Oral History Transcript — Nicolaas Bloembergen

Interview with Nicolaas Bloembergen
by Joan Bromberg and Paul L. Kelley at Harvard University Cambridge, Massachusetts
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ABSTRACT:

Graduate research on nuclear magnetic resonance at Harvard with Purcell and Pound, 1946-1947. Leiden postdoctoral fellowship, 1947-1948. Microwave and nuclear experiments as a Harvard Junior Fellow, 1949-1951. Early years in the Harvard Division of Engineering and Applied Physics. The 3-level maser. Nonlinear optics in the 1960s.

Transcript

Bromberg:

I asked you to begin with how you got to Harvard and how you came to work with Professors Purcell and Pound, and I'd like to know something about what was happening in Holland before, that brought you to that group (if it did or didn't); some of the details of what your background was, what you wanted to work on.

Bloembergen:

Well, I always had the plans to do a thesis research for a Ph.D. outside Holland, just to get a broader scientific outlook. Clearly, at the end of World War II, the opportunities for that were severely limited. Practically nowhere in Europe could I go, because it was devastated. Conditions were extremely difficult there. So, at the advice of my older brother, I just wrote to three places in the United States; tried to see if I could get there as a — you know, as an individual.

Kelley:

Is your older brother a scientist?

Bloembergen:

No, no, he is a lawyer and a businessman. But things were so desperate. So I spent days writing letters in longhand. What could I do? I didn't know much about the United States. I just had seen a few copies of the PHYSICAL REVIEW, and I knew there were some places that had a fairly impressive publication record of papers I usually couldn't understand. I wrote to three places—the University of California in Berkeley, the University of Chicago, and Harvard University; and as I wrote previously, Chicago never answered my letter. This was in June, 1945. Berkeley answered in a letter that surprised me but shouldn't have—that just shows how parochial the view was. I thought the war was over now that the Germanshad surrendered. They wrote back, well, they couldn't have foreign students as long as the war went on, the war with Japan, would I please write them again when the war with Japan had ended and they would consider me. But from Harvard I got a letter from the chairman of the physics department, Dr. Otto Oldenberg, that I should send some more documentation—my degrees and so on; and I did that, along with some letters of recommendation from my professors in Holland (no personal acquaintance whatsoever). Then I got a letter back I was admitted. I wrote [previously] about the other thing, that I had to get money, and the Dutch Ministry of Education had put me on a list of 20 people, covering all fields. There were two physicists—Abraham Pais (who wrote this book about Einstein just recently), and me. But the only people who could actually get money were people in Chinese language and American history because the U.S. government was very cautious about admitting scientists in those days because of sensitivity (about the war effort). Well, so, there I was, without money. But my father said, "I'm willing to give you money, but of course it's Dutch guilders which you cannot exchange, you know." In '45 you needed a permit (for currency exchange) just to show you how bad things were. But I went with the following argument to the Ministry of Education. I said, "You think I'm good enough to go. I'm on your selected list. Harvard University thinks I'm good enough to come. My father's willing to put up the money, so the least you can do is get me a permit to change Dutch guilders to dollars." They appreciated the logic of that. Only one place in Holland could give that permit out, that's how restricted things were. That was the Central Bank in Amsterdam. I came there with my permit and he said, "Who are you? Why have you got all this, a lot of dollars? The most we've had is requests for $500, and you've got $1,800." "Well, I said, "who are the people you've had so far?" "Well, professors who want to take up contacts again, and tulip bulb dealers, who want to sell bulbs for next spring." I said, "Ja, but they go only for six weeks. I go for a whole year so I need more." That was just curious. It was an exceptional permit. And so I just went to Harvard. I didn't know anybody there except Oldenberg whom I had corresponded with, and he took me out for lunch the day I arrived. Then he said, "Well, just look around and see who you can do research with." This was in the spring of '46, Febrary '46, because I had to wait four months before there was a boat that would take me. There were no regular lines going or airplanes either.

Bromberg:

What were things like here?

Bloembergen:

Well, you see, it's all a matter of very fortunate coincidences. The large bulk of students came back just the spring of '46. They'd just been discharged from the Army, and there was an incredible selection of bright people, of about four or five years or generations of students, all coming back. Like Walter Cone and Luttinger and Blatt (of Blatt and Weisskopf); just a galaxy of people, and they all had to take courses. They all had to start after the bachelor's. They had been drafted. They had to go. So practically nobody was ready for research. So I was one of the few; because I'd passed my examinations in Holland, I was one who could start research. So I talked with

Street in cosmic rays and with Purcell, and then I clearly noted that Purcell and Pound and Torrey had just done a remarkable experiment, just six weeks before I arrived, on magnetic resonance. I'd never heard of magnetic resonance before, but I recognized it as something. They explained to me it had something to do with Leiden —magnetism in Leiden by-Garter, magnetic relaxation. So I said, "Well, that's a connection."

Kelley:
So you had done some gradute level work in Holland?

Bloembergen:
Yes. I had published—well, very little. I published a paper on a sensitive light detector using a phase sensitive detection scheme. I had a copy of that paper which had appeared in PHYSICA and Purcell liked that, so he said, "That's what we are going to use, the lock-in scheme."

Bromberg:
Sorry, I didn't get that.

Bloembergen:
Lock-in phase sensitive detection scheme. But I'd used a phase sensitive galvanometer—the old fashioned way of doing it. Purcell used a Dicke amplifier. So, I was familiar with some electronic techniques and so on, and some optics. So we got started doing magnetic resonance.

Bromberg:
Was it just the three of you, Purcell and Pound and you?

Bloembergen:
That's right. Pake came later. And in fact it was just me in the lab, because Purcell and Pound had to work very hard writing volumes for the MIT Rad Lab series.

Kelley:
Did they still have offices over at MIT?

Bloembergen:
They were always at MIT in the spring of '46.

Kelley:
In Building 20.

Bloembergen:
So I had plenty of time to catch up, and my first task was to make mud of calcium fluoride powder, and mineral oil from the druggist, and worm it in through a small hole into a big cavity. Just to show you what those days were like, we were doing a radio frequency experiment at the wavelength of 3 meters, but since they had worked at the Radiation Lab on microwaves, they used microwave circuit, and they had a big capacitatively-loaded cavity, rather an LC circuit. A liter of mineral oil and mud in there, because they wanted to see the fluorine resonance, in addition to the proton resonance. They had only seen the proton resonance in mineral oil, which was a very fortunate choice, because it has a much shorter relaxation time than water.

Bromberg:
This was physically at MIT?

Bloembergen:
This was physically, here, in the Lyman Lab, at Harvard. So they scrounged a big very inhomogeneous magnet—we didn't know what homogeneity we needed anyway—from Street, used for cosmic rays, and that's how the first experiments were done. My first task was to fill that cavity and we put it in the circuit a week later, and we got the fluorine. Then we decided we had to get our own NMR equipment going, and we got another magnet, 50 years old, made by the Societé-Genevoise, and that turned out to be a beast. I had to build a regulating supply for it, because it had a curious oscillation. It was fed by 60 cycles, but there was a big ripple on the supply at about 1 cycle, and we had to build a new electronic feedback and so on, but it was impossible to get that out. At first of course they wouldn't believe me. Then Pound, who was the electronics expert—he's a true electronic wizard, you know—at 12 years he was already making radios—he looked at it, and he finally agreed. I wasted two months on that.

Kelley:
Did you figure out what it was doing?

Bloembergen:
Yes, it was a curious fault in the winding, and the time constants were just the worst possible. So what we did, we bought a set of submarine surplus batteries to run that magnet DC. Then we had to vary the current. We didn't even have a big rheostat to change the current. So I remember taking manganese resistance wire, and stretching that thing to the ceiling, and then, with a hand held clip sliding along the wire I got the magnetic resonance. It was surplus, that kind of equipment. I also built a Dicke lock-in detector. That was my other task that first half year. Sure enough, we finally got a resonance. He talked with Rabi upstairs and then I came up after a half an hour. I had returned the whole thing. There was the proton resonance so Rabi could see it. They were both very happy. It was very very primitive equipment—no money. You ask about money—Purcell had so little money that he couldn't offer me even a research assistantship. And I realized I needed some more money to finish the project, so he put me on for a month at the time and then took me off again. I just extended my stay to a year and a half that way. But by that time, we had all the major results of our BPP.

Bromberg:
It sounds as if you were making all your own equipment. Was there a shop around here?

Bloembergen:
Yes, but I had to do the work at the shop. There was no money to pay the machinist.

Kelley:
That was before the big influx of money from the Office of Naval Research?

Bloembergen:
That's right. I shaped my own crystals of calcium fluoride into a cylinder, shaped so it would fit in the coil.

Bromberg:

So all you got out of it was their equipment to work on, really.

Bloembergen:
And the idea I got. But then, since they were still so busy with the Rad Lab—I had a lot of ideas on my own to do. So they were very happy, and I was too. Things moved very fast. But I knew every little detail, you see, so I could operate that equipment. I built a very primitive RF bridge, because I didn't know that much about electronic theory, couldn't buy those things, so I built my own RF bridge, phase and amplitude adjustments and so on, that kind of thing.

Bromberg:
I was going to ask something about, who was speaking to whom in those days, what the community was like, whom you saw most of here?

Bloembergen:
I saw other people, just as visitors. I talked daily or weekly with Purcell and Pound. And of course, they came back in the fall of '46 here to Harvard full-time, because then the MIT project had been finished. But you know, it just gave me a breather to really catch up with them. Sometimes I knew more than they did, because I was spending all my time on it. That worked out very well. Then of course Purcell had to teach. He was a young associate professor so he had to spend time on that. So I did all the experimental work in BPP. That's why my name is first.

Bromberg:
Was there anybody in the way of other students here—people you were talking with?

Bloembergen:
George Pake. He came on board, oh, in the fall of '46 or very early '47, and then George Pake and I were Purcell's first students.

Kelley:
So you really started the lab again after the war. Basically that's what happened.

Bloembergen:
Yes, and there was very little money, just a string and sealing wax type of operation.

Kelley:
After the laboratory started, you got some very fantastic results very quickly.

Bloembergen:
Oh, and they wanted me to take a course with Julian Schwinger the first term, which I did.

Kelley:
Did that help?

