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Answer To An Assault On Legitimate Research, &

Suggestions Of Fraud, In An Article Published In "Science"

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Background

Work at Texas A&M on the anomalous effects associated with deuterated Pd started immediately after the announcement by Fleischmann and Pons. Soon we had a total of five groups working here, one under Kevin Wolf at the Cyclotron, another under Charles Martin in Chemistry, one under Ken Marsh in the Thermodynamics Research Center and the one directed by me in the Chemistry Department. John Appleby and Supramaniam Srinivasan carried their effort out in the Center for Electrochemical Systems and Hydrogen Research in the Texas Experimental Engineering Station.

Texas A&M was recognized early on as a suitable place for a cold fusion effort because the principal 'disciplines involved are nuclear chemistry, electrochemistry, and calorimetry. There were universities which had schools in either of the first two but few which had both nuclear centers and a strong electrochemistry school and other schools which had research activity in all three.

The Early Stages of Texas A&M's Cold Fusion work. April through August, 1989

The first period of the work in my laboratory covered a period in which we were twenty people and virtually everyone in the laboratory took part in the work although they all had other projects. The contribution of most collaborators was simply once-per-week night watches on the cell, - before automation, - because, with the open cells we had to add D_20 around twice per day and were anxious lest the electrode should emerge into the gas phase and promote D_2-0_2 recombination. Most of the work was carried out by people who had been employed on EPRI grants before March 23 and after we got telephone instructions from Rocky Goldstein at EPRI to spend some of their money on the cold fusion work.

The work in those days was done with great enthusiasm and elan. Daily meetings occurred to discuss its progress and Charles Martin was a part of these.

I had agreed with Martin that as we were exchanging information and ideas every day, it would be reasonable to think of anything that came out of our work of this time to be joint intellectual property. I was therefore surprised when I found out Martin had gone out and called a press conference on his own for Sunday, May 9, 1989.

I found a radiant Martin with Ken Marsh and Bruce Gammon in the Thermodynamics Research Center recounting to admiring reporters their triumph in having observed the Fleischmann-Pons effect for the first time outside Utah. The next day there was a press conference and headlines in the paper.

During this early period we had several successes. Three of our cells went on heat for brief periods of ten hours or so. There were a number of test tube cells which gave large readings of tritium. The taking of measurements in those days, let it be clear, were relatively chaotic compared with what we were able to do later after August.

The initial tritium measurements were made with the help of Milton McClain in the Nuclear Engineering Department. He warned us about chemiluminescence before we had a counter which registers this.

Dr. Ramesh Kainthla, the senior scientist in my Group at that time, was the principal person who ran the changes on the experimental variables. It was he who undertook the melting of palladium to try to evaporate off impurities as Huggins at Stanford had advised us to do. In alternative treatments, we annealed our samples and treated them with intended poisons. Nigel Packham and Jeff Wass collaborated, too. Omo Velev contributed from the earliest days on the calorimetric measurements. After that there were a number of graduate students and post docs who came and went and took part in the work when needed.

In August, 1989, we moved to new laboratories. I separated Packham off from the rest because there had been rumors which were the origin, perhaps, of Gary Taubes' hints of spiking the solutions. I wanted to be sure that further success in tritium measurements were independent of him. Thus, the last and most impressive bursts, October-November, 1989, where the heat lasted thirty-two days, was handled by Ramesh Kainthla and Omo Velev, who measured both heat and tritium.

Relations with Colleagues

We obtained help from the Nuclear Engineering Department but the major thing was collaboration-with Professor Kevin Wolf at the Cyclotron Laboratory. Although I had worked in the 1960's on tritium measurement, - and often on tracer determinations of adsorption on electrodes, - I was glad to have Wolf coming to our laboratories, making his comments, and instructing us as to what to do and above all looking after the contamination question. In fact, Wolf took the initiative and worked with Wass and Packham both in his own laboratories and in ours in the search for contamination. Thus, the floor covering was taken up and examined, among many other contamination-seeking acts. Apart from this we sent samples of our palladium and nickel to Los Alamos National Laboratory. No tritium contamination was found.

