

**INTERVIEWER:** Professor Kleppner, thanks very much for taking the time to come talk to us today.

**KLEPPNER:** Happy to be here.

**INTERVIEWER:** Let's start at the beginning. Tell me a little bit about where you grew up, how you grew up, your early years?

**KLEPPNER:** Well, I grew up in the suburbs of New York City. I lived in a little town called New Rochelle. And I had a sort of a normal, but a very happy childhood. My family had no particular background in science or engineering or mathematics, although I had an older brother who liked mathematics. But when I was a kid, I liked to build things. And I built lots of things, electronic devices. I sailed. I learned how to do woodworking, which has been a hobby of mine through my life.

And then in high school, I had a wonderful physics teacher, Arthur D. Hussey, and he kind of reinforced my interest in physics. So by the time I graduated, I had a very well-defined interest in physics. And it's certainly very helpful if you know what you want to do at an early age.

In those years, physics was extremely fashionable. These were the post-war years. And physicists were considered heroes for having brought the war to a quick conclusion with the atom bomb. And more importantly, although it wasn't as well known, for the development of radar. So physicists were public figures then. And it was easy to be carried along in this spirit of enthusiasm and appreciation for science. So that sort of the background when I went to college, which was Williams College.

**INTERVIEWER:** So tell me a bit about your parents? What did they do? And presumably they were supportive of this interest that you had?

**KLEPPNER:** Well, they were always supportive of it. And they were proud of it, but they didn't know very much about it. My father came from a Viennese family which immigrated to the United States around 1906. So he grew up really pretty much in poverty. And he went into the advertising business and ran a little advertising agency, a Madison Avenue advertising agency, although all my recollections have little to do with *Mad Men*, which is very popular series on that right now.

But he was a very kind and generous father, not only to us, but he helped bring in many relatives from Germany during the '30s. And was someone who was very well-regarded. But his education had been really pretty well focused. He did not have broad intellectual interests, although he was certainly an intelligent person.

My mother grew up in New Jersey and went to Barnard College. She was a lovely woman and a delightful person in every sense. And she was widely read, had a wonderful sense of humor, and sort of made our home a happy place. So we grew up, we being my older brother, myself, and my younger sister, in a home which was really a very happy home.

**INTERVIEWER:** So you mentioned the way that physicists were treated as heroes or celebrities at that point, not too long after the close of World War II, which must have captured the imagination of a bright young high school student at the time. Do you remember what kinds of problems or puzzles or things like that really captured your attention as a teenager?

**KLEPPNER:** Well, as a teenager I just liked building things. And that turned out to be very useful when I went on to become an experimental physicist. We'd build mechanical things. I did not build a telescope. I did build several rowboats and I had a sailboat. So that was a very important part of it.

And I had a crystal radio. So I could listen to radio over earphones. And the thought that the signals were just coming out of the atmosphere, I remember thinking totally remarkable. And actually, I still do. In fact, the idea of the electromagnetic field, although it's very well understood in physics, always seems like a miracle to me.

I had a close friend. We liked to build devices. One device I built was a slot machine. It was a box. And there was a window on the front of it. It was filled with nickels. And there was a little cup underneath it. And there was a lever and a place where you could put the coin in on the top. And there was a sign which said, "Nickels only." And we left that in the classroom. And you put in the nickel and pull down the lever and it lights up and says, "Thank you." So that's the kind of prank, a very benign, sort of harmless, humorous prank. But that's something that we took great delight in making.

**INTERVIEWER:** So you mentioned that you attended Williams College. Tell me a little bit about your undergraduate experience and how your intellectual interests developed during that time?

**KLEPPNER:** Well, I had a wonderful undergraduate experience. Although for reasons I'll explain, I actually left college early. When I went to Williams, I liked physics, but I wasn't all that sure about my abilities in it. In high school I was a good student, but not a star student by any means. And so I was uncertain as to how I would do.

Well, it turned out during my first term at Williams, I did very well in physics, which pleased me and surprised me. So that was a very nice, positive experience at the beginning of college.

What was even more important for me at college is I came across a number of teachers were very supportive of me. I went to Williams with an idea in mind that I wanted to build a device, sort of a forerunner of cybernetics. I just wanted to show that you could make a machine which could do anything, a programmable machine. Now, this was before the days of digital electronics and such. So in that sense, it was sort of an advanced idea, but what I was making was sort of a mechanical kludge.

And the physics department there gave me a little laboratory space in which to do this. And one of the teachers, Howard Stabler, who was quite expert in the electronics, spent a lot of time teaching me about vacuum tube electronics, so I could use vacuum tubes rather than mechanical relays in the device.

And this was very important to me, just doing this, all on my own. I remember the pleasures of working there late at night, sometimes over vacation time. This joy of just making things work, all by oneself, was something the college gave to me. So I was very appreciative of that. And the other teachers were good also.

What wasn't so good about Williams for me was the social atmosphere. At the time, it was something of a playboy school. It had that reputation. And I think the reputation was justified. Life there was--- nearly everyone centered on fraternities. And the fraternities were very cohesive, social institutions. I felt that they were archaic and in a sense anti-intellectual. And I know that many of the faculty agreed with me.

So I went to speak to the president of Williams in my sophomore year because it seemed to me that they must recognize this problem and were planning to do something about it. Well, the president of Williams, James Phinney Baxter III, to his credit, immediately made an appointment with this freshman who just wanted to come in and talk with him. But it turned out that he thought that these were the soul of Williams College. So there was a real misalignment of interest between this freshman or sophomore and the college administration.

So I thought I ought to leave, on principle. And actually my professors that I talked with sort of understood this and agreed with it. And we figured the best way to leave would be to graduate early. So I accelerated and graduated in three years, rather than four years. And then I got a fellowship to Cambridge University. So I went to Cambridge University for a couple of years to make up for my slightly truncated experience at Williams College. So that worked out very well.

I should mention also at Williams, an interest which I did not have before going to Williams, which really flowered there, was an interest in English literature and poetry. I had not really been exposed to that before. And that was very exciting for me. So I not only finished the physics major, I think I may have technically completed an English major also.

So looking back, I was really very busy in those years. And looking back, everything is in a rosy glow. Of course at the time, one is always suffering from anxieties and worries. But the nostalgia is really a very-- well, nostalgia is dangerous. But it's a very nice practice to indulge in. So I'm very nostalgic about my undergraduate experience at Williams College.

**INTERVIEWER:** So you mentioned that you went to Williams. I guess, first of all uncertain about exactly how you would do in physics. And secondly, with just a very strong desire to build things. But you came out of your undergraduate experience at both Williams and Cambridge more focused on physics as a profession. Was there kind of an aha moment along the way where you made that sort of determination?

**KLEPPNER:** Not really. Somehow, even from college, or from high school, I sort of felt that I was destined to spend a life in physics. And I never gave it much more thought about that. I was not terribly reflective. There are lots of other things one can do. And to some extent, it's too bad to narrow your focus too early, not to think about other directions which I might have gone. So you lose something for these early loves. But you gain a lot too, namely you have a direction and you know what you want to do. And if you're lucky, you're able to do it.

**INTERVIEWER:** So tell me a bit about then going to pursue your graduate studies?

**KLEPPNER:** Well, before I left Williams, I'd applied to Harvard University and was accepted there. I applied to Harvard because one of my principal advisers at Williams, a physicist named David Park, who we still see, he's quite elderly now, but I talked about where to go for graduate school. And his advice was very simple. Go to Harvard. So I applied to Harvard and I was accepted.

And then I got the Fulbright Scholarship. And they agreed to defer for two years. So when I went to England, I had a place waiting for me back in the United States. So I came back to Harvard.

