An Interview with

BERNARD MORE OLIVER

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Conducted by Arthur L. Norberg

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Hewlett-Packard Company (Palo Alto, CA)

Charles Babbage Institute
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Abstract

The interview covers Oliver's early life, education, and work experiences at Bell Laboratories and Hewlett-Packard. Oliver began his formal education at California Institute of Technology at the age of fifteen and transferred to Stanford University for his junior and senior years to study electrical engineering with Frederick Terman. There he became associated with William Hewlett and David Packard. After receiving his degree in 1935 he returned to California Institute of Technology for graduate work, from which he joined Bell Laboratories in 1939. His initial assignment there was in the television research group under Axel Hansen. During World War II he worked at Bell on radar. Later he continued his work in television technology and worked with Claude Shannon on information theory. He discusses the organizational climate and objectives at Bell Laboratories in the 1940s and compares it to Hewlett-Packard, which he joined in 1950 as Director of Research. Many aspects of Hewlett-Packard are discussed: vertical integration, distribution of projects, company structure, competitors, associations with Stanford University, military contracts, and recruiting issues. Oliver concludes the interview by discussing his associations with William Hewlett, David Packard, and others at Hewlett-Packard Corporation.

BERNARD MORE OLIVER INTERVIEW

DATE: 9 August 1985 INTERVIEWER: Arthur L. Norberg

LOCATION: Hewlett-Packard Company (Palo Alto, CA)

NORBERG: I'd like to ask you a few questions about your parents and your early education through the CalTech years and so on before we go on to Bell Labs.

OLIVER: O.K., fine.

NORBERG: I know from the usual sources that your father was William H. Oliver and your mother was Margaret E. (More) Oliver, but I don't...

OLIVER: My father was a civil engineer; he went to the University of California in the class of 1905. Following that, he practiced civil engineering in Santa Cruz county, and for the latter decades of his life was a principal in the County Surveyor's Office. My mother, who met my father at UC-Berkeley, was one of the early women graduates of that institution. A history major, she graduated in 1905 and went back and got a master's degree in 1907. I'm not clear on whether she had a year of teaching in between or not. I believe that she didn't, but she may have. She left UC and taught for several years at Sonoma High School, where she was kind of a liberal bombshell, I guess, in those days. Berkeley had its same sort of reputation that it has now. She organized a men's basketball team and all kinds of things there; probably set the town on its ear. She was there for many years, and then I think she went down to Santa Ana for some reason and taught in the Santa Ana High School and then married my father in 1915, which interrupted her career.

I came along in 1916, and in 1920, following World War I, she was drafted back into teaching, this time in elementary schools. There was a shortage of elementary school teachers in the still pioneer west, in those years, and she took over a one-room school house in Aptos, and, having nothing better to do with me, she took me along at the age of four with the understanding that if I did all right, I could go ahead, but if I didn't I could just wait in first grade until I did all right. So I did all right. And that put me two years ahead of my peers. Then the school got divided into two classrooms; she taking the upper four grades, and the other teacher taking the lower four. And that teacher advanced me another grade. I don't know whether she felt it was politically the thing to do or whether she thought I really deserved it, but anyway I was advanced to fourth grade, so that put me three years ahead. That was both a curse and a blessing all through high school and college. I was a social misfit, because I hadn't grown up at the natural rate that kids do. I mean, I hadn't reached the stage of development my classmates had, and I guess that I felt that in many ways.

Also, of course, I lived on a ranch alone. Our farm was about a mile and a third up the old San Jose road from Soquel where there were no other neighbors that had young kids my age. So I, during those formative years, learned to play by myself. I invented games; I did all kinds of things, or just simply sat and thought. So I became, I guess, perhaps more ruminative than most children are; they're mostly shouting and playing social games. I was forced to be alone and adapted to it. I graduated from grammar school, let's see, I guess, I would be six plus eight is fourteen minus three is eleven, at the age of eleven and then I went to CalTech for two years...

NORBERG: When they still had a high school associated with it?

OLIVER: No, I went to CalTech as a...

NORBERG: At 11? Age 11?

OLIVER: No, no, I'm sorry. I'm skipping high school. I went to Santa Cruz High School at 11. I was graduated from there at 15 and then went to CalTech for two years when I was fifteen. I spent the first two years there and then I decided that rather than going into physics, I was going to take radio engineering. So I transferred to Stanford, where Terman was holding forth in those days...

NORBERG: Can we go back for just a few minutes, please?

OLIVER: Sure.

NORBERG: I don't want to rush quite that far ahead because I noticed you talked only about what your mother did and didn't say any more about your father in the meantime. Was he stationed somewhere down here on the peninsula?

OLIVER: No, no. We lived in Santa Cruz. Our home was originally the Oliver Ranch on the old San Jose road as I say about a mile and a third from Soquel. It was purchased by my father's father, my paternal grandfather, in about 1858 and has been in the family ever since, so it's... I think it's the longest tenancy in a single ownership of any property in Santa Cruz county at this time. It was divided between my father and his half sister when my grandmother died. So we ended up with about 75 acres of it with all the improvements on it. And since then, I bought the property of Ben Walker north of there and it's now 165 acres, one piece again, but I don't have some of the original land. What my father did. Well, he was, as I say, a civil engineer. His main work was in highway layout, highway construction, and in bridge design and in some design of sewer plants for the city of Santa Cruz and Capitola. And was just active in those areas.

NORBERG: I see. Now I understand. And you said to me that you went to Santa Cruz High School. Correct?

OLIVER: Yes. Right.

NORBERG: Fine. Now, how did you come to choose physics as a potential major when you were deciding to go to Cal Tech?

OLIVER: Well, it wasn't really... I shouldn't have said physics, I just chose science in general. I was interested in astronomy early on and read a lot about it and then that got me into other books on science and I showed I think a predilection for physics rather than chemistry. I wasn't very much interested in biology or the life sciences, I leaned toward the hard science end of things. But then I discovered that engineering could be fun and I, through an interest in music, had built hi-fi, what I called a hi-fi amplifier in those days, a speaker system, it wasn't very good by present standards, but it was my interest.

At Santa Cruz High, I arranged to have my physical education class be the last one on my schedule. I wasn't much interested in it, although I did go out for football one year. But, most of the time, I would simply cut phys-ed after roll call. I'd go down to the new Santa Cruz theater. Talking pictures were just coming in in those days and I was fascinated by them. I got to know the projectionist there and he was glad to see me because I ran the projector in high school and knew how to run his projector, so he'd go out for a cigarette and leave me in charge. After a few weeks, he'd give me all the records that they had been playing at intermissions, because by then he had a new stack. So I picked up hundreds of records that way. I was interested in sound reproduction very much and that took me into radio and ham radio. I got interested in that whole area, and decided to go into what is now called electronics.

NORBERG: Was there a substantial amount of ham radio activity going on down in the Santa Cruz area at that time?

OLIVER: Yes, there was a fair amount. I knew several people who had "tickets," and actually my physics instructor at Santa Cruz High was more of a ham than a physics instructor. What happened there, he would take the roll call and then he'd have a film or something from General Electric and he'd turn the projector over to me and have me show it to the class and that was our physics lesson for the day.

NORBERG: That's all very amusing. Had you chores to do on the ranch after school?

OLIVER: Yes, oh, yes. I was charged with bringing in wood and kindling, we had a wood stove in those days, and fireplace wood, and with assembling the cream separator and a few other odds and ends like getting the cattle in off the hills. I'd round them up every night. So those were my chores. I got the situation ready for my dad to do the milking. I couldn't do the milking. What would happen is that after a few days of milking, and from there on for weeks, my arms would go completely numb at night. This is a disease known to the Swiss, who have a name for it, I forgotten what it is. But anyway, the development of the finger muscles up here shuts off the circulation. Your artery layout is such they just cut it off and your hands go to sleep. Otherwise, I would have been doing the milking. I did feed the cows, that's another chore.

NORBERG: How about working a lot with machinery on the ranch?

OLIVER: Yes, I repaired various things, vacuum cleaners, motors of one sort or another that got out of whack, and I also overhauled autos. We had an old Model T Ford that I completely rebuilt and I made a rack that came out the rear end of the barn that held the cars up so I could get down under them and work on them, you know, without crawling under them. I wired the barn at the house, I mean at the ranch, also wired up the tank house.

NORBERG: This was before you left for CalTech?

OLIVER: Yes. In fact, some of that was in my early years in high school. I knew a kid in Aptos grammar school, Ferren Cathey who also had scientific interests, but he had no family backing and never went to college, but he was kind of a buddy of mine and we read Hugo Gernsbach publications and all kinds of stuff like that. As a matter of fact, I would say that Hugo Gernsbach played a very important role in my formative years. My dad brought home a copy of *Science and Invention*, which was one of his magazines in the '20s and I just read it from cover to cover several times. You know, I just found it absolutely fascinating. I couldn't wait for the next one. And that was one of the things that moved me in that direction. I was also an early science fiction fan, because *Amazing Stories* came out I think it was in 1927 and I was one of the first subscribers, or would have been if I'd had any money!

NORBERG: Do you remember any more things beside the Gernsbach book that you might have looked at at that time?

OLIVER: I was given a two-volume edition of an English book about astronomy called *Splendor of the Heavens* and I liked that very much, read through that several times. And then I also had some books that were popularizations of scientists and science. There was a book on Edison. There was a book on chemistry and the first discovery of the fact that organic chemicals could be made other ways than through living forms, the synthesis of urea, for example, and other things like that. So there was a general interest in science developing in me before I was even a teenager.

I remember, at the age of about four and a half, looking at the stars through a telescope that a guy in Santa Cruz had -- it was in the wintertime -- and being fascinated with that. And then my dad used to set his transit up and we'd look at the moon and the satellites of Jupiter.

NORBERG: Yes. I didn't think about that. Do you recall what your reaction was when you arrived at CalTech at age fifteen?

OLIVER: I was scared. I went down and I had not applied in time, or I had applied only in time for the last test that was to be given, and I had spent the early fall weeks going back to high school as a post-graduate, though not officially so, and boning up on everything that I should have had. That's when I discovered I didn't have a very good physics teacher, because there was a lot I had to make up in that department. But I went down there and I apparently succeeded well enough to get in there, get in the freshman class, so that was great.

NORBERG: What sort of courses did you take?

OLIVER: The standard curriculum, which was mathematics, a course in mathematics, this happened to be analytic geometry and calculus. Freshman physics, freshman chemistry, and then we had an english and history requirement.

NORBERG: Do you remember the professors at all?

OLIVER: Some of them. I remember an economics professor named Gilbert; Graham Lang was an economics professor that I also knew. He was more of a liberal stripe. The professors, let's see... The physics professor I had was a man named Crowley, but he was not a professor, he was a teaching assistant, which I later became at CalTech when I went back as a graduate student. And in chemistry, we had a guy with a Southern accent that was always talking about "ahrn ahns" (iron ions) and I can't remember his name now, he was quite a laugh. And mathematics, again, I think we just had a teaching assistant. I wasn't in one of the honors sections, you see, my preparation was very poor, and I really, I guess, barely got in. Section F was I in? That's where I was.

NORBERG: I refuse to comment.

OLIVER: Yes. Well, anyway, you know, I felt obliged to go out for football in that freshman year and I did and I got my numeral all right, but it sure took a lot of energy out of me. I remember being very tired and having to work like hell to keep afloat. It was really an intense period of study but I've never had a more rewarding year in all my life. To suddenly be caught up in the things I should have been taught in high school and also being in a college where the standards were high was very stimulating.

NORBERG: Can you be a little bit more detailed about that?

OLIVER: Well, we had a very good course in mechanics, in physics. So I had a graphic realization of the forces at play when any mechanical situation took place, like jacking up a car, or pulling somebody out of the ruts or something. Suddenly it all began to be clear to me what was involved, and it was very rewarding to feel that, to feel I understood it and knew it. I knew accelerated motion backwards and forwards and orbital mechanics and all that sort of thing. The whole world was beginning to make a lot of sense. And also I found differential and integral calculus absolutely endearing. I thought they were tremendous steps in man's thinking. I guess I'm really right about that. They were.

NORBERG: They were. Well, given that sort of excitement and reaction, I'm a little surprised with the change to electrical engineering, given the type of electrical engineering courses that were being taught at that time. It would seem they wouldn't be quite as exciting as the physics in the period.

OLIVER: Well, I took physics courses later on when I went back to CalTech as a graduate student, and I enjoyed them. But I enjoyed, I think, the advanced engineering courses more. The true sciences seemed to me to be kind of in the doldrums in those days. Professors didn't receive much pay, there wasn't any government support. There were only a couple of particles, you know, electrons and protons, and nobody had even discovered the neutron. Classical physics was just being displaced by quantum mechanics at that time and nobody was very sure of the quantum mechanics, because it was kind of in a transition state and I didn't enjoy it very much.

NORBERG: Now, let's return to the trip to Stanford. Why did you return to this area and go to Stanford in your junior and senior year?

OLIVER: Well, primarily I was attracted by Terman. I mean, he put out the bible on radio engineering and I wanted to enroll in his classes. There was some confusion about whether I could or not, because I didn't have some of the prerequisites. I went and asked if I could go into a certain radio engineering class and he said, "Well, you don't have any of the prerequisites." And I said, "Well, maybe I could try." And I guess it was about the first day or two of the class I said, "What is this e^{iwwt}, what does that mean?" He kind of cast his gaze heavenward and gave me a reference book to read on that subject, which I got out and read assiduously. As a result I got an "A" in the course. In other words, he let me stay and I hacked it. That's where I met Bill [Hewlett] and Dave [Packard], actually. They were class of '34 and I was class of '35, but I was screwed up in my schedule so I was in that class with them.

NORBERG: So you would have arrived at Stanford then in 1933, the fall of '33. As I recall, Fred's book was '32, *Radio Engineering*. How did you come into contact with it?

OLIVER: It was in the library.

NORBERG: And one of a very few on the subject at the time.

OLIVER: Yes. I had an earlier book by Moorhead or something like that and Sterling and some of those very early texts, but they weren't as satisfactory. And he was a good teacher.

NORBERG: Who else was there besides Terman that you recall?

OLIVER: At Stanford?

NORBERG: Yes.

OLIVER: I can remember most of the staff there, I think. Brown was there and Bill Hoover was there. Hugh Skilling was there. I'm sure I'm leaving out many names that I shouldn't, but those names come to mind. In mechanics, well, I took hydraulics from a man named Moser and I took thermodynamics from I forget his name now, but it was sure hell. But it was one of the courses I got an "A" in.

I had a fluke situation develop at Stanford. Because of my transferring there and not getting my schedule all ironed out, didn't get into upper division, until the winter quarter of my senior year. And I came down with the German measles in the spring of that year. While I was confined I was notified that I had made Phi Beta Kappa. I couldn't understand it, because my grades haven't been all that good consistently. But it turned out that the rules of Phi Beta

Kappa were that your admission depended upon your grade point average after being put in upper division. And I had just been put in upper division and I happened to go straight "A" that quarter. So, I always felt a little bit of an imposter in that prestigious society.

NORBERG: I think that's too modest, actually. Did you go to the physics department at all?

OLIVER: At Stanford?

NORBERG: Yes.

OLIVER: I took some courses there. I remember Kirkpatrick, for example, as one of my instructors. Oh, my memory is getting rather poor, I did take several physics courses, several math courses, too.

NORBERG: I was looking to see whether you encountered people like Webster and...

OLIVER: Ah, yes. I knew Webster. I don't know... I think I did have a course from Webster, and Kirkpatrick, and Uspinsky -- I took a course from him on probability.

NORBERG: That might have been too early for some of the graduate students like Hansen.

OLIVER: I knew Hansen later, but I didn't know him as an undergraduate.

