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Obituary Note

On Martin Fleischmann: An Obituary and More

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Abstract

After receiving a Ph.D. Degree from Imperial College, Fleischmann (F) went to work in the University of Newcastle in the U.K. and spent productive years there (metal deposition, micro-electrodes, and electrochemical extraction from mixtures metals). His reputation grew at a time (1950–1960) when electrochemistry was a popular subject for university research. British Electricity saw their chance to get a star performer down to Southampton University as professor, so they paid for a chair for a person who could attract research support and found that F could fill the job, although it was open to other candidates. Fleischmann occupied the chair for only a few years, less than had been foreseen, and retired from university life some years before it is usual. By this time he already was a Fellow of the Royal Society and also a director of the Max Planck Institute in Berlin. Fleischmann made a colleague of Stanley Pons, head of the Chemistry Department in the University in Utah. Pons (P) was of independent means and could fund research which the two of them might think out together. Fusion means coming together and F saw in electrochemistry a high performance way whereby this might be done. What they found was that the heat evolved in a particular deuterium solution when they passed current through it was larger than it should have been according to known chemical theory. In addition to this they observed some neutrons. They suggested that the extra heat was due to an unknown nuclear reaction. What was unusual about the next step was that F&P got on the McNeil–Lehrer Hour and announced that they had been the first to carry out a nuclear reaction in the cold. After a short time, the nuclear chemists of the world turned against F&P and said that their claims must be all nonsense. The anti-Fleischmann opinion expressed at meetings was so great that they decided that it would be a good thing to escape to some other country. They had a friendship with a very wealthy man, Mr. Toyota, and he had already founded some laboratories in the South of France. He offered F and P laboratory space there and they could move their operation to it free of the negative atmosphere which reigned in America. At first the news which came from the laboratory in France was good. Alas, this was not maintained and after two years they split up and P retired to live in France whilst F retired to Tisbury in the U.K. But F's creativity would not lie down and he was soon to apply something new, Quantum Electrodynamics. He made a colleagueship with a well-known Italian physicist Preparata. However, fate was not kind to F and he discovered that he was suffering from Parkinson 's disease. This is a slow disease but it's incurable. However, for a couple of years F continued to attend meetings and make intelligent remarks at them. He died on 3 August 2012. Was F a brilliant theorist who did not have time to realize his true vision or had Jack the Flash, his nickname, flashed too much? The field that could have been his greatest is now called condensed matter nuclear reactions.

Keywords: Eagerness, Ideas, Imperial College, Quantum electrodynamics, Royal Society

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1. Introduction

My first memory of Martin Fleischmann was a letter of application to me, then a young faculty member at the Imperial College of Science and Technology in London, to become a graduate student working under my direction.

I was only a few years past my own Ph.D. and already had 10 graduate students. I thought it would be a bit too much to take on another. I deflected Fleischmann's application to another person who was around the same degree of development as I. This man was Dr. J.F. Herringshaw and turned out to have a temperament which was the opposite of that of Fleischmann's, a pipe sucker, and he let it be known that he thought that the objective of his job was giving of lectures rather than the supervision of research.

The first impression one got of Martin Fleischmann in those days, and he changed little in appearance and manner throughout all the 67 years in which I knew him, was, well, I can choose two words, "eagerness, flashy."[a]

2. Imperial College

[b] Dr. Herringshaw's office was about a three minute walk from mine and Martin Fleischmann of course had his experimental bench in Herringshaw's office, but it was not long before Martin found out that there were quite a few fellows within a five-minute walk of the office in which he worked who were capable of providing discussions which was more than you could say for Dr. Herringshaw. I myself was available for discussion too, and there were several occasions in which Martin Fleischmann asked me directly if he could discuss a point with me and we took out for the stone corridor outside my room and walked up and down there: my usual platform for discussions with graduate students.

Roger Parsons and Brian Conway were both my graduate students at this time, but they were already recognized as advanced and could discuss too, so I think that Fleischmann got plenty of advice as he built up his Ph.D. thesis.

My corridor in the imposing modern looking Victorian constructed buildings holding the Chemistry and Physics Departments at Imperial College was a long one and the floor was made of stone. One could work as long as one wanted and seldom get interrupted and that's why I preferred to begin many discussions with my graduate students by pacing the corridor with anyone discussion-worthy.

