Students would also learn little from this lab about the conduct of science; it would probably reinforce the idea that science is dull, procedural, and thoughtless. They have no particular reason to study this topic except to pass the course; they did not participate in planning the experiment or developing the procedures; nor was the stated hypothesis really a question they had. Since there was no internal motivation to study this, they would not really attend to the results or think about their consequences.

The lack of student learning in labs like this is directly related to the lack of thinking it requires. The careful procedures, the concern for safety, and the general atmosphere that penalizes mistakes all mitigate against questioning, risk-taking, thinking, and learning. It is as though both teachers and students subscribe to a mechanistic model of learning which posits that going through certain steps without thinking will somehow magically result in learning. This is antithetical to all science is about.

A Glimpse of Science: Helium Evaporation

I studied liquid helium evaporation for my PhD thesis, and it is useful to contrast this experience with the student lab. The importance of this tale is how I, as a typical graduate student, went about learning about experimental science, which I discovered was so different from the model I had learned in school.

Superfluid liquid helium is fascinating. It is the only substance that is liquid down to absolute zero. Below 2.17 K the common isotope, He-4, becomes a superfluid with amazing properties. For instance, if you put superfluid helium in a cup, it will creep up the sides, over the rim and out, emptying in no time. It conducts heat thousands of times better than silver, the next-best thermal conductor. It pours through molecule-size holes (super-leaks) that block all other substances, including liquid He above 2.17 K. Who knows what practical applications might result from a better understanding of such a remarkable substance. It might bathe future computers, keeping them cold and removing the heat they generate.

When I was a graduate student, the theoretical reasons for these odd properties were only beginning to be understood. Part of the answer lay in the thermal vibrations in the liquid which had to be thought of as particles, in much the way light waves must be considered as particles—photons—when they interact with atoms. If some wild guesses were made about the properties of these thermal wave/particles, called phonons and rotons, then many of the unusual properties of liquid helium could be calculated. But the calculations were esoteric and many questions remained. Experimental evidence was needed in several areas before there could be much advance.

Finding a good experiment that would contribute to this emerging theory was an enormous challenge. (For tale of a fellow graduate student's difficulties in selecting an experiment, see Cohen, 1974). The experiment had to be theoretically interesting and experimentally feasible. It had to provide valuable information whether or not some breathtaking new result was discovered. We (there was a team of graduate students in my group all on the prowl for good experiments) explored many ideas that led nowhere. Either the effect was immeasurably small, or the experiment far too complex, or the result unremarkable. I felt like we fooled around a lot, just wasting months as we played in the shop and speculated about many areas of physics; this was amusing but a bit scary because the end was nowhere is sight. In retrospect, we were sharpening a wide range of skills and giving ourselves enough background to recognize a good project. But this was not a conscious plan, and I was worried because there seemed to be no script we were following like the comforting "scientific method" I learned in school.

If there were a good experiment to be done but not already performed by the legions of other researchers, perhaps it would rely on the particular strengths of our group which lay in measurements on beams of atoms. So, while inventing and discarding experiments, I stumbled into the craft of atomic beams: metal machine shop skills, soldering and brazing, drafting, creating and controlling a high vacuum, electronic instrumentation and atom detection. One of the arts involved forming ultra-sharp needles using electrochemistry and observing them using microscopic techniques borrowed from biology. I also pursued the theory of helium and shopped around for mathematical techniques used in other fields that might be applied. In retrospect, all these activities seem purposeful and coordinated to support the experiment I did. But in reality, the experiment was determined by what skills I learned, and that learning was haphazard, at best.

This was a time of waiting and deep personal doubt and frustration. What was THE experiment going to be? What if it revealed nothing? Who cares anyway? Is this a sensible way to spend one's life given the social crises erupting around us (this was the late 60's)? To stay sane and connected to reality, I consciously spent more than 50% of my time on political action, trying to increase minority employment at MIT. I increasingly saw the graduate school life as unnatural and cloistered; difficult primarily because of the sacrifices it demanded.

The final choice of experiment did not appear in a flash of inspiration but slowly emerged as the best of many contenders. In a way, it was disappointing because it was so obvious; none of our wilder speculations developed into a breathtaking experiment. A hunch that the rotons should influence the speed of the evaporating atoms seemed reasonable. There were supposed to be a lot of energetic rotons in the superfluid and this might directly influence evaporation causing an abnormal number of energetic evaporating atoms. This hunch was confirmed by increasingly sophisticated calculations that indicated that unusual evaporation around .3 K could be measured and would yield a clear answer to the hunch.

