Some Thoughts About Cold Fusion, 20 Years Later: "Schussbooming," Falling into Life and Some Other History

Scott Chubb

It is difficult for me to separate my own human experiences from the ideas and science that have taken place since the announcement of Martin Fleischmann and Stanley Pons on March 23, 1989—in which they suggested they might have created a form of nuclear fusion, through ordinary chemistry. On the one hand, the claims were so audacious and so outrageous that after six weeks, most scientists who tried to repeat the experiments stopped working on the problem and "ran for cover." At the time, I was "fearless," in the sense that I had little responsibility in my life, although this was about to change dramatically. But in spite of the dramatic change, I persisted and stayed involved.

Shortly more than two weeks after March 23, my uncle Talbot Chubb and I literally rushed to get to the office of *Nature* Magazine, fearing we were about to be "scooped" about an idea that we were sure was right. At the time, Talbot said to me, "We certainly will know in six months if what Pons and Fleischmann are claiming makes sense." Adding to the confusion was evidence involving different, lower level effects by Steven Jones and his co-workers at Brigham Young University. The rest, as they say, is history.

There are a number of books about the history of the cold fusion controversy. Even here, confusion still exists. The important books are based on the words and actions of the important, competent people. These include: *Fire from Ice: Searching for the Truth Behind the Cold Fusion Furor*, by my dear friend and the founder of *Infinite Energy*, the late Dr. Eugene Mallove; the later books *Excess Heat: Why Cold Fusion Prevailed*, by Charles Beaudette, and *The Rebirth of Cold Fusion: Real Science, Real Hope, Real Energy*, by Steven B. Krivit and Nadine Winocur. These books provide a very accurate history of the relevant facts.

As opposed to providing a detailed description of the history that is chronicled in these books, I would like to make a number of personal observations, based on my own experiences. Some of these may seem rather personal to share, but I think they are important because they convey a message about perseverance, having a positive attitude and having hope about the future. Gene Mallove captured the essence of this message through his forthright courageous generosity, in basic terms, but also in his book and by taking the kind of firm stand that was necessary to create *Infinite Energy*. My words mirror this attitude.

Carrying on with a particular project, in spite of what appears to be a bleak situation, really can make a difference. Gene stood for this idea in basic terms and in his actions. A second person who also felt this way, whom I really wish had been a dear friend, was the late Guiliano Preparata. Guiliano had an indelible spirit and good will. His underlying ideas were physically sound and important. His colleague, Emilio Del Guidice, has carried on, in the finest sense of doing good science, by re-expressing Guiliano's ideas. Julian Schwinger forthrightly also worked in this field, based on this tradition, as has Brian Josephson more recently. Schwinger, Preparata, Del Guidice and Josephson are all theorists. Arthur Schawlow, an experimentalist and Nobel laureate, also spoke forthrightly about the reality of the associated effects. It is sad to say that these voices have not been heard by the wider scientific community. A reason that I think this has occurred is because of oversimplification and confusion. Hopefully with time, Preparata, Del Guidice, Schwinger and Schawlow will be remembered as they should be remembered with regard to the controversy: as idealists and visionary scientists who took firm stands in seeking the truth and expressing it. Their being so forthright should and must be remembered and acknowledged.

My good colleagues and friends, Peter Hagelstein and Yeong Kim, have also continued in this vein. They have forthrightly presented ideas with perseverance, based on sound physical principles. Others, and there are so many that it is impossible to name all of them, have also been persistent. Mitchell Swartz, David Nagel, Akito Takahashi, K.P. Sinha, Talbot Chubb, Hideo Kozima, John Fisher, Xing Zhong Li, Fanzil Gareev, Yuri Bazhutov and Robert Bass have all persevered. From an experimental perspective, of course, Martin Fleischmann and Stanley Pons, and my colleagues Stanislaw Szpak, Pamela Mosier-Boss, Frank Gordon, Lawrence Forsley, Melvin Miles and Ashraf Imam also deserve significant credit for what they have done, as do Michael McKubre, Irving Dardik, John Bockris, Edmund Storms, Richard Oriani, Yoshiaki Arata and Yuechang Zhang. These are just a few names that should be cited. Steven Jones also belongs in this list, simply because of his heroic stance associated with dealing with explaining a very different effect that potentially might be relevant. Others who have contributed significant experimental results include Jean-

Paul Biberian, Francesco Scaramuzzi, George Miley, Jacques Dufour, John Dash, Francis Tanzella, Alexander Karabut, Irina Savvatimova, Andrei Lipson, Alexander Roussetski, Vittorio Violante, Francesco Celani, Antonella De Ninno, Thomas Passell, D. Russ George, Thomas Benson, Thomas Claytor, Mahadeva Srinivasan, Jirohita Kasagi, Yasuhiro Iwamura, Tadahiko Mizuno, Roger Stringham, Dennis Letts, and Dennis Cravens. The names I am including are the ones that immediately come to my mind. Certainly other people have been involved who have made significant (potentially even greater) contributions to the field.

