

**William Hewlett**

An Interview Conducted by

Michael McMahon

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INTERVIEW: William Hewlett  
INTERVIEWER: Michael McMahan  
DATE: November 27, 1984  
PLACE: Palo Alto, CA

McMahon: I know that you've talked so many times about you and Dave Packard starting up Hewlett-Packard. I'd like you to talk to me first about your studies. I notice you got a degree at MIT.

Hewlett: I started at Stanford. I didn't want to do engineering. I didn't know what to do, but I liked electricity, it seemed to fit my interest. At that time, Terman was teaching a graduate course called Radio Engineering, and Packard had been an amateur radio operator. Another very close friend of ours, Noel Porter, had also been a ham, [and] took this graduate course. I hadn't had any background so I didn't take it, but I stayed on to a graduate year at Stanford, and I took Terman's course in radio engineering, and that really started the wave, because I found it extremely interesting.

McMahon: What did you study as an undergraduate?

Hewlett: Electrical engineering.

McMahon: So you had not gone into the communications or radio field?

Hewlett: Those are synonymous now. There wasn't any electronics then-- it was called radio engineering. It primarily had to do with communications, sound systems, talking pictures, very small industrial application for geophysical prospecting. So we tend to think now of electronics as a major industry, but it was really a relatively minor industry at that time. The term "electronics" wasn't coined until after the war.

McMahon: Well, the Electronics Magazine evidently coined it in this country in 1930. They had a sense of it meaning the wide-spread application of the radio tube, which I

suspect is a decent definition of electronics.

Hewlett: Europe used to call it “weak-current engineering.”

McMahon: I think you’re right about after the war in terms of use of the definition. Terman, I know, in his letters, was still saying radio in the 1940s. So the graduate course under Terman was the first course you had in electronics. So was it right after that that you went to MIT?

Hewlett: Yes, I spent one year at Stanford and then I went to MIT for a year, and took a graduate year there.

McMahon: Having electronics in mind?

Hewlett: Yes. By now I was definitely into that, and there was a very fine professor there by the name of Ernst A. Guillemin whom I worked with and did my thesis under, and he also gave me a very important understanding of the general field.

McMahon: Didn’t he have a text also at some point?

Hewlett: Yes, he did.

McMahon: Who else did you study under at MIT? Do you remember Bowles? Was he still--

Hewlett: He was here, but I didn’t take any course from him. I just had a course from Harold Edgerton, I think, who eventually became an engineer with Hodkin and Grier<sup>1</sup>, and he perfected that flash photography at that time. A fellow named Ralph D. Bennett, who eventually went to the Bureau of Ships as a senior officer. Gardner, who taught a seminar course-- those are about the only ones I remember.

McMahon: How was the atmosphere at the time -- how did you like MIT? Of course, in some sense Terman going to MIT in the early 1920s was like people on the east coast would going to Europe, and coming back with their training, at the turn of the century. Did you have a sense of that?

Hewlett: No. In this field of radio engineering, Terman was the outstanding person. There was Everitt, who was at Illinois, and there was a book. Morehouse<sup>2</sup> we didn't study, and Albert-- but these were broadly based courses, whereas Gilden's<sup>3</sup> course was very specialized in network synthesis and analysis, which was to build a fundamental foundation under an awful lot of the stuff we eventually did. Then I came back. I had only one job offer, and that was in Chicago at the Jensen Speaker Company. I wasn't enthused about that. Terman said he knew an MD here who was interested in having some equipment built, an electroencephalograph, and he said I could use the laboratories for doing the work, so I worked for a couple of years on that. I left MIT in '36, so I worked there until about '38.

McMahon: And that was in San Francisco?

Hewlett: No that was right here, in the laboratory here.

McMahon: Was the doctor in San Francisco?

Hewlett: Yes.

McMahon: In the American Men of Science it reads as if you worked in San Francisco for those two years.

Hewlett: No.

McMahon: So the radio lab was already sizable and going. Villard told me the interesting story of you climbing the walls to get in.

Hewlett: No, it wasn't to get in. I was interested in rock climbing and the walls there are hewn sandstone, and a couple of us were interested in that, so we used to work routes up the walls. The professors had little parking signs, you see, and we referred to Terman's route, or Skilling's route, whosoever sign you started for.

You could get pretty damn high-- if you'd come off you could've banged yourself up.

McMahon: So that was just practice? Isn't it funny what the memory does? Villard remembered that that was when you would forget your key to the lab. So then you were at Stanford, with this group of graduate students, and it was growing, evidently?

Hewlett: Yes, and I took courses simply because I was interested. I had no idea of getting a degree, and one day Terman said, " You know, I've been looking at your record, and you've been working on an interesting project in this resistance capacity oscillator, and I figure if you write that up as a thesis and pay \$25, you could get an engineer's degree." So I did.

McMahon: How much was Terman involved in getting you to see that as a project? I know the story towards it -- you must have had some sense of that.

Hewlett: Well, we put it together and it was an interesting device-- it worked well-- and I guess we started the business, and said, "Well, maybe there's a market for this thing." I'm sure that Terman gave us the names of people to write to-- we wrote to the academic community. The first one was from the University of Washington, if I remember correctly. I'm sure he gave us suggested mailing lists.

McMahon: Names of people that might be interested in purchasing it, you mean?

Hewlett: Yes.

