"ON THE ROPES" - British Broadcasting Corp. Radio 4
Thursday, May 22, 1997
Professor Martin Fleischmann Interviewed by John Humphrys

John Humphrys (JH):
Imagine a world in which limitless energy could be created from a few buckets of water—so cheap they wouldn't even bother to meter it. We would solve the crisis of global pollution at a stroke, no more need to pump carbon dioxide into our battered atmosphere. We might even end global hunger. The waters of the ocean could be desalinated in vast quantities and pumped wherever it was needed to turn deserts into green fields. Oh, Brave New World indeed.

On March 23rd 1989, it seemed that we had taken a great step towards that. Two hugely respected scientists, Martin Fleischmann and Stanley Pons, held a news conference at the University of Utah to announce that they had discovered the secret of nuclear fusion—the Holy Grail of science. The process by which the stars in the very heavens produce their power.

For a few heady days, the world celebrated the great news and dreamed wonderful dreams, then for six nail-biting months, scientists the world over settled down to reproduce the experiment and they failed—or *said* they failed. From that moment on, Fleischmann and Pons were truly "on the ropes."

Professor Fleischmann, was that how it felt at the time for you? Did you feel beleaguered at that point?

Martin Fleischmann (MF):
I thought it had gone wrong. I thought the whole scientific process, in as much as it affected this particular research, had gone wrong, that's certainly true. Quite early on the opinions were polarised into groups who behaved as sceptics. As they said: "We are sceptics and true believers." So, the essential process of criticism was replaced by scepticism and that derailed research.

JH: We'll come to that in more detail, but let's...for those of us who, like me, are not scientists, let's try and understand a bit about this cold fusion. I know it's a phrase that you don't particularly like, but we have split the atom and we have created nuclear fission. OK, in layman's terms we have understood how to do that and we have, therefore, liberated vast energy, but it's an explosion. We can contain it in nuclear power stations but, in the process, we produce a massive amount or, at least, potentially of pollution and risk and so on. It's very expensive to do and we can't exactly use that to put it in our vacuum cleaners to power them as we once dreamed that we would.

Now cold fusion—to use that phrase for the moment—is something quite different from that. It's joining the atoms together is it?

MF: Joining the nuclei together. If we break nuclei which are heavier than iron into fragments, we get energy. If we join nuclei together which are lighter than iron, we gain energy again. And, of course, one of the chief objectives of fusion research is to join nuclei related to hydrogen, heavier nuclei than hydrogen—tritium, which contains two neutrons and one proton, or deuterium, which contains one neutron and one proton—in order to create heavier elements. That is fusion.

JH: And the effect of doing that is to release enormous amounts of energy...

MF: Like in the Sun. The same type of process as the initial steps in the Sun.

JH: And the only way previously, that we had discovered how to do that involved huge temperatures.

MF: Correct.

JH: Like the Sun.

MF: Yes. Huge temperatures or huge energies and one thing, going back to your lead-in, it is absolutely certain that the long term future of the world requires the implementation of fusion. Now we did not actually say we had achieved fusion. We said we had created large amounts of energy which could not be explained by chemistry.

JH: Right, you had—again, to put it very simply—you had a flask, a glass bottle, you had tubes sticking into that bottle, you had water in the bottle and what did you do?

MF: Electrolyse the palladium electrode and—people know what an electrode is, I think, they are used to having batteries...

JH: Right, I can grasp an electrode.

MF: You can grasp an electrode...you polarise it negatively, cram deuterium into the lattice and you get excess energy.

JH: And that is what you did. It sounds very very simple indeed. You put in a certain amount of energy and you created much more energy. Four to ten times as much.

MF: Well, under certain circumstances, let's be quite correct about this. Under some transient conditions, about ten times as much energy out as we had put in.

JH: Now all of this was being done in secret.

MF: Indeed.

JH: Why?

MF: We did not like certain trends in research which we could perceive internationally.

JH: That is you and Stanley Pons?