The good collaboration with Wolf lasted for about a year. It averaged a contact once every two or three days and this was mostly on Wolf's initiative.

About March 1990, Wolf's enthusiasm for cold fusion decreased inexplicably and he excused himself from a plenary lecture at the first Annual Conference on Cold Fusion in Salt Lake City.

Our collaboration with Charles Martin started off hot, - meeting every day, - but after Martin made the solo announcement of his initial verification, these were hard feelings and the collaboration fell away. Not long afterwards, I was surprised to find New York papers announcing that Martin's results contained a vital error and I called him up and asked him whether it was reasonable that a close colleague with whom I had been meeting every day, would let me learn about a reverse like this through New York newspapers?

Martin came to my office where there happened to be several collaborators and made an emotional statement to the effect that the results on which he had based his announcement had been false, a trailing wire he hadn't noticed had given error. He assured us that he was a fine electrochemist, and the trailing wire represented a temporary aberration.

After this, I had the impression that Martin's enthusiasm for the field declined. However, when the ERAB Committee came, he reported two heat bursts.

Charles Martin had been disappointed that he had not been able to find tritium. To be quite sure that any T he might obtain would be genuine, and apparently afraid of contamination in the University, Martin took the cell to his garage to look for T. Finally, at last, and electrode from his own batch of palladium proved in the hands of Kevin Wolf, to give rise to tritium and Wolf and Packham went round to see him to tell the good news. However, by this time, Martin had lost enthusiasm for the field, and, in spite of earlier enthusiasm, seemed uninterested.

The So-Called Correlations

One of the things that Omo Velev had noticed was that tritium had been found to a disproportionate extent on Mondays. This observation was perhaps the origin of the suspicion which became current in the Department that Nigel Packham, - the most active in the collaboration and the bridge-man in contacts with Wolf, - was the origin of the tritium in the solution! When I realized that such suggestions were being taken seriously, I wrote to Dean Fackler in autumn 1989 and asked him to have an investigation made of the rumors.

Other correlations were alleged. Was it true that we seemed to get tritium near the visits of the EPRI sponsors? Was it true that Packham had surprisingly obtained tritium from two titanium electrodes which had been electrolyzing for months and he had finally tested out, - just before he went for a job interview? I insisted upon making a diary, linear, about 10 foot long, from Packham's Lab book and my diary as to when he had observed the tritium, and all the other events then seemed relevant, e.g. sponsor visits, job interviews, scientists meetings, etc. I took this document to the Statistics section of Texas A&M University and asked them if they could find correlations between anything except for the frequency of finding on Monday (see below). They came back with many reservations about the type of correlation possible in events which involved random human actions (e.g.., sponsors visits) and events in the Laboratory. However, the only tendency they could find was that ten days <u>after</u> an EPRI sponsor had been here, we were more likely to get tritium!

Before we went to our new rooms we did not have a system for taking samples from the cells to see if they contained tritium. Thus, early at the beginning of the week, I tended to charge down the corridor asking all I could find, "Did we get any tritium? Have you seen anything?" Of course, this prompted someone to make measurements from the test tube cells. I think <u>this</u> is why we tended to find T more often on Monday (for the rest of the time, no systematic checks on T were made).

<u>The Visits by Gary Taubes and the Attack on Nigel Packham</u>

Science writer, Gary Taubes, visited Texas A&M on three occasions. He told me he was working on a book on cold fusion and that he had a $40,000 advance from Random House, part of which he was using to cruise the country interviewing those involved in the cold fusion effort. He said that his view was going to be that the whole thing has been due to an attempt by Fleischmann and Pons to get money. There was no phenomenon at all. Any positive evidence for the phenomenon was due to sloppy measurements.