McMahon: Did you have in mind General Radio at the time, when you were seeing it as a product? Were you aware of who else had competing products?

Hewlett: Oh yes, very much so. It's interesting. When we were starting out, Mel Vodisten<sup>4</sup>, who was really the founder of General Radio, came out, and was really

very helpful. He took the position that competition was healthy for General Radio, and he encouraged us to move ahead. He in my opinion set a very high standard for the measurement business. He was very open. He would take people through the plant, show them what you're doing, and we kind of followed that practice. So in a sense he set a pattern that got propagated in the electronic instrument business. He was a very fine gentleman.

McMahon: But it wasn't difficult for your company to pretty much best him fairly quickly?

Hewlett: They'd been in the business for a long time. So what you do is you start with a small niche, before you've got a better product, and you go on from that. An oscillator is a source of sound, you put it in a loudspeaker and you could hear it. The logical thing then was something to measure these voltages, so Nick brought out a vacuum tube voltmeter. Whereas it wasn't new, he thought it was better than some of the competing models.

McMahon: You brought the voltmeter out after you brought out the oscillator? So you were beginning to think about the product?

Hewlett: Yes. Then we brought out a wave analyzer, and that was a device to take the complex sound signal information and be able to measure each component separately. Again, this was to get back a way I don't remember the exact time, but I think it was about 1935. The principle of negative feedback was developed by an engineer at Bell Labs, a man named Black. This became a very important principle, and Terman was very much interested in that. He taught courses on this thing, this being the oscillator, a form of negative feedback. Also we used a different technique that General Radio had, for developing a wave analyzer. But also it used negative feedback. The voltmeter was stabilized with negative

feedback. So we built our work on a lot of this work that had come out just a few years before that Terman picked up.

McMahon: That's interesting. The professor that is seen so often-- and I'm talking about the professor involved in research also-- so often was the person as opposed to the industry engineer who would know the research going on. I've interviewed Guy Sids<sup>5</sup> recently, and he talked about professors in that way, and he was talking about advisory groups. We were talking about some 1950 advisory groups. He was talking about their role that they seemed to be more up on the research.

Hewlett: Well, it was strictly research that Terman worked on -- it was theory. And these were examples of applications of theory to practical problems.

McMahon: But that's in a sense what you were doing.

Hewlett: That's what we were doing. Terman had given the basis for it in his course, the presentation of negative feedback, and it was the application of these principles that we started with.

McMahon: Did he tend to also see things in that way too? Could he at other times have a product orientation?

Hewlett: No, not particularly.

McMahon: He would encourage someone to think in that way but not do that himself?

Hewlett: I don't think he thought that way. Having said that, you have to realize that he ran the Radio Research laboratory during the war, in which he was managing indeed exactly that-- but that was a subsequent role. He was accomplished. He was put in charge of the project, like a lot of professors, like the Rad Lab. This was a kind of a parallel Rad Lab on a much smaller scale at Harvard rather than MIT. I mean, he was capable of doing it, but he was very interested in theory. I

look upon Terman not necessarily as someone who was doing research himself, but who took and explained other people's research in meaningful terms. He would take a complex idea and put it in simple terms so it could be used in practice.

McMahon: So he's a translator. When you say other people's research-- whether he's read an article, like the fellow at Bell, or whether he's working at the Red Lab, or I guess at the Radio Lab at Stanford?

Hewlett: He had been sick as a boy, and had poor eyesight, and I guess was confined to home or bed for quite a while

McMahon: Yes, he was. He might have been at MIT, had a career at MIT. He was offered an instructorship there

Hewlett: He learned to read and collate information. He was a voracious reader, and I think he had copious notes and he filed them away and said he could retrieve them. He had a very broad understanding of what was going on, and then could take rather obtuse ideas and put them in elegant terms so they could be well understood. That's one reason he made such an excellent teacher.

McMahon: Was he real verbal, in a casual non-teaching situation? Was he able to speak off the cuff about things?

Hewlett: He had no small talk, but he had a very analytical mind. For instance: he was interested in track at one time, and he studied track. He pointed out that, at that time, the Finns were throwing the javelin farther than anyone else. He pointed this out, and I'm not sure he analyzed what their technique was, but he pointed out that they had a new technique and this soon could be learned, but that for the moment the Finns were THE javelin throwers. Well, that shows that he's got a

mind that's analytical. He would look at these things; he could analyze what happened. It did NOT have to be in engineering.

McMahon: Of course, that's almost an engineering process, when you see that stimulated on a computer--

Hewlett: I'm just saying he had an analytic mind, and he didn't just apply it to academic problems, but broad enough to look at track. I'm sure he had other things; that's just one I happen to remember.

McMahon: Did he have other interests outside of engineering? Athletics?

Hewlett: I don't think so. He was pretty single-minded.

McMahon: He worked really hard, I know. I've spent days reading a box of correspondence between Stanford and the RRL when he was at the Radio Research Laboratory. He was in touch with everything, advising, making decisions, for strictly having a big input, and he ran the RRL at the time. How did he find the time to do that? Your statement about no small talk--

Hewlett: He'd talk your leg off. He had a distinctly one-track mind, when it was on a subject. You might divert him, but he'd come back and say, "As I was saying," and he'd jump right back to what he was saying five minutes previous. He clearly thought in a straight line.

McMahon: That must've been real helpful to you young fellows working in the Radio Lab during those days. The story I got was that he would arrive there about mid-afternoon and stay there right up until the early evening. Is that something you remember?