MF: Yes, and we did this really to satisfy ourselves whether or not carrying out these processes might have unfortunate consequences.

JH: Unfortunate, in what sense?

MF: Well, I think I have to come clean here. In the sense of national security really.

JH: Why?

MF: Well, I mean the real reason we did this was because we thought that this might be one way of inducing nuclear reactions which would be useful in a military context.

JH: And you were worried about that. You did not want to enable that to happen?

MF: Well, if the answer to that was yes, we would have wanted the information classified. At least classified for the time being until the scope of the whole problem had been properly investigated.

JH: Something that puzzles me about this is that you had put a lot of your own money into this, a hundred thousand pounds.

MF: Well, we knew we couldn't fund it and couldn't write a research application.

JH: Why?

MF: It would not have been funded. First of all, it would not have been funded and, secondly, we did not want to reveal that we had the notion that we might carry out these processes in this particular way.

JH: Your critics have since said that the

reason for that was because you wanted to get ahead in this race because, after all, if we do...

MF: There was no race.

JH: No race?

MF: No, there was no race.

JH: But we are talking here about what some would describe as, I think I said, the Holy Grail of science indeed.

MF: Oh well yes, but nobody else was working...at least we believed that nobody else was working in that particular way.

JH: Somebody described you, one of your erstwhile colleagues, didn't he, as a "brilliant scientist with a mad theory," and is that the truth of it?

MF: No, the theory isn't mad at all. The theory is...if you put down the framework in which you carry out investigation, then this is not at all a mad concept. Many scientists might regard it as being mad , because they judge it within the existing paradigm, but if you...I am quite convinced that the paradigm will change, it should have changed in the latter half of this century but I think the paradigm will change, then it will be seen that this particular research was just an example of many other research topics and might very well lead to a positive conclusion. It doesn't follow that every piece of research set up within a new paradigm will be successful.

JH: No, of course, but you believed, you conducted, you carried out this research, you conducted those experiments and, on March 23, 1989, you held a news conference to tell the world that you had succeeded. Now, let me just take you back to the moment of the experiment, and I doubt very much, because we all imagine the professor in the laboratory saying the equivalent of "Eureka, we've cracked it." I suppose it wasn't quite like that, you didn't rush out of the laboratory saying: "My God, we've done it! We've done it! We've done it!"

MF: No, no. Science isn't like that. Eventually we will deposit our papers in a library somewhere and you will see that the usual comment was: "The results are frustratingly interesting, there is no reason to stop." Perhaps no reason to go on, but then you see it depends on the sort of person you are. Many people, when they get an unusual result will say: "This is unusual, I'll go and do something else." If you are a different sort of person you will say: "Should I stop, or should I go on?." It depends on your attitude to the unusual result, and I was brought up in the research school where you always had to explain everything you did, including the unsuccessful experiments.

JH: But what you did, is you held a news conference. Instead of publishing the results of your research in *Nature* or some other respected journal, scientific journal, for your colleagues to pore over and, eventually perhaps, somebody would say: "Yes, we think there's some rather interesting work here." You held a news conference and the world went potty.

MF: Well, that was something outside our control. By that stage, we lost control. Another research group was working on this topic and they believed that they had observed neutrons of the correct energy.

It was the 50th anniversary of the discovery of nuclear fission and the hot fusion brigade were just gearing themselves up for asking for a lot more money for the next step in the research into hot fusion.

JH: There was a race.

MF: Well, it wasn't a race really. I think the other group should have followed our wishes and held back the publication till September 1990. Their work would have been better, on a better basis, and our work would have been on a better basis. But when it became clear that this other research group wanted to publish their findings we, of course, then had to inform the University authorities of what we were doing and we had to ask them the question: "Do you believe that you need to take a patent?"

JH: And it was their decision?

MF: It was their decision, not our decision. It was the University's decision that there had to be a set of patents and that then dictated the subsequent events.

JH: The news conference.

