Opinions Libres
le blog d'Olivier Ezratty

Decode Quantum with Bill Phillips

Welcome to the 75th episode of the Decode Quantum podcast. In our series of episodes recorded in Lindau where dozens of physics Nobel laureates met with young scientists, we had a chance to meet **Bill Phillips**, who is one of them, after the **first episode** with **David Wineland**.

This podcast was recorded on July 1st, 2024, in Lindau, Germany during the **73rd Lindau Nobel Laureate Meeting 2024** with Fanny Bouton (OVHcloud) and myself.

Bill Phillips is American physicist from the same generation as Alain Aspect. He got his PhD in physics at the MIT working on nuclear magnetic resonance on the magnetic moment of the proton in H2O. He later did some work with Bose–Einstein condensates and then worked at NIST. There, he developed (actually, used) a technique to trap cold atoms in vacuum using lasers, called the Magneto-Optical Trap (MOT), in connection with an idea from Jean Dalibard, who was our previous guest. Bill is also a professor of physics at the University of Maryland. He was a laureate from the Nobel prize in physics in 1997 along with Steven Chu and Claude Cohen-Tannoudji, at the relatively early age of 49, by today’s standards. It was for his work on the Zeeman slower and other techniques related to the cooling and trapping of atoms. He was also participating in the panel on the future of quantum computing with Olivier Ezratty at the Lindau conference. By the way, his mother was Italian, and he happens to speak French.

What brought you in quantum science and then quantum computing? What inspired you?

Before we start with that, I probably should say a little bit about this bio, which isn't entirely accurate. I'm not sure where it comes from. The Nobel Prize really didn't have much to do with the magneto-optical trap. The magneto-optical trap is a kind of a workhorse. We all use it. But I neither invented it, it was Jean Dalibard who invented it, nor was I the first to demonstrate it. That was Steve Chu and colleagues at MIT, and then he did some collaboration, I think, with Steven Chu and Mara Prentiss, who were at Bell Labs at the time. I adopted the idea because it's such a wonderful idea, but it was more for the Zeeman slower and optical molasses, and the discovery of sub-Doppler temperatures, that I and my group were recognized for the Nobel Prize.

But anyway, let's come now to the question about quantum. How you fall in love in quantum. Well, the world is quantum. You can't really understand anything about the way the world works without understanding quantum mechanics. So, for me, it was sort of baked in from the very beginning. When I was a graduate student at MIT, one of the courses I took as a first-year graduate student was quantum mechanics taught by Kerson Huang. Now, Kerson Huang is a name that is known to almost all physicists because he wrote a really important book, about **Statistical Mechanics** (506 pages, 2nd edition 1987). And he was the most amazing lecturer. I just considered myself so lucky to be able to go to his class. It was an era when it wasn't so common for people to study quantum mechanics seriously as undergraduates, so I was really getting my first exposure to quantum mechanics from this just amazing lecturer, and I just loved it so much.

And then in the laboratory, we were working on things that were fundamentally quantum mechanical. We were measuring energy levels. We were manipulating the quantum states of nuclei and electrons in atoms in a quantum mechanical way, so it all fit together. So, I just felt that quantum mechanics was at the core of everything that I was doing.

Can you remind us what is the title of your thesis?

Yes, so in fact, I did two completely separate, unrelated theses.

The magnetic moment of the proton in H2O + Inelastic collisions in excited Na, 1976, MIT.

The first one was the magnetic moment of the proton in water in Bohr magnetons and the second was something like inelastic collisions of laser-excited sodium. The first is a measurement that still is the best measurement of that quantity that has ever been made, which bothers me sometimes to think that a measurement that I made something like 50 years ago, is still considered to be the standard measurement because you like it if somebody checks these things. On the other hand, it's sort of satisfying that my measurement is still the best one that it's held up over all this time, but it's held up because nobody has checked it. Well, anyway, so that's one thing. And that was probably the thing that got me my job at NIST, where I am now.

