Journal of New Energy, Vol 4, no 2, 1999, p. 40

EARLY CONTRIBUTIONS FROM WORKERS AT TEXAS A&M UNIVERSITY TO (SO-CALLED) LOW ENERGY NUCLEAR REACTIONS

J. O'M. Bockris [1]

INTRODUCTION: THE SITUATION IN MARCH, 1989

The firm opinion in respect to basic knowledge of the nucleus common among chemists and physicists in March, 1989, was that chemical effects (involving frequencies of ~ 10^{14}), could not affect the nucleus of atoms with frequencies ~ 10^{6} times higher. It was also believed in March, 1989, that high energy neutrons were the path towards atomic fission, as in nuclear reactors; and that collisions of H isotopes at extremely high temperatures similar to those in the sun, constituted the main path towards the fusion of nuclei.

The idea that nuclei in solids could be made to undergo nuclear reactions at room temperatures[2] was far from the thoughts of chemists and physicists in 1989. However, this mental barrier was broken through publicly by Fleischmann and Pons in 1989.

When the initial research funders, EPRI, looked at the competing universities, Texas A&M University was found to be particularly strong in nuclear chemistry and electrochemistry. It possessed its own Cyclotron, funded by the university; and a Thermodynamics Research Center. Each of these groups is an unusual feature for a university. Clearly, precise heat measurements were an essential of the experimental techniques being developed in the study of chemically assisted nuclear reactions. Prof. Kenneth Marsh in 1989, Director of the Thermodynamics Research Center, had spent many years in work in which exact thermal measurements were the essence.

Lastly, research into electrochemistry was a much pursued topic at Texas A&M in 1989. Three professors in the Department of Chemistry (Bockris, Martin, and Soriaga) were electrochemists and other professors in the Chemistry Department, including Cotton, Goodman and Sawyer, used electrochemical techniques. Apart from this, there was the Center for Electrochemical Systems and Hydrogen Research in the Texas Experimental Engineering Station (Appleby and Srinivasan). In 1989, some 90 people were doing electrochemical research at Texas A&M.

All this made it easy for EPRI to choose Texas A&M when it came to investigations stimulated by the claims of the production of heat from nuclear reactions during electrolysis made in March, 1989.

ORGANIZATION OF WORK AT TEXAS A&M IN THE EARLY YEARS

During 1989 to 1991, work on chemically assisted nuclear reactions was supported in four different groups at Texas A&M. At that time, the most well known person in the field at Texas A&M, because of his high reputation as a nuclear chemist, was Prof. Kevin Wolf, who initially showed enthusiasm and positivity towards an investigation of the new phenomena. Wolf (although a professor of Chemistry) worked in the Cyclotron Institute, in which professors from both the Physics and Chemistry Departments play a part.

---

[1]Molecular Green Technology, 4973 Afton Oaks Drive, College Station, TX 77845

[2]Two phrases are used, in abbreviated form, to describe these reactions. One is "chemically assisted nuclear reactions" (CANA). Another is "Low energy nuclear reactions" (LENR). The latter may not be accurate. Thus, there is no reason to think that the reactions concerned take place with any but those described by nuclear physicists, though slightly modified by the interactions with the surrounding lattice. However, the second of the two above allocations has entered the field and will be used here. "Nuclear reactions under solid state confinement at low temperature" correctly describes the field. The historical description "Cold Fusion" is misleading, for only a few of the reactions observed may involve fusion of nuclei.

Then, in the Chemistry Department, there were two active groups. The first was my own group which in the initial stages, had as many as 16 people working on the phenomenon, continued on a 24 hr per day basis for a few weeks, after which between two and four workers were engaged full time. Charles Martin, a Professor in the Analytical Division in Chemistry, had at one time six of his collaborators engaged but this number also fell rapidly as the "quirky" and irreproducible nature of the phenomena (never verified by Martin) became clear.

The third group was in the Center for Electrochemical Systems and Hydrogen Research under the direction of Dr. John Appleby, contributions being made by the Associate Director, Dr. S. Srinivasan, together with two collaborators, one of which was Dr. Oliver Murphy, later to become the director of Lynntech, a burgeoning electrochemical company in College Station, Texas.

The Thermodynamics Research Center had experimental work in 1989-1990, with two people but its major contributor was that of Dr. Kenneth Marsh who collaborated with Prof. Charles Martin and his students in their heat flow measurements. He also gave a cursory examination of the the heat measurement technique in my own group.

Thus, there were five professors [3] and about 10 students working on the LENR phenomena at Texas A&M for 1-2 years, and four students for about five years.

THE FIRST DIRECT MEASUREMENTS OF TRITIUM

In their initial paper with Harkins, Fleischmann and Pons reported the measurement of excess heat together with a very low rate of emission of neutrons. They also claimed that there was evidence for tritium formation by an indirect argument.

It seemed, in my own group, that it was best to aim predominantly for a direct measurement of tritium. The main point was: is the heat nuclear? Tritium would show up easily in the scintillation counter, - be quite unambiguous, - and we therefore pushed our investigations more towards discovering tritium than heat. Also, the cells for tritium were much simpler than the cells for heat. Thus, some of the tritium seeking cells in these early days were nothing more than test tubes with a single palladium wire in the middle, a platinum wire anode surrounding it, allowed to electrolyze for several weeks with tests for tritium made every few days (in an early visit to Fleischmann and Pons at the University of Utah, Pons had spoken about electrolysis for several months before nuclear activity was seen!).

The heat measuring cells involved continuous measurements of temperature in cells immersed in thermostat baths which were kept at ± 0.1° C.

In 1989-1990 we also made a wide swathe of changes in the type of electrode surface, work carried out largely by R. Kainthla. We surmised that the phenomena were surface determined and hoped to hit on the critical surface structure which would always produce tritium. However, we found that cells in which we made no attempt at special preparation of the surface or purification of the solution seemed to produce tritium more frequently than cells in which the electrode had been carefully prepared or with solutions which were super-purified. It turned out that adding an organic "poison" to the solution increased the magnitude of nuclear effects.

The first time tritium was detected in our group came as a surprise. At this time both Charles Martin and his coworkers and my own coworkers were taking our solutions (after electrolysis) to the Nuclear Engineering Department because it was there that tritium measuring capabilities were available. We had already made some half dozen visits without seeing any tritium and then one day (in April, 1989), the technician who made the measurements exclaimed "What have you done to this one?" He reported a very tritium-active cell for which a graduate student, Nigel Packham, along with others, had been responsible. Thereafter we began to observe tritium "every so often" and by the end of 1991 it came to 18 successful experiments out of 58 attempts, about 1 in 3.2 runs.

---

[3]Prof. M. Soriaga collaborated with Prof. Martin in some surface analyses connected with the work.

At first, we took the solutions allegedly containing tritium, to be measured at other places (i.e., outside Texas A&M), in particular, to Los Alamos National Laboratory, and it was confirmed from measurements of the spectrum that it was indeed tritium that we had produced. The tritium in the electrolyzed solution was confirmed also at the General Motors Technical Center. The first published work was in the *Journal of Electroanalytical Chemistry* [1], and the earliest results published in a refereed journal are plotted in Fig. 1.

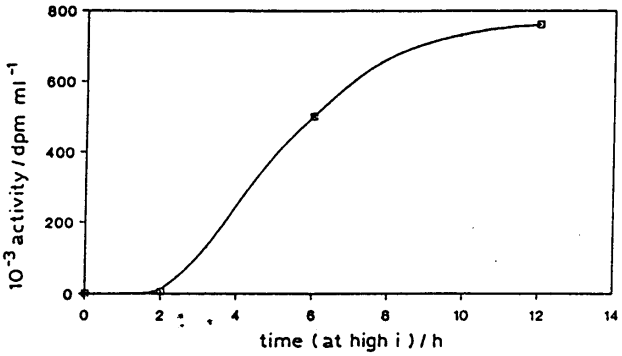


Fig. 1. Time dependence of tritium activity in an early cell.

The fact that we had definitely observed tritium seemed to be an event of importance to me, so much so that I called Martin Fleischmann in Southampton to tell him about it.[4]

Ours were not the only determinations of tritium made at Texas A&M. Parallel to our own work, Kevin Wolf made his independent measurements of tritium [2] and these were indeed presented at the Santa Fe meeting May 6, 1989, showing a strong evolution of tritium in his cells (Fig. 2). Later on, Wolf was to withdraw these measurements, speculating that what he had determined was residual tritium impurities in the palladium. (He surmised that the Pd samples might have been exposed to tritium earlier in their history). The variations of Kevin Wolf's attitude towards chemically assisted nuclear activity will be discussed in a later section.

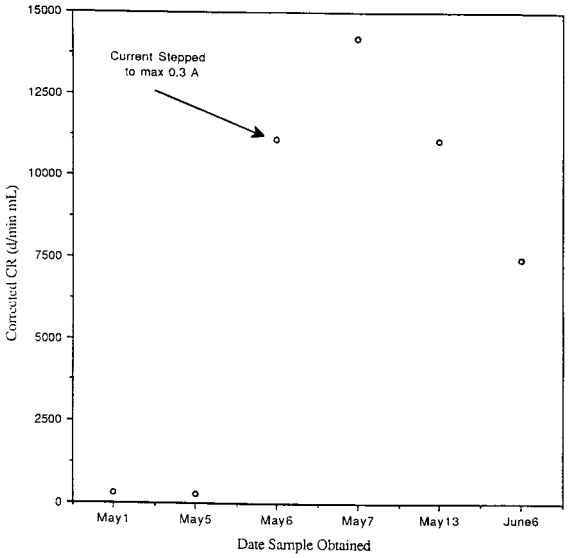


Fig. 2. Some of Wolf's data on tritium produced by electrolysis of LiOD onto Pd.

Tritium was found in solutions after the electrolysis of LiOD in D_2O. During times up to 600 hours, tests for tritium were made on most week days. Some characteristics of successful (i.e., tritium-producing cells) were:

(1) Tritium was found only after prolonged electrolysis, between 50 and 500 hours. If a system did not manifest tritium or heat before 500 hours, it was regarded as a null experiment and disconnected. About one experiment in three showed tritium in this time. Experiments in which the Pd was deposited on a gold base (from LiOD-D_2O contained $PdCl_3$) during co-evolution of D_2 showed a greater probability of finding tritium: small amounts in six experiments out of nine [3].

(2) The tritium was produced in bursts. Each burst lasted a few hours. A further burst could then be initiated by increasing the electrode potential cathodically, i.e., increasing the fugacity of hydrogen within the palladium [Chien et al., ref. 10].

(3) One of the more challenging results was that an electrode producing tritium stopped when fresh D_2O was added to the cell. After 4-6 hours, production of tritium in the solution re-began spontaneously.

(4) After the cessation of a burst, the tritium concentration in the solution would decline exponentially over ~ 12 hours (thus proving that the tritium present in solution was in the form of a gas (DT?) Sparged out by the continued bubbling of D_2. As there was not always a continuous recording of the tritium, it may well be that more cells than those counted produced bursts of tritium production. Were a cell to produce tritium from, e.g., the evening of a given day and be measured at mid-day the next day, the run would count as a null run for the tritium of the burst would have been removed by constant D_2 bubbling.

(5) The only trend discovered which showed how to increase the tritium was to have a solution to which

[4] I also ran into the late Derek Barton, Nobel Laureate, in the parking lot on the day I called Fleischmann. I told him of the discovery of tritium formed in the cold. His reply was "It took me 20 years to get it."

organic additives were present [Hodko and Bockris, ref. 31]. Correspondingly, solutions which were not carefully purified were more likely to produce tritium than purified ones.

THE EXCESS HEAT

Martin Fleischmann had explained to me on the day after the TV announcement his idea of obtaining the excess heat by utilizing the formula **(E-1.54)I** for the heat produced by the cell. Thus, this is the classical heat in watts (E in volts and I in amps). The input power from the circuit is EI but there is a subtraction from the total potential across the cell of 1.54 which is equivalent to the heat which would be emitted if the reaction for the splitting of deuterium oxide were carried out thermally at such a potential that there would be no (thermodynamic) heat evolved to the surroundings or taken up from them, in the electrochemical case, this energy is represented by the electrical potential applied to the cell.[5]

We then constructed a thermostat bath which was relatively simple, i.e., the temperature control was ± 0.1°, not better. The thermometer in the cell could be moved easily from top to bottom of the cell to examine the degree of uniformity of the temperature in the solution. We used a magnetic stirrer within the cell and found it was important to stir at a constant rate.

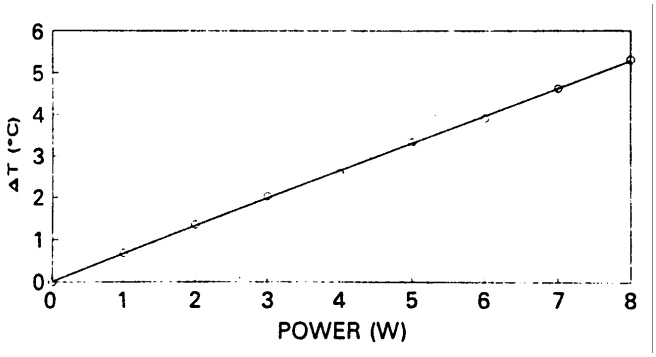


Fig. 3. Calibration curve for the heat measurements.

In order to calibrate the cell we utilized [4] wires of high resistance immersed in the solution and applied a certain current to them, measured the potential difference between its ends and then plotted the power input against the temperature in the cell (Fig. 3).

Now, the argument is that Joules per second of heat generated by our electrolytic cell, containing deuterium oxide, looked at in terms of normal electrochemistry, would be **(E-1.54)I**, and therefore, as we now knew the temperature in the solution of our cell as a function of the rate of heat production passing a current through a resistor therein, we could plot the actual temperature observed and find then the (expected, classical) heat being, in fact, produced by the cell functioning at E volts and passing I amps..

If coincidence occurred between the temperature observed for a calculated (classical) watts from our cell, and that produced by the resistor set to put out the same watts, then our electrochemical cell was behaving normally, indeed emitting heat, but not an excess heat, asking for an explanation.

What we were seeking was a series of points above the line established by our calibration procedure (i.e., the plot of the temperature reached by the classical performance of a cell of potential E and current I), against the power in part.

