

EDWARD M. PURCELL

An Interview Conducted by

John Bryant

IEEE History Center

14 June 1991

Interview # 101

For the

IEEE History Center

The Institute of Electrical and Electronics Engineers, Inc.

and

Rutgers, The State University of New Jersey

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Edward M. Purcell, Electrical Engineer, an oral history conducted in 1991 by John Bryant, IEEE History Center, Rutgers University, New Brunswick, NJ, USA.

Interview: Edward M. Purcell
Interviewer: John Bryant
Date: 14 June 1991
Location: Cambridge, Massachusetts

Bryant: Professor Purcell, we'd like to start with some background. Your family, perhaps your father, and why you chose to get into physics and become a scientist.

Purcell: The reason I eventually became a physicist was because of my father. I grew up in a small town in central Illinois and my father was the manager of a small local telephone company. The company still exists and does very well, I think -- the Illinois Southeastern Telephone Company. My father was a businessman who became the general manager. Through that I got access to certain materials, wire and lead cable and stuff like that. And there came a time when I found myself reading the Bell System Technical Journal which was sent free by the Bell System to this independent telephone company. That was a very exciting and eye-opening experience to look at those articles, most of which I probably couldn't understand, but such a clearly superior product that I decided that I wanted to go into electrical engineering.

When I went to college I went to Purdue, instead of the closer-by University of Illinois, and graduated in electrical engineering. Of course, that electrical engineering was a very old-fashioned kind from the present viewpoint. I know two ways to wind armatures and things of that sort, which most of my physicist colleagues don't. Since I spent most of my life as an experimental physicist dealing one way or another magnets and practical power supplies, in my generation electrical engineering was not too bad a preparation for physics. There are quite a few people who illustrate that, including Ken Bainbridge, whom you

just talked to, Curry Street, and several others.

I really didn't know what physics was when I went to college at Purdue. It turned out that physics is what I had thought electrical engineering was. I began looking around at the physics courses, and I took an independent research course that the Physics Department had listed in their catalogue but no one had taken before. I went around and they took me on, though I was only a junior EE. I was able to do some experiments with spectrographs and ionization chambers. And then as a senior I was allowed to work with a physics graduate student, a very senior and well-advanced physicist named H. J. Yearian. The head of the department at Purdue then was Karl Lark-Horovitz; that's a very important name for Purdue physics because Lark-Horovitz created a department of modern physics where there really had been nothing before. He was a Viennese physicist, really very knowledgeable, and he knew the world of physics. I was allowed to work two steps under him, not directly under him, which is probably a good thing. I worked on an electron diffraction camera that Yearian had built. This was right at 20 kilovolts and produced high-voltage electron diffraction patterns, and I was allowed to run it. In fact my first two published papers are connected with that (I am a coauthor, of course). The first photographic plate that I ever developed had Debye-Scherrer diffraction rings on it when I turned on the light, and I was really hooked on physics. It was the Depression when I graduated in 1933.

Bryant: You took a degree in electrical engineering?

Purcell: Right. But through Lark-Horovitz's help I got an International Exchange fellowship to go to Germany. That's when I went to Karlsruhe for a year as a

physics student. From Karlsruhe I came back to Harvard as a first-year graduate student.

Bryant: May I ask you some questions about Karlsruhe? Did you attend classes there? What was the activity?

Purcell: Yes, I did. Well, you see, I went there in the fall of 1933. Hitler had come to power in January of 1933. That, of course, was a terrible turn for the university as well. And when I got to Karlsruhe, my professor, the professor of modern physics there, had been kicked out, at least temporarily. After a semester he got back in.

Bryant: What was his name?

Purcell: Walter Weizel. He was an excellent teacher and in the spring semester I was able to take his courses. But I took various kinds of courses there because I had plenty of time and I had free tuition so I could sign up for any number. I took a course in art history and one in history of the Upper Rhine Valley. I also sat in on a course in physical chemistry.

Bryant: Did you take any laboratory courses?

Purcell: No. In fact, there really weren't any. In those days, Karlsruhe was of course just a Technische Hochschule, and the physics offerings were really pretty skimpy. There would have been much better places to go, but I had no choice. I was an exchange student.

Bryant: Were you at all aware of Heinrich Hertz's work at that time?

Purcell: Well, I was dimly aware of it. I was not as aware as I should have been, in retrospect. I remember going to a physics lecture, elementary physics (undergraduate, as it were) lecture. I really just went to it to improve my German, because it was stuff I could easily understand. The important lectures for me at Karlsruhe were seminar kinds of lectures on thermodynamics and statistical mechanics.