But this is all terribly naive. There are other good schools too. I mean his advice was good advice, but it was not uniquely good advice. And to tell you how naive I was, when I was at Cambridge University, which has a tutorial system, my tutor in the second year was a young physicist named Kenneth Smith. And he told me about the research that he was doing with a recently developed technique called atomic beam molecular, or atomic beam resonance.

And in describing this work, he told me about how you can use this to measure properties of nuclei. And he showed me a little book, which I read, called *Nuclear Moments*, by a chap called Norman Ramsey. And he also told me about the possibility of making an atomic clock and that you might be able to make a clock which was accurate enough to measure the effect of gravity on time, which sort of excited me. I didn't do anything about it. I didn't rush out to say, that's what I'm going to do. But I remember that idea being fixed in my mind then.

And when I went to Harvard University and was walking down the hall, I saw the name on the door, Norman F. Ramsey. And I thought that's amazing, for a couple of reasons. I didn't realize he was at Harvard. I hadn't even bothered to look at the faculty.

Of course in those days, you didn't Google the faculty. You have to write some letters to find out who was doing what. But I was immensely pleased to see a name that I recognized. And somewhat surprised that a book had a real author to it. I'd never met an author before.

So I came with a predisposition for Norman Ramsey. And fortunately, I came along at a time when he had an opening in his research group. And what's more important, he'd just had a very good idea. So that's sort of how my career at Harvard got launched.

**INTERVIEWER:** So let's continue that story. Tell me a bit about the work that you did together and what resulted?

**KLEPPNER:** Well, Ramsey's work is in the subject called molecular beam magnetic resonance, although his interests turned out to be much broader than that. And one of the applications for the technique that he developed, for which he was awarded the Nobel Prize, the separated oscillatory field method, was that it enabled the development of atomic clocks.

Now, the clocks it enabled, actually he did not work on those himself there. In fact, the first practical atomic clocks were made here at MIT by Jerrold Zacharias. But he was very interested in clocks and precision measurements.

And he had another idea for improving the precision of atomic clocks. It ultimately became the hydrogen maser. And the idea was a rather radical idea. And at first-hand, it sounded nutty. And he agreed it said nutty. But you put in some numbers and it looked plausible.

The art of making an atomic clock is that you would like to observe a natural frequency in an atom and to govern the rate of an oscillator to equal that natural frequency. So the oscillator becomes the ticking device for your clock. Now, the precision with which you can do that depends how long you can look at the atom. The longer you look, the more accurate it can be.

If you're trying to compare two wrist watches and you only have one minute to do it, you can't compare them to much accuracy. If you can compare them for a year, you obviously get much higher accuracy. So you want to look at the atoms for a long time.

Well, in those days, these atoms were all on the fly. They're moving at thermal speeds. And so they go by pretty quickly. You try to make the apparatus as long as possible, but there are practical limitations on the length. Well, Ramsey had the idea you might be able to keep the atoms around a long time just by having them rattle against the walls of a bulb or a vessel.

At first glance that sounds kind of nutty, that here's this atom, you're trying to look at its frequency with very high precision. You have to treat them very delicately. And you're going to let them hit walls and rattle around. It's like trying to watch alarm clocks by batting them back and forth. But it turns out that for the hydrogen atom, this might work. And this was the idea that we pursued.

Now, we couldn't pursue it right away for technical reasons. You needed to have a pretty good idea that it would work well to do the experiment. And the reason is, in a maser if it doesn't work well, you don't see any signal at all. There's no way to kind of start from zero and work up. So we did a preliminary experiment, for which I did my PhD, where we tried the idea not on hydrogen, but using cesium atoms.

And I built an atomic beam apparatus. And we put a little storage region in the center of it and let the atoms rattle around. And we could see that they would rattle for a few times. And on the basis of that, we could predict that the hydrogen maser would work. And at that point, we went ahead and made it. So my PhD work was actually for this preliminary experiment on cesium atoms in a box. And then as soon as I got my PhD, I became a postdoc, and we built the hydrogen maser.

**INTERVIEWER:** So you stayed at Harvard beyond your PhD?

**KLEPPNER:** Yeah. I said I stayed on. I was a postdoc for a couple of years. And then I became an assistant professor at Harvard, so I started teaching also. But during this period, I was dominantly working on the hydrogen maser.

**INTERVIEWER:** So I wonder if you have any thoughts about, and this is maybe something we'll return to later, but about collaboration in science? I mean it sounds as if you benefited from some really terrific mentoring--

**KLEPPNER:** Yes.

**INTERVIEWER:** --at that point in your career?

**KLEPPNER:** Yes, very much so. I mean starting in high school--- Mr. Hussey was great. He gave me permission to work in the labs there, sort of out-of-hours. One of the out-of-hours time was when the whole school was having a pep rally, and I wasn't that interested in cheering football. So I stayed up and worked in the lab. And the high school principal, he noticed that I was in there and called me in and gave me a dressing down for lack of school spirit. But Mr. Hussey was not at all disturbed.

So this was wonderful mentoring. I mean he gave me the signal that you should follow what you think is important. And also I respected him highly because of his intellectual abilities. He was a victim of the Depression. He never went to graduate school. But he knew in the '30s, that quantum mechanics had been developed. And he didn't really understand it. He knew he didn't understand it, but he knew about it. And he told me about that. So that sort of put a bee in my bonnet on that.

Also in those days, calculus was never taught in high school. And I was interested in learning it and he taught me some. So he devoted time to me. So he was a wonderful mentor. And then as I've indicated, at Williams College, they were very supportive in encouraging what I wanted to do and basically making me feel happy. So that was good.

And it was sheer good luck that I bounced in on Norman Ramsey when I did because he was a great mentor in every respect. He was a role model.

He died very recently. And I've just been going back, reading his oral history transcripts. And from what all I know about him, he was a truly exceptional person. He was one of the towering figures in 20th century physics. So I was very fortunate to work with him. And we were close friends for life. So that's just a wonderful mentoring relationship.

**INTERVIEWER:** Just to pursue that for a minute, maybe it's hard to answer this in a constrained format, but what made him such a great mentor and a great scientist?

**KLEPPNER:** Well, first of all, I can't say what made him a great scientist. He was a great scientist. But he had wonderful personal characteristics. He was very generous. He was very modest. And he had this marvelous personality. He was an extroverted person. He was energetic. He made an impression on everyone who met him. So he had these unusual characteristics.

And he was very broad. At the same time he was running his molecular beams group at Harvard, which was a very active research group, he was involved in providing scientific leadership to the particle physics community. He personally was really essential in the creation of Fermi Lab.

And I didn't realize it at the time, it's only reading back now, that I saw all that he did. But he brought together warring scientific communities. He was the one person that they could turn to. They had confidence in him because of his absolute honesty, his absolute integrity, his fairness. And he had this very unflappable disposition. He was always good-natured. I never heard him speak a word of anger in life. So as a personal role model, as well as a professional role model, he was exceptional.

**INTERVIEWER:** So you stayed on at Harvard for several years after your PhD. You worked on the hydrogen maser. Tell me a bit else about your last years at Harvard and then coming to MIT, how that came about?

**KLEPPNER:** Well, first of all--- it came about--- Harvard didn't give me tenure. And I've since learned that there was a big battle in the department over that. But in fact, it would have been unwise for me to stay at Harvard I think. Because no matter how generous and good-spirited Norman was, he cast a long shadow. And it was good for me to be just the right distance. I see a lot of him. We continued friendship. But I was in a different institution.

I started some other research work while I was at Harvard. But I was still continuing with hydrogen maser experiments. And then I became seriously involved with teaching at Harvard too, which was very important to me. Well, it still is, although I'm not teaching in the classroom anymore.

So when I came to MIT, I had a pallet of experiments that I wanted to pursue and some ideas about teaching that I wanted to pursue. And the transition was very simple. I just crossed town. In fact, we had bought a little home in Belmont that we still live in. So we didn't even need to relocate our home.

