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"COLD FUSION"

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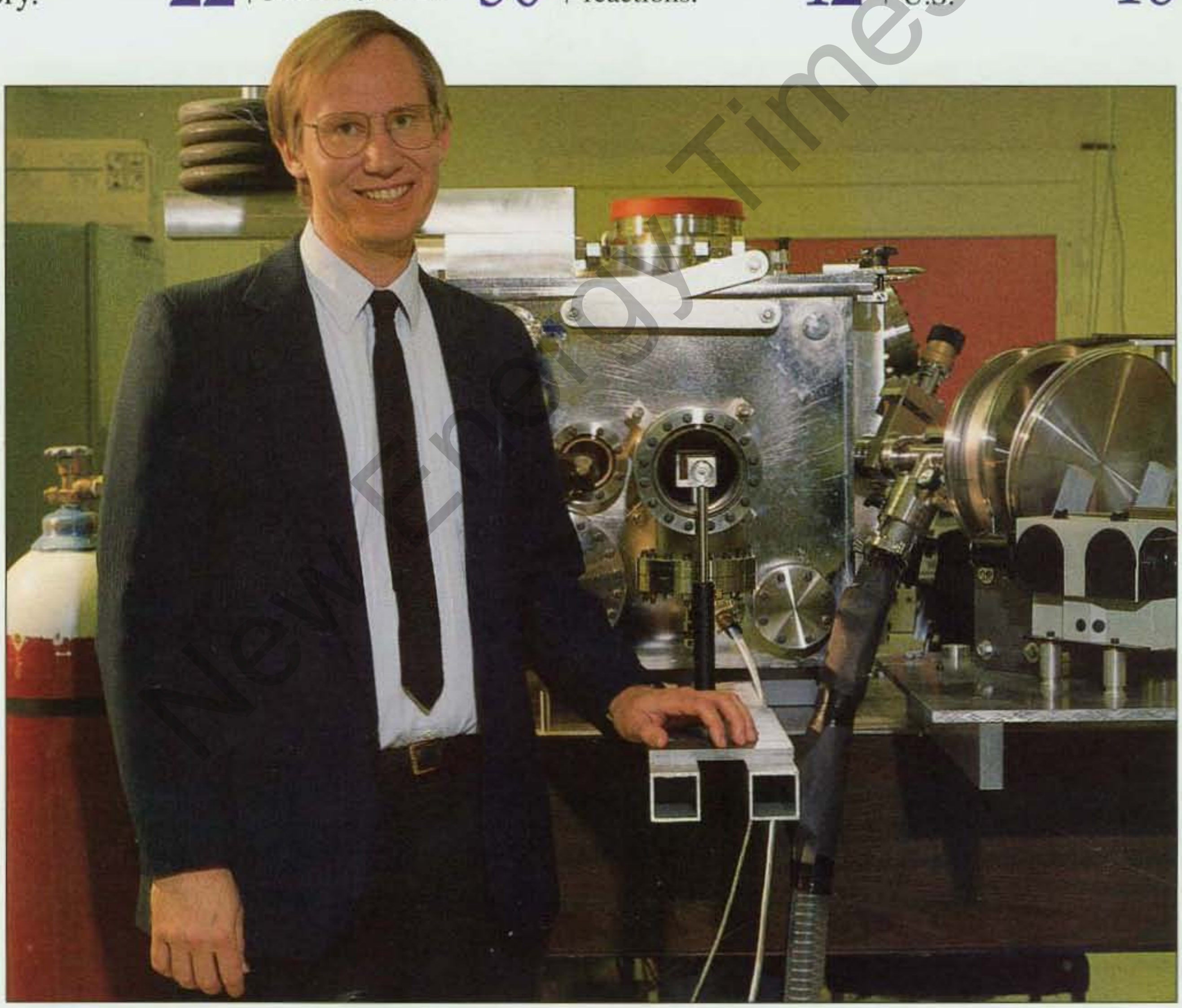

MIT's Peter Hagelstein uses laser physics and hopping neutrons to explain the "miracles" of "cold fusion." p. 22

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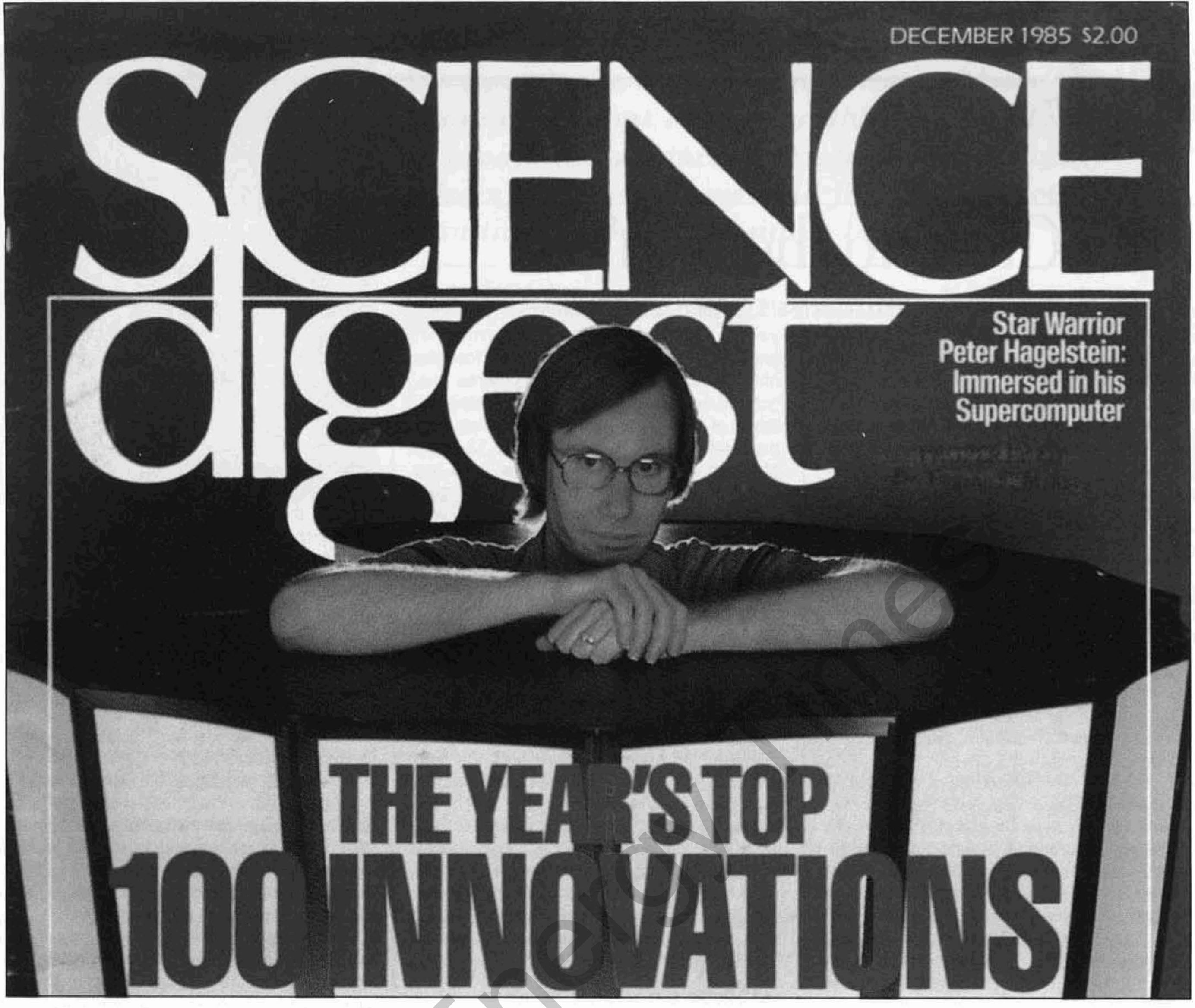


Science Digest cover, December 1985, recognizes Peter Hagelstein, who is shown peering out of a Cray XMP super-computer. In 1984, Hagelstein was named by Science Digest as one of 100 Top Young Scientists. In the late 1970's Peter Hagelstein became peripherally involved in a Livermore project to develop an X-ray laser pumped by a nuclear explosion. At MIT now, Peter Hagelstein works on civilian X-ray lasers. (Photo by Roger Ressmeyer)

A plethora of 'miracles'

'Cold fusion' experimental claims: A theorist's perspective

By Peter L. Hagelstein, Ph.D.

Professor Peter L. Hagelstein of the MIT Department of Electrical Engineering and Computer Science, and of the MIT Research Laboratory of Electronics, is well-known for his pioneering work in laser physics. Here he tries his hand at explaining the difficult work of putting "cold fusion" phenomena within a comprehensive theoretical framework related to lasing phenomena. He focuses on "neutron transfer reactions"—neutrons "hopping" from nucleus to nucleus—which he believes provides the most convincing nuclear explanation for "cold fusion."

His article provides a rare insight into the methods of a pioneering theorist working on the frontiers of physics. It may not be the easiest reading at certain points; often the quantum-mechanical concepts transcend ordinary experience, but the main ideas come across loud and clear. Professor Hagelstein promised not to embellish his text with the extensive, complex mathematics for which his presentations are sometimes known. We are happy to report that he kept his promise.

—Gene Mallove, Editor

RESEARCH LABORATORY *of* ELECTRONICS
MASSACHUSETTS INSTITUTE OF TECHNOLOGY

[Symbol of MIT RLE, where Peter Hagelstein now works.]

[Professor Hagelstein wishes to alert unwary readers that the following introductory remarks are intended as satire. As we all know, satire embodies elements of the truth. Editor's Note]

A theorist who works to develop a theory to describe various anomalies associated with the "Pons-Fleischmann effect," or any of the manifold effects reported in "cold fusion" research, is either brave or foolish—or perhaps both.

