SOHP Interview with Martin Packard
May 12, 1988

Lowood: ... Martin Packard.

Okay, let's start with your family and your childhood. You were born in Eugene, Oregon in 1921. First of all, were your parents and family from the Northwest?

Packard: Yes and no. My mother was from Southern California. My father was from the Puget Sound region, having come from New England; he grew up as a young boy in New England and then came out with his parents to Tacoma, Washington. He first started his undergraduate years at Puget Sound College, which was a very small liberal arts college - it still exists, I'm sure - extremely small at that time. My mother came up from Southern California, Pomona, to the University of California, and so they met there when he was a graduate student and she was an undergraduate. She actually did not finish her degree at Berkeley. He got his first job at Washington and she finished up at Washington.

Lowood: And what field was he in.

Packard: He was probably in the Department of Geology, Zoology, interested primarily in antology. He did an interesting study, which I had no awareness of at the time, on the oyster population in the San Francisco Bay. And interestingly enough, it is a publication which is still looked at. I couldn't look . . . . oh, a few years ago, we received about various publications in the family, and that up. They were
offering me some [of the money?].

Lowood: Did he pass on any of that interest to you?

Packard: Well, not in a geology sense. It was hard to know why; I used to go with him on field trips to eastern Oregon. They set up camps where the kids would go out and do the surveying, mapping the geology, takers, so I used to go over but never as a geologist; never really interested in that aspect. I was more interested in the camping trips and . So there really was no rubbing off of the lifetime biological aspects of it.

Lowood: And then in Eugene, was he connected with the University?

Packard: He was in the geology department, moved from Washington down and actually went to Mississippi for a year and then came to Oregon and he was on the staff at the University of Oregon. Now, in trying to reconstruct why is it one chooses a career, I'm sure that it had a bearing, the fact that his friends, and therefore the children that I played with, were from faculty families or intellectual, academic people. So, by and large, our exposure was more to that kind of a person some of the other people that resided in Eugene.

Lowood: You father's friends were from various departments there?

Packard: Yes, they had set up - having been a small community, one had to make an effort to bring together people with similar interests - and they had what was called a Supper Club and they met as a group for dinner about once a month. As kids we got together in family groups at Christmas and so on, once a month, the family . There was a broad range of people:
psychologists, economists, physicists and chemists, biochemists. They were supposed to ___ their town beside the Eugene ___ a fairly sophisticated group of people, but I was never aware of anything like that.

Lowood: Was there some kind of a book culture in your own family, or was it more out-doorsy, following your father's interests?

Packard: Not very. I think their personal interests - my sister was five years older - wasn't so much oriented towards books. Of course we had books, but I was always a steady client of the library, and I used to use it, used to read a great deal. I can remember my mother saying, "Time to turn the lights off."

Lowood: And any kind of fiction, science fiction . . .

Packard: Oh, a wide range of things ________. I can't remember any particular area.

Lowood: Was there anything in your early childhood that maybe pointed towards an interest in scientific or technological things?

Packard: Well, _______ was attracted by electrical things at a very early time, and I guess one of the first things that I can remember was a crystal set somebody down the street and I did that, that service[?]. ________. But apparently I had some interest in it fairly early, ____ stage.

Lowood: And would that be something that you would spend whole days and evenings on?

Packard: I spent a lot of time _____, yes.

Lowood: What about other kinds of things, like chemistry or mathematics, puzzles.
Packard: No, I was less interested in chemistry. I remember once when my father assembled a chemistry set for me, but I wasn't terribly interested. But I did those - as time went on, I developed more and more fairly simple skills and getting to be more complex as time went on. The number of ___ projects which were not very sophisticated by today's standards, but I can remember working with photocells - I don't have the slightest idea where I got the photocells, but they were vacuum photocells and then we'd make a communication link with mirrors, ______, something like that, that sort of thing.

Lowood: In Eugene, would there have been something like a radio store, something for hobbyists or anything like that?

Packard: No, nothing like that. It just really didn't exist. And I was only ten years old when I left Eugene, but I do remember the crystal set in Eugene. I guess that's about all, but then this continued as we moved to Corvallis.

Lowood: Corvallis was Oregon State then. ________________.

Packard: Well, at that time - this was sort of in the middle of the Depression, so what they did, they got the very logical idea that at the University at Oregon State, that they would set the physical sciences at Oregon State and keep the liberal arts at the University of Oregon; a scheme which never worked. So we went over and [my dad?] was the first dean of the so-called School of Science at Corvallis.

Lowood: So you have any idea why he was chosen for that?

Packard: No, no I really don't. I don't know what other talent was available. It was clear that he had always had a strong interest
and awareness of research - the development of papers, publications was important to him. I had a feeling that that entered in. He did teach, and was a good teacher, a good professor, but he also balanced the interest in research which he actually continued to do until about eighty-five ['85?]. What you get over here at Stanford down at the Menlo Park Geological _____.

Lowood: That seems to be a characteristic of earth scientists.

Packard: Yes, I think the one thing that . . . . I always used to say that's great, because what you learned a hundred years ago is still useful. It's not so in some of the physical sciences.

Lowood: And I think all that walking around ____________.

Packard: Well, that was a good time[?].

Lowood: So, at home then, were both parents pretty supportive of learning activities and that kind of thing?

Packard: Well, I think so, but they didn't seem to be exclusive. I think it's a[?] very good thing to be inclusive, but that's just the way. I had no particular emphasis.

Lowood: So then your secondary schooling - high school, I guess junior high school, _____ middle school - that would have been in Corvallis.

Packard: That was in Corvallis, that's correct.

Lowood: Were there any teachers there, particularly in high school, that stand out in your mind as being an influence?

Packard: I think probably not. I do remember I had a biology teacher; she was quite a good teacher. But it turned out that sciences, physics, the teacher was a disaster, and finally they
promoted him to be _________ at another time. But despite
that, that apparently that didn't have any bearing one way or the
other.

Lowood: In a situation like that, were you precocious? Did you see
the problems with the teacher?

Packard: Well, I think that's undoubtedly true — this was for quite a
while — there was a lot in the school curriculum that I didn't
find terribly exciting. A lot of the _____ was quite boring and
an interest ________.

Lowood: You looked forward to college as a result of that, or you
didn't look forward to it?

Packard: It didn't enter into my mind at all that I can think of.

Lowood: Was that fairly automatic then, that you would go on to
college.

Packard: Yes, that was fairly automatic, and why I'd go to Oregon
State was supposedly because it was cheap, I lived at home, and
tuition was ten dollars or something. That was just a natural
thing to do.

Lowood: So up until this time, until let's say to the age of
eighteen or nineteen, you hadn't really moved in any particular
direction.

Packard: Well, I must have to the extent that by the time I went to
Oregon State, I knew I was . . . . well, I can't remember whether
I could have registered in the physics department, or whether
they had an undergraduate ________________.

Lowood: Oh, I see, as a physics major.

Packard: Yes. And where[?] I came from, I don't know, except that
when had an interest I'd worked in a radio store, selling car radios for . I was doing these kinds of things during high school. We did know one of the physics professors by the name of Yunker. I suppose I had known him from family connections, and had respect for him.

Lowood: What's the name again?
Lowood: Did you consider engineering as a field ?
Packard: Well, I did, on the advice of, again, one of my parents' friends who was in the engineering department, and actually sort of tentatively switched my major to engineering in my second year, the sophomore year, which was a useful experience in learning how to do mechanical drawing and that sort of thing, but that one quarter convinced me that I was not the engineering type, so I went back to the physics.

Lowood: Before I ask about the courses and things in college, I'm just wondering - you would have probably started college around '39 or so . . . .

Packard: '38. I had started a half a year early, because they had some birthday in March, and apparently that allowed me to start, I guess in the middle of the year, and then, in order to get us equalized I was a year ahead.

Lowood: What I'm wondering is, in Oregon one would think those were fairly eventful times for a number of reasons: number one, Depression and sort of pulling out of the Depression; number two the international situation. Did any of those things have a bearing on your career decision and the decision to attend
college or any of that?

Packard: Yes. I think I graduated from high school in 1938, and started at Oregon State in '38. No, at the high school stage it really didn't. In college, we had some feel for it because I took German my first year, and another guy and I were going to go bicycling in Europe, but Hitler's time[?] struck that off. We had some awareness of the international climate. I think Oregon State in those days was pretty well international situation cut that off, but, as I said, Oregon State in those days was pretty isolated. One of the sort of cultural things that they worked out was to set up a series of invited artists, musicians to come and to give concerts in the gym. You know, nothing[?] like here where you can go to something every night if you want to. There would be three or four or five of these a year. Not much of[?] a cultural impact from the outside world.

Lowood: So, as far as you personally were concerned, you were still pretty much moving forward on the same momentum; the Depression and the war and these kinds of things didn't deflect you too much.

Packard: They didn't really. I was, of course, well aware of the Depression, working in the radio shop or delivering newspapers; I did that, and I had endless problems collecting from people and I had to learn when going to have thirty-five cents so I could bill them. But of course as a family, even though the standard of living dropped, it was never a hardship. We did everything we could to save expenses and it was
not a hardship situation for us, in any way.

Lowood: Okay, let's go back to college. You said you had been a physics major. What sorts of courses did you take as a physics major?

Packard: It's a little bit hard to remember. Apparently I had the sort of elementary physics which even involved laboratory, and there was a course taught by a man by the name of Brady which was called "Modern Physics" which was really the . . . . he was up to date, and he followed some __________. I think the hydrogen cycle of the sun was just beginning to be understood, and so he talked about that. So it was a reasonably good course. A course on optics, followed by a thermodynamics course which was really poorly done. Let me see, I don't really remember too many of the __________.

Lowood: Was there anything like an honors ______ or something like that?

Packard: No. That wasn't recognized. Mathematics, I got in the mathematics requirement. At the later end, there was a course which was sponsored by the U.S. government - and I've forgotten what it was called. It seems I want to say EDTA, but that's probably not right. But it was a defense course, and this was really very good, because it brought in some of the latest electronic techniques. Some of the better people, Yunker ______ performed[?], and there was a fellow from engineering who apparently too a course during the summer, and they learned about _____ radar technologies. They learned about _____ and all these great ____. They learned about propagation. They
learned about these kinds of things that were beginning to be so
important on the radar circuits[?] and ______, and sube[?] of
course. We had _______________ I don't even think they had
.... I had some suction[?] for that. Some of the courses
which I taught were undergraduate.

Lowood: So the idea of this particular course was to create a pool
of trained young people, recent graduates.

Packard: Yes, there was a little of that awareness of what was needed
and what was known to be available. It was really very
punctual[?], because it did a lot to elevate the smaller schools,
particularly in Oregon, Oregon State[ ].

Lowood: And probably the thinking was not that the students that
took these courses would become radar operators on ships or
things, but that they would work in industry.

Packard: I think the war effort, yes, clearly. I think it was more
fundamental than that.

Lowood: I remember one of the earlier interviews with Doug Englebart
who's also from Oregon, and also eventually went to Oregon
State. He was a radar operator in the Navy during the war, but
he had that training in high school, right after getting out of
high school, not as a part of college. So it fit in that for the
more advanced college students that they would then be prepared
to perhaps ____________.

Packard: It was a good contest[ ].

Lowood: Jumping ahead a little bit, did that partly shape
your ________ into it ________.

Packard: Oh, I think it must have, because I did go to work for
Westinghouse. I do remember somebody from the Radiation Lab - I probably could recall the name, he was a well-known figure - and he came out and put in a very strong sales pitch for why we should come back[?] to the Radiation Lab.

Lowood: The MIT . . .

Packard: MIT, yes. Which I didn't. Something made me decide on Westinghouse. You know, in just trying to reconstruct the , were not ______ company by ________, not like for a thousand companies[?] ________. And Westinghouse had made a big effort to develop a scientific image it had at the research laboratory - they called __________. I was ______ that was __________ and I decided to go to Westinghouse.

Lowood: But this training program certainly helped you.

Packard: I think that helped, and also I think it made a difference there, because the pattern at Westinghouse was to bring young kids in, and they would assign them someplace. The normal pattern was that you'd go here for six weeks or something, and then you'd go here and then you'd go here and so on. So they moved around through the company. With several of us, the first place that went was where we stayed for the rest of the period. So there was somebody at Westinghouse who was good enough to identify people, the young students, and assign them into areas where they fit in. In other words, I never was assigned to rotating machinery in East Pittsburgh, and some of these other activities, but went immediately to the research laboratory and worked with ________.

Lowood: As a student, did you have a sense that these initial
courses you described - thermodynamics, mechanics and that sort of thing - just the way you described it, you didn't seem terribly enthusiastic about it.

Packard: No, it was not a very good course.

Lowood: Did that compare when you then took a class like this sort of what we would call "cutting edge" field like electronics at that time? That must have seemed a lot more exciting to you.

Packard: Yes. That's very correct. At Oregon State there would have been an engineering course in electronics, but I don't think I took it. Terman, at that time, was famous up there, and he [wrote?] one of the books. There must have been a quirk[?] because I think I had Terman's book.

Lowood: His book on radio engineering.

Packard: Yes. The very first edition on radio engineering. And then you asked [about my] association with Terman. At Westinghouse, I remember that I taught[?] the radio engineering handbook, and I just started at the first page and just looked[?] my way through that continuously. An hour or two in the evenings, I'd read a number of pages, and go on through. At Oregon State I also took a course that was put on by a man who was the chief engineer for the radio station, when the radio station served the Oregon community and was physically located in the physics department. So he taught what an engineer would teach about trying[?] to run a station. I did get an offer and advice[?]. Sometimes we'd be scheduled to go in and watch the dials[, and run the station, and hopefully it wouldn't fall apart.

Lowood: Why would the station be located in the physics department?
Packard: Well, they started that way. I don't know whether there was some physicist that was interested in it, in the early days, or it just was an interesting question.

Lowood: So there certainly were a variety of things that were making you more interested in electronics, ________.

Packard: Yes. I mean it was there, but, you know, all these things are self-selecting; you select out of the environment that which interests you, and that ________.

Lowood: One last thing about your college education. How mathematical was your training?

Packard: The math department was strong at Oregon State at that time. The head of the department was a man by the name of Milne. He was a very confident mathematician, and particularly in numerical calculus. One of the courses I took from him was ______________________. He was one of the __________________ numerical calculus with them as computers became available.

Lowood: So that was one he had designed himself?

Packard: Well, not the calculator, the course he designed, and he knew a lot about the algorithms that went in ____________.

Another strong mathematician . . . but so we had two or three fairly good math.

Lowood: Would you have had a course in mathematical physics or something like that as well?

Packard: I don't really remember.

Lowood: I was just wondering if, you know, a lot of things like how fluent you were with things like the self-standing wave problems,
and all those kinds of solutions to certain kinds of physical problems.

Packard: I wouldn't have been a very sophisticated _____, the pure math.

Lowood: It's more of a formula approach, ________.

Packard: I think so, probably.

Lowood: Okay, so you completed your degree then in '42, during the war, and you've already mentioned that you went to Westinghouse. Were there other options open to you when you graduated?

Packard: Well, certainly the one at the Radiation Lab. But I really don't . . . . I guess I must have been interviewed by General Electric too, for the _____ General Electric. I'd forgotten about that. Probably the science image of Westinghouse made me choose Westinghouse.

Lowood: At that time, as you were being recruited, say by Westinghouse or GE, as opposed to something like the Rad Lab, was the focus still on war-related projects almost exclusively, or were there other kinds of things that you might have been recruited for?

Packard: Well, by that time we were in the war, and so something was ________ and there were no other thought in anybody's mind.

Lowood: And do you remember who recruited you for Westinghouse, or how that worked?

Packard: No, the best I recall was that ________ personnel type.

Lowood: No somebody that visited the campus and gave a speech.

Packard: No. The man from the Radiation Lab was, he was clearly a
Did you consider at all going into the military?

Yes, curiously enough I did. Yes, I'd forgotten about that. I became intrigued by one of the special programs that the Navy had. I don't know how I learned about it, because it was one where we're in the Navy, but focused on scientific questions. I'd been in ROTC the first two years, which everybody took, and then I went on into the advanced portion of it, but was rejected because of ___________. They decided that I wouldn't make a very good engineer. Actually, it ended up that, but I was in - you see, at Oregon State they had in ROTC they had an infantry, artillery and engineer course - and somehow or other I pulled strings enough so that I got in the engineer course, rather than the infantry or the artillery. I think I would become the scoutmaster; I had ___________ sargeants in that group. And I actually signed up to the advanced ROTC and I was selected as a sargeant-major, or something like that, as a junior, but he didn't get to wait ___________, so I dropped out of it.

About the same time, or maybe a year later, the Navy course sounded interesting, and I went up to Portland for a physical, and they rejected me for the same reason. That was fine[?]. ________________ was useful _____.

Let me flip the tape over.

[END OF TAPE ONE, SIDE A]
Packard: But along those lines, Westinghouse handled the problem of deferment well because, independently of that, I never went for an examination or anything else, completely free of that. It occurred continuously on the basis of contributions ________.

Lowood: Okay, so that would have been your ________.

Packard: That's correct, yes.

Lowood: So where did you go first after you chose Westinghouse?

Packard: I was assigned immediately to East Pittsburgh research laboratory, which was the central, Westinghouse central laboratory. And they had just set up a rather new group devoted to radiation, radar systems. In that group they had a group working on magnetrons - that's the group that I joined, which was working on T-R boxes, Transmit-Receive switches. It was a one-man group up here working on high power and very high power tubes, a man who came from the University of California, and then came back to ______ because his name was ______. And then had[head?] a small group working on crystal detector devices for rectifying signals. So it was a group which was working on components, rather than systems ________.

Lowood: You were hired with the title of "research engineer" or was that . . . .?

Packard: I think probably, yes.

Lowood: And you had a bachelor's degree in physics.

Packard: Right.

Lowood: What was it like at Westinghouse? What was the environment like? What kind of labs were there?

Packard: Well, this was a new lab, adequately equipped, an adequate
infrastructure, good electronic support, usually job support. But we really had what we needed to get the job done really. The group was, by and large, confident, people that I worked with. The group leader was a man by the name of Krasik, a Ph.D from someplace. He went on to head up one of the major atomic energy activities that Westinghouse had ___________. A very confident, very easy man to work with. He was from that era when he went to work for Westinghouse or any of the other companies, you always forgot to mention that you had a Ph.D - you did much better getting a job. And the other guy I worked for most closely was Dan Alpert, who ______ through most ____________ the history[?].

Lowood: And Alpert, was he a leader of one of the groups that you mentioned?

Packard: Krasik was the leader of the T-R Box group, and Alpert sort of __________ under Krasik because on the other side was a theoretical man by the name of Ted Holstein, and again a confident __________.

Lowood: Fred Terman, in an article that he wrote about electrical engineering education, said that one of the great mobilizing force or factors for engineering education after World War II was the fact that all the jobs that engineers should have been during, physicists were doing during the war, because engineers in their training were so far behind . . . .

Packard: They weren't able to do it.

Lowood: ... they weren't able to do it. What was the sense in the Westinghouse lab? Was it pretty much people with physics
degrees?

Packard: Well, that's an interesting thing, because you see the laboratory was headed up by Condon, who was a physicist, and, by and large, he attracted into this section, those people who were physicists. In the group that I was in, Krasik had a degree in physics _____ Mid-West school. Alpert had a degree from Stanford. Holstein, a theoretical physicist from around the Pittsburgh area. So those were the three principal investigators, and then there were the two _____ and me and another guy, who were the junior support people. We were in a group that had competent physicists and a broad range of training.

Lowood: Now Alpert, you just mentioned he was from Stanford and he was also the person that, I guess of the people you've mentioned, you worked most closely with.

Packard: That's right.

Lowood: Do you know what he'd done at Stanford?

Packard: Yes. He worked with Hansen on a device called the monotron, a reasonably unsuccessful klystron m___ tube. Apparently it did work, at least well enough to get him a __________. That's as far as it went. He worked closely _______.

Lowood: And he specialized in the same kinds of, some of the kinds of physics as Hansen in microwaves.

Packard: The answer is yes. It was interesting enough - the background was quite well suited to what he was doing at Westinghouse, whether he had chosen because of that, or whether he chose the project because of his _______ skills, because
the monotron is a big tube, which involved microwave power, involved cathodes, involved ________. So Westinghouse undertook to make a device which was a cavity, involved microwave frequencies. He had had enough of cathodes, so he wanted something that did not have a cathode in it, so that the two tubes that can produce ________ produced ______ cathodes _______. But there was a lot of vacuum technology in it, so he could bring the microwave in the vacuum and the cavity technology which fitted very closely in with what we were doing, and whether he was included for that reason or not, is an interesting question to ask.

Lowood: Were the tubes in fact magnetron-type devices?

Packard: More of a klystron type of device, because you see that was in the physics department when the klystrons had been developed. He must have gotten his degree in '40, '41, and the klystron would have been invented a few years earlier than that.

Lowood: I see, so he worked on that project when . . .

Packard: Yes, he would have known Ginzton ______ worked closely with Bill Hansen. Hansen was probably his major professor. He would have known of Bloch and worked with Webster. He was ________.

Lowood: The business probably for you, when you began to hear about what was going on at Stanford, at least a little bit, here and there, you'd get a little information about it probably.

Packard: Yes, apparently.

Lowood: Did anybody from Stanford drop by at all?

Packard: At Westinghouse?

Lowood: Yes. I'm thinking of, you know, maybe Hansen giving a
Packard: Well, Hansen was up at radiation. I don't remember Hansen ever visiting [____], but he was already beginning to develop ill health, poor health, and he did come back to Stanford [__________].

Lowood: And I suppose Terman would not have dropped by either.

Packard: No.

Lowood: Didn't [______].

Packard: Yes, he was working with the jamming end[?] of it and [______].

Lowood: Do you remember the names of the tubes that were developed at Westinghouse in your group?

Packard: Well, the tubes, the ones that we did [______], 124[?], and later on we developed a [______] which is a cavity for beacon transmitters set to frequency radar [______].

Lowood: And these were in use during the war?

Packard: Yes, this one was built when I was in China a couple of years ago [______] manufactured in [______]. This was [______].

Lowood: This is the 1B24, the one I have [______].

Packard: I've got a story which I have to tell, and I've told a million times [______]. The first two, which were just [______] like this, and this was actually for Westinghouse too. Because of the waveguide sizes it had two holes through here, whereas originally they had round waveguide sizes and then converted to rectangular sizes. So we just didn't drill this hole and notched it here, so that it fitted inside the four screws of the [______].
So I'm saying it's still manufactured exactly the same way we did it, which was an expedient solution to a change in the compatibility.

Lowood: consistency.
Packard: Yes.

Lowood: Now this one that I'm holding in my hand . . . .
Packard: Now that's a klystron and those were manufactured by Westinghouse in Bloomfield[?], so I used to go down to Bloomfield[?] occasionally, because of this, to participate[?] in the production at Bloomfield[?]. They were manufacturing this, working with Ginzton and others at Sperry.

Lowood: So this would have been a licensing deal between Sperry and Westinghouse.
Packard: Yes, and that was typical. I think it probably is a Sperry design; you should ask Ed one of these times.

Lowood: Yes, it looks a little inconsistent[?].
Packard: I think it was typical[?]. We used some of these at Westinghouse for research labs, but mostly we used the Western Electric 723 - it's a very small tube and adequate for .

Lowood: Do you have any sense of what kinds of systems the various tubes were used in during the war?
Packard: Yes. These were the three-centimeter systems and they became the work parts of the airborne[?]. We also built the K-band[?], thirty thousand mega. But that became not accepted; it really didn't show on partially because of the absorption of the water base that people found out about
afterwards.

Lowood: In '42, '43, there still would have been a lot of secrecy associated with the klystron.

Packard: It was all highly classified.

Lowood: More so, the way I understand it is that the klystron work was certainly a lot more secretive than is possible at _______.

Packard: Correct.

Lowood: That say magnetron work would have been, because the Germans, up until fairly late in the game, hadn't put their hands on a klystron yet. Does that sound right to you?

Packard: I really don't have _______.

Lowood: The reason I'm asking, for example for airborne, magnetrons continued to be used, shall we say, a little past their optimum usefulness because of not wanting to have a plane shot down with a klystron on it _______.

Packard: I think you might want to check that with somebody, because that may not be correct. My understanding is that magnetrons were produced by the British and they were very good devices. A very early decision was made to use klystrons for the local oscillators that used magnetrons as a power source. Sperry had worked on doppler radar systems, which required a very stable source, but the movement seemed to be towards pulsed systems; things that ____ pulsed operator ____ to come back again. So I think it was a deliberate decision to use the magnetron as a high power source and the klystron for the local oscillator. And then - and I think I've heard Bill and _____ say this - that they came back out here to work on the _______ realized that
you could make very high power klystrons, so the movement was in
that direction, and the movement at Varian was to make some very
high power klystrons. First, some of their earlier ones for
TV[?] performance, some of them for propagation studies. This
was an early antenna built in Cheyanne Mountain, up over near
Colorado Springs _____________ up two thousand feet.
And ___ microwave first, high power was what they used for
propagation studies out over the ________ and discovered a lot
of things. So that's really the way work came out, not the
other.

Lowood: I see. Were you thinking about things like the high power
klystrons at all?

Packard: ______. No[?].

Lowood: And it looks from these devices, you certainly weren't
thinking about that during the war.

Packard: No, we were concentrating on this, and because
of ___________ towards devices with cathodes in them, we
wouldn't have gone into anything like that.

