Oral History of Paul Gray

Interviewed by:
Shayne Hodge

Recorded: July 15, 2013
Mountain View, California

CHM Reference number: X6886.2013

© 2013 Computer History Museum

Dr. Paul Gray, July 15, 2013

Shayne Hodge: This is Shayne Hodge at the Computer History Museum. Today is Monday, July 15 in the late morning and we're here with Dr. Paul R. Gray from the University of California Berkeley. Well, welcome, Dr. Gray.

Paul Gray: Thank you, Shayne.

Hodge: If we could start, if you'd like to do sort of a biographical sketch of your early life before college.

Gray: Certainly. Certainly. I was born in a small town in Northeast Arkansas, in 1942. My mother did many things, but mostly was a schoolteacher. My dad was an army officer, West Point graduate. By the time I was born, he was off at World War II. They subsequently divorced and I was raised by my mom as a single mom. We lived in Arkansas. I went to junior high school in Arkansas and then high school in Arizona. Schoolteachers were getting paid a lot more in Arizona than they were in Arkansas so we picked up and moved to Arizona in 1957. And I spent my high school years in Tucson, Arizona along with my brother, who is a year and a half younger than I. I then stayed in Tucson, and went to the University of Arizona as a undergraduate in engineering and then continued there in graduate school, and ultimately got a PhD there in 1969.

Hodge: What made you go into engineering?

Gray: I was an airplane nut as a high school kid. I loved airplanes, built model airplanes, and flew them. I was involved in an organization called the Civil Air Patrol, which was kind of a Boy Scout like organization. The parent organization was an airborne search and rescue volunteer group of pilots. They had a cadet auxiliary and that got me even more interested in airplanes. So I wanted to have something to do with airplanes and I couldn't fly them because my vision wasn't good enough. So I thought, "Well, the next best thing is to help design them." So that kind of got me into the engineering track. And I was, you know, not wonderful but I was pretty good at math. And I liked sort of engineering, technical kinds of things so I just gravitated toward it.

<crew talk>

Hodge: So out of curiosity, when you were a kid were you building things, taking things apart? Were you that sort?

Gray: Yes, I was that sort. I built model airplanes, As I said. I was a model railroad nut— built model railroads. Interestingly enough, I wasn't in contact with anybody who did electronics or anything like that. It was all kind of mechanical and aero— you know, airplanes, and trains, and those kinds of things. And bicycles, I did a lot of bicycle tearing up and fixing. And then later on my brother and I were car nuts. But it was almost all mechanical. And that's an interesting story we'll get to later how I happened to end up in electrical engineering because I had very little to do with electronics as a young person. But I was maybe a match to that traditional profile of people who get into engineering. Somebody who liked to tinker around with stuff, and take things apart, and put them back together again.

Hodge: Right. That is the traditional profile as you mentioned. It would sound like when you started Arizona, I believe, it was '63 that it sounded like you would be candidate for the mechanical engineering department.

Gray: I actually started in mechanical engineering. That was my path. As I said, I wanted to design airplanes. And there's a lesson in this. I got to be a sophomore and up to the time I was a sophomore I not only was not interested in electronics and electrical engineering or electricity but like many people in the mechanical engineering end of things. I didn't like it. I thought it was hard and I didn't understand it. And then I happened to take a class from a faculty member who was just one of those people that has a gift for making you understand things. He happened by chance to be a person who was teaching the basic electric circuit course for the first time. He hadn't done it before. He was from some other sub-field and it was as if we were learning along with him. You know, sometimes the best teacher is someone who just learning it themselves. And he just made me an electrical engineer. After a semester of that I realized, "Hey, not only do I understand this. But it's really interesting." And I changed my major the very next semester. I took all electrical engineering after that. You hear of that a lot - one professor that happens to have an ability to connect with you. That person did and it really made a big difference for me.

Hodge: Do you remember his name by chance?

Gray: John Wait, W-A-I-T. He had a serious illness and passed away only about 10 years after I knew him. So that was unfortunate but he was a really influential person on a lot of students. Great professor.

Hodge: So as an undergraduate did you have a concentration? And in terms, second part of the question, in terms of electronics what was the— how much of it was tube that point, how much was transistors?

Gray: It was almost all vacuum tubes. Now, we're talking the early '60s, so '61, '62, and '63. Almost all vacuum tubes. The transistor part was kind of stuck on at the end; there was one week on transistors at the end of the electronics courses. And pretty minimal. But I also took a course as a senior from another person who made a big difference for me and that was Doug Hamilton. Doug was a senior faculty member at Arizona, an electronics circuits person. That was his interest. And so as a senior I took his class and that at the time he taught was mostly vacuum tubes, but then because of his influence and the enthusiasm he brought to that I stayed on for grad school. And then he was in the process himself of migrating that whole curriculum from vacuum tubes to transistor electronics. And also at that same time he came out to Berkeley and spent a leave of absence there. Berkeley along with Stanford and then later MIT had the first three microelectronics fabrication facilities, experimental facilities, in US Universities at the time. Those all started in the early to mid '60s. Arizona was a pretty out of the way place- when you think of major research universities in electronics and electrical engineering you think of Berkeley, and Stanford, and MIT and you wouldn't necessarily think of Arizona. But largely due to Doug they got this technology going through his efforts. I got a chance then later on as a grad student to go in and be part of that. I got to push wafers in and out of ovens, design an integrated circuit and make it work, and cut the rubylith, and that was a tremendous experience. I mean, it was just serendipitous. I was at Arizona where you wouldn't just find that but thanks to Doug and all of his efforts it was— I got that experience. So I worked with him in graduate school along with another faculty member named Fred Lindholm, who was a device person. He worked on mainly field effect transistor physics. So he and Doug together were my mentors in graduate school.

Hodge: Correct. I said you graduated in '63?

Gray: Yeah.

Hodge: You then start in '63?

Gray: Right.

Hodge: So was that Microelectronics program the reason why you stayed at Arizona for grad school instead of going somewhere else?

Gray: Well, the reason why I stayed, yes. That's part of it. I liked Doug, but the bigger reason was, first of all that I was a Tucson resident— I lived in town. We were, you know, struggling to make ends meet. I worked park time all the way through high school and college at various jobs and so forth. And going away to college at a big name place wasn't in the cards financially— I stayed there mainly because I was living at home at the time and that was the economically feasible thing to do. But the other reason I stayed on is I graduated in three years. I went to summer school and, you know, tried to get through quickly. And after three years, I just felt like I didn't know enough. You know, I felt that I couldn't imagine why anybody would hire me to do anything with the amount of information I knew and I just thought I really needed to stay and just learn more. So that together with Doug's encouragement I did stay and then I got master and then a PhD later on. Finally, finished up in '69.

Hodge: What was your research on during that time?

Gray: Well, I started working with Fred Lindholm, who's a device physicist, essentially. So my master' degree was on junction field effect transistors, which was a relatively new device at the time. Junction field effect transistors were used a lot as discrete components. It turns out they were later on used in some bipolar ICs. There was a certain classification of bipolar integrated circuits where you can build a parasitic junction FET and later on those were used quite a bit. But at that time, it was mainly a discrete component interest and Fred Lindholm was doing work on analysis of junction and field effect transistors. I did a project to analyze a field effect transistor that had two independent gates. Normally, you hook the two together. It's a two-sided device and has two gates, one on each side, channel in the middle. I analyzed the case where the two gates were electrically independent and could be controlled separately and that was my Master's Degree thesis. That device experience really paid me big dividends later on. You know, when you work on circuits if you can really understand how the transistors work it's a huge advantage, and we'll talk about that, I'm sure. A lot of the early pioneers in bipolar analog circuits were superb device people. They really understood how the devices worked. Anyway, I did that as my Master's project. Doug was more in the circuits domain and he was building up this integrated circuits fabrication facility. I wanted to do something more on the circuits level than the device level. So I worked on something that was really Doug's suggestion. Circuit simulation was just coming along. SPICE hadn't even been invented yet, but there were predecessors to that. And it was becoming clear that there were certain kinds of integrated circuits like power integrated circuits where the thermal effects were going to be important. For example let's say it's an audio amplifier and you drive a speaker. The output transistor that's sitting on the chip dissipates a lot of heat. That end of the chip heats up. There's a thermal gradient created. The whole chip increases in temperature. And that affects the circuit performance. So these electro thermal interactions are going to be important, so my work was on analyzing that, coming up with models for that, an analysis of that. Part of the project was to try to use the thermal effects for signal processing. For example, using it as a timer to achieve a long-time constant. You turn on a power device, the chip gradually heats up. You sense that. You can generate long-time constant timers that way. And we did some of that, but the most important aspect of it really was the more undesirable interactions. And you wouldn't remember this but the problem of building an accurate voltage reference is one that's always been part of Analog IC design methodology. And one of the problems with an absolute voltage reference is that it varies the temperature. Well, one of the applications of these electro thermal circuits is that you can build a temperature stabilizer where you sense the temperature and dissipate power on the chip to keep the chip temperature constant even when the ambient temperature is going up and down. In theory that's supposed to let you have the more stable voltage reference. There were a series of products from National like that in the '70s that actually used that technology. Subsequently, there have been lots of better ways found to make stable voltage references, but it was important at the time. So we used this technique to analyze a temperature stabilized substrate, and the gradients, and all the different second order effects that you get there.

Hodge: So you said you started with computer modeling at this point. What machines were you using and what language?

Gray: It was an IBM mainframe. I don't remember the model number. It was sitting in the basement of a computer science building on the Berkeley campus. And it was this classic thing you've heard a thousand times. I'd take the deck of cards over there at eight o'clock at night and drop them off, and I'd go back the next morning and hope something good happened. And most the time it didn't. It was an unbelievably inefficient way to do computing but it worked. I mean, we did a lot of simulation of these thermal effects using that.

Hodge: Berkeley campus or Arizona?

Gray: Berkeley— oh, I'm sorry. I'm sorry. I'm jumping ahead. This was at Arizona. Same thing though in the early days at Berkeley. Same exact process. Get punch cards, take them over there, dump in there.

Hodge: So did you have to write everything from scratch or were there—

Gray: Yeah. It was all FORTRAN. . So I had a big FORTRAN program to write for doing this simulation of these. Our approach was we did a lumped model, a node mesh model, of the thermal behavior of the chip. And then we put the difference equations in an equation solver and wrote a FORTRAN program to do that. And then have that run in a big mainframe.

Hodge: Okay. That sounds not entirely dissimilar to the approach taken by SPICE.

Gray: Oh, it's exactly the same. Well, it's different in the sense that we were trying to simulate something that was happening continuously across a chip. These are continuous variables across space. We approximated them by doing a set of independent nodes to approximate the continuous behavior. In SPICE, you do the same exact kind of analysis but you're representing circuit voltages at specific nodes., Fundamentally SPICE is simulating circuits that have discrete nodes. But the actual simulation routine is very similar.

