The notion of Bose statistics dates back to a 1924 paper in which Satyendranath Bose used a statistical argument to derive the black-body photon spectrum (Bose, 1924). Unable to publish his work, he sent it to Albert Einstein, who translated it into German and got it published. Einstein then extended the idea of Bose's counting statistics to the case of noninteracting atoms (Einstein, 1924, 1925). The result was Bose-Einstein statistics. Einstein immediately noticed a peculiar feature of the distribution of the atoms over the quantized energy levels predicted by these statistics. At very low but finite temperature a large fraction of the atoms would go into the lowest energy quantum state. In his words, “A separation is effected; one part condenses, the rest remains a saturated ideal gas.”\(^2\) (Einstein, 1925). This phenomenon we now know as Bose-Einstein condensation. The condition for this to happen is that the phase-space density must be greater than approximately unity, in natural units. Another way to express this is that the de Broglie wavelength, \(\lambda_{\text{DB}}\), of each atom must be large enough to overlap with its neighbor, or more precisely, \(n\lambda_{\text{DB}}^3 > 2.61\).

This prediction was not taken terribly seriously, even by Einstein himself, until Fritz London (1938) and Laszlo Tisza (1938) resurrected the idea in the mid 1930s as a possible mechanism underlying superfluidity in liquid helium 4. Their work was the first to bring out the idea of BEC displaying quantum behavior on a macroscopic size scale, the primary reason for much of its current attraction. Although it was a source of debate for decades, it is now recognized that the remarkable properties of superconductivity and superfluidity in both helium 3 and helium 4 are related to BEC, even though these systems are very different from the ideal gas considered by Einstein.

The appeal of the exotic behavior of superconductivity and superfluidity, along with that of laser light, the third common system in which macroscopic quantum behavior is evident, provided much of our motivation in 1990 when we decided to pursue BEC in a gas. These three systems all have fascinating counterintuitive behavior arising from macroscopic occupation of a single quantum state. Any physicist would consider these phenomena among the most remarkable topics in physics. In 1990 we were confident that the addition of a new member to the family would constitute a major contribution to physics. (Only after we succeeded did we realize that the discovery of each of the original Macroscopic Three had been recognized with a Nobel Prize, and we are grateful that this trend has continued!) Although BEC shares the same underlying mechanism with these other systems, it seemed to us that the properties of BEC in a gas would be quite distinct. It is far more dilute and weakly interacting than liquid-helium superfluids, for example, but far more strongly interacting than the noninteracting light in a laser beam. Perhaps BEC's most distinctive feature (and this was not
something we sufficiently appreciated, in 1990) is the ease with which its quantum wave function may be directly observed and manipulated. While neither of us was to read C. E. Hecht's prescient 1959 paper (Hecht, 1959) until well after we had observed BEC, we surely would have taken his concluding paragraph as our marching orders:

The suppositions of this note rest on the possibility of securing, say by atomic beam techniques, substantial quantities of electron-spin-oriented H, T and D atoms. Although the experimental difficulties would be great and the relaxation behavior of such spin-oriented atoms essentially unknown, the possibility of opening a rich new field for the study of superfluid properties in both liquid and gaseous states would seem to demand the expenditure of maximum experimental effort.3

In any case, by 1990 we were awash in motivation. But this motivation would not have carried us far, had we not been able to take advantage of some key recent advances in science and technology, in particular, the progress in laser cooling and trapping and the extensive achievements of the spin-polarized-hydrogen community.

However, before launching into that story, it is perhaps worthwhile to reflect on just how exotic a system of indistinguishable particles truly is, and why BEC in a gas is such a daunting experimental challenge. It is easy at first to accept that two atoms can be so similar one to the other as to allow no possibility of telling them apart. However, confronting the physical implications of the concept of indistinguishable bosons can be troubling. For example, if there are ten bosonic particles to be arranged in two microstates of a system, the statistical weight of the configuration with ten particles in one state and zero in the other is exactly the same as the weight of the configuration with five particles in one state and five in the other. This 1:1 ratio of statistical weights is very counterintuitive and rather disquieting. The corresponding ratio for distinguishable objects, such as socks in drawers, that we observe every day is 1:252, profoundly different from 1:1. In the second of Einstein’s two papers (Einstein, 1925; Pais, 1982) on Bose-Einstein statistics, Einstein comments that “The . . . molecules are not treated as statistically independent . . . and the differences between distinguishable and indistinguishable state counting . . . express indirectly a certain hypothesis on a mutual influence of the molecules which for the time being is of a quite mysterious nature. This mutual influence is no less mysterious today, even though we can readily observe the variety of exotic behavior it causes such as the well-known enhanced probability for scattering into occupied states and, of course, Bose-Einstein condensation.”

Not only does the Bose-Einstein phase transition offend our sensibilities as to how particles ought best to distribute themselves, it also runs counter to an unspo-

3Emphasis ours.

FIG. 1. Generic phase diagram common to all atoms: dotted line, the boundary between non-BEC and BEC; solid line, the boundary between allowed and forbidden regions of the temperature-density space. Note that at low and intermediate densities, BEC exists only in the thermodynamically forbidden regime.

ken assumption that a phase transition somehow involves thermodynamic stability. In fact, the regions immediately above and immediately below the transition in dilute-gas experiments are both deep in the thermodynamically forbidden regime. This point is best made by considering a qualitative phase diagram (Fig. 1), which shows the general features common to any atomic system. At low density and high temperature, there is a vapor phase. At high density there are various condensed phases. But the intermediate densities are thermodynamically forbidden, except at very high temperatures. The Bose-condensed region of the n-T plane is utterly forbidden, except at such high densities that (with one exception) all known atoms or molecules would form a crystalline lattice, which would rule out Bose condensation. The single exception, helium, remains a liquid below the BEC transition. However, reaching BEC under dilute conditions (say, at densities 10 or 100 times lower than conventional liquid helium) is as thermodynamically forbidden to helium as it is to any other atom.

Of course, forbidden is not the same as impossible; indeed, to paraphrase an old Joseph Heller joke, if it were really impossible, they wouldn’t have bothered to forbid it. It comes down in the end to differing time scales for different sorts of equilibrium. A gas of atoms can come into kinetic equilibrium via two-body collisions, whereas it requires three-body collisions to achieve chemical equilibrium (i.e., to form molecules and thence solids). At sufficiently low densities, the two-body rate will dominate the three-body rate, and a gas will reach kinetic equilibrium, perhaps in a metastable Bose-Einstein condensate, long before the gas finds its way to the ultimately stable solid-state condition. The need to maintain metastability usually dictates a more stringent upper limit on density than does the desire to create a dilute system. Densities around 1020 cm−3, for instance, would be a hundred times more dilute than a condensed-matter helium superfluid. But creating such a gas is quite impractical even at an additional factor-of-1000 lower density, say 1017 cm−3, when metastability times would be on the order of a few microseconds;
more realistic are densities on the order of $10^{14} \text{ cm}^{-3}$. The low densities mandated by the need to maintain long-lived metastability in turn make necessary the achievement of still lower temperatures if one is to reach BEC.

Thus the great experimental hurdle that must be overcome to create BEC in a dilute gas is to form and keep a sample that is so deeply forbidden. Since our subsequent discussion will focus only on BEC in dilute gases, we shall refer to this simply as BEC in the sections below and avoid endlessly repeating “in a dilute gas.”

Efforts to make a dilute BEC in an atomic gas were sparked by Stwalley and Nosanow (1976). They argued that spin-polarized hydrogen had no bound states and hence would remain a gas down to zero temperature, so it would be a good candidate for BEC. This stimulated a number of experimental groups (Silvera and Walraven, 1980; Hardy et al., 1982; Hess et al., 1983; Johnson et al., 1984) in the late 1970s and early 1980s to begin pursuing this idea using traditional cryogenics to cool a sample of polarized hydrogen. Spin-polarized hydrogen was first stabilized by Silvera and Walraven in 1980, and by the mid 1980s spin-polarized hydrogen had been brought within a factor of 50 of condensing (Hess et al., 1983). These experiments were performed in a dilution refrigerator, in a cell in which the walls were coated with superfluid liquid helium as a nonstick coating for the hydrogen. The hydrogen gas was compressed using a piston-in-cylinder arrangement (Bell et al., 1986) or inside a helium bubble (Sprik et al., 1985). These attempts failed, however, because when the cell was made very cold the hydrogen stuck to the helium surface and recombined. When one tried to avoid that problem by warming the cell sufficiently to prevent sticking, the density required to reach BEC was correspondingly increased, which led to another problem. The requisite densities could not be reached because the rate of three-body recombination of atoms into hydrogen molecules goes up rapidly with density and the resulting loss of atoms limited the density (Hess, 1986).

Stymied by these problems, Harold Hess (Hess, 1986) from the MIT hydrogen group realized that magnetic trapping of atoms (Migdall et al., 1985; Bagnato et al., 1987) would be an improvement over a cell. Atoms in a magnetic trap have no contact with a physical surface and thus the surface-recombination problem could be circumvented. Moreover, thermally isolated atoms in a magnetic trap would allow cooling by evaporation to far lower temperatures than previously obtained. In a remarkable paper, Hess (1986) laid out most of the important concepts of evaporative cooling of trapped atoms for the attainment of BEC. Let the highest-energy atoms escape from the trap, and the mean energy, and thus the temperature, of the remaining atoms will decrease. For a dilute gas in an inhomogeneous potential, decreasing the temperature will decrease the occupied volume. One can thus actually increase the density of the remaining atoms by removing atoms from the sample. The all important (for BEC) phase-space density is dramatically increased as this happens because density is rising while temperature is decreasing. The Cornell University hydrogen group also considered evaporative cooling (Lovelace et al., 1985). By 1988 the MIT group had demonstrated these virtues of evaporative cooling of magnetically trapped spin-polarized hydrogen. By 1991 they obtained, at a temperature of 100 °K, a density that was only a factor of 5 below BEC (Doyle, 1991a). Further progress was limited by dipolar relaxation, but perhaps more fundamentally by loss of signal-to-noise, and the difficulty of measuring the characteristics of the coldest and smallest clouds (Doyle, 1991b). Evaporative work was also performed by the Amsterdam group (Luiten et al., 1993).

At roughly the same time, but independent from the hydrogen work, an entirely different type of cold-atom physics and technology was being developed. Laser cooling and trapping has been reviewed elsewhere (Arimondo et al., 1991; Chu, 1998; Cohen-Tannoudji, 1998; Phillips, 1998), but here we mention some of the highlights most relevant to our work. The idea that laser light could be used to cool atoms was suggested in early papers by Wineland and Dehmelt (1975), by Hänisch and Schawlow (1975), and by Letokhov’s group (Letokhov, 1968). Early optical force experiments were performed by Ashkin (Bjorkholm et al., 1978). Trapped ions were laser-cooled at the University of Washington (Neuhauser et al., 1978) and at the National Bureau of Standards (now NIST) in Boulder (Wineland et al., 1978). Atomic beams were deflected and slowed in the early 1980s (Andreev et al., 1981; Ertmer et al., 1985; Prodan et al., 1985). Optical molasses, where the atoms are cooled to very low temperatures by six perpendicular intersecting laser beams, was first studied at Bell Labs (Chu et al., 1985). Measured temperatures in the early molasses experiments were consistent with the so-called Doppler limit, which amounts to a few hundred microkelvin in most alkalis. Light was first used to hold (trap) atoms using the dipole force exerted by a strongly focused laser beam (Chu et al., 1986). In 1987 and 1988 there were two major advances that became central features of the method of creating BEC. First, a practical spontaneous-force trap, the magneto-optical trap (MOT) was demonstrated (Raab et al., 1987); and second, it was observed that under certain conditions, the temperatures in optical molasses are in fact much colder than the Doppler limit (Lett et al., 1988; Chu et al., 1989; Dalibard et al., 1989). The MOT had the essential elements needed for a widely useful optical trap: it required relatively modest amounts of laser power, it was much deeper than dipole traps, and it could capture and hold relatively large numbers of atoms. These were heady times in the laser-cooling business. With experiment yielding temperatures mysteriously far below what theory would predict, it was clear that we all lived under the authority of a munificent God.

During the mid 1980s one of us (Carl) began investigating how useful the technology of laser trapping and cooling could become for general use in atomic physics. Originally this took the form of just making it cheaper and simpler by replacing the expensive dye lasers with
vastly cheaper semiconductor lasers, and then searching
for ways to allow atom trapping with these low-cost but
also low-power lasers (Pritchard et al., 1986; Watts and
Wieman, 1986). With the demonstration of the MOT
and sub-Doppler molasses Carl's group began eagerly
studying what physics was limiting the coldness and
denseness of these trapped atoms, with the hope of ex-
tending the limits further. They discovered that several
atomic processes were responsible for these limits.
Light-assisted collisions were found to be the major loss
process from the MOT as the density increased (Sesko
et al., 1989). However, even before that became a seri-
ous problem, the light pressure from reradiated photons
limited the density (Walker et al., 1990; Sesko et al.,
1991). At about the same time, the sub-Doppler tem-
peratures of molasses found by Phillips, Chu, and
Cohen-Tannoudji were shown to be due to a combina-
tion of light-shifts and optical pumping that became
known as Sysiphus cooling (Dalibard and Cohen-
Tannoudji, 1989). Random momentum fluctuations from
the scattered photons limit the ultimate temperature to
about a factor of 10 above the recoil limit. In larger
samples, the minimum temperature was higher yet, be-
cause of the multiple scattering of the photons. While
carrying out studies on the density limits of MOT's
Carl's group also continued the effort in technology de-
velopment. This resulted in the creation of a useful
MOT in a simple glass vapor cell (Monroe et al., 1990),
thereby eliminating the substantial vacuum chamber re-
quired for the slowed atomic beam loading that had pre-
viously been used.

Seeking to take advantage of the large gains in phase-
space density provided by the MOT while avoiding the
limitations imposed by the undesirable effects of pho-
tons, Carl and his student Chris Monroe decided to try
loading the cold MOT atoms into a magnetic trap (Mon-
roe et al., 1990; see Fig. 2). This worked remarkably
well. Because further cooling could be carried out as the
atoms were transferred between optical and magnetic
trap it was possible to get very cold samples, the coldest
that had been produced at that time. More importantly,
these were not optical molasses samples that were
quickly disappearing but rather magnetically trapped
samples that could be held and studied for extended pe-
riods. These samples were about a hundred times colder
than any previous trapped atom samples, with a corre-
spondingly increased phase-space density. This was a
satisfying achievement, but as much as the result itself, it
was the relative simplicity of the apparatus required that
inspired us (including now Eric Cornell, who joined the
project as a postdoc in 1990) to see just how far we could
push this marriage of laser cooling and trapping and
magnetic trapping.

