Oral History Transcript — Dr. Robert Pound

Interview with Dr. Robert Pound
By John Rigden
At his office in the
Lyman Laboratory of Physics at Harvard
May 22, 2003

Transcript

Session I | Session II

Rigden:
Let's start. This is John Rigden. It is May the 22, 2003. I am sitting here with Professor Robert V. Pound in his office in Lyman Hall of Physics.

Pound:
Lyman Laboratory of Physics.

Rigden:
Lyman Laboratory of Physics, and we are on the fourth floor. Professor Pound is ready to talk and I'm ready to start asking questions, so let's begin. By the way, for the purposes of this tape I have known Professor Pound for many years and I will probably call him Bob most of the time, because that is how I think of him, so the informality will probably be apparent. Bob, let's just begin at the early part of your life, in fact the beginning. Where and when were you born?

Pound:
I was born in 1919 in Ridgeway, Ontario, which is a few miles on the other side of the Niagara River from Buffalo, in the Niagara Frontier as it's called, near Niagara Falls and all that jazz. In fact my father's father, my father's family, the name family Pound and my father’s mother's family both were United Empire loyalists who came to Canada in the 1780s during the time of the rebellion, and they acquired Crown grants as a result of being, having demonstrated that they had supported the British during the rebellion. In fact I discovered recently the background of my grandmother, whom actually I never knew because she died when she was twenty-six or twenty-eight. They had become homesteaders in North Dakota where she contracted tuberculosis B the called consumption. Her name was Francis Ellsworth -- and her ancestor was Francis with an i instead of an e, who was granted the piece of land closest to the Horseshoe Falls in Niagara Falls, including Table Rock. So, unfortunately, he sold it in 1817, but never mind.

Rigden:
Okay. So you were born in Canada. How long did you live in Canada?

Pound:
We lived in Ridgeway full time until 1923. My father joined the new faculty of Arts & Science at the University of Buffalo across the river in the Department of Mathematics. He started there in 1922 but commuted from our Canadian house in '22. But then they moved the university to the north side of Buffalo, which was too far from the ferry, and so we bought a house on that side of Buffalo and we moved over there for the academic years but we continued to live in Ridgeway during the summer months. It was a fairly long summer from the academic year, so even I had to start school in Ridgeway when I was old enough, but only for a few weeks, until we went back to Buffalo for the winter.

Rigden:
So you were about four when you came to the United States.

Pound:
I was four when I came there, and I was sixteen when we finally gave up our house in Canada even for summers.

Rigden:
When you were a third grader you learned a new word called physics.

Pound:
[laughs] You read that.

Rigden:
I read that. Was this in any way the beginning of your interest in physics?

Pound:
Well, I suppose not -- I really didn't know what it was, but my father was an experienced man in physics and he had done his Ph.D. at Toronto in 1912.

Rigden:
In physics?

Pound:
Yes, in 1912, I'm sorry, under J. C. McLennan, who was the best known physicist at Toronto in those days. He became Sir John later, knighted for work in WW I. And my father left there in 1913 to go to Queens University in Kingston where he spent the next five years -- four years I guess it was -- before he came back to Ridgeway to help his father in a small company that they jointly held called the Ridgeway Milling Company. And he was general manager for the flour mill. But then he decided to go back to academics. Of course the milling business went down the drain pretty much after World War I. It was a big business during World War I, and then they had a number of other facilities, like being a party to the Bertie Township Gas Company, because several of the wells, gas wells, were on property that people of my family had owned. They used to have their board meetings in our living room. And I used to have a nice seal stamp thing that said Bertie Township Gas Company or something. But the gas companies were all taken over by a provincial gas company, consolidated and that sort of disappeared too.

Rigden:
How do you spell Bertie Gas?

Pound:
B-e-r-t-i-e. That's the name of the township of which Ridgeway was a part. All that's changed now because it's all part of the Fort Erie municipality.

Rigden:
All right. While we're still talking about your early interest in physics, or your interests [unintelligible word], Rabi said there were two kinds of physicists -- one turned to physics because of troubles with their radio kits, the others turned to physics because of troubles with their God. Were you either of those?

Pound:
No, I wouldn't say I was, because I never thought much about going into anything else but physics, although my idea of going into physics never -- I never enthused for the academic side at that point, because my father, being an academic, I knew that they didn't get treated very well, and I thought I was going to become something like a research person in a corporation, you know, something like General Radio, Naval Research Lab or some such place. But my experience in college and then particularly after the war changed all that.

Rigden:
Did you have any particular experiences in high school that were important in your career and your development as a physicist?

Pound:
No, I would say that my experiences were completely self-made in that connection. I became interested in building radios and things when I was about nine or ten and built my first radio, which used the first a.c.-powered vacuum tubes, the 227s, and was built from a circuit in, I think, Popular Mechanics along about 1929. My father helped me in that respect in the sense that he had a large collection of radio parts, because he used to build radios when I was very young. In fact in Ridgeway he had a battery-operated radio that he had built, and I can still remember the family each using a single headphone of his radio to listen to the Prince of Wales dedicating the Peace Bridge a few miles away from Buffalo to Ft. Erie. That was in 1926, and they brought the Prince of Wales over to serve the dedication. My father would drive the old Model T up beside the windows in the study of his in our Ridgeway house and run the leads out to use its storage battery for the filaments of his radios.

Rigden:
Now the radio was an AM radio?

Pound:
Oh yes, an AM of course.

Rigden:
It wasn't a ham radio.

Pound:
Oh no. No. That's right. Those radios were battery powered, then I built this a.c. one which just used two vacuum tubes and had a regenerative detector. That had a lot of influence on my postwar involvement in NMR in the sense that the marginal oscillators for which I'm pretty well known were in fact derivatives of regenerative radio detectors.

Rigden:
That you did back when you were nine years old.

Pound:
Yeah, that's right. And then I went on, when we moved to a different house in Buffalo which was a newly built house and a better than the one we had bought way back in '22. My neighbor was an older boy, four or five, four years older than I, I guess. And his father was the superintendent of lighthouses for the Lake Erie and Lake Ontario, and they had three ships in Buffalo Harbor, the biggest one called the Crocus. And the Crocus was a lighthouse tender, and the lighthouse service was a separate division under the Commerce Department then, not connected with the Coast Guard. The Coast Guard was also under the Commerce Department, but Roscoe House, the superintendent of lighthouses, got the opportunity twice in the summer to use that ship as a sort of cruise ship after they had reset the navigational buoys. They had built quarters for the superintendent, the family who would all go in the early summer, they would go up the lake, up Lake Erie to Cleveland or Detroit and then come back and visit all the lighthouses and the lighthouse keepers along the shore. It's about a two-week cruise. And then later in the summer they would go through the Welland Canal and do the same thing for Lake Ontario. And Bob, as a boy, went along with that, and he got to know the radio operator on the ship. And I remember his name is Steve Gusparovich.

Rigden:
Gusparovich.

Pound:
Yes, Gusparovich. And he operated this long wave ships radio which was running on 500-cycle a.c., and it went buzz-buzz-buzz when they keyed it. And Bob got the idea, or Steve Gusparovich suggested that if he got a ham radio he could actually converse when the ship was out on the lakes. And so he got interested in ham radio, he had also been listening to what they used to call a BCL, broadcast listener, and he would stay up late at night and pick up VK2ME from Australia on the ordinary AM radio and such things. So when we moved there I got involved with him and we started studying the code in order to get a ham license.

Rigden:
How old were you?

Pound:
When I was twelve.

Rigden:
You were twelve. Okay. And did this influence your work in high school science courses?

Pound:
In the sense that I found them stupid and trivial, yes. I mean in the sense that I petitioned my -- I had a great friendship with the physics teacher. He was physics and chemistry, and he'd let us take the chemistry New York State Regents Exam after the first semester, which was the final exam for the full year, but he said we'd have to go on with the course no matter what happened. And we did that, and then he said he wasn't going to do that in physics though, and I talked him into letting me do that, and I got 99 on the physics exam after the first semester. You know, they asked you how to wire, they used to ask you how to wire a doorbell and stuff like that. It was so trivial as far as I was concerned. I skipped the second term.

Rigden:
Well Bob, if you got 99, you had learned a lot of physics. Had you learned that just on your own?

Pound:
Yeah. I had been heavily into photography for years and I knew a lot of optics, geometrical optics, because I was also building a telescope, grinding a mirror, learned about the Foucault knife edge test, things like that and so forth. And I knew about three different kinds of eyepieces -- Ramsden, Newton, and what's the other one? I forget. So I had done all these things, and so that the physics course was absolutely trivial. The 99 was because of the question I missed, was to do with a question of who saw the apple fall. And I never read much of that stuff, so it was multiple choice and I chose the one, I think I chose Galileo instead of Newton. And ever since I've not been one for saying who did what. I say that's a part of social science and not physics in a sense.

Rigden:
Well, we're now sitting here and we're going to try to understand what Bob Pound did as we go on. So okay, you went from high school to the University of Buffalo. Was that choice simply because it was nearby?

Pound:
It was relatively economic I should say, because I never thought much about going somewhere else, and in many ways I think if I had known enough about Rochester at that time which was quite, fairly nearby, I might have been inclined, because they had a wonderful physics department then with L. A. Dubridge and Vicky Weisskopf and so forth. But -- and they were building a cyclotron with Van Voorhis. But -- our university, my father taught in the math department, but he actually taught all the analytical mathematics used by the physicists, so he and then I, knew many of the young people who had gone through as physics students there. There's a man named Marvin Chodorow for example who is a well known applied physics scientist at Stanford in recent times. He was one of my father's students.

Rigden:
Now you entered Buffalo around '37?

Pound:
Yeah. And I had two different scholarships -- a New York State scholarship. That year was in fact, this high school I went to called Amherst Central High School, it had been founded in 1932 I think or '30, and ours was the first class that won New York State scholarships. They gave forty New York State scholarships in Erie County. I think the number of New York State scholarships was equal with the number of something or other, representatives from the county to the state legislature. And that was the first year any of them had been won by the Amherst Central High School, but there were six of the forty in that school that year. Previously they were won mostly in Buffalo high schools as, for example, by my oldest sister. And then it also was announced at the graduation that I had been awarded an Erie County Supervisors Scholarship to the University of Buffalo, which was a full tuition. In those days tuition was the only thing you got covered with scholarships, so I had that plus the hundred dollars a year for the two years -- it was only two years -- I'm sorry, full tuition for two years, and in the last two years, of which I only spent a year and a half, I earned my keep by being a teaching assistant. I was so-called senior assistant.

Rigden:
In physics?

Pound:
Yes.

Rigden:
Did you, as you look back, do you think you got a good undergraduate education there?

Pound:
It was rather spotty I'd say in a sense. I mean there were some very, very dedicated teaching people, but I would say that, for example, a man that was my brother-in-law came there from Yale in the fall of -- sorry -- in the summer of 1940, and in the fall of 1940 he taught a course called quantum mechanics, but he was really an experimental nuclear physicist and his quantum mechanics was what he'd learned from [Henry] Marganau at Yale. And I didn't learn much from it. I took that course. And but my actual supervisor, advisor and so forth was a man named L. Grant Hector [spelling?], and L. Grant Hector had been a student of [A.P.] Wills at Columbia. In fact the predecessor to I. I. Rabi. In fact Rabi once told me that he took over Hector's apparatus when he became Wills' graduate student. But then Rabi said the trouble was that he, Rabi, was so smart that he didn't ever use anything. He invented a way of doing what he was to do for his thesis without building anything. So then he said thus he regrets that he never learned any experimental physics as a result. I guess I can't tell you much about Rabi, but --

Rigden:
Well, that's a true story. Rabi was I think fundamentally lazy, and if he could not find a simple way of doing something he probably didn't do it, and he kept trying until he did.

Pound:
I think he did something about adjusting the susceptibility of the things until it balanced.

Rigden:
With water, against a sample.

Pound:
Yes. A solution.

Rigden:
Yeah. I asked you the question about your undergraduate education because I was setting you up a bit. You have the distinction of being a full professor at Harvard and have an illustrious career and you only have a bachelor's degree. That's your only formal degree.

Pound:
And that was in three and a half years instead of four, because I left early when I was offered an opportunity to go into defense activity. I didn't have quite enough credit to graduate, but Hector stirred up a couple of extra semester hours credit for my tutorial work so that I could make the graduation requirement. In the tutorial program, I had been pursuing a project in dielectric susceptibilities that took most of my time for the last two years, building electronic apparatus for example.

Rigden:
And you left a half a semester early because of the war work?

Pound:
Yes.

Rigden:
Okay. Now how was it that a young fellow with a bachelor's degree, how did you get attracted to or how did you get an offer to get into defense work? And where did you go from Buffalo?

Pound:
Well, of course what was going on then was that the war was getting intense in Europe and our Selective Service began. I had to register for Selective Service October 16, 1940, and it was clear that our future as going to graduate school and going on in science was very prejudiced by all this. And so when my brother-in-law, Howard Schultz, who became a professor at Yale after the war but had got his Ph.D. at Yale in 1937. He had come down as one of the first people being recruited for MIT Radiation Lab. He was also being recruited at the same time, in competition, to a company called the Submarine Signal Company in Boston, in which my other brother-in-law Harold Hart was the head of the radar department. They had developed a radar department entirely on their own starting in 1930, which was long before anybody else. And H.M. Hart had also been a graduate student at Yale and had worked on his thesis work with a man named L.C. McKeehan, who was into magnetostriction which was used in sonar.

Rigden:
How do you spell McKeon?

Pound:
I'm not sure. M-c-k I think. And he was a consultant to this Submarine Signal Company. And so Harold spent his summers working for them, and they talked him into coming full time around 1937, and he took over and he gave up his thesis work at Yale. Howard Schultz had done his thesis work at Yale with -- what's his name? -- Ernest Pollard.

Rigden:
Oh, Pollard, yeah.

Pound:
With Ernie Pollard. They shared in the building of the first Yale cyclotron. So both my sisters were married to physicists who had also graduated from the University of Buffalo.

Rigden:
That company your brother-in-law, involved with radar, what did they use for microwave sources?

Pound:
It wasn't microwave B it was actually 50 cm, 600 MHz.

Rigden:
It wasn't microwave.

Pound:
Well, they were developing it when I came. I came down for an interview in November or December 1940 to see about what this -- and that's when I first saw radar running. And Harold built this 50-centimeter radar and it used a Western Electric vacuum tube called the 316. It was called a doorknob tube and that was nominally a 300-volt vacuum tube, but they were hitting it with 2500 volt pulses and making pulse r-b signals. And the people who investigated organizations trying to develop radar at that time wanted to know how come they were the only company that was playing with pulsed radar at that time. And the reason was, they were doing it by pure analogy to their business, which was sonar. And they had been founded during World War I under Fessenden patents, I used to hear -- I'm not sure about that -- for developing sonar. They were founded, I believe, by MIT people actually. And they were very proud of their economic success because they survived during the big Depression by developing what they called Fathometers and renting them or leasing them to the fishing fleets here in Boston which went out to find fishes. And they thought very lowly of Raytheon, which was a more or less defunct company at the time. The only thing they did then was to build vacuum tubes mostly for ham radio and such.

Rigden:
All right. You didn't make it clear. You went from your baccalaureate at Buffalo to defense work where?

Pound:
In the Submarine Signal Company.

Rigden:
In the Submarine Signal Company. And you were there for how long?

Pound:
Well, I started there on February 1st, 1941 and in the summer of '41 -- well, at the same time I was living, I shared living with my sister Kathleen and brother-in-law, Howard Schultz, who was then at Radiation Lab, so I was quite aware of that combination of things, and the Submarine Signal Company was closely involved with advice and help from Radiation Lab, although they were actually ahead of Radiation Lab in the beginning. In fact we built a pulsar for the magnetron. Sub sic had been brought into the knowledge of the British magnetron as soon as anybody, because the Navy, they were high on the Navy's list of corporations that were important -- because they supplied at least 50% of the sonar to the Navy and they did set up experiments on the U.S.S. Semmes, which was the then sort of sailing test bed for the Navy. The Radiation Lab ended up using it for a radar test bed in later times.

Rigden:
Why did you leave and go to the Radiation Lab?

Pound:

Well, in the summer of '41 after I'd been there six or seven months it became clear that Radiation Lab was going to bypass the whole business. To be in research in a small company like that when there were three of us involved in that program didn't strike me as viable. I saw that the company would end up in production of some kind, engineering, and I wasn't inclined in that direction. But I was sent over to MIT Radiation Lab in what they might call "technology transfer" to get briefed and get experience in some aspects of their radar. So I went over there and spent many weeks. Actually I built the first electrostatically deflected PPI, (Plan Precision Indicator), which is the sweeping kind of radar indicator, and Charles Sherwin was building one which was a magnetic type which is what mostly ended up getting used. This was a group that was headed by Bob Bacher, and a man named Ted Seller was the actual head of that group at the time. So I was in conventional electronics, because that was my big experience from my background. And I could see that if I had hung on for a few more weeks before going to Boston, I probably would have ended up directly at Radiation Lab. So I started petitioning, I started to see if I could make this switch. And I would get in touch with F. Wheeler Loomis, who was the associate director, for personnel particularly, and he finally told me that he'd like to offer me a position but he couldn't because with defense deferment I would have to get a release from the company that had got me deferred from the Selective Service.

So I spent some time, in the fall of 1941, this was before Pearl Harbor, they put the condition that they would give me the release if I would finish the actual model of their 50-centimeter radar that they wanted to have as a demonstration of what they had done. It had been working but it was not fully assembled and so forth. We had a pair of big horns on the roof that were steerable. So in fact I went through that and I did that. Finally I got ill in early March '42. And when I got strong enough to go back I went in to see the vice president, a man named Fay Charles and told him this thing was working and finished and so forth, and finally, they signed my release. So then I was able -- So I joined the Radiation Lab the same day that Rabi came -- I'm sorry, that Jerrold Zacharias came back from Bell Labs and started the microwave components group. So I became one of his first tools in the microwave components group in March, 1942.

Rigden:
When March?

Pound:
1942, three months after "Pearl Harbor".

Rigden:
1942.

Pound:
So I think of myself [as] having been at Radiation Lab before that, you see, because of this visiting and other close contacts.

Rigden:
So you were there almost from the beginning.

Pound:
Yeah.

Rigden:
All right. I'm going to switch this tape, because it's almost done. All right. We just concluded that Bob Pound was at the Radiation Lab at MIT almost from the beginning, and I was going to ask you sort of a cultural question. There were a lot of very well established and rather eminent people at the Radiation Lab, and you came in with a bachelor's degree. Were you treated well?

Pound:
Oh yes. I would say I was treated well. Of course I knew that having come from a local environment I didn't have the advantages that some of the people coming from elsewhere economically in the beginning did, because I know that my brother-in-law for example was given a salary plus living expenses so that it almost doubled what he was getting as an instructor in college, but of course instructors didn't do very well in those days.

Rigden:
Yeah, that's right.

Pound:
It was like $1800 a 9-month year or something.

Rigden:
Uh-huh. But you were not treated as a go-for?

