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December 15, 1993

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Dear Professor Bockris,

It was good to see you at the ICCF4 conference last week. Sorry to hear that your colleagues at Texas A&M are giving you a hard time over your work. I guess to some degree that this letter is as much for them as it is for you.

There are numerous anomalies that have been reportedly observed in this field that many refer to as "cold fusion". As you are aware, I do not believe that fusion (especially $d+d$ fusion) can occur in electrochemical cells. The idea that a lattice could somehow squeeze deuterons together sufficiently hard to get them to fuse seemed to me to be absurd when I first heard about it.

Early on, a number of my colleagues encouraged me to consider what might be happening, in case Pons and Fleischmann were right that a heat effect existed: I first considered a scenario in which fusion could occur as a coherent process. Due to the local politics at MIT, even the mere consideration of such a scenario attracted media attention and came very close to costing me my job. After about six months

of thought and making various estimates, I was able to put my ideas together in the form of a quantitative theory, and show explicitly that even with the most optimistic assumptions about screening that high levels of heat were unlikely to come about from such a theory. This work was presented at the Winter ASME meeting in San Francisco in Dec. 1989, and subsequently published in *J. Fusion Energy*.

My ideas have since evolved, and the experiments have evolved as well. Since the early days of claims of heat, tritium and neutron production, we have seen reports of alpha, beta, and gamma emission and activation of high-Z nuclei. Kucherov claims to have seen energetic fission products emerging from his cathodes in glow discharge experiments.

The heat and tritium results have proven generally difficult to understand theoretically, since so very little positive information is available. Heat is accompanied by the absence of energetic products, by the absence of quantitative neutrons or tritium, and so far neither by quantitative ^{4}He yet as far as I can understand. The glow discharge experiments of Kucherov and colleagues seem to me to be extremely important in this respect, that they give positive anomalous nuclear signals from which very positive statements for and against various theoretical proposals can be made.

It is immediately clear that fusion is not the source of the anomalies. Fusion would not lead to significant activation of Pd cathodes, to the production of continuum gamma radiation peaking at 4 MeV and extending out to 20 MeV, to the production of neutrons with energies also out to 20 MeV, and to the production of what appears to be fission products of Pd.

As you recall, at ICCF4 I presented a theory that seems to have the prospect for accounting for these various effects. These ideas were presented at Princeton in November, and more recently at MIT; at both Universities my presentation was well-received.

I focused on energy exchange between the lattice and nuclei embed-

ded within, starting from a consideration of the lineshape in Mossbauer spectroscopy. Changes in the mass, charge, excitation or final location of a reacting nucleus can lead to changes in the structure of the phonon spectrum in the lattice. Such changes in the phonon modes are generally small and can usually be ignored. In Mossbauer spectroscopy, they give rise to observable effects that go under the heading of the second order Doppler shift.

In the case of neutron capture, the mass of the nucleus can change significantly (by two in the case of $p + n \rightarrow d$), and a few phonon modes may be affected drastically. If impurity phonons modes are involved and if the neutron capture alters the number of impurity nuclei present, then three phonon modes can jump from one impurity band to another. Calculations indicate that in this case, the phonon modes more or less take their phonons with them, which leads immediately to lattice/nuclear energy exchange. The amount of energy transfer is $\Delta E = N\hbar\delta\omega$, where N is the number of phonons initially present, and where $\delta\omega$ is the frequency shift for a gap-jumping phonon mode. Whereas the shift in phonon energy can be up to about 10 meV, if 10^8 phonons are initially present (these are continuum modes, so that the energy is delocalized), then the energy transfer may be on the order of 1 MeV.

These ideas were presented at the International Mossbauer Conference (ICAME93) last August, and were well-received by a critical audience. Mossbauer himself said that the basic mechanism is fundamentally sound.

Most interesting in the theory are the results in the case of Pd vacancy modes in PdD. If the lowest phonon modes of the optical branch are strongly excited, then the removal of one Pd nucleus will change the potential seen by the neighboring 8 deuterons, and lower their frequency. If many similar vacancies are present, then the 8 deuterons will join a "vacancy impurity band;" this allows anomalous energy transfer in principle for high Z nucleus.

The Kucherov experiment appears to work in this fashion. Strong

was disheartened because I thought that the last thing in the world that the field needed was to be associated with alchemy. It was clear that any association, no matter how weak, would be seized by critics to hammer yet another final nail into the coffin that they have constructed for cold fusion. Due to the politics, it would not matter whether there existed and effect or not, that an association with alchemy would ultimately lead to the cry of scandal.

I assume that by the time you receive this letter that your colleagues will have stoked the kindling at the base of a very large stake with your name on it, and will ultimately succeed in this way in cleansing your soul of the scientific errors that in their view you have made. And after you, others.

But after some reflection, what has been discussed in the cold fusion business could be classified under one of the definitions of alchemy in the American College Dictionary. Altering nuclei through essentially chemical means is the heart of alchemy, losing the immediate focus on the production of gold.

In light of the theoretical ideas discussed above, what would it take to turn mercury into gold? Electron capture from ^{197}Hg (unstable with a 64 hour half-life) would produce ^{107}Au (the only stable Au isotope). So how would you get ^{197}Hg? Neutron capture onto ^{196}Hg (0.15 %) or neutron transfer from ^{198}Hg (10.0 %), the latter being more likely. Given that neutron transfer from Pd to Li, and from K to Ni, are at the moment leading candidates for explaining heat production, I would ask whether quantitative gamma emission at 77.34 KeV is observed with a half-life of 64 hours. Given that lattice-induced enhancement of the electron capture process is possible, I would settle for substantial 77 KeV emission with a 64 hour half-life after the material is cold.

My understanding is that there occurred some evidence in these experiments for the production of ^{197}Pt with a corresponding 18.3 hour half-life (also producing 77 KeV). This could follow in my theory from

lattice-induced alpha decay of ^{201}Hg (13.2 %).

As I told you before, my interest in this effort is limited to the observation of chemically-induced radioactivity. Whereas I would be worried that someone might be motivated to spike a sample with gold for one reason or another, it is a very different matter to spike a sample with a radioactive impurity that has an 18 hour half-life, especially if you have tested for its presence beforehand.

In the Kucherov experiments, there is evidence for induced radioactivity that falls off in minutes. Although Morrison has argued that this is due to the use of Pd that is initially contaminated, this is not convincing since if true it should be seen in the initial cleaning discharge runs in the 20 minutes before the "live" discharge begins. Should you succeed in demonstrating chemically-induced radioactivity with Hg, I have to say that I am interested, even if it happens to come from replications of "experiments" that were first carried out 500 years ago.

Good luck in the trials ahead that are facing you. Judging by historical precedent, I suspect that you will in fact get burned at the stake, in spite of my input or input from anybody else. I cannot judge the wisdom of having been associated with Champion and his colleagues (in retrospect, it seems to have been a bad idea), but this is a matter separate from what I can knowledgeably write about.

Best wishes,

Peter Hagelstein