Progress in Catalytic Fusion

Birth of a Revolution in Cold Fusion?

To the delight of many at the Seventh International Conference on Cold Fusion (ICCF-7) in Vancouver, BC last April, a new approach to cold fusion emerged. Dr. Les Case, an experienced chemical engineer with four degrees from MIT, announced what he is calling "catalytic fusion"—to distinguish it somewhat from the original electrochemical approach. He had concluded that the electrochemical method of Fleischmann and Pons was going to continue to be limited by materials issues—palladium cracking, composition, etc.—and the inherent difficulties of working with electrochemical systems. Furthermore, he wanted to achieve the higher temperatures that are allowed by gas-phase systems.

The story of Dr. Case's discovery of catalytic fusion is exciting, including his travel to Europe and Japan in search of the proper path forward. In the account below, we let Dr. Case tell the Edisonian story of discovery in his own words. It turns out that relatively simple catalysts—off-the-shelf "hydrogenation" catalysts used in the chemical industry— seem to catalyze deuterium (heavy hydrogen) gas to helium-4 in a heat-releasing nuclear reaction that is millions of times more energetic than any conceivable chemical reaction. These catalysts are typically activated carbon that has been doped with precious metals such as palladium. Other catalysts may emerge as a result of this line of investigation, ones that perhaps will not require any precious metals. Unlike high temperature plasma fusion (hot fusion), there is no harmful radiation from the process. Thus, the original promise of cold fusion may now be realized in more robust and repeatable experiments. Ultimately, these could be commercialized in relatively straightforward ways that make use of chemical engineering practice.

At the moment, catalytic fusion studies are proceeding at Dr. Case's own lab in New Hampshire, at SRI International in Menlo Park, California and at the Pacific Northwest Laboratory, (a U.S. Department of Energy lab, under contract with Russ George's Saturna Technologies, Inc.). In our own facility (New Energy Research Lab—NERL) here in Bow, New Hampshire, we saw the positive results of a Case experiment first hand shortly after ICCF-7 (see *IE* No. 19). We are beginning a second round of work to demonstrate the process with a relatively simple calorimetric dewar set up. We hope that these efforts help catalyze new work by others in an area of immense potential.

We are pleased to present the following progress reports on catalytic fusion, in the words of Dr. Case and Dr. Michael McKubre.—*EFM*

EDITOR's NOTE: In the course of video-taping our forthcoming documentary about cold fusion (*Cold Fusion: Fire from Water*), our video team visited Dr. Les Case this fall in his basement laboratory in New Hampshire. These are some of Dr. Case's recollections about his discovery and his projections about the future of catalytic fusion technology. —*EFM*

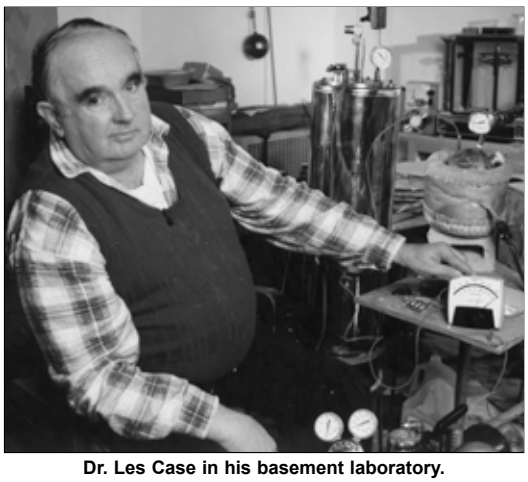
How I Discovered Catalytic Fusion

Prologue

I was going to be a chemical engineer and then head a large corporation. I went to MIT and I got three degrees in Chemical Engineering through the Sc.D. Also, along the way, I took a side degree in Business Administration. I went to DuPont to their Central Research Station, the Plastics Department, or something of the sort. I worked there and it became clear that they didn't want to do business the same way I wanted to do business, so then I taught school for ten years.

I started my own laboratory, studying improved plastics and polymers and I had, for fifteen or twenty years in Nashua, New Hampshire, my own company and my own building, but it never went commercial. I did a lot of research and development, got a lot of patents, and then my wife got very ill. I spent a fair amount of time concentrating on keeping her well. So the laboratory there went inactive. Then when my wife died in 1987, I had a lot of things to do to get the estate together and so forth. I was then following scientific developments, which were then current. I became quite interested in high temperature superconductivity. In fact, I went to the Beijing Conference on Rare Earths and presented a theoretical paper providing the background, what I thought was the chemical background for the physical phenomenon of high temperature superconductivity. For a while I began to play around with the idea of getting a useful device based on high temperature superconductivity.