Pound:
Oh no, no. As a mat -- Well, you see I was in Zacharias's group and I was one of his -- and within a few weeks I got established as sort of one of his favorite subgroup. I had a subgroup that I organized and although that came along when I was in charge of mixers, but I and a man named Louis Smullin were particularly involved in what they call mixers and duplexers, which was the front end of radars. And we had a major problem, which was, because, in the field when colleagues went out to look at Radiation Lab -- developed radars they would find they weren't working well at all because the military had no clue about the fact that crystals could always get burned out even at the first turn-on, and so they were operating tens of db or more below their expected receiving. But you see, they would see local echoes perfectly all right because the inverse fourth power that applies in radar. That meant that you didn't notice the loss particularly on local things but you really needed the sensitivity for remote.

Rigden:
Let me ask this. In 1941, '42, when the Rad Lab was getting started, how much was known about getting power around a circuit at microwave frequencies?

Pound:
Well, there were people who knew about, say, waveguides but they were mostly academics that had never done anything with them. Page for example at Yale had written some articles describing waveguides. Because I know that my other brother-in-law, Harold Hart, told me that on his oral exam there he was asked a question about that and he doubted it. He had never read Page's article on waveguides. But the big thing at Rad Lab was that everybody was trained, got to know more about microwave technology, because of the wonderful series of lectures by W. W. Hansen. Bill Hansen came every week up [from] Garden City, Long Island, the Sperry Gyroscope Company, where he was full time and gave these once a week lectures which got annotated, got transcribed. Who did that? I keep forgetting. It was one of his cohorts from Stanford who served as the Boswell for those notes, and I still have copies.

Rigden:
It wasn't Packard?

Pound:
No, no, no. Packard was a graduate student after the war and he only got there late. No, no, this was a -- I can see him but I can't remember his name. Names are harder to get as you go along.

Rigden:
But things like the Magic T and various components that went into a microwave system, were those invented at the Rad Lab?

Pound:
Well, the Magic T is an example of a disputable issue, because Bob Dicke was the one that really realized the full properties of the Magic T if it were properly matched to a system so that all four arms were equivalent. However, there was a patent issue that came up about that. And Dicke had a patent application from Radiation Lab for it, and there was a man -- now another name I've forgotten -- at the Bell Labs, who had written a book on microwave hybrid circuits. Not a book -- I'm sorry, a memorandum for file is what they call it at Bell Labs, and we had access to much. All that stuff came in through our documents office. And this was -- and hybrid circuits were the low frequency equivalent to what became a Magic T. And he was discussing how to make them in microwaves correctness and waveguides for example. And it was what we ended up calling a rat ring, a rat race with four arms going into a ring of waveguide, and if you put them at quarter wave spacing and halfway and so forth you got the exact same behavior as you got with a Magic T, although that was before there was a Magic T. But then that paper went on to describe that you could put a waveguide on the side which you called a series junction or on the broad side of the waveguide, or the narrow side and call it the equivalent of a parallel junction. But the difference would be that you'd moved a quarter wavelength to go from one to the other, because then the phase issues were the same. But then he had a picture in his report showing that you could even put two of them in the same place. But that picture, which was a Magic T by everyone's understanding later, was a picture of what you might have as a pair of junctions on the ring -- not a recognition that in itself it was just what he was looking for. And yet that picture won the patent issue, so he got the patent on, Bell Labs got the patent on that thing.

Rigden:
One more question in this same vein. To what extent were you guided by any theory that say Julian Schwinger was doing?

Pound:
Very little, because in my design of mixers -- Julian came back to the Radiation Lab only late, you know. He wasn't there the whole time at all. And actually when we were worrying about the crystal burnout problem early on, Julian was brought through by Hans Bethe at one stage, and he brought, Hans Bethe brought Julian as a young man -- this was in '42 -- to worry about the problem we were having about what we called feedthrough for the transmit/receive switch. That was Lou Smullin and I who were doing this. And Julian thought about it and the next morning proposed a way to redesign the resonator for the TR box that might reduce this feedthrough. And we lost many weeks building this thing this way and it turned out it didn't make any difference. It turned out that what he proposed had to be -- it weakened the transmission but it also weakened the coupling in such a way that when you adjusted it with the right way to get back where you had to be, it had the same feedthrough.

Rigden:
So --

Pound:
So that was the early time, and then he went off to Purdue, where he taught for some time. And then when things got tight he came back to Radiation Lab. And then he -- oh, what I was going to say is, I designed a mixer in which I had to have coupling from the -- you have to have the local oscillator feeding into the mixer, but not interrupting the flow of the weak signal. And I did, instead of cut and try as we usually did with these things, I tried to use Schwinger's network theory to design this exactly, and it took me days of hand calculations to do, to get the equivalent circuits to all these corners and things that he -- and when I got done it really was much easier to have done it just by cut and try.

Rigden:
Now at this time you were the section chief for mixers?

Pound:
Yes.

Rigden:
Did that increase significantly your responsibilities?

Pound:
Not really. I had two or three members in my subgroup as it was called, but I shared an office there with the people who were in charge of the crystal development, namely Henry Torrey and Charlie Whitmer, and so we were very close friends and that's how we all of course, Henry and I got together with Purcell on NMR?

Rigden:
Yeah. It was a different environment in the sense there was, everything was more, I guess everything was classified, there was security visible. Did that bother people used to freedom in the academic world?

Pound:
Well, before I answer that question I might tell you how I made my situation with Zacharias and company, because I was assigned the job of designing or finding out -- I'm going to start over again. The lab had decided that they couldn't use bead-supported co-axial lines anymore; they wanted to use stub-supported; stub-supported lines, which means you put a little quarter wave long stub that's short circuited at the end to support the inner conductor, and it doesn't have all this reflection problem of the former plastic beads. But it has to be an effective quarter wave, and at 10 centimeter wavelength, which is what we were using then, there were three sub-bands: 9.1, 10.0 and 10.7 centimeter wavelengths, and I was assigned the job of finding the right length for these stubs for these three different wave bands, and I said, “Hey, there must be a way of combining a couple of things that maybe make it broader band.” So I came up with this idea of broadbanding and combining a half wave transformer -- which was known to be frequency-sensitive with a quarter wave stub -- and learned from Hansen's notes, Hansen's lectures and so forth, how to use -- what do they call them? Smith charts -- and solve the problem as to how it worked. And it turned out that that became the way to supplement microwave things. And in fact I can show you that. Let's see. Yeah. And this thing is B there's a patent applied for by the Army Signal Corp.

Rigden:
We're looking at a figure dated August 10, 1948, which is “Apparatus for broadband radio transmission, inventor Robert V. Pound,” signed by an attorney.

Pound:
Yes. Well, what we had to do was, I had to write a report for the Radiation Lab. And Zacharias, in order to get this report officially approved, and I had to go and see Sam

Goudsmit, who was in charge of these things at that time. And this was in the spring of 1942. And Zach took me over to Goudsmit's office where I was to give Goudsmit a lecture on this device. And I knew the name of Goudsmit at the time, coming from where I did. It was a small scale place, but here was one of the big names that I knew, and so I gave this talk to Goudsmit, and Goudsmit said, "Where did you learn so much?" And so I was rather happy with that.

Rigden:
That was nice, yes.

Pound:
Because what made my status in the -- that was about two weeks after I first came permanently into the microwave business.

Rigden:
So that was right at the beginning.

Pound:
Yes.

Rigden:
Okay. So you established your abilities quickly.

Pound:
That's right. With Zacharias at least.

Rigden:
All right, back to my question. Did the security issues and the classification and all bother people?

Pound:
Well, yeah. Of course it did to the extent that Ajax Allen got shot in the stomach by the security guards.

Rigden:
Oh really? I've never heard that.

Pound:
No. Well, it was -- after one of our Monday night seminars we had regularly, and Allen decided to go back to the lab for something. He was actually in charge of buildings and grounds for the Radiation Lab, and he was challenged at the guard's gate, and he saluted them thinking that they would recognize him and he just drove on, so they shot him. And he had to go to the B to the hospital with surgery for gunshot wounds in the stomach.

Rigden:
Was that early in the --?

Pound:
No, that was around 1944 or 45. Early we had a simple kind of guarding system with retired Cambridge policemen and such people, but after Pearl Harbor then there was an Army group that came with their riot guns and everything and they stood at all the places. I might say that when I got interviewed for the Society of Fellows, the rather eccentric Englishman, Arthur Darby Nock, who was then acting chairman always used to tell people as to how he went down to the MIT secret laboratory and to see Rabi, who was one of my sponsors at the time, and he said he got -- he was so impressed, because he got past the guards there. You know, he got escorted through the guards and so he was quite impressed with that.

Rigden:
How much were you aware and others at the Rad Lab in general about what was going on at Los Alamos?

Pound:
Rather little in the sense of detail, but rather thoroughly in view of who was involved and that they were into this particular aspect of nuclear physics. Because of course many of our colleagues were recruited away, and people don't generally realize how many of all the important people were at Radiation Lab in the beginning, including E.O. Lawrence and including Backer and we referred to it as Shangri La.

Rigden:
Bethe.

Pound:
Bethe, and Ken Bainbridge in particular. He became, you know he was the head of the Trinity Test, and so he, I knew him fairly well from the early times, but it was March 1943 that he was recruited out to Los Alamos. He was the first person actually to have been recruited to Radiation Lab in 1940. Because E. O. Lawrence was a major recruiter for the Radiation Lab, and he came -- but that's because he was a friend of Alfred Loomis, who was the member of the -- what do you call? -- National Defense Research Committee, with Compton Conant Vanevar Bush. And he chaired the microwave subcommittee, and Lawrence knew him very well because with Alfred Loomis' interest in technology he had become the financial source for Lawrence and his cyclotrons. He got his banker friends to help support the cyclotron. So then Lawrence came here to Harvard apparently, as Ken told me, Ken Bainbridge told me, and asked him to come over and walk in the yard where there would be fewer ears close by and talked him into taking leave and coming to MIT which was just founding the Radiation Lab. So he was the very first. He was number one.

Rigden:
Well, when the war ended, there was a Time magazine that was scheduled to have the radar story as the cover.

Pound:
But it was bypassed by the --

Rigden:
It was bumped back to page 82 or something like that, and the bomb took the cover.

Pound:
Oh yeah.

Rigden:
Did the people at the Radiation Lab feel that they had really been overlooked in terms of the importance of the work that they had done?

Pound:
I'm not sure. I think they were also overcome with excitement over what had happened at Los Alamos in fact, but they knew perfectly well that what they -- it used to be said I guess you've read in a number of places that they said that the bomb ended the war but the radar won the war.

Rigden:
Yes. I think --

Pound:
Rabi said that.

Rigden:
Well, I think Lee DuBridge said that during the big final talk he gave.

Pound:
You mean during the closing --?

Rigden:
Yeah, at the closing ceremony.

Pound:
Ceremony on VJ Day.

Rigden:
I think he said that.

Pound:
That's the thing that's in this --

Rigden:
Yes, it's in that book.

Pound:
Oh, that's that picture by the way, in color.

Rigden:
You have said that those years at the Radiation Lab were, quote, “one of the three most exciting and all-consuming of my career.”

Pound:
That's probably, that's true.

Rigden:
Can you just elaborate on that a bit?

Pound:
Well, it was of course the involvement, it was of course the intensity of having to keep going at things and knowing it was going to have some value -- plus the fact of being involved with all those, say 50 percent of the distinguished scientists or physicists in the country. And for example it wasn't just physicists either; I spent the evening the other day, well, I spent lunch on Tuesday entirely with Paul Samuelson. You wouldn't expect that he was a member of the Radiation Lab, which he was.

Rigden:
He went to Stanford later.

Pound:
Paul Samuelson? No.

Rigden:
No, no, no. He's the economist.

Pound:
He's the economist at MIT. And the reason that he got to Radiation Lab was that he was a junior fellow in the Society of Fellows, and there were two other distinguished junior fellows who played very important roles at Radiation Lab. One was Ivan Getting and the other was Dave Griggs, and they were close, they were contemporary colleagues. And Jim Fisk, Jim Fisk who became president of Bell Labs. They were all junior fellows at that same time that Paul Samuelson was, so they put him into -- I can't remember what group he got into. But of course Paul had that particular interest in physics as much. He asks me such things as, “Can you explain entanglement?” to somebody and I said, “Not me, no.”

Rigden:
Two more questions on the Radiation Lab. Were you at ease with the whole writing effort at the end when all of you folks were writing these books? By the way, not all were in the writing operation.

Pound:
I wouldn't know what you mean by being at ease, because we -- I didn't like to spend my time while I was down there sitting at a desk writing, because some of my apparatus was being, had been hijacked into the continuing Research Laboratory of Electronics RLE. And in fact, following a suggestion I had made, Al Hill and Arthur Roberts and somebody else were using it to look for the hyperfine structure in absorption of cesium in the microwave stabilizer. So I would go on kibitz and look at what they were doing whilst I was supposed to be writing the books. And seeing all these people getting on with their lives, and here I was stuck to have to sit down and write this book.

Rigden:
Well, my question was -- my understanding is that this writing effort was launched by Rabi, who said something to the effect that we didn't write it up all of this knowledge would go to Bell Labs, and he felt that --

Pound:
Well, did he put it that way? I see. I hadn't known about the worrying about Bell Labs, but I thought he thought it would be lost, but never mind.

Rigden:
And he thought it ought to be available to everyone.

Pound:
Yeah.

Rigden:
And these books became very important after the war.

Pound:
They did. Right.

Rigden:
So did people see the rationale for that extensive writing effort?

Pound:
Oh, I think that they recognized that there are quite a lot of -- that the changes and advances in technology of that kind was very, very significant and it shouldn't be lost and we should probably gain the credit for it if that's -- I think anybody was taking the credit for it, but I mean you know, people like Brit Chance who had done some great things in precision measurement techniques and so forth, these were things that had to be preserved. And of course my, I have that book I wrote. I don't know if you've ever seen it, but is this the one?

Rigden:
I have that book, yes.

Pound:
That -- this is one of the things that's described in it.

Rigden:
And this is the Rad Lab book, volume number 16, Microwave Mixers by Pound.

Pound:
And there is the most sophisticated microwave mixer, which has three Magic Ts. That was inspired jointly between me, Bob Dicke and Ed Purcell.

Rigden:
Now in something like this, did you present drawings to a machine shop and they made it?

Pound:
Well, it's worse than that. This is the rough B this is die casting which we had made by Yale and Town, who were the people who make Yale locks down in Connecticut. But the first model, the test model of this was made by a very distinguished local flute maker.

Rigden:
Haines? No.

Pound:
William S. Haines, yes. And I went down -- we had a man named Jules Simmons who took charge of our getting things made and getting things done, and he took me over to William S. Haines, which used to be over Mass Station in Boston, and I went over there whilst they were machining this thing out of solid brass. And they were doing a beautiful job. And they such -- a wonderful thing. And they had just -- a man at Case, a University in Cleveland, a mathematician had just died and it was announced in the newspapers that he had a collection of Haines flutes, platinum Haines flutes, and they said they hated platinum because it's awful stuff to work with. But they had one man working on a desk repairing flutes, and everybody else was doing our kind of thing, and so they said would I like to try to play a flute. And I said, “Oh, I couldn't touch it,” and he said, “Do you want to hear the flute?” and I said, “Sure.” So they called a man from the milling machine and he came over and washed his hands and he picked up a flute and he started playing Bach and other things, and he said well, he used to be in the symphony, but he said, “The life of a musician is very tough these days,” so he preferred his life as a machinist. And he was also, they were also making at the time tapered hexagonal -- they had metal violin bows they were making, and they were made out of tapered aluminum, hexagonal aluminum, the backing of the bow. And I said how did they make that? Where did they get that from? They said well, they tried all the aluminum makers and nobody would do it so they had to do it themselves.

Rigden:
All right. One last question about -- When you closed shop at the Radiation Lab, you were certainly aware that a lot of new physical techniques had suddenly become available.

Pound:
That's right.

Rigden:
As you looked ahead was there a great sense of sort of anticipation as to what would be possible in physics following the war years?

Pound:
Well, I think most people thought more in terms of what you could do in the way of accelerators with our microwave technology, and I of course was looking for a way to do something in physics with my limited background. I hated glass blowing among other things, and my mentor Hector for example spent a long time trying to make a vacuum gauge out of a receiving tube, type 59 I think it was, and never succeeded in sticking a pipe onto the side of the vacuum tube glass. And that's because these glasses are not compatible. I mean, you never get glasses with the same expansion coefficient. So that kind of thing put me off a bit on having to get into that kind -- And

for example my students later have quoted me as saying, “Pound abhors a vacuum,” so [laughter] I was also trying not to have to get into things like molecular beams. But I did think, among other things, that microwave spectroscopy was going to be of an interest. And in fact I had been trying to relearn some physics or learn some physics I had never fully learned, and I got the little book of Herzberg on atomic physics, on atomic spectroscopy, and there was a footnote about the fine structure constant of hydrogen, the wave number of which it said had been measured as .3 per centimeters. And I say, “Hey, that's 3 centimeter wavelength, isn't it?” And I thought that might be something to do, but I never got involved in it as did Lamb. I once mentioned that to Herzberg when I met him in Toronto at one time, and he said, Lamb said that's where he found [out] about it too.

Rigden:

Okay, we were just finishing up the comments about the Rad Lab and you were saying?

Pound:

Yeah, I was saying something about the ammonia microwave absorption which had been well known from Leeton and Williams from the early thirties, and they had done marvelous things in that they built their own magnetrons to do that with and so forth. And I was quite aware of that, plus the water vapor absorption. But my particular interest at the time was that I foresaw building an atomic clock, because my -- one of the things I was most celebrated for at Radiation Lab was developing the technique of frequency stabilization, and all kinds of the early microwave spectroscopists referred to the Pound stabilizer as being what they used to get good signal sources and so forth. And so I, but I had -- in fact in my interview with the Society of Fellows proposed using an atomic absorption line like ammonia or whatever to stabilize instead of a cavity which I was using then, because that's not absolute, whereas this other one -- And I foresaw using that to measure the difference in different kinds of timescales, and thus I was thinking in terms of -- I had been influenced by reading an article about E. A. Milne who had some proposals of kinematic and dynamical timescales by which the difference in the two timescales should change by about one over the age of the universe per year, which meant in those days about a part in 10^{10} per year if those two timescales really existed, one based on atomic spectra and the other on gravitation or whatever. So that was one of the things that I had the greatest enthusiasm. And then of course what came along was this idea of Purcell's of looking for the absorption of protons.

Rigden:

All right, before we get to that, let's go back. You almost provided the perfect springboard. How was it that you got the attention of the Society of Fellows and were asked to become one?

Pound:

I was invited.

Rigden:

You were doing classified work. How did they know about you? Of course there were these other people there.

Pound:

Well, you know the Society of Fellows functions only by nomination by sponsors, and the person that first told me that -- My colleagues knew my ridiculous status of never having gone to graduate school, and but it was Al Hill that first mentioned it to me. He said that he had developed the plan of having me, nominating me to the Society of Fellows at Harvard. I didn't know anything about it then, but we had several Radiation Lab people, as I mentioned -- Paul Samuelson whom I didn't know then, but the one I knew best was Ivan Getting, and he had been a junior fellow for six years I think, which was illegal. I mean six years was the maximum, but the one that was illegal was Dave Griggs, who had been seven years, because he spent one of those years in the hospital. But he got into Radiation Lab because he flew a plane. Goetting got him to fly his plane as a target plane for testing the early radar, so that's -- and then he ended up being part of the Radiation Lab. Of course he got a bad name in the long run for his testimony against Oppenheimer.

