INTERVIEWER: You've written that you began exploring the technical world by playing with Legos. I don't want to talk about Legos right away but is there a technical world versus any other kind of world as you understood it when you were growing up? What is a technical world?

KETTERLE: Well, the world of gadgets, tinkering, constructing things.

INTERVIEWER: And this was the world that you embraced as young child?

KETTERLE: Yeah. It may be difficult to rigorously define it, but I think if you see young children, whether they play with stuffed animals, whether they read books, or whether they want to work with tools, children are expressing something about their preferences.

INTERVIEWER: And your preference was clearly grab the tools, build some stuff, and then build it again.

KETTERLE: True.

INTERVIEWER: What first made you think that science was a place to express this kind of impulse in the technical world?

KETTERLE: I found out what I liked at school and then -- wait a minute, start again-- I think when you are a child you don't really know what science is, you don't know what mathematics is. But one aspect is playful explanation and then another aspect is you take those subjects in high school and I always this like physics and mathematics the best.

INTERVIEWER: There's plenty of people, particularly at the time that you were growing up, who viewed physics and mathematics as a domain of thought and abstraction and not necessarily a place where you would build tools to inquire into the nature of matter. Why didn't you take the path of maybe more theoretical physics and how did you embrace the building of tools, so many of which have been revolutionary in the studying of states of matter?

KETTERLE: I actually disagree. I think it was clear for me from the beginning that science has an experimental side. What actually drew me into science was both my love for mathematics, my love for abstract thinking, but also my love to tinker, to play with things. I had a motor bike and I was repairing it and doing some modifications. I had a chemistry set. So I was exploring science on the hands-on side and I enjoyed it.

INTERVIEWER: And so really from the earliest age, there was a unity of abstract science and experimental work that you've carried with you throughout your career?

KETTERLE: That's correct. Actually I was quite interested also in a career in mathematics and I just felt that physics would connect me more with the real world and the real world also means the possibility to do hands-on work. On the other hand, this interplay between experiment and theory during my career has taken me a few turns back and forth. For instance, at the end of my undergraduate studies I did a purely theoretically Master's thesis. So at that point I actually wanted to become a theorist.

INTERVIEWER: Why did you choose that at the end of your undergraduate study?

KETTERLE: Because I was fascinated by that. First of all, most of our education is theory-based. In high school, at least at my time, we hardly had any lab classes. So it was all about learning and understanding the concepts. At the University, we had the equivalent of junior lab. So I went through those lab courses. But the far dominant portion of the education is conceptual and theoretical; and it fascinated me.

INTERVIEWER: What was the first academic setting that you came into where you looked around the lab and saw machines that were really worth tinkering with, that had the capacity to explore questions that you thought were really fascinating?

KETTERLE: Probably when I started my PhD. But I mean it goes back and forth. I think I've always been a person who has been talented for both and who has intuition for both. I remember when I took lab classes and there was a torsion pendulum I was just fascinated by it. I could play with it. I was experimentally realizing the differential equation. So I really felt-- I wanted to feel physics. I wanted to see physics.

But nevertheless, a couple of years later I picked a theoretical topic as my Master's thesis. I think I was always fascinated by the power of describing nature. When I learned about statistical physics, I found it fascinating that you can make a few assumptions about what atoms do, kind of some probabilistic arguments, arguments with entropy. You can deduce the equation of state of the whole system. So from very few assumptions or very few things you know, you can deduce a lot and that fascinated me. So therefore I decided to work on a theoretical subject for my Master's thesis.

INTERVIEWER: Is it an accident that it seems that one of the patterns in your academic career is that you typically would choose problems and projects that were beyond the capabilities of the lab equipment at your disposal, of the machines that were available at your disposal?

KETTERLE: I fully agree. It is the style of my group to push the frontiers of the field. I tell all the students who work with me or who want to work with me, if you want to work with me, you have to be a hardcore experimentalist. Because our job is to push the boundary of what is possible. So my lab-- my group-- is extremely focused on exploring new things which currently seem impossible or out of reach and bring them into reach.

INTERVIEWER: When did you first have the experience of encountering a question that required you to make a brand new tool to investigate that question?

KETTERLE: The question sounds too much black and white. Because new tools are often not brand new. You have ideas, you see something and modify it. It sometimes depends on how familiar you are with the details whether the modification looks revolutionary or brand new. I always felt what I did was the logical next step. But viewed from the outside, it looked to people like a bold move.

INTERVIEWER: Well I'm not trying to over-characterize the boldness or the inventiveness. But I think there's a classic story in science there, where you have a question that you're dying to get to the bottom of and you really need to invent or modify the existing tool set to enable you get there. So it requires a number of jobs to get to the--

KETTERLE: I mean I see an interplay There's an interplay between technological advances and experimental advances. Suddenly you realize I have a tool. Then you find new science. But sometimes you are interested in solving a burning scientific question and then you think which tool can help us? So sometimes I have allowed myself or my group to develop tools in search for the right question, just to be sort of ready to attack the question. Or there was a question, and we developed a tool specific for that purpose. So it sometimes depends. Sometimes your research may be more technological, driven by finding better tools to go deeper into science. Sometimes you are motivated by a scientific question.

INTERVIEWER: You seem to have been drawn to the idea that in spectral analysis of atoms and molecules the individual signatures can tell you a lot about the states of matter. You stayed there for quite a while.

KETTERLE: That's true.

INTERVIEWER: What did you love about that particular domain?

KETTERLE: Well I found it, and I still find it, fascinating that when you look at the spectrum of an atom or molecule, that you can learn all the details about the atomic and molecular structure just by looking at the frequencies in the electromagnetic spectrum at which the atoms or molecules resonate.

I was particularly excited about it when, during my PhD, I discovered a new molecule and there was hardly anything known about it. And I had a spectrograph. I looked at the light which was emitted by the molecules. There were hundreds if not a thousand of different spectral lines. A lot of them were not regular. It didn't make any sense initially. It took me, I think, about two years or so to solve the puzzle. I had to learn a lot of theory. I had to do a lot of computer simulations. I had stacks of papers of simulated spectra just trying to find a simulation on the computer which became similar to the experimental spectrum.