NORBERG: Now, when you got your degree there in '35, it was quite typical for electrical engineering people to stay for a fifth year, but I notice you did not. You went off to CalTech.

OLIVER: I went back to CalTech and I felt that the Stanford experience had been valuable, but I felt I was missing still some scientific support for it. I really began to think that I needed more math, more physics, and a more scientific orientation to undergird it. For example, I had not had a good course in electricity and magnetism. Back at CalTech in my fifth year, I was given a course in E and M that was a whopper. It was a good course. And then in my seventh or eighth year, I took E and M once again under Smyth, which was a rip- roaring course. We proofread his book for him.

NORBERG: How did you come to that view that you needed better under-girding in a scientific way for engineering?

OLIVER: Because some of the things that we did in the engineering school were too formalistic. I mean you just inserted numbers into formulas whose origin wasn't necessarily clearly supplied to you. And I felt a lack there. I felt that I was simply parroting things that were supposed to be learned. Their orientation was much less scientific in those days then it is now. You know, the engineering science, quote, is a term used, but what that means is really engineering with a full explanation.

NORBERG: That I appreciate, but what I'm trying to get at is how you adopted this sophisticated view. Now, Terman has had such a view, as well, it seems to me anyway from looking at some of his materials. Is it possible that he may have passed this on to people like you?

OLIVER: It's quite possible. I don't recall now whether I discussed the matter with him; I probably did and he may have well suggested I go back to CalTech when I mentioned some of my feelings. He could have said to me, "That would be a good place to go." I applied back there and was accepted into the graduate school. The reason I was able to do this and go on rather than going immediately to work was because both my mother and father had secure positions, you see. They were both, not civil servants exactly, but a schoolteacher's position was very secure; she had tenure. My father was a civil servant, I guess, and had long tenure at the Surveyor's Office. So they worked right through the Depression with no interruption. Their earnings, combined with what I made as a teaching assistant, made life very possible.

NORBERG: Having returned to CalTech, was that in engineering?

OLIVER: I took a major in electrical engineering and a minor in physics. I would say that both were good for me. I had a hell of a time with some of the physics courses.

NORBERG: By that time physics had improved a considerable amount, especially at CalTech.

OLIVER: Yes. The physics seminars were very nice, and so were some of the astrophysics seminars. The calendar there was full of very interesting events. You know we had men like de Sitter, Einstein, whatever, to listen to. They would visit there. I remember talking to Einstein on one on his first visits. I guess that was while I was an undergraduate.

NORBERG: He came there in '31.

OLIVER: Yes.

NORBERG: So that was a possibility all right. Who did you take physics courses from?

OLIVER: Watson. Where is that book I have here... He was, as far as I was concerned, a very good professor.

NORBERG: Houston?

OLIVER: Yes. Bill Houston. I don't think he's alive anymore. I think he died. He went to Rice Institute from CalTech and was a professor there for many years. In the EE department, I think there was a rather weak situation. Royal Sorenson was a pretty good instructor, but he was a kind of an old-type engineer. I took a course in dielectrics from him. Maxstadt (?) was kind of a lab instructor, assistant professor there that managed the setups for the lab courses, and so on, but he was mostly concerned with DC machinery and some AC machinery. McKuen, whose nominal role was in radio and communications subjects, divided his time between CalTech and practicing patent law. So he really wasn't around as much as he should have been. The star instructor of the group was Fred Lindvall (?). Fred ended up as head of that division. We all thought he was great. In fact, I still do.

NORBERG: What sort of research problems did you work on?

OLIVER: I helped Ed Doll on his thesis and I did my own, which was on the measurement of dielectric loss at high frequencies. High frequencies then being something in the order of upwards of 100 megaHertz. Dielectrics were just beginning to be characterized up in that frequency range. It sounds silly now, when we're up to hundreds of thousands of megaHertz. That just shows how rapid the progress has been. I developed a method for measuring that loss that is quite sensitive. I could measure the dielectric loss of fused quartz, polystyrene, things that are very good dielectrics. So it was felt to be a fairly sensitive method in those days.

I helped Ed Doll in his thesis, which was a characterization of the propagation paths between Pasadena and Palomar. They had, I guess you'd call it, a VHF link between those two places. They operated it around 40 megaHertz, but the thing was not line of sight. The rays had to diffract over the top of the mountain peaks in order to get to Pasadena or to Palomar from either transmitting station. And the question was: what effect would weather, barometric pressure changes, humidity, and such things have. So we set up a field strength measuring system to measure the field strength of steady transmissions from Palomar. And it was very interesting. Ed and I converted a Western Electric receiver into a field strength measuring set and we built a signal generator using a piston attenuator at that time, which was a novelty. Nobody had made a signal generator that was capable of such an extended range of attenuation as ours. The box was double shielded and it really worked well down to very low signal strength. I remember once we built an antenna, which we put on the roof of Kellog, and I remember once turning that around facing east and picking up an FM station in Detroit and listening to that FM station while it faded through a 50db range. In other words, I could hear it still when it was 50 db down from its peak strength in Pasadena.

TAPE 1/SIDE 2

NORBERG: Let me ask just a couple more questions about CalTech. One of them being who else was there at the time among the graduate students that you remember?

OLIVER: John Pierce?

NORBERG: This is the John R. Pierce that subsequently went to Bell Labs.

OLIVER: Chuck Elmendorf, who also went to Bell Labs; Sy Ramo; Dean Woolridge...

NORBERG: That's quite a stellar group.

OLIVER: Yes. Bill Shockley was there. Well, and many other people, one of whom I came to know quite well by the name of George Marmont, who was in physiology. But, I guess those names are enough.

NORBERG: Now, did they all get their degrees roughly around the same time, before World War II?

OLIVER: Yes.

NORBERG: Did many of them leave for Bell Labs before you got your degree?

OLIVER: Yes.

NORBERG: Does that suggest why you went to Bell Labs?

OLIVER: I know darn well why I went to Bell Labs. I told you about this first dielectrics I took from Royal Sorenson. I arrived one morning about quarter to eight or so and nobody else was there.

NORBERG: Arrived where?

OLIVER: At the classroom. He was there and he said to me, "How would you like to study in Europe for a year?" And I allowed that would be fine if it could be swung financially. So he said, "Well, we have a scholarship here from the Institute of International Education." That was supported by the Alexander Humboldt Foundation, I think. I thought that was a great idea and I called my folks about it and they said go ahead and apply and I did. I had no competition, I guess; I got it. And so the year 1936-37 I took off from CalTech and went to Germany where I was at the Technishe Hochschule Darmstadt. I didn't try for a degree there, although I could have. I just spent the year really trying to learn German. I took some elementary courses that I had already had taken here, because it was a good way to learn German and then I took some advanced courses. I remember taking theory of sound there as an advanced course, and functions of a complex variable I took there. That was an excellent course. So, it wasn't a wasted year, but I didn't go out for a degree hell bent. Coming back from there, I stopped in New York to see Chuck Elmendorf and John Pierce, who were sharing an apartment in the Chelsea District. They suggested that while I was in there I should drop around at Bell Labs and make an application for a summer job next summer.

NORBERG: This would be in the summer of '37 for '38.

OLIVER: For '38, right. And I did that and I was accepted. In the summer of '38, Ed Doll had gotten his Ph.D. and had accepted a job at the University of Kentucky. So we made a trip back in my car all over the parks of the U.S. and finally ended up in Kentucky. He drove me on to Washington and then took my car back, and I went on by train to New York. I felt I didn't want the car in New York City, because the public transport was so good and parking was a problem. I spent that summer working in the vacuum tube development lab under McNally and I remember I worked my tail off there. I don't think we had any great results, but at any rate the reports must have been fairly favorable, because John Steinberg from the acoustics department came around recruiting at CalTech the next year and I interviewed him and very shortly received notice that I was being made an offer. Well, in those days, you got an offer from Bell Labs you didn't think twice. So, that's where I ended up.

NORBERG: Does that suggest that the offer came in mid-'39?

OLIVER: The offer came in mid-'39.

NORBERG: My indications are that you received your degree in 1940.

OLIVER: Technically, that's true. I left CalTech in '39, but I didn't receive the degree until I completed my thesis, which I had a gal type in New York City and mail back from there and it was accepted in the spring and so I came out and actually went through the commencement ceremonies in '40.

NORBERG: Did you go into the acoustics group at Bell Labs?

OLIVER: No, no. I was hired in the television research group under Axel Hansen. That group had as its charter the studying of the requirements on transmission circuits in order to send television satisfactorily. In other words, transmission standards were needed: what departures from flat amplitude and linear phase were permissible? What kind of signal-to-noise ratios did you need? What other kinds of distortions could you tolerate? And so, our first job was to make a high-quality signal generator, T.V. signal generator. We used a film chain, a 35mm projector, to do this. When I arrived the general system had been mapped out. They had a Farnsworth dissector there to use as the pick- up device, because the inconoscopes of the day had all kinds of shading problems and they wanted a real good picture. We had plenty of light for projection so we used a non-storage device.

My first encounter, my first job at Bell Labs was to get rid of the geometric distortion that this dissector had. And I did it by analyzing the situation that would exist if you had uniform field down the dissector and uniform magnetic field. I analyzed completely the geometric distortion that would be produced in those cases and found it to be essentially zero. And so immediately I started worrying about how to make uniform fields in a dissector tube. Well, that was fairly easy. You put a set of rings down the tube and we tried that. That didn't work, because electrons, secondary electrons were bounced off of these rings. So I consulted John Pierce about that and he said, "Why don't you make them cones so the electrons hit the outer surface instead." It worked like a charm. And then I had to make uniform magnetic fields to focus and to deflect this beam, and up there [points to a framed item on the wall] is my first patent which shows a set of coils designed to do this. I discovered a paper by Webster, Stanford's Webster here, which showed that if you wound an ellipsoid with a uniform turn density along any axis, that the field inside the ellipsoid would be uniform. It would not necessarily be in the same direction as the axis of winding, unless that axis coincided with a principal axis of the ellipsoid, in which case it would. So the idea was simply to approximate the uniform turn distribution on ellipsoids. I found out that, with some early experiments on cylinders, that I could do a good job by just having a sinusoidal distribution around the cylinder and having the end turns far enough away so we didn't have to actually make elliptical things. But they are included for generality in the patent.

NORBERG: I'm a little puzzled with John Pierce's suggestion. I am having trouble visualizing the cones, rather than the rings.

OLIVER: There was a large photocathode on one end of the tube and an electron multiplier with an aperture in it's housing at the other end. At first, as I said, we put rings inside formed by conducting transparent surfaces of the glass envelope itself.

NORBERG: Now, would these rings be perpendicular to the blackboard?

OLIVER: These rings would have their axis' coincident with that of the tube.

NORBERG: Yes.

OLIVER: This is the axis of revolution of everything. Initially, I started out with rings like this, but when the beam was deflected so that the lower part of the picture was coming through the aperture and the upper part of the picture was hitting the rings secondaries would come off and flood the tube.

NORBERG: I see.

OLIVER: What we did was simply make the rings conical, at such an angle that they always were struck on the outside surface. This trapped the secondaries.

NORBERG: That's clear to me now. Thank you. What was the interest of Bell in this T.V. problem?

OLIVER: Well, their interest, as I said before, was to develop the standards for television transmission. And to do that what they needed was a source of pictures that was so high quality that it exceeded anything that the studios were offering at the time and then could be degraded by measurable amounts and through psychometric testing determine how far you could go before you bothered people. That was the program. In other words, our job was to construct this camera chain and then upon completing that, conduct a series of tests with high quality receivers and display devices and see when signal-to-noise degradation became a problem, when phase and amplitude distortion became a problem, and when geometric distortion was bothersome and so on. All of the things that might be pertinent.

NORBERG: Was your group aware of the work that was going on at other places like RCA and CBS?

OLIVER: Yes, yes.

NORBERG: Followed closely?

OLIVER: Yes.

NORBERG: Through the public literature or by contact?

OLIVER: By contact, both. And that was particularly true in the later years after the war when the NTSC was formed. We were very much engaged in that, too.

NORBERG: Yes, that's what I was headed for. How long did this research problem go on?

OLIVER: I joined Bell Labs at the beginning of 1940, I continued working on this problem until Pearl Harbor, essentially. So it was almost two years. We had a good operating chain at that time, but we later improved it enormously. With Pearl Harbor the whole of Bell Labs became committed to technical effort involving the war and our particular group went into automatic tracking radar development. My first assignment was to develop some means of tracking a pulse in range. It wasn't known how to do that at that time. And I tried one scheme first and that didn't work very well, because it gave large errors as a result of interference. And then I tried another scheme that really worked and that was the one that was used from then on.

NORBERG: Can you tell me a little bit about the first scheme?

OLIVER: Well, the first scheme was simply to set a threshold on return signals so that noise was eliminated and then the radar return pulse would terminate the pulse from a multivibrator (or whatever generated it) at that time, and you simply measured the charge that had flowed, or the length of that time. And obviously, it's subject to all kinds of problems, because if you get interference from a target that's closer but not the one you're interested in, you get a false range reading. So, I quickly abandoned this method, I spent a lot of time trying to make a satisfactory ranging unit. We had no way of accurately generating delays over a large variable time period. This need was eventually satisfied by the so-called Meacham range unit; a way of continuously phase-shifting a sinusoid and tracking a particular axis crossing. Then I concentrated entirely on the problem of optimum detection.

We showed, for example, that the best way to detect the position of a pulse, in time, is to multiply it by a gating function which has the shape of the derivative of the pulse, and to integrate the product. When that integral is zero, you're on the centroid of the pulse. The method has great immunity to noise, because it excludes all of the noise where there is no pulse. When the pulse appears, only noise in proportion to the pulse derivative is admitted. The derivative in this case is the information bearing aspect of the pulse. If you want to detect the pulse amplitude, you multiply the signal and noise by a gating function which has the shape of the pulse itself. By applying those two principles, we were able to detect both the amplitude and range of pulses with a very high degree of performance.

NORBERG: What was the interaction with other groups working on detection devices at this time? Places like the MIT Radiation Lab?

OLIVER: Close. You know, we shared things that we developed. I mean the range unit was developed at Bell Labs but was immediately described and was known to M.I.T., so when Western [Electric] went into production, MIT used it. We made several contributions: the optimum ranging and amplitude detection we arrived at. I invented what later came to be called the box-car detector, which was a detector for determining the modulation in pulse amplitude, caused by pointing error. First you integrated the pulse and noise multiplied by the gating function. The integration took place by simply letting the plate current of the tube just discharge a capacitor. Then rather than slowly recharging the capacitor using a long time constant, I said it would be better, and it is, to simply suddenly restore it just before the next pulse comes along. So, what you get as a drop proportional to the integral, a restoration and then a new drop. The spikes between recharge and discharge are so short, they're a microsecond long, they can easily be filtered off.

NORBERG: I see. Clever.

OLIVER: So, you've got optimum detection. It was an interesting circuit, because it had the plate and the cathode and the grid of three tubes connected to one node. The plate drained the charge, the cathode restored the voltage, and then the grid measured the output and transmitted it on.

NORBERG: Who else worked in this group with you?

OLIVER: Who else worked in the group? Let's see. There was a Norm Pierce, not John, Norman Pierce; there was a Bob Graham, and Bob published some stuff on linear-servo theory, some papers on that at the close of the war, because we were certainly instrumental in developing the first good servo systems.

NORBERG: Did he stay at Bell Labs?

OLIVER: No, he did not. And Charlie Mattke was our mechanical engineer; J.R. Heffle was a circuit man; and let's see, who else... Oh, there was Bob Nielson; and a number of others. I can't remember when they came in and out. George Mueller was with us for a while. George was the head of the Office of Advanced Space Flight in NASA later on. He was in that same group. He now lives down in Santa Barbara, down at, what's the name of that... Montecito.