**INTERVIEWER:** So you mentioned that you had a pallet of experiments and also some ideas about teaching that you wanted to pursue. Let's take each of those in turn. I'm very interested in hearing what you came to MIT and what you, at that point in 1966 you said, what you were hoping to accomplish?

**KLEPPNER:** Well, there was one experiment, a basic physics experiment. You can use the maser as a clock. But hydrogen is an interesting atom and there are a lot of other things that you can measure with hydrogen. And so we started an experiment to measure the size of what's called the magnetic moment of the proton.

This is important because in nuclear magnetic resonance, its protons you're looking at. And for many experiments, nuclear magnetic resonance was actually used to calibrate the magnetic fields. And one wanted to know the value very accurately. And we could do that with a hydrogen maser.

So I built a new type of maser at MIT. It worked in a strong magnetic field, and set out on a series of sort of basic measurements on hydrogen with a maser. And then I'd had other ideas too about a totally different area, sort of atomic scattering experiments. The techniques for atomic scattering were advancing rather rapidly then. And they were about to advance even much more rapidly. So with a student who started working with me at Harvard, David Pritchard, he came with me to MIT and we worked together on the scattering experiments. So those were the two areas that I started out working on at MIT.

**INTERVIEWER:** And you mentioned also teaching being very important to you and having some sort of things you wanted to pursue in that area. Tell me about that.

**KLEPPNER:** Well, I had been giving one of the freshman courses at Harvard. And at this point, I'd gotten some rather what I thought, in fact I still think, are clear ideas on how you go about teaching freshman physics. This is an ancient subject. Everyone has their own ideas on doing it.

But what I did when I got here, was I started a new course. And I started it with a colleague here, assistant professor, Bob Kolenkow. And this course, it was called 8.01S. It's now called 8.012. And it's still given. And it was designed for a rather special audience, namely very bright, ambitious MIT freshman.

So it wasn't taken by the whole class. But it was taken by 150 or so of the students. And it was a course which tried to teach the very basic ideas, but at a much deeper level than they're normally given. And we worked very hard on that course and developed it over a number of years. And I think it was developed successfully, because the course is still being given very much in the form that we gave it. And the teachers who teach it, enjoy giving it.

And from our first lecture notes, we developed a textbook, which was published in 1972, Kleppner and Kolenkow, which is still in print and still being used. And at the moment, we decided it's time for a second addition because the first edition is roughly 40 years old. But we're very proud of the book. When I visit people, I often see it on the shelf. That makes me feel good.

So teaching can be very rewarding. And that was one way it was rewarding. However, the book is a little bit old fashioned. We decided we shouldn't talk about slide rules anymore.

**INTERVIEWER:** So when you came to MIT in the mid-1960s, what was MIT like at that point? And what was the physics department like?

**KLEPPNER:** Well, first of all, the physics department--- it's a large department. One reason it's large is it encompasses astrophysics and also condensed matter physics. In many schools, astrophysics and astronomy are a separate department and condensed matter physics and materials science are totally in a separate department.

So this department was much broader than in other schools. But that makes it large. So there's always a compromise. When things are large, you lose the sense of cohesiveness that you can with a smaller department.

So it naturally breaks up into different areas. And so I was in sort of the atomic physics area of it. But I must say then that was slightly isolated. It's only in retrospect that I realized that. At the time, I was too busy doing things to appreciate the significance of that.

The department later reorganized, at least we're grouped together in divisions. And the atomic physics area, particularly in the past few decades, has grown. So it's an active community of its own.

And the department itself, I think, in spite of its size, has always been a very cohesive department. We meet together once a week for lunch, which everyone enjoys. And I think it's a pretty essential thing to do with the department of our size.

But my experience here has always been a happy one in the department. For example, when someone is proposed for tenure, I have never seen the proposal contradicted sort of purely on the grounds of intellectual partisanship. If a person is really good, you can tell that no matter what the field is.

There are lots of ways of judging candidates. And MIT's way is a very thorough way. And if the candidate is judged well, I've always seen everyone sort of rally behind him.

That's unusual. In many departments, you have factions and they battle each other. So my experience in the physics department has always been a happy experience here, and particularly in recent years, because my own area has grown so well and is such an exciting area. And now it plays a much more visible role in the department than it did when I first came here.

**INTERVIEWER:** So who were some of the notable colleagues or collaborators, mentors from that time at MIT when you first arrived?

**KLEPPNER:** Well, one of the things that I inherited or sort of took over when I came here, was a graduate class which you've been taught by George Benedict, who was working on well, on the optical and other ways to study the properties of matter. And he had a course, which was a very interesting course for me. And I adopted some of the material from that course when I started my own atomic physics course here.

The provost when I came was Charlie Townes. Now, the laser had just really been invented. In those days, it was still very young and developments were going rapidly. I must say I think I slightly missed the boat in not latching onto lasers sooner, because it rapidly became apparent that they were sort of powerful tools for doing all sorts of atomic experiments. But I do remember going to a number of his group meetings and listening to about the work going on there.

A young physicist, Michael Feld, was in that group, who later became a member of the department. And sadly died, I guess about a little bit more than a year ago. So he's someone else that I interacted with.

**INTERVIEWER:** So let's also talk a bit about how your research interests and the problems that you worked on evolved once you had arrived at MIT and some of the next challenges that you faced?

**KLEPPNER:** Well, the next big area that I went into was the subject of Rydberg atoms. A Rydberg atom, they're just like hydrogen atoms. In fact, they can be hydrogen. According to the Bohr theory, the Bohr model, which is no longer regarded as a rigorous way to look at atoms, but is very suggestive, you think of the proton, it's a planetary model, and the electron goes around like that. And it can go around in a bigger orbit. It can jump from one orbit to another and emits some radiation. And you can account for the spectrum that way.

Well, you can label these orbits, the first one, the second one, the third one, the fourth one. In the early 1960s, radio astronomers found radiation which came from hydrogen atoms going from say the 100th to the 99th orbit, a totally different world. It turns out these are enormously large in size, which is why you don't see them in the laboratory. They fall apart very easily. And the radiation that they emit is not in the optical regime, but it's in the microwave regime.

And they were discovered. And that was while I was still at Harvard. And I remember Ed Purcell sitting down with some of us, some afternoon, just thinking about the properties of these atoms, which are really, everything is different just because of this huge geometry. And thinking at the time, wouldn't it be nice, wouldn't it be fun to study those. But there was no way to make them in the laboratory at that time.

Well, I mentioned that the laser had been developed. And in the early '70s, I realized that you could use these lasers to create the atoms in the laboratory. And we had a weekly group meeting. And we started talking about those.

And then it was about I think maybe 1973, one of the postdocs here, Rick Freeman, went off to an APS meeting and came back and said, well, some other people are thinking about these too. We ought to get to work quick. And so we got to work quick.

And we started out to try to make these atoms in the laboratory. And it turns out they were quite easy to make. And what's more important, they were very easy to do to detect. The problems with atoms is that individual atoms are very difficult to detect. And the Rydberg atoms, well, they give out radiation, but not very much. That's a hard way to detect them.

However, the way you can detect them is that these atoms are so large, they're so weakly bound, that you apply a little electric field, you can pull them apart. And you have charged particles, which are very easy to detect. So we started to work on that. And the experiment started working. I think it was in maybe January '75, that we got the first results.

Now, I'd planned to take a sabbatical leave in Oxford in that spring. And in fact, we got a house there and my family went there. But these experiments had just started working. And I felt I just couldn't walk out at that particular moment. So my wife, who is very supportive and understanding, said, come along when you can. So I stayed behind. And we worked on these atoms. That was a very intensive period.

