Oral History Transcript — Erwin L. Hahn

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Interview with Erwin L. Hahn
By Joan Bromberg
At the University of California;
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Transcript

Dr. Hahn:
You had a question list?

Dr. Bromberg:
I do have a question list.

Hahn:
OK, I don't quite know where mine is. I don't need it, do I?

Bromberg:
If you do I'll just hand this to you. I began by, with the question about how you became interested in physics.

Hahn:
Well, I was a chemistry major in undergraduate school at Juniata College, and I became weary of the pure memorization of chemical formulas, and was impressed by the fundamental concepts of physics, and so I began to major more in physics in my senior year, although I graduated as a chemistry major. Then I went on to Purdue University, and began my major in physics. But I was really a major in chemistry nominally, at college.

Bromberg:
So even as you went to Purdue — now, that was right in the middle of a war.

Hahn:
That's right.

Bromberg:
But you were actually starting graduate work?

Hahn:
Yes, I had one year at Purdue, that's right, even during the war and —

Bromberg:
I think I saw Purdue on your list and then I thought of Lark Horowitz and —

Hahn:

Yes, Lark Horowitz in fact arranged that I should take an examination at Juniata as an entrance check on me, and I passed it, and went on and got a teaching fellowship, a TA appointment, to help me through financially. At the time I was of course not married. So that's how I began, but the teaching staff at Purdue was somewhat decimated by everyone having gone off to various places, and there were a few people there and the courses were rather slim. But Lark Horowitz was there and he taught and a few other people.

Then I was taken off into the Navy. I was drafted, because I initially had been accepted by Los Alamos as a physicist, althugh not completely mature, and then I got married simultaneously and then they wrote back and said I couldn't go because there was no housing for married couples. So I was drafted into the Navy and went into radar.

Bromberg:
Did you go into one of the American laboratories, or you went into doing radar—

Hahn:
No, I was an enlisted man and I went into what was called the RT program, Radio Technicians program, and became what's called a technician's mate. Then I went to Monterey for three months, then came back to the Navy pier, took a nine months course and then was held on as an instructor.So I taught sonar and radar in the Navy for almost two years, and then I departed the Navy and went into civilian life.

Bromberg:

In a sense the combination of chemistry and radar couldn't have been better.

Hahn:

Yes, that's right. I had a variety of things in that span, and I learned — the Navy experience gave me a viewpoint on pulses and so forth, because of radar. That's when I began my pulse interest. And then I went back to see Lark Horowitz, after I was discharged from the Navy, but he didn't have any money for a research assistant or teaching assistant, so I went to the University of Illinois, and began my graduate work there.

Bromberg:

With whom? You went and I assume you just started off taking courses.

Hahn:

That's right, I started taking courses, but I became an assistant to D. (Donald) W. Kerst, the man who invented the betatron. One of my close associates at that time was Bob Carter who was also a graduate student. He had worked with Kerst and had been at Los Alamos developing the bomb, and then came back to finish his graduate work, and he was the one who was instrumental in guiding me towards Illinois. He was a friend of mine and so —

Bromberg:

You were in Pennsylvania,is that where you came from originally?

Hahn:

I grew up in Sewickley, Pa. Five miles down the river, up the river, from Pittsburgh, north on the Ohio River.

Bromberg:

So Carter was a friend of yours that far back?

Hahn:

No. I'm trying to remember where I met him. Oh, I met him at Purdue, that's right. He was at Purdue also and then we parted our ways. I went in the Navy, he went to Los Alamos. And then he, we maintained contact and — there was another fellow at Purdue, very interesting, his name was Harry Daghlian. He was a graduate student at Purdue and he was the first man to be killed by an accident at the Los Alamos labsHe was manipulating a block of plutonium in the laboratory, and he accidentally got a cadnium reflector too close or something, I don't know exactly what happened, but he was irradiated and died in about two weeks. In those days they didn't have any protective devices for dealing with plutonium. So I remember that very starkly. That's incidental.

Bromberg:

How did you get into nuclear magnetic resonance at Illinois and what was going on there?

Hahn:

Well, I was destined somewhat to be a particle physicist in that I had started with Kerst as a graduate student developing a power supply, of various kinds, devices to power high current ignitrons and things of that sort, to power his betatron. At that time, we had a 20 MEV and a 50 MEV betatron that were developed, and I was sort of stuck, I thought, in being an electronics technician. Now, he may not like that but that was the truth. And I wanted to do some physics. Now, I started to do some physics with Bob Koch. He's the retired director of the Aerican Institute of Physics.

And we did a cloud chamber experiment, on triplet production, I remember, and I remember there was a disaster there because after spending day and night for two weeks, we discovered that the cloud chamber coils had shorted and we didn't know what the magnetic field was, after all our data was taken. Anyway, that really wasn't the true cause of my departure from high energy work. I meanwhile became interested in the report that Purcell and Bloch? had developed this technique for seeing nuclear magnetic resonance signals, and Jim Bartlett, who was a professor at Illinois at that time, he taught mechanics and some biological physics, he said, ""Why don't you read this paper? You might be interested in doing this experiment. "" I read it and I said, "I'd like to do it.

It's really something where I can get my hands on a procedure on a table and work on my own, and build as far as my capability will permit," whereas with the betatron I felt I was locked into a group, and I didnt like it. So eventually he took me on as his thesis advisor and I departed from Kerst's empire. Kerst didn't like it, and objected to it, but I went ahead anyway, and from then on I built my own apparatus. I built a Pound-Purcell- aoembergen aparatus for my thesis, and I infact was inspired by the thesis of Bloembergen, in which he outlined the theory of what was called mutations in the nuclear magnetic moment, a calculation which had in fact been made by Schwinger.

This calculation represented, what was involved was the response of the nuclear polarization to a rotating magnetic field that was suddenly turned on and it flopped from the ground state to the excited state back to the ground state like a— actually, it's a misnomer to call it mutation, but nevertheless that's what we called it, and I did that for my thesis. Well, I finished my thesis. In fact I was somewhat guided also by (Arnold) Nordsieck.

Bromberg:

Was he at Illinois?