Bryant: You started to talk about your return.

Purcell: Then I came back to Harvard as a beginning graduate student in 1934. I got my Ph.D. under Kenneth T. Bainbridge in 1938 and worked for a while on the Harvard cyclotron. I helped build a magnet for the first Harvard cyclotron; Bainbridge, whom you have already talked with, was really in charge of that construction. Then I got drafted, as it were, for the Radiation Lab in the fall of 1940, when the Radiation Lab was just beginning. I was teaching an undergraduate course here at Harvard and couldn't go down full time to MIT until February 1941. From then on I was totally absorbed at the Radiation Lab.

Bryant: Did you attend the American Institute of Physics Symposium on Applied Nuclear Research in Cambridge that year?

Purcell: I don't think so, at least I don't remember it. I remember walking down the hall one floor below. On the floor below this there is a room that we called the faculty room, which opens out onto that hall, and one night I was over here walking down the hall and there in the room I could see they were talking, Bainbridge and Rabi and maybe somebody else. It seems possible that at that point they called me in and asked me if I was interested.

Bryant: Was that in this building?

Purcell: Here, in this building, in this physics department. My Radiation Lab badge number is 42.

Bryant: Number 42, that must have been December 1940?

Purcell: Yes, that would have been December because I could go down part-time. I think that's right. Yes, December 1940. I lived nearby here, in fact still do. I'm probably one of the few people who are still living in the same house they were in December 1940.

Bryant: That's very handy.

Purcell: A fixed point in my otherwise rather rusty chronology.

Bryant: I'd be very interested in your description of officially joining and who put you on the payroll. Were there any negotiations?

Purcell: I don't recall any. There may have been some negotiation with the chairman of the department here at the time, who was F.A. Saunders. Of course, Ken Bainbridge was my professor, to whom I felt great loyalty and whose judgment I would trust as to whether I would be useful.

Bryant: What was your job title initially?

Purcell: Heavens, I have no idea.

Bryant: Who was the group leader?

Purcell: If Norman Ramsey were here we could sort this out. We were in the magnetron

group. See, the original groups were: the magnetron group, the modulator group (or what would later be called the modulator group), and the antenna group. I was associated with the magnetron group. There was a transition when Norman went off to Los Alamos, and I became the head of group 41, which we then called Fundamental Developments. I always considered myself under Rabi, whose relation to the lab was special, anyway. But the fixed point in my memory is connected with a trip to England that I made with Rabi in the summer of 1942. I was a junior associate of Rabi and we went over and spent a month in the summer at TRE and various equivalent enterprises over there.

Bryant: Did you visit any industrial laboratories?

Purcell: Yes, I did, and more than Rabi did. I had been charged with finding out the very small details of the construction of the high burnout crystals.

Bryant: High burnout crystals that could tolerate a large energy pulse?

Purcell: Yes. I spent a day or two at GEC, I think it was called. When I came home I had a thick stack of notes about the cookbook witchcraft of making these crystals, which nobody really understood. I remember giving a seminar at Radiation Lab for the crystal people. But when I went around with Rabi, I had a real chance to see the war effort and the radar problems. We were down at Coastal Command, where I spent half a day or so with a man named E.J. Williams, who was working under Blackett.

Bryant: P.M.S. Blackett.

Purcell: P.M.S. Blackett's group was part of Coastal Command. The aircraft that were

going after the German submarines as they transited across the Bay of Biscay.

This is one of the early exercises in operational research that Williams was doing.

Williams was a brilliant physicist who died very prematurely after the war. It was the first time I had myself realized how serious the war in the Atlantic was. We were in the office of the head of Coastal Command (Air Vice Marshall somebody), and there on the gigantic map of the Atlantic was a pin stuck in for every ship sunk. From Maine down to Florida there was hardly room for another pin. The floor of the ocean was littered with iron! Rabi and I went to Oxford because by then I was deeply involved in the X-band development and later the K-band development at the Radiation Lab.

Bryant: Did you get up to the University of Birmingham?

Purcell: No, we didn't. I got to Bristol, which was rather important because ...

Bryant: Was it because of Sutton?

Purcell: Sutton, that's right, Sutton was at Bristol. There was the Sutton tube, which had turned out to be the prototype of the TR box for us. But at this time Sutton had developed the flat face cathode ray tube, which nobody had had up to that point. Sutton had found a way and had trained a bunch of high school students to do the work. He was a very unusual man. They were turning out these tubes in which you had a flat plate of glass, joined by fusion around to a conical top. They learned how to do that so that it would stand up.

