Oral History Transcript — Dr. Edward Purcell

Interview with Dr. Edward Purcell
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Cambridge, Massachusetts
June 8, 1977

Edward Purcell on putting together an apparatus for the first nuclear magnetic resonance experiments

Transcript

Session I | Session II

Sopka:
Professor Purcell, I'd like to begin by asking you to tell me something about your early childhood. I understand you were born in Taylorville, Illinois, but I must confess I don't know exactly where that is.

Purcell:
I grew up in two small towns in Illinois. Taylorville was the first one, and it's down about in the middle of the state a little bit south of Decatur. When I was about 15, I guess it was, we moved to Mattoon, which is a town some 60 miles to the east, south of Urbana. Those towns were my home until I was through college and moved away. I had a childhood that was typical of those times and that place, a very happy childhood as I look back on it. My father was a businessman, manager of the local telephone company. This was an independent telephone company, not part of the Bell system. It survives today, as do many of those small, independent companies. He was manager of the telephone exchange in Taylorville and then later in Mattoon was general manager of the Illinois Southeastern Telephone Company, which had the telephone business in three or four counties in that part of Illinois, a handful of towns with a population of a few thousand to 10,000 each. My father had grown up on a farm near Taylorville and his own education was extremely limited, although it included a spell in teaching a one-room country school, so that he had an interest in and a respect for teaching. My mother, on the other hand, had been rather highly educated. She was a graduate of Vassar and indeed had an M.A. from Vassar in classics. She'd grown up in Decatur, which was always for me the big city of my childhood, and my mother had been a high school teacher in Taylorville, teaching Latin, at the time she and my father were married around 1910.

Sopka:
Were you their first child?

Purcell:
Yes, I was the first child, and I have one brother five years younger also born in Taylorville before we moved away. I had many childhood friends in our neighborhood. It was a rather homogeneous neighborhood. Taylorville itself was a farming town in the middle of a farming community, but also a coal mining town. There were extensive soft coal mines in that vicinity, still busy I believe; and we were familiar with all the sociology of a small mining town, including, as I remember, the early years of the growth of the unions in the mines. I had no interest that you would identify as scientific when I was in Taylorville, being young. However, the fact that my father was working for the telephone company had already begun to have some influence. The telephone office, as we called it, was a place that had a back room where all the switchboards and technical equipment was. In the basement there were discarded sections of cable and wire, and I could bring home items like that from the telephone office. It was my source of wire and of lead, because if you got an old hunk of telephone cable, you could melt the lead sheath and then take the paper insulation off the wire, and you had the makings for a lot of things. We moved to Mattoon, which is a town of about the same size, perhaps a little bigger than Taylorville. I had my last two years of high school in Mattoon. There I had a particular close friend, a boy of my own age, who was interested in chemistry and did a lot of home chemistry experimenting. And I think it was our joint activity and friendship then that probably further stimulated my interest in some kind of science or engineering.

Sopka:
Did you read much as a child? Your mother's background might have encouraged you to.

Purcell:
Yes. Ours was a house that had many books in it and I did read as a child. My reading wasn't vast, but ...

Sopka:
At least a suitable activity for a summer afternoon.

Purcell:
Yes, literature and books — children's books and children's classics and so on — were a familiar part of my childhood, and also I spent a good deal of time in the public library.

Sopka:
Were the churches of these towns — Taylorville and Mattoon -of considerable social influence in the communities?

Purcell:

Yes, yes. (I should have mentioned that.) My mother and father were Presbyterians, and I attended Sunday School at the Presbyterian Church and indeed spent a good deal of time singing in a boys' choir in the Presbyterian Church, which is probably where I heard more sermons than I've heard in total since. It was a town with a number of churches. A town of 10,000 in rural Illinois at those times would have a half a dozen churches of all the well-known denominations. I don't remember any great intellectual concern for religious questions until we moved to Mattoon, where, as it happened, this friend of mine, whose name was Dunlap McNair, whom I have mentioned, attended ... Well, his father and his uncle were rather strong-minded, church-oriented people. Indeed his uncle was ordained, although he didn't have a pulpit. Dunlap and I found ourselves in sharp conflict with the theological views of his father and uncle, and we began reading some Unitarian tracts which greatly attracted us. In fact, I think if there had been a Unitarian church in Mattoon, we might have found a place we could go. As it was, we simply had arguments which I left us disgusted with the whole proposition. And I guess although I'm perhaps still carried on the rolls of the Presbyterian Church in Mattoon, I really have not considered myself an active church member since my bitter conflicts, intellectual conflicts, with Uncle Irving.

Sopka:

The school that you went to in Taylorville, was that a regular eighth grade set-up where you then moved on into a four-year high school?

Purcell:

Yes, actually in Taylorville we had six plus two plus four. I moved after the sixth grade into what we called a junior high, but it was essentially the same as an eight-year elementary school and then four-year high school.

Sopka:

Was there any difference in the offerings between what you would have had available if you'd stayed in Taylorville in contrast to going to Mattoon?

Purcell:

No, I think there was no difference of any consequence. In Mattoon I had high school chemistry and high school physics, both of which were rather important to me or seem so in retrospect. The chemistry teacher, a man, was very good; and really it was the first time I'd encountered any grownup who was a real scientist, if I can use that term. Mr. Thomkins really knew some chemistry. He was seriously interested in what he was teaching. In fact, it was a better chemistry course in some ways than my subsequent chemistry course at Purdue. In physics the teacher was a woman, Miss Edwards...

Sopka:

That's surprising in a sense — although maybe not.

Purcell:

...who didn't know a great deal of physics but... She was a person who understood her own limitations and respected the subject and was utterly honest and sincere, so that in fact I think she introduced me to physics in a humane way that probably was important. One anecdote of those times that I sometimes tell in her memory was: we were using Black and Davis's text, which was widely used in high school physics in those days. Much later in my life I worked as a teaching assistant under N.H. Black, but in those days Black and Davis was the standard high school physics text. It had the famous problem of the man who pulls himself up the flagpole by sitting on a seat from which a rope runs up and over a pulley at the top of the pole and back down. He pulls on the rope and up he goes. And, of course, the question is: how hard does he have to pull? And Dunlap, my friend, and I thought we had figured out the answer and it was that he would have to pull with half his weight. But Miss Edwards said, "No, it says in the book that a fixed pulley has no mechanical advantage, and there's no question that this pulley was fixed at the top of the mast. So the answer must be that he pulls with his whole weight." Well, we couldn't accept this, and as it happened, we had a barn and Dunlap had a scale for weighing ice which would weigh up to 100 or 150 pounds — I forget which.

So we went into the barn after school and rigged this thing up with a seat and hooked the spring scales to the upgoing rope and then pulled on the downcoming rope. I still remember exactly how much I weighed at the time, because I started off sitting on the seat, and when the scales read 60 pounds I started to go up; I weighed 120. And then Dunlap got in and tried it and we ran all the way to high school to announce our triumph, and when we got there Miss Edwards was still up in her room grading papers, where you were likely to find a conscientious teacher in those days. And so we told her all about it. Her response was instantly: "Well, I must have been wrong and you must be right because you did an experiment and proved it," which I thought was a gallant response and exactly the right one. I've always felt that she did me a real service at that moment. Well, at any rate, Dunlap was the avid chemical experimenter. We tried some other things. I remember we tried making a galvanometer and a few things. They were not particularly successful. We tried a Tesla Coil. I think it worked. Again, I had a source of some material at the telephone company. You could always get plenty of the bell-ringing generators that were in the old telephones, which consisted of a series of horseshoe magnets making the stator field and an armature that was wound with what must have been a mile of number 39 wire or something like that.

These made good shocking machines if nothing else. I also did a few odd jobs at the telephone company, including helping to make maps of their distribution of lines in the city, and things like that of kind of engineering nature. One of the important contributions that came to me by way of the telephone company at that time was the BELL SYSTEM TECHNICAL JOURNAL. Although the Illinois Southeastern was not part of the Bell system, they nevertheless did all of the maintenance on the AT & T lines that went through their area, and they regularly received the BELL SYSTEM TECHNICAL JOURNAL. It was not avidly read in that company. There were practical wire men and so on, but there were no people who were interested in articles in the BELL JOURNAL. So I could take those things home and read them. They were fascinating because for the first time I saw technical articles obviously elegantly edited and prepared and illustrated, full of mathematics that was well beyond my understanding. It was a glimpse into some kind of wonderful world where electricity and mathematics and engineering and nice diagrams all came together. Just a little later (I'm not quite sure of the dates of this) the BELL SYSTEM JOURNAL began publishing Karl Darrow's articles on modern physics.

Sopka:

They, I believe, would have begun while you were still in high school.

Purcell:

Well, then I began reading them. That is, I don't think I appreciated them so much until I had been in college and came back to them, since we had them, you know — I kept them.

Sopka:

You would have been in high school around 1926.

Purcell:

I graduated from high school in ‘29.

Sopka:

Karl Darrow was definitely publishing his articles by then.

Purcell:

Yes. I believe that... I think the first thing that really fascinated me about the Bell System's journal as a high school boy was not Darrow's articles yet, although they may have been there, but they didn't make the impression that they did later, but just the technical articles — I mean the wonderful, beautiful diagrams... The BELL SYSTEM JOURNAL was a beautifully edited journal then and it is now. You know, the diagrams were superb and everything was really just right. I had those copies of the Darrow articles for a long time. I had them when I was at Purdue.

Sopka:
While you were in high school did you go into extracurricular sports or drama or any such activity, or were you pretty much oriented towards scientific things?

Purcell:
Well, we played tennis. I played tennis rather seriously in high school — not seriously in the modern way of having teams and so on, but my particular friends began playing tennis in the park. It had rather poor facilities. And we had swimming. I went to summer camp, a small YMCA summer camp, when I was in Taylorville and also in Mattoon. They were just the summer sports of a small town. We played baseball a good deal, didn't play much football. I was a Boy Scout (only in Taylorville) but never got to be more than a Tenderfoot because the main activity of our scout troop was playing basketball. We had a basketball team but we didn't ever do much scouting. That was a very nice time to be growing up in a small town in the Midwest.

Sopka:
By the time that you were ready to go to college, your decision to go to Purdue and major in electrical engineering was not inconsistent with your high school experience.

Purcell:
No, it wasn't. It came naturally out of that. I don't remember anymore exactly why I didn't go to the University of Illinois, which being only 40 miles away was of course the great institution in our eyes. But it was the idea of being an engineer, and Purdue was smaller — of course much smaller than the University of Illinois then. When I went to Purdue it had only 4500 students. My friend Dunlap also wanted to be an engineer, a chemical engineer, and he went to a very tiny but a surprisingly good engineering school called Rose Polytechnic in Terre Haute, which is rather near Mattoon. It's just really across the border in Indiana.