At just about that time, the cold fusion hubbub erupted. I followed it with some interest, but I could not see how it would go commercial. The original conception obviously was a scientific curiosity, but it wasn't at any point in the reasonable future heading towards a commercial operation. So I followed that at arms length until I saw some work by Dr. Yamaguchi at NTT in Japan, in which he had obtained an 800°C-plus exotherm [exothermic reaction] with, he thought, big bursts of neutrons. So I went to visit him—actually in Tokyo at his laboratory—and looked at his equipment. Beautiful stuff! Very careful work. Clearly, he had obtained a result which was very, very definite. And, incidentally, at this time, which was about 1993 or so, it was



Dr. Les Case in his basement laboratory. Photo: Free Spirit Productions

nite. And, incidentally, at this time, which was about 1993 or so, it was still highly controversial as to whether or not anything related to cold fusion had ever really been seen in a definitive fashion. There was no question that he had seen a very definitive result. He'd obtained 800°C-plus.

Well then, I thought, "OK, this is something that needs to be refined and scaled up." And because he was working with palladium and everybody else was working with palladium and platinum primarily, it became sort of obvious to me that probably some sort of catalytic effect was involved. I am a chemical engineer and chemical engineers use chemical catalysis all the time. Platinum and palladium are the preferred catalytic metals. So I then embarked on trying to follow up Dr. Yamaguchi's work in my own fashion. I was initially concentrating on the neutrons as being something important. I then spent, I think, over a year trying to find a laboratory, equipped to deal with neutrons, which would cooperate with me—in which I could sponsor some work and try to work out my ideas.

Off to Europe

There was no laboratory in the United States that I could find that would work with me. After all, it was cold fusion, or something related to cold fusion and most scientists wouldn't touch it—even for money. I finally determined that because all Eastern Europe is known to be very low wage scale—low price scale—that there were some Eastern European neutron laboratories that were of possible interest. So I got myself a plane ticket to Berlin and took the train going east to Warsaw.

I went to the Department of Nuclear Science or something of the sort in the Physics Department in the University of Warsaw. I met a nice lady there and there was a possibility of doing some work. We agreed to meet a little later on my trip to Budapest, for dinner and further consultation. Then I looked at the train schedule and considered going to Lotz. It's not very approachable, so I skipped directly to Prague, which was a lucky shot. I went to Prague and I knew about Charles University there, which is a very famous old university, and went downtown to the old town square to the main campus and tried to find the Physics Department. It wasn't easy, because I don't speak Czech and many of the Czechs don't speak English. I finally found somebody there and she told me, "Oh, you want to go the Physics Department. That's on the other campus, across the river."

So I got the directions to go to the other campus. It's a tower building there and the Department of Nuclear Science was on, I think, the tenth floor of this tower building. So I had the taxi driver let me off and I went to the tower building, found the elevator, and went up to the 10th floor. I walked out the door and there was a sign that said "Nuclear Science." I went in and there was a very efficient scientific looking gentleman with white hair, sitting there talking to, I guess, the secretary. It turned out he was the Director of the operation.

I explained to him I wanted to do this kind of research and he said: "We'll do it!" I said "Really, who has to approve it?" and he said: "We'll do it!" So I hooked up with the Department; actually it's the Nuclear Center, Department of Physics and Mathematics at Charles University. For I guess over a year, maybe about two years, I was doing experiments in their nuclear laboratory, which is associated with CERN. It's a serious nuclear laboratory. It is by no means equivalent of CERN. . .

Catalytic fusion reactor in Dr. Les Case's lab, showing pressure gage and resistance heater collar. Photo: Ed Wall.

Shooting in the Dark

It was empirical work and I was trying to find an effect—the idea was to find some sort of temperature [rise]. I was using the temperature gradient for a catalyst—active versus a blank. I had a big vessel, and I had four samples inside the big vessel. One of these four samples was the blank and the other three were potential candidates. I would change the hydrogen or the deuterium gas over the sample, change the nature of the samples, and look for temperature differences. With neutrons or without neutrons. We also had to measure the neutrons I might be making, so it was empirical. I made a whole bunch of runs, —oh, on probably three or four different trips, and with minimal results for maybe the first two trips. One of the times I started with a plated palladium-on-copper tubing, and I thought that might be catalytic, but it wasn't. I tried some titanium tubing, but it wasn't catalytic, and I finally ended up thinking: "If it's catalytic, you better use catalysts." So I ended up scanning through several dozen available samples of catalysts.

Finally, some of these catalysts I was modifying—I actually had some platinum and palladium acetonate, and I was modifying the surfaces—all of a sudden we started seeing temperature differences in one or two of the samples. That is, we were beginning to find active catalysts that would really show a temperature gradient over the inactive catalysts. And I can remember very clearly, one day it was, I think 1.2°C or 2.1°C above the background in a particular catalyst sample. The physicist that was working with me was amazed, because as far as physicists are concerned, 1 or 2°C might as well be a million degrees, because it's clearly an effect and we were measuring it immediately versus an adjacent blank.

He said. "Well, how did you select this material to do this experiment?" And I said: "Because that's the one that works!" This is what happened: I had scanned through with many different experiments through all the various candidates that I had received from three to five different sources of catalyst, until I found a catalyst, a chemical catalyst that was off the shelf, that actually worked to give some sort of effect with deuterium compared to hydrogen and compared to the other blanks. So it was strictly an empirical result, just blindly following my nose. Changing the conditions, changing the pressures, changing the temperatures, and so forth until I finally found a catalyst that gave me a result. . .