Lowood: And you pretty much stayed in the Westinghouse group then
the three years or so after your graduation at ________.

Packard: Yes.

Lowood: And was it pretty much the component work?

Packard: Yes, it was at the ____ group, and really, we worked on
these two devices only with another __________ with the
K-band.

Lowood: So, since you didn't work on the radar systems beyond these
components, how would, say, a problem be posed to you? What
would be specified and then how would that be relayed to you by the ________?

Packard: Well, the design, the frequency, for example, was all scaled and determined by a whole scheme[?] of ________, and later the electro-magnetic theory - the more junior people like myself were involved in some of the more practical problems. For example, one problem was that these crazy windows could break. So Alpert came up with this convoluted design, and so one of the things that we did was to devise a ________ which would cycle windows and probably look at _______ temperature up and see how it would break. In the case of these things, we had to determine frequency very precisely, and so that meant checking basic frequencies against WBB[?] building measurement instruments, which would allow us to assess the cavities very accurately - watch for the drifts, see what they did. We had to measure Qs[?] of cavities like this in the construction process. Oh, I remember people liked _______ and then this other ________ Q[?] would drop off, so we had to figure out a way to _______ down and what we were going to do about it. So basic design was handled by Krasik and Holstein, and then we were helpful in making the darned thing work.

Lowood: Solving the puzzles.

Packard: Solving the problems, anything that you don't anticipate.

Lowood: Did you work at all with people who were at the system level?

Packard: A bit. I remember one time _______ went down to Baltimore and worked on some of the early radar sets there. Again, more as
an operator understanding things[?] familiarity what you can do with it and all. That was ten centimeters. As far as working with the system, no, although near the end of the war some of the guys that I worked with did develop rather complete radar systems packaged could go in an airplane. So I did have exposure to that.

Lowood: Incidentally, did you fly? Did you have a flying license or anything like that?

Packard: No. Never even went[worked?] on an airborne system.

Lowood: To get sort of a full picture of the impact of the war, I want to ask about some of the other activities associated with a lab like this. Were there any organized lectures or sort of continuing education things that happened at Westinghouse?

Packard: Yes, and probably the most important was a very complete, sophisticated, after-work lecture series which Condon gave on electro-magnetic theory of microwaves and micro-magnetic theory. And, as you know, Condon was a very competent theoretical physicist. He did a good job; they were very helpful.

Lowood: And this is what from your earlier answers had been missing somewhat in your teching.

Packard: Yes, that's right, yes. I didn't have very much of Maxwell's equations.

Lowood: Was there anything else like that, besides Condon's lectures?

Packard: That's the only sort of educational management training that I remember. We really didn't have anyone.

Lowood: Was there any sense, in a more general way, about the
atmosphere, and maybe the funding level, the level of the energy - did that make an impression on you in any way or point you in any particular direction?

Packard: You're thinking of at Westinghouse or just in general?
Lowood: At Westinghouse and maybe even the war effort in general. I'm thinking that one could see, just from what was accomplished in those few years; with the large infusion of money, with the labs that were put together, a lot could be accomplished. Did that move you to continue in the field or to do any particular things after the war?

Packard: Certainly not directly. I knew, for example, that Condon had wanted to establish research laboratories in this area, but it sounds[?] quite impossible that sell-out is a concept in Pittsburgh. I ______ to remember whether or not I expected things to go depression[?] or not, and probably we thought more that it was going to be a serious readjustment period. The rest of the group that came out with me to Berkeley - ______ went back, Alpert went back, N_______ went back, and a couple of other guys; they went back to Pittsburgh.

Lowood: To Westinghouse.
Packard: Yes.
Lowood: Did the ones who didn't have Ph.Ds yet, did they also go to graduate school like you did?
Packard: What McK______ did, he chose what I always felt was the hard way, and he continued his work for Westinghouse, sis get his Ph.D. at Pittsburgh. I think probably, maybe that's an interesting thing, there probably was nothing in Arthur's mind
that said that he shouldn't go out and get a Ph.D. And interesting enough, he came from a family of an entirely different type of background. He lived with his aunt down in Southern California; in other words from the other end of the spectrum from my end of the spectrum. On the other hand, he had a child at that time, and so he felt the obligation to be able to feed the family. So he almost had no choice but to go back to Pittsburgh. He worked hard and got his Ph.D. But again, whether it's self-selection, or whether he, having been with the group of Alpert and Krasik and those people with Ph. Ds, just accepted that's what you do. That could very well be.

Lowood: Did you go to Berkeley before the war ended?

Packard: Yes. I went there in January of '45.

Lowood: What was the reason for the move?

Packard: Well, I don't know what happened in the higher echelons of Westinghouse, but we'd pretty much finished the job with this[?], although I actually stayed another month or two because there were some problems I was working on with Bloomfield, which we were putting in on the production. Westinghouse had had an effort under way at Oakridge on big mass spectrometers for the isotope separation. Westinghouse supplied equipment and all. And so I think that somebody in management thought that it would be helpful to have a team out here; exactly why I don't know.

Lowood: And that's what you worked on, the mass spectrometers at the Lawrence Lab.

Packard: Yes.

Lowood: I see. That's not obviously related, at least to me it's
not obviously related to what you'd been doing in the previous three years. How did you get moved into that group?

Packard: Well, they moved a whole group, so the whole group came as a group. Westinghouse had had some experience in isotope separation. A very competent engineer by the name of Joseph Sluppian[?] had some idea of ultracentric[?] fuses[?] and other devices for separating isotopes. And I guess probably that what Westinghouse wanted to do was to edge all its bets and to see what was happening here at Berkeley, see what direction it's going to be, in case they wanted to go into the business of large scale isotope separation. Even at that time, Westinghouse surely must have known the potential of the power production.

Lowood: And do you think the thinking was, "This is a group of physicists. They're the logical ones to _______."

Packard: Yes, right. I think, again, competent and capable to tackle various problems.

Lowood: And this would have been everyone, at least at Alpert's level down or Krasik's level down?

Packard: Well, if you had[?] ______, you have Krasik and Holstein. Krasik may have come for a little while. Holstein probably not. So it was Alpert, McKoogry[?], me, and there might have been one other guy ________________.

Lowood: And then there were some other people from other parts of Westinghouse, or pretty much just you?

Packard: No, it was just our group, and we fitted in with a group up here and did two things. One is we did sort of crew duty on the lines[?] on the mass spectrometers. We worked on the units.
Lowood: So you did shifts.

Packard: Yes, we did shift work. And we did modifications. I can remember drawing in the big magnets and going with a girl[?] because you can't turn it so you couldn't get in[?] the holes in this direction. So we did that out for sparking - that was one of the big problems where inside. But we would go up and watch and Lawrence would come stomping around and want to see how many sparks we'd seen, so we learned about Lawrence at that time.

Lowood: What are your observations on Lawrence?

Packard: They were great promoters. They really needed it.

Lowood: How much on top of things was he, as far as the projects went, and the physics that being done?

Packard: Oh, I felt not at all.

Lowood: So he would kind of walk through and give things the once-over and then walk out again?

Packard: Yes, that's right. We had meetings - and again there were very competent people up there - we had technical meetings, set-up meetings, there, there. Ten minutes after everything had come on[?], Lawrence would storm in, right in front of everybody, plop down in a big leather chair, and first ten minutes we had. So I found him to be very obnoxious. So you build up a role model and an anti-role model. He was an anti-role model. I don't know, you may want to sanitize that. But anyhow, that's the way I felt about him.

Lowood: By the way, Alpert was presumably a positive role model.
Packard: Yes, he was a _____ . In practice he was a positive role model.

Lowood: How much re-tooling was required to do the kind of work that you were doing, or was it fairly routine?

Packard: It was relatively straight-forward. We had been working with vacuum systems - these things were evacuated - so we had an understanding of vacuum systems, and vacuum gauges and all of that. It was very familiar. We were introduced to some new ideas _______ mass spectrometer system ______ detection. At Westinghouse we had some familiarity, because a man by the name of Johnny Hippel had built mass spectrometers which were used for chemistry.

Lowood: I'm wondering whether you learned a lot about magnets during this time.

Packard: Only in a superficial sense, because we had the big 184 inch magnet there, and so we learned something about it, again superficially. I don't remember having it or calculated use from the magnets, we used the big magnet, again, the familiarity was field strengths and _______ and fringing fields.

Lowood: You picked this up as you went, or did you sit down like with radio engineering?

Packard: No, in the field. In those days, we had dollars that were made out of silver and you put them on the end of the big 184 inch cycotron[?] and it would tip over[?] _____.

Lowood: That was your gauge.

Packard: That's right.

[END OF TAPE ONE, SIDE B]
[BEGINNING OF TAPE TWO, SIDE A]

Lowood: . . . . that would be ____________.

Packard: So maybe for the record[?]. I first met Ed Ginzton when he
- I remember him visiting Westinghouse one time. It seemed as is
it was just a general visit _______.

Lowood: Where? In Pittsburgh?

Packard: In Pittsburgh, yes.

Lowood: Oh, I see. Was he visiting Alpert, do you know? ______?

Packard: I have a feeling it was probably just a general visit. ___

did work fairly well together, as best they could. I know that
he had visited Bloomfield on this tube work, and that would just
naturally have brought him on to Pittsburgh.

Lowood: So this was a Sperry/Westinghouse ______?

Packard: Yes.

Lowood: Okay. At Berkeley, you've mentioned the kind of work that
you did. I think it's the last question I'd asked you when we
took our break was the work with the magnets and what kind of
experience that had given you.

Packard: Well, I actually didn't develop any magnets; I wasn't
involved in magnet circuitry. The question of becoming familiar
with magnetic fields was kind of ironed as a day-quick[?] kind of
currents it would take, what do the fields feel like, what
happens? A general familiarity.

Lowood: In terms of the use of the spectrometer, you didn't do any
sorts of chemical things at all with that? You didn't pick up
any of that from other people?

Packard: No. What happened is, initially as we went in, we did crew
duty, which meant shifts, and the question of really operating these - they were, I guess, the beta[?] class, which was a smaller unit that they ________ designed for, I think maybe the last stages of the separation. There were a number of problems in production things; sparking was one of them. There were some plasma problems, in which we really weren't involved, but we would watch for sparking and watch for general behaviour. We would help remove the _____ and _____ to solve them actually meant being exposed and rubbing against the uranium. The basic starting was uranium liquid[?] chloride, but it ended up as kind of a greenish deposit. Actually we did work with that.

Lowood: When you said rubbing you meant . . . ?

Packard: Physically. I can remember coming home with coveralls with this stuff on them, which by now would have horrified all health students[?].

Lowood: Sure. You weren't concerned then at all?

Packard: No. We were careful, but ________. Same thing about magnetic fields; it never occurred to us to be concerned about physiological effects of magnetic fields, and of course now people are frightened by it in many cases and got very _____ no doubt. Standards which were set that no one has to worry about the strength of magnetic fields that could work in. So those kinds of things we had no concern about.

The first part had to do with running ________. Then we began to develop programs for other technologies which could be used and related to the isotope. Some of these were crossfield electrical magnetic devices, so we would set up. In the big
magnet, an apparatus which we had built of some mechanical apparatus vacuum and electronic _____ machine. It was a bit awkward. One of the other lessons which I learned was about time sharing big equipment, because we would get our particular piece of equipment, put in the magnets, because we could work on it this week and not next week, and just get it in, just get it tuned up, just get it almost to the point where something was going to happen, then your week was over and you had to pull the stuff out and somebody else put their's in. It was not a very productive period of time.

Lowood: Is this correct? You mentioned _____ was a transition from tube work to this kind of work. But also it seems that there was a transition from doing a kind of component work to maybe more production, or a system kind of . . . .

Packard: Yes, that's right. I hadn't thought about it. That's correct. We were attempting to make the system operate, so we were more production oriented. We did not build the equipment ___________. We were simply learning how to use it.

Lowood: Now while you were doing this, were you starting to think about graduate school, or did that maybe start right after the war ended?

Packard: Yes, during that period the focus was on winning the war, and I really didn't think about . . . . At that time, my wife worked down the hill with John Lawrence, and she was a medical technician working with certain cancer patients. They were injecting them with phosphorous 32 procedure, which turned out
not to be useful. But that was her early experience with John Lawrence, and early experience with the cyclatron[,] down the hill in Foster City to ______. And again, health hazards were hardly a factor.

Lowood: Did you talk with your wife a lot? Did you sort of compare notes on the different kinds of work that you were doing?

Packard: Yes, we did compare notes.

Lowood: When did you get married?

Packard: We were married in '43. I had been back at Westinghouse one year, ______. She was a year behind, so graduated in '43. We became ______.

Lowood: So you'd met in college.

Now this work was Manhattan Project work, right?

Packard: That's correct.

Lowood: So you'd been involved, in a sense, in two of the big war efforts, separate war efforts.

Packard: That's correct.

Lowood: What was, say, the relative effect of the two on you personally?

Packard: Well, I think the skills involved, which I picked up because of Maurice Stentley[,] were more in the tube device area, but it is true that I really picked up a lot of concepts, skills and ideas from Berkeley, because again there were very good people there. I learned things - I remember a Wilson seal, which is something that Bob Wilson - who then went on to head up to Arcon - he was there at the time, and so we used that and we learned how it functioned. So we learned a lot of very good techniques
and technology, things which you could not learn any other way, couldn't read them in a book, you couldn't go to a classroom; it had to be on the job training. So just being in the environment, even for a short period of time, about nine months, being in that environment for nine months was important.

Lowood: Was there a different sense of accomplishment associated with the two?

Packard: Yes, because we did accomplish something — there it is. But with the other, we began to do our own research and this was a frustration, because we really could not have facilities long enough to either demonstrate whether ideas were good or bad.

Lowood: So the Westinghouse experience in that sense — or the initial Westinghouse experience — was more positive than the Berkeley one.

Packard: Yes, more positive in the sense of accomplishing something, yes. But the Berkeley experience was very positive in the sense of identifying and learning new skills, both in electronics and in ______.

LowoodP: What about, say, for you career-wise? Did you latch on to interests in one project versus the other that were for you personally more important? Did one of them have more of an ______ effect on your wanting to go to graduate school? Were there any things like that? Or even at the level of the specific skills that you picked up, you know, measurements with devices specifically?

Packard: Again, I can't remember any conscious thinking about going back to school or not going back to school.
Lowood: Okay, let's talk then about your first meeting with Felix Bloch, which I guess occurred in the fall of '45. Now Alpert, he played a role in setting up the meeting. Is that right?

Packard: He set up the meeting. I think I came down independently; I don't think he was here at the time that I first met with Bloch - I don't remember his presence.

Lowood: Could you set up the meeting then - why it occurred.

Packard: Well, I think Alpert either knew or thought that I should go back and see if I could go on and get a Ph.D. And again, I think it's probably something that was understood[?]. He had talked recently with Bloch, and knew that Bloch ______ and had this very interesting idea. He thought that my electronics ______ background in experimental techniques would be helpful. So he suggested we get together, so I went down to the ________ and Felix outlined on the backboard ______ line ____. I could more or less understand it, and I think we essentially decided right there I was ________.

Lowood: And this was the nuclear induction work that he was doing?

Packard: That's right, yes.

Lowood: And so it was really kind of almost like a job interview in a sense. Was it that much?

Packard: Yes, because my job clearly was to support him. It wasn't an admission to graduate school; that was just part of the package. But the point was that I helped him to accomplish his objective, which was to make this idea work.

Lowood: It's interesting to me that - with your background you could have also been useful certainly to other projects that were
cranking up at Stanford in that time; let's say the klystron work, or the other things. Was there a particular reason why Alpert made the connection to Bloch?

Packard: I don't know the answer to that.

Lowood: Could it have been a personality fit that he saw, something like that?

Packard: Well, probably not a personality. I really don't know how well he got along with Bloch. There were certain conflicts within the department at that time. I would not have thought that Alpert and Bloch would have been very closely related. Hansen and Alpert, and Hansen and Bloch had talked about the idea, so probably Alpert - it would be interesting to ask him - probably Alpert knew about it because of Hansen. Hansen either wasn't there or . . . . I guess he wasn't there actually - he was back in the east at that time. But Bloch was here, and probably talked to Felix about the _________.

Lowood: It's come up in other interviews and contexts that Hansen seems to have been the universally-liked figure in the physics department; that other combinations of people who didn't get along so well, would individually each get along with Hansen. I've heard that in another context.

Packard: Yes, I think that's correct. I cannot remember Hansen ever having a conflict with anybody. Most certainly he must have been a smoother-over, because there were problems in the physics department then _________ nothing negative, there always are, in any situation. But Hansen seemed to be mutually
respected by all folks[?].

Lowood: So for you, in the fall after this meeting, it was almost an immediate thing that you would be attending graduate school in the physics department. There were some formalities, I assume.

Packard: There were no formalities.

Lowood: No application or anything?

Packard: No application, no formalities. I suppose I would have one time paid the fees. But it was always interesting to know whether I could have ever gotten accepted to Stanford under the current rule. And I'm not quite sure how the next people[?]. I would have been the first after the war graduate student. But it was just decided, the terms given[?] and made the [_____] graduate student.

Lowood: Some of the faculty appointments occurred that way as well, right after the war.

Packard: I suppose they did too.

Lowood: Just a telephone call and then . . .

Packard: That had to do it.

Lowood: So were you resident at Stanford then? Did you stop working?

Packard: Yes. We shut down the group and the rest of the guys went back. Actually, we never went back to Pittsburgh. [_____] had some friends close up the apartment and ship our stuff out. We never went back, just came out [______].

Lowood: So the Westinghouse group also left Berkeley, but they went back to [______].

Packard: At the same time at the end of the war. This was after V-J
Day, when the war was over.

Lowood: So Westinghouse's participation in that part of the ______ was discontinued.

Packard: Discontinued quite abruptly.

Lowood: Abruptly because of the end of the war.

Packard: Yes. Just a sort of an aside: although we were working on the Manhattan Project, we knew fairly well what we were working on. To some extent this was known even under Westinghouse. And so ______ tried __________________________. The real details of them, we really didn't know. And we knew enough about physics to know why we were doing it, but not enough about the program to know where it stood.

Lowood: Did you feel any sense of personal involvement after the bomb was dropped?

Packard: No, I think only relief that the war is over.

Lowood: Okay, so you moved down to Palo Alto, and you began, probably sometime in the school year '45-'46, to take courses and all of that.

Packard: Yes. And I also did earn my living. I took a section of freshman physics. I was a teaching assistant and apparently that was ______, because Felix didn't have any other money. So I was offered teaching assistant.

Lowood: That's how you paid . . . . you did that for a number of years then?

Packard: I did that for probably a year, and then Felix got ONR funds, so then I went on with the research.

Lowood: What was your curriculum like, say the first year or two?
What courses did you take and with whom did you work?

Packard: I can't really remember the sequence of it, but I had a course from Webster, of course from Felix, a couple of courses from Felix, and maybe even more. Thermodynamics was one, and quantum mechanics was another. Schill wasn't there yet. There would have been a so-called "Modern Physics" course taught by Webster Staub, Staub.

[BRIEF INTERRUPTION]

Lowood: You just had mentioned Staub, that he had maybe taught the Modern Physics class.

Packard: Maybe.

Lowood: Hansen taught that class before the war.

Packard: Yes. I'm trying to remember whether I ever took a course from him. I don't really remember.

Lowood: It could be perhaps after the war he cut down on his . . . .

Packard: Yes, because he was probably ill at that time, and working on the linear accelerator.

Lowood: Did you do any work, say with Kirkpatrick or any of the people who'd been there . . . . with some pre-Klystron people?

Packard: Kirkpatrick focused primarily on the elementary physics . . . . great interest, but I don't remember any close . . . ., and actually - I can't quite remember whether I'd taken optics from him. I don't recall - you . . . . very interesting questions! I really realize what things happened, and I don't remember.

Lowood: Did you do much coursework in engineering, if any?

Packard: I did not take a single . . . .
Lowood: And obviously you had decided to specialize in the kind of work that Bloch was doing, particularly his nuclear induction area.

Packard: Yes. And again, he had ONR support for the research.

Lowood: Were there other students that worked with you under Bloch?

Packard: Yes, there were quite a number. You have some of the names, and there were a number of students beyond that which we could get. Wide[?] background, some theoretical - I'm not sure in there. Another man was Jacobson. The early calculations behavior of the vectors. Equations.

Lowood: Was Elliott Leventhal one of the students?

Packard: Yes. He was one of the students. It might be worthwhile doing. Emmett[?] Rogers was one of them.

Lowood: And all working under Bloch.

Packard: Yes.

Lowood: Well, one question I wanted to ask about was the sort of balance of things in the physics department. Obviously, the accelerator work and the Microwave Lab was a big growing area associated with the, and one wonders if, for example, you felt any pressure to move in that direction?

Packard: No, I did not at all. The first linear accelerator was just built down the hall from where we did the NMR work, so I was quite aware of that first accelerator. Then I suppose under the auspices of Ginzton and others, they began to build bigger ones.
We moved across the street. But the high energy physics was just never of any great interest to me.

Lowood: So it was peripheral to your experience.

Packard: Entirely. Even though I had roommates, officemates in that end of it, it never had any appeal to me.

Lowood: What about for Bloch? Was he antagonistic to that kind of research, or disinterested, or what was his . . . ?

Packard: (Laughs) You probably know the answer to that. Do you want it on the record?

Lowood: Sure.

Packard: There was a great deal of strife in the early days between the applied physics and the physics group. Felix was not always __________, about his interaction, and it really became even more severe at the time of talking[?] about the big accelerator. His argument at that time was that the government money would take over and __________.

Lowood: Was this already this case though in the mid to late 40s when you where starting out as a student? Did you sense that there . . . ?

Packard: Well, at that time there wasn't. See, there was only the catacombs of the X-ray lab. The upstairs lab where the - didn't you see the picture? - where I went into physically was just the waste[?] plan. There was no, really no active project underway. There was nothing __________.

Lowood: One reason I asked that question is I think I see there as being two co-terminants patterns in the chronology: one being the growth of the accelerators, and the other being, well, Hansen's
death I suppose, in '49. Both of those may have been factors
that may have made it more difficult for Bloch to accept ___
work with ______.

Packard: It took a lot[?]. And what you're saying is that maybe
Hansen could have been a smoother, and that's possible. Hansen's
death, of course, was a blow to both science and Stanford. He
was quite unusual[?]. We knew Betsy well, and her mother well.
We knew Bill well. Barbara had a particular good relationship
with the family _______ personal. First the death of the
son, the role that the founder of the Palo Alto clinic, Russell
Lee[?] who we always felt was not a positive influence on ___
in any way.

Lowood: Could you explain that? That's not something that I know
about.

Packard: They were fairly, as a family, they were fairly _______
for a _________ socially and medically _____ Russell
Lee[?]. Barbara's and my thinking was that the medicine was not
very good and, for example, the advice was that Betsy should
drink lots of beer while she was bearing the child, carrying the
child. The first ________ labels on about fetal
syndrome, so you don't know what effect that had. Betsy had in
mind adopting a child and Russell talked him out of that ___
some truth to this. But __________ that the genes must be
there[?] ___________________. I always felt __________
completely medically unfounded. But it was always my kind of
personal feeling that the bronchitis that Bill had might have
been related to some of the early _________ which ________
early, which we worked with[?] for any
of_________________________
bleeding of a _____ could be very, very wrong.

Lowood: So this was something that was already discussed at the
time, as a possibility.

Packard: I'm not sure. It wasn't until after the war that ________
really came up as being recognized. And so you never know that
it was not acceptable thing[?] _____ explanation __________
somebody else.

Lowood: As far as Hansen's war with Bloch, that was pretty much work
that was finished by 1945-46?

Packard: In '46, Hansen was not working directly with _______. He
was involved in setting a program under way which was designed to
automatically search for a magnetic homer, a concept which was
shown not to be feasible because of much difficulty with
samples[?]. But he had used and built what was called a
bitter[?] magnet, which is sort of a self-imposed[?] structure
designed by a man by the name of Bitter[?] at MIT. So the magnet
was built by the graduate students ______________ Jones was
assigned to do the construction on this, and try to bring the
apparatus into being, which never did happen. But I think that
that was a project which Hansen . . . that
started __________ .

Lowood: But is it physical ability to keep on top of the accelerator
work plus the work I presume Bloch must have been ______ with?

Packard: Yes, that's correct. But we[?] really never had that
post-______.
Lowood: Well, obviously the '46 paper, your name appears on the apparatus paper, and there was the earlier theoretical paper in '45.

Packard: We had the two papers come out at the same time, the theoretical paper by Bloch, and Hansen and Bloch and I were on the experimental paper.

Lowood: Can you delineate the roles played? With the second paper, what kinds of roles were reflected in the work? What sorts of things did you do? What sorts of things did Hansen do and what did Bloch do?

Packard: Well, if you're talking about the first set of experiments, that was fairly well separated, and quite easily. I did the electronics portion of it - I built most of that except for what was kind of the front end of it. Hansen did the work on the, shall we say the receiver, the coil, the cross-coil or something; he did the mechanical work. And interestingly enough, at that time, Hansen was very much taken by what is called, I think, kinomatic[?] design: how to design mechanical structures in the simplest possible way. You know, if you want something that lies parallel, you can use a couple of rods with _________ flap over here, so that it doesn't matter what the spacing is. That kind of thing. So on the first design, he had designed it to rotation which was coupled by some wires, and he was a great believer in using wires for hinges, so that he preserved the mechanical properties that still were __________. So he put a lot of effort into thinking about simple mechanical structures. But the very first system he put together that way,
it turned out not to be . . . .