Hodge: So you finished up your PhD in '69. What were you hoping to work on at that point? What were the products that were sort of out there that were capturing your imagination and you thought could be improved?

Gray: Through the time I was in graduate school I really enjoyed reading the papers that people like Bob Widlar were writing in the '60s. The paper describing the 702, and the 709, and the LM109 voltage regulator - those were early pioneers in bipolar analog. You have a picture of Bob Widlar out here on the wall. I really admired those people. You know, when you think about the years before the integrated circuit came along analog engineers did discrete component design. There were discrete component op amps you could buy from Analog Devices and lots of other companies. In order to design and build one of those operational amplifiers, you could pick precision components like resistors and capacitors as accurate as you wanted them, .01 percent for example. You could get components that were as precise as you needed them. You could get different kinds of transistors and then optimize the circuit with all these different degrees of freedom. Then when Widlar and his colleagues came along they had only the devices inherent in the bipolar process, which of course initially had been used for TTL and the predecessors to that to build digital integrated circuits. Initially in the 60s they were trying to use that technology to build op amps.. That was sort of the first test case, "Can we build an op amp?" And they had a really, really good NPN transistor that came along with that technology. They had resistors that were plus or minus 20 percent at best. They didn't really have a PNP transistor at all. It was a real challenge to try to even come close to what the discrete component realizations could realize. And the same was true of other analog blocks like voltage references, D/A converters, [ph?] and things like that. The genius of Widlar and his colleagues along the same timeframe is they figured out ways to take the technology and use its unique strengths. One unique strength is that if you build two resistors side-by-side or two transistors side-by-side if you build a 100 different chips the absolute value of the resistors might vary over a 40 percent range. The transistor V_{BE}s would vary a lot. But the two built side-by-side would be pretty much identical. Matching was really the strength. And in mixed signal ICs, you really need a PNP transistor. They figured out that you can get a sort of PNP if you do the geometry right and you make a lateral device where there's two p-diffusions acting as emitter and collector and you use the n epitaxial region between them as the base.. They were able to take that set of things and build op amps, the 702 and the 709 later on and lots of others after that. They weren't quite as good as the best amplifiers using discrete component, but they were pretty darn good. There was a whole array of design innovation that was happening by Bob, Barry Gilbert and other real pioneers of the bipolar world. And I though the creativity there was, really great. I wanted to work on that stuff. I interviewed around the country when was getting out of school. I still remember those days really well because in 1968, 1969 the IC industry was really, really in its formative stages. I met a lot of great people like Tom Fredrickson, Jim Solomon, Bob Pepper and other folks that later became leaders in the industry. I interviewed at Bell Labs, doing the traditional three-site interview there. I interviewed with Dave Hodges and a lot of other people there. But the place really intrigued me was Fairchild out at the R and D labs and I ultimately ended up going there. And I went into a group that was doing the advanced R and D for linear ICs for the company.

Hodge: You mentioned mixed signal. So just as a definition, that's any circuit where you've got both digital and analog stuff going on at the same time.

Gray: That's a good question. There weren't any mixed signal circuits in 1969: that term came quite a bit later. In1969 the digital guys were building, TTL and ECL. MOS technology was rapidly developing. — Intel had been formed in '68. Semiconductor memory based on MOS was becoming important, based first on, metal gate, and then later silicon gate. Significantly complex digital logic chips based on MOS technology were emerging as products. But in the analog IC world there was virtually no MOS at all. And the integration level was very low; 50 transistors was a complicated analog circuit. It wasn't mixed signal-it was all analog. That was pretty much the state of things in the late '60s.The mixed signal terminology came along later, which we'll get to. That word today it means chips with both analog and digital on the same chip, which were virtually nonexistent in 1969.

Hodge: Okay. What interaction was there between engineers designing digital logical chips and engineers doing analog ICs and analog work?

Gray: That's a good question. Now, you know, again, we're talking about kind of a specific place. We're talking Fairchild R&D—

Hodge: Just in general in the industry.

Gray: In the industry? That would be hard for me to judge, but. , I don't think there was as much as you might think. My only firsthand observation is from Fairchild. Fairchild R&D was pretty unique. That was a really great R&D lab. In one building out there where you had a lot of digital designers, process technologists, device physicists, analog designers, and so forth. Many people from there that went on to be leaders at Intel, Ron Whittier, Sunlin Chou, Albert Yu to name a few. At the time I arrived there they were running R&D groups .By that point and time Andy Grove and Gordon and Bob Noyce had all already gone off and founded Intel. But to get back to your question about interaction, at Fairchild R&D there was a fair amount because you had a lot of different expertise in a fairly small space. But interacting with the product groups in Mountain View was more challenging, I had the strong sense they were pretty independent. I think the digital guys were doing their thing, and the analog guys were doing their thing. They were serving fairly different markets and there wasn't a lot of coupling between them.

Hodge: And then fairly high level, why was analog using bipolar and digital using MOS?

Gray: They both started in bipolar early on, before MOS technology begin to come on. Andy Grove played a big role in developing MOS technology when he was at Fairchild, along with many others of course. It became clear in the late ‘60s that MOS had tremendous advantages for digital, offering self-isolated device, relatively simple technology from a processing standpoint, (at least for the early versions of MOS). It was possible to get much higher densities and much higher transistor counts on a chip using that technology than in bipolar, all else being equal with the same yield factors and everything. MOS subsequently evolved into the product domain, initially for memory, and later on for logic chips and microprocessors. The technology was just better for that because of its density primarily.

The analog world was at that time, around 1970, ’71 was still pretty much bipolar. MOS has a lot of characteristics that make it less easy to use for analog functions. The MOS transistor has relatively low transconductance per unit of drain current at a given device geometry. The matching of two devices in terms of their VGS (gate-source voltage) as opposed to VBE, (base emitter voltage) at a given current was not nearly as good. Consequently the offset voltage of differential pairs is not nearly as good as bipolar. And in the technologies that existed in 1970 the bipolar technology was much faster. . So it didn’t look as attractive for analog. And, by the way, CMOS had not come along yet; at that time was not a mainstream technology. It had been developed and was in the laboratories but the vast majority of early MOS production was in, initially— PMOS Metal Gate and then later in NMOS metal gate and subsequently Silicon Gate. You didn’t really have a complimentary device in the mainstream MOS technology at that time. Of course, that all changed later. So bipolar looked very much preferable.

It was becoming apparent that integrations levels would over time become much higher- allowing integration of much more complex systems economically. This meant thousand transistor chips, ten of thousand transistor chips, and so forth as the digital levels of integration got higher and higher. We were headed for system realizations where the digital parts of an electronic system, might be built out of MOS LSI parts, but the interfaces to the analog world, which most electronic systems have were still going to have to be implemented using relatively low levels of integration, bipolar technology devices. Most electronic systems have digital processing for the main functionality but require analog and mixed signal functions at the periphery to interface with the analog real world of continuous physical variables. Viewing this as a digital egg with a mixed signal eggshell surrounding it is not a bad analogy. Dave Hodges and I, and later Bob Broderson, realized that one possible approach to this might be to integrate the digital part along with the analog interface part in MOS. I remember really well the first day Dave Hodges and I talked about this. And we, you know, “Gee, we really ought to look at what you can do in this MOS technology.” It was kind of a lark- it was heresy at the time that you would even try to build an analog circuit out of MOS technology because everybody knew the performance of the devices just wasn’t as good. So we started working on that. We did some work on early MOS op amps with some graduate students, Bob Blauschild, and Yannis Tsividis, and others.

In bipolar technology when you needed any precision you typically would use resistors .For example, in a D/A converter you might use a resistor arrays and their relatively good matching to define the ratio of currents in the D/A converter. You tend to use resistors for precision elements. That’s really not really practical in MOS technology, since you don’t have a good resister element in most MOS technologies, at least not at that time. But we realized we could use the capacitors, and so we began to explore using capacitors as precision elements in MOS technology, and that turned out to be particularly suitable for MOS because of the fact that as in a DRAM, for example, you are able to store information by storing a charge on a capacitor and then sensing it continuously and nondestructively with the gate of an MOS transistor. You can't do that in bipolar because the base current of the sensing transistor discharges the capacitor. Essentially that was a way of using a capacitor that wasn’t available in bipolar, and if you look at the MOS mixed signal circuits of today they use capacitors everywhere in ways that wouldn’t have been possible in bipolar. There was that compensating degree of freedom, the ability to use capacitors as precision elements. A number of grad students, Jim McCreary and Ricardo Suarez-Gardner, Harry Lee, and a number of others demonstrated the use of binary-weighted arrays of capacitors to do the D-to-A function as opposed to the resistor R-2R ladder, which was used in bipolar. That turned out to be a really good way to build an A-to-D and a D-to-A converter in MOS technology. Today if you fast forward through the intervening 40 years almost all of those analog interface functions can now be implemented around the periphery of a chip that’s got all kinds of complex digital on it, DSP functions and control functions and so on. Of course, there were many, many contributors at universities and in industry over the years to that capability, but that early work by those grad students on capacitors was an important contribution to that.

Hodge: At Fairchild when you were there it sounds like the products you were doing were sort of intermediate building blocks for other engineers to take and design with.

Gray: Yes, in those days the analog products that Fairchild made, as well as other companies like National Semiconductor, were things like op-amps, four-quadrant multipliers based on the Barrie Gilbert multiplier, three-terminal voltage regulators built out of bipolar technology, digital-analog converters, which translate digital information to an analog current or voltage output. These included 8, 10, and 12 bit D-to-A converters, and some other products including various kinds of voltage references, comparators, and so forth. . The methodology at that time for implementing an analog-digital interface function, for example an interface between a minicomputer and an array of sensors for some industrial application, typically involved developing a PC board that had an array of these different kinds of relatively low-level integration devices on it along with maybe a microcontroller, and so the analog elements could utilize whatever technology was optimum along with a whole bunch of TTL and other things.

That was pretty much the methodology through the 1970s or so, and things took an interesting turn in the decades after that. Applications for chips have evolved to include a range from the super-high volume ones like a cell phone where there are hundreds of thousands to hundreds of millions made a year, to those for which the volume of manufacture of the specific function is low- perhaps in the thousands per year range. . For the very high volume kinds of applications both in analog and digital the economics support developing application-specific integrated circuits or custom integrated circuits because the cost of the engineering to do that can be supported by a very large volume of use. But a large part of the world of electronics consists of applications where the volumes of the specific electronic function are relatively low. There are many kinds of electronic systems out there for which the quantity of each one that gets built is much lower, perhaps a hundred, a thousand, ten thousand per year. In the digital world we have lots of technologies to implement high levels of integration for those kinds of applications at low cost. That includes gate arrays and FPGAs [Field Programmable Gate Arrays]. You can do some sort of an ASIC [Application Integrated Circuit], or programmable devices of other kinds. These allow building really complex, tens of millions of transistors, hundreds of millions of transistors, complexity for the digital part of applications even when the volume of manufacture is low. .