Fortunately, the experimental input that provides the starting point for the theoretical deliberations is clear. One thing that is certain, according to many of my experimentalist friends (who report this fact after careful and extensive deliberations), is that there is without question an effect. Equally certain, according to many other of my experimentalist friends, (who have also spent years painstakingly seeking to replicate the effect), is that there is without question no effect whatsoever.

Consensus in science is quite comforting, and there is always a straight and clear path towards consensus. You see, the experimental procedure of those not seeing the effect has been carefully analyzed by those seeing the effect, and it is obvious that those not seeing the effect have not paid sufficiently close attention to detail in the efforts to replicate the effect. The experimental procedure of those who see the effect has also been carefully analyzed by those who do not, and it is equally clear that those who see the effect have not paid sufficiently close attention to those experiments proving that there is no effect.

For now, we shall regard it as an established fact that there either is or is not an effect. This will be quite helpful in our theoretical discussions.

I am pleased to report that the nature of the "Pons-Fleischmann effect" has, during the past five years, been clarified. You see, there is a reproducible excess heat effect reportedly observed in many of the successful experiments. Some say that this heat can be explained easily by elementary chemical reactions, phase changes, or battery-like storage effects. I have trouble with these explanations, since the claimed heat effect in many cases is the production of more energy by at least a factor of 50 than would be produced if the cathode were replaced by a stick of dynamite and simply detonated. If the excess heat is real, then it cannot be a chemical effect. It is perhaps a nuclear effect according to some; it is perhaps an experimental error according to others.

There are, however, some successful experiments that do not show excess heat. This is an extremely important point. The presence of excess heat is a clear signature that the effect is occurring, but not a necessary one. since the effect also occurs when neutrons, tritium, alphas, gammas, and X-rays are produced. Even if none of these nor the excess heat are seen in an experiment, if the cathode is made radioactive, then the experiment has, indeed, succeeded in showing the effect.

As you can see, through a careful analysis of the experimental evidence, we are moving towards a deeper understanding of this effect. Those of you who are practiced in the theorist's art, and those who are especially clever at deductive reasoning, you have probably guessed already the physical mechanisms at work here; you may feel free to skip past the remainder of this section, as it may seem to belabor that which is obvious.

A study of correlations between the different manifestations of the effect can be quite valuable. For example, in some experiments both excess heat and neutron emission have been measured simultaneously. The results are quite gratifying. Neutrons at low levels have reportedly been observed during heat production; neutrons have also been claimed when no heat is present; heat has been claimed coincident with no measurable neutron production; many experiments have shown an absence of neutrons that correlates with an absence of excess heat. Generally, the presence of excess heat or anomalous neutrons is taken to be a very good sign by those who have carried out successful "cold fusion" experiments.

Similarly valuable correlations have been established in various experiments between other observables. For example, studies attempting to correlate heat and tritium have been performed; correlations between neutron emission and tritium production have been carefully studied. The results from these and other efforts have shed much light on matters, similar to the case of the neutron and heat studies mentioned above.

Although my discussion of the existence and nature of the effect has so far been brief, it is my hope that it has been useful in clarifying the essential difficulty of the experimental input to theoretical exploration. Of course, judgment should be used, as many of the experimental claims must necessarily be regarded as preliminary (and perhaps may be wrong). There are those who, even after five years of sustained effort on the part of more than twenty groups in the field, will argue that all of the experimental claims described above are wrong for one reason or another. Of course, a theorist who is worth his (or her) salt will have no trouble sorting out the wheat from the chaff. One thing is absolutely certain: These circumstances make life very exciting for cold fusion theorists.

[Ed. Note: The end of Professor Hagelstein's satirical remarks. Now for a serious discussion of his theory.]

Preliminary theoretical considerations

My own theoretical efforts on this challenging problem began within two days of the announcements of the discovery of "cold fusion" by the two Utah groups. I had heard the day before the Pons-Fleischmann announcement that it would be reported that two University of Utah electrochemists would claim that fusion had been observed in a test tube. I had worked previously on X-ray lasers, and I recalled a claim made many years earlier that Kepros—of that same university—had reportedly observed X-ray lasing, under conditions where it would not have been expected. Unfortunately, Kepros was mistaken.

I missed hearing the actual announcements from Utah. In the following days, I was visiting a national laboratory as a consultant, and found among my colleagues rather animated discussions about the Pons and Fleischmann claims. This was back when it was deemed OK to speculate as to what might be going on, under the necessari-

Peter Hagelstein speaks with Dr. Martin Fleischmann (R) at the First Annual Conference on Cold Fusion (Salt Lake City, March 30, 1990). Dr. Richard Petrasso (L) of the MIT Plasma Fusion Center, a noted skeptic at the First Annual Conference, joined the conversation. (Photo by Gene Mallove) ly skeptical assumption that there was anything to the claims at all. I think that that time lasted only about a week. At this late date, there are not many who will confess to having speculated about a possible mechanism for "cold fusion" during that week.

My colleagues had apparently been in communication directly with Fleischmann for several weeks prior to the announcement. A number of things seemed clear at that time: (1) Fleischmann was, prior to the announcement, apparently well-respected and so was Pons, though he was less famous; (2) The consensus was that a 10 percent heat excess was measurable, and that Fleischmann and Pons were probably capable of making such a measurement; and (3) The effect, whatever it might be, was certainly not fusion. There seemed to be nothing whatsoever to be gained in this by fraud, deceit, or lying; the group probably had the time, the wherewithal, and the expertise to do the measurement. Consequently, the only real question appeared to be: was Martin Fleischmann, in fact, sane?

From conversations with him, my colleagues seemed to think that he sounded sane; the actions of the University of Utah group during the following days and weeks seemed to be consistent with those of a stable, yet very human, Martin Fleischmann. The picture that seemed to emerge was that of some competent electrochemists who had stumbled across a new effect, one that they did not understand, but one that they were reasonably sure was real. An explanation that it might be fusion, coming from electrochemists, was not taken to be a necessary part of the package.

The experimental evidence for 'cold fusion' nuclear effects and excess heat is clear enough, but linking them unambiguously to the mechanism I have proposed is another matter.

At the time, all that seemed to be lacking was an explanation of what the effect might be, given that it was not likely to be fusion. It was decided by my colleagues that I should think about what new mechanisms were involved. My visit at that national laboratory lasted about a week. To help provide motivation, I was told that I would not be paid unless I figured it out. (I did not figure it out during that week, but I was paid.) There were many proposed approaches and theories that circulated early on. I figured that a theoretical explanation would be found by some famous theorist within a month. I was blissfully unaware at the time of the extreme cynicism about this announcement that pervaded physics departments everywhere. Almost no theory papers would be generated to accompany the large number of conjectures.

My initial considerations led nowhere. There simply was no place to start. Fusion was conjectured, yet I concluded it could not be fusion. (I know that some of my theorist colleagues disagree on this point.) There was no way to get deuterons—the nuclei of deuterium— together. Even if deuterons somehow were able to get together, large numbers of neutrons and quantities of tritium would be generated along with heat, and this was not observed.

A reasonable response would have been to cross off fusion from the list and then proceed to whatever was next. The only problem was that there did not appear to be any "next." For a theorist, the problem seemed to be the proverbial nightmare: How do you get large amounts of heat, no apparent radioactivity, and no massive radiation? There seemed to be no theoretical guidance of any kind from the early experiments. At least if the fuel or ash could be identified, then reasonable conjectures could be made about the reaction mechanism. But in this case, even the fuel and the ash had to be deduced theoretically. It was presumed by many at the time that deuterium was the fuel, and that ^4He (helium-4) was the ash; but the proof for this was lacking. The argument that it was nuclear came from the reported observation of low levels of neutrons and tritium.

It was exceedingly troubling later on when the news came that the initial neutron measurements at the University of Utah were shown to be incorrect.

Upon returning to MIT, I attempted (naively and simplistically) to determine methodically what assumptions had been made that led to the contradiction. If the effect is assumed to be real, if the reaction is fusion, if Coulomb's law holds, if quantum mechanics is right, and if Fermi's Golden Rule works [Fermi's Golden Rule is the most general way that physicists calculate reaction rates. —Ed.], then it follows that there will be no effect of the sort described by Pons and Fleischmann. At the time, there seemed to be no serious alternative to the fusion explanation, and I believed in both Coulomb repulsion and quantum mechanics.

Of the various assumptions, it seemed that Fermi's Golden Rule might be the weak link. Transitions between degenerate states can behave differently, and it seemed at the time that the consequences of this might lead somewhere. In order for the approach to work, the reactions would have to be reversible. For deuterium-deuterium (d-d) fusion, the neutron and tritium branches led to fast incoherent decay channels, and could never go in this fashion. The much weaker ^4He decay path might go this route, I thought, as long as the decay energy (24 MeV) went somehow reversibly into the phonons—high-frequency vibrations of the lattice of metal atoms. If this occurred, it seemed that a suitable account of the effect might be in hand.

As such, this was not a theory, but it did represent a direction to go.

My approach to working on really tough problems is to: (1) First take a step, one which may be part right, and may be part wrong; (2) Put it together as best to look like the answer, in as much detail as it is possible; (3) Study and calculate it until it becomes obvious what part of it, if any, is right and what part is wrong; and then (4) Propose an improved version by keeping the correct pieces and discarding what seems not to work (this new model, of course, may be part right and part wrong). This process is to be repeated until, hopefully, convergence on the right answer is reached.

At the time, I was delighted to have come up with a direction to go. I formulated and analyzed this model during the following five months; in the end I understood in detail why it could not work. This led to the generation of an improved model, one that had apparent flaws, but that at least did not suffer from the same problems that the old one did. During the past several years, I have examined a very large number of models and variants [More than 50—Ed.], always methodically using the algorithm given above to zero-in on models that are, I hope, ever closer to the right answer. The theory discussed in the present work is a more advanced theory, and I will very likely continue to analyze and assess it, then improve it.