Hewlett: I remember his presence, but I don't remember what his schedule was. Mine was equally uneven. I don't remember the details of it.

McMahon: So, what about your war work? That was a big decision for you to go to Washington.

Hewlett: It was an involuntary decision. I was on commission. I took ROTC at Stanford, Commission of the Ordnance, which was the engineering side of the army at that time. And in mid-1941 I was ordered to a court field in Washington for aviation ordinance. By that time I was pretty well established in electronics and it really didn't make much sense, so Packard met Colonel Colton, who was running the Signal Corp laboratories, and he wrote Colton. As a result of that my assignment was transferred to the Signal Corps Laboratories at Fort Monmouth. I went there, in July 1941, and then in September I was released as an essential employee. The reason that didn't happen before was that we were just a partnership, and the government didn't recognize that a partnership could be like a corporation except under a different title. If we had been a corporation there wouldn't have been any problem. So I came back September 1941, plus or minus. Then of course when the war came I was called to Washington by the officer for whom I'd worked at Signal Corps Laboratories, Colonel O'Connell. I worked for him for most of the war in the office of the Chief Signal Officer. In the last year I worked for General Wharton, who was part of the special staff, for the new products division. I was responsible for new products introduction, which was kind of a staff job.

McMahon: The second trip must have been voluntary.

Hewlett: No!

McMahon: So they removed your employee [status]-- So it was all involuntary! How did Packard know this man?

Hewlett: He'd met him. He'd called him at Signal Corps Laboratories, as a possible

customer.

McMahon: You had done that very early, then, if you were in the garage in '39 and then already in Washington

Hewlett: It wasn't Washington; it was Fort Monmouth, where the Signal Corp Laboratories were.

Hewlett: I spent most of the war in Washington. At the end of the war I was sent to the Philippines, for some new product introductions. At just about the time the Japanese surrendered, I was transferred to an intelligence mission run by K.T. Compton. It was designed to go in and take a very quick look at what the Japanese had been doing. So that was a very interesting experience!

McMahon: Koral Compton designed this as an intelligence mission. What did you do there?

Hewlett: We interviewed all the people we could to find out what science had been going on in Japan. I think partially it was to find out what they had been doing with the atom bomb, but we weren't told that. It was a very interesting experience, you know. We met the Japanese equivalent of Vannevar Bush, and he was in charge of the civilian research and development-- his name was Yagi. All these reflector antennas we use for our television are Yagi antennas. He was a very frustrated man, because the military didn't want him doing anything, and he found himself being used as a propaganda device. For instance, the government announced they were developing death rays, so he had to develop a death ray, although he knew you couldn't do it. He also was responsible for these incendiary balloons that floated across the ocean. But he also told us where all the technical information lay and whom we ought to talk to in the military. So he was really quite helpful. I remember [him?] coming in in striped pants, and a cutaway coat, you know,

McMahon: Were you surprised by what you found in Japan-- what they knew, what they were doing in electronics?

Hewlett: I remember I thought it was pretty primitive. The thing that impressed me was the lack of cooperation between the army and the navy. There was a device on planes called IFF, which is Identification of Friend or Foe, so you could tell by looking at radar whether you were looking at a friendly plane or an enemy plane. The navy IFF did not work with the army IFF, so if the army plane flew over the navy ship they could get shot at. That just shows the degree of competition between these two groups.

McMahon: And in a more vicious way than how the services in America have always competed.

Hewlett: There's a background for that really. The Japanese army was very provincial. They'd been involved in Manchuria, China, and had met second rate forces, and had infinite confidence in what they could do. But the Japanese navy had been around the world, and they had seen things, so they had a much better perspective of what the relative strength of Japan was, so I think there was an intense competition between these two. It was a very fundamental problem. But anyway, then I came back, right after the war.

McMahon: How did that feel, coming back?

Hewlett: Great! It was also very frustrating. I remember going to Washington, and not being used to working twelve hours a day, and a lot of the stuff I did was classified information. I could stay working until 8:00 at night, until they finally said, "Oh no, we have to close the safe, so you can't work after 6:00." It was a shock. Here you were, you were in the middle of a war. You're trying to do

things, and just for convenience's sake you're told that you have to leave at 6:00.

I'm sure my wife was pleased, but...

McMahon: Did you to return your twelve-hour days when you came back?

Hewlett: I guess so.

McMahon: I was wondering, this being away..... Hewlett-Packard was really an infant company. I knew it grew during the war, but you were not there--

Hewlett: I came back two or three times. Dave would stop by in Washington. When I left we had seventeen people, and at the end of the war we had two hundred. Dave hired a very capable engineering manager who worked for Hansen Kaufman here, so he was of tremendous help to Dave during those war years.

McMahon: Do you recall his name?

Hewlett: Sure. Noel Eldridge.

McMahon: He was with the company a good bit, wasn't he?