Previous laser traps involved expensive massive laser
systems and large vacuum chambers for atomic beam
precooling. Previous magnetic traps for atoms were usu-
ally (Bagnato et al., 1987; Doyle, 1991) extremely com-
plex and bulky (often with superconducting coils) be-
cause of the need to have sufficiently large depths and
strong confinement. Laser traps and magnetic traps were
both somewhat heroic experiments individually, to be
undertaken only by a select handful of well-equipped
AMO laboratories. The prospect of trying to get both
traps working, and working well, in the same room and
on the same day, was daunting. However, in the first
JILA magnetic trap experiment our laser sources were
simple diode lasers, the vacuum system was a small glass
vapor cell, and the magnetic trap was just a few turns of
wire wrapped around it. This magnetic field was ade-
quate because of the low temperatures of the laser-
cooled and trapped samples. Being able to produce such
cold and trapped samples in this manner encouraged
one to fantasize wildly about possible things to do with
such an atom sample. Inspired by the spin-polarized hy-
drogen work, our fantasizing quickly turned to the idea
of evaporative cooling further to reach BEC. It would
require us to increase the phase-space density by 5 or-
ders of magnitude, but since we had just gained about 15
orders of magnitude almost for free with the vapor cell
MOT, this did not seem so daunting.

The JILA vapor-cell MOT (Fig. 3), with its superim-
posed ion pump trap, introduced a number of ideas that
are now in common use in the hybrid trapping business
(Monroe et al., 1990; Monroe, 1992): (i) Vapor-cell
(rather than beam) loading, (ii) fused-glass rather than
welded-steel architecture, (iii) extensive use of diode la-
sers, (iv) magnetic coils located outside the chamber, (v)
overall chamber volume measured in cubic centimeters
rather than liters, (vi) temperatures measured by imag-
ing an expanded cloud, (vii) magnetic-field curvatures
calibrated in situ by observing the frequency of dipole
and quadrupole (sloshing and pulsing) cloud motion,
(viii) the basic approach of a MOT and a magnetic trap
which are spatially superimposed (indeed, which often
share some magnetic coils) but temporally sequential,
and (ix) optional use of additional molasses and optical
pumping sequences inserted in time between the MOT
and magnetic trapping stages. It is instructive to note
how a modern, Ioffe-Pritchard-based BEC device (Fig. 4)
resembles its ancestor (Fig. 3).

As we began to think about applying the technique of
evaporative cooling with hydrogen to our very cold al-
kali atoms we looked carefully at the hydrogen work
and its lessons. When viewed from our 1990 perspective
the previous decade of work on polarized hydrogen pro-
vided a number of important insights. It was clear that
the unique absence of any bound states for spin-
polarized hydrogen was actually not an important issue
(other than its being the catalyst for starting the entire
field, of course!). Bound states or not, a very cold
sample of spin-polarized hydrogen, like every other gas,
has a lower-energy state to which it can go, and its sur-
vival depends on the preservation of metastability. For
hydrogen the lower-energy state is a solid, although
from an experimental point of view the rate-limiting
process is the formation of diatomic molecules (with ap-
propriately reoriented spins). Given that all atomic
gases are only metastable at the BEC transition point,
the real experimental issue becomes: How well can one
preserve the requisite metastability while still cooling sufficiently far to reach BEC?

The realization that metastability was the key experimental challenge one should focus on was probably at least as important to the attainment of BEC as any of the experimental techniques we subsequently developed to actually achieve it. The work on hydrogen provided an essential guide for evaluating and tackling this challenge. It provided us with a potential cooling technique (evaporative cooling of magnetically trapped atoms) and mapped out many of the processes by which a magnetically trapped atom can be lost from its metastable state.

The hydrogen work made it clear that it was all an issue of good versus bad collisions. The good collisions are elastic collisions that rethermalize the atoms during evaporation. The more collisions there are, the more quickly and efficiently one can cool. The bad collisions are the inelastic collisions that quench the metastability. Hydrogen had already shown that three-body recombination collisions and dipole spin-flip collisions were the major inelastic culprits. The fact that hydrogen researchers were fairly close to reaching BEC was also a strong encouragement. It meant that the goal was not ridiculously distant and that one only had to do a little better in the proportion of good to bad collisions to succeed.

The more we thought about this, the more we began to suspect that our heavy alkali atoms would likely have more favorable collision properties than hydrogen atoms and thus have a good chance of success. Although knowledge of the relevant collision cross sections was totally nonexistent at that time, we were able to come up with arguments for how the cross sections might scale relative to hydrogen. These are discussed in more detail below in the section discussing why collisional concerns make it likely that BEC can be created in a large number of different species. Here we will just give a brief summary consistent with our views circa 1990. The dipole spin-flip collisions that limited hydrogen involve spin-spin interactions and thus could be expected to be similar for the alkalis and for hydrogen because the magnetic moments are all about the same. The good collisions needed for evaporative cooling, however, should be much larger for heavy alkalis with their fat fluffy electron clouds than for hydrogen. The other villain of the hydrogen effort, three-body recombination, was a total mystery, but because it goes as density cubed while the good elastic collisions go as density squared, it seemed as if we should always be able to find a sufficiently low-density and low-temperature regime to avoid it (see Monroe, 1992).

As a minor historical note, we might point out that during these considerations we happily ignored the fact that the temperatures required to achieve BEC in a

FIG. 2. Chris Monroe examines an early hybrid MOT-magnetic trap apparatus [Color].

FIG. 3. The glass vapor cell and magnetic coils used in early JILA efforts to hybridize laser cooling and magnetic trapping (see Monroe et al., 1990). The glass tubing is 2.5 cm in diameter. The Ioffe current bars have been omitted for clarity.

FIG. 4. Modern MOT and magnetic trap apparatus, used by Cornish et al., 2000 [Color].
heavy alkali gas are far colder than those needed for the same density of hydrogen. The critical temperature for ideal-gas BEC is inversely proportional to the mass. It was clear that we would need to cool to well under a microkelvin, and a large three-body recombination rate would have required us to go to possibly far lower temperatures. To someone coming from a traditional cryogenics background this would (and probably did) seem like sheer folly. The hydrogen work had been pushing hard for some years at the state of the art in cryogenic technology, and here we proposed to happily jump far beyond that. Fortunately we were coming to this from an AMO background in a time when temperatures achieved by laser cooling were dropping through the floor. Optimism was in the air. In fact, we later discovered optimism can take one only so far: There were actually considerable experimental difficulties, and further cooling came at some considerable effort and a five-year delay. Nevertheless, it is remarkable that with evaporative cooling a magnetically trapped sample of atoms, surrounded on all sides by a 300-K glass cell, can be cooled to reach temperatures of only a few nanokelvin, and moreover it looks quite feasible to reach even colder temperatures.

General collisional considerations gave us some hope that the evaporative cooling hybrid trap approach with alkali atoms would get us to BEC, or, if not, at least reveal some interesting new physics that would prevent it. Nonetheless, there were powerful arguments against pursuing this. First, our 1990-era arguments in favor of it were based on some very fuzzy intuition; there were no collision data or theories to back it up and there were strong voices in disagreement. Second, the hydrogen experiments seemed to be on the verge of reaching BEC, and in fact we thought it was likely that if BEC could be achieved they would succeed first. However, our belief in the virtues of our technology really carried the day in convincing us to proceed. With convenient lasers in the near-IR, and with the good optical access of a room-temperature glass cell, detection sensitivity could approach single-atom capability. We could take pictures of only a few thousand trapped atoms and immediately know the energy and density distribution. If we wanted to modify our magnetic trap it only required a few hours winding and installing a new coil of wires. This was a dramatic contrast with the hydrogen experiments that, like all state-of-the-art cryogenics experiments, required an apparatus that was the better part of two stories, and the time to modify it was measured in (large) fractions of a year. Also, atomic hydrogen was much more difficult to detect and so the diagnostics were far more limited. This convinced us that although hydrogen would likely succeed first, our hybrid trap approach with easily observed and manipulated alkali samples would be able to carry out important science and so was well worth pursuing in its own right.

From the very beginning in 1990, our work on BEC was heavily involved with cold atomic collisions. This was somewhat ironic since previously both of us had actively avoided the large fraction of AMO work on the subject of atomic collisions. Atomic collisions at very cold temperatures is now a major branch of the discipline of AMO physics, but at the end of the 1980s there were almost no experimental data, and what there was came in fact from the spin-polarized hydrogen experiments (Gillaspy et al., 1989). There was theoretical work on hydrogen from Shlyapnikov and Kagan (Kagan et al., 1981, 1984), and from Silvera and Verhaar (Lagendijk et al., 1986). An early paper by Pritchard (1986) includes estimates on low-temperature collisional properties for alkalis. His estimates were extrapolations from room-temperature results, but in retrospect, several were surprisingly accurate. As we began to work on evaporative cooling, much of our effort was devoted to determining the sizes of all the relevant good and bad collision cross sections. Our efforts were helped by the theoretical efforts of Boudewijn Verhaar, who was among the first to take our efforts seriously and attempt to calculate the rates in question. Chris Greene also provided us with some useful theoretical estimates.

Starting in 1990 we carried out a series of experiments exploring various magnetic traps and measuring the relevant collision cross sections. As this work proceeded we developed a far better understanding of the conditions necessary for evaporative cooling and a much clearer understanding of the relevant collisional issues (Monroe et al., 1993; Newbury et al., 1995). Our experimental concerns evolved accordingly. In the early experiments (Monroe et al., 1990, 1993; Cornell et al., 1991; Monroe, 1992) a number of issues came up that continue to confront all BEC experiments: the importance of aligning the centers of the MOT and the magnetic trap, the density-reducing effects of mode-mismatch, the need to account carefully for the (previously ignored) force of gravity, heating (and not merely loss) from background gas collisions, the usefulness of being able to turn off the magnetic fields rapidly, the need to synchronize many changes in laser status and magnetic fields together with image acquisition, an appreciation for the many issues that can interfere with accurate determinations of density and temperature by optical methods, either fluorescence or absorption imaging, and careful stabilization of magnetic fields. The mastery of these issues in these early days made it possible for us to proceed relatively quickly to quantitative measurements with the BEC once we had it.

In 1992 we came to realize that dipolar relaxation in alkalis should in principle not be a limiting factor. As explained in the final section of this article, collisional scaling with temperature and magnetic field is such that, except in pathological situations, the problem of good and bad collisions in the evaporative cooling of alkalis is reduced to the ratio of the elastic collision rate to the rate of loss due to imperfect vacuum; dipolar relaxation and three-body recombination can be finessed, particularly since our preliminary data showed they were not enormous. It was reassuring to move ahead on efforts to evaporate with the knowledge that, while we were essentially proceeding in the dark, there were not as many monsters in the dark as we had originally imagined.
It rapidly became clear that the primary concerns would be having sufficient elastic collision rate in the magnetic trap and sufficiently low background pressure to have few background collisions that removed atoms from the trap. To accomplish this it was clear that we needed higher densities in the magnetic trap than we were getting from the MOT. Our first effort to increase the density two years earlier was based on a multiplet-loading scheme (Cornell et al., 1991). Multiple MOT-loads of atoms were launched in moving molasses, optically pumped into an untrapped Zeeman level, focused into a magnetic trap, then optically repumped into a trapped level. The repumping represented the necessary dissipation, so that multiple loads of atoms could be inserted in a continuously operating magnetic trap. In practice, each step of the process involved some losses, and the final result was disappointing. Later, however, as discussed below, we resurrected the idea of multiple loading from one MOT to another to good advantage (Gibble et al., 1995; Myatt et al., 1996). This is now a technique currently in widespread practice.

In addition to building up the initial density we realized that the collision rate could be dramatically increased by, after loading into a magnetic trap, compressing the atoms by further increasing the curvature of the confining magnetic fields. In a harmonic trap, the collision rate after adiabatic compression scales as the final confining frequency squared (Monroe, 1992). This method is discussed by Monroe (1992) and was implemented first in early ground-state collisional work (Monroe et al., 1993).

In fall of 1992, Eric's postdoctoral appointment concluded, and, after a tour through the job market, he decided to take the equivalent of an assistant professor position at JILA/NIST. He decided to use his startup money to build a new experimental apparatus that would be designed to put these ideas together to make sure evaporation worked as we expected. Meanwhile, we continued to pursue the possibility of enhanced collision cross sections in cesium using a Feshbach resonance. At that point our Monte Carlo simulations said that a ratio of about 150 elastic collisions per trap lifetime was required to achieve runaway evaporation. This is the condition where the elastic collision rate would continue to increase as the temperature decreased, and hence evaporation would continue to improve as the temperature was reduced. We also had reasonable determinations of the elastic collision cross sections.

So the plan was to build a simple quadrupole trap that would allow very strong squeezing to greatly enhance the collision rate, combined with a good vacuum system in order to make sure evaporative cooling worked as expected. Clearly, there was much to be gained by building a more tightly confining magnetic trap, but the requirement of adequate optical access for the MOT, along with engineering constraints on power dissipation, made the design problem complicated.

When constructing a trap for weak-field-seeking atoms, with the aim of confining the atoms to a spatial size much smaller than the size of the magnets, one would like to use linear gradients. In that case, however, one is confronted with the problem of the minimum in the magnitude of the magnetic fields (and thus of the confining potential) occurring at a local zero in the magnetic field. This zero represents a hole in the trap, a site at which atoms can undergo Majorana transitions (Majorana, 1931) and thus escape from the trap. If one uses the second-order gradients from the magnets to provide the confinement, there is a marked loss of confinement strength. This scaling is discussed by Petrich et al. (1995). We knew that once the atoms became cold enough they would leak out the hole in the bottom of the trap, but the plan was to go ahead and get evaporation and worry about the hole later. We also recognized that even with successful evaporative cooling, and assuming we could solve the issue of the hole in the quadrupole trap, there was still the question of the sign of scattering length, which must be positive to ensure the stability of a large condensate.

In setting up the new apparatus Eric chose to use rubidium. Given the modulo arithmetic that goes into determining a scattering length, it seemed fair to treat the scattering lengths of different isotopes as statistically independent events, and rubidium with its two stable isotopes offered two rolls of the dice for the same laser system. Eric then purchased a set of diode lasers for the rubidium wavelength, but of course we kept the original cesium-tuned diode lasers. The wavelengths of cesium and of the two rubidium isotopes are sufficiently similar that in most cases one can use the same optics. Thus we preserved the option of converting from one species to another in a matter of weeks. The chances then of Nature's conspiring to make the scattering length negative, for both hyperfine levels, for all three atoms, seemed very small.

Progress in cold collisions, particularly the experiment and theory of photoassociative collisions, had moved forward so rapidly that by the time we had evaporatively cooled rubidium to close to BEC temperatures a couple of years later there existed, at the 20% level, values for several of the elastic scattering lengths. In particular, we knew that it was positive for the 2,2 state of Rb-87 (Thorsheim et al., 1987; Lett et al., 1993; Miller et al., 1993; Abraham et al., 1995; Gardner et al., 1995; McAlexander et al., 1995).