Rigden:

What was his name again?

Pound:

Griggs.

Rigden:

Griggs. Okay, yeah.

Pound:

Dave Griggs. He was a geophysicist while he was a junior fellow. He was using Bridgeman techniques to observe the creep of rocks and he had an experiment set up in the basement here of Jefferson on that subject.

Rigden:

You were interviewed by [Alfred North] Whitehead.

Pound:

Yes. What they do you see is B and I served on this as a senior fellow about six different years in the postwar era, but they had me come down to Eliott House and they had something like the six Senior Fellows had lunch and then they invited me in to be questioned about what I was interested in and what I was doing, etc. So that group included Whitehead, Arthur Nock, Paul Buck was then provost at the university, and a wonderful man named Fred Hisaw, who was a biologist. We used to call him Mr. Sex or something like that because he was an expert in many respects. He was a Senior Fellow. And I think Henry Shattock, who was one of the old Boston people, once later he told me he was stopped on Beacon Street and asked, “Sir, are you by any chance the late George Apley?” He was a perfect model for such. Anyway --

Rigden:

What did Whitehead ask you? Do you remember?

Pound:

Well, I think I talked a little bit about the interest in these timescale issues with Whitehead. Because of course Whitehead had his own relativistic theory at one stage. But then they also asked me about what I read, and I told him my wife had put me onto reading Dostoevsky recently and what did I think. Oh, I thought it was wonderful but also very tearing you know. I think it was Crime and Punishment or whatever.

Rigden:

So who were some of your contemporaries as a junior fellow?

Pound:

Oh boy, that was quite a group, because one of the first ones I met was a man named John Kelleher, who was a Celtic historian and scholar -- not a linguist particularly, but he was an Irish American, and I soon discovered, the first day I met him -- He ended up being the professor whose professorship was endowed by Henry Shattock. But in any case, he, when he learned I came from Ridgeway he said he knew about the Battle of Ridgeway. And he said -- turned out that his ancestors had helped organize the Irish into a raid which became called Penian raids, which was an attempt to take over Canada for the United States -- or to drive the representatives of the kingdom out. And this occurred in my home village, and there is now a little museum to it and so forth. In 1866. And these Penians, the leader of this group they called him General but he was Colonel O'Neil in the U.S. Civil War, the Union Armies who had driven back some southern raiders who were raiding all along the Ohio River Valley and so forth, and he was considered an extremely able warrior. And he organized two thousand Irish immigrants who had been in the Civil War as well to cross the river and attack Canada. And it was the Battle of Ridgeway and it lasted only one day, but John and I have had that relationship ever since. I just read his obituary because he died January 1, 2004.

Rigden:
Were there any other scientists as junior fellows?

Pound:
Not physicists, but there were -- Don Griffin was a biologist and very -- Oh yeah, and Don Griffin is the man who studied homing of bats -- I'm sorry, the homing of pigeons -- and he was the man who studied the sonar properties of bats. And he had the help of a retired Harvard physicist called George Washington Pierce, who had studied insects as well and had ultrasonic equipment that he lent to Don Griffin. Don Griffin, he was still with us in a dinner party last week. And then there was a man that helped me, it was a chemist. The chemist junior fellow, Martin Ettlinger, got to use the chemistry professor's laboratories at Radcliffe over in the old Radcliffe Byerly Hall. Ettlinger helped me there was -- he helped me make solutions of gallium chloride -- that I could look for the NMR. I measured a lot of nuclear magnetic moments using NMR. Another distinguished biologist was Carroll Williams who went on to discover a growth hormone by his studies of insect physiology.

Rigden:
When did you start your tenure as a junior fellow?

Pound:
Technically July '46, but I actually was involved from July '45.

Rigden:
So when you did the NMR work you were a junior fellow?

Pound:
Yes. And lot of people suspected I think that I got appointed because I had done the NMR work, but I was not; I was already appointed before that came up.

Rigden:
All right. Well let's now look at the postwar world. Just a general question. When the war started physics was really interrupted.

Pound:
Definitely.

Rigden:
And there was building intensity in physics in this country in the late thirties. And then everything stopped, and now suddenly the war is over. Was there a sense amongst you and your other friends that you were going to pick up where you left off, or how did you sort of think you were going to get started again?

Pound:
Well, I think, well as I say, in the dismantling of the Radiation Lab I saw all those people going back to where they'd come from mostly, and the dominant thing of physics in those days was of course nuclear physics and cyclotrons and all that. And Harvard was rather limited in that respect because it had lost its cyclotron to Los Alamos. And Ken Bainbridge, who had built the cyclotron before the war, and it was quite successfully operational before the war, whereas MIT had attempted to build one that didn't work very well, so they used the Harvard cyclotron quite a bit. And I profited from that a little bit in the postwar era because they were generous to us to use the postwar MIT cyclotron for radiations occasionally. And so, because they felt an obligation from our willingness. But anyway, no, I think people were certainly looking for using the new technologies, and of course people like -- one of the most distinguished people at Radiation Lab was Jim Lawson for example, and he went off to General Electric in the new Knowles Lab I guess it was. But he in particular oversaw the building of a betatron there. Now was it a betatron or a synchrotron? I think it was -- I know that he sent some information at one time about the volume of stuff that the pumps had succeeded in pulling out of there. It was tons. It was amazing. But anyway. So if you've read Alvarez's book, people like Alvarez and Bill Hansen, they all tried to make accelerators and apply that aspect of the technology.

Rigden:
So that was the big initiative after the war.

Pound:
That's right.

Rigden:
All right, talk about your lunchtime, going to lunch with Purcell and Torrey.

Pound:
Yes. Well, I had been in the habit going out --

Rigden:
This was summer of '45? No, later.

Pound:
Yes. It was after VJ Day in the fall of '45.

Rigden:
In the fall of '45. Okay. Go ahead.

Pound:
But I, with a man that went to Yale after the war, Bob Beringer, used to go to that lunch there for quite a long time quite often. I would go over and Bob Beringer worked in the same room with Bob Dicke, and sometimes we'd get Bob Dicke to go up there to this Hennesey's Bakery and Deli for lunch. For 40 cents you'd get this marvelous lunch. They made their own bread, they made their own Boston cream pie, and all these things. It was a bakery and it was a delicatessen.

Rigden:
What was the name of it again?

Pound:
Hennesey's.

Rigden:
Hennesey. Okay. Go ahead.

Pound:
And so then as things started disintegrating B I don't know whether Beringer was still around, but he ended up at Yale building accelerators at Yale. But Henry Torrey and I occupied this, Henry Torrey and I had the same office suite, and so we went off to lunch that day and said, "Let's see if we can get Ed to go along." Ed [Purcell] choose a different group. We weren't in his group. He was down the hall and across and I used to spend a lot of time over in his office because in what I was doing in this thing, the Magic T and all, was very close, that was their concern. And so, and another person in his group that I spent a lot of time with was Carol Montgomery who was the co-author with them on that book. He died soon after the war but -- And he had been Bob Beringer's thesis advisor before the war. They did the first directional correlations, they did the directional correlations of annihilation radiation, positrons. Anyway, so we picked up Ed, and he said, "Sure, okay," and he went along. Of course we walked from 77 Mass Ave. up Massachusetts Avenue to Central Square. And it was in that walk that Ed, knowing that Henry had been a member of Rabi's group, asked him this question about whether you think that you could detect the absorption of a two-level system of protons in a magnetic field as an absorption. And he thought, they used to talk about that at Columbia. And of course that was the after effect of [C.E.] Gorter.

Rigden:
Yes.

Pound:
And but Henry couldn't remember quite why they didn't think it would work. So he went home that evening as I've said I guess, that he went home that evening and as was his wont -- he always loved to calculate things, and so he did some calculations and he got quite excited because he decided, "Hey, it should be possible," and he went the next morning to see Ed and told him, and Ed said, "So let's do it." So that's the way it got started.

Rigden:
Well, do you remember there were calculations by Teller and there was one --

Pound:
Heitler and Teller.

Rigden:
B that said that the relaxation time was eons. You know, I mean it was --

Pound:
No, they didn't say that. They said, well, Teller -- the Heitler and Teller calculation, which Henry found in his search -- You see, what happened was we decided to get started on that, and then Henry started thinking, you know, "I assumed thermal equilibrium when I did this calculation. I wonder how that comes about?" And we went over and talked to Ed and together. Henry said he would do a literature search and try to find what happened, what would be the case. And he came up with this paper of I. Waller.

Rigden:
I. Waller. That's what I was trying to think of, yes.

Pound:
And I. Waller had calculated for electrons, not nuclei, and Henry sat down and converted it to apply for protons as compared with electrons in solid and he came out with a view that it might be an hour or so. But he made some -- And this was that he realized it was not the first order effect but the second order effect that dominated because that allowed you to integrate over the whole spectrum of phonons and that at room temperatures and well above the lattice temperature you could have that help, and so he gave out with the view that it might be some hours. But it turned out that he used incompatible numbers in the long run. If he had done it right he would have been more discouraged in the hours because our system was designed to work even though it would be hours. Because once the relaxation had happened our big cavity and the rf level we would use with it would be so low that it wouldn't disturb the equilibrium significantly in less than several hours. And that was one of the reasons we used this big cavity instead of a coil. But the other reason was that we were so immersed in microwave technology and so forth that we never thought of not, of just using coils and capacitors.

Rigden:
Let me ask a little tricky question I think. You and Torrey and Ed Purcell, when you were walking to lunch you were all young and you were all early in your career. What established Purcell as the head of that experiment?

Pound:
Well, he had made the suggestion. He raised the question of could you do it, and as you say, it was the fact that in his book writing he had been writing down the history of the discovery of the absorption due to water vapor, and he came to the realization that there were just two particular levels whose energy difference was just equal to that frequency. And although there was a system of hundreds of levels, and if anybody looked at it carefully enough they might have discovered that particular pair, but [David M.] Dennison I think it was at Michigan had published from infrared spectroscopy a description of all those energy levels of the water molecule. So and of course there you didn't have a population problem. Well, it's the same differential population for those levels because of their energy difference, but it's populated at the atmospheric temperatures through a very large number of those levels. So he said, "But you can get a two-level system in absorption by making the frequency match the energy of the energy gap," and so that was the question that started the whole thing, and that's nominally what made him leader. Although in a sense I think that all three of us were pretty important to it.

Rigden:
Ed told me that when you were doing that experiment that you, Bob Pound, knew more about -- and I think he said signal-to-noise -- than anyone in the country.

Pound:
Well, I think that's an exaggeration, because there were some people at Bell Labs, and there was a man who ended up for a while at British Columbia and then at Michigan, Michigan State, who wrote books on these subjects. But there was a man named Rice. I gave a course here when I was a junior fellow on limits of sensitivity and detection and so forth which actually was under, oh, engineering science and applied physics department, a predecessor of applied science to the division, and that was in '47. Was it spring '48? Maybe. But I learned an awful lot of that, and the real details of what I could talk about from my having the manuscript of Lawson and Uhlenbeck,

which was one of the Radiation Lab's books, and that was really the best work of that kind that existed then.

Rigden:
Do you see your role in the Purcell experiment sort of analogous to Hansen's role in the Block experiment?

Pound:
Not that much, because I think that Hansen's role was greater than most people realize, in that he was the man that really understood how to make the thing work, the circuitry and everything. And I have seen his notebooks from May of 1945, which was well before we even thought about it, in which he was designing a balanced amplifier for this thing. Then he had a question under this that said something about how the nuclei would react to this or that. I don't remember exactly what the question was, but at the bottom he said, “Ah, but leave it to Felix.”

Rigden:
But you essentially designed the apparatus, did you not?

Pound:
Pretty much, yeah, I guess so. Well, Ed and I participated in designing that cavity, and I had my technician build it. I had a very good private kind of -- Charlie Rowe, who had been my sort of electronics and machinist private guy in my little subgroup there all through the last couple of years, and he hadn't anything much to do and was waiting for an opportunity for his new job after the war, so I put the drawings we had made and asked him to build it, which he did. And he was very good. And actually my college mentor, Grant Hector, hired him after the war to National Union Radio Company which he had taken on as the head of something or other. Because you see he had spent the war years after I left, then he left and joined Merle Tuve in the proximity fuse development. So he became very involved in making these little hearing aid, well, these little vacuum tubes for proximity fuses.

Rigden:
Let me say -- I should have probably made it clearer. When we just talked a little bit about the walk from MIT to Central Square, this was the discussion that launched the discovery of NMR in bulk matter.

Pound:
Yeah.

Rigden:
Which of course has had enormous significance in the following years. And so Bob Pound was a part of that along with Ed Purcell and Henry Torrey. Why don't you just tell us about the experiment?

Pound:
Oh, well, as I say, the experiment was based on using a balanced bridge. In one arm the cavity that contained the absorbing sample, which was paraffin wax. We had about 2 pounds of paraffin wax in this cavity, and that's some stuff that Purcell had bought at the little local grocery store on the way over that evening. And we melted that and poured it into the cavity. And on Thursday, the 13th of December, we spent the evening trying -- We had everything set up. I brought everything up from MIT. The only thing is Harvard -- it's always called a Harvard experiment, but the paper is published under the byline of Radiation Lab MIT. And, you know, that's right. All of three of us were full-time employees, and we were doing this clandestinely. And I once said that in the presence of Al Hill and he said, “I knew what you were doing!” So anyway, the 13th of -- we met in the evening of that Thursday, and I have told various people about this fact that I had that afternoon read that article in The New Yorker in the “The Talk of the Town” which is describing the discovery of nuclear fission, and that J. R. Dunning of Columbia had been hearing that talk from Bohr which was given at -- it was at the New York Physical Society Meeting I think.

Rigden:
I think so.

Pound:
And he heard that talk by Bohr, and he decided he wanted to see for himself, so after the talk he went back to the lab at Columbia, set something up and saw these big flashes of tracks on his -- I don't know what kind of detector he was using, but they were big things that indicated he was seeing this big energy from the fission. And so then The New Yorker said he closed it down and thoughtfully went leaning into heavy weather. It was pouring rain or something like that, and walking, contemplating about the new world that was opening up this way. So when we had that Thursday night there was a snowstorm here. And this was almost the first time I'd been here. No, we had come over in preparation for that experiment. We had to measure the field of the magnet and we got -- Ed had the machine shop here make new pole pieces to try to get a uniform field and used Rose shims on those pole pieces which turned out to be overdone. And but we brought down from the attic, from not the attic, the fourth floor electricity lab, a wall galvanometer called a ballistic galvanometer in order to measure the field with the flip coil technique. Of course nowadays the student would say, “Why didn't you use NMR?” But anyway, so we used this flip coil and calibrated the field. And I don't know if I ever showed you that. Did I ever show you that --?

Rigden:
Let's just pause a minute. I'm going to switch the tape. Okay. We were talking about calibrating the magnetic field on the night of Thursday, December 13th.

Pound:
No, that calibration was done sometime before that, I mean a couple of visits before.

Rigden:
All right.

Pound:
And we had the thing, the magnet, calibrated and the pole piece, that was after Ed had had the pole pieces modified by the machine shop here. And the other item that Harvard's contribution was the general radio signal generator. It was applied as a 30 MHZ source. Other than that and the magnet the rest of it all was stolen from MIT. I use the term stolen as a euphemism I suppose, but one of the things that came from MIT was also, it came out of my crystal test set thing, was this Hallicrafter high-frequency radio receiver. That performed the main amplification and detection whereas it was preceded by an MIT Radiation Lab preamplifier that was used from radar, because, see, 30 MHZ was the frequency of the intermediate frequency radar, and so we had the very lowest noise amplifiers available in those days. Henry Wallman designed that fancy circuit which got the noise figure down to 1 or 2 db, and that was at the front end. So, and then it fed the Hallicrafters, which came from my apparatus in my lab at MIT and it had a meter at the output which was the only way of seeing the signal. You watched this meter and you would balance this bridge by adjusting the amplitude on one arm compared with the other, adjusting the frequency into the peak of the transmission through the cavity, and then balancing it down by adjusting the intensity and the phase. And the phase was adjusted by using little line stretchers that were intended for 10-centimeter wavelength and here we were working 10-meter wavelength, so we had a line of these things so that you could get enough adjustability.

Rigden:

But Bob, go back to Thursday evening. You were talking about Thursday.

Pound:
And then by Thursday everything was together, and then we started trying to see if it would work.

Rigden:
So you actually started the experiment on Thursday evening?

Pound:

Yes, we started the experiment on Thursday evening, which meant turning on the magnet and getting the water flowing through, the water cooling on the big magnet. And the control of the magnet was a rheostat on a panel on the wall, which was many feet away from where the experiment was, and the meter sat on that table, and everything in this balanced bridge was terribly microphonic so you didn't want to touch the table or anything when it was balanced down. Because you could only balance it down so far, because the frequency sensitivity of the two arms of that dumb bridge were different, because one had the cavity and the other just had an untuned attenuated path. So you could only balance it down to about, by 60 db or so, because the noise sidebands weren't balanced in the same way on both sides. So that would determine how much rf level we could use. And then we adjusted it down until we could no longer balance more and the meter was standing at, reading noise, so then we started adjusting that rheostat around the field which this calibration said would be the right value, because we knew the g value from Rabi. And we worked from mid-evening until three or four in the morning trying, rebalancing and adjusting and so forth. No result whatsoever. So finally I had to go off in that snowstorm without the contemplation that I could think about what was happening in the future. I was the only one that had driven, and I drove Ed back to his house and Henry back to where the Torreys were renting and living down near Mt. Auburn Street, and my wife Betty had been with Henry Torrey's wife, Helen.

They spent the evening together wondering what we were finding, and I had to say we hadn't found anything. But we made a compact about coming in on Saturday, thinking maybe it was the relaxation time, it was too long. Although having spent all that time, it was relaxing all that time, when you think about it. Because the field was on also at the time. So anyway, Ed agreed to come in around seven in the morning on Saturday and turn the magnet on and let it cook until we would come in. Because in those days we still were expected to work on Saturdays in principle, although I don't think anybody would fine us if we hadn't. But we came in about two in that afternoon and then we started all over again. And the same effect. Everything happened. Nothing happened. The meter never made the bumps. So, about late afternoon, 5 o'clock or so, we decided, “Well, let's shut it down and try to think of what we might do to improve things.” And I said, “Why don't we just turn the magnet all the way up?” That magnet, that generator which was in Cruft, remote from there and it was the field control we had there -- it was very coarse -- we turned it up. It was 100 amperes at 500 volts, so that was a lot of power. We turned it all the way up to the top and then came down slowly. And as we came down through 80 amperes it went bump. There it was. And that's because in this calibration we were seeking it at something like 73 amperes.

Rigden:
Seventy-three amperes I've recorded.

Pound:
Yeah. And it turned out it happened at 82 amperes.

Rigden:
But why, given the uncertainties, did you stick with the 73 for so long?

