An Interview with Professor Martin Fleischmann

By Christopher P. Tinsley

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Christopher Tinsley: Now that you are retired from IMRA, what do you intend to do? Are you really retired?

Martin Fleischmann: I don't suppose I'll ever retire completely. I retired from full-time work at the University of Southampton when I was age 56, but I didn't "retire." I started a number of part-time projects and, eventually, of course went full-time to IMRA Europe. At the moment I am taking a very careful look at some of the work which we have done in the past. It has been suggested at various times that I should start an operation in the United Kingdom but--bearing in mind my age and medical history--I think this would be not a very sensible way to go forward. So I am now interacting strongly with a group in Italy. I anticipate that we will take a very careful look at what we and other people have done during the past eight years and move on eventually to try to implement some of the work which I have wanted to carry out in the past.

T: You've been giving some assistance to Mr. Evan Ragland with regard to his cell. This cell is of course the one which our magazine is hoping to provide to people as a demonstration device of the basic thermal effect.

F: I think my interaction with Evan Ragland will be principally concerned with the form of the electrodes. I have had this view of the optimization of the electrode design for a long time. Historically we went through various phases in the work and eventually worked on large sheets--very large sheets--of palladium. That work was stopped in March 1988 because of concerns about the safety of the device. At that stage we switched to using rods, which, as everybody knows, we have used because we felt it was very important to be able to reduce the scale rather than to increase it again because of our concerns about safety.

T: Are you thinking here of mechanical safety in the sense of the famous "centimeter cube of palladium" incident?

F: Yes. That was always a big factor. You know, as the work moved forward, it included the work on this cube which disintegrated--unfortunately unobserved, because it happened at night.

T: Perhaps, very fortunately it was not watched under the circumstances?

F: Perhaps fortunately -- yes. After that we moved to using sheets under very mild conditions. We tried to reduce the scale of the phenomena. Incidentally, as we were discussing earlier, this included unexplained increases in the temperature of the cell. In March 1988, we decided that we had to take further steps to scale-down the experiments.

There is a famous diagram which has Stan Pons' and my writing all over it, about these unexplained rises in temperature of the cell. As it happened, I was just recovering from an operation here in the UK. At that stage, we decided that this line of work had to stop and we switched to the rods. However, rods are not satisfactory mechanically because there is a stress concentration in the center, so it is obviously better to use something like a continuous sheet. That's why I believe that we should now look at tubes.

T: Perhaps with one anode down the center and another anode as a coil around the outside, so that you make a triode arrangement in that way?

F: Indeed. I think my interaction with Evan Ragland will be along that line.

T: In the matter of the centimeter cube of palladium, the solid block, would you say that the disintegration incident had some effect on you in the way of being a stimulus to your continuing the work?

F: Indeed, yes. It was our incentive to continue with the work but, at the same time, it was a one-off, so you can't really say anything definitive.

T: You don't want to do it again?

F: No, I don't want to do it again. You can specify various things which might have caused it. If you assume that it was a valid experiment, then its disintegration reveals a very substantial part of what has been found since then, including the fact that you can get heat generation at high temperature.

T: You're suggesting it was a thermal runaway of sorts?

F: Yes, you can see that even with a relatively modest enthalpy output and a uniform generation of excess heat in the volume, you would get rather strange conditions in the center of the cube.

T: Rather like a haystack spontaneously combusting?

F: Yes, it is like that.

T: That although the process producing heat is at a comparatively gentle level, if you do that inside a haystack...

F: It'll catch fire. Yes. You can do the calculation on the back of an envelope to show you that this will happen, that it could melt in the middle. It's just strange that people haven't done this ...you know that people say "pooh, pooh, pooh, it can't possibly be," so the discussion never gets going.

So if I could just go back now to something which I am sure we should cover here regarding our original scenario: we have, in fact, four ways--four major potential lines of research. The first was the topic electrodiffusion, I'm sure we shall cover that at some length; the second one was electrochemical charging; the third one was a collection of experiments which really bridged the topics of hot fusion and cold fusion. Interestingly enough, no one has ever asked us about that, they are not in the least bit interested.

T: They perhaps haven't had the opportunity to ask that?

F: Well, what is so interesting is that no one has asked.

T: Well we are interested.

F: The fourth one was another set of experiments which I may do with my friends in Italy. So there were four distinct lines and, of course, we became committed to electrochemical charging although our real intention was always to work on electrodiffusion. A discursive answer to the first question.

T: We're just a few months on from the ICCF6 Conference. What do you anticipate will happen in the field in the course of the future from now on?

F: It is very difficult to say. I've always said there is the "seven year barrier." Yes, we've passed that. Usually, if you have a new idea, you very rarely break through to anything like recognizable development or implementation of that idea the first time around--it takes two or three goes for the research community to return to the topic. So I thought it would probably all peter out in '96 if we didn't break through, but I don't think it has done that. I think this is one of those situations where although people think it is crazy, the value is so high that they will continue with it. If you think about the meeting in Japan, what was revealed was that if you do the experiment correctly--especially with the correct materials, then you will make successful observations. As regards the materials aspects, I'm very keen on Johnson-Matthey material Type "A" or something which looks like Johnson-Matthey material Type "A." If you use that, you will find it relatively easy to to reproduce the findings in a reasonably short space of time. However, I think that the meeting revealed that there are several research groups entering the field who are doing this. I think that the real success will come from the next phase, which will include experiments in electrodiffusion or combinations of electrochemical charging and electrodiffusion.

T: We are seeing a considerable increase of interest in this whole general area --even in recent months there has been a considerable shift. And yet, of course, Max Planck set his "constant" at 20 years for new ideas to penetrate.

F: Did he? Well, he said that all the opposition has to die out, didn't he?

T: He said that science proceeds by funerals.

F: Yes, yes. There is a lot of truth in that.

T: And yet in cold fusion it's really not been the "young Turks" that have been coming in...

F: It's the "old Turks."

T: Exactly.

F: I think that we were starting to talk about that earlier. I think this was a subject for older people who were not afraid to...who didn't care about their scientific reputation.

T: But perhaps in the past there have been periods where people have been able to do science without having to worry about their reputations?

F: That's gone now.

T: Perhaps it will come back.

F: Maybe it will come back. I think that at some time we will want to talk about the general malaise of science.

T: John Bockris has suggested that science had become very rigidified in around 1972. Do you have any comment on that at all?

F: I think there was a very unfortunate development in the 70's, a sort of "anti-Francis Bacon development." People developed a view that a subject is not respectable unless it is dressed up with a suitable overload of theory, and consequently we have had this "top dressing" of theory put on the subject which has tended to make the approach very rigid. Also, the theories are of course written in terms of rather old-fashioned ideas.

T: But we have been seeing a shift in general public attitudes.

F: To science?

T: No. Specifically towards things like towards cold fusion. This may be a kind of pre-millennial tension or something of that kind, but we are finding that companies and individuals are taking the whole field of cold fusion very much more seriously and positively than they were doing even months ago.