What happened was as follows. I have always been very protective of this. Well, not always, but for the last five years or so—very protective of the results—not disclosing them to anybody. I have a series of U.S. patent applications, about eight or ten of them, a basic one which was totally speculative and wrong. I kept filing continuations and amendments to them. Finally, I began to get these results, and then with all of our three or four patent applications prior to my current ones, I

applications prior to my current ones, I began to get results. I kept improving them.

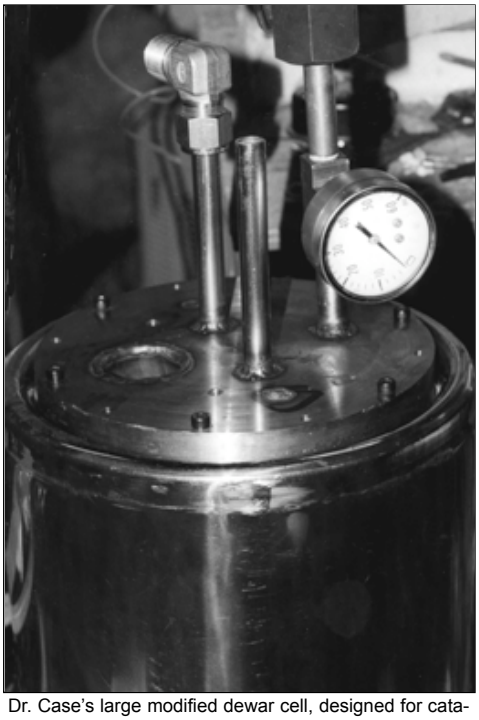
Finally, I got to a set of results which defined the field, basically. With that patent application, I filed for foreign applications and that was published in November 1996. I expected that there would be a very big response when this was published, but there was no response whatever. Nobody was paying any attention. So finally I decided to take the bull by the horns and I appeared at the Cold Fusion Conference unannounced, in Vancouver in April of this year. At this April Cold Fusion Conference, ICCF-7, I gave a brief talk, saying that I had developed an experimental procedure for reproducibly generating a heat effect with deuterium and that it's catalytic. As I say, I can reproduce it and I can scale it up. It created quite a stir at the conference, because people were looking. A lot of people were looking for this: some sort of basic real approach, not just playing around, but a concept of something that made it work reproducibly. The concept I introduced was contacting a certain limited range of standard chemical catalysts with deuterium under standard conditions, and it would work.

Well, there's a little bit more to it than that, but this was new because nobody previously had ever used a standard chemical catalyst. They were always making their own special material and practically nobody thought of a catalyst. It was their particular equipment, and sometimes it was very elaborate. But I was able to buy, off the shelf, standard chemical catalysts which did work. Gene Mallove and I met at that conference. This is how Gene and I came together at the conference in Vancouver.

Latest Experiments

Well the situation basically is this. This is the vessel. It's a modified oxygen tank and in it is a thermo-well, this is a gas inlet and outlet, and this is simply a port for putting solids in or out. Now in the bottom of this vessel, which is heated in this jacket, there are about 40-50 grams of standard chemical catalyst. It's been contacted now with deuterium gas for six or seven weeks and, using hydrogen in this vessel under exactly these conditions, I got a steady state temperature of 181.5˚C. Now, when I switched to deuterium it started off about 180˚C, slowly rose over the space of two or three days, and finally levelled out at about 220˚C, maybe a little bit more than 220˚C. Right now it's about 215˚C, almost 35˚C hotter with deuterium inside than it was with hydrogen. This is excess heat, which is apparently occurring due to deuterium fusing to helium-4.

So, inside this vessel now for six or seven weeks, we have had deuterium fusing to helium-4 and giving this excess temperature of about 35˚C, which is big—a really big effect compared to previous effects of practically unmeasurable temperature increases. This one is now continuing and maybe will continue for some weeks or months still. The idea is to test the reliability of the catalyst. The catalyst must work for some months or it's not a viable commercial process. You have to be able to load up your reactor and have it generate the heat for months without having to re-do the catalyst, because it's expensive and too much of a problem. So this is rather encouraging. It looks like it may be totally stable, or at worst, over the space of many months drop 10, 20, 30% in activity, which is acceptable.



Dr. Case's large modified dewar cell, designed for catalyst beds up to one kilogram and aimed to achieve self-sustaining. Deuterium gas leaks are being fixed. Photo: Ed Wall.