In fact, since we rented out our house here, I actually set up a home in a lab office and lived there for about six weeks. And I certainly don't recommend this to anyone. MIT is a lovely place to do research, but it's a rather grim home. But we lived there and got the experiments going. And then I went to England and joined my family.

**INTERVIEWER:** So do you have any, this might be a good time to ask--- I mean, starting with this sort of very early interest you had in building things, creating devices, and hearing a story like that, that kind of sort of commitment to seeing something through, what kind of advice would you give to experimental physicists or people who may be aspiring experimental physicists, more importantly, in terms of success?

**KLEPPNER:** Well, do things. If something interests you, follow through.

Now, I can't tell people what to do now because all the technology that I learned is now totally obsolete. I mean I learned machine shop practice. Well, machines are automated these days. I learned how to do vacuum tube electronics and vacuum tubes are museum artifacts right now. And I'm in awe of experiments, the techniques which are available now.

But graduate students, well, they're a little bit like young children. Young children just acquire language quite naturally and learn at a furious rate. Well, the graduate students I see these days seem to pick up all these new technologies. Optical technologies, electronics technologies, data processing, all those seem to be sort of second nature to them in a very short time. So I can't tell them how they should go about doing it.

But I think just the idea of pursuing your interests and trying out new things. I mean that's sort of a general principle.

One thing I find regretful is that there's so much schlock around, science kits where everything is done for you. I don't think children can learn that way. When I was young, we had chemistry sets and you could make things smell and burn. And of course, we live in such a litigious society right now, that nothing possibly dangerous can be done. So we've lost all that.

On the other hand, kids can get access to NASA data, astrophysical data online, and just do things in very different ways from before. But I think it's just, how can you teach a person to be energetic and enthusiastic? You can just try to support it when you see it.

But what they should do, I can't tell right now. I mean it could be that some young person can come up with some very important way to try to get a better hook into the problem of climate change, for example. Even though there's a huge community working on that, there's always room for new ideas and good ideas. So my advice is, well, all I can offer is platitudes rather than useful concrete suggestions.

**INTERVIEWER:** So I think you mentioned it was about 1975 that you succeeded in creating the Rydberg atoms in the lab. Was it '75 or so?

**KLEPPNER:** I'll have to check the date. It may have been '76 when that first paper was, I think.

**INTERVIEWER:** Okay. So all right, what did you tackle next? What drew your interest next?

**KLEPPNER:** Well, it turns out the Rydberg atoms are a world of their own. And they led into many new directions. The first was just trying to study some of the basic atomic properties of these atoms. And then I got particularly interested in the structure of these atoms when you put them in electric and magnetic fields. It's quite interesting. If you put the atom in an electric field, it can come apart. But before it comes apart, it shows interesting structure that you can understand.

And you put them in a magnetic field and you're in a new world. The effect of a magnetic field on the spectrum of the atom is called the Zeeman effect, which was discovered around 1897 or 1898. And turned out to be very important as one of the first pieces of evidence that there were charges inside of atoms. So the Zeeman effect was historically an important spectroscopic tool.

But when you put these Rydberg atoms in a magnetic field, the Zeeman effect is just totally blown to bits. You're really in a new world. And we were trying to understand that world and trying to really understand the spectrum. It's such a simple system, an electron, and the proton, and the magnetic field. But it turned out that the theoretical analysis was very challenging.

And we did have some theoretical collaborators in Paris, who were very helpful in this. And others got interested in it. And then it emerged that these atoms made contact with a subject which had really only recently emerged, which is the problem of chaos. Of course, MIT has very strong connections with that. It was Ed Lorenz over here, who really put chaos on the table of important physical problems.

Well, it turns out that these atomic systems, if you look at the classical motion, the motion can become chaotic. And the question is, what is the quantum structure of the atoms? And so the connections between quantum mechanics and chaos were very provocative and elusive. And we started working on that.

We learned a lot from another group in Germany, Karl Welge, who was working along the same lines. And so we became very involved with that for a number of years. So the next big item on the agenda there was quantum mechanics and chaos.

But I also resurrected another old idea, which you could do with the Rydberg atoms. And this is a subject which has become to be known as cavity quantum electrodynamics. And the thought is this, atoms can exist in various energy states. If you have an atom in one of the high energy states, it will inevitably, all on its own, radiate and go down to the lowest possible state. That process is called spontaneous emission.

And spontaneous emission is a very basic process in physics. It's a very important process. For instance, if you're looking at noise in quantum mechanical devices, the ultimate source of noise is spontaneous emission.

I pointed out, actually even before coming to MIT, that in principle you could stop spontaneous emission. If you put an atom into, I'll just call it a tuned cavity, you could prevent the atom from radiating. And the reason for doing that, again was this reason of measuring. You'd like to keep the atom around as long as possible to make measurements on it. And in the optical regime, your measurement time is always limited by the spontaneous radiation rate.

So I proposed that if you put the atom into a cavity which was too small, a little cavity, it's a chamber which is say, all conducting. So radiation can't get in or can't get out. And if the cavity is smaller than the radiation wavelength, there's no way the atom can radiate. That was the idea.

But there was nothing you could do about it for years and years. But then when we had Rydberg atoms, we could start doing something about it.

And we did an experiment which actually demonstrated that. One of my graduate students, Randy Hulet, demonstrated that in our lab. It was called inhibited spontaneous emission. And in a sense, this was a grandfather experiment, because this whole subject of quantum systems interacting with, resonated with classical systems, is now called cavity quantum electrodynamics, and is of very active interest now.

So we did the original experiment on that. And then we went on to do other experiments on it. Other groups became very interested in it. And so that subject has flourished.

**INTERVIEWER:** To return just briefly to the first thing you mentioned, chaos theory and the relationship with sort of quantum theory, again, I don't know if there's any way of helping me to understand it in the time we have, but why is that so important, chaos theory? I mean what we do what you mean when you say that and why is that important?

**KLEPPNER:** Now, that's a very good question because the subject of quantum chaos has been controversial. One of the real leaders in that, Michael Berry, a marvelous British mathematical physicist, had tried to introduce the term quantum chaology, because in fact there is no room for chaos in quantum mechanics.

The way I like to pose the problem is this, every one, every physicist believes in quantum mechanics. In a sense, quantum mechanics underlies classical mechanics. I mean classical mechanics is the mechanics of Newton and large bodies.

Now, it's well-known that quantum effects are only apparent when you're looking at very small bodies. Nonetheless, the belief is that the quantum laws are fundamentally correct. And that in principle, one ought to be able to describe classical phenomena using quantum ideas.

That's a very important principle. When Bohr first proposed his model for the hydrogen atom, he used that principle. He calls it the correspondence principle. If you go to very high quantum numbers, systems just start looking classical. Like these Rydberg atoms, when they radiate, they radiate, you can calculate all that just classically and get pretty good answers.

So this is the question, can you really describe classical motion quantum mechanically? Well, chaos puts that to the test. Because chaos is such a complicated phenomenon and can you really describe chaos quantum mechanically? So that's the essence of the challenge there.

Well, there are the number of answers to it. The basic answer is that in chaos, the smallest change to a system, say to the initial conditions of a system, can produce large effects later on. That's the so-called butterfly effect, that a small perturbation can lead to huge differences.

Well, quantum mechanics has a small size limit to things. Below a certain size, it doesn't make any sense to say, I'm going to change the initial conditions, because you can't establish them in the first place. So in that sense, quantum mechanics does not permit chaotic behavior in the classical sense. Nonetheless, there are many features of the quantum motion which reflect sort of chaotic behavior, the way functions can start looking in certain characteristic ways.

So I think the subject has been quite fruitful, but there's no simple answer to the question. And in fact, in chaos essentially all systems are different.

But the principles of quantum mechanics, they can be applied. Maybe the best way to put it is, you can apply them to understand chaos, but they're wildly inappropriate. It's not the language. It's not only the language of choice, you'd be a fool to try to describe the classical chaotic motion using the language of quantum mechanics. It's the wrong language.

