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Interview with Charles Beaudette and Michael McKubre

The show: RADIO WEST, by Douglas Fubbrezio, on Wednesdays, 11:00 a.m. to 12:00 p.m.

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Doug Fubbrezio: I'm Doug Fubbrezio.

The claims of Stanley Pons and Martin Fleischmann in 1989 were regarded as some of the most significant in science in the century. The two scientists told the world they had achieved nuclear fusion at room temperatures. The implications were mind boggling. If it were true, it meant a potentially limitless source of energy had been discovered. It was only a matter of months before the scientific community declared cold fusion dead.

Almost ever since, controversy, even hostility, has hung over the matter. Now, thirteen years later, other scientists are carefully revealing interest in cold fusion and, in fact, in results that show there was something to the experiments of Pons and Fleischmann.

Today on the program we will talk about the current sense of cold fusion. What kinds of experiments are being conducted today, and how the intangibles of politics and jealousy affected the process. Cold fusion, 13 years later.

After this news . . .

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DF: From KUER News in Salt Lake City, this is Radio West, I'm Doug Fubbrezio.

Thirteen years after the claims of Martin Fleischmann and Stanley Pons were dismissed, even ridiculed, by the mainstream scientific community, there seems to be a sort of subtle kind of thaw occurring. The field, even the term, cold fusion, was regarded with such hostility for so long that it took more than a decade for scientists still interested in the results of Pons and Fleischmann's experiments to poke their heads out. It isn't right to say right now that Pons and Fleischmann have been vindicated at all. In fact, the skepticism that they had achieved nuclear fusion on a tabletop at room temperatures is still intensely felt. But there are still experiments being conducted. People trying to explain aspects of the Pons and Fleischmann claims.

Today on the program we're talking about cold fusion. It may not be the term we use throughout the program, but it's the best term to introduce the topic certainly right now. What is the current sense of science on the matter? And what happened in 1989? How did the scientific community react? And why? And what are the implications for the scientific method and real inquiry about fusion?

You can join the conversation 585-WEST is our number, 585-9378. Or you can send us an email. Our address is: radiowest@kuer.org.

Joining us now by phone is Charles Beaudette. He is the author of the book "Excess Heat: Why Cold Fusion Research Prevailed" and he joins us on the line now. Mr. Beaudette, welcome and thanks for joining us.

CB: Good morning, Doug, glad to be here.

DF: Also, with us is Dr. Michael McKubre who is a scientist with SRI International Laboratory in Menlo Park, California. The Stanford Research Institute, Dr. McKubre, is that right?

MM: Good morning, Doug, nice to be here. Formerly Stanford Research Institute.

DF: Institute, great. Thanks for joining us.

I want to start with you, Charles Beaudette. Let's begin, if we could, with the relationship between Martin Fleischmann and Stanley Pons. Introduce us to these two as players in the story, if you would first.

CB: Yes, well, of course. As you might expect, they are very interesting people when you get in close to look at them. Martin Fleischmann was an intensely brilliant person and, as such, very much an individualist. He graduated from Imperial College in London in 1950. Went to what is now the University at Newcastle in England. He made quite a mark at modernizing the science of electrochemistry - that's really the study of how batteries are made - because running a current through water is a common industrial process and has many uses. So it's a very important field.

He was then offered the Faraday Chair at the University of Southampton and built that into an internationally recognized department of chemistry and electrochemistry. In fact, Michael McKubre, my partner here on this program, was one of his students there.

Stanley Pons is an independently wealthy person who went through school and went into the family business. After about 8 years, he decided he wanted something more intellectually demanding and he went to the University of Southampton where Fleischmann was and got his Ph.D. there. He then took a couple of academic jumps before landing at the University of Utah in Salt Lake City.

DF: In the book, you note that it was in fact, Martin Fleischmann's notion at least initially the idea that you might be able to get a nuclear fusion reaction if you packed deuterium gas into a metal, into Palladium. I know I'm far off on the science, and I won't even try to get close, but you tell this story of Fleischmann and Pons working out this notion on a hike up Millcreek Canyon one-day.

CB: Oh, yes. That would have been in 1984. Fleischmann by then had retired from Southampton and he was visiting Pons in Salt Lake City. They had a lot of common interests. They cooked and skied and so forth, and he would stay at Pons' house when he was there. Fleischmann had been following this for some time. It was known from a paper in 1929 that when hydrogen goes into Palladium the electron gets stripped away and the nucleus starts floating about with the positive charge. He thought that was pretty fascinating. He started to accumulate the equipment to do this experiment that you mentioned in 1972 or so, but just never did get around to it. So here in retirement and - he was appointed a research professor at the University - as a guest, he and Pons had the chance to try this. So they did. Now, their theory of what might happen and why was all wet, very much like Columbus going west to find India that was all wet too. But still great discoveries come out of these things.

