Interview: Prof. Robert A. Huggins

'Something, indeed, is happening'

One of the first researchers to announce that he had replicated the Fleischmann-Pons cold fusion effect was Dr. Robert A. Huggins of Stanford University's Solid State Ionics Laboratory in the Department of Materials Science and Engineering. Huggins, who has been at Stanford for 35 years, initiated and for 17 years directed the Center for Materials Research. Huggins was interviewed Dec. 15 by 21st Century Science & Technology *correspondent Kevin L. Zondervan.*

Q: What were you researching before the Fleischmann-Pons announcement?

Huggins: I've had a couple of sabbaticals in Germany, the first one was at the Max Planck Institute for Physical Chemistry in Göttingen where I worked with Carl Wagner. I had been interested in the diffusion of species of semiconductors, since my group had done some of the early work in this area. I intended to learn about atomic motion in solids from the standpoint of a physical chemist. Wagner, of course, was the world's leader in this area and the person who really developed the field that we now call solid-state electrochemistry.

I spent a year with him, and ever since I've really been doing solid-state chemistry or using electrochemical methods to study solids. In recent years we've been heavily involved in using electrochemical methods to insert or extract species from solids—species that move very rapidly inside solids such as lithium, hydrogen, and sodium. We use electrochemical methods to quantitatively dope these materials and also to study their thermodynamic properties and kinetic properties. It's a very, very powerful set of techniques.

EIR: What were you doing during the year prior to the Fleischmann-Pons announcement?

Huggins: We'd worked with electrochemical methods for studying hydrogen in solids. We'd done a number of things with hydrogen membranes, including hydrogen in palladium, and we studied the properties of hydrogen in metal hydrides and so forth, although we'd done nothing with deuterium.

EIR: When Fleischmann and Pons announced their results, what was your first reaction? Were you totally surprised?

Huggins: Oh, completely surprised. It looked to us that the kind of experiment they were reporting was the kind of thing we ought to be able to do rather readily because of our equipment and the experience and know-how we had. And so, we set out to try to see if we could do similar things or repeat them.

EIR: How was your effort funded?

Huggins: We worked evenings and weekends. We didn't use any funds. There were seven or eight of us. Essentially, my whole research group got all excited about this, so we were doing this with no funds except money from my own pocket. . . . We have some funds now from the Electric Power Research Institute (EPRI).

EIR: Wasn't your team the first to set up side-by-side experiments with heavy water and regular water?

Huggins: Sure. We decided early on, that that was a useful thing to do. One of the reasons that reinforced us here was, as you may remember, Stanley Pons's comments at the American Chemical Society meeting in Texas. Subsequent to that, I believe Harold Furth from Princeton [Plasma Physics Laboratory] in discussing this topic said he'd really believe it only when he saw that it happened in a case where deuterium was present, but not in a case where hydrogen was present. . . .

The Fleischmann-Pons announcement was on March 23, 1989. It took us about a week to collect the various things together to do our first experiment.

EIR: How long did it take before you actually began charging the cell and got some results, some excess heat?

Huggins: We saw differences, very significant differences, in the hydrogen and deuterium cases by April 13, and we made a presentation of this here [on April 18].

EIR: Are you totally convinced that this is a nuclear reaction that's taking place?

Huggins: Let me answer that in a slightly different way, rather than directly. We're totally convinced that something, indeed, is happening. And, it's quite obvious that the major products that people have observed are heat and tritium. A number of people—I think we have a list of more than 20 labs now, including four Department of Energy labs—have observed excess heat. We have a list of around 14 or 15 which have observed large amounts of tritium. . . .

EIR: Let's talk about this question of reproducibility. Are you having any problems reproducing your results? If not, can you give any hints to people that might help them?

Huggins: We know of a number of things that will make the excess heat effect *not* appear. In general, when we use our particular method of preparation and our particular major source of palladium, we always seem to get about the same results. We don't have the two kinds of problems that other people seem to have: A number of people seem to have nothing happen for a long time. They have to wait and then suddenly something happens. . . . We don't have that. We see results within a matter of hours to a few days in every case. Also, with one exception, we have not seen large heat bursts.

