John Bockris on Modern Electrochemistry and the Start of Cold Fusion

Marianne Macy

In July 2008, I had the pleasure of interviewing John Bockris as part of the New Energy Foundation's Cold Fusion Oral History Project for the Marriott Library at the University of Utah. Sadly, John Bockris passed away on July 7. At the time of his death, we were working on selecting excerpts from his oral history to publish in *Infinite Energy*.

In 2000 for *Infinite Energy* #32, Gene Mallove worked with Bockris on a comprehensive history of his transmutation work at Texas A&M and the subsequent issues that arose (see http://www.infinite-energy.com/images/pdfs/MalloveIE32.pdf). We covered this period of his professional career in Bockris' oral history, but the following excerpts instead focus more on the important beginning of contextualizing electrochemistry in the 20th century, and how the evolution of that field and work done by the major electrochemists was influential in the story of cold fusion.

I asked Bockris, who wrote the seminal two-volume *Modern Electrochemistry*, to define electrochemistry.

John Bockris: I think it's pretty easy to define. It's the combination of electricity and chemistry. In other words, if you're not an electrochemist—if you're, let's say, an organic chemist or analytical chemist or inorganic chemist—then you're dealing with atoms and molecules and the various things they do and the forms they make and the reactions they go to, but you don't focus on the fact that electrons may be interchanged at interfaces, in particular at interfaces between a metal and solutions, the most usual way in which electrochemistry comes in. And so the electrochemical part then is whatever happens when electrons exchange with solids in the presence of solutions. There is another definition that I would like to make, quite a different one, and it was given to me a long time ago by a Russian chap and let's see if I can remember it. "Electrochemistry is the science whereby electricity makes things and things make electricity." What he meant by that is that electricity making things imparts an electric current into a solution and here if you have copper metal being plated out, you've made copper. That's making things by passing a current in and then if you want to do the fuel cell thing, you put something in there—hydrogen, oxygen—hey, you get electricity out. So that's the reverse. Both those things—electricity in, electricity out—and substances in between a solution and that's electrochemistry.

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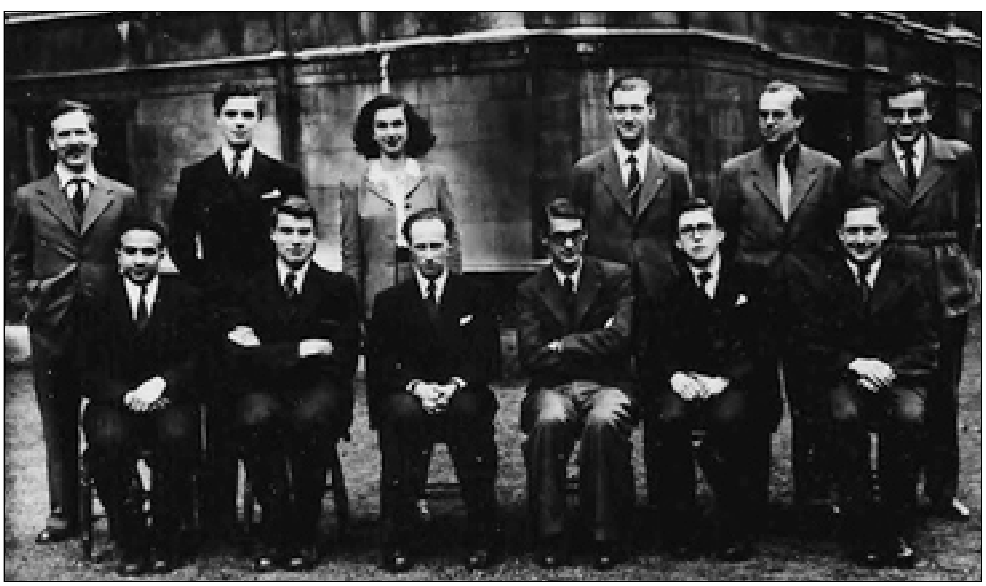
John Bockris was one of a handful of 20th century scientists who played a breakout role in modern electrochemistry. Michael McKubre of the Stanford Research Institute attributes what became "modern electrochemistry" to an influential group, among them Bockris. What distinguishes modern electrochemistry? John Bockris gives the context.