DF: Dr. McKubre, I wanted to ask you, were you ever, at least early on in the 1980's, were you at all following what was going on. I guess later in the 80's. Were you following what was going on with Pons and Fleischmann with much interest?

MM: Well, I was not aware at all of Fleischmann's interest in the conjecture of fusion in the Palladium lattice until everybody else in the world was on March 23, 1989. I have known Martin and Stan for a long time back and, as my partner Charles Beaudette said, I worked at Southampton. I did a post doctoral research fellowship at Southampton University, which under Fleischmann's guidance was probably the preeminent school of electrochemistry in the world at that time. So I've known them, admired them, Martin Fleischmann is certainly the most brilliant experimentalist I've ever been associated with. I followed his career with interest in an overview sense, but I had no idea at all that they were conjecturing fusion in the Palladium lattice.

CB: Well, Douglas?

DF: Yes, please.

CB: You want to keep in mind that they appreciated how wild this idea was and they kept it a deep, dark secret. It was only after they had to bring the management of the University of Utah into it in, really, about February of 1989 that the word started to get out. Then, when the word started to get out, they felt they had to hold a press conference and tell everyone, or it would have been a terrible mess of endless rumor mongering and ridiculing.

DF: Maybe if you could talk a little bit about how this sort of evolved for Pons and Fleischmann? Was there one kind of eureka moment? Certainly you talk about and maybe you could tell the story of - let's call it the melt-down.

CB: Yes. I do have an eyewitness to that.

They had one of their very first experiments set up in Room 1113 of the North Henry Eyring Building on the campus there at the University of Utah. They left it overnight and they came in in the morning and it was mess. My eyewitness says that there was a 14 inch hole in the laboratory bench, there was a lot of particulate matter in the air, and Pons and Fleischmann had a funny look on their face like the cat that just ate the canary. They were really rather pleased with what had happened.

DF: What had happened? Do we know?

CB: Well, apparently a lot of the Palladium in the experiment evaporated, some of it fused, so they have some idea of the very intense temperatures that were reached but, generally, the experiment was well destroyed. It was an electrolytic experiment with a cube of Palladium in heavy water. As some of your listeners may know, hydrogen is the lightest element and heavy hydrogen has one extra neutron in it so its twice as heavy as normal hydrogen. When you make water from it, it is called heavy water. You can buy heavy water today by the tank truck full if you want to. It's quite available. So they had heavy water with two metal electrodes, one of them

Palladium. The Palladium started apparently to generate enormous amounts of heat and ended up evaporating the experiment. That would have been about in February of 1984 [correction: February 1985], from what I've been able to learn.

DF: And this told them what? Why were they grinning?

CB: Well, this told them they were having some kind of a tremendous reaction because they could do all the calculations of what happens when you release hydrogen from Palladium. They knew that they had exceeded that kind of energy by an enormous factor. So they knew they had something new going. They also apparently did some kind of scanning to see if there was a high level of radioactivity. There was not. They learned as the years went by that they had a real source of heat - because they became very good at measuring heat - and that it was without dangerous radiation. So they had something pretty interesting in hand.

DF: Take us then to March of 1989, you referred to it, they obviously, they announced to the world their experiments. Put it in context. How significant was this discovery?

CB: Well, it was quite significant.

They had three revolutionary things to say all at once. And that's pretty awful because you can't really expect anyone to accept it. They were first of all saying that they did get continuous Deuterium/Deuterium fusion in their tabletop electrolytic cell, as we call it. They also announced that they were getting enormous levels of heat out of it compared with any possible chemistry reaction. That was inexplicable by modern science.

They said there was no dangerous radiation with it. That moved the whole thing, for much of their audience, into a fairyland of wishful thinking. They [the audience] just wouldn't believe that such a nice thing could be possible. So they [the two chemists] just opened it up to a huge attitude of skepticism, but they really had to get it out.

You can imagine what would happen if they did not hold the conference and it [the story of their claims] just ran through the university, and from university to university as an endless whispering campaign. They couldn't accept that. They had to get out and say what they were doing publicly - and take the consequences.

DF: Dr. McKubre, how did you react? You heard about it like the rest of us, I guess? How were you reacting at the time? Do you remember?

