ICAP 2012

23-27 July 2012
Ecole Polytechnique
Palaiseau - France

The 23rd International Conference on Atomic Physics

Round table, Tuesday July 24th, 2012

The emergence of a new field, 1985-1995: from atom cooling and trapping to Bose-Einstein condensation

Chair: **Jean Dalibard**

Participants: **Claude Cohen-Tannoudji** (ENS and Collège de France, Paris, France)

Roy J. Glauber (Harvard, Cambridge, USA)

Wolfgang Ketterle (MIT, Cambridge, USA)

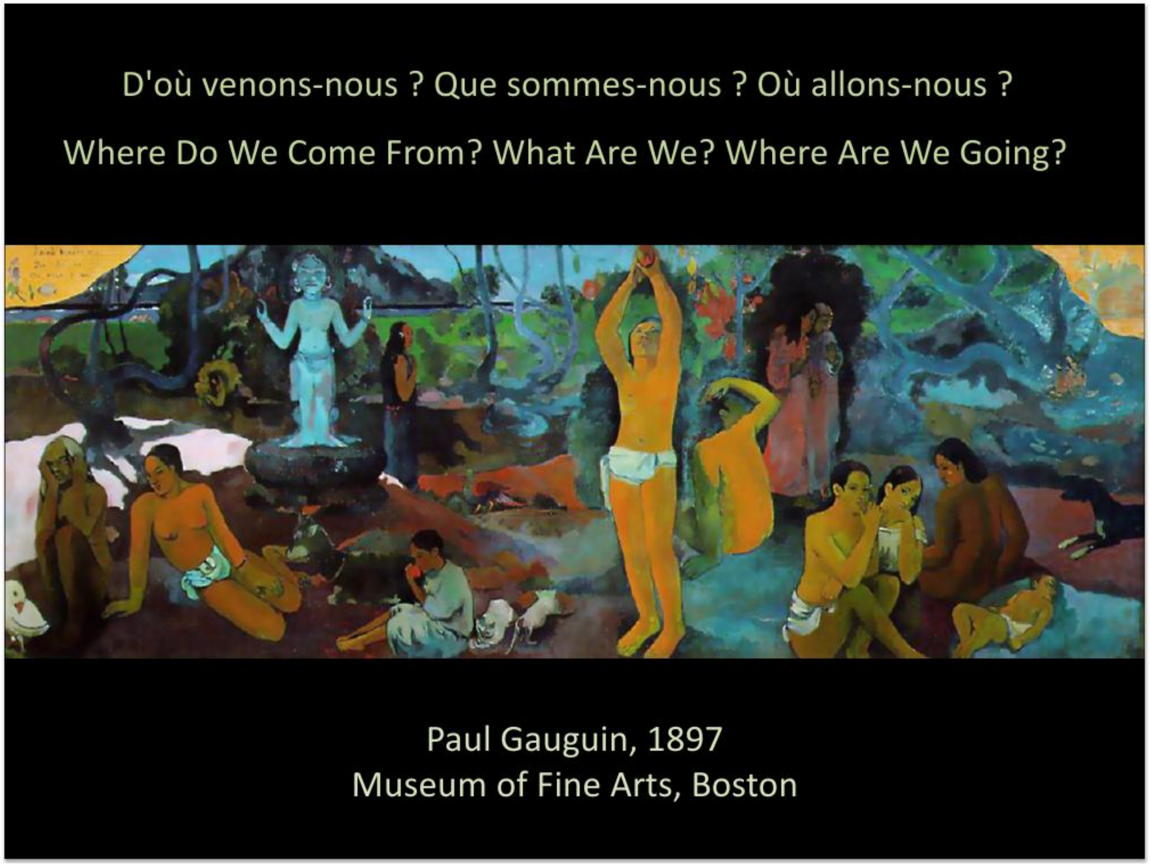
Daniel Kleppner (MIT, Cambridge, USA)

William D. Phillips (NIST, Washington, USA)

Quie2t QIPC in Europe

Photographs: J.-F. Dars. Audio recording: E. Corsini. Realization: H. Perrin & J.-F. Dars.

Jean Dalibard – To introduce this session, I would like to start with one sentence, it’s a sentence in French, “D’où venons-nous, que sommes-nous, où allons-nous ?” For those of you who don’t understand what this means, there is an English translation and it says also from where the sentence is taken, it’s the title of a painting by Paul Gauguin,…



…, so this is “Where do we come from, what are we, where are we going?”, and I think this painting which was painted in 1897 is a good introduction of our guests today because it was painted by a Frenchman and you can see it in Boston. So, I know that Bill, you are not living in Boston but I think your Boston introductions are strong enough that you can also be included into that.

I think this would be a nice subject for this round table, trying to understand these three sentences. We will start with the first two ones and then the questions from the audience can address "Where are we going?" which is probably the most important thing as it is now. Now as a guide for this round table I would like to use as a guideline a few books: I like books because it is a good way to get some landmarks in the history of the field.

Les Houches 1964

Université de Grenoble
Ecole d'été de Physique Théorique
Les Houches 1964

OPTIQUE ET ELECTRONIQUE QUANTIQUES

QUANTUM OPTICS AND ELECTRONICS

Lectures delivered at Les Houches
during the 1964 session of the
Summer School of Theoretical Physics
University of Grenoble
edited by:
C. DeWITT
Centre National de la Recherche Scientifique
A. BLANDIN
Faculté des Sciences — Orsay
C. COHEN — TANNOUDJI
Ecole Normale Supérieure — Paris

GORDON AND BREACH, SCIENCE PUBLISHERS
New York London Paris

DUNOD
PARIS

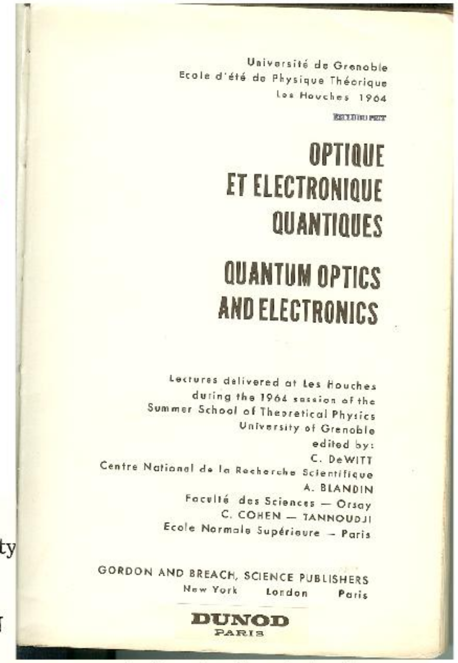
R. J. Glauber
Harvard University

Lecture I. INTRODUCTION

The field of optics, after seeming to have reached a sort of maturity, is beginning to undergo some rapid and revolutionary changes. These changes are connected with things which we have, as a matter of principle, known about for many years, but the extent to which we could put our knowledge into practice has, until just a few years ago, been extremely limited. Thus the electromagnetic character of

And as a starting point I would like to start with this volume of the Les Houches School, 1964. This school was organized by Cécile DeWitt, it's written on the corner here, by Cécile DeWitt, André Blandin and Claude Cohen-Tannoudji, who is here, and one of the lecturers at the school was Roy Glauber, who is also here. So this is the text of Roy, the first sentences of Roy's contribution to this book and I think everyone should read it, at least I'm going to read here the first sentence:

Les Houches 1964



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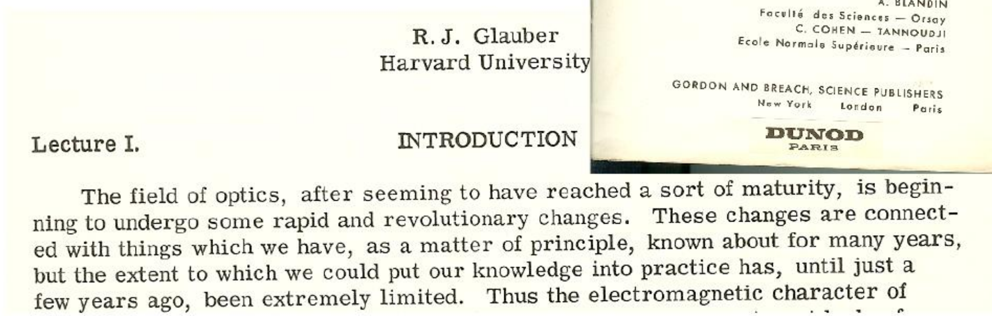

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Lecture I. INTRODUCTION

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"The field of optics after seeming to have reached a sort of maturity is beginning to undergo some rapid and revolutionary changes." And I think we can start with this and with Roy and I think it was kind of a very, very insightful sentence and so maybe Roy can start by telling us where you viewed the field at that time and by saying it was to undergo revolutionary changes and whether these changes that you were expecting at that time have been realized?