Helium Measurements

Now, when this experiment is concluded for one reason or another, a gas sample is going to be taken off through here and analyzed for helium-4. With any luck, it may even read over 100 ppm of helium-4, maybe 200 or 150 parts per million. It won't be going up to a thousand parts but it's going to 50 or 100 or more. This is very very significant, because the helium-4 content of air is 5.2 ppm. So anytime you get above 5.2 ppm you're *making* it. So this vessel is sitting here making, as we watch, helium-4 at a temperature of 215˚C. Now this is a very novel concept: that you can have nuclear fusion occur at 215°C and one atmosphere pressure. Those are very, very mild conditions compared to what they're doing in plasma fusion and the H-bomb.

I had run this experiment several times before and obtained samples which I had analyzed at the OakRidge National Laboratory by the kind people at Lockheed Martin. I had some trouble with leakage and sent some bad samples and one or two fairly decent samples. One sample was contaminated after I adjusted the leakage and measured something like 100 ppm of helium-4. But they were able to analyze a good sample at something like 91 ppm of helium-4. Now the equipment is not ideal, because it's a big magnetic sector instrument and it separates out helium-4 from deuterium, which also has a mass of 4 by a very small difference in mass—something like 1%. That's the only way they do it, they don't trap out the deuterium. Because the helium is at a very low concentration, they see the helium-4 peak as just a bump on the side of the deuterium peak. So it's very iffy.

Now, some of the people at Vancouver [ICCF-7], at least, saw this as not particularly reliable, but certainly interesting. They began to try to reproduce this rather quickly in May. Certainly by June other people were trying to reproduce this result. One of the people who tried to reproduce it was a man named Russ George who has an association with SRI International in Menlo Park, California. He set up their equipment, apparently with permission of the group, and tried to reproduce this. The way he originally set it up, it didn't work. He got no [excess] heat and, of course, no helium. We had a brief consultation about it and I explained to him that you can't run the apparatus that way. I made a couple of suggested changes and it immediately took off with heat generation. Then he used their mass spectrometer instrument to analyze for the helium produced after 24 or 28 days, and he got a helium content up to about 11 ppm, which is far above anything that can be explained from leakage in from the air. And, because it had started at zero and went up to 11 parts per million in a monotonic way, that is, always a rising function, it clearly was coming from inside the vessel and not from contamination.

Now, those data aren't considered by the people at SRI to be definitive enough to be published. They are very, very strongly indicative that there is helium-4 generation by this fusion under these conditions. Now that result is going to be re-confirmed by SRI in a much more

careful and definitive fashion. When the data are finally very very firm and unassailable, "bullet-proof," they call it, that will be published in a definitive paper saying this is now proof that we are getting helium-4 generated and we get a correlation between the helium-4 generation and the heat output. This clearly is a catalytic fusion, it really is working and, in fact, it is a new branch of physics.

Small Case catalytic fusion cell (inside glass dewar, center) set-up for calorimetry calibration and "live" operation. Note: gas cylinder safety cap is used to secure thermal cover on dewar. NERL, Bow, New Hampshire. Photo: Ed Wall.

Scale-Up

My objective always has been not to play around scientifically, because I'm not really a physicist, but to head towards commercialization. I really want to go to a 100-megawatt reactor within two to three years, which is really compressing the time scale, but it may be possible. So the idea is to scale it up. Now I wanted to scale it up, but other people want me to have it so it can sit there and, for instance, unplug this electric heater and it stays hot— self-sustaining heat or, as Gene Mallove says, "Life [sic] after death" [heat after death]. It will stay hot without any heat input from the outside.

Well, I'm trying to achieve both a scale-up and self-sustaining heating by bringing it up to a larger scale. This one has 40 grams of catalyst in it. This is a much larger vessel, this happens to be a modified stainless steel dewar, which is an insulating vessel. In this I will have one kilogram of catalyst, which is 25 times as much as in here. But the heat loss is not 25 times as much as the bigger vessel. The heat loss is maybe three or four times what the smaller vessel has. So if I had three or four times this heat loss and 25 times the heat generation, then presumably this one might self-sustain.

Maybe I'll get 250, approximately 250 watts of heat output from the catalyst inside this larger vessel. So this is a model scale up of the same reaction in this flask. The stainless dewar is as it came from a cryogenics apparatus. This is the cover and these are steam tubes. This is a heating device. The heat comes into this immersion heater, which is transferred to this aluminum fillet, which is transferred through this inner tube. I call this a "hot finger," the heat is being transferred into the hot finger and then it goes into the deuterium gas. If necessary, I will take some heat out using the steam tubes. There's a pressure gauge here and a gas inlet and outlet. I have two thermowells. I can use a thermocouple and stick it into either of these two thermo-wells. One of the thermo-wells is dipping into the catalyst layer, the other is out in the gas phase. However, it isn't that easily constructed. Inside there are some tricks to the way it's been defined and the way it's going to run. But the hope is that this, which will be run within a few days—I finally got it ready to go, work in progress, you know. Within a few days it may reach self-sustaining heating. And then, of course, the idea is: OK, so this is 250 watts, now let's go to 5 kilowatts. Once I go to 5 kW then I'm going to ask someone for some money to design 5 megawatts, or something of the sort.