MM: I was in a particular position. I was already working on the Deuterium/Palladium and Deuterium/Hydrogen system. We'd invented a hydrogen sensor for use in nuclear power systems about ten years earlier. I had been continuously working on exactly the experiment that Martin Fleischmann was talking about for an entirely different reason. And with no evidence at all of a nuclear effect and no expectation of a nuclear effect. So I was at once in a position to be more skeptical than the average person because I thought I knew the Palladium/Deuterium system fairly well. I did not expect that there would be any reason for a nuclear effect. I think if anybody except Martin Fleischmann had made this announcement, I would have rejected it out of hand. The fact that it was Fleischmann . . .

I knew him to be far more imaginative, inventive, and capable than anybody else that I was familiar with at the time. I thought there was probably a 50/50 chance that it was right, just because it was Martin. We co-opted with sponsors permission 30,000 USD of our EPRI (Electric Power Research Institute) contract was contracting us to develop this hydrogen probe. I asked my project monitor if I could use 30,000 USD of the funding for that project and take three months to investigate whether or not this claim was real.

DF: Because if it were real, what would it mean?

MM: The reality of such a claim is really extraordinary.

In essence, if we can convert Deuterium to Helium 4 without emission of dangerous radiation, then we have a solution to the energy problem that mankind inevitably will face in 20 years or so when our main sources of energy run dry. So we have the solution, potential solution, to the greatest problem that mankind faces in the future. I don't think there's any experimental claim made in my lifetime that has more significance than the one that Fleischmann made.

DF: When we come back from a break, we're going to talk about how the scientific community did react to the claims of Stanley Pons and Martin Fleischmann.

You can join the conversation 585-WEST is our number, 585-9378 or email address radiowest@kuer.org. With us Charles Beaudette, the author of "Excess Heat: Why Cold Fusion Research Prevailed" and Dr. Michael McKubre, a scientist from SRI International Laboratory in Menlo Park, California. We'll take a brief break. We'll be back in just a moment. This is Radio West, on FM 90.

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DF: This is Radio West, I'm Doug Fubbrezio.

Today on the program, we're talking about cold fusion. Thirteen years ago, scientists in Utah announced to the world the astonishing results of a series of experiments. They reported they seemed to be producing heat, energy. Eventually, the experiments were dismissed as bad science; some regarded them as a joke. The fact is cold fusion is far from dead really. Laboratories around the world, even here in the United States, continue to test the idea. What happened then, what's happening now?

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Charles Beaudette is with us, he is the author of "Excess Heat: Why Cold Fusion Research Prevailed". Also with us is Dr. Michael McKubre, a scientist from the SRI International Laboratory in Menlo Park, California, a colleague of Martin Fleischmann and Stanley Pons.

I want to ask Charles Beaudette, What was the crucial mistake that Martin Fleischmann and Stanley Pons made? You said they didn't announce this information too soon. But they didn't exactly get it right some would say.

CB: The announcement was, you might say, rather a mess. It was not well planned. In fact, the two were supposed to speak extemporaneously, if you can believe such a thing, for an announcement of this magnitude. Pons does very poorly at that while Fleischmann does very well at it. So that was very poor. Probably, if they made a mistake it was trying to take nuclear measurements at the last minute. This was not their field and they made a mess of it. They botched that part.

At the same time, the physicist tended to take over the task of evaluating the announcement as opposed to say, chemists doing that. And that [the nuclear measurements] was the weak part of what had been presented. The physicists would really have no part of calorimetric measurements of the heat, and they said so. They said this whole announcement will not be resolved by calorimetric measurements and, in fact, *Nature* magazine said that a few months later. So they honed in entirely on nuclear information and there was very little of that. So the whole thing sort of spiraled downwards from there. In other words, there was no concerted effort on the part of the scientific community to do proper, careful, calorimetric measurements of the experiment that they had mentioned.

DF: A lot of what you talk about in the book is how the, I guess, scientific method was applied to the claims of Pons and Fleischmann. What are the lessons of that? How is it that such, I guess, hostility came to be associated with this story?

CB: Well, first of all, physicist have told me that, in their field, it's pretty rough. If you make a mistake on presenting some physics, you tend to get chewed up. That's very much how physicists work.

Its not how the other disciplines work. But also, the world of physics, especially nuclear physics, is becoming enormously theoretical. I am not sure they're prepared to recognize an empirical science, and the excess heat is strictly an empirical science. You go out and you take measurements. Its just like Pierre Curie back in 1903, he had this radium material. He said it can melt its own weight in ice every hour without changing at all. This was a big discovery.