On the other hand, the second part of the thesis, no one cares about it at all. But it got me into the field of lasers. Lasers were a relatively new thing when I started graduate school. The laser had been invented ten years before, but it was just then that we were starting to get commercially available tunable lasers. So, you could buy a laser to do a specific job without having to be a laser expert yourself. And that changed everything about the way in which people did atomic physics. They could buy a laser and then do the physics that they were interested in instead of having to build the laser first and understand all the things about making a good laser in addition to using it as a tool. So, I think of it as sort of the difference between being able to buy a screwdriver and having to make one from scratch. There's no need to make a screwdriver from scratch. There's no need to make a laser from scratch unless it's got properties that don't exist on the market. We now had lasers that could excite sodium atoms, and you could do interesting experiments and that was the thing. It got me started doing laser cooling, because I had the background of knowing how to use these commercial lasers, knowing how to use them to excite sodium atoms. In that era, we didn't have very many different wavelengths available for lasers and about the only atom that was available to excite with an available laser was sodium. So I started doing experiments with sodium and sodium’s been very good to me.

What kind of laser was it?

It was a dye laser. Today people, quite rightly, denigrate dye lasers because they’re so hard to use and they’re not particularly good in terms of their line width and the stability properties but it was all we had at the time. Now we have much better lasers, but at the time, if you wanted a tunable laser, that was it.

Why did you do a thesis with two topics like this? Was it typical, or was it a shift in between on the path, or was it just creativity that drove you in that field?

Well, the first topic was done because Dan Kleppner wrote to me when he was reading my application to MIT and saw that I had done some magnetic resonance work as an undergraduate and proposed that I might be interested in coming to his group and continuing to do magnetic resonance, which I was. But about the time that I was finishing up that experiment, doing magnetic resonance, is when we started to get lasers in the lab. And I thought, well, that’s interesting. What I did for my thesis is the old style of physics. What is now starting up is the new style of physics. If I want to be part of this new revolution in which we do atomic physics with lasers, then I’d better do it now, as a graduate student, learn how to do this. And I went to Dan and I said, look, it looks to me like this is the wave of the future. I’d like to do an experiment using lasers, as part of my thesis. And he said, sure, why not? I mean, as a graduate student, you’re really cheap to the PI, to the principal investigator. He doesn’t have to pay very much to support a graduate student. And I was already experienced, so it looked like a good deal to him to just keep me on for longer. And it was a good deal for me because it meant I got to learn something entirely new that I hadn’t learned before.

It’s a story we heard from Alain Aspect as well, for two reasons. One is when he did these famous experiments around 82, there was a lot of engineering challenge, creating the laser, creating the whole device, and filtering the photons and so on, and the technique to count the photons, photon detectors and so on, and then he had the same experiment with a lot of stuff that came out of that.

Yeah, and Jean Dalibard probably made some of the acousto-optic modulators. And now you just buy them. And when I think about the things we had to do in those early days, we would turn the laser on and off by passing it through a rotating wheel into which we had cut holes. And now we just have acousto-optic modulators (AOMs) that turn things on and off.

So this experiment drove you to get interested in BEC (Bose-Einstein Condensates). What was the path?

Okay, so there, the thing was, after I finished my degree, I was successful in applying for a Chaim Weizmann Fellowship that allowed me to stay at MIT for another few years and do essentially whatever I wanted. So again, I went to Dan, maybe he came to me, I don’t remember, what should I do? And he said, well, you know, we’ve got this idea about doing Bose-Einstein Condensation of atomic hydrogen. It’s a new idea that’s just come up, and would you like to start working on it? That was in 1976, when I finished my degree, but it was 1995 when people first got Bose-Einstein condensation, but Bose-Einstein condensation in hydrogen was not achieved successfully until a few years later, maybe 1998.

I thought it was in 1995 but I’m not sure.