Bloembergen:
It gave me confidence, yes. Oh—I know what you're referring to. Schwinger at that time wasn't formal at all. Schwinger, at that time, gave a course in electro-magnetics which was superb, and very practical, because Julian, which I discovered much later, was working on the synchrotron radiation problem, in those days. Then, the next year, I audited quantum mechanics, which was also very good. Only later did he become so formal. You see, Schwinger was also still under the spell of the Radiation Lab.

Kelley:
He was working on Green's functions, cavities... [crosstalk]

Bloembergen:
—yes, that's right, and perturbation theory. So the community was everybody from the Radiation Lab, and you see, it's no accident that Bloch of Stanford did the same thing independently. The real experimental wheel there was W.W. Hansen; and Hansen traveled around between the Radiation Lab here and other people using microwaves. He was the experimental expert. So this was a tight community, and DuBridge and Rabi and everybody wondered, what can we do with these microwaves when the war is over?

Bromberg:
What about the Harvard Radiation Lab, how is one to think of that in terms of the MIT Radiation Lab?

Bloembergen:
These were all at the MIT Radiation Lab.

Bromberg:
Bloch was here at Harvard, for example.

Bloembergen:
Well, in that case, he and Van Vleck worked together on what is called the Harvard Radioelectronics Lab, which was concerned with matters of noise and counter-measures. And that was a very small group, compared to the MIT group, in the Vanserg Building at Harvard which I think is still up here.

Kelley:
Building 20?

Bloembergen:
Yes that is at MIT.

Kelley:
Building 20, that's the very old building along Vassar Street—that's a two story or three story wooden structure. That was where all the microwave research went on, and development of actual equipment that was then manufactured and used.

Bloembergen:
Yes, they had to develop the next generation of microwaves, so when everybody used the 20 centimeter S or 10 centimeter S-band, they worked on X band, and when the X band became operational, they worked on the 1 centimeter. But there was a group with Bitter at MIT, and he had two students, Alpert and I forget the second name,

who sort of dropped out. They had trouble finding magnetic resonance. They came over, and we told them what to do. You know, now it looks so easy, it's an undergraduate experiment, but at that time it wasn't so obvious, what the best way was. Fortunately the path by Bloch and Hansen was somewhat different from ours. They used the induction dynamic method. We used static susceptibility.

Kelley:
So all these ideas had been basically sitting around during the war and people had been thinking about them.

Bloembergen:
Oh yes, it was a common intellectual reservoir. That was obvious. And then there was a big meeting in the spring of '46 of the APS, and I remember Bloch gave an invited paper.

Bromberg:
Down in Washington?

Bloembergen:
No, here at Harvard, in Sanders theater.

Bromberg:
It must have been a wonderful opportunity to start meeting American physicists.

Bloembergen:
Yes, and I heard Bloch at MIT give a colloquium lecture. But, you know—I was sort of an ignorant fellow listening in. Purcell gave two ten-minute papers there. He didn't give an invited paper. **laughter**

Kelley:
American Physical Society was probably what, a thousand members?

Bloembergen:
Yes, or less. Bloch clearly already had a much bigger reputation, deservedly so—

Kelley:
—he did the Bloch function—

Bloembergen:
—and the neutron experiment—Bloch, Alvarez. You know, neutron beam, magnetic resonance.

Bromberg:
Well, now, in the summer of '47 Gorter was here in your group, is that right?

Bloembergen:
Yes. Gorter came as a summer lecturer. He wasn't in our group, he was a lecturer in the department. He was an old colleague and friend of Van Vleck, and Van Vleck of course corresponded—and Gorter was very anxious to get out of Holland too, for a while, and take up contacts again. Friends wrote him that I was here. Gorter had of course first suggested magnetic resonance spins. In fact, he had suggested it to Rabi, and there is a footnote in Rabi's famous '38 paper to that effect. And of course what Gorter should have done is stayed at Columbia, in those days, and shared in the glory. He just missed out. Gorter had tried to look at nuclear spin resonance during the war, in '42, with negative results. So Gorter was very keen on learning more about the experiments, and he met me, and he read the first chapters of my thesis, and said, "Why don't you come to Leiden? We'll give you a postdoc." Then I remember that we went to Boston to the Radio Shack and we bought Chinese copies of the equipment I had already here at Harvard, and that's what I set up in Leiden in the fall of '47. I was the first one to get the magnetic resonance on the European continent. In England, in Oxford, they just had it a few months earlier. But the nice thing was in Leiden I had all these cryogenic facilities. There, they had a lot of technical help. The organization of American universities still to this day is so different from European.

Bromberg:
What was the comparison you found when you got there?

Bloembergen:
Well, in Leiden, they have infinite assistance in the shops, and that was the foresight of Kamerlingh-Onnes, who founded the Technicians School as part of this lab, and so, they could make anything in house. They had superb trained mechanics people. There were dozens of them. And then, they had specialty in cryogenics techniques. You know, that was all done in house, and these technicians even run the helium liquefaction. No professor could touch it, let alone a student. They provided us with liquid helium.

Kelley:
Well, there's history here in Holland, isn't there, of resonance phenomena?

Bloembergen:
There's a long history in magnetic relaxation, Gorter, and de Haas, and others. But what Gorter did and what he's famous for is paramagnetic relaxation in the non-resonance sense. This is the only thing that was there, and he knew about the relaxation times quite well. But with the resonance techniques this all became very important, and so he lectured here at Harvard on his little book on paramagnetic relaxation, of which we read every page from A to Z, and it was very helpful, to see these non-resonant precursors.

Kelley:
Was that de Haas, van Alphen?

Bloembergen:
Well, de Haas was still alive, you know of de Haas-van Alphen, and Einsten-de Haas effect. You know, he was of the old school. He said, "Bloembergen, I don't trust that electronics. That electricity slips between your fingers." [laughter] He didn't like the modern techniques at all. He wanted to see a galvanometer, you know, or a mechanical balance, which he had used.

Bromberg:
Now, how was this laboratory set up. Was this just a laboratory that you were in, but then you'd talk with people, have lunch with them? Just to get some idea of the

physical setting in Leiden compared to the United States, compared to Harvard. Who was there besides de Haas and van Alphen?

Bloembergen:
There was Gorter. Well, there were lots of people. There was Taconis in low temperature techniques. There were—

Bromberg:
—I mean people you were principally interacting with.

Bloembergen:
Well, there was always a very active group, and Gorter had just come from Amsterdam to Leiden and he really had a big operation there. Of course [I] was the only one in magnetic resonance, but there was lots in paramagnetic relaxation. Lots of people to talk to. And there was Kramers in theory. The Theoretical Institute was sort of somewhat separate; but I mean, I felt I was sort of a bridge, you know. I could talk to Kramers, and there was the famous Leiden Wednesday evening colloquium, with foreign visitors. Well, here at Harvard, just to come back to two years earlier. I mean, it became very stimulating, you know, scientific meetings. I gave my first APS paper in January '47, at the New York meeting; and everybody was there—Rabi, and Townes—and Bill Nierenberg gave a ten minute paper, I gave a ten minute paper, and then after that Rabi said, "Let's all get out of here and discuss things privately." The rest of the papers, I missed...*laughter, crosstalk* So I worked very well in Leiden for a year and a half.

Bromberg:
Why did you return to Harvard?

Bloembergen:
I was offered a position in the Society of Fellows, a junior fellowship. So Van Vleck arranged that. So that was tough competition. Well, I also felt, if I really wanted to learn more, it was such an exciting time, you know, in the United States, between '46 and '49. It wasn't just magnetic resonance and microwave spectroscopy. There was also cosmic rays and mesons and high energy nuclear physics and so on.

Kelley:
Quantum electrodynamics.

Bloembergen:
Everything was breaking. There was a stored up intellectual content from the war.

Kelley:
Q E D, right.

Bloembergen:
Yes, Lamb shift, everything was breaking. So I found Harvard extremely stimulating, and though Leiden wasn't bad, you know, I was the moving force in magnetic resonance there, that's what it amounted to, and I was too young to just stay there and do that.

Bromberg:
When you came back, what direction did you want to take your research program in at that point?

Bloembergen:
Well, I decided that I should use this period to broaden myself, this period of three years at the Society of Fellows. You know what the Society of Fellows is. So that's what I did. I said, "First I have to learn something about microwaves." You know, I couldn't stand it, everybody knew microwaves and wave guides and so on. I only knew about LC circuits, low frequency stuff. So I learned microwave techniques.

Bromberg:
How did you do this principally, studying by yourself, going to lectures?

Bloembergen:
No, I was in the lab all the time. I was in the remnants of the Harvard Radiation Research Lab, RRL, which was in the Vanserg Building.

Kelley:
Did you have some experiments in mind?

Bloembergen:
Well, I said, I'll do ferromagnetic resonance. That had been done on and off, but the people had done it at room temperature, low temperature. I'm going to look at what's happening at the Curie point, transition from ferromagnetic to paramagnetic. So I learned about how to make a microwave cavity, how to design it, put it in an oven. One wall was made of iron, or nickel, and I heated it above the Curie temperature, and followed the magnetic resonance through the Curie temperature. I wrote a paper on that. So I learned microwave techniques and got a paper out that was interesting; nothing great, but I wrote a paper. Then I decided, now I want to learn something else. Everybody talks about nuclear physics. I'm going to work at the Harvard cyclotron. I got a paper out of that, which didn't make a big impact, although it was a novel method to measure the range of protons.

Kelley:
This was in 1949?

Bloembergen:
This was in the period 1949 to the summer of '51.

Kelley:
Before the maser.

Bloembergen:
Before the maser. And then I realized that I was making a mistake, that high energy physics was not for me—too big a machine—and I got an appointment here in the Division of Engineering and Applied Physics, as it was then called, the present Division of Applied Science.

Bromberg:

By the way, before you talk about that appointment, when you say it was too big a machine, I'm curious to have you tell us a little bit more.

Bloembergen:

Well, the operation of the cyclotron. It was a small cyclotron. The Harvard cyclotron was designed by Bainbridge, and it worked well, but its energy was just too low to do significant meson work, and—

Bromberg:

Was it mostly you just didn't like working with such a large group of people?