Imperial College Electrochemistry Group 1947-1948
Back Row: J.B. Reed, J.W. Tomlinson, H. Rosenberg, E.C. Potter, A. Wetterholm, M. Fleischmann
Front Row: A.M. Azzam, R. Parsons, J. O'M. Bockris, J.F. Herringshaw, B.E. Conway, H. Egan

John Bockris: I arrived in London during World War II, and took up a graduate studentship. I was about 20 years old and electrochemistry really was an old fashioned bag of tricks, if you want to put it that way. It hadn't any new inventivity in it for years. There were people who were using it—electrocoplaters were using it, electrodepositors were using it. All the knowledge of it goes way back to Nernst in 1901. And that was how it was when I got there. There was at least one group in the world which was fully pushing ahead, and that was the group in Moscow under a man called Alexander Naumovich Frumkin. He had a large group, about 100 people. And that would be considered enormous in this country; my group was never more than say 45 or 50. But he had these enormous groups, which was quite common in Russia at that time. And I managed to read his papers during the war because any direct sending of things was not going on but the Russians used to send the U.S. new things through Portugal and Spain. These were neutral countries, and then the British would send them to London, and that's how one kept in touch with Moscow. And so I got the scientific journals from Moscow during the war and I used to go down on Saturday morning to the Chemical Society in London and read the latest from the Russians. So I must acknowledge that as a lot of my pulse to take electrochemistry away from this old fashioned rather dark, lonely, smelly background into something new and something productive and something exciting. And to some extent, it's all come true because of course where we are at the moment with transportation is on the edge of a totally electrochemical transportation. People don't recognize it, but hybrids are half electrical. They are electrochemical. But the main thing is that the future is a fuel cell future and I think that within ten years and perhaps less we will see 10 or 20% of the new cars being built will be fuel cell driven cars with hydrogen as a fuel and oxygen from the air. And I think that is part of the dream which I have had and have pushed all along.

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John Bockris came to England from South Africa much earlier, at the age of two when his parents divorced. He left England when he was 30 and came to the United States. What were the antecedents of hydrogen fuel cells in Bockris' thinking and work?

John Bockris: I got to think about things which are particularly modern. Pollution, that wasn't thought of and discussed in paper anyway until 1959. But I was doing a lot of consulting for companies and they were already worried in 1959 about CO_2 and gasses floating over Los Angeles and the smog there, etc. That was already worrying them internally; they didn't want to talk about it yet, but it came out eventually in the newspapers. I was aware of it and I could see that it would be solved if only the gasses that we used were no longer CO_2-producing. You can explode hydrogen because it reacts with oxygen in the air. And that reaction can be controlled and made into a motor, a hydrogen driven motor basically similar to the ones which you now drive with gasoline, but about twice as efficiently with a fuel cell. So that's the way we're going to go. It's going to be fuel cells. But for a little while, maybe 10 or 20 years even, we might have to go through a stage with what I call plug-in hybrids, which would be totally electric. They'd still be battery driven to hold the electricity and the word plug-in comes from the fact that you plug it in for charging.

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How did fuel cells compare, in Bockris' thinking, to electric cars?

John Bockris: With an electric car, if you just went to buy a newspaper, you'd still plug it in for the 10 minutes, which would keep it charged up. When you're home, it will be plugged in and ready for the next trip. If you want to go on a 300 mile journey, it can easily be arranged that instead of stopping for seven hours to recharge it, you just rent a new set of batteries which can be exchanged in five minutes instead of seven hours. So the recharging business is not a great thing against electric cars. The reason to go into fuel cells is not that—not getting rid of a battery because it takes time to charge it. The reason to go into fuel cells is it's twice as efficient, and so basically you can say that it's half the cost. The heart of modern electrochemistry is fuel cells.

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How did John Bockris' evolution from modern electrochemistry to fuel cells occur?