In my previous work on X-ray lasers, I also used this approach. When I started my efforts in 1975, little was known about how to make an X-ray laser—realistically. There were a number of proposals, but it was not known which, if any, would or could work. I analyzed all existing proposals, and then used my algorithm to generate new approaches to evaluate. Ultimately, I evaluated on the order of 50 schemes and variants, a few of which proved to be successful.

In cold fusion, I was convinced that I had a direction to go in April 1989, and I put it together in a paper. This was the first step in a set of iterations, the duration of which I could not guess at the time, but which I hoped would converge soon since it seemed qualitatively to agree with the experimental results. I had no idea at the time that half a decade later that the issue would not really be resolved. I was convinced that there was something important in this first step, and from my earlier experiences in the field of X-ray lasers, I knew that it was important to document ideas lest it become unclear, later, who originated them. So I sent the paper to *Physical Review Letters* in the hope of establishing an early first submittal date, with the expectation that during the time of the review process (which I estimated to be five to six months) I would correct obvious flaws.

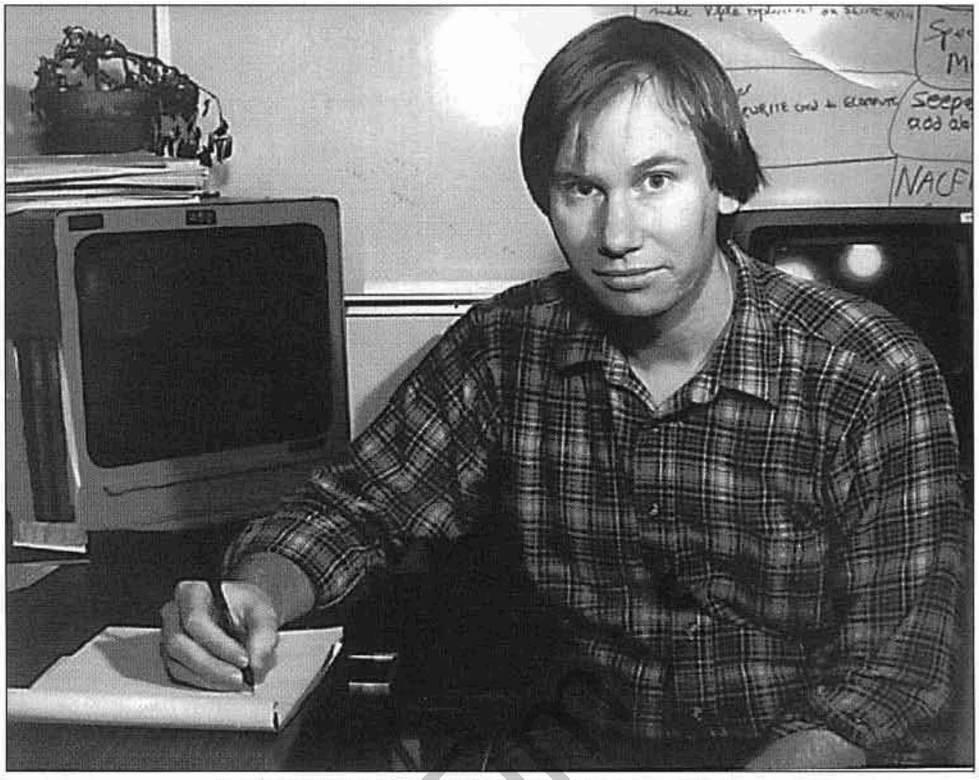
I received a great deal of feedback on my ideas from friends and colleagues, and numerous questions continued to arise, mostly having to do with the big picture of how everything might work out in this scenario, and what the implications would be. I wrote up the new ideas in "real time," and sent them also to *Physical Review Letters*—again, largely with the hope of making a record that I had generated the ideas in question.

My activities generated considerable interest, and there were many requests for me to give a seminar on my ideas. This seemed to be reasonable, and a date and time were selected. I insisted that no press be present; this was overruled by the then MIT President Paul Gray's office, which required that three people from the MIT News Office be in attendance. I had no say in the matter, other than the option to cancel. Given the amount of attention that this episode was drawing, a cancellation was uniformly advised against.

I was surprised at the response of the scientific community at MIT and elsewhere to my efforts. It was extremely negative, highly critical, bitter, and personal. Colleagues whom I had judged to be my friends, now avoided me in the hallways, as if something very heavy was about to fall on me. My students wondered how many days it would be before I would be fired from MIT, which was very relevant since I did not have tenure at the time. (I was not fired.) My papers were rejected; attempts to respond to the referee brought back a response that discouraged any further discourse.

I was strongly criticized for so many "sins" that I could not keep count. For doing science by press conference. For publishing in what came to be known as the "Wall Street Journal of Physics" (How does one manage to keep the press from quoting things that have been said or written?). For thinking that there might be something to the Pons Fleischmann experiments. For aiding and abetting ongoing "fraud." For attempting to impede efforts to completely discredit the field of "cold fusion," and, worst of all, for bringing shame to MIT.

This hurt deeply.



Professor Peter Hagelstein, newly returned to MIT from Lawrence Livermore National Laboratory in 1986. (Courtesy, MIT News Office)

Theoretical iterations

I pursued a theory for reversible fusion reactions in a lattice for nearly six months. It was clear early on that the model had severe problems, in spite of the potential advantages that had been initially attractive. As was clear initially, there were two basic difficulties: (1) how to overcome the Coulomb barrier, and (2) how to couple the energy to the lattice. I was not able to find any satisfactory solution to the first problem, and ultimately concluded that the reactions, whatever they might be, could not be fusion reactions.

The only way to get around the problem of the Coulomb barrier, assuming optimistically that any way actually exists, is to work with reactions involving a charge-neutral system. A number of prominent theorists had speculated that there might exist a heavy, negatively-charged particle that could carry a proton or deuteron, effectively producing a neutral "particle" that could enter a positively charged nucleus. I did not believe that such particles existed on earth— preferentially in heavy water/palladium electrolysis experiments. Consequently, the only serious possibility seemed to be some kind of novel exotic reactions involving neutrons that would be transferred at a distance.

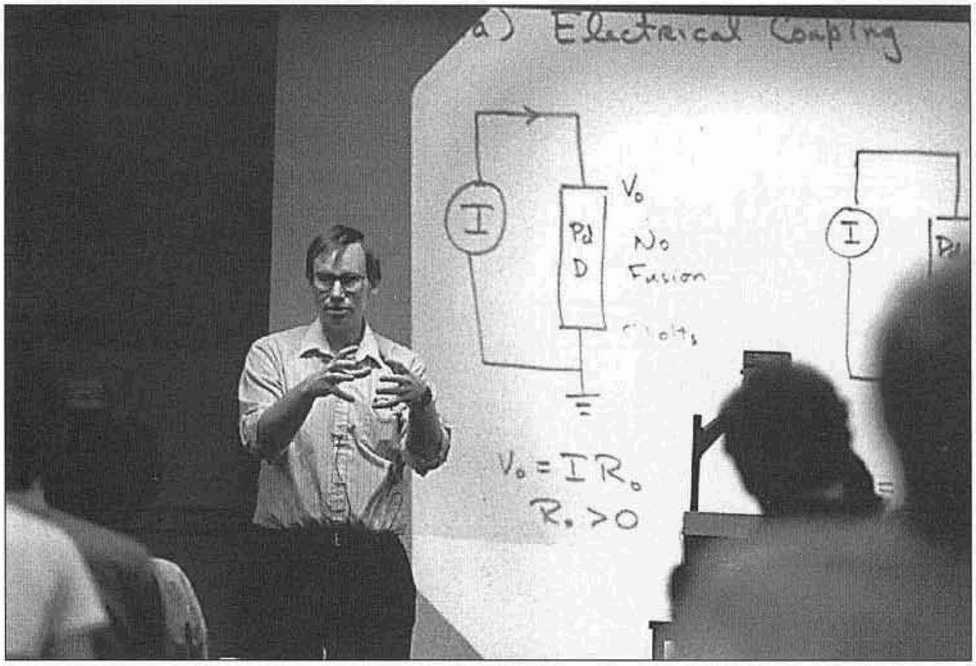
If a reaction is to involve a neutron transfer with a nucleus, it immediately becomes problematic as to where the neutron would come from. There seems to be no obvious source of real neutrons associated with the experiments; even if there were, real neutrons would lead to all kinds of nuclear emissions and activation of materials, effects not consistent with the experimental reports.

But it seemed that it might be possible to arrange for reactions with virtual neutrons, which are actually real neutrons that occupy (a small fraction of the time) states that they do not have enough energy to be in; this may or may not make much sense, but quantum mechanics is filled with paradoxes of this sort. At first, I was not particular where the virtual neutrons would come from, and I chose (in the absence of any compelling reason) to get my virtual neutrons from the weak interaction-mediated capture of electrons on hydrogen or deuterium [*An electron from the electron cloud combines with a proton in the nucleus to form a neutron. Ed.*].

The resulting model, does get around the Coulomb barrier, but at a terrible price. I worked on this model for about a year, and struggled trying to find solutions to the many severe difficulties associated with it. The weak interaction is truly weak. Virtual neutrons do not go very far. And the energy of the reaction still needed to be transferred somehow to the lattice.

Although I considered the model to represent a big step forward, since it was one of the first to be considered that offered a way around the Coulomb barrier, it was generally not recognized as such by my colleagues. As I became increasingly unhappy with the model, I spent time thinking about what could be done to fix things. At some point it occurred to me that if a neutron could be transferred onto a nucleus, that it should also be able to be transferred off of a nucleus; this could be a source of virtual neutrons. Adopting this type of model avoided the weak interaction altogether; it made the problem much simpler, in principle. Thus was born the notion of the neutron transfer reaction, at least the kind that I will discuss in this work, which dates back to December 1990.