**INTERVIEWER:** Let's turn now to Bose-Einstein condensation. I guess I'm just going to start with the most fundamental question, what is it? And how was it that your research interests turned in that direction?

**KLEPPNER:** Well, it's a good question, because that was the question I asked Tom Greytak. There was an article published in *Physical Review Letters* in April of 1976, by Lou Nosanow and Bill Stwalley. And it pointed out the possible Bose-Einstein condensation of spin-polarized hydrogen.

Well, I read this article. And what it pointed out was that if you have hydrogen atoms which are spin polarized, which means the electrons in the two atoms point in the same direction, these can bounce off each other--- these atoms. But they will not form molecules. And so they form an atomic gas. And that atomic gas has unusual properties, namely you could cool it to absolute zero temperature and it would still stay a gas. It would never condense.

Now, the reason that was interesting is if you can cool atoms, if you can get them cold enough at high enough density, they undergo what's called this phase transition, quantum mechanical phase transition--- the Bose-Einstein condensation. And what it means is, if you have a gas of all these atoms moving around like individual particles and all of a sudden they change their phase-- well, I shouldn't say all of a sudden-- as you cool them down, they start dropping into the lowest energy state in the system.

They stop behaving like individual particles. It becomes like one giant quantum system. It's one quantum system which has many particles in it. The easiest way to describe it is they just come to rest. Now, they can't be actually at rest because of the uncertainty principle. But they go into the lowest state which is available. And this article pointed out that you might be able to see this. Now, this had never been observed in a gas.

And I read the article and I thought what they were requesting was kind of preposterous. It meant hydrogen at much lower temperatures and at higher densities than had ever been seen before. So I thought it was a very unrealistic proposal. And kind of annoyed at my friend Bill Stwalley for making such an outrageous suggestion, at least without saying well, this is outrageous suggestion. But they didn't discuss that at all. They just showed the science.

Well, I had a conversation with Tom Greytak. Tom's background was in low temperature physics. It was in liquid helium, studies of liquid helium. And he asked about that article, whether it might be possible to do that? And I said no, that was totally absurd. And he said well, that's unfortunate because Bose-Einstein condensation is really quite interesting. And I asked him what is it, just the question you asked. And he explained it to me.

And so we had a series of conversations, in which they would start out by pointing out why you couldn't do this and then figure out a way why you could do it. I realized that some of the techniques which we used in making the hydrogen maser, namely storing atoms in a vessel, might be applied to make spin-polarized hydrogen, to get to Bose-Einstein condensation.

The art of storing hydrogen atoms in a vessel is to prevent it from sticking to the walls. And at room temperature, there are certain materials that hydrogen doesn't stick to-- Teflon, for example. Well, in making the hydrogen maser, we had often thought the ideal thing on the wall would be a film of liquid helium, because that's very non-interactive.

Well, now we want to get to hydrogen at low temperatures. And so maybe the possibility was you could store it in a chamber with liquid helium, covered walls. And we thought of a very simple way of using a strong magnetic field. So we started out playing with it. The first thing we did was just to see whether you could get atomic hydrogen, whether you can handle it at cryogenic temperatures. That had never been done before. So we started out just sort of playing around with it. It was one of the kinds of things you do in a lab. It's sort of amusing. Maybe it'll lead somewhere, maybe not.

So we didn't stop all the research to set out to see Bose-Einstein condensation because the idea was too unproven then. But it was a clue that we decided to pursue.

So that's how the work started. And it turns out we could cool it down. A student who had just gotten his PhD and wanted to stay on to do some more research for awhile, Bill Phillips, worked on these early experiments. I mention him because he later received one of the Nobel Prizes for developing laser cooling. And it turned out that ultimately laser cooling was a much better way to get to BEC. But at the time, he was interested in, well, he has good taste. He spent some time working on that.

So we were working on this. And extremely enthusiastic because when you start looking at what you want to get to BEC, it turns out that it looked to us like hydrogen was the ideal atom. Because of its low mass, it turns out it condenses at a higher temperature than any other atom would at the same density. And another thing is that the atoms did not interact. They're very much like ideal particles, which we thought was a wonderful advantage, but turned out to be almost a fatal disadvantage.

So we started out and we got a little seed funding. We got some seed funding. NASA was interested. There had been proposals for using spin-polarized hydrogen as a rocket fuel. I mentioned that when the atoms come together, if their spins are like that, they don't form molecules. But if you flip them over, they can form molecules. And they give out a huge amount of energy.

So if you look at the energy per unit mass, which is a crucial factor for rocket fuels, it turns out spin-polarized hydrogen is by far the best rocket fuel. I calculated that you could make a Saturn V rocket I think about maybe 15 feet high if it were really fired by spin-polarized hydrogen. But you can't do that because it just isn't stable at those densities.

But anyway, I mean NASA had done some studies and then they gave some seed funding. And then as soon as we got our very first successes, that program ended. But we got other funding, ultimately from the National Science Foundation to carry forward the project.

Now, a few other groups got interested in this. Particularly Ike Silvera and Jook Walraven at the University of Amsterdam sort of independently started working in this direction, that Walter Hardy at UBC became very interested in the basic science of this. So we had a number of groups. And we learned a lot about hydrogen in the next seven or eight years.

And then we came to a stopping point. We realized that at the densities we were aiming for, hydrogen would not be stable. So we needed to get a totally new approach to it. The idea of storing it in a bottle covered in liquid helium was not adequate. We had a very good postdoc working with us, Harold Hess. And with him, we thought of trapping the atoms in a magnetic bottle.

But the question is then, how do you cool the atoms? And Harold had the wonderful idea that you could cool them just by evaporation. Namely, you could think of these magnetic fields as making a potential well, sort of a hole in the ground if you like where all the atoms are kept by gravity, only it's magnetic fields. If you lower the fields, the very fast ones escape. And so the average energy is lower. So you very carefully lower the fields and the temperature goes down and down and down.

The nice thing about this is that in principle, there's no limit to the low temperature you can get. For ordinary refrigerants, as you pump away on them, their temperature goes down and so does the vapor pressure. At some point, there's just no more particles coming off. But in this forced evaporation, in principle you can go down arbitrarily far.

So we started working on that principle and did trap some atoms. This was about 1988 or so. The problem then that we had to face was how to know what the atoms were doing? Up until that point the way to know that you had trapped atoms was you start them flipping and they all burn up. And you get a lot of energy coming out. And that's a very nice big signal, but it's rather destructive. Your atoms are gone. And there's no way to look for Bose-Einstein condensation.

And we decided to use laser spectroscopy to see the atoms. Now, I mentioned in many respects hydrogen is the ideal atom. But experimentally, it's a very awkward atom. It's not easy at all. In fact, it's rather heroic to be able to see the atoms using laser spectroscopy. So we set out on a program to do that. And that was hard work.

Meanwhile, Bill Phillips and some of our other friends, Steve Chu and Claude Cohen-Tannoudji, who all received the Nobel Prize for the accomplishment, I think in '97 or '98, invented laser cooling. I say others were working on laser cooling too, but I think the prize quite properly went to them as playing the principal role in this.

This gave a totally different way to cool atoms, and not hydrogen atoms, but other atoms. And at first, those experiments they seemed kind of interesting and amusing. And it took a couple years for people to realize that they might work well enough to get to Bose-Einstein condensation. So the race was on for Bose-Einstein condensation using the laser cooling methods.

Meanwhile, we were working away in the lab with our experiments. But our experimental methods by any objective standard were not very good. Very complicated, these low temperature apparatuses are extremely complicated. The laser system was complicated. There was nothing elegant about what we were doing. But that's the way nature took us. So we continued working on that.