F: I think that's probably true.

T: It's a strange thing.

F: I don't think so. I think that it is a question of economics. I don't know whether you have done your calculations but, about two or three years back, I did a first assessment of what the first successful device would be worth and it came out at about 300 trillion dollars. So, at that sort of value, people are prepared to take a rather high risk on the research. You know, for a long time people have always had a list of the first ten projects. I don't think you should over-emphasize the value of cold fusion necessarily, but if you make your list of the ten most valuable projects, high temperature superconductors will always be on the list; fuel cells will always be on the list. It doesn't matter whether you can or cannot achieve high temperature superconductivity or fuel cells, they will always be on the list because if you could achieve them they would be extremely valuable. So these ideas will keep on coming up. Now, of course, cold fusion is the daddy of them all in a way, in terms of value, so I think that viewed in a social way, from the point of social considerations and economics, it will tell you that this thing will stay around.

T: Do you think that physics and chemistry took something of a wrong turning at some point in the last 150 years or so and started to perhaps head into something of a blind alley? That what we now are seeing -- perhaps with cold fusion, and so forth -- is that mistakes have been made? We have something that doesn't appear to fit comfortably.

F: I don't think so. You see, I am a very conventional scientist, really. Extremely. I always explain that -- I'm really very conventional. We arrived at this topic from various inputs to the subject and, in the end, we could pose a very simple question, namely: would the fusion cross-section of deuterons compressed in a palladium lattice be different to the cross-section which you see in the vacuum? Now, I think that was a very simple question -- either yes or no. The answer turned out to be different.....I should explain that what we said was, "Yes, it would be different, but we would still see nothing." That was the starting point in 1983 or whatever, yes 1982-83. Of course, it would be different, but we will see nothing. But it turned out to be radically different than that. Now, of course, you have to say, "What do we do with such an observation?" Many people--as was shown subsequently and even though they were told what had happened--couldn't believe this and ignored their own experimental evidence. But that is not for us.

As for taking a wrong turning -- well it has in an organizational way. I always say that if you recall Leonardo da Vinci and Michelangelo holding a painting competition in the Town Hall in Florence during the Renaissance then you cannot conceive of that happening in the present age. The early development of science was really a dilettante type of aristocratic preoccupation...

T: Lavoisier and company?

F: Yes. You cannot imagine that somebody would now give a latter-day Faraday carte blanche to investigate the interaction of forces.

T: Mind you, for what he cost at the time, we could really afford it. It wasn't that expensive.

F: Nor is cold fusion expensive. One of my theme songs is that if you can't do it in a test tube, don't do it. It is not necessarily true that expensive experiments are not worthwhile doing but there are plenty of rather cheap experiments which are certainly worth doing. So if you haven't got the resources, do think a bit and try the cheap experiments. So has science taken a wrong turning? Well, this is one instance where it has taken a wrong turning, but, of course, there is also this whole overlay of Copenhagen-style quantum mechanics which we have not been able to shake off.

T: You feel that was a wrong turning?

F: Oh, that was a massive wrong turning. Massive wrong-turning, although we have to give credit to Niels Bohr and the Copenhagen school, for a great deal of valuable development of theory. However, that approach should have been abandoned a long time ago. The problem is that replacement of Quantum Mechanics by Quantum Field Theory is still very demanding.

T: Now, how about the difference between, in cold fusion, but perhaps in science generally, the way things are done in Japan and in, for example, the United States? There are obviously significant cultural differences between the countries and this runs into the way they work in every field. A World War II Japanese battleship can't help but look Japanese. Perhaps you could include the UK as examples. How would you characterize the differences?

F: Yes. That's an enormous collection of questions; it's not just one question. I just had an ex-student of mine here, who is now an academic in Coventry. He has a very interesting collection of post-graduate students working on a range of topics. One of these led us to discuss globalization in the context of the difference between Christianity and Islam, and I said, "Well, this is not the question. I think Islam and Christianity can be reconciled but Shinto and Buddha on the one side and Islam and Christianity on the other, that is a much bigger problem." The cultural difference between the Pacific Rim and the Greco-Judaeo tradition is going to be a much bigger problem for the world. And, of course, I think that it is very difficult for people to lock into science if they haven't got the Hellenistic tradition.

T: But the Japanese are notoriously fine co-operators....

F: Yes, they are very good at retro-science for example, where teamwork is very important, but I don't think their system lends itself to innovative research. I think that many senior people in Japan, who are now unfortunately dying out, realize that Japan will have to take a step towards innovative science, they cannot go on using innovative ideas developed in other countries and develop them themselves. Incidentally, this is one of the problems with the development of cold fusion--they went into it too soon. I think they have a very important role to fulfill, but by stepping in too soon--before the boundaries of the subject had been defined --then this was going to create a great deal of difficulty. So I think that as science is organized in Japan at the moment it will not make a great deal of headway in innovative science. That's my own opinion.

T: But, in Japan, is it not also true to say that they hold in very high esteem persons such as yourself--a Fellow of the Royal Society?

F: Outsiders. A prophet is not recognized in his own land.

T: "A prophet is not without honor save in his own country." But is it not generally true that the Japanese have particularly strong respect for high-powered academics from outside Japan?

F: Yes. But this is because they don't recognize their own prophets. Because they don't fit into the system.

T: But then neither do we. That's a universal problem.

F: Well this has now come upon us. I think this was not true--especially if you take the United Kingdom -- this was not true in the past. I mean prophets in other endeavors--politics or the social sciences--might not have been recognized, but in science, prophets were recognized in the United Kingdom.

T: Would that explain the disproportionate role that British science has played?

F: Well yes. I think you know that I classify science as British science, American science, and everybody else. British science has a certain style and, of course, my problem is that, although I was born in Czechoslovakia, I am the archetypal British scientist.

T: You are indeed.

F: I am. I am a caricature of what British science is about in the way I work. American science is much more organized, much more hierarchical than British science has been. I think British science is becoming more like American science--and then there is everybody else, I'm afraid. Is it not true that 55% of R&D, ie. innovative science, since the War has been done in the USA and Britain.

T: So, it is extraordinary...

F: It is extraordinary and now, unfortunately, we have found ourselves in the position where I think some decisions have been taken by the mandarins in Whitehall that Britain should become a "super Belgium." The fact that we have not been able to exploit our ideas is taken as an indication that we should not do innovative science. When in fact, of course, what has been wrong is that we have not exploited our ideas. Removing the ideas is not going to do us any good whatsoever.

T: That's certainly a fascinating view. You say that science is a highly organized endeavor in the United States, but surely a great deal of innovative and exciting work has been done in the United States as well.

F: However, the cost is very high. It is not a very effective system, though they could afford it, or historically, they could afford it but the cost/benefit analysis of science in America is not very good.