Our original idea for the quadrupole trap experiment was to pulse a burst of rubidium into our cell, where we would catch a large sample in the MOT and then hold it as the residual rubidium was quickly pumped away, leaving a long trap lifetime. We, particularly Eric's postdoc, Mike Anderson, spent many frustrating months discovering how difficult this seemingly simple idea was to actually implement in practice. The manner in which rubidium interacted with glass and stainless-steel surfaces conspired to make this so difficult we finally gave up. We ended up going with a far-from-optimum situation of working with extremely low rubidium pressure and doing our best at maximizing the number of atoms captured in the MOT from this feeble vapor and enhancing the collision rate for those relatively few atoms as much
as possible. We recognized that this was a major compromise, but we had been trying to evaporate for some time, and we were getting impatient! We had no stomach for building another apparatus just to see evaporation. Fortunately we were able to find two key elements to enhance the MOT loading and density. First was the use of a dark-spot MOT in which there is a hole in the center of the MOT beams so the atoms are not excited. This technique had been demonstrated by Ketterle (Ketterle et al., 1993) as a way to greatly enhance the density of atoms in a MOT under conditions of a very high loading rate. The number of atoms we could load in our vapor cell MOT with very low rubidium vapor was determined by the loading rate over the loss rate. In this case the loss rate was the photoassociative collisions we had long before found to be important for losses from MOT’s. The dark-spot geometry reduced this two-body photoassociative loss in part because in our conditions it reduced the density of atoms in the MOT (Anderson et al., 1994).

Using this approach we were able to obtain $10^8$ atoms in the MOT collected out of a very low vapor background (so that magnetic trap lifetime was greater than 100 s). The second key element was the invention of the compressed MOT (CMOT), a technique for substantially enhancing the density of atoms in the MOT on a transient basis. For the CMOT, the MOT was filled and then the field gradient and laser detuning were suddenly changed to greatly suppress the multiple photon scattering. This produced much higher densities and clouds whose shape was a much better match to the desired shape of the cloud in the magnetic trap. This was a very transient effect because the losses from the MOT were much larger under these conditions, but that was not important; the atoms needed only to be held for the milliseconds required before they were transferred to the magnetic trap (Petrich et al., 1994; see Fig. 5). With these improvements and a quadrupole trap that provided substantial squeezing, we were able to finally demonstrate evaporative cooling in rubidium.

Cooling by evaporation is a process found throughout Nature. Whether the material being cooled is an atomic nucleus or the Atlantic Ocean, the rate of natural evaporation and the minimum temperature achievable are limited by the particular fixed value of the work function of the evaporating substance. In magnetically confined atoms, no such limit exists, because the work function is simply the height of the lowest point in the rim of the confining potential. Hess (1986) pointed out that, by perturbing the confining magnetic fields, one could make the work function of a trap arbitrarily low; as long as favorable collisional conditions persist, there is no lower limit to the temperatures attainable in this forced evaporation.

Pritchard (Pritchard et al., 1989) pointed out that evaporation could be performed more conveniently if the rim of the trap were defined by an rf-resonance condition, rather than simply by the topography of the magnetic field; experimentally, his group made first use of position-dependent rf transitions to selectively transfer magnetically trapped sodium atoms between Zeeman levels and thus characterized their temperature (Martin et al., 1988). In our experiment we used Pritchard’s technique of an rf field to selectively evaporate.

It was a great relief to see evaporative cooling of laser precooled, magnetically trapped atoms finally work, as we had been anticipating it would for so many years. Unfortunately, it worked exactly as well, but no better, than we had anticipated. The atoms were cooled to about 40 $\mu$K and then disappeared, at just the temperature we had estimated they would be lost, through the hole in the bottom of the quadrupole trap. Eric came up with an idea that solved this problem. It was a design for a new type of trap that required relatively little modification to the apparatus and so was quickly implemented. This was the Time Orbiting Potential (TOP) trap in which a small rotating magnetic field was added to the quadrupole field (Petrich et al., 1995). This moved the field zero in an orbit faster than the atoms could follow. It was the perfect solution to our problem.

Mike Anderson, another postdoc, Wolfgang Petrich, and graduate student Jason Ensher quickly implemented this design. Their efforts were spurred on by the realization that there were several other groups who had now demonstrated or were known to be on the verge of demonstrating evaporative cooling in alkalis in the pursuit of BEC. The TOP design worked well, and the samples were cooled far colder, in fact too cold for us to reliably measure. We had been measuring temperature simply by looking at the spatial size of the cloud in the magnetic trap. As the temperature was reduced the size decreased, but we were now reaching temperatures so low that the size had reached the resolution limit of the optical system. We saw dramatic changes in the shapes of the images as the clouds became very small, but we knew that a variety of diffraction and aberration effects could greatly distort images when the sample size became only a few wavelengths in size, so our reaction to these shapes was muted, and we knew we had to have better diagnostics before we could have meaningful results. Here we were helped by our long experience in studying various trapped clouds over the years. We already knew the value of turning the magnetic trap off to let the cloud expand and then imaging the expanded cloud to get a measure of the momentum distribution in the trap. Since the trap was harmonic, the momentum distribution and the original density distribution were nearly interchangeable. Unfortunately, once the magnetic field was off, the atoms not only expanded but also simply fell under the influence of gravity. We found that the atoms tended to fall out of the field of view of our microscope before they had sufficiently expanded. The final addition to the apparatus was a supplementary magnetic coil, which provided sufficient field gradient to cancel the effects of gravity while minimizing any perturbation to the relative ballistic trajectories of the expanding atoms.

Anderson, Ensher, and a new graduate student, Mike Matthews (Fig. 6), worked through a weekend to install the antigravity coil and, after an additional day or two of
trial and error, got the new field configuration shimmed up. By June 5, 1995 the new technology was working well and we began to look at the now greatly expanded clouds. To our delight, the long-awaited two-component distribution was almost immediately apparent (Fig. 7) when the samples were cooled to the regime where BEC was expected. The excitement was tempered by the concern that after so many years of anticipating two component clouds as a signature of BEC, we might be fooling ourselves.

Almost from the beginning of the search for BEC, it was recognized (Lovelace and Tommila, 1987) that as the sample started to condense, there would be a spike in the density and momentum distributions corresponding to the macroscopic population of the ground state. This would show up as a second component on top of the much broader normal thermal distribution of uncondensed atoms. This was the signature we had been hoping to see from our first days of contemplating BEC. The size of the BEC component in these first observations also seemed almost too good to be true. In those days it was known that in the much higher density of the condensate, three-body recombination would be a more dominant effect than in the lower-density uncondensed gas. For hydrogen it was calculated that the condensed component could never be more than a few percent of the sample. The three-body rate constants were totally unknown for alkali atoms at that time, but because of the H results it still seemed reasonable to expect the condensate component might only be a modest fraction of the total sample. But in our first samples we saw it could be nearly 100%! In the light of the prevailing myth of unattainability that had grown up around BEC over the years, our observations seemed too good to be true. We were experienced enough to know that when results in experimental physics seem too good to be true, they almost always are! We worried that in our enthusiasm we might confuse the long-desired BEC with some spurious artifact of our imaging system.

However, our worries about the possibility of deluding ourselves were quickly and almost entirely alleviated by the anisotropy of the BEC cloud. This was a very distinctive signature of BEC, the credibility of which was greatly enhanced to us by the fact that it first revealed itself in the experiment, and then we recognized its significance, rather than vice versa. It was a somewhat fortuitous accident that the TOP trap provided a distinctly anisotropic trapping potential, since we did not appreciate its benefits until we saw the BEC data. A normal thermal gas (in the collisionally thin limit) released from an anisotropic potential will spread out isotropically. This is required by the equipartition theorem. However, a Bose-Einstein condensate is a quantum wave and so its expansion is governed by a wave equation. The more tightly confined direction will expand the most rapidly, a manifestation of the uncertainty principle. Seeing the BEC component of our two-component distribution display just this anisotropy, while the broader uncondensed portion of the sample observed at the same time, with the same imaging system, remained perfectly isotropic (as shown in Fig. 8), provided the crucial piece of corroborating evidence that this was the long-awaited BEC.

By coincidence we were scheduled to present progress reports on our efforts to achieve BEC at three international conferences in the few weeks following these observations (Anderson et al., 1996). Nearly all the experts in the field were represented at one or more of these...
conferences, and the data were sufficient to convince the most skeptical of them that we had truly observed BEC. This consensus probably facilitated the rapid refereeing and publication of our results.

In the original TOP-trap apparatus we were able to obtain so-called pure condensates of a few thousand atoms. By pure condensates we meant that nearly all the atoms were in the condensed fraction of the sample.
Samples of this size were easily large enough to image. Over the few months immediately following the original observation, we undertook the process of a technological shoring up of the machine, until the machine reached the level of reliability necessary to crank out condensate after reproducible condensate. This set the stage for the first generation of experiments characterizing the properties of the condensate, most notably the condensate excitation studies discussed below.

Although by 1995 and 1996 we were able to carry out a number of significant BEC experiments with the original TOP-trap machine, even by 1994, well before the original condensates were observed, we had come to realize the limitations of the single-cell design. Our efforts to modulate the vapor pressure were not very successful, which forced us to operate at a steady-state rubidium vapor pressure. Choosing the value of vapor pressure at which to operate represented a compromise between our need to fill the vapor-cell MOT with as many atoms as possible and our need to have the lifetime in the magnetic trap as long as possible. The single-cell design also compelled us to make a second compromise, this time over the size of the glass cell. The laser beams of the MOT enter the cell through the smooth, flat region of the cell; the larger the glass cell, the larger the MOT beams, and the more atoms we could herd into the MOT from the room-temperature background vapor. On the other hand, the smaller the glass cell, the smaller the radii of the magnetic coils wound round the outside of the cell, and the stronger the confinement provided by the magnetic trap. Hans Rohner in the JILA specialty shop had learned how (Rohner, 1994) to create glass cells with the minimum possible wasted area. But even with the dead space between the inner diameter of the magnetic coils and the outer diameter of the clear glass windows made as small as it could be, we were confronted with an unwelcome tradeoff.

Thus, in 1994, in parallel with our efforts to push as hard as we could toward BEC in our original single-cell TOP trap, we began working on a new technology that would avoid this painful tradeoff. This approach was a modified version of our old multiple loading scheme in which many loads from a MOT were transferred to a magnetic trap in a differentially pumped vacuum chamber. That approach had been defeated by the difficulty in transferring atoms from MOT to magnetic trap without losing phase-space density. There was no dissipation in the magnetic trap to compensate for a slightly too hard or too soft push from one trap to the other. This made us recognize the importance of having dissipation in the second trap, and so we went to a system in which atoms were captured in a large-cell MOT in a region of high rubidium pressure, and then transferred through a small tube into a second, small-cell MOT in a low-pressure region. This eliminated the previous disadvantages while preserving the advantages of multiple loading to get much larger numbers of trapped atoms in a low-vacuum region. The approach worked well, particularly when we found that simple strips of plastic refrigerator magnet material around the outside of the transfer tube between the two traps provided an excellent guide to confine the atoms as they were pushed from one trap to the other (Myatt et al., 1996).

With this scheme we were still able to use inexpensive low-power diode lasers to obtain about one hundred times more atoms in the magnetic trap than in our single MOT-loaded TOP magnetic trap and with a far longer lifetime; we saw trap lifetimes up to 1000 s in the double MOT magnetic trap. This system started working in 1996 and it marked a profound difference in the ease with which we could make BEC (Myatt et al., 1997). In the original BEC experiment everything had to be very well optimized to achieve the conditions necessary for runaway evaporative cooling and thereby BEC. In the double MOT system there were orders of magnitude to spare. Not only did this allow us to routinely obtain million-atom pure condensates, but it also meant that we could dispense with the dark-spot optical configuration with its troublesome alignment. We could be much less precise with many other aspects of the experiment as well.

The first magnetic trap we used with the double-MOT BEC machine was not a TOP trap, but instead was our old baseball-style Ioffe-Pritchard trap. The baseball coil trap is rather complementary to the TOP trap in that each has unique capabilities. For example, the geometry of the TOP trap potential can be changed over a wide range, although the range of dc fields is quite limited. In contrast, the geometry of the baseball coil trap potential can be varied only by small amounts, but the dc bias field can be easily varied over hundreds of gauss. Thus in 1996, when we upgraded the original BEC machine to incorporate the double-MOT technology, we preserved the TOP trap coil design. Each is well suited to certain types of experiments, as will be evident in the discussions below.

With the double-MOT setups we were able to routinely make million-atom condensates in a highly reliable manner in both TOP and baseball-type magnetic traps. These were used to carry out a large number of experiments with condensates over the period from 1996 to the present. Some of our favorite experiments are briefly discussed below.

FAVORITE EXPERIMENTS

Collective excitations

In this section, by excitations we mean coherent fluctuations in the density distribution. Excitation experiments in dilute-gas BEC have been motivated by two main considerations. First, a Bose-Einstein condensate is expected to be a superfluid, and a superfluid is defined by its dynamical behavior. Studying excitations is an obvious initial step toward understanding dynamical behavior. Second, in experimental physics a precision measurement is almost always a frequency measurement, and the easiest way to study an effect with precision is to find an observable frequency that is sensitive to that effect. In the case of dilute-gas BEC, the observed fre-
A frequency-selective method for driving the excitations is to modulate the trapping potential at the frequency of the excitation to be excited (Jin et al., 1996). Experimentally this is accomplished by summing a small ac component onto the current in the trapping magnets. In a TOP trap, it is convenient enough to independently modulate the three second-order terms in the transverse potential. By controlling the relative phase of these modulations, one can impose $m=0$, $m=2$, or $m=-2$ symmetry on the excitation drive.

There have been a very large number of theory papers published on excitations; much of this work is reviewed by Dalfovo et al. (1999). All the zero-temperature, small-amplitude excitation experiments published to date have been very successfully modeled theoretically. Quantitative agreement has been by and large very good; small discrepancies can be accounted for by assuming reasonable experimental imperfections with respect to the $T=0$ and small-amplitude requirements of theory.

The excitation measurements discussed above were then revisited at nonzero temperature (Jin et al., 1997). The frequency of the condensate excitations was clearly observed to depend on the temperature, and the damping rates showed a strong temperature dependence. This work is important because it bears on the little-studied finite-temperature physics of interacting condensates.

Recent theoretical work suggests that good agreement with experiment may hinge on correctly including the role of the excitation drive (Stoof, 2000; Jackson and Zaremba, 2002).

**Two-component condensates**

As mentioned above, the double-MOT system made it possible to produce condensates even if one were quite sloppy with many of the experimental parameters. One such parameter was the spin state in which the atoms are optically pumped before being loaded into the magnetic trap. As our student Chris Myatt was tinkering around setting up the evaporation one day, he noticed, to his surprise, that there seemed to be two different clouds of condensate in the trap. They were roughly at the locations expected for the $2,2$ and $1,-1$ spin states to sit, but that seemed impossible to us because these two states could undergo spin-exchange collisions that would cause them to be lost from the trap, and the spin-exchange collision cross sections were thought to be enormous. After extensive further studies to try and identify what strange spurious effect must be responsible for the images of two condensate clouds we came to realize that they had to be those two spin states. By a remarkable coincidence, the triplet and singlet phase shifts are identical and so at ultralow temperatures the spin-exchange collisions are suppressed in $^{87}$Rb by three to four orders of magnitude! This suppression meant that the different spin species could coexist and their...
mixtures could be studied. In early work we showed that one could carry out sympathetic cooling to make BEC by evaporating only one species and using it as a cooling fluid to chill the second spin state (Myatt et al., 1997). We also were able to see how the two condensates interacted and pushed each other apart, excluding all but a small overlap in spite of the fact that they were highly dilute gases.