INTERVIEWER: And the molecule?

KETTERLE: The molecule was helium hydride. It was a molecule consisting of the two lightest elements in the universe. It was a very special molecule and after two years I had found the code. I'd really deciphered the code of nature. For the first time I knew what is the size of the molecule, how still is the point between the helium and the hydrogen. All the basic questions one would ask about a molecule, I could answer. I found it extremely fascinating to be able to do that. It was even more fascinating for me since I grew up at an institute which didn't have the tradition of doing that. So almost everything I had to learn by myself or learn by asking other people, or by reading the literature. This was a very important step in my development as a scientist.

INTERVIEWER: That was it the Planck Institute?

KETTERLE: Yes, the Max Planck Institute.

INTERVIEWER: What brought you to MIT?

KETTERLE: Laser cooling and Dave Pritchard. That is actually an interesting story. If you'll allow me to fill in some gaps, we've talked about spectroscopy which was my PhD work and the work I did during my first postdoctoral appointment, still at the Max Planck Institute. I've also expressed to you the fascination and also the satisfaction I had in analyzing a brand new molecule.

On the other hand, I felt after three years as a postdoc I was sort of at a dead end street because this kind of molecular spectroscopy was done early on and they're were not many molecules left to do that kind of analysis. The frontiers of the field were different. So I felt the molecular spectroscopy that I did was neither applied nor fundamental. I wanted to make an important contribution to science, technology and knowledge, I should either do something which is clearly applied or which is clearly fundamental.

So I was at crossroads. I decided to work on an applied project because throughout my studies as an undergraduate student, also during my PhD, I always wanted to do something more applied. Applied means something which is more directly related to the needs of society or the burning questions which we have to solve as a nation.

INTERVIEWER: What are those burning questions and what do you sense is a scientist's responsibility in responding to the needs of society?

KETTERLE: Well, scientists can respond in many ways. One of the needs of society is to protect the environment, to avoid pollution. I felt as a atomic physicist who understood spectroscopy that there was an opportunity for me to contribute, namely to work on using spectroscopy to monitor combustion. So I went into combustion diagnostics.

So I used my knowledge of spectroscopy and applied it not any more to new and potentially esoteric molecules, I applied it to flames, flames in a Bunsen burner, flames inside a diesel engine. So my goal was, and this is what I picked as my second postdoc-- and at that point I intended to make it my career-- to use laser and spectroscopy as a tool for cleaner environment. To make combustion more efficient, to make combustion cleaner.

INTERVIEWER: Monitor the combustion and by doing so come up with ways of making it more efficient and getting rid of pollutants and contaminants?

KETTERLE: Exactly. So the program was that-- the program was a combination of theoretical and experimental work. I was the experimentalist who helped to develop new tools of diagnostics. The goal was to measure molecular concentrations with high spatial and temporal resolution. So in the end, experimentally you can see at what phase of the combustion cycle pollutants form, to figure out how fuel mixes with air, and to permission local temperature fields to see if the flame front is irregular or if it's regular. At the same time, we had theorists -- theoretical chemists-- who used models of combustion.

The hope was-- which eventually became true over the last decade or so-- that by comparing results of theoretical predictions and comparing that we detailed to accurate combustion diagnostics that you could verify the computer codes and help to improve them. So that ultimately we would wind up with a much better understanding of what limits the efficiency and what causes pollution during combustion.

INTERVIEWER: What did you have to do to modify traditional lasers to make this diagnostic tool a reality?

KETTERLE: For a lot of the work you can use commercial lasers but you either have to adapt the lasers to the special purpose or to do some modifications. During that year in Heidelberg, I worked with ultraviolet lasers.

One of the -- it's probably not an earth shattering idea, but I had a nice idea to modify a laser from a single wavelength into a dual wavelength laser. With that we had a two channel capability, which meant we could monitor two chemical species simultaneously. So that was a nice development. It was fun for myself and for the people around it. We even took out a patent. I think in the end, it didn't really have a big practical impact, but that was sort of one example where you want to improve your tools, you want to really master your tools in order to use them for your scientific purpose.

INTERVIEWER: Those skills really would help you later on in your low temperature work. But before we get to that, how did you come to MIT?

KETTERLE: No I mentioned laser cooling and Dave Pritchard?

INTERVIEWER: Yeah.

KETTERLE: Anyway, so I think we are now at that stage. So I've just told you what I did before I came to MIT, namely applied research. Applied research was also for me to eventually realize some of the thoughts or some of the aspirations I had when I was a student. I wanted to work on something practical.

I've already mentioned that I had sort of two forces in me. One was a desire to work on something applied to solve practical problems. The other aspect was my talent and passion for abstract thinking. This drew me to fundamental problems and it also drew me to theory. I mentioned to you earlier that I had worked on a theoretical Master's thesis and then I switched to an experimental PhD thesis. Then eventually after my PhD and first postdoc, I went into hardcore applications: combustion research.

So while I worked on combustion research and did applied research, I learned a lot about myself. I saw on the one hand the deep satisfaction to work on problems for which it is self-evident that somebody has to work on. If you work on clean combustions using laser light, you can talk to all your friends, you can talk to your parents, and everybody understands what you are doing. It's so easy to explain why it is important to use modern technology and laser technology to make combustion cleaner.

INTERVIEWER: It's a little harder to talk about Bose-Einstein condensation?

KETTERLE: Yes. If I now tell people that there is this tiny little amount of gas, it's smaller than a millimeter, but it's terribly cold and it's terribly exciting to study its properties, I'm not sure that everybody understands me. So I think you get the point. I felt the satisfaction to work on applied problems.