Bryant: That's remarkable, I didn't know that. The Sutton tube was an external cavity klystron to start with.

Purcell: Yes.

Bryant: And then they used the electrodes as part of a circuit to make a TR tube, so if you had a gas breakdown between the circuits, that made the switch.

Purcell: Yes, that's right. By that time we were beginning to really understand the TR business a little bit. Not by that time, but another year later, Westinghouse had a TR that was sort of an advanced resonant cavity. I think we understood the TR after about the end of 1942. I did a lot of calculations in my group for making K-band TRs. The TR was a big mystery, you know, when the Radiation Lab started. I remember that when we'd have a general meeting in those early months and outline the system that we were supposed to be working on -- big boxes representing magnetron power, pulser, receiver, and so on -- there was a box put in called TR for "transmit-receive." Nobody knew what was going to be in the box. They had no idea!

Bryant: TR -- to operate a common transmitter and receiver antenna.

Purcell: It was assumed from the beginning that it was to be common. And that being assumed, there was a challenge of what was in the TR box. When I got there, the first thing I worked on was a magnetron, not in the design or development (other than at first), but in measuring power output and things like that.

Bryant: What was the first TR you used experimentally at the Radiation Lab? Was it made in-house, was it made by Bell Labs, or do you remember?

Purcell: I'm not really sure what in fact the configuration of the first TR was. I know what the first 3-cm was TR because we used 6-32 screws in waveguide right in the air,

with two screws making a capacitance across the guide, thereby creating a cavity. And then a third screw, sharpened on the end, was placed in the middle and that was it.

Bryant: It would break down with the power?

Purcell: It would break down with power.

Bryant: That would take a lot of power in air.

Purcell: We had a lot of power. That was the point.

Bryant: That's very interesting. On magnetrons, did you have a lab set up for building experimental units?

Purcell: No, none. No experimental units were built at Radiation Lab; there was no tube lab.

Bryant: Was there ever?

Purcell: Only for some very exotic things like Vic Neher's oscillator, a K-band oscillator. The person you should ask about that is H. Guyford Stever. He's an interesting person because of his subsequent career. He was head of NSF and he's been head of many other things; he was briefly science advisor to President Nixon.

Bryant: We missed on not interviewing him. There are only four of us doing interviews, and between us we have already scheduled 40 interviews.

Purcell: But the reason I mentioned him in this context is that Guy Stever worked directly under Vic Neher and knows more than any living person except Vic Neher about

the Neher tube.

Bryant: So you depended on industry for building experimental magnetrons?

Purcell: For the magnetrons, in addition to Bell Labs there was Raytheon and Percy Spencer, a great name in this context.

Bryant: Apparently he reacted quickly to requests for prototypes.

Purcell: Yes, he was very fast. As it turned out later, he was the one who greatly advanced the manufacture of the S-band magnetrons by his manufacturing technique. He was great at building new tubes and in fact he had a very important role.

Bryant: You were a group leader of RF and systems research at X-band and K-band. In the beginning, say in early 1940-41, what kind of facilities and test equipment did you have?

Purcell: Oh, you mean when I first went down there what did we have? We just had that big room there, 4-133, and we had oscilloscopes and general raw material for electronics.

Bryant: Were there wavemeters?

Purcell: No, we built most of our own. There had been some microwave research and experimental laboratory activity at MIT under W.L. Barrow. But the physicists who flooded in in 1941 mostly came from their own labs, where they had been building things, electronics, and they knew what kind of stuff they were going to have to buy.

Bryant: Did you remember interacting at all with E.G. Bowen?

Purcell: Oh, yes, I knew him very well.

Bryant: Did he contribute much to your basic information about microwaves or circuits?

Purcell: No, not microwaves as such. What we all learned from Bowen was about what you need to fight a war with radar. He was great at keeping our attention on the actual goal of making a night fighter radar that would shoot down bombers.

Bryant: Did he have a regular time for giving talks or were most of these informal and unscheduled?

Purcell: Most of these were informal and unscheduled in a group small enough. I kept in touch with Bowen even in later years because, as it turned out, after the war he went to Australia and became head of the CSIRO Radio Physics Division, where they were doing world class radio astronomy. I was then in radio astronomy myself. Taffy Bowen was a very close friend and I saw him in Australia. I've seen him often here later, too. I don't know if he's still alive; he had a stroke.

Bryant: Yes, I understand that his son has been answering his correspondence. As you were progressing at Radiation Lab, do you have a feel for how many of your devices, magnetrons or circuits, were internally generated designs versus how many came from industrial labs like Bell Labs, or from the British?