This new model seemed to be simplicity itself. As proposed, neutrons would be transferred from nucleus to nucleus, with the excess energy going into the lattice of metal atoms—such as palladium atoms. There were now only two fundamental basic physics problems to deal with: (1) how does a virtual neutron, that normally goes only a fermi or two [1 fermi = 10^{-15} meters] away from its point of origin, find its way to another nucleus that can be Ångstroms [1 Ångstrom = 10^{-10} meter] away; and (2) how does an MeV (million electron volt) of nu-



Professor Peter Hagelstein explains his theory of "coherent fusion reactions" to an audience of MIT scientists on April 14, 1989. (MIT Photo by Donna Coveney)

clear energy find its way benignly into the lattice during a reaction. These two problems had been deemed completely impossible to solve by most physicists with whom I spoke. I was encouraged not to bother even trying to solve them.

I speculated initially that the energy transfer might occur through various recoil and lattice modification effects, noting that highly excited phonon—high-frequency vibration— modes were more likely to be able to transfer energy than a thermal lattice. The transfer of a virtual neutron over a macroscopic distance was to be accomplished by using a postulated coherence effect that would make the Bragg neutron waves be special relative to the rest, which I thought would go a long ways towards giving rise to a long-range interaction. Analysis proved both of these speculations to be incorrect.

From my point of view, the first real break in the problem came when I found a mechanism that appeared to be capable of mediating the energy transfer between nucleons and a lattice.

The basic problem is that the various nuclei in the lattice are relatively weakly coupled; pulling or pushing on a single nucleus in the lattice is not particularly effective in generating new vibrational excitation in neighboring nuclei, at least at the levels that would be required for the lattice to accept a nuclear quantum of energy. Simply stated, it seemed that adding an MeV of new energy at a single site would largely do nothing other than accelerate the nucleus at that site to an MeV, which would not be productive in light of the requirements of the theory to have no observable massive radiation. If this problem could not be solved, then the whole approach was doomed.

I analyzed, in-depth, the problem of energy and momentum transfer with a lattice during a neutron transfer reaction. Out of this analysis came the dominant recoil interaction that I knew could not do what was needed. At second order, there was a much weaker effect that came about from changes in the basic structure of the lattice during a neutron transfer. This effect was apparently well-known to the chemists, in whose field it generally was very important. This effect, known to chemists as the Duschinsky effect, was found to be highly ineffective at creating or destroying phonons. Instead, it was capable of mediating exchange energy through frequency-shifting the phonon modes themselves.

What made this interesting was that if a large number of phonons were initially present in a phonon mode, then if the phonon mode energy changes, energy transfer occurs. For example, if 10^8 phonons change their energy each by a tiny amount, perhaps 10^{-3} eV (electron volt), the net energy transfer is 10^5 eV. Now this was an effect that was capable of doing what was needed, at least in principle.

In discussions with my friends and colleagues, it seemed clear that this mechanism for energy transfer was fundamentally sound, at least in principle. Although the effect has never been demonstrated explicitly, the consensus was that such an effect should exist. In the following sections, this effect will be further discussed, and it will be found that neutron emission and some of the other anomalies could be explained by this effect.

For neutron transfer reactions to work, there remains the problem of how virtual neutrons can find their way to distant neighboring nuclei, given that they normally do not go far from the parent nucleus at all. Although the coupling of bound neutrons to continuum states is significant, what happens is that the coupling is not specific; vast numbers of continuum states are coupled, and these interfere destructively. If it weren't for this destructive interference, there would be no problem with a virtual neutron travelling over long distances.

So it seemed that the key to transporting a virtual neutron away from its point of origin resided in breaking the destructive interference. To test this, I considered the behavior of virtual neutrons in a crystal lattice in which the neutrons Bragg scatter. I found that the basic principle was sound, and that the Bragg scattering ruins the near-perfect destructive interference for those continuum states that are resonant with the crystal planes; these continuum states give rise to a long range tail that extends microns (millionths of a meter, a relatively large distance compared to the size of the nucleus) away from where the virtual neutron is born. Unfortunately, the overall effect is quite small—too small to be observed.

In principle, the approach could work. But how to make it into a stronger effect? Bragg scattering is about the strongest effect seen by a neutron in a crystal, and the only interactions that are stronger are resonant effects. For example, neutron states occur in some nuclei that are nearly resonant with the free neutron energy. When a free neutron interacts with such a nucleus, the resulting scattering or absorption cross sections that are produced can be enhanced by many orders of magnitude over the cross-sections for other nuclei. For example, boron and cadmium are strong absorbers of thermal neutrons, due to the presence of near resonant states.

But what resonant processes could possibly occur in the case of virtual neutrons? Finding resonances seemed to be hopeless, since the nuclear energy levels are not accurately known in the required energy ranges. An examination of the density of nuclear levels indicated that the spacing between the levels were sufficiently great that it would essentially be a miracle should one occur with the precision required to do the job. I generally do not believe in miracles. Even if a resonance existed, the state would have to be long-lived, and have a reasonably correct total angular momentum appropriate for the combination of the neutron and the acceptor nucleus; the odds seemed to be too long. It had to be something else. If not, then once again the whole approach was doomed.

It occurred to me that a host of perfectly resonant levels could be arranged for rather easily, if one considered other nuclei in the lattice that would produce a nucleus equivalent to one where the virtual neutron started. Consider a lattice containing two neighboring isotopes of the same element (X), perhaps ^{A}X and ^{A+1}X (where A is some atomic mass number); a virtual neutron originating from an ^{A+1}X nucleus leaves behind an

^{A}X nucleus, and will produce another identical (and resonant) ^{A+1}X if captured by any of the other ^{A}X nuclei in the lattice.

There are more than 100 pairs of stable nuclear isotopes of this type that occur in nature, or so I have found. Perhaps by chance, the materials used in the experiments seemed to involve elements that included such pairs.

It would be fitting to be able to complete this section by saying that resonant scattering of virtual neutrons succeeds in delocalizing them sufficiently (moving them far enough away from the nucleus of origin) to account for heat production. With the aid of an able young collaborator, a theory for this process has been developed. It is in many ways similar to theories used to describe electronic band mixing in semiconductors [Mathematically and physically similar.—Ed.]. This model is currently being analyzed, and although it looks very promising, we do not know for sure at this point whether the model can indeed do the job. I am optimistic. At this point, I would be surprised if the results of the calculations do not provide an excellent description of heat-producing reactions in "cold fusion" experiments.

It is, of course, up to the experimentalists to verify whether the neutron transfer model does account for the effects reported. There are numerous predictions that are made by the model, and in time, these predictions will hopefully be tested. I believe that some of these are beginning to emerge in the experimental evidence.

The first "miracle"

I remember early on an argument given as to why it was obvious that the "cold fusion" experimental claims were erroneous. It was said that it would take a "miracle" in order for fusion to occur at all in an electrochemical experiment. Miracles are exceedingly rare, according to the argument, but they do occasionally occur. Then it was said that the presence of heat with no neutrons would require a second miracle. The occurrence of one miracle was perhaps a possibility, according to this argument, but two exceedingly rare miracles could not happen simultaneously. This proved, so it was said [And is still being said.—Ed.], that there could be no such effect.

Now as I have said, I generally do not believe in miracles. But when asked to prepare a manuscript describing my explanation for the "cold fusion" anomalies, it occurred to me that at least three miracles were needed for it to work, maybe more. I suppose that according to the argument given above, I have exceeded my allowance of miracles. Then it would follow that the effects that might follow do not exist. [Unfortunately, this line of reasoning appears to stand science on its head—making experimental evidence a slave to rigid theoretical reasoning.—Ed.]

The first "miracle" is the "miracle" of the optical phonon laser. Acoustical phonon lasers—lasers that work at the usual frequencies of sound and that project sound, not light—have been demonstrated, but as far as I know, no one has ever demonstrated optical phonon lasing —much higher frequency phonons. I consider this first, since to transfer a large amount of energy from the lattice of metal atoms a very large number of phonons must be present in a single mode—a single frequency. To make this happen, a phonon laser or its equivalent is required.

To make this clear, we need to consider what it is that makes a laser be a laser, and then discuss how these ideas carry over to the phonon analog of the laser. The word "laser" brings to mind the idea of a small box out of which comes a pencil-thin red beam of light; for others, the word "laser" brings to mind high-power weapons in science fiction movies that, when aimed properly, make Imperial Star Fighters explode.

Although there are many kinds of lasers, all of these that might properly be called lasers contain an amplifier capable of amplifying light. Light that is tuned to the correct frequency at which amplification occurs will increase in strength upon passing through the amplifier. That light can be amplified was not appreciated until the 1950s. To this day, the technological implications of this effect have not been fully exploited.

Some say that this heat can be explained easily by elementary chemical reactions, phase changes, or battery-like storage effects. I have trouble with these explanations . . .

How light is amplified in a laser is quite interesting, because the effect is in some sense a quantum mechanical effect that doesn't show up in classical physics. A simple classical model for absorption holds that an atom (or molecule) responds to the force of the dynamical electric field much like a ball on a spring. The electromagnetic field in a wave or in a laser cavity oscillates sinusoidally (goes up and down) in the vicinity of an individual atom; it is usually a good approximation to take the field to be uniform on the atomic scale. The field pushes and pulls, the atom responds, the charge distribution "sloshes" up and down (or back and forth, depending on one's point of view), and light is scattered.