I also felt I was in the middle of life. I talked to engineers at the Volkswagen company, because we had a collaboration with Volkswagen, and even used our laser equipment there on a test stand to look at a diesel engine. I was really relating to real-life problems with real-life people and diversity and all that. But I was missing something. I was somehow missing the long-term challenge. I was working on problems and solving problems. But I felt the desire to work on problems where you don't even know if there's a solution. So I felt that while I did this applied research, I want to work on something else.

There was also one other thing I learned. I liked that applied research was more directly contributing to the solution of urgent problems. But while I did the applied research, I also realized how much applied research draws from fundamental research.

For instance, my training in fundamental spectroscopy enabled me to do applied research with a different approach than the people who had done applied research all their life. So while I did applied research, I clearly realized that we need a certain division of labor between people who do fundamental research and people who eventually take care of applications.

So after realizing that, after also realizing my talent and my desire to do fundamental research, I then decided to change my career again. I wanted to work on fundamental problems in physics. This was a big step, and it had a lot to do with finding out something about myself and my own motivation, and in some sense I found out what I really wanted.

INTERVIEWER: We associate lasers in the popular world with heat not cooling. How did you suspect that a laser would be a cooling device at the extreme low temperatures that you wanted to explore?

KETTERLE: Laser cooling is pretty cool. Laser cooling is an amazing phenomenon, and it attracted a lot of attention when it was developed in the '80s.

So when I was about to make a decision again, I wanted to work on something fundamental, how did I pick my area? Well, I felt at that point I was around 32 years old, I couldn't start completely over again. So I wanted to work in a fundamental area of physics where I could continue to use some of my tools, namely lasers, optics and spectroscopy. I talked to many people. I looked at proceedings of conferences. What I identified as the most promising and most exciting area was laser cooling.

So laser cooling was already quite well developed at that point. But I felt there was a big potential to study the properties of laser cooled atoms and explore other science with cold atoms. I concluded that it's the most exciting field in fundamental physics that I can contribute to with my tools. So what I did is I sent letters to some of the best groups all over the world, and asked if they would offer me a postdoctoral position. Eventually Dave Pritchard here at MIT offered me such a position. That led to the fact that eventually in early 1990 I arrived at MIT.

Now I haven't really addressed your question about why lasers cool, or when do lasers cool and when do lasers heat?

INTERVIEWER: I noticed that. Yes. You haven't addressed that.

KETTERLE: Let me do that now.

INTERVIEWER: Please.

KETTERLE: Lasers are, of course, a directed form of energy. If you hold your hand in a laser beam, you feel the heat. Because the light, which is energy, is absorbed by your skin, and it is converted into random motion. We feel that as heat. Well, with some of the lasers we have in our labs you can even burn your flesh. So this is how lasers heat.

But if you apply laser radiation to atoms, there's one important difference. The atoms cannot keep the light. When atoms have been excited to a more energetically higher state, they have to re-emit the photon. So a photon which is absorbed is re-emitted with 100 percent probability. Therefore, the energy which the atoms absorb is emitted again as florescence.

INTERVIEWER: But that's a straight ahead trade.

KETTERLE: So that would be a straight ahead trade. Now on a much more subtle scale, when we use the right laser light with the right frequency, we can arrange for situations where the emitted light is slightly more energetic than the absorbed light.

INTERVIEWER: So that means as you apply the laser to the atom and the atom continues to re-emit it loses energy.

KETTERLE: Exactly. It's quite fascinating how that comes about. What happens is we need a frequency shift of the light. But in its simplest form, this is provided by the well-known Doppler effect which makes the frequency of light depend-- if a moving object emits light-- the frequency depends on the direction of the emission. That is sufficient to make lasers cool.

INTERVIEWER: When did you-- I mean, you say that laser cooling was fairly well known at the time you came to MIT, what potential did you want to explore using this tool as you understood it to be so exciting?

KETTERLE: I was drawn to the field by its novelty and its general potential, also by the possibility of analyzing and exploring properties of cold atomic matter. Cold atoms can form cold molecules. That was quite exciting and that was actually the project I was hired for by professor Dave Pritchard.

INTERVIEWER: The mission would be to explore matter at states of extremely low energy in a whole variety of settings?

KETTERLE: Yeah, that was the fascination. I had certain questions about the wave nature of atoms. I was intrigued but as far as I remember I wasn't really focusing on one specific thing. I just felt it was really cool-- a cool area to work in.

But let me give you my assessment, which is I think interesting. In 1989, I thought that laser cooling had gone through extremely exciting times. Sort of between the early '80s and late '80s, new technologies, surprising cooling technologies were developed, and it all came together. that one could cool and manipulate and trap atoms. My assessment was that the field had reached a certain peak and I was expecting that now the field would be at a very high plateau for at least some years to come. This is where I wanted to contribute.

Of course ideally as a scientist you want to contribute to a field while all the pioneering work is done. Well, I didn't see such a field or such an area within my reach given my background. I felt laser cooling was the best I could do. But I had the impression that laser cooling had reached sort of its peak and now there was a long plateau for several years.

In some sense I was right. Because when the Nobel Prize was given for laser cooling, it was given to the people who contributed to the development of cooling techniques in the mid and late '80s. But what I couldn't know at this point is that when laser cooling was combined with evaporative cooling that the whole field would jump up to another level which was completely off scale. So at that point, the best of laser cooling-- the very best of laser cooling-- was still to come.

INTERVIEWER: The combination with evaporative cooling, which is another technique-- when did that come into being or occurred to you and your team as a tool?

KETTERLE: Well being at MIT, you are of course familiar with evaporative cooling. Evaporative cooling was pioneered by Professors Greytak and Kleppner at MIT. Of course, being at MIT I knew about it.

INTERVIEWER: Now, evaporative cooling: I think of that and I think of the very lay, traditional notion of how an air conditioner works, where the energy from evaporation is removed from a space and by doing that continually you can cool air or cool the room. But on the scale that you're talking about it's very, very different. Can you explain?

