Historic Perspective on ICCF1:
Dr. Mallove's Commentary on the Conference

Infinite Energy founding editor, the late Dr. Eugene Mallove, attended the First Annual Conference on Cold Fusion (ICCF1), unofficially representing the MIT News Office and also in part conducting research for his "on again/off again" book contract from John Wiley & Sons (the publisher cancelled, then reinstated the contract for *Fire from Ice: Searching for the Truth Behind the Cold Fusion Furor*, which was ultimately released in May 1991).

The conference ended on March 30; on April 1, Gene recorded notes to himself about the event. We present here excerpted portions of the transcript, which are testament to Gene's commitment to and excitement for the cold fusion field from the very start.

"Why I Believe Cold Fusion Is Real"
Dr. Eugene Mallove (April 1, 1990)

. . .I am recording this message in my hotel room at the University Park Hotel Room 233. I have just completed three days of attendance at the First Annual National Cold Fusion Conference, held here at the hotel, sponsored by the National Cold Fusion Institute right here next to the hotel.

I would just like to dictate these thoughts and I would like to title it, "Why I Think Cold Fusion Is Real." This may be a prospective article for *Technology Review* or for my own benefit, just to get my thoughts in order.

For a year now I have been afraid of being wrong about cold fusion and I guess I have to say that this is the central issue about whether or not to accept or not accept cold fusion, that is the fear of being wrong. Now, one indeed should have a certain amount of fear about being wrong in doing or assessing science. However, there comes a point where the fear gets out of hand and is, shall we say, overwhelming—that is, it prevents productive thinking and further assessment of a scientific problem. This fear of being wrong about cold fusion is helped, for me and I'm sure by others, increased in no small way by the media coverage which increasingly is not good and is wrong-minded. For example, right at the onset of the meeting we have the issue of *Nature* magazine with an editorial saying "fond farewell to cold fusion," as well as David Lindley's article titled "The Embarrassment of Cold Fusion." We also have Bill Broad's article in *The New York Times*, basically throwing cold fusion into the trash bin, at least in its first opening paragraphs, alluding to the "fact" that Pons failed to convince anyone of the merits of his work—a most extraordinary statement because many people here were quite impressed with it, including Peter Hagelstein, who thought it was a very clever idea to apply non-linear regression techniques to assess calorimetry with high accuracy.

. . .I guess I would have to say I am extremely impressed at the moment with the very large numbers of good calorimetry studies that seem to indicate not only excess power and bursts of excess power—excess power over long periods of time, bursts of power over shorter periods of time—and total integrated net energy production, amounting to megajoules per cubic centimeter of the palladium electrode. Now, in general, calorimetry is a very complicated and difficult area and heretofore I'd have to say I wrote it off to some extent as black magic. Not really black magic, but as something that was so problematic that you couldn't get a proper assessment of it. But when you see a number of different techniques in different laboratories being applied in different ways in calorimetry, both open cell and closed system calorimetry, and you find that many of them are getting excess energy production, excess heat, excess enthalpy, you begin to believe it. And I'd have to say I now believe it and it leads me strongly to the position that there must be a nuclear effect going on, because chemical explanations can simply not account for that amount of energy.

I am also impressed by the tritium measurements from many different laboratories. First let me back up before tritium and just mention a few of the laboratories that have gotten some good calorimetry: there's Stanford University; Huggins' group with their isoperobolic calorimetry; there's the Scott group down at Oak Ridge National Laboratory; of course there's Pons and Fleischmann, who have some apparently excellent results; there are people at Texas A&M with good excess heat production results; and it goes on and on. There are many small, individual operations—individuals literally—who are doing calorimetry and getting excess heat.

Now, excess heat is not always obtained by everyone and that's why there are lots of negative results. However, what has become even more central in my thinking about this problem, besides the calorimetry, is the incredible complexity of the electrochemistry and metallurgy of the surface of the palladium electrode. Now it is not certain that whatever is going on is in fact going on at the surface of the electrode exclusively to produce the power. It may be a bulk effect. But one thing is for sure—the surface of the palladium electrode is more complex than I would ever have imagined. There are dendrites, there are build-ups of all sorts of substances; various types of physical analysis has abundantly confirmed this. What we're dealing with on the palladium electrode is by no means a homogeneous material that either works or doesn't work and therefore one believes or does not believe that cold fusion is real on the basis of everyone putting in the same identical stick of palladium. Each stick of palladium is processed in a certain way from a different company,

or even from the same company, but more importantly the exact way in which the cell is set up and will be used results in different types of surface chemistry and physics, which leads to success or failure in generating excess power.

