RESPONSE TO REVIEWER COMMENTS

Edmund Storms

I want to thank the reviewers (*IE* #108) for taking the time to make interesting and sometimes useful comments on my paper, "Cold Fusion from a Chemist's Point of View." This is the first and hopefully not the last time a proposed explanation of LENR has been reviewed publicly in such detail. The process is effective in revealing not only flaws but also how the ideas can be better explained to avoid misunderstanding. I will comment on each review in the order they appeared in *IE* #108.

David Nagel
Nagel understands my basic proposal with a few exceptions. His requirement that a theory be tested by quantitative calculations is normally correct but this kind of test cannot be used here. LENR occurs as individual nuclear events that are invisible as a single event. Only the average heat effect or radiation flux can be measured. The average is determined by variables over which no control exists. For example, the number of active sites present in a sample will determine the amount of generated power. We have no way to measure the number of active sites. Therefore we have no way to predict from theory the total power that might be expected. In addition, each active site will have a different access to the reactant. Consequently, the rate at which fusion takes place will be different at each site, which again cannot be determined from theory. The *only* test of such a model is verified predictions of general behavior.

As for the comment about using the word "chemist" in the title, chemists and physicists do look at the world in different ways and do arrive at different kinds of explanations. We might not like this condition, but it is a fact of life I find necessary to acknowledge because the other theories are mostly created by people trained in physics. Consequently, the approach they use is much different from the one I'm using. This difference is important because physics tends to focus on the cause while my focus is on the result. The latter focus is more effective in understanding LENR than is the former because only the overall effect can be studied, not the cause.

Xing Li
Li took this opportunity to evaluate the early critique by skeptics of the claims made by Fleischmann and Pons, and to supply an explanation of his own. He examines the claimed Ni-p transmutation reaction, which my theory rejects as a significant source of energy, and places this reaction in the lattice, which my theory predicts is not the location of any LENR process. By paying no attention to what I wrote and suggesting processes that are in direct conflict with what I propose, I assume Li does not find my ideas worthy of comment. Consequently, no response is required.

Jones Beene
Yes, many explanations of LENR have been proposed, but this fact does not invalidate my use of Ockham's razor. The challenge is to evaluate these other ideas and determine how many are correct. If my explanation remains as the only nearly correct explanation, my use of Ockham's razor would have proven its value. In any case, all explanations naturally seek to find the least complex path because otherwise the ideas become too complex and numerous to evaluate or test. The important choice is which assumption is retained and which is eliminated. Beene does not address this choice.

In contrast to the statement made by Beene, my theory has absolutely no relationship to fractofusion and it applies to all methods known to cause LENR, not just the method used by Fleischmann and Pons.

Yes, many features used in my theory also have been used in previous theories. Nevertheless, my approach is a unique combination of features that is able to explain behaviors other combinations failed to explain. Using the example provided by Beene, the claims for nuclear heat resulting from using normal hydrogen are not novel, as Beene suggests. Fleischmann and Pons noted extra heat from this source, as did many other researchers. This claim did not result from errors in calorimetry because calibration was frequently based on use of Pt as the cathode in D_2O or on using a resistor, not Pd as the cathode in PdH. But this result has no relationship to my theory other than I can predict this behavior and can explain why it occurs.

Vladimir Vysotskii
Vysotskii understands what I propose, except for one confusion. I do not believe that the D_2 molecule plays any role in the process, neither deformed as he describes or not. The hydroton molecule that I propose to form is based on the "p" electron orbit, not the "s" orbit that forms normal D_2. This difference is important and is not simply a distortion of normal D_2. In addition, the dimension provided here is the equilibrium distance between the nuclei in D_2. If the hydroton is to function as I propose, this distance must gradually decrease as energy is lost. Consequently, the initial distance between hydrons is not important.

In the process of describing what I propose, Vysotskii reveals a basic conflict with how I understand Nature to behave by his description of hot fusion and the law of thermodynamics. These differences are too basic to address here in detail. Nevertheless, I need to emphasize that energy does not and cannot spontaneously accumulate in local regions in a material. If this accumulation were possible, no explosive would be stable. Spontaneous accumulation of energy is clearly limited to magnitudes that cannot affect chemical processes, which are sensitive to much lower levels than are nuclear interactions. Consequently, LENR cannot be initiated by a spontaneous accumulation of energy but must rely on a basic change taking place in the material. Yes, enough energy can be *applied* to a material to initiate fusion, but that

is not the issue I was discussing by invoking the laws of thermodynamics. In addition, when this is done, hot fusion products are formed—not cold fusion.