Hewlett: Yes. We had two Noels with the company, very high-- Noel Eldridge and Noel Porter. Noel Porter I'd known since high school, really. Through college we'd actually talked about going into business together, Hewlett-Packard. But when that time came Porter had a good job and didn't see fit to leave it. So the war came, he wanted to get in the service, and he went to Bowdoin College, I think for training in radar, which is what he was interested in. I didn't know that was his ambition, so I told a mutual friend of ours in the Bureau of Ships that he was a great guy; the motion was discussed, he was hauled out of Bowdoin and put down in the Bureau of Ships. After the war he came up to work for us, so at that point we had two Noels: Eldridge was in charge of sales and marketing and Porter was in charge of manufacturing. Then maybe in 1950 or so we got another fellow

back from Stanford, a fellow by the name of Barney Oliver-- and he was in the paper a couple of days ago for his interest in [?]- he's a very capable and interesting person, and we talked him into coming back. We kind of had a foursome in charge-- Porter in charge of production, Eldridge in charge of marketing, Oliver in charge of research and development, and then we had a fellow here who David picked up who was in charge of finance. Those were the four key people, three of whom were Stanford graduates trained under Terman.

McMahon: Not Oliver?

Hewlett: Yes, Oliver was at Stanford. He was a genius. As something like a sophomore or a junior, he wanted to take Terman's course in radio engineering, and Terman said, "Well, you really don't have the background to take it, but if you're willing to audit it I'll let you take it." So he took the midterm examination and he got the highest test score, so Terman let him continue! Then I guess after he got his bachelor's degree from Stanford he went on and got his doctorate degree from Cal Tech.

McMahon: Did Oliver always have these interests that were, in a sense, intellectual?

Hewlett: Yes.

McMahon: In the mid-sixties I ran across a piece he published in *Spectrum* on rational religion-- it caused a little bit of a controversy!

Hewlett: He retired here. He was very interested in optics. I don't know why, but he was good at it. He got involved in this project at NASA on multiple antennas, and then the astronomy department at Cal decided they wanted to build a much larger lens-- a 400 inch lens instead of a 200 inch lens. The problems go up faster than a cube with the size. If you try to do it conventionally, you're going to have an

awful time. So they decided to try to do it differently, by having a lot of individual mirrors that formed part of a parabola, and they were adjustable so that they'd be exactly right. You had to be accurate to something like a sixteenth of a wavelength. You had to [circle?] these things in position. Barney got interested in that, and got working on that, and as a result after retiring here he was given a position as an associate professor in the astronomy department, which is something that was never done. I'm just showing how catholic his interests are.

McMahon: He came into the company around 1950? So he was an undergraduate in the late forties, after the war.

Hewlett: He was younger than we were. I barely remember him as an undergraduate. I think I knew him as a graduate student. During the war I maintained contact with him-- he was at Bell Labs. Whenever I went to New York I'd always see Barney.

McMahon: He was an undergraduate before the war: you would have known him slightly then, and then he was gone during the war and came back, so he's one of those older graduate students.

Hewlett: Let's see. Barney must be sixty-seven. I'm seventy-one, so I'm four years older than he is. He was a child genius. I don't know what his age was. After he graduated from Stanford, he got his doctorate degree from Cal Tech, and then went to work for Bell Labs, and then we talked him into coming out here in the early fifties. I'm not quite sure when it was, maybe because he'd come from California. His mother was a widow and lived down near Santa Cruz, and we played on all the heartstrings we could.

McMahon: Let me ask you to think about the late '40s and early '50s, when Hewlett-Packard was growing leaps and bounds. I also would like to turn to my interest in what's

happening out there with the university and Hewlett-Packard and with all these other companies. I guess you didn't come onto Stanford land until the 1950s.

Hewlett: There are some things in between. Maybe in 1948 Terman said, "Bill, you ought to run for director at large of the IRE. You spend quite a bit of money advertising your name: people know it!" I ran against Lloyd Berkner, who was a very good scientist, and by gosh he was right, and I won! People knew the name Hewlett, and perhaps they didn't know the name Berkner. I was active in the IRE for a number of years. Terman, when he came back, I guess, was first made head of the EE department. Then he was made Dean. I guess before he left he'd been promoted-- he'd already been executive head of electrical engineering. Dave and I used to go back fairly regularly to New York. We usually traveled by train, because I didn't like air travel, and Terman very often was along. So we had a lot of chance to talk with Terman and see what he thought. It was interesting, because he had very clear ideas about what needed to be done in the field of engineering education. He pointed out that schools of engineering had very much gone into the how-to-do-it side rather than the theoretical side. The best example was that large companies like Bell Labs, GE, and RCA had set up their own schools of advanced study to teach theory. He pointed out that the practice is generated by industry, and theory is generated by the university, but we've got this turned around, and the university was teaching practice and all the industries are teaching theory! He said we should get rid of all these how-to-do-it courses and focus on the fundamental, as engineering science. This does not go over very easily with a department that's pretty traditional, like any university.

McMahon: And the EE department that was in power was hardly research-oriented after

1940.

Hewlett: Well, if you think of things like drafting, and forge, and foundry, and these courses which were nice to have, they were hardly essential to being an engineer. We set about to not make them required courses, and to encourage more theoretical courses.

McMahon: This is right after the war?

Hewlett: Yes. I had a friend who was fairly high up in a civil engineering department, and I knew him pretty well. He said, "You know, when Terman came out, I thought to myself, this is going to be a disaster. [But instead] it was the best thing that ever happened to the civil engineering department." [Unintelligible section]

McMahon: That was here at Stanford-- that was a CE saying that?