The analog part of such low volume systems has always been a challenge, and we still don't have a good solution. Again, if it's a super high volume application like a cell phone, well then you can spend the engineering and just custom design the mixed signal integrated circuits, the RF integrated circuits, the voice integrated circuits, and so forth. But if you only need a couple hundred of them or a thousand of them or ten thousand of them there's no way you can support the engineering cost of custom designing one of these high-integration chips. So to this day companies like Maxim, LTC and Analog Devices have huge businesses making relatively low-level integration, relatively high performance , analog integrated circuits like op-amps and things of that level of integration. It's still a very important technology because we really haven't evolved a low-cost, effective, reliable ASIC technology for the mixed signal A-to-D interface part of the world. Things are getting better; there are a lot of good tools and there is a lot of good IP out there, preexisting designs, but still it's expensive to design mixed signal chips, and that's a problem that lots of people have tried to solve and with only limited success up to now.

Hodge: What's the fundamental problem that just can't be solved?

Gray: It's a whole array of things. Mixed signal circuitry tends to have a lot higher interdependency between the underlying process technology and the performance of the function. Things like device matching really matter, as does the characteristics of the transistor. It gets much more complicated, for example, to migrate from one technology node to another technology node. It's difficult when you're talking about mixed signal. You have a lot of things happening that are second order that really matter in mixed signal. Noise coupling through the substrate for example. It's an array of interdependencies that just make it more difficult. It takes a lot more design verification to predict with good reliability that a given circuit's going to work in a given technology. If technology were standing still, for example, if we'd gone to 65 nanometers and stayed there forever in CMOS then the mixed signal ASIC problem would be much easier. . But the problem is the technologies keep migrating, so you if get a great suite of tools and IP developed for doing analog mixed signal for one technology and now it's time to move to the next node, and it's just proven to be very costly and engineering intensive. . But I think it's mostly the interdependency of the underlying device technology and how that affects the performance of the block.

By the way the power supply is very important. The same basic processor or DSP architecture can address a vast number of applications. The analog world is extremely fragmented. You've got data communications receivers, analog video interfaces, voice interfaces, various kinds, sensor interfaces, and each of those is very dependent on, for example, the power supply. What you can do and can't do depends a lot on the power supply of voltage you're using. And in the analog world we've gone from plus or minus 15 volts in 1970 to 1 volt or 1.2 volts today or less in the most advanced technologies. Every time the power supply voltage takes a step down everything has to be rethought in the analog part of the circuit because dynamic range, — the ratio of the signal amplitude inside the analog part of the circuit to the noise level is critical. And so that's another variable that has really made it difficult to develop the same kind of sort of systematic approach to ASICs that the digital folks have enjoyed.

Hodge: What caused you to leave Fairchild and go to Berkeley and why Berkeley?

Gray: I was really having a good time at Fairchild. I was working on a lot of interesting stuff, but I happened to run into Don Pederson at a conference. Don at that time was a senior faculty member at Berkeley, and he's the guy that made SPICE happen. I'll talk about that more, but to answer your question I ran into him at a conference. He invited me to come to Berkeley as a lecturer and just, "Hey, come on up. Try teaching for a year." I had a brand new baby at the time so the last thing I really needed to be doing was taking off and abandoning my job for a year. My wife Judy thought I was completely nuts. But I was a great admirer of Don, and the chance to work with him was something I didn't want to pass up. And I was at the point where I wanted to try something a little different. So we rented an apartment up in Berkeley and went up, and I taught a junior level electronics course for a year and I really just liked the place. I liked everything about it, so when a faculty position opened up I stayed, because of a number of things. First, the people that were there were phenomenal. Don and his colleagues, Dave Hodges for example, were phenomenally creative, energetic people. You tend to think of the academic world as sort of an ivory tower, but it was anything but an ivory tower. The electrical engineering folks, at least a large subset of them at that time, were very closely connected to Silicon Valley. And Don, of course, had this tremendous design automation activity going on. He and his colleagues and students were in the process of developing SPICE, which later came to be the basis of essentially the entire industry's circuit simulation capability. We can talk about that some more, but I stayed because there was that plus I liked the value system, an environment where the highest values are the search for truth, the creation of new knowledge, and passing it on to the next generation, just the whole academic environment. I just liked the value system as opposed to worrying mostly about what next quarter's going to look like, the things you have to worry about in a company. I also liked teaching a lot. I was able to keep a lot of involvement in Silicon Valley. I went down several times over the years and spent a year and ended up doing different things. And I consulted one day a week for most of the next 20 years in various companies. .

Hodge: What was Silicon Valley like at that time? Did people realize it was going to be sort of the tech hub of the known universe as it seems to be at this time?

Gray: I don't think they did. Of course there were the visionaries. [Bob] Noyce, [Gordon] Moore, and [Andy] Grove, along with others in the valley at the time, were true visionaries. They probably saw it. The people like me who were just doing the day-to-day product development could certainly see that there were all kinds of near-term opportunities. When you had this incredible technology evolving at the rate it was evolving you could just see that there were new possibilities. , "Oh, yeah, well, if we can actually- now if we can move up to twice as many transistors in a chip we can do this." But if you had asked me, "Will the semiconductor industry be a $300 billion industry in the year 2012?" I would've just shaken my head and said, "I couldn't even begin to guess that." I don't think many people, certainly not people in my pay grade, had any idea how important the technology was going to be society as a whole. The Internet hadn't happened yet. The biggest computer we had at Fairchild other than the mainframe was a PDP-8. It was really the early days. The International Solid-State Circuits Conference was the preeminent conference in the field. It was held at that time in Philadelphia every year, and I participated in that a lot, gave a lot of papers there, but I remember one session in about 1975 or 1976, a panel discussion on semiconductor memories. There was a paper at the conference on the 16k memory and the panel was all about how do we get to the next step, the 64k memory? And I remember listening to that panel and each panelist got up there and said, "Oh, we'll never get there- you know, we just- it's so hard. We'll never get the signal noise, and we'll..." and so there was this feeling that, "Boy, you know, there's all these challenges and it's difficult to get to the next step," and we were so focused on sort of the challenge of the next step that only a few people could look at the horizon and see that the steps just kept going. I think that intuitively a lot of people assumed that there was some sort of a limit. We were going to somehow hit a stop. Gordon, of course, was far sighted enough to see that it could go on for a long time, and of course he was right. I don't think any of us would have predicted that we would be building 22 nanometer transistors 35 years later.

Hodge: A philosophical question combined with a geographical question. Is Berkeley part of Silicon Valley?

Gray: Well, of course if you ask that question to most people they would say, "Hmmm, I don't know. I think it's up there in east bay somewhere, isn't it?" But I think of it as part of the bay area ecosystem. When I talk to faculty members about coming to Berkeley, for example, talk to them into coming to Berkeley in engineering, Silicon Valley is critical and important but I stress that the bay area is actually an ecosystem. There's the biotech world fore example, which is really important, and that's mostly around San Francisco. The engineering faculty is very connected down here. A lot of them had careers very much like mine where they worked with Silicon Valley companies, most either in the microelectronics world or in the software world or the computing world, so I would argue we are part of Silicon Valley although geographically, of course, we're a little separate.

Hodge: Okay. So what did you do when you first started at Berkeley? What was your area of research as a junior professor there?

Gray: I worked on microelectronic circuits, mixed signal, and I actually continued for a period of time the electrical thermal interactions. I had a couple of grad students who did some work on that, actually built an electrothermal simulator that was an extension of SPICE so you could simulate an electronic circuit with thermal interactions. Kiyoshi Fukahori, one of my grad students, did that, and that was used by some people at the time. But mostly I just worked on the continuation of the same theme, mixed signal integrated circuits with an emphasis on this analog digital interface. Initially we worked on the building blocks, which were some MOS op-amps and a little bit later on down the line we worked on voltage references in MOS. A couple of graduate students, including Ban-sup Song worked on that. We fortuitously had some great consulting relationships, both Dave Hodges and I, down here. My consulting initially was at Fairchild and I consulted with Signetics and later on with Intel. And that kept us closely tied to what was happening in industry. A few years later in 1975 I got a chance to go down to Intel and do some consulting. The person I consulted with was Ted Hoff, the inventor of the microprocessor. Ted had finished his activity on the microprocessor and he was looking around for what was next to do. He was very good at identifying markets and the way he would identify markets was, “Let’s figure out, things for which there’s one for every person in the world,” and you know, the phone was one thing that came to mind. And so he began working on the electronics that sit at the other end of a telephone line, this was before the days of cell phones, in a telephone central office. Earlier, of course, telephone central offices were made with relays and telephone switching was done in relays. In that era we were making a transition to electronic digital central office switching, but if you’re going to switch telephone conversations in the digital central office you have to have an A-to-D and D-to-A convertor for every telephone line, or at least shared among telephone lines. So Ted saw that opportunity, and in my consulting down there we were working on the set of things that goes along with that analog-digital and digital-analog conversion process associated with each telephone line, and there’s a lot of telephones so that’s a big market. That drove us toward more focusing on A-to-D conversion. We had done some A-to-D conversion work earlier with Jim McCreary, and then one of the graduate students, Yannis Tsividis began working on the particular flavor of A-to-D convertor that you need to do telephony, which is a funny nonlinear companding law kind of A-to-D called a mu-law. Yannis developed that technology and then along with that we recognized that whenever you do A-to-D conversion if you have a signal with some frequency content that’s higher than half the sampling rate of your A-to-D convertor you have to filter out that energy because otherwise it aliases down. It’s called an anti-aliasing filter. So you need a filter that sits in front of the A-to-D convertor, which is called a codec by the coder/decoder, the A-to-D and D-to-A are called a codec. So in front of the codec you have a filter. Now, that particular filter had a very well-defined characteristic because the telephone network had evolved to the point where that filter was implemented in digital central office switches with an LC passive network of a certain specification, and now that had become standardized, so this filter was standard for voice central office switching. And so our job was how do we take this LC circuit, LC passive circuit, and integrate it on a chip? Pretty hard to do. Well a couple of our students, Ian Young and Dave Allstott and then-new faculty member Bob Broderson, and I had been working on this technology called switch capacitor filtering. And we had been working on that for a year or two, and it was a perfect match for that application, enabling you to use an op-amp and some switches and some capacitors to build something that mimicked a passive continuous time passive filter, which was the thing in use then. So we were able to come up with a configuration that mimicked that filter in a telephone network so that allowed you to build that A-to-D convertor, the filter, and there was another filter going in the other direction all in Silicon, all on one or two chips. And that made a big splash. There were several parallel efforts. The Intel effort was one with Ted, and Ben Warren and John Huggins were there at the time, and Harlan O’Hara who I worked with. There were some other companies. Siliconix was building a chip like that. They built an A-to-D coder chip. There was some work at National with Dave Allstott and Bill Black. They built a codec and filter. But that made a big impact. We had this university effort on analog MOS, switched-capacitor filters and charge-redistribution A-to-D convertors and all that. But it was just a bunch of university professors messing around. But when that particular high-volume application came along and those chips came out that really worked in that application that made for a very rapid adoption of those techniques because that was economically quite important. And it got to the point where by around 1990 or somewhere in the '90s if you picked up a phone in the U.S. and made a phone call you had better than 75% likelihood that you were talking through some derivative or some version or some example of one of those kind of chips, so it had a big impact. It made a great sort of first application of that technology. And then today if you look at a cell phone or something like that where you have RF interfaces, you have voice interfaces, you have all kinds of interfaces, the same kinds of technologies in much different form nowadays but are used in a lot of those analog digital peripheries.