Bloembergen:

Yes. Then you have to schedule your experiments very carefully, and—well, I thought that there was more interesting physics to do. You could do theory on the side with magnetic resonance. So I thought that, you know, I had made a mistake. Look, here are all these other people staying with the field of magnetic resonance, lots of interesting things happening—here, I took two years out to do something else.

Kelley:

A little bit more of individuality too.

Bloembergen:

Yes, that's right.

Bromberg:

I guess what I'm really after is to get a sense of how you liked to work, so I was asking you why you didn't like to work on the big machine, to get a better feeling for—

Bloembergen:

Well, I enjoyed it. I mean, I worked a year across the street at the old Harvard cyclotron, which is still being used for medical irradiation purposes. But it wasn't as exciting intellectually as all the new concepts that came from microwave and radio spectroscopy, and so I felt I could have a bigger impact there. So I said, "No more. No more nuclear energy for me."

Bromberg:

So you decided to take the engineering and applied—

Bloembergen:

Yes. You see, I had—

Bromberg:

You had just gotten married.

Bloembergen:

I'd gotten married, too, in '50. But this was spring, '51, my Society Fellowship ran out. And all that Harvard came up with was assistant professor here in the Division of Engineering and Applied Physics. There wasn't a dean yet. Van Vleck became dean just about that time. He was the first dean.

Bromberg:

Dean of this division?

Bloembergen:

Yes. He had been chairman of the physics department. And he attracted Julian Schwinger, Purcell and Ramsey. Those were the three tenured appointments he made in physics during his chairmanship of the physics department. And when van Vleck was dean here, he really made all the key appointments, like George Carrier in fluid dynamics; I in solid state, things like that. He was really very perceptive, spent a lot of time—very, very careful in his appointments.

Bromberg:

By the way, what I'm picking up, and I want you to correct this if wrong, is—what really attracted you the most about that appointment was staying at Harvard, is that correct?

Bloembergen:

No. No. Listen to the story. I was politely told that, well, they had appointed Pound in physics already and then I would be considered sort of redundant, so there was really no space for me in physics; but here was this effort in solid state physics. According to the Vannevar Bush report, the-Gordon McKay endowment could be used for mechanical engineering, but where does engineering start, mechanical engineering, in 1950? They were very perceptive. They said, "Solid state physics is going to be the basis of mechanical engineering." These were just the times that people talked about dislocations, you had the magnetic materials, and so on. And you know, Harvard didn't want to spend that money on the type of engineering that big institutes of technology can do. It wouldn't be sufficient for that. So they had appointed a committee. When I say "they," Professor Conant of Harvard appointed the Vannevar Bush Committee, and they came up with this report that solid state physics was an important field, and the physics department needed all its money in appointments in high energy and nuclear physics, so solid state physics could be done here. I was then one of the first solid state experimentalists. But I had an offer from Chicago as assistant professor, and I just told Harvard, "Well, if that's the best you can do, I'll go to Chicago—(a) because it's an appointment that pays more, (b) because it's in the physics department, and (c) I have already been here for four years. I might as well get exposed somewhere else in the rank of assistant professor." So then they considereed this, and they decided to offer me tenure as associate professor. I said, "If that goes through, of course I'll stay at Harvard. So that's how I came into the Division of Applied Sciences, as you now call it, and that meant I had to do solid state, really, which I didn't mind doing. After all—at liquid helium temperatures, most everything is solid, magnetic things. And even solid state, broadly defined, was really condensed matter. So I could do magnetic resonance. In fact that was one of the key things to get started. And I had to learn a lot of solid state, and give the first course in introduction to solid state, which was before Kittel's book existed, and I sort of had to compose Kittel's book on my own. I didn't have the breadth of view he had, but pretty much the topics I chose and taught were very much along the lines of his INTRODUCTION TO SOLID STATE PHYSICS, as it came out four years later.

Kelley:

You started on crystal fields and rare earths?

Bloembergen:

Well, I know I did a little bit of that. That came really later. No, I really had to teach the introduction, so, mechanical properties, elasticity, tensors, electrical conductivity, diffusion, all those things. And then at the end a little bit of magnetism, but there wasn't room for that much.

Kelley:

You probably don't remember, but I think it was in '58 that I took the first semester from-George Benedek, and there was a course in solid state physics, concentrated a lot on the elastic properties, shock waves and so on, and then I think you taught the spring semester. It was kind of continuation.

Bloembergen:
Oh, it was a continuation. That was the more advanced solid state, and I taught it partly with van Vleck on magnetism.

Kelley:
Yes.

Bloembergen:
That was an advanced course on magnetism. That came later.

Bromberg:
Did this translate into your research work? I mean, did you take up new research directions?

Bloembergen:
I had to look at some significant problems in solid state also involving magnetic resonance because that's what I knew how to do, so I started building a Pound spectrometer, and looking for magnetic resonance in alloys. Everybody was talking about properties of alloys, and I said, "What can you do with magnetic resonance not in pure metal but in alloys? What happens to the Knight shift?" We found a variation of the Knight shift, and then we found the quadrupole interactions of the random arrangements. You get lots of internal gradients. And I had one student, Ted Rowland, who came from the geological lab, I think.

Kelley:
So there was kind of, maybe more an emphasis on the physics of the materials, more than the physics of the interaction of radiation.

Bloembergen:
That's right. We concentrated on the physics of materials.

Bromberg:
Were you involved with any applications to commercial devices or to anything—

Bloembergen:
—I had no time in those days to worry about that. You mean magnetometers and so on? Purcell didn't want to have anything to do with patents, so Bloch got the patent all to himself.

Kelley:
But there was no thinking at the time of sources other than electron devices?

Bloembergen:
Well, the whole atmosphere here was not device-oriented. Just to do physics, what can you do with it?

Kelley:
—of the solids or materials?

Bloembergen:
Yes. Of course, Purcell then went into radio astronomy. He did the stellar hydrogen. Pound worked on quadrupole interactions and nuclear angular correlations. And I first worked on nuclear resonsance in alloys, and I had one student, Rowland—got a few more—and that turned out to be very successful, because in '54, three years after my beginning of tenure, I was invited by Mott to a conference in Bristol, and also one about the same time, a little later, I think, in Ottawa. And Mott at that time considered it, with remarkable foresight, to be just as important for condensed matter as the X-rays. I gave the papers of what magnetic resonance can do in alloys.

Kelley:
—there was so much to do, so much to uncover—

Bloembergen:
That's right, there was never a dull moment. You know, nothing had been done in solids, so that George Benedek, who had gotten his Ph.D. with Purcell, became postdoc with me, and we decided to do magnetic resonance under high pressure. Everybody had varied the temperature by then but nobody had varied the pressure.

Kelley:
That's how George got to go into shock wave—

Bloembergen:
[crosstalk] Exactly. He was a postdoc—[I and] he did nuclear resonance under high pressure, combined it with Bridgeman techniques with Purcell. And then I did this high pressure work. We did microwave resonance under pressure, systematically disentangled the temperature and pressure effects from quadrupole interactions. All kinds of salts and interactions. Brodmberg: Did you take on one student at a time, Rowland and Benedek, or have a whole bunch or what?

Bloembergen:
No, at that time, Rowland was the first one.

Kelley:
And Benedek was a postdoc.

Bloembergen:
Benedek was a postdoc. Not everybody worked on magnetic resonance. One worked on the piezogalvano magnetic effects. I taught a course in solid state physics, and he said, "What can I do with those effects?" He did a thesis completely unrelated to the others. Then we did ferromagnetic resonance, because I'd had two students who were officially Chaffee's students; but Chaffee was already very old and unable to give them even the thesis topics, so I suggested (they were both trained in microwaves, at high power), "Why don't we investigate ferromagnetic resonance at high power?" We found the first evidences for nonlinear effects in ferromagnetic resonance, which we didn't clarify completely, but at least we found significant nonlinear effects experimentally.

Bromberg:
When was that, where is that paper?

Bloembergen:
Well, [R.W.] Damon wrote a paper on it, and I still regret I didn't put my name as a co-author. That was in '53. But there is a paper by Wang, Wang was a post doc with me also, Shih Wang, No. 18. Oh, there is a joint paper with Damon too. In '52. But then he published a paper all on his own in '53, Papers 17 and 18.

Kelley:
That's the Shih Wang at Berkeley?

Bloembergen:
Yes. Those were very very important nonlinear effects, for applications. Then Lester Hogan came here from Bell Labs.

Bromberg:
As a postdoc?

Bloembergen:
As a professor, associate professor. He had done the—what do you call that device? Ferromagnetic isolator, or the Faraday rotator, either one. Well, it's a Faraday rotator with two polarizers 45 degrees different, turned it '45 degrees, and then, back again. He had done that application of ferrites I at Bell Labs. You know, I was very happily into magnetic materials. So he came on board, he did—

Bromberg:
You were working fairly closely with him here?

Bloembergen:
No. But we saw each other and talked quite a bit, but didn't work together, I mean we didn't do a joint experiment. But he was very important in the history, in the sense that when I did the three level maser concept, which involves of course a lot of magnetic relaxation, things like that, he leaked it to Bell Labs. He immediately saw the importance of it. "You're going to have to write it up in a notebook." He knew that I never had big bound notebooks. "This you have to write it up and I'll witness it."

Bromberg:
He was consulting still with Bell Labs?

Bloembergen:
Oh yes, you know, he probably was consultant. He knew Kompfner, and he just leaked the word. I mean, he didn't leak the secret. He said, "You really have to send somebody to see what Bloembergen has." So Kompfner came over one day.

Bromberg:
You knew Kompfner before?

Bloembergen:
No.

Bromberg:
I see. You had not yourself been connected with Bell Labs before?

Bloembergen:
I had given a lecture there. I'd been invited there. They offered me a job. You know, Jim Fisk was director of Bell Labs, but in '49 he was a professor at Harvard. You know, he was in the Junior Society of Fellows before the war, and Harvard tried to attract him back, I think in '47 or so. His wife, who was an old New Englander, she would like to live in New England. That's the story as I remember it. Of course I have it at third hand, I wasn't even supposed to know those things. So he decided to be a Harvard professor, although he really was in line to become president of Bell Labs. So he was sort of apologetic about the course he taught, because, you know, he had sort of been out of basic physics already—in the more important organization of war and research work which was his main interest.

Bromberg:
So you knew him already from Harvard.