These early observations stimulated an extensive program of research on two-component condensates. After Myatt’s original measurements (Myatt et al., 1997), our work in this field, led by postdoc David Hall, concentrated on the $1, -1$ and $2, +1$ states (see Fig. 10) because they could be coherently interconverted using two-photon (microwave plus rf) transitions and they had nearly identical magnetic moments and so saw nearly the same trapping potentials (Matthews et al., 1998). When the two-photon radiation field is turned off, the rate of spontaneous interconversion between the two spin species essentially vanishes, and moreover the optical imaging process readily distinguishes one species from the other, as their difference in energy (6.8 GHz) is very large compared to the excited-state linewidth. In this situation, one may model the condensate dynamics as though there were two distinct quantum fluids in the trap. Small differences in scattering length make the two fluids have a marginal tendency to separate spatially, at least in an inhomogeneous potential, but the interspecies healing length is long so that in the equilibrium configuration there is considerable overlap between the two species (Hall et al., 1998a, 1998b). On the other hand, the presence of a near-resonant two-photon coupling drive effectively brings the two energy levels quite close to one another: on resonance, the corresponding dressed energy levels are separated only by the effective Rabi frequency for the two-photon drive. In this limit, one may in a certain sense think of the condensate as being described by a two-level, spinor field (Cornell et al., 1998; Matthews et al., 1999b).

We got a lot of mileage out of this system and continue to explore its properties today. One of the more dramatic experiments we did in the two-level condensate was the creation, via a sort of wave-function engineering, of a quantized vortex. In this experiment we made use of both aspects of the two-level system—the distinguishable fluids and the spinor gas. Starting with a near-spherical ball of atoms, all in the lower spin state, we applied the two-photon drive for about 100 ms. At the same time, we illuminated the atoms with an off-resonant laser beam whose intensity varied both in time and in space. The laser beam was sufficiently far from resonance that by itself it did not cause the condensate to transition from state to state, but the associated ac Stark shift was large enough to affect the resonant properties of the two-photon drive. The overall scheme is described by Matthews et al. (1999a) and Williams and Holland (1999). The net effect was to leave the atoms near the center of the ball of atoms essentially unperturbed, while converting the population in an equatorial belt around the ball into the upper spin state. This conversion process also imposed a winding in the quantum phase, from 0 around to two pi, in such a way that by the time the drive was turned off, the upper-spin-state atoms were in a vortex state, with a single quantum of circulation (see Fig. 11). The central atoms were nonrotating and, like the pimento in a stuffed olive, served only to mark the location of the vortex core. The core atoms could in turn be selectively blasted away, leaving the upper-state atoms in a bare vortex configuration, whose dynamic properties were shown by postdoc Brian Anderson and grad student Paul Haljan to be essentially the same as those of the filled vortex (Anderson et al., 2000).

Coherence and condensate decay

One of our favorite BEC experiments was simply to look at how a condensate goes away (Burt et al., 1997). The attraction of this experiment is its inherent simplicity combined with the far-reaching implications of the results. Although it was well established that condensates lived for a finite period, fractions of a second to many seconds depending on conditions, no one had identified the actual process by which atoms were being lost from the condensate. To do this our co-workers Chris Myatt, Rich Ghrist, and Eric Burt simply made condensates and carefully watched the number of atoms and shape of the condensate as a function of time. From these data we determined that the loss process varied with the cube of the density, and hence must be three-body recombination. This was rather what we had expected, but it was nice to have it confirmed. In the process of this measurement we also determined the three-body rate constant, and this was more interesting. Although three-body rate constants still cannot be accurately calculated, it was predicted long ago (Kagan et al., 1985) that they should depend on the coherence properties of the wave function. In a normal thermal sample there are fluctuations and the three-body recombination predominantly takes place at high-density fluctuations. If there is higher-order coherence, however, as one has in macroscopically occupied quantum states such as a
FIG. 11. Condensate images showing the first BEC vortex and the measurement of its phase as a function of azimuthal angle: (a) the density distribution of atoms in the upper hyperfine state after atoms have been put in that state in a way that forms a vortex; (b) the same state after a π/2 pulse has been applied that mixes upper and lower hyperfine states to give an interferogram reflecting the phase distribution of the upper state; (c) residual condensate in the lower hyperfine state from which the vortex was formed that interferes with a to give the image shown in (b); (d) a color map of the phase difference reflected in (b); (e) radial average at each angle around the ring in (d). The data are repeated after the azimuthal angle 2π to better show the continuity around the ring. This shows that the cloud shown in (a) has the 2π phase winding expected for a quantum vortex with one unit of angular momentum. From Matthews et al., 1999a [Color].

FIG. 12. Bosenova explosion from Roberts et al. (2001). From top to bottom these images show the evolution of the cloud from 0.2 to 4.8 ms after the interaction was made negative, triggering a collapse. On the left the explosion products are visible as a blue glow expanding out of the center, leaving a small condensate remnant that is unchanged at subsequent times. On the right is the same image amplified by a factor of 3 to better show the 200 nK explosion products [Color].
single-mode laser, or as was predicted to exist in a dilute gas BEC, there should be no such density fluctuations. On this basis it was predicted that the three-body rate constant in a Bose-Einstein condensate would be 3 factorial or 6 times lower than what it would be for the same atoms in a thermal sample. It is amusing that such a relatively mundane collision process can be used to probe the quantum correlations and coherence in this fashion. After measuring the three-body rate constant in the condensate we then repeated the measurement in a very cold but uncondensed sample. The predicted factor of 6 (actually 7.4±2.6) was observed, thereby confirming the higher-order coherence of BEC (Burt et al., 1997).

**Feshbach resonance physics**

In 1992 Eric Cornell and Chris Monroe realized that dipole collisions at ultralow temperatures might have interesting dependencies on magnetic field, as discussed in the Appendix. With this in mind we approached Boudewijn Verhaar about calculating the magnetic-field dependencies of collisions between atoms in the lower F spin states. When he did this calculation he discovered (Tiesinga et al., 1993) that there were dramatic resonances in all the cross sections as a function of magnetic field that are now known as Feshbach resonances because of their similarity to scattering resonances described by Herman Feshbach in nuclear collisions. From the beginning Verhaar appreciated that these resonances would allow one to tune the s-wave scattering length of the atoms and thereby change both the elastic collision cross sections and the self-interaction in a condensate, although this was several years before condensates had been created.

In 1992 we hoped that these Feshbach resonances would give us a way to create enormous elastic collision cross sections that would facilitate evaporative cooling. With this in mind we attempted to find Feshbach resonances in the elastic scattering of first cesium and then, with postdoc Nate Newbury, rubidium. These experiments did provide us with elastic scattering cross sections (Monroe et al., 1993; Newbury et al., 1995), but were unable to locate the few-gauss-wide Feshbach resonances in the thousand-gauss range spanned by then theoretical uncertainty.

By 1997 the situation had dramatically changed, however. A large amount of work on cold collisions, BEC properties, and theoretical advances provided accurate values for the interaction potentials, and so we were fairly confident that there was likely to be a reasonably wide Feshbach resonance in rubidium 85 that was within 20 or 30 gauss of 150 G. This was a quite convenient bias field at which to operate our baseball magnetic trap, so we returned to the Feshbach resonance in the hope that we could now use it to make a Bose-Einstein condensate with adjustable interactions.

The time was clearly ripe for Feshbach resonance physics. Within a year Ketterle (Inouye et al., 1998) saw a resonance in sodium through enhanced loss of BEC, Dan Heinzen (Courteille et al., 1998) detected a Feshbach resonance in photoassociation in 85Rb, we (Roberts et al., 1998; notably students Jake Roberts and Neil Claussen) detected the same resonance in the elastic scattering cross section, and Chu (Vuletic et al., 1999) detected Feshbach resonances in cesium. Our expectations that it would be as easy or easier to form BEC in 85Rb as it was in 87Rb and then use this resonance to manipulate the condensate were sadly naive, however. Due to enhancement of bad collisions by the Feshbach resonance, it was far more difficult and could only be accomplished by following a complicated and precarious evaporation path. However, by finding the correct path and cooling to 3 nK we were able to obtain pure 85Rb condensates of 16,000 atoms (Roberts et al., 2001).

The scattering length of these condensates could then be readily adjusted by varying the magnetic field over a few gauss in the vicinity of the Feshbach resonance (Cornish et al., 2000). This has opened up a wide range of possible experiments, from studying the instability of condensates when the self-interaction is sufficiently attractive (negative a) to exploring the development of correlations in the wave function as the interactions are made large and repulsive. This regime provides one with a new way to probe such disparate subjects as molecular Bose-Einstein condensates and the quantum behavior of liquids, where there is a high degree of correlation. This work represents some of the most recent BEC experiments, but almost everything we have explored with this system has shown dramatic and unexpected results. Thus it is clear that we are far from exhausting the full range of interesting experiments that are yet to be carried out with BEC.

In the first of these Feshbach resonance experiments our students Jake Roberts, Neil Claussen, and postdoc Simon Cornish suddenly changed the magnetic field to make a negative. We observed that, as expected, the condensate became unstable and collapsed, losing a large number of atoms (Roberts et al., 2001). The dynamics of the collapse process were quite remarkable. The condensate was observed to shrink slightly and then undergo an explosion in which a substantial fraction of the atoms were blown off (Donley, 2001). A large fraction of the atoms also simply vanished, presumably turning into undetectable molecules or very energetic atoms, and finally a small cold stable remnant was left behind after the completion of the collapse. This process is illustrated in Fig. 12. Because of its resemblance (on a vastly lower energy scale) to a core collapse supernova, we have named this the Bosenova. There is now considerable theoretical effort to model this process and progress is being made. However, as yet there is no clear explanation of the energy and anisotropy of the atoms in the explosion, the fraction of vanished atoms, and the size of the cold remnant. One of the more puzzling aspects is that the cold remnant can be far larger than the condensate stability condition that determines the collapse point would seem to allow (Donley, 2001).

Another very intriguing result of Feshbach resonance studies in 85Rb was observed when our students Neil Claussen and Sarah Thompson and postdoc Elizabeth Donley quickly jumped the magnetic field close to the
resonance while keeping the scattering length positive. They found that they could observe the sample oscillate back and forth between being an atomic and a molecular condensate as a function of time after the sudden perturbation (Donley et al., 2002). This curious system of a quantum superposition of two chemically distinct species will no doubt be a subject of considerable future study.

An optimistic appendix

Until a new technology comes along to replace evaporative cooling, the crucial issue in creating BEC with a new atom is collisions. In practice, this means that planning a BEC experiment with a new atom requires learning to cope with ignorance. It is easy to forget that essentially nothing is known about the ultralow-temperature collisional properties of any atomic or molecular species that is not an atom in the first row of the Periodic Table. One cannot expect theorists to relieve one’s ignorance. Interatomic potentials derived from room-temperature spectroscopy are generally not adequate to allow theoretical calculations of cold elastic and inelastic collision rates, even at the order-of-magnitude level. Although the cold collisional properties of a new atom can be determined, this is a major endeavor, and in most cases it is easier to discover whether evaporation will work by simply trying it.

Launching into such a major new project without any assurances of success is a daunting prospect, but we believe that, if one works hard enough, the probability that any given species can be evaporatively cooled to the point of BEC is actually quite high. The scaling arguments presented below in support of this assertion are largely the same as those that originally encouraged us to pursue BEC in alkalis, although with a bit more refinement provided by age and experience.

Although there is an extensive literature now on evaporative cooling, the basic requirement is simply that there be on the order of 100 elastic collisions per atom per lifetime of the atoms in the trap. Since the lifetime of the atoms in the trap is usually limited by collisions, the requirement can be restated: the rate of elastic collisions must be about two orders of magnitude higher than the rate of bad collisions. As mentioned above, there are three bad collisional processes, and these each have different dependencies on atomic density in the trap, \( n \): background collisions (independent of \( n \)), two-body dipolar relaxation (\( an \)), and three-body recombination (\( an^2 \)). The rate for elastic collisions is \( nsv \), where \( n \) is the mean density, \( s \) is the zero-energy \( s \)-wave cross section, and \( v \) is the mean relative velocity. The requirement of 100 elastic-to-inelastic collisions must not only be satisfied immediately after the atoms are loaded into the trap, but also as evaporation proceeds toward larger \( n \) and smaller \( v \). With respect to evaporating rubidium 87 or the lower hyperfine level of sodium 23, Nature has been kind. One need only arrange for the initial trapped cloud to have sufficiently large \( n \), and design a sufficiently low-pressure vacuum chamber, and evaporation works. The main point of this section, however, is that evaporation is likely to be possible even with less favorable collision properties.

Considering the trap loss processes in order, first examine background loss. Trap lifetimes well in excess of what are needed for \(^{87}\)Rb and Na have been achieved with standard vacuum technology. For example, we now have magnetic trap lifetimes of nearly 1000 s. (This was a requirement to achieve BEC in \(^{85}\)Rb with its less favorable collisions.) If one is willing to accept the added complications of a cryogenic vacuum system, essentially infinite lifetimes are possible. If the background trap loss is low enough to allow evaporative cooling to begin, it will never be a problem at later stages of evaporation because \( nsv \) increases.

If dipolar relaxation is to be a problem, it will likely be late in the evaporative process when the density is high and velocity low. There is no easy solution to a large dipolar relaxation rate in terms of changing the spring constant of the trap or the pressure of the vacuum chamber. Fortunately, one is not required to accept the value of dipolar collisions that Nature provides. In fact, all one really has to do is operate the trap with a very low magnetic bias field in a magnetic trap, or if one uses an optical trap very far off-resonance (such as \( CO_2 \) laser), trap the atoms in the lowest spin state, for which there are no dipole collisions. The bias field dependence comes about because below a field of roughly 5 G, the dipolar rate in the lower hyperfine level drops rapidly to zero. This behavior is simple to understand. At low temperature, the incoming collisional channel must be purely \( s \) wave. Dipolar relaxation changes the projection of spin angular momentum, so to conserve angular momentum the outgoing collisional channel must be \( d \) wave or higher. The nonzero outgoing angular momentum means that there is an angular momentum barrier in the effective molecular potential, a barrier of a few hundred microkelvin. If the atoms are trapped in the lower hyperfine state \( F=1, m_F=-1 \), in rubidium 87 the outgoing energy from a dipolar collision is only the Zeeman energy in the trapping fields, and for \( B \) less than about 5 G this energy is insufficient to get the atoms back out over the angular momentum barrier. If relaxation is to occur, it can happen only at interatomic radii larger than the outer turning point of the angular momentum barrier. For smaller and smaller fields, the barrier gets pushed further out, with correspondingly lower transition rates.

It is unlikely that the three-body recombination rate constant could ever be so large that three-body recombination would be a problem when the atoms are first loaded from a MOT into the evaporation trap. As evaporation proceeds, however, just as for the dipolar collisions, it becomes an increasingly serious concern. Because of its density dependence, however, it can always be avoided by manipulating the trapping potential. Adiabatically reducing the trap confinement has no effect on the phase-space density but it reduces both the density and the atom velocity. The ratio of three-body to elastic collisions scales as \( 1/nv \). Therefore, as long as
one can continue to turn down the confining strength of one trap, one can ensure that three-body recombination will not prevent evaporative cooling all the way down to the BEC transition.