The measurement fit all the criteria. Even if my hunch was wrong it would be interesting to understand why. It suggested an experiment that used many of the traditional molecular beam approaches and equipment, so I could be fairly confident that the measurement could be done. Measurements could be made on both He-3 and He-4 yielding two theses, so I could team with another student who was weaker theoretically but a better experimentalist.

It still took years to design the apparatus, get it perfected, redesign it, and get it working. The extremely low temperatures were a challenge, the evaporated helium generated a fierce background signal, the rotating chopper just above the helium surface vibrated, and the apparatus generated too much heat to run the experiment long enough. We had to come up with numerous inventions to overcome these and other problems. Once I goofed and dropped an expensive glass Dewar that exploded like a small bomb on the concrete floor; it took weeks to replace. For months the maze of pipes leaked and I really began to doubt whether I could hold out. After we assembled some part of the equipment and finding it leaked for the ninetieth time, I was ready to throw in the towel.

It seemed I was working on a degree in plumbing instead of physics. Days were spent with a leak detector that could find a few atoms slithering into the vacuum system. I had known that science research was not glamorous, but I was not prepared for how non-intellectual but psychologically difficult it was. Weeks slipped by where I never thought once about rotons, where the biggest problem was summoning the psychic energy to find the next leak.

I was in awe of others around the lab who were not put off by these petty failures and setbacks, but just plunged back in and tried again. PhDs, even a young post-doc who was clearly an ass, because they had survived all this and triumphed, took on mythic stature in my eyes, their state of grace seemed so lofty and unaccessible.

But eventually, the beast worked and yielded results; not all at once, but in fits and starts separated by months of additional leak-hunting. Unfortunately, we saw no sign of the rotons in the evaporating atoms , but it did not matter; the result was original and significant, and the thesis was quickly written.

I dwell at some length on this experience for two reasons. First, note that there is hardly a hint of the usual hypothesis-experiment-deduction paradigm in this story. There was a hypothesis—rotons influence evaporation—but it completely fails to encapsulate the thought process. A better description of our thinking was total immersion in a fascinating problem, permitting us to eventually find a niche where our skills and persistence helped uncover something original and valuable. Second, note the broad nature of what I learned while following this narrow, arcane speciality: construction, plumbing, electronics, drafting, microscopy, electrochemistry, mathematics, and invention itself. I learned about heating and cooling, properties of metals, machine screw sizes, sharpening a metal lathe tool, and different kinds of solders. I also learned about boiling under reduced pressures, since the way we reached these low temperatures was to boil He-4 and He-3 in a vacuum. A similar breadth of learning through immersion in focused experimentation can be brought to almost any classroom.

The Real Scientific Method

The "scientific method" is often held up as a script scientists supposedly follow. As a child, the emphasis on scientific method at school–a seven step sequence leading inevitably to a conclusion–lead me to believe the way scientists proceeded was alogical, outside the realm of normal thinking. This is a perversion of the truth. You could map my graduate school experiences onto the scientific method: my hunch can be described as a hypothesis, I certainly had a method that was described in my thesis, and I did conclude something. It is just that converting my bumbling around in the lab to a rigid, semi-mystical, desiccated "scientific method" gives a completely wrong impression.

In reality, there is nothing mystical about the scientific method; scientists approach their problems much the way a carpenter approaches the problem of designing kitchen cabinets or a teacher devises a strategy for a difficult student. In each case, you think about the problem, draw heavily on your experience and intuition, perhaps consult some references to see what others may have done in similar situations, develop a hunch or two, devise a plan of action using that hunch, and see whether the plan was effective. In each case, it is just common sense.

The Lab Notebook provides a perfect case study of the erroneous impression of science schools convey. Most science students are compelled to keep a lab book consisting of a series of experiments, each starting with a hypothesis, detailing the apparatus, method, observations, and ending with a conclusion. All the best notebooks are virtually identical; the poorer ones have parts missing that should be there. The hidden message is that the lab book regimen is part of science; after all, one of the justifications for subjecting all students to science is to convey a sense of its intellectual traditions. And, of course, the intellectual tradition embodied in a school lab notebook is deadly; from it any student would conclude that in science there is no thought, no originality, no half-baked ideas, no departure from the norm.

