

Ed Storms Further Explains

The Explanation of Low Energy Nuclear Reaction

Marianne Macy

Dr. Edmund Storms, who will be presented with the Minoru Toyoda Gold Medal at ICCF24 in Mountain View, California (July 25-28, 2022), is one of the most prolific experimenters, theorists and writers in the history of cold fusion. Storms has a Ph.D. in radiochemistry, and spent 30 years at Los Alamos National Laboratory (LANL) before his life changed to a path less traveled in March 1989.

“If the claims being made by Fleischmann and Pons were correct, my past efforts would become completely obsolete,” Storms wrote in his 2007 book *The Science of Low Energy Nuclear Reaction: A Comprehensive Compilation of Evidence and Explanations About Cold Fusion*. This book starts out with both a dramatic and scientifically thorough account of what happened at LANL upon the Fleischmann and Pons announcement, the immediate start of work there on day one and then a month later, an April 1989 visit and lecture by Stanley Pons, which “gave an impression that the cold fusion effect would be easy to reproduce.” A ten month later visit by Martin Fleischmann followed, but the possibility of F&P working at LANL, one of Fleischmann’s hopes, was soon dashed. Fortunately, Storms was there, to document the history of what happened next, at LANL and after he left in 1993, then in the international community that carried on the work to present day.

The one thing about trying to document or describe what Ed Storms has contributed to this science is that he is an unmitigated, remarkable workhorse who has already done it in every possible form—scientific publications and presentations, Senate testimony, working with early organizations such as ENECO (only to find their patent applications rejected after spending over a million dollars). Storms’ account of these early years are damning and thorough because he was there and witnessed it all, and always concluded the only thing to do was to continue working, which he has done for over 30 years after his “retirement” from LANL.

An excellent 2007 interview by John Allen Rudesill in *Infinite Energy* #75 details more of Storms’ background.¹ See also my 2013 *IE* #111 article, “Ed Storms Honored at ICCF18” for more background.²

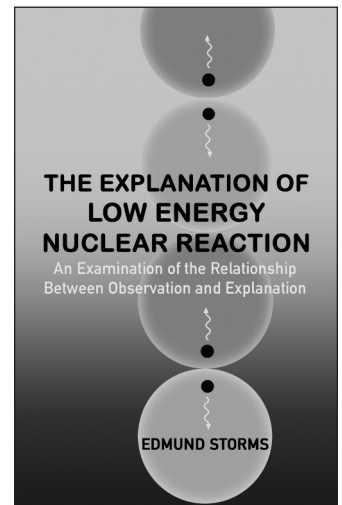
Storms is a force in the LENR community, a generous, communicative, endlessly productive work and information producing giant who at age 91 is writing, doing experimental work and collaborating with NASA on current applications of LENR for space propulsion.

I’ve heard it said and it is commonly agreed that Ed Storms is the only person in the field who has read every paper by every person working in cold fusion, who can also quote liberally from them. Storms’ first book *The Science of Low Energy Nuclear Reaction* details the start and continuance of his work, as well as presenting in depth chapters on his

and others’ theoretical and experimental work.

His second book in 2014, *The Explanation of Low Energy Nuclear Reaction: An Examination of the Relationship Between Observation and Explanation*, carries on with progress in his own work and his parsing and explanation of that of many other researchers in the field. See an interview about the book release in *IE* #116.³ [The book has just come back into print; see the ad on the last page of this article.]

The interview that follows ensued as a result of discussions on the CMNS Forum and other online back and forth. Ed Storms, Michael McKubre, Larry Forsley and I realized that to have a concentrated, in one place discussion on particular points relating to Ed Storms’ theoretical work might help expand and clarify some questions. What follows is a further explanation of Ed Storms’ *The Explanation of Low Energy Nuclear Reaction*.



Marianne Macy: I want to thank everyone on this distinguished panel for being here. Ed Storms is going to be receiving the Toyoda Medal at ICCF24. Have I got that right?

Edmund Storms: Yes, provided they actually have a banquet or some means by which they can give it to me without my catching COVID.