Roy Glauber – There was some number of things going on at that time, that was of course the early period of the laser, the helium-neon laser was in evidence for a couple of years, it was clear that this was going to change many things in optics. All sorts of measurements which were possible or within view were beginning, but there were several other things going on at the same time…

Quantum electrodynamics had reached a sort of maturity, it was clear that there was excellent sense in the theory which was not clear earlier, it was a matter of clarifying, what I call briefly the renormalization program, once that was out of the way we had a theory of electrodynamics, but historically with certain very strong limitations. One was that the complicated calculations of quantum electrodynamics had been confined just to single photon problems or perhaps two-photon problems, but they went no further!

With the laser in the air and an altogether new way of generating light, it was clear that we would need a great deal more. There were a few other things clear as well: it was possible to count individual photons very close to the beginning of the century, that just followed through from the photoelectric effect. That technique had existed for decades but was really never used for anything useful.

The assumption seemed to be in the air that light particles, photons, arrived just as rain drops fall from heaven but a little more intensely, completely randomly! It was assumed that if you set up a steady light beam, and counted photons, you would always have a Poisson's distribution and therefore never measure more than the intensity. For light beams there were much simpler ways of doing that! That was a tool which didn't exist and wasn't used analytically at all.

Things began to change in that period and it was primarily the laser of course that caused the change but there was another source. The radioastronomers Hanbury-Brown and Twiss invented a technique of interferometry that depended on detecting intensities at two points and therefore in any visible light would involve detecting light quanta at two different points. And these chaps who were radioengineers and had read the early chapters of Dirac's book were told flatly that there is no such thing as one photon interfering with another, that the interference process, I think he was talking of course about the Michelson interferometer, the interference was always of one quantum with itself.

They could scarcely believe that and they did an experiment in which they wanted to reveal such an interference on the laboratory scale. They did that and used a highly monochromatic steady light beam with the proverbial half silvered mirror in it, and looked for correlations in the light amplitude for these two parts of the beam and found in fact that there were such things as photon coincidences which were not one hundred percent random in an ordinary light beam. That was the beginning and that in fact was where I started on this business.

Jean Dalibard – Thank you Roy, we'll certainly come back to that in the following… Now, I would like to give the word to Claude since the name of Claude also appears in this book. So Claude, I think that at that time already you had the idea that light was not simply a way to get information but also a way to control atoms, so maybe you can say in a few words how the ideas have evolved from, say, the fifties up to the eighties on this idea of controlling atoms?

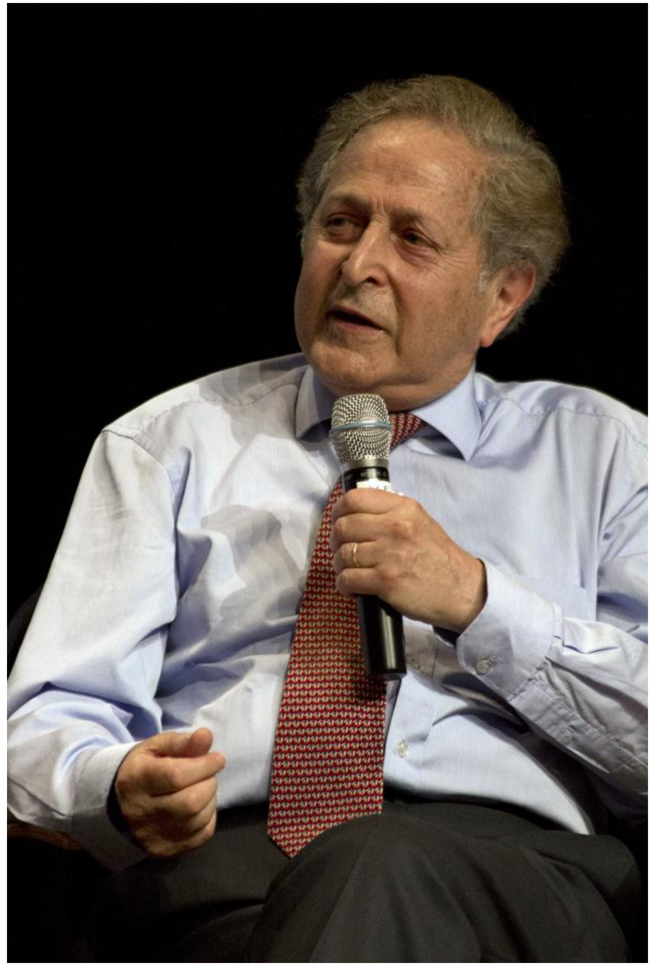
Claude Cohen-Tannoudji – Perhaps I would like to add some words to the moving paper of Dan this morning, concerning the memory of Norman Ramsey. I wanted to say that my first encountering with Norman Ramsey in Les Houches summer school in 1955, where I was a student, and this is where I learned for the first time what are molecular beams, and molecular spectroscopy. Then you know that I did my PhD with two outstanding people, Alfred Kastler and Jean Brossel, who had the same charisma and the same concern with young students as Norman, and I was making findings very similar with them as what was told this morning.

So it was a very nice time for me. And then after that in 1963 there was one of the first meetings on lasers in Paris organized by Nicolaas Bloembergen and Pierre Grivet and that was the first time I heard speaking about lasers. And after one session we went with Cécile DeWitt and Professor Nierenberg who was at the scientific division of NATO occupying the same position as Norman Ramsey a few years before and we talked about what we should do next year for the summer school, and I remember Nierenberg said me: you should invite Roy Glauber, he has done very important things.

And this summer school was fantastic! There was Willis Lamb, he gave a lecture. So people were staying two months, you know, two months in the school! Willis Lamb gave a fantastic lecture on the theory of lasers, and I remember Roy and Willis were fighting every day, asking more questions than Bill! In his lecture, Roy was explaining everything quantum mechanically and Willis Lamb wanted to do everything classically. That was extremely useful for the students!

And also we had a wonderful set of lectures of Nicolaas Bloembergen on nonlinear optics, and Jean Brossel on optical pumping and on QED, on quantum electrodynamics. That was a fantastic session. Then at the time, in the lab we had a lot of excitement, there was a very good atmosphere. In fact optical pumping can be considered as one of the first examples of manipulation of atoms by light. You control the angular momentum which allows a lot of things.

I did my PhD in 1962. We had a lot of collaborations in particular with an Italian group, led by Professor Adriano Gozzini, who was a very elegant physicist. And he found dark resonance and population trapping, which turned out to be useful for laser cooling a few years later. You know, what is nice in our field, is that many effects, which are discovered at some time, will be useful twenty, thirty years later. That was the case of light shifts, the case of dark resonances, we had a lot of good surprises.

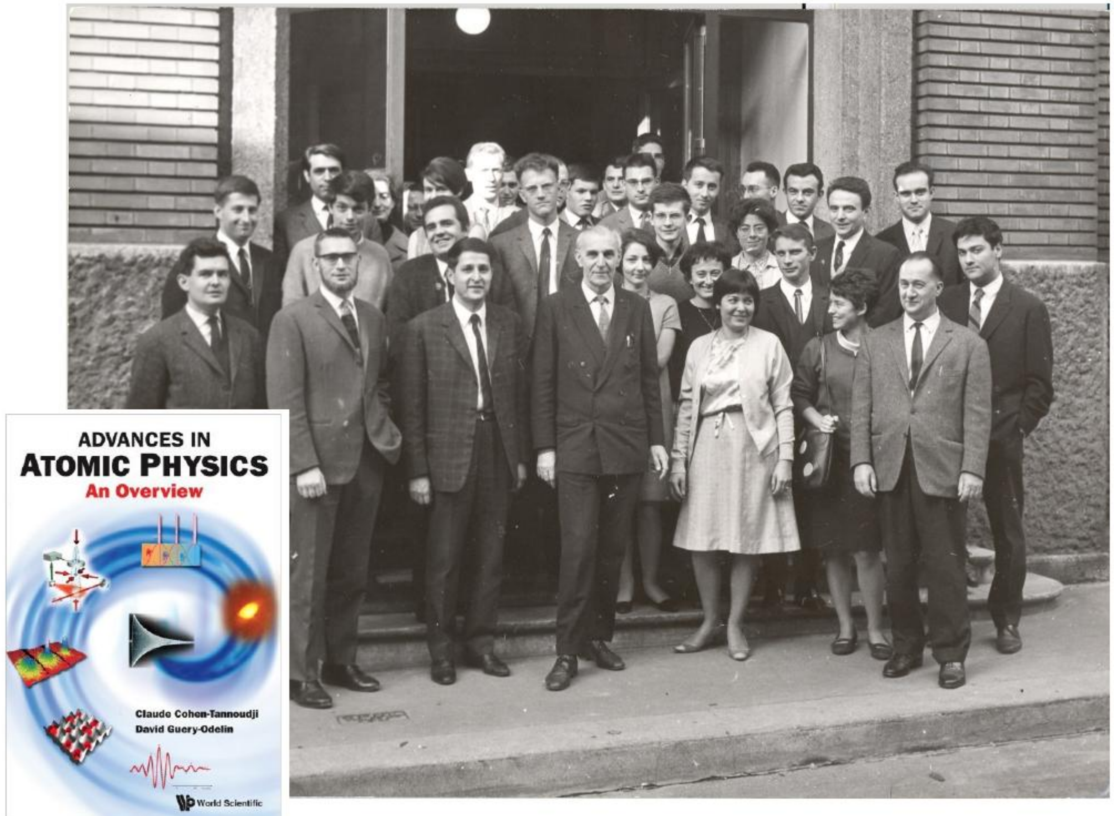


And then one very useful thing for me has been my nomination at Collège de France, in 1973. I stayed there during thirty years, and you know in Collège de France you have to make a different lecture every year, you will know that (*to Jean Dalibard*) ,because you will be there at Collège de France, and I think that the best way to learn something is to teach it! And I think that during the beginning of the nineteen-eighties I gave four years of lectures on cooling and trapping of ions and atoms.