[END OF TAPE TWO, SIDE A]

[BEGINNING OF TAPE TWO, SIDE B]

Packard: . . . . things ______ were important.

Lowood: Uh, no. So you're saying that Hansen's first shot at it was not successful.

Packard: Yes, that really wasn't too satisfactory, but then he developed the concept of ______ paddles for ___ steering the flux, and then that worked much better. But the early feeling about what the parameters should be, the strength of ___ fields, so on and so on, were really quite ______, and so it was designed in terms of very high radio frequency fields. And it turned out that was not necessary at all.

Lowood: Who would have done the calculation? That would have been Bloch's calculation?

Packard: ______ Bloch, yes. Well, calculations in the sense that he understood the physics of it, and it just happened that some of the basic wisdom at the time was wrong. Very quickly, as we began to get results, we could very quickly know what the right parameters were. His equations and all were still quite correct, but he just had the wrong ______.

Lowood: So when you talk about the basic wisdom, you're talking about basic wisdom about fields required to produce certain effects?

Packard: Well, that's right, having to do with relaxation times, and long ___ of relaxation times could be very, very long. It's a
problem[?]. Longitudinal, having to do with some thermo
relaxation times and that the so-called transverse would be very,
very short. Well, it turned out in water, that these were close
to about three seconds. The thought was originally it was going
to take hours _________ signals would be very, very broad. So
under those conditions, the whole set of experiments were
directed to taking care of that problem. And what that meant was
that Bloch put a couple of his iron pole pieces on the side of
the cycatron[?] magnet, and the idea was to pre-polarize the
sample; let it sit there for a few hours so that gradually
the _______ would _______ stretch the magnetic field. With
that sample, put it on my other apparatus which had strong fields
instead of the _________ that the lines were going to be very
broad, so you'd have to have strong field in order to see it.
But essentially, you would see the signal under those conditions,
and again, it would take a long time to _______ that so it
turned out that it probably never would have worked if there had
been those conditions. So the _________ was _____________.

Lowood: So working out those kinds of mistaken assumptions involved
was just basically trial and error work.

Packard: Yes, we would set up and try an experiment and no success.

Lowood: So you'd get no signals. You wouldn't get anything like
what you were looking for.

Packard: Yes, until finally we did. In a way, once you get a signal,
then you can move fairly rapidly. So Hansen did the
cross-coil[?] part and the input amplifier. I did the data
display and the other amplifiers covering the various frequencies
which I recorded[?]. Bloch did the, of course the original
cancept of it. He worked with the magnets. He took a
lecture _________ and he put some, actually just cut out
transform____ and put it on the front. Very, very
crude ______, because we were self-_______, homogeneity was
not required; it was[?] a very, very broad signal. We did
want _______ with sixty _______ so we used a transform
iron[?] _____. Most of those concepts were not correct. Bloch
plotted up the field, and determined about what current we should
set the magnet at to produce the field which was required for the
frequency which we were operating at. So Bloch did the
magnets ______, I did the electronics and display portion of
it, and Hansen did the pick-up concept of it.

It was kind of an interesting division of labor, because one
of the things which Bloch is famous for now is the so-called
Bloch wall, and he did a great deal of early thermo[?]magnetic
studies, outstanding work. _________ magnets, Bill Hansen
had worked with Doppler radar systems, where you receive a very
weak signal and it turns into a very strong signal. So he took
that portion of it. I'd done the electronics,
and _________ display, so I did that portion of it. So
in a way, each of us took something which we had familiarity
with.

Lowood: You were reaping the benefits of . . .

Packard: . . . of the previous work.

Lowood: I just want to ask you one thing kind of related to that.

You'd mentioned that Hansen had had these rather sophisticated
ideas about what a design should be like.

Packard: Right.

Lowood: And Bloch was also doing some design work as far as the
magnets were concerned, right?

Packard: No, he really was just measuring the fields.

Lowood: Oh, I see. But he was doing very simple tasks.

Packard: Yes. He would calibrate a ___-coil. And again, you see,
this went back to the work which he'd done with Alvarez[?]. Even
earlier, because what Bloch was interested in doing was measuring
magnetic moments, and this was carrying on work from what ______
had done, which had to do with radio frequencies and magnetic
fields, which had been ________ signal. So that's what Bloch
wanted to do, and he started with great interest in the neutron.
He and Alvarez[?] did an experiment in Berkeley. I guess
someplace along the line, Bloch learned that the only way you
could measure magnetic fields was to ___-coil with the - you do
like this, sort of cut so many _____________. And that
was __________________ you could do, but that wasn't good
enough for do for the moment, so there were some concepts then of
measuring ______ with respect to cyclotrons[?].

Have you read Alvarez's book?

Lowood: No.

Packard: ______ and Alvarez feels that he was Bloch's mentor with
respect to experimental physics.

Lowood: This was in '39, '40.

Packard: Yes, just before the war. And then the ideas . . . . Bloch,
during the war, went to Los Alamos, worked there for a while and
that wasn't terribly satisfactory and he went back to working for Terman's lab. And actually one of the things he'd worked on was non-reflecting coatings for aircraft like the bombers. But at that time, he developed the concept that yes, he ought to be able to measure proton magnetic moments in material of normal density, and the story is, he caught that idea at a concert. Probably it's true. But he and Hansen had conversations about it, and Hansen knew a lot about signal and noise ratio, and so he was able to calculate and say, "Yes, this should work. We should be able to see it."

So preliminarily, Hansen brought in the radio receiver concepts, the radio transmitter, radio receiver concepts, and knew a lot about signal noise, and a lot about noise, thermo noise, all these kinds of things. He knew what the thermo noise should have been, and the signal was calculated to be higher than that.

Lowood: So his contribution was vital in taking Bloch's ideas from a certain point.

Packard: Yes, that's right. Bloch had the idea of the motion and the recession[?] and the _____ things, and Hansen was able to apply the practical engineering approaches as to how do you get a signal out of it? What's the signal intensity going to be? What's the noise going to be?

Lowood: You were in an ideal situation in a way, working with Bloch and Hansen on a project.

Packard: Yes, that's right.

Lowood: Looking back, how do you assess the influence of working
with them on this project?

Packard: Well, as far as the scientific interaction with Bill, that really was never very strong. I mean, he'd done the work on the calculations and all, and he did the box[?] front end of it, and he got involved in the linear accelerator and other activities. But working with Bloch, us graduate students and all, Bloch was very competent physicist. And from him, we learned many, many things about both details and about classical approach to physical problems, and the importance of this against this. Bloch had a great ability to take any[?] equation and say, "The _____ isn't important, so forget about that" and dropped that out. He was able to simplify things to a level that ____ calculations.

Lowood: And you would have discussed, say, the nuclear induction theory with him, you would have had an opportunity to discuss all those things with him as a student.

Packard: Yes.

Lowood: So in addition to working on the electronics of the device you were also . . . .

Packard: Yes, we worked closely with Felix. He was a tremendously able leader and teacher.

Lowood: Are there any anecdotes you remember about the work? About the experiments?

Packard: Well, there have got to be a lot of them, but I haven't really thought about that.

Lowood: I'm thinking particularly of things that might illustrate
the differences between say Bloch and other physicists perhaps, with respect to approaches to an experiment like that.

Packard: Well, Bloch had, I think, a good understanding, awareness, of precision and accuracy in these kinds of parameters, and was interested in making more precise, more accurate measurements. He understood well - I think I mentioned this, but there was a case where we actually were sweeping through the signals, and you get distorted on one side, _____ sine waves. The argument was that the center of that ought to be some symmetry argument in which ________ had learned in doing these things much, much earlier.

Lowood: In tubes.

Packard: That's right. So it was something we discussed, and yes, we decided that yes, the symmetry ________ center symmetry that would be the proper resonance[?]. And so he understood not only about uncertainties of being with and aligned[?] with, but how you set and then align[?] with, you know, that's the nature ________. So those are kinds of little fundamental concepts which I learned from Bloch, and it turned out to very important. Not understanding those caused a lot of people to get into a lot of trouble. For example, Varian one time had a project on electron beam lithography, which is a good concept, but what happened was that the ________ continued to make advances, even though conventional wisdom said, well, you can only go the wavelength of light. Not true. If you know enough about the parameters, you can _____________. So those kinds of things where some people accepted the - it wasn't basic
physics; it didn't design _______________ and what the problem is we have on hand.

Lowood: Did Bloch know about these things because he thought about them, or because he had a ______ experience, he had a sort of finger-tip feeling for them?

Packard: I don't know whether he understood these very classical concepts because of working with the fellows he had in Europe - he worked with Nils Bohr and Eisenberg, _____ and all the famous physicists at the time - and so he may have learned that ______________. But those kinds of insights which he was able to bring to the problem were found to be extremely helpful.

Lowood: Okay, let's talk a little bit about the funding. You mentioned that the ONR had picked up the project a couple of years after you came on the scene. Do you know anything about how the funding was acquired or what kind of interaction led to it?

Packard: Very little. I've since learned that Walter Meyerhof had an important role in the early ONR contract, because he was almost the contract administrator, because that wasn't one of Bloch's specialties.

Lowood: I didn't know that about Meyerhof. What was his connection to ONR?

Packard: Well, I'm not sure whether he just sort of took the job of being the administrator of contracts or whether he . . . .

Lowood: Or for Bloch you mean?

Packard: No, ______ more for the students[?] _______.

Lowood: I see. So Bloch wouldn't have had to run around.
Packard: Bloch did not have too much to do with it. And the other fellow that had something to do with it was Ed Edelstein. So that was in the very, very early days of ONR. I just saw the original contract that was established, but I'm not sure. It would be interesting to talk to Walter about it. I would think that ONR people, you know, we had a good book on ONR[?]. Do you have a book on ONR?

Lowood: Not that book. I have a copy at home.

Packard: So, anyhow, he had a role in it. But I never had anything to do with that, and I think that Bloch didn't have too much to do with it.

Lowood: But that was what got you out of the blackboard work.

Packard: Yes, that's right. That was very important. I've always had a soft spot in my heart for ONR.

Lowood: Your Ph. D then, which you, I guess, finished in '49, was on the method of nuclear induction, which you called it ________

Packard: I ________.

Lowood: And here, at least until '49, in the dissertation you were talking about things like isotope identification and measuring magnetic fields and all of those kinds of applications. Was that pretty much the extent of Bloch's view of technique? In other words, he didn't consider, during this period, the chemical applications.

Packard: I think that's right. The work of Proctor and Yu on the nitrogen pointed out that chemical effects do exist, and that was considered more of a nuisance than anything else. It was a problem, because it kept from measuring the magnetic moment very,
very precisely. So any chemistry ______ would have been
directed, and were directed toward removing of the effect, and
that's what got us into working with hydrogen gas and all,
because it's ______ that that could be calculated. And so we
could go measure that and then calculate ______ could go
and ______.

Lowood: ______. You could subtract the ___ easily because you
can . . .

Packard: . . . because you can calculate the amount. So that was
the point we were at[?].

Lowood: Of course one person's nuisance, especially in this context,
is another person's interest.

Packard: Absolutely.

Lowood: Did Bloch lose interest as the applications moved more and
more towards these kinds of chemical identifications and such?

Packard: I think that's correct. ___ the question whichever was.
The work was direct[?]. We got a big magnet, which was actually
designed by Harry Weaver. It turned out to be a magnet structure
which remained essentially for a long time - Varian built
hundreds of magnets with that design. The effort there was to
make to make the magnetic fields more and more homogeneous, so
that we can measure very, very narrow lines and measure
frequencies very accurately. We did want to measure the hydrogen
gas for that reason, and they also got a little bit of helium-3
gas which obviously I tried to do something with it, but my
understanding of absorption and all ____ gas _____ not up to the
task, so that just dissipated. But anyhow, the object was to
make more homogeneous _____ and make more precise measurements.

But then as we demonstrated the chemical effects, I really drifted away from the others, and Bloch never said, "Hey, you guys better get back to work on that problem. He was very liberal in allowing us to move in that direction of chemical ______.

Lowood: And the people you were working with - I guess I'm thinking of Dharvatti and Arnold and so forth - they were not all in the physics department, right?

Packard: Yes.

Lowood: Dharvatti I think was in chemistry.

Packard: No. Well, he was a chemist. He was a post-doc and was working for Bloch. And I guess, I don't know exactly what his problem was. Arnold and I were working together. I think by that time I had my Ph.D and Arnold was working in as a graduate student I think, again trying to make more homogeneity which meant we had to sweep more slowly ____ magnets ______. Stability had to be better, and all these kinds of problems which __________. And Dharvatti was a curiosity.

Lowood: At what point did you realize, did you shift from trying to come up with these homogeneous sort of background, you know, some kind of background that you could take away from the fundamental maybe physics work that you were doing to a realization and an interest in studying what had previously been the background?

Packard: Well, we went ahead and did all these alcohols and integrated[?] out - I think it was rather convincing that's what we were seeing. We also noticed a little bit of structure in the
alcohols which made people think about it. That material was presented in Washington, which I think interested a number of the chemists. And not much after that, I went to work for Varian. But I didn't continue much of that at Stanford.

Lowood: I see. You're talking about the ethyl alcohol with the 3:2:1 _____.

Packard: Yes, right. That was really all the chemistry we did at that time, and just demonstrated that it is possible to work with organic _____.

Lowood: I've heard somewhere, as a sign of, I guess, the effect of you - you were saying that was the first time you actually believed what chemists about _____ and things when you saw it on the oscilloscope.

Packard: Oh, that convinced me!

Lowood: I thing I want to ask you about it - start I think when we get together next time - is the work that you did at Los Alamos. I think we don't have enough time to get into that right now.

Packard: No.

Lowood: Because that seems important to me.

Packard: Yes, that was interesting.

Lowood: Can you tell me a little bit, though, about the work on the magnetic field regulator, the fluxmeter and all of that? That was done in this period, right?

Packard: Yes. That again grew from some of the Westinghouse knowledge which I had. Emery Rogers[?] and Staub were setting up to - and I guess Bloch too - to repeat some of the neutron measurement work using the cyclotron as a source. Prior to that
it required very, very long lines[?], and the magnetic fields had to be exactly right, so the proton ________ was ideal for measuring the field. Well I knew at Westinghouse – because either I had worked on it or somebody else did – of the ability to stabilize frequencies ____________________, and so I just took that concept and their needs for it, and the NMR[?], and put together the system which would stabilize and regulate. So I knew about feedback mechanisms and that kind of thing from work for Westinghouse. It all fitted together.

Lowood: It also fits in very well with the earlier program of NMR, the tool[?] and various areas of physics research.

Packard: Yes. Measuring the magnetic fields and ______ controlling and then took one step further. Basically Bloch said, "I want this technology for measuring magnetic fields so I can measure magnetic moments ideally[?]. It was a tool for that purpose.

Lowood: And it worked better than silver dollars.

Packard: It worked better than that and ___ coils and everything else.

Lowood: Where did that line of work lead? Did somebody else pick that up, or was it . . . ?

Packard: Well, I don't know ______ that, but Varian's first product we should talk about. Russell Varian and patents and all. Varian's first product was the so-called flux ________, flux measuring equipment. Because that was really the only useful thing you could do with it at that time, even though Russell Varian had a concept of using it for doing chemistry, but only isotope chemistry.
Lowood: I did know about that. I guess I was kind of wondering about the use and, say specifically in Berkeley ______. Were the devices that you worked on used there?

Packard: No, they weren't even known at that time, because that was before I left.

Lowood: Okay, I see. I've got the chronology backwards.

Packard: Very soon after I ______ our first customers, but they very well could have been somebody up in Berkeley.

Lowood: Okay. I see that ____. Questions I have now fit pretty well together as far as wrapping up your time at Stanford, and I think I want to save those, because I want to talk about the AEC work and maybe, unless you think there's a very short answer, I ______ about the APS meeting.

Packard: Oh that. As far as I was concerned, I didn't know any facts at the time.

Lowood: This 1949 meeting of the American Physical Society.

Packard: You found it. I don't even remember.

Lowood: Well, it was just striking, in looking at the . . . .

Packard: We had quite a lot of people that were active in the field, and that was a hot subject at that time, sure.

Lowood: So there was no particular meeting, or anything that put Stanford work in the limelight.

Packard: No, not that I know of.

Lowood: Okay, so we can put that one aside. Maybe one other quick question - I think we can get this in maybe two or three minutes - is just in general, what was the equipment like that you used? Were you able to buy things, or did you recycle things, or did
you use teaching apparatus? What kind of structure did you couple together the ____________?

Packard: You're absolutely correct. I have to go back again to the simplest - the electronics which I had. We bought a scope in Du Mont[?], scope like the one I used at Westinghouse. I suppose that was a hundred dollars. And I bought vacuum tubes and chassis and panels, and all of this came from the only really visible radio wholesaler, and that was W.B. Drill[?] in Oakland. So we bought a few bits and pieces from them. For my purposes, there really wasn't much around the physics department. The magnet was an electric magnet - old, old, antiquated, never really like a magnet in the first place. Our supply was from the cyclotron. For the ______ box and the receiver/transmitter assembly that Hansen did, that was all fabricated in the machine shop. Later on, we developed, actually built, magnets. We had borrowed one magnet that was used early with was borrowed from Berkeley[?] which is actually the magnet that Lawrence's first cyclotron was operated in.

Lowood: Was that war surplus pretty much by that time?

Packard: Well, there began to be war surplus, and for example the frequencies which we operated this ______ experiment with thirty megacycles, and the reason was quite simple that those were available __________________ receivers. So we just bought one of those and used it. So that established thirty megahertz __________________.

Lowood: So standards were set.

Packard: That's right. That's exactly right. So it was a little bit
of both, and later there began to collect a lot of the war
surplus over in Sequoia, and we used to go over there and try to
find something that would do the job.

Lowood: So that would be a field trip over to what was inventoried
over there.

Packard: Yes, the building was even condemned at that time, _______
rooms full of massive amounts of stuff.

Lowood: Did that seem like a step backwards from what you had
available at Westinghouse?

Packard: No. Well, what we did not have out here of course was the
infrastructure, support machine shop, purchasing, electronics
shop. The infrastructure at Stanford was _____, not what it was
at Berkeley or at Westinghouse.

Lowood: Well, on that note, maybe we should stop.

Packard: Done enough for today?

Lowood: Yes.

[END OF INTERVIEW]
SOHP Interview Status Sheet

Interview with: Martin Packard #1
Date: May 12, 1988

Interviewer: H. Lewood

(Circle one) GENERAL SILICON VALLEY NEWELL
Number of Tapes: 2. Length: 120 minutes; pages.

(Check when completed)
✓ TRANSCRIBING: by PR

✓ LISTEN CHECK: by H. Lewood 7/88

✓ EDITING/SECOND DRAFT: by HEL, JA

BIOGRAPHY PHOTOS APPEND. LEGAL FORM BINDERY
SOHP Interview Status Sheet

Interview with Martin Packard - interview #2
Date: July 21, 1988

Interviewer: Henry Lowndes

(Circle one) GENERAL  SILICON VALLEY  NEWELL
Number of Tapes: 2. Length: 120 minutes; 56 pages.

(Check when completed)
✓ TRANSCRIBING: by M. G.

✓ LISTEN CHECK: by Paul Magnus 5/23/90
(Also given to Rocky to give to Henry 8-10-88)
* Corrections not added

EDITING/SECOND DRAFT: by

NARRATOR CHECK:

FINAL DRAFT:

BIOGRAPHY  PHOTOS  APPEND.  LEGAL FORM  BINDERY
SOHP Interview Status Sheet

Interview with MARTIN PACKARD — #3

Date: 10-27-88

Interviewer: HENRY LOWDEB

(Circle one) GENERAL SILICON VALLEY NEWELL
Number of Tapes: 2. Length: 120 minutes; 62 pages.

(Check when completed)

✓ TRANSCRIBING: by N. BRAINN

✓ LISTEN CHECK: by G. MAURER
SOME CORRECTIONS FROM LISTEN-CHECK ALREDY ENTERED; BUT NEEDS TO BE EDITED

EDITING/SECOND DRAFT: by

5-2-90 Took to JA for her to review

NARRATOR CHECK:

FINAL DRAFT:

___BIOGRAPHY ___PHOTOS ___APPEND. ___LEGAL FORM ___BINDERY

NOTE: ORIGINAL TAPES (SV6 1,2) ARE MISSING — NOTE ON CopiS gayS
"7-5-88 draft given to me with original tapes for listen check" —
but Henry doesn't have them and neither do we.
SOHP Interview Status Sheet

Interview with Martin Packard interview #4

Date: 11-22-88

Interviewer: Henry Kennett

(Circle one) GENERAL (SILICON VALLEY) NEWELL

Number of Tapes: 1. Length: 60 minutes; 14 pages.

(Check when completed)

✓ TRANSCRIBING: by Vrijian

LISTEN CHECK: by Vrijian?

EDITING/SECOND DRAFT: by

Transcribed in a 'Q: A' format, single spaced in electronic typewriter,
may include readable diskette; someone has aloud proofread
and marked fixed in xerox copy or yellow post its

NARRATOR CHECK: le-90 11:30 took it to TS1 and scanned it,
so we should have a diskette copy of it, which
will also give us a chance to alter the format
(But we didn't have diskette? - 10-24-80)

FINAL DRAFT:

BIOGRAPHY PHOTOS APPEND. LEGAL FORM BINDERY
Q: Let's begin by returning to some of the marketing aspects regarding NMR and you were going to talk about the visiting professorships, is that what they were called?

A: Yes, that's what they were called. Actually, in developing the market, I think we did a number of things which were interesting. I'm sure we talked about the seminars. We also had post-doctorate type people, many of them who stayed on with Varian and others who have come back to academia and established themselves.

In addition to that, during the summer we actually had a number of visiting professorships where we would have key individuals from the universities come and work with us on various kinds of projects. At that time, we did quite a bit of rather fundamental research; in other words, it was not directed specifically to a product. It was expected that it would be publishable and part of our strategy to have something to contribute to the research world so that we could expect something in return for it. It seemed to be a strategy that worked because competent people in academia and commercial companies did feel free to exchange information.

Q: Can you name some of the people who came?

A: Strangely enough I can't. I'd have to think some more about it. It would be a good thing to try to recall. I could check with Jim Schoolery and with Larry Biet(?).

Q: Were there any from Stanford or would that have been unnecessary?

A: There might even have been some from Stanford. I'm not sure how many we had, but it was an important part of it because these people would go back to the university and be friendly to Varian. We also did a program in another vein which was kind of interesting. It wasn't so much NMR, more in the magneto-meter line. We, at that time, had the Naval Postgraduate School in Monterey on a program of farming their advanced students to come and work for a period of time with companies. So we had, over a period of several years, quite a number of these kids who came up and would spend a month or 2 months. We could usually identify a project for them. For example, one of the first to practice frequency standards was done with the help of a graduate student.

Q: Who was the coordinator? The Navy?

A: Yes, it was done through the Navy, but I can't remember the professor's name. You might make a note and see if I can't recall or find it later. That lasted for a couple of years and they were good projects. They did all kinds of things which we would not have done on our own. For example, this one project was demonstrate the gyroscope, because the UPI(?) would recess(?) at a constant rate while the coil was turning with respect to that frequency. So the kids built a little gyroscope.
Q: This would have been what, the mid to late 50s?
A: Yes, in that period of time. That was a period when we were reaching out to get our technologies known.

Q: So all these programs were part of this effort to build a community of users around some of the Varian devices?
A: Yes. It was successful. I'm not sure if I mentioned it or not, but there was what developed after, or in conjunction with, the Pittsburgh Conference, a group called ENC. I'm not sure what it stands for, but these were people who were users and they would meet. For quite a long time it was sort of our club or our group. The conference still goes on. It is held annually in Monterey or on the East Coast. It is an important NMR conference, so to some extent this meeting originated, again, from the interaction of the community.

Q: I'd like to move forward a little bit with your own progression with the company. I guess the last time we talked about your specific position you were director of research for the instrument division?
A: Yes, I think we touched on that, but not for the whole company. I think, initially, that I probably reported to Bob Jeppeson who was director of research for the whole company, but that was a transparent relationship.

Q: Right, and in 1963 you moved to general manager of the division and then in 1965 to divisional vice president. Were those actual changes?
A: No actual changes. I was just recognized and made vice president which, of course, gave me authority to sign for the company.

Q: Did that reflect any kind of reorganization? A lot of companies go back and forth in this kind of organizing everything along divisional lines and then organizing everything along functional lines and that sort of thing. Was Varian a company that tended to reorganize a lot?
A: That has been the history, but it doesn't work. We built, about that time, we began to acquire the other companies of Aerograph and MAT and AA company (Techtron). I think we touched on that last time. At that time, then, probably Emmory (?) would have been initially sort of group VP and I guess I was under him as a divisional VP. Then Emmory left and went to HP and Ed, for a while, wore several hats around the group. That could have been around that time. It was part of Ed Ginzton's strategy to build us into a wider ranging, multiproduct company. At that time it was recognized that no way could we develop it within our NMR business. There was plenty to keep us busy there and so that position was a good approach.

Q: Before I get to some of the other more general issues, I just wanted to wrap up a few things about you career here at Varian.
We've already talked about the period when you were involved with Siva(?) and I noticed that in 1975 your title changed to assistant to Ed Ginzton, who, at that time, was Chairman of the Board. What did that represent?

