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THE GLORY OF GOD
IS INTELLIGENCE

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Dear Colleagues,

Today I read your abstract entitled "On the cold fusion rates claimed by Jones" and ask that you consider some comments and questions. You say that "It has been demonstrated that in the Jones experimental conditions, all cations of the solution are first deposited, blocking any deuterium penetration." However, we have measured the deuterium loading in both Pd and Ti and have demonstrated that the loading is greater than zero, although much larger for the Pd. This is an issue that we should be able to resolve by dialogue, so I wish to pose several questions. In addition, I am sending by express mail recent papers which will provide information to you that you may currently lack but which is clearly relevant to your paper. In particular, you state that you have observed no fusion in TiD2, in titanium foils. Please observe that we do not use Ti foils, and that we specifically avoid full deuteriding of metals (this latter is the P/F approach, certainly not ours).

Let us consider first the "Jones experimental conditions." Did you use fused titanium, as specified in our Nature paper? We did not use titanium foils. (See also my paper presented at the Sante Fe Workshop, sent by mail, and accepted for J. Fusion Energy.) If you used titanium foils instead of the material we prescribed, then you did not follow the "Jones experimental conditions." The large surface-to-volume ratio of fused titanium is significant, we judge. In particular, this quality may facilitate deuterium loading into the titanium. A photomicrograph of fused titanium is attached, showing the demonstrably rough surface structure of the material, which is significantly different from that of titanium foil.

You say that the deposition of cations blocks "any deuterium penetration." That is a strong statement, but without numerical value. Can you be quantitative? What is the upper limit on deuterium penetration into fused titanium? Measuring the d/metal ion (d/m) ratio in fused Ti is difficult because the material is multi-faceted and the lattice holds deuterium tightly. The d diffusion rate is low at room temperature, so we expect d/m to be small except near the surface in the cathodes, and we expect the titanium-deuteride phase boundary to move, providing the non-equilibrium conditions we seek. (See E. Brauer et al., Ber.

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Bunsenges. Phys. Chem. 87: 341-345, 1983, and my Sante Fe Proc. paper.) We have measured the deuterium in fused Ti cathodes by first drying the cathode in vacuum at up to 50 C, then by driving off deuterium by heating. The gas was verified to be deuterium (or hydrogen in the case of controls) since it was effectively gettered by hot Ti (over 400 C). We measured d/m in the Ti cathodes to be 0.5% (average). The d/m ratio is calculated as a ratio of the total moles of deuterium (atoms) driven off divided by the moles of Ti in the cathode being tested.

The d/m ratio in palladium, where we used sponge (mostly) and roughened foil forms, was determined by the increase in resistivity of palladium deuteride. Using a four-point probe and milli-ohmmeter, we found d/m to be about 0.6 for the Pd electrodes. Again I emphasize that we have not sought fully-deuterided materials like others including Pons and Fleischmann. Rather, we seek non-static conditions for partially-deuterided metals (see my paper for the Oxford Workshop Proceedings, mailed separately). Please do not confuse our approach with theirs.

I hope this information (and that being mailed) will be useful. And I look forward to hearing your response to the questions and comments posed above.

Sincerely,

Steven E. Jones

cc: A. Bertin, R. Gajewski