Purcell: It's hard to sort it out. We were all really learning together in a funny way. There would be exchanges with the British where the same idea would cross in the Atlantic. Bell Labs was very important. We had great respect for the people at

Bell Labs. I think we earned their respect later on, even though the Radiation Lab guys were a little wilder and bolder and perhaps more cocky than the Bell Labs people. But Westinghouse was building the modulators. I don't think there was anyone at Westinghouse at that time that was particularly influential. At Bell Labs, the production of the copy of the British magnetron was done in the department headed by Jim Fisk, whom many of us knew as a physicist. Relations were very good. Zacharias, who spent time at Bell Labs, came early on to the Radiation Lab. Zacharias was one of Rabi's people from the Columbia Atomic Beams Lab in the 1930s. He stayed at MIT after the War and became head of the Laboratory for Electronics and all that. I have a hard time not thinking of Zacharias, who would have been such a wonderful informant for you... and all the guys who are gone.

Bryant: It's good of you to point this out to us.

Purcell: You really can't understand the spirit of the lab as it really was at that time without more direct access.

Bryant: On interacting with an organization like Bell Labs, I take it there was person-to-person interaction that was quite informal, correspondence or visits or telephone calls?

Purcell: Oh, yes, although at a certain point the informal system sort of broke down. The person-to-person communication got so extensive that any conventional industrial manager would be horrified.

Bryant: Did that include telephone calls?

Purcell: Oh, yes. At some point there went out an edict from both institutions to their members that there was to be no cross person-to-person communication, that all letters to anybody at Bell Labs should be addressed to Dr. Ralph Bown, and all letters back were to be written to Lee DuBridge. So we were always typing these letters and going around and getting them recorded, etc. Worse, at the other end there was a strictly administrative type of guy. We chaffed under this restriction and I think we often used the telephone to get around it. It probably was necessary at that point, because the lab was expanding very fast. I had been down at the Bell Labs on and off. I remember particularly going to the Holmdel Lab that was under Southworth and Friis, particularly Harald Friis.

Bryant: George Southworth and Harald Friis.

Purcell: In a way we envied Bell Labs because they were in the ivory tower and we were out on the front lines. We had deadlines to meet for the military, whereas out at Friis's lab at the beautiful farmhouse in New Jersey, all you had to do there was your experiments at your quiet bench. The thing that Bell Labs represented that was very important, at least to me, was that Bell Labs engineers, such as Friis's people, were extremely competent, careful engineers, just really classy. Whereas we tended to be kind of slaphappy all of the time. We'd measure things and get a rough answer and jump ahead to the next thing. I used to put it this way: when Bell Labs measures something, it stays measured.

Bryant: And it's reliable.

Purcell: It's reliable. Whereas at our lab, the atmosphere was "there was a war out there."

Bryant: Was this also the case with documentation? You probably didn't have much time to document things at the time.

Purcell: That's right. Some inventions would be made overnight.

Bryant: Did you keep a notebook?

Purcell: Yes, I guess I had a notebook, although I have never been a good one for keeping notebooks as a scientist. Ramsey certainly did. There was an official Radiation Lab style of notebook.

Bryant: I believe that the notebooks are in the archives.

Purcell: I guess so. Ramsey kept using those Radiation Lab notebooks during the rest of his career. Of course, in his notebooks there's thirty years of fine physics. The atmosphere was. Did you hear the talks on Tuesday? In particular, the talk by Marcuvitz?

Bryant: Yes.

Purcell: He was in my group, although we were both under Rabi (Rabi was head of the division). I knew Marcuvitz very well. He worked partly in our group, but his relation with Schwinger and his whole talk was rather accurate and it reflected the spirit of the times. I saw him after the talk. We had lunch together and we were remarking to one another on the extremely kind of warm, friendly, but tense atmosphere of the physicists at the Radiation Lab. In the early years, at least. As Mark says, "You know, it's not that way in a lab anymore."

Bryant: Was it because of deadlines?

Purcell: It was because they were all physicists who trusted one another as scientists, had the same kind of values, and the same admiration for a good idea no matter who had it.

Bryant: What created the tenseness?

Purcell: The tenseness was just the pressure.

Bryant: The pressure of work.

Purcell: The pressure of work and the challenge of these things--such as, what are you going to use for a TR box? So you go off and start developing that. Meanwhile, we were all learning on the job from the word (go). Some of us had gotten mixed up with radio frequency resonators while building cyclotrons and things. But in the whole field of microwave transmission, all the things that are in some of the Rad Lab books, we had to learn by doing.