It is critical the way you have the gas in contact with the catalyst, that's clear. That's been shown by the previous experimenters. With careful scale-up and changing the way the thing is done there's no reason why it can't go to 25 megawatts and 100 and then maybe 1,000 megawatts. I'm going to stop there. A thousand megawatts—that's big enough.

Implications

There are very many implications of this for society. One of them is that there's enough deuterium in the oceans to satisfy all the world's energy needs for a hundred million years. So there's more potential energy in the deuterium in the oceans than there is in all the fossil fuels combined by a factor of, what, a million or something, maybe ten million. But that isn't all. It isn't just that there's an unlimited supply of future energy. This is very cheap energy, because deuterium from the oceans compared to the amount of energy it produces is very, very cheap. The fuel cost is very much lower than fossil fuel. Deuterium as a fuel is surprisingly much cheaper than coal, and this is a big shock to people to contemplate an energy source much, much cheaper than coal. As a matter of fact, it may be more than two orders-of-magnitude cheaper than coal.

That isn't the end of it. The byproduct or, rather the product, of this reaction is helium-4, that's pretty clear. Helium-4 is totally inactive and benign. If you want to you can vent it to the atmosphere. It doesn't make a bit of difference. So this has the promise of getting rid of the greenhouse effect [threat]. When you substitute deuterium fusion for fossil fuel combustion, you start cutting down to the extent that you do that substitution. You cut down on air pollution, you cut down on the greenhouse effect, you cut down global warming. So, ultimately, in ten years or so, we will have totally defeated the greenhouse effect and global warming and air pollution—all at the same time. The public needs to really understand that. It's critical to develop this as quickly as possible to cut down on these horrendous problems of global warming, the greenhouse effect, and air pollution.

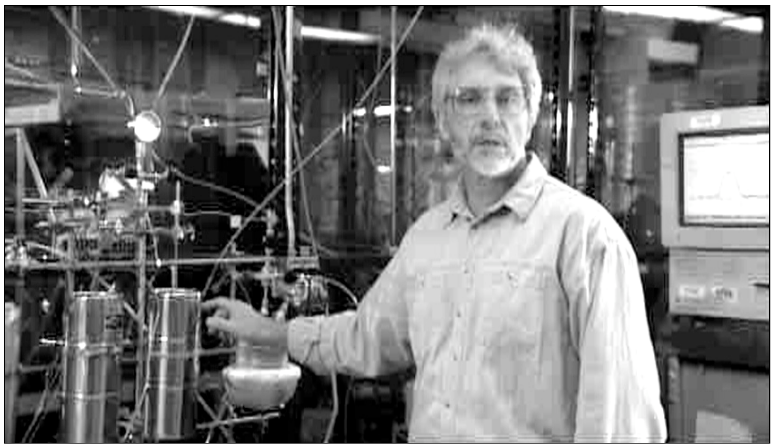
Dispersed Power Generation

It is going to be possible, I believe, to design a passive nonmoving source to maybe 5 kilowatts or 10 kilowatts, using the technology represented by this, assuming that it works. But it's not going to be possible to scale up to megawatts. It's going to be possible to go to a few kilowatts. Now a few kilowatts is sufficient for a house, and it would make steam and electricity at the same time using a small co-generation unit, or it could be made slightly larger for an apartment or for a location such as a mountain top villa or something of that order. But I cannot conceive of scaling this up, this type of technology, to megawatts. So there will have to be a fundamental redesign of the reactor. I have some strong ideas on how that should be done. Also, you are going to have to change the catalyst. This depends on palladium or platinum metal. There is a very definite limitation on the amount of palladium and platinum metal that's available for the world. If you were to use palladium catalysts of the type that's now in sight to built a 100 megawatt plant as a small commercial-sized power plant, you need something like 5% of the world's palladium supply in one power plant. You can't build very many power plants a year without severely impacting the palladium market. So there will have to be a change of the catalyst.

I have some far-distant ideas on that. So there will have to be a way to use titanium or nickel or some other metal—a non-platinum group metal as the catalyst—as one scales up and goes

commercial. That may take some years, but that clearly is the way for the future.

This is the key to the whole thing. I discovered that using certain standard commercial catalysts, one could get this fusion to occur under reproducible, mild conditions. This is the catalyst that I've set upon as being about the most effective that I currently have available. This is a standard palladium on activated carbon catalyst. One-half percent by weight of palladium loaded on this activated carbon—this is the key. You change this just a little bit and it doesn't work—at all! But if you stay within the approved ranges, it works basically all the time. This is my contribution to find that that specific catalyst, within a certain limited range, operates under these standard conditions.