In thinking about the tortuous and difficult task of reviewing all of the remarkable events that have taken place since the day of the initial announcement on March 23, 1989 (an overview which, I feel, has been adequately done by others already), it occurred to me instead to provide a personal account from those early days which typifies the excitement and adventure of those times.

A remarkable event was taking place for me right at the time of the initial announcement. My wife, Anne Pond, was pregnant, with our first child, Scott Chubb, Jr. I was so excited about the Fleischmann-Pons effect that when visiting the obstetrician's office two days before my son's birth (my wife was a week overdue), I could not take my eyes away from a picture on the cover of *Time*, which showed Pons and Fleischmann and the caption "Fusion or Illusion?". Then two days later, within minutes of the time my son was born, I was actually telling one of the orderlies about the remarkable Pons-Fleischmann discovery. I am sure that my wife was not thrilled with my distraction, but I hope that my family can appreciate that my excitement and awe over science also translates into the personal realm. It was easy to be distracted with such a remarkable science breakthrough in the forefront of the news, and the Pons-Fleischmann story adds an important historical backdrop to the birth of my son. They are actually both very important (one personal, one professional) times from my life.

In thinking about the road that those of us in the cold fusion field have followed, I often think about my father and the important lessons that he taught me about life (and science).

One story I would like to relate about my relationship with my dad becomes a sort of metaphor for taking chances and facing failure (and learning how to move on from it). When I was five, my father put me on skis during a family outing in southwestern New Hampshire, at the Mt. Sunapee ski area. Predictably, my first experience was a disaster—in terms of my ability to ski and my general attitude about the experience. But, my father constantly encouraged me to get up after I fell, and I learned an important lesson from that. Two years later, my family went on a second skiing vacation, this time to a smaller, less formal place called, "Dutch Hill." Because "Dutch Hill" was smaller and had a "friendlier" crowd, I began to enjoy skiing in a new way. As we started to walk out to the slope, I saw a teenager speeding down a trail called "Dutchman's Holiday." He was literally "falling," in a sense. His skis were straight together as he "schussboomed" down the hill. To "schuss" a ski slope means that you go straight down the slope. A skier is not afraid of falling even though as he goes down the slope, he goes faster and faster. As I saw this person "Schussbooming Dutchman's Holiday," I felt a sense of beauty and awe. The fun that he felt was all at once infectious and inspiring.

There are interesting parallels between the excitement that "Schussbooming" creates and the excitement that occurs in cold fusion. Skis barely have contact with the snow, but their effect is real, and the cold fusion effect is almost imperceptible but quite real. Like the quiet sound of skis going faster and faster in the snow, the weak signals that came from the initial cold fusion experiments are now becoming more and more repeatable and understandable. Their intensity is also increasing.

The sense of "risk" in performing experiments that superficially appear to be so impossibly simple and so outrageous, in the context of thermonuclear fusion, inspires a sense of beauty and awe that is similar to the beauty and awe that "Schussbooming" provokes in me. The people who have been involved in cold fusion are not afraid, in a figurative sense, of "falling down" (as when a skier falls down) in failing to meet their expectations in their attempts to accomplish something that for many scientists appears to be impossible.

My father inspired me to do things that are fun, based on finding appropriate "trails" (in a figurative sense) that are not too difficult to follow but are also challenging. The idea of being able to fall and not being worried about failing is an important theme that my Dad passed on to the people who knew him. The idea of finding the right "trail" and "falling down" it, and having fun in the process is an important lesson that should be remembered that has resulted from the cold fusion controversy.

Sixteen years after my "Dutchman's Holiday" skiing experience, I spent a few weeks in Aspen, Colorado as a "ski bum" (working for a few hours to earn enough to cover a lift ticket). While there, I was able to reflect and I took time to simply think about a particular problem for many hours. I realized how to generalize a very complicated, esoteric mathematical problem; this resulted in my becoming more intimately involved with my thesis advisor, Professor David Fox. Fox was an extraordinarily idealistic physicist. The problem he introduced me to—associated with the possibility of what is referred to as a Bose Einstein Condensation, involving esoteric forms of "particles," called "excitons"—was so far-removed from "practical" areas of research that what I accomplished in this project had no obvious bearing on my "getting a job" (and, for that matter, for many years, on anything related to my professional career). Even more bizarre is that, although what I accomplished in this work was really quite extraordinary (mathematically), even the experts in the field (the two-dimensional Onsager-Ising Lattice) were not interested in what I had done.