In his first visit to us I treated Mr. Taubes in an entirely normal way. He seemed an erudite sort of person to me in spite of views which I thought strange. He had some kind of physics background and had tried to become an astronaut before becoming a journalist. He seemed easier to talk to at a quasi-scientific level than do many journalists. We opened up our labs and results for him to see and answered his questions. He interviewed several of my co-workers and called many more.

Apparently, Mr. Taubes made a second visit to our university, - but I knew nothing of this. Later on, it appeared that he had gotten a lot of information including confidential letters and memoranda which I had exchanged with Dean Fackler. How Taubes obtained these memos is not at the present time known.

When Taubes returned for his third visit, he appeared to have transformed into the raging lion, no disguise. He started off by an amazing two-hour confrontation with my graduate student, Nigel Packham. He had traveled from Santa Monica to London to investigate him, and his parents, checked his records. Taubes maintained he had discovered Packham had never been registered for a Ph.D. at Imperial College.[1] Then, he accused him of spiking the solutions with tritium from a tracer-tritium solution. The only evidence for cold fusion, he averred, was the work of Texas A&M on tritium and this tritium work had been faked by the graduate student to get a fast Ph.D. In fact, we would never have seen any tritium except for the fact that Packham actually put it in the cells ! ! !

[Footnote: [1] I had Packham's registration at Imperial College for the degree of DIC (a preliminary often at Imperial College to the Ph.D.) faxed to me in five minutes after I heard this. To others, Taubes hinted that I had been only too easy to deceive - "I thought that the tritium measurements would get us a grant for research. I needed the money."]

When Packham continued to deny that he had indeed done these awesomely dreadful things, he tells me that Taubes switched off the microphone and told Packham that he should "confess" to him now, at this time, IMMEDIATELY, - the alternative was an article in Nature describing how he had spiked the solutions. In addition, Taubes would put an immediate piece in the New York Times showing that Fraud had been committed at Texas A&M and by Nigel Packham.

"If you want to tell me what happened now," Packham tells me Taubes said, "I'll give you a year to get your life in order. Then, my book will come out."

When Nigel Packham and Zoran Minevski came to see me later on in the day after Taubes had left, and told me these things, I was at first amused. But I gradually got to see that we were heading for a serious situation. I couldn't believe at first that Taubes really thought that Nigel had spiked the solution, - but at any rate I could see that publishing his suspicions would be excellent for the sales of his article and his book. I began to understand that Taubes had not realized that dozens of Labs were reporting T from the anomalous behavior of D-Pd the world over. He evidently thought our T propped the field up, - he thought that if he could spread the idea that we were wrong, he could break up the new field - for he said that his book was to be an expose', as with that on Rubbia at CERN.

Taubes planned to visit us again the next day after the attack on Packham and I received him normally. He asked me a large number of questions He evidently had been told by one of my friendly colleagues that I was running out of research funds and that I had, - desperately, - sought the EPRI funding to save my group! I showed him the various spending graphs I plot showing the grants which support my group and indicated that, although I was glad to have the EPRI funding, I would still have funding well above the average in the Department without them.

In spite of these rather provocative hints, - which were made not in the threatening manner which he used with Packham, - but nevertheless, in an aggressive and abrasive tone, - I still took Taubes to lunch at my Club and treated him in a gentlemanly and relaxed sort of way.[2] During this hour and a half or so that I was with him he confirmed that he had indicated to Packham that it would be a matter of "Confess now or be ruined." My reply was "Don't be silly, Gary."

It is now up to the scientific community to decide which of us was correct.

[Footnote: [2] On the way to lunch, I heard Taubes murmur: "This is utterly amazing."]

Preliminary Moves to the Publication of the Article in Science

I reported the Taubes attack on Packham and the general stance of his visit and the accusations made to the Legal Department at Texas A&M and, of course, to Dean Fackler.

Their attitude was that "nothing has happened yet" so there was nothing that the question of legal action must be held in abeyance. At the same time I heard that moves had been initiated by other parties to bring suits against Taubes from offenses he was alleged to have committed outside Texas and so I thought it better to let sleeping dogs lie and got on with my work.