MF: That's right. The news conference was a consequence of the patent applications.

JH: And you regret that that happened?

MF: Well, I was never in favour of it. I tried to stop the news conference even the day before but unsuccessfully.

JH: Because the result of that...

MF: I knew it would go bad.

JH: And it did go bad.

MF: Yes. I knew it was not a sensible thing to do.

JH: And the result of that was that it exposed, I suppose, apart from anything else, all the jealousies that operate in the scientific world.

MF: Well, there were plenty of jealousies. It was a singularly unfortunate time to make the announcement. It was the 50th anniversary of the discovery of nuclear fission and the hot fusion brigade were just gearing themselves up for asking for a lot more money for the next step in the research into hot fusion. So it was a singularly unfortunate time for two chemists to make such an announcement, that was certainly true. And, of course, if we had not been put into that situation in March 1989, if we could have delayed—even to December 1989—we would then have published the full paper rather than a preliminary paper. And I thought, my recommendation was, that this should be let out at the lowest possible level. In fact...

JH: Without a great fuss, you mean?

MF: Yes. I wanted to have it published in the *Annals of Utah Science* of which, I believe, they only print seven copies.

JH: Not a best seller exactly.

MF: Not a best seller. I wanted to really let it out in a very, very minor way.

JH: There seems to be a great deal of difference between chemists and physicists that I had not been aware of.

MF: Yes, yes. Well the joke statement is that, the reason for the news conference was, "chemists are interested in chemicals but physicists are not interested in physicals." But, I think that is only partly true. When a lot is at stake, then physicists are really the principal offenders with regard to making premature announcements.

JH: So what followed then was a result, in your view, partly of professional jealousies.

MF: It's very difficult. I always maintain that really this is a job for several investigative journalists to find out what really went on. It was very unfortunate. I thought it would be bad, but I didn't think it would be quite that bad. I was hoping that we would have constructive criticism, rather than scepticism.

MF: But it was not to be, we had scepticism and no criticism.

JH: I was going to say, you had both didn't you?

MF: No. There was no criticism, there

was no constructive criticism at all really. I am not aware of anybody who asked for our results and who analysed our results. We had to ask for the results to be analysed independently.

JH: Some people went as far as suggesting that you'd been guilty of some sort of fraud.

MF: Well, you know this, I always say "Methinks the lady doth protest too much." People who accuse other people of fraud, you should then always say, Well, perhaps they have done a little bit of fiddling somewhere. ***[Editor's Note: A not-too-subtle reference to anti-cold fusion events at MIT in 1989.—EFM]***

JH: Why? Again, looking at this as a non-scientific outsider. Why should there be this sort of thing going on in this world?

MF: It's distressing, I don't know. A precondition for science is total honesty. Of course, I know scientists deviate from this, you know. Even highly respected scientists try to make their results conform to some notion they have had and, later on it turns out that the notion was incorrect and if the new experiments are carried out, you find it is really somewhat different to the way they believed.

JH: But isn't the most simple explanation of all of this, that you got it wrong?

MF: Well, I keep on checking. You see I must tell you that I am again checking up old data. You always worry about that, I don't know how often I have been through the past data to try and see, "Where have I made a mistake?" This is something you must do. You see, I do believe with Sir Karl Popper that you can't prove something right, you can only prove it wrong, so one of the things you have to do is check and check and check and check. Have you made a mistake? Where is the mistake? If you can't find a mistake then all you can say at the end is "I have done the following investigation, I cannot find a mistake" and then you have to stop there.

The consequence of that is that you could go on and maybe create a successful energy source and I have always said that the only thing which people will believe in the end is a practicable device, so this research would have an end result and I am convinced that nobody will believe any of the research results until there is a device.

It's like the Wright Brothers flying their aeroplane. Nobody believed that a heavier than air machine could fly until they saw the plane flying. In fact, the day before the *New York Times*, I think it was, accused them of fraud.