A: At that time, we hired a man to be the group president, I guess, sort of replacing Ed. This was a fellow named Equin(?) and about that same time, Norm Parker came in. It was quite obvious to me and to, I suspect, Norm that our styles were not compatible. So, I would say by mutual agreement I left the instrument division and Ed, somehow or other, took me under his wing.

Q: I guess the thing I was interested in was, when you did that did you start to move into developing longer range corporate strategy and that sort of thing?

A: That's right. We had gone through a period, actually, of hiring a corporate long-range planner which, again, was kind of typical of those times. It was a fad to have long-range planning. That, like most fads, disappeared. There's really no way that you can do that. The planning has to be done more at the divisions and so what turned out was kind of interesting. I really didn't do much, because they had a corporate planner, but I had to hire someone to be the division planner. Sort of like when you add another man to the football team, you have to put another guy on the line. So, all it really did was run overheads up without anything particularly useful coming out of it. I think even Gorbachev is beginning to figure that out about central planning. I was more involved and became more involved as I could see the activity. Ed Ginzton's activities became more long-range, just by the very nature of his own way of looking at things. I supported him in that, but we didn't have any formal long-range program.

One of the special assignments which I was successful at was to see what we could do about selling a plant which we had in Scotland near Edinburgh. That was a plant which, I think, we discussed earlier. We started to build NMR machines there and vacuum division was going to move in, but they didn't, and that was too small of an operation. At that time, I did spend some time in England searching and looking for products. The only conclusion which I came up with was that we should do something with the computers there. I actually did send a man down to Irvine to pick up know-how on the computers. Actually nothing really happened, but part of the reason that nothing happened was that it came out that it was not possible to transfer the manufacturing know-how to Scotland because it was not available in Irvine in the sense that the documentation and know-how had been put together by hand. That was part of the weakness of our whole computer activity. Actually it was too bad because we had a 16 bit computer, at that time, and an 8 bit computer, so there was a window there that had we done it right we could have succeeded.

Q: In that sense, if that was typical then of the kind of projects that you worked on, they were so much things like there was
a breakthrough on superconductivity 5 years down the road there might be a product. They were more geared toward a company's specific organizational type things?

A: Yes, they were all directed toward the major lines of business. One of the things I remember is that Ed and I discussed this quite a bit because we both believed in the importance of the biological industry. We had discussions, for example, with Larry Kief(?) who, at that time, was going with the thought of leaving Varian and going to Hawaii. They were setting up an institute. A man from the Stanford Medical School had gone over to Hawaii and Larry went with him. We actually toyed with the thought could we set up an institute. Could we set up something here. I can remember looking around the parking lot for an area where we might build something. So that was much too early. We really couldn't get it off the ground, but we had been doing some work in the biological area and we did feel that that was an important area for Varian to pursue. In that sense, longer term kinds of things and projects were there. We tried to get to them. Ed was always receptive to new ideas and he would promote them and think of them himself.

Q: In general, though, you were a little skeptical about the value of what you called long-range planning?

A: Yes, that's right. I think, and did at that time, you must go through the exercise of it, but not in the sense of trying to work out how many dollars in sales you might have 5 years from now. What you need to do is try to look at the trends. In the biological area, that was what we were doing. We could not map out a roadmap. So, on the one hand, you try to do return on investments and the cash flow kind of things that are useful when you have projects which you could lay on the table and check and make a comparison between them. But, I think I probably mentioned this, at that time I did feel that the digital technology was important. I hired this group from Fairchild and got them over and told them to go to it, which they did. That was an example of something which could not be justified on a return on investment. It had to be justified on the basis that this was a significant new area and it had importance. To some extent, I did it too early.

Q: Would you say things like that by the year so and so, if we don't have annual revenues of "x" amount, we'll quit? Are you saying that that is the kind of thing you would try to avoid saying?

A: Well, it depended on what kind of a thing it was. If it was a product that was well defined and in the market place, then you could do something like that. If the sales are this, you could do one thing, or if the sales were something else, you could do another thing. If you had enough information to work with, you could do this. If you take something like current superconductivity, even when we were talking, you cannot make a real estimate of what it is going to be. You couldn't do
that with lasers a real long time ago. You couldn't even do it with computers or Xerox machines. Generally, those more important things, people underestimate the scope of it.

Q: It certainly would be interesting to compare the predictions or prognostications of transistors around 1950. I'm sure they were so far off.

A: Well, the story is that IBM felt that there would be a market for 10, or something, for what they called computers now. Not work stations. It was the same sense when we did the first digital thing here at Varian, which was called the C-1024, which was a way of gathering data collectively to see the signal noise ratio. My wise forecast was that we would sell about 10 and that ended up being a new product and we sold several hundred of them. So people have a tendency to underestimate and overestimate. Underestimate on the things that you maybe you're way out and overestimate on the others.

Then, other kinds of things Ed was always active in and interested in were the social end of things. He was with the Urban Coalition and founded the Varian Nesbick, so I just naturally took those over. I worked on the Urban Coalition in Ed's place. We transferred our Nesbick to OCC and so I went on the board and been doing that ever since. So a number of these kind of community oriented affairs I had before Ed.

Q: Did you also work with him on things like the Cosens Committee?

A: Yes, I'd prepare documents, in a staff sense, and do background information, etc. I don't think it did any good, but we did it.

Q: As long as we've started talking about some of these general issues like long-range planning, I have a few more I'd like to ask you about. Now might be a good time. About some of the community oriented things, maybe before we get to the general issues, I'll just ask you about some of your personal kinds of involvements and areas of interest to you. One that occurs to me that's a spin off, I guess, from Siva is this work with Abram Goldstein and his foundation on addiction?

A: Yes, that really came from our association with Siva.

Q: Can you talk about that a little bit?

A: Abram felt out of place in the medical school, primarily because of the dean, a fellow by the name of Rich I think, who is no longer there. Abram didn't feel that he had the independence and that there was too much bureaucracy, too much red tape. He decided to set up a foundation himself. He got a good sized grant from NINH and he arranged to set up this non-profit foundation and take his research grant from Stanford and move it over and then operate as a fairly independent thing. But he always kept at least one little toe in the Stanford pool.
It started primarily as a socially oriented thing. He started doing methadone treatment and actually did work on clinical studies on the advantages of methadone. He had addicts who came down and got their orange juice with methadone in it. Initially, the government organized in a very equalitarian way. This was to involve both staff inside and community outside. They had a large board of directors and the idea was that community people would contribute. As time went on, they found out that maybe that wasn't quite the best way to do things and he began to take more control of it. Gradually he cut the board down to 5 and Ed Ginzton and I. Again, I think it was a useful combination because I knew enough of the science to understand and appreciate what he was doing.

Q: Was there a tie-in with the diagnostic tools that had been developed at Siva?

A: There was a break initially. We had awarded Abram some small stock in the partnership and he had to give that up when he started this foundation because he got a grant from the Drug Abuse Council and they considered it a conflict of interest. So he gave up that small amount. That kind of annoyed him. Then, at a later time, Dave Reubenfein (?) did bring him back in as a consultant and did something for him, but that was not enough.

Q: I guess, then, I'll ask you about some of these environmental and conservation causes because it struck me that there is quite a tradition of ties to the Sierra Club and all at Varian and at Stanford Electronics physics communities.

A: That's correct. Russ and Dorothy met, I think, on a Sierra Club trip actually. Dick Leonard was part of that group and somehow or other Ed was, too. Ed was always interested in the outdoors. We built this chalet up in Alpine Meadows, primarily Varian people. But that was all Ed's field. He put a push behind it because he enjoyed skiing; so did Dorothy Varian.

Q: Do you think that was part of the culture at Varian; their involvement in that kind of causes?

A: I think, in the sense that many or most of the people enjoyed that aspect of it and there was the tie with Ansel Adams, etc. There was a group or collection of like-minded people with respect to those activities. Dorothy, of course, did the Castle Rock thing. Russ and Dorothy were the major push and Varian, from time to time, contributed in one way or another to that activity. Dick Leonard was on the Sierra Club's board for quite a while and played a fairly major role; both he and his wife were quite active in the Sierra Club. I don't know that Ed was particularly involved in it and I know I never was for a very strange reason. When I first came here I used to enjoy backpacking and go to the mountains. As everybody knows, when you're in the mountains there's not supposed to be anybody else there. They should be all yours. So here we would go along a trail and find people scattered along for a few hundred yards and each person had
his little tin cup hooked over his belt with a handle on it. That became my view of the Sierra Club so I never had anything to do with them. But now I don't think much of what they are doing. I think they lost their original purpose, as far as I'm concerned. They could have gone on and quietly done what they could have, but they have become an activist organization without knowing too much about what they're talking about. I remember talking to Dick Leonard many times and he said he wasn't entirely satisfied with it either.

Q: Just by glancing through the photographs, certainly this goes way back to people like Anita Forres(?) - way, way back, were involved with the Sierra Club. It was really more a communal kind of nature experience organization and, of course, conservancy was part of that, but, with a lot of group activities. There was a sense of community associated with it. Now it is much the political.

A: That's right. That was all right, except I didn't really want that. I didn't care for that. I realize that there always had been at Varian, and this relates to the people with a communal interest, that their name was "Varian Associates". The idea was that we were rather equalitarian. Several of the people, certainly Myrl Stearns, probably Sig Varian and I guess others, were involved in the very early days of Ladera. Ladera, at that time, was being developed more as a communal project by, I'm not exactly sure whom, but probably Ed was part of it and Myrl Stearns; that turned out to be a little bit beyond their experience and capabilities. It was taken over by some commercial real estate firm and it was expanded. So, I think some of the Varian people lost money in all of that.

Q: Russ Varian probably wasn't in it because he stayed in Cupertino?

A: To my knowledge, no. He stayed out of it, but there was an early feeling that it would be nice if we all kind of lived together and worked together.

Q: Of course, with the Varians, I guess that goes back to their upbringing.

A: Yes, the socialistic style. For Russ, he got that from his father and was very socialistically oriented. I don't know if with Russ he was ever politically oriented that way, but it was an acceptable way of life for him.

Q: I think it is a very interesting kind of consistency in those kinds of views towards community and everyone kind of being on the same wave length about some of these issues. I think that is characteristic about this company as compared to other companies.

A: I think that is right. It was the nature of how it grew and who headed it. We used to spend a lot of time trying to get involved with people. We followed, at one time, what was known as a Schilling(?) plan, which was to have a shadow board of
directors that would handle certain kinds of problems, try to be innovative and provide a communications link. Marsh was in the company as treasurer, maybe secretary or something, and he brought in the concept of the Schilling plan. We adopted it but we didn't call it that. We called it Management Advisory Board. We had meetings every week or every month or two. So many of us were on that, I guess that was one of the first aspects. We did a number of things. One thing I remember was getting the credit union started. That was a project which we carried out. The idea there was to provide closer communications all up and down the line.

Q: What was the Schilling that you refer to?

A: That was the Schilling that sells salts, pepper and spicy things.

Q: Related to that, I've seen advertisements from some companies as early as 1960 in trade journals and such, where in company recruitment ads there will be an appeal or refers to an intellectual climate, physical climate in California, etc. I imagine some of these people oriented kinds of projects that Varian had also would make a good recruitment feature. That the company policy toward employees, along with the informality of things on the west coast, could have been something that Varian emphasized.

A: I'm not sure it was emphasized in recruitments. We did recruit from all over the world. The fact that things were tough in Europe and nice in California was a real plus. We had immigrants here who were extremely bright and capable. Interestingly enough, it was pretty much with Europe at that time and now it's with Asia bringing in the bright kids who start working. It looked very good to them and it made it very easy for us to bring people in. I think they got a flavor of what happens and if they came in as a post op(?) or a visitor, or visiting professor, that's what they would have sensed was the informality. I'm not sure we viewed it in that way particularly. I've worked for Westinghouse sort of off in the corner and still wore a tie and wear one here. I didn't change uniforms, but certainly we had an open door sort of thing. There was a lot of time trying to figure out on a committee basis what the objectives of the company were, how we should treat people, how we should treat employees, and customers. There was a lot of time spent on that and a lot of time on education programs. There were strong programs with Stanford, etc.

Q: But it wasn't, at least in the early 60s, a conscious thing?

A: No, I just didn't sense that. Somebody else may have a different impression of it, but I really didn't see it that way particularly.

Q: Let me ask about some of these key issues. There was one I wanted to ask about that I know you've worked on, but we haven't talked about explicitly, and that is Varian's program of working with Soviet and Chinese technology-based companies. Can you talk about how that started? What your role was in it? What was in it for Varian? Why did Varian embark on this?
A: As far as Varian's concern, and of course I'm always speaking with a bias to the instrument end of it because I'm not sure about the tube activity, I think we sensed at a very early time, although I don't know if we articulated it or not, that equipment of this type had to serve an international market. It couldn't just serve a domestic market and leave the rest alone. Dealing in scientific matters recognizes very little in the way of political boundaries. We just had a natural propensity to serve all scientific people, no matter what country they were in. We did establish, very early, representatives, if you like, in Europe. I'm not sure we talked about this, but Harry Weaver was one man who spent maybe a year or so in Zurich working with Franz Stough(?). He had gone back for the physics department and he was sort of our man in Europe. He was followed by Warren Proctor(?), who lived in Holland for a while. We actually sent John Moran over as a service man working out of Holland. The reason for Holland was that Warren had sort of an adjunct appointment at Leidman's(?) so he had things to do there. He also worked with Abergam's (?) in Paris. So we established rather early then the scientific presence in Europe. Then we did set up with Warren Proctor a company in Switzerland and they established a research laboratory, like IBM has done. That was in Zurich. The commercial offices in Zug were for tax reasons, but the research was in Zurich for technical reasons.

A couple of things happened in Russia. One of their people who was in New York, I think Aantor(?) who knew what we were doing, thought it was a good idea to have an exhibit in Moscow and that we should decide ahead of time so we could buy and ship stuff over. I think that was in 1967. So we have our first private showing of NMR equipment.

Q: This would have been the first NMR, period?

A: Yes, they may have tried to do a little bit themselves, but to no success. Then Ed went over with the National Academy Group and, as part of that, Kobishiani(??), who had taken an important role in the newly established Science and Technology Committee which was established in about 1965. The purpose was to coordinate technology within the Soviet Union and identify what technologies they needed and get them from the West. Kobishiani threw out the challenge publically for Varian to come and sign an agreement with the State Committee. The agreement was something that would legitimize it from their point of view (the interaction which we had). For us, it really didn't mean much because we don't need that sort of thing, but then we did prepare and did sign such an agreement. We began to explore ways to work cooperatively. For example, I spent a couple of weeks there taking a lot at some of their instruments, facilities, and ideas. Nothing ever came of it, but at least they opened it up to that extent.

Then we did develop a relationship with Moscow State University, which we still have, and it's developing into a little bit more commercial relationship. That was the start of our scientific
presence. The thing that has always amazed me was that, no matter where we've done throughout the world, Varian's been known. They never has been a question that we had to establish ourselves. Everyone knew Varian when you talk about scientific things. We were respected and welcomed in the countries.

The same thing happened with China. They opened up China after Nixon and, I think it was 1972, they sent over a delegation composed primarily of people from the Academy of Science. We were one of the few institutions that hosted them here. It's kind of interesting because at that time we had secret service people all over the place and everything was very formal. Then gradually we began to do more of this, so we had quite a number of people. Incidentally, this is well cataloged and if you decide what you want to do with it, you can have it. I actually started a system where you can track what happened and so all the documents are in the computer as far as a listing of it. They are all numbered and there are long shelves full of these documents that tell how it happened, what happened, etc. in China. If somebody wants it, they're welcome to it.

Q: I think we would be very interested in that. We have an interesting "sister" collection to that in that we have Ed Feidenbaum in Computer Science and Artificial Intelligence was involved in a few of the early contacts with the Soviet Artificial Intelligence community and also in connection with the Naval Academy. It would be interesting to have those.

A: It could be because he actually had contact with Marchuck, and then Marchuck came and took the head of the science technology committee and now he's the head of the Academy. That's quite an interesting thing there. But just for your general background you can have this computer listing of the documents and it's all indexed for you so it would be quite helpful. If you ever wanted to see these other documents, you could. As a matter of fact I have the big tapes here if you wanted to print this out.

Q: Good, I'll give you a call on this.

A: That could be interesting because Ed had some interaction with Marchuck.

But, to finish this China thing, it was our experience in Russia, and the need to legitimize at a higher level, that was helpful in China. When Ed took a trip to China with that Stanford group, he came back and thought about how do we do something with this. We conferred with Tom Finger and John Maliss over there at Stanford and we were part of that group. Together we worked out a concept that maybe we should do the same kind of thing in China and then we had to decide with whom should we do it. So the decision was to work with the Academy. It happened that we had had as a visitor the Secretary General of the Academy. Actually he had been here twice and we knew him fairly well. We wrote to him and suggested that we establish a relationship.
and he agreed. We went over in 1980. It was a rather interesting negotiation because it was all so new to them that they really didn't know what to do. They could not do it through the Academy because they were not allowed to have interaction with foreign companies. At that time, they had the equivalent of a foreign trade organization associated with the Academy and so this had to go through the commercial end of things. As a matter of fact it was negotiated not by a professional scientist at the Academy, but the guy who was president of the Oriental Scientific Company. We worked out some agreement and understandings and that still exists. But, China, did not have the same kind of mentality that the Russians had. They don't have quite that fear of foreigners so they don't feel the need of so much protection. They aren't quite so paranoid and they are a little bit more willing to take a risk. Even though that exists, we haven't called on it too much. We let two years go; Ed met the new president at the National Academy of Science meeting and opened communications regarding the letter so maybe we'll do something more sometime through them. It exists, but it is not a legalistic thing. It is an understanding that we developed.

Q: So far, there hasn't been any impetus to get any real practical payoff on this relationship, is that right? It's been more of a communication?

A: Well, we'd like to have a practical payoff. In Russia, it was extremely important because that is what made it possible for our sales people to interact. They have more end user contact than we would otherwise, but in China it is less important. We can get to the end user. In both cases, it's primarily the reputation of the scientific people which makes it possible to interact with the end users. We know some of these people so we can by-pass the normal bureaucracies.

Q: Is it a substantial market in the Soviet Union?

A: Actually there is a lot of business, but for various reasons we have not done as well as we should have. One of the most profound reasons is our own Government. We were beginning to do quite well at major share of the markets and then the Afghanistan thing came and that just essentially wiped it out. We kept a low profile and continued to work in there. Now, a major difficulty is export licensing, which I feel is entirely unrealistic. Things may be a little bit better now, but not enough to help us particularly. Competitively, it is difficult because the German competitors have much less of a problem than we do.

Q: Can a state-of-the-art NMR device be sent to the Soviet Union?

A: Well, the curious thing is that the NMR itself is not on the list. That's not a problem; maybe it should be, but it isn't. The problem has to do with computers. Here we run into things like the Winchester Disk and the requirements on the computers. To me, it is rather naive on the part of the Defense Department to think that they can do anything by keeping one or two instruments
out. But, the argument is that if you send in this powerful computer, they're going to take it out and they're going to do war calculations. In the first place, they have bigger computers than that. In the second place normally, well one that I did a study on, the computer portion was about $20,000-25,000 out of half a million dollars. If they are naive enough to want to spend half a million dollars to get a computer, why go to it? They could steal them for less money than that. That's not a problem at all.

Q: I remember reading somewhere that in terms of microprocessors, it is actually specific that up to an Intel 8088, the original IBM PC chip, it is now O.K. to send. Anything beyond that isn't.

A: Well, it is not quite that. You can do more than that. What happens is that they raised the standard for the last 2-3 years so that, competitively, that makes it difficult for us because the Germans can bring in the latest and we're lagging way behind. The other bad thing about it, of course, is current production. You can't send two-year old equipment because we no longer make it. We don't know how to make it any more, so we have to send what is currently in production and that makes it very difficult. But it is a curious quirk that it's not really the NMR, but from a chemistry point of view it is extremely valuable to them.

Q: Would the medical electronics be affected at all?

A: Yes, the same problem. If it has a computer in it, there is a problem. We can send them linear accelerators for doing industrial x-ray applications, but if it has an image intensifier on it or a computer with some of the latest things, we can't send them in. This is very curious because you know very well that they can take this accelerator and use it to irradiate rocket fuel. That's what we use it for. They wouldn't have to do anything else. You can also put the welding in whatever you want. But that isn't on the list. It's when you get a computer on it that it goes on the list.

Q: So that the fixation is really with this one thing.

A: Yes. It may be that they have a lot of bright bureaucrats back there and maybe they really do want to stop the rest of this while they look at the legalist holes they have in it. But, it does make it difficult to deal with East Block countries. The thing that is a bit of a problem is, as I mentioned earlier, in order to be successful in the business you have to have market share which means you have to market worldwide. If you lose a sale in the Soviet Union, that cuts your total market share. Maybe that one instrument is not all that important, but what happens is that if you lose market share on a worldwide basis then the guy who has the best market share has the best chance of winning. So you really have to fight these things in each and every country and this is not always recognized by managers.

Q: That opens up a whole question as to national competitiveness and the different kinds of business strengths that one battles
with. How has Varian dealt with that whole issue of foreign competition?

A: In a number of ways. We have gone overseas and set up joint ventures in a number of places.

Q: With foreign companies?

A: With foreign companies, and sometimes not a joint venture, but independently, because of the country. We have a joint venture in Japan and one in England in the tube business. Many of these, particularly the defense oriented ones, really want local control. We had a joint venture with the predecessor with Thomson-Varian. Thomson was in eastern France and, essentially by government edict, we were thrown out of that. That's been costly to us because actually those tubes are now competitive.

Q: That was the American government?

A: No, the French government. One has to tackle various problems in different ways, depending on the local situation. In China, we have several licenses and put kits together.

Q: In a way, this is all consistent with what Varian was doing in the early 1950s with this community building. It was finding ways of marketing?

A: That would be normal business practice. If you have a problem, you have to figure out ways to try to solve it. Management can do two things. They can say that that is going to be more effort than it is worth so I'm not going to do it and I'm going to give up that market and concentrate on this one. That's one approach and one that is often used in the near past. The other is that the manager's job is to figure out how to accomplish it when you have a problem; to figure out a solution. So we had a mixed bag. Some things we did very well, and some we did not. Some things managers, from time to time, back away from it. You can say that is a wise thing, but this is not necessarily true because, again, if you lose market share you've lost position.

Q: When you bring that up, it reminds me on the point that we touched very briefly and said we might return to later. It had to do with consumer products, specifically the microwave ovens and your comment that Varian decided not to produce those kinds of magnetrons because the company really wasn't interested in going with that kind of consumer technology. Is this one those types of decisions that you're talking about where managers make the decision to back off from a certain area because it is not worth the trouble?

A: Yes, that's right and we've dropped products for the same reason. If it looks like it is going to be tough, rather than doing the right thing, you become a hero by getting out of the business.
Q: Was there anything about that microwave oven?

A: I don't remember too much about it, but I do remember that there was a study and we simply were not cost competitive and had we been, I don't know Raytheon or maybe somebody else, might have decided well the answer is to figure out how to become competitive. But then, the mentality was not directed toward that kind of business. It was directed toward something else. I may have mentioned this, but Norm Parker, who came on board initially, seemed like a good match because he came from a high technology company. But the mismatch was that he was not a risk taker and he did not understand that you had to decide that you were going to do these things and then you either succeed or you fail. You can decide to engineer and design a low cost magnetron when you know what the market is. Aerospace companies work the other way. It's a low risk type thing there. However, that was a mismatch which would have kept us from bringing in the commercial business.

Q: Do you think that there is anything we haven't talked about that you think is important, either at the personal level or the company level? Do you think we've pretty much covered all of the major topics?

A: I think you have touched on the major things and I think we recognize that we're in two vastly two different kinds of businesses which always tends to make it difficult for Varian's people. We've touched on the importance of the foundation of the top management and the culture which, by and large, is still with us. Despite the fact that we had a little problem with Continental, that's not our way of doing business. He was an outsider who came in from one of the aerospace companies, so his kind of behavior which is not ethical is not typical.

Q: Thank you. I think that may be a good note on which to end the transcript.
This is an interview with Martin Packard, Varian Associates, October 27, 1988.

Packard: Things that larger companies would not do. That's about the good of it.

Lowood: Yes. (Laughs.)

Packard: Namely, it's very hard for big, established companies to develop new ideas from work, new ideas for a number of reasons. I think that [falls all along on].. But I think it is intriguing as to why, why Varian, why H-P managed to go from an entrepreneurial company, which was maybe a hundred people and twenty million dollars, to a billion dollar company or a ten billion dollar company. So, if it's okay with you, I would kind of try to talk on those subjects which [see how much] have a bearing on that. Does that seem okay?

Lowood: Okay. Sure. That's--

Packard: So that means if there's something specifically that you'd like to have more information, just please go ahead and either interrupt or whatever, if there's some area that you're particularly interested in.

Lowood: So do you want me just then to go through—would you prefer to start with some of these factual questions, to get them out of the way and--

Packard: No, no, I'm sure you've thought a lot about this and so you want—so why don't you speak right up and guide
me _______________ in some cases restrict your comments and in some cases _______ expand.

Lowood: [Spoken simultaneously with Packard's preceding remark]—for there're a few here that _______________.

Okay, sure, absolutely.

Can you clarify, for example, when you arrived in the company after 1951, did you work exclusively in research and development at that point or were there—was that the kind of position you were hired for?