NORBERG: How long did the project go on? This tracking radar?

OLIVER: Our first job was an air-to-air ranging job and that, after we had found out how to do it, was taken out of our hands and we got a new job. The new job was what we called the Mark 7, and it was a Navy contract. The task there was not geared to any particular weapon system. They just said for shipboard use make the best possible radar you can using all the advanced technology you want. And so we had a free hand. The one thing that we did to make it shipborne was to introduce cross-level into the azimuth elevation mount that we had. We first put it together with high gain servos to keep the errors down to what we thought would be necessary. The damn thing nearly was tearing itself apart because of the noise in the system. So we went back and re-thought that one out and we decided that most of our sources of error were coming from mechanical defects like static friction, and notching and so on and motors and stuff like that. And what we really ought to do is to make a local loop feedback using tachometers to stabilize and wipe out those disturbances and then include this new ideal element with its local loop inside the overall loop. We could easily show that the benefits you got from the local loop was the product of both loop gains and so it was enormous. And we did that and the operation was described by our boss as majestic, because the thing just quietly tracked the target. We had a slave telescope on it, so that we could watch the target, you know. It was just great fun to pick him up, get him in the field, and punch it in to automatic and see the thing just zero on there and from then on he was dead in the target. We were tracking planes out to 20 miles or so with an accuracy of a couple of yards.

NORBERG: Did that go into service sometime in the middle of the war?

OLIVER: Oh, yes. That system, the principles of that system were incorporated in the 545, and 547 radars and various other systems.

NORBERG: 1943, as I recall?

OLIVER: Yes. And later Navy ones used it, too. So, it was not time wasted, it was what the ONR did in those days. They gave you kind of a free hand, and said, "Show us, do the best you can." And then they'd find out the way to do it and they'd put it into various other systems.

NORBERG: And then put it into production in places like Western Electric, I assume.

OLIVER: Right.

NORBERG: Now you also worked on some other problems while you were at Bell Labs.

OLIVER: Let me finish the radar thing.

NORBERG: Oh, I'm sorry.

OLIVER: I never stopped thinking about radar. One of the things that bothered us most was propeller modulation, because that introduced a time-varying reflectivity into the airplane that modulated the amplitude of the returning signal and therefore added a lot of noise on top of the thermal noise that was present. It was a principal source of noise for close in targets. It was obvious from the first that what we needed was some way of detecting the direction without going to a scanning system, but doing it all on a single pulse. In other words, if we could compare the return from a single pulse received in a cluster of beams, four different beams, and let that give us our tracking data, we'd be that much ahead. And so I proposed the monopulse system, as we called it at Bell Labs. It involved having four receivers and their IF amplifier and a balanced system and arranging to automatically maintain that balance. This was finally worked out to be the way of doing it, because most modern radars use this method now. Of course, I must add that after a while the propellers disappeared so the problem became less acute.

NORBERG: I don't imagine the system has changed?

OLIVER: I don't know whether it has or not. Anyway, that was a post-war contribution. The one further one I did, one further contribution, was that I am the inventor of chirp-radar. This is a radar in which rather than putting out a short pulse of high power, you put a much longer pulse which is swept in frequency, or chirped. I was able to show that by appropriate delay, a filter having delayed distortion, could recompress this pulse into a short pulse of much higher amplitude. So you've got a lot more energy in an effectively short pulse. You don't lose range resolution, and you gain range. I described this in a memo called "Not With a Bang but a Chirp." This got circulated around pretty heavily. Then I discovered that the idea of using delay distortion compensation to recompress a pulse had been invented by Sid Darlington, but had not been applied to radar. So, he got the invention, all right, he was the earlier one, but I got the credit of applying it to radar, I guess, but I had invented the whole thing. That's the way things go. But anyway, I continued to work on those aspects a little, mainly through analysis and writing memos while I was still at Bell.

NORBERG: Were all of these devices patented?

OLIVER: Many of them were. In the course of returning to T.V. work at Bell, I invented a compensator for the curvature of the cathode ray tube on which the pictures are reproduced. I don't mean the screen curvature, I mean the curvature of the transfer characteristic of beam current versus applied grid voltage. That is somewhere between a square and a cube law. And if you don't do anything about, it results in pictures with a gamma of somewhere between two and three. In other words, if you pick a scene up on a linear camera tube that puts a current out proportional to brightness and have linear amplifiers and you use that to apply the signal to such a CRT, it will take the square of every brightness that was there. And that's like having a gamma of two, or the cube of it, that's like having a gamma of three. I realized that this was very much limiting our ability to render tones properly in T.V., so I developed a non- linear amplifier system that would take anything between the square and the cube root of the signal out of the camera tube. What would have been ideal in many ways would be to have an exponential CRT and a logarithmic camera tube. That combination would be right, because then equal voltage levels would correspond to equal contrast ratios throughout the amplitude of the signal. That means that added noise would be just as detectable in the highlights as in the shadows and it would be less detectable in the shadows then it presently is. We don't go quite that far. Nature has provided us with about 2| power T.V. tubes and we can generate 4/10ths power camera tubes by using what I called the gamma corrector, or rooter I guess we called it. And that's a pretty good compromise. But anyway that is an important invention, because color T.V. would be impossible without it.

It was very interesting. We had this chain set up in which we'd, say, put on a still picture and look at it on the high-quality monitor, that had 10 megahertz bandwidth and everything, so it was a very beautiful, sharp picture with a nice contrast range from white to black. You switched this rooter in and out, and with it out you got your normal picture, with it in it looked like somebody had turned the sun on. In other words, the picture was like moonlight without it and like sunshine with it. The reason is very clear. In moonlight, which is a millionth as bright as sunlight, you have no contrast acuity in the shadows, and that's what this thing was doing. It was compressing all of the shadows down into a deep black and you had no distinction between them. And they were all too dark.

NORBERG: And then what would happen to make it all too bright?

OLIVER: Well, it wouldn't be too bright. When you switched it in, they'd come up to their normal gray levels that they should have had. See, imagine that you have a brightness scale here and an amplitude here and now you square it. Well, you're taking everything down here and compressing it down and then you're enhancing the contrast up here.

NORBERG: When I keep thinking about the technical problems, I lose my train of thought for the interview.

OLIVER: I also developed an equalizer to equalize the frequency response of camera chains and T.V. stations. I had to correct for the aperture itself in the camera tube. It was a transversal filter. Do you know what a that is?

NORBERG: No.

OLIVER: Well, imagine that you send a signal down a delay line, that has a central tap where you take your signal out. Then you can add in the signal from taps on either side, symmetrically disposed, or subtract them to generate an impulse response which is not a simple delta function, but whatever you want. And that means that you can correct the frequency response. You can have enhanced high frequency response if you authored the first side responses, or suppress it if you add the responses. The neat thing was that the circuit was arranged so that you could take the

T.V. test pattern and you'd obtain a uniform contrast between the black and white stripes on the vertical wedge. You'd take one control and turn it until you had the same contrast at the bottom and top of the wedge. Then you generally would have too much contrast in the middle. So you'd take the other control and turn it down so the contrast was uniform down the whole vertical wedge. The whole system was then equalized. The stations went crazy for it. They really loved it. Bill Harrison, who was a friend of mine in that department by that time, decided to moonlight and make the things for the T.V. stations. He did that, and eventually I said, "You know, Bill, you've got to either fish or cut bait. You've got to leave Bell Labs and make this company go or you've got to forget about the company." That was at about the time that I was leaving. So, he decided to leave and I took some interest in the company. Harrison Laboratories was finally bought out by HP, not through my doing.

NORBERG: What happened to the improvements in the T.V. tube after you had made them? Were these patents then licensed by Bell Labs or sold?

OLIVER: Oh, you know what the patent game is like. RCA and Bell Labs had cross-licensing agreements from the beginning and so did IBM and everyone. I mean the patent situation is just something that you employ patent lawyers to do, because they vitiate the whole system with these trading agreements later on. It's a very funny system.

NORBERG: So then all of these improvements were then built into RCA tubes and so on?

OLIVER: Of course.

NORBERG: What about your work in information theory?

OLIVER: Well, Claude Shannon and I met quite early on in our careers. I think he came to Labs about the same time I did. I'm not positive, but I think within a year. And we became friends and so I was the mid-wife for a lot of his theories. He would bounce them off me, you know, and so I understood information theory before it was ever published. I got interested then in applying the principles of information theory to the reduction of redundancy in the T.V. signal. I might say that we share a patent on information theory applications, Shannon and I, because the patent department in order to patent some of his fundamental stuff had to have embodiments of it. And he was not a circuit engineer. So I came in and designed the circuits to do quantizing and to do all the other things that were required. Well, these were really for pulse-code modulations, some of the first pulse-code modulation circuits.

TAPE 2/SIDE 1

OLIVER: There were two aspects of Claude's work that I got involved in. One was PCM. The Bell System wanted to patent the concept of pulse-code modulation right away and so that's where I was called in to design the circuits to accomplish it. And I don't know if they were ever built or not, but they were sufficient to get the patents through. Pierce and Shannon and I wrote an expository paper on PCM called the *Philosophy of PCM*. It was kind of a pioneering paper. It extolled the virtues of digital systems to the world and I guess was largely responsible for getting the Bell system and others to work on digital systems right away.

I chose to take a look at the possibilities of compression of television signals through the use of information theory principles and I was working on that when HP approached me. I was so interested in it that I first turned HP down. I said to Bill, "Look, I'm just in the middle of this. I've got to see it through." And he said, "Well, O.K., but you're not off the hook yet." And so I worked on it for another year and came to the conclusion during the course of that year that what we were trying to do was beyond the state of the art.

NORBERG: Why?

OLIVER: Well, because we did not have frame storage, for example. We couldn't store more of the past of the signal than a few picture elements. And because of interlaced scanning, you couldn't get access to the nearest detail above or below the detail that you were working on. We wanted to do what is called "linear predictive coding." We wanted to say we're only going to transmit the surprises. In other words, to the extent that we can predict the future, we don't need to send anything because the receiver can do the same prediction.

That's the principle. And we couldn't get good enough prediction, because of the fact that we were limited so much in the amount of data we could store. Today, you think of mass memories where you can store a picture frame easily. So now what we were struggling to do would be very easy to do today. But the ironical thing is that as the state of the art improved, and dropped the cost of memories down to a very small fraction of what it had been, it also dropped the cost per megahertz of bandwidth so it became less important to compress the pictures. So, although you can do it now, nobody wants to particularly, except in~p certain applications. And I think that will come into it's own. There are people now that claim to be able, and I haven't really seen them, people that claim to be able to send pretty high quality, you know like commercial NTSC quality pictures over a 50 kilobit, kilobaud line with slight problems. I mean if the whole scene gets into motion, why you can see ragged edges, but if you're just sitting here and someone's nodding their head or something, as in a typical conference situation. Why it will be a good picture. So, it can be done.

NORBERG: What was Bell Labs like in those days? Contrast it, for example, with CalTech as you left there in 1940. The search facilities, type of people, number of projects and so on.

OLIVER: Well, of course, it's a very different environment in the sense that it's not academia. I mean you're very much goal oriented and you're expected to take your knowledge and use it, so there's a certain turn-around that is involved. You're not getting taught; you're supposed to produce. So there's that aspect. I think that there was a good deal more formality when I first joined Bell Laboratories in the East than there was when I left CalTech in the West. Now, how much of that is East versus West and how much of it is company tradition, I can't say exactly, but everyone wore shirts and ties and coats and lab coats were kind of the rule and everybody was addressed by their surnames. I don't think that was true in all departments, but it was in the one I was in. Axel Hansen was a Dane and I suppose that he had brought with him some of those characteristics. But we always called each other by our surnames.

NORBERG: Even old friends?

OLIVER: Yes.

NORBERG: Like John Pierce?

OLIVER: Well, I didn't address John as Mr. Pierce, but I would in a meeting. You know, the first name basis was reserved for social gatherings and not for meetings to discuss projects. I mean, somebody would say, "Well, Pierce here has an idea about that." But it was a surname basis at first. That disappeared during the time I was at Bell Labs. It pretty well went away. And so there was a kind of an informalization of the laboratory going on during the time I was there.

NORBERG: Was there a big increase in staff during the time you were there?

OLIVER: Yes, but it was largely in the military electronics area. When I first went there I went to West Street and I spent eight years working at 463 West Street In the last four years, I was out at Murray Hill. What happened was essentially the entire West Street operation was moved to Murray Hill and then some of it went to Whippany, which was the military part. I think that West Street was simply abandoned. I'm not sure. And then following the time when I left, after I left there, of course, the Holmdel Laboratory was converted from a simple little building that was out in the middle of a meadow to a huge glass edifice that's down there now. It looks like a storage battery.

NORBERG: Did you keep track of other projects that were going on?

OLIVER: Some.

NORBERG: Which ones?

OLIVER: Do you mean in our group, or...?

NORBERG: Your group and then did you have any contact with other groups as well, besides through friends?

OLIVER: Well, yes. I went down to Holmdel rather frequently. I knew Harald Friis and I also knew several people down there. Vince Rideout was down there the first years that I was at Bell and Art Crawford and Mervin Sharpless and some of those guys. I knew Karl Jansky quite well, as well as anybody knew him. And so I was well aware of the work at Holmdel. We used to go down once every two or three months just for a visit and just to look around. We would drive down some nice day. I'd also, you know, go over and visit people in acoustics and other places. I was quite familiar with the work Harvey Fletcher was doing on visible speech. I was fascinated by the anechoic chamber they built at Bell, a really wonderful anechoic chamber. I knew a lot of the work that was going on in traveling wave tubes that John and his group were doing. I was very aware of the components group's work and of the filter group. I had a lot of work with them during the first parts of the war.

You had the feeling at Bell that whenever you needed advice on some particular aspect of engineering or physics or science, there was an expert there that you could go see. And you did. That was one of the things I was loathe to give up when I left. I thought, "Gee, you know, here I am I've got the world's knowledge in electrical engineering at my beck and call. All I've got to do is pick up the phone or go see somebody and I can get the answer. Go see Warren Mason if you have a question on crystals, or whatever." And I said, "I'm going to be all alone and I'm only going to be working in instrumentation and won't that be confining." Well, it turned out it wasn't. I had to cover a much broader field than I did at Bell Labs in television research.

NORBERG: We'll come back to that in a moment. Was this a typical pattern for people at Bell to be able to go around to all of these other projects and visit and so on?

OLIVER: I'm not sure. I was in the research department and I think that the other departments had kind of different ground rules. But in the research department, we were very free. You know, if I had a problem with noise or something, I could go talk to Steve Rice or Harry Nyquist or whoever I wanted to. Even R.V.L. Hartley was still around in those days.

NORBERG: Were you expected to charge some of your time to other projects then, if that turned out to be the case? If you helped them as opposed to them helping you?

OLIVER: If it got to be appreciable, but ordinarily we did not. You let the give and take absorb it, because it's usually a bilateral situation.

NORBERG: When do you remember Bill first coming to talk to you about joining HP?

OLIVER: It was at the time of the IRE convention, which would put it around March or April 1951, I believe... or 1950.

NORBERG: HP was still small then compared to now.

OLIVER: Well, when I came out, it was about 400 people. But they felt it was big enough and growing and solid enough at that point that they needed to organize for the future and could offer people key spots without jeopardizing their future. At least that's the line Bill used on me. He said, "We have a Director of Production and a Sales Manager, and a Financial Manager. We need a Director of Research and that's what I'd like you to be."

NORBERG: Now, up until that time, it was he and Dave who were doing the research, was it not?

OLIVER: Bill was essentially doing it. And Dave. I always think of Bill first, because in the years that I spent he was the more frequent visitor to the lab, but Dave would get out there whenever he could. But he just gravitated toward more of the financial end of things and seeing that all the business aspects of the company were met.