Bryant: Professor J.C. Slater at MIT, head of physics at that time, got interested in microwave electronics and prepared lecture notes and published them in a book.

Purcell: Yes, that was somewhat helpful, but not terribly so, I think. The person that I think of as being extremely helpful as a teacher was Bill Hansen.

Bryant: William W. Hansen of Stanford, working for Sperry Gyroscope Company on Long Island for the duration.

Purcell: Yes.

Bryant: How many times did he come up to lecture here?

Purcell: There was a time when he was coming once a week and gave a lecture. It was very important. In the first place, he knew what he was talking about. And he was talking about just the things that were at the heart of the problems people were having. We were all learning very fast.

Bryant: Resonant cavities and applied electromagnetics?

Purcell: All the waveguide transmission, the art of waveguide junctions, switches, rotating joints,...that whole business. We made an effort to learn about it. As a teacher, I also would like to claim that part of that atmosphere was due to the fact that a lot of teachers had been assembled. These were fellows who, once they understand anything, can't wait until they explain it to somebody else. So there was always that kind of transmission in the hallways.

Bryant: Networking.

Purcell: I guess you would call it networking, yes.

Bryant: Were the Hansen lectures made a part of the regular weekly lectures or were they special?

Purcell: I don't remember the regular weekly lectures as being all that regular; maybe you have some records that show that. As I think back, I think that for a while they were the regular weekly lectures, and then there was something else. Ed Condon started off lecturing early on, and he wrote up notes about it.

Purcell: This will be out of order chronologically, but I would like to say something about the famous K-band wavelength choice issue. Someone who commented on it in

one of the talks this week had it a little bit wrong in ascribing the choice of the wavelength to Rabi. Rabi should not be blamed for the choice which, as you know, turned out to be a poor one.

Bryant: Sitting on a water vapor absorption line.

Purcell: Sitting on a water vapor absorption line which could only have been predicted if we had gone through.... Well, let me back up a little bit. We had been working on K-band, not with any particular wavelength in mind, but roughly with a change [by a factor of 2.5 in wavelength] from the X-band. The Columbia Radiation Laboratory had been making K-band magnetrons. Some of them would come out longer, some shorter wavelengths. The problem was to get enough power out to do something with. And then we had been promoting the development of K-band local oscillators, all the time realizing that when things got under control we wanted to settle on a common frequency for these components. I remember a meeting at a certain point at Columbia on this question. I went to the meeting as head of the group at the Radiation Lab. At the meeting were, in addition to myself, the two Montgomery's, Dorothy and Carol Montgomery, who had been working on K-band measurements in my group; also Kusch, and in particular Kellogg.

Bryant: Polykarp Kusch?

Purcell: Polykarp Kusch from the Columbia Radiation Lab, Jerry Kellogg, and two or three other people--their names don't come to mind. And it was at that meeting that we sort of settled down on 1.25 cm wavelength. That was where we had been hitting, it was a nice number to remember, and it made a quarter of a

wavelength half an inch. Then in later months it began to be clear that there was absorption.

Bryant: When was that actually realized?

Purcell: The way I remember it (but I don't trust my memory!) we had a breadboard radar running from the second floor of the roof building, at K-band. As we got new components to try, crystals and oscillators and so on, we would work them into this K-band breadboard set up there and see how far we could see, and how big the signal from the six-mile water tower was. My memory is that through the spring of 1943, probably, the individual components in our set-up were improved and improving as we expected them to, and as we had measured, but the range got worse. What was happening was that spring was coming to New England, and the water vapor was in the air; there was more water vapor in the air, it was increasing. I don't know just when it was that the actual identity of the absorption line was settled. If anyone bears the responsibility for choosing the wrong line without knowing about the water vapor, it's me and not Rabi. But Van Vleck, J.H. Van Vleck, was the physicist who knew enough of what had been done in infrared spectra to identify the two levels which were involved in this line.

Bryant: So he might possibly have predicted it theoretically?

Purcell: He could have predicted it theoretically, but it wasn't just a matter of the line. You had all the infrared energy levels and you had to find two that had the right gap and were not forbidden by selection rules. That was possible, and the spectrum existed; it had been measured by Dennison at Michigan just before the war. So, if someone had gone off, learned about the infrared spectra, and sat with

this thing to sort it out, he might have said, "Look you guys, there's this line over here that isn't measured." We knew we had something. We couldn't have predicted the wavelength exactly.

Bryant: At that point, the only effort with microwave molecular spectroscopy was the Cleeton and Williams work at Michigan in 1933 with ammonia.