John Bockris: For some reason I can't now fathom, when I was an undergraduate student I was interested in magnets and there's a small area of chemistry called magnetochemistry. And this was my original dream; I turned the key, made extra notes and I studied it privately. Magnetochemistry. Well then I got my first degree, my B.S. degree, the next thing was to go to London—remember, the war was on—to do research. So I went out to Imperial College, which was the number one part of University of London which does science and engineering, and I interviewed there and then they said, "Well, what exactly do you want to do?" And I said, "Magnetochemistry!" And there was a sort of hollow silence, a dead silence, and then they said, "Well, we haven't got anyone who does magnetochemistry at all." So then the man I was talking to, who became my supervisor, a man called H.J. Eddingham, he said, "Excuse me a moment, I'll be back." He came back with another professor whose name was Emelaius. And he took the interview over then, and he said, "Well you know people talk about magnetism and electricity, it's not so different. You can do electrochemistry." I said, "Well, alright." That's how I got into electrochemistry.

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How did the evolution of magnetochemistry compare to that of electrochemistry?

John Bockris: It turned out that magnetochemistry and electrochemistry were both dying fields, but magnetochemistry was only just with us, whereas electrochemistry has some uses. And then came one of the miracles of which I'm totally unresponsible. When NASA started to go into space, they chose fuel cells for the space vehicles because they were lighter to carry the same amount of energy. Thus in a space vehicle everything is about "how much does it weigh." Their thought was, "It's no use taking batteries; they weigh an awful lot. Just take fuel cells." Fuel cells were then only just research items; they'd been around, with a little bit of research for a long time. But once NASA started putting money into it. . .And for about ten years, my group was the leading group on the development of fuel cells on the fundamental side. I'd already gone to Pennsylvania. I was at the University of Pennsylvania from 1953 for almost 20 years. And I left Penn after 19 years because of two reasons. One, a general one that has nothing connected with me, but there was the student revolution around that time and students were causing a lot of trouble, you know coming into your room and roughing up your papers. They didn't actually hit you, but they interrupted things; they were very difficult to deal with. And that was one reason. The other reason was I didn't really like the turn of events in the department because I had been working for 19 years there with great freedom, but around about 1972 there came a new department head and he didn't like electrochemistry at all. He wanted to go in for fundamental physical chemistry. He thought that research should all be done in terms of gasses, and that liquids and solids were too complicated. So he didn't like me and I didn't like his policies. I had been attracted to Australia and thought it was a grand place to live in and also thought, which turned out to be about ten years too early, that China and Japan would develop and where would they look for the raw materials? Aha, whole basket of them down in Australia, they've got everything. So I thought that Australia would boom and it has but it happened after I left. I joined a new university in Adelaide, Australia called Flinders. I was there for a happy seven years. But the reason I left there was that when the centenary came, the chemical society in the United States, American Chemical Society, invited me to come for the centenary and they would pay everything, and so I came.

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During the centenary, John Bockris ran into the famous physical chemist Henry Eyring from Utah, a meeting that would change the course of his professional career.

John Bockris: One of the things he said to me was, "Bockris, what are you doing down there? Everything that you're interested in happens up here. Come back!" So, it took me two years to find the right place to come back.

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John Bockris went to Texas A&M to work at their Institute of Energy, and later in 1989 began cold fusion work, in part because of his long-time relationship with Martin Fleischmann. Bockris knew Fleischmann in their early years, when Fleischmann was studying at Imperial College and Bockris teaching there.

John Bockris: Martin Fleischmann was not my student. I was a lecturer and he attended my lectures in electrochemistry. I was really young for the position, about 23 or 24, and it was just after the war, 1945, and there were people crowding into the university, all those who had been held up by being called into the Army. They wanted to dash back to the university and get on with it. And so every position I had open, I had about five or six applicants. Martin Fleischmann applied to work with me and I said I couldn't take him. I didn't know then of course that he would be very good. He had a first class degree, but other people get first class degrees. That was the first contact and then for the next three or four years I was in contact with him. He was working with another man, Herringshaw, who was less interested in research. So he only had two research students; I had 12. His room was about three minutes walk from my room. Fleischmann used to come down to mix with my students and he used to come on our social events. There were quite a few people who would come to see me to get some free consulting. They would drop in and say, "Just a few minutes, we just want to ask you. . ." And in order to get rid of them, I would call Martin. I would say, "I think that our Mr. Fleischmann might have a word to say about that." Martin then came in and would leap to the board and start writing equations and speaking very quickly.