Dr. Michael McKubre in his lab at SRI International with catalytic fusion experiments. Photo: Free Spirit Productions

EDITOR's NOTE: In the course of video-taping our forthcoming documentary about cold fusion (*Cold Fusion: Fire from Water*), our video team visited Dr. Michael McKubre this fall in his laboratory at SRI International in Menlo Park, California. These are some of his comments about the status of his group's experiments to verify the work of Dr. Les Case in the United States and Drs. Arata-and Zhang in Japan (see *IE* Issue No. 18 for Mike Carrell's summary of the latter). Though understated and cautious, as befits one of the field's foremost scientists, it is clear from what Dr. Mckubre says that much progress is being made.—*EFM*

COMMENTS BY DR. MICHAEL MCKUBRE

The experimental apparatus here is really set up to see whether or not helium can be produced by exposing a carbon catalyst with palladium to deuterium at slightly elevated temperatures and slightly elevated pressures.

This experiment very much follows along the thought process of Les Case and behind me you see five different sets of apparatus. The big vessel here is one of Les Case's, he calls them "footballs," it's a stainless steel vessel—on a heating mantle set up in exactly the arrangement that Les Case himself is doing in New Hampshire.

What we have behind me are four different generations of the Case experiment. There's the original Case experiment in this "football," as he describes it—a cylindrical stainless steel vessel on a heating mantle, a very simple experiment in which you simply put deuterium gas in and monitor for helium production. The first attempt that we had at SRI was formed in these vessels we called "Vessel 1" and "Vessel 2," slightly more sophisticated vessels which you can't see. They are concealed in the stainless steel dewars for heat retention purposes. Originally we had Vessel 1 filled with hydrogen and Vessel 2 filled with deuterium, so we could see whether the helium we were observing was present in the deuterium cell or the hydrogen cell. As it happened this cell Vessel 2 produced something like 11 ppm of helium. Vessel 1 at no stage produced any helium, suggesting that our helium determination process and our leak-tightness was, in fact, satisfactory for this experiment.

The original experiment in Vessel 2, as I said, produced 11 ppm helium. The air that we are breathing in this laboratory now is 5.22 ppm helium, so there is very little opportunity for error. The helium in the vessel, apparently, was produced by some source within the vessel and did not come from the air that we're breathing.

We're running now a second generation of this experiment in these two vessels. It's early stages yet, but we're in the hopeful that we'll be able to reproduce our own result which was, of course, a replication of Les Case's result.

This is a more sophisticated experiment. The question is, does the movement of the deuterium gas play any role in the production of helium. Is convection an issue? Is temperature gradient an issue? In this experiment, which, again, is concealed inside this dewar flask and non-observable, we're simply recirculating deuterium gas over a bed of Les Case's catalyst in a continuous manner and sampling periodically for helium in the deuterium gas. Behind the bullet-proof [transparent] polycarbonate wall here is a high pressure experiment, and this is our most recent attempt to see what the parameter space is for the production of helium from deuterium and carbon catalyst. What is the pressure effect? What is the temperature effect?

Les Case has already explored the temperature dependence somewhat. He finds that the effect occurs in a range of 170°C up to about 270 °C. We have not explored the temperature domain, and until we get a lot more apparatus we won't do so. But we are able to explore the pressure domain somewhat better than Les Case is able to do because we have somewhat more sophisticated apparatus.

In the vessel on the floor, we have a high pressure deuterium gas at intermediate temperature about 200°C. This experiment, in fact, just started about two days ago. We have no reason to expect helium production as yet, and the analysis reveals none so far.

All of these experiments are connected to a common gas manifold. What we are able to do is take a sample of the gas from each of these cells periodically. Initially we did it daily, but now we are doing it every two days, in fact three times a week, so we submit a sample of gas from each of the cells for analysis to the mass spectrometer, a high-resolving, low-mass mass spectrometer. We're capable of separating the two masses of species, deuterium D_2 and helium-4. The sole purpose of this experiment, the sole purpose of this apparatus, is to measure helium-4 in the presence of deuterium D_2.

On the monitor you see displayed, in fact, the mass spectrum from one of these samples. This is a relatively high level of helium-4. The peak here is the helium-4 peak, the deuterium peak would normally appear here; it's completely absent. This particular example shows 10.5 ppm helium. We compare the samples each day that we perform the analysis, we compare the samples of gas from the various active cells and blanks with a sample of room air, which we have measured many, many times and know to be 5.22 ppm. And we have some standards, which we typically use—that is, gas samples of helium in deuterium and argon which we submit to the mass spectrometer for the purpose of calibration.

The mass spectrometer simply sweeps a mass from low mass to high mass, in this case from 3.96 mass units to 4.06 mass units, which encompasses the range in which helium is to be found. In fact, this peak is helium, and deuterium D_2 is to be

found which will be found somewhere in this region. We use a liquid nitrogen cooled carbon trap in order to remove D_2 so that we're able to see quite low levels of helium. We're accurate to probably 0.1 ppm helium and we can clearly resolve the presence of deuterium D_2 and helium-4. This spectrum is, in fact, the sum of a number of spectra that the mass spectrometer simply sweeps for the period of time that we pre-program, and this is the cumulative signal representing the integral of all helium which was present in the sample when we submitted it for analysis. To acquire this spectrum takes us about five minutes.