Bloembergen:
Yes, and he knew me. This was in '47—in '48, he decided to go back to Bell Labs. But he was also a member of the Atomic Energy Commission.

Bromberg:
Right. I think he was director of research (at Bell Labs) at one point, wasn't he?

Bloembergen:
Yes, I think so. And he was asked to clear my visa with the State Department in Washington, because when I became a junior fellow, I had great trouble getting an entry visa, because of the beginning of the McCarthy period, really. What happened, I remember that very clearly, is that I was just waiting in Holland. I didn't know what was happening on this side. But finally the U.S. consul in Rotterdam said that I could come in, and he had a telegram from the State Department, which I had to pay for, with lots of questions he had to ask. He was so embarrassed about it.

Bromberg:
It was a very long one?

Bloembergen:
"What kind of physicist are you?" I said, "Well, I'm a spectroscopist. I do magnetic resonance, radio frequencies." "Oh, but didn't you do something in nuclear physics?" "Oh no," I said. "Oh, but your thesis—what's the title of your thesis, 'Nuclear Magnetic Relaxation,' isn't that nuclear physics?" This is the way it went, you see. They were very sensitive about it. Then he had to ask me what I had voted during the elections in Holland. Official instructions. I said, "I haven't voted, because before the war I was too young, during the war there was the German occupation, after the war I was in the United States. So I haven't voted." He said, "What would you have voted if you had voted?" **laughter** It was all instructions from the State Department, and which I had to pay for. Well, I was mature enough to know how to handle that. Probably sort of middle of the road. I wasn't about to say, "You cannot ask these questions." All my purpose was to get back into the United States. But then I later heard that Jim Fisk

made a special trip to Washington. Awful situation.

Bromberg:
So now, we left you with Rudi Kompfner here. Talking to you about the three level maser. There was a lot of things in between that we should get back to, but I just wondered.

Kelley:
What impact did the maser have on you at the time?

Bloembergen:
The Gordon-Zeiger thing. Well, I heard Gordon's talk, which really excited me, in New York City. It was a very intelligent talk, very clear. I got very excited.

Kelley:
You could see the advantages of not having an electron device?

Bloembergen:
Yes, right. Well, I mean, there were atomic clocks...and people worked with ammonia, working on the absorption as well as the emission, so that I knew there was this work on microwave frequencies. But since I was in condensed matter, that high frequency precision thing wasn't in my bailiwick and I had enough problems of my own. But I also of course had known the Purcell, Pound and Ramsey work on magnetic spin techniques, nuclear spin. I was well aware of that. But they never thought of an application for that. You know, you didn't need an amplifier at the BF frequencies.

Kelley:
I've often wondered, did the concept of negative temperature sort of hold people's thinking back conceptually?

Bloembergen:
Well, many people were reluctant to accept it. But I mean, I was well aware of the validity of the concept, you know, because all of Garter's work too was based on a concept of temperature and magnetism, and it was Casimir who said first the magnetic system has a different temperature from the lattice system.

Kelley:
I was thinking of equilibrium and non-equilibrium.

Bloembergen:
And then we did a lot of work also with the ferromagnetic resonance of different temperatures, with different parts of the magnetic system.

Bromberg:
Extending the temperature concept was part of your work, wasn't it?

Bloembergen:
It goes all through it, in ferromagnetic resonance and nuclear spin saturation.

Kelley:
But I'm trying to get at the non-equilibrium aspects of the negative temperatures.

Bloembergen:
Well, so it was quite clear you could get negative temperatures and then after a while you would lose it again. You know, it was sort of transient. And that's inherent in any two level system, and I knew very well how to invert the magnetization, either by 180 degree folds or adiabatic rapid passage, or by the Purcell method of suddenly reversing the magnetic field, which you can only do for nuclear spin problems, you never can—otherwise—work fast enough.

Kelley:
So it was a question of the time scales and the quasi-static kind of—

Bloembergen:
Yes. Then, you know, there was this colloquium at MIT of Strandberg.

Bromberg:
Now, between the Gordon talk and Strandberg's colloquium, you weren't working on any of this?

Bloembergen:
No, I wasn't working on masers at all. This is what I'm telling you. I said, it's all right for these people, they have precise frequency oscillators and time standards. But that's not my bailiwick. Let them work on it. So—then I asked Woody, "Now, why would you work on getting inversion in condensed matter, because it's no good for a frequency standard? "Why try to invert this magnetization? It can be done. Nuclear spins, we already have. Why do you want to do it at microwave? There's no new science." He said, "Well, but I want to build a low noise receiver." I said, "Aha! Now that is an important new application." And then, clearly solid materials, especially ones of low temperature, would have some advantages.

Kelley:
—the three level came in later.

Bloembergen:
The three level is about to come, and I'm describing now what happened. I'd heard Jim Gordon's talk, and I knew what the molecular beam people were doing, the frequency standards, and I then heard Strandberg's colloquium at MIT. There were lots of noises of people trying to invert magnetization in paramagnetic salts, to which I said, "So what? It's already been done for nuclear magnetic systems, and the physics is pretty clear. It's just harder, because your times are shorter, your magnetic fields have to be higher to do it in paramagnetic crystals. So why this effort?" So I just asked Woody that privately after the colloquium, "Now, why would you go to so much trouble repeating this experiment?"

Kelley:
What about the negative temperature concept?

Bloembergen:
The negative temperature concept had lots of appeal to solid state people. It had no appeal to people in atomic and molecular physics. I thought it always was an important concept, but Gianque and the low temperature people never liked it. Of course, negative temperature is just the opposite of low. You have to go to infinity, it's hotter than hot. I appreciated all of that. So when Woody Strandberg told me, "Well, it may be useful as a low noise microwave receiver, amplifier." Then I said, "Aha, that is worth thinking about." The problem then is clearly that you want a device that works OW rather than pulsed. Just flip it over and you get—that's no good. Then, within weeks I came up with the three level pumping scheme.

Kelley:
You had done double resonance before?

Bloembergen:
I had done double resonance before, cross saturation types of things had been done. It was just a matter of making a saturation between two energy levels which were non-adjacent and having one in between.

Bromberg:
There was a very brief paper by Basov and Prokhorov coming out just at about the time yours was being sent in.

Bloembergen:
That's right. I wasn't aware of it. It was found by the patent people at Bell Labs, who researched it, and they called it to my attention. The point is the following: that they only talked about molecular beams, and for molecular beams you don't need it, because there are physical ways to separate the states. Nobody had ever used it as a three level pump in that situation. Also, they don't mention any relaxation method. Their idea was simply, get the atoms up in a high state, and then spontaneously it can radiate to another lower state if not the ground state, which is a good idea. But in molecular beams, it was never used. You clearly need a relaxation method to get continuous wave inversion. You thermodynamically have to have a hot mechanism and a cold one, so in addition to the pump, you have to have a relaxation. You have no relaxation in a molecular beam, except the atoms leave the active region. So I always still feel to this day that it was a very basic element missing, and that Basov and Prokhorov didn't have a three level pump, in the sense that it is used in all lasers and in the microwave masers. And I think Prokhorov was fair to admit it. He said, "Bloembergen had the right idea, to do it in solids." —And they were very quick to pick up Woody, Prokhorov and Basov were, I mean, they lost no time.

Bromberg:
One of the questions I put on my list was, one of the applications you stressed in that paper was measuring the 21 centimeter hydrogen line, and I wondered whether that was something you'd been thinking about?

Bloembergen:
Yes, well, the ones who had been thinking about that were of course the people here at Harvard who discovered the 21 centimeter line, and all the astronomers were ape about interstellar hydrogen and 21 centimeters, and clearly there is where you wanted the most sensitivity. So it was a very good goal to aim for in academia, and of course the Harvard Radiotelescope then eventually had a 21 centimeter maser receiver. But in retrospect it was unfortunate, that choice, because it turned out that the maser at 21 centimeters was much harder to build than a maser at X-band. And that's one of the reasons why we didn't build the first maser. The first maser was built at Bell Labs, and very soon after they heard of my scheme, and in fact with one material that I explicity suggested, gadolinium ethyl sulphate.

Bromberg:
You must have gotten in contact with them right away, if you were already in contact with Bell Labs as you got this idea.

Bloembergen:
Well, no, you see, it was always via Hogan and Kompfner. Then they invited me out to give a talk there. Sid Millman, he was the director of physics research. He invited me up. I met Scovil and Feher and so on. But, you know, Bell Labs is so big that they don't need any outside consultant, except on this occasion, which they probably didn't like too much.

Kelley:
You also had some effect on Jim Meyer and Al McWhorter too.

Bloembergen:
Oh yes. You should talk to Jim Meyer. He remembers those early years quite well. I mean, I talked to them and Bob Kingston, I talked to him. Ja, I was a consultant for Lincoln Labs and Ben Lax and so on in solid state. So they immediately of course were very interested.

Bromberg:
How did you get involved in the telscope work?

Bloembergen:
Well, because of Purcell, and I mean, there was a Harvard Radiotelescope.

Kelley:
So you saw an application.

Bloembergen:
I saw an important scientic application for it. If you wanted to build a sensitive microwave receiver, what would you aim for? But clearly Bell Labs would have beaten me anyway in the experimental realization. But we would have done it quicker if we had not chosen to go straightaway for 21 centimeters, which we did, and we were still the first maser at 21 centimeters, but it was a year later, than the one Bell Labs made. Then masers sprang up everywhere, you know.

Bromberg:
There are a couple of questions I'd like to backtrack to. One was, to get some idea of the funding situation when you were first an associate professor here, compared to what you had told us about early on. The other is this other question about whether you were already working with any government committees. That's a kind of preparatory question to asking you about IDA.

Bloembergen:
Well, I had no worries about getting money. I was eased into the Joint Services Electronics Program, which Harvard and M.I.T. started at first. It was the major accomplishment of the Office of Naval Research to keep research going after the war. I think it started either the end of '45 or early '46. And even my work at Society of Fellows on ferromagnetic resonance was done under the aegis of JSEP (Joint Services Electronics Program). There was a director, old Professor Chaffee, and I just had to write a progress report, but I didn't even have to write any proposals. It was originally for vacuum tube electronics. That was no matter, you could do transistors if you wanted to, you could do magnetic resonance. You'd never had to write a proposal.

Bromberg:
That would support your graduate students as well as your own work?