Sopka:
But at the time you enrolled at Purdue, was it a general liberal arts type curriculum, or was it definitely oriented more towards engineering?

Purcell:
Well, Purdue was part of the state university system in Indiana, and it was the engineering and agriculture and home economics part of the state university, the other of which was the University of Indiana at Bloomington. So if you wanted to study liberal arts in Indiana, you'd go to Bloomington. Purdue was an engineering school: engineering, agriculture, and home ec for girls.

Sopka:
Were there any problems with regard to your paying tuition in Indiana that you might not have had to pay if you were in Illinois?

Purcell:
Well, it was very small. Goodness, I don't remember — maybe $50 a year that you had to pay. As an out-of-state student you had to pay a little more, but even in those days the difference was not very great. No, that would have mattered. I mean my family could not have afforded easily to send me to an expensive eastern school, for instance. That was not really considered. But I thought I wanted to be an electrical engineer, and the idea of being a physicist at that point just wasn't an image that one had to consider somehow. You see, in the '20s the idea of chemistry as a science was extremely well publicized and popular, so the young scientist of shall we say 1928 — you'd think of him as a chemist holding up his test tube and sighting through it or something. And that was the result of the experience and history of World War I where the United States had to develop its chemical industry practically from scratch because German industry had previously supplied all that.

Sopka:
Did you by any chance during this period come in contact with the book by Slosson called CREATIVE CHEMISTRY?

Purcell:
Oh, yes. Everybody read that.

Sopka:
Several people have mentioned that as being very influential.

Purcell:
Oh, sure, we both read it; and, you see, my friend Dunlap was right on that groove. No, E.E. Slosson's CREATIVE CHEMISTRY — sure, we read that, and it was all very exciting. I don't know why I didn't want to be a chemist, looking back on it. I found the chemistry course at Purdue somewhat interesting. But there was no idea of what it would mean to be a physicist. The subject, I knew about — but being a physicist... I don't remember considering that at that time as something you could be.

Sopka:
The choice was electrical engineering or chemistry.

Purcell:
Yes, sort of like that, yes. In fact, I've sometimes remarked that to me at that time the name Steinmetz was more familiar and exciting than the name Einstein, because Steinmetz was the famous electrical engineer at General Electric and was this hunchback with a cigar who was said to know the four-place logarithm table by heart and all that kind of thing. So it was only gradually as a student at Purdue that I began to learn not only what physics is but what it would be to be a physicist. So I went to Purdue in the fall of 1929.

Sopka:
That was the initial year of the Depression.

Purcell:
Yes, and you know, looking back and trying to remember my own view of my surroundings at that time, I really ... the Depression was never really very much in the forefront of my consciousness.

Sopka:
Presumably your own father's job was stable.

Purcell:
Well, my father's job was stable — that's true — and he hadn't made any catastrophic investments or anything, but in those small towns in Illinois banks were failing all

around. In Taylorville most of the banks in the town failed. It was really a tragic time for a lot of people even in that part of our society, and yet I was so interested in what was going on in college and in learning things and all that that it just didn't make much of a dent on me.

Sopka:
Did you work while you were in college?

Purcell:
No, I had some odd summer jobs at the telephone company, things like that, but I didn't contribute anything really to my own support or tuition. There were very small scholarship awards to be earned at Purdue; if you made a high point grade average you got — I don't know — $25 refunded on your tuition or something.

Sopka:
You remained as an electrical engineering major throughout your undergraduate years?

Purcell:
Yes.

Sopka:
But you began to be aware of physics.

Purcell:
Yes. It was an interesting time at Purdue and in the history of Purdue science and Purdue physics especially, because in the years before 1929 when I came, physics at Purdue as physics was essentially negligible. The person in charge was a professor named Ferry, whose sole distinction consisted in having written a small book on gyroscopes. He was a lecturer of incomparable dullness. Lark-Horovitz came I think in 1929. I wasn't aware of his presence until the next year…

Sopka:
I wonder who was responsible for his coming. Did you ever learn the circumstances?

Purcell:
I don't know the circumstances. He was from Vienna, but a year or so before he'd been an NRC[1] fellow; he had some kind of fellowship I think at Chicago. There is some history of him and of Purdue that I hope someone … many, many people are competent to record that, and it certainly ought to be somewhere in the Institute files because his coming to Purdue was really quite important for American physics in many ways. It was he who subsequently over the years brought many important and productive European physicists to this country; they came to Purdue, passed through. And he began teaching; he began having graduate students and teaching really modern physics as of 1930, in his classes.

Sopka:
That was an exciting time.

Purcell:
Yes, I only began to learn about it as I went on in my third and fourth year at Purdue. In fact, my first course in physics at Purdue was extremely dull. It was given by Ferry and I can remember almost nothing about it. I did have a good section instructor — I suppose he was an assistant professor; he had just gotten his Ph.D. and I found him interesting because he clearly knew physics at a level that I hadn't even come in contact with.

Sopka:
Do you recall what text you used as a freshman at Purdue in physics?

Purcell:
Oh, I think we used Ferry. I believe Ferry had a text. A dull book, but with an enormous number of routine problems.

Sopka:
Were there even then before Lark-Horovitz's arrival a sufficient variety of physics courses to allow a major in physics at that time in Purdue?

Purcell:
No, you couldn't major in physics I believe. You see, Purdue had electrical, civil, mechanical and chemical engineering. It had something called the School of Science, and you could graduate, having majored in science. I was never quite clear on the requirements of that major, but it was definitely below the others in its intellectual demands. In fact, it was a sort of refuge for those who couldn't make it in engineering. I think there you took a number of different courses and probably were prepared to be a science teacher in the secondary schools or something like that.

Sopka:
But then Lark-Horovitz must have been given a fairly free hand to develop…

Purcell:
He had graduate students. He already had graduate students. That was the amazing thing. I don't know just how that sprang into being because the notion of a graduate student under Ferry was really… But there were a few other people who… Well, to come back to physics and my contacts. I was very happy with electrical engineering. I had a lot of fun and really found I could do it well and I really enjoyed the electrical engineering courses. But I guess it was when I was a junior I found in the catalog a course listed under the physics department called something like 'independent laboratory work.' I went around to see about it. No one had ever signed up for that course before, but they let me sign up for it, and my immediate supervisor was a professor named Walerstein to whom I owe a tremendous amount. He was a real physicist. I think the first job he gave me was up in the attic of the physics building where there was a Rowland grating and a mount that had not been used for a long, long time. I was supposed to set it up and get it going and adjust it and everything all on my own and examine some spectra. Then he had some other things for me to do up in the attic, including making an electrometer to measure the halflife of something, and that was just absolutely… I was really hooked at that point.

Sopka:
About how old a man was Walerstein at this point? Was he a young instructor?

Purcell:
He was youngish and he stayed at Purdue for decades thereafter. He must have retired by now. But I should think he was in his thirties at that time, and there were others there. Well, that was I guess my junior year. And then my senior year I wanted to keep on there, and I was allowed to work with H.J. Yearian who was not a professor; he was still technically a graduate student. He was finishing up his thesis. He was doing electron diffraction. He was already a tremendously experienced physicist. Why he was so long getting his Ph.D. was to be explained not at all by any lack of ability but by the way Lark-Horovitz was running things at that time. Yearian had built a

20-kilovolt electron diffraction camera, a Debye-Scherrer transmission camera. And he was going on to build another one and he let me work with the first model, and we began doing electron diffraction on various things — beryllium oxide among other things. And this introduced me to the whole world of the basement of the physics building where research in electron diffraction and x-ray diffraction was going on. These reflected Lark-Horovitz's own interests, and they were, of course, very much in the forefront of laboratory research at the time. People were also living down there in the cellar, sleeping on cots in the research rooms, because it was the Depression and some of the graduate students had nowhere else to live. I'd come in in the morning and find them shaving. Yearian was very nice to me, tolerated my mistakes and taught me a great deal. That was such an exciting time. I'd never really developed photographic emulsion before. Cameras had never been my hobby. I'd never done anything like that. And the first photographic emulsion I ever developed, when I turned on the light in the dark room, I had Debye-Scherrer rings on it from electron diffraction — and that was only five years after electron diffraction had been discovered. So it really was right in the forefront. And as just an undergraduate, to be able to do that at that time was fantastic.

Sopka:
Between satisfying your requirements as an electrical engineering major and your taking physics because of your interest, did you have any time to take courses while you were in college that were what we call general education courses?

Purcell:
Oh, yes. You know, I find people are often surprised to learn that at Purdue at that time you could take a lot of other courses. In fact, my most serious and demanding outside activity was connected with the English department. I had several courses in English; I had a course in European history, a course in economics. I had just in terms of the course subject, not perhaps in the quality of instruction, I had as much in the way of distribution courses as many graduates of Harvard College.

Sopka:
Did you have language training available?

Purcell:
I had two years of German at Purdue. I was very much involved with the literary magazine. In fact, I was editor of that for two years and that took a lot of time. In fact, aside from the people in physics, of whom Walerstein and Yearian were the ones I mentioned, the teacher who was the most influential and the most admired by me was in the English department, a professor named Paul Fatout. I was concerned with writing and I liked to try to write, and these people were interested in good writing. That took a lot of time.

Sopka:
Did you form any close friendships with any of your fellow students that would be the counterpart of your friendship with Dunlap McNair in your high school years?

Purcell:
Yes, I had many friends among my electrical engineering colleagues. All of us who enjoyed engineering enjoyed problems and things and arguments and jokes and the whole life of being an engineering student, and there were many like that. I might mention one in particular since he's not unconnected with the American Institute of Physics. Another person in my class of electrical engineering was J.G. Adashko. He was really a very close friend at that time. I haven't seen him for many years, but he has been the main translator of the Russian physics articles for the American Institute program for translations. He lives in New York. He was an electrical engineer in my class.

Sopka:
Was this very much of a masculine society in the engineering division at Purdue in those days?

Purcell:
Well, there were almost no girls among the engineers or perhaps one or two. I believe there was one girl in civil. And Purdue had coeds most of whom were in home economics and made up not more than 15% I should imagine of the student body, so there was an enormous … It was almost like the old-time Harvard-Radcliffe ratio. I lived in a fraternity at Purdue the last two years. I lived in one of the regular fraternities. In retrospect I don't think it was a good idea. I think fraternities are and were terrible and should have been abolished. But at Purdue then there weren't so many dormitories. I would have been actually happier if there had been the kind of dormitory life there was many years later. I read now that fraternities are coming back at some of these places and I think that's deplorable. But that didn't affect my intellectual life and my growth in engineering and science. It was kind of separate from that. Lark-Horovitz ran the physics department on the European style: a pyramid with the professor at the top and everybody down below taking orders and doing what the professor thought ought to be done. This made working for him rather difficult. I was insulated by one layer from that because it was people like Yearian, for whom I was working, who had to deal with the Lark. In fact, it was he who really helped me go on as a physicist by helping me get a scholarship that I had for a year after Purdue. So there's no question that I owe Lark-Horovitz a great deal. He was a remarkable man. You see, he came from a liberal Central European intellectual …

Sopka:
He was Jewish, too, wasn't he?

