DAC,” Int’l Solid-State Circuits Conf., *Digest of Technical Papers,* (Feb. 1983), pp. 188-189.

78. H-S. Lee and D. A. Hodges, “Self-Calibration Technique for A/D Converters,” *IEEE Trans. on Circuits and Systems,* CAS-30, No. 3 (March 1983), pp. 188-190.

79. H-S. Lee, D. A. Hodges, P. R. Gray, “Self-Calibrating 12 bit, 12 μs CMOS A/D Converter,” Int’l Solid-State Circuits Conf., *Digest of Technical Papers,* (Feb. 1984), pp. 64-65, 319.

80. P. R. Gray and D. A. Hodges, “Performance Limits in VLSI Data Converters,” *Proc. IEEE Int’l Symposium on Circuits and Systems,* Montreal (May 1984), pp. 425-427. Invited paper.

81. P. R. Gray and D. A. Hodges, “Analog-Digital Conversion Techniques for Telecommunications Applications,” *Design of MOS-VLSI Circuits for Telecommunications,* Prentice-Hall, New Jersey, (1985), pp. 212-235. Originally presented at EEC Advanced Course, L’Aquila, Italy (June 1984).

82. H-S. Lee and D. A. Hodges, “A Precision Measurement Technique for Residual Polarization in Integrated Circuit Capacitors,” *IEEE Electron Devices Letters,* EDL-5, No. 10 (Oct. 1984), pp. 417-420.

83. Oscar Agazzi, C-P. Jeremy Tzeng, D. A. Hodges, D. G. Messerschmitt). “Timing Recovery in Digital Subscriber Loops,” *IEEE Global Communications Conf.,* Atlanta (Nov. 1984). *Conference Record, pp.*
1.7.1 to 1.7.5. A full-length version appears under the same title in *IEEE Trans. on Communications,* COM-33, No. 6 (June 1985), pp. 558-569.

84. H-S. Lee, D. A. Hodges, P. R. Gray, "A Self-Calibrating 15 bit CMOS A/D Converter," *IEEE Jour. of Solid-State Circuits,* SC-19, No. 6 (Dec. 1984), pp. 813-819.

85. Joey Doernberg, H-S. Lee, D. A. Hodges, "Full-Speed Testing of A/D Converters," *IEEE Jour. of Solid-State Circuits*, SC-19, No. 6 (Dec. 1984), pp. 820-827.

86. D. A. Hodges, "Information Systems for Design and Manufacturing," *EECS/ERL 1985 Research Summary,* (Jan. 1985), pp. xxvii-xxxii (6 pp.). An oral version was presented as the Opening Address at *Thirteenth North American Manufacturing Research Conf.*, Berkeley, (May 1985).

87. D. A. Hodges, "Million-Bit Memory Chips," *IEEE Electrotechnology Review 1984,* May 1985, pp. 31-32.

88. H-S. Lee and D. A. Hodges, "Accuracy Considerations in Self-Calibrating A/D Converters," *IEEE Trans. on Circuits and Systems,* CAS-32, No. 6 (June 1985), pp. 590-597.

89. C-P. Jeremy Tzeng, D. A. Hodges, D. G. Messerschmitt, "Timing Recovery in Digital Subscriber Loops Using Baudrate Sampling," IEEE Int'l Conf. on Communications, Chicago (June 1985). *Conference Record,* pp. 1191-1196.

90. Nan-Sheng Lin, D. A. Hodges, D. G. Messerschmitt, "Partial Response Coding in Digital Subscriber Loops," IEEE Global Communications Conf., New Orleans (Dec. 1985). *Conference Record,* pp. 1322-1328.

91. M-K. Liu, D. G. Messerschmitt, D. A. Hodges, "An Approach to Fiber Optics Data/Voice/Video LAN," presented at *IEEE INFOCOM 86,* Miami FL (April 1986). Conf. Proceedings, pp. 516-523.

92. D. A. Hodges, L. A. Rowe, C. Williams, T. Muller, "Information Systems for Manufacturing," IEEE *Workshop on Information System Support for Integrated Design and Manufacturing Processes,* Naval Postgraduate School, Monterey, April 1986.

93. J. M. Pendleton, S. I. Kong, E. W. Brown, F. Dunlap, C. Marino, D. M. Ungar, D. A. Patterson, D. A. Hodges, "A 32b Microprocessor for Smalltalk," *IEEE Jour. of Solid-State Circuits,* SC-21, No. 5 (Oct. 1986), pp. 741-749. A short version was presented at the *Int'l Solid-State Circuits Conf.* (Feb. 1986).