T: Yes, I've always been entertained by chauvinism in science, for example, in this country we have Crick & Watson and in the United States we have Watson & Crick. There's an extraordinary and highly inappropriate chauvinism, is there not, in science or would you say that's only in the public perception?

F: It's in the public domain, I don't think scientists themselves do that. Scientists are really very conscious of the fact that they stand on the shoulders of an enormous tree of preceding workers and that their own contribution is not so enormous. What I've always said about cold fusion is that "everything I can say about cold fusion can be condensed onto about half a page now and I will know the subject has arrived when it is a footnote." When there is a lot of verbiage then you know you haven't arrived.

T: Is this your comment about from simplicity through complexity back to simplicity again?

F: Well that is part of it, yes, it is a little bit of it. You have to in the end, distill out that which is simple, to think about and re-investigate that which is simple to do.

T: Yes, that's very interesting. Arthur C. Clarke once had a character in a novel comment that the French make the best second-raters at everything in the world.

F: But that's their objective. It's a conscious decision. Historically they have been very good at mathematics, and occasionally you get a peak like Pasteur and they recognize the peak. I think you could hardly ignore Pasteur, but basically the French system also doesn't lend itself to innovative research.

T: And Russia?

F: Well, the Russians have been extremely innovative considering their resource base. So how one should analyze this, why the Russians were so successful? It's a good question.

T: Perhaps they have been in a continually post-diluvial state.

F: Probably yes, I think they could only escape from the system via some sort of profession. You had to hide within your profession. You know, you had to become immune from the political pressures.

T: If you became a Sakharov no one could touch you seriously - though they tried.

F: But if you even go lower down the scale, scientists were left alone, so the clever people who could make it into science hoofed it and made it into science as fast as they could.

T: To return to cold fusion: if you had to do it over again, would you have participated in that press conference in 1989?

F: Here again, is an enormous collection of questions. Of course, I was opposed to it as you probably know and I tried to stop it -- even the night before--and I failed because there was a key person I needed to contact.

Stan and I funded the first phase of the work ourselves. It was secret. We reckoned we would get our first answers for about $100,000, which was as much as we could afford to spend. In the Summer of 1988 we reckoned that we would need $600,000 to complete the first phase in about September 1990. We planned to review the question of publication in September 1990.

We had at that time, and continued to have all the way through, tremendous hang-ups about whether this work should be published at all. In fact, in '88 we went through several discussions about whether the work should be classified.

T: For reasons of....

F: National security. However, in '88 we had the twin problem that we certainly did not have $600,000 between the two of us to spend on progressing this research properly, and we needed the $600,000. We also had to inform the American Department of Energy in the States, and I had to inform Harwell [Laboratory in the UK--Ed.] about this work. So I said let's kill many birds with one stone: let's write a Research Application rather than a patent--which we submitted to the DOE. Initially, it didn't go to the DOE, but it finished up at the DOE in August 1988 and that, of course, brought us into this conflict situation with another scientist who was interested in the subject, who had been interested in the subject previously. He had not done the experiment in a way in which he could possibly have succeeded, mainly because he had used 10% D2O in H2O and, of course, he would have had hardly any deuterium in the lattice--and he started to work on this topic again.

There is nothing wrong with that incidentally, people object to that, but I don't object to that at all. I think that he should have disclosed his intention to restart his work when he refereed our proposal. What was hard for Stan and me was that he wanted to disclose his results. Now Stan and I were still working in secret at that time but, because of this development, we had to inform the University of Utah because we thought that they might need to take patent protection. They said yes, so then the patent became the driving force. And it was the patent consideration which produced the press conference, the "prior claim." I was not in favor of that at all, but it was that which produced it. Of course, you might ask if we would have done it any other way. Well, I wished we had carried on for 20 years in a mild way and I wish I had started it in 1972 and done it all myself, quietly and over a long period of time. I think the press conference was a mistake. But it was inevitable.

T: Can you, looking back, see any alternative to what happened?

F: No.

T: You would have been stuck with the same situation?

F: Given the situation we had and given the results we had, we had to tell the DOE and Harwell. Given the conflict situation which developed we had to tell the University. Given the results we had, the University had to take a patent. It was inconceivable that Chase Peterson and Jim Brophy would have said, "No, we won't take a patent." The only thing which would have changed would have been the existence of an Ethics Committee to whom it could have been submitted--a National Ethics Committee would have said this is not the sort of science or development which justifies taking a patent, forget the patent, no press conference, no nothing, it would have been OK. But, given the situation in which the Universities found themselves, I think it was inevitable--and it would happen again, and in other fields it will certainly happen again.

T: I for one see no clear objection to what people dismiss as science by press conference. After all, the hot fusion boys do it all the time......

F: I think it is worthwhile to recall Zeta. Zeta [A supposed hot fusion achievement.--Ed.] was announced by the Postmaster General in the House of Commons. What can be more outrageous than that?

T: Quite. But I was thinking that, for example, I would very much have welcomed a press conference by the French nuclear research people, CEREM, on their full replication of your "boil off" experiment. I'm sure you would have done as well.

F: Hmm--hole in the corner.

T: In other words, if you've got it you flaunt it. Did you notice that any mention of the CEREM replication is totally absent from Douglas Morrison's account of ICCF 6?

F: No, I haven't looked at that. But can you imagine something which has been so systematically ignored as that announcement?

T: Surely, but was it announced publicly?

F: Well, Biberian presented this work.

T: Ah, but only at this Conference. Where else has it been announced?

F: I think what is going to happen is that a lot of this work will disappear behind closed doors.

T: For what reason?

F: Three hundred trillion dollars.

T: The energy business?

F: I mean, other reasons as well, just take that as a...

T: But you are thinking, in this instance, of the energy implications?

F: Yes. But there are other implications as well. But let's just confine our attention to energy.

T: Quite. You recall the famous sequence of events at MIT, and Mitchell Schwarz and Gene Mallove's discussion of that on the BBC/CBC documentary. Have you any comments to make on that particular series of incidents?

F: It was certainly very extraordinary. There were three, possibly you could say there were four, investigations in 1989 that we should have taken notice of. One was the MIT investigation, another one was Lewis at Caltech, the third one was in Harwell, and possibly we should take note of Kreysa and his colleagues in Germany. I think the last is a minor thing--a fairly ridiculous investigation. I think the only half-way reasonable investigation was the one in Harwell, that experiment was well designed but badly executed and, of course, totally misinterpreted.

However, to their great credit, they made the data sets available for study. This is Harwell.

T: Of course, MIT did that too in a sense, eventually.

F: In a sense, but see what happened. If you take the Harwell data sets, you cannot say that this experiment worked perfectly and that there is no excess heat. You could only say either that the experiment worked perfectly and there is excess heat, or the experiment didn't. And on those two bases you have to do another set of experiments. As regards MIT, all one can do is shake one's head in disbelief really. I mean, again, if you fiddle about with baselines then you have to consign those experiments to the dustbin and start again. The one in Caltech was clearly very strange because there was a redefinition of the heat transfer coefficient.