1995 was when people got a first BEC. But BECs with hydrogen were not achieved before 1998. Because it turns out that while hydrogen looks and looked to us as being an excellent candidate, because the interaction between atomic hydrogen atoms is very weak, that was exactly the wrong thing. What we needed was something where we had strong interactions between the atoms so that they could thermally equilibrate and we could do evaporative cooling, whereas with hydrogen that doesn't work very well. And the whole idea of Bose-Einstein condensation in hydrogen really is not a very good idea and that work never really went anywhere. But here's something very important. That laboratory, where I started the experiment but really didn't make much progress, produced the basic ideas for the rest of the world to achieve Bose-Einstein condensation, first in rubidium, then in sodium, with magnetic trapping and evaporative cooling. It was Dan Kleppner's laboratory that used magnetic trapping and evaporative cooling on hydrogen and then everybody else realized that's the way to do this with rubidium and sodium and other atoms since then.

So, I started this Bose-Einstein condensation. It eventually achieved it, but the whole idea of doing it with atomic hydrogen, was not the basis for a new direction of research. Whereas it was for rubidium with Eric Cornell, and sodium with Wolfgang Ketterle. And then after they had achieved it, we thought, now it's time to do this ourselves. And we started working on it as well.

I read somewhere that in your thesis, around your thesis, you did some software development. So in the 70s, can you remind that? It's interesting because some of our audience are IT people, so people who develop stuff, software and so on. It's interesting to learn about how you did software in the 70s.

For one thing, the programs were written in the 70s and in Fortran.

Alain Aspect wrote a part of programs on this experiment, and it was really important for this experiment that one of the guys knows how to code for that, it's not only to be scientist.

I certainly had to write computer programs, but I wouldn't really call it the development of software. We did develop some ways of determining the center of spectral lines that were asymmetric, that were both a mixture of dispersion and absorption, but it didn't seem like there was anything special about that. But as I said, it was written in Fortran, which…

It's like whatever you would do today in Python for whatever experiments, data exploration, but it existed then.

We were taking data. It was relatively new to take data with a computer at that time. But it was all very simple compared to what is done today. When I think about it, in the morning, when we came in, we would have to load the operating system into the computer. And that was done with paper tape.

Well, my first work as a student was on punch cards!

Well, yes, I used punch cards as well, absolutely. But the beautiful thing about punch cards is that on an optical table, you can use them to set the height of your optics. Because they're nice and robust, they're thin, and so we always had a good supply of punch cards to put the optics at the right level. But it's a different use case here, of course.

Let's talk about the MOT, which is a key, I wouldn't mean a key part of your life, but it's still an important one. You've been around this, so can you explain what it is? What is this technology, how it was invented, how it's used, because nowadays it's everywhere. It's amazing.

Right, exactly. So today it's what I call the workhorse trap of laser cooling, but it wasn't the first trap by any means. In fact, the first trap was something that we did in our laboratory. We made the first magnetic trap for atoms in about 1985, and it was a modified version of that magnetic trap that was used to make the first Bose-Einstein condensation in Colorado and at MIT in Cambridge. But the problem with the magnetic trap was that you had to laser cool the atoms first and then turn on the trap, and that's both a weakness and a strength. It meant that you didn't heat the atoms anymore, but it also meant that you weren't going to get any more atoms in than what you originally started with. Now a lot of people realized that the ideal sort of trap would be something that combined laser cooling with trapping at the same time. And it was clear that the radiation pressure that you could exert on atoms using lasers, the force that cools them, could also in principle be used to trap them. The question was how. And a number of people came up with a number of ideas for how to do it, which turned out to be unworkable for both fundamental and practical reasons.