NORBERG: Now, when you say, "Get out there", what do you mean?

OLIVER: Into the lab.

NORBERG: It was still all in the same building, was it not?

OLIVER: Yes, but there was a front office and he had visitors in there or he'd be working with somebody in the front office and later on in the day when he got his desk cleared off, he'd wander out to the lab, and we were always glad when that happened.

NORBERG: What sort of a company did you find when you came here in '52?

OLIVER: A spirited, enthusiastic group that really believed that they had the best outfit on earth. They were determined to do the best job that they possibly could. In other words, there was a very high morale and a very high determination to build quality into things. They didn't always do it, because we didn't have all the machinery necessary to do it, but it was a very, very loyal group and busy group. There was much more daily dedication to progress for each individual's work than there was at Bell. The pace was much slower at Bell, things were more relaxed. You know, I spent a lot of time at Bell just talking to other people rather than working at my job. I'm not the only one who did. I'm not saying that was all bad, I learned a lot by doing that, but you didn't feel the blow torch on you to get the job finished to nearly the extent I felt it when I first came to HP.

NORBERG: Why did you feel it when you came to HP? What was the difference between the objectives here and the objectives at Bell Labs?

OLIVER: Because we always had to have some new products, damnit. And they had to be out in a hurry. To get our sales up, you know. Competition was always a threat. So, whenever we'd bring out something, we always startled the competition and tried to better them. And they did the same to us, of course.

NORBERG: I'll come back to the competition in a minute. Were there a group of products on the line, on the development line, when you came here?

OLIVER: Oh, yes. Principally, these were microwave signal generators. The decision had been made by Bill and Dave to go into the microwave test equipment business and into wave guide products. Furnish wave guide parts and all that. General Radio decided not to do it and that was one big difference in their growth and our growth during the late '40s. We augmented our line by all these signal generators and slotted line and standing wave detectors, and Lord knows what. And directional couplers and all the hardware to make wave guide systems. That became a big market.

NORBERG: But all that was done in the late '40s?

OLIVER: Well, not all of it. No. The late '40s saw the beginning of it. Actually, the 608 was developed before I came out here. That was an S-band signal generator, but then the whole concept of filling in the line up to twenty gigaHertz or so took a big part of our time in the '50s.

NORBERG: How did you people work when you came here? What sort of a facility did you set up?

OLIVER: I walked in to an existing and going operation. We were all down in the Redwood Building, all the engineering was down in what we called the Redwood Building. I don't know if you're familiar with the old plant...

NORBERG: No, I'm not.

OLIVER: HP moved from the original garage to what is now Polly and Jake's antique shop as a first step in getting more space. I visited them when they were there, on summer vacations when I'd come out from Bell Labs. Then they built the Redwood Building, and I think that was built, I hesitate to say when. It must have been around '46 or so, sometime around there. That building and a Quonset hut behind it housed HP then for the next couple of years. Then they built the first forward part of the concrete buildings that are on the other side of the Redwood Building. About the time I came out, they extended those concrete buildings back as they needed more space. It was a time of expanding shop facilities and buying machinery and getting geared up to do the job right. We had a philosophy then of vertical integration; we pretty well made things from raw metal up, and were tooled up to do it. I don't know whether it was right or not, but Dave thought it was. And the lab was one open area. There were no partitions in those days. I guess the idea was that they cost too much to put in; we'll be moving them in a few days or a few months anyway. So, we got used to open areas, and that was a problem with some people we tried to hire. I mean they really didn't like it, they got agoraphobia from being out in the midst of everything, so we finally had to do some mild partitioning as you see today. What else would you like to know about it?

NORBERG: Well, that's an interesting point, because one of the things that we see in business publications all the time is the HP dedication to openness among the employees. There's access by everyone to everyone else. What you just implied is that openness in the beginning was a sort of a general warehouse pattern.

OLIVER: It was a physical openness.

NORBERG: For reasons quite different, perhaps, than a philosophy of keeping people interacting with each other all the time.

OLIVER: As a matter of fact, I set a precedent when I came to HP of having an office of my own. I was a little bit ridiculed for that, but I wanted an office where I could take two or three people and sit down and I wanted a blackboard in it so that we could talk and have it reasonably quiet and it was a good thing, I insisted. It was the right thing to do, but it was such a break with tradition that a lot of eyebrows were raised first. You know, Noel Eldred, who was marketing manager at the time, and Cort van Rensselaer was next to me, they didn't have offices. Noel Porter, who was head of production, he didn't have an office. Why should this guy Oliver have an office? Now they *all* do.

NORBERG: Now, in this open space, though, in the laboratory, what sort of instruments were these people using? Were they all HP instruments and maybe a Tektronix oscilloscope or something of that kind that was not being made by HP?

OLIVER: No, they were not all HP instruments. I mean we had some of the old traditional instruments, the Boonton Q meter, we hadn't bought Boonton by that time, but we had that kind of instrument. We had a couple of GR signal generators and things where we didn't have one. Our goal in those early days was what we titled complete coverage. Our goal was to become an instrument purveyor that had something of just about every type you needed anywhere in the spectrum. So we wanted complete coverage from audio frequencies on up to the highest radar frequencies in signal generators and other equipment to make tests. And vacuum tube voltmeters and so on. So largely those first years were filling out the line. We relied a lot on the sales people to tell us where it hurt most, where we were missing something they really could use. And that usually determined what products we devoted attention to.

NORBERG: I see. And then how did you distribute the projects among the staff?

OLIVER: Well, on the basis of talent really. Who was the best person to put on that job as a leader of it. We were making some other changes, too, and these occurred, I would say, about 1958. For the first few years, when I first came here, we were down in the old Redwood Building, we had a situation that had a few problems with it. We had the lab, which had about twenty people in it at the beginning, and then there was a group of production engineering people that would take our lab design and make it manufacturable. There were about eight of those, I guess. What this resulted in was that there were two fences between the lab and the plant. One was that we'd have to leave lab prototypes on the doorstep with the production engineering people, who often would screw it up in the interest of doing something right but do something else wrong. They hadn't been with the thing from the beginning so they didn't know what degrees of freedom they had. So there was a lot of bickering back and forth. And also they weren't particularly anxious to pick a new job up. When they were busy with something else, it would lie there for a while.

Then there was another hurdle getting the production engineering prototypes into production, because they had to choose a pilot, run leader, and deliver the wiring samples and all that. About that time when we came up here, we rearranged things and started operating on an entirely different scheme. We decided to disband the production engineering group and distribute the production engineers among the electronic engineers and let them work on the product from the very beginning, from the very outset, as a kind of a task force. We did not break up the industrial design group, because they had an across the board responsibility of maintaining kind of a company product appearance. What Carl Clement, who headed that group up during those years, did was to develop a kind of a universal style that you could put almost anything in to and it would be a very usable package. HP cabinet design was all done in the industrial design group.

But in another part, we took projects and we essentially said to a guy, "Look, this is going to be your job, this is your project." Usually it would be the guy who was most enthusiastic, or that thought it up. Anyway, the obvious person. "And we want you to assemble a team. You'll need a Product Engineer or two on this, and you'll need somebody who is good at circuit design, and you'll need somebody who's good at microwave plumbing, or whatever the needs were. Why don't you think about it and tell me who you want on this team. We'll then see if we can get them." That team then would start on the project and the same person would follow it through from the very beginning fully to the end. And if there was any problem, he had access to the people who had been on his team to come help fix it up. And he didn't lose responsibility for that product until it was out in the field and running trouble-free. So, by identification of a team with a product, we achieved a coherence that hadn't been present before.

NORBERG: And that was done in about 1958?

OLIVER: Yes, around that time.

NORBERG: Now, isn't it true that around that time as well, the HP Laboratories separated from other research activities going on inside the company?

OLIVER: It was a little later. Let's see. In '60, I guess. We started our first operations away from Palo Alto. What happened was that splinters left this corporate lab and went to those two divisions, splinter groups. And then that process accelerated and about 1965 or so there was very little left of the original group. I mean the role of the HP Labs had to be defined if there was going to be a corporate lab. And we decided at that time that there should be one, but that it should do several things. First of all, it should engage in research that would support the engineering activities of the company and that preferably would be usable by more than one division. And so looking into the materials and the properties of 3-5 compounds and electroluminescent materials and stuff like that was obvious. So we started programs in those directions. Also the development of IC technology was a major thrust from the very beginning.

Also we felt that a role of the HP Labs was to develop techniques, as well as materials, that were widely applicable -- circuit techniques and processing techniques and so on. And also to look at new product areas where we had the technology to do the job, but had not done it for one reason or another. We were the people that got HP into various new fields during that epoch. What happened was that the divisions tended to concentrate on the competition dead ahead of them and were often not concerned about things that we could do but weren't doing. Really, our role could be defined as that of spawning new divisions. We had already spawned the divisions we had and how about continuing to do that? Get some area that's big enough that it's going to develop into something that needs a new division. We did that with computers. We did that with calculators. Some of our activity here was transferred out and became the mass memory division in Boise and so on. There's a lot of things that began here but later ended up as a division. I don't think that divisonalization is going to continue indefinitely. I think that we're about as broken up as we can be at the moment.

NORBERG: Let's go back to the 1950s again and talk a little bit about the competition. Who did you see as the competition for HP in the first years when you came here?

OLIVER: The competition for the first years...

NORBERG: Say '52 to '57, or so.

OLIVER: Yes. Well, it depends upon the product you're talking about, but in some areas we were leaders and in some areas we weren't. In counters, frequency counters, and digital technology related to them, we were, I think, pretty much off to a head start. But then, competition developed. In the first days, it was Potter in those counters, but we out distanced Potter pretty quickly. In oscilloscopes, it was Tektronix. They kept ahead of us pretty regularly, so we were kind of second best in that area. In signal generators, we originally felt we had competition both from General Radio and Ferris and other Boonton generators. There was a New Jersey syndrome in there, but of course the Boston General Radio outfit, they were the ones that we had to beat. I guess that's about all I can say about it.

NORBERG: Yes. How did you go about trying to beat them?

OLIVER: By incorporating newer technology and newer techniques and exceeding them in their specs.

NORBERG: Was this by dissecting their equipment, or just designs that you...

OLIVER: Well, you didn't have to, you knew how it worked. I mean we'd had it apart often enough to trouble shoot it anyway. The difference between an electronic instrument today and in those days is the same as the difference between a modern car and a Model T. You knew how a Model T worked. God, I remember a term paper we had in Terman's class to design completely a five tube superheterodyne. That was an interesting challenge. You know, to get the best performance possible out of those five tubes. I often think people today use 500,000 transistors and think nothing of it. I mean it's...

NORBERG: What was your association with Bill and Dave during those early years?

OLIVER: Well, we remained friends most of the time, I think. Of course, I was eager to earn my spurs here, and worried that I might not. No matter how hard you try, there are always problems. And Bill had kind of a jollier atmosphere about him. When you'd meet him and discuss things, he would laugh and he was kind of friendly. And so I felt more at ease with Bill at first. Dave, on the other hand, was always very concerned, the first thing that he worried about was is the company going to make it? That was on his mind all the time. And so he was less inclined to make light of things and joke about them, and more inclined to be quite serious about them. I remember one time -- this was about 1954, 5, 6, somewhere around in there -- I came in one Saturday to work on something, you know, and Dave was there. I said, "What are you doing?" And he said, "Oh, I've been out in the lab." And he said, "Look, there's several things out there that worry me a lot", and he proceeded to list them. And I...

TAPE 2/SIDE 2

OLIVER: I then started to explain why these things were that way, and he said, "Look, are you going to make excuses or are you going to fix them?" At that point I shut up and learned a lesson, I think.

NORBERG: Why did you feel worried?

OLIVER: Well, hell, until you've succeeded at something, you don't know whether you can.

NORBERG: But you had succeeded very well at Bell Labs in the kind of work that was required?

OLIVER: Oh, very different. There was not the day-by-day pressure, there was not the need. I mean, in Bell Labs you felt very easy. It was an amniotic fluid you swam around in, and you really hadn't felt the cold breath of reality there very much. AT&T wasn't going to fail; Bell Labs wasn't going to not succeed, you know, I mean it was an organization backed up practically by the wealth of the U.S.A! At least that was the feeling you had. You didn't know what the Justice Department was going to do a few years later, but anyway you felt very secure, probably too secure. I mean the individual felt that he could do so little to change the course of Bell Laboratories that there wasn't that individual sense of responsibility that one felt here. Here you knew damn well, by succeeding you could help the company succeed, and if you failed, you would be a failure yourself.

NORBERG: How then did you respond to Dave's comments?

OLIVER: I got to work on the problems and tried to solve them.

NORBERG: Can you describe those problems to me again?

OLIVER: Oh, mainly they involved delays in the schedule. We were behind schedule in some things, and so that just simply meant getting in there and working with the groups and saying now, "What can we do to speed things up?" You know, "What is really holding this thing up? Dave's getting worried." And that would make them pretty serious, too. And so sometimes you needed to put more manpower on it. I've taken people off of other jobs and said, "Hey, look. We've got a crisis over here. The thing you're working on is probably important but not as important at the moment as this, so would you go over here and help?" And so we'd staff up and get the things moving and I guess it worked out all right, because I didn't get fired.

NORBERG: Had you had any of this sort of project management training at Bell Labs before?

OLIVER: No.

NORBERG: So this was brand new?

OLIVER: That's right.

NORBERG: And how quickly did you feel secure in manipulating the situation such that the projects could be at least reasonably successful?

OLIVER: Well, that's one of those things where you never feel secure, you only gradually begin to believe that maybe it's true. There wasn't any sudden day in which I had tenure or anything like that. I'll tell you, it was a kind of a spotty organization when I first came. The lab consisted of people with various degrees of analytic capability. Some were just potentiometer turners, who would optimize things empirically. So I felt a great need to conduct a course in modern circuit theory and in the mathematics pertaining to it, functions of a complex variable, for example. I introduced the concept of poles and zeros and got them thinking about all those things. And I ran that damn course for a year or more every week preparing a lecture for it and giving out tests and grading problem sets. I was kind of a teacher here for a while, because I just didn't feel that the lab was uniformly qualified to do the kind of things that we had to do. There were some very bright people in it and some that weren't so well educated. And I guess that the course did some good.

Then we concentrated a lot on our recruiting program. We felt it was very important to bring in the best people we possibly could. And that wasn't so easy when HP was relatively unknown. It was hard to get the top people. So we had to work at that. I think those results paid off. We had the philosophy that recruiting was far too important a role to leave to the personnel department. The interviewing had to be done personally by us. And so we went to universities and did so. Our philosophy was to build up year-by-year a steady relationship with the key universities where most of our good people could come from. They began to look upon us as a reliable hirer. We didn't just come in on the good years and ignore them on the bad years. They could count on us to come around. And therefore, they did some spade work for us. They would assess their own group and say, "Hey, we've got two or three guys that we think HP really ought to be interested in." And you usually found out they were right. They knew the people better than you could get to know them in an interview.

NORBERG: Did you keep a close association with Stanford in those years?

OLIVER: Oh, yes. Very close with Stanford, close with Cal, close with CalTech, close with M.I.T., and then various degrees of closeness with a lot of other universities like Purdue, and Illinois, and Georgia Tech.

NORBERG: When it came to research problems, was it possible to trot over to the campus and talk to those people directly?

OLIVER: What kind of research problems? You mean if you were stuck on something?

NORBERG: Well, yes. Let's say you were developing a generator at the high end of the band and there was some problem with the circuitry that didn't seem easily solvable, would you have gone to Stanford or was that kind of talent still not available?