Pound:
We didn't.

Rigden:
Why didn't you just explore?

Pound:
Well, we did. We went plus and minus 10 percent relative to that thinking we couldn't be wrong by more than that.

Rigden:
Okay.

Pound:
And the fact was we were wrong by more than that -- not because our calibration was wrong, it was fine, but it was what we had forgotten and didn't look at it properly was that the magnet was saturated. So it took all that more amperes to get 2 more percent field. We were only off 2 percent in the calibration, which is pretty good for that kind of system, but it took 15 percent more current in order to get that 2 percent more field.

Rigden:
So it was on Saturday 15th --

Pound:
That's right.

Rigden:
-- that you discovered the first absorption of hydrogens in paraffin.

Pound:
That's right, that's right. And one of these pieces of paper has Henry's signature on it -- not Henry's signa -- Oh, there is a copy of the circuitry, but -- Yeah, well --

Rigden:
What was the reaction of you and Ed and Henry when that happened? Were you elated that you --?

Pound:
Oh, yeah, we were very -- we first saw that we had now something to go with to what we could see what we were going to be doing for a while. And I would give up my ideas of building another atomic clock for a while -- although I was supposed to be. At Harvard there was a -- you know E. L. Chaffe was. He was the main electronics specialist at Harvard who built big vacuum tubes and things. But he ran the electronic research lab, and he had some graduate students. One was going to make an ammonia-based atomic clock following my suggestions. That guy got -- he went down the wrong route because he instead got enthused not about how the stabilizing system how well it worked, but rather to see what you could do without stabilizing it by stabilizing all the power supplies and protecting the temperature and everything.

So that didn't get us anywhere.

Rigden:

Well you've already suggested it. I'm going to ask the question. When you found that you --

Pound:

That's the one that's Henry's writing.

Rigden:

Okay. All right. When you found that you had succeeded on this NMR experiment, that put your thoughts of atomic clocks on the side burner or back burners, and did you sort of quickly decide that you were going to pursue this for a while? And in fact it had a major influence on your career.

Pound:

Yes, except that it was always in the back of my mind that I might discover a reference system for the concept of the atomic clock that would be better than the ammonia one. And in February of '47 Rabi came and gave a colloquium talk. And in that talk he talked about the work of [Willis] Lamb and [Bernard] Feld, which was discovering quadrupole splitting in molecular spectroscopy in molecular beams. And I went away from that saying, “My God. Surely we will have something like that in perfect crystalline solids” and so forth. So I immediately began pursuing the idea of quadrupole resonance as distinct from nuclear magnetic resonance. And it's for that reason that I built the frequency-scanning marginal oscillators because with quadrupole resonance you have no control over the frequency. It's whatever the solid gives you. So you have to have something you can scan in frequency. And at that time it was almost impossible to make a system sensitive enough it could be flexibly scanned in frequency. So everybody did their NMR by scanning the field, and that brought it through a fixed frequency. There was no problem.

Rigden:

Yeah. Well, we'll come to quadrupole in a bit. Let's go on to your paper in '48. One of the most cited papers I think in physics is the BPP paper -- [Nicolaas] Bloembergen and Purcell and Pound. How did you get involved in that collaboration?

Pound:

Well, I was part of the advisory system when Bloembergen came, and since Bloembergen arrived in the beginning of '46, and Ed got him, appointed him as a research assistant. And I oversaw most of the -- I educated him in the electronics aspect of doing this, and we decided to go for full modulated lock-in amplifier detection system and so forth. So I was part of that whole project from the beginning, including making the apparatus, and then I participated -- we spent a lot of time looking to try to find other nuclei, but that magnet which ended up being powered by a bunch of big storage batteries, the current was controlled by a sliding -- what do you call it? It was a battery clip sliding along a invar strip or -- what is it? -- to vary the current. And you looked at this output meter, the lock-in amplifier that was always joggling away when it was up to the sensitivity of the noise and you tried to find the magnetic resonance of say bromine or something else.

We had seen fluorine of course in the original cavity and the first thing we put Bloembergen onto was to look for the fluerine 19 resonance and with the old cavity, which ended up destroying the old cavity in a sense because that stuff we put in it corroded the hell out of the brass. But anyway, we never found any other magnetic resonances with that system because you had to watch and slide this clip along the maganin strip. But we also did most of the thing as Bloembergen had pointed out that I was the one that showed that you could move this sample around to find the most homogeneous place in the field and get the narrowest line we'd ever seen out of the protons and started to realize about fluctuation narrowing. And I came -- my own idea, you know, I think I really brought up that issue because after we'd found that I went home and that evening I was -- I used to think in my Radiation Lab days largely in terms of Fourier transforms and so forth and was thinking about frequency modulation and what happens to the sidebands in frequency modulation, and it depends on rates as compared with distance in spectral fluctuations, and I realized things average out. You get this narrow thing. And I came in in the morning and said to Ed and Nico that I felt that I knew that it was the fluctuations, and that's how we got started in the whole fluctuation narrowing business. At least we would have started anyway, and then we went and did it much more formally, and which Ed dug up -- what is it? -- theory on correlation functions and so forth. I always remember that [J.H.] Van Fleck was our mentor in a way, because Ed always told Van what was going on. And when Van read about this and saw our paper coming out, and he was thinking about Weiner, and he said, “I think you ought to refer to the sausage.”

Rigden:

Refer to what?

Pound:

The sausage.

Rigden:

Well, that was a very, very important paper.

Pound:

That's right. I think that the people in the magnetic resonance in medicine are pretty aware of that too.

Rigden:

When did the Pound Box come in?

Pound:

Well that's, you see I started that in order to look for the frequency scanning technique, looking for quadrupole things. So I started that in February of '47.

Rigden:

Explain what the Pound Box is.

Pound:

Well, it's really an oscillator held at a level close to threshold so that if it experiences some variance in the rf energy storage situation in its coil it will show up as a change in level. And if you use lock-in amplifier technique and all that, you can do it all through that game and do the same sort of thing that you do in conventional fixed frequency stuff. But the big trouble is that it supplies the rf essentially to drive the NMR and also does the detection, so it works almost like what I was used to in ham radio from listening to a c.w. signal, because there you let it oscillate and you hear the heterodyne between its own self and the incoming signal. And so I went forth, I used this before I was able to find any quadrupole effects to look at a lot of NMR magnetic moments, including phosphorous 31 and quite a list of them. I think I had about ten different nuclei that I studied that way. But then -- and I had, and you asked me about other scientists in the Society of Fellows. One of them was George Kennedy, George C. Kennedy who ended up, he was really a follower of Griggs in a way, because he was a high-pressure geophysicist and he ended up on the faculty here for a while but he ended up at that UCLA institution run by Louis Slichter I think it was. But and he was a junior fellow. He had made his reputation at the Bell Labs developing crystal-growing techniques.

So I got his help to learn how to, a technique to grow a certain single crystal. And the crystal we grew initially was lithium sulfate monohydrate. And I looked at the lithium-7 magnetic resonance in that and found the structure due to its interaction with the quadrupole. I decided to do that. Originally I hadn't been interested in -- you could predict where there would be some quadrupole resonances from the knowledge that came as an interaction in molecules in microwave spectroscopy which was going on broadly, but I wasn't interested in these molecular things because I had this naive idea that I could solve Rabi's problem of getting a quadrupole field gradient by having a perfect ionic crystal and calculating the field gradient from the charges looked at as point charges on the lattice. That turned out to be hogwash, because there's the [R.M.] Sternheimer effect which turns out to mean that the atom gets heavily polarized and contributes much more than -- we have to know all kinds of atomic structure interaction problems in order to evaluate that. So it never really was successful in that respect, but I ignored going to look for things that would have been simple to find, and that's why I got scooped in the pure quadrupole business by [Hans] Dehmelt and [H.] Kruger in Germany who looked at chlorine in -- what is it? It's in a chlorine ethane, dichloroethylene or something like that and --

Rigden:
You were right almost there, weren't you?

Pound:
Oh yeah. I had already done -- I had decided -- it was a needle in a haystack. Actually I have a notebook full of calculations of the field gradient by assuming this thing from mercuric sulfide, HgS and I was going to look for that because it's a nice hexagonal single crystal and I was trying to grow a single crystal. You didn't have to have a single crystal in fact, because if you didn't have a magnetic field there was no directional sense and therefore you could deal with the powdered version and you get a single frequency even so. So I started looking for that, but then I decided this is really a needle in the haystack if you don't really know the field gradient and you don't know the quadrupole moment. So I decided it would be easier to look at light elements where the quadrupole interaction would be small and look for structure. So that's what I did, and then I used corundum the most significant one was the aluminum-27 which I did a lot of study on, which demonstrated it through the second order interactions and so forth.

Rigden:
This was in the fifties?

Pound:
No, this was in '47.

Rigden:
'47. Oh, all right.

Pound:
Or '48B it was only published fully in 1950 but in abstracts in 1948.

Rigden:
Well just let me wrap up one thing. The Pound Box, that got picked up and that was in labs all over the place.

Pound:
Well, the first publication --

Rigden:
Did you patent that?

Pound:
Hmm?

Rigden:
Did you patent that?

Pound:
No. The patent situation for NMR was impossible because [Felix] Block and Hansen had made such a broad patent and succeeded that you couldn't do anything against it. I spent a lot of time consulting with a company called -- well, Perkin-Elmer in particular, who wanted to get into NMR, because they were being pushed out of IR. They were the big name in infrared spectra, but they thought they should get into NMR, and this was around 1950 when Rex Richards, who was from Oxford, came over. He was well-versed in the infrared spectroscopy game but he picked up at Oxford as a chemist NMR in his own right and he became a consultant to Perkin-Elmer when he came over here too. But anyway, in the end they had -- I saw a lawyer concerned with what that patent that Bloch and Hansen, how strong was it, and they concluded it could be broken because it was overplayed but that it would cost a lot of money, and Perkin-Elmer elected not to go with it, so they never did. Because it would run out eventually.

Rigden:
And was the patent the basis of Varian's getting into --?

Pound:
Yes. In fact, in the fall of '47 -- was it the fall of '47? No, it was the fall of '46. Yes, it was the fall of '46 I was at a party at J.B.H. Kuper's. The man that was the editor of RSI for many years, head of the components group or electronics group at Brookhaven but was a friend at Radiation Lab. He is in that picture with me and Henry in the Nice Years book. Anyway, he had a cocktail party at the time of a meeting of the Physical Society in New York in the fall of '46, and Bloch came and told me that they were proposing to patent nuclear induction and would we want to join them. And I said I didn't think so. Actually, as I thought about it later I figured you couldn't do that because you can't join two independent groups in a single patent like that. That would violate the rules. And I came back and asked Ed about it and he had no interest in it. And so, but Bloch had told me that he and Hansen were patenting it and were going to license it to the Varians because he felt that they had been shortchanged. They felt they had been shortchanged on the Klystron because of the war. You know, that was sort of taken away from them as a going thing. So he did that. But now they wrote this patent and they had this man named Hunter who did their patent write-up and they wrote the description of it as if it were a textbook in nuclear physics, and I think that so dominated the whole picture that it looked as if they had invented nuclear magnetism and everything in that patent. Their patent was filed December 23, 1946, one day before a year had passed after our paper was received at the Phys. Rev.

Rigden:
Yeah.

Pound:
And then a little later, two years later, they had it reissued in which they changed all -- they had all the same claims but they were written in italics in their final patent, in

which they changed from nuclei to parts of atoms. And that was so it would extend to electron resonance.

Rigden:

EPR.

Pound:

EPR. And there I think was the built-in vulnerability, because after all EPR had been invented before in Russia by Zavoyski. And so that could have been cited as a prior art for them. So they wrote it in such a way that there was no distinction in the technique. I claimed that their patent would have been excellently justifiable if they made it specifically to the crossed coil concept and picking up of procession. But quadrupole resonance doesn't show that for example, so my technology for looking for quadrupole resonance with marginal oscillators and so forth wouldn't have worked with crossed coils.

Rigden:

Is there anything else that you think I should ask you about the whole NMR period of your life?

Pound:

Well, a lot of question often arises as to whether we had any anticipation of its getting applied to medicine and such things at that stage, and my answer to that would be somewhat like Ed's. We wouldn't be at all surprised about being able to observe NMR in human or other living matter, but that we had no knowledge or expectation of the power of computing that was going to come up, and that's what really made the big difference.

Rigden:

How long after was it that you saw NMR being applied by chemists?

Pound:

That took a little while too. In fact we used to tell chemists that, you know, you have to have sizeable amounts. It's not an analytical sensitive thing for chemists. They are not going to be interested in it. But it was only after the realization of the high resolution aspect and the fluctuation narrowing and so forth that one realized that you could see very small amounts, because the signal-to-noise ratio became so much better because of the narrowness.

Rigden:

And chemical shifts came in.

Pound:

And chemical shifts came in -- well, both chemical shifts and -- what do you call it? -- spinning direction things and so forth, yeah, with all that structural stuff. It's amazing. You get the spectra with forty-seven different lines, all attributed to different structures in molecular things. So it soon became true that no working chemist could be without an NMR spectrometer.

Rigden:

That's right. And that was true certainly by the fifties.

Pound:

Yes, middle fifties I'd say.

Rigden:

Well listen, it's almost noon. We're through --

Session I | Session II

Oral History Transcript — Dr. Robert Pound

Interview with Dr. Robert Pound
By John Rigden
At his office in the
Lyman Laboratory of Physics at Harvard
May 23, 2003

Transcript

Session I | Session II

Rigden:
Okay. It is now the 23rd of May. We are again sitting in Professor Robert Pound's office here on the fourth floor of Lyman, and we're going to pick up where we left off yesterday except to start by my asking Bob if there are things that have occurred to you since yesterday about the things that we discussed and you may want to add some items.

Pound:

Thank you. Yes, it occurred to me that in connection with my first coming to Harvard as a member of the Society of Fellows, I came unofficially -- well, I was already appointed, but I took a leave of absence for the first year from July '45 through that year because I had to stay at MIT to write the books. But I did come to Harvard and attend the newly started graduate courses that were now expanding because of postwar. And I audited -- because officially as a junior fellow I wasn't supposed to take courses for credit but I could do anything I wanted. So I listened to all of those courses, including three years worth of Julian Schwinger, who started from scratch, Newton's laws and the like, and went all the way through the highest level of then known nuclear physics theory. And you know, all the other people in Cambridge were there, people from MIT, Herman Feshbach and John Blatt and so forth were a part of that audience, and all our graduate students were there, like George Pake, Nico Bloemberg and etc. And the funny thing is, George Pake has written this up saying he felt so intimidated because he found these courses very difficult, and then he discovered that most of the audience were professional guys from MIT and therefore as a first year graduate student he felt a little better about not -- So anyway, but then the other thing I thought I should mention is that the denouement of coming to Harvard officially in July '46 was to become subject to the draft, and I was immediately called up and our then beloved president, James Bryant Conant, had made the pronouncement that no one at Harvard under his administration would be allowed to request deferment for any member of the university, because in wartime this was in the interest of public policy, but that in peacetime egalitarianism suggested that there should be no such thing.

So I got called up in July of 1946. I had to get up at five in the morning to go over to Fort Banks in East Boston and -- or I guess South Boston. I don't know. It's Winthrop I think, I'm not sure. But I had to go through the physical exam, and one of the things they had was a written test first, and I went through all this physical exam, and later in that morning the captain -- I had to go and see a captain who was a psychiatric officer in the medical corps and he said, “I noticed your answers to the question was asked ‘Did you bite your fingernails?’ and you said ‘Sometimes,’ and how about, do you have sweaty palms?” And I said, “Sometimes.” And he said, “How do you feel about riding crowded subways?” and I said, “I'd much rather drive if I can.” And he said, “Haven't I seen you somewhere?” He said, “Do you belong to the Cambridge Association of Scientists?” Which was the Cambridge version or affiliate of the federation in Chicago. And I said, “Well, yes, I was on the steering committee.” And here was this Army Medical Corps captain going to our meetings. I had never realized. And so he sent me to his boss who was a major, and the major looked at the papers he had given me and he asked me, “Now you don't want to be in the Army, do you, by any chance?” And I said, “No way.” After six years off my route, I figured it would be quite a comedown. So he signed the paper making me 4F.

Rigden:
Isn't that nice? Isn't that nice? So you were part of the Chicago Federation Group.

Pound:
Oh yes, yes. Well, we were independent in a way, but affiliated therewith, because all those people that had come back -- Bruno Rossi, Viki Weisskopf and even people like Percy Bridgeman came to our meetings, and Percy Bridgeman's -- one pronouncement I always remember from there was he said he wasn't going to go and ask the government for money to support his research, not going to have them telling him what to do and so forth.

Rigden:
Was [James] Franck from Chicago active at that point?

Pound:
In Chicago I think so, yeah.

Rigden:
In Chicago huh?

Pound:
Yeah, but you know, the people from Los Alamos had come back. Ken Bainbridge was back and so forth. This was in the fall of '45 and going onwards to '46. And I stayed

with -- and Bernie Feld at MIT and Herman Feshbach, and that's where I first got to know Herman Feshback, who had been in MIT through the war but in another area. He wasn't in Radiation Lab. He was with [Philip] Morse and they did demagnetization of ships and things.

Rigden:
Was Philip Morrison part of it? No, he was at Cornell then.

Pound:
Yeah, he was at Cornell.

Rigden:
Well that's very interesting. Did you maintain that connection?

Pound:
Yes, for some years, because we used -- Bruno Rossi had meetings in his house over the few years following after that, and most of the people that went there were MIT people, but I kept going.

Rigden:
We're going to come back to that a little later. So you are now -- we're going to move ahead, all right?

Pound:
All right.

Rigden:
I have just one question. At the NMR work, was the first work in the fall or December of '46?

Pound:
Five.

Rigden:
'45.

Pound:
'45, yes.

Rigden:
And that in a real sense had a determining influence on your subsequent years as a physicist. You've done a lot of work in the area of magnetic moments, and what I'm now trying to do, Bob, is connect the period from '45 to '60 when your famous experiment was done and in between '45 and '60 you did a lot of work in the domain of magnetic, nuclear magnetic properties, and a specific question I wanted to ask -- and you touched on it yesterday, but what brought your attention to quadrupole moments?

Pound:
Oh, the first concept, the first time I became aware was when, for one thing we had already looked at the relaxation of heavy water. And since we had developed BPP, the theory of that kind of relaxation due to the fluctuations, if you thought it was magnetic as it was in the case of protons in water because of the smaller g-value then it would scale and be very much longer, slower to relax for heavy water because of the smaller magnetic moments, but instead of being slower it's faster. And so we realized, as from Rabi's further experiments, that the quadrupole moment of the deuteron was the dominant effect there. So we did the same concept of the fluctuation of a quadrupole interaction in the molecule.

Rigden:
So the quadrupole moment was interacting with the electronic --?