Hahn:

Yes. He left Columbia and came to Illinois, and he guided me-you might say he signed my research request chits — but his interest wasn't in magnetic resonance at all and he didn't really steep himself in that discipline. But nevertheless — I more or less on my own. There was nobody else in NMR at Illinois, but Gutowsky, Herb Gutowsky who came from Harvard and started the field in chemistry.

Bromberg:

Did you have a lot of discussion with him?

Hahn:

Not much. The fellow who I had the most discussion with was, after I got my degree, I stayed on for a year as a post-doc, and Charlie Slichter came from Harvard as an instructor, and I overlapped with him about six months, and I profited a great deal from his presence, because he'd done a thesis with Purcell on magnetic resonance.

Bromberg:

I see. I had quite a wrong idea. Somehow I thought there was a flourishing NMR group.

Hahn:

No, it was just beginning. There wasn't any at Illinois at the time. I started my own work independently of Gutowsky. Gutowsky was in the chemistry department, and I did my own in physics, and we knew of each other's works, but our procedures and experiments were entirely different. He was doing CW, continuous wave excitation in the tradition that had started at Harvard when Pake and his group, Purcell, studied line widths and shapes of nuclear signals and relaxation and, but Gutowsky's interest was more in structural chemical problems, using NMR. I was more or less phenomenologically interested in doing, finding effects, and seeking novel ways of applying NMR in a different way, I suppose.

Bromberg:

So now I guess that Stanford and Harvard were the major places, is that correct?

Hahn:

Yes, that's right. NMR ws discovered more or less simultaneously by the Bloch group at Stanford and by the Purcell group at Harvard, and they didn't know — Bromberg;— we look at Illinois as a little group just beginning—

Hahn:

Yes, I was just one of the many people who started it up all over the country. There were a lot of people who took up the field, having read the papers of the two major sources, Stanford and Harvard. So a lot of people started up the field. There was a group at MIT. Who's the one who ran the magnet lab? Well, anyway, he died. I forget his name. Bitter, Francis Bitter, he started up a group. And there were other groups in the Midwest and down at Oak Ridge, Livingston — people did all kinds of follow-ups doing NMR. There were so many things to look at that you couldn't go wrong in doing something different from anybody else. Bromberg; I see. OK, that "s illuminating as a picture of what was going on around 1950. So naturally you wanted to go to Stanford, is that correct?

Hahn:

Well, what happened was, after I was there as a postdoc at Illinois for a year, I discovered spin echoes, and having discovered that, that gave me a mark of distinction, if I may put it that way, because I was told this following story. I had applied for a National Research Council Fellowship, and I won it. I won the grant to go to Stanford, and the story was — who's the guy at University of Virginia? Well, one of the judges had noticed that my name was at the bottom of the pile, practically, well, near the bottom, but he remembered I'd found spin echoes, so he pulled it out and put it on the top. Somebody told me this in confidence. Beams, J.W. Beams is the man, and he pulled it out and put it on top, so that's how I won.

I don't know if it was on top, but that's how I got one of the National Researcg Council Fellowships. So with that in my bonnet, Nordsieck, who was there at Illinois, had of course previously worked with Felix Boch. They "d done some theory together. He said, "Why don't you go to Stanford? Block is a good man and you'd learn a lot." So I said, "OK," and California attracted me, since I'd been there during the war in the Navy in training school, so I went to Stanford and that's how I became a post-doc for one year, a fellow, and then I was an instructor for one year. So there I continued my work on spin echoes and directed a student of his, under me as well, a PhD, and we did a problem together.

Bromberg:

What kind of atmosphere was that? What was going on?

Hahn:

Oh, at Sfanford? Oh, it was booming in detecting spins and moments by nuclear induction, as the technique was with Bloch's group. There was a big basement downstairs, and there was Warren Proctor and a Chinese fellow, Fu Chung Yu, who's now retired at Peking University, and there was Warren Proctor, Martin Packard, Harry Weaver.

Bromberg:

Was Packard Huling in existence at that point? Or is this a different Packard?

Hahn:

Oh no, Martin Packard was a graduate student, nothing to do with Hewelett Packard, no no. He was a student with Bloch who worked on the apparatus to present the first evidence of nuclear induction, Bloch, Hansen and Packard. That was the famous trio. Packard was just about finishing, and then he went on to Varian Company, and I stayed on of course as a post-doc and instructor, and there were other young people there who have since migrated to other fields.

Bromberg:

Now, was that entirely the academic study of the properties of nucleii and —?

Hahn:

Well, that was the specialty of Bloch's group at the time, studying spins and magnetic moments by that technique. The Harvard group concentrated more on dynamical statistical processes that could be studied by NMR.

Bromberg:

No, I meant y our work.

Hahn:

Oh, my work? Oh, I pursued the study of an interaction which is now quite well known. It's called the indirect spin-spin interaction. We found that, I found it as a phenomenon which affected the signal amplitudes of the spin echo. It was a modulation which I didn't understand when I saw it at Illinois, and I set up equipment again at Stanford in order to pursue it further, and developed an empirical formula, relationship, an exchange interaction which explained the phenomenon, but I didn't pinpoint the actual mechanism, and then Ramsey and Purcell got wind of this through a publication of ours, and they published the real mechanism. Now, simultaneously Gutowsky and Slichter were looking at the same phenomenon in a different way, at University of Illinois, so in a sense we both independently saw this new effect in different ways. I saw it by the transient responses of the nucleii. They saw it by the steady state signals in the frequency domain. So —

Bromberg:

And that's this interaction by the electronic -

Hahn:

Yes, the electron-electron,nucleus couples from one atom in a chemical compound, couples to another nucleus via the electron clouds. And that's a boon to chemists because it enables you to distinguish groups from one another by looking at the spectra. So, meanwhile, of course, I was always interested in looking for other phenomena involving the dynamics of the echo with regard to exchange between wandering ions in liquids and so forth, and I wrote a paper with Maxwell on the subject. So those were exciting days, because everything in the field was new. Everything anybody did practically you could discover something new. The chemical shift, the work by Proctor and Yu in the laboratory — there was always something that wasn't understood, which was very exciting. Bomberg: It sounds a little bit like the early days of lasers.