OLIVER: We used consultants from Stanford in certain areas. It doesn't happen that was one of them, because we were kind of at the vanguard of tube development for a while there ourselves. I mean this was before Watkins-Johnson got into business. We were making traveling wave tubes at one point, and microwave oscillators at another. We had Karl Spangenberg as a consultant. He was good on electron beams and he was good on cathode ray tubes and that sort of thing. So we kept him on a retainer, not as a crisis solver or anything like that, but we used him as a consultant on a regular basis. And we had other consultants as the years went by when we got into hand-held calculators and other calculators. We used Velvel Kahn, who is a numerical analysis man at UC Berkeley, one of the top people in that field. So we have leaned on the universities for specialized help. I don't think we do it quite as much as some companies do.

NORBERG: In this same period, in those middle years of the 1950s, what proportion of the activity that you were directly involved in had anything to do with say Department of Defense contracts or military applications?

OLIVER: When I first came, we were making some signal generators under military contract and I guess some other instruments. And we had a Navy inspector that was a resident at the plant and he was monitoring the whole operation of producing those things. We made the decision, and I don't know whether if it was at any one point or not, but gradually made the decision that it would be best to set up our own environmental quality control operations and test our instruments to mil specs just as a regular thing and then forget about going out on a bid for military equipment. Our own commercial equipment will meet their specs, take it or leave it. And that way we got out of all the paperwork and all the hassle that accompanies those contracts and got a higher quality of instrument for the general user as well.

NORBERG: So that suggests that there were very few of that sort of contract.

OLIVER: That's right. What happened is that that contracting diminished and finally for the last twenty years of my being involved here, I would say that our government contracts were entirely in research and involved such things as ternary alloys for photodiodes and things like that, materials contracts in an area in which the results would soon be known to everybody anyway. In other words, we weren't after a proprietary thing, we'd take them when we had that work to do ourselves and so the support would help us. But it wasn't in the way of producing equipment, it was in a way of producing discoveries. Much easier to administer that kind of a contract, we felt. And the total amount amounted to $500,000 a year or something like that. It wasn't any big deal. People think, you know, that we are kind of like the aerospace industry, that we exist on government contracts. We don't.

NORBERG: I'm fully aware of that. In fact, when I wrote that piece on the origins of the electronics industry on the Pacific coast, that was the one point that Bill wrote me a note about. I said that HP was involved with military markets during World War II and after it. He wrote and said, "We were not. We were selling instruments and if the military bought them, that was fine. But we were not out to sell anything to the military particularly."

In this same period, the company was still thinking about continuing to be vertically integrated in all of the things that it did, partly, I understand, because -the quality of the materials from outside was not high enough to meet HP standards. But there seemed to be some other reasons as well for setting up subsidiaries. Do you remember that? Things like Palo Alto Engineering and Dymec?

OLIVER: PAECO and Dymec, yes. Well, we've done that two or three times. I wasn't involved with PAECO, that happened just before I got here. The idea of PAECO was that we needed transformers for a wide variety of our instruments, power transformers in every case. That was in the days before switching regulators had come in, you know, the whole power supply business is different today. A good way to get these would be to develop our own source of supply, take the skill we had and put it in there and start it out as a small company with some of the HP executives putting some money into it originally and helping to manage it and run it along, doing the hiring and everything. And when it got on its feet, if it was a successful operation, then having HP buy it back. And that was a way of getting a little more stock ownership in HP and a little financial reward to some of the people that helped do it. It was a kind of an in-house entrepreneurial concept. That has sort of vanished. Let's see. Dymec, PAECO...

Dymec was going to be the automatic measuring division, and that's really one of the things we developed our first computer for. We backed into the computer business. We really didn't start out to make a computer, you know. We wanted to make a controller for our instruments so that we could hook together a lot of instruments and they could perform an automatic measurement. So the 2116 was designed as a controller. That automatic measuring business never did get off the ground, because it costs too much to make specific systems for people. You know, they never could understand that putting just some standard things together wasn't simple as pie. What else do you have to do? So, they said, we can do that, we'll just buy the equipment. So they bought it and they put just as much money into designing that thing as we would have, only they had the fun. And meanwhile, computers were selling as stand-alone dedicated computers in other applications and we found we had a bear by the tail and we better get busy. So, that's how we got in.

NORBERG: I'll come back to that. Let me ask you one more question and then we can stop for the day, because there are two things I want to do between now and the next time. You mentioned the association with Bill and Dave and the way in which the management of the organization was run. How about your association with other people, like Porter and Eldred...

OLIVER: Oh, we were all very close. I knew Porter a long time before... He was one of the Stanford group, too. He was always a character, a funny person, you know, always good for a laugh. And so I felt very at ease with Ed. As a matter of fact, I shouldn't give the impression that I felt ill at ease with Dave. It's just that he was always serious and he didn't goof around and so you felt more like getting a stern look on your face yourself.

NORBERG: I've just been through six hours of interviewing him. Yes, I know the feeling, exactly.

OLIVER: Frank Waterfall once said he characterized the two this way. He said, "I've known Bill really for only about fifteen minutes, but every time I talk with him, I get the feeling I've known him all my life." He said, "I've known Packard all my life and every time I talk to him I feel I've known him fifteen minutes." But let me say in deep respect that time has changed that situation greatly. I certainly have not lost any of my appreciation for Bill, but I've gained a lot for Dave, and I just think his leadership has been terrific.

NORBERG: I'm thinking more in terms of the way decisions were made in the company in those early years. How much give and take there was among this small group of people, five or six men?

OLIVER: Well, we evolved, usually, the specifications on an instrument. We set up some initial ones that would be desirable and then we made some investigations to see whether those were really in the cards or not. Often times it wasn't possible. We could do this, but we couldn't do that. And then we'd get together with Eldred and others and say, "Now, if we deliver you something, that will do this, but won't do that, can you sell it?" And Dave would often be in on those conferences and so would Bill and they'd say, "No, we can't do without that. How much would it cost to put it in?" And so it was that kind of a discussion. Kind of a group discussion of the problem that arrived at decisions and often times they were made at the very top.

TAPE 3/SIDE 1

DATE: 14 April 1986

NORBERG: Last time, in the H-P story, we discussed your arrival at Hewlett-Packard and some of the early interactions among management people as those interactions affected R&D here in the company. At the end of our last session, you indicated the personal characteristics of Bill and Dave in their approach to business and people.

Remember the comment you got to know one person within 15 minutes but you didn't seem to know that you... Someone had commented that anyway... Leading into comments on how products were discussed at that level in the 1950s. This is the decisions about what sort of products to promote. Your last comment on this question, and I quote, was, "It was the kind of a group discussion of the problem that arrived at decisions and often times they were made at the very top." I'd like to pursue this a little bit further as to what was meant by these decisions "at the very top". Can you offer an example of a product developed in the 1950s whose circumstances illustrate this kind of high-level discussion?

OLIVER: Well, let me think... Let's see... Some of the major projects that we developed in the '50s included our first oscilloscopes, our sampling oscilloscope, which was done in about '58, I believe, and let's say wave analyzers, and I guess some of the frequency standard work, although that may have been a little later. But in the '50s we had just moved up to this building here, '58 I think was when we came up here. Before that we were down...

NORBERG: In the Redwood Building.

OLIVER: No, not the Redwood Building, we were in the sawtooth roof building down in the valley plant. We'd moved out of the Redwood Building a few years earlier. In those days, in the '50s, it was customary for both Bill and Dave to circulate through the lab. They would get their morning's work taken care of and then they'd come out and participate in some of the engineering. And so it was not a question of a formal conference being convened to discuss something, rather it was a question of saying we might be winding up one product and then they'd say where do we go from here. And we discussed that, and then in the course of that discussion ideas began to gel as to what the next item in the line should be. Or if we were in the course of the development of a project, they would help assess the relative importance of various specifications that we had tentatively put down. And if we were having difficulty meeting a certain specification they would say, "Well, that's probably not too important, don't raise the price just to meet that, it's probably good enough what you've got." So they were very good in giving casual advice. It often shaped the specifications and the final nature of the product. I don't remember that we had product design review meetings formally as such. We occasionally may have done so, but it was more of a day-by-day management by walking around sort of syndrome that we operated under.

NORBERG: And would you say the whole string of products then developed in the '50s were probably influenced in that way?

OLIVER: Our inspirations for new products came from several sources. Sometimes there would be a need expressed by our field salesmen, the representatives that we had at that time. They'd say, "Hey, you've got a wonderful line here, but there's a big hole in it." And they'd describe the hole and we'd look at it and we might agree with them, so we'd design an instrument to fit in there. Maybe we had a signal generator that went up to a certain frequency and another one that began here and there was a gap in between. And so we'd say, "Okay, we'll take that on." Other times it was obvious that some of our products needed a facelifting. We had very early on an instrument called a wave analyzer and it sold for many years and it got into a lot of government contracts and things and so it kept selling. But it was hopelessly out of date in the late '50s, or I guess it was the middle '50s. And so we redesigned it. It was our first transistorized instrument, actually, it's the 300A. We had some really very, very good specifications for that machine and that represented an enormous advance over the previous state of the art. There were several examples like that of instruments that simply we brought out a B model or a new model that was a vast improvement over the former one. So that was another source of new products.

A third came about when you looked for developments in science or in technology that would permit a new product that you couldn't make before. And an example of that was the sampling oscilloscope. At that time, with vacuum tubes the upper limit on oscilloscopes was around 10 to 20 megahertz. And we discovered a device called the step-junction diode - I guess we called it the Boff diode because Frank Boff here was the one who really discovered it - that would generate a pulse that was very, very short indeed. He discovered it, in fact, since he was using some diodes to try to accomplish frequency multiplication. He was getting much more output than he thought he had any right to and it turned out that these diodes were generating extremely short pulses. And so that suggested to us that we had a way here of sampling a wave form with an extremely short sampling pulse and then we could hold that and we could stretch the wave form out to make it, say, a thousand times longer than it was. Every time the wave form recurred we'd sample a new part of it. So if we had a thousand samples per wave form, why our frequency was a thousandth of that of the wave form itself. So we could take things that were up in the microwave region and examine them for the first time with an oscilloscope. And we made the sampling oscilloscope, a new device.

NORBERG: Yes.

OLIVER: So new concepts, face-lifted old products, and simply requests from the field to do something to fill in our line were the major sources.

NORBERG: Right. Now in this third source, the first two seem very obvious to me from your descriptions, but the third source where you commented that you would go out looking for areas in science and technology where new products might be developed. How was this done? Was this also done through the field representatives or was this much more sophisticated?

OLIVER: Oh no. This was done in the lab. Done by reading the literature, keeping up with new developments, asking ourselves what can they be used for and so on.

NORBERG: Were there developments, then, in the lab that H-P decided not to exploit?

OLIVER: Well, sure, there are lots of developments that we didn't see any immediate application for. But a fundamental thing like the laser comes along, we decided we ought to be working with them, getting to understand them because there undoubtedly were going to be applications for them. So we did. We learned about a lot of things that way. We learned about optical modulators, learned how to do heterodyne detection at optical frequencies, things like that. I guess...

NORBERG: Let me ask that same question a slightly different way and that is, did anything get developed that after either an impromptu conference with Dave and Bill or some of the other executives here or a formal conference did they decide, did you people decide not to develop further a given idea.

OLIVER: Well, there was occasionally an item that we might have been working on that either turned out to be very difficult to realize, to meet the specs that we had envisioned for it, or it was superseded by something that appeared from some competition beforehand and changed our thinking. You know, this is a ferment, this whole business we're in, so yes, many projects had been dropped. But I would say that we had a kind of a two-stage process for developing an instrument. There was what we called an investigation phase in which we assigned an I-number to it. It wasn't given a project number, but an I-number, and if the outcome of that was satisfactory, and everybody was enthusiastic about going ahead, then we converted it to the L-phase, a lab project, and it was assigned a lab project number. And I would say that of those that made it past the I stage into the L stage that our batting average was pretty high, it was around 70-80%.

NORBERG: That is of the L phase, 70-80% made it to market.

OLIVER: Made it to market. But there was a lot of infant mortality in the I-phase.

NORBERG: How long did this process continue, in time that is, as the number of products and the size of H-P increased in terms of the impromptu conferences, the way in which ideas generated and so on?

OLIVER: The first step that occurred was the decentralization of the laboratory. That began in the '60s. The establishment of the Boblingen division, the Loveland division, the Santa Clara division broke up what was originally the only lab in H-P into several laboratories. And in that break-up, groups that had been working together generally moved together. I mean, for example, when the Santa Clara lab was established the frequency and time division of the lab moved down there in toto. People in the labs that had been working on those instruments went together to the new location. When the Colorado Springs group was established the oscilloscope people went there and so on. And so what happened, then, was in the early '60s, the central laboratory lost many of its personnel to the divisional labs and we had to do some head scratching to say what is going to be the role of the central laboratory or is there going to be one? Are we going to be a corporation that just has a lot of divisional laboratories and that's it or is there still a role to be played by the central laboratory and we rather quickly arrived at a set of points that we thought were the main reasons to have a central lab. The role of a lab was defined in those early years, and it was established in '65, as I recall, about that time.

NORBERG: And what was that role to be?

OLIVER: Well, it was to be several things. We began to experience a kind of a narrow focusing in our divisions. In other words, they would become very concerned in meeting let's say the competition in oscilloscopes, or trying to do so, or meeting the competition in pulse generators, or whatever their product line was to such an extent that they almost put blinders on and thought about nothing but pulse generators. And so we felt that there ought to be a reconnoitering central agency that looked for these new developments and looked for new ideas and things that would take us into new fields. In other words, we felt that one role of the central lab could be the spawning of new divisions or new product lines and that would be a developmental function. But we also thought that there was need for certain areas of research that the divisions were not doing. They were more bottom line oriented, immediate profit oriented, and we felt that we needed to beef up the research activities so that down the road, in the long term, we would be producing ideas that would benefit the corporation. So, we tried to establish some areas of research, for example, we got into 3-5 alloys and did a lot of work on photo luminescence and the result of that was we came out with the digits for the calculator, the read-outs for the calculators we've had. We were leaders in that whole area for a long time, we were marketing those as products. So we had the solid-state lab and it was doing largely materials research work. It was asking questions of nature, which is what you do in research. And we also looked into various other effects and whether we could use them or not-liquid crystals, optical memories, all this sort of thing. We decided to go ahead and start an effort in E-beam development and followed that to its logical conclusions. We made some E-beam machines that are in use in the company today that are capable of actually doing production. In other words, we have high enough beam currents in them that they can actually produce masks in a short enough time, produce circuits in a short enough time because you don't have to make masks. Just a lot of things. We did a lot of research on printing. We developed some things like the ink jet printer, you may be familiar with that.

NORBERG: Yes.

OLIVER: That's a neat little development. It happens we were in a bind with Canon on it because they thought of the same thing. So there's a patent matter to be resolved there, but it was original with us and we developed our version of it. Just a bright idea, that's all.

NORBERG: Well, can you be more specific about this bright idea? That was a very important development as I understand it.

OLIVER: Yes. Well, the question is how do you squirt a little drop of ink out in the cheapest way. And the answer to that is you heat a resistor and blow a bubble in the ink. And the increased volume squirts out a little ink. You don't need pistons, you don't need piezoelectric crystals or anything like that, just a little resistor that you can film deposit. So that was the idea. It turned out to be a winner.

NORBERG: Did all these people continue to report to you as head of R&D, even though they were in the divisions?

OLIVER: No. The divisions in H-P were considered to be profit centers and were to be managed more or less autonomously, I mean they behaved almost autonomously. However, I had a coordination function with the divisions. I was expected to go around the circuit annually and review the product programs of all the divisions and if I found overlap to call that to their attention. I'd say, "Hey you guys are both doing the same thing and no point in both of you doing it. Decide who's going to do it. We have too few people." Or if they thought that they were omitting some area of activity that should be in we'd point that out. And then we'd review the products. So I had that review responsibility along with Bill and Dave and other top people, top management people. But I had... That was, what I would call a staff activity. In other words, I would make my recommendations and if they accepted them, then they became my responsibility and if they refused them they were on their own. You see, I make a recommendation and they're not compelled to take it. But if they don't and they should have, that's their problem.