Hodge: What was Intel's stature like in the late 1970s? At the time right now they've been a massive company with sort of a near monopoly for 20 years or so. What were they like back then?

Gray: Well, that's an interesting question, a good question. I think they had several things that they did really well back in those days. Going back to Fairchild years the one thing that despite all the incredible people that came out of Fairchild and the ideas and technologies, they did have a lot of trouble at Fairchild translating things that were developed in the R&D labs to products. Silicon gate MOS technology is one example. And a lot of the startups that came out of Fairchild happened because frustration at getting some of those ideas into production in Fairchild products. It was partly because of the separation. So one of the things that the Intel folks did well is they didn't really have a centralized R&D. They would take their high-powered research people and cycle them through. I remember Ron Whittier, who's a superb Ph.D. research device person. In his career at Intel he one time went and ran a factory. They would cycle the R&D people down into their product groups, and I think that was really a big part of their success. Another part of it I remember well at the time was the decision to get out of the memory business, which had happened along in that same timeframe, an incredible, bold decision to make. When I talk to Gordon these days he says it wasn't bold, it was obvious. They had to do it. But most people at the time thought that was a pretty bold decision, turned out to be a great decision. A third thing about it that I remember is the presence of Andy Grove. Andy was a presence. His management by, I don't want to use— confrontation is the wrong word, but confrontational management style, and it had a big effect on the company. I think it made people accountable. It gave a sense of urgency to things. I think having somebody like that in your top management team is important, and one Intel anecdote I'm sure you've heard many times is that there for a period of time in the '70s there was a sign-in desk at the front of every facility, Santa Clara 1, Santa Clara 4, all the way. If you were late by more than a certain amount, you had to sign in when you came in in the morning. And if you showed a consistent pattern of showing up late like 9:00 or 10:00 your supervisor was notified, and this was part of Andy's management style. You know, you could argue management styles but I think it made the company more aggressive and more accountable. We could go on and speculate a lot about Intel in the subsequent years but at the time it was a dynamic place. When I first went there as a consultant in 1974, I think it was or maybe '75, it was a small place. Santa Clara 1 and 2 were the only buildings. I had an office just down the hall from Gordon and Andy and Bob Noyce. I saw Gordon relatively frequently, and it was a dynamic start-up high-energy place and really fun to be there.

Hodge: So your time at Intel when working on the—

Gray: Oh, excuse me. Let me just cite one other thing that I thought they did well. They had a management pattern of creating product teams with a product manager who had god-like authority over the project, like the 8086, these different large projects. This was not common in the semiconductor industry at that time. The typical semiconductor company tended to be an individual contributor-driven place where engineer A over here is doing an op-amp and engineer B over here is doing another product, and maybe these two engineers something else, so there wasn't a well-established culture in the semiconductor industry of managing large projects. Well, at Intel in the microprocessor days the projects become very large, and they did I thought a good job of recognizing that you need strong project management— it's a lot like designing an airliner. It's a big project with a lot of different sub-pieces, and you need a project management kind of structure that's effective, and they did a good job of— and in the projects I was involved in I thought a good job of structuring project management, which was not true at a lot of other chip companies.

Hodge: How did your time at Intel shape your future research?

Gray: Well, it was transformational for me because all of my previous experience had been in bipolar technology at Fairchild and other places. When I went down there my job was to design a real product, an analog mixed signal real product, in what at that time was five micron N-channel silicon gate technology, depletion mode NMOS. I spent a lot of time learning the details of what that technology was like and what it could do and what the real differences are between bipolar and MOS and what the degrees of freedom are. That particular technology had two layers of polysilicon, making capacitors out of two layers. It was transformational because I really could see the power of the scalability of that. I could see why MOS was going to go down the scaling curve in a way that bipolar would've had great difficulty doing. And so I really could see that the future was going to be large complex, ultimately CMOS. You know, the big debate in the 1977, '78, '79 timeframe was whether CMOS was going to take over? And there were some holdouts, I was probably one of them, that said, "Oh, it's too complicated," but then the power ended up making it a complete no-brainer because the chips were getting so complicated so fast, much larger so fast that the power dissipation didn't leave any alternatives. Depletion load NMOS is a static technology. The power was way higher in digital functions, so CMOS came in very rapidly, and then in the years that followed that went down the scaling curve, and in many respects is better suited to analog functions than N-channel. It was a real challenge to design some of those early mixed-signal circuits in depletion load NMOS. That was a challenge. We had a very talented person, one of the key people in that, a guy named Dan Senderowicz, a Berkeley PhD student, , who was with me at Intel and designed those first N-channel operational amplifiers that went into those first products. That was a really,

really fine piece of design that really enabled those. Became much easier later on when you had CMOS technology with a complementary device.

Hodge: So besides telephony what were the major applications for what you were doing?

Gray: Initially telephony was the biggest one. The other large one at the time was modems. I guess we were at the 2400-baud modem going to the 4800 and 9600. You know, it was in those days when telephone modems were a really big market. There were some companies that were using this same technology to build modems, a lot of the same kind of things, A-to-D converters, D-to-A converters, filters go into a voice band data modem. I should say I'm talking about voice band data modems, the ones that send data using voice frequencies over a telephone line. You don't use them that much anymore, so you don't see them very much these days, but that was a big market. So that was probably the second wave. And there were some companies formed at the time to do that. That went on for a while, then there were a number of other communication applications that followed on. ISDN transceivers, cable modems, etc- anytime you have a communication medium with a complex modulation on it or a continuous time signal. In the case of cable modems on a cable like the one coming into your house you have the whole array of TV signals all digital but encoded in a very complex multipoint constellation that's essentially treated as an analog signal. It's filtered, A-to-D converted in a high-resolution A-to-D converter and then processed digitally after that, so those analog digital frontends look a lot like in some ways the analog frontends for the old codecs. They are just 30 years later and probably 10,000 times faster. And so there's a generic analog digital interface that looks like some kind of an amplifier or signal conditioner, some filtering, some sort of an A-to-D converter, usually there's some sort of clock recovery. If it's a data signal you have to recover the clock, and then there's the corresponding set of functions going the other way. And that kind of a function is used a lot in a very wide variety of kinds of data communications applications.

Hodge: Did the emergence of the personal computer industry, did that drive any of this modem uptake?

Gray: Oh, yeah. If you had a PC back in the '70s that's how you hooked to anything. You may be too young, but you'd have a PC or a laptop and in the PC you'd typically get a modem card and you'd plug your telephone into it and you'd dial up, and often in those days you're dialing into a mainframe or you're dialing into some kind of time-shared system or something. And for a long time that was the way— I remember the years I was at Intel I'd be at home and I'd dial in. They had a remote connection capability on whatever, I forget what system they were using, some mainframe, and you could be a remote user. So you'd log in and you'd look like a remote user— it looked like you were on a Teletype sitting there at the computer. And a lot of the engineers worked from home using that, but it was a voice band data modem that made that connection. And then over the years that's evolved to be then into ISDN, and then the cable companies got into the data business, and nowadays of course for remote access it's cable modems and various other flavors of technologies that operate over a combination of fiber and telephone lines.

Hodge: So continuing on that line this sort of takes us back to software tools again. Xerox PARC was famous in the late ’70s for sort of the development of VOSI with Carver Mead out of Caltech. Did this start impacting you? Were you using software tools more?

Gray: Well, in the mixed signal and analog world mostly it’s about simulation and then verification. So the simulation tools became, as they did for everybody, critically important, and in the analog world they were super important because you had to be able to simulate these complex analog functions. SPICE was pretty much the standard tool for that. Again, I wasn’t directly involved in the development of SPICE at Berkeley but I worked a lot with Don’s students and I was kind of a guinea pig. One of the things Don did was try to get the circuits people at Cal— myself and Bob Meyer, Bob Brodersen, and Dave Hodges to use those tools, and that was a big factor in helping him develop the programs. But, no, Silicon Valley wouldn’t exist as we know it without those tools. You know, it’s not just analog. It’s any kind of digital design, you have the high-level tools, synthesis tools, all the verification tools but down at the lower level somebody’s got to actually figure out the transistor level circuit behavior of these things, so the whole suite of tools is needed— we wouldn’t be sitting here if we didn’t have those. But again, in the analog and mixed signal it’s mostly simulation, and the tools have evolved a lot. In the early days it was really just pretty much time domain and frequency domain, circuit simulation.

There have been some great tools that have evolved for RF. One important evolution that really has had a big impact is that, starting in about 1990 or so, faculty members principally UCLA and Berkeley, as well as other places, began to recognize that you can do radios with these technologies. The MOS technology had gotten fast enough when you’re down below 1 micron channels you have enough bandwidth to build RF functions. So over the course of time from about 1990 on into the 2000s the capability evolved through a lot of people’s work to today where the entire cell phone, all the RF functions, not including one or two, the power amplifier for a cell phone is usually still implemented separately with GaAs or some other technology, but everything else, all the frequency synthesizers, the low noise amplifiers, the mixers, the baseband demodulation, all the voice functions, the encoders, D-to-A converter functions, those are all done in a monolithic silicon implementation up to and including the 4G, all the latest standards. And that’s one reason why cell phones are so cheap. The bottom end cell phones are very, very low cost, and it’s that level of integration that allows that.

You started by asking about CAD tools and the reason I mention the RF is that the simulation tools had been critical for that because in RF mixed signal you have inductors, for example, that are very important, and you need models for those inductors. And the simulation tools that allow you to take an inductor physical layout and develop the circuit modeling for that, the losses and the amount of inductance, the amount of mutual inductance, all of that, those are critical. And there are a lot of other things about RF circuits that are particularly challenging for simulation, the crosstalk through the substrate and things of that sort, so simulation was especially critical for the RF part of that development that happened a little bit later on, but nowadays that’s a really big chunk of the mixed signal world is the RF functionality.

Hodge: Do you still have a working wafer lab at Berkeley?