For several weeks, we carried out heat experiments but always got coincidence between the temperature, which the cell would be expected to develop by the number of watts given by the classical equation **(E - 1.54)I**, and the line calibrated with the heater. This gave rise to pessimism among the coworkers who got bored with the idea of constantly confirming the equation **(E-1.54)I**, - and the temperature expected, i.e., the heat output was simply that of the classical equation. At this time I was visiting the lab more than once per day and on one occasion (3-4 weeks after we began), I thought that the temperature readings in the notebook seemed to be larger than usual. I did the plot myself, of temperature versus input watts by the classical equation, then

[5]The (E) of the cell contains several heat producing components which were well realized before 1989 (cf. Bockris and Srinivasan, 1969)[3]. For example, there are several overpotentials at the electrodes; and, of course, there is I^2R heat due to ohmic effects in the solution. However, these components are included in E, the total potential applied to the cell. Thus, if the heat produced by the cell exceeds (E-1.54)I watts, then there is an abnormal origin of this extra heat. If nuclear products are observed with the excess heat, it is reasonable to conclude that the excess heat is nuclear in origin.

and there, standing at the bench. The line I got was, on average, 20% (in terms of equivalent watts) above that expected from the classical formula. The difference between the line I plotted from the notebook and that expected from the classical equation increased with increasing current. Thus, the excess heat in the measured cell was, on average, some 20% more than the well confirmed classical heat (Fig. 4).

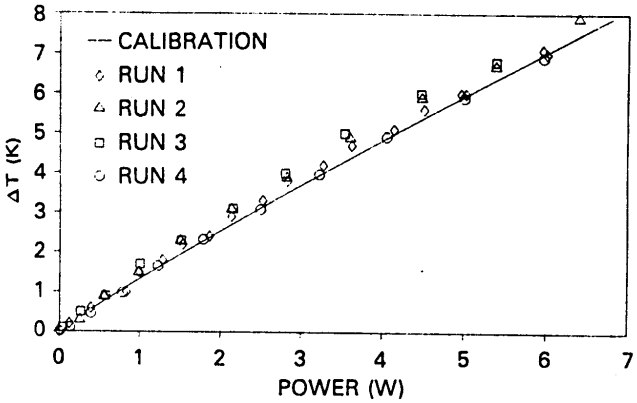


Fig. 4. An example of early heat measurement from Kainthla et al. [4]

During the succeeding year, we got six experiments which showed excess heat [4]. The number of completely separate experiments (fresh electrodes, fresh solution) in which we measured heat was about thirty (i.e., one cell in five). The smaller number of these measurements compared with those seeking only tritium was due to the relative ease of doing the tritium measurements, compared with those of the heat measurements.

Fleischmann and Pons [5] have discussed in much detail the thermo chemistry of their heat measurements. However, our simple procedure seemed unambiguous. We had put in a certain known number of I^2R watts from the resistor and measured the temperature rise it caused in our cell. We had been careful during the calibration with the resistor to stir and bubble nitrogen at the same rate as that at which we bubbled deuterium. In most of the runs, our electrodes put out heat corresponding to the classical electrochemical formula, i.e., this confirmed the heat measuring method (and the classical behavior of these cells).

Other experiments were made to check the possibility of recombination of H_2 and O_2 on the electrode as a source of excess heat. That the excess heat was due to this was the a theme of later work [6]. While this explanation was incapable of explaining the large heats (many hundreds of percent above the classical heat evaluation) reported by some, a recombination explanation could have explained the lower excess heats, such as those obtained in my laboratory. To test this we made experiments in which we withdrew the tops of the electrodes from the solution, one mm at a time. If the hypotheses were correct, a spurious excess heat would be proportional to the extra area exposed for catalytic recombination. However, we found (surprisingly) no extra heat produced by this maneuver, probably due to the film of solution which covered the exposed electrode. Diffusion through this film of hydrogen and oxygen together causing recombination on the catalytic palladium was apparently too slow to be significant.

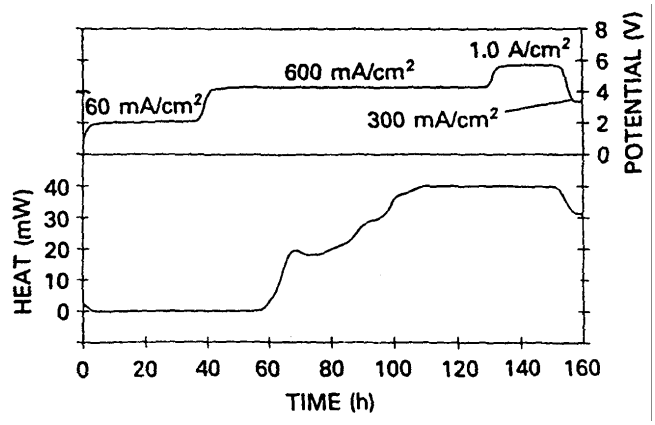


Fig. 5. An example of early heat measurements made in the Center for Electrochemical Systems and Hydrogen Research at Texas A&M (Appleby and Srinivasan [8])

The experimentation on heat production at Texas A&M included work by Appleby and Srinivasan in the Center for Electrochemical Systems and Hydrogen Research. A switch on of excess heat after only about 60 hours of preliminary electrolysis was obtained [7] (Fig. 5). O. Murphy of this Center carried out SIMS work on the penetration of Li into Pd and found Li had diffused to 4000 Å into the Pd.

An early experiment carried out in the Center concerned whether there was evidence for recombination in some other, undefined, way. Thus, deuterium and oxygen which were being evolved were collected and then sparked and the amount of water thus produced measured. If recombination in the cell had been a significant part of the extra heat evolved, the water found from the recombination of the off-gasses would be less than that calculated assuming no prior recombination had taken place. Three measurements made each one

showing 100% ± 2%.

Another interesting experiment from Appleby and Srinivasan [7]concerned the effect of replacing LiOD by NaOD (Fig. 6). Solutions containing Li were more effective. Later experiments were carried out at the Center to differentiate the effect of Li^6 and Li^7 but here no defining result was obtained.[6]

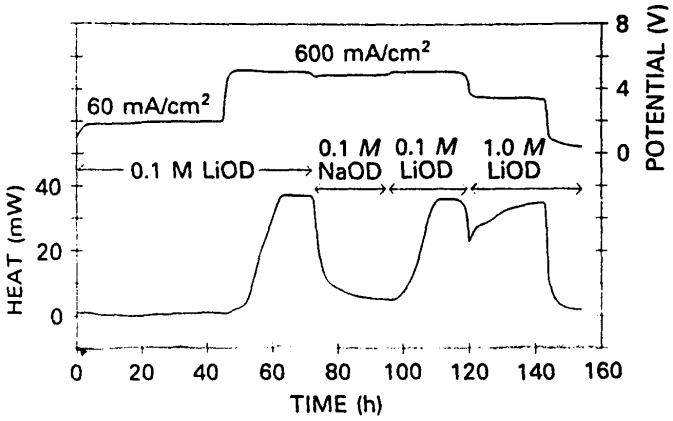


Fig. 6. Effect of replacing Li with Na in the solution. Work done in the Center by Srinivasan and Appleby [7]

There may well have been more heat bursts in our experiments than those we recorded. We did not have continuous automated recording of the cell temperature until mid-1990. Measurements during the night were made only for a brief six week period at the beginning. As the heat, when observed, comes in bursts, lasting minutes to hours, it is possible that our measurements missed some bursts occurring during the two-thirds of the day in which no measurements were made.

To confirm our calorimetry, we called upon Prof. Ken Marsh of the Thermodynamics Research Center. I had been acquainted indirectly with Dr. Marsh's work at the University of New England in Australia, where he had worked for many years with Robin Stokes, a renowned solution electrochemist who had made many thermal measurements with Marsh. His opinion was that heat in the order of watts and needing resolution to only 0.1% was an easy calorimetric measurement. He told us that the heats he was accustomed to measuring were less in magnitude than 1/10th of the heats we were measuring. His view supported our calorimetry.

An important type of heat-seeking experiment we did with Hodko concerned the effect of anodic/cathodic pulsing on the heat production. We thought that if we had a period of anodic polarization we would dissolve the hydrogen out of the surface of the electrode and then bring in H freshly that this might provoke the heat bursts which we observed sporadically.

We had some success in this which is shown in Fig. 7 [8]. After a period of low d.c. current, lasting about 100 hours, the electrode was subjected to cathodic-anodic pulsing, of such duration and potential range that it was reasonable to regard the surface portions of the electrode as being filled and emptied of dissolved D. The excess heat bursts thus provoked increased in magnitude with successive pulses (although the potential range and duration of the current pulsing were kept

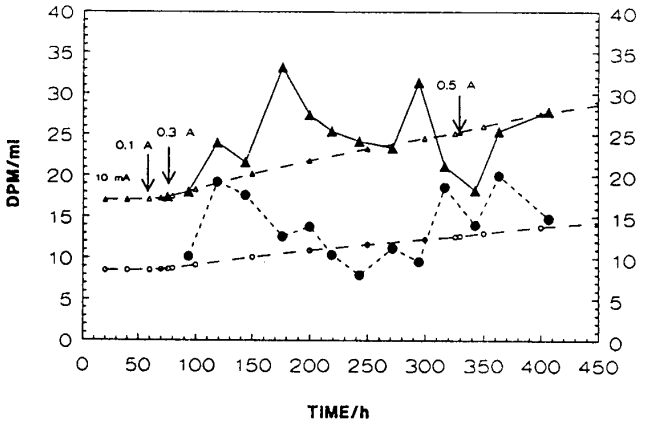


Fig. 7. Tritium production in systems involving co-deposition of Pd and D_2. The switch on time tended to be lower than when cold Pd electrodes were used (though 50 hrs preliminary electrolysis was still needed).

[6]Appleby wrote up some of the work done at the Center and submitted it to *Nature*, whose Editor at that time, John Maddox, who wrote two editorials in *Nature* encouraging scientists to denigrate work he called "Cold Fusion." Appleby received criticism from a referee and rejection of the publication. However, he replied to the criticism. The referee replied again, developing new criticisms to which Appleby replied. Three exchanges with the referee occurred, with (as seen by Appleby) completely satisfactory answers to the referee's questions being given. Indeed, this was supported by the referee who, in his final letter, wrote he could find no further fault but that the paper must still be rejected, "for if what is claimed is true, cold fusion would be evidenced, and that is impossible."

constant).

Lastly, in this account of attempts to obtain the excess heat at Texas A&M, it is necessary to tell of the work led by Charles Martin. The competitive Martin was eager to "beat" my colleagues and me in being the first to register an excess heat. I was in my university office one Sunday afternoon when I got a phone call from Nigel Packham, one of my graduate students. Packham's wife worked in one of the television stations in College Station and she had called to say that the news had come in that Martin was going to hold a press conference that Sunday evening. His group had been the first to observe an excess heat! I was surprised because up to this time it had been agreed between Martin and myself (at that time in frequent conferencing) that if one team got there before the other in heat measurements, it would lead to a joint announcement from the Chemistry Department at Texas A&M.

I went to the Sunday evening press conference, held in the Chemical Engineering Department, where Martin's group seemed to have some cells. I had not seen their setup before. I found many people there including the Head of the Chemistry Department, at that time, Michael Hall. I was not allowed to go into the room where the apparatus was, for this was locked. There was an air of secrecy and great expectation.

Martin made a brief speech, stating the essential results to the small gathering, and the next day there was a major meeting with the Press presented in an auditorium in the Engineering Department to which Martin and Ken Marsh and others were on the podium and described their work in detail.

I congratulated Martin on the Sunday and was pleased that Texas A&M had come out ahead as the first university to report an excess heat corresponding to that of Fleischmann and Pons. However, the euphoria was short lived. The next we heard was, surprisingly, from an announcement in a New York newspaper, to the effect that "Texas A&M" had withdrawn its heat results. There had been an error of measurement! I asked Martin to come to my room and gathered relevant coworkers to hear his explanation. He said he had discovered that there had been a "trailing wire" in the cell and an unrealized evolution of hydrogen on this second electrode had increased the heat, but classically.[7] At any rate, his success had been short lived and the disappointment which followed may have contributed partly to Martin's decision to leave Texas A&M for a position at the University of Colorado, a year or two later.

THE MEETING AT SANTA FE

Admiral Watkins, at this time (1989) Head of the Department of Energy, was evidently more than interested in the claims that nuclear reactions could be carried in a simple electrochemical cell at room temperature. His interest was understandable because DOE was supporting a huge nuclear research program on hot fusion (sums on the order of $1billion per year were being spent worldwide) in an effort to attain the Holy Grail of a practical heat source which would last for a thousand years! Could the alleged discovery of "fusion in a test tube" lead to a radical cost reduction in nuclear heat research and the avoidance of nuclear pollution if the wastes (found later to be so) were simply helium? And this had been discovered outside DOE's long lasting program on fusion?

Admiral Watkins sent out an edict to the U.S. National Laboratories to reproduce the results of Fleischmann and Pons "immediately". However, the national laboratories contained few chemists, and a very small fraction of this small number had experience in operating electrochemical cells (with their Luggin capillaries and potentiostatic electrode control). These few had evidently not understood the reality of Fleischmann's comment, made in the original TV announcement, that the experiments were difficult to carry out. The physicists and chemists obeying the Admiral's command were, for example, extremely unlikely to wait 500 hours for the switch on of nuclear phenomena. **Most of those who tried to replicate the phenomena put two electrodes in D_2O-LiOD, turned on, put a neutron detector near the cell and found nothing. After a few hours they turned off and reported that the phenomenon did not exist.** One imagines that, in the

[7]The local press in College Station remained ignorant of this event although it became active in ridiculing all aspects at the discovery of low temperature transmutation reactions. When the American Nuclear Society began to accept sessions on "Cold Fusion" including papers describing transmutation, the College Station press did not report this significant event (although made aware of it by me).

circumstances, such reports were received with a sigh of relief.

However, positive results were coming in from other parts of the world and the extreme importance of deciding an answer encouraged Admiral Watkins to have organized a well attended meeting in Santa Fe, New Mexico on May 23-25, 1989, some eight weeks after the Fleischmann-Pons announcement.

In respect to the very long switch-on time needed to obtain positive results for the phenomena, the date of the meeting was premature, because, assuming that it took around one month to have made cells, put them together and operate the new and unfamiliar equipment and get going by May 1, the time necessary for the switch on of the nuclear effects (up to 500 hours sometimes) would hardly have been reached by the beginning of the meeting!

I sat up front at the meeting with Norm Hackerman (ex-President of Rice University and an electrochemist) on my left and Nobel Laureate Bob Schrieffer, - whom I had known during my time at the University of Pennsylvania, - on my right. Norman Hackerman kept a score on the papers in a simple way. Either the authors observed anomalous behavior or not and his conclusion was that, as far as the Santa Fe Meeting was concerned, it was 50-50.

I made several discussion contributions at the meeting. Fleischmann and Pons were not there and I tried to explain some of the things which had not been understood about their work. The majority of the people present were nuclear physicists and their comprehension of the electrochemistry was fragmentary.