NORBERG: How about vice versa? If they take it and they shouldn't have?

OLIVER: That would be my problem.

NORBERG: I guess that implies it didn't!

OLIVER: So I continued as the director of H-P laboratories and as VP of R&D. I did this around-the-circuit review. At the laboratories, after being decimated by the break-up, we started to grow again and it became desirable to appoint a second echelon of management. So we had laboratory directors for a while. And then the number of laboratories grew and I interposed a third level: the centers. We ended up with three centers, and each center had two or three labs in it so it made it manageable.

NORBERG: When did these two successive changes take place? Do you remember?

OLIVER: No, I don't exactly. The lab directors was fairly early on, I would say like '67, '68 or so. Centers were later, they were in the '70s. The thing didn't get too big until about the '70s, see. We're, I don't know, a thousand people now in the corporate lab. I mean it's too much for one person to keep track of.

NORBERG: Yes. In choosing people in the divisions for heads of research laboratories there, did you play any role?

OLIVER: You mean in hiring for their engineering efforts?

NORBERG: Yes. And I'm speaking of the management level, not of the engineering level.

OLIVER: I didn't personally pay very much of a role. They ran their own recruiting operations. But we would often get a person into H-P corporate labs who thought he wanted to be in there, but who found that later on his tastes were more to be in the production part of things, to get with a division that was producing something. So we had an influx of talent come from H-P labs. We furnished a lot of people, having brought them into H-P labs, we furnished them to divisions eventually. And so in that sense we contributed to the personnel. We have at H-P labs a special Ph.D. hiring program. We hire a higher percentage of advanced graduate students, like Ph.D.'s, then the divisions. Divisions tend to go more or less with a master's degree, or even bachelor's degree. But for the work we had we felt that Ph.D.'s were more suitable and we hired a higher proportion of them. I guess that's about all to say about that.

NORBERG: Okay.

OLIVER: Oh, I was going to say about recruiting that it's been our philosophy that recruiting is too important a function to leave in the hands of the personnel department. In other words, the interviewing and the selection of who you take has got to e done by people with technical understanding. And we so have always asked our top people to go out on recruiting trips and experience the problem of recruiting and make contact with the universities and cultivate their friendly relations. We have also made it a policy to not fluctuate very much in our hiring. In other words, during a period of rapid expansion we don't double our rate or anything like that. We go up by maybe 20%, so that when the doldrums come along we can still hire some. And we like to stabilize the population in engineering, because we like to be able to assure people that they're not going to get laid off.

NORBERG: In recruitment, it's relatively easy to understand the criteria applied to hiring say young Ph.D's, new Ph.D's, and so on, because one can read the advertisements either in a newspaper or in HP statements, but it's less certain as to what sort of criteria are applied to the selection of say project managers or managers of the centers later on in the '70s. Can you characterize some of the criteria that you would use when trying to select people for managing?

OLIVER: We didn't select for managing much in the lab, unless you mean what criteria did we use to select our personnel manager or something like that.

NORBERG: No, I wasn't thinking of that. I was thinking of the lab directors, and then later on the center directors.

OLIVER: They were, in general, for those decades at least, experienced engineers. They had been members of the technical staff; they had been associated with one or two projects, and then they either had an idea of their own for a project or were especially excited about something and were given the opportunity to lead that development. And so normally an engineer graduated from a person working under a supervisor who was running the project to being the project supervisor or director, and then he could go from there, if he liked that sort of thing and did well at it, to a department manager that would be running several projects and ultimately lab manager and so on up. So we grew our own managers, pretty largely.

NORBERG: But not everyone can become a manager, because there simply aren't enough slots. So how does one distinguish among them? Is it a question of success or...

OLIVER: Yes. It's a question of the facility shown by the person on a trial. Suppose we give a guy a chance to do a project and it doesn't come along very well. It's slow and he has trouble getting people to get things done and so on, you can see that. So we wouldn't be inclined to do it again. Or we'd be inclined to put him in a training program and say, "Look, you're doing fine technically but you're having a problem with relating to people here so we want you to take this course." The personnel department has run many training courses of which we availed ourselves. We think that the development of those skills is sometimes possible. There's some people that come equipped with them, but others have to have them developed and we would do that when necessary. If that failed, of course, we'd have to leave that person as an engineer. There are worse fates.

NORBERG: Another item you mentioned in passing last time was Hewlett-Packard's move into numerically controlled systems. It was also in connection with these last few questions that I had asked you then. Were there direct stimuli for this move into numerically controlled instruments? For example, in 1960, there was a tape controlled milling machine that was developed. Why? Do you recall?

OLIVER: That we developed?

NORBERG: Yes.

OLIVER: I'm trying to think what that was. I know that Francis Moseley developed a tape controlled milling machine, or he made a little gadget that went on a milling machine that responded to tape control. I am not familiar with the tape control milling machine. Sorry.

NORBERG: That's fine. How about numerical control in general? Were these things... Numerical control is the wrong phrase here, automated systems in general, in 1960, '62, '64 and so on.

OLIVER: As electronic computers became smaller and more powerful, a trend that we could see even in the early '60s, but certainly came with a rush in the late '60s and '70s, it became apparent that sequencing and automatic programming of instruments was going to be very important because repetitive measurements then could be made and the data taken and reduced by a small computer. So we started a campaign to make all of our instruments computer controllable. We called it programmable, but it's a misnomer because you don't write a program for the instrument. The instrument doesn't accept a program, but it accepts commands. So we developed a language to do that in and buses to connect them up and so on. And actually our first thought was to produce automatic measuring systems as products. That has never gotten off the ground. We tried very hard at it and it doesn't work. The reason it doesn't work is that nobody can believe how much work it is to make such a system. They think that where you're buying off the shelf items all you do is put them together and write a little software and you're in. Well, that isn't that easy. There are interferences between the instruments, there are ground currents, software is not easy to write, you have to watch everything you're doing and you end up spending an excess amount of engineering on it and you have to charge them for that. They will say, "No, don't do that. Give us the instruments and we'll do it." And then they spend that excess engineering on it but they don't count it. And so that's the hooker in that thing. We've ended up selling lots of automatic measuring systems as parts. We simply unbundle them, you can have this and this and this take what you want. But all those instruments, you see, are controllable. That means they play together and they do the job and the customers are generally very happy now, but we don't have an automatic measuring division.

NORBERG: Now did any of these devices get generated because of internal needs to H-P's own production systems?

OLIVER: Yes, to some extent. Some of our first measuring systems were in the microwave region and had to do with measurements we were making on our microwave components, and amplifiers, and other things. We found a need to measure repetitively instrument after instrument in a certain frequency range and we clearly saw that we could do that more quickly, less expensively with an automatic system. So we made a lot of them for ourselves. Yes, they were used in-house. If nobody in-house had wanted them, we would have never sold them.

NORBERG: Why not?

OLIVER: Nobody would have bought them. You see, we had our own built in market research. People don't realize that. We were making instruments for electronic laboratories and for electronic production people, and we were an electronic laboratory and we did electronic production and we knew what we wanted. And if we made something and the pilot run disappeared into the rest of the plant, we knew we had a winner. We didn't have to go out and ask anybody. We had a good gut feel for it and when we were really right, which was the case most of the time, those instruments just scattered all over the place.

NORBERG: And did other parts of the plant decide to try something with automated systems in this early period when it was very difficult to do that?

OLIVER: Well, every division, I think, appreciated the need to make its instruments programmable, as we called it. And there was, I think, a pretty general tendency to use them in that mode.

NORBERG: I'd like to diverge for a moment in this discussion and using this as an example, the automated instrument attempts, to ask you about a stated position of Hewlett-Packard, the company. A number of times over the years, H-P documents state that H-P tried to do only developments that made a contribution. How was this determined? How does one determine when something is going to be a contribution?

OLIVER: We didn't like to do a me-too operation. And we found in cases where that happened that we were never very successful. That was the case with oscilloscopes for example. We didn't come into the oscilloscope field with any significant contribution. We made a pretty good scope, but it had some troubles. But people were buying pretty good scopes from Tektronix already. Tek had a big effort riding on oscilloscopes, and only part or our effort was on oscilloscopes. We could never leap frog them. We weren't really making a contribution, except when we put out the sampling scope. Now that went like hot cakes. And there have been other things... See, the sampling was jumped from 20 megahertz - top band-width at the time - to 1,000 megahertz, that's a contribution. That means that you can now look at microwaves. You couldn't used to see them. That's what we meant by contribution.

TAPE 3/SIDE 2

NORBERG: I'm trying to remember a couple of your earlier answers. Let me ask this question anyway and see whether it will elicit something else. Do you recall any directions rejected because they did not seem to be contributions? The oscilloscope example might be one of those, but I'm thinking of some sort of a new product as opposed to a product which is already on the market, perhaps even developed by someone else like the oscilloscope was.

OLIVER: It's hard for me to sort one out, but I'm sure there were many cases where we looked at what we had and said, "That is not enough of an improvement over so and so's product. We're not going to even look at it."

NORBERG: But you recall that that was a policy.

OLIVER: That was a policy, yes.

NORBERG: So there's no question about it. Okay. Did H-P have any special constraints on marketing that affected R&D?

OLIVER: The marketing problem affected the kinds of things we undertook in several cases. It's difficult to market a product to a group of customers that are not your usual customers, you know, strange customers, unless that product is accompanied by a number of others. In other words, you don't market a product to a new group, you market a line. Otherwise the salesman is calling with just a single thing to show and he's pretty scanty. The customer says, "Well, is that all you got?" And so we would hesitate very much to get into a new customer area unless we could see ourselves following up this development with several others that would also be in that area. So that's one way in which marketing has affected what we do. Noel Eldred used to have a thing that he emphasized, and that was his far-out graph. He had a two-dimensional graph. One direction was technology, the other direction was market. And down here was our usual technology and our usual market. And he would stand us being far-out in technology if we stuck to the usual market, or far-out in market if the technology was usual, but he didn't like to take things that were far out in both.

NORBERG: Was that helped by the acquisitions then? That would bring in a whole range of products and then you could just simply expand on that?

OLIVER: That was one reason for making acquisitions, was to acquire a line. For example, when we got into power supplies, we didn't just try to develop a power supply and try to market it, we bought a company that was already in business making power supplies - that it was Harrison Laboratories. And that solved that problem. They were going already gung-ho.

NORBERG: Now who was making decisions about the acquisitions then? Were you involved in that?

OLIVER: To some extent, but it was really largely Dave Packard and Bill Hewlett and, to some extent, Noel Eldred I think.

NORBERG: I see. So they would be deciding on what sort of field might be interesting to expand into.

OLIVER: We had also some people from time to time that were charged with the responsibility of pointing out possible acquisitions. Bob Rawlins was in that position at one time.

NORBERG: Were these people in marketing or were they in some technical areas?

OLIVER: John Cage did that for a while. He caused us to acquire a couple of companies. And he been an assistant to me in the laboratories here. John is dead now; he died last year.

NORBERG: Did any of these acquisitions have R&D operations going that you simply absorbed or did they stay with the divisions?

OLIVER: We never absorbed their R&D operations into H-P laboratories. They stayed with the division, company.

NORBERG: From 1950 to about 1965, the number of products changed from somewhere in the neighborhood of 100 to 1500 by 1965. Can you characterize the principal product lines again for me in that period '50 to '65? There were signal generators, there were vacuum tube volt meters, which were still around...

OLIVER: Signal generators, vacuum tube volt meters, pulse generators, oscilloscopes, microwave test equipment, that includes not only signal generators, but detectors and slotted lines and wave guide components, spectrum analyzers, and beginning in... Well, '65 is where we begin to get into the digital stuff.

NORBERG: Right, that's why I broke it at that point.

OLIVER: Yes, fine. Would you like a catalog for that area?

NORBERG: No, that's all right. I can go look for that. What I'm after in that question, in looking at those, I believe it was the 1963 Annual Report pointed out that 50% of the '63 fiscal year revenue came from products that had been developed since 1959. Now, if there's that much advance, which of the older lines were still in vogue? I won't say profitable, you probably can't remember the numbers and I don't have them with me, but were really still in vogue that you could still sell products that had been developed before 1959?

OLIVER: Well, let me answer that question in a round about way, or try to. We used to have what we called a vintage chart, which showed sales by year with each bar broken up into the year of introduction of the products. In other words, at the top would be the products of that year and the next thing down here would be the previous years products and so on down and then finally it got down to those more than ten years old or something like that. And those tended to be more or less straight across. In other words, it was as if each year's products after growing to maturity, which happened in the first year or so, then tended to produce an incremental stratum of business. And we used to look at that and say well gee, we're just integrating everything we've done, you know. But really what was happening was that we were in an exponentially growing business. If the market that we addressed had remained constant, then the products would have decayed exponentially and those vintage charts would have had falling bands on them and we would have had to introduced new products to stay where we were. As it was, the growth of the business itself shifted the band level in the whole top group. See what I'm saying?

NORBERG: Yes, I do.

OLIVER: So the moving target we had for a market size meant that our older instruments persisted in volume for a longer time than we might expect. But there were periods in which we got into especially fruitful lines, and this thing you say about the '63 Annual Report probably addressed such a thing. I forget now what it must have been, but... '63, what was that? We had the sampling scope out then, that came out in '58 as I recall. I forget now what products they were at the time.

NORBERG: That's perfectly all right. Yes. That's a nice answer to the question. One last point about it, though, how long did this persist to the present? Is there a time when vintage charts really are no longer effective?

OLIVER: Well, I think... I suspect that they look a little different today that they did then. And I also have to add that sometimes we terminated the vintage. In other words, we recognized that although a product was still selling it could sell more if we were to give it a facelift as I described earlier.

NORBERG: Yes, you described that.

OLIVER: I have an example of one that has survived for a great length of time without any attention and that is our cesium frequency standard. It's the world's accepted standard, but it could be so much better than it is if we put any amount of technology into it, you see. So we're looking at doing that now.

NORBERG: I see. Many new products often require new machines for their production. How did H-P acquire this machinery? Was it all developed in-house and was it done through the labs?

OLIVER: The principal machinery involved today has to do with the machinery to make integrated circuits, printed circuits and that's about it. We have some machinery for loading them. It used to be we had more metal working stuff and we looked more like a regular machine shop. We still do have a lot of machine shop machines, but the place where the money is going is in step-and-repeat cameras and all the stuff that you get involved in with making chips. We have started several times to make things like our E-beam equipment, but this has been such an explosive field that other people realize there's a market there and then very quickly somebody comes along with the equipment you need by having the whole ferment concentrated here in Silicon Valley, why you can just call up somebody in the local area and he can come over and show you a new machine. They're very expensive. I think we were putting in over 100 millions dollars a year of new machinery when I last looked at it. It's probably bigger now.

NORBERG: But all of it coming from outside?

OLIVER: Yes, most of it.

NORBERG: Most of it. But now back in, I've forgotten...

OLIVER: No, we made... We made some plasma etchers, for example, up at IC lab, up in building 25, when we first got into dry processing as it compared with wet chemistry for working with resists. We developed our own plasma etchers. Now you can buy them. To what extent we shaped the thing, I don't know.

NORBERG: I was looking for a note here from... Yes, it looks like in the 1967 Annual report in which there was a discussion about instrumentation, in the manufacturing of IC's there was an automatic step-and-repeat camera designed and built by H-P. Microwave detecting devices: it was a machine to make 1,000th inch diameter tungsten wire, which was designed and built here. Castings: automatic, sanding and buffing machine. Digital computers: wiring machine and a testing machine and so on. That was highlighted in the report as one of the reasons why H-P was ahead of the market.