In fact, Jean Dalibard made a visit to our laboratory and stayed for a while. And we worked on the idea theoretically and determined that one particular scheme wasn’t really going to work very well. We determined that instead what would happen would be that the atoms would be sort of viscously confined, and this is what Steven Chu called optical molasses. They did the experiment, completely independently of what we had done, but we realized that this idea of viscous confinement, not real trapping, was going to work really well, and it did. We did lots of experiments, and other people did lots of experiments on optical molasses, and in fact it was while using optical molasses that we discovered sub-doppler laser cooling, which was a big deal. But people still were trying to figure out how could we use radiation pressure to trap atoms, because the beauty of radiation pressure was that a relatively low-powered laser could exert a lot of force on the atoms, and you could make the laser beams big, so that meant that you could put lots of atoms in there, and you could capture the atoms very effectively from, say, a Zeeman slower, which was being used fairly widely, and there were other ways of slowing down atomic beams.

So this was obviously a way to get lots and lots of atoms, but people were trying to figure out how to make it work and as I said there were a lot of different ideas and they didn’t work and Jean Dalibard came up with this brilliant idea that made it work and it was to use a magnetic field that had the same configuration as what we’d use for trapping but to use it as a way of not directly exerting a force on the atoms but shifting their energy levels so that they would absorb light from the laser in different ways and this was what led to the magneto-optical trap (MOT). And it just changed everything. Almost overnight it changed laser cooling from something that was done by a few select groups to essentially everybody, and it’s still like that today. Atomic physics is dominated by cold atom physics and cold atoms starts in the laboratory with the magneto-optical trap and when was that?

When was the idea of using the three directions and the six lasers invented?

That idea was part of the optical molasses. That idea was already implicit in the first theoretical idea for laser cooling in 1975 by Theodor Hänsch and Arthur Leonard Schawlow (see **Cooling of gases by laser radiation**, 1975, 2 pages). They were the ones, along with David Wineland and Hans Dehmelt, who first came up with the ideas of laser cooling, which were really equivalent. But the Hänsch and Schawlow (method) was specifically directed toward neutral atoms and the Wineland and Dehmelt (method) was specifically about ions, but the physics is basically the same. I can’t remember exactly what Hänsch and Schawlow said in their paper, whether they actually described a 3D configuration (*they did*). It was sort of obvious that that was what you would want to do and it’s what we did with Jean Dalibard in that theoretical study that led to the idea of optical molasses and what Steve Chu and his colleagues actually realized in the laboratory in about 1985.

And so that configuration already existed as sort of an obvious thing to do and so adapting that to the magneto-optical trap was a natural step. Now when Jean thought of the magneto-optical trap, he was thinking about it in one dimension. Now it turns out that it generalizes to three dimensions without any difficulty at all. Maybe that isn’t so obvious, but it does. And the other thing that’s amazing about it is it was developed independent of the idea of sub-Doppler cooling but the sub-Doppler cooling works just fine in the magneto-optical trap as well. So that means you get lots and lots of atoms at high densities, but you get pretty cold temperatures.

But then you work with Claude Cohen-Tannoudji on the so-called Sisyphus effect, which is another one.

I wouldn’t say I worked with Claude. I mean, it was Claude and Jean who came up with the idea of — see, we discovered sub-Doppler cooling, but we did not understand it. So what do we do when we don’t understand something? We call up Jean Dalibard and Claude Cohen-Tannoudji and say, look, we found this thing. This thing that we don’t understand at all, and can you help us understand it? I later learned they were working on a completely different idea to do sub-Doppler laser cooling, what’s called velocity-selective coherent population trapping. And their first thing was they’re thinking, oh, did these guys accidentally do velocity-selective population trapping? Well, we didn’t—it wasn’t that simple at all. And then they thought, okay, what’s going on? And they’d already done a theoretical study and, in fact, an experimental realization of a kind of Sisyphus cooling that was not the kind of Sisyphus cooling that we were seeing but had some common features.