Macy: They are clever folks, so I’m sure they’ll figure something out along those lines...I am here with Ed Storms, Michael McKubre and Lawrence Forsley. Ed is one of our most prolific writers and experimenters. He’s been there, as Michael has, since the very beginning of the field. And Larry is the Deputy Principal Investigator for the NASA Lattice Confinement Fusion Project.

Lawrence Forsley: That project grew out of the earlier NASA Advanced Energy Conversion Project.

Macy: Ed, can you describe at this point what you are working on? Even today you have an experiment going.

Storms: I’ve had experiments going pretty much off and on

now for the last several months while looking at various kinds of materials. But the materials I'm looking at right now are designed based on my model. I'm trying to see whether or not I can actually implement the concepts that my model identifies as critical variables. And it turns out my model is working, so I'm very happy.

Macy: We had passed a number of questions back and forth regarding your further explanations of low energy nuclear reactions. So we're trying to drill down on your extensive writing and your book and focus on some particular areas. So I'm going to re-ask you some of the questions that we passed in writing. Do you mind doing that?

Storms: Yes, anything you want. I will give you some answers.

Macy: And what we can do then is both Mike and Larry and I can ask follow up questions. So the first one is: Your nuclear active environment (NAE) is something that you write about extensively. Could you give us a little introduction to what that is and how it fits into the whole paradigm and then we have a couple of specific questions about that?

Michael McKubre: Can I just step in before you answer? I'd like to go right to the beginning. I've heard your explanation so many times and either I don't understand it or I don't agree with it. So there's a disconnect somewhere between what you're saying and what I'm hearing. There's a bit missing. I'd like to start at the beginning. Why cracks? Be as simple as you would like to be. Don't be shy about being simple. If this can be described in a simple way that can be sent out to the world, I think it will do us all a huge amount of good.

Storms: I welcome your question. That's exactly the approach I want to take. Lots of people have examined the behavior of this particular phenomenon. As a result, a huge literature is available. We now have a large data set showing how this particular phenomenon behaves. One of the characteristics is that LENR occurs in a material. It doesn't occur in plasma, it doesn't occur in air. We know this as a fact. The second fact we know is that it doesn't...

McKubre: It behaves in condensed matter is the fact. I don't know that you can rule the liquids part as the site of this reaction.

Storms: Whether or not it can happen in a liquid is debatable, because there's very little evidence for that. For example, liquid Li has been bombarded by D ions, but the nuclear products were typical of hot fusion, not cold fusion.

McKubre: I do agree it's not a phenomenon of free space.

Storms: OK, now we have to create a theory that identifies the real reality of where it occurs. We know that it doesn't occur uniformly within the material. We know that it's only in certain spots. We know it's only in certain batches of material. If you have a certain batch of Pd, you can study it all you want and nothing will happen. If you take a different batch and study it, you might discover that most members of that batch work. So there's something about the material itself, as a result of its manufacture, that is distributed

throughout all of the pieces regardless of their physical form. If the Pd metal doesn't have this special condition, it doesn't matter what you do, it simply will not become active. So, something unique about the material itself is important.

McKubre: I'm not sure about that statement. I'm not sure that you can't make an inactive material active, but I would agree that those of us who have tried have succeeded very poorly at that, right? So turning inactive material into an active material I claim might be possible, but neither you nor I figured out how to do it.

Storms: I agree. But the big question is: What is in a material that has the characteristics consistent with these behaviors—what can make the entire batch active? A number of people have suggested several possibilities. One requirement, I might point out, is that a location exists into which a number of deuterons or light hydrogen atoms could accumulate. Fusion cannot happen unless an accumulation of atoms can occupy the same space at the same time. Fusion does not happen when atoms are separated from each other. They have to be close enough together so that they can interact in a nuclear way—not in a chemical way.

McKubre: The wave functions mostly overlap. I don't know if they have to be exactly sitting on top of each other, but there must be a common state wave function for these collected hydrogen or deuterium atoms.

Storms: The nuclear theory identifies how close nuclei have to be in order to interact. When you say not on top of each other, that's a rather broad statement. Precise calculations have been done to show that when nuclei are too far apart, they cannot possibly interact from a nuclear point of view.

McKubre: In a pairwise interaction there are only two things that interact. And then the model of tunneling over a barrier and bashing into each other is a decent analysis, but I don't know if that's true of a million.