Gray: We do, we do. That's been through a number of incarnations. I'll give you a little history there. The first lab there was built in the '60s. Don Pederson and Dave Hodges were the instigators of that. There was a similar one at Stanford and a little bit later at Arizona and MIT. That lab stayed in its pretty much original form then until the '80s. I guess it was in the early '80s another faculty member, Bill Oldham, who's somebody you might want to interview at some point. Bill spent a years at Intel, and worked on one of the original memory chips at Intel. Bill was able to get the state legislature and the regents to approve funding for a new lab on the fifth floor of Cory Hall, much better, much more capable. Then that stayed in that form up to about two years ago, and now we've moved to the third incarnation, which is a much more capable large laboratory in a new building called the Sutardja-Dai Building. That was enabled through a lot of fundraising and donations from companies and equipment gifts in kind. Now, that shifted its functionality a little bit. We've reached a branch point, starting in the 1970s, up until the mid 1980s; every one of my graduate students would design and then fabricate a chip. Then they'd in the early days cut the rubylith, go in the lab, actually make the chip, push the wafers in and out of the furnaces. And that was a fabulous experience. I went through that as a grad student, and boy, there's nothing that gets you to appreciate what's involved in the technology more than doing that. But by the time we got to 1985 a lot of things had happened. Processes were getting much more complicated. The chips were getting much more complicated. Avenues became available for building chips outside and Carver Mead deserves a lot of credit for this because he developed the concept of silicon chip design as kind of a publishing exercise where you could use a foundry to build the actual silicon and so through his conceptualization and through a lot of other people's efforts it became possible to go out through MOSIS at the time and actually through other individual companies and get stuff made. And we just had to start using that because it just became too much for a grad student to design up a new integrated circuit with 500 or 1,000 devices on it and then lay it all out and then go in the lab and build it, it was more than you could really expect. So about the mid 1980s we moved to a model where the students get their chips fabricated outside, and that's the mode we're still in today. I mention that because the second and third incarnations of the laboratory tend to focus not on mainline technologies where you're replicating something that's happening in industry, but instead on unique technologies. One example would be MEMs There is an awful lot of MEMS technology in the lab now. These are micro-machined devices that build accelerometers, other kinds of sensors and other unique kinds of electronic devices; they do a lot of nontraditional non-silicon kinds of experimental devices. So they're not focused so much on the mainstream, although they do advanced technologies, for example, FinFETs. Chenming Hu, who is one of our advice physics faculty members at Berkeley, he and his students and one of his colleagues, Tsu-Jay King, did the early work on the FinFET, which now of course forms the basis of the 22 nanometer Intel latest technology and the following technologies and looks like the way we're going to get around some of the scaling issues associated with very, very short channel lengths. So that's a great example of what you can do in a university lab, but in doing the FinFET they didn't have to build million transistor chips. They built test chips that had FinFETs on it, so it's that kind of thing that's the focus of the lab these days.

Hodge: So moving our timeline up to the mid ’80s, you were at Intel in the late ’70s then in the ’80s you were at Micro Linear for a year. What did you do there?

Gray: Well, I went to Micro Linear at the invitation of Alan Grebene, who was the founder, and Jim McCreary, who was one of my students, was also the vice president of engineering there. The company was founded on the premise of solving this problem I mentioned earlier, that the mixed signal world does not have a good ASIC technology, and the concept that Micro Linear was based on is let’s figure out how to do analog and mixed signal ASICs. That seemed like an intriguing possibility to me, so I wanted to go and get involved in that, and I spent a year. First I consulted there for a while and then I spent a year down there working on that, but it became painfully obvious after <laughs> about three months that this was not going to be viable— it’s like many startups. You know, often in startups you have a nice looking business plan and a nice product concept, but a lot of times you get three months into it or six months into it and you realize that that is not going to work, and it just became obvious that the tools weren’t there to cost effectively allow you to crank out custom designed mixed signal circuits— the tools weren’t good enough. And as it’s turned out they’ve really never quite gotten good enough, but it was certainly not anywhere near there at that time. So about halfway through that first year we split the company into a standard product piece and a mixed signal piece .I went to the standard product piece and we did some A-to-D converters and things like that, and Micro Linear ultimately made some degree of success out of those. They went public in about 1991 or ’92, I think. But they were one of those companies that never quite found a niche and they ultimately got acquired.

Hodge: Okay, how are we doing on time on the tape or whatever the tape analog is?

<crew talk>

Hodge: Okay. So after your time at Micro Linear you started doing various administrative things at Berkeley. What prompted you to move into that?

Gray: Well, in the academic world everybody has to take their turn to do the administrative jobs. It’s a little bit different than in the business world where high executive positions are sought after for many reasons but in the academic world, at least in my view of it, it’s more about doing your part to pay the institution back by helping out with the jobs. It’s hard to find people sometimes to do these jobs, so I served as Department Chairman of Electrical Engineering from ’90 to ’93, and then I was the Dean of the Engineering School from ’96 to 2000 and I was the campus provost from 2000 to 2006. I did those jobs primarily because I felt I could make a contribution and help the place out , and in some cases nobody else wanted to do them so I did them. I felt like I could help them and so I did those. When you think about a university Berkeley is a fabulous place, but it’s really very different than a business enterprise. People don’t realize the degree to which university faculty are really entrepreneurs. Running a university like that is a lot like running a big law firm with 1500 partners, each of whom is out there seeking their fortune and trying to find a niche for the skills they offer, and faculty members are a lot like that in a big research university. They're out trying to get research funding. They're trying to create things that change the future and benefit society in whatever way they can. University administration is a lot about just creating an environment that allows them to do that. You try to shield them from the pressures of the real world to the extent you can, try to create a good environment for creativity, for everybody to talk to everybody else, to have the opportunity to create and pursue crazy ideas, really important that people have a chance to try things out, be adventuresome, take chances. Another really critical thing, which I'm sure we'll talk more about is this interdisciplinary dimension. A campus like Berkeley is one of the few places where you can have all of these disciplines working together, rubbing shoulders, and I cite Don Pederson's work as a good example of that. If you look at the CAD development, the simulator that he and his students worked on, he was a circuits guy, and to make a simulator you need other things. You need people who could take the semiconductor devices and create numerical models. You need somebody that understands the physics really, really well and knows what kinds of approximations you can make. You need device physics, and then you need numerical analysts. You need people who know how to write big simulation programs. You need people from the applied math department. This is obvious today but back in 1970 it wasn't so obvious. The key thing that allowed Don to do that was being able to bring in all these other people easily. I mean, these are people whose offices were five minutes away from his, who he could have lunch with every day, and they weren't sitting in some other company in some other part of the country. So that mixing of expertise and knowledge is sort of really the most important single ingredient universities can bring to the party. Almost all the great new ideas like bioengineering, mixing engineering and biology, and a whole array of other new things that have come along are really coming at the boundaries between disciplines where people work together that hadn't worked together before. And so fostering that is a big part of what being a university administrator is all about, so trying to help with stuff like that was kind of why I got involved.

Hodge: On that note Berkeley has a combined electrical engineering and computer science department. Why are those two departments combined?

Gray: Well, that's a great question because as you well know it's different across the country. They are separate in a number of places. Stanford has two separate departments. The critical thing is that those two disciplines work together closely, so what the organizational chart looks like isn't so important but if you look at the range of things that engineers do in the theoretical end of CS and then you move down through software engineering, operating systems then into hardware, computer engineering and then into the systems applications of EE like communication systems, and then into the microelectronics, there's a continuum there. And you want those people talking to each other, and the ability to collaborate across those boundaries is absolutely critical. And the argument we've always had— we've had this debate on the campus many, many times. There are people who think the CS department and the EE department should really be separated, and they could be. There's no reason we couldn't do it. But when the debate happens and the faculty talks about what they think is really important every time that debate has happened it's always come out, "We're better off together because it forces a collaboration and a rubbing of shoulders and a common view of the world, common department meetings, common thinking about who are the kinds of people we want to hire, what are the right fields," and so the argument has always come out it's better to keep them together so we have this close linkage. You don't have to have people in one department to get a close linkage. You could do that lots of ways. It’s just that we thought it was best to leave it the way it was. There are some great departments like Stanford CS and EE that are separate. MIT is together. It varies, but the most important thing is you’ve got to make sure those people don’t have some sort of artificial barrier between them. It’s also important for the students. Think of the students that come out today. Even if they’re in microelectronics, if they’re going to be a part of a team that’s designing a cell phone they’re going to be spending a lot of time on DSP software. They’re going to be worried about operating systems. You know, there’s a spectrum of knowledge that the graduates need today that isn’t just EE or CS. They need a spectrum. And so you want the curriculum to be porous. You want it possible for students in EE to easily take the basic software courses and the more advanced software courses they want. You want your people in computer science to get some grounding in some basic hardware concepts, and so that’s another reason to try to keep it together.

Hodge: At some point in your career you wrote a book on analog integrated circuits. What year was the first edition and what brought that about?

Gray: Bob Meyer and I wrote that together. The first edition was published in 1977. The timing was good because at that time there were only one or two other books on analog ICs. I had a lot of experience to bring to bear because I’d been at Fairchild and I had exposure to actual product design. And Bob, of course, had been at Berkeley and he’s a superb writer and a person with exceptionally high standards, so between us we came out with a result that’s had reasonably good acceptance and it’s lasted a long time. I think it’s in the fifth edition, But an awful lot of what happens in life is serendipity. If we trace through the history we’re talking about an awful lot of things are just accidental, and we just decided to write a book on that particular subject at just the right time. We were there first with a book that had the collection of topics that was really needed in U.S. university curriculum about that time because the semi industry was growing. The analog part of it was growing, and people needed to teach courses in that area, so it met a need that was there right at that time.

Hodge: What made you continue with it after the first edition?

Gray: We did the second edition, Bob and I together, which was fairly minor adjustments, and the third edition, which brought in some MOS devices and circuits. Then when it came time for the fourth edition I was already involved in administration. I just didn't have time to work on it, so we were extremely fortunate to get Steve Lewis and Paul Hurst involved. Steve Lewis was my student and Paul Hurst was Bob Brodersen's student. They had gone to UC Davis and are still faculty members at UC Davis. They came in and said, "Hey, we'd like to do the third edition as coauthors". It was great because I couldn't do it and Bob was pretty busy, so they actually did both the subsequent editions. Bob and I helped. I mean we reviewed some of the material, but they did all the writing in those later editions so I can't claim much credit for spending a lot of energy on that but we were lucky enough to have two great colleagues, Paul and Steve, who did.

Hodge: When you started at Berkeley in 1971 Berkeley at that time nationally was probably more known for the protests and hippie movement it would seem than anything else. How much of that was grounded in reality? What was it like—

Gray: Well, I remember well when Judy and I drove to the Bay Area to take that job at Fairchild. That was sometime in June or July of 1969 and we were listening to the radio and that was People's Park—People's Park was going on and there were these accounts of what was going on at People's Park with police firing rifles from the rooftops and boy, we just thought what on earth are we getting into. Well, of course we were going to Palo Alto, not Berkeley, but still it was the Bay Area. By the time I got to Berkeley in '71 we didn't really have anything like People's Park after about 1970. But there was continual student activism. There were an awful lot of war stories every lunch hour I got to listen to, Berkeley demonstration war stories, all I wanted to hear. I should say engineering was pretty remote physically way on the northeast corner of the campus. Most of the activity was down in Sproul Plaza. I didn't have much consciousness of that. I was a young assistant professor with my nose to the grindstone trying to do my thing. I didn't have a lot of awareness of it. The engineering faculty other than having experienced walking through didn't really have a tremendous involvement in it, so I can't say it was a big part of my experience there. Now later on when I got involved in administration we had demonstrations and then I really started to listen to those war stories because you learn a lot. They had a virtual playbook of how to handle student demonstrations in various circumstances and so I became much more aware of those years later on when I was dealing with some of those kinds of things in California Hall. But in the '70s other than that experience of driving up in the car I don't remember that the engineering faculty were that affected by it.