Hahn:

That's right. That's right. It was all a booming virgin territory, and Felix Bloch was a marvelous source of inspiration and he, at that time, and since, he's always been interested in physics first and not in wheeling and dealing. That's what I admired about him. I had the grand privilege of spending afternoons, countless afternoons with him, just talking at the blackboard about various things, and speculating about what this would do and what that would do, doing calculations. I learned quantum mechanics from him just casually by these tutorial sessions which were unconsciously given to me without any planning. He was a great man, to me, and to everybody else. And he got his Nobel Prize after I left. I'm sort of glad he didn't get it while I was there because then he would have been distracted from— Hahn-11

Bromberg:

Yes. It's very European, as you describe it. I mean, I had the chance to spend a little time at the Berlin Institute and that was in the seventies, but you get the feeling for what it must have been like in the thirties.

Hahn:

Yes. Yes. Everything was personal, personally exchanged, in ideas and insights and so forth.The community was small. In fact, in those days I remember I could read every paper in magnetic resonance for about a year, perhaps, and after that it got out of hand.

Bromberg:

Really?

Hahn:

Oh yes. And then papers boomed and you couldn't keep up with it.

Bromberg:

Now, between this very academic, almost European academic atmosphere and IBM, what happened? were you already doing anything about technology while you were at Stanford?

Hahn:

Well, OK, let me say this. At Stanford the policy was not to keep on young people. The policy was — and it was considered a mistake much later, in fact — that they would build their staff from the top down, not from the bottom up with young people. So they told me I couldn't aspire toward tenure there. I would have to leave eventually, but they wanted me to stay a third year. I only stayed at Stanford for two years. Bloch and Schift? worked on me, trying to talk me into staying a third year, but I was offered this job at, a position at IBM as a research physicist.

In fact, as I remember, Bloch said that (I.I.) Rabi had told Watson Laboratories about me, and Bloch sort of in a poite way complained, "I wish that Rabi hadn't done this," so I would have stayed another year. But anyway, I decided, since I wasn't going to be a prospect for tenure or getting promoted at Stanford, I said I might as well leave, so I left and went to IBM. Now, IBM at the time —now, here's a funny thing- - there was a little conflict of interest which I didn't like. When I came to Stanford, Russell Varian approached me and said, "You know, you have this technique here which is very good for chemical analysis. We'll help you to patent it and we'll pay for the patent if you right it up." So I did that, and they took the patent.

They adopted the patent for a period of time and wrote one or two apparatuses, but then dropped it, didn't — pulse apparatus. At that time, of course, the computer wasn't developed enough to do four way (Fourier?) analysis, as it is now,and the computer, not being developed, didn't give the pulse technique much of a promise commercially, so they dropped it. So I had this patent in hand, but at the same time, the pulse technique had some capability of storing digits or pulses for memory, and IBM=— nobody knew at the time whether it had any potential. Of coruse, it did't.

The capacity was too small. Nevertheless IBM, not wanting to miss its bets on anything, expressed interest in hiring me, but I felt that there was a side interest in getting me to exploit the echo technique to see if it was good for memory. And I didn't really want to do that. I wasn't intereted in that. I knew that they wanted to have me advise on it, which I said, "OK, I'll advise on it, but I'm not going to per se develop a digital memory for you.

I'm interested in doing physics." So that's what I did. I mean, I did advise on that. When I went to IBM, a number of patents came out using the echo technique which were curious and interesting activities but didn't go anywhere commercially, and did get a couple of people awards. That's about the best advantage it gave for anybody.

Bromberg:

There are a couple of papers here with your name on them which sound as if, I mean "Spin Echo Serials" — (crosstalk)

Hahn:

— that's right. it's the — that was the IBM pursuit, that's right. So it was good. I mean, technically it had some good physics in it and it's been used and reviewed by others later, certain procedures in physics. But at IBM, I advised on it and got involved, but I did other things. I worked on quadrapole echoes and double resonance experiments, and I started up and published a few other papers.

Bromberg:

Herzog was your big —

Hahn:

— that's right, he was my graduate student there. He was a Columbia graduate student, and he was assigned to me to be his advisor, and I had a sort of an adjunct position at Columbia, and I lectured, and I was an adjunct teacher, so I functioned with IBM that way, whereby graduate students did come in from Columbia and interacted with Watson Laboratories.

Bromberg:

That's right at Columbia, at 116th St. ?

Hahn:

Yes, it's right at Columbia, 112th St. Yes. And there was always this conflict of graduate students — if they worked late, there was a problem of insurance and accidents and all that. There were some restrictions, in a way. But it was a very free place and IBM was very reasonably liberal, considering how they started this project from scratch, and allowed the doors to be open relatively freely, compared to the previous traditions of being strict, you know, protocol a la T.J. Watson, you know. {

Bromberg:

So you were really initiating an NMR program at the Watson Labs.

Hahn:

that's right, at the Watson Labs, yes, I did.

Bromberg:

Well, what was going on at Columbia with respect to that?

Hahn:
Well, at Columbia those were the booming days. I overlapped with Townes. He was there a year or two, and he was formulating at that time the laser principle. Bromberg; Yes, I think he probably got it to work before you left.

Hahn:
Yes. There were lots of good ideas floating around. There was Rabi and Foley and Kusch and, see, Lamb was not there then, Lamb was-I overlapped with Lamb at Stanford, when he was there. Bromberg; He went to Stanford around '52.

Hahn:
Yes, I was at Stanford from "50 to "52. Yes, overlapped him a year, I think.

Bromberg:
Did you have much interaction with Townes about the maser, was that any —?

Hahn:
Not really. I was more interested in NMR in a ground state regime. Townes was doing microwave spectroscopy at the time. He had lots of students, and he was concentrating on the ammonia maser at the time, so, he was developing it, but I wasn't following in detail what he was doing. There was a guy named Gould, you know, have you heard of this guy Gould?

Bromberg:
I have indeed.