Now a very revealing comment was made after the meeting yesterday by Nate Hoffman of E-Tech, which is a DOE-funded research laboratory that is assigned the task of, among other things, analyzing material properties, and has been involved in analyzing the material properties of the palladium rods from various operations. Nate told me that the arch-skeptic Nate Lewis of CalTech, his electrodes for example, are exclusively covered to a depth of some several thousands angstroms with crud, as most others are, but many other people—a small percentage of them actually have. . .a small percentage of the successful ones even, 5% he said and I don't know how accurate that is—are also covered with crud, that is the build-up products, and don't have palladium in that coating. Now, you can get success with or without significant evidence of palladium within the upper crust of the coating apparently, but it seems more likely, at least intuitively at this point, that success tends to favor those experiments where the palladium does somehow manage to stick its head above the murk. I had a revealing conversation with Dennis Cravens, who's an individual who thinks he's beginning to get excess heat; he operates in Vernon, Texas. He reverses the polarity of the cell using the palladium as the anode first, to get some of the impurities onto the platinum electrode apparently, and thereby making a cleaner electrode; so he does that for some hours first before doing his more extended run.

Dr. Eugene Mallove and Dr. Martin Fleischmann at ICCF1

Okay, so the palladium cathode is incredibly complicated. And that is enough to suggest to me that you can't make these blanket assertions that some of the skeptics have been making, such as Douglas Morrison, for example, of CERN who say that the negative results outnumber the positive results, or at least initially, no longer he says because negative result people are just dropping out of the race and therefore the ratio of positives to negatives is higher. You can't make these blanket assertions. The complexity of the electrode surface is enough to convince me that we're dealing with a very difficult phenomenon, whatever it is, and that you shouldn't expect always for the thing to work. There may be unusual conditions that are required to trigger it, to extend it, to make it happen.

Now, if it were just for excess heat—which is impressive enough, I must say—and there were no other suspicious nuclear events going on, well then one would be hard-pressed to even begin to suggest that this might be cold fusion. On the other hand, what we have is a very significant number of nuclear products and nuclear anomalies that seem to be seen. Now tritium is by far the best. Many laboratories are seeing tritium, both in the liquid solution and in the gas outflow. Now, one's first skeptical reaction to tritium, which is measured by standard techniques like liquid simulation counting, is to suggest that the tritium is a contaminant, but of course many of the experimenters have of course realized this over the past year and have done extensive precautionary and testing procedures to rule that possibility out. I am particularly impressed with the results of Talcott and Storms at Los Alamos; their tritium results and attempts to rule out contamination are very impressive. But you get about 15 or so different laboratories, according to them at least and they have a chart of that, that show tritium production in palladium electrodes. Now, the skeptics keep saying, "This is contamination, contamination, contamination." Well, my answer to that is, "Are all of these people so completely deluded that they have all convinced themselves that they have tritium based on pure contamination?" My own feeling is that that can't be.

Now, at the same time that we have the tritium, we also apparently have neutron production, on and off in different types of experiments, low level neutron fluxes. Now those are the most problematic kinds of measurements, very problematic. The ones that are the best are not even in the electroanalytic cells, apparently. Those are the ones by Scaramuzzi in Italy and Howard Menlove, who gave an excellent presentation on the fantastic detecting system that they have arranged for titanium chips and D_2 gas. They bring the temperature up from -190°Celsius, liquid nitrogen temperature, and they go up to room temperature but when they pass approximately through -30°Centigrade they achieve bursts of neutrons in various groups, low level to be sure, nowhere near consistent with any sort of power generation, but nonetheless a nuclear effect coming from a palladium-like element, namely titanium. And neutron bursts, of course, have also been seen by others in the test cells, the electrolytic test cells. Now, the neutrons and the tritium in all cases—all cases—are insufficient to explain the power generation levels in the heat or excess heat-generating experiments. Now, first of all, in many cases the excess heat-generating experiments have not had simultaneous neutron and tritium measurements and have found those things at the same time. In some cases, yes; one case in particular Bockris mentioned the other day. But what's clear is that if you use classical deuterium, deuterium fusion reactions leading to tritium plus proton or helium-3 plus neutron for the neutron case, you are not going to get enough classical energy out even if it did go into the lattice in some way in some as yet unknown mysterious way, you're not going to get enough energy into that system from those types of fusion reactions to account for the excess heat.