Pound:
With the electronic nonspherical distribution. And so the deuterium relaxation is faster than the proton relaxation in heavy water. So that was the first realization that quadrupoles were important in this game, but then I had this -- I. I. Rabi came to give a colloquium talk in which he described some of the more recent things in molecular beamery. And the particular thing that struck me was at the end of his talk he talked about the structures found in the spectra of things like sodium bromide and such diatomic molecules and in which the quadrupole splitting turned out to be showing up as a structure factor, and in fact he realized that that was a large interaction so that they could go to very weak fields and see basically the zero field and depend on the quadrupole splitting as the basis of the energy gaps that had been studied. And that struck me immediately, and after that colloquium I said to myself and to Purcell and others, “Hey. Can't that happen in solids that are not spherically symmetric?” and I immediately started trying to develop a system to try to look for that zero field quadrupole interaction or splitting.

Rigden:
Zero field, so pure quadrupole.

Pound:
Pure quadrupole. Well, it's not pure in the sense that the rf transitions are all magnetic, but the energy states are caused by the quadrupole interaction. So I think it's a misnomer to call it pure quadrupole interaction. Because there is also a pure quadrupole interaction thing in which you depend on the fluctuations to drive the transitions. And you can apply electric field things that will cause transitions through the quadrupole interaction, and that would be more pure quadrupole than what is usually used for it. Which reminds me. Did you talk to Norberg about my article you were going to --?

Rigden:
I haven't yet. I haven't seen him since I talked to you. Well you said yesterday that you got scooped on the pure quadrupole.

Pound:
Well, in the sense of success in doing pure quadrupole. Now I had already published a number of things, mostly in abstracts and so forth, on quadrupole structures in magnetic resonance, but I set my goal too high for the pure quadrupole thing. I decided I wanted only to deal with ionic systems, because I thought one could calculate the field gradient. And this was partly inspired because Rabi has always advertised the concept that the only quadrupole moment nuclear property that was known properly was the deuteron, which he had done. And he had a standing offer of a bottle of scotch or something for anybody that could measure a quadrupole moment properly. And I thought, “Here it is,” because I thought I can calculate -- So this notebook or some other one nearby has whole page's worth of attempting to evaluate the 1 over R-cubed values for the lattice structures, and the one I was particularly concerned with, because it seemed like a nice clean case was mercuric sulfide. Cinnabar. And I got myself

lots of mercuric sulfide. And I knew there were tables that suggested the quadrupole moment of mercury, mercury-201, and I could therefore if I could get a field gradient value for that structure I could predict the frequency. The trouble was, every time I summed these series I got not only a different number but often a different sign. It's a nasty business, because it doesn't converge. Because in shells you sum R2 and 1 over R3 and you end up with 1 over R which doesn't converge. So it was a mess. But you have to do the partitioning of the lattice in a certain way in zones in order to do it properly.

Rigden:
Was the inconsistency just the long calculation and little calculational problems?

Pound:
More or less, well it was, yes, that's right. I guess I went to different distances in the sums and so forth. So I, but I had built the apparatus to look for it and that was the first scanning spectrometer using a marginal oscillator kind of thing, and actually it began a little differently in the sense that I thought I was going to actually even see the variation in the impedance through its noise properties when you get a resonance, because the shunt impedance of a circuit would be reduced when you hit an absorption as well. So the Johnson would show that, so that was essentially driving the resonance with the thermal energy which -- Anyway, so but after a year or so I sidetracked and used the same apparatus to look for magnetic moments of many things, because we found it was almost impossible to do with the conventional thing we were using otherwise. I mentioned yesterday how we tried to find it with the Bloembergen spectrometer apparatus we had built down in the basement. And it was studying this fluctuating output meter from the lock-in detector, and sometimes it would go like so, but you'd go back and it didn't do it anymore and things like that, so you could never -- So I was the one that invoked this continuously scanning -- what do you call it? -- chart recorder way of looking at the spectra. And that's what made a big difference in examining spectra. I should mention my originating double resonance -- satisfying one resonance and obscuring the charge in another component. Slichter calls the technique Pound-Overhauser effect.

Rigden:
Well, would you agree it's accurate to say that the period between NMR, '45, and 1960 your work was pretty much focused on nuclear moments?

Pound:
Yes, except that a very important diversion in that was that in the year 1953 when Anatole Abragam came as a postdoc, as a fellow on our Shell fund at Van Vleck invitation. We got together and we jointly realized that one of the things that the nuclear physicists were having trouble with was understanding the directional correlations of gamma rays, alpha and gamma rays and so forth B alpha-gamma correlations and gamma-gamma correlations, directional properties. And that year we had a very intensive collaboration and I particularly made the point to Anatole Abragam that in a liquid state the quadrupole fluctuations would be a dominant relaxation mechanism, and together particularly with his help we came up with a very elegant group theoretical basically, or a matrix mechanics basically image of relaxation of many different multipoles in nuclear distribution, not just in that dipole distribution but for quadrupole and so forth. Because in directional correlations, depending on the spin value changes between a couple of levels, you get radiations at various higher level multipoles, and that paper we wrote jointly in the summer, well in the spring of '53 goes through that in considerable detail. If we had --

Rigden:
Let me just say, that paper is “The Influence of Nuclear Quadrupole Moment on the Alpha-Gamma Angular Correlations.”

Pound:
That was the first one.

Rigden:
And then “Angular Correlations from Liquid Sources.”

Pound:
That's right.

Rigden:
That was '53.

Pound:
That's the one. He made my -- my name came first on that paper I think. It's the only one of that bunch. When you collaborate with a man whose name is Abragam, you don't get to be first author very often unless you have a dominant condition. And but then the big paper was about all of those things. It was in Physical Review, whereas those others were Letters I think.

Rigden:
And that was “The Influence of Electric and Magnetic Fields on Angular Correlations.”

Pound:
That's right. That's right. So that, you see, I think gave me a considerable leg up on understanding the Mossbauer spectroscopy when it came along. Because it's the same bloody thing. It's the combination of NMR and this, looking at it from a multiple point -- Therefore we knew what kind of lifetimes, and a lot of the people that went into Mossbauer spectroscopy did not realize that you had that kind of relaxation issue and that the narrowest lines you could produce would be limited by spin-lattice relaxation.

Rigden:
Okay.

Pound:
Frauenfelder was our main competitor in that game by the way. He was still then --

Rigden:
Hans?

Pound:
Hans Frauenfelder. He was still in Switzerland, and he did some of the best early experiments on the alpha-gamma directional correlation and then on the indium-111, which was the sort of model of understanding this kind of business. Then in the summer of '53, I spent that summer working with Jack Kraushauer at Brookhaven, wherein we did some studies of cadmium-111 which was an isomer that didn't have to go through a chemical change in its decay. You see, indium-111 went to cadmium-111 in its decay, but then you always had the problem, does it recover from the electronic change that goes in that beta decay, is it an electron capture decay leading to chemical charge. And so I went to Brookhaven where we could make cadmium-111 by neutron absorption directly. And I got Abragam and I, I got the Abragams to have residence at Brookhaven that summer too, just before they went back to France.

Rigden:
Right.

Pound:
And then I started an experimental program when I came home in which Gunther Wertheim was the first experimenter to carry that out, and he studied the directional correlations of mercury-199. I insisted at the time, because I was still pursuing the issue of the quadrupole moments properly to measure the quadrupole moments, so I wanted to use isomers, not things that decayed by chemical change. Because then you could put it in a lattice of its own type, whereas you can't do that with something like indium-111. So, but Frauenfelder had shown in their work that the recovery from the capture -- or I can't remember whether it was electron capture or beta decay -- but the recovery in the metal was fine. But in insulators they don't work because the chemical recovery is not fast enough. So that's why I think I was in a better position than most of the people that tried to follow up to the Mossbauer spectroscopy, for knowing these things, because some people wasted a tremendous amount of their time Bob Dicke in particular -- trying to find the Mossbauer line of things with 44-second lifetimes. Because they had the image that the inverse of 44 seconds would be a really narrow line, and that the energy involved that would have given you a fractional of line width 10^{-22} or so, and when I published the first paper on iron-57 -- no, it was before that. When I published that first letter on the possibility of extending the Mossbauer spectroscopy to that point, he wrote me saying, “I'm afraid it looks as if we're treading on the same research interest and maybe we ought to collaborate” or something. And I wrote him and said we already had the iron-57 with which we were going to go ahead and try to do the experiment directly. And oh his letter, you've seen that in my article.

Rigden:
Yeah, yeah.

Pound:
His letter said sometimes he thought this was such a difficult thing that nobody in his right mind should try it. He said that in the bottom of his letter. But I was going to try it, and --

Rigden:
Before we get into that -- we will come back to that in just a minute -- but before we leave the period before the Mossbauer-motivated experiment, you worked with magnetic dipole moments, electric quadrupole moments. Did you ever give any thought to a monopole?

Pound:
Well, only as a sideline in the sense that I was quite aware of what Ed Purcell's interest there was. And of course I was also aware of the suggestions by people like Dirac and those. But the thing I had more -- Oh, that's another thing I was going to mention, is that a driving force for what I chose to try to do in NMR was still my old interest in the atomic clock question. And the quadrupole, initially I thought, “Hey, this is going to give me a resonance line that's going to be better than any of these microwave spectroscopy ones” because I supposed that the line breadth would be the same as if it were magnetic resonance. And the frequency might be as high as a thousand megahertz. And so the fractional line width might be very competitive with anything else. And that's why I pursued only these very high frequency cases. Not just that, but also the fact that if I could do it with an ionic thing I would be able to measure -- And you know that I made this suggestion then that the quadrupole interaction ought to be strong enough to produce nuclear orientation at low temperatures because the energy states are comparable with kT. And that's why I went to Oxford in the summer of '51. I got a Fulbright grant to go and help participate in research at Oxford in the Clarendon, and that's when Nicholas Kurti was in fact developing the first nuclear polarization experiments. And Jim Daniels, who was then his graduate student on the side was also pursuing materials that could have enough quadrupole interaction. In particular we were pursuing crystalline things with iodine-127, which ought to have had 1000 MHZ kind of splittings, and he was doing that as well as working on this high field low temperature nuclear orientation following the proposals of -- well, some people claim that Gorter and Rose, but the actual best suggestion that they succeeded with was that of Brebis Bleaney who followed my idea of nuclear orientation rather than polarization.

Rigden:
Do you know how to spell that name?

Pound:
Bleaney? Brebis, B-r-e-b-i-s. He is one of my closest friends there along with Nicholas Kurti who died a couple years ago. But anyway, so I had a very interesting year there in the Clarendon Lab with those people.

Rigden:
And that was what year again?

Pound:
'51.

Rigden:
'51.

Pound:
I left here. As I said, I had been helping, in a way helping Doc Ewen working across doing this Hall in the autumn 1950, because that quadrupole resonance search that I was doing was right in the same frequency band that we expected the hydrogen, interstellar hydrogen might be. So I had all that apparatus up on the fourth floor, on this floor here in this building whilst he was in the room across the hall, and I was technically his advisor during the period that Ed was away. Ed was in a study that was begun from MIT really, called the Beacon Hill Study I think, and it was for the State Department. Of course in those days nobody ever said what these studies were about, but this was about surveillance of some kind.

Rigden:
You know, one thing we didn't cover, or I didn't ask. You were a junior fellow. When did you become a faculty member?

Pound:
Well, I lost a year as a junior fellow, so I was only, even though I never went to graduate school and so forth, I was only a junior fellow for two years in action, in active version, because I stayed on at MIT that first year. And I was invited by Van Vleck in the spring of '48 to become assistant professor. So I became an assistant professor in July of '48. But I was still -- it was supposed that I could have got a substitute year for the year I'd lost if I wanted to at the Society. And Crane Brenton, who was the chairman, was very friendly with me, and I told them that I had an invitation to go to the first Radio Frequency Spectroscopy Conference after the war at Oxford in the summer of '48. And I had never done any traveling on the Society of Fellows, so he extended the support for my travels that summer although my term had ended in July and I spent most of the summer and took my wife along and we went to Holland and then England and then France and then back to England and we had quite a wonderful time. And the important thing, it brings in Van Vleck again. Because it was impossible to get a ship reservation to go across in the summer of '48 unless you had some help. And Van, I talked to Van about this, who was party to getting me invited to Oxford and to report -- that's where I -- There's a paper, there's an --

Rigden:

Okay. Continuing. You first described --

Pound:
I first described the marginal oscillator and the observation of quadrupole splitting in crystalline solids.

Rigden:
And this meeting, Radio Frequency Spectroscopy meeting, was where?

Pound:
At Oxford University.

Rigden:
Oh, at Oxford. Okay, okay.

Pound:
That was at Oxford. But what Van did for me in addition, then was to get me invited to a metallurgical conference in Holland because the Dutch were giving priority on the Holland America line to participants in the conference. So I got to go there and we got our ship passage on the New Amsterdam going to Rotterdam and it was quite a wonderful experience. I had not ever traveled across the ocean on a ship before, and among the people on that ship were the Van Vlecks, Karl Darrow the APS Secretary and somebody else at high level from the -- oh, I think Sam Collins of MIT, the inventor, the man that designed the helium liquefiers. They were all in first class and we were in cabin class, which was much better than the tourist class. They had three classes. And in our class as well was -- the person we spent a lot time with was Charles Kittel, and he was at our table in the dining room, and I got to know Charlie pretty well then, and he was debating about whether he could possibly leave the Bell Labs and take an offer of a teaching job at Berkeley. And because he had this dreadful speech impediment.

Rigden:
Yeah.

Pound:
And what he always did then was to pull out a little pad from his pocket when he was starting to get hung up and he'd write it down, and that would relieve him. But he took the job and has done very, you know, it worked out very well.

Rigden:
He's done very well, yes.

Pound:
Yeah, Charlie. So that was quite a trip. And there were a number of Dutch people we got to know on that trip. There was a man who was in a harbor transport business, a man named Peterson. We spent the evenings at the bar with him. We asked him was he in Rotterdam when the Germans bombed and, “Oh yes,” and “What was that like?” He said, “Have you ever pumped water into a grand piano?” And he hated what they had done to Rotterdam. He said all that new construction they had built which covered up quite a --

Rigden:
Yeah.

Pound:
He said, “It's all banks and government buildings,” and he didn't like it.

Rigden:
Well okay. Let's move now to somewhere around '59, late '59. You, from what I understand, when you saw the results of the Mossbauer work, specifically the narrowness of that line --

Pound:
That's right.

Rigden:
You recognized immediately some potential. Why don't you just talk about that?

Pound:
Well yes. You see, as I was mentioning, I kept looking for narrower things than say ammonia, which was the best resonant spectrum we then knew, and when I saw that I immediately extrapolated to the possibility that if one chose other -- Well, actually it wasn't Mossbauer's work that I saw directly; it was that of the two groups that repeated it -- one at the Argonne and the other at Los Alamos -- and they both published in September 1959 Physical Review Letters.

Rigden:
No no no --

Pound:
Yes.

Rigden:
Oh, oh, yes, yes, okay.

Pound:
No, I'm sorry. 1959.

Rigden:
'59. Yeah, we're talking about Mossbauer.

Pound:
1959. Sorry. Yes.

Rigden:
Okay.

Pound:
1959. They both published those repeats. I had spent that summer in a defense study group at La Jolla run by a man who was at the Scripps Institution. It was on oceanography well, it was related to oceanography because it was relating, it was called “Project Sorrento” and it was related to submarine detection. And we had all kinds of people there which included G. I. Taylor, the British hydrodynamicist I guess you'd call him, and I shared that room with Tommy Gold and --

Rigden:
Cosmologist.

Pound:
Yes. He was an English physicist really, but then he became a cosmologist and he was party to that -- what do you call it, continuous creation theory?

Rigden:
Steady state.

Pound:
Yeah, steady state theory. But he'd come to Harvard by then. Actually he was already at Cornell. He spent a couple years here at Harvard first in the astronomy department. And George Carrier was in that group. So then I came -- I hadn't been thinking too much and watching the Physical Review. When I came back it was Glen Rebka that called my attention to these two letters in the September 15 issue of Physical Review Letters, which was a newly then created publication then. And then because of that we came to my office and, one evening very soon after reading those papers, because I foresaw that there must be a better case. And we studied the isotope tables and came up with three examples that should be considerably better than the iridium case that Mossbauer and the Los Alamos and Argonne people had pursued. They were respectively zinc-67, iron-57 and germanium-73. We never pursued germanium because the data showed that it had a terribly high internal conversion coefficient, which meant that really nobody had ever seen the actual gamma ray in those days. They only saw the conversion electron. But its energy and lifetime were such that it's line should be naturally very narrow. And long after that some Bell Labs people had been able to pull it out and see it, but it took a whole month of data accumulation before you could get enough intensity, and it only came out when they finally made, developed a fancy solid-state high-resolution detectors that could separate that gamma ray from the X-rays. Anyway, so we immediately considered exploring the iron-57 and the zinc-67.

Rigden:
Let me just pause here a minute. Did Glen Rebka bring that to your attention because he knew of your long-term interest in --?

Pound:
No, he wasn't thinking in terms of the high-resolution aspect at that time. He was thinking in terms of maybe -- well, we had been pursuing directional correlations, and here was something fairly closely related using nuclear interactions in solids you see. Because we were pursuing this business that I started with Abragam.

Rigden:
Yes. Okay.

Pound:
And he had spent. He was -- let's see, he got his degree in '55, his undergraduate degree in '55 I think, and this was four years into his thesis work, but the year I was in France in '57-'58 he spent that whole time building elegant power supplies to get high stability and had charts on the wall showing how little drift there were in the output of the power supply and so forth. Frank Pipkin became his temporary advisor during that period. But Glen was such a -- what do you call it? -- perfectionist, that he didn't, he wouldn't push on with something. What we were trying to do is to develop many pieces of electronics to build what we call a coincidence device that allowed simultaneous observation of coincidences on the delay line at -- I guess we call it a chronotron. This was before you had things like multichannel analyzers you see, so we had to devise that we were going to make a 10-unit thing to look for the directional correlations at different time delays at taps on a transmission line. Because that, our theory covered that. And so he was building all this apparatus to do that. And in fact he had a fancy chassis in those days, vacuum tube electronics, that was to be the central part of that system, but he needed something like six or ten of them, and we were able to farm that out and have them wired. He did all the mounting parts and layout of chassis but he had the wiring done by an outside company. And when he got it back he was furious because they didn't use -- what's it called? -- a eutectic solder? So that he could see that the solder showed. Eutectic solder shows a nice polish when it freezes because it doesn't break up into crystalline stuff, whereas this solder was bad. He just wouldn't touch this thing because he thought it was junk. And he would go through each of them and start re-soldering everything his way. But then when this other project came along, I sort of took over and provided some push -- A lot of that apparatus was there. That's what put us one leg up, because we had a lot of things that could be adapted to this.

Rigden:
Well when you saw these narrow lines, at what point did you suggest to Rebka that this might open the door for --?

Pound:
Oh, right away when we saw that. I said right away -- I guess maybe the first time I realized how it could happen was that a couple of weeks later than that first publication it was a publication by a man named -- what's his name Husein Ylmatz? A Turkish friend who was always trying to contribute to relativistic ideas, and he had written an article that got published in Physical Review Letters suggesting a way of using lasers to try to observe redshifts -- which I thought at the time that that idea wasn't very good in itself, but I said to myself, “There must be a way to use these narrow lines in the Mossbauer spectroscopy to get a redshift.” And we worried about that, and I never realized until Rebka mentioned just separating the source from the absorber you see and letting the gamma ray traverse the distance between.