Macy: Can I interject something? I was going to say, Ed, you talk about the nuclear active environment. Could you make a brief explanation about that? And then can we ask you certain questions? Like the physical structure of it? But first, would you describe it and then can we talk about it?

Storms: The nuclear active environment is an environment that causes or allows atoms to come together in such a way as to allow nuclear interaction. How the atoms go about doing this requires more detail. The environment allows the atoms to come together with a relationship to one another that would allow fusion to take place by some mysterious mechanism. The kind of environment that would allow atoms to come together is determined by the chemical nature of the material, because this happens in a chemical structure. Therefore, the rules that apply to a chemical structure must apply. These atoms don't come together with the anticipation of fusing. They're in a chemical environment doing whatever the chemical environment requires them to do. Chemists study and understand the nature of such an environment and the rules that apply. I'm trying to apply those rules to what's happening.

McKubre: But by rules you're talking about equilibrium thermodynamics.

Storms: Yes. Also the Phase Rule applies, the Laws of Thermodynamics apply, as well as bond energy interactions and electron transfer processes. The chemical structure involves a very complex theory and understanding that has been developed over several hundred years. People understand how atoms and electrons interact within a chemical structure. They know that a fusion reaction cannot take place in such a chemical structure. The big mystery is how the observed nuclear reaction can happen. We need to solve the mystery of how the atoms are able to move and assemble. That's the first step I've tried to take—identify what it takes to put them into a position such that they can interact in a nuclear way.

Macy: As you write about the NAE, what would you say is the physical structure of it? You've written before, I think, that it has three dimensions with the spherical shape involving a large collection of electrons and hydrogen nuclei. So how would you describe that physical structure?

Storms: I believe the NAE is a gap. It's a physical gap. It's a place where the atoms that were normally interacting in a chemical crystal structure have moved by some amount that causes them to be no longer acting as if they're in a crystal structure. When these gaps are large, they're called cracks. The gaps I'm talking about are really quite small, so they act like pre-cracks. They are where atoms are absent but with walls made of atoms having a crystal arrangement. Hydrogen atoms can accumulate in these gaps by virtue of chemical interaction. This is pure chemistry at this point. There's nothing mysterious about it. Chemical interaction allows the hydrogen to go into this gap. If the gap is too big, the hydrogen atoms would form a molecule, which cannot fuse. But if the hydrogen goes in the gap as individual atoms, the individual atoms can interact with each other in a different way. At some point during this interaction, a sufficient number of electrons become concentrated in a particular spot to cause the Coulomb barrier to be overcome, with the result that a nuclear reaction can happen. This process forms the spherical shape you mentioned.

McKubre: Hang on. I may have misheard. Did you say that the gap is too large for a molecule to form, or too small?

Storms: If the gap gets sufficiently large that the molecule can form, it will not be nuclear active. It has to be small enough...

McKubre: OK. So they've got to constrain the molecule formations, to prevent two atoms meeting and making molecules because the gap is too small for that.

Storms: When the gap is too small, the molecule can't form.

The molecule requires a certain amount of space. In the case of the nuclear active environment, the atoms interact in a way that is different from the way they interact in a molecule. When atoms interact within a molecule, they're too far apart for nuclear interaction.

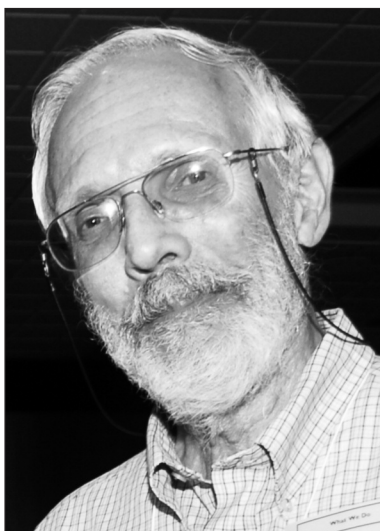
Macy: Ed, you talk about a crack. Is the crack the same as the gap or is that different?

Storms: A crack is a large gap. But they are otherwise the same.

Macy: OK, sometimes you said large, but sometimes you also write about nano cracks. Why?