To summarize, given (i) a modestly flexible magnetic trap, (ii) an arbitrarily good vacuum, (iii) a true ground state with $F=0$, and (iv) nonpathological collisional properties, almost any magnetically trappable species can be successfully evaporated to BEC. If one is using a very far off-resonance optical trap (such as a CO$_2$ dipole trap) one can extend these arguments to atoms that cannot be magnetically trapped. In that case, however, current technology makes it more difficult to optimize the evaporation conditions than in magnetic traps, and the requirement to turn the trap down sufficiently to avoid a large three-body recombination rate can be more difficult. Nevertheless, one can plausibly look forward to BEC in a wide variety of atoms and molecules in the future.

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Nobel lecture: When atoms behave as waves: Bose-Einstein condensation and the atom laser*

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I. INTRODUCTION

The lure of lower temperatures has attracted physicists for the past century, and with each advance towards absolute zero, new and rich physics has emerged. Laypeople may wonder why “freezing cold” is not cold enough. But imagine how many aspects of nature we would miss if we lived on the surface of the sun. Without inventing refrigerators, we would only know gaseous matter and never observe liquids or solids, and miss the beauty of snowflakes. Cooling to normal earthly temperatures reveals these dramatically different states of matter, but this is only the beginning: many more states appear with further cooling. The approach into the kelvin range was rewarded with the discovery of superconductivity in 1911 and of superfluidity in helium-4 in 1938. Cooling into the millikelvin regime revealed the superfluidity of helium-3 in 1972. The advent of laser cooling in the 1980s opened up a new approach to ultralow-temperature physics. Microkelvin samples of dilute atom clouds were generated and used for precision measurements and studies of ultracold collisions. Nanokelvin temperatures were necessary to explore quantum-degenerate gases, such as Bose-Einstein condensates first realized in 1995. Each of these achievements in cooling has been a major advance, and recognized with a Nobel prize.

This paper describes the discovery and study of Bose-Einstein condensates (BEC’s) in atomic gases from my personal perspective. Since 1995, this field has grown explosively, drawing researchers from the communities of atomic physics, quantum optics, and condensed-matter physics. The trapped ultracold vapor has emerged as a new quantum system that is unique in the precision and flexibility with which it can be controlled and manipulated. At least 30 groups have now created condensates, and the publication rate on Bose-Einstein condensation has soared following the discovery of the gaseous condensates in 1995 (see Fig. 1).

The phenomenon of Bose-Einstein condensation was predicted long ago, in a 1925 paper by Albert Einstein (Einstein, 1925b) using a method introduced by Satyendra Nath Bose to derive the black-body spectrum (Bose, 1924). When a gas of bosonic atoms is cooled below a critical temperature $T_c$, a large fraction of the atoms condenses in the lowest quantum state. Atoms at temperature $T$ and with mass $m$ can be regarded as quantum-mechanical wave packets that have a spatial extent on the order of a thermal de Broglie wavelength $\lambda_{dB} = (2\pi\hbar^2/mk_BT)^{1/2}$. The value of $\lambda_{dB}$ is the position uncertainty associated with the thermal momentum distribution and increases with decreasing temperature. When atoms are cooled to the point where $\lambda_{dB}$ is comparable to the interatomic separation, the atomic wave packets “overlap” and the gas starts to become a “quantum soup” of indistinguishable particles. Bosonic atoms undergo a quantum-mechanical phase transition and form a Bose-Einstein condensate (Fig. 2), a cloud of atoms all occupying the same quantum-mechanical state at a precise temperature (which, for an ideal gas, is related to the peak atomic density $n$ by $n\lambda_{dB}^3 = 2.612$). If the atoms are fermions, cooling gradually brings the gas closer to being a “Fermi sea” in which exactly one atom occupies each low-energy state.

Creating a BEC is thus simple in principle: make a gas extremely cold until the atomic wave packets start to overlap! However, in most cases quantum degeneracy would simply be preempted by the more familiar transitions to a liquid or solid. This more conventional condensation into a liquid and solid can only be avoided at extremely low densities, about a hundred-thousandth

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*The 2001 Nobel Prize in Physics was shared by E. A. Cornell, Wolfgang Ketterle, and E. Wieman. This lecture is the text of Professor Ketterle’s address on the occasion of the award. The lecture of Professors Cornell and Wieman is reprinted in the July 2002 issue of Reviews of Modern Physics.

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FIG. 1. Annual number of published papers which have the words “Bose” and “Einstein” in their title, abstracts, or keywords. The data were obtained by searching the ISI (Institute for Scientific Information) database.
the density of normal air. Under those conditions, the formation time of molecules or clusters by three-body collisions (which is proportional to the inverse density squared) is stretched to seconds or minutes. Since the rate of binary elastic collisions drops only proportional to the density, these collisions are much more frequent. Therefore thermal equilibrium of the translational degree of freedom of the atomic gas is reached much faster than chemical equilibrium, and quantum degeneracy can be achieved in an effectively metastable gas phase. However, such ultralow density lowers the temperature requirement for quantum degeneracy into the nanokelvin to microkelvin range.

The achievement of Bose-Einstein condensation required first the identification of an atomic system which would stay gaseous all the way to the BEC transition, and second, the development of cooling and trapping techniques to reach the required regime of temperature and density. Even around 1990, it was not certain that Nature would provide us with such a system. Indeed, many people doubted that BEC could ever be achieved, and it was regarded as an elusive goal. Many believed that pursuing BEC would result in new and interesting physics, but whenever one would come close, some new phenomenon or technical limitation would show up. A news article in 1994 quoted Steve Chu: “I am betting on Nature to hide Bose condensation from us. The last 15 years she’s been doing a great job” (Taubes, 1994).

In brief, the conditions for BEC in alkali gases are reached by combining two cooling methods. Laser cooling is used to precool the gas. The principle of laser cooling is that scattered photons are on average blue-shifted with respect to the incident laser beam. As a result, the scattered light carries away more energy than has been absorbed by the atoms, resulting in net cooling. Blueshifts are caused by Doppler shifts or ac Stark shifts. The different laser cooling schemes are described in the 1997 Nobel lectures in physics (Chu, 1998; Cohen-Tannoudji, 1998; Phillips, 1998). After the precooling, the atoms are cold enough to be confined in a magnetic trap. Wall-free confinement is necessary, otherwise the atoms would stick to the surface of the container. It is noteworthy that similar magnetic confinement is also used for plasmas which are too hot for any material container. After magnetically trapping the atoms, forced evaporative cooling is applied as the second cooling stage (Masuhara et al., 1988; Ketterle and van Druten, 1996; Walraven, 1996). In this scheme, the trap depth is reduced, allowing the most energetic atoms to escape while the remainder rethermalize at steadily lower temperatures. Most BEC experiments reach quantum degeneracy between 500 nK and 2 μK, at densities between $10^{14}$ and $10^{15}$ cm$^{-3}$. The largest condensates are of 100 million atoms for sodium, and a billion for hydrogen; the smallest are just a few hundred atoms. Depending on the magnetic trap, the shape of the condensate is either approximately round, with a diameter of 10–50 μm, or cigar-shaped with about 15 μm in diameter and 300 μm in length. The full cooling cycle that produces a condensate may take from a few seconds to as long as several minutes.

After this short overview, I want to provide the historical context for the search for BEC and then describe the developments which led to the observation of BEC in sodium at MIT. Finally, some examples will illustrate the novel physics which has been explored using Bose-Einstein condensates. A more detailed account of the work of my group has been presented in four comprehensive review papers (Ketterle and van Druten, 1996; Ketterle et al., 1999; Ketterle and Inouye, 2001; Stamper-Kurn and Ketterle, 2001).

II. BEC AND CONDENSED-MATTER PHYSICS

Bose-Einstein condensation is one of the most intriguing phenomena predicted by quantum statistical mechanics. The history of the theory of BEC is very interesting, and is nicely described in the biographies of Einstein (Pais, 1982) and London (Gavroglu, 1995) and reviewed by Griffin (1999). For instance, Einstein made his predictions before quantum theory had been fully developed, and before the differences between bosons and fermions had been revealed (Einstein, 1925a). After Einstein, important contributions were made by, most notably, London, Landau, Tisza, Bogoliubov, Penrose, Onsager, Feynman, Lee, Yang, Huang, Beliaev, and Pitaevskii. An important issue has always been the relationship between BEC and superfluidity in liquid helium, an issue that was highly controversial between London and Landau (see Gavroglu, 1995). Works by Bogoliubov, Beliaev, Griffin, and others showed that...
Bose-Einstein condensation gives the microscopic picture behind Landau's "quantum hydrodynamics." BEC is closely related to superconductivity, which can be described as being due to Bose-Einstein condensation of Cooper pairs. Thus Bose-Einstein condensation is at the heart of several macroscopic quantum phenomena.

BEC is unique in that it is a purely quantum-statistical phase transition, i.e., it occurs even in the absence of interactions. Einstein (1925a) described the transition as condensation "without attractive forces." This makes BEC an important paradigm of statistical mechanics, which has been discussed in a variety of contexts in condensed-matter, nuclear, particle, and astrophysics (Griffin et al., 1995). On the other hand, real-life particles will always interact, and even the weakly interacting Bose gas behaves qualitatively different from the ideal Bose gas (Huang, 1987). It was believed for quite some time that interactions would always lead to "ordinary" condensation (into a solid) before Bose-Einstein condensation would happen. Liquid helium was the only counterexample, where the light mass and concomitant large zero-point kinetic energy prevents solidification even at zero kelvin. Erwin Schrödinger wrote in 1952 in a textbook on thermodynamics about BEC: "The densities are so high and the temperatures so low—those required to exhibit a noticeable departure [from classical statistics]—that the van der Waals corrections are bound to coalesce with the possible effects of degeneration, and there is little prospect of ever being able to separate the two kinds of effect" (Schrödinger, 1952). What he didn't consider were dilute systems in a metastable gaseous phase!

The quest to realize BEC in a dilute weakly interacting gas was pursued in at least three different directions: liquid helium, excitons, and atomic gases. Experimental (Crooker et al., 1983; Reppy, 1984) and theoretical work (Rasolt et al., 1984) showed that the onset of superfluidity for liquid helium in Vycor has features of dilute-gas Bose-Einstein condensation. At sufficiently low coverage, the helium adsorbed on the porous spongelike glass behaved like a dilute three-dimensional gas. However, the interpretation of these results is not unambiguous (Cho and Williams, 1995).

Excitons, which consist of weakly bound electron-hole pairs, are composite bosons. The physics of excitons in semiconductors is very rich and includes the formation of an electron-hole liquid and biexcitons. As nicely discussed by Wolfe et al. (1995) and Fortin et al. (1995), there are systems where excitons form a weakly interacting gas. However, the initial evidence for Bose-Einstein condensation in Cu2O (Lin and Wolfe, 1993) was retracted (O'Hara et al., 1999). Recent work in coupled quantum-well structures is very promising (Butov et al., 2002). When excitons strongly interact with light in a cavity, they form polaritons. In such polariton systems, stimulated scattering and nonequilibrium condensates have been observed recently (Yamamoto, 2000; Saba et al., 2001; Baumberg, 2002).

### III. SPIN-POLARIZED HYDROGEN

Dilute atomic gases are distinguished from the condensed-matter systems discussed above by the absence of strong interactions. Interactions at the density of a liquid or a solid considerably modify and complicate the nature of the phase transition. Hecht (1959) and Stwalley and Nosanow (1976) used the quantum theory of corresponding states to conclude that spin-polarized hydrogen would remain gaseous down to zero temperature and should be a good candidate to realize Bose-Einstein condensation in a dilute atomic gas. These suggestions triggered several experimental efforts, most notably by Silvera and Walraven in Amsterdam, by Gretyak and Kleppner at MIT, and by others at Moscow, Turku, British Columbia, Cornell, Harvard, and Kyoto.

The stabilization of a spin-polarized hydrogen gas (Cline et al., 1980; Silvera and Walraven, 1980) created great excitement about the prospects of exploring quantum-degenerate gases. Experiments were first done by filling cryogenic cells with the spin-polarized gas, and by compressing it, and since 1985, by magnetic trapping and evaporative cooling. BEC was finally accomplished in 1998 by Kleppner, Gretyak and collaborators (Fried et al., 1998). See Gretyak and Kleppner (1984), Silvera and Walraven (1986), Gretyak (1995), and Walraven (1996) and in particular Kleppner et al. (1999) for a full account of the pursuit of Bose-Einstein condensation in atomic hydrogen. Evidence for a phase transition in two dimensions was reported in 1998 (Safonov et al., 1998).

The work in alkali atoms is based on the work in spin-polarized hydrogen in several respects:

- Studies of spin-polarized hydrogen showed that systems can remain in a metastable gaseous state close to BEC conditions. The challenge was then to find the window in density and temperature where this metastability is sufficient to realize BEC.
- Many aspects of BEC in an inhomogeneous potential (Goldman et al., 1981; Huse and Siggia, 1982; Oliva, 1989), and the theory of cold collision processes (see, for example, Stoof et al., 1988) developed in the 1980s for hydrogen could be applied directly to the alkali systems.
- The technique of evaporative cooling was developed first for hydrogen (Hess, 1986; Masuhara et al., 1988) and then used for alkali atoms.

### IV. LASER COOLING

Laser cooling opened a new route to ultralow temperature physics. Laser cooling experiments, with room-temperature vacuum chambers and easy optical access, look very different from cryogenic cells with multilayer thermal shielding around them. Also, the number of atomic species that can be studied at ultralow temperatures was greatly extended from helium and hydrogen to all of the alkali atoms, metastable rare gases, several earth-alkali atoms, and others (the list of laser-cooled...

Some papers and proposals written in the early and mid 1980s, before and during the developments of the basic cooling and trapping techniques, listed quantum degeneracy in a gas as a visionary goal for this new emerging field (Letokhov and Minogin, 1980; Chu et al., 1985; Pritchard, 1986). However, major limitations of laser cooling and trapping were soon identified. Although there is no fundamental low-temperature limit, the final temperature provided by polarization gradient cooling—about ten times the recoil energy—was regarded as a practical limit. Subrecoil laser cooling techniques, especially in three dimensions, were harder to implement, and required long cooling times. The number and density of atoms were limited by inelastic, light-induced collisions (leading to trap loss; see Walker and Feng, 1994; and Weiner, 1995) and by absorption of scattered laser light (Walker et al., 1990), which results in an outward radiation pressure (weakening the trapping potential and limiting the density). Furthermore, since the lowest temperatures could not be achieved at the highest densities (Drewsen et al., 1994; Townsend et al., 1995; 1996), most trapping and cooling techniques reached a maximum phase-space density $n s^3_{\text{DH}} = 10^{-2}$; a value of 2.612 is needed for BEC. This was the situation when the author joined the field of cold atoms in 1990. It was only more recently that major increases in phase-space density were achieved by laser cooling (DePue et al., 1999; Ido et al., 2000; Kerman et al., 2000) but so far laser cooling by itself has not been able to reach BEC.