Indeed, the next thing was a phone call from John Maddox, the editor of Nature, in London. Dr. Maddox, along with Douglas Morrison in CERN, is a known super-foe of research on the anomalous behavior of deuterated palladium and he had encouraged an article by David Lindley, the American editor of "Nature", in which Lindley advises all scientists that they should take aggressive action to ridicule work on the anomalies, and shut down any investigation of them.

Dr. Maddox and I have had a passing acquaintance, not always entirely positive, for many years, and he started off by saying, rather hesitantly, - that he had received a communication which indicated that Fraud was being committed in my laboratory!!!

So, I told Dr. Maddox that I could hardly say anything about the matter unless I saw what was being claimed. If he would fax me the article I would tell him at once over the telephone if any of the statements were true and if any were untrue. He would have to decide what to do then.

I immediately realized: Taubes had struck.

So, I told Dr. Maddox that I could hardly say anything about the matter unless I saw what was being claimed. If he would fax me the article I would tell him at once over the telephone if any of the statements were true and if any were untrue. He would have to decide what to do then.

He said he could not show me <u>all</u> of the Taubes article but would be willing to fax me part of it overnight and the next morning I came in expecting to see the article hanging out of the fax machine. Nothing was there. After an hour, I phoned Maddox in London and he seemed to have a very different attitude from the rather accusing tone of the former day. He had spent the day with his lawyers, and said that "Nature" had decided to "put it on the back burner." There had been, therefore, no need to fax me the article. We talked a bit about Taubes and his role in the book attacking Nobel Laureate Rubbia, the Director of CERN, but Dr. Maddox seemed to think that that part of Taubes' book devoted to sketching Rubbia's character was fair comment.

The first time I ever heard of "the trouble" which climaxed in the article of June 15 in

Science, was that Packham told me in May 1990 that Wolf had found some tritium contamination in three samples of palladium, among forty cut from a wire. I must *say* that I found this difficult to accept because I had checked particularly the purity of the Hoover and Strong palladium we were using and knew that it had been separated from other noble metals by potentiostatically depositing it from solution. Being an electrochemist, I knew something of the theory of this method of extraction and a reference to the Handbook of Physics and Chemistry showed that nearly I volt existed between the standard potential of the deposition of palladium from solution and that of the deposition of relevant aqueous ions. The standard electrode potentials of the H isotopes differ by only 10-20 mv from that of hydrogen, (though the rates of evolution differ at several times). I thought, therefore, that it was extremely unlikely that any tritium could remain in the extracted palladium, even if (perhaps because this piece of palladium had been in contact with tritium in an earlier incarnation) there had been some possibility of the trapping of T in Pd. Anyway, I respected Wolf's view but asked him if I could visit him and talk about it. He offered to visit me. At this visit in May, he said that he thought the tritium he had detected in the Pd could explain the amount of T he had detected in his electrolysis of Pd in LiOD (5.10^4 dpm ml^{-1}) as due to spot contamination of the Pd with T. He hypothesized that the T was there in a hydroxide inclusion. I didn't try to argue with him about his analysis, the details of which I didn't know at this time[3]. But I did point out to him that, even if we had per chance stumbled across Pd which had done service at Savannah River, the likelihood that tritium could still be present after potentiostatic re-deposition (thus separation from other metals) and melting (thus decomposition of the hypothetical hydroxides) was very, very slight, I indicated, too, that I now had the names of twenty-five other laboratories where occurrence of tritium had been reported as one of the anomalies of the deuterated palladium saga, - it being absurd to think that all those people in all those different countries (some of whom I knew to have used Johnson Mall-they super pure) would have palladium contaminated by tritium in the virgin state.

At this time Wolf did not mention to me the other discovery that he had made, - see below, - or that he had been in contact with Gary Taubes in spite of the fact that he knew of his attack on Packham, so then any report to Taubes throwing doubt on his validity of the measurements should indeed be like pouring gasoline onto the fire.