Packard: I was actually hired by, directly by, Ralph Kane, who was one of what I think was a co-manager at that particular time, and the finance thing, which was important, came from Office of Naval Research, and this was to work on Russ Varian's complex _____; in other words, learning to make ends meet. We used outside financing, which paid my salary, but that didn't mean that I didn't take an interest and involved in other aspects of it. And part of what you did on the black box was applicable to the more commercial end of things.

Lowood: I think we talked about the black box a little bit. Was that actually the position when you moved in here? Was that group put off in a corner?

Packard: Well, that group was me.

Lowood: Were you put off in a corner, or were you part of——

Packard: At that time we were extremely informal and located in essentially a warehouse structure, and so there [were] almost no corners; there were no walls, basically, maybe little half-partitions, and also we each had little cubbyholes and had
a desk alongside three or four other desks. And initially I worked more or less by myself until we began to build apparatus and then in a [in a half pepper and guys][?] working was beyond that.

Part of the time I would do other things, like we once made a—well, one of the products which Ralph Kane was working on was a fluxmeter[?] and it had some certain characteristics and had what we called a probe, which was the part that had the—was the sensing, the transducer part. I undertook to do that, which then got shipped out as a product. So there were things like that that happened. And also—no, very early [___ it that these mornings][?] selling one of Russell's ideas which had to do with all the logging by virtue of the free precession magnetometer, and that was before the free precession magnetometer had been reduced to practice. It was patented, and Russ had the idea that nobody demonstrated it.

So I think, somehow or other, early in the game, I got involved in seeing who could make up that work, and also did some technical work and eventually worked with Byron Jackson and other people on that particular project.

Lowood: So you worked on a variety of projects.
Packard: A variety of projects—that's exactly right.
Lowood: Was this in what was called the Varian Applications Lab.
I ran across that--
Packard: No, no. We'll probably talk about that in a minute. But, no, that would not have been . . . . What you have here about research—I suppose I came as an ordinary engineer or
something, but then I did become director of the Worby[?] instrument research. I think I actually reported to Bob Jepson, who I guess was research director for the company. But this was a transparent appointment. I don't know whether he signed my time cards or not, but at least we had very little interaction, because I worked primarily with the instrument people, which points out some of the informality of our organizational structure, with not having structure in any sense of the word, and then people generally were free to tackle whatever kinds of problems needed lid[?], but that size of organization that I and others would be involved in that faceted[?] problems which had some marketing to management to whatever—in other words, our span was broader than the particular job which we were ostensibly hired to perform.

Lowood: Was that informality very intentional or kind of accidental or just a reflection of the personalities of the Varians?

Packard: Well, I think both. In the first place, Russell was a strong believer in egalitarianism. He had a father who was a socialist-minded individual, and, as I'm told, the name "Associates" really meant that, that everybody would be essentially the same. And the stock was initially held by employees, and there was an objective to make them—no big block of stock held by either the founders or other individuals. So, the atmosphere was certainly one of informality, to a large extent kind of predating the concept of ___________ and the "hang loose" sort of philosophy.
Well then I also think that it's fairly natural that you have people who are primarily technically oriented interacting with people with primarily technical backgrounds, which we were at the time—all the top management was technically oriented, so that there were very few people who were brought in because they were accountants or what have you.

At one time there was kind of the feeling that it would be easier to teach technical guys what they needed to know about accounting than vice versa, and there's some truth, of course, in that.

Lowood: So, as you mentioned, you were director of instrument research, and that was your position through probably the middle and late fifties?

Packard: Yes. And that became, of course, more formalized as time went on, but still working closely with the other functional groups. We became gradually functionally oriented but still with a history and a culture of interchanging and communicating and understanding.

Lowood: Can you talk a little bit about how that change occurred and maybe, roughly, when it occurred to a more functional organization? I guess maybe if you could say what sorts of things that were happening within the company gradually caused that change, or if it was gradual, or was there a conscious decision to do it.

Packard: This was gradual. I don't think there was ever a suggestion in the company that we should have one centralized manufacturing, one centralized development and one centralized
marketing, although that's not entirely true. And that was because the businesses were quite different. That actually continued and still exists at Varian, as they're working with the defense contractors quite different from what you do with commercial enterprises. That may be one of the questions you ask later on, and that's part of the answer to that. We're not that homogeneous, even in technology, which is probably not so bad, but less homogeneous than a customer base that we worked with. But I think that's a good question and we might return to that.

So the instrument division always had a good deal of autonomy. Initially we were just all one, and probably it was all R and D; I don't think there was anybody else but technicians, who were in the R and D area. They built the stuff, because we only make one or two or a small number of pieces of equipment.

Then Emory Rogers was appointed as sales whatever, in charge of sales for the division. Now, I think he probably actually reported at that time—he could have reported at that time to a man by the name of John Clark, who was the sales manager, marketing manager for the company, because we had a number, from our point of view, of kind of horror examples where John Clark wanted everything to go through him; we weren't to make any contact with any outside customers without going through him. So, we'd write a letter on a ___ bill, and it had to be approved by John Clark. I've got visions of him taking his briefcase home every night and trying to
(laughs) read these letters and sign off on them. I don't know how long that went on, but that was management's effort to try to get control of what was happening. That was a phase in the control-centralizing, if you like, aspect of it. Well, that didn't last too long. Just the sheer weight of it was so ponderous that it disappeared, and, before too long, John Clark disappeared, too. He went back to Cornell Aeronautical.

So then, I suspect that gradually the division became more autonomous and less part of the company, although there were some aspects of it, like accounting and all, which became expanded, because we'd added government contracts, and so there were two divisions.

Lowood: Each time you say "the division" it's the instrument division.

Packard: It's the instrument division I'm talking about. At that time, the tube division was the predominant activity, and even though we had some quarrels with the tube division's way of operating, it's quite true that without the financial support and all of the tube division, the NMR activity, I don't think, would have survived. So that happened to be an extremely important aspect of it. It was both a hinder but it was a help.

Lowood: It was a big brother, in a way.

Packard: It was a big brother, that provided—that kept us alive. And that was extremely helpful.

Lowood: I assume we're talking about around the mid-fifties.

Packard: Yeah, that's right.
Lowood: There was a tube division at that time, the instrument division. The accelerator—

Packard: That hadn't happened yet.

Lowood: —hadn't started yet. The vacuum stuff hadn't—

Packard: Hadn't started.

Lowood: —started yet. And the medical, that's electronics—

Packard: Oh, that came much, much later.

Lowood: So it was basically a two division—

Packard: It was just a two-act company, on NMR on the one side, and—it really was just NMR and not instruments, just the NMR activity. And then the tube-activating company.

There were efforts on the part of the top management to try to keep people informed. I think that was the important aspect of it. We used to have little technical advisory committee meetings, which involved technical people from both sides of the house and topside people like Marvin Chodorow and others, although Marvin's expertise really lay on the tube side.

So, anyhow, somehow or other we began to develop a little more functional—this had been under Ralph Kane. So we had R and D, which I guess was under me; manufacturing—they may have begun to break that out, probably did, but that again would have been a jobshop with one guy in charge and a few others working on up. We didn't have production engineering, in the sense that we think of it now, for serial production. It was just a matter of technicians putting stuff together.

Lowood: How long did that last, by the way? When did things
Packard: That's a good question, because it is an important aspect of what the management did, and by management I guess I mean Myrl Stearns and Ed Ginzton and others. Let's pursue this, and then we may want to go back to the other.

Lowood: Okay.

Packard: Ralph had wanted to improve our manufacturing operations, and to do this, he made a deal with George Quist, who was a San Francisco golf player[?] and became one of the early venture capitalists and _______ fairly early. To buy this product, which was a strip chart recorder, one of George Quist's start-up companies assumed up the design and got the entrepreneur that went with it. And that was liberally done on Ralph's part to provide a bit of a basis for building it in more of a serial manufacturing operation. It happened to be a product which we needed, because there weren't any really suitable recorders for what we were doing, and Ralph was dying for something else.

Lowood: So that's a good start. Did the goldish—

Packard: Yeah, that means it's probably not the millionth, but that the house has the ten thousand.

So we had that in mind for quite a while, and it was useful. Then, along the way, it developed Bob Jepson and Lew Hall, I guess. I don't remember who the inventors are, and the--

Lowood: What was the other name?

Packard: That was Lew Hall.
Whether or not—this part of it I'm not sure, but he was a
guy from ______ who invented the vacuum pump, which was an
improvement over something that had been done before by a guy
by the name of Pennick[?]. The reason for that development was
that we were obviously interested in vacuums, because this was
the problem of all tubes: how do you make them get all the
stuff out? The electrons are free to do what they wanted to.
And so—we're doing research in that area. And that was
recognized then as a possible activity, partially because of
the interest and the awareness that two people had in the need
for it, and probably partially because one of the early
consultants—and, as a matter of fact, he had been considered
as central research—which I think we want to come back to
later, too—as the first director with DeCrime[?], but
nevertheless, he worked as a consultant fairly closely—and he
had been working at Westinghouse on higher vacuum systems to
get down. At the time I was at Westinghouse, we worked with
vacuum systems with ______ minus six, and Alfred had been
working to get ______ further down so he recognized that
(phone ringing, other noises and general confusion).

And I mentioned Dan Alpert, which may be a name which you
didn't come across. I used to work for Dan Alpert at
Westinghouse. In fact, he's the chap who introduced me to
Felix ______, and Ed Ginzton and others knew him.

So, Lou Malder, who had been hired to do the central
research, saw that this vacuum pump was a good opportunity and
could see some problems in central research, and so he elected to set up a little division on the vacuum end of things, and they started up with a separate facility over here on Park Blvd. and began to produce systems of ultrahigh vacuum, then got to the point of making do for manufacturing it all.

And then the concept developed—and again, I'm not quite sure who pushed it; I suppose Morrell Nother[?]—of setting up a combined manufacturing operation, which would serve us—both the instrument division and the vacuum products—to try to get enough scale to be able to mount a reasonable effort. And so, there was set up such a kind of a separate division, which was supposed to service these two embryonic divisions. And the guy’s name [long pause]—well, we'll find that.

Lowood: Maybe that's something we can fill in later.

Packard: Yeah, we can fill that in.

We hired a man by the name of Red Sulzbach, who came in and set up, in Building Four, manufacturing operation, which was really quite well done. He brought in good equipment, brought in good people, good manufacturing methods, and set up what was a competent operation.

Lowood: So this was after the ______ was beginning, so maybe it was the early sixties already.

Packard: Yeah, I guess so; it was beginning to be an important product line.

So this, again, was kind of answering the questions which I raised: how did Varian manage to survive? And that's part of it—that somebody had the judgment to try to set up and that
direction was up.

Even though it was a useful and really quite a good operation, it turned out not to be successful, and for reasons which are, again, rather commonplace: that the sort of general managers of the divisions would get into quarrels about the manufacturing operation, and it's generally true that people who are general managers don't feel happy if there's any ambiguity in a system. They really want to be able to hire and fire and direct their subordinates, and their feeling as general manager is that, "I don't want to be told by anybody above me what do to, but I want to sock everybody down the road with what to do." That's the way you can decide whether he's a general manager type.

So what happens is that—you know, it's quite interesting, because the reasons having to do with the divisions—something doesn't work right, either the sales clerk has this wrong, so they tell the manufacturing to build too few or too many of something. Then you say the building factor is quite long, so you buy a bunch of inventory, and it's no good and you have to write it off, so you don't have enough parts to supply the marketplace. Or, as it turns out, that the engineering group—a problem develops, which will always develop, and the engineering group has to make a change in order to make it work, and so on.

Lowood: Or the manufacturing has ______________.

Packard: Yeah, right, right—or it doesn't function. So, in my view, there's several kinds of problems which are not
necessarily related to the manufacturing operation, but related to deficiencies on the part of engineering staff or sales. I'm not saying that these deficiencies could be corrected, and that's bad, but it exists that way, and what a general manager always likes to be able to do—and do in a low-visibility way—is to be able to have his manufacturing manager shift something to match what the immediate demands are. And if you run a joint kind of thing, then you've got pressures from both sides, and you can't make those adjustments. So the system really is almost foredoomed to failure. And the result was that the instrument division inherited that activity, and the vacuum division set up their own manufacturing.

And just one further—it isn't important, but just supports this—one of the efforts at Varian a few years ago—I'd say when we brought in the new president from Minneapolis Honeywell and Tom took over as CEO—was we brought in a man from General Electric whose objective was to enhance the manufacturing at Varian, and he was to do this by any of a number of ways that he could do, but he had manufacturing skills. And so they began to try to get the divisions to beat the manufacturing work done in a central organization, and you build up a plant down in Tempe, which was quite a nice operation, but it didn't work and exactly for the same reasons, although people don't want to say that. But that's what it amounts to. So we tried to centralize manufacturing in a way which was sensible, going to back in the fifties, and again we tried to do a written[?] thing and said, "We don't want it
--or successful." And along—part of that—in the sixties, about '65 or so, Ed Ginzton had the concept of let's build the instrument base up beyond NMR and make it a broader scale. He was smart enough to know that it didn't do any good to say—by that time I was general manager of the division—"Well, why don't you go out and do this," because like all general managers, your focus gets to be (laughs) narrower and narrower. The way to do it was to acquire a company, so we acquired a company called Aerograph, and we acquired a mass[?] spectrometer, and we acquired an AA company in Australia and an optical company in Monrovia, all of them important, strong companies.

---

Let's see, Aerograph was the--

Aerograph.

No, that was the gas chromatography.

Okay.

And Carey was the—Carey Instruments down in Pasadena. And a company called MAT in Germany, doing mass spectograph, spectrometers, and a company--

Is that M-A-T?

M-A-T. That [was an] acronym for something.

So those were the three companies: Carey--

Well, and we bought one down in Australia on AA, called Tektron. So, actually we had a broad, broad—we had the
NMR-EPR; we had mass spectrometers; we had UV and optical spectrometers; we had atomic absorption spectrometers; and we had gas chromatography, which made it really a powerful bundle of activity.

At this time, we were the dominant manufacturing operation, and Aerograph people were just kind of starting up. D. Z. Chu was the manager; he was the general manager of the Walnut Creek Aerograph—now we'd probably call it something different. And I thought that we could—I can't do what hadn't worked before (laughs)—and that we had set up punch presses and sheet metal bending and all that kind of stuff, and that we could do it for the Walnut Creek people. So we tried it, but then had them succeed, even though, at the thought level, we thought it was a good idea. It simply did not function, and so we were unable to develop any real cooperation in a manufacturing sense.

Lowood: Let me just get the tape over.

END OF TAPE I, SIDE A

BEGINNING OF TAPE I, SIDE B

Lowood: —encourage you to go on this tangent on manufacturing operation. I believe you had been talking about the research and development, what it had been like when you had ________.

Packard: Right, because we were talking about how did the organization become more a functional ________—

Lowood: Uhm. That's right, that's right.

Packard: ________ carry that on.
Lowood: You had a thought there that you wanted to continue—

Packard: Yeah (laughs). Well, that saves you putting the bits and pieces together. So many of them taken apart again that put 'em in the way you want.

In the very early days, we R&D people, which we mostly were, worked closely with Emory, who had a Ph.D. from Stanford under Stout[?], and he became the marching[?] guy at Personnel, and it more that way than it would development. And it was interesting in the early days, because we all got involved in the selling and the ________, and I remember one time—well, it sort of seemed like Emory sold the same customer three or four times, because he would tell us he made a sale, and then we would find out, well, gee whiz, that's really ________.

(Laughs.)

So, out of that you ask about applications laboratories, and I think—here again, I don't know exactly how it happened, whether it was Emory's idea, whether it was other people's concepts, maybe Jim Shoolery—but it was very early that we needed an applications lab in which we could work with customers and demonstrate to customers what could be done with the instrument. You know, we were charging lots of money for these—forty, fifty thousand dollars in those days—and until some early innovator, some scientist, we thought this was a useful technique, and it developed some justification, because quite often I'd have it going to the Board of Directors, and he had to convince his boss, and . . . .

So that was the purpose, to come back, and so that he
could say, "Hey, look at this problem which we solved"; and at the same time, to try to build a capability of demonstrating what could be done, because, in this case we were developing a market; there was not a market—there was a big market, potential market, but not for this particular technology. So we had a technology which had not been established in the marketplace, and my job was to develop it, and that leads to another interesting question, and maybe we should get at it. (Laughs.)

So, we did set up—and I think I mentioned in one of the things that Jim Shoolery needed a spectrometer, so we got him a soldering iron (laughs), and he took care of that.

Then, by the time—we didn't have any particular stock[?] up in the other building which was useful, but at that time we moved down here to Building One, over here. We wanted to identify a corner of the work area as being an applications lab, so several of us came in on Saturdays and Sundays, and we erected some walls to build a little cubbyhole. You may have seen some of the pictures.

I guess, somehow or other—I don't know whether anybody asked appropriation to do it, or whether there wasn't any money and somebody said "no"—and so we just bought some material, and the corporation paid for and we did it ourselves, something which you'd be hard pressed to do now: you'd either violate the code or violate somebody's sense of aesthetics. As a matter of fact, it wasn't a very beautiful structure, but it worked.
And so Jim spent his time in that as a chemist, and he would run the applications and do the problems. And so that was the origin and the sense of how we came to an applications laboratory.

Lowood: I see. So it wasn’t a lab to generate—it wasn’t producing research that was going to be used for products; it was really using the products in situations that were realistic and that people who might want to buy them would encounter. It would something that would correspond to work.

Packard: That’s right, and that we felt at that time—and I’ve really changed my mind—that it was important for us to do effective, publishable work. So we would publish a paper that Jim might do; it might be a paper which was done by somebody from industry that would come in—they would have a problem, and so Jim would begin to solve that problem, and that would be published as a paper; and this way to bring to the attention of prospective customers that we had something.

And this was an essential ______: we felt that if you’re going to expect people to share something with you, you have to have something to share with them. You can’t just go in a corporation or any place and say, "What’s new?" But if you’re respected and you have some information, then you can start a dialogue. And that was extremely important to me, to build confidence and rapport with the technical people who were using our equipment, because that’s part of the feedback that you’ve mentioned in here. And so, it was more than just a marketing gimmick. It was a research activity, and I can give some
examples of that.

One of the things which did come—this is kind of anecdotal, but I'll present it because it's one of the kind of strange ways that people's minds work. Here we're building—go back to when we were building this little structure—and for some reason or other, I've no knowledge why or what, it occurred to me that if we applied another R.F. frequency at the frequency different from the viewing, we would have to—you'd get a piece like this, and you'd have spins adjacent to it, and you'd get the splitting of it. Now that could be hydrogens which caused the splitting, and that's also some nearest neighbors[?]. Those could be an entirely other species, and so you get a splitting which is caused by the other nuclei.

And so, it occurred to me that—and, again, I don't know why—that, gee whiz, if we applied a radio frequency field on this, we could stir up those other spin systems, so instead of getting aids[?], we would collapse, and you wouldn't have anything. There would be none of this interference. And so we kind of talked about it, and we wanted to demonstrate it and set up a . . . . Well, I had Arnold Bloom, who was the theoretical guy—he'd done a lot of our early theoretical work; he was very effective in doing that; he worked a lot on some of our government contracts, too—maybe Jim, but Virginia Royden, who worked for us, worked for Jim in the applications lab. It may or may not be a name that strikes a bell, but her husband is Royden mathematic —

Lowood: Really!
Packard: —and became provost or subprovost or something.

Lowood: I see.

Packard: So it was his wife who was working with us and ______, and so there's a paper that came out that actually did not have my name on it, but Virginia and somebody else, and to a large extent that was the start of the—well, it's been interaction which has been so important in multiple radiation.

As part of the culture—and it's probably important; I think it would have stemmed from Russ and probably Ed, to some extent—but we were not as structure or as whatever—we leaned over backwards to letting other people have ideas, even if ideas came—if it came from a mutual conversation, and I would step aside and let the other person—which may or may not have been a bad strategy on my part. (Laughs.)

So we did not put on, for example, names on patents because of the director in a laboratory, or names on papers because of the director of a laboratory. This would not be good university policy. That's where the __________.

Lowood: I see. So you considered that part of your—as a manager of this group, part of what you did was to work, supply ideas, more to let other people [remainder of remarks obscured by Packard's voice].

Packard: That's right. You would want to let—you'd try to give as much credit as possible to the people in an organization, and I think that helped a lot, because we did attract a very powerful bunch of people in the research lab.

Lowood: Now, the applications lab was just a part of all of this
activity, right?

Packard: It was part of research. Jim reported to me for a long, long time.

Lowood: But there would be—publications and such would result from work that was done in other parts of the research lab as well, or was it pretty much concentrated in the applications?

Packard: No, there would be ______, because we were doing other things in the research area, like the magnetometer activity, like some of these other things, and those would be published differently. We once had a project on microwave spectrometer, which we abandoned—you were asking about what did we do in abandoned . . . . So, there were probably a lot of other areas where, yeah, you could build one of them, but it didn't appear to have enough usefulness to do it, to proceed.

Lowood: I wanted to follow up—when you described the applications laboratory, maybe rightly or wrongly, it led me to think about this sort of later question I've had about the "NMR at Work" series and NMR-EPR workshops. You were talking about forging these kinds of relationships with your customers. Can you talk about how these ideas, these two ideas in particular, came up?

Packard: Yeah, that's a good point. To my mind, I feel—I give credit to Emory for having originated that concept. Now again, you know, I don't know for sure, but at least I give him the credit for it; and I'm sure Jim Shoolery had some input on it. This was part of this overall effort to develop a market, there's plus[?] are these things good for. So, we put together . . . . And it's interesting to look at them, because you can
see the progression of improvements in instrumentality as we go along, both higher frequencies and more resolution and greater stability and all these kinds of things. So it makes an interesting diorama of the progress of the instrumentation, which, again, was a feedback kind of arrangement.

Let me give you one example. The spinner, which had to do with homogeneity, is Felix Bloch's idea, but Les Anderson and Jim Arnold worked on the concept of how to make it go, and they came up with an air-driven spinner and a few things like that. So that idea came, and actually Felix had a patent on it, so we paid him royalties. That brought us into the range of improved homogeneity, which was important, improved resolution. It made a tremendous difference. It's always—it's been a constant struggle over the years to improve that aspect of it.

The other aspect was that we had to sweep through fairly rapidly, because my main field would jitter, and we were looking at remarkably narrow lines. We had to have stability to a part of ten million, something like that, so that you can sweep through fairly slowly; so it had a part in ten million over a period of five, ten, fifteen, twenty seconds. And that was hard to come by. We did know about using nuclear resonance to stabilize, 'cause actually I had built a piece of equipment at Stanford which Emory used in his experiment, which was a proton stabilizer, field stabilizer, field controller. But this—at that time, it in itself was too noisy. It would have a very, very good long-term stability—it could stay forever,
within a very precision; but it didn't have a short-term stability, which would take the ____________.

So one of our customers, probably a guy by the name of Charlie Riley up at Shell development, mentioned to us about something which he had seen or heard done by a chap down in the Shell laboratory in Houston, which was called flux[?] stabilizer. It could sense the change in flux and fed that back to achieve the stability.

Well, the minute that was suggested as an idea, why, the value of it became apparent. And I remember a couple of us went down to Houston and met with the chap had built it, and what he had was a set of coils, I guess, and worked with magnets and used the galvanometer as a feedback system. And so we essentially were involved ______ in the job, with doing something with it. And so he put together a system, and ______ trying to use as much as possible what this chap had ended up with.

So we had—then, that was the next major improvement was the flux stabilizer, and in the EPR work you can see this, where we used to have generators, and every trace would be very solid. So that was a good example of where a very, very important fundamental idea came from one of the customers, improvements which he had made, and we put it into operation, and we were actually able to do that and very quickly, in a matter of a few weeks, so we had ______________.

Lowood: Clearly, I guess it was the recognition of that that led to the development of these workshops, and so on. Was that—
Packard: Yes. I mean, the whole overall feeling was, again, we had to have interaction; we had to be with these people. So we had the workshops, which were held physically here. We did that for a number of years. We'd get a hundred people to come in, and they were more or less no charge in answer to whether we charged or not, but it ______ a nominal amount. And this set up—oh, these were quite—people who could attract folks from industry, and some speakers of significance. One of our invited speakers was Brice Crawford, who was an infrared expert from Minnesota, and we had Gerard Piel, whom Emory knew, who came out as one of our speakers. We also had (laughs)—Cagert[?] talked about—I guess he was from Stanford—he talked about the consciousness-modifying features of LSD; he was one of our early talkers, before LSD was widely—

Lowood: This was within the NMR-RFR workshops?

Packard: Well, this would be at the banquet sort of thing.

Lowood: Oh, I see. So it was really—

Packard: It was high level—

Lowood: It was like a professional conference or something of that sort.

Packard: Oh, yes, very definitely. It was not oriented in any sales pitch way. Our people would give lectures, and I guess we even had invited lecturers from outside.

Lowood: I've seen the proceedings from one of around 1957 and '58, and I noticed, I think it was either Shoolery or Rogers gave—you know, this was published in there—a talk on the
fundamentals of _____ of NMR, something like those, and it was a very clear sort of presentation. And I guess that made me wonder—that kind of gave me the idea that the workshop was sort of like a training workshop, a kind of an instructional workshop. Was that a component of it?

Packard: That was a component of it. We had hands-on contact with the guys, if that's what they wanted, or lectures; you could interchange views. And that was important.

Lowood: Were some of the people that came people to whom these topics were new, or—?

Packard: We had both.

Lowood: Both.