Martin Fleischmann was seen often in my corridor and with my group, particularly on the weekends off and trips which we took together for social reasons[c], so that it seemed to be understood, as I have been told in later years that

[a] After I had left London in 1953, F was in the University of Newcastle and I heard, "from the grapevine," that he had developed there the nickname "jack the flash."

[b] It's worth saying a word about Imperial College where both Fleischmann and I got our Ph.D.'s. It is the nearest that the Brits have to MIT and certainly the principal education establishment in England which tends to look towards applications as well as giving a sound education in the fundamentals. Imperial College surrounded itself with high barriers. In the days in which I was teaching there entry students were selected by considering only those who could prove that they had come out first in their high school in chemistry. After that the firsts were called to London in the examination halls. There they underwent a stiff examination in chemistry, - all 500 of them, - and we selected the best 50 for further training and perhaps to graduate work. I had one year in which I was an examiner of this barrier and it certainly wasn't an easy exam. So we had the cream of the cream.

[c] Our trips were salted with girls and we used to collect them from the local nurses training institute which was around the corner from Imperial College Chemistry where we worked. Unfortunately the building in which they lived received a direct hit around about 1944 when Martin was still an undergraduate and as I was a member of the fire brigade of Imperial College Chemistry had to stand by while their building burned down. We didn't have any more girls from there!

part of the discussion and therefore the direction of Martin Fleischmann's thesis depended upon me as well as upon Herringshaw. However, I must admit that I don't remember much of it now.

One of the ways in which Martin Fleischmann helped me as much as I helped him, was with visitors.

In England the feeling is that the scientists who all have teaching positions at the great universities, government rewarded, should be open to serious citizens who would call in and ask a few questions.

When the discussions with the visitors began taking too much of my time, I would call on Martin Fleischmann and see if he had time to meet these people.

After stating the subject which they wanted to talk about, I would retreat to my desk and continue my work.

The story was the same for all of them. It was impressive how often Martin conjured up some theoretical hypothesis to meet the type of result that they were trying to expand on. Of course the level at which Fleischmann would present his ideas was a rather high one and sometimes involved beginning with a second order differential equation and some applications of Fick's law. It took about five minutes for the visitors to think that they had no place in this and make an excuse for catching the earlier train.

I would like to give you two more snippets from my contacts with Martin Fleischmann in his graduate student days.

First of all, I would like to stress that in my knowledge of him I never experienced harsh words. He was a smooth talker and bountifully possessed with ideas, though some of them were not clearly stated.

Long after the time when he got his Ph.D. and moved to Southampton and had several years there he was awarded with what most chemists regard as not quite the Nobel Prize but a good substitute indeed, A Fellowship of the Royal Society! He visited America about this time and of course came to see me. At this time I was at the Texas A&M University which is better financed than MIT because of the large amount of oil money which is at the disposal of the Board of Directors.

Martin, just after his FRS, and talking to me in the visit never mentioned it and jumped right into a scientific discussion. Before he left I briefly congratulated him on his attainment and all he said was, "oh, well..."

One story I would like to tell relates to the early seventies and I had been on a trip to Moscow to see Frumkin. In those days flights directly from Moscow to New York were absent and one had to change in London and this I did and thought well, why not go and see Fleischmann and have a talk with him. I met with Martin on a Saturday morning and we had lunch in a pub and I tried to discuss the electrochemical problem which was worrying me. We retreated to my rental car that I had used for coming down from Heathrow and I tried to engage Martin to open up some fundamental ideas, but it was difficult. He never wanted to engage with me and kept on telling me that I was quite right, excellent, etc., but of course that was not what I was looking for. What I wanted was intellectual engagement. I had had many with him when he was a graduate student. When I got home to Philadelphia I looked back on the day in Southampton as a waste of time.

So it was not easy to get Martin Fleischmann to open up and it may well have been that he objected to giving his ideas to other people, but I think it was a different thing. I think he needed to be alone and as with many people including myself in open discussions with people except with very rare people like Brian Conway or Srinivasen, seldom develop in an original way.

This next instant comes from the 1950s whilst I was still in London and Martin Fleischmann had his Ph.D. I want to describe to you his persona at that time which I remember well. He really did look like a "European intellectual." He was dressed usually in a jacket which certainly wasn't made in London. His most frequent phrase in those times, "what a gas," was to the fore in his conversation and he smiled and laughed a lot. He was a jovial sort of fellow to be with.