Today, you present this calorimetric evidence to the physicists (and who are the outspoken people in this matter) and they will not go into the laboratory. They will not talk about it. They're not interested in that [calorimetry]. All they want is nuclear information. So it's rather an impasse.

DF: Dr. McKubre, you talked about in March of 1989. I guess scientists from throughout the world sort of dropped everything, many did anyway. You, yourself, were able to come up with, what was it, 30,000 dollars?. What did you come up with? What did you find?

MM: I didn't know that whether this was a good thing or bad thing in retrospect. But our first experiment produced fairly clear evidence of more heat being produced by the experiment than the amount of electrical power that we were putting into it. But we knew at that time that we were not calorimetrist. Our expertise here at SRI, at that point, was not measuring heat. We got a positive indication in an experiment that we weren't very comfortable with. We simply submitted another proposal to the same funding agency for a much more extensive campaign to design a proper calorimeter, a stable, long-term, accurate measuring instrument that we could be much more comfortable with the results. We didn't publish the first results; we just used it as information. The information, is on the surface of it, that the claim made by Fleischmann and Pons is not outrageous and may even be true. By the time we had completed fifty one experiments covering two more years, we were certain that there was an unaccounted for heat source in the Deuterium/Palladium system.

If you electrochemically load Deuterium into Palladium more or less with the means that Fleischmann and Pons had described, we had convinced ourselves we understood the circumstances under which it [excess heat] occurred. There is undoubtedly more heat being produced in these experiments than you can explain by chemistry.

If I can just clarify one of the points that you were speaking about before. This question of heat may be difficult for some of your listeners to comprehend. The power of a nuclear reaction is approximately a million times the power of a chemical reaction. So simply by measuring the amount of heat produced, you can quite clearly distinguish between a nuclear effect and a chemical effect.

This was something that the physics community really didn't want to accept, in part because they did not want to accept the possibility that there was another pathway to fusion that they had themselves not been investigating, and 1989 was a particularly difficult time for the hot fusion community. They were under investigation. Questions were being asked why all the money had been spent and why so little progress had been made. Funding was being cut. The last thing that community wanted was the suggestion that there's a much simpler and cheaper way to achieve the same result.

A lot of the hostility and a lot of the fire that was directed to Fleischmann and Pons was not directed with scientific purpose, it was directed with the cynical purpose of getting the cold fusion clash off the newspapers, out of the media, and away from public attention so congress would not be distracted by this possibility.

DF: Is it too simple minded to say that politics had a great deal to do with sinking, I guess, the progress of Pons and Fleischmann. Is that simple minded?

MM: You can't stop reality from eventually surfacing. The reality of the field is that the claims that Fleischmann and Pons made with respect to heat are largely sound. They've been largely verified. The effect when all the shaking is over and all of the results are analyzed and summed, it will turn out to be what we are observing as cold fusion. It's fusion in a sense of ambient temperature environment, so even the name was right and the experiment was right. The methods of presentation and all of the data presented was certainly questionable, but the overall claim is a sound claim. And the political action has really been a stalling action. It's to defer the day upon which a judgment is made.

CB: Doug.

DF: Please, Charles Beaudette.

CB: One of the milestones you can look at. The *New York Times* ran an anniversary article on the 10th anniversary of the announcement. There they had Mike making a simple scientific statement. He said, in the literature you do have this excess heat, it has been verified and is virtually without challenge. Opposite that, they [the NYT] had a statement by Dr. John Heizenga, where he said, the field is dead and it always will be dead. Well, that's not really a scientific statement. And it really doesn't address what's in the literature. So there's sort of a milestone of the nature of the clash at the 10-year mark.

DF: Is it a crucial sticking point? It seems to be clear to many, anyway, that there is excess heat. Is the question then whether this heat was produced from a nuclear reaction?

CB: Not really. I think the big argument that the physicist have is that you . . . I get the impression that their position is that mankind does not know how to measure heat. That's really what comes out of my discussions with the critical physicists. They simply will not accept it. They will not go into the laboratory to help Mike take his data down and to help him calibrate his system and see [for themselves] what's going on.

The outspoken physicists who have continually berated this field will not go into the laboratory. It is very reminiscent of Galileo's problem in 1610 when his associates at Padua would not bend over to look through his telescope to see what he was seeing. It's very similar to that in my mind.

DF: Dr. McKubre, talk about that. It must be frustrating, I suppose?

MM: Yes, it is very frustrating.