Packard: Both. They were high enough level so we would get people to repeat and people who were very, very competent. And from this kind of interaction we would get, oh, requests where the guy in the industry or academia would want to do something, and he would ask can we build some equipment, so somebody . . . .

So we used to work with Jack Roberts at Cal Tech, who was an important chemist and became eventually provost and all, and we had to build a special kind of equipment. He had an idea that we build a piece of equipment that should be a kind of a test vehicle. I remember we built, I think we called it at first carbon 13 spectrometer ran it at Utah. So these were almost always themselves money losers, but, on the other hand, [we] preferred to look at it as a shared research, because he would show what you could do with it, and he paid a lot of the cost of it. So we did that kind
Lowood: Just an information question: In seeing some of the things that were done by the instrument division during these years, there was something that came up that I haven't seen called "News Note Bulletin"—

Packard: Oh, yes, you mentioned that in there. Was that done by the instrument division—

Lowood: What I—

Packard: —'cause there had been a company newsletter.

Lowood: Okay. Oh, that was the company—was that the company newsletter?

Packard: Well, we may have had a divisional, but I don't remember.

Lowood: Okay.

Packard: But they said there was a company, and that started very, very early, that Dorothy Graham started, and that is a very rich archival source.

Lowood: Oh, okay, I've seen those. I see, okay.

Packard: It started as being a [heterographed?]—

Lowood: Right.

Packard: —version, and if you want to know who did what on Saturday night—

Lowood: Exactly.

Packard: —why, that was—

Lowood: Was that with the Halloween party?

Packard: That's right—which house, who came to the Halloween party and—

Lowood: (Laughs.) An Eskimo.
Packard: So it was very gossipy and full of that kind of old stuff, I guess, as far as dates, place, people and—

Lowood: So it was nothing—it doesn't really belong with these other things, like the "NMR at Work" series?

Packard: I don't know, it may have. Yeah, but it_____ Very early, there was an internal house organ, probably much earlier than most companies. But, again, I think that was part of Russell's conduct, that information should be shared and people should—well, he had a famous saying that "Everybody has a right to know what they're doing," and so he— he, in a way, was not a good articulator, communicator, and at least he felt very strongly that that was an important aspect of it.

Lowood: Since you've been talking now about this interaction with customers, maybe now would be the time to ask how NMR was distinct from Varian's other bread-and-butter lines, you know, like the klystron or microwave devices—the tube division. How were the markets for the customers for NMR and for the tube divisions products different, and how did that cause differences between the divisions?

Packard: Well, I mentioned about John Clark. But very soon after that, then the marketing really spread apart. Initially we did have a set of representatives that—Neeley[?], for example, was one of them, and we had some representatives across the country, and we've actually had the same representatives that H-P had. We just followed in their footsteps. And I think they were good on marketing the tube side of things, 'cause really they couldn't do what we were talking about—much, much
too technical and not a high-volume product. So they really didn't do anything. We started international marketing the same way—Rock[?] did histories of something with our international marketing outfit for the tube division, and Emory followed along in the same footsteps, but fairly quickly separated from that.

There's always this big question that's gone on forever and will go on forever in marketing of high tech products: do you do better to have product specialists or do you do better to have generalized sales people to do your products? And so we organized and reorganized at Varian every two years one direction or the other. And I think now at Varian we're drifting back towards the product specialist end of it; but two years from now we'll begin to relax[?] again. In fact, you have to have both. You need somebody who knows the purchasing agents, who knows where the company gets their money and the university gets their money; but you need somebody who can talk to any customer.

Lowood: Was there a difference between the two divisions in terms of, say, military versus industry versus university types of customers that characterized one division or the other?

Packard: Yeah. The tube people worked primarily with defense-type people, whether they were contractors or whether they were defense department directly. So that was a portion of the government purchasing, and all this fairly restricted.

One other aspect of it—and this, I think, is probably important, too—is that tube division people, by and large,
take low risk in the sense of market risk; they don't develop a tube in expectation that it's going to be useful. The government or somebody comes to them and says, "This is what we would like to have; tell me how much it's going to cost to get it." Then the job becomes one of delivering, and delivering within the funds or going and getting an extension and a waiver; so it's a low risk in a market sense. Now the commercial aspect of it is high risk in a market sense. You spend a million dollars to develop a product; if it doesn't go, that's tough. And that became an increasingly difficult problem—well, let me backtrack from that. I think that that becomes useful—I'll talk a little about the split between the divisions.

Lawood: You want to do it now?

Packard: Yeah. All right. From the very earliest point in time, we had really quite different markets. Myrl Stearns was the general manager. He had come from Sperry, where he dealt with government contracts, felt very comfortable and was confident in marketing. He did a lot of the early marketing in the klystron area with the defense department, but really couldn't do any amount in the instrument side of it, and didn't try to; he was not—we were beyond that abortive attempt with John Clark—why, he really let things go.

Even in the early days there was conflict between the two sides of the house, because of the difference between the promotion[?] and defense industries. And at one time, I think probably Emory and Ralph and I went in to Myrl Stearns—this
was the time we were going to hold an election with the board of directors, and we were staging a minor revolt or something. And Atkins[?] came down in this section and they calmed us down, and so his report had been late the right party. [Last half of preceding sentence doesn't make sense; something has been left out.] So that was a problem which occurred. And we had accounting kinds of procedures, which were set but didn't fit what we were doing, but other kinds of activities. We still have that.

Then Ed became president and then CEO, and Ed, as I said before, did a remarkable stand in the ability to cover a lot of areas, and so he could recognize and understand the difference in what we were going to approach. And one way of coping with that is to divisionalize, decentralize, set up this division and this division and this division, and all of them functionally organized so that every one got by without an overall manufacturing or marketing or whatever objective.

Then we brought in from the aerospace industry a new president by the name of Norm Parker, who didn't help matters, in that Norm came from the aerospace industry—that's kind of interesting because my view has been such that, at that particular early time—[Outside interruption.] And my initial reaction was, hey, that's probably a pretty good choice, 'cause he knows how a high risk. But then one of our other—a very, very sage guy by the name of Ed Barlow[?], who had come from—do you recognize Ed?

Lowood: I know the name, yeah.
Packard: Well, he's very, very—a very, very bright guy, and he set me straight and said, "No, it's exactly the other way around," because of what I said earlier: that the aerospace is a low risk, except you got to get a waiver or something like that. And so there's just simply no relief at all in that aspect.

So, basically—and this jumps back to one of the earlier questions—what Varian has not been able to do, over the years, is to identify those things which should be decentralized and those things which should not be decentralized.

END OF TAPE I, SIDE B

BEGINNING OF TAPE II, SIDE A

Lowood: Some things should be centralized and some things shouldn't.

Packard: Some things should be centralized and some things shouldn't. These things are so complex that you can't expect very many managers who would have an ability to talk with analysts on the one hand and to understand what klystrons are and understand with NMR and understand the different market places. It's just asking too much of a person. The kind of thing that Gorbachev's going through now is that you really cannot do central planning with these things; you have to break them down.

On the other hand, there are certain things which you can do better together. There are some things which you have to: taxes, for example, have to be filed, and you have law suits which come in. So, that seems to work pretty well. But in
selling, for example, and if it's the Russians or the Chinese, you need to have a posture, a company posture, but not fragmented by individual division people. And in dealing with other countries—particularly, Japanese culture is quite different from ours—but we'll find a guy, make him manager of a division, and then he runs off to Japan and screws everything up in short order.

So that's something which the company has never managed to do and still hasn't. But Norm was no help. Actually, we're swinging the other way right at the moment under Norm Hawns[?]—it's towards a great deal of decentralization, which creates a lot of waves in Europe and other places.

Lowood: Currently, you mean.
Packard: Yeah, currently. So, that's how we back and forth _________.

Lowood: There's a tension you set up between the low-risk and the high-risk parts of the company, the way you put it. It reminded me of an analysis that's been applied in the semiconductor industry with regard to these specialized chips that the defense department wants and how, in certain contexts, these kinds of contract-oriented efforts take away from competitiveness, because you don't develop the kinds of chips that you're going to make billions and billions of and compete with the Japanese.

Packard: That's right.

Lowood: Can you think of any examples in Varian history that are maybe a similar sort of thing, where maybe a product which
should have had a high-risk strategy went with a low-risk
strategy, or vice versa, where somehow the way something like
that was handled in the company wasn't quite appropriate?

Packard: I'm not quite sure that—I can't think of it to identify
that, but I can't say that even on the instrument side—and I
used to spend quite a bit of my time working in making sales
calls—Ralph Kane and I used to go back to Washington all the
time—and what we would do would be to look for contracts that
we felt we had an interest in and that the technology was
important to us or it allowed us to build up a group of people,
like allowed Varian to hire me, which brought up the effective
mass of the organization.

So we tried to balance this reasonably well between the
contract—now some of the contracts which we had in the
gеophysical area really didn't support the NMR business at all,
and I'm not sure whether it detracted or not. You might in a
case that it did; I don't know.

Lowood: I just reminded myself when I asked this question and
mentioned the word "competitiveness" that I haven't—I don't
think we've said a word about a competitors of Varian's at
all. Were there any substantial competitors in the fifties and
early into the sixties?

Packard: Well, the first few years, as far as the NMR—is that what
you're talking about?

Lowood: Yeah, yeah.

Packard: Not too much in the way of competitors. We had it pretty
much to ourselves, which we really—one, as we had a patent on
it; but, two, because we were serving the community. We really were dominant, through the workshop, through these other things, and we were highly respected. So at one time we had an organization that met after the Pittsburgh conference called ENC[?], or maybe it was something different, and that was essentially a Varian ________. It was interesting, I remember in going back one time, that Broucker[?] began to compete with us and began to, not upstage us but at least to become important; because when you have too much of a monopoly eventually it comes to a point where people just sit back and, if you're the biggest, they look to support underdogs. So, Perkin-Elmer had NMR activity, which they ran out of England, never terribly effective. Japanese came in, JOL, Broucker[?], oh, I guess there must have been several other people like—Beckman[?] had looked at it but decided, no, it wasn't really for them.

So, in the days when we were small and, if you were with a large corporation, you could very easily decide that, well, you're only going to sell four of these a year and what's the point in working with it, so ______ elected not to do it.

Lowood: This was probably different—the tube division was obviously in a different situation; there must have been competitors in those areas.

Packard: There were competitors, but what happened was that, after the war, I think at the end, people began to feel that there wasn't much market for these things, and at the same time, Varian came in with a design for a special tube. _______ had
to do with that, fuse, which was really very good. And so it was a combination of people reducing their effort in that area because of a feeling there wasn't enough market.

So, for a while—and not because it's important to Varian's survival and Varian's being able to go from an entrepreneurial to a company—was that we did provide a service which was not provided. Now competitors came up in the TV business. We had Hymack[?]; General Electric was doing something; we finally captured that people market. Then people—Sperry was never a competitor; Westinghouse I worked for that we built—that was one of the tubes up there—never a competitor.

Lowood: So they never followed through on the war work.
Packard: They never followed through at all. And this was quite typically true, that they just simply dropped out.

One of the tubes which I worked on at Westinghouse, which is a 1B24 up there, was built by Westinghouse; it was a Westinghouse development. It was built by Westinghouse, but Sylvania was set up as a second source. Sylvania went out of the business. Two guys[?] bought the equipment, people that had worked on it, moved to McCarthy[?], built up the equipment, started a group he called Beumac[?], which Varian then bought on the East Coast. So these guys of entrepreneurs took the equipment which Sylvania wanted to get out of, built Beumac, _______ brought this to the government. So there was quite a shifting, and I think our timing was probably right. But, of course, competition then did build, and the
bigger companies came back in and have traveling right through to back where we lost theirs.[]

Lowood: Let me just ask you one more question, and then we'll move forward into some of these later developments at Varian. You mentioned briefly the introduction to spinning sample and all of that. I have heard this anecdote about—

Packard: TE[?] tubes.

Lowood: Yeah. Stirring the tea and all of that. Can you explain the origins of that anecdote and what—maybe it'll go into a little bit the kind of interaction between Felix Bloch and the company that led to the development of the spinning sample, if it is, in fact, true, the way the anecdote is generally told.

Packard: Well, probably Wes or Jim Arnold would know better than I. But I have no reason to question the anecdote. I don't remember whether hearing this from Bloch or from other people, but it's a very logical anecdote.

We knew from earlier times—Winovac[?], Chemarmel[?] and I—I guess it was Chemarmel and I—did an experiment at Stanford when I was still there which involved—we were looking at the alpha[?] alcohol, which was one of those first traits in , and we found as we varied the temperature that this peak, this hydroxal[?] peak, shifted and wasn't fixed temperatures. So we published a paper on that. We didn't know why, but wise guys from M.I.T or Harvard pointed out that there's what's called hydrogen bonding. The hydroxels will bond together for a short time and then they'll separate for a short time and do this and, depending upon what the
temperatures are, it will either be coupled together for a longer or shorter length of time.

The result of that was that we would see a single peak due to the hydrogen bonding, but it was the average; it just happened in a time scale which is short compared to the lifetime of the spin system. Then it averages to something in between. So instead of getting two lines, which you do if it were completely fixed and static, you get one line, because it's the average of how long it's spent here and how long it's spent here.

Now, at the time I was at Stanford, which was '51, we knew this, and Bloch recognized it and he was helpful in seeing this, because it was a lot of what he did to death. And then, we spent quite a bit of effort making more homogeneous magnetic fields without doing this—it was just a question of group force. It was this that then allowed us to do the three spin alcohol.

So, I can see that it would be very, very simple and reasonable for Felix, in stirring the tea, to see that you could move the molecules around, and if it were spun a certain length of time in this field and a certain length of time in this field, it would simply average to the average value of all the fields. And that's the kind of a great idea that, having put together these ingredients, you excited it to somebody else, because, "Of course, gee whiz, I wish I'd thought of that." I mean, it's so obvious that you lose sight of the brilliance of having taken the need to make homogeneous
fields and the fact that it would average and how to do it.

Then Jim and Wes were apparently instrumental in actually physically doing it. That would be beyond Bloch's abilities.

Lowood: I see. You mentioned that he did draw the patent on it. Who would have helped? Would that have been something that ________?

Packard: We probably would have. Well, by that time he was kind of mad at us, and he had an attorney called Paul Spier[?]. Paul might have done it. We might have done it, except till the ________.

Lowood: And then there was some licensing and an agreement worked out with the company.

Packard: Yeah, yeah, because we had the license on the other part of the keys[?]. Our agreement may have even said that he had to license this.

Lowood: Was there anything more you wanted to say on that one, or shall we push forward into the other developments?

Packard: Let's go ahead.

Lowood: Okay. I had a few things here—just to mention—if you think—I'll let you be the judge of how much there is to say on some of these.

Packard: Okay.

Lowood: But first I want to ask you about—this is something—well, maybe I should try to be a little more chronological and ask you about the medical accelerator, the clinical accelerator, and the development of that line, beginning, I suppose, in the late fifties, after the
collaboration between Kaplan and Ginzton at the microwave lab. How did that effort get started at Varian, and to what extent were you involved in it, as well?

Packard: Okay, I'll tell you what I know. There's no question in my mind but what the activity was here because of Ed. I think there's no question about that. It turned out that, as director of research in the instrument division, the responsibility was given to me; but like Bob Jepson and I before, I had very little to do with it in any real sense. They hired Craig Nunan and somebody else to do that. I didn't even hire them on; I'm not quite sure who did.

But Craig reported to me initially, and so it was my job to work with him. I can remember we talked about building this building over here and making a pit, too—whether we ought to build it. We had our separate building, and, to some extent, I probably sold the first research-type accelerator to DuPont; at least I had a hand in it. But as far as the engineering and the production of it, really, I had nothing to do with it. To a large extent, I was transparent to that activity. I suppose I signed his time cards or something for a while, but—

Lowood: I see. When you said "research-type accelerator," was it the pre-clinic-type accelerator, just the—

Packard: Yeah. We built—actually Lou Malder had a hand in that; I've forgotten how the heck he got involved. In the early days, there was no cancer-type market for it, and so, what did you do? Well, there were needs for accelerators by research people. Piscotti[?] was one in Italy. The Hamburg[?] machine
was another one that I recognize, and I think we must have built these accelerators for various places.

All of these, being special, ended up being money losers, and so there was a lot of criticism from the company, but, again, I think I can say unequivocally that it was Ed's insistence and forebearance and belief in it that kept it alive; otherwise it just would have disappeared.

And then, I don't think I really know who conceived or who implemented the first Varian transfer machine. But then, from that time, it became more commercial, and we dropped the—simply would not take any more of the research items, and gradually did a good job of building a market and awareness of the machine, and built up a responsible business.

Lowood: Did that develop as a separate division eventually?
Packard: Yes, that was a separate division and a separate group. For a while, Craig Nunan actually ran that, but then somebody was brought in to handle it. He really wasn't very at all.

Lowood: Was there anything else that you wanted to say on that? My impression is that probably we should go on to some of the other--
Packard: Yeah, I think--
Lowood: You mentioned the VacIon line a bit. You started talking about it a little bit. How did that one start up? Did that also, as you explained, develop in the instrument division?
Packard: No, that one started in the tube division. These guys
worked for the tube department; it was the joint manufacturing which we had.

Lowood: Oh, excuse me, right, of course. That's precisely what you just explained (laughs) about an hour ago. Has that developed into a separate division? And that's called what?—vacuum products, or something like that.

Packard: Yeah. And that developed, again, in several ways. It started as being a division that put together, probably first of all, systems which had a high vacuum in it, and then they began to develop components, and ________ called a conflat[?] flange, or something like that. So they had a line of bits and pieces which you would buy to put together your own vacuum system, and then have vacuum gauge. There was a vacuum gauge invented at Westinghouse by Alperton[?] Bayard[?], in which he murdered[?] the structure and then a large and measurable vacuum. John Helmer[?] did something important in the vacuum gauge business.

So our immediate, our first emphasis was on these very, very high vacuum systems with parts that people could build. Then we began to move in the direction of supplying systems for specific—and the first ________ specific thing had to do with coating. And we set up and moved in that direction. We built specialized equipment for coating perfume bottles. We built a tremendous big line for coating bumpers for General Motors, a big affair which was kind of a disaster. They bought one, and we lost money, and they didn't use it. So we tried to set up a commercial business of doing the coating for people,
and that didn't work.

We've never been very successful in that sort of thing. We had a microwave heating activity at one time. We thought: take these klystrons and components and do something with them in a commercial sense. That didn't go anywhere. So we've never been terribly successful at ________.

Lowood: I might as well ask the obvious question: At this point the microwave oven technology was not developed?

Packard: That we were interested in. Litton at first demonstrated the possibility there. I remember, we did talk about it; we did talk about magnetron[?] and thing[?] with a good deal of skepticism with it. But we simply weren't good enough in manufacturing-oriented design to produce the very low cost, very high reliability tubes. I think the Japanese who did it pretty much dominating that work on the basis of costs, and also we never did anything there.

Lowood: Let me ask you, then, about another line, which I assume did come out of the instrument division—correct me if I'm wrong—and that would be digital technology?

Packard: Yeah. It was—well, I guess that's not important[?]. I became convinced fairly early—probably too early—that the digital world was going to stay with us. I think probably it was first Bob Jepson and I [who] sort of beefed up the—I guess it was probably a central research group—I suspect that was—I don't know whether he was director of research or not—but anyhow, it brought in more people in applied mathematics and computing-type people and encouraged the development of
concepts. We had a course in very, very early times on
porchment[?], back before the term was known by very many
people. We established a relationship with what at time was
IBM down here. I tried to encourage our scientific people to
become more computer oriented by getting a punch card punch,
which we sat down in the hall in the research area as visible
reminder, and you don't leave the access to cutting up punch
cards. This was partly successful, because it got us into the
fourier transform, where you used the big computer for fourier
transform.

Then there was a group that approached us from one of the
chip manufacturers.

Lowood: Here in Silicon Valley, or—?

Packard: This was Fairchild. This was a small group that had been
hired and put together by Fairchild people to do more with the
digital end. That was before we had integrated circuits per
se, and these were discrete components. And they decided not
to do it, and that I was kind of taken by it, so I took on the
whole group, and—I was kidding with Jim Monohan out there—Jim
was control and our comptroller at the time. One of the things
I tried to do was to do return on investments and that sort of
thing—I guess I'd gone over to Stanford and it seemed like
kind of a nice idea. So I remember Jim asked me the question,
"Well, how can you ever justify this? How can you do a return
on investment if you don't know what this group is going to
do?" (laughs.) And I had to allow as how he was right, but we
hired them anyhow.
So they set it up, I suppose initially, this portion of it, and then set it off as a separate group. The result of that was that we began to think in terms of bringing digital circuitry, and one of the early NMR machines had digital circuitry. Part of my argument and feeling was that we had more access to digital circuits and semiconductors than the Japanese had, and therefore it would give us an advantage.

Lowood: When you say early, this was still in the sixties, though, wasn't it?

Packard: Yeah. And about that same time, for reasons which were—I don't know—independent; they were independent in the sense that in the two parts of the company—Emmet Cameron maybe was in charge of development or something—came across this computer company down south called Data Machines, actually run by an entrepreneur, and these guys had a sixteen-bit computer. It was rather a nice architecture, and it was a good device, and it was very competitive with _______ at that time had only an eight-bit computer, and we had a sixteen-bit.

So, Emmet bought the company, which was fine. I thought that was a good move, because I could see that strengthened what we were doing—applying it—and what they were doing—building the computers.

And neither one of these operations per se were successful. The group that I hired was run by a fellow by the name of Rabin, Dick Rabin. It did bring the technical knowledge into our products, tried to develop systems uses for the computers, and at that time we got the 6201s, which were
made by Data Machines. But at that time they had core memories in them—if you had 64k, why, that was a big memory. And so we were trying to do something which was really not practical and build, for example, a data system for the clinical laboratories and hospitals. But it took different kinds of hardware, and by the time you write everything in machine language and you try to cram it in, it really couldn't be done, as we soon found.

Lowood: So the Varian mini-computer line was based on these data machines, which—

Packard: Initially, Rabin and his guys were building discrete kinds of things, but as soon as we got the data machines, then they went to the computer and incorporated that into the _______.

Lowood: Okay, so that was the sequence.

Packard: It did do one thing that I always felt good about. When John Hammer[?] built up an instrument called ESCA—this was based on a Swedish idea—and he built this entirely around a 620L; it was entirely computer controlled, computer operated, computer driven. To my knowledge, that was the first total instrument which was a computer-based machine. And it happened to be the right size, and that fitted well. The Russians liked the equipment. I saw some fifteen or twenty years after we built it, Chel[?] Development is still using it, and it's still working fine.

Lowood: Eventually, I guess, the Varian line computer division, or whatever it was, was sold to Sperry, right?

Packard: Yeah. Both of these things were among an unfortunate management activity. The Varian data machine had this very
difficult guy who was the entrepreneur of it, and we didn't move quickly enough to move him out and bring some management in. As a matter of fact, the guy who ran it was Ted Marino, who was a real nice guy but didn't know anything about computers. A very bright guy but—Ted's okay but he did not put the computer cult at all, and he didn't know how to operate this kind of a company. So gradually we got rid of the entrepreneur and brought in another guy who didn't function and began to lose market share and get even with Damar[?], and other people began to do more, so we simply did not manage that successfully.

We've had far more hands with these three activities and ability to become an important—we were way ahead of H-P, but H-P brought in the right management, invested money and built it up. And we were ahead—

END OF TAPE II, SIDE A

BEGINNING OF TAPE II, SIDE B

Lowood: Your opinions—that's the—
Packard: That's what it's all about.
Lowood: (laughs.) That's what we want.
Packard: So if you find that my opinion doesn't agree with other people, why, okay.
Lowood: You were just saying that you'd had this opportunity by being ahead, and that H-P, through what was essentially a different strategy, really—
Packard: They had government and a different strategy, just
gradually hung in and now they're important.

We had a small window in Europe and, after I had left the division, one of the jobs I was asked to do was to see if we could find some products to build in the Donnybristle activity. The Donnybristle had started sort of thumb-knee in a not-a-very-well-thought-out way, and it was kind of a flash in the pan.

We had problems in Europe because of the government restrictions on importing American spectrometers, and they had to have a certain amount of UK content or EEC content or something. And so I figured, well, the way to solve that is we'll set up and manufacture kit in England. We ended up, sent a guy over who was pretty good with Ellis NMR, Ernie West, and he decided to set this up in Scotland, which was a good place to do it.

For various reasons, the volume didn't build up, and one reason that I felt—this was, again, to have been a joint operation between vacuum products and us. [When] I started, this was expected, vacuum products. Well, vacuum products chose not to stay with us, so here we were in an activity which was to be supported entirely by the instrument group, and that really wasn't that big.

And you learn something which is still true but I didn't recognize at the time, and that was that you take kits of equipment and send them off to someplace else to be put together, send ten or twenty kits of parts off someplace else, and that may take them three months or six months to build.
Now what's happened is that the production, which is back home, has got lots of changes going on. This would happen all the time—components don't work, something didn't happen, so you have engineering changes which occur in there. Now, how do you transfer that to this off-site operation? And the fact is that nobody has ever figured out how to do that. You either have to abandon a bunch of parts you have over there, or you have to somehow or other get it in. So it turned out to be a very, very difficult task, and that didn't help at all.

So, anyhow, after I left the division, one of my jobs was to see if I could find out, get something in that operation in Donnybristle to . . . . Well, I went to England for a while and drove around the country and talked with people in Edinburgh. At that time they had a good artificial intelligence—it's now called artificial intelligence, actually it was a strong computer activity under a guy by the name of Michie, and--

Lowood: Donald Michie.