It was soon apparent that the lack of heat observed in some cells (presumably after a one day experiment) was compensated by the temperature of the comments of some of the participants. I had not previously been acquainted with red faces and angry shouting about scientific results and it surprised me to find people becoming so worked up. One thing I could not understand was the supposition, only thinly veiled, that FRAUD was involved.[8]

These emotional difficulties of the meeting were not helped by the fact that all was being televised. For this purpose, a bank of very intense lights shone on the speakers and when one got up on the podium, one could hardly see the audience because one was being “lit up” so intensely. This led to angry complaints by speakers.

I was asked to chair the discussion group (more than 100 people) in the evening of the first day, and it certainly was a lively session which lasted for about 1½ hours. I tried to be as even-handed in dealing with the angry contributions as well as the quieter more positive ones. About 9:30 p.m., - somebody came up to me as I was standing in my chairman’s position on the podium, and said “The buses want to leave now, can you please close the meeting?”

At this time we had been going for about 1 hr - 30 mins., and as the buses were leaving to take people back to the hotel, I announced that on these grounds, the meeting would now be closed. The next day I found that some people accused me of having stopped the meeting to avoid the hostile questions! The attitude of many was indeed hostile towards anyone who was willing even to entertain the possibility that two chemists had made a Major Discovery in a central area of Nuclear Science.[9]

Two other aspects came out: one concerned tritium. In my own research group [1], we thought we were the

---

[8]The physicists at the National Labs who were supposed to examine the Fleischmann-Pons experiment suffered not only from a lack of knowledge that they had to wait a few hundred hours if nuclear events were to be observed but also the expectation was that the order of magnitude of heat reported by Fleischmann and Pons would give a huge neutron flux. However, even when, eventually, some excess heat was confirmed, the neutron emission was negligible. To a classical nuclear physicist, this meant that, if a nuclear reaction were occurring, then it was doing so only at a negligible rate.

[9]Thus, a Nobel Laureate in Physics is reported as having commented that, if “Cold Fusion” had been discovered at a “Senior university and in the Physics Department,’ it would have found acceptance. Work of chemists at a state university which contradicted basic ideas of physicists, was to be treated with contempt.

first to observe tritium (we announced this at the meeting) and we were certainly the first to publish our results in a refereed journal, but at the meeting it transpired that Prof. Gozzi in Rome thought he had also seen tritium once but wasn't willing to publish until he had a confirmation. There was an older retired physicist from the University of Florida, who told me privately that he could produce tritium "at any time." However, no publication was seen from his work. (He offered his cell and methods for sale at a very high price.)

The Santa Fe meeting was also my baptism into the phenomena of the Suppression of Scientific Information. I was walking along an upper corridor at the meeting place when a middle aged man came up to me carrying a brief case and asked me if he could speak to me alone. We found an empty room and closed the door. He took out some papers and said: "My boss would kill me if he knew I'm showing you these. They are positive heat results." He then showed me graphs which seemed to show a substantial excess heat, though I examined them for only perhaps three minutes.

This man (who came from a National lab) then told me that he had showed his boss the results on the flight over to Albuquerque, and had been told that he must forget them, that such results were against established theory and therefore could not exist. He should stop reporting the results of his experiments, unless they were negative, whereupon he should publish them at once.

An incident having the same implication as that related above occurred later at another government laboratory, where I was told that a member of the ERAB Committee was shown positive results of tritium production but then had flown into a rage, thrown the lab book containing the graph showing the results on the floor and removed himself hurriedly from the meeting room.

THE ACHILLES HEEL OF THE FIELD: IRREPRODUCIBILITY

The Santa Fe meeting left the field in an ambiguous state. There were those, about half the speakers, who recorded results which suggested that nuclear events were going on in the electrochemical cells. On the other hand, there were those who had tried and failed, in particular those who had switched on and seen no neutrons.

Speaking from results in my own laboratory, our score in respect to tritium production was approximately a positive result for every three experiments. By "an experiment" I mean a completely fresh electrode, solution, cell, and electrolysis for up to 500 hours. To me this was hugely evidential. However, it would be equally true (if rather non-discerning) to say that "most of our experiments gave nothing anomalous at all."

Very significantly we got more reproducible tritium results, when (later) we made the electrodes (following the suggestion of Boss and Szpak [9]) by precipitating the palladium onto gold with coevolution of deuterium. This implies that the irreproducibility depends on internal structure: thus if one saturated the Pd quickly, with D, reproducibility increased [13] (Fig. 8).

Another improvement in reproducibility (this time in respect to heat) was achieved by Sundaresan in my lab when, - following a suggestion due to Takahashi, - he used a potential ramp, gradually [11] increasing the overpotential on the electrode and then decreasing it (Fig. 9). This gave an immediate success under conditions mildly similar to those of Hodko [3] and is worth further investigation.

By 1991, Dr. Charles Martin seemed to me, began to show signs of work stress. He appeared to have become anxious in respect to the inability of his own team to obtain tritium or heat. He was said to have taken

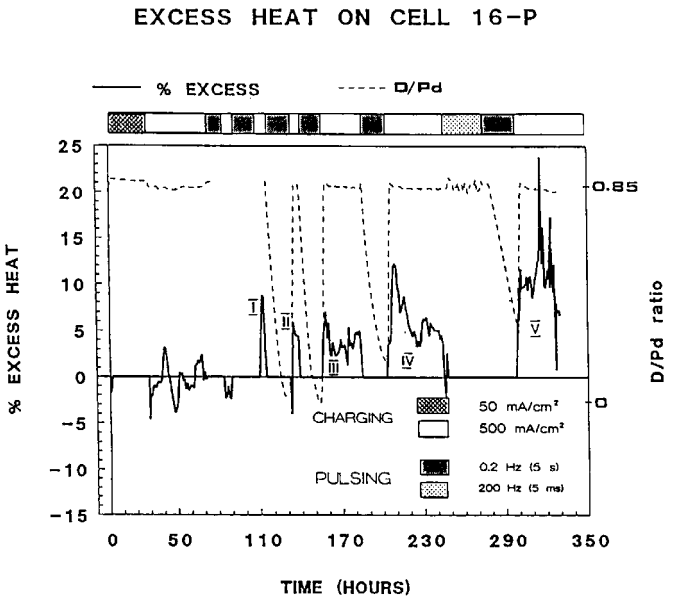


Fig. 9. Increasing excess heat produced in bursts as a result of cathode-anode pulsing (Hodko [8].

cells home to work on them in his garage, undisturbed. After Stan Pons expressed anxiety to me about Dr. Martin's emotional health, I called his wife to ask after her husband.

When Martin's group continued to get no tritium, some in Department of Chemistry started the rumor that results gotten by Nigel Packham must be due to his spiking the solution with tritiated water! The situation of the failure of Martin's group to observe the tritium was featured in a letter sent by Martin to the Dean of Science.

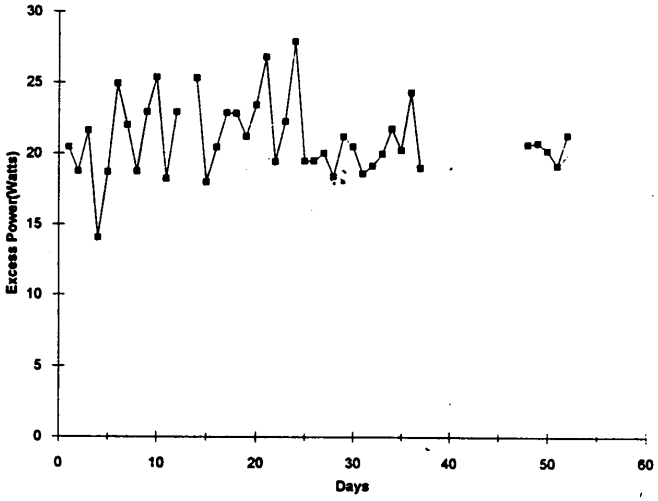


Fig. 8. Excess energy production, lasting ~2 months, obtained by Sundaresan [11] using Takahashi's ramping technique. Switch on still needed several hundred hours.

To meet this unfounded rumor,[10] I asked Packham to start writing his thesis, and replaced him by giving the heat and tritium work to Dr. Kainthla and Mr. Velev who carried out runs which gave rise to a relation between excess heat and tritium and as shown in another section.

Viewing all this in retrospect, and now knowing the results of more than 100 other groups, irreproducibility is a key characteristic of the field. Any theory of chemically assisted nuclear activity must be able to embrace and explain this fact. (None of the theories hitherto has addressed it.)

Of course, the irreproducibility (and very long waiting time) has an extremely negative effect on the acceptability of the phenomena by other scientists. Here is a phenomenon which is anti-paradigmatic. But the phenomenon, so attested throughout the world in hundreds of laboratories, which has generated (1999) more than 2,000 positive papers [12], does not yet allow defined conditions such that any worker can promise to switch on the phenomenon at will.

We made attempts to probe the problem of irreproducibility. We thought that a certain kind of surface would be the thing to have, but numerous surface preparations made by R. Kainthla gave none which consistently evidenced nuclear phenomena. We thought [13] that the amount of light water could affect the happenings inside the palladium because hydrogen bonds to palladium in a stronger way than deuterium: perhaps the deuterium did not bind to Pd inside the metal if hydrogen was there first. But, later, there were found to be solutions which contained several percent water and which gave tritium and some heat.

The only generalization which came to be agreed was that solutions containing impurities were better than those which we had striven, e.g., to purify or to produce a surface in a certain way.

The other curious aspect was time. When to call a run "unsuccessful"? Thus, in one sense all runs were unsuccessful and the phenomena do not exist if, in respect to the original palladium-lithium-deuterium oxide systems, one turns them on and seeks heat or tritium within 24 hours. When success is attained, it comes after hundreds of hours of electrolysis. We arbitrarily set the time limit of observation at 500 hours.[11] One might surmise that unsuccessful runs were simply runs which had not been left to electrolyze long enough.

Another feature with respect to time was the burst-like nature of the phenomena, both heat and tritium production. We found in respect to tritium, that a burst would occur - lasting several hours - and then tritium production would stop. Thereafter, the tritium content of the solution underwent an exponential decay (sparging out DT by the constantly bubbled in D_2). Thus, a burst in the evening might leave nothing to examine by the next day.

This burst-like character was well evident in a technique used by Hodko to obtain heat [8]. He subjected electrodes to cathodic-anodic pulsing, contrived so that the surface of the electrode could be emptied and filled successively. This technique produced corresponding bursts of heat (Fig. 9), each successive burst larger than the last.

THE RELATION OF TRITIUM PRODUCTION TO HEAT

By the mid-1990's we had undergone two changes in the LENR work in my group. On the one hand we had eliminated Nigel Packham from it, not because of any question I had of Nigel's reliability and integrity, but because of the damage done to him by an article in Science by Taubes [14] which suggested that the finding

[10]Why Martin's workers did not detect tritium remains unclear. After the incident with the solo Press Conference, collaboration between my group and that of Martin, ceased. One wonders if the 50-500 hours which was the waiting time before tritium appeared was not allowed to have its effect in the work of Martin and his group.

[11]This applies strictly to heat measurements. The tritium experiments were sometimes allowed to run on for weeks before tritium was seen.

of tritium by Packham was fraudulent.[12] Ramesh Kainthla, a very reliable senior post-doctoral fellow, and Omo Velev, who later went on to become Acting Head of the Center for Electrochemical Systems and Hydrogen Research at Texas A&M University, took over the work.

There was also an improvement of technique by later 1990. Earlier we had had simple cells which were for producing tritium; and more complex cells in thermostats in which we could estimate excess heat. By 1990 we had automated the measurement of the temperature on a continuous basis and were taking samples both from the gas phase and solution to measure the tritium.

The results in the new set up were qualitatively as before. We could not rely on a given electrode to show nuclear activity within, say, 500 hours from the start of electrolysis, but on the other hand, some electrodes did give anomalous results and we could now measure heat and tritium at the same time, and examine whether there was a relationship between the two. A good example of this is shown in Figs. 10a and 10b (15).

These results indicated is a lusty heat evolution in bursts but also bursts of tritium production which seemed to correlate with the heat bursts.

When we first saw this, it seemed critically important : a nuclear reaction giving rise to tritium must be the origin of the heat. However, calculations showed that the tritium was less than 0.1% of that needed to produce the heat. We had to admit that the tritium was the result of a side reaction, not the reaction producing heat (cf our 1991 discovery of He^4 in the electrodes).

About this time, mid 1990, it became clear that there was a distinct (but utterly unexpected) relation between the rate of neutron evolution and that of tritium production. Because physicists had been used to seeking neutron as the *sine qua non* of nuclear reactivity, most of the first few years of work by physicists on CANA concentrated on neutron production and the negative opinion expressed as a result of these was that, - when, indeed, - neutron emission was observed, the rate was extremely low (1-10 neutrons cm^{-2} sec^{-1}). Figures in this range were reported by K. Wolf from the Cyclotron Institute at Texas A&M in the 1990's [2]. Indeed, Wolf

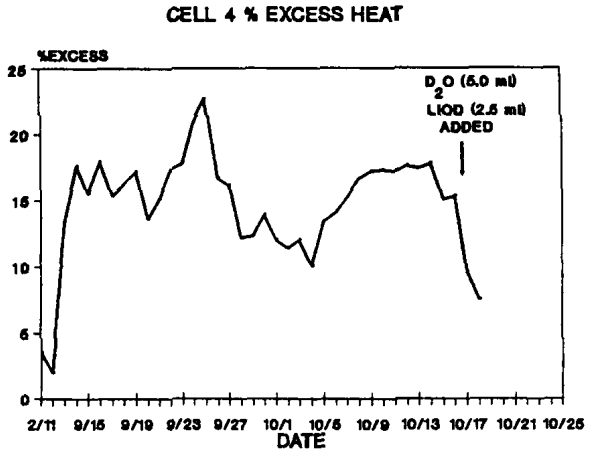


Fig. 10a The excess heat.

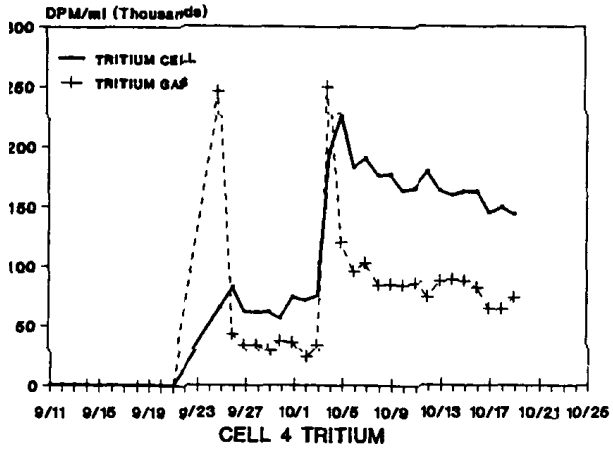


Fig. 10b The corresponding tritium production(Kainthla and Velev (5).