KETTERLE: I was referring to the work of Dan Kleppner and Tom Greytak who applied evaporative cooling to cool hydrogen atoms in an atom trap. So it's more talking about how can you cool atomic samples and to find-- and this was really pioneering work-- to apply evaporative cooling to this whole new system in a whole new range of temperature.

INTERVIEWER: Basically you're taking the high energy atoms relative to the system and bumping them out of the picture so that the average energy goes down, down, down.

KETTERLE: It's as easy as that.

INTERVIEWER: Yeah easy for you to say. But on an atomic scale how do you identify the hot atoms and kick them out of the system without adding more energy?

KETTERLE: Well that can be done in two ways. One is, we keep the atoms in a trapping potential. So the atoms are like in a-- you can think of it as marbles in a cereal bowl and they move randomly around. By lowering the rim of the bowl, you allow the most energetic ones to jump over to the rim.

INTERVIEWER: That's good.

KETTERLE: Another method is to use radio frequency or microwave spectroscopy, but it's actually a variant of that scheme.

INTERVIEWER: By combining this with laser cooling, the low temperature potential you could see was what?

KETTERLE: So that's now an interesting question, because now we are talking about the early '90s.

So what happened is with laser cooling and evaporative cooling, the cooling techniques both were well known. There was some speculation that those two powerful cooling techniques could be combined, but the general opinion was that it will not be possible. Laser cooling only works at low density where the laser light is not absorbed by the atoms. Evaporative cooling only works at high density because the atoms have to collide and stay in thermal equilibrium. So it seemed-- that's what many people said-- that there was a big gap between the highest density you could reach with laser cooling and the lowest density at which evaporative cooling would set in.

INTERVIEWER: So did you set about to bridge that gap or--?

KETTERLE: When I was a postdoc with Dave Pritchard, we bridged the gap. It was interesting how it happened.

I think it was just the ideal combination of somebody who joins as a postdoc from a different field and somebody who has all the experience like Dave Pritchard. I know as a postdoc I have to learn, read all the papers in a very short time because I have to come up to speed very quickly. I can't go through a year or two of preparation like graduate students do. As a postdoc you have already learned how to learn, and you should be able to learn rapidly. So I was reading all the papers, and asking questions, and having my own thoughts. I had discussions with Dave Pritchard as asked, "Why wouldn't evaporative cooling work? Why not?" He referred me to some papers.

I analyzed the papers and said, "But shouldn't there be a window?" We started talking about it. Our conclusion was that there was a gap to be bridged, but the gap was not very large. Ultimately, we were building a new cold atom machine. Dave Pritchard had a grant with work-- with approval for a proposal to study the formation of molecules from cold atoms. We were building an experiment along those lines, but kept on discussing how can we bridge the gap? How can we do one more improvement to laser cooling which would then hopefully jump start evaporative cooling?

INTERVIEWER: How did you acquire lab space to investigate these ideas?

KETTERLE: Dave Pritchard had labs. I was working in his labs.

INTERVIEWER: But is it unusual for a postdoc to come in and suddenly get lab space?

KETTERLE: No, I was assigned to an experiment which was started by Dave Pritchard. He had an earlier postdoc who had worked on that, and I took over. What was usual was that the experiment at that stage was sort of the concepts were not fully worked out. So it was an experiment which was just being put together. There was an opportunity to come up with your own ideas and your own concepts.

INTERVIEWER: But in any institution there's a limited amount of lab resources and it seems that a lot of your colleagues were as excited by your investigations as you were and voluntarily gave up lab opportunities so then you could have.

KETTERLE: That actually came later after I was appointed as an assistant professor. But we are just talking about the postdoc time when the opportunity arose to bridge the gap between laser cooling and evaporative cooling.

INTERVIEWER: Alright, well let's talk about the postdoc moment then before we get to the assistant professorship-- when did you know you had bridged the gap?

KETTERLE: Well, that's a moment I will never forget. I was two years into my postdoc and we were laser cooling atoms and we used this new idea which Dave Pritchard and myself had come up with. What we saw were completely pitch black clouds of red atoms which absorbed all the light that we were sending at them, because they were so dense. So we had reached higher density in large clouds than anybody had ever reached before. Very soon it became clear that this could bridge the gap.

INTERVIEWER: What element were you using?

KETTERLE: It was the element sodium. Sodium used to be the workhorse of atomic physics in the '70s and '80s. Dave Pritchard has worked with sodium for most of his scientific life. That was our natural choice for laser cooling.

What then happened was also a formative moment. We started to discuss what can we do next? I-- and it wasn't first very clear. Dave Pritchard had worked out all these ideas about how to form cold molecules from cold atoms. He felt this higher density would just be a boost to it and he felt we should continue along those lines.

Myself, as a postdoc, I said "Well, why don't we go for the holy grail?" Let's combine laser cooling and evaporative cooling-- there is a chance it will work. For -- I don't know how long it was-- for a few meetings, we discussed it back and forth. The graduate students who worked with me were ambitious. They wanted to go for kind of the big challenge. I will never forget that after a few such discussions, Dave Pritchard just said, "I'll let you do what you want."

So Dave Pritchard had really the greatness of abandoning his own ideas. Some other people got famous with photoassociation, you know, putting cold atoms-- associating cold atoms into molecules. But he allowed me as a postdoc to go for this combination of laser cooling and evaporative cooling, which some people regarded as uncertain and speculative.

I know in 1995, a few months before Bose-Einstein condensation happened, I got reviews on my proposal where people felt that there was such a long way to Bose-Einstein condensation it would probably never happen. So it wasn't clear that this was a golden road. But Dave Pritchard was a great person. He realized that I was so enthusiastic about it, he wanted me to try it out.

INTERVIEWER: It's fair to say that that was a moment of extraordinary generosity in your career?