Storms: The concept I am trying to get across is that of a gap, which is a physical space where no atoms are present. That's a crack. A crack, as you would see in a piece of concrete, is too big. So I'm talking about the same kind of structure, but on a nanoscale.

Macy: Mike, do you want some further clarification here?



Edmund Storms, ICCF14, 2008.

McKubre: Yes. We could go on a long time. This is very good. The crack cannot propagate to the surface. If it did, it would leak hydrogen. So it's a lens inside the material.

Storms: A crack, any kind of crack, starts small. It starts on the nanoscale and then grows bigger. And normally when a crack propagates through a material, it starts small and gets bigger and bigger and goes further and further and gets eventually large enough that you can see it with a scanning electron microscope and then eventually with the eye. That's how a normal crack behaves. I'm talking about a crack that is still very, very small. I'm saying that its width has a critical value. I don't know anything about its length.

McKubre: I'm happy with that. It's just that I was trying to probe as to whether this crack could terminate at the surface and, if so, we have a problem because the monolayer on the surface and the monolayer inside will connect. The crack will be continuous and will be expressed by the same thermodynamic properties and so the hydrogen can all just shuffle along the walls as atoms, meet at the entrance and leave. So that would be a way of taking hydrogen out of the system, something I always tried to avoid.

Storms: Well, it's true. If one of these cracks goes to the surface, then it is an exit and entry point for hydrogen.

McKubre: Not much of an entry because the electrochemistry can't propagate down the crack. I spent years working on that exact problem.

Storms: The hydrogen originating as a result of electrolysis originates on the surface as an ion, and then it reacts with the sample and goes inside. After it's inside, it looks around for places to go based on the concentration gradient. If it finds a

crack, it goes into it. When the crack happens to go to the surface, the hydrogen could find its way to the surface.

McKubre: OK, we agree.

Storms: The question is: What about this crack would allow those hydrogen atoms in the gap to achieve a relationship that would allow a nuclear reaction to occur? That's the big mystery. The unique condition provided by the gap is required because the atoms simply cannot assemble in a way that could support fusion in a crystal structure. The atoms are too far apart and they are not interacting with sufficient energy to cause a nuclear reaction. Yet the reaction occurs. In order to explain this conflict, the nuclei must be brought close enough to fuse only after the Coulomb barrier is reduced. This is an absolute requirement. How can the Coulomb barrier be reduced? Only two ways are known. Kinetic energy can be applied, as is the case when hot fusion is produced. Or electrons can be used to neutralize the barrier. How do the electrons manage to accumulate and why should they? Apparently, some kind of mechanism takes place that is consistent with the laws of thermodynamics, but allows something to happen that you would not ordinarily predict could happen.

McKubre: I just want to put a placeholder comment but not pursue it. I think the Coulomb barrier is a red herring and I don't think that it's possible to achieve cold fusion by a pairwise interaction of two charged objects. The explanation we're looking for has got to be deeper than that, which is one of the reasons why I like your polymer. You know, -e-D-e-D-e-, that monomer is an extended structure and then I can sort of see how fusion might take place, but it wouldn't involve individuals going over a Coulomb barrier.

Storms: May I ask you a question? Why do you believe the Coulomb barrier is a red herring? I mean, if you told this to a nuclear physicist, they would think that you were a little bit uninformed. So why do you believe that?

McKubre: This is the original argument and you will remember, you were there—if it's fusion, then the products are tritium and neutrons. If you don't see tritium or neutrons, it's not fusion. This all comes through still on barrier penetration arguments. But we know that the products of cold fusion are not quantitatively consistent with hot fusion.

Storms: That's right. And that's because they are not thinking about using a different mechanism compared to the hot fusion mechanism.

McKubre: Yes.

Storms: LENR brings the nuclei together under voluntary conditions. Nevertheless, the Coulomb barrier must be overcome. That's almost like a religion to nuclear physicists.

McKubre: That's why I said I don't want to pursue it. But as a placeholder objection. I think the fear of the Coulomb barrier has held us back. And if you look at Kevin Wolf's results, the most stunning thing about the gamma activation that Kevin achieved was it looked exactly like somebody had

switched off the Coulomb barrier for all of these reactions.