Hahn:
Well, you know who you ought to talk to about him? He knows more about him, is Gene Cummings. He's a graduate student at Columbia at the time, and Gordon Gould had this idea and he was very secretive about it, every little iota on the maser, and he would have everything —

Bromberg:
— of course, he couldn't have been having that idea at that point because —

Hahn:
— I don't know when it was.

Bromberg:
It was "57 before he claims —

Hahn:
— I don't know, but I met him, and I didn't know what was going on at the time, but anyway, Gene Cummings knows more of the details. It's really a fascinating story. Bromberg; Let me ask you, then, one of the questions I asked you was, what impact if any did this IBM stay have on your style of work? Did you just go on more or less as you had been, or did working in an industrial company change anything?

Hahn:
I went on more or less the way I had been, as much as possible. In fact I was somewhat of a maverick. I led a rebellion against punching the time clock, and in fact, Bell Labs had gone through a rebellion just about the same time. And I always felt, I didn't like commuting, and I always felt, much as I liked IBM, I felt I was being preened to be group leader or something in alter work in applications for the company. I just felt that I wanted to get into the academic world. There were other people there who were very good people who stayed on. Some of them left. But I think, the style of work, I am grateful to IBM that I had the freedom that they instituted at that time which was rather novel for the company, because usually, they were just coming out of the electromechanical era, and applying of course, test, vacuum tubes to computers; same time they realized they had to get into the research area because of the development of the transistor and so forth, and seeing what Bell Labs had accomplished, they thought they'd better get into the research business too, and IBM was really a training ground for research leaders, that's really what it was, and many of the leaders now at Yorktown came from that era, that time. But I felt I wanted to get into the university, and I did. I still maintained connections wth IBM. I still consulted with them for a long time afterwards. It was a very amicable relationship and I'm very grateful to them.

Bromberg:
Before you get to Berkeley, I want to ask you about (Robert) Dicke's super-radiance paper, which he in a way built on your work. how it came to your attention, relations between you ahd Dicke;

Hahn:
Well, I remember Dicke published his paper in which he had some ideas far ahead of his time. Dicke is a very far-sighted creatively minded analyzer of the significance of things. And he realized, I felt, that something that already — well, I think it was Feynmann and Halworth had proposed that any two level system can exhibit properties of what magnetic resonance exhibited, namely, it radiates coherently.

Bromberg:
Now, that of course is later, though.

Hahn:
Yes, that's later, but Dicke had that in mind when he said, if you have a collection of particles all cohered together, then they radiate in coherence proportionate to the square of the number of powers proportionate to the number of particles, and in a way this was not original. I remember Felix Bloch, when I asked im, "What did you think of Dicke's paper?" he said, "Oh, that's in magnetic resonance already, why do you hvae to repeat it?" But Dicke had the idea also however of a new, of a form of statistics, Dicke states, which allow you to analyze the behavior of very few particles irradiating, and then connecting the property of very few to the property of very many, and there's a region in between where the particles behave like, you don't know whether they behave coherently or incoherently; that is to say, you have very few particles, they irradiate at a rate proportionate to the number of particles, you have very many, they irradiate according to the square of the number. And the intermediate region is the region of critical interest which the Bloch equations can't resolve.

Bromberg:
This business of your reactions to Dicke versus other people is I think pretty interesting, because I don't think many people picked up on that paper. I think you were —

Hahn:
They didn't. Well, his paper had more interest in it after the laser developed. In the beginning, his paper was more or less, many people thought of it as just a reiteration of something that was quite obvious. That is to say, if you take a single particle and watch it radiate, it radiates according to dipole, single dipole. If you have many of them,

they radiate like a calssical antenna, and that's what Dicke calculated, for many spins. But he had in his paper the concept of Dicke's states, these particles act like Boxe particles and if you choose certain states, and the properties of these states are very critical and of interest to situations in which you've studied the radiation of statistical fluctuations of fewer particles than an number is worth, like in, let's say, Rydberg states have been studied currently which reflect the calculations that Dicke posed in his earlier paper, and which were very important — Hirosch and others who've studied these states draw upon the Dicke paper, draw upon that tradition. So it has developed more importance and significance. People used the significance of Dicke's paper much later. When he first put it out, it wasn't realized.

Bromberg:

Were you in touch with him about the paper at the time?

Hahn:

Not really. No. He was of course impressed by the spin echo effect, as demonstrative of what he wrote about. I was in touch with him. I gave talks and so forth, and I talked to him once or twice about the coherence of the situation, but I didn't really analyze anything with him. He presented certain things and said this is the way it was, and that's very interesting — I had no influence on him, except from the fact that I had discovered spin echoes. That probably stimulated him to think about it. But personally I didn't have the theoretical notions that he did about it. I didn't (crosstalk) use Bloch's equations —

Bromberg:

He might have sent you a preprint, I would guess, just the way the publication dates — OK, you were going to tell me and the tape recorder, you decided you wanted to be in a university, and I think it's pretty clear from what you said why you decided to leave IBM. Why did you decide to come to Berkeley?

Hahn:

Well, actually I was solicited by two, I was solicited by the University of Minnesota, I remember. They offered me a job, and then Bill Nierenberg came from Berkeley. He was on a recruiting binge, and he said "We're interested in hiring you, do you want to come to Berkeley?" and I said, "Yes," and that was it. I think the wind of it was that Segre learned from Brillouin about my merits, and then Segre must have passed it on to the — this is how I got it. The staff here. And of course the Berkeley staff then was pretty small.

Bromberg:

Oh, really?

Hahn:

"55, well, you could put the whole department in a room like this. It grew like wildfire in the fifties and sixties. That was one of the — see, Jeffries and Knight were already here. There was an NMR group here, and I added my own two cents to it.

Bromberg:

Jeffries and Knight were in NMR?

Hahn:

Yes, they were in NMR inclined. Bromberg; I see, so you weren't going to start p a group at this point.

Hahn:

Well, I did my own specialty at the time, work. In NMR. I went into lasers later, certainly not immediately.

Bromberg:

Yes, I'm going to ask about that, but how did you get involved with Lyons's Hughes group?

Hahn:

I was a consultant down there.

Bromberg:

What problems were they —?