[12]One of the less pleasant realizations which came from the Taubes' article in Science [14] is that a scientist, defamed, has, in practice, no means of redress by appeal to the law. If he had $M to risk on a law suit, he would have to prove not only that the defaming statements were untrue but also that the publishing house concerned was intentionally trying to damage him. To prove malice is, of course, extremely difficult. In fact, Taubes (who told me that he writes scenarios in the movie industry part-time) certainly had no personal malice. However, he certainly wanted to sell his book [14], and books for public consumption do not sell on the basis of descriptions of the importance of eliminating errors in radioactive experiments. They sell best if they report evidence of something wrong. That there is a public which likes this is beautifully confirmed by the German word "Schadenfrende," which means, essentially, "Injury Joy or Happiness in Hurting."

continued to report neutron emission from his experiments until the end of his life.[13]

In classical nuclear chemistry, the fusion of two D atoms (which was assumed at this time to be occurring in the cell) should give a neutron and He^3 in equal amounts (or tritium and protons in equal amounts). Thus, a rate of 10 neutrons cm^{-2} sec^{-1} would suggest a similar rate for tritium. In fact, the tritium was being reported as 10^6-10^{10} atoms cm^{-2} sec^{-1}. If one takes a mean of the logs of these figures, the ratio $\frac{tritium\ cm^{-2}\ sec^{-1}}{neutrons\ cm^{-2}\ sec^{-1}}$ is about 10^7, whereas unity was the result expected from classical physicists in 1989.

There were two attitudes toward these newly discovered facts. The one was to say that such a ratio was "impossible" and must be due to errors of measurement, but, it had been obtained, by 1991, in several laboratories. The other was to say conclude that there must be some new nuclear process which was almost a-neutronic.

DEPENDENCE OF THE RATE OF TRITIUM EVOLUTION UPON THE ELECTRODE POTENTIAL

The work on tritium continued into 1992 and was encouraged by the work of C. C. Chien, a worker from Taiwan, who came to my lab after successful experiments on LENR in which he had demonstrated electrochemical tritium production independently in his own lab [16].

Chien [17], working with Hodko and Minevski, carried out several runs on electrodes, but observed no nuclear activity. In a series of experiments with a single rod, he cut the rod into three parts and on one part, he had a result of spectacular success.[14]

Thus, tritium began to evolve after about 100 hours of prior electrolysis, but the evolution was in bursts, after each of which the tritium content of the solution did not change though electrolysis continued. Very significantly, we found, however, that we could revitalize the electrode into a new burst of tritium if we increased the overpotential of the working cathode. Now the electrode evolved tritium at a higher rate than that at the less negative potential. However, this only lasted for a time of hours, whereupon it was necessary to stimulate the electrode again with an increase of overpotential, whereupon the electrode produced tritium at a greater rate than at the lesser overpotential for a few hours and so forth. By taking the slope of the concentration of tritium in the solution versus time line, we converted these results into rates, i.e., atoms of tritium produced per sq cm per sec and plotted the log of these against V, the calculated electrode potential (but only 4 points), whereupon we got (very roughly) a straight line and the slope of this line (i.e., $\frac{\partial V}{\partial\, log R_T}$ where R_T is the tritium production rate) was about 0.2, the same order of magnitude as that which one would expect were the evolution of the tritium to be rate determined by discharge of tritium containing ions onto the surface of the electrode (Fig. 11). This was an interesting (and entirely original) result though lessened in significance by the small number of points. This result has as yet to be taken into account in theories of the tritium production.

[13]Most unfortunately, Kevin Wolf's productive research career was ended by his premature death in 1997.

[14]This result - a part of a rod only active, - is a telling result. Can it mean that the nuclear activity depends critically on unknown impurities which exist in minute concentrations heterogeneously in one part of the rod? The theory supported by Kucherov [26] would be consistent with the interpretation.

But the most baffling, and hence, significant aspect of Chien's experiments was that the production of tritium stopped when fresh D_2O was added to the solution! The cells were open, i.e., the deuterium escaped into the surrounding atmosphere, and the deuterium oxide was gradually used up so that every few hours we had to add D_2O. This stopped the tritium evolution. However, after some hours, tritium evolution into the solution started again spontaneously without any intended change in the system.

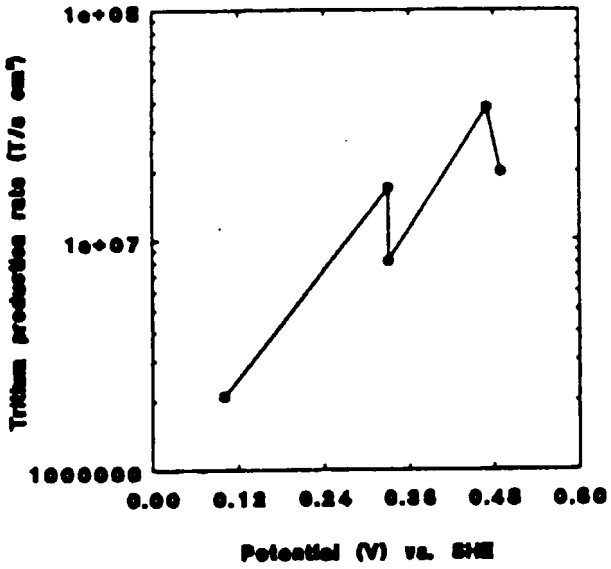


Fig. 11. The dependence of the rate of tritium evolution on the electrode potential (Chien, Hodko and Minevski [17]).

Eventually, after about one month of continuous electrolysis, with intermittent bursts of tritium production, throughout, we switched the cell off. The maximum production rate of tritium reached was 10^{10} atoms cm^{-2} sec^{-1}.

Why were the results on this particular piece of palladium[15] so good? Observation of the electrode with a light microscope under 500x magnification showed what seemed to be a copper mosaic pattern upon the surface. We noticed that in immersing the palladium electrode in the solution, we had let a metal contact with a copper lead come *into contact with the solution* so some copper had dissolved and deposited upon the palladium. Would this, then, change the mechanism by which hydrogen would have been desorbed from the particular (Cu covered) Pd? Suppose, thus, that the mechanism of evolution of deuterium on Pd had been rate determined by the atomic combination reaction, and then on Cu by the mechanism called "electrochemical desorption." In the former, the steady state mechanism would correspond to a low θ (the degree of coverage) and in the latter a θ. Could the high θ have given a more rapid diffusion per unit area of D into the Pd and hence given rise to that of prior tritium evolution compared to Pd pieces not exposed to Cu?

Is the production of tritium decided by goings on on the surface of the Pd? One might perhaps see the remarkable result by which the tritium evolution into the solution was stopped by the addition of fresh D_2O as indicating this. Thus, this addition certainly cannot alter the situation inside the Pd. Can fresh solvent alter the steady state situation of whatever is adsorbed on the electrode surface? This change in surface adsorbed material would then change the surface reaction which forms tritium? Thus, it could change the mechanism of H or D desorption from one with small to large θ, i.e., from low fugacity to left fugacity.

PSYCHO-SOCIAL ASPECTS ON THE TRITIUM PRODUCTION IN THE DEPARTMENT OF CHEMISTRY AT TEXAS A&M UNIVERSITY

During the long period (about one month) in which Chien's electrode sporadically evolved tritium, I invited several colleagues in the Department of Chemistry to come see a result so unexpected, so anomalous within the chemistry of 1991. One said that his son had a birthday party that day so that he could not come to see the remarkable experiment, and the other that he was going to Germany to do some experiments in an Institute there. I asked two other professors, telling them that, by staying with cell for one hour, they could themselves use the scintillation equipment in the next room and become convinced that the tritium concentration in the solution was increasing without the means of addition of tritiated water by stealthy secret nightly visits by graduate student Packham. These two individuals also declined to come and see, which reminded me of Galileo and the cardinals who, in the 16th Century, would not look through his telescope,

[15]This was freshly made Pd from Tanaka, Inc., not Pd extracted from old jewelry. It was the second hand nature of the palladium in the early tritium experiments which had given rise to Wolf's speculation that, maybe, the Pd had seen tritium earlier in its history, e.g., perhaps in use as a membrane in a nuclear reactor and it was this tritium which was being recorded (e.g., in his own measurements).

because it showed an irregular mountainous moon, although at this time, the moon was supposed in the Ptolemaic view of the Church, to be Queen of Heaven and "perfect".

On the other hand, when looked at psycho-socially (rather than in terms of the spirit of scientific exploration), the attitude of these professors, as careerists rather than scientists, was rational. They had read the Taubes article in *Science*, they knew the production of tritium in the cold (according to current texts) was impossible, and they clearly preferred the quiet life rather than having to face a result which they could not explain, and which would, in fact, put into doubt the basis upon which billions of dollars was being spent.

In respect to the process of "conviction," the experience of Dr. Dalibor Hodko, formerly a Research Scientist at the internationally known Ruder Boskovic Institute in Croatia, and latterly a postdoctoral fellow in my Group, is relevant. When I think of Hodko, the words "serious, formal, competent, official" come to mind. Being a physicist, he had at first a very negative attitude towards this so-called "cold fusion". It was his own experiments which convinced him. Having heard the rumors concerning the spiking of solutions with tritiated water, he decided, without telling me, to stay overnight with a run, taking measurements of tritium every two hours. He would then see the tritium concentration increase - and certainly would have detected the nocturnal interloper, armed with a bottle of tritiated water.

Hodko showed great surprise by finding the electrochemical formation of tritium in Chien's cell.. His new enthusiasm was put to the test, however, when our EPRI project manager, - on hearing from me of the night vigil, - said: "Tell him to stay a second night." Some years later, Hodko, at my request, wrote me a letter describing how the independent measurement, made during the night, had convinced him that tritium was indeed being produced in the cold.

ACADEMIC FREEDOM AT TEXAS A&M: PACKHAM'S THESIS

Nigel Packham, the graduate student who first observed tritium, eventually came to his Ph.D. oral examination. Normally, this event occurs smoothly, there being a committee of at least four academics present, including a representative of the Graduate School, who is there to see "fair play". However, the rules are that any member of the University can come and attend the Oral. By this time, rumors about Packham's discovery (and its denial by some) had spread and a large number of students and some faculty turned up.

As an insurance against trouble in a hostile atmosphere, I had invited Dr. Norman Hackerman, a President emeritus of Rice University and Prof. Ernest Yeager, a professor from Case-Western University, to be a part of Packham's Ph.D. committee. Hackerman had previously called me, saying that he had observed the electrochemical formation of tritium while on a visit to a Taiwanese university. Yeager had worked with Robert Adzic, a highly respected researcher, and had witnessed several experiments in which tritium had been produced.

At first the oral went normally, although in the oral presentation of the two parts of his work, Packham was slow in leaving his description of his work on hydrogen from the action of bacteria on water. I interrupted his discourse at half of its scheduled time, and told him to tell us about tritium.. At question time, I, as Chairman, gave Kevin Wolf (the most informed man present) an abnormally long time (eight minutes) to question Packham. Then, I moved to other questioners (there were many hands raised). Just as the discussion was beginning to wane, a younger faculty member walked down the lecture theater towards Packham and gave him what seemed to be a sheet of paper. It turned that these papers contained a whole series of questions. "Answer those" said the younger faculty member (who was on Packham's Ph.D. committee).

This put me in a dilemma. The public examination had already lasted longer than normal, and I had a limousine waiting to take the eminent Hackerman back to Houston. I therefore turned to the graduate school representative, an authoritative figure in the context, and whispered the question "What now?" He told me I should rule the younger faculty member's procedure out of order but that Packham would have to write answers to the questions "afterwards".

The large audience, and Packham, were then instructed to disperse and Packham's Committee began its deliberations as to whether Packham had passed this last hurdle to the Ph.D. degree. It seemed to be

generally agreed that Packham had done very well under some stress and it was agreed by all except one person that he had passed. However, the younger faculty member refused to sign, i.e., the student would be failed (after five years research) to get his Ph.D. degree. It is noteworthy that the Head of the Chemistry Department remained in the room during the succeeding arduous and bitter wrangling.

Finally, it was agreed (largely on the suggestion of the Department Head) that this young faculty member would sign if Packham wrote out answers to the questions in the back of his thesis, it to become a permanent part of the document. The signing of each member of Packham's Committee then occurred, normally the End of the Struggle for Ph.D. students.

The examining committee then left the examination hall. I recall seeing the Department Head go up to Packham (who had waited outside), shake his hand, and congratulate him in the traditional way on successfully passing the Oral Exam for the Ph.D. degree. I invited the senior academics to dinner at my club. It seemed that an intense academic battle on which depended the livelihood of Mr. Nigel Packham, had been won by a whisker.

However, the next day, the warm congratulations of the Department Head and grudging agreement of the younger faculty member had dissolved and the Department Head (who signed off routinely on all Ph.D. theses) announced that he had decided not to sign this thesis!!! He gave no specific reason but it was understood that the presence of a description of tritium formation in the cold was the problem. Thus, it was finally agreed (largely on the suggestion of Kevin Wolf) that the Department Head would sign if Packham removed all the data and experiments on nuclear reactions in the cold from his thesis! The papers published in refereed journals could remain in an Appendix. This reduced the size of the thesis by about one half. The rest was about the biological production of hydrogen, and the biochemically oriented examiners on Packham's Ph.D. Committee were then asked if they could agree that this alone was enough. They did. So, Packham got his Ph.D., as it were, by the skin of his teeth. It had been a process which must live in the history of the Chemistry Department at Texas A&M as a clear and unquestionable example of the RESTRICTION OF ACADEMIC FREEDOM in the Department. Packham was left with the alternative of maintaining the report of what he had found, and losing the qualification for which he had worked for six years, or suppressing it in so far as his thesis was concerned.

THE DISCOVERY OF EXCESS HELIUM IN THE ELECTRODE PRODUCTION AFTER ELECTROLYSIS

During the work of 1991, we were occasionally in contact with Nate Hoffman, of North American Aircraft, a physicist with metallurgical background, who later wrote an instructive book entitled "A Dialogue on Chemically Induced Nuclear Effects" [18]. With his principal research officer, Brian Oliver, he had invited us to submit our electrodes for examination by means of their unusually sensitive mass spectroscopy. He wanted to see if he could detect tritium inside the electrodes and also he was eager to see if his apparatus could detect absorbed helium (a difficult task for the At wt of He^4 is about the same as the Mw of D_2).