Bloembergen:
Oh yes. They were then all of them on research assistantships under that program.

Kelley:
The enterprise itself was much smaller then, fewer universities, and smaller in size.

Bloembergen:
Smaller in the number of faculty members. So, contrary to the other institutions where they had Joint Services Electronics Program, at Harvard it always was a mainstay, a major part. But then the ONR just volunteered some additional money for magnetic resonance. It was never any problem of really extensive proposal writing.

Kelley:
The national scientific enterprise grew so much after Sputnik. It just seems to have...

Bromberg:
So what we want to talk about now is really pre-Sputnik.

Bloembergen:
This is pre-Sputnik, like the three level maser was, and it was all magnetic resonance, and as I told you, although not quite string and sealing wax physics, it came close to it. You know, you needed very modest equipment.

Bromberg:
Then the other part of the question was, to get a picture of what other things, what applications you may or may not have had in mind, I'm interested whether you had any role in some of these summer study projects, on one or another program. The thing that occurs to me is Project Hartwell but I guess that goes back before you were involved, but that kind of thing, to get a feeling for whether you were consulting or—

Bloembergen:
Well, Zacharias asked me to take part in the project at Lincoln Labs, was it Hartwell? '52. And I had to explain that I had to go back to Holland, because even though Harvard considered me a permanent appointment, the United States considered me a temporary visitor, and I had to go back to Holland to get my visa changed. That was summer of '52, so I couldn't take that. Then after I became a legal immigrant, I was a consultant for Lincoln Labs, but only part time and never in a big study group. Then Schlumberger-Doll asked me. They asked Purcell because they wanted to apply magnetic resonance to well logging and he suggested that they take me, and so, that's what I did. I was supposed to advise them on magnetic resonance down in the earth bore hole measuring T1 and T2, as it is called. That started in '53. I always enjoyed my contacts with industry, because it does give you a different perspective, but I never felt inclined to really make it my goal to build devices.

Bromberg:
Were there things which you did in industry which came to influence the direction you wanted to take your research in?

Bloembergen:
Well, to give you a good example, take this three level maser. You know, we built one, but we never built a useful instrument that you could really put on a radiotelescope—you needed traveling wave, you needed bandwidth—Bell Labs does it so much better. I had some postdocs to really build a device they could use. But my interest was in some of the basic scientific questions, such as what is the concept of temperature in such systems? The point is, they don't work if the concentration of magnetic ions is too high; if the chromium concentration will be too high, the microwave maser doesn't work. We elucidated that question, and that's the famous paper on cross-relaxation. Not in ruby, but he [Pershan] did it in nuclear spin systems where you have much better control and can really pursue it quantitatively, and you can get one temperature here and and another temperature there, and really, by varying the magnetic field, you can change the rate of cross-relaxation and follow it in great detail. It has since become a very big field in magnetic resonance. But it's really an extension of the concept of temperature, just as the three level maser is an extension in the following sense, that it is a thermodynamic machine operating between two temperatures, the hot temperature provided by the pump, the low temperature provided by the relaxation mechanism. And the difference is that for the first time now these two temperatures occur in the same volume element in space. They refer to different pairs of energy levels, but not to different regions of space, as they usually are in thermomechanical engineering.

Bromberg:
I also have a question here about your collaboration with Pershan, or any other of your collaborations which were specially important. I just thought that might have been an important one because there were a lot of joint papers.

Bloembergen:
Yes. Well, I have had over the years between 55 and 60 Ph.D. students, and I guess, though I haven't counted them recently, an equal number of postdocs, at one time or another, over the years. So clearly I couldn't have done all the work without a fair sized group.

Bromberg:
Do you have a certain style that you prefer to work with them in? How do you work with a graduate thesis student? Do you divide up the work and say, "Here, why don't you do this calculation?"

Bloembergen:
Well, I always found that even for the brightest ones, you have to suggest a thesis topic. Because they come, they may know a lot of course physics and so on, but they don't have the perspective, and they don't know the limitations either. They don't know what you can do in the lab with the equipment available, and what is feasible to buy and what is clearly not. But even more important, they don't know what the competition does, and one very severe boundary condition on the choice of topics is, can the student do his work without being scooped, in the four or five years it takes him to get the Ph.D. done? In magnetic resonance, this was all right because industry didn't do too much of it, although it sort of kept you away from ferromagnetic microwave devices. And in the maser field, of course, the thing was that most people just didn't have the background in magnetic temperature concepts and relaxation concepts. So there we could do it. In optics it was much more severe, in the laser field, which we'll come to later. I really had to find topics that were unpopular. Like measuring non-linearities in absorbing materials where you have to get at the non-linearities in reflection. You know, people didn't see that as a practical concept. But that's where all the basic laws of non-linear optics come in, the full extent.

Bromberg:
Fascinating boundary condition. I was completely unaware of that, as a part of choosing theses. So you choose a thesis, and—

Bloembergen:
The feasibility for the student. You know, there are some people who probably, you know, just shoot off the hip and don't mind if a student becomes a slave for ten years. I

think you have to get him problems that have a reasonable chance of success.

Kelley:
Gene Cummins of Berkeley was the classic one, I remember, for giving students impossible thesis topics.

Bloembergen:
I think, once you build up a reputation if you do well, you attract more of them. And clearly, Peter Pershan was one of the brightest students. I mean, I had another very bright one who became more famous later on; that was Peter Sorokin at IBM. He did his thesis with me on magnetic relaxation. And then there was Mickey Walsh [W.M. Walsh, Jr.] who became a group leader at Bell Labs. Peter Pershan stayed on as a postdoc, then as an assistant professor, and we hit it off very nicely. He was a very bright fellow, much more quick at analytical manipulation than I was. But then later, after he did the early work on nonlinear optics, he decided that he didn't want to be a clone of me, and so he went in his own way into liquid crystals and X-rays and this kind of—

Kelley:
Yes, it's kind of bad to stay at the same institution and do the same or similar work.

Bloembergen:
Although for the outside, we would have had a much more powerful group if he had stayed in nonlinear optics.

Kelley:
But for his career, say—

Bloembergen:
Yes. That's why I say that one of the best things that happened to me was that I wasn't in the physics department, to stay under the shadow of Purcell and Pound. I always kept myself busy enough so that for long periods, I had very little interaction with them. And in retrospect, I did the right thing.

Kelley:
Your identity emerged.

Bromberg:
Well, before we get to the switch to nonlinear optics, is there something you would like to cover in this period?

Kelley:
The laser itself, Joan. What happened at the time of the invention of the laser?

Bloembergen:
Yes, now, that's a very good question. For better or for worse, I made a very conscious decision. I realized that this pumping scheme would apply at other wavelengths, but I either lacked the far-sightedness of Townes and Schawlow (although they did it while they were at Bell Labs) to really take it seriously and try to build a device that would work at optical frequencies.

Kelley:
How about Kastler?

Bloembergen:
Well, he did optical pumping.

Kelley:
Did he have an influence on you?

Bloembergen:
Not really, no. I'd rarely read his papers, although his papers are clearly related, in the sense that you saturate at one frequency, or pump at one frequency. But he really did this with polarization, while I did it with frequency discrimination—but it is a double resonance type of experiement. But he didn't have a direct influence on us. Although I was aware of the Brossel and Bitter type experiment at MIT. Bitter really had a considerable share of Kastler's accomplishment. No, that only comes in retrospect. When I went on sabbatical in 1957, I went to the Ecole Normale Superieure, so in that sense, yes.

Bromberg:
When was that?

Bloembergen:
In '57. I went in the fall of '57.

Bromberg:
What did you do then?

Bloembergen:
Well, I just got acquainted with what they were doing, and I was thinking a little bit more about cross-relaxation and masers, while the boys here were trying to make the 21 centimeter maser work, which they did in my absence. I always found it very good to go away!

Kelley:
Leave them alone.

Bromberg:
Were you working with the Kastler group at this point?

Bloembergen:
Oh yes. I gave a series of lectures in '57, fall of '57. I interacted. I got aware of what they were up to.

Bromberg:

So it is the fall of '57 that you got to know them.

Kelley:
You say you were aware of the possibility of working at other frequencies, for the three level system?

Bloembergen:
Yes. But I thought that it would be very hard to build a laser, an optical maser, (because the word *laser* hadn't been invented yet) and that even if somebody succeeded, it probably wouldn't be done at a university. And certainly not by me. So maybe a defeatist attitude. But in retrospect, it's very remarkably true, because most types of lasers were first made to work in industrial research labs, in the United States. And the point is that the U.S. universities are not set up to do that work, and bring together to a common focus very different techniques—optics, and microwaves, and all this together. And I had no experience really in optics, except for my early work in photoelectric detection, and I'm sure I would never have been the first to make it work. Well, even the Bell Labs, Art Schawlow, missed out to Maiman at Hughes. And I know that [Irwin] Wieder, I don't know where he is now, had ideas on fluorescence and ruby. He was another student of Willis Lamb. He came back once and we discussed it. So I was aware of efforts going on, these groups with powerful technical support working on it. "I certainly won't be able to do it." So—

Kelley:
But how about developing the concepts and the ideas?

Bloembergen:
Well, I wish I'd written the Schawlow and Townes paper! **laughter**

Kelley:
Are you saying you could have written it but you just didn't bother?

Bloembergen:
Well, I didn't put all that together, I guess. It really was, I was too busy, with clearing up the cross-relaxation, doing basic questions on the microwave masers, and I had all these graduate students with their theses. I didn't have the courage to just let it all go. And the only thing I did is, because I saw some optics coming, is to write the lone paper that is hardly ever quoted on microwave modulation of light. And then, Maiman got the laser going. Then I said, Aha, we have to get in, but we cannot do work on new laser types or improve them. We just have to do something with them, to use them. And because we had worried about interaction of microwaves and light, here was an interaction of light on light and general nonlinear phenomena, which I knew from ferromagnetic resonance and what not. And so I said, "We have to concentrate on that." Now, I remember, in '60, I asked Peter Pershan to build us a duplicate ruby laser, just copying what Maiman did, and we had a hell of a time, you know. If you have no optical experience, it just isn't that easy. It isn't that easy, in the first year. So, we really got going in '62, when we could use the so called Trion ruby laser. The Trion Co. manufactured it. Then our experimental program could really start, only in '62, which is after the Franken paper on second harmonic came out.