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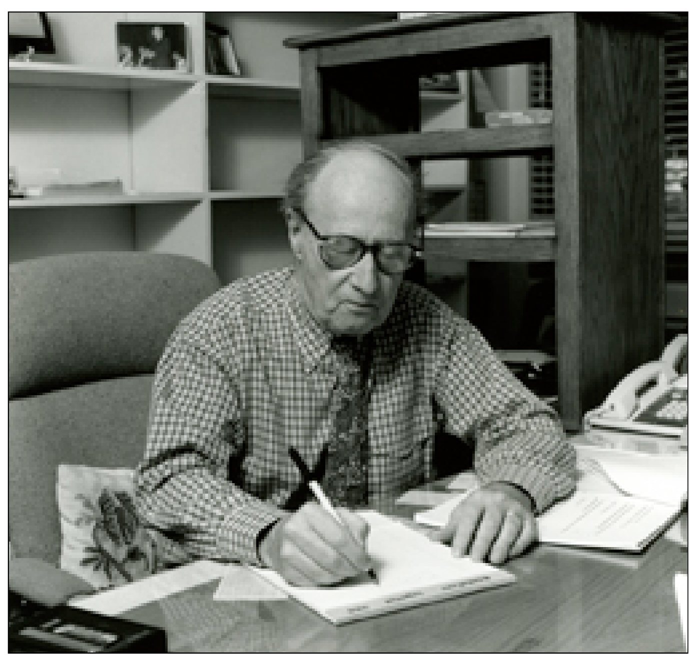
How would John Bockris say his and Martin Fleischmann's interests were shaping up and would lead in the direction of modern electrochemistry?

John Bockris: You couldn't get a paper published unless it was modern. Now everything is modern electrochemistry. Modern electrochemistry I would say, as far as Europe was concerned, was taking place between 1945 and 1953. What I did was to come along and say, "No, come on, it's not an isolated old subject, it's something new and vigorous and it's going to develop." Martin was stuck with Herringshaw. In respect to what developed later with cold fusion, it's interesting to note that he was working on palladium in his Ph.D. thesis with Herringshaw. Palladium is the main metal that was involved in cold fusion. That was because Herringshaw wanted to work on it. He learned some interesting things about palladium which are totally disconnected with cold fusion. For example, it's a good dissolver of hydrogen. It has a splendid appetite for it and therefore it's interesting to note that you can put the hydrogen in, you can push the hydrogen out, you can fill it up with hydrogen, and all this is at that time not connected with cold fusion. Now, when Martin Fleischmann and Stan Pons were having those talks down in Utah in 1985 before they came up with the announcement of a nuclear reaction, I think it was in my understanding or at least in Martin's mind and people have


John Bockris at Texas A&M

asked me, "Did anything you said start Martin off on the cold fusion thing?" And my reply is this, that I absolutely did not have any part in that announcement or that beginning. The basic idea of fusion is you have things coming together—fused. Fleischmann and Pons did go around talking about the high pressure. I didn't have immense knowledge, or even think that it could lead to fusion. I must be absolutely clear about that.

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What would John Bockris attribute to the role of palladium and hydrogen in Martin Fleischmann's later thinking?

John Bockris: I think that palladium was a kind of preliminary to his ideas. And I think that he thought about it and as a matter of fact we still think about this a bit. What does it mean when you calculate, as you can easily, that the internal pressure of hydrogen and palladium is some fantastic thing, like you know 10^{10} atmospheres or something like that? What the hell does it mean? What's happening to that hydrogen inside the palladium—10^{10} atmospheres? That would be terribly compressed then, wouldn't it? See, that's the sort of thing I think that happened and how he got to it, and that's what he used to say. The very first time he appeared in the United States after the announcement, I went down specially to be with him and Stan Pons was here and the two wives, because I knew it would be a terrible torment to them.