Hodge: What was the relationship like between Stanford and Berkeley at that time and continuing through your career?

Gray: Almost all collaborative, very positive. I'm a close friend with a number of Stanford faculty members. Several of them came from Berkeley, Bruce Wooley for example who is long-term EE department chair there. On the football field there's a lot of rivalry but in engineering kinds of things there's a lot of collaboration. We had some joint centers over the years. We've worked together on road maps for semiconductor technology. Later on, I was a department chairman, an engineering dean and provost. There was a little different aspect because of the faculty recruiting. If you go back to that picture I painted of 1500 attorneys in a law firm, it's a little bit like that. The job is to hire the very best people you can. That's what defines the quality of the place - the faculty. You want to do everything you can to make them successful, create the best environment, but the hiring piece is really competitive. The department chair, the dean, and the provost, spend a lot of energy making sure to identify the best young people either coming out of their Ph.D. that year or a lot of times of course they're out in industry or they're teaching somewhere, but identify them by papers they're giving at conferences or other ways. You try to identify who the most promising people are.

At Berkeley some people are hired who are senior in their career or later in life but most of the hires are junior people, so that's really important. A lot of times Berkeley is competing with Stanford in hiring. I did that a lot and over those years and with Stanford we were about 50/50. We lost half the time, won half the time. We're both in the Bay Area; they're a great place. We did way better than that against almost everybody else and that was really behind my comment earlier about the Bay Area ecosystem. People really want to live in the Bay Area and especially people who are interested in technology and engineering, they want to be here. MIT is a tough competitor; we certainly fight tough battles with them. I think if you looked versus MIT we're a bit above 50 percent; I think we win more often than we lose against MIT. If we're competing for recruitment, we usually win against almost everybody else. Stanford is a tough competitor on hiring, but everything else is very friendly. We have a lot of great joint projects with them and it's a good partnership.

Hodge: Stanford seems to have more of a reputation for spitting startups, spitting new companies out, than Berkeley does. Do you think that observation is true and what do you think accounts for it if so?

Gray: I think it's very true. They started that way ahead of everybody else. I think in the '70s and '80s and the '90s they did a fabulous job. You've really got to admire Fred Terman with his vision, and everything he did to start the industrial park and all of the things that followed on from that. It really got Silicon Valley going in many ways. Also, they led the way in creating the culture of entrepreneurship that they did on campus. However, if you look at the last 20 years, and it's not just Berkeley, but many U.S. universities, the degree to which entrepreneurship has become a central focus is really phenomenal. A large fraction of the bachelor's degree graduates at Berkeley at one point or another have been a part of a business plan competition, or taken a course in entrepreneurship. That's really new; that was not true 20 years ago. It's difficult to get hard data on startups out of universities but the EECS department at Berkeley has done a pretty good job in recent years on this data gathering on their own faculty, students, and graduates. Since 1970, there have been 120 startups by faculty members and alumni. A substantial fraction of those are by faculty members. Most of that's in the last 20 years, prior to that it was that way lower. I think the reality is Stanford pioneered this model and it has really propagated. Universities around the country, especially in California like UC San Diego, and UC Berkeley, are trying very hard to create a spirit of entrepreneurship in their faculty and to get the benefits of that, local economic growth and regional economic growth. You get a faculty that is more closely connected to what's happening in the real world, better able to see the next logical target, and you get impact. You get situations like Google where you can trace the connection between research being done in the university and some development that ten years or twenty years later that has had a big impact on society, There are a lot of different models for that kind of technology transfer. There's industrial collaboration, there's industrial sponsorship of research, there's faculty consulting at companies, there are graduate students and faculty starting companies, but entrepreneurship's a very important part of that. And so there's a lot more of that at Berkeley now than there used to be and not just in computers and electrical engineering but also in the biotechnology world. We worked very hard both at UCSF and at Berkeley to create through the Quantitative Biosciences Institute (QB3), one of the governor's institutes, a set of incubators and all the other ancillary infrastructure to promote entrepreneurship and formation of small companies and it's really paid off; there's a lot of activity there. So I think we're catching up with the Stanford model pretty quickly.

Hodge: Does it to some extent detract from the academic research you do? One example is I believe Dr. Chua and his sort of invention of the memistor in the '70s, a completely ivory tower exercise that later got commercialized. Are you afraid that by focusing on practical applications too much you might miss some of these things?

Gray: It's— that's a great question. There's a danger there of that happening and balancing that is critical. The most important thing that happens at a university is fundamental discovery-discovering new planets, discovering new physical phenomena, and really basic fundamental research in the biological sciences especially. That's central, we've got to do that, so the translational part, taking that and moving it out into the real world can't become the whole ball game. You've still got to do the fundamental part and so striking that balance is critical and I worry a lot about that. I tell you what I really worry about. I'm not too worried about it at Berkeley and MIT and Stanford because we have a lot of people who are doing a lot of basic research and I don't think we're particularly close to getting to a dangerous overemphasis on applied or translational research. I'm using the word "translational" to mean more applied and moving things into the real world. I think where that can happen more easily is in sort of institutions that don't have as strong a research tradition, maybe schools where they see the entrepreneurship as a way to generate revenue for the campus. There could be a consequent overemphasis of that and I think it can potentially be damaging, I think it's just something you have to be careful of. You have to have that balance.

There's another dimension of that. If you look at the things that have really changed in engineering higher education in the last 30 years so, one of them is entrepreneurship, but another big change is the emphasis on multidisciplinary activity— I already mentioned that— this emphasis on working in teams and the fact that so many of the innovations come by bringing different disciplines together. But by the same token you still want this creative professor sitting in his office with these far out ideas working away. Like the Leon Chua idea. You don't want to lose that. In other words, you don't want everybody working exclusively on these team-based projects. It's a balance -- you still want individual investigators; you still want people who have nutty ideas to go off and work on it by themselves if that's the right way to attack that particular problem. It’s not one-size-fits all and we need to protect that.

By far the biggest change in engineering education over the last several decades has been the bioscience revolution. 30 years ago there were a few people working on medical electronics but 90 percent or 95 percent of engineering was physics and math based. The explosion of knowledge in the biological sciences, which by every indication will continue for the foreseeable future, has changed that dramatically. . It became clear that a whole new science base was emerging that was going to be very, very important and that there were going to be huge societal impacts in understanding the genome and everything about the way the body works and the way organisms work. That led to the evolution of bioengineering, the establishment of bioengineering departments, and the establishment of translational biotechnology efforts like QB3. This is a big change in engineering. .

Hodge: Is this because of the emergence of biology as more of a computational and informational science?

Gray: That's part of it— that's a big part of it but I think the most important thing is that the biologists are just figuring it out. If you participated in conversations with the biologists back in the '70s it was an observational science, you do this to a certain organism and it does this. There was not much in-depth understanding of the molecular processes at work. If you just look at the discoveries that have happened in recent decades, that is changing. For every level they understand it turns out there are two or three more levels below that that they realize now they need to understand and we're far from really fully understanding organisms, but the progress that biological science is making is just phenomenal. Someday we'll have a BioSPICE where you can simulate an organism and predict its responses to things, but we're a long, long way from that because of the complexities of all the interactions of all the different parts of the system, But I think the level of understanding is getting so great that it enables a lot of drug discovery, a lot of medical devices, a lot of invention, man-machine interaction, things like thought-activated machines, prosthetics, robotic surgery, etc- an array of things that were impossible 30 years ago just 'because the knowledge base wasn't there.

Hodge: What does the impact of industry abandoning or at least shying away from basic research have on universities?

Gray: Well, it's put universities much more in the center of activity when it comes to both basic research and translational research. If you look at the first half of the twentieth century, the things that were developed then, aviation, television, those kinds of inventions, while some of the basic science happened in universities, most of the translational work was done both by individual entrepreneurs and inventors l like Marconi , Bell, and people like that. At Bell Labs and some of the industrial labs they did some basic work but they also did a lot of translational work; that's fallen away as you suggested. And so if you look at the corresponding set of important inventions in the last third of the twentieth century, the internet, cell phones, things like that, you can see a much more pronounced role for universities in those inventions. Take cell phones, for example. While the fundamental cellular network architecture was developed at Bell Labs, a lot of the elements of what makes cell phones work today came out of universities. One example is CDMA invented by Andy Viterbi at UCLA, and of course he later went off and helped found Qualcomm. Almost all the fundamental research today is being done in the universities but in addition much more of that next step toward translational and applied research that used to be done at Bell Labs, places like Bell Labs, and in more traditional corporations, are moving more to universities. In technology industries like semiconductors and computers, due to the day to day competitive pressures, it's hard for them to do research that has an application time horizon beyond maybe three or four years That's where universities come in, picking up that gap. It has dramatically increased the importance of the role of the research universities because they have taken on a lot of that.

Hodge: How are research universities going to fund that in the future?

Gray: Great question. I should have mentioned one other big thing that is happening in engineering higher ed and higher ed in general. Public research universities in the United States have seen a 30-year-long diminishment of the state support they get to operate, the investment by the states in the operating budgets of research universities. There are a lot of reasons for that, a lot of demands on state budgets from healthcare, from an aging population, from crime, from all kinds of things. University students can pay tuition. Prisoners in prison can't pay any tuition for example so the result has been defunding of higher ed by the states. If you look at UC's percentage of state funding over a 25-year period we have good years and bad years - it's not monotonic,. But there's a very distinct long-term trend and it's still going down. The consequence of that has been dramatically increased cost of attendance so the tuition and cost of attendance has gone up dramatically at virtually all the public universities and particularly the research universities. It’s likely that will continue. The long-term impact is kind of hard to predict. I think that the public universities at the top tier, examples being Berkeley— Michigan, Illinois, and Washington, assuming they can solve the political dimension, do have the avenue of charging higher tuition and can create operating income by simply selecting their student body from the entire United States, taking more out-of-state students, that tier of students who can afford to pay higher tuition, and continue operating in something resembling their current mode by doing that. That has a lot of negative impact. There is a potential negative impact of access to disadvantaged students, and while there are some ways to counteract that it’s still a problem. But they do have that way of surviving and assuming that the politics of the states allow them to go to increased tuition, they can probably continue to do what they're doing and do it fairly effectively and they'll be all right.