I had actually thought of dropping out of this field in '91 and just waiting to see what other people would make of it in order to go back into it in '93 myself, but I was persuaded to go to France.

T: Just one moment, to track back, you were talking about the Caltech experiment -- you said something very strange happened and there was a redefinition of the heat transfer coefficient.

F: The heat transfer coefficient. I'd have to refresh my memory, but my own view was that it was much more plausible to re-interpret the MIT and Harwell and Caltech results in terms of the generation of excess heat.

T: Yes, but that experiment was discussed in a paper in the Journal of Physical Chemistry some while later, was it not? Did you see the paper there which largely refuted the Caltech experiment and showed there had been excess heat? That was one of Miles' papers.

F: Was it? Well, yes, you know it's not very difficult to show that you get excess heat if you use the right material. Of course, it's a materials science question. If your electrodes crack you will not load them electrochemically. You can load them some other way but not electrochemically.

T: This is all connected with the same period that you were effectively accused of fraud by Parker of MIT. What were your feelings about this?

F: It's insane, really. But Parker had an axe to grind and Parker tried to deny he was the lead of the paper. I think it was Mitchell Swartz who caught up with him mainly, and Gene Mallove, then Parker somewhere said, "I don't know, nothing has happened," and then someone said, "But you are the , the lead !"

T: In fact, the BBC documentary showed Parker in a very poor light.

F: So who called me a fraud?

T: Who indeed?

F: I shouldn't say fraud. Fraud is not an acceptable word, but who created a deception.

T: "Inappropriately interpreted the data."

F: Who inappropriately interpreted the data, which is very common? Incidentally, I was recently writing to my Japanese colleagues about misinterpretation of data. Science is full of misinterpretation of data. Because data interpretation doesn't hold a very high priority in science, it is driven by the Research Student Syndrome: "Let me get all these results now and I will interpret them next year." Next year, of course, never comes.

T: One of the first things which convinced me to study the field very much more carefully in the early days was Professor Close's book on the subject. It seemed to me he was tying himself into logical knots to try and explain the results away. I felt that there must be something in it for a man of that calibre to have to go so low. Such comments of his that when heavy water was later found to be contaminated with ordinary water, that this showed that somebody had been tampering with the experiment--and I can't believe a nuclear physicist is unaware that heavy water is hygroscopic.

F: Well, contamination is a big problem, you know. I think this is a very interesting point. If you have a very low level of contamination by light water you will certainly destroy the effects due to deuterium.

T: He was explaining the presence of tritium by saying that it must have been contaminated by tritiated water, because there was tritiated water available and the presence of ordinary water in the heavy water proved that somebody had been contaminating it. In fact it proves nothing of the kind.

F: This thing about the tritium was very interesting for us, because this was something we never wrote up properly at the time, and we have never returned to it because we have got certain hang-ups about this aspect. But, to explain our results with tritium, we would have had to have an isotropic separation factor between deuterium and tritium, this is the ratio in the gas to the liquid phase at about thirteen and a half. You can't get that. There's no way we could have got that much tritium by isotropic separation. So it had to be generated, you see, and other people have found that since.

But Close, I don't know, I can't understand Close. Frank Close came to see me. I had to return in February 1990 to Salt Lake City, and he wrote to me saying that he wanted to come and talk to me because he had been in Oakridge and he had seen the results in Harwell which were negative, and he'd seen the results in Oakridge which were positive. There were two groups in Oakridge who had positive results at the time that was Hutchinson and Scott, but there was a third group which had positive results in Oakridge and I had just finished with all this calorimetry here. So I said, "Come, I'll discuss it with you." And he came here and it transpired that he really wanted to talk about the gamma rays. I said, "I haven't got those data here. Come to Salt Lake City because we've done a new set of measurements there on gamma ray generation," and he never came. He came to Salt Lake but he didn't come and talk to us, so he never had access to all the stuff which is in Il Nuovo Cimento. So that's my knowledge of Frank Close.

T: That leads onto another question. Do you feel that there is any further clarification you can give in your answer to the accusations from Close and others about your supposedly unethical shifting of the gamma ray spectrum?

F: There is a whole set of files upstairs and you are welcome to have them in due course. I think that the point about that was that I went to Harwell when I came back from the States, and I used the diagram which I had prepared in February '89 from data which had been given to me by my collaborators. There was something obviously wrong with those measurements. I went from Harwell to Switzerland and I asked for the final version of the diagrams to be sent to me in Switzerland.

So it's one of these unfortunate things. You can't really say what happened, but the diagram I used in Harwell was a preliminary diagram and when David Williams asked me whether he could have these diagrams and I said, "Yes, if they are for your own study, please don't distribute them; for your own study you can have them." These are the diagrams which Frank Close then got, which he shouldn't have got. I would certainly not have vetoed his use, but I would have wanted to add a word of explanation about how they had arisen.

There is one important issue here. By March 1989 we had decided that these measurements had to be done with a high resolution Ge-detector, not the low resolution Na-I detector. The results of these measurements were available in Salt Lake City in February 1990.

Subsequently, I tried to get these transparencies back to see how they might be related to the material in Frank Close's book. I was told by various people in Harwell that they had been lost. So Frank Close got them and Harwell lost them. The whole thing looks rather doubtful to me.

In other words, I don't take kindly to being accused of unethical doings by people who clearly have been involved in unethical activities themselves.

T: Speaking of people being accused, what do you feel about John Bockris and his various problems--like horse manure in his letterbox?

F: I didn't know that. Did he have horse manure in his....

T: Yes, recently.

F: Really.

T: Well you know he held a conference on low energy transmutations and had to hold it off-campus. I just wondered if you had any comments, because Professor Bockris is a fine, forceful old gentleman, is he not?

F: Well, he's another one who doesn't care about his reputation. Well, he does, but not to the extent where he would let it cloud his judgement.

T: Yes, that's a very interesting point--the matter of reputation. If one was looking for the world's most highly regarded electrochemists at the time one would have to include yourself and Professor Bockris in a very short list. This is interesting--that both of you have been perfectly happy to take such a stance, rather than resting on your laurels.

F: I think I must interject something here. People said, "Why would you do it?" We can come back to that, but I said in reply to them, "Well it is not clear that it should have been me, but I think it would have been very likely that it would have been an electrochemist who would have done this research." Because of the nature of the subject you see.

T: What is the "nature" of electrochemistry, then?

F: Well, it is the interaction of physical chemistry and theory. You know, it is the combination of knowledge. Your knowledge base which would make you pose the question, "Is it not possible to induce anomalous nuclear reactions from deuterium in palladium?"

T: So, you would say that an electrochemist is rather like someone standing where three countries meet?

F: Yes. A gas-phase man wouldn't think of it all.