And I learned a lot of things and we had an idea that we decided to start a group with Alain Aspect, Jean Dalibard, who just finished his PhD, Christophe Salomon. It was a fantastic adventure, we had a lot of collaboration with Bill’s group, with Steven Chu’s group also, so it was a fantastic time! So I think the best thing I can remember of this time is the excitement, we were very excited to do this kind of work, I think Bill probably would say he was feeling the same!

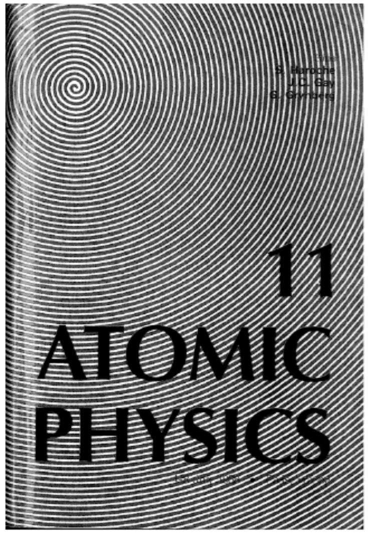
Jean Dalibard – I have a picture of the lab, that you sent me, I don't know which year exactly…
Claude Cohen-Tannoudji – You see Alfred Kastler, ...

You see Alfred Kastler, Jean Brossel is at the right, Serge Haroche is here, he was my first PhD student! I'm here, left of Alfred Kastler, Michèle Leduc is here, Bernard Cagnac, who did two-photon spectroscopy, and Marie-Anne Bouchiat was the first to suggest that you can detect parity violation on atoms. So you see that there was a fantastic atmosphere!



Jean Dalibard – And since we were speaking of books, there is this book…
Claude Cohen-Tannoudji – Yes, after thirty years of teaching at Collège de France, we decided with one of your students, David Guéry-Odelin, to write a book summarizing and giving an historical perspective of all these developments. It is exhibited at the World Scientific booth at the conference.

ICAP 1988



Proceedings of the Eleventh International Conference on Atomic Physics

4-8 July 1988 Paris, France

OBSERVATION OF ATOMS LASER COOLED BELOW THE DOPPLER LIMIT

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Harold J. Metcalf
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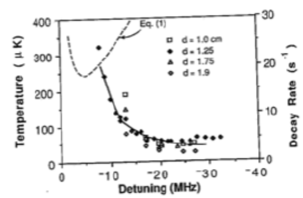
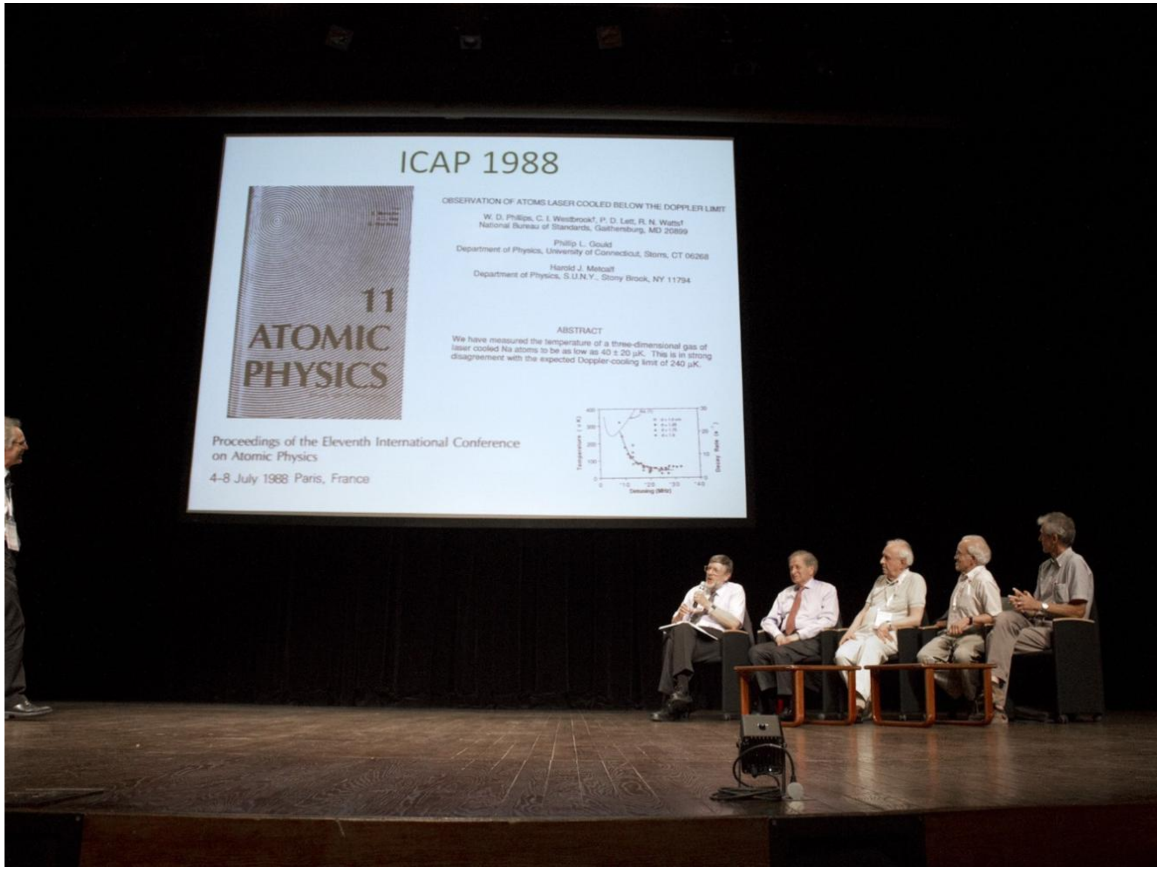
ABSTRACT

We have measured the temperature of a three-dimensional gas of laser cooled Na atoms to be as low as 40 ± 20 μK. This is in strong disagreement with the expected Doppler-cooling limit of 240 μK.



Jean Dalibard – So since Claude already mentioned it, let's make a jump. This book has been mentioned already yesterday, this is a book of the Atomic Conference which took place in Paris in '88, so twenty-four years ago, and there were many, many interesting events at this ICAP, like this one, and one of the major things was a report by the group of Bill, about observation of atoms laser-cooled below the Doppler limit. The Doppler limit was a kind of dogma at that time, and so Bill maybe you can say what was your feeling when you started violating dogmas…



Bill Phillips – Well, of course this was a story in which you played quite a big role as well! You may remember that I first met you, I think it was in 1983, when I was returning from a Laser Spectroscopy Conference, and I stopped at the Ecole Normale and I met you, and I had met Claude before at MIT and I started to get to know him better, and you came to what was the National Bureau of Standards the next year, and one of the things that we discovered in doing some numerical simulations was that there was this idea of optical molasses, that somehow atoms would be stuck in laser beams, that were cooling them…

… and a year later Steve Chu saw this in his lab and observed that the temperature was exactly the lowest temperature that was predicted by the theory! And then we reproduced those experiments a little bit later and we confirmed that the temperature was exactly the lowest temperature predicted by the theory! And then we did something that I think everybody needs to do, we started to fool around a little more.

But the first thing that we did when we fooled around is that we studied the stickiness of the molasses, because it was so hard to measure the temperature, and we couldn't figure out what was going on, because it was not behaving correctly. And, do you remember, in 1987 I spent a month in the summer at the Ecole Normale, and we two worked together trying to figure out some way of explaining the bizarre behavior of the stickiness of the molasses. And we couldn't, and I went to another Laser Spectroscopy Conference that summer and reported on this strange results and nobody paid any attention…

And then we went back to the lab and decided, well, we can't understand what's going on, let's measure the temperature. And let's do a really good job in measuring the temperature. Now, you see, the problem was, it was really hard to measure the temperature. Here we were, at the National Bureau of Standards, and we didn't have a good way to measure the temperature, so we invented four ways of measuring the temperature. And got this rather remarkable result, where the Doppler cooling limit said that you couldn't get any lower than that, and we found that we couldn't get any higher than that! Well… or almost no higher than that!

If you tuned your laser to the place where the theory said you would get the lowest temperature, sure enough you would get the temperature that was about as high as the old theory said, but when you start tuning the laser further from resonance, the temperature just kept going down! And of course what we thought immediately was: “we are doing something wrong”! So we spent the next several months trying to prove that we were wrong, and when we finally ran out of ways to prove that we were wrong we had to accept the fact that we were right!