KETTERLE: Yes. Number one; and number two was that it was also a moment of sudden change. We had this new technique called the dark spontaneous optical trap, where we had reached higher densities than people before. But before even writing and submitting the paper, we ordered a list of critical pieces of equipment in order to go for evaporative cooling. So there was really this sudden moment where kind of all your thinking, all the resources of the lab-- whether it's student manpower or equipment money-- everything became suddenly focused on a new goal.

INTERVIEWER: So you made the big bet on your question. All the resources were devoted towards figuring out if there was an answer. An exciting but scary moment.

KETTERLE: I thought it was exciting. I wasn't scared at this point.

INTERVIEWER: Yeah, you don't get scared. Not much scares you. So what was the target then? Explain perhaps at the end of that pathway of low energy lay Bose-Einstein condensate.

KETTERLE: I tried to be reasonable and conservative. I knew about Bose-Einstein condensation. But it was also clear that with laser cooling, we were missing a factor of a million in density, or more accurately, in phase space density, which is a more technical term.

So we were a factor of a million away from Bose-Einstein condensation. I felt it was almost frivolous to say we are now going for Bose-Einstein condensation. So for myself and for my lab, we declared the goal: we want to combine laser cooling with evaporative cooling and we want to get at least a little bit colder and a little bit denser than anybody before us. We knew that ultimately this could lead to Bose-Einstein condensation but we felt we were far away from it.

INTERVIEWER: Let's describe what that condensate actually means. At a certain density and at a certain low energy, the atoms would do what?

KETTERLE: The atoms would fall into lock-step. Instead of moving randomly, they would literally all march in lock-step, which means they would form one big coherent wave, a giant matter wave.

INTERVIEWER: This matter wave-- does it merge quantum events and other events in a way that you can't see anywhere else in the world? What is it about that matter wave that is fundamental?

KETTERLE: Well, coherent matter wave is for atoms what the laser is for light. If you have light from a light bulb, the light is emitted randomly in all directions. There are little waves going everywhere in contrast, the laser light is just one coherent wave. Similarly, in an ordinary gas, atoms move randomly and in a Bose-Einstein condensate, they march in lock-step. So it's not that the atoms are merged, it's just that all the atoms do to the same thing exactly as the photons in a laser they all do the same thing. They're not moving in random directions, they're just in a single mode of the electromagnetic field.

INTERVIEWER: Does that constitute a different state of matter? A new state?

KETTERLE: It is a different state of matter which is very, very different from ordinary states of matter. However, I would actually adopt the suggestion made by Fritz London in the middle of the 20th century or so, that we should use the fifth state of matter for systems which are quantum mechanically that show a macroscopic coherent quantum state. This class of matter would include superconductors and all superfluids. The Bose-Einstein condensate would be another member of this very distinguished class of quantum matter. Some people have labeled the Bose-Einstein condensate as a completely new state of matter. I disagree.

INTERVIEWER: You would say it's part of the class of states?

KETTERLE: Yes.The Bose-Einstein condensate has a lot in common with superconductors and superfluids, which are a very special club of systems, and the Bose-Einstein condensate is the first gas in this club. The fact that it's a gas makes it distinctly different, but still I would put it into the same category as these other systems.

INTERVIEWER: Can you describe the experiment and the time frame of the project that actually delivered that result? Getting the atoms to be of a sufficient density that they constituted this Bose-Einstein condensate state?

KETTERLE: Yep. So we've talked about the combination of laser cooling and evaporative cooling. In 1992, we bought some vacuum equipment, electric equipment, optical equipment and put it together.

INTERVIEWER: That was fun, right?

KETTERLE: --that was a lot of fun. But what I had never expected was that only a few small ideas were needed to take the system to Bose-Einstein condensation. We're talking about six orders of magnitudes-- a factor of 1,000,000, and it was almost like we took it in one step. I expected that every time we get a little bit colder, we find something new, something unknown, we have to learn something about it and implement something new. But it really happened that there was only one modification to the atom trap we had to implement. But other than that, we could jump over those six orders of magnitude with one big jump. It was really dramatic.

INTERVIEWER: That is dramatic.

KETTERLE: So we got the machine to work. There was one major modification we had to do just to built sort of a leak-tight trap. A trap that atoms couldn't leak out.

INTERVIEWER: A magnetic trap?

KETTERLE: It was a magnetic trap. I've used a combination of optiomagnetic trap, but eventually there was a leak in the trap, we had to plug it. But after that, Bose-Einstein condensation just happened.

INTERVIEWER: You saw it how?

KETTERLE: We looked at the atoms by now I would say digital photography. You can image a cloud of atoms by illuminating them with light, and then the atoms cast a shadow on a camera. The shadow told us how the atoms were moving.

In a normal cloud of atoms, if they're no longer in a trap, just move away from each other because they have random velocity. But if the cloud is very cold it's a Bose-Einstein condensate, the motion is so slow that the cloud is hardly expanding at all. So it was a little bit like that. If you trap a normal cloud it just expands. If you trap a Bose-Einstein condensate it drops like a rock. So that's what we saw.

INTERVIEWER: Do you recall what you said to your colleagues when you observed that result? What did Pritchard say?

KETTERLE: Everybody was delighted. I remember the moment we saw it in the lab, which was full off tension and anxiety. The situation was slightly more complicated. Over several years-- for about three years -- before Bose-Einstein condensation happened, my group was in a head-to-head race with my colleagues at Boulder. The group in Boulder actually pushed ahead of us in the summer of '95. They had observed Bose-Einstein condensation in June of 1995.

INTERVIEWER: You were using different elements, right?

KETTERLE: We used different elements, we used different methods, we used a different trap. But in both cases it was about the combination of laser cooling and evaporative cooling. So when we discovered Bose-Einstein condensation in sodium-- that was in September, three or four months later-- the first reaction was a big relief.

We realized that Bose-Einstein condensation would start a new era in atomic physics. We were, of course, afraid that with our atom and our techniques, we may have bet on the wrong horse. We had been pioneers of the field. I had put all the resources into the pursuit of Bose-Einstein condensation, and came very close. But then, after the success in Boulder, we were afraid of missing out on all the excitement. So when Bose-Einstein condensation finally happened at MIT, the immediate reaction was a big sigh of relief rather than the excitement of a new discovery, because the first observation had been made a few months earlier.