Bromberg:
What about that? Were you in contact with Franken?

Bloembergen:
Not before. I was very excited, and I asked—years later, I said, "Peter, how did you do it?" He said, "Nico," Peter is very modest sometimes, very modest. He said, "If you had been at Ann Arbor with Trion next door, you would have taken one of the ruby lasers."

Kelley:
And shot it at something?

Bloembergen:
That's what he said. And it is remarkable that all these nonlinear effects, like the stimulated Raman effect at Hughes are all done with ruby lasers, and the people who had a working ruby laser, the two photon absorption and everything was done with the ruby laser, and if you had that piece of equipment, then you could quickly discover lots of interesting phenomena.

Bromberg:
You cite about six people in that first paper—you cite Kaiser and Garrett and Braunstein and a couple of others—but I assume that these are just people whose work you read as they came out. They weren't people you were in contact with.

Bloembergen:
No. Well, I knew Garrett because he had been at Harvard.

Kelley:
But really nonlinear optics has a very long history. It's just, people forget or don't define things as nonlinear problems, sort of the stuff that the Russians did and lots of other people did, in saturable absorbers and so forth. It's really—

Bloembergen:
Yes. When did they do that?

Kelley:
In the early part of the century.

Bloembergen:
Bleaching of dyes. Of course dyes always bleach.

Kelley:
—and the laser itself is a nonlinear optical device, because it stabilizes, it's an oscillator, it stabilizes itself.

Bloembergen:
I was well aware of that, that nonlinearity stabilized oscillations. I learned that from the maser.

Kelley:
But there was a formalism that needed to be developed, and a field that needed to be developed, and that's what basically Nico, I think, was involved in.

Bloembergen:
So I had to make them a new (nonlinear) theory, and lasers that were realized (by others). I merely wanted to use them.

Bromberg:
And you were able to set aside the other work you were doing?

Bloembergen:
Then, you know, we had to slow down on our microwaves, on our magnetic resonance stuff.

Bromberg:
There's a real shift of emphasis then.

Bloembergen:
Oh yes, but for a while, you know, we did still both, because we had the electric-field induced effects in magnetic resonance, as well. So at that time I had a group of 12 students and about half were on NMR and the others were on nonlinear optics. And as I say, I had to find thesis topics for them. So we decided to do some theory, building on this microwave modulation of light, and our nonlinearities of magnetic resonance.

Kelley:
Conceptually it took sort of a change of thinking. Everybody thinks of optics as linear processes. Some people were kind of vaguely aware there were these things about saturation occurring. It feels like a discontinuity occurred in thinking, and it took off in a slope. It's amazing.

Bloembergen:
And where you see the nonlinearity in optics most clearly is in Franken's experiment. Even the experiments of Kaiser and Garrett, Braunstein, in the photo absorption, it wasn't so clear, you know. We now know how to describe it as non-linearities. In fact, I'm still very proud. I think the first case where the nonlinear susceptibilities were considered as complex quantities was done by me at the end of '62. It's published in the IRE paper of '63, that the imaginary part describes two-photon absorption and the stimulated Raman effect. In fact, the stimulated Raman effect came out while I was thinking about the imaginary part for two-photon absorption. I remember hearing about it from the Hughes people over the phone. Hellwarth asked me what I thought, and [I said] I think I can explain that with a formalism. And then at the Paris conference in '63, I presented this paper, very explicitly on the complex nonlinear susceptibilities. But the '62 papers were just on the real part of it, and they of course had parametric mixing and second harmonic generation, third harmonic generation.

Kelley:
So that [was] the ABD and P.

Bloembergen:
Yes, the ABD and P.

Kelley:
You may have seen in the literature references by ABDP and BP. That's the classical theoretical paper in nonlinear optics.

Bromberg:
You were sort of conceiving these as a pair, this one and the one on boundary conditions?

Bloembergen:
Yes, they developed at the same time, in late '61. There are precursors out as technical reports. So, though they appeared I think in April '62, maybe June, they actually were widely distributed in February.

Bromberg:
It would be nice, by the way, if that's in your files. That should be saved, those technical reports.

Bloembergen:
They are in the library here. I don't think I've got them. There's a complete collection of Harvard Technical Reports. And that was just the time that things weren't always published as separate technical reports, but rather as reprints, but in those days, we still had pretty complete technical reports. Certainly for the important papers, they first came out as technical reports, then served as a manuscript to be sent to a journal. These TR were distributed, and we can find the dates in the library, these technical reports, but I think it was February and March, and they were very widely distributed at that time.

Bromberg:
It's good to know. I didn't realize about that mode of distribution. You said last year that you would tell us a little bit about the IDA, that laser committee, and that comes in about here chronologically.

Bloembergen:
Yes. Well, in '61 I was asked to join Perkin-Elmer as a consultant, and I was also asked by Keith Brueckner who was the director of IDA at that time.

Bromberg:
Was this Perkin-Elmer, because they were to go into lasers, they wanted you?

Bloembergen:
Yes, because they were quite ambitious and they started building a research lab as a consequence, which was ultimately built a few years later, with John Atwood, and they hired Zernicke Jr. and some other competent people. They really wanted, like any optical company, the progressive ones, to get into the laser field. And they were quite serious about it for a few years. They also had Javan as a consultant.

Bromberg:
What did you do for them?

Bloembergen:
Well, I just visited them, and talked research with them. I didn't do really too much in a concrete sense. But they wanted me to lecture to them and get them started. They were much better in laser construction than I was. But they were interested in the nonlinearities.

Kelley:
Yes. I guess what Joan is trying to get at is, when did the military people start to leap on these ideas that they could use lasers for weapons?

Bloembergen:
Well, almost immediately. And that happened in IDA committee, in 1960 or '61, very early.

Bromberg:
Culver told me the end of '61, does that sound right to you?

Bloembergen:
That sounds right. We had the first meeting in Washington. Keith Brueckner thinks not too much about all my other work, but what he remembers of me, is that I suggested that the military required a million joules of light as a ballistic missile system. But then I make a simple calculation, a million joules of light means just one cubic meter of neodymium glass. And then everybody got fired up, that's feasible! [laughter] You know, it's a simple extension, you melt a glass, and you pump it and you shoot it.

Kelley:
Just a small matter of scale.

Bloembergen:
A small matter of scale, but you see it was much more realistic, in that time, than the gas flow regime. There was a fellow named Welton who had something about a plasma discharge. No plasma laser had yet been built, you see, so it was very hard to see how that would work.

Bromberg:
So it sounds as if the first thing you were asked was, can you feasibly make weapons.

Bloembergen:
—Well, that's what the newspapers—and clearly some military types wanted, and they obviously still want it again. They want it again. But that was very heady stuff, also scientifically, because then very soon came all these propagation characteristics, through the atmosphere, stimulated Brillouin, stimulated Raman in the atmosphere—
Bloembergen - 30

Kelley:
—breakdown—

Bloembergen:
Breakdown became very important for me personally. Nobody understood the breakdown mechanism.

Kelley:
Thermal blooming.

Bloembergen:
Thermal blooming. You name it. That was all discussed in these IDA meetings. And there was of course Charles Townes. Then there was Bob Terhune and some of the early ones. Some people at the ONR and Army Research Office and IDA people.

Bromberg:
So the IDA thing stimulated your research on breakdown, is that it?

Bloembergen:
Oh yes.

Bromberg:
Were there other problems?

Bloembergen:
We also had Kidder and some of the Los Alamos people.

Bromberg:
He was on the committee?

Bloembergen:
Of course Brueckner was interested in laser fusion. That was beyond me. I didn't even know about it in those days. So really, you know, it became very clear that properties of matter at high light intensities were going to be very important, and that's the field of nonlinear optics.

Bromberg:
How long did that go on, those committee meetings I?

Bloembergen:
Well, there were regular meetings, '61 through '65, and then there was another one I think in '67, when the gas dynamic lasers came on.

Kelley:
Wood's Hole?

Bloembergen:
Wood's Hole, and gas dynamics was the big thing there. Then the thermal blooming problems. Then IDA sort of petered out.

Kelley:
But then there was a lot of funding, of course. They were starting to go into big labs and big projects, and various research organizations like Naval Research Lab and the Air Force Weapons Laboratory and Lincoln Laboratory. A whole lot of money to do experiments on testing some of these ideas.

Bloembergen:
But Charles Townes can give you probably even a better account because he had already more perspective on these items. Brueckner too.

Bromberg:
Do you have that kind of documentation in your files?

Bloembergen:
No.

Bromberg:
That's all classified somewhere.

Bloembergen:
I certainly don't have any classified things in my files. don't have the storage facilities.

Kelley:
There are the Jason reports, but they're all classified.

Bloembergen:
You have to go to IDA to find that.

Bromberg:
Do you think it would be worth our going to IDA?

Bloembergen:
Oh yes. They have reports of those early meetings.

Kelley:
Yes, I don't think there's anybody left at IDA any more who was involved at all. Alex Glass—

Bloembergen:
There must be an archive.

Kelley:
Alex Glass came in and Bill Culver was there.

Bromberg:
So I asked you here, did your funding support change as a result of your going into nonlinear optics? What was going on?

Bloembergen:
Here the point is that I could start optics just on Joint Services Electronics Program, without ever writing a proposal for optics. They accepted it as a matter of course and no questions asked, in 1960, '61 what you did, and although I'd been doing masers and magnetic resonance, they thought it was fine that I was going into optics. Without any proposal.

Kelley:
So you retained your independence.

Bloembergen:
Yes. But I needed more money, and then this was just the time that ARPA came along with the MRL stuff. ARPA had programs for Materials Research Labs, and Harvard was one of the beneficiaries of that big umbrella contract, and clearly, materials research with high light intensities was an important part of that, so I got a chunk of that money.

Bromberg:
By the way, you said you also consulted for NASA. How did NASA get into it?

Bloembergen:
In March—well, I was asked to serve on the elctrophysics advisory committee panel in '64, '65. That didn't amount to much. That was just a little committee, and they never listened to our advice. We advised them what to do with their cyclotron in Cleveland and so on, and then NASA was pretty poor, except of course for JPL. They didn't need advice at all.

Kelley:
So this government service was sort of a side issue.

Bloembergen:
Was a side issue, but IDA was interesting.

