

NOTE CONCERNING AN ARTICLE BY TAUBES WHICH WAS PUBLISHED IN SCIENCE

ON JUNE 15, 1990

The background of the note which was published in Science on June 15 is the following:

A. Mr. Gary Taubes who is a part time journalist and part time screen writer in Hollywood, approached me on two occasions. In the first he was mild and normal and said that he was trying to write a book on cold fusion. In the second visit he was extremely aggressive and negative and said that he intended to attack cold fusion and to prove the work we had done was fraudulent!

It is obviously to Mr. Taubes' advantage, if he makes his book as sensational as possible. He has a history of writing books which are aggressively and negatively worded descriptions of famous professors. For example, he has attacked Nobel Laureate Rubbia of CERN in this way in a book called Nobel Dreams.

I am not entirely sure whether Taubes really believes what he is saying about the work at Texas A&M but if I look at it from his point of view it is possible that he thought the following.

B. At Texas A&M, as is well known, two groups have found that tritium is formed on some occasions after prolonged electrolysis of D_2O in contact with palladium.

One group, that directed by me, working largely with a graduate student, Nigel Packham, but also with a post doc, Ramesh Kainthla, and an advanced Masters type technician, Omar Velez, and established 13 occasions out of 58 in which the electrolysis leads to the formation of tritium.

When we first found these results we were one of two or three labs, only, in the world in which they had been found.

As you know, the whole fusion situation is an extremely contentious one in which large numbers of people in the field have not been able to replicate the results obtained by other people. To some extent this mystery has had light thrown on it. It is now known that one only gets the anomalous results which seem to hint at the existence of a fusion reaction within the palladium only when the electrolysis is prolonged and leads to a D/Pd ratio of 1.0 or more.

At the time that Taubes started all this, and the very many negative results probably made him think that our own results were wrong and the negative results were right.

In this he was greatly helped and aided and abetted, in fact, by the many physicists who declared that it was impossible to obtain the results which we were getting and then the question arises, "Why were we getting them?".

Taubes found out several things which made him wonder. For example, upon

long forgotten test tubes which had not been examined for many weeks. Taubes interpreted this as an example of tritium finding "just at the right moment." This aroused his suspicion.

Another factor which is behind the article is the role of Charles Martin who competed with Kevin Wolf and myself as professors who were working on this. Martin's cells only gave rise to one tritium finding out of about fifty. In the case of Wolf it was about two out of about one hundred whereas with us it was thirteen out of fifty-eight. One can see that a considerable amount of jealousy existed and the explanation if you cannot get it yourself and you think you are a great electrochemist and you try very hard, - but it is Bockris and co who get it, - well, there is something very funny about it....

I think this is the background of it all.

C. This would all be par for the course and quite good game if the thing had been done fairly. The fair thing, of course, would have been to write an article, and for Science to send it to me and to ask for comments. Then the editors could have decided to publish or not publish as they felt fit and with the use of other referees.

In fact, nothing of this sort was done. The article was kept secret from me except for a few days before it was to be published. It was published in Science magazine without any chance of rebuttal and without my having seen the article except one day before publication.

So, this, of course, was unethical and unfair and something which no decent magazine would ever do. It is also extremely damaging in respect to myself, my graduate student, and my University. What got into Science to do a thing of that type I do not know and that is a mystery which we have to solve. It may well have been that there was some kind of getting together of people to attack me and my co-workers. The field is contentious and there is a great deal of fear in the background on the part of the hot fusion people who are strong in influence at Science. They are over funded in a hopeless task and anybody who gives a chink of light in another direction would get tremendous funds which, of course, would be taken from their budget.

D. Now, let us come to the scientific part. The allegations are easy to disprove. In the case of the allegation that there was contamination with tritium leading to the high results we got.

1. Thirty-nine labs have now observed tritium under these circumstances and, of course, it would be not a credible proposition to say that all of them had contamination of their palladium by tritium. It seems an extremely unlikely thing, anyway.

2. The particular palladium pieces which we used came from a manufacturer called Hoover and Strong and it was true that the palladium was old in the sense that it had been used before and was recovered from scrap jewelry. However, it was recovered by a method which involved two actions which I think would have completely removed any possible tritium in it. (One might possibly conceive that tritium had been used in a tritium helium separation plant some 15 or 20 years ago.)

The two processes which would remove any tritium which might be there were the electrolytic separation of all the metals and the redeposition of palladium. The tritium ions would have come out at OT^- and would have been deposited about 1.8 volt separate from the palladium. Of course, the likelihood of co-deposition then becomes virtually zero.

In addition to this the palladium is melted on the way to making a wire and as the tritium hypothetically inside would probably be as a hydroxide and as even the most refractory hydroxides decompose at the temperature of melting of palladium ($1534^{\circ}C$) it seems very unlikely that any tritium could have survived.

Now, after all this let us accept what has been claimed by a man called Kevin Wolf. He claimed that he did find 2000 dpm (disintegrations per minute) in a piece of palladium which was 0.5 cm long and 1 mm in diameter.

In an actual electrode which he had previously used and worked with he found $1 \cdot 10^4$ dpm per ml was 5 cm long and would have to multiply the figure he found by 10 and get 20,000 dpm of impurity in the electrode.

We then compare this with the $1.4 \cdot 10^4$ dpm ml^{-1} which Wolf found and realize that this means evolution of the tritium into a test tube of about 10 cc in volume giving something like $1.4 \cdot 10^5$ dpm. It is seen that the total amount of tritium in the Pd is only about one-fifth of the amount needed if it all came out. It is, of course, most unlikely that it would all come out. Electrolytic dissolution at a cathode is effectively impossible and the only way that one can conceive that it came out is that there is a kind of "sneezing" effect in Pd charging where there is a temporary diminution of the D/Pd ration. Some could have come out then. When the sneezing occurs the total amount of gas deuterium or tritium decreases from 10% so one could have had a 10% evolution of the tritium meaning that the amount which could have come out would have been about 1/50th of that necessary to explain Wolf's own result. Since the incident of the letter we have had analyzed 12 more samples. They didn't contain any more than background amounts of tritium.

Now to the major matter: fraud.

This is very easy to answer in one sense. Unbeknown to Taubes, people have been getting tritium right, left and center. I enclose a partial list of the laboratories at which tritium had been obtained. But, of course, the idea that Taubes had, - we were the only boys on the block who had got it, - has been blown to pieces and, therefore, the pressure to "explain" the JOMB results is now passé.

However, there is a straightforward method of disproving the allegation that the graduate student spiked his solution with HTO. It is shown on the enclose graph.

We owe much here to Ed Storms of the Los Alamos National Laboratory who purposely spiked his solutions with known amounts of HTO and watched the decay of the activity of tritium under certain circumstances which correspond to those used when examining a cell in respect to its T content.

Briefly, if one does spike the cell with HTO the decay of tritium is extremely slow. We have worked out a theory by now which does rationalize the slow decay and get agreement with the results of Storms.

Now, when we do get bursts of tritium from the electrodes there is a tremendously different type of behavior and it is shown in the accompanying graph. I suppose that the tritium formed in the burst is in the form of DT and as we are constantly bubbling copious D_2 and O_2 through the solution the DT gets sparged out and therefore the exponential decay at a much faster rate than the HTO disappears.

So much then for the spiking. It didn't occur.

Enc. 60 Labs
Storms Graph