Of course, there is no doubt that one of Martin's faults was that he overstated himself, but do not think for a moment that I am suggesting that there wasn't behind the overstatement a REAL BRAIN, but he did not agree with the British habit of understatement.

Well, let us now shift a bit in the direction of the McNeil Lehrer Hour (1989) and all the unpleasant times that we had to go through then. We haven't met Stanley Pons yet.

3. Fleischmann after Imperial College

I think that Martin's thesis must have gone through around about 47 or 48. In spite of my relationship in the work before the thesis I do not remember reading it, but I do remember that the first job he had after he left with a Doctor's title was an academic job in Newcastle and those who are only faintly familiar with the UK, you can think of Newcastle as about three-quarters of the way up the eastern side of Great Britain.

Reginald Thirsk was the man with whom Martin collaborated there and Thirsk had his day as a leading electrochemist in the country a little bit before the time Martin gathered with him in Newcastle. He was a slow speaking man and not one who would rush at a new idea, but there's no doubt that Reginald Thirsk was a full-time electrochemist and had a good reputation for solid work.

There was indeed a time around about 1960 when my own co-workers Damjanovic from Belgrade and Asa Despic from the same city were collaborating with me and my colleagues particularly in the early stages of metal deposition and Thirsk and Fleischmann were doing something very similar. It wasn't metal deposition at its complex multi crystalline, but was what happened when you turned a current on and metal ions deposited on the surface of the originating metal and we made it simple by having, for example, silver on silver, because if, of course as we did later, you had A depositing on B then everything was a good deal more complicated.

Here I think Martin Fleischmann's ability to handle equations which showed up with the visitors, came out in full and how much he helped Thirsk and how much Thirsk helped him I cannot say, but I suspect that the equational part was at least in a fair amount due to Martin Fleischmann because he was facile with manipulations of the type that you need when you are considering surface diffusion of metal atoms, meeting growth sites and rotating spiral.

Another thing that Martin Fleischmann did and became well known for in those days, the late sixties, was to help with the equations for porous electrodes. It is of course completely wrong to tackle fuel cell electrodes while assuming that the depositing substrate is a plain surface. It's all pores that you have to deal with and most of the porous electrodes used in fuel cells have some kind of catalyst deposited in the pores. So you have to consider the pores, the diffusion in them, the growing resistance of the solution with depth of the pore and finally the electron transfer and redox reaction which occurs on this metal catalyst.

Then there was another line which Martin Fleischmann developed. There was the micro electrode field. This was something which I had to teach myself I think independently of Martin Fleischmann, mainly the fact that below a certain radius you can get a much higher current density on the tip of a porous electrode so that dendrites can be developed quite easily and sometimes you can make some interesting looking patterns especially if you show how they can easily dissolve again and etc., but Martin Fleischmann made a useful kind of short book on micro-electrodes in which he published around about 1980. You can see how the micro electrodes and the diffusion problems which gather there towards the tip do have relevance to the porous electrodes. Its all electrode kinetics but not just depositing on a plain surface.

One of the fields which Martin Fleischmann helped a lot in his later days in Southampton was the use of spectroscopy on the surface of electrodes. Obviously the major problem is the liquid which prevents one using the many methods used in a vacuum.[d]

[d]Whilst I am talking or hinting at Fleischmann's character let me tell you a memory from early times when I knew him well in London in the graduate days. We all had nicknames. I am talking about this first group that's in the famous picture of 1947 and I do not know how these nicknames get made but they're remarkably accurate in many cases. We called Fleischmann "ephemeral transient and diffuse." I think this does get the middle part of Fleischmann, the lower part being that he would just be diffuse and not really tell you anything and the upper part being that he had the good ideas, some which he had developed with Thirsk.

Most of the time Martin bloomed and the work that got his FRS perhaps was done when I was away in America or Australia, but I did keep an interest in it and looked at his publications not only on those he sent me but I searched for them. Roger Parsons is the man who knows most about those days and I expect he will write something about them in the same volume to be published.