INTERVIEWER: How did you extend on your relief and get into an area that was fundamentally new? I guess I'm speaking of the expanding or contracting clouds that begin to tell you a little bit about how the atoms behave.

KETTERLE: Well, in hindsight, in 1995 after we had observed Bose-Einstein condensation here at MIT, we made the right decision for the wrong reason. In these moments of anxiety, where we didn't know if our method would work, I was feverishly analyzing everything I knew. I was in a very competitive situation. It was an exciting time. I was well-positioned with my group. But I had to make the right decision. I had to steer the group into the right direction.

I thought very hard about what used to be a bottleneck in the experiment, namely the magnetic trap. So I came up with a new design for the magnetic trap. Even before we observed Bose-Einstein condensation, I divided the effort of the lab and had some people work on the new magnetic trap. The moment when we observed Bose-Einstein condensation, the new design was sufficiently well advanced that we took the risk and decided that we're not even studying the Bose-Einstein condensation, which we had observed. We had only seen it on two nights. We had written this important paper on our observation of Bose-Einstein condensation, which was crucial for the Nobel Prize. But only after two nights, we took the whole machine apart and implemented the new idea.

When I said it was the right decision for the wrong reason, my analysis was at that point that there were so many groups in atomic physics who were immediately jumping at Bose-Einstein condensation. I knew that those groups had more resources, more experience with-- I mean all these established groups-- and I felt that our set up needed an improvement just to be competitive.

I also thought that we would put in the improvement in a few months. What happened was that for six months we couldn't produce Bose-Einstein condensation. The machine was down. We had lost the vacuum.

But six months later, we had a set up which was more controlled, produced 10 times more atoms, and was robust. We had a dream of an experiment. More what had happened is, the competition hadn't caught up. All the other labs were struggling to learn how to combine laser cooling and evaporative cooling. They had to learn what my group and the Boulder group had developed over two years. It took all the groups, even with abundant resources and experienced people, it took all the groups two full years before they could repeat Bose-Einstein condensation. For those two years, my group and the group in Boulder had the only working machines to produce and study Bose-Einstein condensates.

INTERVIEWER: Your machine was better?

KETTERLE: Our machine was much better. We had 1,000 times more signal than the Boulder people. We had a better trap. We could just study the condensate in one experiment after the other. That was an amazing time. It's a dream for a scientist. You have the stuff which everybody tries to get, and nobody gets it, but you have it. You can just study it, look at it, have ideas what to do next-- it was a gold rush.

INTERVIEWER: When you say right decision for the wrong reasons, it sounds like you're saying that that is the position that every scientist wants to be in-- finding yourself with the machine and the ability ahead of everyone else. But that's a great thing. but the competitive motivations and the worry about being upstaged by your colleagues is maybe one of the unfortunate things in experimental science that you wish wasn't there.

KETTERLE: No absolutely not. First of all--

INTERVIEWER: So what was the wrong reason?

KETTERLE: I was in an advanced position. I had one of the two machines in the world which could produce a Bose-Einstein condensate. Most reasonable scientists, if they have reached a stage where they can do unique experiments, they wouldn't take the machine apart. But we took it apart and improved it by a big margin. The reason was that I underestimated what we had accomplished. I underestimated the uniqueness of our accomplishment and felt other groups could soon follow up and would have improved setups.

INTERVIEWER: Your worry was misplaced?

KETTERLE: My worry was misplaced. On the other hand, I think competition brings the best out of us. Especially in our field, it's friendly competition. It's competition where your competitors acknowledge what you have done, where people are excited about real good science even if it hasn't happened in their own lab. This is competition, which brings the best out of you. Without competition I would not have thought so hard about what is the best solution. We worked even harder because we knew there was competition.

INTERVIEWER: You've talked repeatedly about wanting to do things that are rooted in real world problems and to find results that have the potential of helping real world people. How does this experiment satisfy that in any way?

KETTERLE: To some extent I have detached myself from the goal by a time scale which is maybe 20, 30, 40 years. The work I'm doing right now is fundamental. It improves our understanding of matter. It leads, in conjunction with theoretical colleagues to an improved description of matter. But right now the matter which we use as a test object to observe and understand new phenomenon is sort of so cold and you can say so special that it cannot be used for anything applied. On the other hand, a better understanding of materials should lead to novel materials and novel devices in the future.

INTERVIEWER: Do you think there are applications of these extreme low energy states for the development of superconductors and materials which will make energy use more efficient?

KETTERLE: That's the hope. The hope is that cold atoms may provide critical insight for future development of superconductors.

INTERVIEWER: I've heard described that some of the behavior of matter that you've observed at low energy is remarkably similar to the apparent behavior of matter at extremely high energies in things like plasmas as or inside of a neutron star. Do you buy that or do you think that's an oversimplification?

KETTERLE: That's not an oversimplification because we work at extremely low density. What happens at low temp-- very, very low temperature, very low density, can be equivalent if not identical to what happens at higher temperature and higher density. So the scaling is completely sound and in neutron stars for instance, you have much higher temperatures but also much, much higher densities and you may encounter physics which is similar or exactly the same as we study in our laboratories.

INTERVIEWER: Is it possible to understand the nature of the interior of a neutron star billions of light years away by observing the atoms on your desk?

KETTERLE: In principle, yes, but it is not realistic. But it is possible to contribute one piece of understanding. For instance, when you want to figure out if you have extremely cold neutrons and they strongly interact, what is their energy? We can simulate that with cold atoms. But there are, of course, many other properties of neutrons, many special properties in the environment of the neutron star which we cannot stimulate. But a question like that, what would be the base energy for a neutron star at zero temperature, we may find the answer with cold atoms.

INTERVIEWER: That's a result that you might not expected to come out of this journey that you began in the begin-- middle of the '90s or early '90s.