Table 1a Tritium results

T_{total} in electrolyte	T in gas phase (assuming T_g/T_l=5)	T in Pd (by dissolution in aqua regia	T in Pd (by anodic discharge method)	Total amount of T produced (electrolyte, gas, bulk)
1.6×10^{14} T atoms or 1.5×10^{13} T atoms/g or 2.5×10^{13} T atoms/cm^2	8.1×10^{14} T atoms or 5.4×10^{13} T atoms/g or 1.2×10^{14} T atoms/cm^2	5.1×10^{9} T atoms/g Pd (or total in Pd = 7.6×10^{10} T atoms)	8×10^{9} T atoms/g Pd (or total in Pd = 1.2×10^{11} T atoms)	~10^{15} T atoms or 6.9×10^{13} T atoms/g or 1.5×10^{14} T atoms/cm^2

Table 1b. Helium results

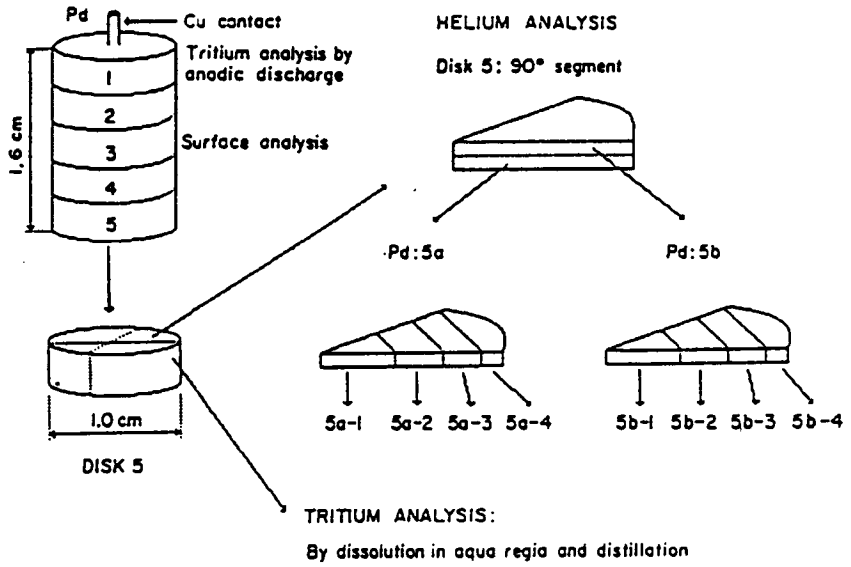
Samples near the surface (outer slices)			Samples away from the surface (inner slices)		
Sample	Sample mass[a] /mg	^{4}He/10^9 atoms or (10^{11} atoms/g)	Sample	Sample mass[a] /mg	^{4}He/10^9 atoms or (10^{11} atoms/g)
5a-1	30.92	3.8 ± 0.3 (1.2 ± 0.1)	1b-1	27.79	1.9 ± 0.3 (0.7 ± 0.1)
5a-2	39.70	166.8 ± 3.3 (42) ± 0.8)	1b-2	30.01	2.5 ± 0.3 (0.8 ± 0.1)
5a-3	42.37	3.4 ± 0.3 (0.8 ± 0.1)	1b-3	23.85	0.4 ± 0.3 (0.17 ± 0.12)
5a-4	20.22	2.1 ± 0.6 (1.0 ± 0.3)	1b-4	33.05	1.7 ± 0.6 (0.5 ± 0.2)
5c-1[b]	44.63	1.9 ± 0.5 (0.4 ± 0.1)			
5c-2[b]	30.40	-0.1 ± 0.5 -0.03 ± 0.1			

[a] Mass uncertainty is ±0.01 mg.

Hoffman instructed us to slice up the electrode using a diamond saw just after the run and put them in liquid nitrogen until we were ready to transfer them to him by mail, whereupon we were to pack them in solid CO_2 and send them overnight. This was largely to prevent the escape of tritium from the electrodes. A significant escape of helium was unlikely because the diffusion coefficient of helium in palladium is very low. We cut each length in a Brie cheese form [17](Fig. 12).

Eventually we got back the results from Hoffman and some of them are shown in Table 1. The most important concerned helium. We first got the results over the telephone and when we heard them, it was only partly encouraging because we were told that there was a mixture of He^3 and He^4.

The He^3 would not have been a happy result because, as stated earlier, Kevin Wolf had suggested that the tritium which we were seeing in our electrodes came from impurities in the original samples. If this were the case, the tritium would be decaying to He^3. Thus, observation by Hoffman and Oliver of He^3 in our electrode



12. The Pd electrodes cut in preparation for determination (Hoffman and Oliver) of the tritium helium therein (Chien, Hodko and Oliver [17]).

would have supported Wolf's claim.[16]

However, the day after the telephone call, Hoffman called to say that, indeed, they had done the analysis again and had "heated properly this time". It then turned out that all the He was He^4, a result of significance because it suggested the presence of the reaction $D + D \rightarrow He^4 + \gamma$ (See Table 1b).

These results also gave an indication of the ratio of tritium in the solution per gram to that in the electrode per gram. It turned out to be about two thousand. The tritium and helium results were repeated later by Arata and published as though original.

The finding of He^4 in our electrodes gave only a qualitative support for $D + D \rightarrow H^4$ as the origin of the heat. However, Bush and Milburn Miles [19] made measurements of He^4 in the gas phase (made possible by removing all the D_2 from the gas mixture). They showed a proportionality of He^4 to the heat produced. The He^4 was about half that necessary to explain the heat.

Although the He results of Bush and Miles (first reported in 1991) had eventually more weight than ours, - because they were quantitatively related to the heat, - there was at first suspicion of leakage of He from the air leaking through the glass capsules in which (in the Bush and Miles work) were kept, the effluent from their cells. No such criticism could be leveled at our electrodes examined by Hoffman and Oliver[17] so that the publication of these results in a refereed journal in 1992 made an important contribution to the establishment of D+D fusion to He^4 as the likely nuclear reaction pathway (tritium formation would be a side reaction of historical importance because it gave the first independent evidence for a nuclear process in electrodes in the cold). By this time (1999) the appearance of tritium in small quantities has become so often reported in LENR experiments, and further results retains little interest.

TRANSMUTATION AMONG ELEMENTS OF HIGHER ATOMIC WEIGHT

---

[16]However, although Wolf's hypothesis had been a possibility when the palladium used had been recovered from old jewelry (and just possibly had had an earlier incarnation in a tritium using reactor), it could not be taken seriously for the freshly made Pd from Tanaka or Johnson Matthey, which were the materials now being used. They had never been in contact with tritium.

[17]Can He^4 leak into Pd electrodes stored in air? It does: The electrode after electrolysis contained 10-100 times more He^4 than those before electrolysis. However, substantial quantities of He^4 were in the electrodes before electrolysis.

An electronics technician, Joseph Champion, approached me in 1991 and presented an idea for forming not only tritium and helium but also elements of higher atomic weight in the cold. On the other hand, although Champion had spent his life in laboratories and with invention, he had no higher degree (an electronics technician's certificate only), and this made us read his ideas (which were written up in great detail) with considerable skepticism.

Champion's idea was to bring the energy of the nucleus up to that necessary for fission by injecting energy in a very large number of tiny steps. Thus, certain nuclei have quadrupole moments and the energy associated with these could be overlapped (i.e., absorption would occur) by the application of a combination of electric and magnetic fields of matching frequencies, in a model reminiscent of nuclear magnetic resonance.[18] I could see that there could be a build up of energies to the Mev status needed for fission in this way, but I worried about the kinetics of the decay path, which Champion had not calculated. What was the net build up rate?

Before he came to work in my lab, Champion had built apparatus by means of which he could put onto an electrode wave forms which he had calculated should energize the atoms on the electrode surface and cause a change in their atomic number. He predicted he would have success only for those nuclei with certain quadrupole moments [28].

Champion set up his radiation producing apparatus in my lab (his status a "guest worker") and tried to achieve the promised "electromagnetic nuclear change", but after about six weeks he had to admit that he could not prove that any new nuclei had been formed.[19]

After the failure of Champion's first proposal, he told us about a "detonation method" (later called the impact method). He said that he had used it in some work at the University of Guanijuato in Mexico and that the use of the method had given rise to new atomic species.[20,21] He produced a laboratory notebook, partly in Spanish, which seemed to support his contention. The method used an explosive mixture of potassium nitrate, carbon and sulfur. In this mixture were also some simple compounds, lead and mercury chlorides largely. All was contained in a metal crucible.

A muffled ("woof") type detonation and a momentary flame accompanied by fumes , followed ignition and was contained in the fume hood. The reaction lasted for around five seconds, then the mixture was allowed to remain for 2-3 days before analysis. At its maximum temperature, it seemed (estimated by me with my earlier experience as an optical pyrometer user [20]) to be around 1000°C. The reason for leaving the mixture for 2-3 days, long after it had cooled down, according to Champion was that this was a period needed for the solid state nuclear reactions to take place.

---

[18] Much later, I was privileged to be shown a description of some calculations by the Russian physicist, Y. Kucherov, which related the phonon spectra of D-Pd to the energy of quadrupoles which exist in certain nuclei [26], an idea qualitatively similar to that of Champion, although, of course, worked out at a much higher level.

[19] From time to time we thought that there were very small amounts of gold produced and on one occasion an x-ray photograph of these minute residues did show some lines indicative of gold, but this could not be reproduced in later runs.

[20] In a phone conversation with me, Prof. Garcia of the Chemistry Department of this university did indeed concede that he had carried out analyses of samples provided by Champion. Those which Champion claimed to be after his detonation experiment, showed gold and were radioactive. Those which were alleged to be the same mixtures but before detonation contained no Au and were not radioactive. The Mexican chemist was cautious and said that he had not witnessed the experiment and knew nothing of the history of the samples. I asked Champion for witnesses and he produced an American laboratory assistant type who was extremely unconvincing on the phone.

[21] Much later I talked over the phone to an elderly miner in Arizona. He said the detonation (or impact) method was well known in the mining industry and was brought to the USA by Englehart in 1908. He said he had often used it and that it increased the yield of gold from ores by 2-3 times. I asked him if it was due to transmutation but he did not know the word. His explanation was that the detonation "shook more gold out of the ore." However, our analysis involved a method in which the ore was dissolved in the solution. Analyses of these solutions on mixtures before impact show limitatingly low values of gold, increased ~ 100 times on impact.

Table 2. Analysis of a number of runs of the detonation method suggested by Champion but tested by Bhardwaj and Lin. (The analysis carried out at the South African National Institute of Metallurgy).

Sample	Ru	Rh	Pd	Ag	Ir	Pt	Au	Method
NT-2A	<1	1.8	1.8	436	0.03	-	986	0.3g sample and Na_2O_2 fusion - dilution and ICP-MS
	-	0.03	2.7	-	-	0.18	848 [719]	10g sample - fire assay - Pb collection. Pressure dissolution of prill - ICP-MS
	0.07	1.4	1.1	-	<0.1	0.2	471	1g sample and Na_2O_2 fusion - Dowex 50Wx8 column separation (3x); dilution - ICP-MS
	-	0.13	2.2	-	0.12	-	830 [642] [824]	1g sample and Na_2O_2 fusion - dilution - ICP-MS
NT-2B	<1	.85	0.01	493	0.08	<0.1	15.8	0.3g sample and Na_2O_2 fusion - dilution - ICP-MS
	-	0.02	0.34	-	-	0.03	8.4	10g sample - fire assay - Pb collection. Pressure dissolution of prill - ICP-MS
	<0.1	.78	<0.1	-	<0.1	<0.1	<1	1g sample and Na_2O_2 fusion - Dowex 50Wx8 column separation (3x); dilution - ICP-MS
	-	<0.5	0.34	-	0.09	<0.1	0.5	1g sample and Na_2O_2 fusion - Te/$SnCl_2$ precipitation and ICP-MS

Although the method which had failed had some rationalization, there was precious little for this impact method. Champion had no equations for it and when we challenged him as to what might be happening, he only talked vaguely about phonon frequencies which were generated in the mixture and which could transfer energies to nuclei in a way similar to that which he had tried to do with the applied magnetic and electric fields. (Table 2)(Fig. 12).

One of the things which made us take note of these experiments which Champion was persuading us to do, was that the product showed radioactivity. We measured the number of counts at a function of time, plotting the log of the number versus t (Fig. 13). The first half life came to 18 hours which was near that of of Pt^{197}, which was an intermediate in the process of converting lead to gold.[22]

We set up our team to test out Champion's impact method with two experienced post-doctoral fellows. One was Dr. Guang H. Lin, a physicist in his early 50's who had much experience with nuclear work in Chinese Government Laboratories. The other, Dr. Ramesh Bhardwaj, was an experienced physical chemist in his later 30's who had been working with me on the STM work applied to electrode surfaces. These two post-

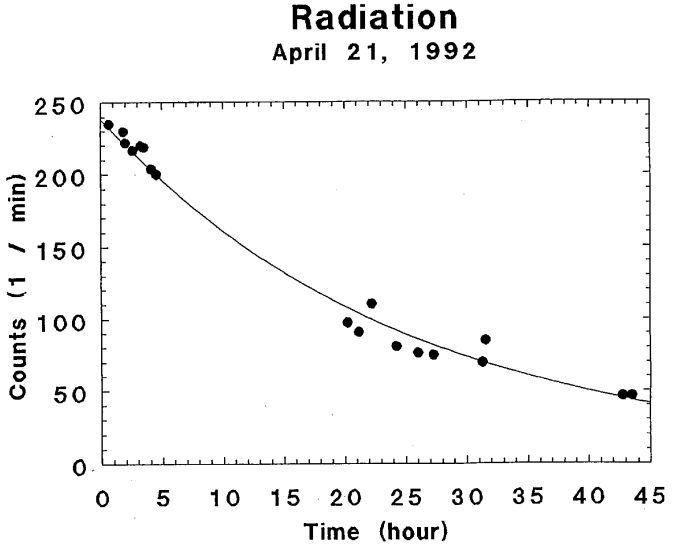
[22]Sometime late in 1991, I talked to the Research Director of a large and well known US precious metals refining company to whom Champion had brought his experiment for testing. At the time of the telephone conference (I called from Auckland, New Zealand), this man was euphoric about Champion's work and greatly appreciated the successfully tested Champion process about which he exclaimed "We could even identify the intermediate isotope Pt^{197} using mass spectroscopy." Some weeks later this man called me up. He was angry and said that I should never have reported what he had said. "Anyway," he said, "the process, of course, does not work at all and is a fraud." It seemed to me that the sudden change in stance might possibly have had a commercial origin. The company he worked for sold gold and precious metals obtained in the normal (expensive) way.

doctoral fellows were assisted from time to time by the graduate student, Zoran Minevski, who was investigating hydrogen evolution on Pd with special reference to the structural changes which went on inside the electrode, as a function of H or D absorption.

An addition to this group was Roberto Monti, who had been writing to me from 1990 advocating the existence of cold transmutation after my earlier tritium work but before Champion had come to me and claimed that he had already established it. When I first received Monti's letters, I thought them to be nonsense and pictured the author as a man of great age, perhaps suffering from illusions brought on by senility. Then I met Monti in Como, Italy, in 1991, and was astonished to find a youngish man of high intelligence and a lively knowledge of nuclear chemistry, rationalizing transmutation in terms of what he called the alpha particle model of the nucleus, a theory which bore some resemblance to that later described by Ron Brightsen. When Bill Telander heard that Monti had independently claimed that transmutation in the cold occurred, he invited Monti to join us in College Station.