Purcell:
Yes.

Sopka:
There were probably not very many of them in the academic community at Purdue.

Purcell:
No, and the whole Middle West at that time was as full of anti-Semitism as any place you could find. Not only that, but he was politically liberal. And yet he managed to hold his own in that setting.

Sopka:
Was he married? Did he have a family to fit into the community as well as himself?

Purcell:
Yes. In fact, the hyphenated name — Lark-Horovitz, Lark was the name of his wife. That is, when they got married, they hyphenated their name. His name was Horovitz before he was married. And Betty Lark — she was a remarkable person herself. They loved music and arts and they were sort of a little island of Middle European culture there in central Indiana.

Sopka:
About how old were they when they came? Had he recently finished his own doctoral work in Vienna?

Purcell:

He'd been a post-doc at Chicago, as I recall.

Sopka:
Was he under 30, would you guess?

Purcell:
Well, no, he must have been at least 30. Of course, everyone seemed old to me then. Thirty now seems ridiculously young.

Sopka:
In order to have stepped in and become the top of the pyramid, he must have had some seniority.

Purcell:
He must have been in his thirties already. I have a marvelous picture of him complete with spats taken at that time. Imagine wearing spats at Purdue. His own scientific interests more and more led him in the direction of what we now call solid state physics. He was interested generally in crystals, the physics of crystals, and that sort of thing; and of course later on he developed the really very well known and very important solid state research laboratory that did work on semi-conductors during World War II. The trouble with the Lark was he was very slow to write things up and publish them. I think perhaps he didn't get as much credit as he might have for his work on germanium in the middle '40s prior to the transistor.

Sopka:
You mentioned then that he was instrumental in helping you to get the opportunity for the year you spent at the Technische Hochschule at Karlsruhe.

Purcell:
Yes, I'm sure that his recommendation was what made that go through. You see, actually my first published paper was on work I did at Purdue as a senior. If you were doing electron diffraction, part of the game was to get a film of the material thin enough to be clearly transparent to electrons; and nowadays it's not much of a problem. But Lark-Horovitz had an idea about how to do that, which he put me to work to try to carry out, which was to evaporate the material that you wanted to study onto a substrate which was of some material which would sublime, evaporating without turning into a liquid in the vacuum, because ordinarily if you evaporated onto anything, the surface tension when it melted or anything would tear a film. So I spent a long time figuring a way to evaporate copper onto napthalene or mothballs. What we did was to make a slit, fill it with napthalene, and polish the base of it, evaporate copper, and then put it into a vacuum, keeping it very cold all that time. And then after you got it into the electron diffraction camera, you let the stuff evaporate into the vacuum and the mothballs just sublimed, leaving the film in pretty good shape. So there was a little paper on that in RSI, [2] of which I am one of the co-authors. I think that it turned out, in the end, that what we had was copper oxide rather than copper. Somewhere along in this process it had, gotten oxidized.

Sopka:
So by the time you had graduated was your leaning definitely toward a physics career?

Purcell:
Yes, I decided to go into physics. It was, of course, the Depression; and whether I could have gotten a job as an electrical engineer anyway is doubtful. I don't remember that being the overriding reason somehow. I can't remember worrying about it. By then I really wanted to go on in physics, and I'd been really fired up by those experiences.

Sopka:
Under what program was the fellowship?

Purcell:
The Institute of International Education ran the exchange fellowships then and does again now. They ran them in different countries, but the German program was a particularly active one., German students came to this country for a year, and American students were sent to Germany for a year.

Sopka:
Was that inaugurated not only for the academic exchange but also for the cultural exchange in terms of say binding up the wounds after World War I kind of a program?

Purcell:
I honestly don't know. I've seen something about the history of it lately. I think the thing got started about 1925 or '27 maybe — I'm not sure — but it hadn't been going many years.

Sopka:
I know that when Arnold Sommerfeld came to this country he said that he was coming more to try and be an ambassador of good will as much as to share the physics... He felt very strongly about the anti-German feelings that had come into being.

Purcell:
Yes, but that was a little earlier, wasn't it?

Sopka:
Yes, that was 1923 or '24 he was here.

Purcell:
That's right. I don't know where the money came from for the Institute. Not only Germany was in this program — I think France was, too, and others. But we had had just in my last year at Purdue a German student in engineering, and I had gotten acquainted with him somewhat — as it turned out, a man with a very remarkable future. His name was Fritz Leonhardt. Well, Fritz Leonhardt was an exchange student. He was a young German civil engineer, structural engineer, who then went back to Germany; and then when I was in Germany as an exchange student the following year I visited him. He later became the leading proponent and innovator in Germany of prestressed concrete construction and built some spectacular things. Then much later in life he became I think Rector of the Technische Hochschule at Stuttgart, among other things. But his home was in Stuttgart. He had been at Purdue as an exchange student on this international exchange the year before, so I knew him, met him at Purdue, and then I looked him up when I was in Germany after he'd gone back and was in Stuttgart again in '33.

Sopka:
About what kind of a budget were you allowed for that year in Europe?

Purcell:
Oh, very slight. I had free tuition and I had free board and room at the Studentenhaus. And that was it more or less. I may have had some small stipend in addition but

very, very small. Things were very cheap at that time.

Sopka:
And presumably your travel costs ...

Purcell:
It cost me, or cost my family, a total of $700 for the whole year, including travel. No, it was rather an austere life. I didn't have enough money to eat meals except the free meals at the student mensa.

Sopka:
Was this a hardship? Was the change of diet appreciable?

Purcell:
Well, no. We survived perfectly well. It was just not luxurious at all. It was of course a very bad time to go to Germany, the beginning of very bad times. Hitler had already come into power a few months before. So through the year I had very few friends among the German students, who were already most of them caught up in the movement. My friends there were from other European countries who had come to study engineering.

Sopka:
So were you still studying engineering rather than physics?

Purcell:
No, I was studying physics. You could study physics there. And there was one real professor, Walter Witzel, a young vigorous, excellent teacher. His own interest was in band spectra. In fact, I think he had just written the volume on band spectrum in the HANDBUCH DER EXPERIMENTALPHYSIK. And he, however, had been kicked out by the Nazis just before I got there because he'd been a socialist or communist as a student once or something, and for the first semester he wasn't allowed to teach. But he was allowed to come back in the second semester, and I had then some lecture courses from him, graduate courses. He was a wonderful teacher at that level. He went to Bonn a few years later where he served out his entire academic career. In fact, I've seen him and visited him in Bonn after the war. But it was not a very good place to go. I had no choice. I would have much preferred to go to Munich where Sommerfeld was teaching and where many other professors were.

Sopka:
Were you assigned?

Purcell:
I was assigned, yes. I had no choice.

Sopka:
Were your two years of German at Purdue adequate for you to get along both socially and academically in classes or was it a struggle?

Purcell:
Well, it was a struggle. That was part of the fun really. Some of the exchange students spent a month in Hamburg early in the fall just studying language, which helped in many ways, and I enjoy foreign languages, and in fact during the semester that Witzel was not teaching we used to go around and see him at his apartment. But I took all kinds of courses then in other subjects; because I had free tuition, I could take anything I wanted. So I took several courses for the purpose of hearing that professor speak German if he was a particularly elegant lecturer. There was an historian at Karlsruhe named Schnabel, a marvelous scholar, whose lectures were such beautiful lectures. I took his lecture course and I took a seminar with him. I went to a course in the history of art, standard type lantern slide history of art course; and I took a course in physical chemistry.

Sopka:
In general did you find the European academic atmosphere more sophisticated than what you had been exposed to at Purdue?

Purcell:
Yes, they were more sophisticated in many ways. The student life, of course, was quite different. As is well known, they didn't have exams as we did — regular, frequent exams. And I also found that my colleagues in the graduate course — you might call it sort of a first-year graduate level course in physics — these people hadn't had much practice working problems, whereas of course I'd been doing nothing but working problems for four years. So although my mathematical preparation was not as good as theirs and my physics in some ways not as good, I was better at working problems. My mathematical preparation I might say was still rather weak and was to remain so throughout the rest of my life. That was one respect in which I came out of Purdue not too well prepared. Well, I had calculus and differential equations in the engineering style, but I didn't really find out what mathematics really is until I came to Harvard.

Sopka:
I guess if you were on such a limited budget, you didn't have much chance to travel in Europe and with the political atmosphere in Germany...

Purcell:
Well, I traveled in Germany because it was very cheap. You see, if you were a registered student in a university in Germany, you could travel, get all kinds of reductions in your travel costs. And we, of course, exploited that very much. No, I traveled a fair amount and did some hiking and things of that sort. I went into Austria all alone to ski at the time Germans were forbidden to go there. But it was a grim time for Europe because the grisly Nazi era was right ahead and beginning to be obvious.

Sopka:
I believe you told me one time that you met your future wife during this year.

Purcell:
Yes, yes, that's right. She was also on an exchange scholarship. She was sent to Munich. Her field was German literature.

Sopka:
Where had she gone to college?

Purcell:
Bryn Mawr. We met going over on the boat and then at Hamburg that time, and I visited her in Munich. She went around to hear Sommerfeld lecture in Munich. I said she should go hear him and at least listen to Sommerfeld. I remember going to a German physical society meeting. That just came back to me. I hadn't thought of it for a long time. It was in what must have been the early spring of '34. It seems to me this meeting was in Freiburg. And everybody was sitting around and laughing about the neutrino idea. I remember sitting around a table drinking beer and the senior physicists joking about the neutrino.

Sopka:
When your year in Europe came to an end, did you go back home or did you come directly to Harvard?

Purcell:
Well, I had to get a scholarship of course before I left Germany. So I wrote letters and again I think Lark-Horovitz helped me there and wrote to two or three different places for a graduate scholarship and got a reply from Harvard that said (I forget what they called it) ... but it was just free tuition. I think it was $400.

Sopka:
That's what it was in those days.

Purcell:
That's what it was, and you never saw the $400. It was just canceled out against your tuition. That wasn't a terribly munificent scholarship, but that was the best one and I decided to take that. I had to consult my parents and see if they thought they could swing the rest of it. So before I left Europe I knew where I was going to be the next year. I was coming to Harvard.