94. C-P. Jeremy Tzeng, D. A. Hodges, and D. G. Messerschmitt, "Timing Recovery in Digital Subscriber Loops Using Baudrate Sampling," *IEEE Jour. on Selected Areas in Communications,* SAC-11, No. 8 (November 1986), pp. 1302-1311. Full-length version of item 89, above.

95. Mark Hill, ... D. A. Hodges (18th of 21 co-authors), "Design Decisions in SPUR," *IEEE Computer,* November 1986, pp. 8-22.

96. M. F. Klein and D. A. Hodges, "A Modular Framework for an Interactive Design Synthesis System," *IEEE Int'l Conf. on Computer-Aided Design,* Santa Clara, CA., November 1986. Conference Digest, pp. 510-512.

97. D. A. Hodges and L. A. Rowe, "Information Management for CIM," *Advanced Research in VLSI: Proceedings of the 1987 Stanford Conference*, The MIT Press, Cambridge, March 1987, pp. 201-208.

98. D-K. Jeong, G. Borriello, D. A. Hodges, R. H. Katz, "Design of PLL-Based Clock Generation Circuits," *IEEE Jour. of Solid-State Circuits*, SC-22, No. 2 (April 1987), pp. 255-261.

99. D. A. Hodges (principal author), G. S. Ansell, G. F. Bolling, J. G. Bollinger, J. A. Decaire, J. F. Lardner, M. E. Merchant, R. N. Nagel, M. A. Steinberg, "Manufacturing Systems Research in the United States: An Overview," in *Directions in Engineering Research: An Assessment of Opportunities and Needs*, Report of the Engineering Research Board, National Academy Press, Washington, D. C., 1987, pp. 216-238.

100. C. Y. Fu, N. H. Chang, K-K. Lin, D. A. Hodges, "Expert System for IC Computer-Aided Manufacturing," SEMICON/EAST, Boston, September 1987. *Technical Proceedings,* pp. 21-30.

101. K-K. Lin, C. Y. Fu, N. H. Chang, D. A. Hodges, "Recipe Generator for LPCVD Polysilicon," *Symposium on Automated Semiconductor Manufacturing,* Electrochemical Society, Honolulu, October 1987.

102. Wenbin Hsu, D. A. Hodges, D. G. Messerschmitt, "Acoustic Echo Cancellation for Loudspeaker Telephones," *IEEE Global Telecommunications Conference,* Tokyo, November 1987. *Conference Record,* pp. 1955-1959.

103. Norman H. Chang, K-K. Lin, C. Y. Fu, D. A. Hodges, "BIPS Application for LPCVD Polysilicon," *Texas Instruments Technical Journal,* Winter 1987, pp. 30-38.

104. Hyun J. Shin and D. A. Hodges, "A 250 Mb/s CMOS Crosspoint Switch," *IEEE Int'l Solid-State Circuits Conf.,* San Francisco, February 1988. *Digest of Technical Papers,* pp. 114-115, 320-321.

105. D. A. Hodges and H. G. Jackson, *Analysis and Design of Digital Integrated Circuits,* Second Edition, McGraw-Hill, N. Y. (Apr. 1988), 462 pages.

106. Joey Doernberg and D. A. Hodges, "A 10-bit 5 Msample/sec CMOS 2-Step Flash ADC," *Proc. of IEEE 1988 Custom Integrated Circuits Conference,* pp. 18.6.1-18.6.4, Rochester N. Y., (May 1988).

107. Joey Doernberg, P. R. Gray, D. A. Hodges, "Flash A/D Converter using Capacitor Arrays," U.S. Patent No. 4,742,330 (May 1988).

108. D. A. Hodges, "Opportunities in Computer Integrated Manufacturing," *IEEE Design Automation Conf.,* Anaheim, CA, June 1988. (Invited presentation.) *Conference Proceedings 1988,* pp. 82-83.

109. D. A. Hodges and L. A. Rowe, "Computer Integrated Manufacturing of VLSI," *SRC TECHCON '88,* Dallas, TX, October 1988. *Extended Abstracts, pp.* II I-1 12.

110. D. A. Hodges and W. C. Holton, “CIM at Japan’s Best VLSI Manufacturers,” *SRC TECHCON ‘88*, Dallas, TX, October 1988. *Extended Abstracts*, p. 130.

