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**RESEARCH LABORATORY of ELECTRONICS**MASSACHUSETTS INSTITUTE OF TECHNOLOGY
CAMBRIDGE, MASSACHUSETTS 0213938-290
(617)-253-0899

May 10, 1993

Dear Lowell,

I got your fax - I know some about the China Lake work and I have their paper.

When I first got a preprint of their initial work, I was under-impressed. It looked sloppy, and given that I am not a believer in helium as the main product from the heat production, perhaps I was more skeptical than normal.

When Miles presented the work at Como, for the first time it was clear that these guys were quite serious about this, and that they had in fact done a sufficient number of controls that you could not so easily dismiss the claims. Miles answered some very rough questions at that conference quite adequately.

This work was also presented at Nagoya. On the minus side, Miles had been having trouble getting the heat to come back, which I think was attributed to the fact that they were forced to change their supply of heavy water. On the plus side, they had done further studies of leakage of helium in and out of their samples, and had further quantified their signal levels.

The helium work at Austin was done by Ben Bush. After Austin, Bush went to work with Miles at China Lake, where Bush was supported on a fellowship (I think an NRC fellowship). This support was recently terminated, and Bush was picked up by McKubre. He is currently working with the SRI guys to attempt a replication of the helium measurements at SRI. This arrangement I think is of somewhat limited duration, but

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at least follow on work is going on.

I have met him; Bush himself is an experimentalist of relatively narrow focus - that focus is on helium collection and measurement - these days some folks say that he knows more about it than anyone else, and that he is the right guy to do this work. He is certainly preoccupied with this measurement, and I think Mike did OK to pick him up. Should Ben Bush get a He result at SRI, and have it pass Mike's review (and my review), then I think that I would believe it.

Currently, I think it is an interesting and serious study that is pending confirmation. There exists papers saying that there is no such effect, for example: A. Zywockinski et al, *J. Electroanal. Chem.* **319** 195, (1991).

Bush may be right. Potential confirmations have been claimed by Bockris and also by Yamaguchi at Nagoya. Today, I am not sure how much value a confirmation by Bockris is worth. Yamaguchi's result I think is also awaiting confirmation. Yamaguchi has the better hardware, but his result is too new for me to have any confidence in it.

If Bush is right, he is talking about a large effect that accompanies heat, but is not quantitatively consistent with the heat. My favorite mechanisms for producing ^4He would be neutron transfer on to ^3He (except that I don't think that there is enough ^3He present initially to make this work), or neutron capture induced alpha decay from something high Z (Pd isotopes). The latter pathway has supporting evidence in quite a few experiments, but I am not sure the magnitude of the effect matches Miles electrolysis experiments. Time will tell.

In conclusion, on a scale of 0 (hopeless) to 10 (good), this is about a 4 or so, which at the moment is the best there is for ^4He .

Other aspects of this whole business: Presumably you got a copy of my DOE CF final report. In it you will notice an updated copy of my Nagoya review, which summarizes the Hagelstonian world view up to about February. Corrections past then are relatively minor, and include:

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- 1) Kevin Wolf has about a year's worth of data on neutron emission from electrochemical cells that he has not gone public with (if you can count EPRI reviews as not being public) that is quite good - possibly the most careful work in the field - and he is seeing Jones neutrons.

Much more important is an experiment of Kevin's in which three cathodes were "activated" in an electrolysis experiment; these cathodes produce enormous gamma signals, with identifications of about a hundred gamma lines corresponding to short-lived isotopes. This work by all rights ought to be published in *Phys. Rev. Lett.* Kevin will not attempt publication until he can reproduce it, and he and everyone else who is aware of it is looking.

SRI has recently gotten in a brand new \$60 K Ge gamma detector, and they are planning a survey of all cathodes done to date by anybody that have shown any result to see whether they were activated a la Wolf.

- 2) Correction on the review; the SRI record is 200 watts/cm³.
- 3) We are attempting a replication of the Kucherov experiment. To date, we have seen nothing, except those spurious little bumps that mother nature keeps throwing; our way to keep us interested. We have been on the phone with Kucherov, and have gotten samples from Kucherov - but still no signal. Our very first live runs took place about 10 days ago, and Ed Cecil came out and fielded a Ge detector and charged particle detector. We have an excellent idea about what background looks like, but no signal.
With Kucherov, we have identified about 3 areas where our experiment could be improved. Prof. Smullin has subsequently improved the vacuum, added a LN2 trap, and replaced a BN cathode holder with a quartz version. These upgrades should be completed soon, and we will start again. Kucherov is supposed to be here on May 15 or so, pending the acquisition of visas; this represents a delay of over a month from when he was originally due here. Many have gone and visited his lab at LUTCH; apparently they

are up running and continually take data of all kinds of anomalies. If Kucherov does not come soon, I am going to hop on a plane and go see it myself.

- 4) SRI has recently gotten in a few million dollars of new toys. There new setup looks very serious - a sophomore electrochemistry experiment no more. In fact, it is beginning to acquire the smell of BIG science, which in my old age I have become more allergic to. They have developed a new labyrinth calorimeter that is more nearly a first principles calorimeter, with conduction losses below a per cent. They are fielding a Seebeck calorimeter, reminiscent of T. Droege's system. And they are working on other kinds of calorimeters to improve throughput. They have seen nothing in their new systems yet, but they have yet to get through their first experimental cycles with them.
- 5) We will attempt a replication of Claytor's experiment on our system, with Claytor's help. This will happen hopefully this summer.
- 6) As is typical, there is really very little news about what is going on in the field. It is more than six months since Nagoya, and I have heard of very few new results or confirmations. We have no positive experimental results; SRI has nothing new; LANL is having luck with their tritium work; Jones is working, but I have not heard of any new results; Lincoln is trying to replicate the light water experiments, but are not up yet; there is a rumor that IMRA Japan has replicated P+F's boiling episode, but I cannot confirm it; NRL is either out of business or censored; there are rumors that P+F have figured out how to extend their boiling episode, but Fleischmann would not confirm it when I saw him last; and on and on... Be nice if we could have semi-regular meetings.
- 7) I got invited to give a (not invited) paper at the big International Mossbauer Conference on my CF theory. This was really very nice. Mossbauer will be there, as will Goldanski. I suspect that I will be ignored, but at least it is a start.

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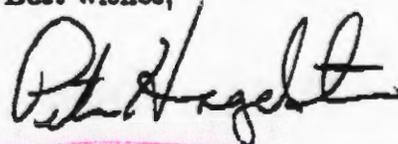
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I spent the weekend reading a copy of *Bad Science: the short life and very hard times of cold fusion* by Gary Taubes. It is a 450 page distillation of essentially every bit of dirt, scandal, politics that Taubes has collected in following the field over the past several years. It is really very negative and quite depressing, as was advertised. I think that it represents the global view of the Happers, Koonins, Huizengas, and Garwins of the world; namely, that there just isn't anything at all there, and that wild optimism is all that is keeping anyone going.

I wish that we had some data - I would feel much better if gammas, neutrons, particles, heat, etc would come gushing out of our experiment. I suppose that it takes more than a week of running to get a signal... Wish us luck.

I will be in the bay area from June 7 to June 11, and would be delighted to stop by to check in for a visit. Let me know when would be best.

Best wishes,



Peter Hagelstein

F+P have a paper in May 4 issue of Phys Lett A (I have heard).
I have paper III in latest issue of FusionTech.
Ed Storms gave a talk here last Friday, and it was reasonably well received.
I got data from Boeksis on his radioactivity from his H₂ expts - if it is real, it is interesting.

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