Sopka:
When you actually arrived in Cambridge did you go into one of the graduate houses here on Oxford Street?

Purcell:
I lived in Perkins the first year up on Oxford Street-and Conant the next two years.

Sopka:
Presumably you had to pay for room and board there from your own pocket.

Purcell:
Yes, that's right. I had to pay the Perkins room out of my own pocket.

Sopka:
There wasn't any proctorship or something like that that you could earn your way?

Purcell:
No, I guess there were such things, but I didn't have one.

Sopka:
Probably not for a first-year graduate student anyway. It might have come later.

Purcell:
No. So I was a full-time student, didn't do any teaching until another couple of years... Well, the Harvard graduate student in those days up in this part of the University really was quite disconnected from the life of the college and everything that went on in the houses and the yard and everything. I mean that almost was nonexistent as far as we were concerned. We were working very hard and working up in this vicinity, eating in restaurants up and down Mass. Avenue.

Sopka:
You mentioned before that when you came to Harvard you learned mathematics that you hadn't been exposed to before. Would you want to comment on what courses and faculty members stand out in your memory?

Purcell:
Well, the great thing that stands out in my memory was Math 13, as it was called then: complex variable theory.

Sopka:
Who taught it that year?

Purcell:
David Widder. I took it my first year here as a graduate student, and it was a tremendous experience. It was really one of the intellectual experiences of my life. It was also very nearly my Waterloo, because I could easily have flunked out. But I had never seen mathematics, a course from a mathematician's point of view. The whole idea of what a proof really is was totally new to me. All this business with epsilon and delta, you know, and those arguments: an absolutely new idea. And I worked very hard. It also showed me almost immediately that I was certainly not cut out to be a mathematician because in that class there were graduate students in mathematics who were clearly operating on a very different plane from mine. I went to the exam the first semester, the final exam, in January, and I had worked tremendously hard. The night before, 1 had proved every theorem in the course; and I went to the exam and simply froze. I couldn't do anything. I couldn't solve a quadratic equation.

Sopka:
I know exactly how you felt. That's happened to me, too.

Purcell:
I remember exactly where I was sitting over in the geology lecture room. I can almost pick out the seat over there but they've probably got new seats in the room by now. And I sat there for three hours and I couldn't do anything, just miserable little scribbles. It's never happened to me since, but it's always made me sympathetic to students who come around and say, "Gosh, I couldn't do anything on that exam."

Sopka:
How did you manage to salvage that experience?

Purcell:
Well, in those days it was a full course, you see, and your grade for the first semester went in in pencil, and then your final grade. So my first semester grade was I think C-, which was as low a flunking as you could get for a graduate course in those days. But I had done all the problems and everything. I had a good record otherwise, and in the second semester I just went at it again and pulled it out. I don't think I got an "A" but I at least... Stayed respectable. But quite apart from that, just the whole depth and vision of that mathematics as seen from classical analysis, which is of course a beautiful subject in its self-consistency and completeness. On the other hand, there are still many parts of modern mathematics that I didn't get all along the way. Modern algebra: I never really had anything in that, which of course handicaps one in a way one is particularly conscious of in recent years.

Sopka:
Math 13 then was the only course you took outside of the physics department?

Purcell:
Right, yes. Well, the only mathematics course, yes. I audited a course in cosmology by A.N. Whitehead, who was still here then.

Sopka:
Oh, he was around at that time?

Purcell:
Yes, a beautiful gentlemen. I didn't get very much out of it. I did some of the reading.

Sopka:
Under which department was he?

Purcell:
Philosophy.

Sopka:
But then within the physics department itself you must have taken some courses.

Purcell:
Oh, yes, I took lots of courses ... Well, I took courses with Kemble. That first year I guess I had electricity and magnetism, classical E & M, with Kemble; 32 I believe was its number.

Sopka:
Yes, there was a whole sequence of courses in the '30s — in the 1930s and the sequence was labeled the 30s.

Purcell:
And then Kemble's quantum mechanics. And I had a course with Furry — I don't know what, I can't remember exactly what that was called.

Sopka:
Introduction to Mathematical Physics or something like that?

Purcell:
Well, something like that was given, but I think I did not take that. I'm not sure anymore. I've forgotten. Oldenberg's lab course I remember.

Sopka:
Did you take that?

Purcell:
I took that.

Sopka:
That was a nice one.

Purcell:
That was a nice course. In fact, for me again that course was something so new and lovely in that we didn't have to write lab reports, and here at Purdue, God, as an engineering student, I had to write lab reports all the time. And I don't like lab reports, and here was a course ... I remember Oldenberg's practice was: you just put the result down on a little card, a 3" by 5" filing card, and you wrote down the result of your experiment on it. As a physicist, you see, I've never kept a laboratory notebook. I don't think I've ever had one.

Sopka:
So you won't have to worry about which archives will get your ... laboratory notebooks.

Purcell:
No, I have nothing for the archives.

Sopka:
When did you begin to do individual research that would lead to a doctoral thesis?

Purcell:
Well, I can't remember how soon it began chronologically, but I remember the subject involved. Professor Chaffee gave me a thesis problem, and I began with the idea of doing a thesis under him. And not only did he give me a problem, but I had a room, a room actually right below this, to start work in; and I started building apparatus. I had the whole room to myself. Down there now there are six fellows in that room. And then the problem he gave me was an excellent problem and would have been a tremendously fruitful thing, but I talked myself out of it and talked him out of it. Chaffee had the idea that it would be interesting to look at a gas discharge in a magnetic field and then put in a couple of condenser plates in the gas discharge and measure the AC impedence of those plates as a function of frequency. And if you did that, wouldn't you see some kind of an anomaly at the cyclotron frequency — what we would now call the cyclotron frequency of the ions? And if you did, couldn't you use that as a mass spectrograph? See, by going through the cyclotron resonance, we would now say in modern language, "We're looking for the cyclotron resonance in a plasma, or at least a partially ionized medium, not a real complete plasma." So I started out to do this experiment. I had the coil to make the field and built the thing. And then I began doing some calculations which were very discouraging, and I went to Chaffee with them and convinced myself and convinced him that the thing wasn't going to work. Well, it just shows you. Actually, the calculations were correct, but not completely relevant, and I fell quite sure that if I or anyone else had gone on energetically with that idea experimentally at that time, he would have discovered the trick he needed to do to make it work. In which case he would have had what was called the Omegatron ten years later. He would have invented that well ahead of its time. And I simply calculated myself short-sightedly out of that. Well, that evaporated then, and in casting about for something else I went to Bainbridge, and Bainbridge suggested that I might try something with tracks in emulsions, which was just developing at that time: nuclear tracks in emulsions. So I spent some time trying to see if I could see beta ray tracks in photographic emulsions.

Sopka:
What kind of a source would you have been using?

Purcell:
I used old radon tubes. We had to get the plates. They were Ilford plates from England, and they were plates that you could see alpha tracks in easily; and the game was to expose these to beta rays going in more or less tangentially into the emulsion so there would be a long track and then look for electron tracks. Well, it was really hopeless, as I really should have been able to calculate. But this time instead of calculating myself out of it, I spent quite a little time actually looking for the beta ray tracks and didn't see any. And in retrospect it's clear that I should not have seen any; that the emulsions at the state of perfection they had reached then, you could not see a minimum ionizing track. The background was much too high. Then Bainbridge suggested what became my thesis topic of looking at the focusing of charged particles in a spherical condenser. At that time of course everybody was all full of the lore about the focusing in magnetic fields, including the sector focusing, you know, where you have a sector of a magnetic field, which in fact was one component of Bainbridge's mass spectrograph and the mass spectrograph of Aston and others. So I did a little calculating on that — it looked very interesting. I set out to build an electron model, so to speak, to show that electrons would be focused by such a device, and to make it actually run. Again, it isn't clear why building it was a useful exercise except to get a Ph.D. thesis; there was nothing dubious about the equation of motion for the electron in the electrostatic field. Nevertheless it was a great experience to try and make this thing, which I finally succeeded in doing. It wasn't good for anything. That is, I was just focusing ordinary thermionic electrons to show that it would work. I wasn't measuring anything that needed to be measured. On the other hand, in working out the theory I did discover an interesting general property of the spherical electrostatic analyzer — the equivalent of what was known as Barber's rule for magnetic sector focusing. The spherical electrostatic analyzer is used now, oddly enough. It's been revived after lying dormant for 30 years and is now used in slow electron surface work. Its advantage is its big solid angle. We weren't so rushed in those days and spending that much time fooling around trying to find a thesis was not so unusual. And there was more space to work in.

Sopka:
You got your degree in '38?

Purcell:
Thirty-eight, yes.

Sopka:
Had you spent about two years actually on this thesis topic?

Purcell:
Well, no, it was less than that.

Sopka:
Because you were four years as a graduate student, and the first one you were presumably taking courses full time...

Purcell:
But on the actual spherical condenser topic, I think I spent not more than a year and a half.

Sopka:
Was it during this period that you did get married?

Purcell:
Yes. I was married in '37.

Sopka:
I see, which meant that you moved out of Conant or Perkins, whichever hall.

Purcell:
Conant, yes. And my wife got a job teaching out at Milton Academy, so we lived one year out in Mattapan and then two years up here in Cambridge near where we live now.

Sopka:
What had your wife done between the time when she was in Germany and ...

Purcell:
She went back to Bryn Mawr and did more graduate work.

Sopka:
I see. Did she ever complete her own doctorate?

Purcell:
No. She took graduate work in philosophy. She wanted to take more philosophy, and she did not complete a Ph.D. and she was teaching at Bryn Mawr also.

Sopka:
It's now the afternoon. We've taken a luncheon break and we'll pick up again with our discussion of Professor Purcell's years as a graduate student.

Purcell:
One of the very important chapters in my education as a graduate student in physics is associated with Professor Van Vleck. This had nothing to do with my thesis work, but beginning in perhaps my third year in graduate school in I suppose it must have been '36, I took Professor Van Vleck's course in electric and magnetic susceptibilities, which is the subject, of course, of his famous book. There were only two people who signed up for the course — Malcolm Hebb was the other one. Malcolm had been a graduate student of Professor Van Vleck's at Wisconsin — I guess I mean Wisconsin, don't I? —

Sopka:
Yes, he came here from Wisconsin.