It's clearly not possible to produce helium from a chemical process. If we observe helium in our experiments it's either because it leaked in from the atmosphere—we can rule that out by the blanks that we do and the fact that the helium signal that we have seen is larger than the helium in the ambient. It's possible that the helium pre-existed in the sample and was simply released to the gas phase with long term exposure. We can rule that out largely because we've analyzed the catalyst that we're using and found that it contains no measurable levels of helium.

The only possibility that remains, and remains to be checked, is that the helium is produced by a nuclear process. If the helium is produced by a nuclear process, then necessarily there will be an associated release of heat. Although these experiments were not initially set up to be rigorous calorimeters, we have monitored them with a sufficient number of temperature sensors that we can know, to some degree with some confidence, whether or not heat is being produced and at what time heat is being produced.

From the best of my ability to analyze the thermal record, it appears that, yes indeed, in the vessel that was producing helium there was some evidence of excess heat and that the amount of heat produced was approximately quantitatively correlated, that is, the right amount of heat was produced compared to that of a nuclear process involving deuteron-plus-deuteron producing one helium-4 nucleus which releases 23.8 meV.

I'd like to re-state that the calorimetry was largely retrospective, this experiment was not set up as a calorimeter and, therefore, the calorimetry is not rigorous, but the temperature record quite clearly indicates in these experiments, as it does in Les Case's experiments, that there is an unexplained source of heat and the magnitude of that source of heat is approximately the right value to account for the observed helium.

Part of this generation of experiments is to improve the calorimetry and the central question in the cold fusion field is: "Is there excess heat?" If "Yes," then, "Is that heat the result of a nuclear process?" So the central question that we're all seeking to answer is: "Is there a quantitative and temporal— is there a quantity-related and time-related correlation between the appearance of anomalous excess heat and the appearance of the product of a nuclear reaction such as helium-4?"

So the thrust of our work is very much to find the heat and quantify it accurately and find the nuclear process and quantify it accurately so we can correlate the appearance of these two products.

View inside dewar showing Case catalytic fusion cell mounted on resistance heater. NERL, Bow, NH.Photo: Ed Wall.

We have determined that there is excess heat and we have to do a better job of measuring it with accuracy. This laboratory here is really set up to do highly accurate calorimetry. That work has largely been associated with the electrochemical experiments, such as Arata's experiments and our own experiments. So we are quite capable and willing to do the calorimetry. We just haven't applied those skills fully yet to the Case experiment, but this is obviously our plan.

One of the difficulties in the cold fusion field is the apparent lack of replicability of experiments: many people performing the same experiment get apparently different results; different experiments performed in the same laboratory give apparently different results. So it's obvious that if you do the same thing you must always get the same result. What this is telling us is that there are some important parameters of our experiments that are not under our control. Some of them I know and understand, and still [we] can't control some of these parameters we don't know about yet. We just don't know what the process is that we are studying, so we don't know what parameters we need to control in order to yield a consistent result.

An experiment which always gives the same result—can be performed in several different laboratories to yield the same result—would be very valuable to us, in part in helping to convince the remaining skeptical scientists in the world that there is a phenomenon to observe. But, in fact, in order to use the scientific method to observe scientific results, we have to be able to reproduce the results of our own experiments so that we can see what the effects of small changes are on these experiments.

The Arata-Zhang Experiment

One experiment which has been reported to produce consistent and reproducible results is that of Professors Arata and Zhang, both of them are very, very experienced and very well recognized scientists in Japan. They performed a very careful experiment, reproduced it apparently a number of times in their own laboratory—producing both anomalous excess heat in fairly significant levels and helium-4 and, perhaps more interestingly, helium-3. The helium-3 to helium-4 ratio that they observed in their experiments is different from that in the air that we're breathing. [Editor's Note: This isotope ratio is off by a *huge* factor—see the Carrell review in *IE* Issue No. 18.—EFM]. Sufficiently different to indicate that there is clearly an anomalous nuclear reaction occurring. The difficulty only with Arata and Zhang's experiment is that it's only been performed by them and only in their laboratory. What we're attempting to do here is to produce their same results with their apparatus and with their help. This is a collaborative effort between Arata and Zhang and the SRI group, to produce in *our* laboratory the same results as they have obtained repeatedly over the years, which would indicate that we have some degree of mastery over the experiment.

The experiment that we have running here, in fact, is relatively young; it hasn't been operating for very long. One of the difficulties with Arata's experiment is that it requires many, many months to produce a

result, and quite literally we're not very experienced with Arata's methods, so we've had some difficulty getting his experiment set up and operational. Certainly, it's caused me to have an increased level of respect for Arata and Zhang's technical competence. They are very, very good scientists. Within a month or two, we hope to have reproduced their experiment faithfully and reproduced their result. And the benefit will be in part sociological. We will demonstrate that an experiment can be transported from laboratory to laboratory and yield the same result. It will also give us something that we can do again ourselves and define somewhat the parameter space in which these experiments yield excess heat and, apparently, helium-3 and helium-4.