And I must say the students who were working were wonderful, because the experiments were very frustrating. If you get a leak in your dilution refrigerator, which happened now and then, it would be at least six months to really warm the system up, diagnose the leak, fix it, and get back into business. As I say, it's a very awkward technique. But we had some wonderful students and they were very tenacious.

Well, in 1995, Bose-Einstein condensation was discovered. First, I guess in June, at JILA in Colorado by Eric Cornell and Carl Wieman. Well, Carl had been an undergraduate in my laboratory here. He started in his freshman year. And he was sleeping in the laboratory in his senior year, either out of love of physics or a desire to save rent. But anyway, he was a terrific physicist as an undergraduate. Eric Cornell had been a graduate student of Dave Pritchard. I mentioned that Dave came with me to MIT. And he did very well and joined the faculty.

So you feel very proud of your students. But I sort of felt they'd kind of overdone things in this case. But it was a great success. They got it. And then a couple months later, Wolfgang Ketterle, whom Dave had brought here to MIT to work in this area, and then Dave stepped aside, so that Wolfgang could get a faculty appointment, he discovered it too.

So it was discovered. And it was a wonderful thing. Everyone who knew about it, thought this is pretty exciting. No one I think knew how exciting it really was.

But meanwhile, we were plugging on and feeling-- well, we weren't discouraged enough to stop. But we wanted to do it. And we had two students who really were exceptionally good, Tom Killian and Dale Fried. And I remember, because we were still being plagued by these breakdowns, and I felt so sorry for them because they're just working away and trying to get the thing going, and then finally in '98 it did go. So we got to Bose-Einstein condensation in hydrogen. So we were obviously feeling very pleased about that.

And there was a conference on the subject in Italy, that summer in Verona, one of the Verona conferences. And this happened just before the Verona conference. And I reported on it there. And everyone gave me a standing ovation, which was a very moving thing. But I realize that people knew that we'd been working on this for over 20 years at that point. And I think it's kind of embarrassing when you have this group plugging away year after year. You sort of feel sorry for these guys. And it's embarrassing to feel sorry for people.

Anyway, I think everyone was very much relieved. And we were certainly very much relieved to have finally succeeded in that. However, it turned out that although we did get there, hydrogen is not a good atom for studying Bose-Einstein condensation of quantum gases. We did pursue it some more, but then terminated the research because we were in a very different area of that world. And it wasn't going to be a really very productive area. So we finally achieved the goal after many years, but we didn't get there first.

However, obviously you'd like to get there first, but it's a wonderful field. And we take great pleasure in having at least-- we helped put it on the agenda. It was because we were working on it that people started thinking about it earlier. So we certainly contributed to the field. And Harrell Hess's invention of evaporative cooling is widely used in the field. So we made some technical contributions to it also.

However, I mentioned that no one really understood how much that field would grow. And it has been transformative I'd say in establishing a new relationship between atomic physics and condensed matter physics.

And there are groups all around the world working not so much on Bose-Einstein condensation anymore, but what we would call more generally, on quantum fluids. And there's two classes of gases, the Bose particles and Fermi particles. And they have basically very different properties. Well, it turns out the electrons are Fermi particles. So many of the properties of matter depend on the properties of fermions. And one can study Fermi particles, as well as Bose particles, with it. So the subject now, it's still in the exponentially accelerating portion of the curve. And I don't think anyone would have predicted that.

**INTERVIEWER:** I have a little bit of trouble understanding what it means that you have a gas that then starts behaving as one quantum system. Is there some way of explaining that to me that I would have any hope of understanding and what the consequences of that are?

**KLEPPNER:** One of the reasons that the subject works so well is because there's such beautiful diagnostic techniques for looking at these gases. Not with hydrogen, but with these other atoms, the alkali atoms, you can actually photograph them. And when you photograph these atoms, you keep them in a little trap. And it's rather small. You let them go. Well, they start falling, but they start expanding. And what you see when normal atoms expand is some go very fast and others go rather slowly.

If you make a plot of the distribution of speeds, it looks like a bell curve. And that's what the photographs look like. They look just like Gaussian bell curves. Then you get to Bose condensation and you notice that there is a little peak at the center. And then you go down far below the Bose-Einstein condensation, and you notice the atoms are no longer expanding rapidly like that, that most of them are going very, very slowly.

And the picture actually looks instead of like a broad distribution, you have a mountain wave standing at the center. Well, that wave at the center is composed of atoms which are in a single quantum state. And the reason that it expands so slowly is it's the minimum quantum state available. It has to have a little bit of energy. But that energy is just what the uncertainty principle says you have because you've confined them. So that's one way of literally seeing that the atoms are all in the same quantum state.

I don't know whether that--

**INTERVIEWER:** A little bit. Tell me also again a bit more about why this has been so crucial and why sort of study in this area is expanding exponentially? I mean why has this been such a fundamental breakthrough?

**KLEPPNER:** Well, it's fundamental breakthrough because it turns out that there are many theories of condensed matter physics which apply to these gases, but which you can study in fantastic detail. Which you can't if you're looking at, for instance, a phase transition in a solid.

One famous example is where the solid can go from being an insulator to a conductor. One of the theories of this, a very famous theory, the Anderson theory of localization, says that the reason that certain materials are insulators is due to disorder in them. So that particles which try to propagate as waves, the waves get scattered back randomly and they can't go anywhere.

But at a certain threshold of randomness, they can start propagating. So you actually get a phase transition. Well, this is known. The theory is generally believed. But it's hard to study in very much detail because you can't change that. There's no knob to change from order to disorder in matter. Well, you can see this effect with atoms.

Another development, which has come along with the developments for laser cooling, are methods for holding atoms with traps made of standing waves of light. Now, that's a little bit difficult to explain. But if you have a standing wave of light like you do in the laser cavity for instance, the atoms can be attracted or repelled by the field. And the field varies periodically in space. Now, those forces are very, very small. But at the ultra low temperatures you can get to with these atoms, they can be large enough so that you could hold the atoms in these traps. And you could photograph them in there too.

So it's a very, sort of a new world. And then you can start doing things. For instance, you can make the trap slightly irregular by shining other light on it. Or if you want, you can turn down the trap. You can turn down the trap so that you just get one atom in each one of the little standing waves. It's almost like a crystal. But the atoms don't interact with each other. They're interacting with this light wave.

Then if you want, you can lower very carefully the trap a little bit further and then the atoms do something permitted by quantum mechanics, they can start tunneling from one to another. When they start doing this, the system which looks like a solid, suddenly becomes a liquid. They can flow. And you can see this. And you can study it with great accuracy. This was something you could never do before.

So you can see these fundamental theoretical predictions in matter. You can see them under controlled conditions, which you could never see before. Furthermore, you can create new structures which don't occur in nature, but which you can make on your own. It's not only just being able to make them, but to be able to control things so accurately and to adjust them.

One of the important properties of these gases is how much the atoms interact with each other. Well, there are now experimental techniques so you can turn a knob and you can make the atoms repel each other or attract each other or not do anything to each other. So you can turn on and off these interactions. So it's a whole new world that's been opened, that's being explored, both to understand the theories of matter, but also just to understand newer and manipulate new types of quantum phenomena.

I mean everyone's heard about quantum computing, which is still sort of a visionary idea. But experiments in quantum entanglement and quantum information theory can be realized using these ultra-cold atoms. So that's why I say no one dreamt of these things when BEC was discovered. Certainly not when we set out to work for it. You just don't know. So it's just a wonderful story of how rewarding science can be.

**INTERVIEWER:** I also want to return to this question of mentoring because although as you said, you sort of ended up not being the first to achieve BEC, your students went on to sort of achieve things in other forms, using other experimental techniques. And to sort of have mentored and fostered work beyond just the work you were doing with your colleagues in the lab, must be quite exciting?