V. THE EFFORT AT MIT 1990–1996

A. Improving laser cooling

When I teamed up with Dave Pritchard at MIT in 1990 as a postdoc, the initial goal was to build an intense source of cold atoms to study cold collisions and pure long-range molecules. However, Dave and I frequently talked about the limitations in density and temperature of the current techniques and tried to develop ideas on how to get around them. One limitation of magnetic traps is that they can hold atoms only in weak-field-seeking hyperfine states. Therefore a collision between two trapped atoms can lead to a spin flip, and the Zeeman energy is converted into kinetic energy (dipolar relaxation). This process has been a major limitation to the experiments in atomic hydrogen.

First, we asked ourselves if the inclusion of electric and gravitational fields would allow the stable confinement of atoms in their lowest hyperfine states—but the answer was negative (Ketterle and Pritchard, 1992a). One loophole was time-dependent magnetic fields, and building on an earlier proposal (Lovelace et al., 1985), I designed an experiment to confine sodium atoms with ac magnetic fields which looked feasible. However, we learned that Eric Cornell at Boulder had developed a similar idea and experimentally implemented it (Cornell et al., 1991)—so we left the idea on the drawing board. It wasn’t the last time that Eric and I would develop similar ideas independently and almost simultaneously!

Trapping atoms in the lowest hyperfine state was not necessary to accomplish BEC. Already in 1986, Pritchard correctly estimated the rate constants of elastic and inelastic collisions for alkali atoms (Pritchard, 1986). From these estimates one could easily predict that for alkali atoms, in contrast to hydrogen, the so-called good collisions (elastic collisions necessary for the evaporation process) would clearly dominate over the so-called bad collisions (inelastic two- and three-body collisions); therefore evaporative cooling in alkalis would probably not be limited by intrinsic loss and heating processes. However, there was pessimism (Vigué, 1986) and skepticism, and the above-mentioned experimental (Cornell et al., 1991) and theoretical (Ketterle and Pritchard, 1992a) work on traps for strong-field-seeking atoms has to be seen in this context.

In those years, there were some suggestions that time-dependent potentials could lead to substantial cooling, but we showed that this was not possible (Ketterle and Pritchard, 1992b). Real cooling needs an open system which allows entropy to be removed from the system—in laser cooling in the form of scattered photons, in evaporative cooling in the form of discarded atoms. Dave and I brainstormed about novel laser cooling schemes. In 1991, at the Varenna summer school, Dave presented a new three-level cooling scheme (Pritchard and Ketterle, 1992). Inspired by these ideas, I developed a scheme using Raman transitions. Replacing the six laser beams in optical molasses by counterpropagating beams driving the Doppler-sensitive Raman transition, we hoped to realize Doppler molasses with a linewidth that was proportional to the optical pumping rate and therefore adjustable. We had started setting up radio frequency (rf) electronics and magnetic shields for Raman cooling when we heard that Mark Kasevich and Steve Chu were working on Raman cooling using laser pulses (Kasevich and Chu, 1992). For this reason, and also because around the same time we had developed the idea for the Dark SPOT (spontaneous force optical trap; see later in this section), we stopped our work on Raman cooling.

Our experimental work in those years focused first on generating a large flux of slow atoms. In my first months at MIT, when I overlapped with Kris Helmerson and Min Xiao, we built a sodium vapor cell magneto-optical trap (MOT). The idea was inspired by the Boulder experiment (Monroe et al., 1990), and our hope was to vastly increase the loading rate by additional frequencies or frequency chirps added to the red side of the $D_2$ resonance line. The idea failed—we first suspected that nearby hyperfine levels of sodium may have adversely interfered, but it was later shown that it didn’t work for cesium either (Lindquist et al., 1992) because of the unfavorable duty cycle of the chirp. Still, except for a cryogenic setup which was soon abandoned, it was the first
magneto-optical trap built at MIT (Dave Pritchard’s earlier work on magneto-optical trapping was carried out at Bell Labs in collaboration with Steve Chu’s group). We (Michael Joffe, Alex Martin, Dave Pritchard and myself) then put our efforts on beam slowing, and got distracted from pursuing Zeeman slowing by the idea of isotropic light slowing (Ketterle, Martin et al., 1992). In this scheme, atoms are sent through a cavity with diffusely reflecting walls and exposed to an isotropic light field. For red-detuned light the atoms preferentially absorb light from a forward direction and are slowed. The experiment worked very well and it was a lot of fun to do. However, the requirements for laser power and the velocity capture range of this method were inferior to Zeeman slowing, so we decided to build an optimized Zeeman slower.

We adopted the new design by Greg Lafyatis in which the magnetic field increases rather than decreases as in a conventional Zeeman slower (Barrett et al., 1991). We realized that at the magnetic-field maximum it would be possible to apply some additional transverse laser cooling to collimate the slow beam. Michael Joffe, a graduate student, wound a solenoid which had radial access for four extra laser beams. The collimation worked (Joffe et al., 1993), but not as well as we had hoped, and we felt that the small gain was not worth the added complexity. Still, even without collimation, our Zeeman slower provided one of the largest slow-atom fluxes reported until then, and soon after we had a magneto-optical trap with a large cloud of sodium atoms. In hindsight, I am amazed at how many different schemes we considered and tried out, but this may have been necessary to distill the best approach.

The 1991 Varenna summer school on laser cooling was memorable to me for several reasons. I had joined the field of cold atoms just a year earlier, and there I met many colleagues for the first time and established long-lasting relationships. I still have vivid memories of one long afternoon when Dave Pritchard and I sat outside the meeting place, which offered a spectacular view of Lake Como, and brainstormed about the big goals of our field and how to approach them. Dave’s encouragement was crucial to me and helped to increase my self-confidence in my new field of research. We considered options and strategies on how to combine laser cooling and evaporative cooling, something which had been on our mind for some time.

Following the example of the spin-polarized hydrogen experiment at MIT (Masuhara et al., 1988), evaporation could be done in a magnetic trap using rf-induced spin flips, as suggested by Pritchard and collaborators (Pritchard, Helmerson, and Martin, 1989). Magnetic traps and laser cooling had already been used simultaneously in the first experiments on magnetic trapping at NIST (Migdall et al., 1985) and MIT (Bagnato et al., 1987), and on Doppler cooling of magnetically trapped atoms at MIT (Pritchard et al., 1989; Helmerson et al., 1992). In 1990, a magnetic trap was loaded from a magneto-optical trap and optical molasses in Boulder (Monroe et al., 1990). The laser cooling route to BEC was summarized by Monroe, Cornell, and Wieman (1992). So most of the pieces to get to BEC were known in 1990, but there was doubt about whether they would fit together.

Laser cooling works best at low densities where light absorption and light-induced collisions are avoided, whereas evaporative cooling requires a high collision rate and high density. The problem is the much higher cross section for light scattering of $\sim 10^{-7}$ cm$^2$, while the cross section for elastic scattering of atoms is a thousand times smaller. In hindsight, it would have been sufficient to provide tight magnetic compression after laser cooling and an extremely good vacuum to obtain a lifetime of the sample that is much longer than the time between collisions, as demonstrated at Rice University (Bradley et al., 1995). However, our assessment was that one major improvement had to be done to laser cooling to bridge the gap in density between the two cooling schemes. Dave and I discussed possibilities on how to circumvent the density-limiting processes in magneto-optical traps. We considered coherent population trapping schemes in which atoms are put into a coherent superposition state which does not absorb the light. We developed some ideas on how atoms near the center of the trap would be pumped into such a dark state, but the numbers were not too promising. A few months later, a simple idea emerged. If the so-called repumping beam of the magneto-optical trap would have a shadow in the center, atoms would stay there in the lower hyperfine state and not absorb the trapping light, which is near resonant for atoms in the upper hyperfine state. In a MOT, the density is limited by losses due to excited-state collisions and by multiple scattering of light, which results in an effective repulsive force between atoms. When atoms are kept in the dark, the trapping force decreases by a factor which is proportional to the probability of the atoms to be in the resonant hyperfine state. However, the repulsive force requires both atoms to be resonant with the light and decreases with the square of this factor. Therefore there is net gain in confinement by keeping atoms in the dark. Of course, there is a limit to how far you can push this concept, which is reached when the size of the cloud is no longer determined by the balance of trapping and repulsive forces, but by the finite temperature of the cloud.

The gain in density of this scheme, called Dark SPOT, over the standard MOT is bigger when the number of trapped atoms is large. So in 1992, we tweaked up the MOT to a huge size before we implemented the idea. It worked almost immediately, and we got very excited about the dark shadows cast by the trapped atoms when they were illuminated by a probe beam. We inferred that the probe light had been attenuated by a factor of more than $e^{-100}$ (Ketterle et al., 1993a). This implied that we had created a cloud of cold atoms with an unprecedented combination of number and density.

B. Combining laser cooling and evaporative cooling

The following weeks and months were quite dramatic. What should we do next? Dave Pritchard had planned
to use this trap as an excellent starting point for the study of cold collisions and photoassociation—and indeed other groups had major successes along these lines (Heinzen, 1999; Weiner et al., 1999). But there was also the exciting prospect of combining laser cooling with evaporative cooling. We estimated the elastic collision rate in the Dark SPOT trap to be around 100 Hz (Ketterle et al., 1993a) which appeared to be more than sufficient to start runaway evaporation in a magnetic trap. After some discussions, the whole group decided to go for the more ambitious and speculative goal of evaporative cooling. It was one of those rare moments where suddenly the whole group’s effort gets refocused. Even before we wrote the paper on the Dark SPOT trap, we placed orders for essential components to upgrade our experiment to ultrahigh vacuum and to magnetic trapping. All resources of the lab were now directed towards the evaporative cooling of sodium. The Dark SPOT trap was a huge improvement towards combining high atom number and high density in laser cooling. It turned out to be crucial to the BEC work both at Boulder (Anderson et al., 1995) and at MIT (Davis, Mewes, Andrews, et al., 1995) and seems to be still necessary in all current BEC experiments with sodium, but not for rubidium.

The next step was the design of a tightly confining magnetic trap. We decided to use the spherical quadrupole trap, which simply consists of two opposing coils—this design was used in the first demonstration of magnetic trapping (Migdall et al., 1985). We knew that this trap would ultimately be limited by Majorana flops in the center of the trap where the magnetic field is zero. Near zero magnetic field, the atomic spin doesn’t precess fast enough to follow the changing direction of the magnetic field—the result is a transition to another Zeeman sublevel which is untrapped, leading to trap loss. We estimated the Majorana flop rate, but there was some uncertainty about the numerical prefactor. Still, it seemed that Majorana flops would only become critical after the cloud had shrunk due to evaporative cooling, so they shouldn’t get in the way of demonstrating the combination of laser cooling and evaporative cooling. After Michael Joffe presented our approach with the quadrupole trap at the QELS meeting in 1993, Eric Cornell informed me that he had independently arrived at the same conclusion. In 1993, my group reported at the OSA meeting in Toronto the transfer of atoms from the Dark SPOT trap into a magnetic trap, and the effects of truncation of the cloud using rf induced spinflips (Ketterle et al., 1993b).

At about this time, I joined the MIT faculty as assistant professor. Dave Pritchard made the unprecedented offer that if I stayed at MIT he would hand over to me the existing lab, including two grants. To make sure that I would receive the full credit for the work towards BEC, he decided not to stay involved in a field he had pioneered and gave me full responsibility and independence. Dave told me that he wanted to focus on his other two experiments, the single-ion mass measurement and the atom interferometry, although what he gave up was his “hottest” research activity. Even now, I am moved by his generosity and unusual mentorship. The two graduate students on the project, Ken Davis and Marc-Oliver Mewes, who had started their Ph.D.’s in 1991 and 1992, respectively, deliberated whether they should stay with Dave Pritchard and work on one of his other experiments, or continue their work on BEC in a newly formed group headed by a largely unknown assistant professor. They both opted for the latter and we could pursue our efforts without delay, along with Michael Andrews, who joined the group in the summer of 1993.

For a few months we got distracted from our goal of evaporative cooling. Our optical molasses temperatures were higher than those reported by the NIST group (Lett et al., 1989), and we felt that we had to learn the state of the art before we could advance to even lower temperatures. We suspected that the higher density of atoms played a role, but we had to improve our technique of temperature measurements. Our goal was to characterize the interplay of parameters in “dark” molasses where most of the atoms are pumped into the dark hyperfine state. It was also a good project for the graduate students to hone their skills and develop independence. After a few months we had made some progress, but I became concerned about the delay and the competition from Boulder. We decided to drop the project and resume our work on evaporative cooling. Up to the present day, we have never implemented accurate diagnostics for the temperature obtained in laser cooling—it was just not important.

In the spring of 1994, we saw first evidence for an increase in phase-space density by evaporative cooling. We reported these results at an invited talk at the International Quantum Electronics Conference (IQEC) in May 1994. At the same meeting, the Boulder group reported similar results and the limitations due to the Majorana flops as the temperature was reduced. It was clear that the next step was an improvement of the magnetic trap, to trap atoms at a finite bias field which would suppress the Majorana flops. During the meeting, I came up with the idea of plugging the hole with a focused laser beam: a blue-detuned laser beam focused onto the zero-magnetic-field point would exert repulsive dipole forces onto the atoms and keep them away from this region (Fig. 3). This idea seemed so obvious to me that I expected the Boulder group to come up with something similar. It was only at the next conference (ICAP 1994) in Boulder (Davis et al., 1994), when I presented our approach, that I learned about Eric Cornell’s idea of suppressing Majorana flops with a rapidly rotating magnetic field—the so-called TOP trap (Petrich et al., 1995). However, we didn’t implement the optical plug immediately. We wanted first to document our observation of evaporative cooling. We realized that our fluorescence diagnostics were inadequate and implemented absorption imaging which is now the standard technique for observing Bose-Einstein condensation. In those days, we focused on direct imaging of the trapped cloud (without ballistic expansion), and Michael Andrews and Marc-Oliver Mewes developed a sophisticated computer code to simulate absorption images in inhomogeneous mag-
In early 1995, I had to tell my three graduate students that we were rapidly using up startup money and urgently needed one of our two pending proposals approved. Otherwise we would not be able to continue spending money in the way we had done until then and would slow down. Fortunately, in April 1995, the NSF informed me that my proposal was funded. It is interesting to look at some of the reviewers’ comments now, seven years later: “It seems that vast improvements are required [in order to reach BEC]...the current techniques are so far from striking range for BEC that it is not yet possible to make...an assessment...”; “The scientific payoffs, other than the importance of producing a BEC itself, are unclear.” And a third reviewer: “...there have been few specific (or realistic) proposals of interesting experiments that could be done with a condensate.” Despite the skepticism, all reviewers concluded that the proposed “experiments are valuable and worth pursuing.” After we received the funding decision, the whole group celebrated with dinner, and a fourth graduate student (Dallin Durfee), who had expressed his interest already months earlier, could finally be supported.