Purcell:
— and had come with Van from Wisconsin a year or two earlier. Malcolm was a theoretical student who knew already a good deal of theory, which I did not, but during

this one-semester course where I learned a great deal of quantum mechanics, among other things (many people have learned a lot of quantum mechanics from Van's book), Van assigned as a term problem a consideration of the theoretical aspects of cooling by adiabatic demagnetization, which was then a rather new development in low-temperature physics actively pursued at Leiden and at Oxford. We started working on this as a term problem. In fact, we worked on it for the following year, and our results were eventually published in a joint paper by Malcolm and me and a companion paper by Van Vleck himself. This was a real turning point in my development as a physicist. I didn't contribute too much to the work. I really didn't understand extremely well what was going on in the calculations I was doing. But Malcolm did and of course Van understood it with crystal clarity, and our paper was the first substantial theoretical paper on magnetic cooling and was widely referred to later by people working in that field. In the course of it, through Van's generosity and help, I remember becoming acquainted with R.H. Fowler who had come over on a trip that year and was interested in things like this. And of course, as always, Van treated you as if you understood what was going on as well as he did, and he continues to speak to me about that paper as if I understood what I had been doing.

Sopka:
Was it an entirely theoretical undertaking? Or was there any attempt to follow up with ...?

Purcell:
No, it was entirely theoretical. You see, at that time there were only two places in the world where any experiments could be done — Leiden and Oxford. They were the two low-temperature laboratories. Well, there was some work at Toronto. And Giauque at Berkeley was doing some experiments and should be included, too. The data that we were trying to fit finally with our calculations I think mainly came from Oxford, if I recall properly, from Simon's laboratory. That may not be correct. But at any rate, this was an introduction to some physics that I was going to have to come back to again and again in my later work in nuclear magnetic resonance.

Sopka:
That was all part of the course that you were taking? Or was that an extension beyond the course?

Purcell:
Beyond the course because the course ended ... It was a one-semester course and it had a final exam...

Sopka:
For two students?

Purcell:
For two students. In those days, of course, Harvard, as you remember, Harvard exams were printed, set up in type and printed and there was a serial number up in the corner, and I guess Malcolm had # 01, and I had # 02. I don't know how they stopped the presses quickly enough. No, we went on working then for roughly a year after that on our term paper, writing it up and working out the details of the calculations. My half of it was concerned with iron-ammonium alum and Malcolm's was cesium-titanium alum, the two substances that the experimenters had used in their adiabatic demagnetization cooling. Well, after I got my Ph.D. in '38, the Harvard cyclotron was being put together and my research time was really turned in that direction, and with a number of other people, under the leadership of Professor Bainbridge, the pre-war Harvard cyclotron was built.

Sopka:
Was there any question about your staying on immediately? Did you consider going elsewhere or was the opportunity to stay on at Harvard so attractive ...?

Purcell:
I stayed on just as an instructor, I guess, whatever the lowest rank was, I believe... Well, the first year or two must have been just as an instructor. But then the faculty acquired a rank called Faculty Instructor.

Sopka:
Instructor of Physics is the first rank. And then comes Faculty Instructor.

Purcell:
Faculty Instructor — that's right.

Sopka:
Which apparently was almost equivalent to assistant professor, because there wasn't assistant professor.

Purcell:
For some reason they temporarily abandoned the title of assistant professor and introduced this faculty instructor, which, as I recall, was a five-year appointment. And so the war years I spent as faculty instructor on leave. But faculty instructor meant that people in that category could vote in the faculty meeting I believe. Well, so we worked on the cyclotron until it got running in '39. My contribution to that was work on the magnet power supply and controls mainly. People I worked closely with were Jack Livingood and Rubby Sher, now at Princeton; Binx Curtis. Then in the fall at the end of 1940 the Radiation Lab at MIT started up and before the end of that year I was committed to go down there.

Sopka:
While you were here as the lower level instructor, in addition to your cyclotron building activities, did you have teaching duties?

Purcell:
Oh, yes.

Sopka:
How heavy were they?

Purcell:
Well, I don't remember ... I don't think they were terribly heavy. You see, I had begun as a teaching fellow in 1937 before I got my Ph.D., and I'd worked in old Physics B under Professor Black. It seems to me that in the late '30s I remember working with Professor Saunders in what we called then Physics C. I don't remember too well what courses I was doing in that period of '38.

Sopka:
You were in assistant capacity rather than having the responsibility for working up your own lecture material.

Purcell:
Yes. I think the first time I gave a lecture course on my own responsibility was ... possibly in '39, but I'm not sure of that. It's not very important in any case.

Sopka:
I was wondering how your time tended to be divided in those post-doctoral years in terms of your cyclotron commitments and your teaching commitments.

Purcell:
Oh, it was roughly 50-50, and we had tutorial then, so that I had a number of tutorial students, undergraduates. I shared an office with Jack Livingood. The interesting physics course that was created in those years was Physics F and G, which was a sort of really advanced two-year physics course, which, as it turned out, has as undergraduates many people who later became well-known physicists, including some in our own department. But I had nothing to do with that, although in sharing an office with Jack, who did, I kept following it. I think I was engaged perhaps with Physics B at that time. In fact, I may have been responsible for Physics B in '39, because Black was ready to retire. Now, so after the cyclotron got running and then came Radiation Lab at MIT and the threatening war clouds ahead, but I went to work full time at Radiation Lab I guess in January, at the end of the first semester of '40-41, having gone a little part-time before that.

Sopka:
Who recruited you to the Radiation Lab, do you recall?

Purcell:
I would say it was Bainbridge, who was one of the original group. Bainbridge, Rabi and others were in on it. When I went to Radiation Lab, I went into Rabi's division. We were just really in the process of forming the divisions then. But Street and Bainbridge had already gone down. Ramsey wasn't at Harvard yet. He was there from Illinois. And Zacharias was there. Lee Du Bridge was, of course, the director. And physicists were gathering from all over the country many nuclear physicists: McMillan, Alvarez from Berkeley, Ray Herb from Wisconsin, Pollard from Yale, Milton White from Princeton ...

Sopka:
Was it immediately clear what your mission was at the Radiation Laboratory?

Purcell:
Yes. Well, you mean what the laboratory's mission was?

Sopka:
Yes.

Purcell:
Yes, in fact it was extremely definite. Our mission was to make a radar for a British night fighter using 10-centimeter magnetron that had been discovered at Birmingham. And from that narrow assignment grew an enormous breadth... But our first assignment was very definite, which was probably a good thing. We didn't know how to do it, mind you. I mean, there were many components of the radar that had not even been invented yet, but their necessity was already recognized.

Sopka:
Did you then spend the entire war year period in Cambridge?

Purcell:
Yes. I was in Cambridge the entire war years, including clear up into '46 because I stayed to help write some of the books, the Radiation Lab books. But I was there all that time — five years, more or less. I spent one week at Los Alamos shortly before the Trinity Test at Alamagordo, helping to design some of the equipment for that test. I was imported to Los Alamos as a transmission line expert, so to speak, by Bruno Rossi, and in that way learned what was going on at Los Alamos. By then many people from Radiation Lab had gone out there, including Bainbridge, Ramsey and Rabi. Well, Rabi kept a foot in both places throughout the war. He went back and forth. But others — Alvarez, McMillan, Ramsey — had gone out full time.

Sopka:
Do you remember what the response of the people here at the Radiation Lab was to the word of what really was going on, the atomic bomb, when the information was released? Was this a total surprise? Did people react ...?

Purcell:
Well, how should I say it? For one thing, as I recall it, we were preparing a very elaborate news release on radar, which had been kept very secret, and it was decided that there would be a general news release on the nature of radar telling everything. People had been working for weeks getting all this ready, and the release date for this was just the day that the atomic bomb news came out, so...

Sopka:
You were snowed?

Purcell:
We were somewhat snowed, yes. I don't know. I myself, just as one example — we all knew they were working ... You know, there was a big effort going on out there that had to do in some way with uranium fission. When I went out there whenever it was in February of '45 or something like that, I was surprised to find it was a bomb. I thought it was probably going to be power, you know, an energy source. So I didn't guess correctly, but maybe others were better guessers than I was. And we knew, of course, that there was an enormous, big thing going on. There were many people, of course, like Rabi who knew the whole story.

Sopka:
In recent years there has been so much discussion and retrospective criticism of the scientific community for the development of the atomic bomb in the '40s. We're interested in knowing if people can recall how they and their groups felt about war work in general and about the atomic bomb in particular.

Purcell:
Yes. Well, as far as war work in general is concerned — let's say at the Radiation Laboratory where we worked on radar during World War II — there was really not the slightest discernible feeling that we should not be doing this. Everybody, so far as I know, completely accepted the proposition that it was a matter of desperate urgency to beat Hitler. I suppose in particular types of weapons a question might have arisen: even to beat Hitler is it right to do so-and-so? But certainly in the use of radar that question never arose. Well, you might say it could have, because one of the uses of radar in the latter part of the war both by ourselves and the British, of course, was for blind bombing by the heavy bombers of Bomber Command and of the American Air Force. In retrospect one might say the firebombing of Dresden was as great a human catastrophe — indirectly assisted by radar — as you could imagine. But at the time no one would have really raised that. The situation was too desperate. Hitler was about to win. Also, in the beginning of the war, the place our radar really paid off first, I think, was in the anti-submarine campaign. Ten centimeter radar really was one of the very important weapons that finally beat the German submarines. And the seriousness of that threat alone, those of us who knew what that was ... In the summer of 1942 I accompanied Rabi to England to talk to various opposite members in the British radar development. It was at the headquarters of RAF Coastal Command that we saw for the first time (I saw — maybe Rabi had seen it before but I hadn't) the big map of the Atlantic with a pin stuck in for every sunk ship. Along the coast of the United States from Maine down to Florida ... there was scarcely room for another pin. And Coastal Command at that point was really beginning to make some progress in killing

submarines as they came across the Bay of Biscay on the way back to their home ports on the French coast. So, you know, there was no argument there about what you'd better be doing in fighting these bastards. There was great concern at the Radiation Laboratory after the war — and this is I think an interesting chapter on all that debate, you know, about setting up the Atomic Energy Agency. Should it be under civilian control or military control? And that whole argument about the McMahon Act and stuff. And the Radiation Lab people were by then — with the war pretty much over — much concerned about that. We had meetings and discussions and committees and people signing petitions and so on and calling up Washington, very much in on the argument, which culminated in making the AEC a civilian agency.

Sopka:

I see. Were you personally involved in that, in those discussions or arguments?

Purcell:

No, I was not conspicuous in those arguments. I went to them and was involved in them, but there were a number of people who were the leaders in organizing those things. My own involvement at the end of the war in government matters grew in a different way. It came through — I guess through DuBridge. The Air Force was setting up a Science Advisory Board. Lee DuBridge was on it and took me along as a junior assistant. That was my initiation into the government advising activities that took a good deal of my time in later years. I was a member of the Air Force Science Advisory Board for quite a long time.

Sopka:

Did you find this kind of activity satisfying and did you feel that you were accomplishing something as a spokesman from the scientific community or was it difficult going that you felt that somebody had to do and ...?