Purcell: That's right.

Bryant: Their equipment was not good enough to resolve anything we're talking about.

Purcell: No, that's right. I remember that when Vic Neher got a K-band oscillator, he could fool around with that. Just for fun he looked at the ammonia resonance by putting some ammonia in a waveguide. The thing I remember is that he was so horrified by the extreme result. It took him a long time to get the ammonia out of the waveguide. I forgot what he would have used, but Guy Stever will remember that because he may have been the one that did it. Well, I wanted to bring that in because of the history of that water vapor line.

It was clear that, for other reasons, K-band was not going to be very useful for very long distance radar anyway. We didn't have the power, and so on. So the fact that you had a few db loss per kilometer even was not going to be a very serious handicap for what K-band might be good for. However, the military, used to radar at longer wavelengths where absorption could be ignored, understandably took it very seriously. In retrospect, a better choice than 1.25 cm wavelength would have been 1.8 cm, giving up a little bit of the short wavelength advantage. I think that it even has a name now, Ku-band, or something like that. This was when Van Vleck was extremely important. He was one of the few people who

were joint members of the Radiation Lab and the Radio Research Lab here at Harvard. Do you know about the Radio Research Lab? I myself had nothing directly to do with RRL.

Bryant: It was started officially in January of 1942; they hired Professor Terman. Just a few MIT Radiation Lab staff members went over there. RRL did most of their own recruiting.

Purcell: That's right. They were mostly engineers in contrast to physicists.

Bryant: Why would Van Vleck have joined RRL?

Purcell: Because he was a professor here at Harvard. He was well-known and close to physicists at both places. I don't know what his personal relation with Terman were, but at this point Van Vleck was one of the most distinguished physicists in the whole bunch. The arrangement that gave him an entree in both places could have been made by many people; I don't know exactly who made it. Most of us at the Radiation Lab had no contact with the RRL; in fact we were excluded from it on the theory that somehow the RRL stuff, because it concerned countermeasures, ought to be one level more secret.

Bryant: Could we talk a bit about the organization of the Radiation Lab? There was a steering committee and also a coordinating committee. The coordination committee apparently was considerably larger, referring to page 122 of this book.

Purcell: I remember the steering committee as a prominent set of people. I don't know what the coordinating committee was; I don't even remember the term. I was not on the steering committee.

Bryant: The list of the Coordination Committee members goes all the way to here, and your name is on it.

Purcell: Oh, yes. Some committee. I don't know what that was. Maybe that was to make all those guys feel they weren't left out.

Bryant: It must have included all group leaders at least.

Purcell: Yes, that's right. Uhlenbeck was the theory leader, and those would seem to be group leaders. Ernie Lyman, yeah. Well, group leaders are people that were involved in a particular project, key people perhaps. Otherwise, they were mostly group leaders.

Bryant: I have a specific question: To what extent did the flow of information fall in the channels defined by the formal organization of the Radiation Lab?

Purcell: I can't answer that. I don't know what an answer would mean.

Bryant: Did you have the feeling you had sufficient input on decision making?

Purcell: What would it mean to say yes or no to that? If I say no, it means that there were times when they asked me to make the wrong decision, if they only would have listened to me they would have, you know... I can't answer questions like that, I don't think questions like that mean anything.

Bryant: I'm pleased to hear you say that. You had the freedom to communicate as you found it proper to do your job, which I think is great.

Purcell: It's like those questions that the journalists are always asking now: What went through your mind when, you know.

Bryant: Of course. Another question: did you have much contact with customers, doing research, and being in the component business?

Purcell: Very little in our case, because we were so far removed from the actual application. Ours was sort of the breadboard set that got the first echoes on this new frequency. There was such a chain of development before you had anything that you could let anybody use. The only project we worked on with a military interest, in fact encouraged by a particular military guy, was, curiously, a mortar fire radar at K-band. This thing would detect an incoming mortar in its trajectory and extrapolate back to the source so that the counterfire could be aimed right at the source.

Bryant: We later called that counter-mortar radar, or something like that.

Purcell: Yes, counter-mortar radar. There was a guy at the Signal Corps, whose name might come to me as I talk (Captain Vollum), a young officer who came up to see us and he was interested in K-band and he was interested in this problem.

Bryant: K-band for resolution?

Purcell: Yes, and he came up to my group which was then concerned with working on it. We had just developed the most remarkable antenna called the Foster scanner. This captain in the Signal Corps was very smart; in fact he was the smartest guy from the Signal Corps we talked to. We were really impressed and he urged the group and got them going to work on this mortar, which you say is called counter-mortar fire. I don't think it ever got into Bell, but it was a good idea. It was a handsome thing and it produced a fan-like beam. There were two fan beams.