Hahn:

They were working on the maser. Spin dynamic problems with the maser, how to get masers to work efficiently, how to utilize masers for applications as amplifiers for communications and things of that sort.

Bromberg:

So they wanted you for solid state masers?

Hahn:

Yes, for my experience with spins and echoes and coherence. And I consulted for Harold Lyons. George Birnbaum was the guy that I chiefly interacted with. And then there was, what's the guy, (Robert) Hellwarth was there and others I don't remember, but there was a whole crew of them, and they were searching around for methods and means of setting up solid state lasers, masers, and I remmber while I was down there —oh, the interesting thing about it is, I consulted there for a while, and Ted Maiman was not there at the time I began to consult, so I met Ted. I knew Ted Maiman as a graduate student after that, also at Stanford, when I was at Stanford, and I met Maiman at a meeting somewhere and Maiman said, "You know, I'm looking for a job." I said, "You know, I can tell you where to get a job. Just call up Harold Lyons at Hughes. They need guys like you." So that's where he got his start there, and ever since every time I've seen Maiman he's said, "Hahn, if it weren't for you I wouldn't have got the laser to work." That's just an incidental story. So, in fact, we both got the Wolfe Prize together, you know.

Bromberg:

Oh, was he the other person?

Hahn:

There was a third person, Hersch? Herschberger? I can't get that name. Oxford, he was in electron microscopy. I, he and Maiman and this other guy from Oxford got the Wolfe Prize. We shared it. Maiman said, "Hahn, I wouldn't be here — should I give you my prize?" Just a joke.

Bromberg:

Do you have any memories of the Hughes work?

Hahn:

Yes, well —

Bromberg:

— the quality, the —

Hahn:

Well, it was very good, in the sense that they put out reports on developing materials for masers, or developing amplifiers and oscillators. I remember a guy named Max Stitch, he was more on the practical side for the company, but the rest of us were more or less speculators on how to get the maser to work more efficiently. And during that time, well, let's see, the three level maser had been developed by the Russians, and see, Bloembergen came out with this notion of the inverted system pumping, and I remember that all kind of came together.

Bromberg:

Bloembergen first published his proposal in October of "56, maybe it was on the desks in late summer.

Hahn:

Then (A.M.) Prokhorov and company did it, I don't know whether they did it earlier or later, there's a question about that.

Bromberg:

Well, they had a three level gas system idea earlier. On the other hand, when I read the paper,and I'm not a physicist, I can't really judge, but even as a non-physicist it seemed a very primitive paper compared to Bloembergen's.

Hahn:

Yes, I thought Bloembergen's was very elegant. He was conscious of mixed states and how you could have matrix elements between any set of levels, so you could pump from one level to another, and then look at the radiation on the final level, excited state level, to ground state. So -

Bromberg:

So this is really your first immersion in maser physics, isn't it?

Hahn:

Yes. Yes, at that time, I was a consultant. I wasn't doing it here. I remember Ted Maiman at that time was quietly trying to get the ruby laser working, and he was ignoring Harold Lyons's behests about doing this and doing that. Maiman was very independent. He did just what he felt he knew what to do. Maiman said that he ignored a paper by Schawlow, although the claim is that Schawlow was talking about something else. Schawlow had claimed, according to Maiman, that the ruby wouldn't work in a laser, but Maiman went ahead anyway. I think the argument is that Schawlow was talking about a different transition from what Maiman worked with. Anyway, Maiman went ahead and got the laser to work.

Bromberg:

So this consultantship lasted for a few years, and now I remember where I picked it up. I was reading a Hughes report, progress report, it was either "56 or "57, so I didn't really know how long it went on.

Hahn:

Yes. Yes. Then of course it sort of broke up. I think I left the consultantship shortly before he got the laser to work. Then I began to consult, I consulted for other companies hither and yon, but anyway, that's the way it went.

Bromberg:

Now, is it through that that you came to know the FeynmanVernon-Hellwarth paper on?

Hahn:

Somewhat, it began that way, yes, but I became interested in it in a funny way. At first I was curious about how electrons spin moments in garnets or in spheres of ferrites would couple to a wave guide. I was in EPR (Electron Paramagnetic Resonance) quite a bit at the time, and I tried a couple of experiments that were failures, in that I wanted to know what — if I put a magnetic moment in the wave guide here, how would it couple to a magnetic moment in the wave guide down here? So I set up an experiment in which I had two spheres of ferrites —one here, and one here — trying to find out how they coupled, and it was too complicated because there were too many modes.

Then I even set up one with NMR. But then, as the laser became more reproducible, they were carried out in different laboratories, I figured, "Well, the best way to do it is to use, to look at it in terms of a laser beam exciting crystal," and crystal had a lot of electron moments in it, electric dipole moments, so what happened was that there was a misconception in my mind, in a lot of people's minds, about seeing what was called superradiance. Dicke had said that if you take an optical system and put it in an excited state, and if the optical system decided to radiate coherently, it wuld radiate so fast that you couldn't get any results out of it because it would just dump very quickly. And it had something to do with the calculation of the density of states.

Bromberg:

That's in his original paper.

Hahn:

Yes. It would dump too fast. And along the way my student (Sven) Hartmann at Columbia, having done a thesis here in—

Bromberg:

— you supervised him when you were at Columbia?

Hahn:

Yes. Yes. And he was, well, he was steeped in the lore of spin echoes. We had done double resonance work togetehr. He latched onto the idea that you could do photon echoes, and did the experiment at Columbia. Now, when he did the experiment, he used the FeynmanHellwarth ideas, and other people were aware of the ideas, and I was aware of it, I was interested also in thinking of doing echoes of a sort, but he just did it first. And I don't make any claim to having perceived how to do it. But he just did it, and at first it didn't work for him because there was a perturbation in the crystal which prevented the signal from appearing until he turned on the magnetic field.

Just empirically. Anyway, he got it to work, and then I perceived that in the experiment he did, he didn't worry about how the light field itself would be changed by the spins as it went through. He assumed the light field would be very intense and would come out the same as it was before except for the response of the spins. So I set up an experiment with a student named Sam McCall who worked for Ford Motor Co. research laboratory down in Sunnyvale, and I picked him up, I said, "Look, you're a technician here. You know too much. You should be in graduate school and get a Ph D."