KETTERLE: You're absolutely right. If I remember-- if I think back about the year I described in '95 and '96 when we were brainstorming what other new experiments we can do with Bose-Einstein condensates we had some good ideas. But even our boldest imagination was not sufficient to anticipate the developments which the field has taken in the following years.

INTERVIEWER: So there was no plateau?

KETTERLE: No. It was just shooting up and some people feel the field is still expanding.

INTERVIEWER: When you became an assistant professor, what explains the generosity with which your colleagues appeared to donate, voluntarily give up lab space, to allow you to explore the questions that you were working on?

KETTERLE: The support and mentorship I had as a young scientist and as a young professor here at MIT was unprecedented for me. There is the issue of lab space, but what came even earlier was the generosity of my mentor and postdoctoral adviser, Dave Pritchard.

Dave Pritchard has been a pioneer in laser cooling, and in collaboration with me as a postdoc we pushed things even further. Dave Pritchard at this point had three experiments in three different subfields of atomic physics. It was probably clear at this moment, but definitely clear in hindsight, that the laser cooling experiment was his most promising and best experiment.

When there was a possibility that I would join MIT as an assistant professor, he told me that, well, he didn't want to create a shadow problem, that I would do laser cooling in his shadow. He told me if I were to stay at MIT, he would get out of laser cooling and hand me over all the experimental resources.

This is a highly unusual offer-- almost unheard of-- that a creative, active scientist is getting out of the field he has help to create, is handing over his best experiment, his best lab, to a young assistant professor. At the same time he told me, "This is your own experiment, we will not publish papers together. You can ask me for advice, but I want to make sure that you have your full independence."

INTERVIEWER: Can you imagine you yourself doing the same for some young assistant?

KETTERLE: Yeah, I've just done it. I handed over one of my labs to Martin Zwierlein, who's a young assistant professor, and one of our young stars. But coming back to 1993 when Dave Pritchard made me this offer, it was hard for me to-- it felt hard to accept it. He was this famous person and with what expectation would I take over this experiment? I wasn't sure that I would really succeed, and whether I could earn this enormous trust and credit which was given to me.

INTERVIEWER: Well he knew one thing about you-- pressure works with you.

KETTERLE: Yeah and I know there was one of these remarkable discussions. I sat down with him and told Dave all about my uncertainty whether I would succeed or not. I told him that I'm here with children and a German wife. I can't even say for sure whether I will stay forever at MIT or in this country." Dave just said, "I know all that. All I expect from you is to work hard and continue what you have started." So he took the pressure off my shoulders, and just said, "Well, just go with it.

INTERVIEWER: Focus on science. Do what you do best.

KETTERLE: What happened is almost history. Two years later my group had realized Bose-Einstein condensation. If I had started at any other university, I would not have been able to build up a lab in two years. So after two years, my group contributed to one of the most exciting developments in physics. We were right in the middle.

So what I did at that point-- I had actually thought Bose-Einstein condensation could motivate me for my whole lifetime. I would reach colder and colder temperatures, and study this and that. That this is the big goal which keeps you focused on good science for years and decades. But after two years I was there.

So I went to Dave Pritchard and said to him, "I would not have done it without you. I was to some extent executing ideas we had developed together. I was taking over the lab from you. I want you to co-author the paper." It's now one of the most highly cited papers in physics. Dave said, after thinking about it for a moment, he said, "No, I want you to take the full credit."

INTERVIEWER: So finish the story with Dave.

KETTERLE: So I still feel that what Dave Pritchard has done for me, how he has supported me and mentored me, was highly unusual. There were moments it was just difficult for me to even understand it. So let me try to use some words which Dave Pritchard used to explain his magnanimous behavior.

At some point he said, "Well, I gave up an experiment, but I got a wonderful colleague at MIT." At another time he said, which I think it's just wonderful expressing kind of the feelings-- he said, he said it in a speech-- he said, "I gave Wolfgang the keys to the family car because he could drive it faster than I could." So, driving the family car is sort of the two of us being a couple for so many years, collaborating on various levels, has really been a wonderful experience.

INTERVIEWER: Is that a value that permeates MIT in your experience?

KETTERLE: At MIT, there is the value of enjoying science and being exciting and using that as one of the, I would say, one of the most important motivations I think is true. I think the special circumstances of supporting somebody are very unusual. I think the atomic physics groups at MIT has with Professors Kleppner and Pritchard, has sort of this wonderful tradition and history and it's a very, very friendly family.

INTERVIEWER: Describe your relationship with Professor Kleppner.

KETTERLE: There are two more things about Professor Pritchard.

So that's how it developed and what was very satisfying for me, after I was established and became as famous as Dave Pritchard, or to some extent even more famous, the two of us could collaborate again.

Dave Pritchard told me we won't publishe papers together because he wanted me to get fully independent. But then as two independent people, we have now worked together, jointly supervised students for about a decade. In a way, what I also find remarkable that initially he, of course, was the leader. He taught me many things. He mentored me. But right now Dave Pritchard is very focused on educational science. So he doesn't really have the capacity to run a lab completely by himself. But in collaborating with me, he can still do wonderful physics.

So it's for me almost like a life cycle of the two of us have always been a team. A team when I was his postdoc, a team when we were independent, but-- the independent research groups, but that was a lot of advice and camaraderie and now we are collaborators again. It's just so life cycles.

One way how I was able to express what Dave had done to me was when he we came back from the ceremony in Stockholm. Back at MIT, I went to his office and gave him the original of the Nobel medal. I told him I couldn't share the honor of the Nobel prize with him, but that he has been a big part of it and this is how I want to express it. I know it meant a lot to him.

INTERVIEWER: What was it like to get the call?