You could see a mortar at 2,000 yards with no problem at all.

Bryant: You could see the shell?

Purcell: You could see the shell coming at you.

Bryant: To track it would have required an analog computer.

Purcell: Sure, there would have been an analog computer, but we never got that far. We recorded the testing, but after the war I ran into somebody two or three years later, and I said: Whatever happened to that Signal Corps Captain. And he said, "Oh, he went out to Portland, Oregon, and he started a little company, Tektronix, making cathode ray oscilloscopes."

Bryant: Oh, Vollum I believe was his name.

Purcell: Captain Vollum, yes.

Bryant: Yes, he was founder and head of the company.

Purcell: Yes, that's right.

Bryant: What was the designation of your experimental radar--did it have an internal number?

Purcell: Oh, no, just K-band. It was just a K-band experimental radar as far as the Radiation Lab was concerned. See, that was my group, group 41 under Rabi. We had, as it were, a hunting license: if we had good idea, we could follow it up. I didn't have to fit it into a particular program for anything. That's why the radiometer came out of our group. Dicke was actually in my group when he

invented the radiometer. I contributed nothing at all. But the point is that under Rabi we had protection to work on a good idea, even if it wasn't obvious what it was good for.

Bryant: In what radio frequency band did Dicke do his first radiometer experiments?

Purcell: K-band.

Bryant: Oh, I see. He had a fair amount of bandwidth.

Purcell: Yes, he did, at K-band.

Bryant: What were his first targets?

Purcell: Regular targets that were here. The thing in Dicke's lab that we took people to see was his little box, which would detect thermal radiation at K-band.

Bryant: Soldering irons and the like?

Purcell: Well, cigarettes, typically. In fact, Dicke told this story on himself. He said that one day Rabi came in with a cigar, and the cigar at the end of the waveguide made the meter read. I told Dicke afterward that I didn't think it was Rabi because he didn't smoke cigars. George Uhlenbeck's cigar was the first one: it was K-band radiation. But that was a great thing. Here you were physically detecting black-body radiation from that object there.

Bryant: What was his receiver? Just a crystal detector and an amplifier?

Purcell: Yes, that's right. But it was lock-in amplifier. The heart of it was the waveguide in which this little rotating disk dipped, and as it spun, the waveguide was either

clear or was terminated by the temperature of the little disk. So anything at a different temperature gave you a modulation at the frequency it carries. So it was the first; it was a simple application of what we came to know as a lock-in amplifier. It was Dicke's understanding of the lock-in amplifier that made it possible for him to invent this thing. The first thing we did with it, as Dicke himself explained in his talk, we applied it to the K-band water vapor absorption problem.

Bryant: I missed his talk.

Purcell: Yes, the famous picture, what we called the shaggy dog picture, it's in there. It was part of the work that was done within my group. It also was done under the umbrella of Rabi's division.

Bryant: Did some of the experiments lead toward nuclear magnetic resonance experiments, NMR?

Purcell: Well, the genesis of the NMR involves that, certainly. In my own mind I can see how I got the NMR idea from various things that were happening, including the water vapor problem. There's a very strong intellectual thread running back that way for all of us in the NMR, not merely myself but Pound and Torrey, and Hansen and Packard and Bloch, and others at Stanford University. Bloch had been at RRL. I hadn't met him, actually. He was back at Stanford when he did the experiment.

Bryant: That would have been fairly late in the year?

Purcell: Well, the first NMR signal obtained by Pound, Torrey and myself, was in

December 1945. Just before Christmas, 1945.

Bryant: In what laboratory?

Purcell: Here-- in that red shed that sticks out from the building down there. The magnet's no longer there (we don't know what happened to the magnet), but the shed is still there.

Bryant: Does the experimental apparatus exist in some archive?

Purcell: The cavity is in the Smithsonian, we think; at least we sent it to them. The other thing in my life that came out, that is firmly rooted in the Radiation Lab, is the discovery of the 21 centimeter radiation in the galaxy. The 21 centimeter line.

Bryant: When was that first realized?

Purcell: In the spring of 1951. The antenna was just at the end of the hall, sticking out that window back in that room. Our information processing consisted of taking the ---
- tape and running it down this hall out here and looking down it lengthwise to look for a bump.

Bryant: How does the thread, the genesis of NMR go back to the Radiation Laboratory?