Bromberg:

Were you consulting down there?

Hahn:

Yes, I was consulting down there. So I picked him up. I said, "Look, why don't you come to Berkeley and start as a graduate studesnt? You're being wasted here." He was virtually a floor sweeper as far as I was concerned. So he came to Berkeley and did a thesis with me on, we did self -induced transparency. Now, there's an interseting thing about this. I had set up a calculation to see what happened to a comair? pulse of laser radition sent into a system of ground state optical particles ready to be excited by the pulse, so we put it on the computer, and we found that the computer predicted self- induced transparencies. But it was put in the literature and was ignored for two years.

Bromberg:

This is the article with McCall.

Hahn:

McCall, yes, but there was an abstract published a year or so before. We followed that up with an experiment, and when the experiment proved it, then everybody opened up their eyes. It was igored before because we put it in such a way that it just looked like an academic exercise on the computer.

Bromberg:

There are a couple of things I want to go back to one is how you decided to go to sest up this laser lab. Now, at this point, I have talked to Mike Bass, and he told me he was hired early on.

Hahn:

Oh yes, he was a post-doc with me, yes.

Bromberg:

But he was up — what made you decide altogether, was MNR getting to be —?

Hahn:

Well, MNR was turning out to be a routine technique and graduate students looked upon it as something that, there wasn't much physics in it, it was for chemists, and the chemistry department here is taking up MNR, and meanwhile the ideas of MNR were spilling over into the laser, as far as I was concerned. There were thingsyou could do optically that were asnalogues of MNR and moreover, coherent, propagation experiments looked rich, in what could happen. So I felt I could attract graduate students, obviously, as well as my interest coinciding with it, and I felt, to get graduate students into the latest avant garde work, I should go into lasers. I did.

Bromberg:

I have not seen this leitmotif in other people's work, of the analogies between MNR and optics. Is this something which is your special contribution or where there a lot of people (crosstalk) or what?

Hahn:

No, I mean, the Feynman-Hellwarth paper really stresses the analogue. Any two level system manifests behavior of the states in such a way that, when you take averages of radiating dipole moments, they can be couched in equations which are like the Bloch equations, and this is true for electron spins, optical two level systems, in a simple two level case, and the Bloch equations have been used even for phase transitions or for the electron accelerated beam laser. No, it's not original with me. It's just that I championed it, and since I've done a lot of transient work in MNR, I just made it a hobby of mine to see if they worked with optics, and other people have done similar things. I mean, Tang, for example, has shown that optical lasers exhibit free induction, after a laser pulse, and so other people have done it too. It's just that I have more or less tried to bring all the experiments together and show it in a more general way, that's all.

Bromberg:

So when you set up this laboratory, is it just your decision? It's possible at Berkeley, you just say, "All right, I'm going to —"

Hahn:

Well, yes, if you feel you have an idea and you write it up and you can convince NSF that you have some productivity in prospect, they'll give you the money. That's the way I did it.

Bromberg:

So you did it on NSF money.

Hahn:

Yes.

Bromberg:

That was one of my questions.

Hahn:

I began with the ONR in NMR, but the NSF took up a great deal of it. And so if you havse something of a track record, they'll allow you to carry on in another field, if you have a good idea.

Bromberg:

Is that a big expense, to set up a —?

Hahn:

Well, it became expensive. Yes. In fact, it got so expensive that now I hardly have enough money to continue certain laser experiments, so I've sort of gone back to the womb by doing NMR again, but using a very clever device developed by Professor John Clark here called the squid, superconducting quantum interference detector. And we found some interesting new phenomena with that, even with NMR. So there are things left in various fields if you have the right instrument to find them.

Bromberg:

Because when we met briefly, you gave me your materials, you told me that money is a real factor in —

Hahn:

— well, in doing optical work, yes. Laser work, the expense of ion pumps and so forth. If the tube goes bad, it can chew up a good fraction of your contract, contracts being as small as they are, you know. A laser tube costs as much as $12,000, and if it goes blinko, you're disabbled. And we don't have the kind of oney they have in Livermore or anything like that, where a thing like that is just a minor perturbation.

Bromberg:

$12,000 isn't much.

Hahn:

No, but you undergo a big financial trauma when a thing like that bugs you. And of course there's this generalized problem of lack of instrumentation in the universities. I've suffered from it. We all suffer from that.

Bromberg:

When did that start to come in?.

Hahn:

Oh, the lack of instrumentation? I'd say it began to be hurtful to me five to eight years ago.

Bromberg:

OK, and in the sixties it was?

Hahn:

Well, you know, when you started up, most of the laser instrumentation was more or less all the same, and less expensive, and you could more or less make it, but as experiments became more and more sophisticated, you began to purchase equipment which wasn't worth building in the laboratory any more because of a waste of graduate students' time. As experiments become more and more difficult and sophisticated, the expense of the equipment is correspondingly more expensive. You want to compete. The net result is, in recent years it's Bell Labs and IBM that have done the progressive experiments with lasers, not the universities. It's reached that point. I'm not sahing universities haven't, but — now, some groups still do well, but since I'm not one of the top laser experimentalists, some just — partly that, really. Maybe it's not as important for me. But it's been hurtful.

Bromberg:

I just became aware of this whole problem, by the way, from reading an article on research in Russia, and it tries to explain how both the Soviet and American scientists deal with it.

Hahn:

Yes. But I must say, the NSF has been very very good. I mean, they, for example, do provide extra instruments if you show promise and are doing really novel experiments, there's no doubt about it. They've done that with regard to the squid work for us.

Bromberg:

So now you in the mid-sixties are setting up this laboratory and Bass is helping you set up this laboratory. As far as I know, the only thing that was going on was in electrical engineering at the time.

Hahn:

Let's see, Ron Shen, when did he come aboard? I can't remember, was Ron Shen here? I can't remember.

Bromberg:

I don't remember.