**KLEPPNER:** Yeah. No, it's wonderful. I'm very proud of these students. So it's sort of nice to have had a finger in the pie of their education, if that's the right metaphor to use. Yes, certainly. And I mean this is one of the reasons that one can take so much pleasure in science and so much joy in the achievements of others too. I mean obviously it would have been fun for us to get to Bose-Einstein condensation first.

But I have certainly no regrets about not doing that. I'm delighted that it was discovered. And I'm delighted that the subject is flourishing at the rate that it does. And it makes me feel very good about being a physicist, not just about Bose-Einstein condensation. But physics is a wonderful subject. It's an elevated subject. If you look at the broad range of things being studied in physics, it's a great privilege to be part of that big picture. So it's been marvelous.

**INTERVIEWER:** Is there anything to be said about, or any pattern to how you've chosen the problems to work on throughout your career? I mean have you consciously set out to do certain things or have things sort of just kind of just come across your radar screen?

**KLEPPNER:** No, it's more like things coming across the radar screen. Because the interesting new things are never that obvious to begin with. I mean certainly BEC, I had no idea where it was going. Well, the cavity quantum electrodynamics, I mean our first interest is that this seems like a curious effect. Wouldn't it be kind of fun to see that? And well, for Rydberg atoms, if I'd had some more imagination, I might have realized all the sorts of things you can do with them. But this is just something curious that it looked like it would be fun to investigate.

Now, some people can have a grand vision. You'd like to--- I mean, start out to look for dark matter, which is one of the principal problems in physics right now. And start out early in your career with a goal for doing that. And just work steadily on your career for that. But most physicists have more than one interest. And a very good one can have quite a few interests.

**INTERVIEWER:** You mentioned dark matter. I actually wanted to ask you in your mind, what are some of the really exciting questions in experimental physics that are out there? And what ones particularly pique your intellectual curiosity at the moment?

**KLEPPNER:** Well, I think dark matter is universally recognized as a fundamental challenge to physics. And although it's universally recognized, that doesn't mean that everyone is working on it. You have to have some good ideas for how you might do it.

I mean there are other areas. The question of strong gravity right now is one which is provocative. The things that appeal to me most are those which have an experimental contact, because I approach physics as an experimenter, rather than as a theorist.

I mean in experimental physics, I'm just struck by the number of new approaches that have been developed recently for using what I'll simply call quantum mechanics, quantum devices which are sort of on a macroscopic scale. We can use quantum mechanics in ways no one had ever dreamt of before. So I mean there's obviously lots of room there.

The question is often asked me, well, if you're starting out in physics, what would you do right now? And I don't think I can really give a good answer to that question. I mean something fascinating is sort of the new understanding of neuroscience. And the question is, what physics can bring to that? At some level, it must be able to. And one would hope that you would get a very profound understanding of neuroscience.

I'm sure that 30 years from now, our understanding of neural processing will be totally different from what it is now. And whether physics contributes the major new understanding or is peripheral is a little bit hard to say right now.

I mean that's a good example of where you are when you're at sort of the frontiers of knowledge. It's only obvious what you should do in retrospect. And so I mean this question of choosing problems, that's the crucial problem in physics. But that's why the advice I gave to young people, it's just follow your interest and do something. And if you're lucky, it'll lead someplace interesting.

**INTERVIEWER:** I also wanted to ask you about sort of your MIT career. You've been at MIT obviously for more than 40 years, led RLE for a time. How has the Institute changed or has it not changed in that time? And what has been your sort of experience in some of the sort of institutional endeavors that you've been involved with?

**KLEPPNER:** Well, it's changed considerably. I mean the way science is taught has changed a lot. And the undergraduate population is clearly just totally different from what it was when I started teaching here.

In my early years of teaching, I was quite close to the students. For instance, I would have my freshman advisees over to the house once a term or something like that, to get to know better. In later years when I got busy, I didn't do so much of that.

The style of teaching for freshman teaching has changed tremendously now. There's been a great deal of interest in it. There is a great deal known about the learning process, which wasn't known then.

But when it comes to their own behavior, most physicists, most professors are pretty reactionary. I mean the way that I taught, which was basically by lecture course, I talked, they listened. They copy it down. This is why they deprecated. I would talk about blocks sliding down planes, which is usually used an archetype for dull, academic physics. I think there's a lot to be learned from that.

So I watch how teaching is done now. But it's so different. And it's changed quite rapidly, subsequently to my early teaching undergraduates. So I don't have much of a sense for that. So I can't really give a good definitive answer to that.

Except that clearly the freshmen teaching, the teaching in the core courses has changed. The core courses themselves has changed. I mean in the old days, biology was not considered essential. And now, it certainly is an essential subject for everyone to know. So I guess I'm too disengaged to be able to give a definitive answer to that question.

**INTERVIEWER:** I also wanted to ask you about MIT students. Obviously, you mentioned how they've changed. And clearly, it's a much more diverse group. But in your experience and up to the point where you maybe had less contact with them, what were MIT students like? Is there a way of generalizing?

**KLEPPNER:** Yeah. First of all, the range of students and student interests is quite large, like it is at most places. But I think the number of students who are seriously interested--- the fraction is relatively high here at MIT. And the really good students are superb. I mean clearly they must be, because they go off and become leaders elsewhere.

So we're very fortunate to have such students at MIT. And as a research institution, the most valuable thing MIT provides to research isn't the superb faculty or the facilities, it's the fact that we can attract some of the very best graduate students here. And it's these young students who really make the place, not to deprecate my colleagues. But you find really excellent physicists sort of all over the country. But it's just in a few institutions where you can really attract the very best graduate students. And those students are wonderful.

And the undergraduates too, I mean there are quite a few undergraduate students with extraordinary abilities. And for them, MIT is a very nurturing place because we make such an effort to give them research opportunities.

I mentioned I had this at Williams College. But that was exceptional. I mean I asked and they did it for me, which was very nice. But there's so many opportunities here for students who want to take advantage of them, so that this is a wonderful place for these students.

Well, I shouldn't have to reassure anyone that the MIT student population is a very good one. And so I consider it a privilege to be at an institution with such great students and such great colleagues.

**INTERVIEWER:** So speaking of colleagues, I mean we've touched on several of your colleagues and research partners along the way, from early mentors through collaborators. Are there people we really haven't mentioned, that bear mentioning that you'd like to talk a little bit about your work with?

**KLEPPNER:** Really, my principal collaboration here was with Tom Greytak. I haven't collaborated formally with Dave Pritchard since he joined the faculty. But we see each other all the time, and Wolfgang Ketterle. And in fact, the Center for Ultracold Atoms right now, which is joint between Harvard and MIT, is a very nice community with people you can sort of bounce ideas off and talk with. So these are all part of my life, even though we don't write papers together or haven't gone into joint research.

**INTERVIEWER:** Tell me a bit about that center? I mean its formation, its roles?

**KLEPPNER:** I mean for many years, I had regretted the fact that we have really good people here at MIT and they had good people over there at Harvard. And really, we're quite isolated from each other. Now, in roughly 1996 or 1997, the NSF established a program for science. It was the Science and Technology Centers. These were to be rather large programs. They encouraged collaborative work and such.

And so we started talking with some of our friends at Harvard. One was Rick Heller, who sort of resonated to this NSF possibility. John Doyle, who had worked with Tom Greytak and myself on the hydrogen work, and was now an assistant professor at Harvard, was involved with it. And there were several other people at Harvard who sort of worked in this area.

And it was clearly, Bose-Einstein condensation had been discovered by then. It was sort of recognized as this hot stuff. So we thought somehow we should get together. And we did get together and put together a proposal, which did not get funded. Now, at the time I was very angry because it was clearly a top-level proposal.