Packard: Yeah, uh huh, you recognize his name.

Lowood: I would never have come upon any connection with Varian by myself, but I certainly know who he is. (Laughs.)

Packard: (Laughs.) Yeah, that's part of the—yeah. And there were some features of our machine which they liked. We had micro-hardwire—we didn't call it that, it was something else—that allowed you to get into it, and the 620L was a pretty good machine. So I was looking around. I felt that we had an opportunity and that would be to increase our computer
activity there, particularly because, in my view, we had this 16-bit window compared to Dedeck[?], which had a fairly short window.

We actually did start to go into production on that, and this guy, Ernie West, was sent down to Data Machines and discovered, much to our horror, that they really didn’t know how to build the machines down there, let alone transfer them to a remote site. And although we got some separate parts from them, it never really went any place.

So about that time, Norm Parker, in his great wisdom, decided to shut down the Donnybristle. We shut Donnybristle, and then we opened up another one, and then we opened up a couple in Ireland; we just now shut down the ones in Ireland. So this, again, I bring back to point out this lack of being a company, or it ought to be a company, and having an entrepreneurship, which is necessary in the divisions.

But after then, the divisions would not work with the Donnybristle thing, the set up their own in Ireland, and then you had two fiascos on your hand. We've had a lot of those, an awful lot.

Okay. That's my view. I've forgotten how we got—

Lowood: (Laughs.) Well, let me, then, ask about another initiative that came from Varian, which was the founding of Syva, this joint effort with Syntex, I guess originally based on—

Packard: Bill Little.

Lowood: —on Bill Little's work with organic superconductors. How
did that come together and what happened with it?

Packard: Well, that's both a success and a failure. It came about because—really the focal point was that Bill Little knew us at Varian and came with this concept which he had. We were fairly high-risk oriented in those days. And I made the argument to the board of directors that the success of this was probably a fairly low probability, but if it succeeded, the rewards would be very great, because if it really worked, it would just revolution [you known?] the whole world; and that the expectation value was reasonable. And there's a product between what probability, but the pay-off—well, the expectation value is reasonable.

Now then, having made that tremendous analysis, what do you do with it? You can't spend an infinite amount of money on it, or a large amount of money. So how much money do you spend? Well, you use the same criteria which you use in Reno if you're a rational person, which is you will spend what you can afford to lose. And so, the board did agree to do that.

And so we set up a small operation here, and we and we had some chemists. But it didn't take very long for me to realize that I didn't know enough chemistry to evaluate what they were doing. Nobody at Varian knew enough chemistry to know whether they were doing anything useful or not.

Lowood: So far everything you've described is purely Varian.

Packard: Purely Varian. So, it wasn't very many months before—maybe the idea came from Elliot Levinthal and maybe it
came from somebody else, but the suggestion was to talk to Syntex about a joint venture, because they had the chemists and they had Djerassi, who was a very competent guy, and Zaffaroni, who's a dreamer; and we had the physics end of it, so if it ever got to a point in physics, then maybe we could do something in our end of it.

So we did put together the system, the joint venture, and that was very carefully crafted by all kinds of lawyers, both from our place and back East. Some of the lawyers felt that we'd never get superconductivity, but bring together a bunch of competent people and something would be bound to happen—that was on the Syntex side of it. So we did put it together, and a board of governors was Ed Ginzton and me. This was at the time I was no longer the division manager; I was reporting to Ed.

Lowood: This was when you became assistant to Ed Ginzton? I'm not quite—corporate—

Packard: Well, it probably was earlier than that. I became that—I was a division vice-president. And then when I left the division, they sort of automatically made me corporate vice-president. I worked with Ed as assistant to him, and maybe we'll go back and talk some more about that.

Lowood: Okay.

Packard: So, Ed was interested—and I had forgotten, but recently, when it turned out people can make higher temperature superconductors, we looked a bit at history and, actually Hardin McConnell had been working for us for a summer when he was down at Cal Tech on some very interesting ideas which he
had on superconductivity. And we included Hardin, not because of the superconductivity, but because he had this other idea, which was the spin label, which were free radicals useful at EPR, and it turned out to be an extremely useful aspect of the business. So we threw those two ideas in the pot.

We ended up with a modest amount of money, I think a hundred, a hundred and fifty thousand dollars a year, initially. That was enough from both sides to make things go. We hired—and this was Djerassi—hired Ted Ullman, who was a very bright guy, out of Harvard, fairly hard to work with but extremely competent. That in turn brought in other people that were good. And they really did work hard and diligently on the concept, which, in my naivety, I thought if you could draw it on the blackboard, you ought to be able to build it (laughs) in a chemical sense. But it turned out to be extremely difficult and probably still beyond the state of the art.

So we were wondering what to do, and we had a conference up at this chalet that Ed Ginzton and Myrl Stearns and I and Dorothy Varian and a couple of other people had up in Alpine Meadows. We had a conference there, and one of our consultants was Avram Goldstein. Avram—again, a very wise, very effective individual—Avram said, "Why don't you measure small molecules with antibodies"—I'm not sure whether he suggested the EPR at the moment, but I think that came very quickly—"and in particular, why don't you measure drugs that are abused. Measure heroin. I think there's an important need for that."

So we did, in fact, come up with a system which was called
FRAT, which was based on EPR and which was out of Varian, and
spin labelled antibodies which we really didn't know too much
about but very quickly built up enough expertise there so the
chemists were able to conjugate heroin to the quotence and
check them in rabbits and goats and grow antibodies and use
rats as a technique for measuring the drugs.

The timing was just exactly right, because they were hard
pressed in Viet Nam. So we put together three or four
equipments or half a dozen, utilizing the EPR machine and
utilizing the chemistry from the Syva people, and tested the
drugs, and—

Lowood: It was FRAT, like the fraternity.

Packard: Free radical assay technique.

Lowood: Aha, okay.

Packard: This was never very satisfactory to the chemists; the
instrumentation was too complex and too expensive. So Ted
Ullman and those guys came up with what's called EMIT, enzyme
multiplied assay technique, and began to drill and built
more drugs and more tests and went to drugs in abuse and then
on to hormones like T-3, T-4, on to therapeutic drugs. Again,
Avram wisely pointed out that there are many therapeutic drugs
that have a fairly small window: You get too much, it's toxic;
if you don't get enough, it doesn't work. So we worked
initially on anti-epileptic drugs. Then this guy from
Columbia, who was kind of a leader there, again, following the
earlier—we had good rapport with chemistry, and technical
people who had enough competence inside to be able to
communicate with them.

Then this began to be successful and get up to eighteen and twenty million dollars worth of business. That began to attract Norm Parker's mind, and he said, "Well, we don't understand that business; let's get rid of it." And my argument, which didn't prevail—and Ed Ginzton's didn't prevail—was, "Well, maybe you don't, but some of the rest of us do, and if you're trying to build a diversity, you're trying to build activity in the instrument and the chemical, this is a good way to do it." So either we continue to run it as a joint venture, which we could have done, except that ambiguity is contrary—it's what I mentioned earlier—and that was something that was completely out of question as far as Norm and those people were concerned.

So it was decided that we should sell this to Syntex, and so we went through a series of negotiations. This kind of tickled me, because the lawyers worked hard to decide what would you do if the thing failed, but they never really came to grips with what do you do if it's successful.

Actually, in the meantime, we tried to go public with it, but that was the big crash of '73, and nothing happened there. So there're lots of little problems that went into the joint venture. But anyhow, we did sell to Syntex, which gave us enough money to make our part whole, and then they paid us royalty, which was a nice piece of change over the years. During some of the very low spots, we'd get four or five million dollars a year, and in some cases, that was a sizeable
chunk of our profits from that.

So it was very successful in that sense, and it's now a hundred and thirty million dollar business. It broke to a hundred million, and then that dragged because they and we didn't do something right. Again, partially Ted Ullman, who was a very strong individual, and he focused a little bit on the wrong thing. They did not build their product for digitoxin, and they should have.

Lowood: That they should have was the problem, or they didn't build one at all?

Packard: The product never worked.

Lowood: Okay.

Packard: We were partially responsible, because we were going to build a machine for them, which was going to do everything, but we never could really get technical—that was a fiasco.

So, what was good about it was, it was a rather remarkable exercise in a joint venture between people of good faith. The company was started in one direction, ended up in another, was successful and was bought. The unsuccessful thing was that I wish we could have bought it, for two reasons. These were, to my mind, extremely ______, as it began to be clear by that time to Ed Ginzton and to me, as he and I had talked quite a bit about increasing our biological content in the company, because we could see that's where instrumentation was moving. We had some of that in the instrument division, because we had people like Larry Spivak[?], who were oriented in that direction, and later a guy by the name of Steve Smalcomb[?]
involved. But we were trying to do more in that area. And this, to my mind, was an extremely important way to bring all this biologically oriented expertise in this company, so that we could build on that. At least I thought that's the way that instrumentation could.

So I was quite unhappy that we didn't do it, and Norm—well, it's one of these cases that I feel he made the right decision to sell it, but all for the wrong reasons, and he should have gone the other way and made some _________.

The end result of that is that Varian has not been able to move into the biological area. We're not in that area at all. And so we've simply not progressed in that direction, and I think that we should have.

Lowood: I check[?]. When you said that there was this one reaction, that this was not an area that Varian was very familiar with, I would have thought that, if the company was setting out to diversify—and, indeed, that means picking up unfamiliar areas—that you would have been well situated with the experience in NMR and chemical analysis and all that sort of thing, plus the interest in medical instrument—you know, medical electronics—

Packard: That's right. We were moving in the medical area, because at that time we had the linear accelerator, but then we were even trying to move in the medical area. We set up a sales organization—which didn't work—and it was to peddle products from other people as we built up our own lines.

So, no, to my mind, it was a natural activity. But it was
too foreign for the management; and it may be correct and it may be that they could not have handled it. I think it would have required that we would have split the company apart at that time, and I think it would have been better to have spun off—keep the ______-oriented and spun off the other part. Maybe the managers, they could understand it.

Lowood: I see. In your resume, it lists Syva—Syva's listed on there. Were you actually the first that was in charge of the operation of the company during those—I listed '75 to '77?

Packard: Well, we worked together, and, as I said, on the board of directors it was Ed and I on this side; it was, from their side, Djerassi, who played a very dominant role. That's only initially; then he went off to Alza. He was replaced, I think first of all, by John Fried, who was the important R&D director, and he'd done a very good job; and then by John Sedgwick[]. In order to keep people in the companies happy, we brought in some accounting-type guys from our side and from their side.

Lowood: And it was run that way, really, as a group—

Packard: It was run that way. Initially, when it was all research, it was done—Ted Ullman really did the day-by-day activities, and the board of directors didn't have too much to do with it. But then as we began to become more product oriented, then we set up, actually, a joint co-manager between John Fried and myself. And so we would meet as a staff and we would have—it worked, it was sort of fun, because—but it worked, and actually it was a good training ground for John Fried. But we
did the kinds of things, during that stage, of bringing in the manufacturing manager, setting up a .... Ted Ullman, his first reaction was, "Well, we don't need manufacturing. I can build a lot."

So we did that, and we built up a sales force and all, during that period. Then we brought in Rubenstein, who took over as the general manager, and then we stepped back from it. So I like to think I had some share of a nurture role in it. Ed was important, and Ed's role—most important role—was to protect the activity, because in any big company you always have people who—well, an embryo needs some protection, and Ed was able to do that—same way he did it with the linear accelerator.

Lowood: Now in these years, Ed's position was chairman of the board and Parker was the—

Packard: Well, Parker probably became CEO, I guess.

Lowood: Okay, so he was CEO and Ed was—

Packard: Ed was chairman of the board. For a while Ed was vice-president and CEO and chairman of the board ________.

Lowood: That's right. For a short period of time, yeah. Okay, I just realized—

Packard: Did you run out of tape?

Lowood: I'd say we—

Packard: You've done a good job of pulling together areas that—

Lowood: Okay. So, what I wanted to ask you about now was the development of the A60 NMR spectrometer line, which I guess was introduced in the early sixties. And as I understand
it—correct this if I'm wrong—this was really the first high-volume spectrometer that Varian put out.

Packard: Yeah.

Lowood: How did that come about and what kinds of changes did that introduce in the way that the business was done?

Packard: Our early machines were really very specialized, and as you've watched them grow, we would add a box here and a box here and, I don't know whether you've any pictures, but it's ___ looking ___ machinery. We used cabinets which were supplied by somebody, and we just kept adding stuff. Of course, those were in the days of vacuum tubes that took a lot of space in order to do any of these things.

So I suppose, I think I have to guess, that probably Emory, Jim Shoolery, played an extremely important role in the marketing end of recognizing that what we needed was an instrument which would have wider utility in both the academic and industrial markets, and a simpler machine, not a research-oriented machine, but a machine which would do analyses. A lot of our early machines went to people who were doing NMR research, for the sake of the NMR activity, not for the sake of the analytical activity of measuring products.

So this was an effort to bring to the marketplace an instrument which would have much wider utility and was to be of much lower cost, a machine easier to operate, all these things were ___________.

Lowood: These took a general chemistry kind of work, then?

Packard: Yeah. People who played an important role in the
technology of it—probably most important was, of course, Nelson[?]. He was one of our very quiet engineering-type guys, not a product engineer but a research engineer. So he could put together systems and try them out. I suppose by that time, we hired somebody who did the magnet[?](?) structure of the __________ box. Bob Gang[?] maybe did that, the magnet. ______ may not have, probably did not.

Well, we had a guy, we hired a guy, sort of industrial design, and he probably did the industrial design. We were beginning to have all these good things: product engineering, industrial engineering—

Lowood: So you began to worry about other kinds of things that would have tried customer's insurance of _______ device and all of that.

Packard: Yeah, that's correct. __________—oh, for a while, it was very popular to complain that the reason for Varian's lack of success was that it was just technology oriented and did technology for technology's sake, and that's something which, I think, Tom ______ and other people propagated. It was a good thing for the new president of the company to find some reason ______.

It wasn't really true. You had to work hard with the technology in order to make it do what the customer wanted. It wasn't that we did it just for the sake of the _______ people up in the tube division.

And we guy by the name of Bob Davis. Ed Ginzton—this is something we should talk about, because we spent a tremendous
amount of effort trying to educate people and get fortunate[?] on this and ________ on that, and brought in Bob Davis from Stanford, who spent sort of a sabbatical working here. Bob's a real nice guy, but one of his main messages was, you got to go out into the marketplace first and then come back and decide what technology can you use to serve that market. And that was very good; he was good at marketing coffee and stuff like that. But he, for example, never did accept or understand that there're two ways of doing it: One is, if you have a unique technology, that you must find a market and develop a market, and that's what we did. But we worked hard at that, and we were aware of the market and ________ and all that dumb and all that naive.

Lowood: So Davis was selling from business school or something like that.

Packard: Yeah, and he still is. He's a real nice guy; I love him, but—except I was a bad salesman, I could never convince him of (laughs) this side of the argument.

So that was the A60 and that was the first, and then we had the T60 and all, and we became less concerned about the special defect[?] of the supercon[?]. And then, as general manager, I adopted a philosophy of having locked up—I think I called them X-suits[?], which were experimental products—so the customer knew that this was a product which we would build, and we might build one of them and we might build two of them and we might build five of them, 'cause we might stop supporting it after a certain length of time, as contrasted to
the other products, which we expected to stay in business.

Lowood: And there was probably, along with this other, this effort to produce something that was of more general use mostly for chemists, I suppose, there must have been dramatic changes in the pricing of the—the A60 ____ must have been a lot ch—

Packard: Yeah, we began—the original initial[?] on the part of Emory and Jim Shoolery was that we really wanted to keep the price down. It shouldn't be higher than the red[?] technology[?]. So in that sense, we weren't terribly market oriented. We tried over the years to get more of the concept of what would work for the customer, but not too successfully. We always had a problem with low margins and the cost[?]
mark[?].

Lowood: And you weren't in such a highly competitive field that the _____ --

Packard: Well, this time we were more competitive than--

Lowood: It was, ah.

Packard: Yeah. Groupder[?] was—they caused us trouble. We made some management decisions which weren't correct; they were well thought out. One of them—we did not go into fourier transform and that in the carbon-13 machine as early as Groupder[?] did and--

END OF TAPE II, SIDE B

END OF INTERVIEW
Third interview with Martin Packard, Varian Associates, Thursday, 27 October 1988

1. IMPROVEMENT OF NMR AT VARIAN
   a) Let's talk about the position for which you were hired by Varian. Where were you located in the company, and to whom did you report, when you arrived in 1953? Did you work exclusively in the Stanford R&D facility?

   b) Besides the ones we have mentioned, which projects were assigned to you in 1954? How did this set of problems evolve during your first few years with the company?

2. What was the "VARIAN Applications Lab"?
   a) According to your resume, you achieved the title of "Director of Research" in 1955. (Incidentally, another source says "Director of Instrument Research". Is the difference important?) That is a rapid climb. How did it happen?
   b) Did the position as Director of Research put you in charge of all research efforts at the Industrial Park site?

3. Describe the transition for your own work. Was somebody else in charge of development? Which people within the organization did you interact most often? (What kind of roles did the Varians and Ed Ginzton play during the early and mid fifties? Was Varian, in particular, a "hands-on manager"?

4. Did the spinning sample, from the original idea through R&D in progress, work rapidly for NMR in this period? Varian brought out its first high-resolution NMR spectrometer in 1952: the spinning sample was introduced in 1954, the flux stabilizer and electron paramagnetic resonance (EPR) in 1955. Can you describe your role (and that of your group) in these major product and improvement efforts? Can you take one of them, the spinning sample, from the original idea through R&D in detail? Are there other developments, anecdotes, or contributions you would like to mention here as part of the ongoing improvement of NMR during the 1950s?

5. Returning to a theme we have touched on earlier: could you summarize and perhaps give examples of the role VARIAN's customers played in the improvement of VARIAN's NMR line during this period?

6. Can you talk about some of the early marketing ideas that were closely related to R&D, such as the "NMR Work Series" or the "NMR-EPR Workshops"? How did ideas like this come up? What seems to you to have been distinct about VARIAN's programs in this area? What was the NEWS-NOTE BULLETIN of the Instrument Division and does it fall into this category? Were there other ideas and programs that you would like to mention in this area?

7. How was NMR (research, development, marketing) distinct from VARIAN's other "bread and butter" lines, above all microwave devices? How were the markets for NMR and for klystrons different? Both of these directions, presumably, had proven themselves and were firmly established by 1955; is that correct? Which
Bergy -
pls. call
Henry

Henry can't do
listens and before he
leaves, suggests
someone else do it -
Rocks. July?

Packard
Interview
(3rd)
10/27/88
Given
John
2/22/89

Report for Harry
needs listeners. Henry
and edit.
Greg

June has Oct. 88 (2)
Packard tapes at
Henry to make back up
copies 1/21/89

McPhee
new areas seemed most promising around that time: accelerators? geophysical instruments? special areas of electronics? Were there any failures during this period, that is, whole areas that did not pan out.

2. TRENDS AND PROGRAMS AT VARIAN AFTER THE MID-1950S.

Here are some fairly broad subjects within Varian's development. In each case I would like you to comment on your own role and insights in the projects or products, particularly the ways in which they exemplify Varian's particular approach with respect to research, development, marketing, or management. If you could give a few of the key names or events you remember from each, that would be very helpful:

\* Syva, 1965-77. (Varian's joint effort with Syntax, starting with Bill Little's work on organic superconductors and then diagnostic instrumentation)

The clinical accelerator (up to CLINAC-60).
The Vacion line ------- (internal)
The A-6B NMR spectrometer line, introduced in 1961.
Digital technology and minicomputers

3. A CAREER AT VARIAN

a) Progression through the company

1) We left you as Director of Research during the mid-1950s.

In 1953, you became General Manager of the Instrument Division and in 1965 Divisional VP. Describe this transition in company and personal terms; that is, what changes in responsibility were involved?

2) While you headed Syva, you became Corporate VP and after 1976 you were appointed Assistant to Ed Ginzton, the Board Chairman. Again, which of these transitions represented breaks in your own pattern of work or significant changes of responsibility?

a) While working in these positions, were you involved in Ginzton's committee work at the national level, such as the DOSENS committee?

b) Is it fair to say that during this stage you moved from direct management to developing long-range corporate strategy? Can you talk a little about this kind of activity and what it means for a company like Varian? How does it function within the company, say, in changing directions of research or product development? Can you give examples?

- Another key issue: central R & D versus decentralized laboratories. How has Varian's approach evolved over the years? Have there been discussions and changes over the years, such that significant issues have developed? What is your opinion?

- Describe Varian's program of working with Soviet and Chinese technology-based companies. Did this start while you were Corporate VP, or earlier?

b) How would you describe the "corporate culture" at Varian. Describe the community of employees during the early years of the company and compare it to recent years.

1) What would you say were the key changes that occurred during
Martin Packard-Henry Lowood Interview, November 22, 1988

Q: Let's begin by returning to some of the marketing aspects regarding NMR and you were going to talk about the visiting professorships, is that what they were called?

A: Yes, that's what they were called. Actually, in developing the market, I think we did a number of things which were interesting. I'm sure we talked about the seminars. We also had post-doctorate type people, many of them who stayed on with Varian and others who have come back to academia and established themselves. In addition to that, during the summer we actually had a number of visiting professorships where we would have key individuals from the universities come and work with us on various kinds of projects. At that time, we did quite a bit of rather fundamental research; in other words, it was not directed specifically to a product. It was expected that it would be publishable and part of our strategy to have something to contribute to the research world so that we could expect something in return for it. It seemed to be a strategy that worked because competent people in academia and commercial companies did feel free to exchange information.

Q: Can you name some of the people who came?

A: Strangely enough I can't. I'd have to think some more about it. It would be a good thing to try to recall. I could check with Jim Schoolery and with Larry Biet(?).

Q: Were there any from Stanford or would that have been unnecessary?

A: There might even have been some from Stanford. I'm not sure how many we had, but it was an important part of it because these people would go back to the university and be friendly to Varian. We also did a program in another vein which was kind of interesting. It wasn't so much NMR, more in the magnetometer line. We, at that time, had the Naval Postgraduate School in Monterey on a program of farming their advanced students to come and work for a period of time with companies. So we had, over a period of several years, quite a number of these kids who came up and would spend a month or 2 months. We could usually identify a project for them. For example, one of the first to practice frequency standards was done with the help of a graduate student.

Q: Who was the coordinator? The Navy?

A: Yes, it was done through the Navy, but I can't remember the professor's name. You might make a note and see if I can't recall or find it later. That lasted for a couple of years and they were good projects. They did all kinds of things which we would not have done on our own. For example, this one project was demonstrate the gyroscope, because the UPI(?) would recess(?) at a constant rate while the coil was turning with respect to that frequency. So the kids built a little gyroscope.
Q: Let's begin by returning to some questions regarding NMR and you were going to talk about the market. Is that what they were called?

A: Yes, that's what they were called. I think we did a number of visiting professorships, and I'm sure we talked about the sort of people, many of them who have come back to academic life, who have come back to academia.

In addition to that, during the period of visiting professorships when we did some fundamental research; in other words, it was not directed specifically to a product. It was expected that it would be publishable and part of our strategy to have something to contribute to the research world so that we could expect something in return for it. It seemed to be a strategy that worked because competent people in academia and commercial companies did feel free to exchange information.

Q: Can you name some of the people who came?

A: Strangely enough I can't. I'd have to think some more about it. It would be a good thing to try to recall. I could check with Jim Schoolery and with Larry Biet(?).

Q: Were there any from Stanford or would that have been unnecessary?

A: There might even have been some from Stanford. I'm not sure how many we had, but it was an important part of it because these people would go back to the university and be friendly to Varian. We also did a program in another vein which was kind of interesting. It wasn't so much NMR, more in the magnetometer line. We, at that time, had the Naval Postgraduate School in Monterey on a program of farming their advanced students to come and work for a period of time with companies. So we had, over a period of several years, quite a number of these kids who came up and would spend a month or 2 months. We could usually identify a project for them. For example, one of the first to practice frequency standards was done with the help of a graduate student.

Q: Who was the coordinator? The Navy?

A: Yes, it was done through the Navy, but I can't remember the professor's name. You might make a note and see if I can't recall or find it later. That lasted for a couple of years and they were good projects. They did all kinds of things which we would not have done on our own. For example, this one project was demonstrate the gyroscope, because the UPI(?) would recess(?) at a constant rate while the coil was turning with respect to that frequency. So the kids built a little gyroscope.
Q: This would have been what, the mid to late 50s?
A: Yes, in that period of time. That was a period when we were reaching out to get our technologies known.

Q: So all these programs were part of this effort to build a community of users around some of the Varian devices?
A: Yes. It was successful. I'm not sure if I mentioned it or not, but there was what developed after, or in conjunction with, the Pittsburgh Conference, a group called ENC. I'm not sure what it stands for, but these were people who were users and they would meet. For quite a long time it was sort of our club or our group. The conference still goes on. It is held annually in Monterey or on the East Coast. It is an important NMR conference, so to some extent this meeting originated, again, from the interaction of the community.

Q: I'd like to move forward a little bit with your own progression with the company. I guess the last time we talked about your specific position you were director of research for the instrument division?
A: Yes, I think we touched on that, but not for the whole company. I think, initially, that I probably reported to Bob Jeppeson who was director of research for the whole company, but that was a transparent relationship.

