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(Ed: Page One is missing)

In 1988 Dr. Ramesh Kainthla, working with me, attained 8.2% practical efficiency light to hydrogen by photoelectrochemical conversion, without any battery.

(5) Several of my recent papers had been "wrong." Such a statement cannot, of course, be answered until one tells which papers and what is wrong.

With regards to attempts to discredit Nigel Packham:

Your case was that Nigel Packham had intentionally put the tritium into these cells which have yielded tritium.

I enclose a map showing the cells which yielded tritium and the dates at which the tritium was observed compared with the dates of fusion oriented events as culled from my diary, my secretary's and that of Packham.

The observation of more results before August 1, 1989 is explicable. Between March and the beginning of August we worked in a different laboratory in a different way from the way we worked afterwards. Until August we had running 40 to 50 electrodes in test tubes (aimed only at T) and electrolyzed them for various times between one week and five months and under various conditions. The volume to surface ratio was smaller than it is in the present cells which are Fleischmann -Pons type.

About August 1 we moved and changed from about 45 test tube cells to 9 (relatively large) Fleischmann-Pons cells and automated the system for continual recording of heat. Thus the rate of finding tritium should be more than five times less. The congregation of "finds" before August in small groups is also explicable in the following way. At present we record automatically day by day and the heat inputs are computerized. In earlier times our testing was more advanticious. Someone would go along "every week or two" and see if he could find tritium in the cells. We did not have a T counter at the time in the laboratory and used to have to take the cells to the Cyclotron or Nuclear Engineering across campus. Under these circumstances, out of the 45 cells, three or four of them will turn up with tritium after a couple of weeks when tests were not made.

In respect to Wolf who has had two cells given tritium, the measurements were made in his way, in his counter, in his part of the Cyclotron Institute. Wolf maintains his equipment was locked up. Dr. Wolf seems to be an exceedingly cautious person.

Further, to contamination:

1. Contamination from within palladium.

This is a possibility and the fact that the results occur in bursts goes along with work which I did some years ago in which I took discs consisting of pure iron, charged one side of the disc with hydrogen to a high degree and then watched the escape of the hydrogen on the other side after I had turned

off the supply. The hydrogen escapes in bursts and over a week, say, there may be two or three such bursts. I interpreted this as the exit of hydrogen from its aggregations of impurities in the iron.

However, one has to ask where the tritium would come from. The Pd used in Savannah River (where it might have contacted Pd) is buried after use. Even if it were not, the diffusion coefficient of tritium in palladium is high. I calculate this would take around a day to drop effectively to zero. When palladium is in the form of a wire, or etc., it is melted in manufacture and the diffusion coefficient during melting would be many orders of magnitude increased. The aggregation of tritium in the alleged impurities could decrease the diffusion coefficient by many orders of magnitude. On the other hand the total tritium production from an electrode of volume about 0.1 cc is something like 10^{15} atoms so that we can say 10^{16} atoms per cc. Were the tritium concentration 1 ppm, then the tritium could possibly be accounted for, but one still has to ask where the specimens got the tritium in the first place.

A better argument against contamination within the metal is the fact that titanium gives tritium. In the Bhabha Atomic Research Station 10^{7} or 10^{8} counts per min per ml have been observed from titanium and at Texas A&M the amounts have been about 10^{5}.

But Ti is cheap and is thrown away after use. It is an unsatisfactory cathode material for technical hydrogen evolution. Its diffusion coefficient for hydrogen is extremely low. It is inconceivable that Ti used by either institution would have earlier been exposed to T.

(2) Intentional contamination:

Coming now to your concept that somebody put the tritium in the solution, Dr. David Worledge has made a discussion of this and has listed more reasons than I am going to give you why it is not a likely hypothesis.

Thus, were a contaminant added it would have to be HTO. Bubbling D_2 through a mixture containing a small amount of HTO and an overwhelming amount of D_20 would, in any 24 hour period, lead to a slight extra evaporation of the D_20 and slight increase in concentration of the HTO. (After that the solutions are reconstituted and the amount escaped, recombined, is added back to the cell.)

In fact, the amount of tritium present is noted to decrease with time and qualitatively this is what would happen if the surface which had entered the solution had been in the form of DT. This is consistent with the fact that in a cell in which one measures the escape of the tritium in the recombining solution, there was, for the same time period, more tritium per ml in the escaped material than had remained in the solution. These facts are consistent with the DT being the form at which the tritium has been introduced into the solution. The feasibility of adding DT to the solution is very small indeed.

Further, the branching ratio was measured to be 10^{-8} at Texas A&M as early as April, 1989, but this is the same branching ratio obtained in several other institutions much later. It would be inconceivable for someone to add "just the right amount of tritium" to get the right branching ratio when the neutrons are being determined in another establishment, and when this branching ratio is wholly unexpected and difficult to explain.

In my view you should write Packham an apology for voicing suspicions which are groundless. They are also rather stupid. How could Packham add T to solutions at Case-Western, Oak Ridge and Los Alamos (two institutions), to say nothing of 4-5 foreign institutions including the massive reports from the Tritium Center at the Bhabha Atomic Research Institute?

I think you should also apologize to Packham for telling him you had discovered that he had never been a student at Imperial College. I enclose Packham's registration form. The D.I.C. (Diploma of Imperial College) is equivalent to a Master's Degree qualification. But, at Imperial College, it is taken prior to the Ph.D..

I voiced skepticism as to the sale-ability of the book you are writing. If you are going to make its great point that you have disproved the reality of cold fusion, the book is unlikely to sell because more than 90% of all scientists have written the phenomenon off already. (Though you could write a Best Seller showing Cold Fusion <u>does</u> occur.)

I was interested to hear how you manage the two occupations. I believe that the time is right for a book on the relation between government research spending and the forces which control it. 93% of DOE's funding is going to support the polluting older sources of the fuels. I believe that delving into the reasons why this extraordinary state of affairs is allowed to continue (with $1 billion per year for hot fusion) would make a very interesting book, indeed.

Good wishes,

Sincerely,

J. O'M. Bockris

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