What I really worry about is the next tier of the higher-education system, the universities who don't have enough market draw to be able to pull students from around the country who will pay very high tuition rates, and so I’m not sure what the future holds for those universities. I think it's going to be difficult for them to stay in the top tier of the research institutions because they're not going to be able to hire the best faculty and they're going to have trouble keeping their teaching loads light enough to enable the full research University model. — So you could see a kind of a tiering with the elite privates and the top publics kind of in one tier and the University of the public universities in a second tier where research is deemphasized. That would be a loss to the US Research enterprise. I think that might be the outcome but it's hard to predict. I don't think anybody can really predict but whatever it is it's not likely to be good. For the average high-school graduate of the U.S. who wants to study engineering or any other subject at a top U.S. research university, it's just going to get more expensive.

Hodge: Over the course of your career, what has been the impact of technology directly on education? There's a lot talked about today for distance learning, things like that. Obviously, computers have played a bigger part but what's been your observation?

Gray: I think the real answer is not that much, Of course we use technology more to communicate with classes and a lot of courses are taped, but an awful lot of professors still stand at a blackboard and do their lecture on a blackboard or on PowerPoint, which is sort of equivalent. In other words, the lecture style, the professor delivering 25 lectures over the course of the semester, the students sitting there taking notes, going home, studying the notes, reading the book, doing the homework and then taking a test- that model is still pretty prevalent. Certainly technology has helped, but it could help a lot more and I think this massively online technology that you're kind of referring to has the potential to change the whole model of instruction so that at least in a subset of the courses you don't have the professor sitting up there lecturing. The students look at the material in some form and the professor answers question. You can think of a whole bunch of different models. You now have very sophisticated testing and quizzing tools online that the students can use to test their knowledge, you have crowd sourcing and the ability for the students to talk about things among themselves, ways to really change the model, and those have the potential of giving a better outcome and of course they can't fit everything. There's never going to be any substitute for that small senior-level class where you have the world's most famous professor in X sitting there, telling you why he did something with a small group of students. You'll never replace that. But for the 300 people sitting in a lecture hall for chemistry 1A there might be a way to do that differently so I think it'll have a big impact. I think we're just beginning to see it though. I think in most U.S. universities we really haven't seen dramatic changes yet but I think we will be driven by the cost equation. The cost is really too high, higher ed costs too much, and there or must be ways to use this technology to reduce the cost of higher ed in a way that doesn't diminish the learning experience and doesn't have a negative effect. Most people think well, that's going to diminish the experience. Well, the whole promise of the technology is that it doesn't diminish the experience; it enables people to learn just as effectively but maybe without as much cost involved.

Hodge: You had mentioned earlier your engineers even if they're more on the analog side they need to know more software these days. You've talked about engineers needing more biology, which implies more chemistry. How are you fitting all of this into the curriculum?

Gray: Another great question. This is a debate that goes on every single year between the people who would like to see a generalist approach— arguing that a bachelor's degree in engineering should be someone with a broad background, know some biology, know some of these ancillary technologies, knows a lot about software, etc the generalist, which we don't do today by and large, then across the whole spectrum to those arguing for a specialist. The classic case of that viewpoint is the first-level supervisor of a Silicon Valley chip company who comes to campus, does an interview and says, "I want somebody who can hit the ground running. I want that person to come to my company and I want them to be able to design chips right out of the box." Well, you can't have it both ways and the debate is between those. Often I find the first level manager will say that. But if you ask his CEO, who's been out there for 25 years, the CEO might well say, "Well, you know, we can actually teach them what they need to know about that. What I need is a person who has a breadth of understanding, because these people may be working on something totally different ten or fifteen years from now. They need breadth. They need to know something about the adjacent fields that they're studying rather than just spend their entire senior year taking courses in one specific area." Often the more senior people would ask for more breadth. But the people doing the actual hiring tend to be the first level people. And often they really want relevant, practical expertise to do something right now. And I'm more on the generalist side. But where these decisions get made are in the faculty meetings. The faculty of EECS meets, and they have a debate. The debate never seems to conclude we should reduce the number of required specialized courses. It's nice to talk about it so that you could have more electives, so that you could have more breadth, but our graduates go out there and they compete with graduates from Stanford and MIT and lots of other places. And you want your graduates to be the ones that get picked, you want to produce the output that the market is looking for. It might make more sense is to have a generalist undergraduate degree, and do the specialization as masters degree. I think that's a solution that almost everybody would endorse, but it's a solution that almost— everybody kind of has to do it at the same time, because no one wants to be left at the competitive disadvantage that that might ensue if the other institutions don't do it.

Hodge: Electrical engineering is almost defacto at that point right now, isn't it?

Gray: Defacto at the point of being more generalist, you mean?

Hodge: Being almost a master’s degree required as an entry level.

Gray: That's right, you pretty much have to have a masters degree. The valley companies here really look for MS people. They hire some BS graduates, but not very darn often. They really prefer somebody with a master’s degree most of the time.

Hodge: So skipping to sort of the educational side of things. We've sort of glossed over about 25 years of your career on the technical side. What have been some of the highlights of what you've done in and around the herding of cats, administrative side.

Gray: Well, as I said, when I served in the Dean's Office, I think the thing I have the most memories about is establishing a Department of Bioengineering, which is a difficult thing to do. The academic world is organized in a way to be resistant to external political pressures. They're sort of designed to make change slow. I don't necessarily use the word difficult, just slow. You can kind of understand why. A big part of the value system in higher ed is to have faculty who are experts in what they do. They're the world's authority on what they do. And then you'd like to be able to call on those people, and have them give independent unbiased opinions in matters of public policy in Washington or Sacramento, or in a number of other kinds of venues. And you want them to be independent; you don't want them to be beholding to some political entity or some company or something like that. So it's a long-winded way of saying the institution has a lot of things about it that are there because you don't want things to suddenly be changed by external political influence overnight. So those same things make it difficult to do things like start new departments. But it just became so compellingly obvious that the biological sciences were going to be so important in engineering, and that the worlds of medical devices, genomics, DNA computation world, and the synthetic biology, and so forth, this array of new things were really going to be about engineering. You're going to be engineering new organisms. And we just had to be a part of that. So it took a lot of work to convince the campus administration and our biological science colleagues, who understand basic discovery, but they don't really understand engineering very well. We had a lot of work to do to persuade them that this was the right thing to do, but over time we got that done. We were able to get the Whitaker Foundation, a big charitable foundation to fund the startup of this. And we got—through the work of mostly other people, not me— we got the building built, the QB3 Institute on the Berkeley Campus, Science Institute. And that was completed about 2006,

Hodge: Do UCSF and UC-Berkeley share resources?

Gray: Yes, QB3 is a joint institute across the two campuses. And so we have joint faculty, actually the bioengineering department is a joint department. UCSF and Berkeley are members of that, and we do a lot of joint work. The Institute has two buildings, one on UCSF campus, one on Berkeley. This has been a great thing, and I think we'll have immense long-term payoffs. You know, starting anything new, you have a lot of bumps in the road and so I feel very happy about that. I think the other thing that I think about, when I got to the provost office, we had some great plans, but then I started in 2000, and remember the economic bubble bursting in 2001, and that was painful. That was another one of these dips that never quite came back. And so we spent a lot of time just managing the financial challenges associated with that reduction in state budget. But actually, compared to what went on later in 2008/'09/'10, that 2001-04 problem was relatively minor. One thing we did do during those years between 2000 and 2006 was to create a bunch of interdisciplinary initiatives, which enabled us to bring in faculty and introduce in cross-disciplinary areas, things like the intersection between engineering and the arts. That example, the new media initiative, takes people from the arts and humanities and puts them together with people from computer science and engineering, We also had climate change, environmental, and other kinds of initiatives. Some of those have borne some good fruit.

Hodge: In terms of your electrical engineering research over of, I'll call it the second half of your career—just looking through your papers, you did a lot of, you know, we talked about the codex stuff, the various sample data, the switch cap filters. And then looking at your later papers, there becomes more of high-speed analog to digital, and it looks like more RF stuff. Is that sort of a fair assessment of—?

Gray: That's exactly right. To simplify things a bit, for the first ten years or so you talk about, it was actually really easy to develop a research program - just take whatever they're doing in bipolar, figure out how to do it in CMOS. And then later on, it became more a matter of finding the new applications - new analog-digital interfaces. There was a tremendous need for high-speed A to D's, so there was some high speed A to D implementations that we looked at. And we did a lot of work on RF. Bob Meyer is a real pioneer in that. I think his student, Asad Abidi at UCLA, was probably the most important single person to really drive the CMOS-RF and MOS-RF activity. But we did a lot of work in that that made incremental contributions in various ways.

Hodge: So were cell phones the impetus behind this?

Gray: It started out with WiFi, and WiFi-like standards, although actually, the earliest projects we did were, believe it or not, for a cordless phone. You know, cordless phone, short range, about 100 milliwatt transmit power, relatively straightforward. And we did some experimental chips around 1994/'95 that were for that, or maybe a little later than that. And then WiFi became a big driver. But the difficulty of implementing those RF functions is much lower, those are less challenging from a technical perspective than the cellphone, , because the transmit power is much lower, the bandwidths are higher. The real difficult specs are in the cell phone world. All of us who were working on this were thinking about the cell phone, wristwatch, Dick Tracy cell phone, all integrated. But cell phones are very challenging from a RF performance standpoint. The distances are great. The transmit power is high-depending on which standard, it's on the order of one watt transmit as opposed to 100 milliwatts. The channel spacing can be very narrow in certain standards. The data rates can be high. The modulation, the signal-to- noise ratio required, the noise figure, it's just generally speaking pretty challenging to do cell phone monolithically. But I mean, we're there. That's basically a solved problem, the cell phone radio The Holy Grail nowadays is one chip that does everything, multi-standard. You come up with some sort of a programmable multi-standard RF interface that can do a whole array of different things, and all digitally. So you have a very high-speed A to D, and a very high performance analog interface. And then after that, everything is digital. And you can do multiple standards— that's the direction the R&D part of it is going. The cell phone, per se, even for the latest standards, it's pretty much an integrated chip. You can buy those chips commercially from a number of vendors.

Hodge: Is the analog engineer of the future then basically just going to be working on high speed, high resolution A to D's and D to A's?

Gray: There are always going to be new kinds of interfaces other than just the cellphone— I think of the cell phone as kind of like the modem, in that it's a solved problem to a first approximation. There are always new problems. Super high-speed interfaces in server farms, for example, 10 gigabit Ethernet on copper. There are always new interfaces coming up that will always challenge the analog performance. Having said that, this is why you need an engineer that's more broad., I think of it as an analog/digital interface engineer. You need to know everything there is to know about the signal processing in the analog domain, analog design, A to D conversion. But you've also got to know signal processing, because you're always making this decision about where the optimum place to put the A to D? Do you put it closer to the digital end or the analog end? And that decision usually has a big impact on power, performance, economics, die size, and you have to know all those disciplines. I'm not sure we should be training pure analog guys anymore. At least in the microelectronics world. We’ve got to be training people who know all those domains.