So the conclusion is that there could be a dominant reaction with as yet unknown end products that has with it an auxiliary reaction perhaps, given the tritium and the neutron fluxes and bursts. There may be more than one kind of nuclear effect going on. Lithium in the electrolyte may be involved, for example, as well. The light water, hydrogen, may also be involved as a fuel in this particular model. So, you're left with the notion that there are end products that we don't know about yet, such as deuterium, such as helium-4. The helium-4 testing in cases of heat production has not been good. The helium-4 is easily masked by the deu-

terium and experiments to ascertain it in the gas stream or in closed cells or whatever you want to say or use will be difficult and expensive. Now, we know of such experiments and ideas that are perhaps underway, but they are not done yet. So what we're left with is the clear impression that there could be a nuclear process of some kind generating the heat with as yet unknown end products, but end products which nonetheless mask themselves within the deuterium oxide (D_2O) or in the gas stream as helium-4 and so forth. Now to me that's a very satisfying picture. Now, what kind of theories have been offered to explain this process? I am less concerned in some sense about theory, more interested in the experiments as far as my degree of belief about cold fusion. Yet, if there were no theories to account for it, to even begin to account for it, and no one was coming forth with any remotely conceivable theory, then I would say we have a real dilemma. However, that is not true. There are a half a dozen good theorists—Peter Hagelstein, Bush, there's Preparata from Italy who makes a very strong case. . .Schwinger—and all these people talk about collective effects within the palladium lattice, coherent effects, collective effects. In some sense, a bulk, multi-body system of effects that produce the power and/or the nuclear end products.

. . .We had experimental results, we have heat, nuclear products, and how can I forget the autoradiography that has been done not only now by the Indians at Bhahba, the BARC Institute, a fascinating hot spot looking events with fogging the film, some kind of ionization reaction perhaps, coming out of the electrode, and as well McKubre's group at SRI, Shroud of Turin looking images that show some incredible localized ionization process that's fogging that film post-electrolyzing. And one can say, "Okay, maybe it's some kind of chemical reaction that's fogging the film if the film was in proximity with the outgassed electrode," but some precautions to that effect have been taken and it seems to me not likely that the chemical outgassing or whatever it is, the influence on the film, the possible influence on the film, it seems unlikely that it would come out in such spots—maybe overall fogging, but spots and streamers and lines? To me that doesn't look quite right, so I am very excited about that aspect.

I may have lost track of the trend of thinking here, but I am saying that we have heat, we have nuclear products, both with tritium and neutrons and autoradiography and possibly someday we'll even have some good low-energy X-ray results. People are trying to measure those things, but that's difficult. It all adds up to a phenomenon that appears to be new. It certainly is anomalous. By definition it is anomalous. These people are good people, serious people, doing serious experiments reporting anomalies. And the anomalies have to be explained and I believe that at this point that even though we do not know for sure what the mechanism is for cold fusion, whatever it is, we have a situation where the burden of proof is now on the skeptics, not the people who tend to believe that the results confirm cold fusion, but on the skeptics. The burden of proof is on the skeptics to show how all of these, and I repeat all or the bulk of these at least, experiments are seriously awry. What is the multi-dimensional explanation of error that can get around all the interesting anomalous results? My own feeling is, it's so tough to do that that it's easier to try to think of a theory of cold fusion. I think, though, that it's good for critics and skeptics to be involved to try to knock it down. But I don't think they've been able to so far. They've been grasping at straws and many sightings of such grasping at straws can be given, but I think one of the most significant is Dr. Petrasso's effort, noble effort, to knock down the tritium results. For example, in discussing the Bhahba results he speaks of how they have a heavy water with tritium contamination moderated reactor there and they periodically release lots of tritium into the atmosphere and that could be contaminating. But those people are very knowledgeable about tritium and they are very knowledgeable about the contamination problem and they've done careful checking, and so have Ed Storms and Carol Talcott. So I feel that indeed it's appropriate to continue questioning those tritium cases. I think less energy should be put on coming up with very wild scenarios for tritium contamination. If you want to talk about tritium contamination, it might be better to address any possible unknown source of the tritium within the system, although it seems that they have just about analyzed everything including rubber stoppers and glass and everything else, so I think there comes a point on the tritium where there's diminishing returns.

Even though the tritium in certain experiments might not be massively elevated above normal background, I really think that it's anomalous and it has to be considered as a nuclear end product. And if it is a nuclear end product, then that opens up the gates of belief, to me, to fully accept the heat, the excess heat, the excess enthalpy, coming from the other experiments. The association of the two in the same kinds of experiments, even though they don't always come together that's not important, the association is presumptive evidence of a new nuclear process and that is why I believe that cold fusion is real.