It was not until June 4th (cf the publication date of "Science" Article June 15th), in a meeting with Ken Hall, the overall administrator of the EPRI project on Cold Fusion at Texas A&M University, that Kevin Wolf at last came out with the news. There would be an article in Science that would do the Chemistry Department a lot of harm, he said. It would be by Gary Taubes and it would feature his own discovery of contamination as the source of the tritium found at Texas A&M, and his finding of light water in a sample of recombinant D_20. I was, of course, amazed by all of this suddenness. Wolf stressed that he had been talking to Taubes.

At this time, Wolf sent me copies of several letters, - one of them was dated April 11, - and from these it seemed for about eight weeks Wolf had been telling EPRI about his results, and finally Dean Fackler. Wolf's letter to Dean Fackler seemed to be a report of his alleged discovery of T contamination in his Hoover and Strong Pd and a furious attack upon me. He seemed to be telling Dean Fackler that the methods by which I was getting tritium do not work, - and then, - he had found this light water in the recombinant ....

[Footnote: [3]Now, I do know them. It is borderline work with solutions which are at first colored. There is also the possibility that Wolf was seeing Pb^{210} contamination from U^{235}. This gives a T-like spectrum.]

To say that I was surprised by all this would be a statement even under the understatement a man having a doctorate from Imperial College would be likely to make. I realized that a widespread attack (Taubes, Wolf, Science Magazine, perhaps others) had been developed against me, personally as well as my graduate student, Packham. The originator of this attack had been Taubes, but he seemed to have had plenty of help from others. Dr. Martin's part in all this is not known. The fact that it had all been done in secrecy, that neither Wolf, nor Science had contacted me and told me about the forthcoming attack on, me, nor asked my comment on all the innuendos and suggestive statements, disappointed me, to say the least. It contrasted so poorly with the actions of john Maddox of Nature, who, - even though so strongly on the other side of the fence on Cold Fusion, - had acted as an honest man.

The Role of Dean Fackler

I sought my Dean's advice but first of all the advice of Vice Dean Clearfield who also knew about the forthcoming article and seemed very sad, - if somewhat reticent, - about it.

I began to realize that Dean Fackler, himself, had known about the article for some time. I had much earlier asked the Dean to instigate a Committee of Enquiry when I heard rumors of spiking the solutions, but he saw this as pointless in the absence of either a compression or something clear to investigate in terms of a standard of reproducible results, etc.

It seemed that John Fackler had dealt in his calm and unflappable *way* with Pool and that he had striven to get the article revised to a basic expression of anxiety concerning the balance between academic freedom and the need to investigate the possibilities of fraud. Later, in a letter to Pool, Fackler expressed dismay that the heavy innuendo that fraud had actually taken place had not been removed from the article. In fact, the final text of the article published in Science was not very different from the one which Fackler finally gave me a couple of days before the attack on me was published.

In fact, when I first read the Taubes article my initial reaction was something like: "Well, in a literal sense, this is mostly true. There are a few wrong statements but they are not essential." But then on rereading it again I realized that the whole thing was an immense misrepresentation because what it tries to say, - and there are many who have read it and told me that this is their impression, - is that my co-workers were the only ones getting tritium in any serious quantities, and therefore this getting must be fraudulent (as cold fusion is not possible according to plasma theory). If the tritium was really formed from D_20 in, or on, the electrode, this would be strong evidence for fusion. Indeed, Taubes suggested that their findings influenced the setting up of the Institute in Utah. The essential misrepresentation of course was to put the T forward as Texas A&M's result when it had been reported ahead, at Santa Fe (May 1989) for four Labs -- and now from twenty seven. A&M's work could be entirely neglected but the evidence of the production of T would not go away.

Thus, during the writing of this article (July '90), Norman Hackermann called to tell me he had just returned from Taiwan where he had witnessed the work of Professor Wan and his team. They claim to see - 100x background reproducibility - better than we can do.

Spiking

There are many reasons why I think that spiked solutions are entirely improbable, although I state at once that it will never be possible to disprove tampering 100% (the same goes for all other research results).