4. The McNeil Lehrer Hour and what Followed

We are now in the 1980s and Martin Fleischmann has retired early from the University of Southampton and taken up a moreorless freelance position to tackle whatever he wanted to do. I expect that until Stanley Pons's writes his version about it all we shall not know too much what happened in those days at the University of Utah. They talked about going for long walks and discussing what interested them most and this of course was fusion. At this time, fusion was a much sought after but totally mercurial and unavailable source of energy. Because of the bomb everyone thought that taming fusion would be the ideal way for getting more energy for the future. There are still a few people who think that, but two methods failed. One attempted to hold mixtures of hydrogen isotopes at temperatures near those of the sun in a magnetic field!

Another idea was to hit the mixtures of deuterium and tritium with a tremendous whack delivered by a laser which was two houses long and that failed too. The laser was set up at great cost and it failed. I remember being in the University of California just after the trial of the laser method was found to be a failure and hearing that the calculations now said that the power of the laser would have to be increased by ten times! It was news like this that delivered great blows to the attainment of fusion but it made a person like Martin Fleischmann more and more eager to contribute and his goal was to find out if it was possible to make a fusion reaction work in ordinary temperatures.

I suppose that the failures of these high pressure super energized methods was the basis of Martin's thinking that he would go in the opposite direction and try something with very little energy but with some thought behind it. Maybe that would work.

What Stan Pons and Martin came out with in March of 1989 was the electrolysis of deuterium oxide in which they had dissolved LiOD, lithium deuteride. Electrolyzing it they found something which excited them. It was, and everybody else thought this was all just due to instrumental error, that when they measured the heat of the reaction more heat was being given off than was classically possible. It was not much, perhaps 30% of the total heat energy which was evolved in the normal functioning of the electrolysis of deuterium oxide, but if the calorimeter worked well, it was significant.

Well, after the announcement on the McNeil Lehrer Hour there was a great counter reaction and much of it went on trying to hit Fleischmann and Pons' calorimetry. It took a great time, perhaps as much as five years for many who were expert in the calorimetry area really to believe that the calorimetry that F and P used was good enough. Eventually with EPRI support in his hands, it was Mike McKubre who built the ultimate calorimeter and his results were really believed.

So at this stage we were left with, as Martin and Stan put it an unknown nuclear reaction. Why was it called nuclear? Well, for one reason there did not seem anything else. Also there were a few neutrons.

I myself wrote a paper entitled "Eight Explanations of the Pons' and Fleischmann Effect," and in it I made simple calculations of all sorts of things that could happen and none of them came up to the needed heat and so although the title of my paper sounds threatening to F&P, it's the other way around. It looked as though there was no other explanation except something nuclear.

So I think it would all right to put the time now as somewhere about 1990. I was at Texas A&M University but it was getting late to be there because I was being punished so much by those who said that my support of Pons and Fleischmann had condemned me. "Obviously he must be wrong because everybody knows you cannot get fusion and Bockris is supporting them." I had to bear a very great deal of criticism because of this and indeed the criticism spread to the university who then subjected me to two academic trials, and although the final answer was very good. Nevertheless, it's like having mud slung at your face and it's difficult to wipe it off.[e]

In the meantime (1991) Fleischmann and Pons had developed a relationship with Toyota and Mr. Toyoda offered the two miscreants an escape. They could leave all the mucky stuff behind in England and America and go and enjoy themselves in Southern France and of course prove their point because they would have excellent laboratory facilities and money and help there which they couldn't get elsewhere and so it seemed to be the ideal escape. Now they were in the Japanese/French laboratory for 2.5 years and I am glad to tell you that things looked good enough for two years. Looked good means that they were able to get more efficiency heat which is what they wanted. Ideally they wanted enough heat to make something commercial, but then, I do not know if either of them have an explanation, things began to go wrong. They could not get the results they had had in 1989.

One of the difficulties of their situation was due to poor management on the part of the Japanese funders.

A man like Martin Fleischmann will not work well if he is restricted in what he does or if he is told what to do. Now he was under supervision and made to write reports and do what the sponsors wanted so Jack did not flash anymore. Whether there was a bad interaction between Fleischmann and Pons I do not know. I suspect there was. Martin Fleischmann could be dictatorial and the man who says what had to be done. Stanley Pons saw that things were failing and it may be that at last he turned against the man in whom he had put his trust and walked out on Martin Fleischmann? I do not know. Something of both?

Anyway, it broke up and Martin Fleischmann headed for Bury Lodge, Tisbury, whilst Stanley Pons, he was independently wealthy, settled down in France more or less on the French Riviera, a pretty good place to retire in.