KETTERLE: Not bad, but it's a crazy moment. It's sort of a moment where-- you're called up at five o'clock in the morning. So your brain is not fully working. You hear somebody telling you that you just wake up, get to the phone, and somebody tells you that he's from the Swedish Academy of Sciences and they want to tell me that I've been awarded the Nobel Prize in this year. I reacted with joy, with pride for MIT, for my collaborators, for myself. It's a wonderful acknowledgement, and yeah, it's very, very special. It's sort of great. You're struck by it, you are hit by it.

INTERVIEWER: How did the family react here at the Center for Atomic Physics?

KETTERLE: It was joyful. It was a celebration of pride, sharing. It brings together the whole community. It also brought together the whole field. Because for Bose-Einstein condensation, the Nobel Prize was given only six years after the discovery; the field was in full swing. So everybody who was working in the field felt elated because the whole field became distinguished as one of the important frontiers of physics.

You asked about my relation with Dan Kleppner--

INTERVIEWER: Yes.

KETTERLE: --well, let me fill in. Dan Kleppner was the PhD adviser of Dave Pritchard. To some extent, the way how Dan Kleppner was educating Dave and nurturing his career and allowing him to become independent and famous, that this sort of repeated itself between Dave and myself.

Now Dan is the senior member of the atomic physics group. I've always-- I mean, I've appreciated his-- the science he's done. He has enormous scientific skills. He has shown great taste through his career. He has worked on many important frontiers. But he also has this wonderful balanced personality. When I was an assistant professor, and I felt I was in competition and needed his advice, he always gave advice. So his balanced personality and his friendship and his calmness were very, very important. As a young assistant professor, I had this dynamic environment at MIT with all these wonderful students. But with my two mentors, Dave and Dan, I also had the experience, the tradition, and the wisdom. I felt when I-- also I was professor-- but there were situations where I didn't know what to do, I needed a sounding board. I always found it.

INTERVIEWER: Is that something you're passing along to your colleague Martin Zwierlein, a young physicist who's somewhat in the position that you were nearly 20 years ago?

KETTERLE: I hope so. At least I'm trying. I'm not sure if I have the same balance and wisdom as Dan Kleppner, but I'm trying to give him good advice whenever I can.

INTERVIEWER: What's the excitement that he brings to the department that you see in his work that maybe reminds you of experiences in the past?

KETTERLE: Well Martin is just a bundle of energy and joy. He can be excited about physics, he's understanding physics, he conveys physics. You just realize that everybody who's working with him is pulled along by him or even more positively, he brings the best out of people. But you really realize that after he came back to MIT and started his own lab, the sort of level of liveliness and energy and excitement in our hallway has increased.

INTERVIEWER: Does that mean different music being played on the CD players in the hallways in addition to the great physics?

KETTERLE: If you want to use that as a metaphor, yes. It's like a new, an additional dance with a lot of intensity. Intensity is good, where he brings sort of intensity to it. That's also what MIT is about. MIT is a high intensity place.

INTERVIEWER: How important is teaching and interacting with students in your experience here, in addition to being able to have access to fundamental research?

KETTERLE: I love to teach. Even if somebody would offer me to opt out of teaching I would not take this offer. I want to teach because it keeps me grounded, it keeps me connected to the basics, but also it forces me to, again and again, present the logical structure of physics and the basics of physics to learning students.

INTERVIEWER: What groups of students do you teach at this point? All graduates?

KETTERLE: I actually alternate between undergraduate and graduate students. The past semester I was recitation instructor in quantum physics for undergraduate students, for juniors. In the spring semester I will be teaching the graduate course in atomic physics.

INTERVIEWER: For undergraduates, what can you can convey to them that sort of maybe reminds you of yourself or is important about teaching? When you're presenting to them material that is well worked out in your mind but is a discovery in their mind? KETTERLE: I think it is this deep understanding. I-- when I teach, I think it is even more important than teaching the material to convey your own personality. To convey your personality as a working physicist. What young people need are role models, people they can identify with in terms of the passion they have for their subject or the intensity or they way they think, the way they analyze. That's what young people have to learn.

When I'm in the classroom, I like it if a question puts me on the spot. I may not know the answer but I have to derive it. I'm struggling, I'm reiterating. But I show my thought process to the students. Tell them what I check out. How I may construct the answer of something I know and then add something I have to think about it. I think to present yourself as a working physicist to the physics student is important.

INTERVIEWER: To convey that Legos are important over the long haul.

KETTERLE: Yes. I think what is maybe what I think is-- I think showcase here for MIT is these kind of mentorship. I mean there is something about-- I know that in the physics department we rarely hire senior people. We want to nurture ourselves young talent. So almost all the hires in our department are done at the assistant professorship. So we want to identify talent early on and then nurture and develop it.

A lot of the people have become the stars in their fields. Many of them have stayed at MIT for many more years or for their whole lifetime. This focus on young people and introducing young people or bringing young people to MIT and make sure they succeed, this is really a MIT tradition. I may have experienced it in an even more unusual form when Dave Pritchard was stepping back, was handing me over his lab. But in a way, what I experienced in that form reflects the culture which we have in the physics department.

INTERVIEWER: That's great. Well said.

KETTERLE: I don't know how much it really applies to all the departments. I don't know the hiring kind of policies of other departments.

INTERVIEWER: Well, I mean I think this mentorship idea is a consistent theme throughout all the interviews that we've done across many disciplines at the Institute. So it's not-- I don't think we've necessarily heard as dramatic story as you--

KETTERLE: This may be one element which also fits into tradition and mentorship, and that is when I received the Nobel Prize, I shared it with two of my colleagues and competitors in Boulder, Eric Cornell and Carl Wieman. But it's interesting that Eric Cornell was a graduate student of Dave Pritchard. He left shortly before I arrived. Carl Wieman was an undergraduate student with Dan Kleppner.

INTERVIEWER: Well there you go.

KETTERLE: So it's clear that there are other very famous people in the field who have really been educated by Dan or Dave.

INTERVIEWER: It's all in the family.

KETTERLE: Dan or Dave have really created a family of atomic physicists. Their impact on the field cannot be overestimated.