So, I think that made it easier for them to understand what was going on. The main thing was that they understood that this was working because of the fact that the atoms we were using were not two-level atoms. And people often joke that I said, “there are no two-level atoms and sodium isn’t one of them.” Well, another thing I should say is that before we discovered sub-Doppler cooling, we realized something anomalous was going on in optical molasses. We knew how optical molasses should work with a two-level atom. We thought that that should describe what was going on with our sodium atoms in optical molasses because, even though we realized it was not a two-level atom, we thought that it didn’t matter because all the upper levels decayed at the same rate, and the temperature you come to with laser cooling is dependent only on what the decay rate is from the upper state, as well as the detuning. And all the upper states are degenerate if you are in zero magnetic field, or so we thought, and so everything should work.

Well, it didn’t. When we started making careful measurements of the behavior of atoms in optical molasses, it did not agree with the theory. The molasses got stickier as we tuned the laser further from resonance, and that was not what was supposed to happen. It was supposed to maximize its effectiveness at about a half a line width detuning, and it did not, and other things.

And I went to spend a month in Paris in 1987, and one of the things that Jean and I worked on very hard was, can we explain what is going on? And the answer was no, we could not explain it with the two-level atom. And I remember going to a conference after I’d spent that month in Paris and giving a talk about how none of this stuff was behaving the way it was supposed to, and saying, does anybody have any ideas? Well, nobody did, and I kept asking, and in fact, I remember talking with one theorist and I said, you know, this whole thing would make more sense if the temperature of the atoms was a lot colder than what the theory says is possible. Is there any way that that can be the case? And he said, absolutely not. There’s no way the temperature can be colder than what the prediction of this theory that everyone believed in, the two-level theory, because there’s only one time scale in the problem, and that’s going to set what the energy is.

That was totally wrong, but that’s what everyone believed at the time. And then we started making measurements of the temperature, and that’s what broke everything loose was we found out that the temperature was lower than what theory said was possible, and that changed everything. Once it was clear that that was the case, then people realized they better come up with a different idea, and that’s what Jean and Claude did. They explained the laser cooling. We now often say Sisyphus. They realized there were two different mechanisms that could work. One was the Sisyphus mechanism that we all think of today. The other one was a more subtle mechanism that was characteristic of a one-dimensional system in which the two counter-propagating laser beams have opposite circular polarization. Now, the experiment was done with linear polarization. And the Sisyphus theory that Jean and Claude worked out was a one-dimensional theory that involved the polarizations being perpendicular to each other. Now, that’s not what we had in the experiment. But the thing is that in three dimensions, you can never have all the polarizations in the same direction. If they’d all been in the same direction, then we would have seen the two-level result. That wasn’t what was going on. And so, their model, in a sense, captured the key things that were going on in the three-dimensional situation. Their one-dimensional model captured what was going on. And it was enough to propel us to optimize the process. We could understand enough from their simple model to really make things work beautifully.

There’s a back story to your story, which is international collaboration.

Well, sure.

Because we hear a lot about competition between China and the U.S. and other places. So you are, even though it’s about three decades old, it shows that international collaboration is fruitful. It helps science.

Absolutely. Ideas come from everywhere, not just one place. And there’s even more to it than that. Now I can’t prove that this is true, but I’ve always felt that the fact that I spoke French was a contributing factor to the close relationship that I developed with Jean and Claude. You know, I spent a month in their laboratory and then the next year I spent a year in their laboratory. And I insisted, even though they speak English far better than I will ever speak French, I insisted that we should always speak French in the laboratory. Because, well, that’s what you did. And it sort of became a joke that I always carried around with me a little notebook. And every time I learned a new word, I would write it down in my notebook. But after a while, I got to be pretty good at physics French. Not so much at Le Français Quotidien, but for the laboratory.

I know the sensation, I was really bad in cooking, in English, but in computing it was okay for me (Fanny).

Yeah, exactly. You have a certain specialized vocabulary, and the vocabulary in science is pretty international.