T: Are you interested in any other, shall we say, "controversial" areas of science at all? Are there any things which most people would perhaps dismiss, but perhaps you have a less certain view.

F: Yes. Well, cold fusion is part of a much wider area, and I have been really quite uncertain that our theory and understanding of condensed matter is at all satisfactory. However, I'm not interested in some of the more extreme ideas which have been put forward and which interest you, you know in the future of energy.

T: I will say that some of this gravity modification stuff does, in fact, appear to have a theoretical basis as well as some experimental evidence...

F: Well, if you think about gravitation, until we have a unified field theory, then you can't be sure what is going to happen.

T: Even Frank Close said that we don't know much about gravity, and anything might happen.

F: We really have an incomplete understanding. This will change, but there are one or two notable exceptions, which I don't want to talk about now. We have no understanding of quantum gravity and until that happens we can't be sure that nature won't play some rather strange tricks. As I told you when we were talking before, we had about four projects which we were working towards, one was to do with gravitation, one was actually to do with the behavior of electrons in metals. We actually started to collect equipment together to investigate the behavior of electrons in metals. But...

I have told you there have been certain themes which have run through my work, although they have never really been disclosed. I have often worked on topics where something short of the final answer would nevertheless be quite interesting.

When I think about what I have done, I find that I have failed to achieve any of my longer term objectives.

T: A pretty impressive failure, surely?

F: I have been content with what I have achieved, but I have not achieved what I wanted to achieve.

T: Which was?

F: To gain a better understanding of condensed matter. In order to do so, as with the cold fusion story, I find the answers to the global questions have eluded me.

T: Most of the truly exciting science over the last half century has been in condensed matter, you are saying?

F: Yes.

T: In terms of value to humanity, it has been the area of science which has been of the greatest benefit.

F: However, there is a lot to be said for working in high vacuum. Curiously enough, I am again extremely interested in the behavior of thermionic diodes. I find I do not understand how a thermionic diode behaves. As I am interested in the interaction of charges in electrolytes, I think about simpler systems, and from that try and understand the behavior of the thermionic diode. I do not understand it; and I don't think that anybody else understands electrons in a vacuum either.

T: In that case, to me, the number of things I don't understand is increasing all the time.

F: I sometimes believe that I don't understand anything.

T: I'm happy to say I'm at least beginning to make some progress in the direction of not understanding...

F: Well, you have worked in this field, haven't you. Just think of the space charge around a cathode, you understand that?

T: Well I must admit, to be honest, I've rather tended to take things like the thermionic diode pretty much for granted.

F: Well, before our next meeting try to tell me whether you understand the space charge around a cathode.

T: I will. If something had prevented you from becoming a scientist, is there some other...

F: Oh yes.

T: What would you have liked to have done?

F: I could have done many things. Basically, I was more interested in history and English literature than I was in science. It is, you know, very common for chemists to be interested in history and it was really very difficult to choose. Shall I tell you why I became a scientist?

T: Please do.

F: I did not think I could have a rewarding career if I went into arts.

T: In what sense a rewarding career?

F: Well, an intellectually rewarding career. I decided to do science because I could see.....this seems a very sort of cold-blooded decision. Well, it was really. A somewhat mature decision for a child of 16.

T: Yes, I can believe that. Staying with your history, what can you tell us about the route your family took to come to England from Czechoslovakia before the Second World War?

F: I have told Gene some of this.

T: You have, but I think he's rather hoping you might still say a few words for this interview as well.

F: Well, it was quite sort of accidental, as so many things--really formative things--in one's life are accidental. We had got caught up in the German occupation of Western Czechoslovakia and we managed to get out. I always tell people I had the unique and unpleasurable experience of being arrested by the Gestapo at the age of 11. These things tend to concentrate your mind somewhat, you know, and my father was very badly beaten up by the Nazis. However, we got out. We were driven across the border by a First World War comrade in arms of my father.

T: He had been with your father in the first war?

F: Yes. He was a fighter pilot in the Austrian Army, and my father was an artillery officer, but they were very close friends. They were big heroes locally. He drove us across, he had a taxi firm. He himself drove us across into the unoccupied part of Czechoslovakia. That was the first time we got away, and the second time, it wasn't clear where we were going, we might have gone to Canada or Argentina -- or South Wales actually. But we couldn't get any money out. My parents were going to start a factory in South Wales, but this couldn't be arranged, so we lost everything, and in the end my sister was adopted by a Methodist minister and his wife in Cheadle Hulme and the wife's brother lived in Llandudno and she told him that he had to adopt me. Which he did. He was a bachelor and he adopted me.

I find this very difficult to talk about. I must say, when Gene asked me about it, I burst into tears--which I am prone to do when I recall this ancient history. At that time, my parents also got permission to come to England, and we all got on the train in Prague and came to the Dutch border and the Germans cleared the train of all refugees and we were in the last coach and my father said, "No, sit tight, don't get off the train," and the train pulled out of the station. So that's how we got away the second time, and arrived at Liverpool Street Station with 27 shillings and sixpence between the four of us.

T: And how were you treated afterwards?

F: Marvelously.

T: This country treated you well?

F: Yes.

T: In what way?

F: In everything. We had the most unbelievable consideration.

T: Because not all people coming into this country nowadays as refugees are so well treated.

F: Well, it's gone.

T: The old spirit has gone?

F: The old spirit has gone. Maybe it was a luxury of the upper classes. Or whatever.

T: You think so? After all, do you not recall the battle of Cable Street when the British fascists were put to rout by the mob in the East End of London?

F: Yes, that is one thing, but the consideration of the refugees I would have thought was a middle class/upper class aspiration, really.

T: So you were set up as it were, in this country?

F: No. My sister went up north and I went the other way to Wales and then my parents were going to start a chicken farm in Sussex, but then my father died and then my mother started this toy firm.

T: Really?

F: Yes. During the War, converting unusable scraps of materials into toys and dolls. The stuff she used would have been burned, you see. And it was her lucky break because her first doll -- we used to keep it -- resembled Benito Mussolini and she said, "This is the Mussolini doll," and she said the only reason she succeeded was that there was no competition! The dolls improved very quickly. Actually, she had training at the Art School in Vienna so she was a good designer.

T: So things improved for you?

F: Well, at times it was a little touch and go!

T: Moving forward, what are your recollections of meeting with Julian Schwinger in Salt Lake City?

F: Well, I didn't meet him as much as I would liked to have done. Julian Schwinger came to talk to various people in the Chemistry Department, including Jack Simons. Julian Schwinger didn't have such a closed mind, and he could see that such a process in condensed matter could not be interpreted in a conventional way.

I was so preoccupied, I didn't talk to Julian Schwinger as much as I should have done. Subsequently, of course I talked to Giuliano Preparata and that was really a meeting of like-minded people, because he thought of it in much the same way as I did. Of course this may mean that we are both wrong!

