

Introduction

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The simplest way to characterize my physics career is that I blast atoms with laser light. I actually have been doing this from my early undergraduate days in Dan Kleppner's lab at MIT. However I like to think that my work has been guided by somewhat more elevated intellectual theme and as a result my research career has followed a somewhat meandering but fairly continuous path that is reflected in my papers. The general theme is that I looked for how I could use the technical capabilities of laser light to study atoms in new ways and in new regimes, with a preference for tackling relatively fundamental physics questions. This meant that I was not only carrying out new physics experiments, but I was also working to advance the technology itself and experimental techniques.

As a graduate student with Ted Hansch, I was interested in utilizing the new (at that time) capabilities of narrowband tunable lasers to do high resolution laser spectroscopy in hydrogen. This simplest of all atoms could be quantitatively understood in terms of basic quantum electrodynamics, and thus lasers allowed more precise measures of the energy splittings and correspondingly new tests of QED. By the end of my graduate school days, however, I had become convinced that QED had been so well established that testing it was unlikely to lead to any interesting surprises. The theory of neutral currents and electroweak unification was just coming into vogue at that time, and as such it was the natural extension of my interest. Here was an area where one could do high precision spectroscopy in atoms to measure parity nonconservation (PNC), and thereby explore territory that was far less trod than QED. Of course the downside was that the PNC effects were incredibly tiny and so the experiments were very difficult. This challenge however appealed to my inclination to see just how far one could push experimental capabilities.

By pushing the state of the art in several aspects of laser spectroscopy, my group was able to see, and then ultimately measure parity violation in atomic cesium to quite high precision. These were very difficult experiments requiring large amounts of technology development followed by many years of tedious careful measurements and vast amounts of checking and double-checking for possible experimental errors. This was the work that first established my reputation as a scientist, and in retrospect, there were a lot of easier paths that I might have followed.

In the midst of doing the PNC experiments, we used portions of the apparatus to carry out numerous other precision measurements on the cesium atom to test the atomic theory that was needed to interpret our parity violation results in terms of fundamental physics. This work also honed our experimental skills and provided publications in the long intervals between each new PNC result.

Ultimately, we achieved a much more precise measurement of PNC in atoms, and a correspondingly more precise low energy test of the Standard model of elementary particle physics that, as of this date, is still significantly more precise than any other work. Rather to our surprise, in our second generation measurement we observed that there was the strong suggestion of a small part of the atom PNC that depended on the nuclear spin. After long agonizing over this unexpected and undesired effect in the experiment, we discovered that in fact there were theoretical predictions that such an effect might arise from a strange and never before seen entity called an "anapole moment". Our third and last PNC experiment, which took more than ten years to complete was sufficiently precise that it provided a reasonably good measurement of the anapole moment. Although this result attracted significant interest and raised a number of significant questions about parity nonconservation in the nuclear forces, it is a bit remarkable that as of this writing, no experimental group other than ours has been able to achieve sufficient sensitivity to observe an anapole moment in any system, now some

twenty years after our original observation.

To carry out the third generation of atomic parity nonconservation experiments required four tunable very narrowband lasers. At that time the only laser technology available were dye lasers and although, or perhaps because, we were experts in that technology, we knew that the cost and complexity of four suitable dye lasers would be unreasonable. So my graduate student Rich Watts and I began to explore the capabilities of inexpensive, but rather badly behaved, diode lasers. This led to many years of development of diode laser technology and the application of diode lasers in atomic physics research. One of our first and most recognized applications was to show that diode lasers could be used to slow atoms, thereby reducing the cost of the laser system required for atom slowing by nearly a factor of one thousand compared to what had been used. Although the original work was intended merely to show off the capabilities of the inexpensive new laser technology, this work led me to a long and profitable involvement of exploring new capabilities to use light to cool and trap atoms and the study of the light-atom and atom-atom interactions in this new ultracold atom regime.

After some years of that work, we had progressed to the point that I thought my group understood enough about the atomic physics and we had developed enough technological capabilities for cooling and trapping atoms in various ways that it would be worth taking the gamble of pursuing the “holy grail” of Bose-Einstein condensation. Eric Cornell joined me in that quest and after five years of work we were successful. After making BEC, there were so many new and exciting experiments that could be done with BEC, and those experiments were so easy compared to the PNC work, that BEC soon became the dominant focus of my research.

Throughout most of my career I have been interested in physics education. I always had many undergraduates working in my research labs, and I regularly worked on innovations for teaching undergraduates. I was always struck by the way that students seem to learn little or nothing in classes towards becoming an actual functioning physicist. I could see this in the courses that I taught and in seeing them starting to work in my lab as undergraduate and graduate students. This was in dramatic contrast to the way that a few years in the research lab routinely transformed them into highly competent physicists; something that 16+ years of schooling seemed incapable of doing. From this, it was clear to me that there was some sort of intellectual process present in the research lab that was sorely missing from the traditional education process. As my physics research career was reaching the point where it seemed like there were few if any new heights to reach, I became increasingly interested in science education as a research activity and an area where I might be able to make a substantial impact. I began to see how doing careful research on how people learn physics, and how guiding one’s teaching by the results of that research could work as well in education as it did in the physics lab. This convinced me that science education could be dramatically improved if teachers could be persuaded to break with tradition and follow a new more scientific approach. After receiving the Nobel Prize and realizing the potential for using the stature that comes with the Prize to advance this idea, I have devoted an increasingly large amount of my time and effort to education. This involves both carrying out research in physics education and serving as a public advocate for improved science education and how to achieve it.