I agree with Charles that a lot of the criticism is we can't make accurate heat measurements. The criticism, its not a scientific criticism and, in fact, is ludicrous. All of thermodynamics, as we know it, which is the foundation of chemistry and physical chemistry and biology, too, was established by accurate calorimetric measurements. So we can measure heat very accurately. We know how to measure heat. Here at SRI alone we probably spent half a million dollars developing highly accurate, highly sensitive calorimeter that was stable for long periods of time.

So we have measured heat that is ninety times the measurement uncertainty . . . This is 90 sigma. Physicists will accept a nuclear effect if it's three times the measurement uncertainty. Here we have an effect 90 times the measurement uncertainty which is still not accepted. The attention is actually diverted away from the heat observation - which was Fleischmann and Pons claim at the outset - diverted from the heat observation to where are the nuclear products.

You know, if you've got heat and it's a nuclear effect, there must be nuclear products, where are the products. And the last six years of my career here at SRI, I've spent pursuing the nuclear products. It turns out the nuclear products are Helium-4, Helium-3, and Tritium, which are the products of fusion. I use the phrase cold fusion now. I didn't use it two or three years ago because I wasn't certain what the effect was. But I know certainly that we have a nuclear effect that produces heat and fusion products. We're entitled to use the phrase cold fusion.

DF: I must say, I'm a bit surprised that you used the world cold fusion now having read some of the interviews with you a few years ago. You were somewhat uneasy about the term.

MM: I thought it was unreasonable to assert a mechanism when you had no proof of that mechanism. Now we have adequate proof that the mechanism is a fusion reaction. It's probably even more general. I think the fact is - it is an exciting fact - that nuclear effects can be influenced when they occur in a crystalline body. The fact that these nuclear effects occur inside the crystalline Palladium lattice very much changes the rates of the reactions and the distributions of the products.

The nuclear reactions that people are claiming are not restricted to fusion. They encompass a wide range of nuclear effects.

DF: So where did Pons and Fleischmann get it wrong? Were they entirely right or did they . . . had they made some mistakes along the way?

CB: Their theory as to why they should get the reaction was pretty much wrong, as best I understand it. The idea of there being a certain amount of pressure in there. That they would get two particle type reactions of the conventional sort, where two atoms would simply be pushed together very hard. I don't think there's any support now for that approach.

MM: I don't know that the fault was that their theory was too simple. I think that may have been presented in too simple a way. The greatest mistake I think that Martin Fleischmann and Stan Pons made was the assertion that this was an easy experiment to perform, which led individuals with limited electrochemical experience . . . Fleischmann is the master. Fleischmann is one of the top three electrochemist on this planet. If Martin says something is easy, it might be that this was neither easy nor within the compass of most of the people who attempted the experiment.

CB: Yes, I would agree with that entirely.

There was another large confusion factor - in fact I list eleven in my book - but another one was the business of the difficult experiment. In chemistry and in biology everyone knows there are such things as difficult experiments, difficult to do. But if you talk to the nuclear physicists, their idea is that once you have an experiment, anyone can go do it, and it always works and always comes out the same way. Some long conversations have been carried on trying to explain that maybe there is an experiment that takes some learning process. The nuclear physicists don't tend to accept that. So this is another case of a change in scientific protocol from one field to another, you see, and therefore there's a disconnect.

DF: Why are physicists so resistant? Have you been able to answer that question?

CB: Yes, its nuclear physicists. Because their field has matured, as they see it, during the last half century. As a result, its become a little iron-clad. It is completely immersed in theory and simply is not open to accepting empirical results. As I said, just experimental work that conflicts with their theory. Then let them conflict, and let the best man win. They are just not in that mood; not in that way of operating at the present time.

DF: 585-WEST is our number, 585-9378. Chris, joins us now on a cell phone. Chris thanks for waiting.

CHRIS: Oh, no problem.

I just finished a physics degree at the University of Utah this summer. The question I had at the beginning of the program and elsewhere, You said that cold [fusion] research is still going on. I want to know how much is going on? To me that seems like it would be a more accurate statement if we could all find out. How many laboratories?

MM: I keep in touch with most of the workers in the field.

I would say there are fifty established laboratories around the world. We have conferences every year, year and a half, which attract 250 to 300 people. The number of active researchers in the field is probably in excess of 500. There are at least a half dozen major institutions in the United States that are pursuing this field with a multi-man effort. There was initial furor in 1989, thousands, tens of thousands of people got involved perhaps, but only transiently. They got involved for one experiment and if they succeeded in obtaining a positive result, which most didn't, they tended to pursue the field. If they failed, they said well, its wrong. I can't do it, it must be wrong. They got out of the field, but I would say a core of, you know, 500 to 1,000 people pursue this field. I'm in correspondence with at least 100 of them.