Q: Right, and in 1963 you moved to general manager of the division and then in 1965 to divisional vice president. Were those actual changes?
A: No actual changes. I was just recognized and made vice president which, of course, gave me authority to sign for the company.

Q: Did that reflect any kind of reorganization? A lot of companies go back and forth in this kind of organizing everything along divisional lines and then organizing everything along functional lines and that sort of thing. Was Varian a company that tended to reorganize a lot?
A: That has been the history, but it doesn't work. We built, about that time, we began to acquire the other companies of Aerograph and MAI and AA company (Techtron). I think we touched on that last time. At that time, then, probably Emmory[?] would have been initially sort of group VP and I guess I was under him as a divisional VP. Then Emmory left and went to HP and Ed, for a while, wore several hats around the group. That could have been around that time. It was part of Ed Ginzton's strategy to build us into a wider ranging, multiproduct company. At that time it was recognized that no way could we develop it within our NMR business. There was plenty to keep us busy there and so that position was a good approach.

Q: Before I get to some of the other more general issues, I just wanted to wrap up a few things about you career here at Varian.

P-2
We've already talked about the period when you were involved with Siva(?) and I noticed that in 1975 your title changed to assistant to Ed Ginzton, who, at that time, was Chairman of the Board. What did that represent?

A: At that time, we hired a man to be the group president, I guess, sort of replacing Ed. This was a fellow named Equin(?) and about that same time, Norm Parker came in. It was quite obvious to me and to, I suspect, Norm that our styles were not compatible. So, I would say by mutual agreement I left the instrument division and Ed, somehow or other, took me under his wing.

Q: I guess the thing I was interested in was, when you did that did you start to move into developing longer range corporate strategy and that sort of thing?

A: That's right. We had gone through a period, actually, of hiring a corporate long-range planner which, again, was kind of typical of those times. It was a fad to have long-range planning. That, like most fads, disappeared. There's really no way that you can do that. The planning has to be done more at the divisions and so what turned out was kind of interesting. I really didn't do much, because they had a corporate planner, but I had to hire someone to be the division planner. Sort of like when you add another man to the football team, you have to put another guy on the line. So, all it really did was run overheads up without anything particularly useful coming out of it. I think even Gorbachev is beginning to figure that out about central planning. I was more involved and became more involved as I could see the activity. Ed Ginzton's activities became more long-range, just by the very nature of his own way of looking at things. I supported him in that, but we didn't have any formal long-range program.

One of the special assignments which I was successful at was to see what we could do about selling a plant which we had in Scotland near Edinburgh. That was a plant which, I think, we discussed earlier. We started to build NMR machines there and vacuum division was going to move in, but they didn't, and that was too small of an operation. At that time, I did spend some time in England searching and looking for products. The only conclusion which I came up with was that we should do something with the computers there. I actually did send a man down to Irvine to pick up know-how on the computers. Actually nothing really happened, but part of the reason that nothing happened was that it came out that it was not possible to transfer the manufacturing know-how to Scotland because it was not available in Irvine in the sense that the documentation and know-how had been put together by hand. That was part of the weakness of our whole computer activity. Actually it was too bad because we had a 16 bit computer, at that time, and an 8 bit computer, so there was a window there that had we done it right we could have succeeded.

Q: In that sense, if that was typical then of the kind of projects that you worked on, they were so much things like there was
a break through on superconductivity 5 years down the road there might be a product. They were more geared toward a company's specific organizational type things?

A: Yes, they were all directed toward the major lines of business. One of the things I remember is that Ed and I discussed this quite a bit because we both believed in the importance of the biological industry. We had discussions, for example, with Larry Kief(?) who, at that time, was going with the thought of leaving Varian and going to Hawaii. They were setting up an institute. A man from the Stanford Medical School had gone over to Hawaii and Larry went with him. We actually toyed with the thought could we set up an institute. Could we set up something here. I can remember looking around the parking lot for an area where we might build something. So that was much too early. We really couldn't get it off the ground, but we had been doing some work in the biological area and we did feel that that was an important area for Varian to pursue. In that sense, longer term kinds of things and projects were there. We tried to get to them. Ed was always receptive to new ideas and he would promote them and think of them himself.

Q: In general, though, you were a little skeptical about the value of what you called long-range planning?

A: Yes, that's right. I think, and did at that time, you must go through the exercise of it, but not in the sense of trying to work out how many dollars in sales you might have 5 years from now. What you need to do is try to look at the trends. In the biological area, that was what we were doing. We could not map out a roadmap. So, on the one hand, you try to do return on investments and the cash flow kind of things that are useful when you have projects which you could lay on the table and check and make a comparison between them. But, I think I probably mentioned this, at that time I did feel that the digital technology was important. I hired this group from Fairchild and got them over and told them to go to it, which they did. That was an example of something which could not be justified on a return on investment. It had to be justified on the basis that this was a significant new area and it had importance. To some extent, I did it too early.

Q: Would you say things like that by the year so and so, if we don't have annual revenues of "x" amount, we'll quit? Are you saying that that is the kind of thing you would try to avoid saying?

A: Well, it depended on what kind of a thing it was. If it was a product that was well defined and in the market place, then you could do something like that. If the sales are this, you could do one thing, or if the sales were something else, you could do another thing. If you had enough information to work with, you could do this. If you take something like current superconductivity, even when we were talking, you cannot make a real estimate of what it is going to be. You couldn't do
that with lasers a real long time ago. You couldn't even do it with computers or Xerox machines. Generally, those more important things, people underestimate the scope of it.

Q: It certainly would be interesting to compare the predictions or prognostications of transistors around 1950. I'm sure they were so far off.

A: Well, the story is that IBM felt that there would be a market for 10, or something, for what they called computers now. Not work stations. It was the same sense when we did the first digital thing here at Varian, which was called the C-1024, which was a way of gathering data collectively to see the signal noise ratio. My wise forecast was that we would sell about 10 and that ended up being a new product and we sold several hundred of them. So people have a tendency to underestimate and overestimate. Underestimate on the things that you maybe you're way out and overestimate on the others.

Then, other kinds of things Ed was always active in and interested in were the social end of things. He was with the Urban Coalition and founded the Varian Nesbick, so I just naturally took those over. I worked on the Urban Coalition in Ed's place. We transferred our Nesbick to OCC and so I went on the board and been doing that ever since. So a number of these kind of community oriented affairs I had before Ed.

Q: Did you also work with him on things like the Cosens Committee?

A: Yes, I'd prepare documents, in a staff sense, and do background information, etc. I don't think it did any good, but we did it.

Q: As long as we've started talking about some of these general issues like long-range planning, I have a few more I'd like to ask you about. Now might be a good time. About some of the community oriented things, maybe before we get to the general issues, I'll just ask you about some of your personal kinds of involvements and areas of interest to you. One that occurs to me that's a spin off, I guess, from Siva is this work with Abram Goldstein and his foundation on addiction?

A: Yes, that really came from our association with Siva.

Q: Can you talk about that a little bit?

A: Abram felt out of place in the medical school, primarily because of the dean, a fellow by the name of Rich I think, who is no longer there. Abram didn't feel that he had the independence and that there was too much bureaucracy, too much red tape. He decided to set up a foundation himself. He got a good sized grant from NINH and he arranged to set up this non-profit foundation and take his research grant from Stanford and move it over and then operate as a fairly independent thing. But he always kept at least one little toe in the Stanford pool.
It started primarily as a socially oriented thing. He started
doing methadone treatment and actually did work on clinical
studies on the advantages of methadone. He had addicts who
came down and got their orange juice with methadone in it. Initially,
the government organized in a very equalitarian way. This was
to involve both staff inside and community outside. They had
a large board of directors and the idea was that community people
would contribute. As time went on, they found out that maybe
that wasn't quite the best way to do things and he began to
take more control of it. Gradually he cut the board down to
5 and Ed Ginzton and I. Again, I think it was a useful combination
because I knew enough of the science to understand and appreciate
what he was doing.

Q: Was there a tie-in with the diagnostic tools that had been developed
at Siva?

A: There was a break initially. We had awarded Abram some small
stock in the partnership and he had to give that up when he
started this foundation because he got a grant from the Drug
Abuse Council and they considered it a conflict of interest.
So he gave up that small amount. That kind of annoyed him.
Then, at a later time, Dave Reubenfein(?) did bring him back
in as a consultant and did something for him, but that was not
enough.

Q: I guess, then, I'll ask you about some of these environmental
and conservation causes because it struck me that there is quite
a tradition of ties to the Sierra Club and all at Varian and
at Stanford Electronics physics communities.

A: That's correct. Russ and Dorothy met, I think, on a Sierra
Club trip actually. Dick Leonard was part of that group and
somehow or other Ed was, too. Ed was always interested in the
outdoors. We built this chalet up in Alpine Meadows, primarily
Varian people. But that was all Ed's field. He put a push
behind it because he enjoyed skiing; so did Dorothy Varian.

Q: Do you think that was part of the culture at Varian; their involvement
in that kind of causes?

A: I think, in the sense that many or most of the people enjoyed
that aspect of it and there was the tie with Ansel Adams, etc.
There was a group or collection of like-minded people with respect
to those activities. Dorothy, of course, did the Castle Rock
thing. Russ and Dorothy were the major push and Varian, from
time to time, contributed in one way or another to that activity.
Dick Leonard was on the Sierra Club's board for quite a while
and played a fairly major role; both he and his wife were quite
active in the Sierra Club. I don't know that Ed was particularly
involved in it and I know I never was for a very strange reason.
When I first came here I used to enjoy backpacking and go to
the mountains. As everybody knows, when you're in the mountains
there's not supposed to be anybody else there. They should
be all yours. So here we would go along a trail and find people
scattered along for a few hundred yards and each person had
his little tin cup hooked over his belt with a handle on it. That became my view of the Sierra Club so I never had anything to do with them. But now I don't think much of what they are doing. I think they lost their original purpose, as far as I'm concerned. They could have gone on and quietly done what they could have, but they have become an activist organization without knowing too much about what they're talking about. I remember talking to Dick Leonard many times and he said he wasn't entirely satisfied with it either.

Q: Just by glancing through the photographs, certainly this goes way back to people like Anita Forres(?) - way, way back, were involved with the Sierra Club. It was really more a communal kind of nature experience organization and, of course, conservancy was part of that, but, with a lot of group activities. There was a sense of community associated with it. Now it is much the political.

A: That's right. That was all right, except I didn't really want that. I didn't care for that. I realize that there always had been at Varian, and this relates to the people with a communal interest, that their name was "Varian Associates". The idea was that we were rather equalitarian. Several of the people, certainly Myrl Stearns, probably Sig Varian and I guess others, were involved in the very early days of Ladera. Ladera, at that time, was being developed more as a communal project by, I'm not exactly sure whom, but probably Ed was part of it and Myrl Stearns; that turned out to be a little bit beyond their experience and capabilities. It was taken over by some commercial real estate firm and it was expanded. So, I think some of the Varian people lost money in all of that.

Q: Russ Varian probably wasn't in it because he stayed in Cupertino?

A: To my knowledge, no. He stayed out of it, but there was an early feeling that it would be nice if we all kind of lived together and worked together.

Q: Of course, with the Varians, I guess that goes back to their upbringing.

A: Yes, the socialistic style. For Russ, he got that from his father and was very socialistically oriented. I don't know if with Russ he was ever politically oriented that way, but it was an acceptable way of life for him.

Q: I think it is a very interesting kind of consistency in those kinds of views towards community and everyone kind of being on the same wave length about some of these issues. I think that is characteristic about this company as compared to other companies.

A: I think that is right. It was the nature of how it grew and who headed it. We used to spend a lot of time trying to get involved with people. We followed, at one time, what was known as a Schilling(?) plan, which was to have a shadow board of
directors that would handle certain kinds of problems, try to be innovative and provide a communications link. Marsh was in the company as treasurer, maybe secretary or something, and he brought in the concept of the Schilling plan. We adopted it but we didn't call it that. We called it Management Advisory Board. We had meetings every week or every month or two. So many of us were on that, I guess that was one of the first aspects. We did a number of things. One thing I remember was getting the credit union started. That was a project which we carried out. The idea there was to provide closer communications all up and down the line.

Q: What was the Schilling that you refer to?
A: That was the Schilling that sells salts, pepper and spicy things.

Q: Related to that, I've seen advertisements from some companies as early as 1960 in trade journals and such, where in company recruitment ads there will be an appeal or refers to an intellectual climate, physical climate in California, etc. I imagine some of these people oriented kinds of projects that Varian had also would make a good recruitment feature. That the company policy toward employees, along with the informality of things on the west coast, could have been something that Varian emphasized.

A: I'm not sure it was emphasized in recruitments. We did recruit from all over the world. The fact that things were tough in Europe and nice in California was a real plus. We had immigrants here who were extremely bright and capable. Interestingly enough, it was pretty much with Europe at that time and now it's with Asia bringing in the bright kids who start working. It looked very good to them and it made it very easy for us to bring people in. I think they got a flavor of what happens and if they came in as a post op(?) or a visitor, or visiting professor, that's what they would have sensed was the informality. I'm not sure we viewed it in that way particularly. I've worked for Westinghouse sort of off in the corner and still wore a tie and wear one here. I didn't change uniforms, but certainly we had an open door sort of thing. There was a lot of time trying to figure out on a committee basis what the objectives of the company were, how we should treat people, how we should treat employees, and customers. There was a lot of time spent on that and a lot of time on education programs. There were strong programs with Stanford, etc.

Q: But it wasn't, at least in the early 60s, a conscious thing?
A: No, I just didn't sense that. Somebody else may have a different impression of it, but I really didn't see it that way particularly.

Q: Let me ask about some of these key issues. There was one I wanted to ask about that I know you've worked on, but we haven't talked about explicitly, and that is Varian's program of working with Soviet and Chinese technology-based companies. Can you talk about how that started? What your role was in it? What was in it for Varian? Why did Varian embark on this?
A: As far as Varian's concern, and of course I'm always speaking with a bias to the instrument end of it because I'm not sure about the tube activity, I think we sensed at a very early time, although I don't know if we articulated it or not, that equipment of this type had to serve an international market. It couldn't just serve a domestic market and leave the rest alone. Dealing in scientific matters recognizes very little in the way of political boundaries. We just had a natural propensity to serve all scientific people, no matter what country they were in. We did establish, very early, representatives, if you like, in Europe. I'm not sure we talked about this, but Harry Weaver was one man who spent maybe a year or so in Zurich working with Franz Stough(?). He had gone back for the physics department and he was sort of our man in Europe. He was followed by Warren Proctor(?), who lived in Holland for a while. We actually sent John Moran over as a service man working out of Holland. The reason for Holland was that Warren had sort of an adjunct appointment at Leidman's(?) so he had things to do there. He also worked with Abergam's (?) in Paris. So we established rather early then the scientific presence in Europe. Then we did set up with Warren Proctor a company in Switzerland and they established a research laboratory, like IBM has done. That was in Zurich. The commercial offices in Zug were for tax reasons, but the research was in Zurich for technical reasons.

A couple of things happened in Russia. One of their people who was in New York, I think Antor(?) who knew what we were doing, thought it was a good idea to have an exhibit in Moscow and that we should decide ahead of time so we could buy and ship stuff over. I think that was in 1967. So we have our first private showing of NMR equipment.

Q: This would have been the first NMR, period?

A: Yes, they may have tried to do a little bit themselves, but to no success. Then Ed went over with the National Academy Group and, as part of that, Kobishiani(??), who had taken an important role in the newly established Science and Technology Committee which was established in about 1965. The purpose was to coordinate technology within the Soviet Union and identify what technologies they needed and get them from the West. Kobishiani threw out the challenge publically for Varian to come and sign an agreement with the State Committee. The agreement was something that would legitimize it from their point of view (the interaction which we had). For us, it really didn't mean much because we don't need that sort of thing, but then we did prepare and did sign such an agreement. We began to explore ways to work cooperatively. For example, I spent a couple of weeks there taking a lot at some of their instruments, facilities, and ideas. Nothing ever came of it, but at least they opened it up to that extent.

Then we did develop a relationship with Moscow State University, which we still have, and it's developing into a little bit more commercial relationship. That was the start of our scientific
presence. The thing that has always amazed me was that, no matter where we've done throughout the world, Varian's been known. They never has been a question that we had to establish ourselves. Everyone knew Varian when you talk about scientific things. We were respected and welcomed in the countries.

The same thing happened with China. They opened up China after Nixon and, I think it was 1972, they sent over a delegation composed primarily of people from the Academy of Science. We were one of the few institutions that hosted them here. It's kind of interesting because at that time we had secret service people all over the place and everything was very formal. Then gradually we began to do more of this, so we had quite a number of people. Incidentally, this is well cataloged and if you decide what you want to do with it, you can have it. I actually started a system where you can track what happened and so all the documents are in the computer as far as a listing of it. They are all numbered and there are long shelves full of these documents that tell how it happened, what happened, etc. in China. If somebody wants it, they're welcome to it.

Q: I think we would be very interested in that. We have an interesting "sister" collection to that in that we have Ed Peindenbaum(?) in Computer Science and Artificial Intelligence was involved in a few of the early contacts with the Soviet Artificial Intelligence community and also in connection with the Naval Academy. It would be interesting to have those.

A: It could be because he actually had contact with Marchuck(??) who was cybernetics at _, and then Marchuck came and took the head of the science technology committee and now he's the head of the Academy. That's quite an interesting thing there. But just for your general background you can have this computer listing of the documents and it's all indexed for you so it would be quite helpful. If you ever wanted to see these other documents, you could. As a matter of fact I have the big tapes here if you wanted to print this out.

Q: Good, I'll give you a call on this.

A: That could be interesting because Ed had some interaction with Marchuck.

But, to finish this China thing, it was our experience in Russia, and the need to legitimize at a higher level, that was helpful in China. When Ed took a trip to China with that Stanford group, he came back and thought about how do we do something with this. We conferred with Tom Finger and John Maliss(??) over there at Stanford and we were part of that group. Together we worked out a concept that maybe we should do the same kind of thing in China and then we had to decide with whom should we do it. So the decision was to work with the Academy. It happened that we had had as a visitor the Secretary General of the Academy. Actually he had been here twice and we knew him fairly well. We wrote to him and suggested that we establish a relationship
and he agreed. We went over in 1980. It was a rather interesting negotiation because it was all so new to them that they really didn't know what to do. They could not do it through the Academy because they were not allowed to have interaction with foreign companies. At that time, they had the equivalent of a foreign trade organization associated with the Academy and so this had to go through the commercial end of things. As a matter of fact it was negotiated not by a professional scientist at the Academy, but the guy who was president of the Oriental Scientific Company. We worked out some agreement and understandings and that still exists. But, China, did not have the same kind of mentality that the Russians had. They don't have quite that fear of foreigners so they don't feel the need of so much protection. They aren't quite so paranoid and they are a little bit more willing to take a risk. Even though that exists, we haven't called on it too much. We let two years go; Ed met the new president at the National Academy of Science meeting and opened communications regarding the letter so maybe we'll do something more sometime through them. It exists, but it is not a legalistic thing. It is an understanding that we developed.

Q: So far, there hasn't been any impetus to get any real practical payoff on this relationship, is that right? It's been more of a communication?

A: Well, we'd like to have a practical payoff. In Russia, it was extremely important because that is what made it possible for our sales people to interact. They have more end user contact than we would otherwise, but in China it is less important. We can get to the end user. In both cases, it's primarily the reputation of the scientific people which makes it possible to interact with the end users. We know some of these people so we can by-pass the normal bureaucracies.

Q: Is it a substantial market in the Soviet Union?

A: Actually there is a lot of business, but for various reasons we have not done as well as we should have. One of the most profound reasons is our own Government. We were beginning to do quite well at major share of the markets and then the Afghanistan thing came and that just essentially wiped it out. We kept a low profile and continued to work in there. Now, a major difficulty is export licensing, which I feel is entirely unrealistic. Things may be a little bit better now, but not enough to help us particularly. Competitively, it is difficult because the German competitors have much less of a problem than we do.

Q: Can a state-of-the-art NMR device be sent to the Soviet Union?

A: Well, the curious thing is that the NMR itself is not on the list. That's not a problem; maybe it should be, but it isn't. The problem has to do with computers. Here we run into things like the Winchester Disk and the requirements on the computers. To me, it is rather naive on the part of the Defense Department to think that they can do anything by keeping one or two instruments
out. But, the argument is that if you send in this powerful computer, they're going to take it out and they're going to do war calculations. In the first place, they have bigger computers than that. In the second place normally, well one that I did a study on, the computer portion was about $20,000-25,000 out of half a million dollars. If they are naive enough to want to spend a half a million dollars to get a computer, why go to it! They could steal them for less money than that. That's not a problem at all.

Q: I remember reading somewhere that in terms of microprocessors, it is actually specific that up to an Intel 8088, the original IBM PC chip, it is now O.K. to send. Anything beyond that isn't.

A: Well, it is not quite that. You can do more than that. What happens is that they raised the standard for the last 2-3 years so that, competitively, that makes it difficult for us because the Germans can bring in the latest and we're lagging way behind. The other bad thing about it, of course, is current production. You can't send two-year old equipment because we no longer make it. We don't know how to make it any more, so we have to send what is currently in production and that makes it very difficult. But it is a curious quirk that it's not really the NMR, but from a chemistry point of view it is extremely valuable to them.

Q: Would the medical electronics be affected at all?

A: Yes, the same problem. If it has a computer in it, there is a problem. We can send them linear accelerators for doing industrial x-ray applications, but if it has an image intensifier on it or a computer with some of the latest things, we can't send them in. This is very curious because you know very well that they can take this accelerator and use it to irradiate rocket fuel. That's what we use it for. They wouldn't have to do anything else. You can also put the welding in whatever you want. But that isn't on the list. It's when you get a computer on it that it goes on the list.

Q: So that the fixation is really with this one thing.

A: Yes. It may be that they have a lot of bright bureaucrats back there and maybe they really do want to stop the rest of this while they look at the legalist holes they have in it. But, it does make it difficult to deal with East Block countries. The thing that is a bit of a problem is, as I mentioned earlier, in order to be successful in the business you have to have market share which means you have to market worldwide. If you lose a sale in the Soviet Union, that cuts your total market share. Maybe that one instrument is not all that important, but what happens is that if you lose market share on a worldwide basis then the guy who has the best market share has the best chance of winning. So you really have to fight these things in each and every country and this is not always recognized by managers.

Q: That opens up a whole question as to national competitiveness and the different kinds of business strengths that one battles
with. How has Varian dealt with that whole issue of foreign competition?

A: In a number of ways. We have gone overseas and set up joint ventures in a number of places.

Q: With foreign companies?

A: With foreign companies, and sometimes not a joint venture, but independently, because of the country. We have a joint venture in Japan and one in England in the tube business. Many of these, particularly the defense oriented ones, really want local control. We had a joint venture with the predecessor with Thomson-Varian. Thomson was in eastern France and, essentially by government edict, we were thrown out of that. That's been costly to us because actually those tubes are now competitive.

Q: That was the American government?

A: No, the French government. One has to tackle various problems in different ways, depending on the local situation. In China, we have several licenses and put kits together.

Q: In a way, this is all consistent with what Varian was doing in the early 1950s with this community building. It was finding ways of marketing?

A: That would be normal business practice. If you have a problem, you have to figure out ways to try to solve it. Management can do two things. They can say that that is going to be more effort than it is worth so I'm not going to do it and I'm going to give up that market and concentrate on this one. That's one approach and one that is often used in the near past. The other is that the manager's job is to figure out how to accomplish it when you have a problem; to figure out a solution. So we did not. Some things managers, from time to time, back away from it. You can say that is a wise thing, but this is not necessarily true because, again, if you lose market share you've lost position.

Q: When you bring that up, it reminds me on the point that we touched very briefly and said we might return to later. It had to do with consumer products, specifically the microwave ovens and your comment that Varian decided not to produce those kinds of magnetrons because the company really wasn't interested in going with that kind of consumer technology. Is this one those types of decisions that you're talking about where managers make the decision to back off from a certain area because it is not worth the trouble?

A: Yes, that's right and we've dropped products for the same reason. If it looks like it is going to be tough, rather than doing the right thing, you become a hero by getting out of the business.
Q: Was there anything about that microwave oven?

A: I don't remember too much about it, but I do remember that there was a study and we simply were not cost competitive and had we been, I don't know Raytheon or maybe somebody else, might have decided well the answer is to figure out how to become competitive. But then, the mentality was not directed toward that kind of business. It was directed toward something else. I may have mentioned this, but Norm Parker, who came on board initially, seemed like a good match because he came from a high technology company. But the mismatch was that he was not a risk taker and he did not understand that you had to decide that you were going to do these things and then you either succeed or you fail. You can decide to engineer and design a low cost magnetron when you know what the market is. Aerospace companies work the other way. It's a low risk type thing there. However, that was a mismatch which would have kept us from bringing in the commercial business.

Q: Do you think that there is anything we haven't talked about that you think is important, either at the personal level or the company level? Do you think we've pretty much covered all of the major topics?

A: I think you have touched on the major things and I think we recognize that we're in two vastly two different kinds of businesses which always tends to make it difficult for Varian's people. We've touched on the importance of the foundation of the top management and the culture which, by and large, is still with us. Despite the fact that we had a little problem with Continental, that's not our way of doing business. He was an outsider who came in from one of the aerospace companies, so his kind of behavior which is not ethical is not typical.

Q: Thank you. I think that may be a good note on which to end the transcript.