1. My main reason is the one left out by Gary Taubes: The 27 labs.[4] Why pick on my lab and pretend there is something special about it which has to be explained by the very extreme hypothesis of purposeful interference? Whatever the cause (and surely it is not contamination in all 27 labs), T is sometimes formed in bursts on the electrolysis of D_20 on Pd.

2. The bursts of tritium which we observe in some of our cells have corresponding upshoots of tritium in the gas phase. Now, it is difficult to see spiking with anything else but HTO. The amount of gas bubbles going through the solution is constant. Why would there be the sudden increase of T in the gas phase (coincident with that in solution) if it did not come from new production at the electrode?

3. Decay. Storms from Los Alamos has argued convincingly that the decay rate from the electrolysis of HTO is slow and entirely different from the exponential decay which arises from sparging out of a tritium-containing species in an open cell. Our results of decay on our open cells correspond more or less to those of Storms. T production decays exponentially and seem to suggest that the T being produced is DT gas.

4. The branching ratio of tritium to neutrons in the deuterated palladium anomalies is an important quantity. It has turned out from work at four laboratories that this ratio is the same for two different methods of carrying out the electrolysis of palladium. It is around 10^{-8}. It seems unlikely that it would lie in this unusual region (the expectation from plasma physics is unity) were we examining anything but the same phenomenon in the various labs concerned. If spiking were occurring in all those labs it is not believable that all the spikers would have spiked to about the same extent.

5. Added to these objective statements is my estimate of the people who work with me. I think it too far out to suggest that some unknown spiker from another group got in during the night to tamper with our solutions and the only person we could suspect must be one of those working with me. Every advisor knows his co-workers. I don't think that it is very likely that anyone of mine would do such a horribly dishonest thing. Why would they do it? Fifteen times?? For what purpose? With the danger of total ruin as a scientist upon discovery?

[Footnote: [4] This is a conservative number. The huge Indian effort is counted as three labs in the 27. But in fact Bhabha Atomic Research Centre had 11 separate groups in different departments of whom 8 found T. Further, there are verbal reports from labs in other countries.]

Contamination Again

All those who bring tritium into their work are afraid of contamination and indeed, led by Kevin Wolf, we have made a tremendous examination of T contamination (described in a paper in Press) in numerous objects in the laboratory, even taking up the floor covering. We also sent our palladium and nickel to Los Alamos for analysis for tritium and in all cases the answer was zero. Now, recently Kevin Wolf claims he has found traces of tritium in samples of palladium from Hoover and Strong. I respect his measurement though it was very much, I think, on the borderline of detectability.

My reason for rejecting the idea that his results interfere with the general picture, that bursts of tritium production occasionally occur in D_20 electrolysis on palladium, arises as follows:

1. It is difficult to believe that there is tritium in virgin palladium at all. It is melted in making a wire. Many samples are annealed just specifically to get the hydrogen out. (Hydrogen comes out at 400°C and tritium and deuterium a little before this.) If tritium clung to some kind of impurities such as a hypothetical refractory hydroxide, say, zirconium hydroxide, - this would be converted to zirconium oxide before the melting point of Pd at 1535°.

But even this reasoning is less relevant than the fact that many of the people who get the tritium use the virgin palladium from Johnson Matthey when they are guaranteed to be the first users. To assert that there is tritium in <u>this</u> palladium would infer that the hydrogen which is in all palladium by virtue of water adsorption thereon and absorption of hydrogen has from this moisture led to the formation of tritium in the palladium via the H + D nuclear reaction, favored by Professor Schwinger. But such a view implies Cold Fusion.

2. Let us now assume that Wolf's measurements are right. It still remains for us to show how the material could possibly come out of the palladium at the measured rate (10^7-10^{10} atoms per cm^{-2} sec^{-1}). This needs a detailed calculation but the essence of it is that there is a negative field gradient across the palladium-solution interface, and this then acts to inhibit the anodic step of transfer of the tritium ion into the solution, hence the exchange of T & D. When one adds this factor to the others concerned, the rate of evolution of tritium turns out to be many orders of magnitude below that observed.