DF: Are the laboratories in the United States and how much is it elsewhere?

MM: The U.S. probably has the largest number of people, but perhaps not the largest funding. It is pursued extensively in Japan in universities and in major commercial institutions. A number of them automotive manufacturers, who's cars we drive everyday are pursuing this field in Japan. In Italy it is pursued at the governmental level, and they are funded government research projects where they are equivalent to the Department of Energy laboratories. And the EMEA, where several multi-man groups are pursuing this field actively.

DF: Charles Beaudette, the Department of Energy in 1989 or even later seemed to be fairly dismissive of the idea entirely. Where's the United States government in regards to all of this?

CB: Right now, I think most people in the government feel bound to that [1989 government] report at a public level, any level that could touch upon their funding or the congress. The Naval Research Lab has done quite a bit, but they tend to do it under the radar so that nobody can stand up in congress and say they're wasting money on this foolish enterprise, sort of thing. So the public opprobrium against the field is the tremendous limitation at the present time.

DF: So Dr. McKubre, is that much of an obstacle to the work you're trying to do? Trying to get grants, those kinds of things, acknowledgment from the U.S. government?

MM: We've been continuously funded for the entire time 13-1/2 years. First from the Electric Power Research Institute. Then we had funding from the Japanese government, and now we have funding from the U.S. government. Its not been so much of a problem for us. We have managed to keep mainstream funding. But it has been a problem for others. And it's a problem in the field in the sense that the [scientific] problem is a difficult problem. It requires many more people to investigate the materials aspects of it than presently are.

Without a reliable funding source, people are not going to attempt to do the experiments. Without the possibility of publishing - and there's a fairly strong disincentive to publish - editors are very dubious of anything, because of the form of public hostility. Without some sort of academic pathway to publish then the academic institutions are not strongly involved. So there's a disincentive to move the field forward, a discouragement from the experiments that need to be done. But nevertheless, progress is made.

CB: You know, Doug, there was an article in "Science" at the beginning of the month, beginning of November. It was by an M.I. Hofford, et. al., on, What is the energy source to be used if we have this global warming effect. Of course, this excess heat is exactly the right answer to that question. They don't mention it in the article, of course, because it might not get past the editors. So that is what the opprobrium does. But I think the National Laboratories really are there to take an idea of this sort, and to take the experimental work that's been done, and run with it to see where it goes. And that means to find out what the nuclear reaction is that's feeding it.

DF: But editors at some scientific journals are still not interested, still weary of the idea?

CB: Oh, very much so, and it can still hurt an academic career.

DF: Let's see, Charles Beaudette is with us. He is the author of "Excess Heat: Why Cold Fusion Research Prevailed". Dr. Michael McKubre is also with us, a scientist from SRI International Laboratories in Menlo Park, California.

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DF: This is Radio West. I'm Doug Fubbrezio.

The claims of Stanley Pons and Martin Fleischmann in 1989 were regarded as some of the most significant in science in a century. The two scientists told the world they had achieved nuclear fusion at room temperatures. The implications were mind boggling. But it was only a matter of months before the scientific community declared cold fusion dead.

Now 13 years later, other scientists are carefully revealing an interest in cold fusion and results that show there in fact was something to the experiments of Pons and Fleischmann.

You can join us 585-WEST is our number, 585-9378 or email address radiowest@kuer.org. Dr. Michael McKubre is with us. He's a scientist from SRI International Laboratory in Menlo Park, California and Charles Beaudette is with us. He's the author of "Excess Heat: Why Cold Fusion Research Prevailed".

Let's take a call now from Salt Lake City, Steven, is on the line. Steven, welcome.

STEVEN: Hi. I remember when the dispute or the announcement was first made. There was all of this speculation how you'd be able to buy a home water heater or a power generation unit or some kind of commercial application for this thing. Still haven't seen anything on that yet. I wonder if you could comment on how far away some kind of a product that involves this is. It seems like the kind of taint in the scientific community (or in part of the scientific community) this thing has is going to be pretty rapidly removed if a product comes out.

CB: Well, it would, certainly. If I may speak, Doug?

DF: Please, Charles Beaudette.

CB: But, first of all, you have to get the science down before you get the technology going. There have been some historical cases where people jump-started the field, but usually not. In fact, the money that's been blown away, the 5 million dollars spent out of the government in Utah and then much more spent out of the government in Japan in the mid 90's, all was aimed at a finished product, a power supply or a power source, before the science was found out.