And I remember giving you a call, and I think I talked to both you and Claude and explained what was going on, and it didn’t take you very long before you figured out what was happening! Theoretically the two of you described what we now know as the Sisyphus effect, as well as a whole bunch more stuff. And we used something that is also very interesting, your description of the Sisyphus effect was absolutely beautiful.

But of course today we understand that you should really describe things completely quantum mechanically. It was a semi-classical theory and it really doesn't capture everything that is going on. But it captures so much of what's going on and it is so beautiful that we still teach our students about the semi-classical Sisyphus effect even though we know that the right way of describing it is a fully quantum mechanical description.

And then I came to Paris for that famous ICAP, the eleventh ICAP, and presented these results but probably the most exciting thing was that when the three of us were sitting together in a small conference room at the Ecole Normale, talking about what we could do to test whether your new theoretical explanation was the right one, and I remember that I called back to the lab while we were having discussions, and told them what new experiments they should start to do to test the new theory. And this was an incredibly exciting time.

Jean Dalibard – There was no mail at that time.
Bill Phillips – There was no email! Yes, and telephone calls were very expensive at that time! But it was worth it! But you know, just sitting here, that was an incredibly exciting time. I think we all agree. And in fact, Jan Hall said that it was an excitement that was akin to what it must have been like during the time of the impressionists in Paris. And I have a picture of you and Hal Metcalf and myself on Monet's bridge, at Giverny, to sort of emphasize that excitement. But you know, listening to all things that are going on at the present ICAP, if somebody was to ask me: What was the most exciting time in history for cold atoms? I would have to say: It's right now.

The Green Book (1993) & Varenna (1997)

Prospects for Bose–Einstein Condensation in Magnetically Trapped Atomic Hydrogen

Thomas J. Greytak

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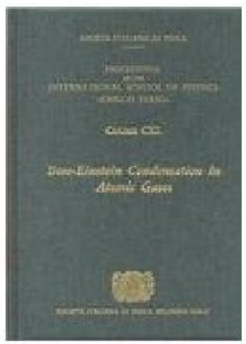
Massachusetts Institute of Technology
Cambridge, MA 02139
USA

Acknowledgments. The research on spin-polarized hydrogen at MIT has been, since its inception, a collaborative effort between the author (a condensed matter physicist) and Daniel Kleppner (an atomic physicist).

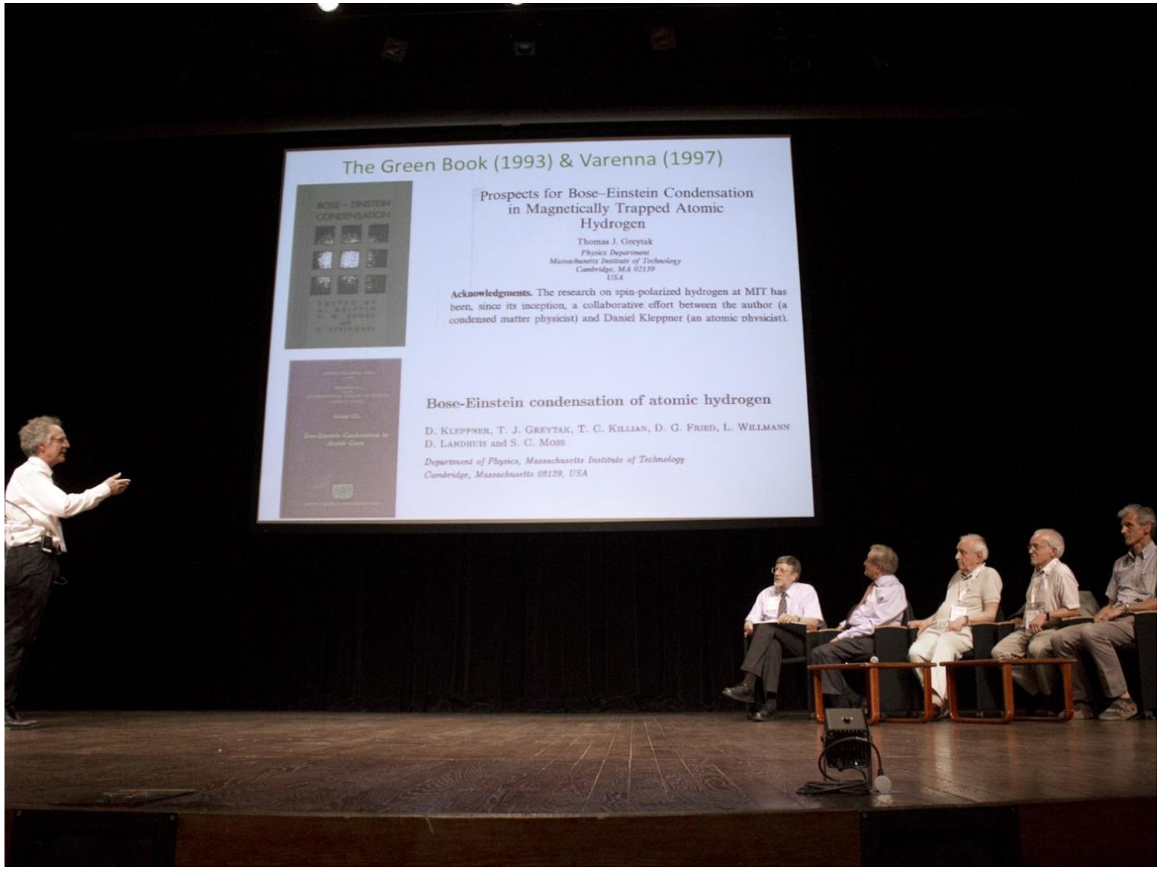


Bose-Einstein condensation of atomic hydrogen

D. Kleppner, T. J. Greytak, T. C. Killian, D. G. Fried, L. Willmann
D. Landhuis and S. C. Moss

Department of Physics, Massachusetts Institute of Technology
Cambridge, Massachusetts 02139, USA

Jean Dalibard – We are precisely coming to the current period. Still with books: I would like to point out two books which have been also very big landmarks in the field. The first one is the green book, and the next one is a few years later, in '97 [*Jean means '98*] the lecture notes of Varenna. And here, I would like to give the word to Dan, because I think, in the green book, there is a paper by Tom Greytak.



Tom and you are colleagues. Tom signed the paper but it is mentioned at the end that it is a collaborative effort since its introduction between a condensed matter physicist and an atomic physicist, which at that time was not so common, actually. Now, we have all big connections between condensed matter and atomic physics, but at that time you were already, for that also, a precursor. So maybe Dan you can explain what has been, actually, the path to BEC, seen for hydrogen and for all other atoms, BEC of a gas. Because hydrogen was the starting point.

Dan Kleppner – Well, I can give my account of that history of those early days, but I should say it's only half a history, because the pursuit of BEC in hydrogen, which started the whole pursuit of BEC, was really carried out independently by two groups, Tom Greytak and myself at MIT, Isaac Silvera and Jook Walraven in Amsterdam. And, so, I can only give the MIT view of things. Jook is here, and it would be great to get his view of that history. Now, as it turned out, we continued on, and eventually did get BEC in hydrogen, but we would never have done without their work too.

So that is a tribute that really belongs principally, I'd say, to two independent groups. Anyway, for our group, at MIT, I remember the history very clearly because there was an article in Physical Review Letters in April 1976 by Lewis Nosanow and Bill Stwalley, on the equation of state of hydrogen, deuterium and tritium at very low temperatures, and it made a comment that the hydrogen would remain a gas, spin-polarized hydrogenif it is in a triplet state, would remain a gas down to absolute zero, and one may be able to achieve Bose-Einstein condensation.

And they mentioned some of the numbers that you would need for that: a density of ten to the fifteenth, a temperature of a tenth of a degree Kelvin, something of that. And I remember reading the article and thinking: that's absolutely preposterous, why do they make suggestions like that which are so ridiculous! I had a fair amount of experience with hydrogen and a hydrogen maser where we had densities of ten to the fourteenth at room temperature, and the possibility of actually cooling hydrogen seemed rather remote.

Tom Greytak also read that article, and asked me about it. And I explained: Yes, I've read it, but it's just a preposterous suggestion, and by the way, what is Bose-Einstein condensation? Because I'd never heard of it! Well, Tom explained it to me, and said it was a pity we can't do it, because it is really kind of interesting. And out of that conversation, where I started bringing up one objection after another, and then pointing at a possible way of solving it, really based on my earlier experience on the hydrogen maser.

In a hydrogen maser atoms go into a bulb, and rattle around, and come out. And you have to be very careful about what you put on the surface of the bulb. We tried various things. It turns out teflon works quite well, saturated hydrocarbons and things, but during those discussions we were thinking: what a pity we couldn't have a noble gas surface, because that would be the ideal surface. So when this idea came up, the possibility of using liquid helium coated surfaces suddenly became very attractive.

