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June 14, 1989
(Via FAX to 9-(409) 845-4205)

Professor John Bockris
Texas A&M University
Department of Chemistry
College Station, TX 77843-3255

Dear Professor Bockris:

Thank you very much for the draft paper sent 06/09/89, "Observation of Tritium Production ..."

I have two major problems with this paper. I will refer to the text by page and decimal fraction of a page.

Page 2.9 You say that approximately 10**10 atoms of tritium are produced per second, neglecting losses to the gas phase. Of course, in the mechanism that you eventually cite, we know very well that a number of tritium atoms per second produced would be equal to the number of neutrons produced, and you mention (page 3.2) "50 neutrons per minute", but where are the rest of the 10 billion neutrons per minute that you should be producing if your tritium is due to fusion?

So that is my first problem-- that not only are you assuming fusion under some very strange circumstances, you are assuming fusion in a bombarding energy range that has been well explored (I designed and participated in an experiment in 1951 to measure exactly this cross-section), but also the mechanism that you cite will not work.

Page 4.1 Here you say "Here, at the low radius of curvature tips, local electric fields of approximately 10**10 V/cm may bring the D+ ion in transfer to an energy of 10 keV sufficient to fuse with an absorbed D+ on the electrode surface." Well, even though one may get such very high electric fields, the energy that a charge can reach is simply the electric field times the charge times the distance over which the electric field operates. And this distance is very small. In fact, the distance is limited by the applied voltage, which is 10 volts rather than 10,000!

Therefore, no matter how low the radius of curvature of the tip, one can still not by this mechanism obtain deuterons of energy in eV greater than the voltage applied to the cell.

That has always been the problem, and it remains the problem.

Please let me know your views on these remarks.

Very best regards.

Sincerely yours,

Richard L. Garwin
Forwarded in his absence

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TEXAS A&M UNIVERSITY

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PLEASE DELIVER THE FOLLOWING PAGE(S) TO:
Dr. Richard L. Garwin
IBM Research Division

FROM: Dr. J. O'M. Bockris

DATE: 6/14/89

TIME: 3:30 pm, cst

TOTAL NUMBER OF PAGES INCLUDING THIS PAGE IS 4

OUR TELECOPY NUMBER IS (409) 845-4205.
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THANK YOU!

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June 14, 1989

Dr. Richard L. Garwin
IBM Research Division
Thomas J. Watson Research Center
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Dear Dr. Garwin,

Thank you for your faxed notes on a draft of our tritium paper. My answer:

(1) Experimental Results in scientific measurements have a certain probability of being "real" (i.e.is acceptable to the majority of scientists believing in the paradigms of the time). The solutions from the electrode which gave tritium were analyzed by two independent methods (different buildings, independent workers) at Texas A&M; three governmental labs at two of which extremely experienced tritium analysts did the work and two private organizations, at one of which an extremely experienced tritium analyst did the work. All results agreed to ±2%. Under such circumstances, the result is effectively certain, - certain, that it, that tritium to the stated degree was in the solutions given for analysis.

We have examined multiply the probability for it being there illicitly. We conclude that the wilful injection in secret of tritium by some person is the only possibility. However, the tritium-time relation, - particularly its final value, does correspond to the amount which would be expected to be there had an electrodo been producing energy by means of the process:

$$D + D \rightarrow T + H + 4.02 \text{ Mev.}$$

for the time of the electrolysis. I doubt if anyone who handled the measurements knew enough of the physical chemistry of solution - gas-equilibria to do the calculations necessary to put in the right amounts.

If these statements convince you that in 7 out of 11 electrodes, tritium was produced during the electrolysis of D_2O (LiOD) then the objections you make (both which come from theory) can be set aside without argument. The assumptions to the theory used must be inapplicable to the circumstances.

However, there would be the following to comment.