Rigden:
When he said that, did you have any sense of what the magnitude of the separation would have to be?

Pound:
Oh yes. We immediately -- it depended on of course what resolution it would be. I knew what the fractional effect per meter was. The letter we wrote and [for] which we got an awful lot of flack saying “you had no right to write a letter like that, you were plagiarizing” and we were told we were stealing because this idea had been kicking around. And it hadn't been published by anybody or heard by us, and you know Singer had published a proposal of putting atomic clocks in orbit to do this thing, and that's just, that's a simpler concept than adapting this and nobody had suggested it. Now if you've read these articles by G. Henschel, this German, he's a member of your committee overseas, the historian of science.

Pound:
Henschel. He had this one which is called -- well, I always liked his article in which he talks about the Conversion of St. John.

Rigden:

I don't know that.

Pound:
Oh, you don't? You know, Edward St. John had tried to observe the redshift back from the sun in the teens and came to the conclusion that there was no such thing. He couldn't find it in his optical spectroscopy in astronomy. And then somehow -- the people took that pretty seriously, and somehow he got to realize that the spectrum from the Sun was rather more complicated and there were other causes that may have been a problem. So a couple of years later he started to say, “After all, it may be there,” though his first papers had said it wasn't there. So when Henschel reviewed these things, he called the paper “The Conversion of St. John,” which I thought was a very cute way. But in any case, he has articles covering the issue of the redshift up until 1965 or something like that. But he interviewed Mather Leibnitz, who was Mossbauer's mentor in Gottingen, and he asked him what did he think of this and he said, of course applying the Mossbauer spectroscopy to do a redshift was obvious. Oh, of course they thought about it, but they thought it was so obvious nobody would talk about it and so forth, but they never mentioned, you know, in Mossbauer's own papers he never mentions that this has a uniquely narrow resonance. He said its main B he said the applications that he cites is to observe nuclear lifetimes in a domain not easily accessible otherwise. He doesn't say anything about its being a narrow line.

Rigden:
Well Bob, you have in the back of your mind for many years this atomic clock idea.

Pound:
Yes.

Rigden:
But when you saw the Mossbauer lines you must have had in the back of your mind also this idea of testing -- did you have this idea of testing relativity?

Pound:
Oh yes, yes.

Rigden:
When did that come into your thinking?

Pound:
Well, that came in towards the end of the Radiation Lab days when I was developing atomic clocks. Well, I was really developing frequency stabilizers which I envisaged I could make into atomic clocks by using a reference instead of a cavity -- a fundamental thing like the ammonia line or whatever. And so in the summer of '45 there was an article by J. B. S. Haldane summarizing the work of -- and promoting -- the work of that E. A. Milne, a mathematician at Oxford. And that's the sort of thing that kicked me off on thinking about these multiple time scales issues and something relativistic as an application of these high-resolution timers.

Rigden:
Okay.

Pound:
And that was actually in the American Scientist, the journal of the -- was it the journal of Phi Beta Kappa?

Rigden:
Sigma Psi.

Pound:
Sigma Psi, yeah.

Rigden:
So here is a wonderful example of the prepared mind. You saw these narrow transitions, and while Rebka was thinking of something else, you saw immediately another experiment that you could --

Pound:
Well as a matter of fact Rebka came up with the desire to look for hyperfine structure when we got the iron-57 working. And well, before that we wanted, he had iron-57 in mind before we actually pursued it from that point of view as a possible way of looking at hyperfine structure. And a number of people had -- nobody had seen any hyperfine structure in this kind of spectroscopy as yet, but Maurice Goldhaber visited our lab, visited this place about that time and we talked to him in the corridor and I told him we were thinking about trying iron-57 partly and he said oh his people, what's his name, Kistner was one of them, but the other one that -- Andy Sunyar. Sunyar was going to pursue iron-57 to look for the hyperfine structure down there at Brookhaven. So we knew there were other people in the game. And as I told you once, I guess when I finally had -- I got so enthusiastic that we wrote this up as a paper that got published in Phys. Rev. Letters, and I had a number of feedbacks from that, one very nasty one from Frauenfelder.

Rigden:
And this was the first paper that you wrote --

Pound:
Yes.

Rigden:
Essentially laying out the possibility of doing this.

Pound:
That's right. And we envisaged that we might have to go to a mine shaft or something like that and so forth, because we put in the numbers for the two cases, iron-57 and zinc-67 for the width of the line as compared with the height that it would take to get that shift. But we never said that you would have to have that height. Because a lot of nuclear physics people assumed that you would have to have a shift equal to the line width, not thinking that by high counting rates and data and stable issues you could split the line into a very small part. That's what we realized immediately.

Rigden:
This Phys. Rev. Letter was in October of '59.

Pound:

Yeah. It was only a few weeks after we had first seen that phenomenon.

Rigden:
And you really had data to report in early '60.

Pound:
On the redshift, yeah.

Rigden:
On the redshift. Then in December the New York Times picked it up.

Pound:
That's right. Well, actually that man, Harold Schmeck, Jr. who wrote that up, he called me on the telephone sometime around early November and said his friends at MIT had been telling him about this project that I was into and he'd like to come up and talk to me about it. And a week or so later he came and sat and we talked, and then sometime he sent me a manuscript of what he wanted, proposed to publish, and that was maybe around the beginning of December. It was all going rather slowly. By the time that we had met, we had already succeeded in detecting the resonance of iron-57, and we simply mentioned that that had already been successful in that letter, in that article. But then he called me on something like the 10th or 11th of December to say that his editors were going to publish his article, and he had to shorten it, and he had submitted -- he in fact, how ever did he get me? He got me a copy of what he proposed for it as a shortened version that would appear and wanted me to approve that before it got published. And I thought it was pretty well done. The main thing I had changed in the original thing that he wrote was to give more emphasis to the Mossbauer role in having developed this concept, not the concept of the redshift. But often in the literature Mossbauer gets credited with having done that you know, but I made sure that the concept of that nuclear physics line or resonance was given to him. And so then it appeared on December 13 on a Sunday issue on the front page. And that was a great shock to me.

Rigden:
With Einstein in the headlines.

Pound:
Probably, yes. As I used to say then, anytime you touch base with something that has to do with Einstein immediately the journalists pile out and I got proposals for buying pictures of Einstein and all kinds of crazy things. The actual letter on iron-57 was published two days later.

Rigden:
In Phys. Rev. Letters.

Pound:
In Phys. Rev. Letters, yeah.

Rigden:
Let's deal with these two things separately. Both of them caused a controversy. The Phys. Rev. Letter, people were upset because they thought --

Pound:
I hadn't done the experiment yet.

Rigden:
-- you should have waited until you had results.

Pound:
That's right.

Rigden:
As you look back, would you do it differently?

Pound:
I don't think so. I know that at the time when I was thinking and had realized that this could be, as I had mentioned somewhere, I ran into Ed Purcell in the hall and told him about this idea, and he was extremely enthusiastic about it, and Ramsey came along and he stopped Ramsey and he said, “Bob has this most elegant experiment in the world.”

Rigden:
In your writing I think you said Ed Purcell said it would be the experiment of the century.

Pound:
Something like that. That's right. And so they encouraged me to publish it. And I thought well, I hadn't really sensed any great reluctance to publish it, because if I had been known as a theoretical physicist there would be no question. And we always here, particularly this group of us, felt that we were not just experimenters, that we had -- you know, Van used to tell us for BPP that we had outclassed theorists in that. Theorists should have been able to anticipate what we had discovered in fluctuation, but experiment drove our ideas. As Ed used to say about the NMR, that if it hadn't worked it would have been an outrage because it had to work. There was nothing newly discovered there.

Rigden:
Did that Phys. Rev. letter --? You got letters from Dicke you said --

Pound:
Yeah.

Rigden:
Did that Phys. Rev. letter, do you think, clear the road for you? Did Dicke sort of drop out?

Pound:
Well, he didn't -- yes, he did drop out as far as pursuing the redshift, because he was having his student -- Stevens? Was that his name? He asked if it would be okay to

send his he wanted to send his student up here to find out how to do the iron-57 because he was going to put him on some other application, which he did. He put him on this spinning thing to look for an isotropy in space, and so that was fine. But you see, he had had him pursuing this silver-109 is it? It's the silver isotope that has about a 44-second lifetime, which was a completely unrealistic concept, because there is going to be spin-spin interactions that will broaden it way beyond that, and that dilution of the resonance through this broadening would be such it to kill its detectability. And people have been pursuing this. There have been people at Los Alamos, a guy named Taylor and others have been pursuing this kind of esoteric high-resolution example for years. It's still going on I think.

Rigden:
And what was Sam Devons' -- why was he upset?

Pound:
Because -- [laughs] Yes. Sam had just come to Columbia for a sabbatical year, had taken leave from the group that he was with at Manchester. He held the professorship at Manchester that was once held by Rutherford, as you may know, and he had a group there that had been -- he said they had spoken about this even in the summer of '59 at some conference. But it wasn't something that spread as far as I was anyway. But they were pursuing it, and we didn't know how far they had got, but they were pursuing it entirely on that limited concept that you had to have a line whose width was as narrow as the shift you would expect to produce, so they were pursuing the zinc-67 example. And they never got anywhere. One of the people on that group was named Bumbury and Bumbury is the name of Earnest's friend in The Importance of Being Earnest. He's the one who went to having an ill aunt was it or something like that, in The Importance of Being Earnest. Do you know that?

Rigden:
I know the name. I don't remember the -- Well, it's an interesting historical example. I think people are going to find this fascinating. Are you --?

Pound:
I found that actually you know I got very upset over the fact that to Sam Devons -- no, I'm sorry, that Goudsmit wrote that thing that was interpreted as being against me.

Rigden:
That followed the New York Times, did it not?

Pound:
Yes, that's right.

Rigden:
So the New York Times, because you published in the media these ideas --

Pound:
I didn't. My proposal had been published in Physical Review Letters in October.

Rigden:
Well, but that's what Goudsmit thought.

Pound:
He told me that that letter which he had had in his desk drawer ever since the Columbia press conference that was held on parity conservation, which made him furious at that time. And it hadn't really to do with me, but everybody out there assumed it had to do with me. Because after all we had done everything he required in the editorial before he wrote it.

Rigden:
And what was the letter on parity? Wait. Let's just wait until we get a new tape. I'm going to stop this.

Pound:
...week before it changed this time.

Rigden:
Okay. Okay, we're back. Now Sam Goudsmit was upset following the New York Times feature article and you said he had a letter concerning parity. I don't know --

Pound:
Well, in '56, was it, that the Columbia group did three different ways of demonstrating the nonconservation of parity.

Rigden:
Yes.

Pound:
Of course there was a theoretical group, then there was Miss Wu and the group at National Bureau of Standards, with Ernest Ambler in particular, and then there was the group that did the muon orientation with Garwin.

Rigden:
Lederman and --?

Pound:
Lederman and Garwin.

Rigden:
Garwin.

Pound:
Yeah. And they held, before they actually even submitted to the Physical Review Letters they held a press conference to reveal this terribly important thing, and that's the one that violated all the conditions that Goudsmit argued should be held.

Rigden:
So the parity paper was three years old.

Pound:
Oh, that letter was, yeah, three years.

Rigden:
And he had never published it.

Pound:
He never published this letter that he wrote that he claimed he had in his drawer. I don't believe that completely because he must have modified it for the -- because it didn't mention parity as such or any example as such, but the example everybody had in mind was this B and you know one of my colleagues and friends previously to that gave a colloquium talk at Columbia -- I was told my old friend Alan Sachs who was chairman there -- which cited me as a publicity hound because of that. And he had a slide on his colloquium talk showing me in some kind of cartoon that suggested that -- Now I believe that was Hans Frauenfelder, because Hans Fraunfelder was our closest competitor for the iron-57 because he published almost exactly the same results that we did about a month later in Phys. Rev. Letters.

Rigden:
But this was not the redshift.

Pound:
It wasn't the redshift.

Rigden:
This was the hyperfine.

Pound:
It was just the spectrum of iron-57.

Rigden:
Yes. Well, would that --?

Pound:
But he didn't get all the satellite structure either. The people that got complete proper satellite structure were the people back at the Argonne Lab. Hannah, Hannah was the leader.

Rigden:
Do you think this controversy colored the way people responded to the experiment?

Pound:
Probably not in the long run. I think the main competition that we sensed and felt and knew about was the one with the Harwell people where John Schiffer who was from a Harwell group -- no, I'm sorry, from the Argonne group that had repeated. He had been party to that experiment that repeated Mossbauer's original spectroscopy but hadn't extended it. They did extend it to another similar isotope to that and to lower temperatures than Mossbauer had, but they did not get a high resolution examples. So when he went on sabbatical leave to Harwell, he talked them into getting into the Mossbauer spectroscopy thing, and it was Ted Cranshaw [spelling?] there who thought he was unique in having suggested applying this iron-57 resonance to the gravitational redshift. And then they saw my letter, and that caused them to push much more hard because they realized they might not be alone. And the only person I sent a copy of our preprint, a preprint of our article on iron-57 -- well, two persons -- one was Rudolf Peierls because I had heard that he had a different view of how the Mossbauer thing came about. He had been in Columbia the summer before or something. And the other was Walter Marshall, who was head of the theoretical group at Harwell, and I had known his particular theoretical interest was in interactions in ferromagnetics and the nuclear spin interactions in particular, so I sent him a copy of that letter that we were the first ones to see some hyperfine structure in iron-57. And that's what Rebka wanted to continue to do more thoroughly, but our apparatus really wasn't good for it. I had a nice -- you've seen that letter back from Walter Marshall, who was an old friend in a way. I had great attachment, because he had been here in the summer before visiting Harvey Brooks and the division. And I had told him -- he was wondering if that Ford station wagon that he had acquired during his year here would be sensible to take back to England, and I said do it, and he did. And every time I saw him after that he was very grateful because he said it was wonderful having a big Ford station wagon.

Rigden:
Listen. You gave a talk in January of '60 on the experiment.

Pound:
That's right.

Rigden:
And Viki Weisskopf rearranged the program to allow some British group, English group to share this --

Pound:
Well, that was Schiffer and Cranshaw I guess.

Rigden:
But that was done in part because -- I mean they were competing with you.

Pound:
That's right. They had actually carried out and had gotten data on a redshift.

Rigden:
Why did Viki do this?

Pound:

Well, Viki was very aware -- Well, he did it in the first place because they submitted a post-deadline thing and were scheduled to give their talk on Wednesday, whereas I had a longstanding commitment to give an invited paper, although I had no idea when I committed to giving the invited paper that I would be giving any data report on the redshift experiment itself. I was going to be reporting on the discovery of iron-57, because we assumed that we were unique in that. And so I was scheduled to give a full invited talk on the Saturday morning. And Viki called me to say that -- Well, actually Schiffer called me when he arrived from England about Monday of that week and said they were coming to give a post-deadline paper on the results on the redshift experiment. And here we had started data taking on Sunday that week. And it looked as if it was working and so forth, and our data rate was basically eighteen times greater than theirs, so that in the one week that we had to run before I'd give the talk I realized I should have data that would be pretty good. Probably about an uncertainty of 10 percent due to statistics. So I was kind of decimated by the idea that they were coming to give the result that they had counted for thirty days. But they were using a 12-meter-high outdoor water tower on the site. And they got written up in The Observer, the English newspaper.

They were the front page of the magazine section of The Observer that week about how this group at Harwell was in competition with a group at Harvard University, and I said that that group was a man and a boy and so forth. Well, Glen was more than a boy, but still, that we did not have the facilities say of the National Laboratory that Harwell represented [correct word?]. Because they had the radio chemists and the machine people and so forth they could, who could help them do a lot of things that would be very, hard for us. So anyway, Walter replied that had he seen our paper he wouldn't have submitted theirs because they had also observed the resonance of iron-57 but they did not study the hyperfine structure; they only inferred it from the weak intensity of the absorption. And so anyway, they had pushed on, and I had a letter from Schiffer that said, "I think we probably should not exchange details as to how our experiments are going to work." When I saw that I felt he thought he had a better idea than we had about how to do it. And it turned out he did have the idea of modulating the Doppler velocity as we had, but they did not have the idea of the need for inversion or the need for looking to using a system that could independently see distortions in the modulation system -- which we called our monitor system. So we had much more overall control on our system already built in at that first run, with continuous calibration with a hydraulic moving system.

Rigden:
We'll come back maybe a little bit to this, but why don't you now just describe briefly -- describe the experiment.

Pound:

Well, the experiment consisted of developing a source that was as strong and as unbroadened by other causes as we could make it. And it contained iron-57 -- I'm sorry, it contained cobalt-57 electroplated onto a thin sheet of natural iron, pure natural iron which was about 2 inches in diameter. The dimensions are important because the amount of material you electroplate onto this thing determines how much activity you can get into it, radioactivity to get gamma-ray intensity. But you don't want so much material as to make an alloy out of the iron with the cobalt; you want a very low concentration. So that's why we had to have, in the later experiment we used a 4-inch diameter disk in order to use a larger amount of cobalt. And we also were concerned about the degree to which the cobalt was carrier-free as it were. The cobalt was created in a cyclotron at Oak Ridge through the intermediary of a company in Pittsburgh called Nuclear Science and Engineering. Anyway, I had a letter from Oak Ridge saying that they were so interested in the experiment that they had spent Christmas Eve running their cyclotrons, a special thing to try to make our source [unintelligible word]. And it was basically at the time the company was miscalibrating their intensity so they called it 400 millicurie, which is a pretty big source, have radioactive sources, but they later changed their calibration so it was really only about 250 but it was still the biggest source anybody had purchased. But they did not then have the facilities to do the diffusion themselves -- once the cobalt was electroplated onto this iron disc they did that for us.

But then I had to take care of getting it diffused into the iron by heat-treating it in a hydrogen atmosphere. And the way that we were doing that in our small-scale things was in our little oven in the shop down here, but in this case we couldn't imagine it being stable and we didn't want to risk this whole thing. It cost us seven thousand bucks for the source I think, and that was the highest piece of money that was in the whole experiment. I don't think, in terms of experiments today, this is considered a very significant expenditure -- But anyway, I got the help of my old friend who actually worked for me doing Rad Lay days, Fred Rosenburg, who was the head of the tube shop at MIT in the old Building 20. And amusingly, he asked that I not bring any counters and things because the other people in the shop would be concerned, and they were sort of -- what do you call it? -- gun shy about radiation. And so I didn't, but I had carefully put this thing, I had an iron box made machined out inside so that the iron disc would be lying below the level of the break in the box where the lid and the bottom were bolted together, and then it had holes drilled to allow the gas, the hydrogen gas, to flow through. And on the outside of that box the counter showed, I don't know, 50 -- what is it? MR per hour or something. And we got 3 feet away from it and it's down to the radiation level that nobody would think about.