Purcell:

No, it really wasn't quite like that at the beginning. It was not so much that we were representing the scientific community at all, because we didn't identify then really any interests of the scientific community as such. It was that we had become rather knowledgeable, technical experts on this whole new military technology. The Radiation Lab people became accustomed to dealing with military plans and technology during the war and had many close associations with the problems. So these people needed our advice. For instance, one of the first tasks of the Advisory Board to the Air Force, which was run by the famous fluid dynamicist, Theodor von Karman, was to try to predict the nature of things to come technically and scientifically. I was involved in that. I'm not sure our predictions were any good, but that's what we were set up to do. You see, the scientific people evolved during World War II entirely new relationships with military people, one of mutual confidence and understanding of the problems and working together, so that the Radiation Lab finally put itself in a very strong position with respect to scientific undertakings with the military for military problems. It wouldn't do anything just on order. It had to know the whole purpose and would attack the problem in the large but insisted on being in on that whole ... The British had developed in the same way in complete contrast to what happened in Germany, where the scientists were never really trusted by the military or the government at all. They were used, some of them, and some of them like von Braun were used to do new things. But the relationship was totally different. Then, of course, one result of that on the other side was the creation of the Office of Naval Research after the war, which was the thing that kept research going in physics until the National Science Foundation finally could take over. But it was the Navy and particularly a few officers and a few civilians in the Navy who realized that the kind of research that we now needed to carry on in pure science was going to be expensive and sophisticated and needed support. And the military at that point were the only ones that had the budget to support it. So the Office of Naval Research began right after World War II supporting pure physics research universities with no strings at all. And I think one can trace that to the working relationship that did develop between scientists and the Navy during the war, when we were right in there helping them solve their problems.

Sopka:

Apparently they did realize that they couldn't have gotten far without the scientific community's help.

Purcell:

Oh, yes. I mean the ship, except for its hull, was full of stuff that science had produced for them during the war. Well, let's see, I don't know whether we should say much more about Radiation Lab here. Many people will be talking about that in this series of interviews I'm sure. I might just remark that what I learned there helped my own career in physics. I really consider myself extremely lucky, and I mean lucky, to have been able to profit personally from what I had to do there at the Radiation Lab. Not only did I learn a whole new armament, acquire a whole new kit of research tools — microwave technology, transmission lines, signal to noise theory, just a lot of different things, all of which were going to be useful one way or another — but perhaps the most important thing was being thrown together in a working relationship with a number of physicists from other places: in particular, physicists from Rabi's laboratory at Columbia. Here I'm thinking particularly of Ramsey and Zacharias and Henry Torrey; and then at Columbia still during the war, Kellogg and Kusch, and of course Rabi himself with whom I was very closely associated through all that time, Rabi being the head of the division to whom I reported.

Sopka:

Which division was that and what was your specific responsibility?

Purcell:

Well, the designation of the division was Division 4, Advanced Developments, I think. But it was an interesting collection of groups which operated a little bit outside the pattern of some of the others and with a little more freedom to choose what they wanted to do.

Sopka:

Were you responsible more for developmental rather than perfecting techniques?

Purcell:

Very much so. In fact, we were responsible for exploratory things. The group I associated with longest was called the Fundamental Developments Group, and what we did, roughly speaking, was to make the first breadboard models of radar at a new wavelength, which meant that we had to develop a lot of the plumbing and the circuits and for the ever shorter wavelengths. The Lab started at 10 centimeters, and then our group was working on three centimeters, and then after that was well established and in production, we were working on one centimeter radar. We were also free to try and invent things and measure things that were relevant. There was no problem in keeping people at jobs that were in some way relevant. Another group in Rabi's division had to deal with microwave propagation, which turned out to be a rather more complicated and subtle problem than had been anticipated. Another group under Jim Lawson was really responsible for establishing or perfecting and proving and teaching the theory of signal to noise as applied to detecting radar signals. And then there was the theoretical group, which was also under Rabi. Most of their theory was concerned with electromagnetic fields and signal to noise, things of that sort. George Uhlenbeck was in charge of it for quite a long time, and Bethe was in it for a while; Schwinger was in it; Frank Carlson; David Saxon, now president of the University of California; Goudsmit also. Came the end of the war and we were all thinking about what shall we do when we go back and start doing physics. In the course of knocking around with these people, I had learned enough about what they had done in molecular beams to begin thinking about what can we do in the way of resonance with what we've learned. And it was out of that kind of talk that I was struck with the idea for what turned into nuclear magnetic resonance. Another very important association there was with Bob Dicke, who was a member of my own group but who worked pretty independently. It was during his work there that he invented the Dicke radiometer, which is the basic tool of radio astronomy, and applied it to measure molecular absorption. By the end of the Radiation Lab, in '46 even, Dicke had measured the absorption of water vapor in the atmosphere; working with Beringer he had measured the oxygen absorption at six millimeters; and he had observed a partial eclipse of the sun with a K-band radiometer. And, in fact, he had made a measurement of the sky temperature which set a non-trivial upper limit on the density of what we now know as the isotropic microwave background radiation, although I believe Dicke himself later forgot that he had done that.

Sopka:
This was done at the Radiation Lab?

Purcell:
At the Radiation Lab. That was measured from the roof of the Radiation Lab, yes. You see, toward the end of the war we had made a terrible mistake, and my group had contributed to it or I contributed to it certainly. Namely, we had picked the wavelengths to settle on for K-band almost on top of a water vapor absorption line. This turned out to be fatal to the use of K-band for long-distance radar (for which it wasn't very good anyway). We were greatly chagrined as a matter of fact — it should have been possible to predict this by using some data that were already contained in a paper by Van Vleck. When we began to suspect that we were having trouble with water vapor, Dicke used his radiometer to measure the water vapor absorption line. This led, of course, directly into microwave spectroscopy, was indeed microwave spectroscopy. And the techniques developed during the war provided almost the basis for the explosive development of microwave spectroscopy after the war. There were other things that we thought might turn out to be interesting physics but didn't. One of the extremely important results was in semi-conductors, because the physics of semi-conductors had already been stimulated during the war through their use in the shape of crystals and crystal detectors. In fact, the Radiation Laboratory had been supporting Lark-Horovitz's program at Purdue on germanium as well as other more empirical, edisonian research on the properties of semi-conductors.

Sopka:
It would appear then that the existence of the Radiation Lab not only served the function of helping the war effort but that it brought together a group of scientists who in other circumstances would never have worked together, and that in this meeting of minds other things were begun that have later proved quite fruitful.

Purcell:
Yes. Oh, very much so. It worked in so many different ways. It provided us with the tools, not merely the hardware, but with a basic understanding, which for most of us, certainly for me, came absolutely for the first time of what you have to do to detect a signal in noise. So in all future experiments, whether by radio astronomers or microwave spectroscopists, people knew how to deal with the problem of the ultimate sensitivity of their apparatus and what you have to do to detect a weaker line and so on. That was all essentially worked out. One of the key people there, in the later years, was Bob Pound. And Bob's crucial contribution in our first nuclear magnetic resonance work with Torrey and myself was his understanding of amplifier noise. Young as he was, he was as good as there was at the practical business of noise figures and inputs and receivers, which he had been working on under Zacharias. So we came out of that with that tremendously useful equipment, but we had to rethink values back into physics to see what physics would be worth doing. Now, of course, I'm sure the same thing can be said for the people who worked together in the Manhattan District on bomb physics or whatever, because there also was a tremendous advance; and they were directly in physics, so they didn't even have to figure out what to apply it to. What they were doing was physics.

Sopka:
Was your electrical engineering background from under graduate days of value to you in this ...?

Purcell:
To some extent. It had already been of value to me as a research physicist because I was doing practical work in making magnets and things and running D.C. generators. In fact, it was probably of most use to me when I was working on the cyclotron, because my job was essentially an engineering job. But it was useful in my work at Radiation Lab, although looking back on it, I didn't learn anything at Purdue that was directly applicable in the Radiation Lab because I didn't learn about electromagnetic waves at Purdue.

Sopka:
Wave guidance and terms like that probably weren't even in use then.

Purcell:
That's right. I often tell my freshman students, when I get Maxwell's Equation on the board for them, "I got a degree with honors in electrical engineering and I'd never seen these equations written down. Not only that, but the fact stated in the fourth equation was unknown to me, the fact that the curl of B is equal to E dot. We didn't need that at Purdue in 1932 because we never considered radiation problems. We only did waves on telephone lines. You do that all with telegrapher's equation. You don't need to know about displacement current. So I literally graduated in electrical engineering without even knowing there was a displacement current. But by the end of the Radiation Lab I was very familiar with the displacement current." We learned about circuits in a general way. Bob Dicke was one of the leaders in developing the microwaves circuit theory in general terms, the scattering matrix and the impedence matrix. There were many things we didn't have, of course. We didn't have nonreciprocal devices. One of the things we would have dearly loved to have in Radiation Lab was a pipe in which the signal could go through one way and not the other. But the possibility of that had to wait for another several years until circulators were discovered, gyrators. And of course we didn't have transistors or anything like that. Everything was built with vacuum tubes. Well, that's how NMR[3] started, with that idea which, as I say, I can trace back to all those indirect influences of talking with Rabi, Ramsey and Zacharias, thinking about what we should do next. And at the end of Radiation Lab, when our thoughts were turning back to physics, Rabi had the bright idea of having some lectures to sort of rehabilitate us in physics. So he got Pauli. Pauli was then at the Institute. He got Pauli to come up every other week and give a lecture. By then the Radiation Lab was enormous, you know, thousands of people, and many of them were physicists or people who wanted to be physicists. At the first lecture, an enormous lecture room was packed with people to hear Pauli. It was almost totally un-understandable to most people, including me. At Pauli's next lecture, attendance had shrunk by about a factor of four, as we still didn't get much out of it. But then Rabi had the great idea to get Julian Schwinger to lecture in the weeks in between. So Julian was there in the theoretical group — and, I don't know, — was all of 27 years old or something. Julian began to lecture. And that was just marvelous. He reviewed the recent developments, pre-war developments in physics, going back to the '30s and coming on — where things had gone, what were the puzzles, and that was just marvelous. So gradually the attendance at Julian's lecture went up and Pauli's went down. I remember the lecture when Julian was talking about the quadrupole moment as the deuteron and all that it implied and how you measured it. That was really exciting — the quadropole moment having just been determined by Ramsey and Rabi and Kellogg and Zacharias just before the outbreak of the war. But that helped us to get back into real physics. We wanted to do something more than merely apply the tricks that we had learned.

Sopka:
You came back up the river then in early 1946, back to Harvard?

Purcell:
Yes.