Kelley:
It was seminal, the IDA was.

Bloembergen:
Seminal.

Bromberg:
Was it a place also where you would exchange research ideas?

Bloembergen:
Oh yes. Oh yes. Interpretation of what was going on.

Bromberg:
I'd also like to track the invisible colleges. You know that term which historians of science use about the 17th century. They want to know who your most intimate colleagues are at any point in your research career and how they change, just to see who's speaking to whom, what the informal communities are. So, as you get into nonlinear optics, if there are people who become important intellectual contacts?

Bloembergen:
Clearly I had never met Peter Franken before. There was the Physical Society, annual meetings. In '61, '62, they were very important, the ten minute papers there. We had our ten minute paper on our ABDP in Washington.

Kelley:
It was tremendously competitive at the time. That was the time I was getting into it, into nonlinear optics, and there was tremendous competition.

Bloembergen:
So everybody can say, "I had ideas," and they probably did, but the question is, who really followed it up, and brought it to fruition.

Kelley:
Lots of confusion, lots of lack of understanding, lots of questions that needed to be answered.

Bloembergen:
Absolutely, and that's why I think that people who later come and say, "Look, I suggested that, and even had a note of the conversation"—but you know, at that time it really confused the issue rather than clarified it. Then when it got clarified years later, then he said, "I told you so."

Bromberg:
What I'd like to ask, though, is, the International Quantum Electronics Conferences, were you involved at all in the organizing of that sequence of conferences?

Bloembergen:
In the third one, I was. I'm not an organizing type. But the first one was organized by Charles Townes, the Schawanga Lodge one, and that was almost pre-history!

Bromberg:
That was '59.

Bloembergen:
'59. And then there was the one in '61, and that was Singer, who sort of didn't make big contributions to the field, although I think he wrote a nice little book on microwave masers. It isn't that bad. He was the editor. He, you know, wanted to take active part, and the best way he could do it is by organizing a conference. It was a very successful conference. Hellwarth spoke there,—on the Q-switched laser for the first time. Really high powers already. It was quite amazing. And then in '63, the conference was held in Paris because Grivet wanted it. But it took him some time to realize that the important topic wouldn't be much microwaves, it would be optics. He didn't see that, I thought. So he did the local work and publicity and I was arranging the program and editing the Proceedings.

Bromberg:
Mostly selecting—?

Bloembergen:
Well, at that time I made it a policy, we were going to accept all the papers. We could just barely do it without any refereeing, have an open meeting with so many ideas floating around. It would be very easy for me to say, "Look, that paper is less of quality than this," but, you know, the United States was so preponderant at that time, certainly we have to keep all the papers, no matter almost how poor, some contributions were from Bulgaria, Germany and so on. So I counted all the things that had been submitted and said, "We can just do it in two sessions, two parallel sessions." That would be acceptable.

Bromberg:
How'd you get money for that?

Bloembergen:
To publish?

Bromberg:
No, to run the conference.

Bloembergen:
We never worried about it. Wasn't there already the Joint Council on Quantum Electronics at that time?

Kelley:
I don't know. I wasn't involved.

Bloembergen:
It probably says at the beginning of the volume. At least, I never did any work to raise money. It just happened to be there. I think the ONE started the first one, and they continued in support.

Kelley:
Yes, they probably supported it in this country.

Bloembergen:
And clearly the French must have had some, because they wanted it there, so they clearly had to chip in. So I think it's just that the services were well aware of the importance of these things. And there was no problem raising money.

Bloembergen:
[refers to list of questions] How was the work of ABDP divided among the four authors? Well, John Armstrong had been a student of George Benedek, who was assistant professor at Harvard at that time, and he wanted to do something else, and George recommended him, a very capable fellow. I had him as a post-doe. And he had learned his quantum mechanics really well, not as I did, self-taught in the war, and he did the part on the quantum mechanical perturbation theory, which is a big section on how

to calculate these nonlinear susceptibilities quantum mechanically. I told him, you just take the linear susceptibility theory, and see how Kramers and Heisenberg did it for their dispersion formula in quantum mechanics, and you go to the next order of perturbations...And that's what he did. Ducuing did all the solving of nonlinear differential equations. He had come from the Ècole Polytechnique, and he knew a lot about solving classical differential equations. But I had found the prototype solution first, you know. On the hyperbolic secant comes in, and the second harmonic completely phase matched situation, so I knew what sort of things to expect. He did all the details, though, of those elliptic functions that come in and so on, general solutions of the coupled equations. What did Pershan do? He did the local field stuff. We all wondered what happens in dense media in the nonlinear case. I said, "Look, there are two things. You have an acting applied field and a local field. We have to extend that to the nonlinear." He worked it out especially, all the details, what happens in nonisotropic media, which is quite complex, that sort of work. And then, I did the central section 4, on the beams being coupled, and the walk off, because the Poynting factor is not parallel to the k factor.—I'd really written down the basic set of coupled equations, both for three-ways "X1, and four-ways X3, and I remember John Armstrong coming and he_ said, "Look, there are these other terms, that you have X3, not times E1 x E2 x E3 but E1 squared." "What do we do with this?" I said, "John, that's marvelous." You know what that is? It is intensity dependent index of refraction. And he wouldn't at first believe it, and he was worried, but I said, "We put it in. That belongs there." But we didn't have any idea of following it up, even thinking about some of it. That came two years later. As far as me, I could hit myself for I hadn't taken it up, because the intensity dependent index is there. So that is how the work was divided. It really goes by personal inclination, you know. You have a group of co-workers. We had joint sessions here in this office, lots of bull sessions, talking about all the physics that might go on, and then each went off and worked out details. Most of the detailed algebra and analysis was always done by my co-workers, because they're much more fluent in the use of these mathematics than I was.

Kelley:
In reference to your question 6, on most nonlinear optics, think there are some exceptions but in most nonlinear optics the semi-classical approach is perfectly adequate to an understanding of phenomena that go on.

Bloembergen:
Yes.

Kelley:
And the thing that ABD and P missed, which Bloembergen later developed, was the density matrix approach to the nonlinear susceptibility calculations.

Bloembergen:
Yes. We couldn't do the imaginary parts with the ordinary perturbation theory of energy levels, and we had to put the damping in, and that meant going to density matrices, which I knew. I had applied them in magnetic resonance. But that was in the paper by Ron Shen, that was published in July '63. We did it in the fall of '62. I knew how to get imaginary parts in, and they could explain 2-photon absorption and Raman scattering. But then we wanted to put this on a complete integrated formal basis, and that we did in the spring of '63, and it was published in July of '63.

Bromberg:
Part of the reason that question is in there is because when we spoke last year you said, "Well, I can tell you something about the Rochester-Harvard controversy on quantum mechanics."

Bloembergen:
I can tell you a little about it. You ought to talk with Glauber.

Bromberg:
Well, certainly I shall.

Bloembergen:
Let me tell you this. I talked very little with Roy Glauber, because I was busy enough, and I knew that I could give a good account of practically all the important effects on a semi-classical basis. And I also knew one effect that I wouldn't be able to account for, and that is spontaneous emission and spontaneous emission noise. But I knew from the microwave masers that even that you can sort of fake semi-classically by just allowing the thermal noise not to go to zero at zero temperature, but keeping a quantum mechanical term in there, and if you allowed that, what you might call the zero point vibrations of the vacuum field oscillators, that then you even had most of the so-called pure quantum mechanical effects. Then this is still true. There are some very fine points, and there's a lot of discussion even in the seventies, on the distinction between Dicke and Bloch states.

Bromberg:
Dicke and Bloch states?

Bloembergen:
Yes, Felix Bloch. You see, the quantum mechanics only comes in at the first time of inversion, but as soon as the population... ??? Einstein. Spontaneous emission. *laughter*

Kelley:
Yes, but I think it's important, that Karplus and Schwinger paper, although it's forgotten.

Bloembergen:
—Karplus and Schwinger—Al Clogston of Bell Labs had a very famous, very important, not famous unfortunately, paper on the three level maser, using the density matrix formalism. I had done only the population, because I had a gut feeling that the angles of coherences were not important in the microwave case, and they weren't, but he analyzed that in detail.

Bromberg:
Does that mean that some of these papers simply were not picked up, and so people re-did it? I mean the Karplus and Schwinger paper—is that something everybody knew?

Bromberg:
The Karplus and Schwinger papers were discovered by you much later.

Bloembergen:
Right.

Bromberg:
And Clogston I assume then also?

Bloembergen:
No, Clogston I was aware of. He used the density matrix formalism to analyze the three level maser much better than I had done, in particular keeping in the coherence, as the off-diagonal element.

Bromberg:
—that did come into the knowledge of the community?

Bloembergen:
Yes. But then everybody realized that for the microwaves it was not important. But you had to pick it up for the optics stuff, and that's what Ron Shen and I did.

Kelley:
It's interesting, (maybe Nico would like to say something about this), my observation is that quantum electronics is an experimentalist's field, and that a lot of the good theory that has been done has been done by the experimentalists because they want to solve practical problems related to their experiments; and there has been really not that much fundamental that's come out of the theoretical work that's been done by the theorists in the field.

Bloembergen:
Glauber's work is maybe one aspect; but yes, clearly the electromagnetic interaction with matter was solved basically by Dirac and Breit and Wigner in around 1930. And if you look at Breit's paper in '30, you can find already some nonlinear developments.

Bromberg:
Now, your work is both experimental and theoretical.

Bloembergen:
But I consider myself an experimentalist. But you know, the theoreticians didn't carry it far enough. And didn't foresee all the implications.

Bromberg:
So for something like the ABDP, you said to yourself something like, "Well, the theory has got to go farther than this for us to—" So the spontaneous emission comes in only at the very beginning. Practically all observed effects can be understood, and all the important macroscopic effects are all semi-classical.

Bromberg:
Somehow when you said that, I thought maybe you had refereed some of the articles or something like that.

Bloembergen:
They never asked me to referee.

Kelley:
Well, I wouldn't call it a Harvard-Rochester controversy. It's a Glauber versus Mandel and Wolf—I think that's the fair way to put it. There wasn't any institutional controversy.

Bloembergen:
I think the Hanbury-Brown contribution was very fundamental in the fifties. Higher-order correlation functions—really, I think he didn't get enough recognition, because he was so far away in Australia. And remained there.

