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MEMORANDUM

TO: Prof. E. Schweikert, Department Head

FROM: John Bockris

SUBJECT: Meeting on Ultra Low Energy Nuclear Reactions

The meeting took place on September 13 and 14 at the Holiday Inn with 23 papers and 42 attendees. It was chaired by George Miley and the writer. The organizer was G. Lin. A security guard was posted to prevent disruption by A&M faculty, one of whom in 1995 had attacked attendees.

Some of the papers were as good as any in the field. Three or four should not have been accepted.

Miley. Evidence for multiple element formation in thin films of Pd on subjection to protons at high fugacity. Three methods of analysis used. Abnormal isotopic abundance used as criterion of synthesis. High quality work.

Mizuno. Remarkable evidence for new element formation up to 3μ depth in Pd at high D fugacity. Xe and many metals synthesized. Isotopic abundance measured. Impurities from solution carefully distinguished. High quality work.

Dash. Excess heat correlated with formation on surface of new elements. However, isotopic abundance not measured.

Szpock: X-ray emission from cathodes evolving D_2.

Minevski. New element formation at 1μ depth. Distinguished from impurities in surface layers, which were correlated with measured impurities in solution.

George. He^4 found in Pd electrodes irradiated with ultrasound. New elements found at edge of craters detected on surface.

Fox. A semi-presentation of electrochemical method for reducing activity of nuclear wastes (method being patented). Method was said to reduce activity 60-95%.

Lin. Presentation for first time of Philadelphia Project work of 1992. Au, Ru, Rh formed as result of explosive heating of Pb, Hg salts. Level 100-1000 ppm for Au; 1-10 ppm for others. Multiple methods of analysis used. β activity observed but irreproducible and apparently not always connected to significant new metal production.

Grotz. Threw doubts on Sundaresan and Bockris C $\rightarrow$ Fe experiment.

Shoulders. Electron "clusters", fired at metals, give plasmoid shapes and new elements in surface.

Michrowski. Attempt at survey of cold transmutation. But, mixed up with apparently irrelevant study of properties of H_2-O_2 mixtures ("Brown's gas").

Monti. Work presented by Bauer. Claimed α model of atom could rationalize huge variety of transmutations in the cold. Monti's verbal contribution in discussion incomprehensible to me. Paper lacked indication of detection methods, guard against impurities, etc. Work lacked credibility.

Kim. A theoretical paper which I found difficult to understand. Kim known to Natowitz who praised him as solid character. Conditions found whereby Gamow factor canceled out, would allow nuclear processes in cold. Physical model appeared to involve excitation of nuclei whereupon they became ellipsoidal and unstable. But (cf. Natowitz comment below) theory did not involve solid lattice!

Cau. Has rationalized formation of noble metals by alechemists. Pitchblende was basic substance used and radiations from it caused other nuclear reactions.

Lewis. Haphazard, ill prepared talk, stressing priorities of Japanese transmutationist Matsumoto who began in 1991. But Mizuno claims priority over Matsumoto.

Rabzi: Incomprehensible talk, - though well translated from Russian by student from Humanities. Claimed his group had had success in cold transmutation over 15 years. Esoteric theory not understood by me. Rabzi is backed by a group of about 15 Russians who have written to me.

Fox. Speculations on details of proton capture mechanisms.

Nagel. Reviewed largely unsuccessful NRL work on cold fusion. Admitted privately that effort partly staffed by semiretired persons. Said government apparatus unsusceptible to his (Division Chief's) pleas for funding of cold fusion. Only person who had success (Chambers) succumbed to downsizing shortly after his experimental results became known (according to Claytor).

Claytor. Five year successful effort tritium production. Plasmas emphasized. Most sustained tritium effort. Apparently convinced Natowitz, as to reality of tritium production. High quality work.

Ohmori. Remarkable paper on isotopic distribution of new elements formed as a result of H and D diffusion into Pd. Isotopic abundance studies as chief criterion of new material creation. High quality work.

A fair summary of this summary would be to say that twelve papers gave evidence that nuclear reactions take place in solid lattices in the cold. If this contention obtains still further confirmation, I suggest it constitutes a discovery of magnitude comparable with that of atomic disintegration with high energy neutrons (Rutherford, 1919), and nuclear fission by neutron bombardment (Hahn and Meitner, 1939). It opens a new area of great potential. It has the potential radically to change the ideas of nuclear stability.

It is noteworthy that, throughout the century, there have been occasional suggestions of easy (low energy) nuclear change in agriculture, geology and biology.

A two hour open discussion occurred from 10:00-12:00 on Saturday. I stressed the need for a reproducible experiment. Jim Redding of CETI Inc. and John Dash (Portland State University) both claimed 95% reproducibility. Natowitz criticized work for overriding well known nuclear principles, and the reporting of many results seen as impossible on present paradigm. He accepted the tritium synthesis but supposed all the other papers must contain undetected errors. Why had no radioactive products been detected? Mizuno rejected this, saying he had detected Pt^{197} and its 18 hr decay. Few people had sought radioactivity which in any case had shown up in Kevin Wolf's work. Natowitz pointed out that Kim's theory did not involve the solid lattice.

The 1992 work of Kevin Wolf was raised. Natowitz thought it undesirable that it should have been finally presented by Wolf's sponsors (in 1995.) I pointed out that Natowitz was the Director of the Institute in which Wolf worked. Why did he allow Wolf to bury for 3 years work of such significance?

Lin raised the question of the formation of an organization. There was little enthusiasm. I suggested the Society for Scientific Exploration would be a good umbrella organization. Ed Storms supported this.

It was rumored that one member of the meeting had a commercial production in gram quantities of hafnium, using a cold nuclear reaction approach, and that another (not overtly present) was on the brink of commerciality in respect to the cold nuclear synthesis of noble metals.

A number of persons observed that, in the absence of the Philadelphia Project work at Texas A&M (1992-93) with ideas originating with Joseph Champion, and funding by William Telander (experimentation by Lin and Bhardwaj), the new field of cold nuclear reaction would not now be the subject of study.