Liquid helium is the ideal material for coating a surface, because it runs everywhere, all by itself. And then there was the problem of supplying the atoms because in the hydrogen maser, if you use the state selector, and getting enough atoms with the state selector seemed very difficult, but then as the temperature goes down, the requirements on the state selector go down, and in fact, we suddenly realized that at low temperature all you need is a magnet! If the atoms were in the right state they'll come in, and otherwise they won't.

So a few things came together, and we decided to start some experiments. Now, the spirit of this experiment was very much like the spirit of Ramsey's laboratory, where he felt that a good laboratory had somewhere going on where the objectives are quite clear. And other work, where you don't know what you're doing! And this would clearly be in the 'you don't know what you're doing' category!

But, we started playing with it, and one of our fresh PhDs there joined the group cause he was kind of intrigued by it too, and that was Bill Phillips! So his start out in this area really was in cooling, cryogenic cooling of atomic hydrogen. So we did some experiments, and found that we could get hydrogen down to below liquid helium temperature and so that intrigued us, and we wanted to carry on from there.

Something that was very influential was a conference that Franck Laloë organized, that was in Aussois, and I forget exactly the year, when it was... Franck, do you know? Was the year '78 or '79? Anyway, that brought together a lot of groups which were interested in this. There was a session at the American Physical Society meeting in San Francisco in 1980, devoted to spin-polarized gases, and so other groups started working in it.

But we started working really very seriously at that point, and we thought that we were the only people in the field until we saw this article led by Silvera and Walraven, where they stabilized atomic hydrogen for the first time! They confined a gas of atomic hydrogen in a cell. And that, really, sort of electrified the whole field! And, of course we doubled our efforts to move forwards.

It was one of these projects where everything seemed to getting better and better: Hydrogen, we thought, was the ideal gas, because its low mass, and has the highest temperature for BEC at a given density... We were delighted that hydrogen had a such a small scattering length, because we wanted something as close as possible to an ideal gas... All the signs were in the right direction. And then we came up against a brick wall, namely we found that the hydrogen, before we got the density that we needed, three-body recombination did us in!

And the techniques that they used, they were some of them we used, one of them we used later: they had a very simple way to measure the hydrogen, just you have a little resistor, and you heat it up, in order for the liquid helium surface to go away, and hydrogen all recombines, and you get a pulse of energy. And that invention of theirs was a very important one for us. So, anyway, that's where it started and we were going great guns.

We were within a factor of ten in density, but we couldn’t overcome that, we needed a new strategy. And we had a wonderful postdoc, it was Harald Hess, and we felt, the new strategy would mean going to much lower temperatures, which meant confining the atoms magnetically so that it didn’t hit any walls, and then you have to have a way to cool them. And Harald suggested this evaporative cooling, which turned out to be a very important contribution to whole field.

So we set to work on that. But it wasn't so easy, and we really struggled. And retrospectively, we were very lucky to get to BEC at all... This small scattering length which we thought was such an advantage, it was really almost a fatal disadvantage, because evaporative cooling depends on collisions. So we managed the battle, to find a new way to look at the atoms at low temperature which was with laser spectroscopy, which is not easy in hydrogen in a cryogenic system.

So we did push through, and we eventually got there, and we were very happy to get there, and I think that everyone else was happy that we got there, because we were working on it for over twenty years, and it's kind of embarrassing to have a group working for over twenty years! You know, everyone sort of feels sorry for you...

So, anyway, we did, eventually, get there, but it turns out that hydrogen is not a particularly good atom for studying quantum fluids. So this is the way Nature is. Sometimes, Nature assists you. Sometimes, Nature falls short... But it's certainly great to be part of this community. And to know that the work that we did helped attract interest and stimulate other works.

Varenna (1997) & Varenna (2006)

Making, probing and understanding Bose-Einstein condensates

W. KETTERLE, D. S. DURFEE and D. M. STAMPER-KURN

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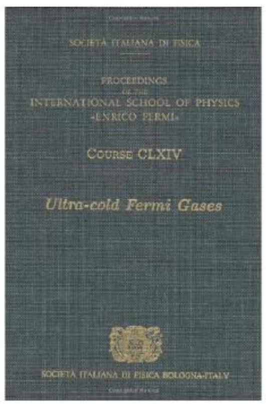
Massachusetts Institute of Technology, Cambridge, MA 02139, USA



Making, probing and understanding ultracold Fermi gases

W. KETTERLE and M. W. ZWIERLEIN

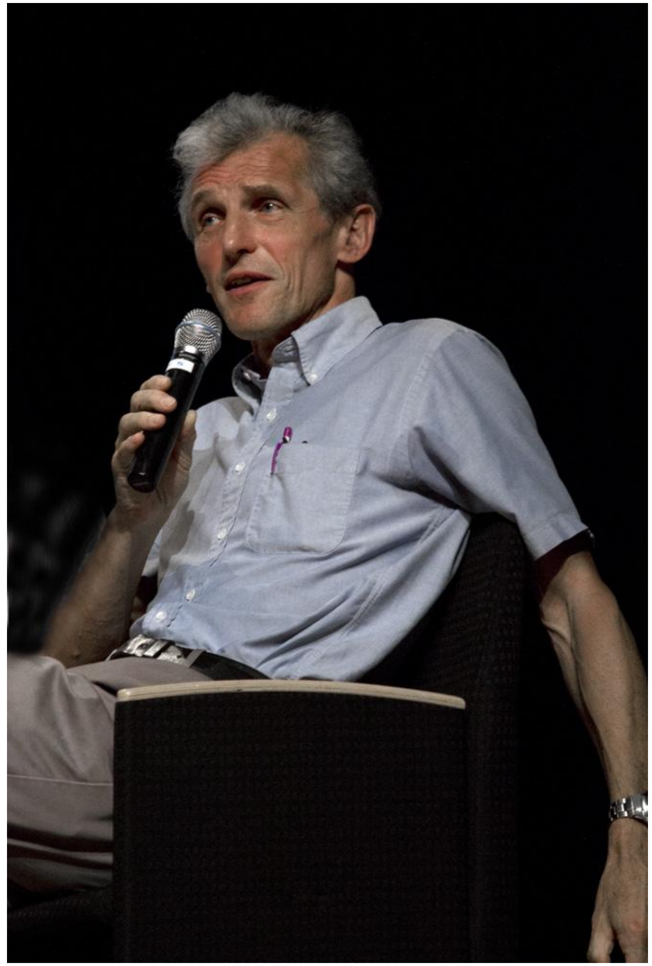
Department of Physics, MIT-Harvard Center for Ultracold Atoms, and Research Laboratory of Electronics, Massachusetts Institute of Technology, Cambridge, MA, 02139, USA

Jean Dalibard – Thank you very much. So, we come to the last books. Well, actually, the first one is already the one I showed, the book of '97 of Varenna [*Varenna 1998 in fact*], the second one is Varenna 2006. I chose those two books because Wolfgang made a contribution in those two books, and you are not so inventive with the titles, for the two years, it's exactly the same thing... And just for the curiosity of seeing this, if you were invited another time to Varenna, what will you make, probe and understand? Have you an idea?

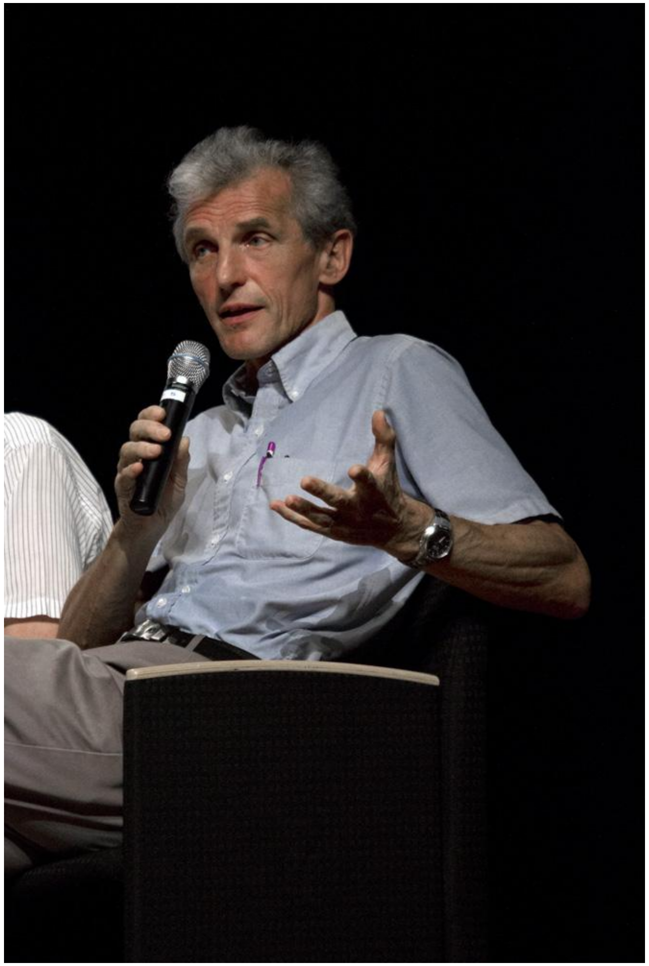
Well, this is not the real question... I mean, what is impressive is really that you could write the '97 paper (*JD means '98*) only two years after discovering BEC. And retrospectively, you can comment on how fast the things went, from the time you observed BEC, I think it was the summer or fall of '97 (*JD means '95*), and the mastering of everything to write this chapter, because it's really a wonderful chapter. If you want to make a BEC, everyone should read this chapter.