OLIVER: Well, it may have been a good reason and we certainly have done a lot of that. All I was saying is that it very quickly happens that you don't have to make additional copies of that machine, in another year or two you can buy one.

NORBERG: Okay, but I was trying to get at the issue of whether or not H-P labs was developing this type of machinery.

OLIVER: Some of it, but not much. And a lot of that is divisional activity.

NORBERG: I had one other thought. Yes, were those machines sold to others as well, that you recall?

OLIVER: I'm trying to think of a definite instance that I can substantiate. I don't know. I think we sold the step-and-repeat camera; I think we sold the rights to it to somebody. We got into that because we made the laser interferometer. It's been a very successful product, in fact, it's the only one anybody buys. And having a good laser interferometer, it was very obvious that we should make a step-and-repeat camera, because we had the precision to do it. And so we did that but then, I don't know for what reasons, it was sold to somebody in the valley here and formed the basis of their business.

NORBERG: It wasn't someone who spun off from Hewlett-Packard was it by any chance?

OLIVER: I don't think so. There have been such cases. Trimble Engineering is a good example. They are in the navigation business. We had a project down in Santa Clara to develop a portable low-cost Loran-C Receiver, which we did, and then the decision was made not to market it. Because that was a different market, you see, we'd be talking to yacht owners and fisherman and God knows who. And so we never went ahead with it. Meanwhile, we had a program in the lab to develop a receiver to work off the global position satellites. We developed that receiver and we hooked it up to plotters and it was pretty spectacular. You could take a map of the peninsula and put in it a station wagon and drive up 101 and 380 and back on 280 and the pen would just follow the road right around wherever you went except at one place it didn't follow the road. We got all worried about it until we realized that the map was old and the road had been moved since then. That had about, I would say, 10 meter accuracy. A very nice little portable machine. We didn't make it. And about that time some of our people started saying, "Hey, why the hell not?" So Charlie Trimble, and Ralph Eshenbach, who had worked on these things, left H-P to do it themselves and they're doing great business now. So more power to them!

NORBERG: Did H-P simply release the rights to them or sell the rights to them to the design?

OLIVER: I don't know what sort of business relation...

NORBERG: After 1957, and I think you've answered this but I want to be sure, a new product centered approach replaced a process centered approach according to the Annual Report of the time. Is this a shift from the facility here to the divisions being established elsewhere?

OLIVER: Yes, it's a consequence of that. I mean, when we were all concentrated here and had 1500 products to handle, we did center things on processes - casting, metalworking, folding, and so on. Those were all integrated operations. When we broke up, then that wasn't as advantageous because different products take different processes, and you have to have access to the processes that you need for your product. You're concentrating on making the product and sometimes duplicate certain processes at your facility.

NORBERG: Oh, I see. Is there any advantage to one of these over the other when you think about them in the abstract?

OLIVER: Sure. If you have process centering it means that you can run an efficient shop to do the processes involved. But if you're scattered all over the country then you have to ship it from that shop to all of the others. So then you get in the question should we ship or should we replicate the shops and after a while you do the latter. Because it's much better for the division to be able to walk down to it's own shop and say, "Hey, you guys, you're goofing here. You're not on schedule," then it is to call Palo Alto and tell them that. Here it doesn't matter.

NORBERG: It doesn't work. What went into the decision to manufacture such materials as silicon diodes?

OLIVER: Oh, gee. That takes me back to 1954 or so. We had problems getting good diodes for our vacuum tube volt meters. The 400, let's see, yes, the 400D vacuum tube volt meter uses a diode bridge in it and a meter as some of the earlier vacuum tube volt meters did. And they had frequency characteristics that were unpredictable. We'd make amplifiers that would deliver the current to the bridge all right and those amplifiers were just as flat as they could be. They had more feedback in it than you'd care to think about. But the bridge itself was not a frequency independent device because of the diodes. And that was what was limiting us. We could get up to a few megahertz, but we couldn't go up to 10 to 20 megahertz with our volt meters in those days. Horace Overacker, who had been at Bell Labs at one time and was kind of an experimenter with a green thumb, decided he'd like to fool around making some diodes. So he fixed himself up an old drill press as a Tchakraiski puller, pulled some crystals of silica and learned on how to zone refine it and started in making diodes. And we tried his diodes and by God they were the best diodes we could find anywhere. We didn't know why, but they were damn good. So we put them in our vacuum tube volt meters and had no more trouble. But having been encouraged by making a product that really worked, then we thought, well, why not go a step further and try some transistors? And we were fumbling around with that when we decided that what we really ought to do is set up a solid state division and get into the business. And so we hired Martin Atalla from Bell Labs to head it up and formed H-P Associates. Horace went with them and we continued to make special products there. Initially products for our own use and eventually products that we marketed.

NORBERG: Well, but... Did you ever learn why your diodes were better than what was available?

OLIVER: Yes, we finally did. We finally did.

NORBERG: Can you tell me or is that proprietary?

OLIVER: Well, it's not proprietary, it's simply that they were point contact devices and the... I don't remember now specifically what the reasons were that they were better, but you couldn't use a junction device in those days, because the junctions were too big and they had too much capacitance. And most of the up-to-date diodes we got were junction devices. So what we really needed was a good silicon point contact rectifier and that's what we were making.

NORBERG: Did this occur over and over again so as to encourage integration within the company as opposed to continuing to use outside sources for supplies?

OLIVER: Well, when we had the solid state capability, we didn't hesitate to go to the solid state lab and say, "Look, if we had something like this, it would be a big help to us." And they'd turn around and make it. So we did develop a lot of in-house products, yes.

NORBERG: Now, in the setting up of something like H-P Associates, that seems like it's a separate R&D division to me?

OLIVER: It was quite separate at first. We brought the people out here and furnished them with a place to live, that is to say in the lab, and some stock rights and the idea was that they would eventually after having developed this we would evaluate it and buy it. And so eventually it was absorbed into H-P. It was a mechanism of acquiring talent in a time when everybody was after that same talent.

NORBERG: Did H-P Associates disappear then when it was absorbed into the company?

OLIVER: Yes.

NORBERG: At this time, about 1960, I'm still in that period, competition seemed to be increasing. There were many more companies involved in the same sort of business that H-P was specializing in. H-P, according to reports in several places, most notably the annual reports, reacted in several ways. Where was the competition coming from, as you remember it?

OLIVER: Well, I've never been aware of not having competition, actually. Is there a particular product line that you're thinking of?

NORBERG: Well, no. These statements tend to be rather baldly presented in the annual reports without a great deal of evidence for them and I'm wondering what the meaning is. I asked this same question of Dave, by the way and he gave me an answer, but that's... In fact, the answer was almost the same as yours, now that I remember. That I don't remember a time there was never any competition and so it didn't increase, it was there. Then the question arises why would it be stated in that way, if that's the case?

OLIVER: 1960. That is about when Japan was beginning to get on its feet and we were beginning to be aware of potential competition from the Japanese who came out with some counters and some other things in traditional H-P lines. And I guess Fluke, for example, was established by then and Fluke brought out some digital volt meters and we just began to see little companies springing up around, nibbling away at the kinds of things we were doing. And so I think the statement is true that we began to be more aware of a competitive threat. It's probably true. I think H-P's greatest weakness is that we have so many fronts on which to maintain competitive advantage. We're fighting a battle on 1500 or 6000 fronts, you know, however many products. No not that many, how many product lines we have, maybe 100. We've got all those battles, which means that we've got an enormous range of competition to consider.

NORBERG: How does one deal with that in a strategic sense in research and development?

OLIVER: Well, we deal with it by having the divisions and they focus on a fewer number of lines and they have product managers that focus on one area and study it and are aware of all the things that develop in that area. We just have somebody assigned to it, that's all, a platoon.

NORBERG: Did you begin to see the data processing crowd as competition at that time? Early '60s.

OLIVER: No. I can say that because we had not made the decision to get into computers as yet, so that wasn't a competitive area.

NORBERG: Considering that H-P has traditionally invested about 10% of its profits into research and development at least and sometimes more than 10%, how were decisions made about its allocation?

OLIVER: Actually it's 10% of sales, I believe.

NORBERG: I'm sorry, you're right. Ten percent of sales.

OLIVER: How did what?

NORBERG: How did decisions about allocation be made?

OLIVER: Well, first of all where did the 10% come from. I don't know that there's a very good answer to that question. If you go much higher than 10%, it turns out that you are introducing products at a rate that can not be assimilated by your sales force. There's a balance between the education and the hiring of them and that side of operation and the rate of productivity. And if you go much less than that, you tend to lag behind the competition is about all I can say.

NORBERG: Did you learn these things through experience here in the company or was this a fairly standard...

OLIVER: There was a time in the '50s, I think, when we almost choked the salesman. We were putting out things, you know, trying to put them out so fast and run and get ahead that they began to complain and so we had to make the decision to go to fewer, but more important products. That can always be helped by making a few big things rather than a lot of little ones.

NORBERG: Now, turning then to the break up of the 10%, how did that get distributed? Is this what you described before in terms of who comes up with a bright idea and how much is going to be in the investigative phase and so on?

OLIVER: Part of the strategic plan of every division for every year is an allocation of its resources. The division managers are free to depart from that 10%, but they have to do it for cause. It may well be that they feel they're in a competitive situation and they need to advance their engineering and they may elect to go to 10% themselves or 11% themselves, ignoring the 1| for corporate labs. But they'll have to justify that. In other words, they'll have to show that by doing so they will be in a much favorable position in two years or three years than they are. And if they are in a kind of position where they need a lot of other things like more machinery or buildings or something like that so that their budget is low, their budget is taken from other things, then they may hold off a little bit on their engineering growth. So there is a lot of local decision that comes into it.

NORBERG: Do I imply correctly from your statement that 10% of sales means 10% of the division's sales goes back into...

OLIVER: No, it's a 10% average for the company, that's the way it's held. But the divisions tend to gravitate toward that same figure, but are free to vary about it a little.

NORBERG: With some sort of tax for the central lab.

OLIVER: Yes.

NORBERG: Were there any special circumstances that affected allocation like government money for R&D?

OLIVER: When I first came to H-P, we had a few government contracts and those continued, I think, into the early '50s. But we decided along about that time to install a lot of environmental testing equipment so that we could meet the mil specs and on, in fact, we've had that equipment from the beginning we really expanded it. Having a good environmental test lab and learning how to do those things and learning how to build things so that they would meet specs, we made the decision to build everything to meet specs. And we then said to the military, "Look, if you want something you can come and buy it off the shelf. That's our policy. If you want to build a special box for it that's up to you." So that really took us out of the military contract business. We were never in it for systems design; we were only in it for instrument design or computers. So our contract work faded fairly fast. About in the '80s, I would say that our total involvement with contracts would be on the order of $700,000 or maybe a million dollars a year, and that was usually in materials development or something that was of a scientific or researchy nature, where the findings would not be a product but would be a technique. And that was done in the solid state lab. And that was about all we had.

NORBERG: I see.

OLIVER: So you see there have been companies who have grown faster than Hewlett-Packard, but they haven't grown on their own money. That's the difference.

NORBERG: Were there any times when external circumstances other than competition affected the rate of investment in R&D? For example, a downturn in the economy. Could this affect...

OLIVER: We tried to mitigate those, as I mentioned earlier. We tried not to go through years where we didn't hire anybody even though the economy was down. We have had hiring freezes, but they have been on the order of three months or so and our year's average has not fluctuated as much as that would seem to indicate. We do this primarily to win the favor of the schools. The placement offices, obviously, of the schools are very fond of you if you come around every year to interview.

TAPE 4/SIDE 1

NORBERG: I want to contrast two long periods in H-P's history. I sense that from 1939 to the mid-1960s, the period we've been talking about mostly, management, product introduction, expansion, and negative decisions insofar as there were any, all flowed from the business philosophy of Bill Hewlett and Dave Packard.

OLIVER: To a large extent.

NORBERG: My question was do you agree with this. You say to a large extent, well what do you mean by a large extent?

OLIVER: Well, by 1965, we had divisionalized to some degree and, consistent with the principal of autonomy of the divisions, we expected those decisions to be made at divisions but they were reviewed by top management. And I don't think they were very often reversed, but they were commented on. However, I will say that people went out to lead those divisions that were imbued with the philosophy that they learned in their earlier years here. So there was a spreading of that philosophy to the divisions.

NORBERG: How would you characterize the philosophy? What are its principal points?

OLIVER: Well, it's pretty well embodied in our corporate objectives. It's a question essentially of good behavior, treating people fairly, seeking the best people you can find for the jobs you have, paying them well, being a good citizen in the community you're in, and all of those things. If you haven't read our corporate objectives, you should.

NORBERG: I have. How did it affect your way of thinking about R&D? Did it at all?

OLIVER: No, it didn't affect my way of thinking about R&D, because I'd been at Bell Labs, and Bell Labs is another example of a company that works with a very high level of talent and with a good philosophy. But it did affect, having worked both at Bell Labs and at Hewlett-Packard, my political philosophy. I used to be much more of a democrat and a radical person. Seeing how well free enterprise worked in those two instances completely changed my ideas. I'm a Republican now; I believe in the free enterprise system; I'm slightly to the right of Ghengis Khan.

NORBERG: Do you think people who subscribe to the Democratic philosophy don't believe in free enterprise?

OLIVER: They believe in the government doing more things than I do.

NORBERG: Okay. That's a sharp distinction I think is valuable. Did it change your way of thinking about the company in general, that is, this philosophy. You mentioned to me last time that there was a difference between the job at Bell Labs and the job here and the principal difference was the continual pressure at H-P to get products out. To get the job done.

OLIVER: There was a much more relaxed attitude at Bell.

NORBERG: Yes. Now does that emerge from this philosophy or is that just because this is a corporation which depends on the customer as opposed to Bell Labs where the customer base was more defined?

OLIVER: Bell depends on the customer, but they're older and bigger and, as an employee, you're more insulated than the customer. I mean I'm sure that there are parts of the original Bell system, like Western Electric or someplace, where there was a great drive to get something out and on time and there was the kind of production urgency that we feel here at H-P. It's just that Bell Labs was so far removed from that. If you thought of an idea at Bell Labs, it went through so many stages before it ever saw production that you were around to something else by that time and you never felt any of that pressure. At least I didn't. Maybe I was wrong, but I didn't. But I certainly was conscious of it here, I mean we had time schedules to meet. We were expected to go to the IEEE show, or the IRE show as it was then, and have some new products to show there, by God. It was not an event that could be postponed. So timeliness was certainly something that came with H-P.

NORBERG: But one can have a situation in which there is the high pressure for new products and a time schedule to put them out and not have such a philosophy as seems to be inbred into the culture of Hewlett-Packard. And what tends to happen there is people get exploited and they burn out, as the common phrase is applied to this situation.

Why do you think that doesn't happen here? After all, the pressure must be just as heavy here as it is in some companies which are less reputable, which I will leave unnamed.

OLIVER: I think it's because of a sensitivity to the individual that is not ubiquitous. (Pause)

NORBERG: I also sensed that after, say, 1965 that the direct imposition of this philosophy began to be less straightforward. You cited one example that the divisional structure made it more difficult to at least impose it directly from the top. I would say that there are some other things that are involved, though, in the post 1965 period. I'd say that market and other forces began to play a much greater role in decisions, and the decision to move into computers is probably the best example of that. Product choices were more driven by developments in other businesses than by natural extension as they had been before. Expansion of the line now we're going into new developments altogether. For example, the early introduction of numerically controlled instruments and machines seems to fit the early style of just expanding the line, but the computers do not. Can you comment on how you appreciate this shift? Is this a reasonable statement on my part?