111. D. A. Hodges, J. D. Plummer, B. Wu, “Computer Integrated Manufacturing,” pp. 13-79 in *Computer Integrated Manufacturing (CIM) and Computer Aided Design (CAD) for the Semiconductor Industry in Japan,* Japanese Technology Evaluation Program, Science Applications International Corp., McLean, VA, December 1988.

112. D. A. Hodges, L. A. Rowe, C. J. Spanos, “Quality and Productivity in Semiconductor Manufacturing,” Highlight article, *EECS/ERL 1989 Research Summary,* (Jan. 1989), pp. xlii-xlix (7 pp.).

113. Wenbin Hsu, Frank Chui, D. A. Hodges, “An Acoustic Echo Canceller,” *IEEE Int’l Solid-State Circuits Conf.*, New York, February 1989. *Digest of Technical Papers,* pp. 264-265, 362.

114. Joey Doernberg, P. R. Gray and D. A. Hodges, “A 10-bit 5-Msample/sec CMOS 2-Step Flash ADC” *IEEE Jour. of Solid-State Circuits,* SC-24, No.2, pp. 241-249, (April 1989). Expanded version of item 106, above.

115. Hyun J. Shin and D. A. Hodges, “A 250 Mbit/s CMOS Crosspoint Switch,” *IEEE Jour. of Solid-State Circuits,* SC-24, pp. 478-486, (April 1989). This is an expanded version of item 104, above.

116. Gregory A. Uvieghara, Y. Nakagome, D. K. Jeong, D. A. Hodges, “An On-Chip Smart Memory for a Data Flow CPU,” *1989 Symposium on VLSI Circuits,* Kyoto, Japan. *Digest of Technical Papers,* pp. 121122, (May 1989).

117. Ming-Kang Liu, D. G. Messerschmitt, D. A. Hodges, “Time Slot Switching for Integrated Services in Fiber Optic PBX/LAN,” *IEEE Trans. on Communications,* COM-37, 7 (July 1989), pp. 685-693.

118. Shing-Ip Kong, D. D. Lee, R. H. Katz, D. A. Hodges, D. A. Patterson, “From Chip to System: The SPUR CPU Experience,” *VLSI 89,* Munich, Germany, August 1989.

119. D. A. Hodges, L. A. Rowe, C. J. Spanos, “Computer Integrated Manufacturing,” International Electronic Manufacturing Technology Symposium, *Conf. Proceedings,* pp. 1-3, (Sept. 1989). (Invited keynote presentation.)

120. David D. Lee, Shing-Ip Kong, M. D. Hill, G. S. Taylor, D. A. Hodges, R. H. Katz, D. A. Patterson, “A VLSI Chip Set for a Multiprocessor Workstation -- Part 1: A RISC Microprocessor with Coprocessor Interface and Support for Symbolic Processing,” *IEEE Jour. of Solid-State Circuits,* SC-24, pp. 1688-1698, (Dec.1989).

121. Deog-Kyoon Jeong, D. A. Wood, G. A. Gibson, S. J. Eggers, D. A. Hodges, R. H. Katz, D. A. Patterson, “A VLSI Chip Set for a Multiprocessor Workstation -- Part II: A Memory Management Unit and Cache Controller,” *IEEE Jour. of Solid-State Circuits,* SC-24, pp. 1699-1707, (December 1989).

122. Wenbin Hsu, Frank Chui, D. A. Hodges, “An Acoustic Echo Canceller,” *IEEE Jour. of Solid-State Circuits,* SC-24, pp. 1639-1646, (December 1989).

123. Gregory A. Uvieghara, Y. Nakagome, D. K. Jeong, D. A. Hodges, "An On-Chip Smart Memory for a Data Flow CPU," *IEEE Jour. of Solid-State Circuits,* SC-25, pp. 84-94, (February 1990).

124. Fariborz Nadi, A. M. Agogino, D. A. Hodges, "Use of Influence Diagrams and Neural Networks in Modeling LPCVD," Int'l Semiconductor Manufacturing Science Symposium, Burlingame, CA, May *1990. ISMSS '90 Proceedings, pp.* 11 I- 1 12.

125. David A. Hodges, "Manufacturing Quality Control for ULSI," *VLSI Technology Symposium,* Honolulu, June 1990. *Proc. of Workshop on New Process Technologies for ULSI,* pp. 207-219. (Invited presentation)

126. D. A. Hodges, "VLSI Manufacturing in Japan and the United States," Advanced Semiconductor Manufacturing Conference and Workshop, Danvers, MA, Sept. 1990. (Invited Keynote) *ASMC Proceedings*, pp. 1-2.