In late December 1994, our paper on evaporative cooling was submitted, and we were free to focus on plugging the hole. We had to learn how to align a powerful argon ion laser beam and image it through many attenuators without major distortions. When the plug was aligned, the result was spectacular (Fig. 4). We could immediately cool down to lower temperatures and keep many more atoms. During evaporation, the cloud became so cold and small that we couldn’t resolve it anymore. The highest phase-space density measured was a factor of 30 below BEC, but we may have been even closer. We had only a few runs of the experiment before we ran into severe vacuum problems. We focused initially on spatial imaging and became limited by resolution, whereas ballistic expansion and time-of-flight imaging would not have suffered from this limitation. We also thought that BEC would be accomplished at lower densities and in larger clouds, so we worked on adiabatic decompression and ran into problems with the zero of the magnetic field moving away from the plug.

In those months, we were plagued by vacuum problems. The coils inside the vacuum showed some strange outgassing behavior and the vacuum slowly deteriorated...

FIG. 3. Experimental setup for cooling atoms to Bose-Einstein condensation. Sodium atoms are trapped by a strong magnetic field, generated by two coils. In the center, the magnetic field vanishes, which allows the atoms to spin flip and escape. Therefore the atoms are kept away from the center of the trap by a strong (3.5-W) argon ion laser beam (“optical plug”), which exerts a repulsive force on the atoms. Evaporative cooling is controlled by radio-frequency radiation from an antenna. The rf selectively flips the spins of the most energetic atoms. The remaining atoms rethermalize (at a lower temperature) by collisions among themselves. Evaporative cooling is forced by lowering the rf frequency.

In late 1994, we had a “core meltdown.” The magnetic trap was switched on without cooling water, and the silver solder joints of the coils melted. Since in those days the magnetic coils were mounted inside the vacuum chamber, we had a catastrophic loss of vacuum and major parts of our setup had to be disassembled. I will never forget the sight of coils dripping with water behind a UHV viewport. This happened just a few hours before MIT’s president, Charles Vest, visited our lab to get firsthand information on some of the research done on campus. He still remembers this event. We had lost weeks or months of work in a very competitive situation. I was despondent and suggested to the group that we go out for a beer and then figure out what to do, but the students immediately pulled out the wrenches and started the repair. I was moved to see their dedication and strength, even at this difficult time. We replaced the magnetic trap by a much sturdier one. This turned out to be crucial for the implementation of the plugged trap where the precise alignment of a laser beam relative to the magnetic field center was important. So in hindsight the disaster may not have caused a major delay.

FIG. 4. Absorption images of atom clouds trapped in the optically plugged trap. Cloud (a) is already colder than was attainable without the “plug” (Ar ion laser beam). Cloud (b) shows the breakup of the cloud into two “pockets” in the two minima of the potential. The size of cloud (c) reached the optical resolution of the imaging system (≈10 μm) still absorbing 90% of the probe light. This sets an upper bound on temperature (≈10 μK) and a lower bound on density (5 × 10^{12} cm^{-3}).
rated. We went through several bakeouts of the ultrahigh-vacuum chamber in the spring and summer of 1995. Furthermore, Ken Davis had to write his Ph.D. thesis and stopped working in the lab. It is interesting to recall my assessment of the field in those months; I didn’t realize that BEC was just around the corner. In Tom Greytak’s and Dan Kleppner’s group the BEC transition was approached to within a factor of 3.5 in temperature in 1991 (Doyle et al., 1991), but it took several more years to advance further. So I prepared for a long haul to cover the last order of magnitude to BEC.

By this time, the group was reinforced by Dan Kurn (now Dan Stamper-Kurn), a graduate student, and Klaasjan van Druten, my first postdoc. After months of working on vacuum and other problems, we were just ready to run the machine again when we heard about the breakthrough in Boulder in June of 1995 (Anderson et al., 1995). We feverishly made several attempts with traps plugged by focused laser beams and light sheets, and tried different strategies of evaporation without success. The clouds disappeared when they were very cold. We conjectured that some jitter of the laser beam was responsible, and when accelerometers indicated vibrations of our vacuum chambers, we immediately decided to eliminate all turbo and mechanical pumps. Unfortunately, when we were exchanging the turbo pump on our oven chamber against an ion pump, we caused a leak in the ultrahigh-vacuum part and had to go through another long bakeout. We also implemented a pointing stabilization for the optical plug beam. But when we finally obtained BEC, we realized that it didn’t improve the cooling.

These were difficult months for me. The Rice group had cooled lithium to quantum degeneracy (Bradley et al., 1995). A new subfield of atomic physics was opening up, and I was afraid that our approach with sodium and the plugged trap would not be successful and we would miss the excitement. I considered various strategies. Several people suggested that I adopt the successful TOP trap used at Boulder. But I had already started to study several possible configurations for magnetic confinement. I realized that a highly elongated Ioffe-Pritchard trap with adjustable bias field could provide a good confinement that was equivalent or superior to the TOP trap. Around August 1995, Dan Kurn worked out an optimized configuration, which was the cloverleaf winding pattern (Mewes et al., 1996a). I considered having the whole group work on this new approach, but several in my group wanted to give the plugged trap a few more attempts and at least characterize how far we could approach BEC with our original approach. Fortunately, we followed that suggestion—it is always a good idea to listen to your collaborators.

C. BEC in sodium

This was the situation on September 29, 1995, when we observed BEC in sodium for the first time. The goal of the run was to measure the lifetime of the trapped atoms and characterize possible heating processes. For our ultrahigh-vacuum pressure, rather slow evaporation should have been most efficient, but we found out that faster evaporation worked much better. This was a clear sign for some other loss or heating process, e.g., due to fluctuations in the position of the plug. Around 11:30 p.m., an entry in the lab book states that the lifetime measurements were not reliable, but they indicated lifetimes around ten seconds, enough to continue evaporation. Fifteen minutes later we saw some dark spots in time-of-flight absorption images, but they were quite distorted since the optical plug beam, which we couldn’t switch off, pushed atoms apart during the ballistic expansion (Fig. 5). Still, the sudden appearance of dark spots meant groups of atoms with very small relative velocity. For the next few hours, we characterized the appearance of those spots, but then decided that further progress required an acousto-optical modulator to switch off the optical plug. Between 4:00 and 5:30 in the early morning, we installed optics and rf electronics and were finally able to switch off the argon ion laser beam during ballistic expansion. A few minutes later, we observed the bimodal distributions that are now the hallmark of BEC. The lab book of this night captured the excitement of the moment (Fig. 6).

Those first measurements were done by imaging the atoms in the lower hyperfine (F=1) state. For the next run, which took place a few days later, we prepared optical pumping and imaging on the cycling F=2 transition, and obtained a much better signal-to-noise ratio in our images. The occurrence of BEC was very dramatic...
MIT with its long tradition in atomic physics was a special place to pursue the BEC work. The essential step was the combination of laser cooling and evaporative cooling. My next-door neighbors in Building 26 at MIT have been Dave Pritchard, a pioneer in laser cooling who conceived the magneto-optical trap, and Dan Kleppner, who together with Harald Hess and Tom Greytak conceived and realized evaporative cooling (see Fig. 9). I feel privileged for the opportunity to combine their work and take it to the next level. It is hard to overestimate the roles which Dave Pritchard and Dan Kleppner have played for modern atomic physics. The family tree of atomic physicists (Fig. 10) shows some of the remarkable physicists who were trained and inspired by them.

Looking back, it seems that many techniques such as the Dark SPOT, compressed MOT (Petrich et al., 1994), the TOP trap and the optically plugged trap were critical for first demonstrating BEC, but by no means indispensable. This is best illustrated by the experiment at Rice, which used only Doppler cooling to load the magnetic trap—a technique which had been developed in the 1980s. The collision rate was slow, but an excellent vacuum made a very slow evaporation process possible (Bradley et al., 1995). So in hindsight, BEC in alkali gases did not require major innovations in cooling and trapping. It merely required enough optimism to risk a few years in the attempt to combine laser and evaporative cooling. Such an attempt needed a few years of very focused work as it involved the integration of several technologies that were not standard in the field, including ultrahigh vacuum, sensitive CCD cameras and image processing, high-current power supplies for magnetic traps, and flexible computer control of a multistep cooling and detection process. Figure 11 compares a state-of-the-art laser cooling experiment in 1993 to a BEC experiment in 2001 using the same vacuum apparatus in the same laboratory at MIT. A lot of components have been added, and I continue to be impressed by my collaborators, who now handle experiments far more complex than I did some five years ago.

D. The cloverleaf trap

After our first observation of BEC, we made the right decision for the wrong reason. We expected many other groups to quickly upgrade their laser cooling experiments to magnetic trapping and evaporative cooling, and to join in during the next few months. Nobody expected that it would take almost two years before the next groups succeeded in reaching BEC (the groups of Dan Heinzen, Lene Hau, Mark Kasevich, and Gerhard Rempe followed in 1997). I was concerned that our plugged trap would put us at a disadvantage since the trapping potential strongly depended on the shape and alignment of the laser focus. So we decided to install the cloverleaf trap instead and discontinue our plugged trap after only two experimental BEC “runs.”

Since we didn’t want to break the vacuum, we installed the new trap in an unfavorable geometry. The

FIG. 6. One page of the lab book during the night of September 29, 1995, when BEC was first observed at MIT. The handwriting is by Klaasjan van Druten. At 5:50 a.m., we had installed a new acousto-optical modulator to switch off the optical plug (Ar ion laser beam). Fifteen minutes later, we had the first definitive evidence for BEC in sodium. (Fig. 7). Our animated rendering of the data obtained in that run (done by Dallin Durfee) became well known (see Durfee and Ketterle, 1998). We had obtained condensates with 500,000 atoms, 200 times more than in Boulder, with a cooling cycle (of only nine seconds) 40 times shorter. Our paper was quickly written and submitted only two weeks after the experiment (Davis, Meewes, Andrews, et al., 1995).

In my wildest dreams I had not assumed that the step from evaporative cooling to BEC would be so fast. Figure 8 shows how dramatic the progress was after laser and evaporative cooling were combined. Within less than two years, the number of alkali atoms in a single quantum state was increased by about 12 orders of magnitude—a true singularity demonstrating that a phase transition was achieved!
magnet coils for the plugged trap were oriented vertically in reentrant flanges, and when we replaced them with cloverleaf coils, the weakly confining axis of the Ioffe-Pritchard trap was vertical. In such a geometry, the gravitational sag would reduce the efficiency of rf-induced evaporation since atoms would only evaporate at the bottom of the cloud (Ketterle and van Druten, 1996; Surkov et al., 1996). But before breaking the vacuum and reorienting the coils, we wanted to see the limitation. Around December 1995, when we were just starting to look at the efficiency of evaporation, we lost the vacuum once again due to a cracked ceramic part in an electric feedthrough and decided to reorient the whole experiment, with the weakly confining axis of the trap now aligned horizontally. Since that time, now more than six years, the machine has been under vacuum. This is in sharp contrast to the conditions in 1995, when we had to open the chamber, pump down, and bake out every couple of months. Finally, we had learned from our previous mistakes and developed a very systematic procedure for pumpdowns and bakeouts.

I still remember the night of March 13, 1996, when the experiment was up and running, and Klaasjan van Druten and I had fine-tuned the bias field of the magnetic trap, so that the switchover to the new magnetic trap was finally completed. It was already after midnight, too late to start some serious work, when Klaasjan asked half jokingly why don’t we just try to get BEC. Without knowing what our temperatures and densities were, without having ever measured the trap frequencies, we played around with the rf sweep that determines the cooling trajectory, and a condensate showed up around 2:10 a.m. We were relieved since we hadn’t produced condensates for almost half a year, but also the ease at which we got the condensate in a new trap told us our setup was robust and that we were ready to

FIG. 7. Observation of Bose-Einstein condensation by absorption imaging. Shown is absorption vs two spatial dimensions. The Bose-Einstein condensate is characterized by its slow expansion observed after 6 ms time of flight. The left picture shows an expanding cloud cooled to just above the transition point; middle: just after the condensate appeared; right: after further evaporative cooling has left an almost pure condensate. The total number of atoms at the phase transition is about $7 \times 10^5$; the temperature at the transition point is 2 $\mu$K [Color].

FIG. 8. Progress in evaporative cooling of alkali atoms up to 1996. The number of atoms in the lowest quantum state is proportional to the phase-space density and has to exceed a critical number of 2.612 to achieve Bose-Einstein condensation. For $N_0 < 10^{-3}$, the increase in phase-space density due to evaporation is plotted. For the Rice result of July 1995 see Bradley et al. (1995) and the erratum (Bradley et al., 1997).
switch from engineering cooling schemes and traps to the study of the condensate. The cloverleaf trap and other winding patterns for the Ioffe-Pritchard configuration are now used by almost all BEC experiments. Figure 12 shows the experimental setup during those days.

Why hadn’t we considered this trap earlier and avoided the detours with the quadrupole trap, Majorana flops, and plugging the hole? First, the quadrupole trap was simpler to build, and it allowed us to pursue evaporative cooling faster. Second, we initially favored the quadrupole trap based on an analysis which shows that confinement by a linear potential is much stronger than by the quadratic potential of the Ioffe-Pritchard configuration (Ketterle, Durfee, and Stamper-Kurn, 1999). However, a very elongated Ioffe-Pritchard trap provides effectively linear confinement in the two radial directions, and it was only in 1995 that I realized that it would be easy to adiabatically deform the round laser-cooled cloud to such an elongated shape.

The next weeks were exciting and dramatic; we implemented dispersive imaging and saw for the first time the condensate in the trap. We could take images nondestructively and recorded two sequential images of the same condensate. After year-long concerns of how fragile and sensitive the condensate would be once created, it was an overwhelming experience to observe the condensate without destroying it. Figure 13 shows a spatial image of a condensate; it was taken in nondestructive dispersive imaging. We first implemented dispersive imaging using the dark-ground technique (Andrews et al., 1996), but soon upgraded to phase-contrast imaging, which was the technique used to record the figure.

FIG. 9. MIT faculty in ultralow-temperature atomic physics. Dan Kleppner, W.K., Tom Greytak, and Dave Pritchard look at the latest sodium BEC apparatus [Color].

FIG. 10. Family tree of atomic physicists. People with names in italics are Nobel laureates.
In the first week of April 1996, there was a workshop on “Collective effects in ultracold atomic gases” in Les Houches, France, where most of the leading groups were represented. It was the first such meeting after the summer of 1995, and it was not without strong emotions that I reported our results. Since no other experimental group had made major progress in BEC over the last few months, it was our work which provided optimism for further rapid developments.

E. Interference between two condensates

After we got BEC in the cloverleaf trap, both the machine and the group were in overdrive. After years of building and improving, frequent failures and frustration, it was like a phase transition to a situation where almost everything worked. Within three months after getting a condensate in the cloverleaf trap we had written three papers on the new trap and the phase transition (Mewes et al., 1996a), on nondestructive imaging (Andrews et al., 1996), and on collective excitations (Mewes et al., 1996b). Klaasjan van Druten left the group, shortly after Christopher Townsend had joined us as a postdoc. As the next major goal, we decided to study the coherence of the condensate. With our optical plug, we had already developed the tool to split a condensate into two halves and hoped to observe their interference, which would be a clear signature of the long-range spatial coherence.