Our ever present suspicions that fraud might be occurring in the Champion experiments gave rise to unusual precautions. I myself took the early samples to the mail, to send to the analysts, about more of whom below. We kept the powders from the runs overnight in locked cupboards and drawers. Champion was not allowed in the laboratories while the work was going on. However, Champion was in the building, in fact, he had a room in the corridor in which lay the laboratory and frequently discussed the technique to be used by the postdoctoral fellows, Lin and Bhardwaj. *De facto* he directed the experimental work on the impact method, though I had frequent discussions with him and the experimentalists when the results arrived.

As far as the all-important analyses were concerned, I suspiciously considered the possibility of financial influence being exerted on the Analysis organizations for a favorable result, and therefore I at first used only local analyses. We operated an ICP machine in the Geology Department, or else we submitted our samples for analysis by the neutron activation method available from the reactor on the university campus. Later, we also used a US analyst in Nevada, but I was eager to send the samples outside the country, to colleagues unknown to Champion and his financial backer. I began with some friends in an Australian Government Lab in Melbourne; a commercial firm in Canada which was efficient, and for one experiment, the National Metallurgical Institute in Johannesburg, South Africa. Analyzing ores for noble metals is their specialty here



13. The radioactivity produced after a detonation experiment of common materials including Pb and Hg in a mixture of KNO_3 and C powder. The half life is near to that of Pt^{197} (R. Bhardwaj, G. Lin and J. O'M. Bockris, 1992).

and they got the most out of the analyses.[23]

Analytical results from the various organizations differed by up to ± 50%.[24] However, in spite of this, the meaning of the results was quite clear: except for the possibility of fraud, we were making noble metals from simple metals. The dominant metal we got was gold (up to 300 ppm). We got ruthenium, platinum and rhodium, too, but in the 10 ppm range. It is possible, of course, that other metals were produced but we did not seek them (See Table 2).

Could Champion have interfered fraudulently with the work? One can always invent scenarios whereby fraud seems possible (e.g., the secret return of Champion during the night with a locksmith to salt several noble metals into the powders, to be analyzed, stored in locked cupboards and drawers). However, there are two aspects which to my mind, made it improbable that this was going on.

1. Champion came to us with claims for his electro-magnetic method which he had persuaded his backer, Mr. William Telander to donate $200,000 to Texas A&M University so that an independent examination could be made. When this method failed (even in his own hands), he suffered a loss of face. He risked abandonment by his backer, who was paying his living costs in a good hotel. However, while Champion was working on his alleged electromagnetic method, we left him alone, for we had little confidence that anything noteworthy would occur. He could have salted away anything he wanted. On the other hand, nothing came of analysis of the products from these runs. His credibility with the financial backer was duly damaged.

2. It might well be possible, by some slight of hand, for gold to be inserted in the final mixtures, but how could Champion have inserted relatively large amounts of gold and then also the small amounts of several other noble metals which we found? He was never alone with the equipment in the impact method during experiments. Although my team felt happy about the surprising results from the impact method, the broker-sponsor was unhappy with them. His ambitions had grown with the success of the work which Champion was reporting to him and now he wanted grams and even talked of manufacturing chemically produced gold! Thus, Mr. Telander spoke contemptuously of such "tiny quantities" and told us he would withdraw and go to a commercial lab where it would be done on a "proper scale."

After the successful summer period with Champion in 1992, we in the team at Texas A&M, had to make up time on the projects we had neglected. Thus, the coworkers were being paid 50% on other contracts. So, we did not get back to repeat the runs again until several months later, namely in December, 1992.

Now, we come to the nadir of the story: we could not repeat the transmutational results. We tried about eight runs and in none of these did we get significant quantities of gold. Just one of these runs gave rise to radioactivity again, but, curiously, did not give significant gold.[25] Why?

At the time, most people who knew the facts pointed out contemptuously that, when Champion was no longer present in his office in the corridor, the results were no longer positive. On the other hand, one direct, independent and successful repetition of the impact method has been reported by Cau [21] in Paris, and a Russian group reporting at ICCF7 (1998) that use of the impact method had altered the isotopic distribution of some Cs isotopes. Further, we did observe anomalous radioactivity in one of these December-January runs.

---

[23] The financier who funded the work had by now indicated that his concern extended beyond disinterested inquiry. He appeared to think there might be commercial possibilities!

[24] It is appropriate here to mention the methods of analysis. One was straight chemical analysis, i.e., wet analysis and weighing; another was spectroscopic, i.e., one dissolved in solution and looked for the appropriate line; another was neutron activation; another was coupelling, a metallurgical technique whereby the non-noble metals are extracted from the mixture in a porous matrix and the noble metals remain on top and seem to be visible; and another was ICP-MS (Inductively Coupled Plasma and Mass Spectroscopy).

[25] We analyzed only for gold after Champion left. We also relaxed the analysis simply to methods available at Texas A&M. Hence, 8 runs could be done in 4-5 weeks though it had taken 10 weeks to do 4 runs when we sent out the sample to four independent analysts, and waited for results on up to six metals.

A personal factor may have affected the later unsuccessful results in my own laboratory. It was the attitude of Dr. R. Bhardwaj, who was responsible for most of the later work. Dr. Bhardwaj's character was one of great rectitude and he took exception to the free-wheeling ways of Champion and the sponsor, particularly, when wine was consumed at the table. Gradually, his estimate of the moral character of Telander and Champion declined. Did this unconsciously affect his handling of the later experiments? Could he, perhaps, have been too impatient to have left the material for the requisite three days before analyses?

While we were attempting to react to this frustrating lack of ability to reproduce the values we had signed off on six months earlier, other events began to cloud the horizon and from March, 1993, we were not permitted to continue using the funds.[26]

Although the "Philadelphia Project"[27] at Texas A&M ended in the valley and not at the peak, there is no doubt that it, - and particularly the pioneer suggestions of Joseph Champion, - should be given their rightful place in the history of nuclear science. When Champion came to Texas A&M, the proposition that one could carry out nuclear reactions with metals in solids in the cold ("Alchemy") would have received unhesitating rejection and ridicule. Now, eight years later, not only are scientific papers describing such phenomena available from many groups; but scientific meetings in Russia, formerly entitled Cold Fusion, have been changed to meetings on "Transmutation". The American Nuclear Society has hosted sessions on Low Temperature Nuclear Reactions for three years in succession.

Champion's courage in maintaining that nuclear change in the cold could be brought about could probably not have withstood a formal training in nuclear science. He would then have known that such a suggestion would have been STUPID. A historical analogy seems appropriate. Napoleon Bonaparte had conquered large slices of Europe for the French and eventually went down to defeat by the British Army at Waterloo in Belgium. It is said that before the battle, he told his generals to be careful. The British, he said, were too stupid to realize that they could not win against his triumphant army.

UPS AND DOWNS IN THE CONTRIBUTIONS OF KEVIN WOLF

The contributions to LENR of Prof. Kevin Wolf from Texas A&M University Cyclotron Institute are important because of Wolf's high standing in the nuclear community in the United States. Wolf was well supported by government research agencies and had excellent instrumentation. When EPRI funded work on the new nuclear phenomena, in 1989, Wolf was the first to obtain funding and always had more funding from EPRI than we did in the Chemistry Department.[28]

In the Cyclotron Institute, Wolf set up separate apparatus from that which we had in Chemistry and did not ask for advice about the electrochemistry, although he was eager to supply information about the nuclear techniques about which we asked for his advice. An early triumph of Wolf's was the tritium which he found copiously and published in a paper in 1990 [2] (Fig. 13).

Prof. Wolf worked in the nuclear community of the Cyclotron Institute and this community depended for its funding partly upon grants from governmental agencies in which followed rules which one might say came

[26]The broker was accused of misappropriating the funds which he had promised investors to use in arbitrage operations legal in Swiss banks, and the University was warned by the SEC in California that it should not continue to use the "gift". As of June, 1997, $47,000 of it lay in funds controlled by Texas A&M University, but at that time, unspent.

[27]It was reasonably called this in resonance to the story about a destroyer allegedly levitated in the Philadelphia Navy Yard during secret experiments in WWII.

[28]In the early years, 1989-1992, Wolf's support from EPRI was in the hands of David Worledge who also monitored the support given to my group. However, Worledge gave up this part of his duties. EPRI was divided internally into those who thought the work could lead eventually to a heat source, - and those who thought it must be nonsense because the results were inconsistent with the reigning theory. Later, Wolf's support was monitored by an EPRI manager who explained to me at each of our several meetings that positive results must be fanciful, for they did not fit the theory. When, later, Wolf obtained results confirming Champion's but by a different method, this EPRI sponsor suggested in a letter to Wolf, that there were seven reasons not to report them at the ICCF meeting in Maui.

from the Old Testament. Being a discoverer and indeed a founder of New Nuclear Chemistry (tritium in the cold and transmutation!), Wolf was in a position similar to that of the biblical Daniel in the lion's den.

After a collaboration with journalist Taubes (which led to the article in *Science Magazine* hinting that Nigel Packham's results were fraudulent!), Wolf changed his evaluation about his own publication reporting tritium and announced that he must have made a mistake. It must, he now thought, have been tritium already present as an inclusion in the palladium! Wolf's explanation, though now sounding rather forced, seemed not unreasonable at the time. Thus, the palladium used in the early work at Texas A&M was old material, reconstituted from jewelry, and MIGHT have been exposed to tritium earlier in its history.[29] Thus, some processes in early nuclear plants used diffusion of H and D through Pd to enrich tritium in H-D-T containing mixtures.

When Wolf announced that his original tritium results had been due to tritium impurities hiding in the palladium, there was rejoicing in a *New York Times* article. It was stated that the last shreds of evidence in favor of cold fusion (the tritium evidence from Texas A&M) had been put to rest.

Unfortunately for this thesis, Fritz Will, formerly President of the Electrochemical Society, a man particularly known for his solid integrity, and with a sterling reputation as an experimenter, turned his attention to examine Wolf's idea about tritium impurities already in the palladium. In collaboration with a post doctoral, Cedzynska (22), he examined a large number of samples of Pd (from various manufacturers) and found no tritium in these samples before electrolysis. Will and Cedzynska [22] also gave details about Wolf's colorimetric method for detecting tritium and criticized it as overestimating tritium. They, themselves, confirmed the formation of tritium in the cold using acid solutions.

So, at this point (1991), Wolf had confirmed cold fusion with his tritium work and denied cold fusion with his impurity hypothesis but found that this had been disproved by Will and Cedzynska.

For a time, after this *volte face*, Wolf's position was that of Steve Jones [6]. Thus, Wolf had plenty of evidence for a tiny rate of neutron production in aqueous electrolysis and began to support the position: nuclear reactions do occur in electrolysis but to a very small degree - and only produce neutrons. Heat and tritium, as found by so many, and He^4 as found by a few, must, then, all be due to errors of measurement. This position was 99% consistent with the reigning paradigm in nuclear science and would have been met with another sigh of relief from the large number of scientists in the USA being supported within the huge fusion program.

If all had been left there (in 1992) it might have been said that Kevin Wolf had made some remarkable tritium contributions, withdrawn them, but still had a minimalist position, in the field with Steve Jones. He was not out of it (and still supported by EPRI) but now, it seemed, his new manager was more interested in his finding negative results which would then lead to a reasonable termination of EPRI's interest.

Then, something happened which might, at another time and subject have formed a theme in a Greek Tragedy. For it befell Kevin Wolf that he made, unintentionally, one of the greater discoveries in the field of chemically assisted nuclear activity. Thus, some of his electrodes, electrolyzed for a prolonged period, turned out to have become strongly radioactive. These remarkable electrodes, – *prima faciae* proof of transmutation, – were examined at Los Alamos by Tom Claytor where γ ray spectroscopy identified the new elements and their concentrations (Fig. 14).

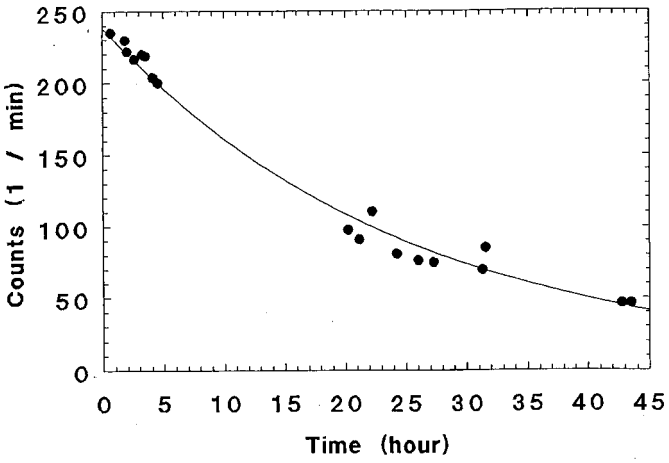
[29]However, detailed inquiry showed that the manufacturer dissolved the old jewelry in solution and potentiostatically separated the Pd (and other materials) by electrodeposition. The deposition potentials of Pd and H are too far apart to make it possible for co-deposition of both.. Had the Pd in the jewelry been exposed to tritium, it would have lost it in dissolution and it would not have re-deposited with Pd.

In view of his minimalist position, his denial of his earlier tritium, his work with the journalist Taubes, this new discovery may understandably not have been a welcome to Wolf. His sponsor hastily advised him not to present the work at meetings.[30]

In fact, Dr. Wolf chose to let the vitally important results lie in EPRI's files. Finally, in 1995 (three years after the work had been done) an EPRI manager, Tom Passel, presented Wolf's transmutational results (which, legally, belonged to EPRI), at ICCF5, and at the first Transmutation meeting at Texas A&M. The matter of the ethics of the non-publication of the early transmutational results was raised in the second meeting on Transmutation in College Station, Texas (1996). Here, Prof. Joseph Natowitz, - Head of the Cyclotron Institute, - was asked publicly why he had allowed such a significant work to remain unpublished. His answer was that the results were not reproducible.

The lack of publication of Wolf's results reminded me of the earlier refusal of the Chemistry Department Head to allow the tritium results to remain in Packham's thesis; the man from the National lab whose boss had told him that only negative results were to be published; and he from the ERAB Committee who threw the tritium-indicating results on the floor while making a hurried exit from the Conference Room.

I thought, again, of Galileo and the Cardinals.



14. Some of Wolf's transmutational results obtained before October, 1992, but not presented publicly until ICCF5, 1995. Gamma ray analysis by Claytor, Los Alamos National laboratory.