An overpotential of - 0.6V during D_2 electrolysis on Pd would mean a deterrence factor for dissolution of at least 10^6.

Added to this is the fact that Dr. Wolf did indeed use one of the samples in which he had later found tritium in a light water cell. - but observed no tritium appearing in the solution.

Thus, although contamination must always be sought and tested for with T work, a pan contamination theory of the 27 laboratory reports of T formation in D_20-Pd electrolysis is as unlikely as plasma physics tells us is a fusion explanation.

The Importance of Facts Around the World on the Anomalies of Deuterated Palladium

These are attached. It is clear that many people in many countries do observe the anomalous effects, whatever their interpretation.

The Special Importance of the Work at Case-Western Reserve University

The Electrochemical Center at Case-Western Reserve undoubtedly has a very high, - perhaps the highest, - reputation in the country as a Center at which solid reliable work in electrochemistry is performed. However, it is just here with the work of Yeager[5], Cahan[6] and Adzic that tritium has been seen to form at about 50x background during the electrolysis of palladium in D.O.

[Footnote: [5] 0ne of two or three most experienced physical electrochemists in N. America.]

[Footnote: [6] An experienced electrochemist, famous for his experimental technique and care in solution preparation.]

Do Studies of the Behavior of Deuterated Palladium Give Evidence of Cold Fusion?

I think this is a question that has to be considered in light of the above. My own present stance is that it is difficult to avoid the conclusion that nuclear reactions are occurring during the (occasional) bursts of nuclear activity (in some labs, neutrons, T, [gamma] and heat together), which one sees in some of these systems after a month or more of electrolysis at high current density. I'm a great believer in the 90% view of Science, - all has a degree of probability, nothing is forever. I'm open to a normal explanation in terms of a current chemical theory if one can be given. Or, even a very far out explanation in terms the hypothetical CHAMPS, large uncharged particles which may exist sporadically along with muons.

Reproducibility

If the, field of nuclear reactions at electrodes is to be accepted by the scientific community, there is a need for reproducibility and that we don't have at the present time. (Although at Case-Western Reserve it is said that about one electrode in two is active.)

The Issue: Science and Politics

I think that the issue here in respect to the Science article is, how far do you go in publishing an investigative journalist's account of his gathering of gossip that a graduate student has been spiking solutions with tritium? Does one simply publish it in a great feature article without giving the people concerned any chance to comment on the allegations or do you consider what will happen to the graduate student involved, particularly if there is no direct evidence stated for the allegations?

It seems that in the present case it was journalist Gary Taubes who wanted to publish the article, but there is no doubt that Science helped him do that and a number of others collaborated in giving him information. Was this justified when, - as quoted in the article, - "There is no smoking gun." In particular, the principal question which has to be faced, by the Scientific Community is was it proper procedure, on the part of Science, within the unwritten rules understood by all?

How different would everything have been had Science sent the article to us and had we been able to present the experimental tests which have been done and which make the spiking seem unlikely and the effects of contamination at the levels claimed by Wolf nugatory.

Was it abnormal for Science not to send a copy for comment to the persons involved?

Would it have been reasonable to have first made an independent investigation as to the truth of the allegations before they were published?

In the case of such allegations, they can only be shown to be improbable (never 100% disproved) by the study of calculations involving physical chemistry, electrochemistry and radiochemistry. How many readers of Science are going to read such calculations in depth and study them so that they can believe in them?

The allegations made are perhaps what a number of physicists would like as a solution to the puzzle of the anomalous behavior in Pd-D_20. Because they were not addressed to the several hundred people who have taken part in experiments giving positive results on Cold Fusion but were focused on me and Nigel Packham, it is difficult not to sense a political element, clothed in talk of Fraud and Government Investigation, etc. Its publication wasn't good science and what the Chairman of the Board of Directors of Science now has to decide is who took the responsibility for the decision to publish such material and whether that decision was good for Science?