PLAY AUDIO Well, see, we actually did the first NMR experiment here, not at MIT. But I wasn't officially back. In fact, I went around MIT trying to borrow a magnet from somebody, a big magnet, get access to a big magnet so we could try it there and I didn't have any luck. So I came back and talked to Curry Street, and he invited us to use his big old cosmic ray magnet which was out in the shed. So I didn't ask anybody else's permission. I came back and got the shop to make us some new pole pieces, and we borrowed some stuff here and there. We borrowed our signal generator from the Psycho Acoustic Lab that Smitty Stevens had. I don't know that it ever got back to him. And some of the apparatus was made in the Radiation Lab shops. Bob Pound got the cavity made down there. They didn't have much to do — things were kind of closing up — and so we bootlegged a cavity down there. And we did the experiment right here on nights and week-ends. We were still working down there.

Sopka:

This was after the war had ended in August?

Purcell:
This was December, 1945.

Sopka:
But then there was a certain period of winding up.

Purcell:
Yes, and we were still working. We were working on the book, writing the books — all three of us: Torrey and Bob Pound and myself were among those people. And so we did the experiment here, but we were still employees of Radiation Lab. It was a mixed up thing.

Sopka:
Had Harvard already approached you about returning? I notice that you came back as an associate professor, having spent your faculty years on leave.

Purcell:
That's right. All I remember — it must have been the summer of '45, something like that. I remember Ted Hunt and somebody — who else it was I can't remember — coming down to see me at lunch. We had lunch in a little hamburger joint down around MIT, and they said they'd been authorized to come down and ask me if I was interested in coming back as an associate professor, to which I said yes, and that was all there was to it. There was no haggling — nothing — and that's all I know. That must have been '45, I should think.

Sopka:
And your home base had been Cambridge really since you came here as a graduate student in '34?

Purcell:
Yes. We are living in the same house we were living in in 1940 when I was at the Radiation Lab, and we've been renting it now for 37 years and I'm just about to buy it.

Sopka:
That seems like something of a record.

Purcell:
It's the house that Roger Hickman lived in just before us, and they moved out to Belmont in 1940 and Beth and I moved in then and we rented the house from an old gentleman who wasn't so old then who was a physics teacher and professor at Northeastern, Mr. Coolidge. He died in January this past year, and we're about to buy the house from his estate.

Sopka:
I see — he held onto it.

Purcell:
He wouldn't sell it before. I tried to buy it.

Sopka:
So you can soon call your home your own.

Purcell:
We soon can call our home our own. So this stretch of Mass. Avenue I know. But during the war years I scarcely ever stopped off at Harvard. I mean there were years there where for six months at a time I never set foot in Harvard. I would just get on the subway and go down to MIT.

Sopka:
I guess that the atmosphere here was entirely different then.

Purcell:
Yes, it was full of the training programs. But then, of course, there was the Radio Research Lab over here, which was working on radar countermeasures. And they were allowed to visit us, but we weren't allowed to visit them. It was an unsymmetrical relationship with respect to security. Whipple and Felix Bloch were over there and Van Vleck was over there and Fred Terman ran it. But we didn't have much contact with them. The Radio Research Lab was recruited largely from engineers, not physicists. As witness the fact that in the Presidential election of 1944 — a straw vote at Radiation Lab was overwhelmingly Democratic, at Radio Research Lab overwhelmingly Republican.

Sopka:
That's interesting.

Purcell:
Somebody I knew up here said, "We found one guy who voted for Roosevelt up here. He was the janitor."

Sopka:
Well, I gather that the Harvard atmosphere in the post-war years was very different and quite exciting in terms of picking up your physics research and in terms of the influx of graduate students primarily but students at all levels following the wartime experience.

Purcell:
Yes. We had those years when we had really very exciting courses with the older returnees in them who were very serious. The course that Chaffee gave for so many years, I gave after the war a couple of times, Physics 28 we called it.

Sopka:
Yes, I took that with you.

Purcell:
You took that. And, gee, that was an exciting class. Those classes were full, as you remember…

Sopka:
They certainly were well attended. I mean they were large groups and they were really earnest.

Purcell:
Everybody was very serious, yes, really working.

Sopka:
But it was about then that the now Professor Bloembergen appeared on the scene and became involved with you.

Purcell:
Yes, he appeared in the spring of 1946 and I took him on as a research assistant, and he started right in producing. Of course, as you know, he got his degree back at Leyden.

Sopka:
Yes, he told me that on thinking about the idea of going through all the regulations here when he was already within sight over there, it seemed sensible for him to do it.

Purcell:
Right. No, those were great times. We had some wonderful students in that period — George Pake and Charlie Slichter, among others.

Sopka:
Do you recall how the evolution of ideas and then the realization in the laboratory went with regard to the development of the nuclear magnetic resonance work and the nuclear relaxation ...?

Purcell:
Oh, yes, I remember. We weren't prepared for one aspect... Well, we realized that the crucial aspect in the experiment was the relaxation problem, whether we could get the nuclear spin system relaxed. And at the time we did the experiment, there was only one mechanism for relaxation that had been theoretically studied. That was in a paper by Waller in the late '30s, Ivar Waller, the Swedish theoretical physicist, which was written to analyze electronic spin relaxation in crystals and was applicable to the problem. It later turned out that it was not the primary mechanism. But at any rate Henry Torrey made a calculation with the Waller theory to see what the relaxation time would be, and we decided that although it looked as though, according to this theory it would be quite long, that at least it couldn't be much longer than that because this process had to work. So that our experiment was designed to take account of the possibility that the relaxation time might be many hours. Our first experiment was done on paraffin, which I bought up the street at the First National store between here and our house. For paraffin we thought we might have to deal with a relaxation time as long as several hours, and we were prepared to detect it with a signal which was sufficiently weak so that we would not upset the spin temperature while applying the r-f field. And, in fact, in the final time when the experiment was successful, I had been over here all night — I can't remember; it must have been all day — nursing the magnet generator along so as to keep the field on for many hours, that being in our view a possible prerequisite for seeing the resonances. Now, it turned out later that in paraffin the relaxation time is actually 10^{-4} seconds. So I had the magnet on exactly 10^{8} times longer than necessary! The approach of Bloch and Hansen to the same problem was quite different, although we of course didn't know it at the time because we didn't even know they were doing anything. They did their first experiment on water and bubbled oxygen through the water in order to promote relaxation. That also was unnecessary, although perhaps not by such a big factor. Interestingly, we had approached the problem ... mainly in terms of inducing transitions between two quantum mechanical levels. Bloch and Hansen and Packard apparently thought about the problem much more in terms of the precession of the classical spin moment. And, of course, these two descriptions are totally equivalent. Nevertheless, I remember that the first actual personal contact between our two groups came when Bill Hansen came east a couple of months later — he frequently came to the Radiation Lab anyway — and we talked to him. We were talking at cross purposes for about 15 minutes before each of us understood the other's experiment in his own terms.

Sopka:
When was this, around 1945, '46? Was it that early?

Purcell:
Our experiment was done just before Christmas, 1945. And Bloch and Hansen's was done a month or so later. And the time I speak of must have been perhaps March, '46, something like that. Although Bloch had been at Radio Research, I saw very little of him during the war years; and we, of course, didn't know they were doing it — they didn't know we were doing it. And, as I say, we conceived of the thing in quite different terms. But the thing that we did not understand and it gradually dawned on us later was really the basic message in the paper that was part of Bloembergen's thesis and came to be known as BPP (Bloembergen, Purcell and Pound), was the important, dominant role of molecular motion in nuclear spin relaxation, and also its role in line narrowing. So that after that was cleared up, then one understood the physics of spin relaxation and understood why we were getting lines that were really very narrow, which of course eventually became very narrow indeed when the high resolution NMR came in.

Sopka:
At what point along this evolution did the concept of the negative absolute temperatures come into the picture?

Purcell:
I can't remember the exact date, but it came in a little later than that.

Sopka:
Did that concept pose a hurdle or a barrier?

Purcell:
Well, it didn't bother us. In fact, I regarded it at first as just a mere pedagogical point. It came at the time when Bob Pound was doing a lot of his work on nuclear quadropole resonance in crystals. I had been thinking a little about the spin temperature and had just noted with amusement that if I had inverted levels, I could describe it with a negative temperature, that the thermodynamic relation between entropy ds, and dq/t was perfectly valid for negative temperature, and that everything would go through formally with no particular trouble. And in fact I had started to write it up as a communication for the AMERICAN JOURNAL OF PHYSICS, thinking of it purely as a kind of an amusing pedagogical point. Then Bob came up one day with this crystal that had a five-minute relaxation time, the longest spin-lattice relaxation time we'd ever seen. So I said, "Look, Bob, if we've got this, why don't we just do it? Just for fun let's invert the spins and show that they behave as if they were at negative temperature." So we devised an experiment to do that and it behaved just the way we thought it would. And so then people began taking it a little more seriously, and when we began talking about it, we found that it really bothered people. Well, we wrote up our little letter about the spins in the crystal sample and pointed out that it could be said to be at a negative temperature. And then some people, especially chemists, were terribly bothered by that — old-line thermodynamicists.

Sopka:
I had heard that. That was why I was wondering whether it bothered you people or not.

Purcell:

No, it didn't bother us at all, but I didn't want to get into any arguments ... Fortunately, I didn't have to because Norman Ramsey shortly thereafter spent a sabbatical year at Oxford. Simon, the great thermodynamic physicist at Oxford, had been greatly intrigued by the negative temperature idea from the start and was not bothered by it. He enjoyed it very much, and he kept urging Norman to write it up. So Norman, while he was at Oxford, started writing an article on negative temperature which he published. Then Norman began getting these really stern and indignant letters from Giauque at Berkeley. Norman showed me one of these letters from Giauque one time. The only conclusion you could draw was that Giauque just plain didn't believe in statistical mechanics. You know, he was an old thermodynamicist. Well, there are indeed difficult questions that can be raised about the concept. The whole thing — I must say, if we had tried to deal with it seriously then, we would have had to face some questions that I think we would have had some trouble with: namely, what is the state of the system when it's demagnetized, the spin system? And later on, a number of people, most especially Anatole Abragam, contributed to the clearing up of that issue.

Sopka:
At what point did it become clear that this had some relevance to the question of masers? Do you remember how that came about?

Purcell:
Well, the idea of the inverted population was of course basic to it and that when you had the population inverted, it was an emitter, not an absorber. This whole sort of complex of ideas were knocking around. I would put in there Dicke's idea of the super-radiant state.

Sopka:
It's interesting to think about what ideas were floating around in the active scientific population.