We don't want to continue making that mistake. We have to get the science down. Right now, we just have this empirical result of a chemistry experiment. We need a concerted national effort to find the nuclear reaction that produces the heat. Then you have the science down, then you look at the technology, and you start to build reactors.

STEVEN: I think you mentioned . . . Are they doing this just as basic research or are they looking at products, or do they have some kind of a schedule to produce a product?

CB: I think this is just stay-in-touch. What do you think Mike?

DF: Dr. McKubre?

MM: Both ways.

A major effort was instituted in Japan to basically commercialize, establish the infrastructure for the commercial reality of a cold fusion product, and that a large amount of money was spent in the Federal new hydrogen energy program. A lot of it [was used] to establish the bureaucratic infrastructure for this new technology. They did all of that before the technology was mastered. Very little of the money [was used] in understanding the science. It was only late in that program that they recognized that the science wasn't sufficiently well under control to imagine even what the product might be.

I agree with the listener - the questioner. In that if we had a product that we could put on a tabletop which clearly demonstrated more heat coming out than power going in, then scientists be damned. A product like that could convince the commercial world and the commercial world is much more powerful than the scientific world.

But, a word of caution. I think one of the mistakes that was made early on with the excessive optimism that this quirky electrochemical phenomenon with a nuclear root, which was called cold fusion, is going to solve mankind's energy problems. There was no evidence given at that time that, even if the effect and the claim were real, that a commercial product that would solve mankind's energy products would necessarily result. If you have a nuclear effect and you attempt to scale it up, it very much is in everybody's interest to understand the science, or the scale up can result in nuclear effects that you have not anticipated.

STEVEN: Is understanding the science of this thing a 20-year proposition, a 5-year proposition, what do you think?

MM: Depends on the level of funding.

But we're actually pretty much there. We have both experimental results, clear evidence of heat, clear evidence of nuclear product, and a competitive and complete theory to explain the effects. We work very closely with the scientists at MIT who have developed theories which perfectly well explain the phenomenon.

So we have, I think, within two years with reasonable funding, we could have the whole thing locked up. What is going on? What is the heat effect? What are the products? What is the reaction? What is it good for? Whether we do that or not will entirely depend upon whether the funding is made available for that purpose.

DF: Steven, we're glad you joined us.

STEVEN: Thank you.

DF: Thanks.

585-WEST, 585-9378.

That is the question that interests, I suppose, most of us. Certainly the uninitiated, I'll include myself in that regard. When are you going to be able to run a heater? When are going to be able to drive a car with that sort of thing?

CB: That's right.

It's a question first of the field becoming respectable, I think. For example, most physicists in most universities today think that only Pons and Fleischmann were able to get heat. You go to your nearby university and ask the physics professors there. They are of the belief that only Pons and Fleischmann got it and no one else was able to do the experiment. The story has not been told of what's been going on.

MM: And this, despite the fact that there are at least a thousand published papers in refereed journals which confirm the Fleischmann/Pons heat effect.

DF: How much I wonder if there hadn't been such a hostile reaction from the scientific community and if scientists hadn't behaved a certain way in relation to the idea of cold fusion, if we wouldn't as a scientific community be further along in experimentation and perhaps, again, the use, the technology of cold fusion?

MM: Oh, absolutely.

In fact, I think that with the resources that was spent in the man-years that were spent, if it had been organized better and the work had been done with better intent and purpose, we could have had the answer already. We have spent enough man years and we have spent enough money to solve this problem. Just that it was done in a very unusual scientific environment where the normal rules of scientific reporting and analysis were basically abandoned. We just didn't work on it in a very constructive way and its not really the researchers in the field's fault. It is the whole scientific community.

DF: Let's take a call from Salt Lake. Keith joins us now. Keith, welcome to the program.

KEITH: Oh, thank you.

I have a question. I read an article a couple of years ago and it talked about that often the result depended upon . . . This researcher had found that if he had different samples of Palladium that he could almost kind of identify, look at it, and go this one might give me a good result, this one probably won't work, and that a lot of it [success] depended upon that. Have you heard this?

MM: Most of the material is critically important and when I mentioned before the need for materials research, the material that we need to research is Palladium. The way its put together, what the impurities are and are not to . . . We had a particular lot of material that we obtained early in our experimental program, which we obtained from Englehart. I think over 50% of all of the experiments run with that lot of Palladium were successful in obtaining very high Deuterium to Palladium loadings, which is a pre-cursor to excess heat production. And were successful in producing measurable, believable levels of excess heat. We've never had another material in our hands that has anything like [that] success rate.