But in retrospect, I'm very happy because it was at a level which was too large for us. They wanted us to do things that we really shouldn't have been doing, like get industrial involvement and such. So that got turned down.

But in the physics division, NSF knew about this. I think they were disappointed also. And so we approached them and asked whether we could submit a proposal at a much smaller scale for a center? And they said, yes, bid away. So we prepared a proposal. And out of this proposal, grew the Center for Ultracold Atoms. And it received funding from NSF for a five-year year period.

It was basically, at that time the first phase of it was to develop new methods for making and studying Bose-Einstein condensates. And the work went very well. And then we put in a renewal proposal. And the renewal proposal wasn't really for making Bose-Einstein condensates anymore, but it was studying new properties of the gases.

So the scientific interest had changed somewhat. And the experimenters changed. Part of the original proposal, we were working on hydrogen. So Tom Greytak and I were principal investigators in there. And that point, it was obvious to us that hydrogen was not going to be productive. So we dropped that out. But then we could put in some other people into it.

And with Wolfgang Ketterle as the head of it and the emphasis on it really was on the basic study of these quantum fluids, both bosonic fluids and fermionic fluids too. So it's sort of on that basic science.

And it was approved last summer for another term. Again, the scientific interest had changed now into really studying the interfaces with condensed matter theory. There's a real merger between these fields. And the emphasis is on studying what's called many body physics, using these quantum fluids.

And it does work as a joint center. Namely, we have seminars every week, with the alternate between the two institutions. And people go back and forth. And there are some collaborations between the MIT and the Harvard workers. So it's a very nice community. And I'm very happy to be part of that community.

My only really active participation right now is that I started when this started, a summer program to try to recruit college students into high school teaching. And it's called the TOPS program. And it's now in its 10th year.

And we did this really as an outreach program, although when the center started, you didn't have to have an outreach program. And I thought we should do something. And in trying to figure out what the problem was with education, I decided the most crucial problem was just simply the lack of teachers.

Well, we couldn't solve that problem on our own. But we could make some contribution to try to get undergraduate physics majors who are thinking about teaching, to try to get them as enthusiastic as possible and help launch their careers.

So I run that program for the CUA and sort of benefit from seeing all this great research going on around me and the young faculty members in it. We've got some extraordinary young people I feel, both here at MIT and also at Harvard. And I think CUA has helped to attract these people here. So it's I think in my judgment, a very successful center.

**INTERVIEWER:** So it's interesting that you mentioned teaching, as we sort of wind down. I mean clearly that's been something we've touched on a number of times as a kind of a continuity of interest for you. I mean you benefited very early on from it sounds like some extraordinary teachers. Tell me just a little bit again about what makes a good teacher and why in the sciences is it so important?

**KLEPPNER:** What makes a good teacher? I mean there are all sorts of styles of teaching. You can be permissive. You can be a great disciplinarian.

It has to be an engagement with the students and with the subject. The model teacher I think of is actually my wife. Now, she teaches--- not science--- she teaches psychology at a high school. And this is her 54th year of teaching at that school. And she's still going strong and loves it. And may wind down soon.

But she just has this engagement with the students and also with the subject. I mean she's been teaching child development for a number of years, which is unusual in a high school. But she is very much abreast of what's going on in child development and also on the classic work on that.

Well, I think the same thing holds true for teaching physics at the high school level or at any level. For one thing, you need to know your subject very well. You need to be well-grounded in the subject. And you have to really be interested in the students. You have to be patient, because sometimes students can try your patience.

But I don't know of any real prescription beyond that. I think it's terribly important to be in a place where good teaching is appreciated. And it is recognized here at MIT. It is important here, not teaching in spite of the system. I mean public school teachers in deprived communities are very solitary and trying to do their jobs without help from the schools and particularly help from the homes, too. So that's a very tough teaching job.

Fortunately at MIT, the students are so great. There's nothing tough about teaching here, except trying to stay ahead of the students.

**INTERVIEWER:** So is there anything we haven't touched on that you sort of came prepared to talk about that I haven't asked you about or that you think you'd like to add here?

**KLEPPNER:** Well, I, like many of my faculty members, have been involved in various science policy issues and such. Now, one reason for that is the example of my mentor Norman Ramsey, who was seriously involved and has played a major role in the scientific community in that.

I feel that scientists have an obligation to respond to these needs when they arise. Some years ago, I co-chaired a study by the American Physical Society on Boost-Phase Missile Defense. It was proposed by President Bush. Not very much was known about it. The APS felt that there should be a study which just explains it and to some extent, evaluates it.

And so I spent a lot of effort working on that. I felt that was appropriate. It was the point in my life where I could do that. And I feel that you have an obligation when the need arises and you can do it, to do that.

In the mid-'80s, I helped chair a session on a study on atomic physics for the National Academy of Sciences. These are sort of decadal studies, which are very useful. Although it's kind of it amusing, when you look back on them, they always miss the really interesting things which come about. I mean you're trying to explain why your field is so great and why it should be funded. So you're talking about all the great things which you think are going to happen.

What happens actually, well, it's very nice. You usually miss them. But things which happen are more exciting than you predict. So I worked on that. And I've been involved in offices in various organizations.

I think that's a natural responsibility for physicists who can do it or have some talent at doing that. I mean it's a privilege to be a scientist in this country. And I think that one has some obligation to pay for the privilege, when you can.

And I think this outlook, I'm sure I just inherited from Norman Ramsey, although he taught it by example, which is the best way to teach things, not by preaching. But anyway, that has been an important part of my life.

And another part has been in writing over the years. I've written columns for *Physics Today*, "Reference Frame" columns on all sorts of subject, which I've enjoyed very much. I said I got very much interested in English at Williams College. Well, I eventually found some avenue for expressing that interest in trying to write columns which were on all sorts of different subjects and trying to write them in a readable fashion. So that's been another important part of my life.

**INTERVIEWER:** In terms of engagement policy issues, could you perhaps just tell us a bit about what issues in particular have driven your engagement and what you've been interested in and why?

Well, in the mid-'80s when I'd chaired this report on atomic physics, I helped to represent the study on various congressional committees, which I found not a very satisfactory enterprise. I mean the congressional hearings, they're sort of bizarre. You start talking to one person. And you look up and you notice you're talking to somebody else. That's just the way they operate these things.

But I think this was helpful. I mean the committees, the science committee of the House, for instance, they are advocates for science. They feel responsible. They want to be well-informed. So I think participating in that is something if one is asked to do, is worth doing.

The study on boost-phase intercept, this is a subject on which I had no technical background at all. But the APS felt that it was important to have information for the public because it had been announced as a presidential initiative at the beginning of President Bush's first term. And no one knew anything about it. And so I co-chaired this with Fred Lamb at the University of Illinois, who fortunately had a very good technical background. And we carried out the study.

It was meant to be not a policy study at all, just a technical study. Will the technique do what it's intended to do? And that was a rather intensive study.

And one of the problems with these studies, you really don't know just what the impact of it is. What we found is that it's really not technically well-founded. Technically, it really would not succeed, except in very limited circumstances. Whether that's had any impact on policy since then is hard to see.

That's one of the things in the policy game, which is unsatisfactory I think for scientists. Cause and effect are generally not so obvious as they are in science.

Let's see, I'm just trying to think. Have I been involved with other-- well, I mean that was the major part. Well, a couple years ago, the American Physical Society had a hot potato, in that they issued a statement on global warming, which was rather hastily prepared. And members objected very much. And the society wanted to know what to do.

Well, this isn't really a science policy study. But it was a study. A group of us got together in trying to advise the leadership on APS on what they should do about, really making a blunder. It wasn't a major blunder. But it was a minor blunder. And it got a lot of people excited.

And now, the Physical Society has a working group on climate. And I think it'll be able to discuss these issues in a much more deliberative process than it did in the past.