If the charge distribution of the atom is initially at rest, then in the classical calculation, the atom extracts energy from the field to fuel the sloshing motion. If another atom bangs into the driven (and sloshing) atom, then the sloshing is disturbed, and more energy from the field must be put in to get the sloshing going again. Any process that robs the atom of its sloshing-energy leads, on average, to a commensurate loss of energy in the field, and this is the same as saying that absorption occurs.

After this classical discussion of absorption, it must seem that the only way to arrange for energy to go back into the field, is to arrange for atoms to be perturbed—banged into—in such a way that the sloshing motion is increased, on average, from what is induced by the field. Surely this will cause the atom to add energy to the field on average, but this is not how most lasers work (it would be possible perhaps to argue that the way that so-called Raman lasers work is a little bit like this).

The quantum mechanical analysis is good fun, and leads to equations that permit a classical interpretation that is pretty much the same as that given above in the case where the atom starts in the ground (low-energy) state. The sloshing motion comes about due to the mixing of the ground state with a small amount of the excited (higher-energy) state; in the end, a video clip of the resulting quantum mechanical charge density would likely show the electron cloud of an atom sloshing very much like the classical analog discussed above. Collisions or other processes that interrupt this motion lead to absorption, which is accurately predicted by quantum mechanics.

So far, there seems to be no mystery, and it is not obvious that these arguments would ever lead to the amplification of light. The magic, as it were, comes about when the atom starts out in the excited state, and the dynamical electric field again induces sloshing through mixing with the lower state. In this case, the increase in sloshing corresponds to a lowering of the atom's energy, as the probability that the atom is in the lower state increases. Energy from the atom is now transferred to the radiation field.

Although there are an enormous number of other technical issues, this argument does get to the heart of the matter, at least from the viewpoint of the atom. An amplifier simply contains more atoms (or molecules, or ions, or electrons, or whatever) in the upper state than in the lower state (modified by some statistical factors that we shall ignore here). The electric field induces transitions; those in the lower state are mixed with the upper state—taking energy out of the radiation field, while those in the upper state are mixed with the lower state—putting energy into the radiation field. One of the tricks to making a laser amplifier then is to provide a pumping mechanism that creates many upper state atoms, and hope that few lower state atoms are generated, or that those present are rapidly destroyed.

From the point of view of the radiation field, the energy coupled from the excited

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Miracles

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atoms into the radiation field will go in-phase to add to the fields present that caused the atomic transitions. This is an electromagnetic analog of the proverb: "The rich get richer." Energy is added preferentially to resonant fields that have the most energy already. The field that stimulates the transition gets the energy.

There are strong random fields present, due to quantum mechanical vacuum fluctuations. These fields can stimulate transitions and carry off the energy from excited states (this is fluorescence), which works against making a laser beam brighter. The ideal situation for making a good laser is to arrange for the field of the laser beam to be stronger than the random fields associated with the vacuum fluctuations. In this limit, it is possible to extract the energy in the excited atoms efficiently, so that the energy is not dissipated in random directions.

These basic ideas also apply to the phonon laser. In a solid, sound waves are made up of vibrations between atoms that propagate as waves. It is possible for the sound waves to participate in atomic and molecular transitions. If a solid can be found in which there are more excited state atoms (or molecules) than lower state atoms (or molecules), and if the coupling with the phonons is strong (and if a host of other technical conditions that are of little interest here are also satisfied), then energy transfer to the sound waves becomes possible.

That sound waves can be amplified in the same sense that light can be amplified (as discussed above), has been recognized for more than 30 years. Although phonon lasers have been demonstrated, they have so far been of academic interest only, largely because no one has figured out anything particularly useful to do with them. While many groups throughout the world spend quite a bit of time and effort studying lasers and their applications, I have not yet found a group anywhere similarly devoted to the study of phonon lasers.

We know that optical phonon lasers can be made, at least in principle. However, how to make them is still poorly understood. Although phonons are certainly capable of stimulating electronic transitions in atoms, or in electron or hole bands, it is not obvious that these mechanisms can be operative in the "cold fusion" experiments produced to date. There must be some other mechanism at work in optical phonon lasing.

In searching for a "new" mechanism to drive a phonon laser in these experiments, I took the approach that an accountant might take, if the accountant were a physicist. To drive a laser, power must be supplied to the light amplifier, and much of the engineering work in designing and building a laser goes toward arranging for sufficient power to get to the amplifier. In an efficient laser, much of the power supplied from the wall plug ultimately goes to create upper state atoms or molecules. If a phonon laser existed in the "cold fusion" experiments, surely the way to find the pump mechanism (if any such actually exists) is to track where the input energy goes.

For example, in an electrochemical cell of the sort used in "cold fusion" experiments, the power supplied to the cell primarily ends up resistively heating the electrolyte, and breaking apart water to make hydrogen and oxygen. The reactions that are involved in breaking up water heat the cathode (e.g. the palladium where hydrogen gas evolves) and cool the anode (e.g. the platinum, where oxygen evolves). The resistive heating of the electrolyte is unlikely to produce a phonon laser, but exothermic (energy-releasing) gas formation at the cathode surface seems to be far more promising.

A bit of detective work revealed that the theory for a related part of the problem, specifically gas release (desorption) from a metal surface, had been developed; the basic equations describing energy transfer between the lattice and gas are closely related to the laser equations. That this is so does not appear to have been of interest to others developing the quantum mechanical models for desorption. For our purposes, it seemed that a very happy coincidence occurred; following the flow of energy seems to have uncovered a possible clue.

Let's consider how this might work. We require reactions that are stimulated by the vibrations associated with optical phonons; in the case of the formation of molecular hydrogen, the vibrational action of the phonons can help both to bring two hydrogen atoms together, and to push them away from the surface. There is really no question that the optical phonons are capable of stimulating molecular hydrogen desorption. So far so good.

We require more excited states to be present than lower states. In this case, the atomic (single atom) hydrogen at and near the surface is the excited state, and the hydrogen molecule (double atom) is the lower state. To an excellent approximation, there are no hydrogen molecules at the surface; in desorption, the molecules form several Angstroms (several atomic diameters) away from the surface, and molecules coming in break apart

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several Angstroms away from the surface. There exists a class of molecular lasers that operate using a variety of rather strange diatomic molecules that have the property that they are bound as long as they are excited in the upper state; when they decay to the unstable lower state, the molecule breaks up as the atoms push each other away. Having the molecule in the ground state break apart constitutes an excellent strategy for arranging for there to be more upper state molecules than lower state molecules. The ejection of molecular hydrogen from a metal surface is analogous to the breaking apart of the lower state of these strange diatomic molecules. In both cases the lower state is unstable.

Now, for a state to qualify as being the "excited" state, it must occur at a higher energy than the "lower" state. In PdH (palladium hydride) and PdD (palladium deuteride), this requirement can create a problem, since the relative energy of the two states change place. For example, at very high hydrogen loading, the hydrogen atoms in the metal are in states that are at a higher energy than the molecular hydrogen states. At modest loading, the situation is reversed. This is important, since if more lower state hydrogen than upper state hydrogen is present, then there will be no amplification of sound waves near the surface. It is only when the desorption is exothermic (that is, when the hydrogen in the metal are in states of higher energy than the free molecular hydrogen states), that optical phonon gain, and the associated effects, discussed below, can be present.

At some point it occurred to me that if a neutron could be transferred onto a nucleus, that it should also be able to be transferred off of a nucleus; this could be a source of virtual neutrons.

This effect is limited neither to palladium, nor even to hydrogen desorption. Hydrogen desorption in many metals is exothermic at high concentration; of course, metal hydrides in which the hydrogen lowers its energy greatly through molecular hydrogen formation will also be metals with low hydrogen solubility. The exothermic desorption of other atoms or molecules from metal surfaces should also be able to produce phonon gain—amplification, at least in principle, as long as the desorbed product is unstable on the surface. Any exothermic surface chemical reaction, in which the reaction product states are unstable at the surface, should be able to produce phonon gain.

The effect is not even limited to chemical reactions, or to surfaces. It is well-known that energy can be coupled from light into vibrational motion; monochromatic light, such as that produced by lasers, is capable of stimulating phonon production with "Raman" phonon gain present. That this was true was strongly emphasized to me recently by some of my friends after I gave a seminar at a physics institute in Moscow.

So this is the first "miracle," which after this discussion perhaps might not seem like very much of a miracle. Phonon gain can produce very large numbers of phonons in a mode, because exothermic reactions stimulated by resonant phonons present will transfer energy preferentially to increase the number of phonons present. Or more simply put, the rich get richer; phonons go to those resonant modes that already have the most phonons.

Many of the "cold fusion" experiments work in a regime where exothermic desorption occurs (as was noted a year and a half ago at the Nagoya Third International Conference on Cold Fusion by Y. Fukai). That this implies the possible presence of phonon gain is the theoretical proposal. It remains to be demonstrated that such gain is actually present. If the field of phonon lasers weren't such a backwater, the scientific community would likely applaud an experimental demonstration of optical phonon gain. As things stand at the moment, such a demonstration will most likely be ignored, since no one has figured out anything practical to do with an optical phonon laser. [So it goes for small "miracles;" they are the poor children of science.—Ed.]

The second "miracle"

Ah yes, the second "miracle." This is the "miracle" of anomalous energy transfer between phonons and everything else, including electrons, atoms, and nuclei. This is the "miracle" that can give rise to "cold fusion" neutrons—also fast electrons, alphas (helium nuclei), betas (electrons), and gammas. It can also, at least theoretically, induce transmutations (the latter are actually implied by the previous list, but we are getting ahead of the story).

There are two ways in which vibrational energy can be transferred from a metal lattice to wherever it is going to go; either lattice phonons can be destroyed (fewer phonons means less energy in the lattice), or the frequency of the phonons present can on average be lowered (frequency being related to energy by Planck's constant means that if the frequency is lowered, the energy is lowered). These routes for vibrational energy transfer have been recognized for at least 60 years.