EIR: Your heat output is uniform, or steady?
Huggins: We tend to get what we might call quasi-steady-state effects, rather than nothing, a big burst, nothing, a big burst, and so on. Milton Wadsworth [at the University of Utah's Cold Fusion Institute], for example, has gotten [this burst pattern]. But we use a different material and we treat it and prepare it in a different way. . . .

So, the reproducibility problem is a very serious problem. It seems to be dependent upon the material, the synthesis and preparation of the material, and probably, to some extent, the way you run the experiment.

EIR: Are you still running open cells?
Huggins: We are now running closed cells where we can demonstrate that we have 100% recombination of evolved gases. We've been using an automatic data acquisition system so that we're able to measure things, more or less continuously with time, rather than just making measurements every day or so by hand, as we were doing before.

EIR: What do you mean by a closed cell?
Huggins: In the same cell we have a catalyst which causes the gases, the deuterium and oxygen gases, to recombine and stay within the cell. So the catalyst and everything is inside the same cell. The cell is connected to a bubbler, and we see no bubbles coming out after an initial transient, so we know that we're recombining 100% of the gases that are being formed.

EIR: As far as measuring the heat, do you place the whole contraption in a calorimeter?
Huggins: Yes, the whole thing is in a new design calorimeter. We have a calorimeter accurate to 1% now, and we're seeing effects that are much more than 1%.

EIR: How does your calorimeter work? Do you try to maintain a constant temperature?
Huggins: No, we like to let the temperature rise in the cell. We capture the heat in a large aluminum cylinder and we measure the difference between this cylinder and another larger, concentric one. The cylinders are separated by a small space filled with insulator. . . .

EIR: So you record the change in temperature?
Huggins: And by proper calibration this works very, very well. There's no question related to stirring or mixing in the cell. There's no question with regard to the location at which the heat is produced, because the temperatures are homogenized in these large metal blocks. It's a much more foolproof method.

EIR: Could you quantitatively describe the effects you are seeing?
Huggins: One cell that started operating the day after Thanksgiving and had been running steadily for about 12 days or so produced energy on a more or less steady-state basis—*lots* of excess energy. It was over 23 megajoules of excess energy per mole of palladium in the sample, over about 12 days. There is no way you can get 23 megajoules of excess energy per mole by any chemical reaction.

EIR: How much current and voltage were you applying to the cell?
Huggins: It varied. We were trying various different things during that period and no matter what we tried, we seemed to get excess energy out. . . . A lot of things that we and others have presented before have been excess powers, and there's always the question: "Is this system getting energy back that's been somehow stored, and, what happens if you keep going for a long time? Do you ever get above breakeven in energy?"

Our data showed a very, very large amount of energy above breakeven. Stan Pons also showed energy above breakeven at the National Science Foundation meeting in Washington on Oct. 17, 1989 [see *EIR* Vol. 16, No. 48, Dec. 1, 1989].

EIR: What do you think the future portends for your group? Do you see any application at scaling this up?
Huggins: Oh, I don't see any reason at all why it cannot be scaled up.

EIR: Are you going to attempt it?
Huggins: I'm not sure that we're going to do much on scaling, but there is expected to be an effort in another laboratory with which we may cooperate, aimed at scaling.

EIR: Any predictions on where this new research might take us?
Huggins: Well, no, of course not. But, as I said before, we're convinced that something indeed is happening. That the major products at this moment seem to be heat and tritium, and that's not all bad. Tritium is probably one of the more innocuous products you could think of. Helium is my favorite product but my second favorite one would be tritium because it has a relatively short half-life. It is a soft beta emitter, so it doesn't go through your skin, and if you don't breath it or drink it, it's not a very big hazard to you. And people know how to handle tritium. It's a lot better than having neutrons . . . because essentially there's no radiation damage from the presence of tritium. . . .