Hahn:

People had lasers hither and yon. I mean, in those days, you could set up, you could call yourself a laser physicist if you just got a ruby rod and a pump and a flash lamp and so forth. It wasn't that big a deal. I wouldn't call it a huge development, in that I was setting up a laboratory on a huge scale. I was just doing one experiment. I was just trying to do one thing. I was interested mainly in just doing something with this transparency with Sam McCall. It wasn't Bass who helped me build it, it was with Sam McCall that the whole thing started by our curiosity in asking the question, what happens to a laser beam when it interacts with a two level system as it passes through the two level system over an extended distance? And we set up as primitively as possible the equipment to do it.

In fact, when we got the experiment to work, the first time we got evidence of it, and then the effect went away, because of the crudeness with which we set up the experiment. The rod was distorted. We had to twist it and get rid of imperfections in it by pressure and so forth. That were getting in the way. And we got the effect back, you see. I mean, it was't that we had a grand array of equipment, we had — Bromberg; — OK, I was mistaken. Hahn; — you see, and then one thing led to another. Atter we got that to work, then I went into look into self-induced transparency with sodium vapor.

Then I did it with electron spins. Then I did it with -that led to other experiments, and then we set up a CO_2 laser, but it was done over a succession of years, you see. One thing led to another. One success stimulates NSF to give you more money to do another. And another success gives you — so from contract renewal to contract renewal, I started to build up enough laser apparatus so I now have a big lab, relatively speaking.But it all started, if self-induced transparency hadn't worked, I might not have gotten the money to do the other. That's the way it works.

Bromberg:

That's kind of interesting. Hahn ; Yes. When Bass came here, he tried an experiment, it didn't work, but later on actually his experiment was repeated in a different way. I didn't really interact with him that well, to consult effectively. He was told his experiment would work by somebody else.

Bromberg:

And it didn't — which I'm sure you usually don't hear.

Hahn:

I was busy directing students in two different fields. I was still pursuing NMR, and so I've been sort of having a foot in both fields. I had the interesting experience of going to conferences in the two fields, NMR and lasers, and in the laser business, it's a fierce comepetitiveness. In fact it's unpleasant. Whereas in NMR, it's been very pleasant. It's an established field. Everybody knows what phenomena are where and what, and nobody's trying to leapfrog over another guy to claim, you know, credit for this or that. In the laser business, it's vicious. Bromberg; Is it vicious since the sixties, to jump to today, or is it —

Hahn:

It's still kind of competitive today.

Bromberg:

At that time, then, this adjective really applies.

Hahn:

Yes, I think so, yes. But even now, it's, well, it's still a burgeoning field, that's the reason. I think it's still developing, there are things going on. Unfortunately it's going into the classified era. Star Wars and stuff like that. But that's — I don't know anything about that.

Bromberg:

— that I have on the list is your summer at Bell in "67, was that of any significance?

Hahn:

Well, that, Kumar Patel had developed the Co2 laser then and was applying it to different experiments, so they set it up to do selfinduced transparencies with the Co2 laser driving a pulse into SF 6 sulfur hexachloride, so I went there, simply because Dick Slusher who had been my student, but not in optics, he was in NMR, went into optics at Bell, and I was invited to spend some time working with the experiment on SF 6. It wasn't very long. I wasn't there that -I doubt I was there the whole summer, I was just there for a week or so, and, maybe it was two weeks, I can't remember — but anyway, they got SIT working in the SF 6, and using a different technique for setting a pulse by rotating mirror. It was relatively primitive then but it worked. Of course, then, my subsequent students who have gone there since, including Slusher, McCall went there, a number of sophisticated experiments were done with self-induced transparencies, including Hyatt Gibbs, and so that started an era of investigations into coherent pulse propagation in Bell Labs.

And since then of course they've gone into fibers, but that's, the techniques there are entirely different, although the principle is somewhat related. All I can say is that self-induced transparency was a curious cute academic effect that had a — it flared up, for a couple of years, and now it's gone its way into a special case of soliton phenomena. There are many other solitons now besides Self Induced Transparency. and it's just one event among numersou that manifest soliton behavior, although I must claim Self Induced Transparency is the most clearly defined one, what you call soliton. Now, soliton mechanism is now in vogue for all kinds of things, particularly in solid state physics, where one accounts for the motion of chains and groups, solids, migration, disturbances and so forth. Even in particle physics — I haven't followed the grand significance of the whole business. Other people have specialized in that, I haven't. So I left Self Induced Transparencies as an event, and have gone on to other things.

Bromberg:

Well, what then should we now talk about, as the principal researches that ought to go on this record? I've got some things here about the pulse area, concepts — work with Brewer —

Hahn:

Yes, that's right, Brewer. Yes, I've had a very profitable interaction with (Robert) Brewer. We did three level studies of optical behavior theoretically and showed that the density matrix which is somewhat akin to the Bloch equations in certain special cases can account for the interaction of light with three level systems, on and off resonance, and that leads to generalizations for other systems, and that's been invoked quite a bit in the literature. And Brewer's work on Raman? scattering has been taken up by us here as well, and that was an outgrowth of his previous work with Shoemaker. So I would say Brewer's work also stimulated me to do some other studies involving spin rotation interactions in molecules.

He's had a number of my graduate students down there working for him after they graduated here as post-docs doing optics. One of them was a Japanese guy named Mitsunaga who did some interesting work with Brewer on very special studies of interaction of light with optical ions. What I can say is that my work with Brewer has been along the coherent optical response region, as well, so many, I've always, I've contended that a lot of the effects in optics that have been rediscovered are effects that have already been in NMR, but that's more or less somewhat true for simple cases but not try for general cases, with more quantum mechanical requirements that have to be injected that the Bloch equations don't include. People have generalized —

Bromberg:

How did youg et to work with Brewer? Were you consulting at IBM?

Hahn:

Yes. Phase conjugation is considered a kind of an echo phenomenon, for example. A number of people have made that — it's an echo phenomenon involving the refocussing of phase, phase vector. That's been stated by (Amnon) Yariv, published as such. If you want any analogues, that's one. Four-wave mixing is really a manifestation of analogue of echo phenomena in simultaneous time domain. You know, the man who ought to be recognized in the laser business is also Shiren, Norman Shiren. He's the man who first pointed out phase conjugation using sound waves. And then other people have taken it over and ignored him. Now, I don't know who was the first to-he called them backward wave echoes, but that's really phase conjugation, and he should be given credit for that, and I think he's ignored by the optical people. He did it first with sound waves in combination with microwaves.