T: I suppose that, in a sense, your sort of early experiences, you say that to be arrested by the Gestapo at that sort of age is something that would wonderfully concentrate the mind. It would put the sort of difficulties you have had since 1989 in perspective, I would imagine.

F: I'm sure.

T: When you think of such people as Fred Hoyle, for example, who take this very Yorkshire approach to their difficulties--that's fine, but for yourself I would say perhaps it was based on your past experience? You might have been a Fellow of the Royal Society and everything, but ...

F: I might have been dead.

T: Yes, somewhere inside yourself you would be the 11 year old boy with the Gestapo, so you just don't take some of these people very seriously.

F: Not really. No.

T: Gene mentions that he's heard that you don't aspire to such things as the Nobel Prize, and I've heard there's a lot of politics in getting the Nobel Prize, but what are your comments on that?

F: I think that's another thing which has gone wrong, you know. I know of quite a few Nobel Prizes which have been awarded to people for work which is manifestly incorrect.

T: Like Millikan, for example?

F: That's an early example, but more recently... It's accepted--socially accepted, but obviously flawed. So has it been a positive influence or not--I don't know. First of all there are a whole lot of Nobel Prizes awarded to people for work which is incorrect, or prizes which are awarded which clearly should have been shared between several such workers, and prizes which have been awarded to people who did not do the original work--that is very common.

T: Or not awarded to people like Fred Hoyle, perhaps, for his work on solar nuclear processes.

F: Yes.

T: But should have been.

F: Yes.

T: Because he was not playing the party line.

F: Well, he's had some cranky ideas which has colored the rest. The question is whether Nobel Prizes are judged for an original contribution or are they judged for the totality of the work. Or can the totality of the work detract from the original contribution. Unfortunately, of course, this has happened in recent years.

T: There have been two "branch points" in cold fusion: the nickel/light water thermogenesis or whatever you would call it, excess heat, as particularly exemplified by the Clean Energy Technologies' cell and the work of Mills and of Miley. That is one branching point which the science has taken, and the work in very recent years which points to host metal transmutations - hydrogen/metal fusion. These are two diversions away from the classical process, even if the latter would be more of an alternative explanation or interpretation, whether you look at them as great heresies of cold fusion or great branch points.

F: Well, I have commented on the light water work before. To put it into perspective, Douglas Morrison came to see me when I was in Switzerland and said: "Martin, if I were a man from Mars would you expect me to believe this?" I said, "No, Douglas, no of course not. I realize all the difficulties." So, I realize that there is a credibility problem for d-d processes. I realize that it is much more difficult still to justify H-whatever processes, and then I said I had not done enough work on that myself to express an informed opinion, but that is as far as I will go. I can see there are difficulties with regard to light water, I can see the difficulties.

Stan and I set down the protocol for the experiment we did so as to exclude as many difficulties as we could: secondary reactions, all sorts of things. Not potassium carbonate, please. We used lithium deuteroxide, the simplest thing, prepared the simplest possible way--the simplest possible system we could set down. No chemical complications.

I think it would be quite difficult to prove absolutely that there are never any chemical complications in the light water work. Also, of course, we use high current densities and they use low current densities, so there are always problems with possible side-reactions. But I would never pooh-pooh it because I think that I just don't know whether you might not induce peculiar reactions with protons. I don't know. So that's one thing I would say about that.

Now the transmutation. Of course, I can think of several ways in which something like transmutation could take place.

T: Any form of nuclear reaction is transmutation anyway. So it's a very, very small step.

F: But we do now know that there are high energy X-rays. Gozzi has observed them to over 120 keV.

T: That's a big number.

F: That's a big number, which, incidentally, can't arise from the electrons in k-shells.

T: What is the maximum for that? About 15 keV?

F: Well, whatever it is, but.....

T: It's a lot more.

F: Yes. It cannot arise from anything in the electronic shells.

T: 100 keV? No way.

F: No way, no way. So this has got to be some peculiar phenomenon. Incidentally, this is a fairly important question because, as Preparata pointed out in Japan, if you have got high energy X-rays coming out - and this goes back to Stan Szpak - lots of people then say, "Well it's soft X-rays," but soft X-rays would never get out of the cells. So they had to be hard X-rays. Those could dump their energy outside the cell, so you can see a lot of the complications with the thermal measurements could be just that people have missed the excess enthalpy with their cell design: the cell is too small, it won't catch the excess energy, and in any case it's only the lower bound that you catch, you must design a cell to trap all the energy in the X-rays. Once you have got the X-rays, you can ask what sort of X-rays, what is going on? Are these coherent X-rays? What would they do? Will they yield some sort of photo-fission processes in the nuclei?

So I could think of lots of processes which could be going on, and it will take a long time to sort that out.

T: Would you say that we are talking about systems whose complexity compares with normal nuclear physics in the way that perhaps biochemistry compares with inorganic chemistry? Are we are talking about things that are at a wholly different level of complexity, in a very complex multi-body process?

F: I think that we will find that when people have got some sort of explanation for condensed matter physics, based on single-particle descriptions, they must find it extremely distressing to now get this body of information which cannot be fitted into this framework. And there's much more to it you know, there is a great bag of physics which simply will not fit into the existing paradigm.

T: Could this be some kind of "complexity" effect in itself? That we are now beginning to understand that systems built out of units which individually behave very simply can, in conjunction, produce extremely complex effects?

F: Well, yes. I went through this in the 1947 understanding of the work of Alfred Coehn on electrodiffusion, and understanding at that time that even with the existing understanding of quantum mechanics that I had at that time, I think all that anybody had--you could then conceive of changing the conditions of deuterium in a lattice so that you would change the fusion process.

Then I did a lot of work in the 1960's which led to this idea that solutions really have to be understood in terms of quantum electrodynamics. Not in terms of classical mechanics or even quantum mechanics. It had to be in terms of quantum electrodynamics, and then came all the work on palladium which I have worked over several times in my life. There was one very big slug of work in '67/'70 which convinced me that you could not talk about anything to do with hydrogen or deuterium in palladium in terms of single-body processes. These had to be many-body processes. The explanation for the behavior had to be in terms of many-body effects, and that then triggered the cold fusion work. It's that which convinced me that it was worth going on.

I still didn't have the whole explanation, in the way Preparata has achieved the whole explanation, I only had 50% of the story. If I'd had all Preparata's insight into this I would have dropped everything else and gone for electro-diffusion, even if I'd had to do it in my kitchen.

T: It's very interesting that you are talking about cold fusion as really being a single aspect of a much larger idea of condensed matter physics.

F: I think, really, that a correct understanding of condensed matter physics in general, and electrolyte solutions in particular, is a pre-requisite for taking our next steps in chemistry and biology.

T: Biology as well?

F: Biology especially. And that's going to be more significant than cold fusion.

T: Where would you think this would lead us to, for example, in biology?

F: A totally different understanding of biological processes.

T: Could you amplify on that at all?