Hewlett: As you know, Fred was brought up on the campus. His father was a professor here. He knew exactly how a campus worked. He looked at the engineering department and said we once had a very fine department of aeronautical engineering. We were not in the mainstream anymore. We had two guys, and they were very good, but were in very limited fields, and it's anything but an outstanding school. He said, about rectifying this, that the first thing is to be sure that these professors are treated with dignity-- that they're not just thrown out, that they're given the chance to retire with pay and everything or if they want to stay on that they get the kind of support they need. But let's go out and get someone to run the department. He got financing from the airframe people and set to rebuild the aeronautical engineering department. The thing that impressed me was that he did this by careful planning, and with a complete understanding of how the university worked, and with an ethical way to deal with these people

who'd been on the staff for a long time. Again, he did not let that stand in the way: he wanted to build a department. And I think that that's typical of his modus operandi. Dr. Sterling was a very remarkable guy: he was a historian. He was a very personable person. He played the piano. He was a very charming person-- and an administrator. But Terman was really in charge of running the faculty, and he ran it with an iron hand, he made these guys tow the line. I can't say for sure, but I would not be at all surprised if he was responsible for getting guys like Letterburg and Cornberg here in the chemistry department and various other places. He said that you have to get top people. There's a story I love on Terman: a very good friend of mine-- this was the '50s, I guess-- had been made dean of the business school. He'd come in from the outside-- Ernie Arbuckle. I always took the position that people who are very good researchers didn't necessarily make good teachers, and an article came out and pointed this out, way to the extreme. I showed this point to Arbuckle and I said, "Why don't you bring this up with Terman?" So Arbuckle innocently went to Terman and Terman looked at him and said, "If you believe that, you don't deserve to be dean!" I think the rise of Stanford in the 1950s was really due to a number of factors, not the least of which was the fact that Sterling was running it from an overall standpoint, and Terman was running it from the inside. They also had a board of trustees that was really an outstanding board. I understand one of the old guys just died. Herman Fleterer. He was a fine person. So Fred had a great influence on the university and obviously other places too.

McMahon: When you say Sterling was running it from the outside, you mean donors and potential gifts, like dealing with people that dying may will something to them

twenty years down the line.

Hewlett: That's kind of denigrating what Sterling did. He was superb in dealing with alumni, potential givers, and being the president on the board of trustees. But also, he had Terman there tending the shop at home, who obviously was not on the outside, but who really understood how the university worked and how to make it work.

McMahon: And in so far as he's dealing with outside people, it's really funds coming in, with industrial sources?

Hewlett: Not so much. He was good at that, I guess, but I would say he was committed to excellence in the faculty. He concentrated on getting top people in.

McMahon: I suspect that if he was not looking for money too often in the 1950s, he really got that going in the '40s. If he brought in these good people, they were perhaps able to go out and get that funding for the electronics lab, or whatever. I know in the 1940s he was often there, almost like with you fellows in the late 1930s, suggesting this and that. He's often there with a contact.

Hewlett: Yes, he had contacts, no question about it. Probably more than anyone else he was responsible for the development of the industrial park here. I think he looked at this thing, and he said, "Stanford's got all this land, they're paying taxes on it, the taxes are going up; I think we ought to set up an industrial park with high-technology companies (although he didn't call them that in those days) on the basis that there should be a synergistic relationship between industry and the university." And this is exactly what happened. Then sometime later he said-- typical of Terman, "Tuition pays about half the cost of a student's education. There ought to be some way that these companies around here could get their

young engineers to get a master's degree. We could work an arrangement whereby they go half time, and work half time. But the company ought to pay that difference, so the student would pay the normal tuition and the university will receive an equal amount from the company, so there's no loss to the company." This became known as the Honors Cooperative Program, and I think there were fifty students at one time; it was a very successful program. The eastern universities looked down on this and now they're realizing it is a hell of a deal. Again, just an example of his always thinking about the role of the university and how it could benefit from industrial neighbors.

McMahon: Terman always seems to be on the cutting edge of this kind of thinking. So you're talking about Sybil.

Hewlett: She was interested in lots of other things-- in reading deficiencies, she did a lot of work on that, but in my view she was an intellectual counterpart of Fred. When she died Fred really fell apart. You could almost see him go downhill after she died, because she gave intellectual support at home: she was really a very remarkable person.

McMahon: I read a letter yesterday that she wrote to him during the war when he was trying to get off to England during 1942. She talked about doing all the things she needed to do to make him more comfortable, but then she talked about how she wished that she were doing what he was doing-- like she was missing that opportunity of being in there. That sounds like what you might mean.

Hewlett: I think very often that's the kind of thing that gets missed in a biography, is the role a wife plays or doesn't play, and I think Sybil played a very important role in his life.

McMahon: Do you think as a professional in herself that that helped her to get inside his world?

Hewlett: No, she lived her own life, she was an independent person, and I'm sure he wasn't the easiest person to live with. He was absent-minded, and so on, he'd go off without his coat... but she understood him and I think she was a real support.

McMahon: I've been struck by some of the letters that she's going to be a very interesting person to get into. Did you know the father at all, Louis Terman?

Hewlett: No.

McMahon: Did you have any sense of Terman's relationship with him, would he ever talk about him?

Hewlett: Not to my knowledge.

McMahon: He died in the '40s or right after the war. Let me ask you about this philosophy that's developing: earlier you were talking about practice and theory. I know that in the history of engineering education, those are the dominant themes. But you and Packard are industrial people, and yet you're real open to this-- you like this idea of perhaps taking engineering students from Stanford and teaching them quickly the practice and letting them learn the theory at school. Why concretely was that interesting to you?