Most physicists will be quick to point out that it is hard to create or destroy a very large number of phonons. For example, phonon energies are measured typically in units of tens of meV (milli-electron volts, thousandths of an electron volt). Nuclear energies are a billion times larger—measured typically in units of MeV—million electron volts—(there are of course exceptions, but these are not important here). A very large number of phonons must either be created or destroyed to put together enough vibrational energy to make even one nuclear energy quanta. This is very unlikely, and most physicists would bet against it. Even I would bet against it (I am sometimes optimistic. In this case I personally spent some months trying every route I could think of to find a way to make it work).

On the other hand, if there are a large number of phonons present in a vibrational mode, and if the frequency of that vibrational mode is lowered, then the energy is reduced by the product of the number of phonons and the corresponding energy shift. Whatever mechanism that causes the vibrational mode to change frequency must make up the energy difference, either by giving or taking energy from the lattice. If the energy transfer is large, the results of this energy exchange can be rather interesting. This one works, as far as I'm concerned.

But if all of this is so, then why is the energy transfer "anomalous?" Why does this rather obvious effect deserve to be described as a "miracle?" In my view, this is because even though it has been staring at all of us in the basic equations for years, no one has ever seen any significant amount of energy be transferred from vibrational modes of a solid before, in studies done to date. In molecular physics, energy transfer through vibrational mode frequency shifts is studied routinely, both theoretically and experimentally, but the energy transfer is always quite modest (this is because individual molecules are relatively small, and cannot hold anywhere near as much energy as a lattice).

The enormous number of phonons that can be put into a single mode by a phonon amplifier, as we have described above, is the key to this. I have found no studies done so far in which anyone has seriously considered the consequences of energy transfer in the presence of the huge phonon populations inevitably generated by a phonon amplifier. Anomalous? Hardly. "Miraculous?" I think not. Unfamiliar? Well, maybe.

It might reasonably be asked how it can be arranged for a vibrational mode to change its frequency. The simplest example arises in the case of impurity phonon modes. For example, a hydrogen atom embedded in a metallic lattice will generally oscillate at a higher frequency than the metal atoms. There will be three localized vibrational modes associated with the hydrogen; if the local potential were spherical, the three modes of oscillation would correspond to motion along the three axes. If a second hydrogen atom is added at an equivalent site, then three more vibrational modes will occur; in general, there will be $3N_H$ vibrational

Isotope	Neutron Binding Energy (MeV)	Isotope	Neutron Binding Energy (MeV)	Isotope	Neutron Binding Energy (MeV)
^{2}H	2.2244	^{117}Sn	6.9453	^{130}Xe	9.2555
^{3}H	6.2570	^{111}Cd	6.9752	^{118}Sn	9.3263
^{119}Sn	6.4870	^{113}Sn	7.5462	^{112}Cd	9.3980
^{113}Cd	6.5420	^{29}Si	8.4740	^{124}Te	9.4204
^{125}Te	6.5718	^{114}Cd	9.0431	^{116}Sn	9.5629
^{129}Xe	6.9081	^{120}Sn	9.1055	^{30}Si	10.6099
^{123}Te	6.9379	^{126}Te	9.1139	^{4}He	20.5817

Table I. Binding energies of neutrons of stable nuclei (and tritium) for isotopes that will interact more strongly in neutron transfer reactions [that will have configuration mixing with stable parents and s-wave continuum neutron orbitals].

modes for N_H hydrogen impurities. If N_D deuterium atoms are added to the metal lattice, then there would occur $3N_D$ vibrational modes at a lower frequency than the hydrogen vibrational modes.

If a neutron is added to a hydrogen nucleus, making it into a deuterium nucleus, then the vibrational mode structure must change to correspond to the new distribution of hydrogen and deuterium impurities. Three of the hydrogen vibrational modes at high frequency must jump down to join the deuterium vibrational modes. If the hydrogen impurity level is very low, then each of the vibrational modes will be localized around the hydrogen atom, and the three vibrational modes that jump from one vibrational band to another will also be localized; this case is not so important since little energy may be transferred, but it is easy to visualize. At higher impurity levels, the local modes "talk" to each other, and mix to form complicated vibrational modes that may extend over a very large number of atoms. Adding a neutron to a hydrogen in this case is much more interesting, because when the three vibrational modes jump to the lower vibrational band, the complicated mode structure must rearrange itself to accommodate the new vibrational mode structure. The vibrational modes in this case are no longer localized, and they may contain a very large excitation with a correspondingly large energy transfer.

The net result of this process in the case of a neutron capture, as described above, is a shift in the capture gamma line—the frequency of the emitted gamma ray—to significantly higher energy. Such an effect has never been observed, but it would be quite an interesting thing to see, because there are not very many ways to produce tunable gamma line radiation.

It is conjectured that in metal hydrides, the production of vacancies can result in an impurity mode structure. Typically, in metal hydrides, the spectrum of vibrational modes includes low energy modes that are produced predominantly by the vibration of the heavy atoms of the metal, and high energy modes that result from the vibration of the hydrogen. The low energy modes are termed "acoustical" modes, and the high energy modes are termed "optical" modes. Often, as in PdH or PdD, the two types of modes are separated by a large band gap. Near a host metal atom vacancy, the potential seen by the hydrogen atoms is softer, and the vibrational frequency of these hydrogen atoms will be reduced. It is conjectured that they may fall within the band gap between the acoustical and optical modes.

The existence of impurity bands for vacancies would do the trick. For example, any physical process that produced a new Pd vacancy in PdD would cause 24 phonon modes (three for each of the eight neighboring deuterium atoms) to drop down from the optical band to the vacancy impurity band. If these phonon modes were very highly excited, then a significant amount of energy transfer would occur, and the energy would be available for the reaction that caused the vacancy to be produced in the first place.

If, among the lattice cell containing host atom vacancies, there were variations in the number of light atoms present, then a splitting of the impurity vibrational mode structure would occur. Some cells might have eight light interstitials and some might have seven; the frequencies of the impurity bands corresponding to these cells would likely be different (cells with seven interstitials would have an even softer potential than cells with eight). The resulting vibrational band structure would allow energy transfer from the lattice to be induced by interstitial vacancy production. This is very important in explaining neutron production, as we shall see.

So what would one expect to see if all of this were actually true? It would surely be interesting.

We first consider processes that would change the number of host lattice vacancies. For example, the lattice might induce alpha-decay in the nucleus of a metal atom. Alpha decay is akin to a nucleus exploding—a part goes off in one direction, a part goes off in another direction. At the end of all of this, the nucleus that has split is no longer intact where it initially was, and a vacancy is produced. This is one possible lattice-induced reaction, and it is calculated to be a dominant process in PdD when the lattice energy transfer is on the order of 5 MeV.

In the calculations done so far on these processes, it has been convenient to consider the various decay modes of the lattice as a function of the energy transfer from the lattice. Presumably it is easier to transfer a little energy from the lattice than it is to transfer a large amount of energy. The processes that occur with the least amount of energy are recoil reactions, in which two nuclei push off of each other. In PdD, two Pd nuclei should recoil with as little as 20 eV of energy supplied by the lattice. Such a gentle recoil would be very difficult to see, unless a very sensitive experiment were set up to monitor for slow atoms coming off of the surface in vacuum. A deuteron might recoil off of a Pd nucleus; if driven by Pd vacancy production, this might occur with as little as 500 eV energy transfer. Generally, little else can occur without significantly more energy transfer from the lattice.

If the energy transfer is more than one MeV, then electron recoil is predicted. Fast electrons are easier to detect, if you know that you are looking for them. At higher energies still, processes involving the nucleus itself should occur. If enough energy is supplied to drive beta-decay reactions backwards, with extra to spare in order to cause the nucleus to recoil sufficiently to produce a vacancy, then element transmutations should occur. [Yes, modern-day "alchemy."—Ed.] The lowest energy decay of this type is electron-capture of ^{105}Pd to produce ^{105}Rh, requiring almost 2 MeV to make the reaction go as a lattice-induced process. (An electron from the electron cloud is captured by a proton in the nucleus, creating a neutron—this is electron-capture.) One nice feature of these reactions is that the new isotopes produced are radioactive with relatively short half-lives (hours to years), and are hence relatively easy to detect in low quantities. These reactions can be accompanied by "prompt" gamma emission, due to the formation of unstable excited states, and delayed gamma emission resulting from the beta decays of the unstable species. [*There exists solid evidence now that these transmutations have been observed in several cold fusion experiments.—Ed.*]

At still higher energies, the nucleus itself will begin to blow apart. As mentioned above, alpha decay requires on the order of 5 MeV, and proton decay and neutron decay cuts in near 10 MeV. Binary fission channels involving more highly charged daughter nuclei continue to open up at up to 50 MeV, where the Pd nucleus can split in half. Ternary fission and more violent higher order decays persist at even higher lattice energy transfer.

Lattice-induced reactions involving interstitial nuclei are also possible if the lattice is highly, but incompletely, loaded. For example, in PdD where a modest concentration of both Pd and D vacancies occur, the creation of D vacancies can cause vibrational modes to jump across band gaps as discussed above. The lowest energy transfers are simple recoils, as in the case of host metal nuclei considered above. However, if the interstitials are deuterons, then the recoil can lead to neutron production at low levels through d-d fusion. Within this theory, this is the route towards the so-called "cold fusion" neutrons—2.45 MeV neutrons— (assuming that they are, in fact, real).