127. Fariborz Nadi, A. M. Agogino, D. A. Hodges, "Use of Influence Diagrams and Neural Networks in Modeling Semiconductor Manufacturing Processes," *IEEE Trans. on Semiconductor Manufacturing*, TSM-4, No. 1, pp. 52-58 (February 1991).

128. D. A. Hodges, "University/Government/Industry Collaboration as a Contributor to Competitiveness," Ninth Biennial University/Government/Industry Microelectronics Symposium, Indialantic, FL, June 1991. (Invited Keynote speaker)

129. Gregory A. Uvieghara, W. Hwu, Y. Nakagome, D. K. Jeong, D. Lee, D. A. Hodges, Y. Patt, "An Experimental Single-Chip Data Flow CPU," *IEEE Jour. of Solid-State Circuits,* SC-27, 1, pp. 17-28 (January 1992). A short version was presented at *VLSI Circuits Symp.,* Honolulu, June 1990. Digest of Tech. Papers, pp. 119-120.

130. D. A. Hodges, R. C. Leachman, "Competitive Semiconductor Manufacturing," Spring Meeting, Electrochemical Society, San Francisco, (May 23, 1994). Invited Keynote Address.

131. R. C. Leachman, D. A. Hodges, "Benchmarking Semiconductor Manufacturing," *IEEE Transactions on Semiconductor Manufacturing,* TSM-9, pp. 158-169 (May 1996). http://andros.eecs.berkeley.edu/~hodges/BenchmarkingSM.pdf

132. R. C. Leachman, D. A. Hodges, "Benchmarking Semiconductor Manufacturing," European Solid-State Device Research Conference, Stuttgart, (Sept. 1997). Invited Plenary paper. Conf. Proceedings, pp. 1-9. (entirely new, updated version of item 131.)

133. R. C. Leachman, D. A. Hodges, "Benchmarking Semiconductor Manufacturing," IEEE Int'l Integrated Reliability Workshop, Lake Tahoe CA. (Oct. 1997). Invited Keynote address. Conf. Proceedings, pp. 1-6. (similar to item 132.) http://andros.eecs.berkeley.edu/~hodges/IRWpaper.pdf

134. J. T. Macher, D. C. Mowery, D. A. Hodges, "Reversal of Fortune? The Recovery of the U.S. Semiconductor Industry," *California Management Review*, 41,1 (Fall 1998). pp. 107-136. Full paper available at: J. T. Macher, D. C. Mowery, D. A. Hodges, "Semiconductors," Chap. 10 in *U.S. Industry in 2000: Studies in Competitive Performance,*

D. C. Mowery, Ed., National Academy Press, (1999), pp. 245-286. Full text available at: http://books.nap.edu/books/0309061792/html/245.html#pagetop

135. D. A. Hodges, “Darlington’s Contributions to Transistor Circuit Design,” *IEEE. Trans. on Circuit Theory-I,* 46,1, (Jan. 1999). pp. 102-104.

136. M. Pecht, W. Liu, D. A. Hodges, “China’s Semiconductor Industry,” Chapter 2 in *Electronics Manufacturing Update,”* International Technology Research Institute, Baltimore MD. (Sept. 2000) pp. 25-45. Full text available at: http://itri2.org/ttec/aemu/report/c2.pdf

137. M. Pecht, W. Liu, D. A. Hodges, “Trends in China’s Semiconductor Industry,” *Semiconductor International*, (Sept. 2000), pp. 134-144. Article based upon above report with added illustrations. Full article available at: http://andros.eecs.berkeley.edu/~hodges/SemiconductorIntl.htm

138. D. A. Hodges, “University-Industry Cooperation, and the Emergence of Start-Up Companies,” invited keynote presentation at a Public Symposium of the Research Institute of Economy, Trade, and Industry, Tokyo, (12/11/2001). http://www.rieti.go.jp/en/events/01121101/doc.html Also available at: http://andros.eecs.berkeley.edu/~hodges/UIC&ESUC.pdf

139. D. A. Hodges, H. G. Jackson, R. A. Saleh, *Analysis and Design of Digital Integrated Circuits,* 3rd Edition, McGraw-Hill, N. Y. (July 2003) 580 pp.