So we took it down there and he did this diffusion for us and I thanked him in the letter that we finally wrote in the publication, and he got a feedback from that because it turned out the health physics people at MIT read the Physical Review Letters and they said, questioned him, "When did you do that?" and "You didn't clear it with us or anything." And so he called me to say he had trouble from this. How much got out of that, well what I thought he meant was how much cobalt-57, which was the dangerous part, because the initial decay is much more energetic than the one we were using. The one we were using this only happens in 10 percent of the cases, but the high-energy 120 keV kilovolt] which goes through the iron and everything, it was what cobalt-57 really existed for in the -- what do you call it? -- nuclear medicine trade, because it was used as a calibration for nuclear medicine detectors and so forth. So when he said that I thought he meant that how much came, got out into the room of the actual activity. But what it turned out he meant was how much was the gamma-ray intensity. But I didn't think anything got of the box. I was a little nervous that it might though, because in this high-temperature oven it would evaporate.

Rigden:
Yeah.

Pound:
So anyway, I guess that all got --

Rigden:
All right. Well, back to the experiments. You got -- this now your source.

Pound:

Yeah. That was the source, and that was mounted on a transducer that vibrated it by enough so that the Doppler shift due to the vibration would carry the resonance from one side to the other side of the absorber line which was in an enriched iron foil at the other end of our 75-foot tower. Now we had to do something to allow this 14-kV gamma ray that was the basis of this Mossbauer resonance to get from one end of the tower to the other, although we couldn't do anything at that time about the inverse square law. We were just 75 feet away. It's considerably less strong than it is at the source. But it also would have been absorbed in 3 feet of air, so we couldn't send it through an air column. So we installed a Mylar, and originally it was only polyethylene but later it became a Mylar cylinder -- a bag as we called it -- which was inflated with helium. Now we had a continuous flow of helium from the bottom to the top with windows allowing the gamma rays in the tower so that then the absorber was at the bottom. And of course every few days we would reverse it so that the source was at the bottom and the absorber at the top. But we installed as well as these two main components, we installed an absorber with its own -- I'm sorry, a detector -- with its own -- these were, by the way, sodium iodide scintillation detectors, which are not very good for this low energy.

We had absorbed such a detector nearby the source so that this modulation effect, if it were the waveform of the modulation were to change it would show up in the data on this nearby thing and be extractable from the data of the main run because it would effect the two the same way. And that was one control that we did for trying to control the systematic error, and the other one was the inversion. And I always said that we looked at that when both directions and said the effect would be doubled that way but also would be demonstrated to be having -- that the only thing we would change was the direction. And that made it an experiment as compared with an observation. I always said the astronomers could only make observations of what came from the sun, and they couldn't turn it on and off, whereas in effect we could more than turn this on and off; we could invert it. Whereas the Brits did not do that, the people from Harwell who gave their report in January. You asked why did Viki Weisskopf do what he did about that. He thought it was improper to let them have a post-deadline paper that was connected with the one that had been an invited paper on Saturday, so he moved that post-deadline into the same session that I was in.

Rigden:
And did they come after you?

Pound:
Yes.

Rigden:
Okay. What turned your thinking onto the temperature effect? That was sort of a subtle thing, wasn't it?

Pound:

It was. And what I reported in January was the data was coming in, and initially it started to look as if we were seeing what we were hoping to see, but that it was unstable. It wasn't consistent each time; we inverted and so forth. We got the right change of sign but we didn't get the right value of what we mean expected this value. And the data fluctuated all over. In fact on the Monday morning -- we started taking data on Sunday of that week. I had a telephone call from Schiffer on Monday. But Monday morning I had gone over to Harvard Coop to buy an electric blanket which I wanted to keep the apparatus in the penthouse because that penthouse was unheated and suffered the New England climate going up and down day after day. And I thought -- I didn't think anything about the Mossbauer effect being having anything fundamental to do with the temperature, but I thought there would be thermal expansion that I didn't to have producing Doppler effects in the apparatus. So I got this electric blanket. I had trouble convincing the salesman at the Harvard Coop that I knew how they worked. [laughs] He said, “People buy them for the wrong reasons.

The thermostatic control of an electric blanket is controlled by the room temperature, not by the blanket temperature. If the room temperature is low it turns it on more” and so forth. But anyway, that was silly because it turned out that after we started pursuing, when we came back from New York, we started trying to put various tests of why it was giving such unstable data. There was some idea that we didn't know how to make stable electronics as compared with the Harwell people. But I didn't think that was true because I had great confidence in Glen's abilities in that respect. And I had some of my own at the same time. But in any case, about two weeks later we had done a number of tests -- One of the things we first did was I thought, “Maybe the temperature affects this ceramic transducer” we were using to produce the differentiation of the line shift. And so I had been very proud about having obtained that ceramic transducer, because we tried to make some locally.

There was an outfit that made ferroelectric devices of this brand, and I put in an order for them and I kept calling them and said, “Where is it?” and “How is it?” and they said, “Well, the one we tried, when we put it in the oven it cracked.” So this went on and on, and it turned out they had made seven to start with, and every one of them broke when they heat treated it. So I went back to Ted Hunt, my acoustics colleague in the Division of Applied Science, and asked him -- and he called up a former associate of his that he had known very well during the war years because he ran the underwater sound lab, who was at Clevite Brush Development Company in Cleveland. And he said he'd give a look. He thought he might have something like that in his shelf. And so he found this and sent it to me very kindly. So that's what we were using for the modulation in a very neat way. It's a silver-plated ceramic cylinder with ferroelectric material, so you put an ac potential on its walls and it vibrates in length and by just the amount we needed. So I took that out and put in a magnetic gadget, moving coil device, and it didn't make anything any better. And then we tried to see whether the Earth's magnetic field or other magnetic fields were doing anything, wound the coil around the thing and we would turn it on. Now it took quite a little while to see whether something had an effect because you had to collect data enough to get the statistics for it. And at first it didn't look as if it did anything, but in the longer, in longer times it seemed as if it finally was doing something, so maybe there was something in that after all. But the real thing that happened was that the coil was heating the thing. But I didn't think of that at the time.

So what happened next was when I was sitting in the upper level experimental lab which I was running as my teaching duty at the same time, where also the control system for our experiment was at the base of the tower, or the midpoint of the tower. And this student came and asked something about what I was doing and I told him about this Mossbauer effect and I said that the fluctuations, thermal fluctuations of the nucleus in the solid were at such high frequencies mainly because of the nature of the phonon wave that they averaged out during the time of emission of the gamma ray, because these gamma rays were 10^{-7} seconds long and these frequencies were typically 10^{13}. So I said, “but of course the second order doesn't go away.” So then I quickly wrote down on a piece of paper an estimate of the mean squared atomic velocity in thermal vibrations compared to C2 and found the fraction to be about 10^{-13}. So I realized that one degree of temperature would have exactly that as much difference. So that evening I went home for supper and picked up an old coffee can, came in and made a little cryostat out of it in the evening in the machine shop where we could make a test of the temperature effect. Because we could put liquid nitrogen and dry ice and things like that to vary the temperature (?). And sure enough, there the shift fit specific heat curve of a Debye function over the solid quite nicely. So then we sent that off and published that in the middle of March -- well I guess we sent it off about the 1st of March.

Then we discovered -- The Harwell group had this letter from B. D. Josephson which suggested to them that there should be a temperature effect, and that was a few weeks later. Our letter was already in press. And Josephson was an undergraduate at Cambridge University. Harwell, uh, Walter Marshall told me that he tried to -- that's the old story. He tried to call this person called Josephson at Cambridge, Trinity College and the porter answered the telephone and said, “Oh, that's an undergraduate. We don't take telephone calls for our undergraduates.” And so they said, “Well, this is the Atomic Energy Research establishment calling and it's a very important issue,” so they finally did get to talk to him. And then they went and made a measurement, but unhappily they made their measurement with the same apparatus they did their redshift test, and they came out within a factor of only half as big a shift as they expected between the nitrogen temperature and room temperature. They expected the shift to be 2 times 10^{-13} I think, and they found just one, but they said that was okay. But then I sent Cranshaw a telegram to say the correct number should be 4, which is what we get, because – “Your number calculated must be wrong.” And that of course exposed the fact that their apparatus really wasn't capable of measuring this gravitational redshift.

Rigden:
Okay. You just said that the group in England really were not able to measure it because --

Pound:
I sent them a telegram, and later I got, when I visited Oxford Brebis Bleaney had a dinner -- well, a luncheon -- in which he got Walter Marshall to come because -- we've got together again, and Walter was telling me that he was really quite annoyed with his colleagues who in the first place had invoked him into helping in the experiment though he was the head of the theoretical department and on the original iron-57. But in the second case they came to him with a question about this letter from Josephson, could this be so, and he looked at it and said, “Oh yeah, it could be,” and then he did a number calculated on the back of an envelope, and that's the number they were going to publish. And Walter said they never checked it themselves. And he was embarrassed by the fact that he made that factor of 2 error. And so --

Rigden:
Did they ever withdraw their --?

Pound:
They didn’t publish that but did publish Josephson’s article on the effect of temperature.

Rigden:
They never published. Okay.

Pound:

They never withdrew their original one though, in which they did publish the thing that they reported at that January APS meeting. And Viki had suggested that if I wanted to get in on the publications, saying he knew that they had, that they had submitted the paper to Phys. Rev. Letters. And he suggested if I wanted to be along with the earliest publications I should have a paper in as well.

Rigden:
Did that compromise priority at all do you think?

Pound:
I don't know quite what you mean by compromising.

Rigden:
Well, did people -- did anyone --?

Pound:
Cite them as the number one?

Rigden:
Yeah.

Pound:
Not many. I usually get cited. Pound and Rebka usually get cited as the ones who did this experiment, although some people say Mossbauer did it. But a few places that only Cranshaw and company, Schiff or Cranshaw get mentioned, because one such case is the first book of Stephen Hawking who describes the redshift had first and finally been measured determined by the group at Harwell and never mentioned us then. But when Hawking was here a year or so ago, a couple years ago -- he's a remarkable fellow B he came in a special van in which he could get his chair in and out, or his assistant, his now wife, came with him. And I went over to say hello because I had known him from when he came here in -- what was it? '67 or something like that B and I told him who I was and so forth, and I said, “You remember?” And he sat there with his computer and typed out -- I had to wait around for 5 minutes or so to hear his response. And he said, “Of course I know you. We consider you with having proven that the relativity was correct” and so forth, and he was very generous in this little conversation.

Rigden:
Let me ask you a question. It may be -- I don't know how you feel personally, but you know sometime in the late sixties, or in the sixties as I recall, there were people talking about you getting a Nobel Prize for this. Did you ever think of that?

Pound:
Well you know, when you said, “Did you ever think of it?” I never supposed that -- I know that having that kind of competition and having so many people involved in this and that means you don't get cited. Actually when you come down to it, I have said that I won a prize once that had some money attached to it, namely in 1948 I won the B. J. Thompson Memorial Award of the IRE, Institute for Radio Engineers, and I had a hundred dollar check. But that's the only prize I've ever won that had any money. And except that when I gave the Lauterbur lecture to the International Society for Magnetic Resonance in Medicine, there was a, a stipend came with that which is the biggest prize I've ever got. So anyway, as I say, I know that a colleague who has a Nobel Prize himself, namely Sheldon Glashow, told me he wanted to nominate, put my name in as a nomination for a joint prize with Irwin Shapiro [spelling?] for our contributions to experimental relativity. That's the last I ever heard of it, but he did do that.

Rigden:
But you surely have been nominated earlier. Are you aware of it?

Pound:
No.

Rigden:
You're not. Well, I know in my conversations over the years I know on a few occasions when you talk about the Prize and so forth, people have mentioned that Pound should have gotten a Prize for the redshift experiment.

Pound:
Well, especially it's awful -- I don't really like prizes as such. They seem to change people.

Rigden:
No.

Pound:
Of course you know I lived through both the fact that this was essentially bypassed by the fact that Mossbauer got a prize for his thing, but I don't think his thing got much notice before we started this high-resolution stuff. And that's what -- it was the iron that really turned into the tool of all the condensed matter physics and chemists and such people. And 90-odd percent of Mossbauer's spectroscopy went on to that. And its role in nuclear physics was nil.

Rigden:
Yeah. Well, are there other -- anything else you want to say about this wonderful experiment that will live forever?

Pound:

Well, one thing is that it's always cited as this 10 percent experiment of Pound and Rebka but that we went on for four years later, three years more, and it was a comparable amount or more effort put into trying to improve. And then after that, with the help of Joe Snyder, we got a 1 percent experiment, probably a little better than 1 percent. I was fairly generous in attributing the uncertainties. But then of course I have continued with other students and such to pursue the possibility of developing a light pipe to overcome the inverse square law. So that the original experiment is independent of height because the intensity of the radiation falls at the inverse square law, and the uncertainty in being able to measure it improves because the shift is greater. And these two things cancel exactly so it means that the statistical data is independent of the height. So you could do it in the height of a tabletop in principle, but not in practice because there's the issue of systematic error.

So if you could make a system that could avoid the inverse square law then you can use a much taller system, and I looked into using locally the Prudential Tower and went to New York and studied the Empire State Building and decided that like Edwin H. Hall, our colleague of 1902 who used this tower here, that there were advantages in having the results of your colleagues in the laboratory at hand that over-weighed some of the opportunities of having a larger place. Because he had looked into using the Washington Monument as a basis for his tests of whether objects fall south, move south when they drop. And I had the same concept of the advantage of the laboratory. The real thing that put me off about the Empire State Building was that all the high-energy radio and TV emissions for the New York area go from the top of that building and the electromagnetic background must be something pretty fierce. I didn't think that it would be much fun to try to run sensitive detectors in such an environment.

Rigden:

Let me just ask one more question. If you had named that, if you had titled your paper something like “Verification of Einstein's Redshift,” it would have attracted more attention than “The Weight of a Photon.” Why did you name it that?

Pound:

I named it “The Apparent Weight of Photons” in the original paper, and I got flack from Jerrold Zacharias when I gave a talk at MIT because Jerrold came -- he was my boss during World War II -- and he said he had made a reputation over trying to develop atomic clocks sufficiently well to be able to do the redshift experiment, and that's one of the things that put it in my mind because he talked about it off and on. He came up with this idea of the fountain kind of atomic clock basis and he, but he was thinking of measuring between the top and bottom of mountains in Switzerland for example. But he hadn't got that far. And he said, “You know that was a great experiment, but it was a terrible title. Why did you use such a title? You know it isn't that, because nothing happens to the photon. It's only that the timescales of the two ends are different.” And I said, “Yes, that may be, but I can't prove it by my experiment because we only measure that the effect is the same as it would be on anything else falling in that length of time.” I think he may have partly accepted my argument. And I also had flack from Lev Okum, a Russian.

Rigden:

Landau?

Pound:

No. A more, a man that's still relativist and -- oh, he writes on -- he dislikes the idea of implying that something happens to the photon also. I'll think of that name eventually. But anyway, I thought it was less pretentious to talk about that, to talk about it this way. Because it doesn't overplay what we actually operationally could test.

Rigden:

Later, in fact it was around -- I think 1980s, early '80s when you did your little experiment with heating yourself with microwaves?

Pound:

Well actually I never really got to doing an experiment on myself. That thing over there in that box in the bottom is a 500-watt 5300 MHZ backward wave oscillator which once had a classification that should not have allowed it out, but they gave it to me from Raytheon because a man named John Osachuk was an officer there and he's one of the strongest exponents of opposition to all the business about the dangers of electromagnetic fields. That's partly because Raytheon owns the company that builds -- well, they started the building of microwave ovens and so forth. Of course they got all kinds of flack and lawsuits and things about the dangers and the hazards and the troubles produced by things like that. So in that general area of science if you call it, he's one of the people I had interests with, and the other person of significance was -- what's her name? Ellen R. Adair. And Ellen R. Adair is the wife of Bob Adair of Yale, and she is an experimental psychologist and she worked with -- what is it called? The J. B. Johnson? No, the foundation which is associated with Yale University Medical School in which they are concerned with the effects -- They had a big project there for measuring the effect of infrared radiation on people and so forth. And she had an oven, a microwave thing in which she was studying the effect of microwaves on squirrel monkeys, the behavior of squirrel monkeys. And she was very excited when I published this paper in Science in 1980 on using it for heating people, because she had never found any significant impairment or any problem with her monkeys that -- Well, she in fact has done some very worthwhile experiments under the auspices of the Air Force, I think it was an Air Force base she went to in Texas, and she got written up in the New York Times a couple of times in the last year or so about having -- She finally got around to doing experiments with people.

I never got around to doing anything. I was going to build a demonstration facility here, and I got approval of the IRB, the Institutional Review Board, for carrying out experiments on human subjects, with some conditions. Among the conditions were that I could only use subjects who were knowledgeable enough about all the controversies to know what kind of risk they were taking. And I knew they weren't really taking any risk, but that's all right. Of course there was a large group of -- many people really felt that the evidence was strong that cataracts of the eye were strongly produced by the effect of microwaves, but the man who had originally done that said, “Look. If I had radiated anything but the eyeball with that, people would have fried,” because he said he was using a tremendously high level compared with anything anybody would think of being exposed to just for this purpose. And there was a man in Alabama, the University of Alabama I believe it is, where they had a primate center, and he wrote me one time that he was having trouble getting Science magazine to publish this article which he had written describe these thorough experiments he had done with primates, monkeys too, in which he had them go suck on a tube from which they could be fed honey, and every time they'd do this the 3-centimeter wave microwave source would blast them in the face. And he had a team of ophthalmologists and clinical psychologists, all kinds of medical people that analyzed them, after doing this for several months on these beasts, to see whether they had shown any problem with their eyes.

Rigden:

None.

Pound:

So, and this of course, the people that look into these questions always doubt their validity because they know that the project in this case was supported by the U.S. Navy and it was in the interest of the U.S. Navy because they were under suit from former radar operators from ships that had 3-centimeter radar who were claiming that they had damages that had been caused by their employment. And I gave a talk, as I mentioned the other day, to this group that Heinz Barschall chaired at the University of Wisconsin. I was there actually to give the lectures on, in the physics department there is an endowed lectureship which is named for Julian Mack used a spectroscopist to compose the tables of nuclear isotope tables? X-ray spectroscopist? What was his name? Anyway, they have that lecture series that I was to give, and Heinz got me to give this talk, and he told me that he was about -- he had been attempting to organize a conference on the effects of electromagnetic fields and there was one person whose name I won't mention who is a dominant antagonist in this area -- sort of the biggest known name attacking people -- he wrote and was going to get him to come and this man replied that he had seen that Pound was on the list of invitees and anyplace that he was going to be he wouldn't think of going.

Rigden:

Oh dear.

Pound:

And that's how Barschall introduced me to my talk in Wisconsin.

Rigden:

So that experiment, really you never did fulfill that.

Pound:

I never carried it out. I had a little shed out here which --

Rigden:
Is that the old little red shed?

Pound:
No, it was another. There were three sheds out there beside what was then named "Research Laboratory of Physics," later renamed the Lyman Laboratory of Physics.

Rigden:
One of the sheds you did the NMR experiment.

Pound:
Yes, that's right, the big one. The big one. And that's going to come down. It was supposed to come down last December. I was asked by one of the people in the lab here who was doing some of the historical things, he wanted to know about the history of that and so forth, and then he asked if I had qualms about, sensed loss in its coming down, and I said not really, because it had changed its use so much and been fancied up and wasn't at all like what it was then and I said I would feel much more loss in the disappearance of the tower situation in Jefferson -- which is gone too.