Macy: Ed, do you have any comment on that?

Storms: If you make your assumption that the Coulomb barrier can be overcome with electrons then there's a natural logical series of conclusions that come from that result. My theory concludes that hydrogen-4 is produced when deuterium fuses. If you have a light hydrogen and a heavy hydrogen, it's tritium. And if you have two light hydrogens, it's deuterium. Now the big question is, can we prove that?

McKubre: Right. Well, at the moment, we were just trying to delve into what are your arguments for the crack or nano crack being the...

Macy: We have a few more questions back to the crack...How do you think the gap is formed?

Storms: I think it can be formed in a number of different ways. In my particular case, I'm forming it by incorporating inert particles in the material. I think that's the condition in the successful palladium. I'm introducing particles of different sizes and different materials into pure Pd. When the palladium is reacted with deuterium or hydrogen, it expands and the expansion causes a gap to form between the surrounding hydride and the included particle. The relationship between the particle size, the composition and the gap size can be determined. I believe if these variables are manipulated properly, excess power can be made every time. So far I've been successful in doing this.

Macy: Did you say if you manipulate them?

Storms: When the right particle size is used with the right composition, LENR can be made to occur without any uncertainty, which so far has been my experience.

McKubre: That's wonderful. We attributed this to being the magic behind the Englehart batch one. Our first batch of Englehart, 99.7% pure filled with slag—basically calcium, borosilicate oxide particles. So it came filled with dissimilar materials and yet it produced excess heat more reliably than any other material I've ever had in my hands.

Storms: Your experience is one of the pieces of evidence that caused me to go down this road. Other people have had a similar experience including my own. If particles are added on purpose, the Pd can be made to work every time. Nobody has tried this method as far as I know. Silicon dioxide and calcium oxide produce about the same amount of power. The power doesn't appear to have any relationship with the chemistry of the material that's added.

Macy: How much description can you make of your current experiments you're referencing? Can you describe exactly what you're doing or do you prefer not to at this point?

Storms: I told you the basic approach. I melt the palladium with the various oxide particles. It's a very simple, straightforward experiment. I don't want to go into too much detail. I would like to have some control over how people go about

doing this. If anybody wants to replicate the method, they can contact me for information.

McKubre: But the argument is, the crack has got to be a crack because it cannot be anything else. I cannot think of anything else that it could be, but I could think of what it might be. But that's the gist of your argument. For the conditions you have set up, the crack fits the bill.

Storms: If you examine all the possibilities, it's the last man standing.

Macy: What about physical evidence, Ed? What would you tell people is the physical evidence for it?

Storms: There is no physical evidence other than the fact that a very limited number of places exist where hydrogen can accumulate in a material. When all the other possibilities are examined, gaps have the fewest logical conflicts with basic understanding. Only the crack remains as a potential location for the NAE.

Forsley: Would you care to address superabundant vacancies (SAV) and why they may or may not fit the bill?

Storms: First, the conclusions of Fukai, who proposed formation of SAV, have not been supported by other studies. Second, these vacancies are only present either at very high compositions, or at very high pressures. Neither of those conditions is present when LENR actually happens. In my case, I can make LENR happen at D/Pd ratios less than 0.01.

McKubre: On average.

Storms: Yes, on average but with only the alpha phase being present.

McKubre: And the NAE presumably is due to D to Pd ratios.

Storms: The high composition you found being required, I believe, is only needed when the embedded particles are very, very small. In which case, very high compositions are then needed in order to get a sufficient gap. When the particles are sufficiently large, high compositions are not required.

McKubre: How big are your particles?

Storms: Less than 35 microns, but the active particles are much smaller.

Macy: I need a little more explanation...How do they get to be sufficiently large?

Storms: Because I add them to Pd at that size. The size in most of the materials that Mike used would be much smaller because the purification process Johnson-Matthey used would not have removed the small particles. When the Pd was melted during the last stage of the process, the large particles would float to the surface and be removed. The small particles would reach the surface at a much slower rate and not be removed. So particles left behind in Mike's material would be small. In order to make them active, high compo-

sition would be required. In my case, I make them large enough so that I don't have to reach a large composition.