Kelley:
Well, I was going to make a kind of comment and see what you think about it. In relation to the Glauber versus Mandel and Wolf, I've always thought that, if an electrical engineer pointed out to them, to Mandel and Wolf, that an oscillator was an amplitude-stabilized device, and wasn't just narrow gain Gaussian noise, that that would have solved the whole thing, and it wasn't even necessary to develop all the formalism that Glauber developed, although it was useful at Harvard I think for other problems in quantum electronics. But the real central issue in the controversy initially was that Mandel and Wolf mis-analyzed the statistics.

Bloembergen:
They made a basic error in some of the early publications.

Kelley:
That's right.

Bloembergen:
That started it.

Kelley:
I was going to say, in reference to the density matrix formalism, I think I wrote the earliest or one of the earliest papers using the density matrix formalism, but I missed the damping part of the problem. I feel sort of sorry I hadn't done that part. **laughs**

Bloembergen:
That came later. That got to the imaginary parts, and then everything fit into a grand scheme. But of course the density matrix was used by Schwinger and Karplus in '48.

Kelley:
That's right, and they had in fact nonlinear susceptibilities in there. He has fourth rank tensors.

Bloembergen:
You know, if you go far enough back, you come back to

Bloembergen:
I mean, Lorentz could have done that. That was purely classical, except for the Armstrong paragraph. Lorentz could have done all that. He didn't do it because, as I say, he lacked the stimulation of stimulated emission of radiation, 80 years ago. But it could be almost done now completely classically, if you take a classic anharmonic oscillator model, which we did describe in that paper. And then do the classical Maxwell's equations. All the optical effects roll out. Then you can calculate the constants of such an anharmonic oscillator which are nonlinear susceptibilities, quantum mechanically. And that we did by higher order perturbation theory, first John Armstrong, and then with the damping terms in, as Paul Kelley says, with the density matrix formalism—which was completely developed before, not by us. It was developed by the pure

theoreticians like Schwinger. I don't know where Schwinger got it from in '48. Probably he doesn't claim that he developed it. Density matrix goes back to Fowler, statistical mechanics handbook, and that's the thirties. Von Neumann. So I think, you can always go back farther. But the question is, how to make use of it and how to apply it to new circumstances?

Bromberg:
Now, on the Raman anomalies, you gave a very nice picture there of the Puerto Rico Conference, where you said in effect that things were ripe but had just not jelled by that conference, and it would be interesting to hear more about that.

Bloembergen:
Oh, the self-focussing, yes. The Raman anomaly. Yes. Well, of course, there was a lot of talk of Raman anomalies and other anomalies, and I always had the gut feeling that they had nothing to do with the effects they were supposed to be anomalous for, but with something else. That something else turned out to be the self-focussing. And the thresholds that people measured for Raman effect and Brillouin and so on were often not the thresholds of these effects at all, but the thresholds of something else.

Kelley:
Yes, the Raman anomalies were a symptom.

Bloembergen:
Yes, a symptom, but there was a lot of discussion. There was a lot of excitement for a whole year.

Bromberg:
Which year are we talking about now?

Bloembergen:
Yes, the end of '63 and '64.

Kelley:
There was a self-trapping paper by Garmire and Townes and Chiao.

Bromberg:
How did that fit in? Was this already beginning to seem as if this must be implicated?

Kelley:
—that was pointing in that direction.

Bloembergen:
Absolutely. But it wasn't clear how you got to that stage, and that was the self-focussing of Paul comes in. And then there was this one issue of PHYS REV LETTERS where your paper is and one by Shen and one by us. Now, we had found, just before that, and we had submitted that as a ten minute paper at the Chicago meeting in the fall of '64, and that was, that the anomalous Raman effect was due to self-focussing. And then we heard that Ron Shen had something submitted to PHYS REV LETTERS, probably you too heard. I said, "Now we have to write in that same issue of PHYS REV LETTERS, because the Chicago meeting won't count."

Kelley:
I came back from Puerto Rico convinced that that was what was happening. In fact, I'd even been thinking in that direction before Puerto Rico, but then I was absolutely convinced—

Bloembergen:
Because at Puerto Rico, we had a picture of the thing in CS2 from the Russians. Rostimov and Philipetsky.

Kelley:
Yes.

Bloembergen:
—and Zeldovitch. Those. Yes, he showed a picture of streaks of light in CS/. It was very suggestive. But even before that, a year before that, there were these streaks in glass of Herscher, and then they remained a mystery. I think they influenced Charles Townes to think about it.

Kelley:
Townes saw that a steady state—

Bloembergen:
—because you look in glass and you see the steady state. You can literally see the four sided track of what had happened. You didn't see how they originated, but you see them propagated.

Bromberg:
CS2, cesium something?

Bloembergen:
Carbon disulfide. In fact, shortly after Paul's and these other two papers in PHYS REV LETTERS came out, you on self-focussing, and me that that is the origin of the anomalies in the Raman effect, came out this paper by Elsie Garmire and Chiao, in which they actually saw these filaments. They had put a number of beam splitters in the CSt solid and made pictures sideways.

Kelley:
Very clever.

Bloembergen:
Very very nice, and very convincing. And I asked Pierre Lallemand, "Now, I asked you to do that, and why did you never do it?" He said, "I did it with benzene, and then you don't get enough reflection from glass, and benzene..." You see, he didn't have the guts, and he never told me, but then he had to do something. You either have to use other beam splitters, if you really believe it, or you have to use another liquid. Now, if he had accidentally used CSz he might have found it. But that's how these things go.

Bromberg:
He was working as a graduate student here?

Bloembergen:
He was working as a graduate student.

Kelley:
That was a suggestion I was going to make to you. If you can get hold of Elsa Garmire's notebook for this historical archive—she had a notebook just full of marvelous effects, and I think about half of them never got explained, but all these nonlinear effects, the problem with the stimulated Raman and self-focussing and the light by light scattering and the phase modulation—they were all going on at one time.

Bloembergen:
It was very hard to sort out, and then the lasers were ruby lasers of multi-mode, sometimes up to 100 modes simultaneously.

Kelley:
—that's right—In fact, everybody's laser was slightly different.

Bloembergen:
—oh yes. So it was very hard to do controlled experiments. Very hard.

Bromberg:
It seems late for people to be using just ruby lasers. Maybe my chronology is wrong.

Bloembergen:
Well, they were using glass too, but they were also multi-mode. And those were the ones that easily gave high power.

Kelley:
I think there are things in Elsa's notebook which have never been explained. I'm sure other people have notebooks like that too.

Bloembergen:
That is, they never go back to them and explain them. Yes, you see the problem was not really to find the facts, but you had to identify them.

Kelley:
There wasn't that much really, fundamental physics, in a sense, I don't think. I mean, once the stimulated Raman and self-focussing were understood, and self phase modulation, I think then all the details, all the shapes of the rings and—

Bloembergen:
Yes, that all fell into place.

Kelley:
Yes, and the interest dropped precipitously after that.

Bloembergen:
It is quite remarkable that it still took several years before people identified this as the origin of breakdown.

Kelley:
There was a real excitement till about '66, and then I think —

Bloembergen:
Yes, well in '68. I remember even in '71, Charlie Townes and Prokhorov arguing about moving focal points.

Kelley:
Yes, but it began to get a little bit a question of angels on the head of a pin. You knew that there wasn't that much fundamental left any more. There wasn't any really new physics.

Bromberg:
I think that there was even a little bit of doubt early on about whether you had the nonlinear optics working, and these anomalies raised a problem that you might have to rethink the theory. Is that it?

Bloembergen:
Yes, people thought that maybe something fundamental was lacking in the theory, at one time, when they talked about the anomalies.

Kelley:
That you just couldn't take the cross-section that you observbed spontaneously and apply that to the stimulated problem. There was a lot of talk about that.

Bloembergen:
That's why I thought that Lallemand's experiment was very basic. He found that initially it was just exactly the theoretical shape, and that's why we did our experiments in hydrogen gas, because there the self-focussing can be really eliminated, and in one cell you can just get the theoretical cross-section. And then, in liquid cells, suddenly the anomalous gain takes over on a logarithmic scale. That's our first indication that we are reading it all right, that the anomaly in the Raman effect is due to something else, probably due to filament formation.

Bromberg:
Now, the only other question I have here is whether you were involved in any of the applications of non-linear phenomena?

Bloembergen:
As I said, I took a patent out on optical modulation and one on harmonic generation in cubic materials. Where you had to make a slab structure. But you know, I consulted for Perkin-Elmer and so, but I never wanted to build an optical device myself, because we're not equipped to do that here. But I'd rather talk with other people.

Kelley:
But there are certain practical nonlinear optical devices that have come out, certain harmonic and mixing devices that are used routinely.

Bromberg:
That was my question, whether Professor Bloembergen was involved in any of that kind of—

Kelley:
—no, he was involved in developing the fundamental understanding that led to the—

Bloembergen:
I don't claim any particular practical realization, and neither did we ever do any developmental work on any type of laser. May I say one other thing about it, because I have been misquoted. I had an argument with Art Schawlow at the Optical Society interview, last November in Tucson, because it was my thesis, that all the important lasers were done first in industrial organization labs, (which includes Lincoln Labs for this purpose, on the semiconductor laser). I mean organizations where you can really bring different technologies to bear on a single focal point of issue. When I say, academia is not suited for that, I mean in the U.S. In Europe it's different. You have academic institutes with a professor director, and you can focus, because Art Schawlow said, "Look at the dye laser independently developed by Sorokin at IBM and by Schafer at the University of Marburg. But that was a Max Planck-Institute at the University of Marburg. And the organization, you see, it's a research organization, just as, [though] not as big as Lincoln or Brookhaven. And that's where you have a director in charge. Now, American professors, they don't have a permanent technical staff. They work with post-docs who stay for three years, usually not longer, graduate students who leave after they get their Ph.D., and a very limited support for technical shop personnel and so on. And under those circumstances, with those boundary conditions, you cannot build a device.

Kelley:
You need crystal growers and people like that. Good machine shop that can fabricate things, people to do optical coatings.

Bloembergen:
Yes. And usually it wasn't set up that way at U.S. universities. And then somebody who really says, "Now, all these facilities and all these things will be concentrated on that goal."