JH: What happened was that one team of scientists after another tried to reproduce the results, including the people here at Harwell, and failed. So, therefore, after all this excitement the conclusion was the whole thing, was a nonsense.

MF: No, but you see, the problem is—let's take the group at Harwell—the apparatus used at Harwell was deficient in many regards. You have to design the experiment and then you have to analyse the results and this problem of analysing the results is where most of the failures in science take place.

JH: This is puzzling again, to a non-scientist like me. This is puzzling. What's going on here? Why if you can do it, and you can take that set of results and analyse them in a certain way, those of us who aren't scientists have always believed that the one thing you can be absolute about is science. Alright, philosophy, poetry, music, Heaven knows what any of it means, there are a million interpretations, but a scientific result, we've always wanted to believe, at any rate, is a result, is a certainty.

People who accuse other people of fraud, you should then always say, Well, perhaps they have done a little bit of fiddling somewhere.

MF: Well exactly. But you have to carry out the analysis. And if you cannot analyse your data you may have to change your experiment so that you can carry out the analysis. I'm afraid scientists are not very good at analysing data. I always say to people who don't believe this, write a research application which is going to deal strictly with the analysis of other people's data and see whether you get any funds. You will never get the funds, and you see research is driven by the Research Student Syndrome, where the Supervisor (I can only speak for University research though) a Research Supervisor has to get the maximum number of results and says "I will analyse those data later," but of course, he never analyses those data.

JH: Because he wants to carry on researching?

MF: That's right. I mean, the biggest loss of information in science is the lack of analysis of existing data.

JH: You are not saying, Professor Fleischmann, are you that you are the only one who is right about it, the rest of the army is out of step?

MF: No, there are plenty of people out there. I mean, this has all been published. It's all accessible. We don't need to talk about this any more, if people really wish to know what these research groups obtained, it is in the literature.

JH: Of course, we wouldn't be able to understand a word of it, that's the problem.

MF: Well, you see, this is always the way. I think the problem is that science sometimes proceeds very slowly because of the misinterpretation of results.

JH: Well, alright then, let's in nice, simple terms...You conducted the experiment, you got, as you put it, excess heat. The world got very excited about that. Other scientists tried and failed to reproduce the experiment and—adding on the very important caveat—to analyse the results in such a way that they came up with the same thing as you.

MF: No, no, let me correct you. They just did not analyse their results.

JH: OK, didn't analyse the results.

MF: Therefore their experiment is useless.

JH: But, we are talking here, about as important a piece of scientific work as it is possible to imagine, certainly in layman's terms, in anybody's terms for that matter, and you are telling me that the reason the thing came to grief was because scientists didn't analyse the results of the experiments that they themselves carried out to try and replicate what you had done. Now, the question has to be asked again, why not? We are talking about something that is unimaginably important here.

MF: The resources required for analysing data are much larger than the resources required for gathering the data.

JH: But here we are talking about work that...

MF: Well absolutely, it is one of the diseases of present-day science.

JH: Billions of pounds is spent on research in science and if these experiments had delivered the goods, in layman's terms, would be worth unimaginable sums of money, billions, trillions of pounds. So for the sake of a few million...?

MF: Yes, that's the way it is.

JH: That is stunningly stupid, if that's the case.

MF: I'm afraid that is so but that is the way it is. The number of people who analyse their results in great detail and with sufficient and adequate care is really quite small.

JH: Well then, why isn't there somebody out there now, saying "Fleischmann was onto something, we believe in what he did."

MF: Well, there are people who say that.

JH: And why aren't they putting hundreds of millions of pounds into it?

MF: Well, you see then you have the question of the sociology of the subject. How can you put a lot of money into a field of research which has been discredited?

JH: But it was discredited, you say, because the analysis of the results was not carried out.

MF: Correct. I think people should nominate the pieces of research which they believe are critically important and those investigations have to be re-investigated maybe ten times.

JH: Why are you not continuing to work on it yourself now?

MF: Well I am.

JH: Where?