Hewlett: Because, what you're doing is employing brains and education. Then you need to mold that to what you're trying to do. So in getting these people, we were getting well-educated people. That was one of the main attractions of this Honors Co-op Program. They might go out to some not-as-well-known university and hire the number one and number two guys, and then be able to have them go to Stanford for a year or two, so they could really get a good return and you get this kind off

topping off of their education.

McMahon: And they probably have been trained a good bit in practice, if you're getting them from those schools.

Hewlett: Well, it wasn't just that: It was that with what you were doing you were able to get the top people out of the class. Not all smart people in the world go to MIT and Cal Tech.

McMahon: Interesting you would say Cal Tech. Is Stanford competitive with Cal Tech? Certainly in this area--

Hewlett: Sure, in the technical areas.

McMahon: But not the University of California so much after the 1930s?

Hewlett: Oh, there's all sorts of competition there. It starts with two women: Mrs. Stanford and Mrs. Hearst. Each had her university. Mrs. Hearst was a schoolteacher, herself. You can almost see the ego of these two gals, each trying to build up her own university, and that has sort of continued in a friendly rivalry, but they certainly look at each other as rivals.

McMahon: So in the '50s when the industrial park gets started: do you still have a close relationship with Terman? You and Packard and he were talking in the '40s about theory and practice and curriculum.

Hewlett: I think we saw less of him then. He came to us with "package deals", like the Honors Co-op Program. Both of us had associations with the university and the board of trustees, and of course we saw him there. We saw him many other places, but it was not as an important role as it had been earlier on. We were kind of heading our own course now.

McMahon: Could you talk about that some to me, about Hewlett-Packard in the 1950s? Part

of this story has to be the story of the electronics industry here in California.

Hewlett: I think that-- we weren't very sure in the early '50s about how the electronic industry was going to do. We got into a couple of things that just didn't turn out. We got into trying to apply electronics to agriculture. Then by the mid-1950s, we'd pretty much decided that if we're going to be successful we'd have to concentrate on the measurement business. So we put blinkers on and said let's forget everything else and concentrate on doing a good job in that. In the winter of '57, we were concerned about the company growing and the fact that we were afraid we'd lose the personal touch we felt was so important in the company. We had a summer place in winter, and about twelve top people trying to talk about where the company was going. Out of this came the concept that what we probably should do is divisionalize; we had 1500 people at that point and we thought it was too big. By dividing up into two or three small units, we might be able to keep that personal touch. I think we were the first people to initiate this program known as Management by Objective. And so we drew up some six objectives-- they were very broad in nature-- and that was the guideline to these people, and it worked very well. I went back and looked at these objectives, and we've only added one additional objective since then, and we've changed some of the wording. We used to be in just the measurement business, and now we're in the electrical engineering business or something like that. But the fundamental things have not changed. These were really the formative years, and after '58 a lot of things happened. We set up divisions, we set up the manufacturing operation abroad, the company went public in the early 1960s, and we made our first acquisition. You see, we were growing from a very small technical company

where only Packard and I owned the stock into a publicly owned company with a different appearance. That's kind of a watershed period. You might say the first twenty years.

McMahon: Has something pretty good been written on that, on that watershed period? That sounds like a really exciting story. So what happened with you in the company, and your own work? I'd always had a feeling, because I've followed you somewhat in the Proceedings of the IRE, and I knew about your presidency in the '50s in the IRE, so I had a sense of your remaining more associated with the technical area.

Hewlett: We both shared the management pretty much-- it got to be a joke. People are like children: when they don't get the answer they want from one person, they move on to the next person, and they very quickly found that independent of each other we came up with the same answer. Dave and I worked together for so long we really felt very much alike. Dave was a fine engineer.

McMahon: I knew that, but I had this picture of him staying here during the war and you going off--

Hewlett: That's true, but he was doing all the engineering too!

McMahon: I guess going from only seventy to 200 people is a big growth, but nonetheless you're still very much working with your people.

Hewlett: It's a formative period, in a sense: we settled a lot of policies involved.

McMahon: So what does this late '50s watershed do? You're basically a manager in the '50s, right? Does that distance you more, or --

Hewlett: No, I followed engineering pretty closely. We had a good staff.

McMahon: Oliver was Vice-President of Research by then. Does he ever become president

of the company?

Hewlett: No. He doesn't have the slightest interest in managing anything. Don't put this in the record. When he was on the board he didn't do anything, because he just would think about problems and not pay attention-- he just wasn't interested in that.

McMahon: Why wouldn't you want that put in the record? It seems like a great way of seeing the inside of Oliver.

Hewlett: I don't think Barney would like that.

McMahon: But he certainly does see himself as an R&D man?

Hewlett: Absolutely. He's just superb that way. One of the divisions painted itself into a corner with a product and he just invented them out of it.

McMahon: One of the things that made me ask that question about you and your own relationship to the company in the '50s is that the Professional Group System of the IRE is very important in those late 40s and early 50s.

Hewlett: It was just coming in then.

McMahon: It was formed in 1948, but in 1950 the number of groups really just grows tremendously and then those early groups are computer and all that and then by the '60s modern electronics really dominates. But you wrote an article on that, I mean you were interested in that as a structure and a way of dealing with things, and I was wondering if that was reflecting your position in the company.