Wolfgang Ketterle – Well, the years '95, '96, '97 where really explosive. When Bose-Einstein condensation happened, it was unusual, that it was like a masterpiece of Nature which was unveiled in one moment. It was just there, it was robust, and it allowed experimentation. I know that over the years before BEC happened, there was a lot of skepticism, and a lot of, you know, speculation what might happen.

I was part of discussions where people said: “Well, when Bose-Einstein condensation happened, well, if an ideal gas will just be in the ground state in the trap, the density will go up and due to the high density, there will be inelastic losses, everything, molecule formation from hell, and the Bose-Einstein condensate will just disappear in a split millisecond.” There were people who discussed: “Well, when will cool down, the condensate forms, can we trigger a detector to see sort of the fleeting moment before the BEC disappears?”

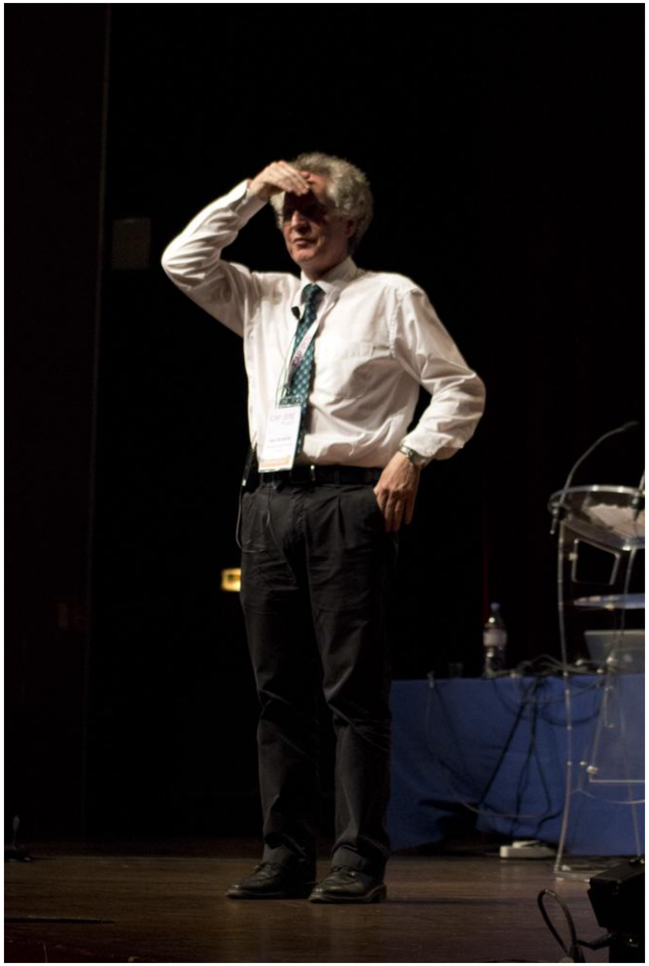


Well, we know now that the non-ideal behavior of the Bose gas with relatively large scattering lengths compared to hydrogen prevents the collapse for the BEC because of mean field repulsion! So, that's sort of, people were maybe not fully aware of that, but ultimately, it was a gift of Nature. And when BEC happened, well, the atoms in the condensate lived for, it was too good to be true, it lived for seconds! We could immediately pursue to do experiments.



And of course, it happened already when the field of laser cooling was very mature, so a lot of techniques, in terms of lasers, in terms of detection, in terms of imaging clouds was already developed. But I would say, mostly importantly, our understanding of Bose-Einstein condensation was based on the pioneering work of the people who pursued atomic hydrogen.

You showed the green book of Bose-Einstein condensation. There were several articles, in particular Greytak and Dan Kleppner's article and another beautiful paper by Jook Walraven which clearly shaped the concepts. That helped enormously, and then the field just exploded.



Jean Dalibard – Well, thank you. I think we can answer the first questions, the first two questions of Gauguin, “where are we coming from”, and “where we are now”. And I think now, I would like to ask questions to where we will being, next ICAP or so. So, now, I think I can take questions from the audience, from here or from the other amphitheater downstairs. Please, don't be shy! Bill is not in the audience...

photo Eric Corsini

Hal Metcalf – I just want to add a comment to Bill's comment. Bill: don’t panic! Don’t panic! It was clear, when we made these first measurements, that the temperature was way below the Doppler limit, we knew we were doing something wrong. And when you’re doing something wrong, it's very embarrassing. So you don't tell anybody, until you’ve checked everything. And then, still wrong! And then you only tell your best friends, and amongst our best friends were Claude, and Jean, Dave Wineland, a couple of other people. And finally the advice was: “submit it for publication and see what happens”.

Jean Dalibard – No comment? From the members of the round table? Ok, other questions. Yes.
A participant – How do you see the field of ultra cold gases expanding, and sort of filling in with the other fields of physics? For instance, quantum simulations is a big and growing field, and it's a little unclear to me how well ultra cold gases experiments are received in the other fields.
Jean Dalibard – Who wants to take it? There are two volunteers.

Bill Phillips – So the question was how well are ultra cold gases as quantum simulators being received in other fields of physics? And I think the answer depends upon the individual researchers. There are some people in condensed matter physics who were incredibly enthusiastic about ultra cold gases as a new kind of quantum system that is quite different from what you have available in the condensed matter systems, and, as a simulator, that might teach you something about condensed matter systems.

So we now have a generation, a new generation of theorists who began their carriers in condensed matter and who are now concentrating on the quantum mechanics of quantum degenerate gases. So I think there is, at least among some subset of the condensed matter community, a very enthusiastic embracing of cold atoms as these two things. And I think it's important to understand there are two different things: not just quantum simulators of Hamiltonians that are of interest to condensed matter, but as completely different kinds of quantum systems.

Jean Dalibard – Dan?
Dan Kleppner – Yes, I'd just like to add a little anecdote as a preface to that. When the first announcement of BEC came out, it hit the newspapers, everywhere on front page news, it was tremendous excitement. I think the reason of course was that the name Einstein was involved, if it had been called, as it really is, the observation of a quantum degenerate Bose gas, it might have not been on the front page... but that's what that was...

But I received, somewhat after that, communications from two distinguished condensed matter physicists who suggested that, as a favor to my friends, I had to warn them, that they really made fool of themselves, that Bose-Einstein condensation was a well-known phenomenon, it had been observed in liquid helium, and it was something they knew all about! And to pretend that you had something new at this point was a little bit embarrassing and I ought to save them from this embarrassment by just giving a little heads up they hadn't discovered anything new!

Well, I felt the field needed a little defending, and I wrote one of these reference frame essays called "the fuss about BEC" to suggest that maybe it was worth making a fuss about! What, to me, is most interesting is the merger of condensed matter and atomic physics. It's what the naturalist and philosopher E.O. Wilson called the "conciliance" in science, where you have two different streams, two different intellectual streams which come together, and reinforce each other. And I think it's not an exaggeration to say that we now essentially have a new discipline. And, so, no one would have imagined this, but it does mean that it was worth making a fuss about BEC.

Claude Cohen-Tannoudji – It took some time before condensed matter physicists were convinced of the interest of BEC. It took about ten years. But now they are enthusiastic about it.
Bill Phillips – And I think I know who the person was, who suggested to Dan to warn his colleagues. And that person came and give us a seminar at NIST to explain to us why there was nothing new. But even that physicist is very enthusiastic about cold quantum gases today.
Jean Dalibard – OK. Other questions? Chris.

Chris Westbrook – I was just thinking of... I was just chatting with Roy Glauber a couple of hours ago. And he made a statement that I can very much sympathize with, that looking at all the talks that we've seen in the first couple of days of this conference, it seems really really sophisticated and really really complicated what people are doing these days, and how can we possibly follow this? And so, I want to phrase this question, "where this field is going", in terms of, well, "is the field getting hopelessly complicated and hopelessly diverse, or is that just our reference frame and we just haven't learned how to look at the things in a simple enough way?

And I'd like to ask the old timers in this business, because, to think about the analogy with the laser, well, we always talk about lasers and their relationship to BECs. And surely in 1964 lasers seemed to be terribly complicated and new kinds of lasers kept being invented all the time and I bet that laser conferences were very complicated as well! But nowadays, you just buy them and you use them! And my students don't even know what's inside! I don't know what's inside, neither... So the field can go either way, and it could collapse under its own weight, or it could become, you know, sort of a turn key system. So I'd just like a few comments and reactions to this point, to that.

Jean Dalibard – Well, Roy can start and maybe Wolfgang also.