McKubre: Actually the particles we saw in the Englehart material were observed in section to have quite a wide size distribution. You could find micron rocks inside your piece of palladium.

Storms: Yes, a spectrum of sizes is always present. Nature then chooses the size that works. The rest are ignored. Based upon the theory and based upon my calculations, the gap size and its relationship to the composition can be calculated. So, let's assume the critical gap size is 2 nanometers. If the average particle size is 0.1 micron, a composition of about $D/Pd = 0.8$ would produce the active gap size. But if the particle is, let's say, 0.35 micron, the D/Pd ratio of 0.2 would produce an active gap. So the gap size determines the required composition. It just happens that Mike had, on average, small particles and I, on the average, have larger particles.

McKubre: Can you share that calculation? I don't know off the top of my head how I would make that go.

Storms: The change in lattice parameter with composition is known. Consequently, the expansion around an embedded inert particle can be calculated.

McKubre: OK, got it. So that's the mismatch. As the palladium lattice increases in size, no account of pressure was taken inside these cracks. The hydrogen nucleates molecules inside these gaps and blows the lattice up like a balloon, so the excess free volume was because the lattice was being expanded by means other than changing lattice parameter.

Storms: No, that is not why the gap expands. I don't think it's possible on a nanoscale to generate or to achieve that kind of gross expansion as a result of a local gas pressure. Bockris liked to believe that void spaces could form and grow as higher compositions produced increased gas pressure. Such a gap would already be too big to cause LENR. I'm arguing that active gaps are too small for the gas to accumulate. I would argue that on a nanoscale, the Pd cannot expand as a result of local gas pressure.

McKubre: I think there's plenty of force as long as you allow molecule formation. And you admit that possibility by saying that molecules can't form in your gaps, which I agree molecules can't form in those gaps, or you would change pressure and the gap size would increase. But if molecules form, then you have a physical pressure inside the body. If molecules can form, you create physical pressure equivalent to the chemical potential that the loading would dictate.

Storms: Yes. OK.

McKubre: I don't think we have an issue, and your argument is self-consistent. The gap must be sufficiently small that a molecule cannot form.

Storms: That's right. That's a prerequisite for this idea working.

Macy: Are you consistently using the same material in your

run of experiments?

Storms: I'm using palladium.

Macy: I thought you were using a particular batch of palladium that you made reference to...

Storms: Right now I'm using palladium that was given to me by the Japanese about 20 years ago for a different kind of study, which I did and reported. I just kept this palladium. I didn't have anything else to do with it. The precise nature of the palladium is irrelevant to my model. So therefore, I don't really care about the small differences that might be between these samples. And indeed, when I use different pieces of Pd, I get the same result.

McKubre: That's the TTK 1 millimeter plates that Takahashi was using?

Storms: No.

McKubre: Japanese palladium.

Storms: The Japanese palladium was pure. There were 90 samples and they were treated in eight different ways. But the different treatments involved the annealing treatment and not the purity of the sample itself. The largest impurity was a little bit of silicon. Only five of these 90 made excess energy. The ones that made excess energy had a somewhat higher silica content than the average. But other than that, there didn't seem to be any characteristic other than their ability to form excess volume. The study of this behavior was the reason why this experiment was done. I had made the case that certain palladium forms excess volume when it is loaded and deloaded. Also, when the hydrogen is removed, the volume does not go back to the same shape and size it had originally. Consequently, the material gains extra volume. Samples that did not expand more than 2% made excess energy. If the material expanded a large amount, it would not make excess energy and would not load very well either.

McKubre: What is the precise method of introducing the impurities, the solids, into your palladium? Do you use an arc-melting process? There's nothing fancy or you stick it in the crucible or how do you melt the palladium to introduce it to the particulates?

Storms: I use a hydrogen+oxygen flame. I would prefer to use an arc-melting, but I do not have one. I can also melt with an oxygen+LP flame but not as easily.

McKubre: Which might be significant, but the hydrogen+oxygen flame is going to introduce different characteristics than an arc-melting would, for example.

Storms: That's true, and H_2+O_2 does introduce hydrogen, but I remove the hydrogen by subsequent treatment I have not described.