Hodge: So we sort of skipped over— we've mentioned a few of them in passing, but anything you want to say about various people you've worked with throughout your career? I know you mentioned Broderson, Messersmith, Hodges. I don't think we mentioned Brokaw, but I know you worked with him.

Gray: Well, let me start chronologically with that. You know, I just should mention again Don Peterson [ph?], who's really the most formative person I worked with professionally, because he got me up to Berkeley, and he was a great mentor. Dave Hodges, Bob Broderson, and Bob Meyer at Berkeley they were all colleagues of tremendous importance to me. We did a lot of brainstorming— you know, when you have a brainstorming sessions sometimes you can't remember who actually had the idea. These were all things we worked on at great length together. Professionally, yeah, there were all those early pioneers. I mentioned several. Paul Brokaw was one. I worked with him in the sense that we saw each other at conferences, had technical talks on occasion. He visited Berkeley quite a few times. Just a tremendous guy, and of course, he was one of the key inventors of the band gap reference, a particular version that worked really well, and a bunch of other things- D to A convertors and things of that sort. So he's one of those early, early pioneers. I think I mentioned Ted Hoff, Ted was very important for me. And then later on, lots of people. I think I mentioned most of them. Alan Grebene, and a lot of graduate students who were very important. And then later on in my career, I got a chance to work with some great chancellors. Chang Lin-Tien was Chancellor of the Berkeley campus when I became Dean of Engineering there. He was the first Asian-American Chancellor of a UC campus and was an amazing person, a great mentor. One of those people who led by sheer commitment. He was just very popular, very well liked, loved on the campus. And was so committed to the place that, you'd sit down with Chang Lin for ten minutes and you'd suddenly realize that this guy, if there was a war going on outside, and it was about keeping the campus healthy, he'd go out and pick up a gun and start shooting, He was just totally committed. And I really enjoyed working with him. He was a great mentor. Then two subsequent Chancellors, Bob Birgeneau and Bob Berdahl, both were great people to work with. I was their #2 person; I worked four years with Bob Berdahl, and two years with Bob Birgeneau. They were great, great mentors. I think those are the— oh, I should mention Gordon Moore from the Intel days. One of my most memorable times at Intel was when I was down there working on these mixed signal chips. They brought this crazy college professor (me) down to work on these mixed signal-chips for Telecom with Ted Hoff. And you know, nobody else around there could figure out what in the world we were doing. They were all doing memory and microprocessors. So Gordon took an interest. He'd stop by my desk and chat now and then. And then we shipped this thing off, and it came back and it worked. And I sent him an email note, or left a note on his desk, I forget which. And he came over and just was really great. He was, "Oh, this is wonderful. This will mean—," he really saw the same potential that Ted Hoff saw. And so that started a relationship I've had for many years with Gordon. At Cal, he's been a huge supporter of the campus. And done all kinds of great things for the campus. And then I got involved with his foundation five years ago, I'm on the board of his foundation, and we work a lot together on things connected to philanthropy and the foundation. So that's a relationship I value tremendously, along with the Andy Grove one. I worked with Andy quite a bit. He's also got a Cal degree, and so he comes to campus a lot. And he's a real inspiration, but he's about as different a personality from Gordon Moore as you could imagine. But together they made an incredible team. You really see why that company had— they had the management team that the skill sets of the three were so complementary, and it really worked well.

Hodge: The third one there being Robert Noyce.

Gray: Bob Noyce, yeah.

Hodge: And you won the IEEE Robert Noyce Medal. What was that for?

Gray: That’s kind of a career award for technical contribution and leadership, and they give that to people who make some technical contributions and also do some— have some positions like I had, Dean of Engineering and Provost. So that was what that was for. I did meet Bob a few times. I didn't really work with him very much, but you know, he was the visionary leader and statesman. He was extraordinarily articulate and could just communicate so well. He was at his best when he was out talking with customers and government agencies. And then Gordon was the scientist. He was the brilliant engineer. And Andy, of course, was the driver. He was the operating guy that really made things happen. Boy, the three of them really made a great team.

Hodge: So Andy Grove has endowed your professorship, I believe.

Gray: That's right.

Hodge: You're on the Moore Foundation, you won the Noyce Medal, so you're sort of connected to all three.

Gray: Yeah. Well, there's an interesting story about the— about Andy's chair. Andy didn't actually make the endowment. An anonymous employee of his did. This was a gentleman who's chosen to stay anonymous all these years. And again, I was Dean at Cal when he did this. And he just admired Andy so much, and he had a lot of financial success, so he decided to make this gift. But he didn't want Andy to know who did it! Then we sent Andy a letter saying, "This endowed chair has been established in your name." And I remember to this day when he called me up, says, "Wow! This is fantastic! I didn't make a donation or anything for this, but I have an endowed chair in my name, that's a wonderful thing! And I guess I really ought to help you folks out." So he subsequently was very generous. But he also was very, very kind. Subsequent to that, I happened to be named as the holder of that endowed chair, which is done by a confidential independent committee. He and his wife were very gracious. They had us down to dinner a few times. I remember those very, very well, but he's really a special person— well, all three were very special people. So I held that for a few years, and then now it's held by Costas Spanos, who's a former chair of the Department and very deserving chair holder.

Hodge: What would your advice be for a student who wants to— you know, high school age student who wants to start out in engineering, particularly electrical engineering now?

Gray: Well, it's a great field. And I would encourage them to do that. It's a great field. And as you probably know, we are having trouble getting US high school graduates, men and women, but especially women, to elect engineering. You know, the percentage of engineering bachelor degree graduates in the US has dropped fairly precipitously over the past 25 years from the general vicinity of six percent of the total down to just over four percent of the total. So we're losing ground. So we need more high school graduates to select engineering. I would advise trying to wait as long as you can to decide which specific field of engineering you like, and I'd cite my own example. I thought I wanted to be a mechanical engineer until I got into a class that really inspired me in a different part of engineering. I really like the way Stanford does their undergraduate curriculum. At Stanford, you don't even declare engineering, but you sort of have to know you want to be an engineer, because of the first year curriculum, you have to elect— but you don't have to pick which specific kind of engineering until later, until you're a junior. And I really think that's very, very desirable. We don't do that. If I was going to try to advocate for a change at Cal, I would advocate for that. But try to delay as long as possible, the decision about that, because you never know what you're going to find interesting. And you know, the first few course in the freshman and sophomore years can be sometimes dry and intimidating, some of the math and physics for example. Now they're getting better. One thing that is getting better in engineering curricula generally is there's more of a sensitivity to the fact that it takes until you're a senior or a junior before you even understand why are you learning all this stuff? For example, at Berkeley we now have a whole array of what are called "Freshman Seminars." So this is pure interest. Pure interest and motivation. You just learn about, you know, for example the Internet. How the Internet works. Or medical technology. And it's just an introduction to a subject area, so that you are motivated, you sort of understand why you're doing all this work to understand engineering. So most university curricula are better now than they used to be at getting the students to understand more about why they will find engineering a fulfilling career to have. So they're getting better at that. But you do have to hang in there. The mathematical tools that you learn in those early courses and calculus and advanced calculus, and so on and so forth. You need those tools, but the universities could do a lot better job of making you understand why you need those tools when you're learning them, as opposed to two years later, which is kind of the way it had been. But it's a great profession. I'd be a big sales person, especially if it was a female, because we need to get more women in engineering.

Hodge: Was there anything else that I've missed that you'd like to touch on?

Gray: Well, you know, when I talk to graduating seniors, I give them these pieces of advice, like always— one piece of advice is always, "When somebody hands you a problem to solve, and they tell you that you have a set of constraints on the problem that they just hand to you, never just accept those. You have to understand what's behind them, and why they're there. Because a lot of times, they're not real. They're artificial. And a lot of times the real ability to innovate comes from, not ignoring those constraints, but just reinterpreting them, or getting at what's behind them. So you got to make sure and understand the things behind the constraints. Make sure they're not artificial, or that somebody just didn't make them up because they were too lazy to do the more thorough analysis of what's really required." I think that's very important. Along with that goes, "Make sure you understand the adjacent disciplines". If you're a person doing microelectronics, you need to understand some devices; you need to understand some systems aspects of the area you're working in; you need to understand CAD tools. Breadth is very important. Because chances are 20 years from now, you'll be working on something that you haven't any idea what it is today. And you need that breadth. The other thing that's so important now is just the international perspective on everything." Almost every engineering team here in the Valley is working with colleagues in another continent. You know, almost every company I go to there are big screens on the wall, they talk to Asia in the evening, and Europe in the morning, and the teams work together. It's a multi-cultural, intercontinental, international approach to teams and problems. It's just a part of the landscape to a degree now that was just not there 20 or 30 years ago. Just not there at all. And so the cultural appreciation of that, and the ability to see other people's cultural views and perspectives is really important, the internationalization. It's really a different world for engineers. Especially engineers in the high technology part of it. So emphasize that. And the last one I always use is, "Don't start believing your own press releases. You know, when you start doing that, you're in serious trouble." I think those are probably the little pieces of advice I'd give.

Hodge: I have two concluding questions for you. One is based on a magazine article you wrote in 1969, "Gondola azimuth stabilization using a simple lunar tracker." What was that?

Gray: Well, I worked my way through— as I said, I had all these different jobs, and one of the jobs I had for a couple of years was at the Lunar and Planetary Laboratory in Tucson. The U of A is one of the leading astronomical institutions in the world. They do a lot of different things in astronomy. They have a lunar and planetary laboratory, which in those years, was studying the moon and the planets using balloon-borne telescopes. And the thing I worked on was a lunar tracker. You'd launch this balloon, and I went to many balloon launches, which was quite a dramatic thing. You'd have this gondola under the balloon, and the balloon goes up, and the telescope has to be pointed at, in this case, the moon, You need a device that keeps it pointed at the moon since the balloon suspension is moving around. So this was a optical electronic device using photo-multiplier tubes, with a couple of V-shaped slits, so that as the slit moved across the moon, the amount of light would go up and down, depending on the angle. It was an analog controlled system using a bunch of discrete components, op-amps and things like that that we bought from analog devices and companies like that. It drove a torque motor that positioned the gondola. And I worked on that for two years. That was really the first real engineering design job I ever had. I learned an immense amount, and it worked. We finally got some data. These balloons were launched in a location in Texas, and we spent many a night out there. We'd spent all night launching these balloons and then watching the telemetry data come back in the trailer. After waiting till the balloon came down, then drive out and get the it. By then the sun's come up. Fascinating stuff!

Hodge: And for the person watching this or reading this in the future, starting from today with a clean slate, who will have won more big games, Cal or Stanford?

Gray: It's got to be Cal! Got to be Cal! We've got more students. We've got a brand new stadium, so how can we not win? You know, we got a practically new stadium. It's actually a few years newer than the Stanford one, so maybe that'll be the advantage that takes us over the top.

Hodge: And on that extremely optimistic note, we are done. Thank you, Dr. Gray, for your time.

Gray: Okay, thank you. That was a pleasure.

END OF INTERVIEW

© 2013 Computer History Museum