Recoils with electrons occurs at a few KeV (thousand electron volts). The onset of the electron recoil channel is predicted to quench deuteron recoil as the lattice energy transfer increases. According to this model, elements with more deeply bound deuterons will be able to sustain a higher level of neutron production, since the neutron production rate is strongly increasing below the point where electron recoil quenches the reactions. For example, the barrier energy for deuteron-hopping in Pd is about 0.25 eV, and in Ti (titanium) is about 0.50 eV; assuming that a larger barrier energy inhibits deuteron recoil, then Ti should be capable of achieving a higher neutron production rate than Pd in the presence of roughly comparable phononic excitation.

Little else is predicted to occur until quite high lattice energy transfers. Continuum gamma production is predicted when local microscopic electric or magnetic fields Compton scatter (with assistance from the lattice) off of deuterons, at transfer energies above 60 KeV. Lattice-induced neutron ionization from deuterium nuclei (mediated by local electric or magnetic fields) is predicted at transfer energies above 2.225 MeV; this could result in quite high rates of neutron production.

The predicted lattice-assisted reactions that are discussed above seem in some cases to agree with the results claimed in "cold fusion" experiments. There has been much discussion of low-level neutron emission in "cold fusion" experiments, including experimental claims of both random emission and bursts. Claims for gamma emission, including both line radiation (single frequency radiation) and continuum radiation, have been reported. Activation of the host lattice—the production of short-lived radionuclides—has also been claimed. These phenomenon, if true, may simply be the result of energy transfer from vibrational modes highly excited by a phonon laser amplifier. More importantly, whether the claims are true or not, such phenomena would be expected from a properly prepared metal hydride sample that is suitably excited.

The third "miracle"

There is really nothing that is retrospectively surprising or "miraculous" about the first two "miracles" discussed above. If the essential physics described here were presented to physicists of the late 1930s (hypothetically transported here for a discussion), I think that they would be impressed by the optical laser (which could have arrived in the 1930s). Yet the basic physics had been developed by the end of the 1930s. With their background, they would be able to understand the physics that has been discussed.

Being conservative (as physicists have always been), they would of course not believe it to be true until they saw it demonstrated cleanly experimentally (or unless they demonstrated it themselves), so that they could be absolutely sure that it was true. In this, they would be disappointed, as physicists today are. Although the ideas are straightforward, there exists presently no experiment, in which a metal deuteride with a demonstrably suitable vibrational mode structure, with known loading, and with measured levels of high vibrational excitation, that has yielded an unambiguous signature of any of the mechanisms discussed above. I do not believe that such an experiment will be in hand any time soon, given the general level of interest and support presently available. The experimental evidence for "cold fusion" nuclear effects and excess heat is clear enough, but linking them unambiguously to the mechanism I have proposed is another matter.

Neutron transfer reactions may be an interesting route to the clean production of nuclear energy, with stable fuels and ashes, and with very low radioactive emissions. This is perhaps the most exciting prospect.

To some degree, the situation is worse in the case of the third "miracle." This is the "miracle" of the neutrons that hop from one nucleus to another. In doing so, they appear to be oblivious to the demands of Heisenberg (the Heisenberg Uncertainty Principle) that they should remain localized in the very deep potential wells of their parent nuclei. In fact, in such hopping, the neutrons are always subject to the laws of quantum mechanics—it is only our intuition that might require modification. That such an effect exists in principle is certain; that such an effect is sufficiently strong to lead to heat production at the levels claimed is unknown, although the arguments in support of this being so are strong.

Unfortunately in science, the fact that one person believes something to be true—based on the experimental evidence and on theoretical grounds—and has reasonable quantitative arguments, ultimately never settles anything. In some cases, one's colleagues will suggest a good night's sleep or perhaps some aspirin; in other cases, these colleagues will go much further and bring forth accusations of incompetence, or worse. I propose to try to settle the matter by calculating (or by convincing some colleagues to calculate) in some detail the relevant so-called nuclear matrix elements (which have thus far only been estimated); this is where I think the essential uncertainty lies. Better, of course, would be to measure hopping rates and determine the matrix elements experimentally. As a result, this "miracle" presently requires some faith that unknown interaction matrix elements indeed have a sufficiently large value, and not some value a factor of 10 smaller.

All right, so what is this "miracle" all about? If neutrons can hop from one nucleus to another equivalent nucleus, it would be at best of academic interest. If, instead, neutrons hop to a non-equivalent nucleus, and if the difference in binding energy can be transferred to the lattice through the mechanisms described above, then quite an interesting effect would result. This type of reaction would generate heat from stable nuclei, producing stable nuclei as an ash, and do so cleanly. Consequently, neutron hopping, combined with lattice energy transfer through highly-excited frequency-shifting vibrational modes, might well be an explanation for the heat effect in Pons-Fleischmann-type experiments.

Now why should this be considered to be "miraculous." Neutrons that are bound in isolated nuclei are known to be strongly localized. They may wander a few fermis (a few nuclear diameters) from the nucleus through tunneling; they may also wander a few fermis distant by coupling off-resonantly to free-neutron states. These mechanisms are altogether different, and sometimes confused, since the end result of their effects are similar for isolated nuclei.

In order to get a neutron from one nucleus to another in the lattice, it has to be arranged for the neutron to wander much further than a fermi; typical atomic distances are measured in Angstroms, instead of fermis. The probability that a bound neutron can be found so far away from a nucleus is vanishingly small in the case of an isolated nucleus. That the lattice should make any difference at all in an effect that seems to be entirely local, is what makes this effect seem to be "miraculous."

In the case of tunneling—of a positive charge penetrating the Coulomb barrier, I think that the argument is sound; there can be no significant enhancement in tunneling due to the presence of a lattice. But the second-order coupling to continuum states does not work the same way. The coupling of bound neutrons to free states is rather strong; the small excursion distance that occurs in the isolated system is due to interference between the different free neutron states. This interference effect is at the heart of quantum mechanics; in the famous "two-slit" experiment, the presence of a second path can interfere with the first path. Electrons and neutrons diffract from crystals because of this interference. The interference that occurs between the different "paths" of all of the free states is a straightforward generalization of this effect.

The mathematical description of this effect leads to an expression for the spatial extent of the neutron away from the nucleus that is written as a sum over all of the continuum states including interference effects. The result in the case of an isolated nucleus is a probability distribution that decays rapidly exponentially away from the nucleus. The end result is similar to the result in the case of tunneling, but the physics is very different.

How can the lattice change this? Each of the individual free neutron states extends out, far away from the nucleus. These waves "sample" the surrounding environment, and they will surely notice the presence of a nearby lattice. If the surrounding environment interacts with the waves strongly, the waves will no longer interfere so precisely as before; in this case, the destructive interference that caused localization in the case of an isolated nucleus, can be broken. The neutron can be delocalized, at least in principle. Neutrons generally interact only weakly with other nuclei in a lattice. Calculations of the effects of Bragg scattering on the free waves in fact demonstrated an effect, and there occurred a quite miniscule probability that the bound neutrons could be found quite far (microns) away. Unfortunately, the effect is much too weak to be observed.

A much larger effect is predicted if the Bragg scattering is resonantly enhanced. Thermal neutrons are observed to be strongly absorbed or scattered by nuclei that have a bound state that is nearby in energy. This effect is about the only process that competes successfully with Bragg scattering in a crystal. A bound neutron that mixes with free neutron states does so in a way that is peculiar; the momentum of these states is precisely what would be expected for normal free neutrons, but since the neutrons are actually still bound, the corresponding energy is many MeV less than that of a free neutron. Consequently, the free waves from the bound neutron will not be absorbed or scatter with the same cross sections of true free neutrons, because their energy is very different. If new resonances can be found with an energy that roughly matches the energy of the bound neutron, then resonant scattering can occur.

Perhaps the most interesting example of this is when other equivalent nuclei are present in the lattice. For example, if ^{29}Si mixes with ^{28}Si and a free neutron orbital, the free neutron will consider all other ^{28}Si nuclei to be potential sites for resonant scattering, since if the free neutron were to be captured by these nuclei there would be a precise energy balance over all. This resonant scattering process can be coherent, which is another way of saying that Bragg scattering with a dramatically enhanced scattering cross section is possible.

To make this work, a lattice must contain a mixture of neighboring isotopes; for example, ^{28}Si and ^{29}Si. There are symmetry requirements that predict which isotopes will show the largest effect; the angular momentum of the free neutron ultimately determines the strength of the coupling. For example, for the silicon isotopes ^{28}Si and ^{29}Si, the free neutron has no net angular momentum, which is optimum. A free neutron coupling to deuterium to make tritium is also a zero-momentum interaction. The stable palladium isotopes all couple to neutron states with two units of angular momentum; the centripetal potential associated with this much angular momentum keeps the free neutron away from the nucleus, and the resulting interaction is predicted to be quite weak.

Isotope	Neutron Binding Energy (MeV)	Isotope	Neutron Binding Energy (MeV)	Isotope	Neutron Binding Energy (MeV)
^{13}C	4.9463	^{7}Li	7.2499	^{188}Os	7.9607
^{195}Pt	6.1051	^{208}Pb	7.3682	^{172}Yb	8.0203
^{183}W	6.1918	^{184}W	7.4120	^{200}Hg	8.0287
^{201}Hg	6.2299	^{77}Se	7.4195	^{156}Gd	8.5373
^{187}Os	6.2914	^{57}Fe	7.6458	^{54}Cr	9.7194
^{157}Gd	6.3594	^{202}Hg	7.7548	^{58}Fe	10.0454
^{155}Gd	6.4349	^{61}Ni	7.8200	^{78}Se	10.5009
^{171}Yb	6.6147	^{196}Pt	7.9225	^{62}Ni	10.5978
^{199}Hg	6.6640	^{158}Gd	7.9383	^{15}N	10.8344
^{207}Pb	6.7376	^{53}Cr	7.9393	^{11}B	11.4548

Table II. Binding energies of neutrons of stable nuclei (and tritium) for isotopes that will interact less strongly in neutron transfer reactions [that will have configuration mixing with stable parents and p-wave continuum neutron orbitals].