Macy: You said that all occasions when LENR is observed are consistent with gaps being present and not consistent with vacancies being present. Also, the larger gaps are frequently

seen, as when I asked about physical evidence. Would this facilitate anything else?

Storms: I don't quite understand that question.

McKubre: You say not consistent with vacancies. I would probably agree that the characteristics that we understand are not consistent with the effect being caused by point defects, but there are many, many classes of defects, as you know very well. So I cannot rule out defects on the basis of what I know and I cannot rule in or out cracks on the basis of what I know. I think it just awaits physical proof.

Storms: Well, there we have a somewhat difference of opinion. I can rule out vacancies and other flaws within a crystal structure as the NAE. The locations within crystal structure have to play by the rules of the crystal structure. There is no freedom to just have things happen willy-nilly. For atoms to accumulate, they must do this outside of the crystal structure. Also, no mechanism exists in a crystal structure for the Coulomb barrier to be overcome. I'm proposing that these gaps allow atoms to accumulate chemically, but also have a condition that is not present in a chemical structure within the lattice itself. It's only by virtue of this unusual characteristic of the gap that the next step toward a nuclear process is possible. Of course, that's an assumption based upon, as you might say, the lesser of two evils. But, that's all we've got in this field.

McKubre: Well, yeah. No, I'm not being critical. I'm actually really, genuinely trying to understand. But when you say that the hydrogen doesn't have to play by the rules of chemistry, the atoms that you're thinking of as participants in the nuclear reaction are adsorbed on the wall of the palladium. You've got, as you say, a half crack—now you've got palladium, you've got atoms and you've got the additive that you threw in to make the hole in the first place. So the atoms adsorbed are in the thermodynamic environment of the lattice. They must have a defined chemical potential which is influenced by potential of the D in the lattice. I don't see them being decoupled. I see them being separate and subject to different equations, but they are nevertheless coupled; as you raise the chemical potential in the lattice you will raise the chemical potential of the D atoms adsorbed on the inner surface of your nano crack.

Storms: I agree with you. This is what I call a problem of vocabulary, mostly. Yes, all atoms in a chemical structure, no matter where they are, have to play by the rules of chemistry. But the rules of chemistry within a crystal structure are different from the rules of chemistry outside of the crystal structure, because the crystal structure itself generates a collection of rules. That's why the crystal structure is stable and it's present in the first place. So if you try to modify the crystal structure, you have to violate the rules that cause the crystal structure to happen in the first place. If you go outside the crystal structure, on its surface, for example, or in the crack, now the laws of thermodynamics still apply, but the rules that apply to the crystal do not apply. That's the difference. I mean, it's a subtle difference, I'll grant you. So we're not arguing about anything really basic. We're essentially discussing details.

McKubre: I think it's the difference between a chemist's perspective and an electrochemist's perspective. I'm just writing some notes for our workshop for the conference and, for my sins, I've been listed as electrochemical loading. I'm going back over my electrochemical loading stuff, but it's in the electrochemical perspective there is a continuum. You know there's a chemical potential of the D in D₂O. There's chemical potential of the D adsorbed on the surface that comes from the results of reduction of the D₂O molecule. Then the adsorbed state is now in equilibrium with the chemical potential of the absorbed state. So it's a continuum, and then if you've got a crack inside, or inside Arata's double structured cathode, for example, there is a continuum of chemical potential from the free surface to the internal wall. They each communicate to the other. And palladium being so permeable, the efficiency of that communication is high.

Storms: I agree with you, but the difference in scale is important. If you start on the atomic scale and ask what each atom is doing, you get a somewhat different perspective than the one you just described, which is an average behavior. In order to understand the nuclear process, which occurs on an atomic scale, you need to think outside of the average that you're describing.

McKubre: I sort of get it, but I'm still not jumping up and down and saying, "Yeah it is right. I really understand what he's talking about." The proof of your pudding is going to be in the experiments that you're running now, hopefully.

Storms: I might add too, all of my work is done with gas loading. I've moved away from electrolysis. Electrolysis gives the same effect, but at a much higher cost of complexity. The gas loading allows me to achieve a much higher temperature and to have a simpler process.

McKubre: Well, calorimetry is so much easier if you don't have high electric power going in.

Storms: Yeah, that's right. That's definitely a problem.