Rigden:
That's gone, yes. You said that.

Pound:
The tower is still a separate masonry structure there, but the top penthouse and the attic space through which we had to carry all that junk back and forth and down to the basement Sabine reverberation chamber, there was a lot of physical effort that had to go into getting that experiment done. They are now modifying that space above the fourth floor in Jefferson into office space for more theorists and so they, they called a meeting a few months ago to see whether there was anything of historical significance still lying around up there. And there was one thing that was there which they have collected and Andy Strominger, who is actually a -- what do you call it?

Rigden:
Curator?

Pound:
A string theorist. No, he is the head of the theoretical people, but had got all that modification done on the fourth floor, people somewhat cynically call it the Taj Mahal the way they modified the fourth floor at Jefferson, but he wanted to have this thing preserved as part of the art work in memory of this experimental thing in that new space they are going to build. And what the thing that was found and I knew was there actually was one of the two big proportional counters that Ron Drever designed when he was here that year in 1960. 1961, wasn't it?

Rigden:
Uh-huh [affirmative].

Pound:
By the way, you know the book you sent me that had a picture of me and the experiment or something?

Rigden:
Yes, yes.

Pound:
On the other page it had the gravity wave detector from Cal Tech.

Rigden:
Yes.

Pound:
In the one picture -- I showed this to Ron Drever, and he was a bit concerned over the fact that he was involved in both of those pictures. Because that thing at Cal Tech was his, and within that picture on my page of my experiment was his, quite significantly observable was his proportional counter which he built for our project.

Rigden:
Okay. And I wrote the captions on those things, so -- Well, let's move on. We'll probably have a little more -- In fact, we're almost done but we'll keep on going here. All right. Bob, you said you wanted to say one more thing in the experimental vein.

Pound:
Well, on the issue of being able to test the effect of microwaves as a heating source for people. I made applications for a patent on the idea and that got canned by a patent examiner who took an illegal position of judging it as being medically dangerous. And that was not his role. That had nothing to do with it. But we were not successful in fighting him that way, and I decided it wasn't worth the effort because there was so much, so many people were gun shy of the idea that I knew it wouldn't ever go anywhere. But then the other thing was, that I thought I could get some support from the DOE because they in fact had participated in some conferences they set up, but then they -- there was one conference at Yale with Ellen R. Adair and company, and it was the, one of the divisions of the DOE on environmental safety or something like that, and they behaved in the beginning as if they might be interested in seeing some support in this thing. But when I got around to talking to them directly about that possibility they thought that they would have to put so many conditions on how it was done that they wouldn't do it. But then they got me involved with the new sources of energy kind of group at the DOE. What was his name? A Polish name that chaired that division. And he encouraged me to submit a proposal and I submitted a proposal to get some funding to do this thing where I thought you could have some people go sit at the desk and read with the surroundings at 40 degrees but feel happy because they were in this radiation field. And they had to send that out for review and they sent it to ten reviewers, and the list of their responses was that some were extremely enthusiastic. A couple were moderately, but I think it was three that were very negative, in particular the most negative was this person I mentioned otherwise, saying he was shocked that a major university with a reputation would tolerate anybody making proposals like this and so forth. And so -- And it sounds to him as if he would have to make a tutorial report here to explain what electromagnetic fields are all about to this man. And he didn't -- Perhaps he didn't know that I had taught the course for many years in intermediate electricity and magnetism, but never mind. So anyway --

Rigden:
That's interesting.

Pound:
That's what happened to that, and that's why these things just sit here, and that's the power supply for that klystron, that backward wave klystron, and my students -- I had some wonderful students back in the seventies and eighties who put this whole system together. And I never got to use it really.

Rigden:
One of your long-time students -- who I got to know -- was Vetterling, Bill.

Pound:
Oh yes, Bill.

Rigden:
Is he still at Raytheon?

Pound:
No.

Rigden:
Okay. All right. Well listen --

Pound:
No, not Raytheon, Polaroid.

Rigden:
Polaroid. Yes, that's right.

Pound:
Polaroid. And he's the fair-haired boy there, because the one thing that they want to push of their own now is new technology for printing in digital domain, and he got some fancy prizes from them over the last couple years for the development of their fast-printing high-quality thing that they are going to peddle to the Ritz camera types and so forth.

Rigden:
Very good. Bill is a nice guy.

Pound:
And he's a great teacher. I've talked to -- well, I was hoping that they might bring him back to the division here.

Rigden:
Yeah. Okay. This is the end of the tape now. Okay. I would like now -- I gave you an outline yesterday.

Pound:
Oh yes.

Rigden:
I'd like to sort of talk to you about your life as a professor, and I'd like to ask you about how you maintained a balance between your various responsibilities, most specifically between your research and your teaching.

Pound:

Well, that's -- I always have said that one of the great things about being in this institution is the quality and interests of the graduate students in being in continuous contact with this developing young people, remarkable for -- and you mentioned one just now who was such a person. And of course I started with -- my first student of significance, that I was solely responsible for but never really completely because there was always colleagues in the background, was George Watkins, who is now a member of the National Academy and so forth. But he came to me in '49 I think when I was first a faculty member, and in those days the students I had were mostly veterans from World War II, as he was. He had been in the Navy during the war. And then the next student was the one named Christopher Dean, and he followed up on the Dehmelt and Kruger studies of chlorine in his thesis work and pure quadrupole resonance, and we developed and continued to develop for quite a time when he went to the University of Pittsburgh the super-generative kind of NMR detector which I was aware about the behavior of super regenerative devices from my ham radio days, because that's what we used for UHF kind of reception in those days.

It was a very crummy kind of thing, but it turned out to have some qualities that also made it into a pretty good EQR detector. And so far in my -- and my teaching as an assignment was I began in -- I said the other day that I had given a special course of my own design as a junior fellow. That's not allowed in -- well, it was, in those days there was a taxation issue as to whether people could be asked to do things for the university as junior fellows because the junior fellowship was then not taxable. But now it is taxed like every other fellowship and so forth, so it would be different now. But I gave that course with, oh I don't know, eighteen or so students, including Dave Middleton who was a specialist in the theory of noise. He had worked during World War II with Ben Van Vleck on that subject and so forth.

But when I first started teaching as a member of our physics department I got assigned initially a course called “Physics 10.” And the theory of Physics 10 was that it was a one-semester course that brought the students who had studied Physics 1, which was sort of a non-calculus-based physics mostly for premeds up to the level of Physics 11, so 1 + 10 is 11. It was an impossible thing actually, because in principle you supposed that these were students who had developed an interest and wanted to learn, but actually they were students who wanted to satisfy some requirements that required them to have the equivalent of 11. And then they were doing this struggle in the least painful way they could. So that was my first teaching assignment, and I say it was impossible because you had to cover the whole of elementary physics in one semester at the level that was higher than anything they'd [had]. And you soon learned they hadn't learned much of anything from Physics 1 yet, so it didn’t really work. Then I took over the teaching of the intermediate level of electricity and magnetism, which was 131 in our system, and I taught that. I have a book here with many, many names in it of people that I still meet when I go out to various places who say they were in your course and so forth.

Rigden:
Is that the course Purcell eventually took over his EMM book came out of?

Pound:
No, he didn't take that one over. His EMM book was aimed at the introductory level, which was 11B, and it became 12B.

Rigden:
That's right. That was the Berkeley course.

Pound:

And then it became 13B. It was always regarded here as a bit too hard for our first year students.

Rigden:

Yeah. Okay, you go ahead, I'm sorry.

Pound:

And I used his book when I taught. I did teach 11 and then 12 -- or then 13, which 13 was the course of the same sort of curriculum but which was designed to take advantage of advanced placement students that had had calculus and things since high school and so forth, and I taught the electricity side of that, and I had advantage of the manuscript from Ed's book before it was printed. And then during the time I was doing the redshift experiment I had been assigned to give 12A, which was the mechanics course. Is that right?

Rigden:

I think that's right, from things I've read about you.

Pound:

Yeah. That was the first time I had taught that course, and there were 230-odd or something like that number of students in the course, and that was in the spring of the time that I was in trouble about what the instability was. And I guess I wrote it in that article about the night that I had come in in order to make my notes for the 9 o'clock course the next morning, and at 12:30 the telephone rang and it was the Harvard Police.

Rigden:

Yes, go ahead.

Pound:

Who told me that my wife had been trying to get in touch with me because she said there had been an explosion in the oil furnace downstairs. And so I rushed home and the oil burner man was there working on it, and he couldn't figure out why, but he thought he had fixed it and put it together and I came back to continue what I was doing, and it was by then 1:30 in the morning. And then shortly after I got back Betty called me again and said it had exploded again. So I went back and the oil man was there again and he said, “Do you have to work this time of day?” He said, “I'm a night man for the oil company.” I said I have to work all times of day. And so then he suggested that well, he didn't know quite what it was, but it was a very cold night and you couldn't be without the furnace, so he said it would get it running and let it stay running and if it got to satisfy the thermostat -- and it has a high-temperature shutoff, because that house had radiant heat in which the floors and the ceilings and so forth were heated. It was a contemporary house designed by a contemporary architect named Carl Koch. And so he said I should listen for it to shut down and if it did then I should turn the switch off so it wouldn't try to start again. Well, I went to bed but fell asleep. And at 7:30 in the morning it went blam again, so it was lucky because I could get up and go and make it to my class.

Rigden:

Do you feel that your teaching and research were comfortable partners, or was there tension between them?

Pound:

No, I never sensed any particular tension between them. No, I didn't. Because -- well, I think I learned always from teaching things that were significant in my knowing of things. The second course I gave for example was electron physics, which was a graduate level course, 231, and I picked up the syllabus for that. Purcell had given that course the year before, and before him it was E. L. Chaffee that had given it, called Physical Electronics or something. But there I learned about things like magnetic lenses and the reason why betatrons had to have the right kind of magnetic field shape and not fall off more rapidly, the 1 over R, whatever it is, and the fact that it made the vertical and horizontal oscillations at certain frequencies. I got through a lot of things there that were quite useful to know. And I had quite a group of students. Do you know the name of George Benedick for example?

Rigden:

Yes, yeah.

Pound:

Well, George was a student in that course as I always remembered, and George was rather pushy in the sense that he didn't want me to get away with not explaining things completely or something. But I of course have been friends with him ever since.

Rigden:

You have been privileged to work in a very special environment. Let me just tell you a little story. Your former colleague, Schwinger, I spent time with him in UCLA. He told me he would never have dreamed the difference between Harvard and UCLA. He said, “At Harvard I had four good students a year. At UCLA I have four good students a decade.”

Pound:

Yeah. I can believe that. Well, I oversaw Julian Schwinger's decision to depart from Harvard, and I was department chairman you see, and so I am looked upon as the one that lost Julian. And the thing was, that he had this longstanding friendship with Saxon [spelling?] who had moved up by that time to be the president of California University System.

Rigden:

Yeah.

Pound:

But he had been trying to get Julian to come to the UCLA physics department while Dave was there, but Julian had become concerned about physical health being dominant in determining mental health in the long run. So, unknown to his friends here -- except he told me -- he and Clarice, his wife, had hired the YWCA swimming pool for once a week in Cambridge and they would spend the whole morning at the swimming pool. But he said he could go to California and have his own swimming pool. And then this close friend of mine called Asim Ylilig who was a Turkish fellow who had been an engineer and had become graduate student of Julian's after having actually also held a professorship at the University of New Hampshire. He told me that Julian, he went to Julian and asked him if he would teach him physics, what kinds of physics he meant, and he said he made a compact with Julian that if he would teach him physics Asim would teach him tennis. And so Asim was a world class tennis player actually, and he got me back to playing tennis and I belonged to the Badminton and Tennis club in Boston, because he was the one that got me there. But he had been invited to the Longwood Cricket Club when he came here as a student because he had got a doctorate in engineering from Yale already but he wanted -- he claimed it was hearing me give a talk at New Hampshire where he had this professorship that got him concerned and interested in physics. And so he came and applied to get a Ph.D. in physics, but under Schwinger. And he had been in Turkey a member of the Turkish Davis Cup, was it, Davis Cup tennis team and was taught tennis by, his tennis was created by that famous German, prewar German tennis player.

Rigden:

Well, actually Julian Schwinger told me about that.

Pound:
Oh did he?

Rigden:
Yeah, yeah. Let me shift now again to -- we're sitting here in 2003 and this will be the last topic. Your life in physics started, well, started with the Rad Lab, and so it's a good, an amazing period in the history of physics that you have been one of the principal --

Pound:
Well in a sense it even started in college, because I always remember having to give a talk about analog computers back as an undergraduate, and the research apparatus I had to build.

Rigden:
I have two questions. The first one is, as you think about all the physicists you have known and seen come through Harvard and whatever, how would you -- would you care to comment on any of them? In terms of your sense of the contribution they made and --?

Pound:
Mmm, well that's a pretty broad and --

Rigden:
Yes, it's perhaps too broad, but I just thought there may be a couple that stand out in your mind or --

Pound:
Well, I hadn't thought about it that way. Of course Purcell, having been such a close friend with whom I shared a lot of things and most of the time when he was doing things that I would hear about it more or less directly from Ed. Of course the other person that I had a great attraction to was Henry Torrey, the other member of our team, because I always felt that he had really been very helpful in turning me back into being a physicist, because really what we were doing at MIT Radiation Lab was engineering, was communications engineering basically. And I have a feeling that Henry, he was always very supportive, and you know there's that picture of the three of us. The other person I was trying to remember was Desmond Kuper, JBH Kuper, do you remember him?

Rigden:
No.

Pound:
K-u-p-e-r. He was the editor of RSI for many years.

Rigden:
Oh, okay.

Pound:
Those two, and I have my little communications thing which was based on my stabilized oscillator, and Henry always said he was embarrassed to be in that picture because he had nothing to do with that, and neither did Kuper. That was entirely my own thing. And I don't know if I told you about the fact that Louis Ridenour, who came back from the Office of the Secretary of War came and wanted to learn enough about microwaves to set up another group to develop that thing into a communications system. And he did. He said he got the dregs from lab, because at that time it was late.

Rigden:
Well let me ask one name. Did you know, did you ever meet Bohr?

Pound:
Yes, I did.

Rigden:
What did you think of Bohr?

Pound:
Oh, I thought he was a very fine man, and I went to Copenhagen in the summer of '51 and I knew Bohr's son Aage rather well, and he suggested that I come. I was in England as I told on this Fulbright thing, and he suggested at the time to come to Copenhagen might be one -- Bohr was running a reunion at the Institute for those people who had been fellows there over the years, and he thought I should come then, because then I could participate and listen in on that. And people like Bethe -- And in fact the person that flew from England and helped us get to the airport and so forth at that time was Hans von Halban, Hans Halban, who had been in Los Alamos during the war. But he was a very close friend, and he had left Germany to go and work with Joliet-Curie in Paris, and when France was falling Joliet-Curie sent Halban and Kowarski with all the collected heavy water to England, and the Germans tried to catch them and sank a cross-channel boat, but it was not the one they were on.

Rigden:
The wrong boat, huh?

Pound:
Yeah. So Halban -- You see one reason I wanted to go to -- part of the reason I wanted to go to Copenhagen is that my wife's family are all Danish, and she's very well connected in Denmark. But we got invited then as a result to the reception at Bohr's house. You know, Bohr lived in the Carlsberg Foundation house which is on the grounds of the Carlsberg Brewery. It's the only party I've been to where it ended up with fireworks in the garden at the end.

Rigden:
That's nice. All right, let me ask one more question. Not people, but just physics, over the period of your active career from start to now, physics, the culture of physics has changed, the support for physics has changed, the scale of physics has changed. How do you think about it?

Pound:
Well, there are aspects of all of that that depress me considerably because I ask myself if I were starting over now would I find myself oriented that way. I don't know what else I would be oriented towards, but I don't find it very attractive to be, to have to deal with the kind of things that people have to deal with nowadays -- especially the raising big monies and being parties to big groups in which you have a little piece of the action and so forth.

Rigden:
And the degree of specialization is what you're saying, yeah.

Pound:
That's right. And when I've given talks I've described the difference between prewar, postwar and now as evidenced by the number of pages published in the Physical Review, and in 1939 one month of the Physical Review contained about 200 and some pages. In 1946, the year of our first letter on NMR it contained 53 I think, for the whole month. That was two issues jointly put together. But then by the year I was writing this up in the 1990s sometime one month of the Physical Review contained 8,000 pages in its -- what is its nine or ten versions.

Rigden:
Yeah. A, B, C, D.

Pound:
Yeah. And the Letters is a separate issue.

Rigden:
No, it's a different world.

Pound:
And you know, even in 1960 when I gave this talk on the redshift everybody knew -- everybody that went to the Physical Society meeting was interested and came to that big auditorium where I gave that invited talk. But nowadays it would be so fractionated -- one little thing here, one little thing there. I mean, there will be big crowds at every one of them because there are so many big crowds available.

Rigden:
But how do young physicists get enculturated into the discipline? I'll tell you, that January meeting, annual meeting of APS as a very young person, I went to hear the talks of the big players, and I became aware of physics through that meeting. You know, it was one big meeting. That doesn't --

Pound:
That's right. Exactly.

Rigden:
A young person today has no idea as they look through Phys. Rev. Letters whose papers they should pay attention to because they are important people. They don't know that.

Pound:
That's right. I agree. And of course there is so many, so much volume of it that you can't possibly cover anything significant of the totality, whereas back in those days you looked at the whole issue of what's coming out and you knew that everybody was excited by one thing or another thing at a given time. Let's say Charlie Townes describing the possibility of making a maser back in the 1950s, and Charlie has also cited the fact that our experiment called a spin system at negative temperatures or something like that was part of his inspiration for it because he realized from that that stimulated emission was going to be a significant factor. Because that's the first publication which specifically described that, because the inverted system when it was tickled with rf emitted rather than absorbed.

Rigden:
What do you think about the relationship between theory and experiment today as opposed to fifty years ago? And you're building new wings for theorists here. Have theorists lost touch with experiment?

Pound:
I don't really think so. This man, Andy Strominger whom I mentioned as having overseen that thing, he teaches the course on general relativity in the graduate school and every year he asks me to come and give one of the lectures there on the redshift because he said, “This is where it was done.” And he has on the wall up in that Taj Mahal the number which was the energy shift due to the redshift in that building, taken from my paper. And he had it engraved into the glass of the department office up there.

Rigden:
Have you taken a picture of it?

Pound:
No. [laughs] He's the one that I mentioned that wanted to collect that counter thing as a part of the artwork of the renovated attic.

Rigden:
Bob Pound, this has been a wonderful period. And I want to thank you for myself and for AIP, but before I turn off the tape, is there anything you want to add?

Pound:
Well, I thank you for thinking of me in this respect, and it's been a pleasant opp but I'm afraid I have a tendency to over-talk.

Rigden:
You've done very well. I don't think you should feel that. And I will tell you, long after you and I are gone, people will be looking at these transcripts and bringing insights into the work you've done and into the period you've been a part of. These are very frequently used results at AIP. So once again thank you, and this tape is almost done so I'm going to turn it off.

Pound:

Okay.

Session I | Session II