As a consequence, with negligible support for the work, Fox advised me to find a different advisor. By chance, I contacted Martin Blume, who was an adjunct professor at Stony Brook. Blume was involved with the development of the National Synchrotron Light Source at Brookhaven National Laboratory, and he told me that he would not be an appropriate advisor. But he recommended I contact one of his colleagues, Victor Emery, who also was at Brookhaven. Emery suggested that I talk to a relatively new theorist, Brookhaven's James Davenport. Davenport suggested a problem that I realize, with hindsight, was very different from the kinds of mainstream problems that most graduate students were involved with—studying the behavior of electrons on the surfaces of palladium hydride. I share this part of my journey because these somewhat "obscure," off-the-beaten path problems, involving seemingly unrelated areas of research, are responsible for my becoming involved with cold fusion.

An important lesson that I learned from my time studying is the unexpected possibility that something obscure might in the end be important. An important point is that great science involves intensity and persistence and the belief that "failure" is only in the eye of the beholder. As I look back at what happened, I realize my father's guidance about having fun doing science and in life in general has had a lasting effect that has helped me in the cold fusion field.

My father recommended that I apply for a job at the Naval Research Laboratory (NRL). Eventually, I did this and I became an NRL employee, but only after I was employed first as National Research Council Research Associate and then as an outside contractor.

In the fall of 1988, when I was an outside contractor, things changed dramatically when I started to collaborate with my uncle, Talbot Chubb. Dr. Michael Melich suggested the collaboration, based on work on developing re-usable rockets. Beginning shortly after March 23, 1989, I started to talk to Talbot about the possibility that cold fusion might actually not be a colder version of conventional fusion but that it could involve something very different. I suggested the bizarre idea that it might be possible to explain what Fleischmann and Pons had discovered by combining the two "obscure" dissertation problems that I had been exposed to as a graduate student. Specifically, the kind of Bose Einstein Condensate (BEC) associated with particles (similar to excitons) that are bosons that either are created or are fundamentally associated with interactions involving a periodically ordered lattice is very different from the kind of BEC that occurs in free space. The situation involving the lattice does not require that the BEC form at low temperatures. This observation suggested that a very different "problem," involving deuterons (which are also bosons, on length scales that are far from a potential collision) in a periodic lattice, could be relevant.

The idea that inspired our theory was that in particular situations (involving high-loading in palladium deuteride) deuterons might behave just like "excitons" (like the initial problem Fox suggested to me), which means they can behave like electrons in an ordered lattice, in a certain sense by being wave-like, but (as a result of the second problem I had studied) this basic picture actually made great sense. Then, as opposed to being a colder version of conventional fusion, the deuterons could occupy (or, as I later suggested, interact through) "energy band states" involving ions. Although the language that I am suggesting here seems esoteric, this is actually very far from the truth. What I am (and have been) suggesting (for 20 years) is that it actually is quite reasonable that in fully-loaded palladium deuteride, what is known about how deuterium (hydrogen) behaves in a solid can involve effects that are well-known that are extremely different from the kinds of effects that occur in the absence of the solid. An important point, however, is not merely the fact that a solid is involved, it is the fact that a fully-loaded palladium-deuteride solid is involved. The key point is the assumption that in palladium-deuteride, important details associated with the structure and the behavior of the electrons can play a fundamental role in initiating the associated effects that were observed by Fleischmann and Pons.

I knew from my work involving palladium hydride that this kind of picture can make sense. The point is that when hydrogen is loaded, at low concentrations into palladium, it is known that each hydrogen nucleus (a proton) can behave as if it can become wave-like (and can occupy a kind of energy band state, that we have called an "ion band state," that is similar to the kind of wave-like energy band state that an electron can occupy). I had learned this through the second problem associated with my dissertation at Brookhaven, which Jim Davenport had suggested (although in an indirect manner). What was novel, for both Talbot and me, was the idea that deuterium nuclei (deuterons) could also behave this way. The reason that this idea was new and interesting is that since a deuteron has a proton and a neutron, it is a boson (on the length and time scales associated with the ion band states that we suggested could be important), while a hydrogen nucleus (a proton) is a fermion. The novel and interesting aspect of this difference is that in the seemingly esoteric problem involving "excitons," new and potentially "exotic" forms of interaction can occur, through the formation of the "higher temperature" kind of BEC that can take place associated with excitons.

The very idea that such an outrageous possibility could be relevant stirred the imagination. Refinements based on this seemingly different, seemingly "obscure" idea formed the basis of a somewhat remarkable set of conjectures that appear to have been confirmed: 1) "High-loading," associated with a "well-defined limit" (which does not have to be exact) can initiate the conditions that are necessary for the deuterons to become wave-like; 2) Because deuterons involve proton-neutron pairs, at distances far from the location of possible forms of nuclear overlap, the inherent statistics that are associated with Bose Einstein Condensates are necessary, which means that the associated forms of interaction inherently are different than in conventional fusion, most of the time. In particular, there does exist one reaction (deuteron (d)+d→helium-4+gamma ray) where these kinds of effects are important.