Hewlett: You know more about me than I do! I forgot that.

McMahon: Well, obviously that's an important chapter in the development of the literature.

Hewlett: You know, up until recently there were three western presidents. First there was Terman, then myself, and then Oliver, and Oliver was there when the IRE and the

AIEE combined together.

McMahon: Right. He was the second president, perhaps.

Hewlett: I think he was the last president of the IRE.

McMahon: And was again president of the IEEE in the '60s.

Hewlett: Yes, I think it was right at the formative period. But you can look that up.

McMahon: I was just wondering if you were also starting to think about organization and structure and even starting to write on it some, or speak on it, about Hewlett-Packard, and thinking about this. How did the management objectives come about? Who was thinking in that direction?

Hewlett: There were twelve of us; Dave probably had the basic objectives, and we modified them.

McMahon: Now that term comes from outside, Management Objective, right?

Hewlett: That's a much later term. We never heard of that before.

McMahon: Yes, I've been through an objectives course at one time with company.

Hewlett: It's not looked on with favor in all places, but it works for us.

McMahon: What term did you use then, do you remember?

Hewlett: No, we just basically said that we believed that if managers know what kinds of decisions are wanted, they are best able to make those decisions from their level rather than from above. So what you need to do is give them some guidelines of what's expected, and let them run with the ball.

McMahon: So the major objectives that are laid out are really the company's objectives?

Hewlett: Yes, they are. They're regarding profit, responsibility of employees, responsibility to customers; I can give you a copy.

McMahon: So then the person running some kind of book organization within the company

would develop his own objectives within that framework.

Hewlett: We encouraged this initiative at all levels. Even at the beginning we encouraged employees who had an idea about how to do something to go ahead and try it out. This was not in a sense a brand new idea; it was more a formalization of a lot that had been going on. When you formally set up different divisions they are geographically separated, and you need something like that.

McMahon: Was that similar to what was happening in the large old corporations? You know, the idea of decentralizing? I guess Ford did that in the '40s right after the war-- were you aware of those kinds of things? Peter Drucker wrote a book published in the early '40s and Henry Ford Jr. read it, and I think Dupont had perhaps moved first in that in the '20s.

Hewlett: I don't know.

McMahon: Well let me ask you then about what's happening in the Valley here. How aware are you and Packard of these other companies beginning? That you're becoming a part of a cluster? I know that there were other industries: Heintz and Kaufman et cetera, but --

Hewlett: Well, let me go back and give you a quick history. I'm sure you've had it, but let me get it all together. When the Stanford's founded the university, they got David Starr Jordan to come out, and he came from Cornell. He was a scientist, an [?]. He selected some of the key faculty: other very good scientists and engineers. He had a very good school of engineering. One of the early graduates was an Australian by the name of Cyril Elwell, who was from [?]. That time, transoceanic radio was just coming about, and it wasn't very efficient. But he heard about a fellow in Denmark, named Poulson, who had a much better method

of generating these high-powered radio signals. So he went over and basically committed to buy these things, and he didn't have a nickel. Then he came back and raised the funds in San Francisco. He started the old Federal Radio. There was a whole group of people in early Federal Radio, many of whom were from Stanford, who were very good engineers. The Federal was finally sold to Marconi interests, and moved east, and some of these guys moved east and then came back west again. One of them is Charlie Litton, and of course Litton Engineering indirectly came from that. But there was a whole lot of other ones. So there was in the 1930s a residue of these people, and they were helpful. Litton was very helpful to us. He had peculiar work habits. He got up at 6:00 in the evening, had breakfast, worked until about midnight, had lunch, worked until about 5:00 in the morning, had dinner, and then went to bed. But he had an engraving machine, engraved characters, and we'd go up every night and engrave the dials for our oscillators there. He had interesting seminars, and people like Alex Poniatoff there-- he's the guy who started Ampex-- and had a whole series of people like that.

McMahon: How does Poniatoff come into this picture?

Hewlett: He's a Russian immigrant-- a very smart guy. It was right after the war. The ideas really came from the Germans, tape-recording, and he picked them up. Anyway, this is before the war. So there was quite an intellectual engineering community here, that really had its roots in Stanford, and you might say a supernova exploded and left lots of pieces around, and these people were helpful to us in many ways. Some of these other people would act as consultants or something like that. That kind of represents the background. Then you had

Heintz and Kaufman. Heintz and Kaufman was very important in the early days of radio, because they had the first ground-air radio. One of the great, dramatic things was the Dole flights to Hawaii. They had a radio on these things, and it had a Windram generator on it, and you could hear the whine of the generator. But they didn't have any navigation equipment, so on one of these planes with half of the world listening in, this guy says, "McCollach, I'm not quite sure where I'm going", and all of a sudden they heard this pitch increase sharper and sharper. Then it stopped, and they realized he'd gone into a dive, not knowing, and this was heard the world over. But that was Heintz and Kaufman. And [?] came from Heintz and Kaufman.