Bromberg:

Is he an IBM person?

Hahn:

Yes. Yorktown. I think you ought to put that, for my money I would like to see that in the record, but that's —

Bromberg:

Well, it's now in the record. What I'm really asking you is to go back and think through some of the most significant or the most difficult things that came out and —

Hahn:

Well, let me point this out. Self Induced Transparency phenomenon to me was most exciting, because it was very peculiar in that the computer predicted it, and even we didn't believe it, and didn't make a big splash about it. When we did the experiment, even, a number of people wouldn't believe it and said it was wrong, and finally it justified itself, by virtue of being reproduced by others. Some people felt that the contribution Sam McCall and I made — just a very small minority said it was only theoretical, not experimental, because experimentally our work they felt was somewhat ambiguous, but I deny that. But it was not a theoretical piece of work, it was experimental. Otherwise we wouldn't have published the theory. Nobody would have believed it. Anyway, as far as my own work is concerned, obviously the spin echo apparently is considered pivotal, more than anything, and that was found by just, I wasn't looking for it, it happened. I found it by accident.

Bromberg:

Yes, you wrote up a citation classic on that which was very nice.

Hahn:
Yes, did you read it?

Bromberg:
Yes.

Hahn:
Well, that pretty much repeats what I've been telling you, I guess, some of it.

Bromberg:
Well, that's very good, and part of it is found by previous suppositions, which is always very good. You say that an interview has been a success if it......

Hahn:
Not one of making any specific measurement of any specific numbers. I usually look for phenomena and techniques and behavioral — general behavioral character, and I just work along like a tailor, you know, cutting a cloth, seeing where to go next. I don't quite know where the scissors are going to end, you know.

Bromberg:
You don't have a kind of research direction?

Hahn:
Not in the sense of wanting to understand the universe, no. I — Bomberg: Do your consultancies, do you fee that they have an impact on your work, the kind of problems you get interested in?

Hahn:
Yes, they do. Consultancy is an extremely provitable — for example, finding McCall. Having consulted him, I found him. Working with Brewer and being stimulated by other people's ideas or interests, just talking with somebody else who has another line of interest is good for you, because you get bigoted in your own, you get narrow minded in your own endeavors, and you become seedy, even though you might be productive, you might just be doing a lot of busy work and not knowing it. And not knowing that it's not relevant to somebody else's interests which are different and connecting you with a wider community that's more signficant. So consulting is extremely important. Bromberg; You got some government committee work, and I wonder, is that the same thing? For example, even at the very beginning, you were down here as consultant for the Office of Naval Research.

Hahn:
Yes.

Bromberg:
Was that anything —

Hahn:
— that was long ago at Stanford. The ONR had a program for memory. I remember looking at, it was a project run by Bloch and Warren Proctor, trying to apply spin echhoes for memory even then. At Stanford.

Bromberg:
So you were really somewhat involved even before the—

Hahn:
— yes, before, but it was sort of very preliminary stuff, looking at echoes. I remember that. I consulted for the Bureau of Standards. Ramsey was on the committee, and who else? He died. And I consulted for IBM for a long time. Bromberg; You're down here, for example, in 1959, special consultant, US Navy.

Hahn:
Yes.

Bromberg:
Is that anything of interest?

Hahn:
I spent a summer at La Jolla, where a group got together to investigate means for detecting submarines. Non-sound techniques, in other words, not using sound, and Bob Pound? was there and a number of people of various kinds, just a think tank.

Bromberg:
Did that have any spin-off for you?

Hahn:
Well, I wrote a small paper on it, on some ideas I had, in regard to the motion of sea water, using NMR. And then I also contributed some other things to an internal report on propagation of radio waves, something like that.

Bromberg:
Now, there are some people for whom DOD or government consultancies have been very important. To give you an extreme case, I was just down at Livermore yesterday interviewing Ray Kidder, and there the military work, the weapons work, forms the whole context.

Hahn:
That's right.

Bromberg:
Is there much relationship between your —?

Hahn:
Well, there was. Let's see— when I, I was a private consultant, when I was working for IBM. It may have been illegal. Maybe it shouldn't be recorded.

Bromberg:

Well, you'll get the transcript and you can do what you like with it.

Hahn:

In New York, while I was at IBM, there was a guy — I was asked to give some advice on using pulse techniques for microwave signal processing, storing the information in spins and then getting it back out somehow, and I remember Herman Carr was a sort of consultant with me, but that was a very brief thing. It wasn't anything that, violation of anything. It was sort of semi-confidential, but it didn't go anywhere.

Bromberg:

That was not government? That was another company?

Hahn:

It was a company, yes, but I think they in turn were contracting with the government.

Bromberg:

Another thing is Oxford. That's, is that just because they have such a?

Hahn:

Oh, Oxford.

Bromberg:

You were there many times.

Hahn:

I did an experiment with Nicolas Kurti, Professor Kurti on using magnetic resonance technique for measuring temperature, low temperatures, but that wasn't laser-inclined at all.

Bromberg:

So Oxford is really outside the —

Hahn:

It's outside the purview of your interests, yes. I want to point out, there's something interesting about the history of lasers. You know, when Pound, Bloembergen published the paper on radiation damping, this is NMR, they had in it the possible principle of the laser or the maser, where, you see, theyw ere coupling nuclear spins to a cavity or a circuit, and they were very close to getting the principle of the laser in that paper. If they would have asked the question can we use the spins to get energy, can we get energy from the spins to the amplifier? A very curious situation. You can develop all the principles of lasers just by using NMR formulae, just by iverting the levels and by looking at the circuit and overcoming the losses in the circuit, you can get everything that Townes predicted. You didn't have to use optics. You didn't have to use mirrors. It's the maser, of course. In other words, the maser could have been invented by Bloenbergen and Pound in —

Bromberg:

That's —

Hahn:

...rather than (crosstalk)

Bromberg:

I want to thank you very much.