And sometimes you have two words in French when you have only one word in English.

De temps en temps, c’est comme ça, oui.

It happened very early with Alain Aspect, because when entanglement was, let’s say, experimented in the Bell inequality test, there was “Enchevêtrement” or “Intrication”. So, Alain chose “Intrication”, which was much better than “Enchevêtrement”. Because the correlation is better with Intrication than with Enchevêtrement.

Okay, well I didn’t know that.

It’s an example. I worked with him on that, on how you are very picky in choosing the right terms for…

And he and I often have conversations about fine points of French grammar and French usage. And we both agree that I am a pinailleur, which in English would roughly translate into a nitpicker, someone who is concerned about very tiny issues. And he was my French professor when I was in the laboratory there, because he was at the École Normale.

He was there back then, at LKB.

Yeah, he was at the École Normale when I was spending first a month and then a year. And we shared an office. And he would correct my French for me. And one of the things that I think Americans do this commonly is we…We think that “ne” is sufficient to make a sentence negative, but it’s the “pas” that really makes the sentence negative.

Souvent, les français disent, “il faut pas faire quelque chose”. “Pas”, “il ne faut pas”. Parce que le “ne”, ça sert presque à rien. And it took a while for me to learn that. And he constantly would have to remind me to not forget the “pas”.

We are talking about things that happened about three decades ago. What's been your journey in cold atoms, control and experiments since then? You worked a lot.

It seemed like we were continually learning new things and finding new directions to go with these cold atoms. So, for example, we decided we should study collisions. And that opened up a completely new direction of research that we didn't anticipate at all. Fortunately, we had some of the best theoretical people in the world to do the theory for collisions. Paul S. Julienne, trained as a chemist, was really just the right person to figure out the quantum theory of the collisions of cold atoms. So, he just created the quantum theory of cold collisions. And this was really important because everything that we do involves interacting atoms and understanding the way in which they behave when they collide or undergoing collisions in this very strongly quantum regime. Before then people would typically think of collisions as involving a trajectory, and now it was clear that was not a way that you could do things.

You had to think of these things as being partial waves rather than trajectories. Now everybody knew about the partial wave way of thinking about things, but it wasn't necessary for a lot of the atomic collisions until the atoms were so cold, and that was just something that came up because we started to explore some area, and then we accidentally discovered optical lattices, and then optical lattices became a big thing, and today the new generation of atomic clocks are probably going to be atoms trapped in optical lattices, and then of course Bose-Einstein condensation, and optical tweezers. It just seemed like a big thing every so often something new would come up and we'd have a new direction of research to follow. And then Deborah S. Jin comes up with Fermions.

Yeah, Fermionic computing is a new topic in town that we have to study. By the way, aren't you like a kid in a candy store when you see all the stuff we do right now with Mikhail Lukin's experiment?

Oh, absolutely. It's marvelous when you compare it to…Look at Mikhail Lukin's experiments. We made a cartoon movie of moving atoms around in an optical lattice as sort of showing what we were dreaming about for a quantum computer. And now Misha is doing it. Almost exactly what our cartoon movie was showing. He's actually doing those things.

One last question because we're running out of time. How do you feel to be here at Lindau and meeting with all the brightest young scientists around the world? It's a special moment.

Absolutely so. It's lovely to be here. I mean, of course Lindau is beautiful, although it's been raining, and being on Lake Constance and going out to Mainau. But the reason that we come, as I see it, is to interact with the young people. And everything is done to make that work really well. The students are highly selected. They're going to be students who are eager to ask questions. I encourage them to ask questions by offering them a prize. For anybody who asks a question, I give them a wallet card of the fundamental constants of nature. But I always ask lots of questions. Whenever I'm in a talk or when I visit somebody's laboratory, I'm always asking questions. So, I'm very happy to have the students here.

Thank you, Bill Phillips, for this discussion, and thank you to all of you for being here.

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