KEITH: What is the different between these samples?

MM: More of a matter of impurity than purity.

It turns out that pure Palladium is not a very strong material. In order to obtain very high atomic ratios . . . If you have any sort of void within the Palladium, you nucleate very high pressure Deuterium gas, which tends to wedge the lattice apart, produce cracks, then de-load the Deuterium. So you can't obtain the conditions necessary for the effect with a mechanically defective material. The Englehart material that we were using early in the experiments was somewhat impure. Impurities tended to strengthen, to toughen the material. These are things that metallurgist know very well. With a properly organized program to understand the mechanical aspect of loading Deuterium into Palladium, we could just solve that problem, knock it out of the water, and move on to the next phase. At the moment we're stuck with some of the experiments work and some of them don't. Scientific community looks at you and says you're experiment is irreproducible.

CB: Yes, that's right.

DF: Sort of a red herring.

CB: Let me add to that, Doug.

DF: Yes.

CB: The physics community that wants to evaluate this and declare to the world whether its right or wrong, does not allow the experiment that is somewhat irreproducible. It's not part of their world. At the same time, you look at the experiment Dolly, the cloning of Dolly that was announced to the world. That was announced as science, but they only got one successful clone out of 227 attempts.

If Fleischmann had announced that he had made 220 cells and one produced excess heat and the rest did not, he would be laughed out of the physics community. The physicists really don't accept the idea either of an experiment that's going to take some years to perfect, or of the idea that an experiment is very difficult to do and any one man may struggle at it for some months before he gets it. They feel that if an experiment doesn't work when the next guy follows the rules and tries it, then it's a fatally flawed experiment and should be dismissed. So you have, again, this conflict of protocol between physics and biology, or between physics and chemistry.

DF: Keith we're glad you called.

585-WEST is the number, 585-9378.

Merle is on the line. She's in Salt Lake City. Hi Merle.

MERLE: Hi. I just wondered if there was any correlation at all between the process that goes on in the physical body as far as chemical/electrical that maintains body temperature without outside energy sources? Is there any correlation between that process and this process of cold fusion?

DF: Wow, Dr. McKubre.

MM: Certainly, I've never thought of it.

MERLE: Really?

MM: I would think not. In the effects that we examine involve heavy water D2O, heavy water is largely toxic and the body loads there 1 part in 8,000 inside your body, I very much doubt that Deuterium is participating in any nuclear effects inside your body.

CB: And also, of course, the amount of heat that's needed is of an order that can be provided by biology and chemistry.

MERLE: Okay, thank you.

DF: Merle, thank you.

So I wonder, Charles Beaudette, what ever happened, What ever happened to Stanley Pons and Martin Fleischmann? Where are they now? What are they doing?

CB: Well, Stanley Pons was very badly hurt by the reaction of the world against what he had done. He was driven out of the country. He renounced [correction: dropped] his U.S. citizenship and in 1995 [correction 1998], I believe it was, he bought himself a farm in Provance and settled in there. He's an independently wealthy person so he can do that. I guess he does some lecturing around in France, but that's about it.

DF: But no experiments?

CB: No.

Fleischmann is getting along in years now and he lives at home in Tisbury, England and does a certain amount of analysis and research and travel concerned with the field here. He still does writing and analysis work.

DF: Dr. McKubre do you ever hear from your old colleagues?

MM: Oh, yes, I speak with Martin probably once a month or so.

He's still very active and he's , . . . If my brain works as well as his does at his age, I should be very pleased, in fact. He's still very much interested in the field. He's reanalyzing old results. He doesn't have an experimental program. He's not associated directly with any university but he certainly is in a position to think, to theorize, and to analyze. Stan Pons I don't see or communicate with quite as often, but he is very happy and he's content and he's working on scientific problems, although not specifically cold fusion problems in the south of France.

DF: Dr. Michael McKubre is a scientist from SRI International Laboratory in Menlo Park, California. Dr. thanks so much for the time.

MM: Thank you.

DF: And Charles Beaudette is the author of "Excess Heat: Why Cold Fusion Research Prevailed". Thank you so much for your time.

CB: It's my pleasure, sir.

DF: Radio West is a production of KUER news. Technical help from Larry Holt and Lewis Downey. Thanks also to Mike Anderson. The program is produced by Anne Milliken. I'm Doug Fubbrezio.

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