If one is concerned about whether what's happening here is nuclear or not, he ought to pay attention to the tritium results. There are lots of people who have seen large amounts of tritium. In very quickly perusing the copy I just got of the ERAB [the DOE Energy Research Advisory Board] report, it appears to me that that committee didn't pay much attention to the tritium observations. . . . They seem to pay much more attention to neutrons, which are evidently not that important. If you believe the tritium results, you've got to believe that something nuclear is happening.

EIR: So, that's a whole new ball game. It looks very exciting for the future.
Huggins: We think it's certainly very, very interesting and potentially could be very important. . . . It's a big surprise to us as well as to a lot of other people. And, I believe, that anybody who feels that the whole thing is an experimental artifact has got his head in the sand.

Interview: Nigel Packham

'Something is producing tritium and excess heat'

Nigel Packham is part of the Texas A&M team, working under John Bockris and Kevin Wolf at the Department of Chemistry and Cyclotron Institute, that reproduced part of the Fleischmann-Pons cold fusion experiment soon after the initial Utah announcement. The Texas A&M group was also the first to announce the detection of large amounts of tritium in a cold fusion cell. Packham was interviewed by 21st Century *managing editor Marjorie Mazel Hecht on Dec. 5, 1989.*

EIR: What's new in cold fusion at Texas A&M?
Packham: Recently we've had a cell in which we saw both heat and tritium at the same time. It shows that the tritium we have found can only really account for about 0.1% of the heat that we see at the same time. . . .

EIR: Can you explain that in a little more detail?
Packham: If you take into account all of the energy that could have been produced by the tritium evolution, where each act of tritium production gives you 4.02 MeV (mega-electron volts), and you know the rate at which the tritium is being evolved, then you can calculate the power that is produced in that time.

If all the heat was being produced from, for example, a deuterium-deuterium fusion reaction producing only tritium, for example, and if you take into account at the same time the amount of heat (or excess heat) that is being produced and integrate that and find the total power produced during the same time, it should be able to be accounted for totally by the tritium energy.

Well, when we do the calculation, it comes out that it isn't, in fact; the tritium accounts for only about 0.1% of the heat produced.

EIR: That's very low!
Packham: Yes. So really what it shows is that there is something else going on. We don't know *what*, but it's something else. . . .

EIR: Has this amount of tritium been seen in one cell or more than one cell?
Packham: The tritium with a direct correlation to the heat has only been seen in one cell. I do know that Dr. Guruswamy at the National Cold Fusion Institute in Utah has obtained a similar result, but not as high tritium values as we have seen. Really, I think our experiment is the first time that tritium and heat have been seen in the same cell. . . .

EIR: What is your thinking about a theoretical explanation for the production of tritium?
Packham: One of the theories that needs to be developed is how to account for the tritium with no neutrons. Apart from the Japanese people that just came out this week saying they had large numbers of neutrons—40,000 a minute—there really aren't that many reports about neutrons from anywhere in the world.

In general, when I was in Utah, the feeling was that this is a so-called aneutronic process. Again, theories abound as to what may be going on, but let's say that it is not deuterium-deuterium (D-D) fusion. Let's say that it's hydrogen-deuterium (H-D) fusion. Now, I'm not enough of a theoretician to know whether that's possible or not. I've got a feeling that it would just form an unstable product and then fall back to H-D.

We've discussed that around somewhat. But it would account certainly for the fact that we usually get large amounts of tritium without neutrons. I think that's another thing that we just have to work on.

A theoretician in our group is working on the nuclear structure of the deuteron. Perhaps, just perhaps, when a deuterium becomes adsorbed on the surface of an electrode, the nuclear distances which are normally present may be extended because of the field that they are in, up to maybe 20 fermi. In that case, the structure or the tightness with which the neutron and the proton are bound together is lessened or weakened.

In that respect it may be able to direct the reaction toward tritium, rather than neutrons. But that is still something *very* speculative. I would love to be able to tell you that we have a theory that can account for all of this, and prove that theory, but I don't think anyone really can. . . .