(1) Branching ratios: the neutron production by all the people who have measured neutrons from electrodes (and this ("The Jones Effect") now seems established), are all far too small to produce heat. Because of the rare character of their appearance, and the fact that they seem to turn up after very long electrolysis, I am at present in favor of the idea that they occur in a Kiliow mechanism, - cracking due to embrittlement (A field in which I have much experience). There is a sonic technique whereby such cracking can

be heard and we plan to listen for cracks and attempt to correlate such cracks with neutron emmission.

Measurement of tritium to the same order as that found here has also been observed by Grozzi (Rome) and by Schoesser and Wollingford (Gainesville). If one accepts it as established (and it is much less firm than the neutrons), then it seems to provide evidence that fusion does occur (whether the source of the heat or not) and that the branching ratio in solid state confinement differes from that in plasma. Dr. Guang Hai Lin, a physicist working with me, has a model which seems to him qualitatively to rationalize this difference.

The vital point in your letter concerns my attempt to rationalize fusion in the electrical double layer. My attention was first drawn to this idea was a telephone conversation with Csikei (Hungary), who reported in early April that he was getting neutrons but that they faded after an hour or two and could be brought back by cleaning the electrode surface.

At first I didn't think Csikei's confident statement that the potential difference in the double layer could give energies sufficient for fusion was credible. However, more recently I have argued as follows: order of magnitude of temperature for D + D fusion - 10^8°K. Corresponding energy in ev - 10^4ev. When a deuteron in the Helmholtz layer at an electrode discharges, (deuteron transfer) one can see it (I simpify) as travelling about 1Å though a field of 10^8 volts cm^{-1} and having, therefore, at the point of impact with an adsorbed deuteron ion on the electrode surface an energy of 1 ev. If, therefore, it is possible to find heterogeneous points in the double layer where the local electrid field is 10^{11} volts cm^{-1}, the deuterons arriving there will have an impact energy of 10^3 ev. (note, not 10^4 ev).

What is the probability of this? It is possible to show that

$$\frac{\text{field at promitary of radius r}}{\text{field at surface of radius R}} = \frac{R}{r}.$$

Now dendrites have tip radii of10^{-5}-10^{-6} cm taking R as ~ 1mm (our electrodes), one easily obtains a 10^4 magnifying factor, more than enough.

There is much more to say, - too much for the letter. In recent years, Henderson (IBM, Almaden) and others have shown that the metal electron gas extends out a few Å and would envelop the region in which the D^+ -$D^{\delta+}$ impact occured. Screening! I cannot here develop the model for $D^{\delta+}$ (the adsorbed deuteron) but I think I can show its reduced charge (charge transfer to the metal) will also ease repulsion.

Thus, one begins to see some rational model. I have written these words in the interests of collegial co-operation and discussion. I don't believe their substance should be included in my NOTE. This is intended as a brief communication and its intention is to make credible the massive production ot tritium at some electrdodes. I don't believe more can be expected at this time. I am, after all, not a nuclear theorist working in solid state

confinement. Criticisms of my Note, I believe, should try to shake the validity of the facts it reports.

I stress that only 7 out of 11 electrodes gave tritium. Further, an excess heat producing electrode (Fleischmann-Pons Effect) gave an increase of tritium which could be explained in terms of isotopic enrichment due to electrolysis. Conversely, the tritium reported in the paper is enough (counting the gas-phase amount) to be consistent with heat production through $D + D \rightarrow T + H$ (although we do not know if the tritium producing electrodes produced heat).

Sincerely,

J. O'M. Bockris

P.S. I don't think these matters will be resolved for a year or two. The time of charging of electrodes which have radii of curvature sufficient to give heat to be measured by most calorimeters available is 1-3 months. Few valid experiments have been done (those at SRI (McKubre) seems to avoid all the criticism I can bring to most).

After the facts are known, and these nuclear particles connected with the heat (if such a connection is made), -this is the time to think about fusion and theorize. To be frank with you, theorists can predict where the horse is only after they have been told it has left the stable.