F: Well, no - not at this stage. I'm just writing a proposal. But I will talk to you about that in due course.

T: We made up a list of people you might like to comment on--Steve Jones, John Maddox, Huizenga, Frank Close, Mark Wrighton, Gary Taubes, Richard Petrasso and Doug Morrison?

F: I should explain to you that I have not read Close's book. I have not read Taubes' book. I have, however, read Gene's book.

T: Which was wonderful, of course!

F: Yes, and I have not read all Douglas Morrison's messages and newsletters. People have stuffed various things under my nose which irritated me intensely. But what do I think about them? Let me tell you.

Steve Jones? Well, he's an ambitious person. Let's give him some credit. He has some vision, he's very ambitious but I think his ability is not up to the vision he has. That's my comment on Steve Jones. This is not to say that he is a bad scientist, second-rate in science is very good. The problems he wants to do, he just hasn't got the technical competence to achieve them.

John Maddox? Well, he's a typical establishment figure, isn't he? We have to have people like that. He's out of it now--but I can't understand how his brain functions. Sometimes you are confronted with these people and you say, "What makes you tick? How can you function? I don't understand...."

T: "What's your problem?"

F: Yes. "What's your problem?"

Huizenga? Well, I thought he was just a front man for some organization which the DOE had cobbled together, really. I still think its a piece of disinformation, I think a lot of Frank Close and Huizenga is disinformation. If you could ever get into it, you'd find it is disinformation.

Up to a point Maddox probably as well. I think Close is a better scientist than Huizenga. A disappointed nuclear chemist who sees his field disappearing; his life's work is disappearing. And could easily be manipulated by people unknown.

However, I must tell you that at the outset, when Admiral Watkins was in charge of the DOE, I said to Stan Pons, "Stan, what if Admiral Watkins had been me and I had been Admiral Watkins? I would have done to him exactly what he is doing to us." I could not conceive of Admiral Watkins welcoming the notion that the American Universities and goodness knows who else working on nuclear physics in chemistry departments. This is where we came in. "We've got to keep it secret, we've got to have it classified! We don't know what's going to happen!" I think we will be proved right. In '88 we had no idea of the totality of the subject. We proposed to the DOE some things which shall be nameless at this stage, but we had no idea what would happen. We knew what we had got, which I think was sufficient indication it should have been classified.

And then in 89, of course, we said to the University, "We will go to Oakridge or to Los Alamos for two years and see how far we can get." And they said, "Do you really want to work with the Government? Wouldn't you rather work with General Electric?"

I wasn't asked that question but my answer would have been, "Yes, I do want to work with the government, thank you very much, I'm off to Los Alamos tomorrow." [prolonged laughter] If they would have had me!

Frank Close? I don't understand him either, really. He's a theoretician, not top flight. Well, he's OK, but again I think he has been manipulated.

Mark Wrighton? I shake my head. He's out of science now, isn't he? He's become a provost somewhere.

In days gone by when I used to be asked to referee a lot of material for promotion in the United States, I used Mark Wrighton as a benchmark for excellence, also Rick van Duyne and Al Bard from the older generation, so I obviously thought highly of him as a scientist.

Up to a point Nate Lewis too. I used him as a benchmark.

Gary Taubes? Well, nothing. A second-rate science writer. Primarily, he is a very bad journalist.

Richard Petrasso I think is a capable fellow, quite frankly. I think he is a capable fellow.

Douglas Morrison, I think, is another disappointed man. Quite a good analytical mind in some ways, but again I think he is manipulated. I think that if you look at this, you would say Jones can't forgive himself for what he did, so he keeps on trotting out these negative ideas. Jones is in a difficult moral position, and so some of his actions post-1989 had to be, as we said in Czechoslovakia, "holier than the Pope."

However, regarding Huizenga, Close, Morrison, I feel that if you really could penetrate behind the smokescreen you'd find that other people have been manipulating them.

T: What sort of people? We're coming close to conspiracy theory. Is this the "Protocols of the Elders of Britain?" What are you really saying here?

F: Well, that there are always groups of people who decide policy, aren't there? For example the Jasons. Lewis is a member of the Jasons, Garwin is somewhere near the head of the Jasons. They advise the Government. So what role do the Jasons have in this? Maybe none, maybe some. Garwin was interested, so was Teller. So who manipulates whom? Or perhaps they do not manipulate, I don't know. I don't think these things are spontaneous.

T: I think one difference in opinion--I suppose an inevitable one because of our unique approach--one difference between ourselves and yourself has been that we have argued and pushed and are, as you know, working for clear public "in your face" demonstrations. I don't knowwhether you read Rothwell's comments on the Wright Brothers. He mentions for example that these so-called mechanics actually predicted the performance of the first air screw to within a percent before carving it. They were obviously very competent people, but they were beguiled by an idea that they had to do secret deals with all these Governments. They didn't realize that until there would be some general recognition of the existence of flight, you couldn't sell aircraft.

F: This is precisely the point I have made here: "When do you anticipate that the course of public opinion will turn in favor of cold fusion?" The answer is that you have got to get to a demonstration device.

T: Well, that's what we are trying to do, as you know.

F: Yes, and I absolutely agree. We have all the science, we had systems of Type A, systems of electrodiffusion, systems of Type B, systems of Type C which made the link to hot fusion, systems of Type D - very interesting, but I am not prepared to talk about systems of Type D at the present time. Nevertheless, we focused absolutely at systems of Type B to try and bring this to some sort of demonstration device, because this is the thing which will change people's opinion.

T: But I thought we were in disagreement with yourself in this area - that we were the ones who are arguing most strongly for....

F: Absolutely not.

T: Well, I'm very glad. This has surprised me.

F: I have disciplined myself severely and constrained myself in order to try and capture this position, unsuccessfully because I find that people won't take my point. You see, this really takes us along what you should do, you have to say: "I have a sufficient understanding of Johnson-Matthey material Type A. I am going to freeze my design on that. I am going to explore the operating condition of J-M Material Type A. It won't be the best, but it's acceptable to lead to a demonstration." And I find myself in conflict with everybody.

T: So you would be very happy with our going public with a working excess energy machine.

F: Good luck to you.

T: Well, if it doesn't work, I'll be on the phone to you. You can tell me where I'm going wrong.

F: One of the things which was very clear all the way through was that if you followed a certain line of development, which I'll call the Utah branch--the Utah system, you would get to a demonstration, but then the point is why don't people want to do that? The question is: who wants the Utah line to succeed?

T: Well quite, but I would have thought for example that the people at Toyota would see the benefit.

F: They've got their own axes.

T: Yes, but in 1947 the basic idea of making a point-contract transistor was released to the world, and labs all over the world then spent a fair number of years before the very first transistor radio was able to be made. In other words, the thing was out there and everybody was working on it. I would have thought that, for example, Toyota had stuff to sell in demonstration kits, which I think could have been done if the effort had gone in that direction.