CARBON TO IRON

Work attempting a transmutation of carbon to iron was stimulated by R. Monti who described some research on this field which had been carried out in Bologna, Italy. At first this seemed a most unlikely reaction (as Prof. Natowitz stressed to us) and it was not until we heard informally about similar work (with positive results) going on at the Bhabha Atomic Research Institute that we began to take Monti's suggestion seriously.

There were many experimental difficulties which were well taken care of by the experienced electroanalytical chemist, Sundaresan (who came from the Bhabha Institute but had not been associated with the large amount of pioneering work on "Cold Fusion" which had been done there under M. Srinivasan). First of all, one had to get sufficiently pure carbon rods. So called spectroscopically pure carbon was found upon on our analysis to contain several ppm more Fe than stated in the description of the remaining impurities in the "spectroscopically pure" carbon.

The next thing was to realize that the temperature of tiny areas on the end of the rods where the arc was formed would be more than 4,000° and that although the area was tiny it might cause heating along the rod (and hence rapid diffusion of the Fe trace impurities to concentrate at the tip).

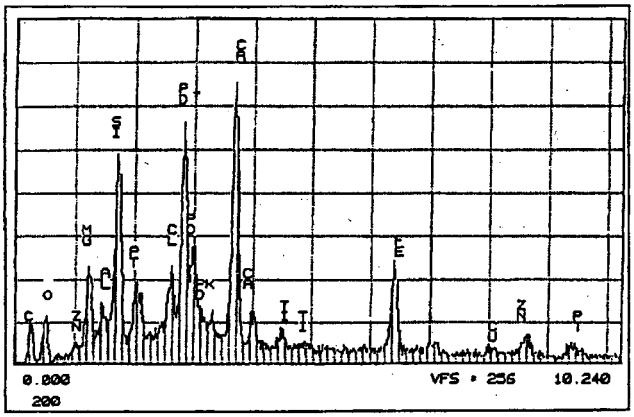
The next thing to look into was <u>electrodiffusion</u> of iron to the tip.

The last thing was to control the oxygen which came into the solution. We found that the absence of oxygen stopped the formation of iron. We feared to use air oxygen because air contains trace quantities of iron. Eventually sufficient results were obtained to make it possible to plot the "new iron" against the total coulombs

[30]But Wolf <u>did</u> present the results privately to a group at the Stanford Research Institute. He told them that the new radioactive materials which electrolysis had caused arose from the impact by some large (hitherto unknown) particles which by chance must have came in from outer space and struck his electrodes during the electrolysis.

passed. Some rather scattered points support the expected trend. The newly formed Fe was 10-100 times more than the total iron present in the original carbon rod [23] (See Fig. 15).

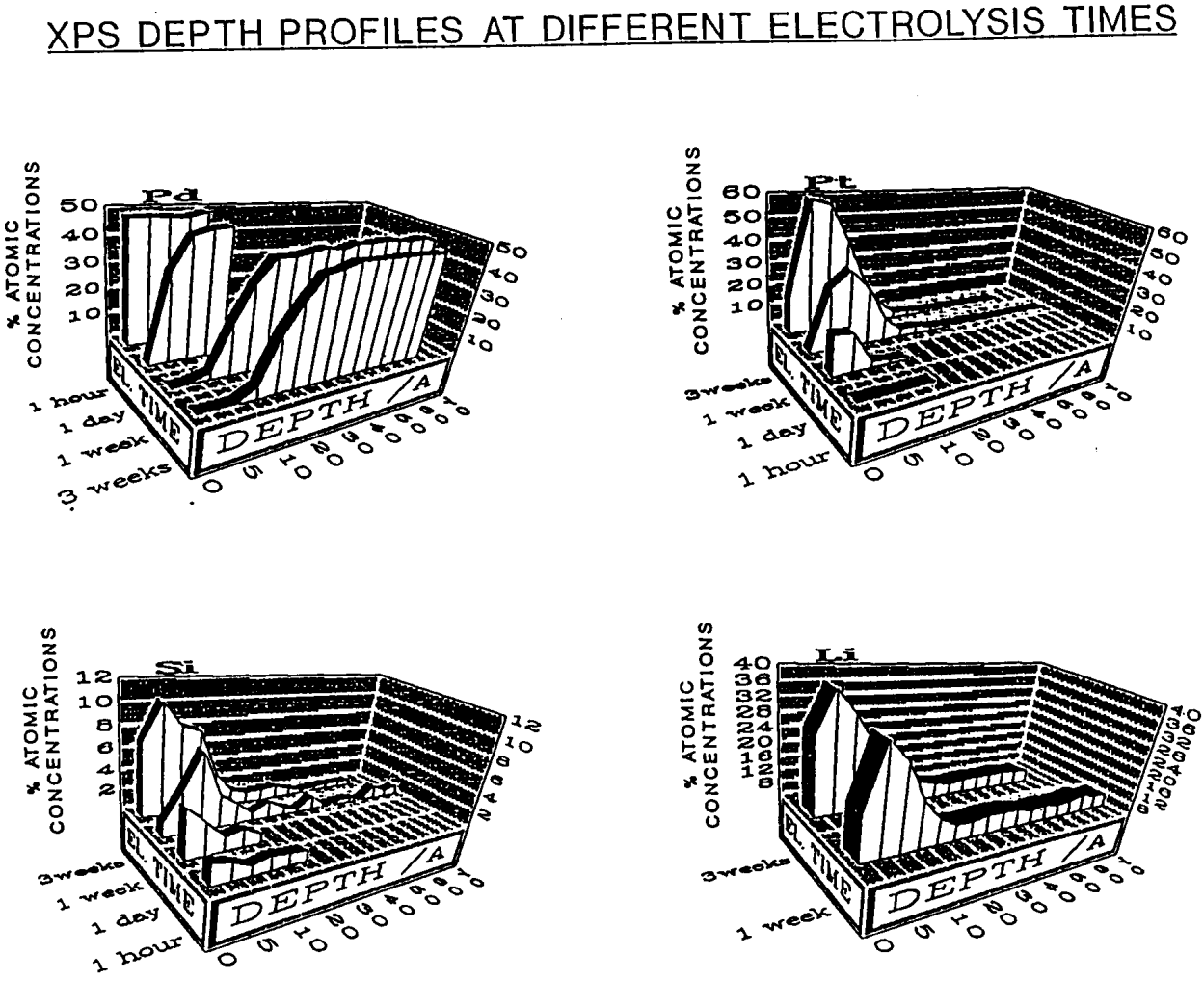
Our interpretation (in which we obtained help from G. Lin) had to take into account oxygen which we had shown was necessary if the reaction was to occur. We had, also, to check if was consistent with the heat observed (part of this was due to the I^2R from the arc). All checked fairly well and our net conclusion was that probably the result was to be trusted. (Though no analysis of the isotopic abundance in the new iron was made.)



16. Impurities from solution. The amounts are given as a function of time and depth (work of Minevski, 1994 [40]).

After we had published our work, a remarkable article by Robert Nelson appeared [24] which supported this work (Fig. 16), and gave information on earlier results on transmutation.

DISCOVERY OF NEW MATERIALS IN PALLADIUM AFTER ELECTROLYSIS (1993)



15. Sundaresan's figure.

The concept of damage in materials loaded with hydrogen is well known, and on the mechanism of the hydrogen embrittlement of iron has been studied for a century. The internal pressure of molecular hydrogen in voids depends upon the mechanism by which hydrogen is desorbed from the surface of the electrode [10]. Consequently, impurities in the solution (later adsorbed on the electrode) can affect the surface desorption mechanism and thus the internal fugacity of H_2 in voids within the Pd has been measured. The main absorbents and their depth of penetration into the electrode by means of diffusion had been examined as early as 1991 by my graduate student, Zoran Minevski.

It seemed interesting. therefore, to explore damage within the electrode, its dependence on overpotential, temperature and time of electrolysis. The experiments on this were also carried out by Minevski.

The most interesting part of the Minevski investigation came when a comparison was made between what he had found on the surface of the electrode, - and what he found inside. Thus, his first step was to analyze the solution by ICP and find a certain number of impurities present in it. He also found that the same impurities were present on the surface of the electrode. On the other hand when he used sputtering to remove various layers of the electrode, he also found that he could get a different set of "impurities" inside, not associated with impurities in the solution. These "internal impurities" came as a result of three weeks of electrolysis.

This suggested that the latter impurities arose transmutationally, for they came only after the prolonged electrolysis of Pd in aqueous or deuterated solution. During the time involved, H & D would have permeated through the specimen.

Subsequent to this work, performed in 1994, came the work of many others (e.g., Ohmori, Mizuno, Notoya, Miley, etc.). However, although these studies by nuclear chemists were more sophisticated than those carried out by Minevski in my lab, particularly in the use of isotopic abundance studies to distinguish "new" elements from those which might have come from the solution and which would have the normal isotopic distribution, none of them presented a differential analysis of surface impurities and their correlation with impurities in the solution, i.e., made certain that a different series of new materials was found inside the Pd, not connected with solution impurities.

THEORIES OF IRREPRODUCIBILITY

The major stumbling block to the acceptance of chemically assisted nuclear activity in solids by scientists who have not themselves observed the phenomena, is the irreproducibility of results in the Fleischmann-Pons system in which most of the work has been done. Characteristics of this phenomenon may be summarized thus (See also Storms, Ref. [25]):

1. There is a latency period which (using 1 mm wires) may be from 50 to 500 hours (or even longer) in the electrolysis evolving D_2 before nuclear effects occur. Few experimenters wait for such a long time for new phenomena to show themselves, - phenomena which are contradictory to the classical theory of nuclear chemistry.

2. The phenomena occur in bursts, which last for times in the order of 10 hours. The heat delivered per burst increases for several bursts and then declines. The bursts can be provoked by anodic discharge of the hydrogen in the metal and a new flow of hydrogen inside, i.e., anodic-cathodic pulsing [3]. An electrode which has "finished" its activity can be stimulated to activity again by increasing the overpotential applied to it [17].

3. Within the same rod of palladium some sections are active and some inactive [17].

4. Unpurified solutions work better than clean ones. Additives, e.g., thiourea, increase the ease of observance of nuclear activity [9].

5. Phenomena in thin films are more reproducible than on (e.g., 1 mm radius) wires.

6. There is no switch on of heat unless the D/Pd ratio is 0.9 and the current density > 0.5 amp cm^{-2} [25].

These characteristics can be compared with the predictions of three leading theories of how the excess heat is formed..

1. The phonon resonance theory [26] depends on the presence within the lattice of certain impurity atoms which have quadrupole moments and resonate with the phonon spectrum of Pd-D (cf. Champion's electromagnetic theory). The critical impurities must have a quadrupole moment having a frequency which overlaps with the frequency of the phonon spectrum of the hydride lattice. The lattice energy would thus be "pumped up" until the nucleus undergoes fission. Of course, there are decay rates of the excitation which must also be calculated.

2. The critical factor is damage within the palladium. When damage is too small, there are not enough dislocations and "active sites" to allow the critical nuclear reaction to occur. When the damage is too great, there are channels to the surface which allow D_2 to escape from voids in which it has congregated at high fugacity and D/Pd to sink below 0.9. There is an optimal range of damage in which the conditions needed are present.

3. Free neutrons circulate in the lattice created, speculatively, by proton discharge on which are dendrites and aperites at which electron concentrations are abnormally high. Neutrons do not interact with the coulomb barrier, therefore can freely enter nuclei making new isotopes.

In Table 3, these three theories are compared with some of the characteristics of the irreproducibility phenomena.

Table 3. Concentrations of impurities (Atomic weight percent) found in virgin Pd after three weeks of electrolysis (EDS).

ELEMENT	ELEMENTS AT 1 μm DEPTH (ATOMIC %)		
	VIRGIN Pd		ELECTROLYZED Pd 3 weeks/EDS
	JOHNSON MATTHEY/ICP	PRESENT WORK/EDS	
Mg	$< 1.0 \cdot 10^{-4}$	-	6.7 ± 1.0
Ag	$< 1.0 \cdot 10^{-4}$	-	1.9 ± 1.0
Si	$8.0 \cdot 10^{-4}$	-	10.2 ± 1.0
Cl	-	-	3.0 ± 1.0
K	$9.0 \cdot 10^{-4}$	-	1.1 ± 1.0
Ca	$3.5 \cdot 10^{-3}$	-	19.9 ± 1.0
Ti	$< 3.0 \cdot 10^{-4}$	-	1.6 ± 1.0
Fe	$< 4.0 \cdot 10^{-4}$	-	10.5 ± 1.0
Cu	$4.5 \cdot 10^{-3}$	-	1.9 ± 1.0
Zn	$< 4.0 \cdot 10^{-4}$		4.2 ± 1.0
Pt	$1.0 \cdot 10^{-2}$		7.1 ± 1.0
Pd	99.80	98.10 ± 1.0	31.9 ± 1.0

Table 4

CHARACTERISTICS OF THE D/PD SWITCH ON OF HEAT AND TRITIUM COMPARED WITH THE MODELS OF THE PHENOMENA

Several models for Low Energy Nuclear Reaction contrasted with some phenomology of the heat evolution on Pd-D. The model which seems more consistent with these results than others is one in which internal parts of the electrode are increasingly damaged with time.

Model	**Long Switch on Time**	**Bursts and their character-istics**	**Only parts of samples of Pd active**	**Unpurified solutions better**	**Thin films better**	**Only works if D/Pd > 0.9**
Depends critically upon phonon resonance interaction with impurities having right quadrupole moment.	No	No	Yes	No	Maybe	Yes
Depends critically upon "just enough' damage.	Yes	Yes	Maybe	Yes	Yes	Yes
Depends on production of neutrons at surface	No	No	Perhaps	Yes	No	No

Hence, according to the comparisons, the irreproducibility depends on the internal damage. For example, the internal pressure of D in the neighborhood of dislocations is much greater than in its other areas [13]. With increasing damage, the number of dislocations increases, although when the damage is large enough, the deuterium begins to escape from the surface and reduces fugacity. It has been reported [27] that damage in Pd for a given potential is highly variable with the sample.

The theory fits the long switch on time. These are times necessary for sufficient damage to occur. (This is larger than the time needed to saturate the sample.)

The heat bursts work like this: when a new damaged area is opened up (as the diffusing H penetrates the electrode), it gives more activity because of the increased number of dislocations available for D adsorption, but when these dislocations have had their effect, - the burst is ended and one must wait for more damage revealing fresh dislocations (and point of high H concentration [29]) before the phenomena switches on again.

The fact that parts of a wire are active and not others is consistent with the idea that critical impurities are necessary to encourage damage. Hence, it fits the phonon resonance theory because there, the occasionally found impurities create the phenomenon: they may be heterogeneously situated.