Purcell:
That's right. It was the general idea I would say of people thinking about what happens if I have an inverted population so that I have more in the upper state than in the lower state. How does the radiation interact? I have stimulated emission that exceeds the absorption, put it that way. Now, if I make the stimulated emission exceed the absorption and then use it to make itself a bootstrap, I have a laser. This super-radiant state idea of Dicke's is a slightly different situation, but all these ideas are aspects of a situation where we've suddenly moved out of the conventional realm of upper states always having lower populations than lower states.

Sopka:
The kinds of things you thought of when you took Oldenberg's course.

Purcell:
That's right. Here things are backwards. Here things really can happen the other way around. So I would regard our introduction of negative temperature as just one of the ideas and papers opening the door on that whole world.

Sopka:
This post-war research work that you did in the late ‘40s, was that financed under this naval office or was it locally financed?

Purcell:
No, at some point — I've now forgotten just what the years were — I had a grant from ONR. [4] We were working under an ONR contract. In fact, we had a sort of blanket nuclear physics contract under which this could go, so I didn't have to spend much time myself getting a separate contract or anything. That was what was so nice, because we had the support we needed: we didn't have any strings; we didn't have to have a lot of individual contracts. They were quite willing to see nuclear physics interpreted broadly. Other things I did were not so supported. For example, the 21 centimeter work, which came shortly thereafter, that was done on our own funds and a tiny grant from the American Academy: $500 I think we got from the Academy, which we used to make our plywood antenna, and the rest of the stuff we borrowed...

Sopka:
How did you get into the 21 centimeter line work? I realize that the young man, Harold Ewen, that you worked with was at least partly, if not basically, an astronomer. Or wasn't he? His degree was in astronomy and physics?

Purcell:
No, no, his degree was in physics, and he had an interest in astronomy, but he came to me for a thesis topic. Ewen originally thought he might be able to use microwaves somehow to detect or measure something in the upper atmosphere. He had been in the Navy during the war and had been involved with a lot of technical things. He was already technically pretty adept. And he was interested in astronomy and knew some astronomy because in fact he'd been teaching, among other things, celestial navigation to naval air cadets during the war, including Ted Williams. Well, we talked about the upper atmosphere prospects. We didn't see anything particularly exciting and we were still talking about it when Ewen went off to an astronomy meeting somewhere. When he came back he said, "You know, somebody was talking about this hydrogen hyperfine line, whether you could detect it." So we began thinking about that and just looking to see what were the sensitivity problems, whether we couldn't... and decided maybe there was a fighting chance to do it. So he took that on as his thesis topic. Now, at the time, to the best of my memory, we did not know the origin of the suggestion — namely, van de Hulst. I am quite sure, though, that the chain going back ends at van de Hulst. He was a young Dutch astrophysicist at Leiden who had made the original suggestion that one should look for a 21 centimeter line, and I feel sure that it's an echo of that suggestion that Doc heard at the meeting that put us onto it, although at the time we didn't know about that. We were fairly well along having decided to do it and making our plans, when we made actual connection with van de Hulst.

Sopka:
This was in the early '50s?

Purcell:
Yes, the early '50s, that's right. The first observation was in the spring of '51.

Sopka:
I see your trophy.

Purcell:
Yes, the boys made that for me last year to commemorate the 25th anniversary. As it happened, the actual time Doc made the observation van de Hulst was here as visiting professor at Harvard.

Sopka:
Oh, that was a coincidence.

Purcell:
And came over and we talked to him about it and everything, and he then cabled back to Leyden where they also had been getting ready to look for it. They were a little behind us. But I have never doubted that the credit for the original suggestion that one should look for 21 centimeters must go to van de Hulst, because I don't think... I

know of no shadow of a doubt on his claim to have that priority. Doc had great nerve to look for it though, and he did a superb job of getting the stuff together. I told him that it was going to be a very tough thesis problem because, "if you don't find the line, you're going to have to put in a hell of a lot of work to establish the limits. If you find it, of course" — as he did luckily — "then it's great, it's fine. But if you don't detect it…" And of course the original detection was such that if it had been even five times weaker, he probably wouldn't have seen it. It was just barely there.

Sopka:
There were fortunate circumstances all around.

Purcell:
Yes. But on the other hand, we claim we went at it right. We used the simplest possible antenna but one which was good for the job. We couldn't have done much better with a larger antenna. And Doc's receiver was really very good. It was probably as good a receiver of that type as existed in the world at the time he did the experiment. It was superior in one important respect to the receiver at Leyden.

Sopka:
Since van de Hulst was here, did he discuss with you what approach and what difficulties they had been pursuing in Leyden?

Purcell:
No, but we knew a little bit about it. They were making a bigger antenna. They were going at it in a more elaborate way, which eventually enabled them to get better results when they finally got it. They had had the misfortune of a fire in the antenna, which set them back a week or so. So, in fact, it was I forget how long after I had asked … The original publication consists of three communications in NATURE of the same adjacent pages, [5] because I asked NATURE to hold ours up until they heard from Leyden so we could publish together. And at the same time I wrote Pawsey in Australia and the Australians had not thought of looking for this line, didn't know anything about it, but they were so expert in the radio astronomy business and had so many things going that it didn't take them very long to throw together a 21 centimeter receiver for one of their antennas.

Sopka:
This was just when the whole field of radio astronomy was really bursting …

Purcell:
That's right, bursting open, and the Australians were very active. So then Pawsey cabled in that they had confirmed our results, and that cable is also with the two pieces in NATURE in that first publication: the letter from Doc and me and one from Oort and Muller at Leyden and a cable from Pawsey. I've always been glad that I handled it that way.

Sopka:
It certainly seems like a very gracious way…

Purcell:
Well, if we hadn't done it that way, there would have been long-standing rancor. There probably is even some residue of that, a little bit.

Sopka:
Was this immediately recognized for its later potential as a tool, or did it take a while for this to…?

Purcell:
No. It was certainly apparent to Oort right away. You see, Dort is the grand old man of Galactic astrophysics, and right away he saw it as a tool for learning something about the structure of the Galaxy. I don't think they recognized perhaps immediately the trick of using the Doppler shift to locate the hydrogen they were looking at, but that came very soon. So there was no question but what… No, even in my mind, knowing as little astronomy as I did, it was clear that once you could do this, you could learn a lot.

Sopka:
I know that in recent years you've become increasingly interested in problems in astronomy and astrophysics. Did this all stem from this period?

Purcell:
Well, pretty much. That was certainly the first time I'd had anything personally to do with astronomy.

Sopka:
Did you have a course in astronomy?

Purcell:
Oh, no, no. I couldn't find any stars or anything like that. Now, that's not true of Doc Ewen. I call him "Doc" because everybody called him Doc even before he had his Ph.D. It doesn't have anything to do with that. In fact, he had to tell me where the Galactic ordinates were and everything.

Sopka:
I looked him up in AMERICAN MEN OF SCIENCE. After he did this work, then he went in business for himself, did he, and he's still in this general part of the country.

Purcell:
That's right.

Sopka:
He lives in Natick or Weston or someplace like that.

Purcell:
That's right. He has a small company called Ewen-Knight Associates, and for a while they were in the business of making very advanced electronics, including radio astronomy receivers, and various military receivers and things. In recent years his company has gotten out of hardware production and he's mostly doing consulting now. He's a very interesting fellow. He was sort of an entrepreneurial type, extremely good at getting…

Sopka:
He was among the older group of people who came through after the war?

Purcell:
Yes. Well...

Sopka:
You said that he had taught...

Purcell:
Yes, but he'd been pretty young at that point. I don't know. I don't know. I don't think he was more than in his late twenties when he did this.

Sopka:
I see. He wasn't somebody who'd had a large interruption of his life.

Purcell:
No, I don't think so. As it happened, various members of the Boston Red Sox were friends of his who had been in the Naval and Marine Air Force: Johnny Pesky and Ted Williams. Ted Williams once visited the lab here and came over with Doc. Everybody was all aflutter.

Sopka:
That was back when the Red Sox were going strong.

Purcell:
Yes, I remember the year Ted batted .400, one of the years.

Sopka:
I noticed that you've had 18 Ph.D. students.

Purcell:
Is that right? I never counted them.

Sopka:
And all but two of them were before 1960 and the ones since 1960 were joint, had other advisers in addition to yourself.

Purcell:
Oh, those were perhaps people that I really didn't... where I was just serving as a formality. Who are those?

Sopka:
I'm not sure. I didn't jot down the names. They're in that little book that the department published last year.

Purcell:
Well, I tell you, they were people like Pat Palmer, for example, who did his thesis in astrophysics — a brilliant thesis working with Ed Lilly — and I was simply his physics department signer upper, so to speak. I was following what he did and had great interest in it, but I wasn't his supervisor. He's not my graduate student in any real sense.

Sopka:
Well, I was wondering how the evolution of your own research style went in the period from the '40s to the '50s to the '60s. On the surface it at least appears that by the '60s you preferred to work alone rather than have the responsibility of guiding people. Is that a valid conclusion to reach?

Purcell:
Well, yes. I guess the real thing is that after a long period with NMR stuff, I guess I just didn't have any more ideas for that that I wanted to work on; and I really have been one to develop a kind of coherent ongoing self-perpetuating program of research. So I was dabbling really in different things in kind of an opportunistic way, which is not particularly good for graduate students, and I just kind of slipped into the role of not taking graduate students. I'm not broadly enough trained certainly as an astronomer, so that if I were to take a student on in astrophysics, he wouldn't be getting himself really connected up with a central part of the field. I've worked with a number of people, published with some people, who are other people's graduate students, but I haven't felt I really had any field as it were to offer a graduate student where you can come and do a thesis and then you'll have made a position for yourself in a field. I don't have any such field.

Sopka:
Your own interests seem to have continued to evolve and to range fairly broadly.

Purcell:

That's right and that's a luxury which I'm fortunate to be able to indulge, and the price of it is that you don't have graduate students carrying on the work. I'm not particularly proud of this way of doing business. In fact, I often regret that I haven't attempted to keep a central field going so that I'd have a base and have a continuing turnover of graduate students, as Norman, for example, has, to the great profit of the department. In that way I've really not contributed my share to the educational processes around here, and it's a selfish way to behave in some respects. It's just my style. Now I'm interested in interstellar medium and in this biophysical stuff I'm doing, which is really becoming more interesting to me now. So as I look now ahead a little bit, I would see myself perhaps tapering out of the interstellar medium and into bacterial environment. All through the '50s and '60s, of course, I've done an awful lot of government consulting stuff and things of that sort that really took a good deal more time than is visible around here. I do not do that anymore at all.

[1]National Research Council

[2]REVIEW OF SCIENTIFIC INSTRUMENTS

[3]Nuclear Magnetic Resonance

[4]Office of Naval Reserach

[5]NATURE 168 (1951) 356-358

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