Around the same time, the idea came up to extract atoms from the condensate using rf induced spin flips—the rf output coupler. Some theorists regarded an output coupler as an open question in the context of the atom laser. I suggested to my group that we could simply pulse on the radio-frequency source that was already used during evaporation, and couple atoms out of the condensate by flipping their spin to a nontrapped state (Fig. 14). The experiment worked the first time we tried it (but the quantitative work took awhile; Mewes et al., 1997). I have never regarded the output coupler as one of our major accomplishments because it was so simple, but it had impact on the community and nobody has ever since regarded outcoupling as a problem!

In July 1996, we had the first results on the rf output coupler, and also saw the first fringes when two condensates were separated with a sheet of green light and overlapped in ballistic expansion. I was in Australia for vacation and for the IQEC conference in Sydney. By e-mail and telephone I discussed with my group the new results. The fringes were most pronounced when the condensates were accelerated into each other by removing the light sheet shortly before switching off the mag-

FIG. 11. Comparison of a laser cooling and BEC experiment. The first photograph shows the author in 1993 working on the Dark SPOT trap. In the following years, this laser cooling experiment was upgraded to a BEC experiment. The second photograph shows the same apparatus in 2001 after many additional components have been added [Color].
We concluded that some of the fringes might be related to sound and other collective effects that occur when two condensates at fairly high density “touch” each other. I presented those results at the Sydney meeting only to illustrate we were able to do experiments with two condensates, but now we had to sort out what was happening.

It took us four more months until we observed clean interference between two condensates. When two condensates that were initially separated by a distance $d$ interfere and the interference pattern is recorded after a time $t$ of ballistic expansion, then the fringe spacing is the de Broglie wavelength $\frac{h}{mv}$ associated with the relative velocity $v = \frac{d}{t}$. For our geometry with two condensates about 100 mm in length, we estimated that we would need at least 60 ms of time of flight to observe fringes with a 10-μm period, close to the resolution of our imaging system. Unfortunately, due to gravity, the atoms dropped out of the field of view of our windows after 40 ms. So we tried to gain a longer expansion time in a fountain geometry where we magnetically launched the atoms and observed them when they fell back through the observation region after more than 100 ms (Townsend et al., 1997), but the clouds were distorted. We also tried to compensate gravity by a vertical magnetic-field gradient. Some time later I learned about new calculations by the theory group at the Max Planck Institute in Garching, showing that the effective separation of two elongated condensates is smaller than their center-of-mass separation (Röhrl et al., 1997). This meant that we could observe interference fringes after only 40 ms, just before the atoms fell out of the observation region. We immediately had a discussion in the group and decided to stop working on fountains and “antigravity” and simply let the atoms fall by 8 mm during 40 ms.

We made some ambiguous observations where we saw low-contrast fringes together with some optical interference patterns of the probe light, but the breakthrough came on November 21, 1996, when we observed striking interference patterns (Fig. 15). I still remember the situation late that night when we wondered how could we prove beyond all doubt that these were matter-wave interference patterns and not some form of self-diffraction of a condensate confined by a light sheet and then released. We came up with the idea of eliminating one of the condensates in the last moment by focusing resonant yellow light on it. Whimsically, this laser beam was dubbed the “flame thrower.” If the fringes were self-diffraction due to the sharp edge in the confinement, they would remain; if they were true interference they would vanish. This was like a double slit experiment in
optics where you cover one of the slits. It took a few hours to align the new laser beam, and we verified in phase-contrast imaging that we were able to selectively eliminate one of the two condensates.

We had a switch in our control panel which toggled between condensate elimination on and off. Then we went back and aligned the setup for the observation of interference. When we toggled the switch we had to wait for about half a minute until a new condensate was produced. This was the moment of truth. If the fringes appeared without a second condensate, then Nature would have fooled us for the whole night—but they disappeared and an enormous tension disappeared, as well. It was already early the next morning, with people arriving to work. I walked to Dan Kleppner’s office and told him there was something he should see. So he shared the moment with us where we toggled the switch on alternating cooling cycles and correspondingly, the interference pattern disappeared and reappeared. Interference between two light beams is quite a sight, but with atoms it is more dramatic. Destructive interference means that atoms plus atoms add up to vacuum!

The evidence for interference was so compelling that we submitted our paper based solely on the data of one experimental run (Andrews et al., 1997). This run is memorable to me for another reason: it was to be the last time I played a major role in preparing and running an experiment. During the night, I had put in the optics for the “flame thrower.” Up to then, I was familiar with every piece of equipment in the lab and never thought this could change quickly, but it was like another phase transition. Hans-Joachim Miesner had just arrived, the first postdoc who stayed for more than a year, and he soon took over much responsibility for organizing the lab. There were more demands on my time to write papers and give talks, the group grew with the addition of two more graduate students (Shin Inouye and Chris Kuklewicz), and we had intensified our efforts to build a second BEC experiment. All this coincided in a few months. After earning my Ph.D. in 1986, I had spent eleven more years in the lab during three postdoc positions and as an assistant professor, but now began to play an advisory role.
The papers on the rf output coupler (Mewes et al., 1997) and the interference (Andrews et al., 1997) of two condensates appeared in the same week in January 1997. Together they demonstrated the ability to create multiple pulses of coherent atoms, and have been regarded as the realization of an atom laser. The period starting with the early dreams of pursuing BEC and ending with the observation of the coherence of the condensate was remarkable. It was full of speculation, dreams, unknown physics, failures and successes, passion, excitement, and frustration. This period fused together a team of very different people who had one common denominator: the passion for experimental physics. It was a unique experience for me to work with these outstanding people (Fig. 16).

VI. THE MAGIC OF MATTER WAVES

Many studies of BEC's have been performed over the last several years. The progress until 1998 is nicely summarized in the Varenna summer school proceedings (Inguscio et al., 1999). The studies that were most exciting for me displayed macroscopic quantum mechanics, the wavelike properties of matter on a macroscopic scale. These were also phenomena that no ordinary gas would show and illustrated dramatically that a new form of matter had been created. The interference of two condensates presented above (Fig. 15) is one such example. In the following, I want to discuss the amplification of atoms and the observation of lattices of quantized vortices.

These two examples are representative of the two areas into which research on gaseous BEC can be divided: in the first (which could be labeled “the atomic condensate as a coherent gas” or “atom lasers”), one would like to have as little interaction as possible—almost like the photons in a laser. The experiments are preferably done at low densities. The Bose-Einstein condensate serves as an intense source of ultracold coherent atoms for experiments in atom optics, in precision studies or for explorations of basic aspects of quantum mechanics. The second area could be labeled “BEC as a new quantum fluid” or “BEC as a many-body system.” The focus here is on the interactions between the atoms that are most pronounced at high densities. The coherent amplification of atoms is an example of atom optics with condensates, and the study of vortices addresses the superfluid properties of the gas.
A. Amplification of atoms in a Bose-Einstein condensate

Since atoms are de Broglie waves, there are many analogies between atoms and light, which consists of electromagnetic waves. This is exploited in the field of atom optics where atoms are reflected, diffracted, and interfere using various atom-optical elements (Adams et al., 1994). One important question was whether these analogies can be extended to the optical laser, which is based on the amplification of light. When our group demonstrated a rudimentary atom laser in 1997 we had solved the problem of outcoupling (or extracting) atoms from the BEC and of verifying their coherence. The atomic amplification process happened during the formation of a Bose-Einstein condensate (Miesner et al., 1998) which is quite different from the way light is amplified in passing through an active medium. It was only in 1999 that our group managed to observe the amplification of atoms passing through another cloud of atoms serving as the active medium [Inouye, Pfau, et al., 1999 (simultaneously with the group in Tokyo; Kozuma et al., 1999)].

Amplifying atoms is more subtle than amplifying electromagnetic waves because atoms can only change their quantum state and cannot be created. Therefore, even if one could amplify gold atoms, one would not realize the dreams of medieval alchemy. An atom amplifier converts atoms from the active medium into an atomic wave that is exactly in the same quantum state as the input wave (Fig. 17).

The atom amplifier requires a reservoir, or an active medium, of ultracold atoms that have a very narrow spread of velocities and can be transferred to the atomic beam. A natural choice for the reservoir was a Bose-Einstein condensate. One also needs a coupling mechanism that transfers atoms from the reservoir at rest to an input mode while conserving energy and momentum. This transfer of atoms was accomplished by scattering laser light. The recoil of the scattering process accelerated some atoms to exactly match the velocity of the input atoms (Fig. 18). Not only were the atoms amplified, but they were in exactly the same motional state as the input atoms, i.e., they had the same quantum-mechanical phase. This was verified by interfering the amplified output with a copy of the input wave and observing phase coherence.

This direct observation of atom amplification in the summer of 1999 was preceded by a surprising occurrence late one night in October 1998 when we discovered a new form of superradiance (Inouye, Chikkatur, et al., 1999). We were studying Bragg spectroscopy (Stenger et al., 1999) and illuminated a BEC with two laser beams. I had no role in the running of the experiment and was working in my office, when around midnight the students came from the lab and told me that they saw atoms shooting out from the condensate with a velocity component perpendicular to the direction of the laser beams. We expected atoms to receive recoil momentum only along the laser beams, and all motion perpendicular to it to be diffuse due to the random direction of spontaneous Rayleigh scattering.

The whole lab started to discuss what was going on. With a running machine, everything could be tried out immediately. The first ideas were mundane: let’s illuminate the condensate with only one laser beam and see what happens (the directional beams remained). We scrutinized the experimental setup for bouncing laser beams or beams which had not been completely switched off, but we found nothing. Increasingly, we considered that the observed phenomenon was genuine and not due to some experimental artifact. Knowing that the condensate was pencil shaped, the idea of laser emission along the long condensate axis came up, and this was already very close. We decided to stop the general discussion and continue taking data; the machine was running well and we wanted to take advantage of it. So some students, including Shin Inouye and Ananth Chikkatur, characterized the phenomenon, while Dan...
Stamper-Kurn and I went to a blackboard and tried to figure out what was going on. Within the next hour, we developed the correct semiclassical description of super-radiance in a condensate. In the lab, the predicted strong dependence on laser polarization was verified. A few months later we realized how we could use the super-radiant amplification mechanism to build a phase-coherent atom amplifier. However, the labs were undergoing complete renovation at this point and we had to wait until the machine was running again before the phase-coherent amplification was implemented.

The demonstration of an atom amplifier added a new element to atom optics. In addition to passive elements like beam splitters, lenses, and mirrors, there is now an active atom-optical element. Coherent matter wave amplifiers may improve the performance of atom interferometers by making up for losses inside the device or by amplifying the output signal. Atom interferometers are already used as precise gravity and rotation sensors.

### B. Observation of vortex lattices in Bose-Einstein condensates

Quantum mechanics and the wave nature of matter have subtle manifestations when particles have angular momentum, or more generally, when quantum systems are rotating. When a quantum-mechanical particle moves in a circle the circumference of the orbit has to be an integer multiple of the de Broglie wavelength. This quantization rule leads to the Bohr model and the discrete energy levels of the hydrogen atom. For a rotating superfluid, it leads to quantized vortices (Nozières and Pines, 1990). If one spins a normal liquid in a bucket, the fluid will finally rotate as a rigid body where the velocity smoothly increases from the center to the edge (Fig. 19, left). However, this smooth variation is impossible for particles in a single quantum state. To fulfill the above-mentioned quantization rule, the flow field has to develop singular regions where the number of de Broglie wavelengths on a circumference jumps up by one. One possibility would be a radially symmetric flow field with concentric rings. Between adjacent rings, the number of de Broglie wavelengths on a circumference would change by one.

However, the energetically most favorable configuration is achieved when the singularities in the velocity field are not distributed on cylindrical shells, but on lines. This corresponds to an array of vortices. In contrast to classical vortices like those in tornados or in a flushing toilet, the vortices in a Bose-Einstein condensate are quantized: when an atom goes around the vortex core, its quantum-mechanical phase changes by exactly $2\pi$. Such quantized vortices play a key role in superfluidity and superconductivity. In superconductors, magnetic flux lines arrange themselves in regular lattices that have been directly imaged. In superfluids, previous direct observations of vortices had been limited to small arrays (up to 11 vortices), both in liquid $^3$He (Yarmchuk et al., 1979) and in rotating gaseous Bose-Einstein condensates (BEC's) by a group in Paris (Madison et al., 2000).

In 2001, our group observed the formation of highly-ordered vortex lattices in a rotating Bose-condensed gas (Abo-Shaeer et al., 2001). They were produced by spinning laser beams around the condensate, thus setting it into rotation. The condensate then exhibited a remarkable manifestation of quantum mechanics at a macroscopic level. The rotating gas cloud was riddled with more than 100 vortices. Since the vortex cores were smaller than the optical resolution, the gas was allowed to ballistically expand after the magnetic trap was switched off. This magnified the spatial structures 20-fold. A shadow picture of these clouds showed little bright spots where the light penetrated through the empty vortex cores as if through tunnels (Fig. 20 shows a negative image).

A striking feature of the observed vortex lattices is the extreme regularity, free of any major distortions, even
near the boundary. Such “Abrikosov” lattices were first predicted for quantized magnetic flux lines in type-II superconductors. However, Nature is not always perfect: some of the images showed distortions or defects of the vortex lattices; two examples are shown in Fig. 21. The physics of vortices is very rich. Subsequent work by my group and others has started to address the dynamics and nonequilibrium properties of vortex structures. How are vortices formed? How do they decay? Are the vortices straight or bent? Such experiments can be directly compared with first-principles calculations, which are possible for such a dilute system. This interplay between theory and experiment may lead to a better understanding of superfluidity and macroscopic quantum phenomena.

VII. OUTLOOK

The rapid pace of developments in atomic BEC during the last few years has taken the community by surprise. After decades of searching for an elusive goal, nobody expected that condensates would be so robust and relatively easy to manipulate. Further, nobody imagined that such a simple system would pose so many challenges, not only to experimentalists, but also to our fundamental understanding of physics. The list of future challenges, both for theorists and for experimentalists, is long and includes the exploration of superfluidity and second sound in Bose gases, the physics of correlations and nonclassical wave functions (phenomena beyond the Gross-Pitaevskii equation), the study of quantum-degenerate molecules and Fermi gases, the development of practical “high-power” atom lasers, and their application in atom optics and precision measurements. These scientific goals are closely interwoven with technological advances to produce new single- or multi-species quantum-degenerate systems and novel ways of manipulation, e.g., using microtraps and atom chips. There is every indication for more excitement to come!


FIG. 21. Vortex lattices with defects. In the left image, the lattice has a dislocation near the center of the condensate. In the right one, there is a defect reminiscent of a grain boundary. (Reprinted with permission from Abo-Shaeer et al., 2001. Copyright 2001 American Association for the Advancement of Science.)


have contributed to this rich and exciting field. Some of these colleagues are depicted in Fig. 23, which is a group photo of the lecturers at the Varenna summer school on BEC in 1998. In particular, the yearlong competition with the group at Boulder led by Eric Cornell and Carl Wieman inspired the best from me and my team, and despite tight competition, there has been genuine collegiality and friendship. I want to thank the Office of Naval Research, the National Science Foundation, the Army Research Office, the Joint Services Electronics Program, NASA, and the David and Lucile Packard Foundation for their encouragement and financial support of this work.

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