So when we got here, there were a few small companies-- Heintz and Kaufman, a small company called John Kaar. But nothing very large. We didn't have much company here. Litton was making glass-blowing lays. And then the Varians started, really from the physics department at Stanford, and the invention of the Klystron: that was moved east when they were basically sponsored by Sperry, and then they came back. Then Deckman, I believe, decided that the semiconductor industry was a promising industry, and talked Shockley into coming out here, and set out Shockley Laboratories. Shockley was a genius, but very difficult. He attracted a bunch of star people, but he wasn't any manager, and this thing quickly fell to pieces. They were the seed that started the semiconductor business here. This was not of any great interest to us. We didn't perhaps get into that as quickly as we should have, but nonetheless it began to grow around us. Then at some point we decided we needed to get into this business [specialized semiconductors], so we put together a program called HP Associates. This was a

separate company in which we gave some of these top scientists out here an equity position, and had an agreed-upon formula for buying them out. That was very successful, we actually bought a HPA out; one of the guys was Jack Melcore, who is now one of the principle venture capitalists here. Another guy was John Atah, who came from Bell Labs and was an Egyptian, who has been very successful. That's how we got into the semiconductor business; our objective was not to be selling semiconductor products, but using specialized semiconductor products to make our own products better. That's a different twist, and we followed that. We now have a lot of our own proprietary products and make them in several spots, which is quite unusual. Usually you make them in one spot. We have a small semiconductor operation we sell to outside-- but the bulk of them are really for our own use.

McMahon: Well that's it, because you were such a young company, fifteen years, I guess, and then the semiconductors were coming in. It might have been harder for some of the older companies, which were so into tubes, to react to them.

Hewlett: I don't know when HPA was formed--

McMahon: I'm thinking '57?

Hewlett: It's got to be after we went public.

McMahon: So even after '57.

Hewlett: We had a bunch of companies we spawned, mainly because we didn't want to dilute our own interest. We would start these captive companies and let our employees share in them. Then when we went public we had to buy them all off because there were conflicts of interest...

McMahon: You must have sensed the real difference in those years, the 1950s, I think.

Hewlett: Well, we had an HP Associates in '61. I'll work backwards. It must have occurred in '60.

McMahon: What did you do about semiconductors before then?

Hewlett: We didn't use them very much here. [...] with the establishment of HP Associates. So that was '60. We bought them, but we made some specialized ones, when we had to. We made a little diode for our voltmeter, we bought that from Sylvania, and then one day Sylvania said, "We can't meet your requirements anymore. Sorry." So here we were with a complete product line, and no products. Bell Labs had come up with a technique for making these things, so we took one of our senior engineers, a very good "how to do it" guy, and in a matter of weeks he put us in that business.

McMahon: And that was the HP Associates?

Hewlett: No, this was before HP Associates. So we had a background for making our own products.

McMahon: You don't remember any kind of problem because you were working with tubes, and resistance to this new product....

Hewlett: There was nothing there.

McMahon: This was a pattern in the old tube departments, at places like GE, they really fought-- that's why I ask you--

Hewlett: I know the first product to have it but I can't remember exactly what it was. The transistor was kind of an inexpensive, not too reliable device, and we tended to design it way up on the design characteristics and everything. We just didn't trust it. As a matter of fact, when we brought out our desktop calculator, maybe in 1963, we chose not to go to a solid-state memory, because we didn't think it

would work. We went to a different type of memory. That was a conscious decision.

McMahon: I see; you're making that fully aware of what's going on in 1963.

Hewlett: I remember I made that decision. I said, "We don't know enough about it, we know this will work, just go ahead and do it this way." But that was about the end, and then I think the tide swept over us.

McMahon: I'm thinking also of the miniaturization -- where is your funding coming from in the late forties and early fifties?

Hewlett: Earnings

McMahon: All right, but I mean, your clients is what I meant really. Your instruments are being sold.

Hewlett: They're basically being sold to people who are making measurements, in three areas. Research and development, manufacturing, and maintenance. We got into the computer business because our customers were asking us to control our instruments remotely, and to reduce the data and put it in a form they wanted it, not in the way that the instrument gave it. I remember telling these guys "Look, we're not in the computer business: we're in the data reduction business." We very carefully stuck to that area. Now that had a secondary advantage that we hadn't appreciated at the time: our instruments had to pass Class B specs for shock, vibration, and so on, and we made them that way because we wanted them to work well. So it was natural to make the computers pass those standards. Well, most of the computers at that time were "hothouse devices", and suddenly we found our computers out on Texas Towers, which was a very hostile environment, simply because they were more reliable. And it wasn't very long

after that that some of our guys discovered that their reliability was a great asset in time-sharing work. So we had our own time-sharing unit set up, and some customers also bought them for that purpose because they had this reliability, very high up time.

McMahon: Shockley wasn't in California?

Hewlett: His father taught here. I don't think he was a Californian.

McMahon: And it was that atmosphere- this beautiful country.

Hewlett: I know that the Stanford Park had already been set up at that time. The first group to come in was Eastman Kodak, and I think we were second. Maybe Varian-- I don't know if we came first or Varian came first.

McMahon: This is a whole complex story, and Terman is certainly an important part of this, but he is not THE story.

Hewlett: Not the sole story.

McMahon: But he is necessary to it.

Hewlett: If you're interested in the whole development of the semiconductor business you'd better talk to people like Bob Noyes and so on. Because he is one of the key people in the industry starting here.

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<sup>1</sup> Probably Germeshausen and Grier  
<sup>2</sup> Probably J.H. Morecraft, *Principles of Radio Communication*  
<sup>3</sup> Probably Guillemin  
<sup>4</sup> Probably Melville Eastham  
<sup>5</sup> Guy Suits