5. Fleischmann and Preparata

After he had retired to England, Martin Fleischmann was not to be forgotten and not to stop work. He continued to attend the meetings on Cold Fusion and usually made some pithy comments at them although I only attended up to the one in Vancouver, but Martin Fleischmann, keen on high sounding theory had developed an interest in quantum electrodynamics and he found in Professor Preparata a soul mate. This is the kind of stuff Preparata liked indeed and Preparata had an attractive model which was a good thing for discussion. It consisted of the idea that in solids there would be patches in which all atoms reacted together and gave rise to a glow and this was of course the heat observed. This was the type of thing to which Martin Fleischmann contributed after he returned to England. I don't know whether it produced anything permanent but at any rate it was joyful to them both and to those who were watching and hoping.

Preparata died first and then Martin Fleischmann entered his long illness.

6. Fleischmann after Preparata

Until he was stricken with Parkinson's, Fleischmann kept up regular attendance at the international meetings on Cold Fusion and as said by those who were present at such meetings he usually made some consequent comments and often praised the younger men coming up with new results.

I think that as I remember, perhaps far back on this matter than most, Martin Fleischmann was really keen on trying to get people to infer that that was how he had come to his great conclusion about fusion, but I think it was an entirely different idea. What would happen in the Nernstian Equations if we went to high pressures and I think that was the origins of his thoughts about fusion when he first talked to an American audience? But to say that your basic idea came

[e] My wife did much to help me. A lawyer's daughter, she supplied my lawyers with legal points which made onlookers see there was two sides to this mudslinging game. But one thing she said spread among the governing body of the University. She had spent a year in Vienna under Nazi rule, forbidden education, and she said that her years at Texas A&M were worse for her than that that Nazi rule.

from a Nernst Equation 1903 was not good. It was much better to quote quantum electrodynamics which we had heard about for some years after 1990.

I have four long letters from Martin Fleischmann after 1990 and I have given copies to a man who said he is interested in the history of this whole development. The letters I have from Martin were very interesting. They dealt really with politics and philosophy.

One of Martin's ideas which I believe is false was that the military had to do with delays and obstructions. I do not know whether he thought of CIA or the British MI5 but I believe both of those organizations worked together but it would not be worth developing as I am sure that his idea was not correct.[f]

Parkinson's disease is a death sentence but it is slow and I knew something about it because Ernie Yeager, one of my closest colleagues in electrochemistry, died from it. The early stages in Parkinson's is easy on the patient and by taking the right kind of drug, he can have normal behavior for maybe three hours, give lectures, etc., but of course as with all drugs you have to take more and more until it becomes impractical and you have to prepare for adventures in the next plane as the spiritualists call it. So Martin Fleischmann took a path quite parallel about ten years behind Ernie Yeager.

I found that no visits at the end time are a good idea. I had known both men for so long and so I never saw either man in their setting sun days and I think it's a good way to end this obituary by saying that when the sun shone on them, it seemed very, very good.

[f] The fact that there is a relationship between the CIA and AISIO the Australian equivalent was proved in my own case. When I was in Australia I developed producing hydrogen with light and had I succeeded a bit more than I did and increased the efficiency of recovery from the 9.6% to say 15 or even 18%, I might have challenged Exxon itself. So it was not particularly surprising when one of my co-workers who was keen on Japanese fighting styles, dressed up in his Japanese Togs, one night about midnight saw that my light was on when he came across the bridge joining my part of the Flinders University to another. He thought I might be working late and so he wanted to say goodnight Prof so he opened my door and said: "Good…" Before he could say a second word he found there were two men at my desk taking photographs. They decided that rapid exit was the next part of the program and so they dashed past my graduate student, knocking him aside and made it outside the university and I suppose they had a car and went home, but my own interpretation of the event that was that that was ASIO and I suspect that somewhere deep buried in the record is that CIA had told them to "See what Bockris is doing." In case you think this is absurd, you should recall what would happen if somebody found an easy way of getting hydrogen out of light and water. In fact there is a group in the California Institute of Technology at this time who has got very strong funding just to do that, and of course if it is done and accepted and the whole thing worked our financially then the oil companies would have to shiver. In fact I used to have several dinners per year with the reigning CIA man in Philadelphia and when I told him that I was going to immigrate to Australia he said (typically) that of course he knew that and he thought that "we might be in contact later". I think that that meeting of my graduate student and the men at my desk were the result of what he said.