I don't know that Arata and Zhang have monitored their experiments for neutrons. We routinely monitor in this laboratory for neutrons at the radiation hazard level. We have a continuously operated neutron detector for personnel hazards. Clearly, this has not alarmed at any time or I would not be standing here right now. Whenever we've made attempts to look for neutrons in active heat-producing experiments, we have not observed neutrons above background level. That indicates simply that the neutrons, if they are produced, are not produced quantitatively with the heat in the same way that a hot fusion process occurs, but we've never had very sophisticated neutron detection applied to a calorimetric experiment producing large levels of excess heat. The problem is a very simple one, the criterion, the conditions necessary to do a first class calorimetric experiment of an electrochemical process—these conditions are incompatible with those necessary to do a high quality neutron determination. So you either optimize your experiment for the eletrochemistry and calorimetry or you optimize it for the neutron measurement; you can't do both.

From the best of my ability to analyze the thermal record, it appears that, yes indeed, in the vessel that was producing helium there was some evidence of excess heat and that the amount of heat produced was approximately quantitatively correlated, that is, the right amount of heat was produced compared to that of a nuclear process involving deuteron-plus-deuteron producing one helium-4 nucleus which releases 23.8 meV.

In fact, behind me this large black box is a neutron spectrometer designed for us by [the late] Kevin Wolf who, in my view, is the most able nuclear experimentalist that I have ever met. A first class man with a first class talent for low level nuclear determination. It's a beautiful neutron spectrometer and cost us, or EPRI, perhaps $3,000. We've never used it with serious intent because we've never had an experiment which would make it worth our while mounting and manning this apparatus. The neutrons that are present in these experiments, if they are present at all, are present at very low levels. Levels so low that they can't be directly connected to the heat producing process. They may be indirectly connected, but they are not present in large quantities and therefore they are not very interesting to me.

Advantages of the Case Technology

The Case device is attractive for several reasons. It's simply deuterium gas and carbon catalyst—commercial catalyst—something that can be obtained in 55 gallon drums, and the vagaries of the manufacturing process have already been mastered. So that if the Case experiment works to produce heat by a nuclear process, then it's something that can be very easily scaled up. Most of the work that's been done in this laboratory has been done on electrochemical systems which are very sensitive to handling issues, the metallurgy of the palladium, the purity of the electrolyte, and really only people that have been trained for many, many years in electrochemistry are able to perform electrochemical experiments satisfactorily.

In Case's experiment, you have a gas, an easily accessible temperature, modest pressure in a sealed vessel. This is an experiment which many people can do and facilities exist to perform the experiment and understand its sensitivity to the various parameters and it's easily amenable to engineering scale-up.

The big question, of course, if we do have a heat-producing system, if that system requires significant quantities of palladium then its application is necessarily limited. Palladium is a precious metal. In fact, its a by-product of the platinum metals industry. But if palladium were to have a use all on its own, it's price would go up dramatically. It's availability is scarce so that a commercial system based on Case's concept would require a metal other than palladium or a very efficient way of recycling the palladium. We don't know as yet whether other metals produce the same effect in terms of the helium production. Les Case has studied several of the platinum group metals— palladium, ruthenium and the like, platinum and osmium, and has found that the effect is present with most, if not all, of the platinum group metals. This doesn't help much, because they are all precious, so we really need to find non-precious, non-platinum group metal which produces this effect.

My own view is the attempt to scale up is premature. We need to understand the mechanism, the process that we're studying. Once we understand what the mechanism is we will understand what metals or alloys might be satisfactorily used and perhaps optimized; maybe we'll get a larger effect. And only then can we explore the engineering applications.

This is the first Case vessel which we, in fact, obtained from Les Case in exactly the form in which he is performing his experiments in New Hampshire. This vessel, which we call "Vessel 2" and its twin experiment "Vessel 1" are our attempts to do Case's experiment in a similar geometry but a slightly more sophisticated apparatus.

The experiment over here on the left is an attempt to explore whether the convection of gas, that is the recirculation of gas, affects the rate of helium production.

The important parameters of all of these experiments are being recorded by a computerized data acquisition system are displayed on this screen. . . This is an indicator of one of the temperatures being recorded but, in fact, we are recording anything up to 10 or 12 different temperatures in any set of experiments. All of these signals are recorded by computer and we are displaying the most important of them on this computer monitor, that is, the current, voltages and, therefore, powers going into each experiment; the temperatures and the pressures are recorded in each of these experiments. In this present configuration, we are making a measurement every five minutes and recording it to file so that we can analyze it off line to see, for example, whether there is any excess heat, pressure anomalies, leakage and the like.

In an experiment where we are interested in measuring the presence of excess power, we obviously have to record power very accurately and what we use is a Hewlett Packard computer-controlled power supply. Each one of these slots has a different power supply, all of them commanded by the computer to produce either constant current or a constant voltage displayed by the displays here but also recorded by the computer. It's a very stable power supply, very accurate and very constant.