Furthermore, for our initial hypothesis to make sense, any helium-4 that could be created (which is the product that would be expected) would have to be created in such a way that the conditions that could be responsible for it being created would not be disrupted. Three predictions immediately resulted from this: 1) High Loading (values of $x \to 1$ in PdD_x) appeared to be necessary in order for the situation to involve a periodically symmetric environment that would be consistent with the associated wave-like picture; 2) The reaction would have to preserve the kind of Bose exchange symmetry (associated with the deuteron (d)+d→helium-4+gamma ray reaction) that is necessary far from the location of the reaction; and 3) In order for the helium-4 to be wave-like, the gamma ray could (and should) be repressed, but this would be possible only if the helium-4 was "effectively" created, in a wave-like fashion, at locations outside the crystal, associated with PdD_x.

Considerable criticism initially occurred about what we suggested. But because information from a second laboratory associated with the Navy suggested that we might be correct, work began. Because of the need for inter-laboratory cooperation within the Navy, even involving such a potentially controversial area of research, the research continued for many years. And evidence has been accumulating that what we initially suggested might be right. Refinements have occurred since we made our initial conjectures. Regardless of whether or not the initial assumptions are right, the conjectures have inspired new ideas. This is how science progresses.

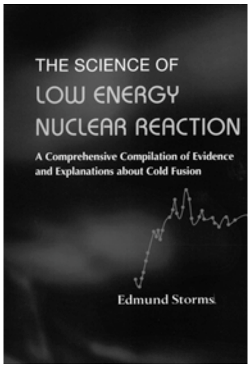
What happened seems to be the result of a remarkable collaboration that was only possible at a place like NRL, which has an environment of scientists who are open-minded and where much of the work requires scientific expertise in many different fields. I like to think that what happened in my work with Talbot reflects the important lesson my Dad taught me: the process of "falling" (or making mistakes) should be fun. When we have trust in ourselves, regardless of how many times we are "wrong," we must do what a skier does when he falls—we "get up" and we try again to be right.

The present issue of *Infinite Energy* includes three articles that have been written by scientists who have been directly involved with cold fusion efforts at NRL. These include: "Recent Progress in Condensed Matter Nuclear Science," by Talbot Chubb; "Questions and Answers About Lattice-Enabled Nuclear Reactions," by David Nagel, and "Strategies and Agenda for ICCF14," by Michael Melich and David Nagel. The first article, by Talbot Chubb, includes an interesting summary of his perspective of recent progress in Condensed Matter Nuclear Science (CMNS), which emphasizes some of the key excess heat results developed using nanometer scale size materials. It is interesting to note that Chubb was motivated to write this article after he prepared a report that covers the same material for one of the more prominent scientists from NRL, Peter Wilhelm (who is the head of the Naval Center for Space Technology). David Nagel's article summarizes some of the important questions associated with the "cold fusion"/condensed matter nuclear science field—using a new term (Lattice-Enabled Nuclear Reactions) that has the same acronym (LENR) as the name (Low Energy Nuclear Reactions) that has frequently been used to describe CMNS phenomena. The Melich/Nagel article discusses issues associated with organizing international conferences in our field.

Other articles in this issue pertaining to the "cold fusion" field are: "Dual Laser Stimulation of Excess Heat in a Fleischmann-Pons Experiment," by Dennis Letts and Peter Hagelstein; "Paradigm of Cold Fusion: A Perspective on Scientific Philosophy," by Wu-Shou Zhang, and "Cold Fusion Collaborations: Further Selections from the Cold Fusion Oral History Project," by Marianne Macy. Letts and Hagelstein present important results involving the observation of a triggering effect involving dual lasers that are tuned in such a way that particular optical phonon modes (involving 8, 15, and 20 terahertz frequencies) are excited. The associated effect may be consistent with the theoretical model that Hagelstein has suggested. Zhang presents an interesting perspective about "technical differences between cold fusion and hot fusion, and scientific distinctions between LENR and classical nuclear reactions." Macy provides material from some of the oral histories that she has conducted as part of her ongoing effort (through funding provided by the New Energy Foundation and the University of Utah) to document the work of key scientists in the LENR/CMNS field. *Infinite Energy* published a first segment of interview excerpts in #80; this new piece highlights the collaborative work of Dennis Letts, Dennis Cravens and Peter Hagelstein.

The Science of Low Energy Nuclear Reaction:
A Comprehensive Compilation of Evidence and Explanations About Cold Fusion

by Edmund Storms



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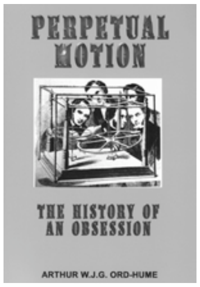

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