Estimates so far suggest that when the free neutron has one unit of angular momentum, that the centrifugal effects are strong enough to preclude sufficiently large coupling to give reaction rates fast enough for heat production. However, experimentally it is claimed that many Ni (nickel) light water experiments appear to give heat at quite modest current densities (and hence possibly quite low lattice energy transfer). It is tempting to conjecture that neutron transfer reactions involving nickel occur, specifically that a near resonance (12 KeV) occurs for ^{62}Ni as donor and ^{29}Si as acceptor. For this to be true, since the nickel isotope couples to a free neutron with one unit of angular momentum, the interaction matrix element must be more than 20 times larger than the crude estimates made so far. For this reason among others, it will be quite interesting to see what the results of detailed calculations show.

The answer to the question 'where is this all going?' may well be, for the United States at least, 'nowhere.'.....

If the only nuclei participating in these reactions are those coupling to zero-angular nuclei, then the list is possible. Silicon would be the most promising, which is convenient, since there is quite a bit of silicon. If nuclei that couple to neutrons with one unit of angular momentum can participate, then the list is bigger (see Table II). In this case, at least one of the reactants (neutron donor or acceptor) must be from Table I, in order for the transition rate not to be quite small.

The theory for these processes at this point has other requirements. For example, in order for the resonant Bragg scattering to be effective, the free neutrons must undergo a change in either linear or angular momentum during a hop. In the case of zero-momentum free neutrons, phonon exchange can change the linear momentum of the neutron. Thermal phonons are predicted to be much less effective than a very strong narrow-band phonon field in producing a strong scattering effect. In the case of free neutrons with one unit of angular momentum, the translation symmetry of the lattice is capable of changing the angular momentum, so no additional stimulation is required.

So what are the implications of a neutron hopping effect? The simplest example of this effect is in the case of neutron hopping between equivalent nuclei, which would look like an enhancement of the self-diffusion process. For example, if a silicon crystal were specially constructed with alternating layers of ^{28}Si and ^{29}Si, then the effects of self-diffusion (under a narrow band phonon drive) could be monitored by neutron Bragg scattering. Neutron diffraction peaks corresponding to the imposed order would be present initially and would disappear in time due to the self-diffusion. This effect should be observable on a day to week time scale. A similar crystal with alternating layers of ^{28}Si and ^{30}Si would show no such effect.

If neutron hopping between equivalent nuclei that couple to a neutron with one unit of angular momentum occurs, the resulting experiment would be even simpler. In this case, a crystal with alternating isotopic layers would spontaneously show self-diffusion with no external stimulation. Even if the matrix elements are small, as in the crude estimates, this effect should still occur, but it would occur at a much slower rate and therefore be harder to see.

Neutron transfer reactions may be an interesting route to the clean production of nuclear energy, with stable fuels and ashes, and with very low radioactive emissions. This is perhaps the most exciting prospect. This would be the case if neutron hopping between non-equivalent nuclei occurred with energy transfer to the lattice through mechanisms described above. If some of the "cold fusion" heat experiments are right and there in fact is excess heat, then this reaction mechanism would be a strong candidate to explain what is going on.

Tritium production is also claimed in "cold fusion" experiments. Neutron hopping to deuterium would be much like the heat producing reactions, except that most reactions leading to tritium production are endothermic (energy-consuming), instead of exothermic, as required for heat production. Since there is considerable evidence for "cold" tritium production in many "cold fusion" experiments, this is very positive for this mechanism.

Where is this all going?

This has outlined my approach to the theoretical problem that I think is behind the "cold fusion" effects that have been reported by many experimenters during the past several years. To date, there has been little interest on the part of the wider scientific community, and on the part of funding agencies in many countries, with the exception of Japan. I suspect that this situation will continue unabated for some time. The answer to the question "where is this all going?" may well be, for the United States at least, "nowhere."

Why should this be so? The scientific establishment is by necessity very conservative. This is also true of funding agencies; no one wants precious research dollars to be wasted on something that will not produce results. Those working in the area of "cold fusion" are in some sense scientific "pioneers," who are willing to work in an area where there are possible questions about whether their efforts will lead to solid results. This is not conservative science, at least as viewed by the much more conservative scientific community.

Those working in the field have grown used to years of incessant criticism from their colleagues and from the press. The results of many years of effort on the part of those in the field have led to numerous experiments that have appeared to work again and again. Conservative programs, such as the IMRA programs in France and Japan, as well as at SRI International, have focused on what seems to be the most solid and most reproducible effects in the field (the heavy water excess heat production). Many others have pursued the newer light water heat experiments. Still others, such as the Claytor group at Los Alamos National Laboratory, have achieved reproducible production of cold tritium.

But the scientific community in general has not kept up with developments in the field. They do not believe that there can be an effect, and they have no interest or patience to find out what has been done and what the current ideas are. Many in the field have a dream in which at some point enough experiments to sufficient precision will have been done that will make the scientific community sit up and take notice. I think that this is wildly naive, based on past experience. No amount of experimental results alone is likely to have any significant impact, either on physics communities or on funding agencies. Sad, but true.

Neither will an apparently workable theory have a significant impact. There are many theories that have been developed in the field, and I think it is safe to say that there is no consensus at all in the field as to what is the correct theory for the effect.

What I think will make a difference to the scientific community and to funding agencies is a conservative demonstration of understanding, both experimentally and theoretically. Let me illustrate this idea under an assumption that the theoretical ideas described here are largely correct.

A program could be established that would focus on the different pieces of the puzzle, one at a time. For example, a program could be established on optical phonon lasers, with the goal of demonstrating a clear theoretical and experimental understanding of the effect and associated physics. A three to five year program of about 10 research groups would settle this in a conservative manner, so that at the end there would be no question in anyone's mind as to whether optical phonon lasers can be made and what their parameters are.

What I think will make a difference to the scientific community and to funding agencies is a conservative demonstration of understanding, both experimentally and theoretically.

Similarly, a program could be established to study vacancy phonon mode structure, again both theoretically and experimentally. Such a program could examine the generation and characterization of defect impurity bands in metal hydrides of all sorts, and very conservatively answer questions about the occurrence and spectroscopy of these bands.

The two efforts could be combined, which according to the ideas presented here, would lead to reproducible experiments demonstrating large energy coupling between lattices and nuclei. Neutron production, fast electron production, alpha production and host lattice activation could be studied; this would be a very exciting program.

A conservative and sustained effort to understand neutron hopping, both theoretically and computationally, could also be carried out. This would also be a rather exciting program. Much of the condensed matter community has relevant expertise, and the results of such studies have the potential to open up a new field.

The combining of all of these efforts could then lead to assaults on the heat production problem. The difference is that this time, the assault would start from a position of deep understanding. The goal of such an effort would be to explore novel heat-production technologies; to optimize heat producing systems for various applications; and to work with industry to bring these technologies to the market place.

These are wonderful dreams and fantasies. They could even come true. But we must be a bit more serious, and face up to the fact that we will continue to be the recipients of severe criticism from colleagues and from the press, and that there will be little support for this work. Let us hope that the Japanese will be able to sustain their efforts, and be successful; and let us be content with contributing to science in the hopes that these ventures will be one day successful and appreciated. CF

Biography:

When Peter Hagelstein returned to MIT in 1986 as an Associate Professor of Electrical Engineering and Computer Science at age 32, he was already regarded as one of the nation's brightest young scientists. He had just left the Lawrence Livermore National Laboratory in California to return to his alma mater and the very department where he had received the SB and SM in electrical engineering (1976) and his PhD in 1981. In addition to his MIT teaching, Professor Hagelstein's work focuses on extreme ultraviolet and "soft" X-ray lasers, as well as on the physics of "cold fusion." He has been prominently involved with cold fusion research since 1989. Among the subjects he has taught at MIT are: numerical simulation; quantum mechanics; and electrodynamics. Under his supervision a table-top X-ray laser facility has been developed at MIT. Prior to his return to MIT, he held these positions at Lawrence Livermore National Laboratory: Physicist, Special Studies Group, Physics Department (1981-86); Principal Project Scientist, X-ray Laser Program (1981-1986); and Group Leader for the NLTE Computational Physics Group (1982-1985). In 1984, he was awarded the E.O. Lawrence award for National Security. He has been a consultant to AT&T Bell Laboratory and to other corporations. Presently he is on the scientific advisory board of ENECO. He was also a JASON for several years.

Some of Professor Hagelstein's Publications on Cold Fusion:

"Coherent and Semi-Coherent Neutron Transfer Reactions I: The Interaction Hamiltonian," *Fusion Technology* Vol.22, 172-180, (1992).

"Coherent and Semi-Coherent Neutron Transfer Reactions II: Transition Operators," submitted to *Fusion Technology* (1992).

"Coherent and Semi-Coherent Neutron Transfer Reactions III: Phonon Generation," *Fusion Technology* (May 1993).

"Coherent and Semi-Coherent Neutron Transfer Reactions IV: Two-Step Reactions and Virtual Neutrons," submitted to *Fusion Technology* (1992).

"Coherent and Semi-Coherent Neutron Transfer Reactions," in *Frontiers of Cold Fusion*, Proceedings of the Third International Conference on Cold Fusion (Nagoya, Japan), H. Ikegami, Editor, Universal Academy Press, Inc., Tokyo, 1993, pp.297-306.

"Lattice Induced Atomic and Nuclear Reactions." *Proceedings of the Fourth International Conference on Cold Fusion*, EPRI (1994)

"Neutron Transfer Reactions, P.L. Hagelstein and S. Kaushik, *Proceedings of the Fourth International Conference on Cold Fusion*, EPRI (1994).