MF: I would prefer not to discuss that at the present time. At the present time, I am re-analysing—actually, this is very interesting—I am re-analysing old research data obtained by ourselves and other research groups, and I am starting work with another research group.

JH: There are those—the conspiracy theorists in this world, and there are plenty of them—who believe that the reason some of the great discoveries never get made is because there are vested interests and if we did have limitless energy, not necessarily free, as you say, but compared with today's prices, very very cheap indeed and non-polluting, it would destroy whole industries. The oil industry clearly, the people who make the internal combustion engine and so on and so on, the implications of it would be profound beyond belief. Anything in that theory?

MF: It is always tempting to resort to conspiracy theories. But you should only do it as a last resort. However, one extremely intelligent person I know, a scientist I know, says, "When you have assembled all your facts, there has to usually be a single explanation of all the facts and, if the explanation of all these facts is that there is a conspiracy, then you'd better take that seriously." Am I answering your question?

JH: Well, so far, but then, as you would say "and then...and then"?

MF: I'm afraid that if you assemble the facts, if you assemble all the information about the development of the subject you would have to come to the conclusion there is a conspiracy.

JH: And who is...?

I have always said that the only thing which people will believe in the end is a practicable device, so this research would have an end result and I am convinced that nobody will believe any of the research results until there is a device.

MF: That you don't know. But it looks strongly like a conspiracy. Several conspiracies, well, conspiracies within conspiracies.

JH: You could understand why an oil-producing company would be concerned about cold fusion. It's difficult to understand why a government, which also spends a great deal of money on research, would not seize it with both hands and say "This we must, for the good of mankind, develop." So doesn't a conspiracy theory come unstuck at that point?

MF: It depends on what the conspiracy deals with, doesn't it?

JH: How do you mean?

MF: What is the motivation of the conspiracy? If there is a conspiracy.

JH: In the case of the oil company, for instance it's obvious...

MF: That not be the motivation of the conspiracy, may it?

JH: What other motivation could there be?

MF: Well, I think that is one which people have to work out for themselves.

JH: Well, you tell us, you know.

MF: I'm not going to tell it on BBC, on Radio 4, no, I am not going to say that on Radio 4, I'm sorry I'm not.

JH: But can there be any reason why a government should be less than enthusiastic...

MF: Of course, of course. Right at the beginning, in that article which perhaps you have read, I said at that time the Head of the US Department of Energy was Admiral Watkins and I said, "Would Admiral Watkins welcome the notion of nuclear research being carried out in Chemistry Departments?" It is ludicrous, of course he would not. The motivation would have to be this must stop, if this work is going to be done at all it's going to be done in national laboratories. Something which I agreed with at the time.

JH: But this is a matter of individual pride getting in the way.

MF: No, it's not a question of individual pride, it's a question of sensible security. Supposing you have this type of research carried out in university departments, goodness knows what will be discovered. Should it be done in university departments?

JH: Well then fine, why aren't governments, why is not the US Government taking your work and saying this will now be done in a government laboratory?

MF: Well, perhaps they are. Well, you don't know, do you?

JH: Do you know?

MF: No, I don't know.

JH: Is it conceivable?

MF: It's certainly conceivable. But I don't know.

JH: So how far are we away from another news conference such as you held back in 1989 with somebody saying: "Well, we have cracked it."

MF: It could happen at any time.

JH: Really?

MF: Yes. The production of a demonstrable, useful device could happen at any time now. It doesn't mean to say it will happen but it could happen at any time. The creation of a useful, commercially useful, device of course, will take a considerable time.

JH: By which you mean years?

MF: Yes, years.

JH: But you, now in your 70s, are still working and you believe that in your lifetime you will see this work?

MF: Well, you know, I am not a spring chicken and I'm not very fit so I don't know whether it will be in my lifetime, but I think it is around the corner.

JH: Professor Fleischmann, thank you very much indeed.

MF: It has been a pleasure to talk to you.

[end]