OLIVER: I've often said that we kind of backed our way into the computer field. Our original intent was not to make computers but to make a controller to control instruments, to permit automatic measuring systems to be made. That was what the 2116, our first computer, was designed as; it was an instrument controller. We put it out as a product and we soon found that more people were buying them as mini-computers than were buying them as controllers; there was a message there. So that's when we began to look at the market for computers and say, "Hey, we can do that too. Let's not try to take IBM on at the top, let's try to come in with some modest sized things that won't even attract their attention. But let's see what we can do." And so we began a computer line. Now this is one example of many where we got away from our original situation in which we didn't need to do market research. We did need to do market research with computers and we didn't do it very well. We did need to do market research in medical electronics and we only learned how to do it well after quite a while. In some of the areas that we've gotten into that are not furnishing laboratory instruments, we've had to learn a lot about how to make contributions in those areas. Now, where are we. That changed the company's operations some.

I think we made a lot of mistakes in computers. For example, the Cupertino people in about 1970 or so, '69 or '70, were gung-ho for a project called Omega. A product that was never brought out. It was canceled by top management. It was a 32-bit machine. It would have been the first 32-bit machine on the market. I think it would have done a terrific job, made history, but the profits of the division were down at that time and it was felt that this was too big a thing to bite off for them to chew. Well, it was a case where the corporation could have financed them so to speak for good ends, good results. But we lost a lot of good people because they said, "All right. If you're not going to make a big computer we don't want to stay here." We made a lot of mistakes like that. And I think it was a mistake, for example, when we moved the hand-held calculator division to Oregon. We lost a lot of people off the job there. They didn't want to go off to Oregon; that's the hinterlands as far as they were concerned. You know, prejudiced, but very real.

NORBERG: I certainly understand that. I have my own prejudices along those lines.

OLIVER: So we were far from perfect.

NORBERG: At times in these reports, I sense a tension between Hewlett and Packard, which is not always obvious, but it seems to me that especially around 1970 there is a tension existing of the following kind. That Hewlett's interest in the company is in broadening of product lines, whereas Packard's concerns, I won't say interests, but concerns in this case, is guarding against spreading too thin. Did you observe a tension of this kind?

OLIVER: I would never describe it as a tension, because I'm not aware of any friction there.

NORBERG: No, I didn't really mean it that way either.

OLIVER: There is a different emphasis in the two people and I've always felt that their talents were supplementary, more supplementary than identical. In other words, where one tended to emphasize things the other did less so. But now Hewlett has always been more interested in the technical aspects of the program. In other words, how instruments work and what can be done. He's an inventor and he was more inclined to follow closely the work in the lab or come in and actually suggest something. There are several projects that originated with Hewlett, I left that out when I was talking about that earlier, but it's true.

NORBERG: Such as?

OLIVER: Let's see. Well, the surveying instrument, for example, the distance measuring instrument, which I don't think we any longer make and we sold it then to Wyle. That's another example of something we developed and sold. He worked that whole thing out. He said, "Here we got a solid state diode that will put out light, we can modulate it at many frequencies on up as high as 50 megahertz, that ought to mean that we can make a distance measuring optical radar. And he put the numbers in and sure enough it was good. And so that made us come out with our first ranging equipment and then we combined that with a theodolite and we had a one instrument full station. We put a lot of work in that whole business. I hated to see it go down, I mean, I wish we were still in it.

NORBERG: All right. He also had some other things to do with light emitting diodes, perhaps, and developments like that in the company.

OLIVER: Those were used in this DMI equipment, but we're getting off the subject. Packard, on the other hand, was always very... I think his driving motivation was not to let his people down. He wanted to take every precaution he could to make sure the comp any was sound, that the company prospered and flourished. He was very concerned about the well-being of the company because of its people. And I think that's well illustrated, this difference is well illustrated by an event that occurred when he came back from Washington. He had been there under Laird as Deputy Director of Defense, you know. I think he came back in 1971...

NORBERG: Correct.

OLIVER: ...and came back to find that the company was owing some $120 million dollars they didn't have. It was in debt to that extent, short-term debt. There was a resolution put before the Board of Directors when he first came back to authorize $150 million dollars or something like that of long-term debt. The Board of Directors approved it. I was a little uneasy about that because we had never had any long-term debt before and I didn't want any and I could see that Packard wasn't very happy either. What happened was that he thought this over and he did some calculations and he figured that he could turn this situation around. He got on the warpath and he went around to all the divisions and lectured all of them about how they were going to change their ways. And one thing he said, I don't want to hear any more of is market share. I don't want you to price a product low so you're going to get a big market share. I want you to price a product so it makes money. We can't do anything else unless we make a profit. And he said, "Your accounts receivable are a disgrace, they're 120 days behind and we can pick up a lot by getting that lag shortened." We need to cut our inventories down. We've got, in some cases, years of stock in inventory. That's just stuff that isn't doing us any good. Get rid of it; use it up. Don't buy any new until you have to. It was very interesting. Instead of working with a year's inventory on hand there was kind of a race between the divisions and the backlog came down first to 9 and then 6 months and it finally got down to 3 months.

NORBERG: That's interesting. That's over 4 times a year turnover.

OLIVER: Right. And the accounts receivable got down to about 60 days. I don't know what they are now. The pricing of products was readjusted. The result of that campaign was that in one year we went from $120 million cash deficit to $100 million cash surplus and there's never been any long-term debt. I think we had $7 million dollars in long-term debt last time I looked. It's a 4% mortgage against some property in England. Now that illustrates a difference in the two people, because Bill didn't do that. He could have done that, if he had seen the thing from Dave's viewpoint and been as sensitive to the implications of a long-term debt.

NORBERG: I'll get off this subject in just one more...

OLIVER: But I'm illustrating the difference. You wanted to know the difference.

NORBERG: Right. And it was well put.

OLIVER: I think that brings it home.

NORBERG: It was well put and the word tension was inadvisably chosen here. On this question of the computer development and the shift in the company from the earlier style to the later style. How long did it take to become effective in the computer business?

OLIVER: It's taken until now.

NORBERG: It has? That long?

OLIVER: Well, yes. Look at it this way, the computer business was a rapidly growing field just as instruments were when we first got into instruments. And you'd have to be awfully bad not to be able to sell any computers. But I don't think we were very sophisticated in our computers. One of the things that we've done in the last few years is to try to correct that; we've brought in people who have great expertise in computers - you may have heard of our Spectrum line, that's going to be the next line of H-P computers. It's about an order of magnitude better in cost-performance ratio than anything we've had before.

NORBERG: Now is that your definition of sophistication, cost performance?

OLIVER: No, sophistication is the way you get that cost performance. I don't know whether you want to hear about these new machines.

NORBERG: Well yes, I do, but I'm asking another question I think, and that is, when you say that H-P wasn't very sophisticated in developing computers, was anyone at the time do you think?

OLIVER: Yes. I think that both IBM and DEC had much more sophistication. IBM because they'd been in it longer. DEC because that was their whole business and they got to know their market. And they scored a great deal by doing stuff that we're doing a lot of right now and that's by giving computers to universities so all the students there became familiar with their computers and that's what they ordered when they got out. And they also got a lot of free software and operating system design suggestions from the universities. The universities did research on the computers that they were given and the fruit of that research funneled right into DEC. I mean, they had access to a lot of great ideas. We could have done that; we didn't.

NORBERG: Was that done for Spectrum?

OLIVER: In the case of Spectrum we imported the know-how. The guy that heads H-P labs now is Joel Birnbaum. Joel was director of computer research for IBM when we hired him. He was discontent there, because of a lot of red tape and stuff. It took too long to get things done, I won't go into it. But anyway, we offered him a position here and when the word got out that he had come, applications just came in over the transom from all kinds of people. We acquired so many good people in such a short time I'd hate to tell you. And so we set up a computer science laboratory. The first product of that has been the Spectrum design. The guy that coordinated it and is responsible for a great many of its features was Bill Worley. He's now out at Cupertino. But it is a very, very smart design.

NORBERG: I want to switch to one last topic, still on its effect on R&D. In 1958, H-P established its first foreign facility. When did foreign activities begin to have an affect on R&D activities and allocations in this country?

OLIVER: Well, as divisions they of course ran their own development programs, but then there became a tendency for those divisions to pick out some segment of the market and concentrate on it. For example the Edinburgh division, South Queensferry, is largely in the telecommunications business. They make instruments and products that relate to telecommunications and they gravitated to that. They first were simply manufacturing products that were given to them from the states. But in looking for a role to play, they gravitated to that one. Boblingen has made a lot of contributions to the medical field, possibly because of the interests of the people there. I'm not sure. Also because they had access to continental medicine, which sometimes in some ways is ahead of ours. Now, you say when did it begin to have an effect on R&D, I would say as soon as the division started it had an effect because they were doing R&D. In many cases, stuff that we wouldn't be doing here. So it changed the picture and broadened it somewhat.

NORBERG: Can you give some examples then of technology flow back across the Atlantic in our direction?

OLIVER: Yes, the fetal heart rate monitor is an example. That was a machine that was developed at Boblingen and has been marketed over here, I haven't followed it for the last few years, but it's quite a machine. It's a more important machine than most people realize, because birth can be a traumatic event for the fetus. Quite often what happens is the umbilicus will get tangled or kinked and there will be a sustained period of anoxia. If that goes on for too long, then there's permanent brain damage and you get a retarded child. In fact, almost all retardation in children is caused by anoxia at birth. You put a microphone in there, an electrical contact, and monitor the heart and you can tell as soon as it's happening. And if it happens well you do a Caesarean immediately and that's it. That's saved a lot of kids.

NORBERG: Can you cite other examples?

OLIVER: Let's see, of something coming from abroad. Yes, the Japanese division Yokogawa -hp- has developed a fine millimeter for measuring contact resistance etc. It's a very beautiful little machine; it's been out for many years. I could look through the catalog and give you some...

NORBERG: No, that's all right.

OLIVER: There has been a reverse flow.

NORBERG: You didn't mention Grenoble?

OLIVER: Grenoble?

NORBERG: Yes. They are quite proud of the transfer back in this direction, I understand.

OLIVER: They have concentrated on point-of-sale devices to some extent. I'm not aware of how well that program is going; I just simply haven't followed it. That's why I didn't mention it.

NORBERG: Did you keep the same type of responsibility for oversight in the foreign facilities as well as the United States facilities?

OLIVER: Yes, approximately, although my rate of sampling them was less frequent. I couldn't go to all of them every year. We used to put on an H-P labs road show in which we took our story, our products activity to all of the divisions. Actually, it was a sharing situation. They told us what they were doing, we told them what we were doing. It was an exchange.

NORBERG: As a way of bringing this to a conclusion, can you contrast for me the company R&D in two years, say 1955 and 1980, as to what the differences and the similarities were?

OLIVER: Well, the biggest difference has been the design methodology, because in 1955 everything was point-to-point wiring with large components, and vacuum tubes. You could easily make a breadboard. It had a few active devices in it, like maybe 10 or 20 vacuum tubes. Today, very few instruments are made with less than several thousand transistors or with integrated circuits of one kind or another. And to the extent that those circuits have to be designed specially for the equipment, then there is a design phase in which you use all the technology of IC design and lay-outs, computer aided design checking, rules checking, computer aided simulation of what you've got, you simulate the device, the whole system, before you ever make anything. And so there's a long period of investment, of effort, before you get your first operating device. So it's changed the whole way the thing works. Things were a lot simpler in the beginning.

NORBERG: Well, what would you say is the company's future, besides saying that it looks good and there are going to be some nice new products on the line. Would you say that it will continue in this phase that it's in now as primarily computer company dealing with...

OLIVER: Well, it isn't primarily, it's about a 50% computer company. I hope the computers grow fast with the new line. Maybe I'm out of date, is it more than...

NORBERG: Well, no I wasn't suggesting that, but back as early as 1978 the company was recognizing, they say, that there is a perception out there in the customer base that H-P is a computer company, and that brings with it a new role, and new responsibilities and so on. Now to what extent are those new responsibilities going to be developed in the future? Now, granted I haven't told you what the new responsibilities are, I have to rely on you to tell me what they are.

OLIVER: Well, we certainly gained recognition in some areas of computers, I think the calculator area was probably where we scored most heavily. And I think we're going to continue to pursue that. I would like to see us get into some new lines, myself. I would like to see us get into composition. I would like to make a word processor that had the ability to type-set so that I could put mathematical equations in it with full beauty. I would like to have a machine flexible enough to let somebody score music on it, maybe make a machine which when you played a piece, scored the music for you. There are all kinds of things like this that we should be looking at.

NORBERG: But I take it the company's not?

OLIVER: No. I would say that I have misgivings about H-P. I think there is a tendency now to go for the quick profit that wasn't there under Bill and Dave. The bottom line on this year is more important than it used to be, at the expense of things that lie farther ahead. I think we're sacrificing long-term research and not giving it the weight we should.

NORBERG: But this seems to be a common phenomenon throughout industry today.

OLIVER: I know it is, but I'm saying, "Why us?" And I also feel that we ignore a lot of things that are nice contributions now because they're too small to bother with. I don't see really any reason why something that makes a ten million dollar sales increment shouldn't be of interest to us, especially if it's a nice thing. We have had a great deal of trouble keeping our frequency standard business alive, because nobody in the company wants to bother with it. Yet it's very prestigious. H-P is looked up to as a highly capable company partly because of the frequency standards that we've had. We've been able to out-do the competition for years, but that product hasn't been redesigned for 30 years or so now. Twenty-five years, I guess. And it's high time it was. I see a lot of things that need attention and they're not getting it. But I'm getting old and crotchety.

NORBERG: In any interview of this kind the questions are obviously selective. I selected them because of what interests me in the company and what stood out as I read the reports and various statements of the company over the years. But invariably you miss some things. Now in the specific questions that I asked when I seemed to miss something you brought it in. But are there areas that I have not discussed that you think are at least equally as important if not more so about your activities here at H-P?

OLIVER: Well, we didn't finish the discussion that you asked a while back about what made the difference in atmosphere here. I said concern for people I thought was a big factor and I'd reiterate that. I think I told you before a story about the bonus plan and Dave's reaction to John Fluke's questioning on it?

NORBERG: No, you did not. You may have told me privately, but it's not on tape.

OLIVER: Well, this is back I guess about 1954 or so. I'd been with H-P only a couple years and John Fluke was setting up Fluke Industries in Seattle. One night in the seafood grotto of the Sheraton Hotel in Chicago - the place no longer exists save in memory - he was admiring our bonus plan, which was to give all the employees each month a bonus computed on the production of that month. It was smoothed a little bit, but there was a formula for it. When I first came it was around, oh, I don't know, 25% or something and by the time the year had elapsed why it had gone to 60%. We raised everyone's base pay and cut it back to 15% because we found people didn't believe the bonus would stay. They wanted a higher guaranteed salary. And so then it climbed up again to about 70% before we finally did away with it. And Fluke was admiring this; he thought this was a great thing and I expected Dave to agree with him. And Dave said one thing that I've never forgotten. He said, "Yeah, it's probably all right. It probably doesn't do any harm," but he said, "I'll tell you this, that if your people think they're working for you a bonus plan isn't going to help you. And if they think you're working for them, you don't need one."

NORBERG: Now can you explain that to me? I really don't think I understand it, Barney.

TAPE 4/SIDE 2

OLIVER: ... If every employee feels that management is working for them rather than the other way around, you don't need a bonus plan. You'll have good morale. If your people see that the decisions of the management are working to the advantage of the company and if managers always come around and are helpful when they make contact, if they find out employee problems and try to solve those problems and don't shove them under the rug, in other words, if they do their job, that's what management is all about, you don't need a bonus plan. You'll be a healthy company, you can afford to pay a higher base.

NORBERG: Any other issues?

OLIVER: No, that's good.

NORBERG: Well, I want to thank you very much. This has been quite an experience for me.

END OF INTERVIEW