Again this is a perversion. Most scientists do keep notebooks, but they read much like the personal journals a writer might keep, except with more math and equipment thrown in. Notebooks are used to help scientists remember things too easily forgotten–where to order special tools, what solder worked best at low temperatures, bright ideas, notes from interesting articles. They do also contain information about experimental apparatus and procedures, so again, school has conveyed a partial truth, but one that has all the juices squeezed out of it.

The common thread in all of science is the *search for simple, mechanistic explanations that tie together the largest number of observations*. The word "simple" may seem surprising here, since science is so often perceived as "hard". This is unfortunate, and probably derives more from bad teaching than from the underlying concepts. The teaching about science too often emphasizes mathematics because it seems to simplify the concepts to the lecturer although, unfortunately, it usually obfuscates them for the learner. "Mechanistic" is also important in this definition to distinguish science from other perfectly valid human pursuits, such as art, poetry and astrology that do not put a premium on objective, verifiable chains of cause and effect. Finally, the idea that scientific explanations have power, in the sense that they tie together the largest number of observations, is an amazing comment on the world in which we live. It could be that general laws did not exist, or that huge numbers of scientific principles were needed. The remarkable fact about our reality is that a very few laws and principles have very broad applicability. Hence, we owe it to our students to convey an impression of this, that the search for general principles is often rewarded with understanding that brings order into our life.

Science progresses through the *application of common sense and involves communicating ideas that convince critics*. There is no special thought process scientists use. They are careful in their own work and critical of others' because they want to be right and have learned that it is easy to make mistakes. They eschew arguments from authority because the authority might be wrong, and they assume that their audience is as cranky as they are, only believing statements that can be reproduced by an objective observer. A consequence of this that surprises scientists and outsiders alike is that the ability to communicate is essential to success in science. The scientist must be able to present his or her views and defend them, often using persuasive writing and speaking skills that go beyond pure objectivity.

A final observation about contemporary science is in order: it is *collaborative*. The pervasive image of an Einstein independently inventing general relativity is highly misleading. While there continue to be a few isolated Einsteins, the vast bulk of science is a collaborative affair, in two senses. First, much of science is undertaken in groups, with the result that original scientific papers usually have multiple authors, sometimes dozens. My thesis work is a case in point, where the initial ideas were tossed about in a group of 5-10 and the actual research was done with one other graduate student. This is a commentary on the scale of much of science, requiring expensive equipment, big labs and team funding. It is also a commentary on human nature, which is stimulated by others with similar, but differing, outlooks, sharing an environment and goals. But, in a second sense, the collaboration extends even to the isolated researchers who, while working alone, must read the literature, attend meetings, obtain funding, and communicate results with the society of peer scientists.

This view of the work of scientists as simple, careful, common-sense, and collaborative is quite accessible to students and must not be transformed into some mystical, incomprehensible and illogical "scientific method". It should be easier for students to imagine becoming scientists using this demystified view of science. Still, it seems a long way between a simple lab on vapor pressure to thesis research on Helium vapor. Fortunately, it is not necessary to be a graduate student to ask scientifically interesting questions.

Science is a Web; Science is Everywhere.

How do you ask questions that lead to interesting science? Good science to investigate is all around us. The most prosaic objects can lead to a wealth of science, if you learn to ask the right questions.

The Faraday Christmas Lectures on a Candle

In the late 1850's Michael Faraday, the great English chemist, was fond of delivering Christmas lectures about candles as his way of illustrating how much science can be found in something this humble. His lecture began:

> I propose to bring before you, in the course of these lectures, the Chemical History of a Candle. ... were it left to my own will, I should prefer to repeat it almost every year–so abundant is the interest that attaches itself to the subject, so wonderful are the varieties of outlet which it offers into the various departments of philosophy. There is not a law under which any part of this universe is governed which does not come into play, and is touched upon in these phenomena. There is no better, there is no more open door by which you can enter into the study of natural philosophy, than by considering the physical phenomena of a candle.

He goes on to perform dozens of fascinating experiments on fire, heat, capillary action, gas flow, convection, solid/liquid and liquid/gas transitions, light, spectra, and much more. Even though we now have a far better understanding of atomic phenomena, his lectures of 130 years ago remain a lively and extremely broad introduction to science that we would do well to emulate.