Macy: So do you feel like we've done a decent job of augmenting your explanation or would you like us to focus on something right now? Is there anything you'd like to add?

Storms: The more detail I get into, the more I'm obliged to use my imagination and the more opportunity there is to find fault. So I try to keep my response as close to reality as possible just to avoid that problem. If you want me to get into a more imaginative part of the explanation, I can do that.

Normal people are guided by models that they take as being so convincing and so accepted that they forget that they are models. So when I worked in my previous life at the LANL, I applied the laws of thermodynamics, which is only a model. So, everything that I saw was interpreted in terms of that model. And if I saw something that conflicted with this model, it was considered an error.

In cold fusion we don't have a good model and everybody interprets what they see in terms of their own unique model. If the model is wrong, then the interpretation is going to be wrong. It's as simple as that. I'm trying to find a model that

is closer to reality than any other and not based upon some arbitrary mathematical description. I do this by looking at everyone's work, including my own. I make one assumption that only one mechanism is operating regardless of the conditions or material. If the model is close to reality, it could be used as a guide to obtain information more effectively.

Macy: By the time we come to the end of July to ICFF24, do you think you'll have enough of your experimental work done that you'll feel like you will be able to present the experimentation that backs this up? How close will you be?

Storms: I think that I have enough understanding right now. The big question is how much detail is required to convince the skeptics. The paper that I submitted to ICCF24 takes a general view, anticipating what I'm doing now is correct. I don't want to give too many details about what I'm presently doing because it's still a work in progress. To some extent it will be up to Larry and NASA because they are paying me to do this and so they have some say in how much is made public.

McKubre: Let me make a pitch to both you, Ed, and Larry about independent replication. As I've said many, many times, one experiment is no experiment; the results obtained by one person is not a result. It has to be backed up by independent replication. So if you were able to supply your samples to somebody who has calorimetric competence and they confirm the result, that's so much more valuable. I know how painful that is and I've resisted it myself, but that's basically what it takes to get people to believe, if somebody else is doing it.

Storms: I agree totally. Fortunately NASA is set up with calorimeters that are based on my design. Larry is working very close with me to duplicate what I've done. NASA will eventually be in a very good position to replicate.

McKubre: Super.

Storms: And they have a reputation for honesty that should be believable.

Forsley: We really did go to the moon...

Storms: There aren't very many people out there that are competent.

Macy: Ed, we are looking forward to seeing your presentation at ICCF24.

McKubre: I genuinely believe and have said many times that you, Ed, have done more in understanding other people's experiments, you've written more about, codified the field better than anybody else, which is why you're going to get a gold medal and you deserve it.

Storms: Thank you. This was a big surprise to me. The second Toyoda Medal was to be awarded to Pons. But Pons showed no interest in the field. I really never expected I would get it unless I had solved the problem. I may have solved the problem, in which case maybe I feel justified in

getting it. We'll see.

McKubre: Well, none of us are going to get any credit and none of us are going to be heroes until somebody solves the problem. And I don't care who it is. If it's you, that's great.

Storms: I'm working on it with Larry's help.

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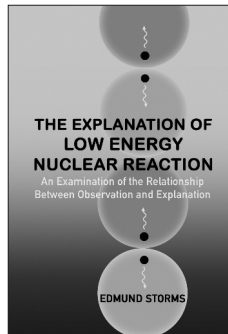
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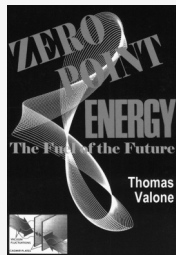
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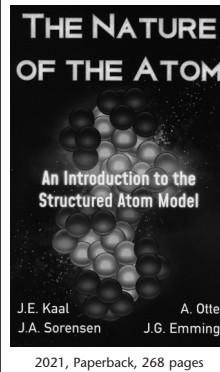
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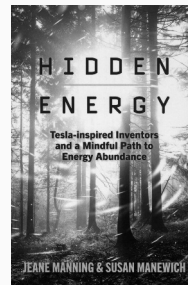
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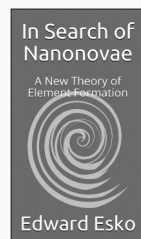
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