LETTERS TO THE EDITOR

COMMENTS ON "NUCLEAR ENERGY RELEASE IN METALS"

In Ref. 1, Mayer and Reitz included some palladium isotope ratio measurements that were made at BP Research as part of a collaborative effort with Texas A&M University. They used these data to support their theory that a $^{106}Pd(t,d)^{107}Pd$ reaction occurred. Unfortunately, Mayer and Reitz misinterpreted the palladium isotope ratio measurements in Ref. 1. In fact, the data do not support this reaction taking place. We feel this mistake must be cleared up to prevent further confusion in the field of cold fusion.

In Fig. 1 of Ref. 1, Mayer and Reitz use secondary ion mass spectrometry palladium isotope depth profile ratios from our work. The depth profile ratios included $^{104}Pd/^{102}Pd$, $^{105}Pd/^{102}Pd$, $^{106}Pd/^{102}Pd$, and $^{108}Pd/^{102}Pd$ from cathodes that were electrolyzed in both H_2O and D_2O. This plot indicated that the mass 106/mass 108 ratio was 1.15 in the cathode electrolyzed in H_2O, while it was 1.01 in the cathode electrolyzed in D_2O. Mayer and Reitz concluded that the 106/108 isotope ratio measured in the cathode electrolyzed in H_2O was correct, while the cathode electrolyzed in D_2O was depleted in ^{106}Pd. However, they did not consider the contributions from hydride and deuteride interferences. The authors did not have sufficient information from this single plot to determine the contribution from hydride and deuteride interferences. However, it is unfortunate that they did not consult us on interpretation prior to publication. We have performed a large number of measurements on these and other cathodes and have carefully quantified the hydride and deuteride interferences as well as the mass discrimination of the mass spectrometer (heavier isotopes are detected less efficiently than lighter isotopes).

In the case of the cathode electrolyzed in H_2O, ^{106}Pd had an interference from ^{105}PdH, while ^{108}Pd was interference free. Comparison of the measured intensities of ^{102}Pd and ^{102}PdH would give the relative hydride contribution. The cathode electrolyzed in D_2O was more complicated because it contained both PdD interferences and small PdH interferences. The relative hydride interference could again be determined from masses 102 and 103. However, the deuteride contribution cannot be determined cleanly from any single mass; it must be calculated using a set of simultaneous equations. We have done this and found that the PdH peak from the H_2O-electrolyzed cathode was 12.7% of the intensity of the corresponding palladium peak. For example, if ^{105}Pd had an intensity of 10^5 count/s, ^{105}PdH would contribute 1.27×10^4 count/s to mass 106. The PdD intensity was calculated to be 6.5% of the corresponding palladium peak in the cathode electrolyzed in D_2O. We found that the D_2O cathode also had a small (0.8% of palladium) interference from PdH.

We also verified that no other impurities interfered with the palladium peaks. Two of the common interferences with palladium are ZrO and Cr_2. These impurities were not detected at significant levels in these cathodes, however.

Once the interferences were stripped from each of the palladium isotope peaks and the mass discrimination of the mass spectrometer (−1.17%/amu) was removed, the measured isotope ratios from *both* cathodes were within 1% of the natural abundance values. Therefore, there is no evidence for a change in the palladium isotope ratios between the cathodes electrolyzed in H_2O or D_2O, as was claimed in Ref. 1.

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REFERENCE

1. F. J. MAYER and J. R. REITZ, "Nuclear Energy Release in Metals," *Fusion Technol.*, **19**, 552 (1991).

RESPONSE TO "COMMENTS ON 'NUCLEAR ENERGY RELEASE IN METALS'"

We agree with Bryan, Gibson, and Murphy[1] that their corrected data do not support our assertion[2] that the $^{106}Pd(t,d)^{107*}Pd$ reaction was taking place in the Appleby, Murphy, and Srinivasan experiment. It is unfortunate that we used their preliminary data and did not consider the possibility of hydride and deuteride interferences.

Although there may be no experimental evidence that the $^{106}Pd(t,d)^{107*}Pd$ reaction is taking place, this does not change the basic thesis of our paper—that "cold fusion" observations result from resonant direct nuclear reactions (RDNRs) mediated by short-lived resonance particles (which we call hydrons). A hydron is a compact, charge-neutral, short-lived resonance particle consisting of an electron and the nucleus of a hydrogen isotope. We wish to point out that since the publication of our paper,[2] we have been studying the dynamics of hydron populations and have concluded that in hydron annihilation, following a nuclear reaction, the electron can carry away a substantial amount of the reaction Q. This has broadened the base of possible RDNRs for "cold fusion" considerably compared with those we previously listed.[2]

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REFERENCES

1. S. R. BRYAN, J. H. GIBSON, and O. J. MURPHY, "Comments on 'Nuclear Energy Release in Metals,'" *Fusion Technol.*, **21**, 95 (1992).

2. F. J. MAYER and J. R. REITZ, "Nuclear Energy Release in Metals," *Fusion Technol.*, **19**, 552 (1991).

REPLY TO "COMMENTS ON 'EXCESS HEAT PRODUCTION BY THE ELECTROLYSIS OF AN AQUEOUS POTASSIUM CARBONATE ELECTROLYTE AND THE IMPLICATIONS FOR COLD FUSION'"

In response to the comments of Mayer in Ref. 1, I have measured the current of my cell by shorting the cathode and anode directly through an ampmeter and have measured 0 A. The operating cell voltage is 2 to 3 V, and the cathode-anode separation is 1 cm. A 1.3-MeV beta particle would travel 0.4 cm in water, which would change the energy of an emitted beta particle by a maximum of ~1 eV. Given that the ^{40}K β-endpoint energy emitted in all directions is 1.3 MeV, which corresponds to $P_\beta = 3.6 \times 10^{-30} N_{40}$ W (N_{40} is the number of ^{40}K atoms in the cell), I conclude that this decay energy is irrelevant to the V-I characteristics of a potassium carbonate electrolysis cell. Furthermore, ^{40}K's natural abundance is 0.01%, and this isotope has a billion-year half-life; thus, decay is inconsequential to the conductivity of the cell. In fact, increasing the concentration of potassium carbonate from 0.57 M to 1 M does not appreciatively decrease the measured resistance of the cell. This increase in concentration represents an increase of charge carriers of $>10^{20}$ times that of the beta particles emitted per second that actually form an ion radical in 10^{-15} s. Ion radicals with a half-life of 10^{-10} s react to yield free radicals. The free radicals have a half-life of 10^{-5} s and, of course, are uncharged; therefore, they do not affect the conductivity of an electrolytic cell. The steady-state concentration of charged species from beta decay is essentially zero.

I acknowledge that quantum mechanics is strongly entrenched, but even the founding scientists were not convinced of its validity. Quantum mechanics was only begrudgingly accepted over a period of decades, and after decades of development, quantum mechanical theory is plagued with inconsistencies. My theory of the one-electron atom is derived from first principles, predicts four quantum numbers (including spin), and is consistent with experimentation. Quantum mechanics is based on postulates and fails to predict spin. I do not accept incumbency as a validation of scientific argument. Each prediction should be tested against experimentation without prejudice of quantum mechanical preconceptions.

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REFERENCE

1. F. MAYER, "Comments on 'Excess Heat Production by the Electrolysis of an Aqueous Potassium Carbonate Electrolyte and the Implications for Cold Fusion,'" *Fusion Technol.*, **20**, 511 (1991).