Purcell: Again, it was a case of going after a very weak signal by means that were optimized to detect a weak signal. We had learned at Radiation Lab how to do it. At the time we did the experiment, all of us were employees of the Radiation Lab, not of Harvard University. In the initial publication [of the 28-volume Radiation Laboratory Series] we are listed as Radiation Lab MIT, not Harvard.

Bryant: You were still on the payroll?

Purcell: I was still on the payroll because we were writing the books. Pound and Torrey and I were working on books. The experiments were physically done at Harvard because it was the only place I could borrow a magnet big enough.

Bryant: Were there still laboratory facilities at Radiation Lab?

Purcell: No, there were none at that time. We were supposedly writing books, which we were doing, but a little slowly. By the way, on the book thing, several times during this week, people referred to the series, the Rad Lab series. I wondered if you people have dug out the actual number of those books sold over the years. Wouldn't that be worth doing?

Bryant: Not that I know of. It would be interesting, wouldn't it? There have been several publishers [of reprints]. It's rather a complex thing.

Purcell: Well, just take McGraw-Hill. You could start with McGraw-Hill.

Bryant: Yes, McGraw-Hill published the first series. Then there was a reprint; some company here in Massachusetts reprinted the whole thing in paperback. Now I see that IEEE has them on sale again, so that's at least the third publication.

Purcell: It would be interesting for one thing to see the turnover for individual books, and for the different fields. They ought to be informative to the present generation of people. There are some books in the series that are as obsolete as the slide rule; in particular, the one on computing by little linkages by Svoboda. There may be some others, but some of them I know aren't obsolete. People have tried to buy

them.

Bryant: Tried to buy your own copy of your book?

Purcell: My copy of my own book, yes.

Bryant: I was happy to see them on sale again. They seem to be doing a brisk business.

Purcell: It would just be interesting to know. Furthermore, you know, the large share of the credit for pushing that project, conceiving of it in general, goes to Rabi. In any Radiation Lab history, that should be a very prominent fact. Ridenour became the editor to organize the whole thing. Rabi, I think, had the main idea of what we should do--that we really must get it done. Rabi was the guy who helped us keep our eye on the ball. He would say what we should really be doing at this or that point: we had a war to win.

Bryant: I gather he was quite accessible to talk with.

Purcell: Oh, yes, sure.

Bryant: Did he, on his own initiative, go around to work areas?

Purcell: No, he was interested to see but he didn't look over your shoulder at all. Some things were boring. Under his wing also came the propagation group, group 43 or 42 I have forgotten what the number was; never mind. Rabi found what they were doing very boring.

Bryant: Don Kerr and others.

Purcell: I had to take it over for awhile when Don was in the Pacific, and I found it boring

also.

Bryant: Was there any outside test range work?

Purcell: Oh, yes, they did a lot of stuff like that. People then were greatly concerned with anomalous propagation, trapping of radar waves over the ocean, and they did lots of work on that.

Bryant: Who did radar cross-section measurements there?

Purcell: Well, nobody did anything very systematically on that.

Bryant: What else can you say about Rabi's contribution, the pros and cons? There must have been conflicts as well as successes.

Purcell: Oh, well, sure there were conflicts. Yes, Radiation Lab was full of bright guys who had a strong belief in their own opinions.

Bryant: Well, the three directors were ...

Purcell: Well, the directors were terrific. Rabi, Wheeler Loomis, and of course, DuBridge, are the three. I was closer to Rabi than to the others, but Wheeler Loomis was a marvelous, tremendously important guy. He and DuBridge just had a wonderful understanding, a mutual understanding of what each was going to take care of.

Bryant: Who would make a decision?

Purcell: Wheeler's job was the organization of the Lab and dealing with the people; getting good people and helping them. Wheeler was just the guy you would

dream of having as chairman of your department, because he would go to bat for you when you needed him. And he would keep out of your hair when you didn't need him. He was superb to everyone. Most of the physicists, when they went back to their universities, wished they could take Wheeler Loomis with them. Wheeler himself went back to Illinois. But while he was at the Rad Lab he did not take part in the technical decisions, although he was a perfectly good physicist and understood the situations: that was all under DuBridge. The British had it turned the other way around: the director of the TRE lab, the top man, was the bureaucrat and under him came the true scientific technical director. Whereas we had it the other way around. Well, many other people were involved--wise people such as Lee Haworth and the steering committee. So I think the steering committee was about right.

Bryant: This has been most interesting, Professor Purcell. I enjoyed talking with you.

Purcell: It's hard to cover such a big subject.

Bryant: It was a great opportunity.

Purcell: There are so many aspects of it, both human and technical.