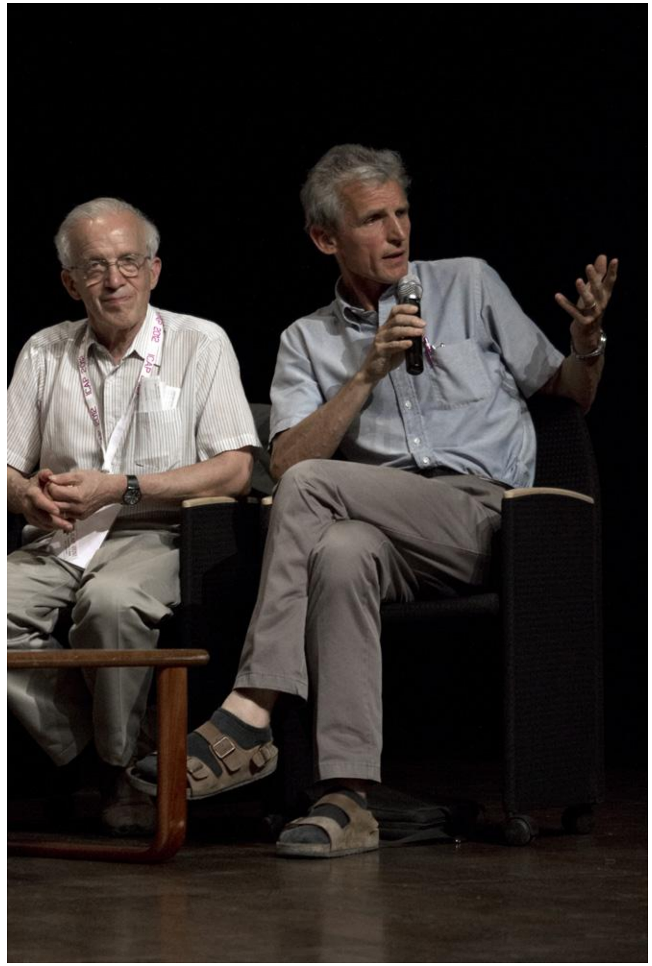
Roy Glauber – Oh… Well… You could buy lasers, I'd say, well before we had a really clear idea of how they work! The theory of the laser in fact, it was originally the theory of the microwave Maser, embellished rather more, then conveyed to the optical domain, and correctly by Townes and his collaborators. What it remained for Willis Lamb to develop the theory which was far more detailed and which was still semi-classical, as far as the electromagnetic field is concerned.

And it was quite a long time in fact before we had a fully quantum mechanical theory of the laser, I don't know if that really produced any last insights, but we waited for it quite a while! There I can just recall now the controversies that existed in this 1964' session at Les Houches, which was a remarkable thing, by the way. Cécile DeWitt, who started, had an evangelical attitude toward educating France in the new quantum theory, even though the quantum theory in a sense had begun there. These difficulties with Willis Lamb, who was a good friend of mine, had mainly to do with the fact that he had developed the ultimate theory of the laser and it was semi-classical.

And to raise any hand that it ought to be seen in a context of the quantum theory and that there were quantum theoretical predictions to be made, really seemed to irritate him. Now that in fact was not true: he was in the background trying himself to develop the quantum theory of the laser and he no sooner was back at Yale that he gave the problem to a graduate student, Marlan Scully, and worked it out himself! This seeming controversy was in fact a running joke but none of the students understood it and it really seemed to be an irritation!

Claude Cohen-Tannoudji – I would like to add a small story: in Les Houches in 1964 I remember going to the Pic du Midi [*Claude means the Aiguille du Midi*], the teleferic cable, a cable [*car*] going to the Pic du Midi, and there were Willis Lamb and Ali Javan discussing about the line width of the laser. And discussing very, very strongly and not looking outside. And when we arrived at the top, said "Oh! What is this?"

Jean Dalibard – Wolfgang, you want to react on the fact that our field may become overcomplicated?



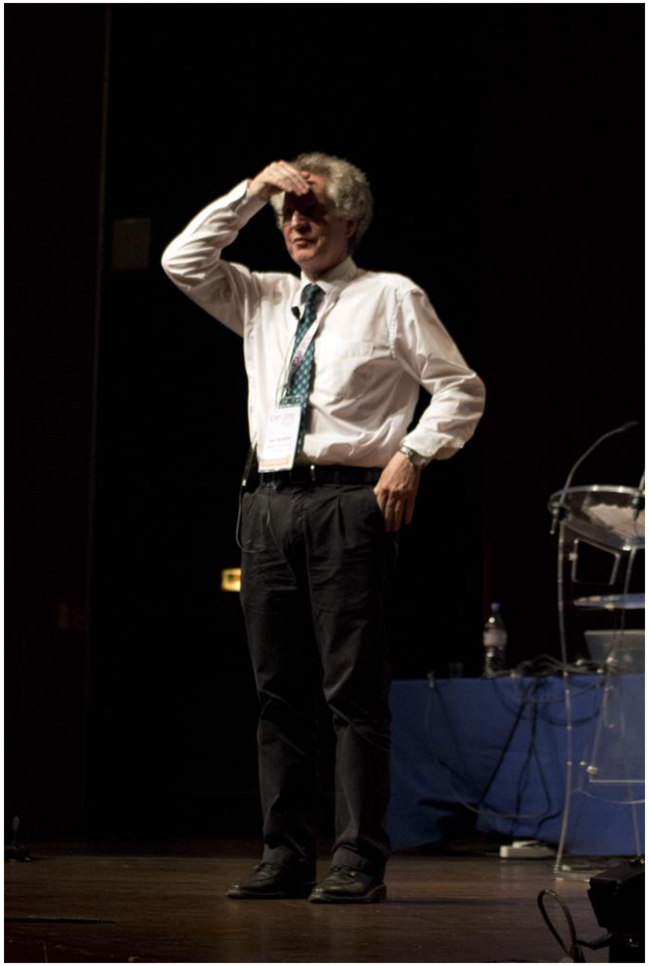
Wolfgang Ketterle – Actually, when I look back over the development of the field, I often see that complexity is the fertile ground for simplicity. It's actually often the people who are looking at very complicated solutions that suddenly, they find something which is very simple. When I started as an atomic physicist, in my third postdoc position, everything was about laser cooling, and I have to say, laser cooling was very complicated.

There were all these different sub-Doppler schemes, called, I forgot, orientational Zeeman cooling and such, there were gray molasses, blue molasses, I meant, there were different ways of doing sub-recoil cooling, Raman cooling, dark state cooling and such, and we had to learn all that! I taught that in my graduate course in atomic physics for a while, but now I don't do it any longer, because we have distilled out extremely simple and powerful techniques! Pretty much 90% of the research on laser cooling, which dominated the field, was wiped away in a few months, when evaporative cooling was shown to have virtually no temperature limits.

And I see other simplifications, like, you know, with for instance Feshbach resonances, nobody thought of Feshbach resonances when Bose-Einstein condensation was discovered. Now I would say a large fraction, 30, 40, maybe even 60 % of the cutting-edge research on quantum gases uses Feshbach resonances. This tool did not exist! And actually this tool has greatly simplified the physics, because we can make the interaction parameter zero or infinite, and something in the system is simple again!

Or people discuss very, very complicated techniques to analyze gases, we learned today, we learned at these sessions this week that if you simply look at the profile of a trapped atom cloud, you can get with high precision the equation of state! This is really simple! So I'm counting on that Nature simplification will happen and that this is what sort of cap is on! Of course one thing is inevitable, the field will broaden, and there are so many different subfields and eventually you can only be…

When I joined the field as a postdoc in 1990, I could read every single paper which appeared on cold atoms, whether it was theoretical or experimental, whether it involved the technique or collisional physics, I read every single paper which came out. For a number of years. But then eventually it became impossible because the field broadened. But in your subfield, I think simplifications will happen which allow the next generation of the young people to just build on top of it without being overwhelmed by the complexity of certain things.



Jean Dalibard – Thank you. Maybe we can ask the other lecture hall whether there is a question or there are questions? Arago, do you hear us ?
Erwan Bimbard (Chairman in Arago) – Can you hear me ?
Jean Dalibard – Yes!
Erwan Bimbard – So we have questions, yes…
Jean Dalibard – Yes, please.

A participant – My question is to Professor Dan Kleppner. He allowed that in atomic hydrogen the BEC has been achieved. So my question to you is in two parts. First, we heard a very interesting talk yesterday about the trapping of anti-hydrogen. So my question is: is it possible to achieve BEC for anti-hydrogen? The second part of the question is: if the answer is yes, are you going to do it?

Dan Kleppner – I’m amazed at the progress which has been made on the trapping of anti-hydrogen… They've been working on it for a long time, it was very complicated, terribly ambitious, and yet, they’re making progress, slowly… The next goal is to see an optical signal from this hydrogen and that would be wonderful, if they can do that. Compared to everything which is going on, to go get a BEC in anti-hydrogen seems totally out of sight! I mean with anti-hydrogen you're talking about one atom or a thousand of atoms... For BEC, well our first BEC with hydrogen had over a million atoms in it, so it seems to me that that would not be a very good goal…

Jean Dalibard – Yes?
A participant – So I’d like to just follow on from Chris' question and ask: we know that our field is wonderful and fascinating, and has lots of interesting things for us all to do, but are there ways in which our field can actually inform and answer some of the bigger questions in physics? And we touched on a few of this during the conference so far.