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Had John Bockris imagined what the reaction to the Fleischmann and Pons 1989 announcement of the discovery of cold fusion would be like?

John Bockris: Oh yes, I knew there would be a great opposition. I knew there was going to be angry shouting and all sorts of unpleasant things. I'd been through it all already when they weren't there. People hated it, absolutely hated it. It would mean that we had all been wrong all this time, that a great many of the things we had said, a great many things that are in books still, was all wrong and a great new thing was being developed, and people didn't like that at all. And so I went down, it was in Los Angeles I think, to meet them. And indeed it was quite true that they were really nervous, particularly Stan Pons. We went to dinner at a nice restaurant. I had invited them and I fully meant to pay for it but halfway through the dinner, Stan got up and said, "I'm going," and he just left us. And paid for the meal. But he was so worried about it that he just couldn't eat. I had breakfast with him the next morning to try to calm him down. But it was still pretty scary; I mean people got up and shouted and there were people who quite literally made faces. I don't know what it was, but they were furious.

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Did John Bockris understand what Fleischmann and Pons were working on at the time?

John Bockris: At that time I would've gone along with the theory that when you increase the cathodic potential of a cell, that you can calculate hypothetically that material inside the metal will be at a stupendous pressure. And if it really is a stupendous pressure, it must fuse. So that's what I would've said then. I would've said, "Yes, yes, I can see that." Now, of course many of us have gone back and what I think now is a bit complicated, and I'll try to explain it to you. You see, the whole thing, the basic thing to understand is "pressing together." You've got this hydrogen atom and another hydrogen atom or a deuterium atom if you want and if you press them together with sufficient force, well indeed they will fuse. And so how can we—this is me thinking it out in your presence—how can we have a situation in which that colossal pressure is exerted? Well now I think that I did some work—that's before I left the university—on breakdown inside the palladium. Breakdown means that until holes are formed, cracking occurs, change of crystal structure occurs and we've photographed all this. And as time goes on in terms of hours, but in quite large hours, like 100 hours, you find that the interior of the palladium is all well, let's use the words all messed up, all cracked and damaged. Now on these damaged parts, I think that there were little protruding parts between which the hydrogen was squeezed and I think that squeezing is the reason why it happens and how it happens. It diffuses inside and then in sufficient time. It's why nuclear activity takes so long to develop.

What made so many people be against it in the early years was that many people tried the experiment. They waited for something to happen. You have to have supreme patience. I do think Martin was a little bit wicked here, that during the "McNeil-Lehrer Hour," he didn't say "And you have to wait 100 hours." He said it was a difficult experiment. It was only too true. Most of those who are not electrochemists, not knowing very much about cells, would switch it on and they used to have neutron detectors near. They would then find there were few neutrons, which is a sign of nuclear activity. No heat, no neutrons; so they say, "It didn't happen. Fleischmann and Pons were wrong. It was just a stupid mistake." We waited. And if you wait long enough, even sometimes 100 hours. But I used to stop after 500 hours and I would say, "Switch it off, this one's not going to work." I went down to Utah to visit them three weeks after the "McNeil-Lehrer Hour" and I saw some cells going in the lab where I was, and I said to Stan, "What's happening there?" He said, "I'm waiting six months for that one." Six months? "Some time it is going to heat up," that was his attitude, but I used to tell my students, "Run it for no more than 500 hours." That was just something practical. And we used to find that about one electrode in five would fire up, the heat would come in 500 hours. Sometimes it came much earlier, like 100 hours, but the normal physicist or chemist who was trying the experiment didn't know anything about it. He would turn it off after a few hours and say, "No neutrons, there's no heat! Doesn't work!" And then they'd be annoyed and say that Fleischmann and Pons were frauds, as they all did at that time. ❏ ❏ ❏