As far as the advantage of the unpurified solutions is concerned, the internal pressure of hydrogen or deuterium within voids of the metal if given by an equation

$$P_D \alpha e^{-x\eta F/RT}$$

where η is the overpotential and is negative for cathodic reactions [40]. The x varies from 0.5 through 2, depending on the mechanism of H desorption which depends on impurities adsorbed on the surface. A small x corresponds to situations in which the recombination desorption reaction on the surface is coupled to the proton discharge. A rate determining desorption, involving discharge of protons onto adsorbed D, means a large θ.

Now, what θ will exist on the surface, - for a given current density, - depends on the impurity adsorption because this affects the desorption mechanism. This may change the rate determining step from that of coupled proton discharge (low x and low p_{D_2}) to one of rate-determining D-desorption. The damage, - and thus the switching on of heat, - therefore depends on the impurities in the solution, their adsorbability and the effect of this on the rate determining reaction and hence on the H concentration within the metal.

Thin films are better than bulk metal. This follows because the right amount of internal damage in the thin film is more quickly obtained.

Lastly, the D/Pd → 0.9 phenomena corresponds to high damage: thus at lower fugacities, the internal pressure is insufficient to cause damage.

Thus, the irreproducibility depends critically upon the development of internal damage within the electrode, the least examined aspect of the phenomena and the one most dependent on trace impurities and alloying in Pd. This would explain why the phenomenon is less observed in highly purified solution for there θ remains low. As the solution impurities are usually uncontrolled, so is the reproducibility.

REACTIONS OF THE ERAB COMMITTEE, THE MEDIA, AND TEXAS A&M ADMINISTRATION TO DISCOVERIES OF CHEMICALLY ASSISTED NUCLEAR REACTIONS

1. The reactions of the media were extreme and entirely negative. It was immediately assumed that the new phenomena were fraudulent! Lurid headlines in the local press were a daily occurrence [30]. Headlines about "alchemy" reached the media worldwide. Newsweek published a description of the metal to metal reactions which made the work sound either idiotic or fraudulent.

2. The reactions among Texas A&M chemistry professors varied within divisions. Some organic faculty showed scholarly interest and approached me for discussion and explanation (though the Distinguished Professors in organic chemistry, both of whom were from Imperial College, London, my alma mater, withdrew all relations from me!). Professors in the Cyclotron group, - the nuclear chemists, - behaved as scientists and offered hesitant and skeptical discussion. The Head of the Cyclotron Institute (Prof. J. Natowitz) took a laid-back and skeptical interest in our results, which he thought must be due to errors of measurement. However, what discussion he gave us was normal and scientific in attitude.

A Dean accused me officially of "Misconduct in Research" but the verdict (from a committee of my peers) was complete exoneration. A professor in the inorganic faculty stimulated a second, much longer, investigation which (a press report alleged) was aimed at unseating me from the university! It ended positively for me after I had lodged a formal complaint concerning my treatment to the American Association of University Professors.

Remarkable, indeed, was the attitude of the Distinguished Professors group to which I had belonged since 1982, and most of whom I had entertained in my home. When the transmutation phenomena were first announced, one of its members gathered signatures from 21 out of 34 of the Distinguished Professors to a manifesto, sent to the Provost asking for my demotion from the Distinguished Professor rank because my coworkers had discovered reactions inconsistent with the reigning paradigm. The word caper was used to describe the tritium work with its world wide confirmation and the idea that one could have transmutation of metals in the cold was described as equivalent to claiming to mine green cheese on the moon. None of those who signed asked to see the publications on these subjects which had been made in refereed journals. None

of the Distinguished Professors called to ask me what was going on.[31] Only Frank Vandiver (formerly President of the University) met with me privately to express regret at having signed the manifesto.

Lastly, it is interesting to comment upon the ERAB Committee [31] which investigated the phenomena in 1990. The members of this committee included a well known electrochemist, Prof. Alan Bard of the University of Texas. The attitude taken by most of the committee members was that a crime had been committed and was being investigated. On occasions when clear positive data were presented, committee members became disturbed and abruptly ended discussion.
It seems reasonable seriously to compare the attitudes present in investigation of this ERAB Committee on cold fusion [31] as similar to those of Inquisitions by the Roman Catholic Church of a heresy from doctrine.

HISTORICAL SIGNIFICANCE OF THE WORK IN CHEMICALLY ASSISTED NUCLEAR ACTIVITY AT TEXAS A&M UNIVERSITY

First of all, it must be admitted that there was an element of luck for Texas A&M in the fact that several research groups there were already working with EPRI upon fuel cell oriented subjects so that when it came to the initial announcements in 1989, these groups could easily be turned (temporarily) to investigate LENR without months for formal proposal writing and budget arguing.
Research papers describing low temperature nuclear activity in solids appeared before those of Fleischmann and Pons, but were ignored. Thus, Fermi et al. [32], electrolyzed deuterated ice in 1937 and found helium and neutrons. Borghi [33] found neutron evolution in clustrons with electric fields of 5,000 volts therein. Keveran [34] concentrated upon biological transmutation in the 1960's, and published, finally, a book on the subject. He was put up for the Nobel Prize. Komaki continued this work in the 1990's.

US government labs demonstrated how nuclear reactions in the cold by the passage of very high currents through wires in the 1970's [35].

Three points summarize the historical significance of the discovery of LENR:
1. The discovery of nuclear reactivity in solids in the cold by means of electrochemical surface reactions may have the same degree of significance to nuclear physics and chemistry as that of Hahn and Meitner, who discovered atomic fission by means of neutron bombardment in 1939. This work was amplified because it was seen to have weapons significance in WWII.

2. The work must give rise to modifications of the theory of the nucleus. However, new physics is probably not needed. Classical nuclear physicists have maintained an agnostic stance for so long because their knowledge of fusion is concerned with reactions in plasma, and they have paid little attention to the effects of a solid lattice upon nuclear activity within it after injection of H and D at high fugacity, or to the effect of free neutrons in the lattice.

Moreover, a *Chemical Review* article by Greiner and Sandalescu [36] already showed pictures which implied that the classical concept of great stability of the atom must be revised for nuclei which are activated. Thus, when there is sufficient activation of a nucleus, there is a change in shape from spherical to ellipsical, the very short range of the strong force is exceeded and the strong repulsion between protons in the nucleus begins to compete with the strong force, and gives rise to nuclear instability.

3. Ron Brightsen [37] should be mentioned here. He has a novel theory which leads to an interpretation of some of the phenomena which have been detailed in this paper. His theory predicts transmutation in the cold.

4. Are there potential engineering consequences of LENR? Rutherford said there would be none from his pioneering work on nuclear reactions. Thus, it would be unwise to suggest that LENR has only academic interests. If the irreproducibility can be explained and conquered; if the main product of the heat producing reactions are indeed He^4, then the pioneering work done on LENR may provide a nuclear heat source which has less pollutive consequences than that in fission reactors. Thus, c. 2020 will be the time to look at the field

[31]The absence of such a call from of the late D. H. R. Barton (Nobel Laureate) is particularly to be noticed. I had had collegial relations with him at Imperial College, London, as early as 1946.

and see if there is sign of usefulness. All depends on the degree of Government Research Support. Without it, no progress can be made.

REACTION TO NEW IDEAS IN SCIENCE AS EXEMPLIFIED BY THE REACTION TO CHEMICALLY ASSISTED NUCLEAR ACTIVITY

It is a sad (and moreover ridiculous) fact that, in each generation, scientists of the time think that their's is a FINAL TRUTH and fight to prevent its evolution. In this light, the Distinguished Professors at Texas A&M who signed the manifesto, are certainly to be seen less as badly behaving men, counteracting the central tenets of Academic Freedom and Responsibility: but they have good company. But it is sad to conclude that the most fitting comparison which springs to mind is behavior of Church dignitaries in medieval times.

ACKNOWLEDGMENTS

The author recognizes that his role in the work here described has been that of a leader of a band of colleagues, post-doctorals and graduate students, - who did the work. If he has virtue it is that he was brave enough not to be held back by old ideas, but to seize the baton and advance a stage. He would like to acknowledge Nigel Packham for special appreciation in withstanding the defamation to which he was subjected by journalist Taubes; and Joseph Champion, who, though relatively unschooled and scarred by collisions with the law, made a seminal initiative in demonstrating transmutation in the cold before others (in modern times). The wide ranging contributions of physicist Guang Lin must also be recognized. Without him, the two first international conferences on Transmutation (both held in College Station[32]) would not have occurred. Gene Mallove and Hal Fox, as editors and organizers, showed bravery and determination in organizing journals in which new ideas can be published without fear of censorship.

I must also thank those who contributed funds to support the work described here. Thus, William Telander must be thanked: he at least had good intentions; EPRI gave maximal support for the tritium work; ENECO helped the later transmutation experiments.

REFERENCES

1. N. J. C. Packham, K. L. Wolf, J. C. Wass, R. C. Kainthla, J.O'M. Bockris, *J. Electroanalyt. Chem.*, vol 270 (1989), pp 451-458.
2. K. C. Wolf, N. J. C. Packham, D. R. Lawson, J. Shoemaker, F. Cheng, J. C. Wass, International Cold Fusion Conference, vol 1, 1989.
3. D. Hodko, J. O'M. Bockris, *J. Electroanalyt. Chem*., vol 353 (1993), p 33.
4. R. C. Kainthla, O. Velev, L. Kaba, G. H. Lin, N. J. C. Packham, M. Szklarczyk, J. Wass, J. O'M. Bockris, *Electrochimica Acta*, vol 34 (1989), p 1315.
5. M. Fleischmann, S. Pons, M. W. Anderson, O. Leonard, J. Li, M. Hawkins, *J. Electroanal. Chem.,* vol 287 (1990), p 293.
6. J. E. Jones, L. Desmond, S. Jones, D. S. Skelton, J. M. Thorne, *Physical Chemistry*, vol 99 (1995), p 6973.
7. S. Srinivasan, A. J. Appleby, EPRI-NSF Workshop on Anomalous Effects in Deuterated Mtls., Washington, DC, October 1989, p 17-1.
8. J. O'M. Bockris, D. Hodko, Z. Minevski, ICCF2, *The Science of Cold Fusion*, edited by T. Brisani and G. Preparata, published by the Italian Physical Society, 1991.
9. S. Szpak, P. A. Mosier-Boss, *J. Electroanal. Chem.*, vol 302 (1991), p 255.
10. J. O'M. Bockris, P. K. Subramaniam, *Electrochim Acta*, vol 16 (1971), p 2169.
11. J. O'M. Bockris, R. Sundaresan, D. Letts, Z. Minevski, Proc. ICCF4, Maui, Dec. 1993, published by EPRI.

---

[32]The first international meeting on Transmutation was held in the Department of Chemistry at Texas A&M with the agreement of the Department Head. A request for permission to hold a second international meeting there was referred to a Committee of the Department of Chemistry which voted unanimously to ban the meeting (Academic Freedom!). A senior member of this group told me that the opinion of its members was that the topic must refer to a fraud or a hoax. Each committee member had received (before the meeting which voted to ban the conference) an up-to-date review of Cold Fusion by Ed Storms with 459 references.

12. Hal Fox, private communication, 1998.
13. J. Gittus, J. O'M. Bockris, *Nature*, vol 105 (1989), p 339.
14. Gary Taubes, *Science Magazine*, vol 248 (June 15, 1990), pp 1299-1303; Bad Science, Random House, New York, 1991.
15. G. H. Lin, R. C. Kainthla, N. J. C. Packham, O. Velev, J. O'M. Bockris, *Int. J. of Hydrogen Energy*, vol 15 (1990), p 537.
16. C. C. Chien, T. C. Huang, Proc. Anomalous Nuclear Effects in Deuterium/Systems, October 1990, Provo, Utah.
17. C. C. Chien, D. Hodko, Z. Minevski, J. O'M. Bockris, *J. Electroanalyt. Chem.*, vol 338 (1992), p 189.
18. N. Hoffman, A Dialogue on Chemically Induced Nuclear Effects, American Nuclear Society, La Grange Park, IL.
19. R. Bush and M. Miles, "Progress in New Hydrogen Energy," ICCF6 Hokkaido, October 1996. Results on gaseous He^4 firstly presented in 1991 at ICCF2.
20. J. O'M. Bockris, J. D. McKenzie, J. O. White, Technique for Physical Chemical Measurements at High Temperature, Butterworths, London, 1952.
21. M. Cau, private communication. Paris, 1996; reported by A. Mickrowski in the *J. of Planetary Association for Clean Energy,* 1996.
22. F. Will, K. Cedzynska, *Fusion Technology,* vol 22 (1992), p 146. *Fusion Technology*, vol 20 (1991), p 108.
23. J. O'M. Bockris, R. Sundaresan, "Anomalous Reactions during the Arcing between Carbon Rods and Water," *Fusion Technology*, vol 26 (1994), p 261. K. Sensaki, *J. New Energy,* vol 2 (1996), p 53.
24. R. A. Nelson, *J. of Borderland Research* (1998), p 22.
25. E. Storms, *Planetary Association for Clean Energy*, vol 10 (1999), p 11.
26. J. Kucherov, Proc. ICCF7, Vancouver, April 1998, p 85.
27. M. McKubre, Proc. ICCF7, Vancouver, April 1998. Discussion comments.
28. Champion's electromagnetic theory.
29. P. K. Subramaniam, *Comprehensive Treatise of Electrochemistry*, vol 4 (1981), p 411.
30. E.g., *Eagle Newspaper*, Brya-College Station, TX, Dec. 1993.
31. Cold Fusion Research, Energy Research Advisory Board, U.S. D.O.E., Nov. 1989.
32. E. Amaldi, E. Fermi, C. Resetti, *La Richerche Scientifica*, Ser. 2, Anno 8, vol 2, p 1-2, Rome Italy, July 1937.
33. D. C. Borghi, *Nuovo Cimento*, vol 1 (1943), p 1.
34. Y. Keveran, 2, Biological Transmutation, Beckman, NY, 1971; M. Puri, private communication, 20th October, 1997.
35. Cf. P. Graneau, N. Graneau, in: *Einstein Versus Newton*, Carlton, NY, 1994.
36. W. Greiner, A. Sandalescu, *Scientific American,* March, 1990, p 50.
37. Ron Brightsen.
38. J. O'M. Bockris, S. Srinivasan, *Fuel Cells*, McGraw-Hill, 1969.
39. J. O'M. Bockris, Z. Minevski, *Infinite Energy*, vol 5 (1995), p 66.
40. J. O'M. Bockris, Z. Minevski, *Infinite Energy*, vol 5 (1995), p 68. (First evidence published for transmutation after injection of H and D into Pd. However, transmutational results with impact method were obtained in April-September, 1992). The materials observed by EDS at a depth of ~ 1 μ differed radically from those near the surface observed by XPS. The latter corresponded to materials adsorbed from the solution.