F: Well that was very clear. I saw ICARUS I as a stepping stone to a low-cost demo device.

T: Could you define ICARUS?

F: Isoperibolic Calorimetry Research and Utility System. This is the data acquisition system, the whole lab with thermostat tanks and a data acquisition system...a data interpretation system. This was all behind Icarus 1, the thing which went to NHE Labs. I saw this as one of the logical developments, to make a low-cost version of Icarus I which people could play with. And more of my foolishness, more of my follies, yes. As I said, I should have called it Daedalus but I couldn't think of a good acronym for Daedalus.

T: The Wright brothers had their arms twisted into giving a public demonstration, and the gasp from the huge crowd who saw them finally fly was the sound of the paradigm shifting. If Toyota had produced and sold demonstration devices--with a great public press conference announced the sale of the Size 1, Size 2 and Size 3 cold fusion demonstrators, things would have changed.

F: But tell me, what interest would they have in that?

T: Well, I think their longer term interest might have been served, do you not think?

F: No, I don't think so.....Look, if people had said to me how do you develop this thing into a demonstration device, I could tell them. In fact, in '89, when we thought the Select Committee of Congress would come out of Salt Lake City, I dropped everything else in order to try and make a demonstration device, but this is a non-trivial exercise. Very difficult to do. Since then, if people had said to me, "How do you set about making a demonstration device?" I would tell them, but nobody asks me that.

T: But you are happy with our approach, the Ragland cell?

F: Yes, but I have my own view of how I could do it, but I now don't have a budget, so I'm not going to do it. I'd need a lab and I'd need a budget and I haven't got that so I'm not going to do it.

T: Well, fair enough, but you feel our prospects are good?

F: Well, yes.

T: It doesn't really matter what kind of device it is.

F: There will be a hundred different devices.

T: Yes, exactly, we don't really care. We just want to make sure that one of them gets in front of enough people, so that interest will be then taken.

F: If I'd pursued this as a piece of science, I wouldn't have done the research which I have done.

T: It seems to me that you and our group are much closer in outlook than I had thought we were.

F: I think I discussed with you the question of why we did it and, here, I think again if you are doing the transcript I would put this in. This whole thing started off in 1947--at the end of 1947 when I ran across the papers of Alfred Coehn--and I realized that there was a very, very big problem here the incompatibility of the dissolution of hydrogen as protons in the lattice, and at the same time, the high diffusion coefficient, the high mobility.

The first paper was published in 1929, and in fact Alfred Coehn showed that the hydrogen in the lattice had a unit charge. I knew about the work of Lange on the Galvani potential in the lattice, and the work of Gurney and Butler before the War on interpreting the quantum mechanics of processes at interfaces; and you could stick all this together and come to a conclusion. And at that time there was also a lot of interest in exploding wires and making metal films, so I realized that you could create very strange conditions by applying a field to the wire. But that would have involved rather heroic instrumentation, and I parked it in my head. Then in the early 60's I came to the realization, as I said to you, that we have a very poor understanding of electrolyte solutions.

So on we go from there, and then I come to the end of the 60's, the beginning of the 70's and realize that the behavior of the hydrogen in the lattice or, of course, at the interface, can only be understood in terms of many-body effects, so now the whole thing is complete; you know--bang-bang-bang now we can go on--there is enough basis to think that we should go on to explore whether the nuclear cross sections are changed.

Well, I think that was then the point at which I had decided it was worthwhile starting, but I still didn't start it because I was still in full-time employment and I realized that this research was incompatible with being an academic, it was too outrageous. So, in 1983 Stan and I discussed a number of projects--we had room for one more project in Salt Lake and we had several options. I told you one was the behavior of electrons in metals, one was the strange thing to do with gravitation, and one was cold fusion and there was a spectroscopic one and the spectroscopic one we needed too much money for. In fact, we needed too much money for all the projects except the one on cold fusion. So we decided to do this thing in a rather low level way because we didn't really think it would work, so we had five years of on-off experimentation. But we did actually have four different systems which we defined, which would be interesting, of which the lead one was going to be electro-diffusion; and the reason we did the thing in the particular way in which we did it is a long story.

So then the results were really totally surprising, and we got into all these other subsequent difficulties. The best result in a way would have been if we had found nothing. You know, historically. I think even we would have been happy, but in the end we had enough information which did not fit non-Poisson distributed neutrons, certainly something in the gamma ray spectrum--goodness knows what - ray at the end of '89.

T: And does this not bring in the whole question of the moral dimension in science?

F: Well, the question is, what do you do with a set of results? The publication was premature, there's no question about that. Our original protocol called for three independent methods of measuring the excess heat, of which we had only done one, so we did not want to publish until we had three independent measurements with as much confirmation as we could muster. But we even had some indication of helium, you know, but that was unpublishable, not even we could be persuaded to publish that. We needed huge resources for that. I don't think one could have done it in '89 actually.

T: Might you consider the helium to be less the result of fusion, more the result of the stimulation of alpha-emission?

F: Well it could be. You'd always have to budget for that.

T: When I asked about the moral dimension, I was thinking less of how well one is fitting into the protocols of science, I'm talking more of the moral dimension to society as a whole, of the individual scientist confronted with an interesting result. There is a problem, is there not?

F: Well it depends on what sort of person you are. I'm sure that most people with the information which we had in '89 would have suppressed it.

T: For good and sufficient reason.

F: Because it just didn't fit in, they didn't understand it. Unfortunately, I think I understood enough about it to realize it was possible.

T: That it wasn't quite as absurd as it looked.

F: No, because that's how we came in.

T: Because you came in from a concept which was rather more sophisticated a concept than simply shoveling deuterium into a lattice

F: But that's crazy.

T: But people do, as I say, people think of the Wrights as a couple of lucky mechanics.

F: But they were very good engineers.

T: And very good scientists as well.

F: Yes, indeed.

T: Yes but most people see them as a couple of lucky tinkerers and most people see your idea as very naive.

F: This is because they cannot conceive that anybody would ever be able to work something out.

T: Well, I think it's the same reason that Shakespeare can't have written the plays because no glove-maker's son could produce work of such high literary quality.

F: But they don't understand that because they can't do it, that somebody else might.

T: I'm beginning to understand now what you are saying, that you had a vision of solid state--shall we say physics, shall we say chemistry--which included this, and your reasoning for cold fusion was simply as an example of something much more complex.

F: That's true as far as I am concerned, yes.

T: And so the common idea that you were simply thinking in terms of, "Ho, ho, let's squash some deuterium and make it fuse!" is as much of an over-simplification as saying, "Oh, the Wrights were lucky because they happened to have an engine that would pull an airframe."

F: Yes, I have been through this thing before, in a much less extreme way. People have said, "You go in the lab, you fiddle about and get this result and then everybody else finds that you were right, you must have just gone in the lab and fiddled about and got this peculiar result."