We've looked at the variation of the fundamental constants, the issues about the standard model, and there are other issues about the foundations of quantum mechanics... So as much as we might be useful in addressing areas like condensed matter physics, etc., etc., are there real prospects extend out to and be able to answer these big questions?
Jean Dalibard – Who wants to take it? Bill? Or…

Bill Phillips – Well, I’m not sure which other big questions you imagine, for example one of the big questions we face is: what is dark matter, was is dark energy... I don’t know whether cold atoms are going to have anything to say about that. I’m amazed that we have experiments today that are being planned that can address some of these questions in other ways. I don’t know whether, whether cold atoms will be useful to that, but at the same time I object a little bit to the idea that somehow, in order to validate our bona fides as a field, that we have to address the most fundamental issues.

There is a lot of really good stuff that we don’t understand about what happens in many-body quantum systems, that isn't that the same, of the same character as finding out what dark matter is, but it's still really, really important and interesting and so, I don’t want to denigrate in any way that kind of work which I think is going to continue to produce lots of wonderful results…
Jean Dalibard – Wolfgang?

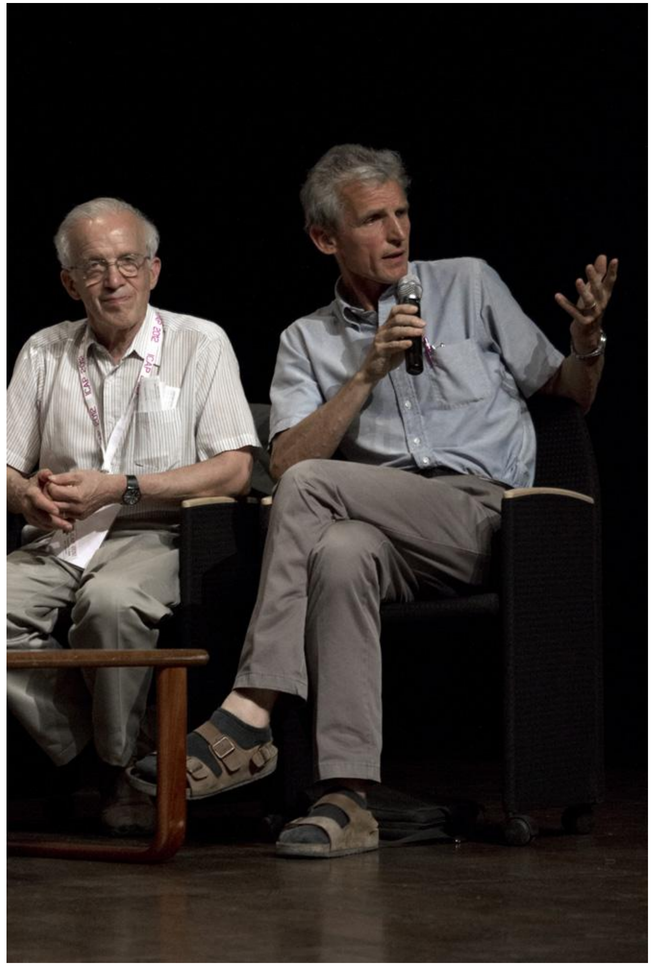
Wolfgang Ketterle – Well, I think there is a phrase: “More is different”. If you put many atoms together, they show behavior, which you could never predict, just from Schrödinger's equation. I mean, if you want a tip: Schrödinger's equation predicts all of chemistry, but nobody can solve Schrödinger's equation at that level of complexity. And we have to accept that, or embrace that most of our research is to find completely new phenomena when many particles come together. And this is for me one of the big challenges in physics, it is one of the big open questions in physics!

The nature of high-temperature superconductivity, the deeper understanding of entanglement, and the discovery of novel properties of novel materials… At the same time I would say our field successfully contributes to deeper understanding of quantum physics: quantum error correction, reversal of decoherence, nobody thought about that, that this is possibly quantum physics!

Or even the so-called “demonstration experiments”, which just show beautiful quantum physics, and we love to do them in atomic physics… They really shape the understanding, what quantum theory is, and they provide the insight which then allows researchers to make other discoveries… So I think we can be proud of how we contribute to the foundations of physics in our field…

Jean Dalibard – Another question…
A participant – What questions do you want to be solved in the next ten years? What do you all dream in the next ten years?
Jean Dalibard – Now, everything...
Dan Kleppner – You know, why would I tell you? (laughs...)
Jean Dalibard – Claude, do you want to answer? A more positive answer than that. You don't want to tell either! OK. Bill, would you? OK. They all have an answer!

Dan Kleppner – Let me make a relevant comment but is doesn’t address your question directly… It’s this, that if you were to see the ICAP program ten years from now, you would be totally amazed! And if you like to take me up on that, go ten years from now and look back at this conference! I’ve gone back to the previous conferences and looked at them, over the years, and what one sees, are just these seeds of the things which are coming. And it'd just grown and grown and grown and there's no indication of the process slowing down!



Wolfgang Ketterle – I would also say, the real excitement is usually not extrapolation of what we do right now… There is the moment when suddenly, some new system is very stable or opens a whole new window to advance much deeper with our understanding of quantum behavior… And suddenly the field changes direction… And we see that all the time, I mean, singular developments like the magneto-optic trap, singular developments like the evaporative cooling had no practical limits, singular developments that when you cool fermionic atoms at the right magnetic field, they pair by themselves and become superfluid pairs!

I mean, that was, I could not even dare to ask Nature for that, because I thought it would be too broad to put that on the wish list. And those things happened and within a few years, they have created a major subfield in this community! And I think that half of the people who are here in a few years will work based on a development which has not happened yet! Our field is very dynamic.

Jean Dalibard – We are getting close to the end, but... Yes, Olivier.

Olivier Dulieu – Now, I think because we were talking about what could be the goals and so on, it seemed to me that what kind qualifies of all those fantastic researches, is the fact that we can indeed control the Nature at a very, very extreme limit, and I think, I mean, we don't know where it will go. Well, the fact that we can control all these degrees of freedom by light or either internal degrees of freedom or external degrees of freedom, just would open any kind of door that we can dream of or not dream of by the way, but I think that this word, control, is... You are not using it too much, up to now, it seems to me, but it seems to me that this control is just the word, the central word of all that research. Would you agree with that?

Bill Phillips – Well, I certainly agree with the idea, that control has been a theme that has been getting more and more strong, in atomic physics. I think Claude, was right. Optical pumping was one of the earliest forms of control. It allows to control the internal state of atoms, while laser cooling allows us to control the external state of atoms, and Bose-Einstein condensation allows us to control that external state even better, and then the various techniques of atom optics, that we haven't talked about, it's also been another big development in our field, allowed us to control that external state of atoms in a different way.

And it seems like we keep developing more and more tools that will allow us to control things better and better. But the real pay-off is when we use those tools, towards something that we didn’t know before. I think that’s what we're doing now in many-body physics, and any number of other themes that are part of the modern day scene. So, yes, I agree but I think that in the end it's really just a tool and the things that we can do with that tool are just fantastic!

Claude Cohen-Tannoudji – I think it was another important idea that very precise measurement performed in atomic physics, allow one to get information about high energy physics. You know, what Ed Hinds said yesterday: attovolt physics gives information about tera-electron-volt. So, another beautiful example is parity violation, parity violation in atoms give some linear combination of weak charge of the standard model.

And the error bar of this measurement is nearly orthogonal to the error bar given by high energy methods… So in that respect, I think that cold atoms allow higher and higher precision and can contribute to fundamental physics, fundamental problems.

Roy Glauber – I'd say we investigate what is fundamental first and the issue of control is rather secondary. But we certainly can enjoy the fact that atomic physics has so much more gentle way of finding, about trying to find the fundamental things about the world, much more gentle than say, high energy physics! And the amount of overlap is remarkable!

Jean Dalibard – Wolfgang?
Wolfgang Ketterle – I agree that a lot in our field is about control, but for me the larger fraction is that we start to control what we didn't know about. So that is the role of discovery. So I would rather raise the question: Are we really controlling Nature, or are we discovering the windows when Nature allows us to control her?

Dan Kleppner – Maybe I will end this conversation with a philosophical comment by Pierre Teilhard de Chardin, who is really talking about the evolution of living forms, and what he said applies exactly to the development of science and to our field, which is that “the history of the living world is an elaboration of evermore perfect eyes in a cosmos in which there is always something interesting new to be seen!” What this field does is to give us new eyes and when we have these new eyes we see interesting new things…

Jean Dalibard – I would say it's for me a nice place to stop, because anyway, Philippe won't be happy with me if we continue too much, because we have another talk, so I thing that's a perfect sentence to conclude. So, thank you very much to the five of you!

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The emergence of a new field, 1985-1995: from atom cooling and trapping to Bose-Einstein condensation

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