The LENR phenomenon presents a dilemma for any explanation. Two D must eventually combine to form He—an event that requires mass-energy to be converted to heat-energy. This process must occur in a unique condition in a material. Yet, all proposed conditions fail to support such a process without violating some law or expectation, including the one I suggest and the one proposed here by Vysotskii. The solution to this problem generally degrades to applying mathematical equations based on concepts that are so complex to defy understanding or to simply ignore the problem. An approach needs to be found to encourage agreement about the basic requirements a theory must have, because at the present time many theories are in conflict with fundamental and basic concepts about how Nature is known to behave.

Jean-Paul Biberian

Biberian summarizes my claims well, but his understanding of how I claim the hydrogen nuclei can float in the gap is not complete. Also, the conclusion that my claim cannot be easily proven is not correct. The unique feature about the gap is that it allows the H+ to be equally attached to both surfaces. Of course, if the gap is too large, the nuclei will favor one surface over the other, as Biberian imagines might be the case. Achieving this critical distance creates great difficulty in causing the effect and in maintaining nuclear activity in a material.

As for a test of my model, I suggest three. A search for deuterium production can be made using the Ni-H_2 system, a search for the effect of the D/H atom ratio on tritium production rate can be undertaken, and cracks can be made by nano-machining followed by examination for nuclear activity. These studies would test several predictions. Failure of any prediction would immediately invalidate my model.

Andrew Meulenberg

Meulenberg fails to understand much of what I wrote in spite of many private discussions. I do not object to my idea being rejected, but this must at least be based on a correct understanding of what I propose. I will attempt to address the major misunderstandings.

1) The two-in-two-out rule is simply a restatement of the conservation of momentum law when applied to a nuclear reaction. When two atoms come together to make a single nucleus, the energy cannot be released without another particle being emitted. In the case of D+D to make He, the second particle is either a gamma ray or the helium nucleus splits into fragments consisting of two particles. In general, energy can only be communicated from a nucleus to the surrounding material by emission of something, which results in two particles being produced; thus the two-in-two-out rule.

2) Ignore triggering of the reaction because according to my model, once the gap is formed and filled with the required resonating structure (the hydroton), mass-energy conversion starts spontaneously and continues until all hydrons in the gap have been converted to the expected nuclear product. Once the nuclear product leaves the gap and the reactants are again assembled, the process repeats. The only limit is how fast the hydrons can enter the gap and form the hydroton. This rate is determined in part by applied energy, but it is not triggered by applied energy.

3) My model has absolutely no relationship to the process Schwinger proposed, although I admire his willingness to suggest an idea that I'm sure he would have modified later. Andrew does note the conflict but nevertheless says, "This resonance model [mine] is essentially that proposed by Schwinger," which is not the case. I would not mind standing on Schwinger shoulders as Meulenberg suggests, but my model is not even close to what Schwinger described.

4) My description of H^4 formation and subsequent decay results from the need to explain tritium formation by LENR. To form tritium from H and D, the plausible reactants, an electron has to be added to the final nuclear product. I assume this addition occurs in all cases of LENR, regardless of which hydrogen isotope is used. This means D+D+e gives H^4, which has to decay by beta emission in the same manner as H^3 decays. If Meulenberg wants to reject this idea, he should note that H^4 is thought to decay by neutron emission, not by beta emission.

5) I make no effort to prove anything in the paper. Such proof is, in fact, impossible. I only explain how the model is created, what it explains, and what it predicts. Proof comes if the predictions are confirmed and the explanations provide a better guide for experiment.

Ed Pell

Pell makes his disagreements clear, but again his conclusions are not based on what I wrote. I do not address the Ni-p reaction except to note in several papers the conflict this claim has with what is observed and what is required to initiate such a reaction. This reaction makes only one product, which does not permit the energy to be released. If this reaction is to be believed, this problem must be addressed. I do not attempt to explain transmutation in this paper even though my model can explain this process as a very minor part of the main fusion reaction.

Pell objects because I have not explained every aspect of the resonance process. This kind of detail requires a different paper and many more pages available to provide such information. This detail is gradually being provided.

I have never claimed that the process evades quantum mechanics. In fact, several people have been encouraged to apply quantum mechanics. I await their success.