SOME HAPPENINGS AT TEXAS A&M UNIVERSITY WHEN UNEXPECTED NEW RESULTS IN RESEARCH ARE OBTAINED

John O'M. Bockris[1]
Molecular Green Technology
4973 Afton Oaks Drive
College Station, TX 77845

7/29/99

GENERAL

Texas A&M is a large university of about 45,000 students and is located in the somewhat remote town of "College Station" which is attached to the old Texas town of Bryan. The University is well known for it's football team "the Aggies" and for its officer Corps. Until after World War II, it was entirely male and dry. Booze and girls arrived in the same year. The University has a great endowment, one of the largest in the country, from land donated to it in the 19th Century, before it was known to contain oil.

There are great plans for Texas A&M University. The ambition is to make it one of the first ten Universities in the country. Some departments are very good now and contain world leaders in their fields. The official major emphasis is on agriculture and engineering, but there are other departments, and one is in chemistry, which ranks among the first ten in the country, as measured by the activity of their graduate schools.

One of the features of Texas A&M, - certainly exemplified with the Chemistry Department, - is to appoint famous professors who are well on in their careers, say in their mid 50's, and who lend instant distinction to the department concerned. In 1997 there were seven distinguished professors in chemistry out of a total of about twenty-eight active (i.e. non-retired) distinguished professors.

As far as the administration is concerned, one feels something of the "military command" structure. In the mid 80's, the President of the University at that time saw a professor in his running shorts, exercising during the lunch hour, and memorialized him to get back to work. Research is not the main thing that is emphasized at Texas A&M, in spite of excellent spots of research, and eminent researchers, here and there. The Board of Regents is much of the trouble because they are appointed mainly because of their wealth and less for their knowledge of

[1] Distinguished Professor of Chemistry, 1982-1997.

academe. Thus, the Chairman of the Board of Regents when I left in 1997 was a wealthy woman farmer who supported the abolishment of tenure.

Before I begin to tell of the various things which happened to me as a result of research discoveries, I would say that I began my career at Texas A&M in 1978 and between then and 1992, when the troubles began, had published around 250 papers from this University. For physical chemists, this is a very good score indeed. I had been very active in getting research grants. In fact, from 1979 until 1991, I ranked first or second in respect to total research funds per year contributed to the department although quite a lot of the funds which I recruited were from private sources largely due to the fact that NSF has no program in my field of physical electrochemistry.

TRITIUM

The Fleischman and Pons announcement of the re-discovery of cold fusion came in March, 1989 and was announced in the McNeil Lehrer hour. I did not hear the actual announcement, but learned about it the day after. Martin Fleischman had been a graduate student when I started as a lecturer at the Imperial College of Science and Technology in London, it was thus easy to phone up Martin and ask him what gives. He told me a few things about the way he prepared his solution and the technique he used to attain the abnormal heat which he thought must be nuclear in origin. I used this in the work which began at once in my group.

At the time, I was supported by a number of sources including particularly EPRI and for a short time, perhaps 4-6 weeks, turned the whole group in the direction of confirming the Fleischman and Pons claim. We tried to observe heat and tritium and in these few weeks, manned a 24-hour a day effort, largely with the assent of our sponsors and not with any formal contracts which would have taken months to work out.

Because Texas A&M contained a thermodynamics group, several electrochemical groups, and a strong nuclear science organization, it was an ideal University in which to place funds from the point of view of a research-funding organization. I encouraged, them to fund several groups at Texas A&M and indeed they funded 3 groups in Chemistry, one in Chemical Engineering and one in the Center for Electrochemical System.

In this account, I shall emphasize aspects which illustrate the reaction of the University to the news that results were being engendered in my group and in others, too, which were entirely inconsistent with the present model in Nuclear Physics. The first act of an unfolding drama was connected with a graduate student called Nigel Packham. He, and several others, had been taking samples of the solutions which had been electrolyzed on palladium to the nuclear engineering department because there it was possible there to have tests made on the solution for the presence of tritium. We thought that it was important to look for this because if the solution consisted of deuterium oxide (following Fleischman and Pons) the most obvious thing to evidence nuclear activity would be tritium formation. We realized also that helium might be produced, but the detection of this was much beyond us at the time and we concentrated on the tritium.

One of the groups that was funded in parallel to my own was one led by Charles Martin, a professor in the electroanalytical chemistry division. His students were very enthusiastic too and when to the same source to test their samples for tritium. I am not sure now how many times and how many solutions Packham and the others took samples to nuclear engineering without any tritium being detected , but sometime in May, 1989, Packham reported that the operating technician said “what have you done with this one?” It contained a large concentration of tritium, in the 1,000 dps (disintegrations per second) range. We were happy about this We had managed to take four samples from the solution at different times and these results showed tritium climbing to an asymptote (i.e. the tritium production stopped after a few hours). It was agreed the people in my group that someone had been present in the laboratory all the time the tritium was “coming”. The activity occurred during the day. However, it took around 400 hours of electrolysis for the electrode to begin to produce hydrogen.[2] We quickly put together a note for publication in the “Journal of Electroanalytical Chemistry” but it was returned twice for revisions, and finally accepted for publication still in 1989. It was the first published account of tritium formation in a refereed journal and the first confirmation of the claims to have observed a nuclear reaction in the cold which were made by Fleischman and Pons in March, 1989.

The fact that we had got tritium (as verified in the meeting in Santa Fe called by DOE in May, 1989), - was encouraging We continued to observe tritium sporadically for the next two years. The total number of publications devoted to reports of tritium formation in the cold was three. One of these contained a comparison of tritium and heat, although this showed that the amount of tritium being produced was too small to account for the heat (later on, it was shown by Miles et al. that helium was also being produced at a rate nearer to that needed for the production of heat). Looking back now, the number of experiments devoted to tritium production was 58 and the total number of times we observed tritium was 18, i.e. many runs gave no tritium, although in retrospect, I wondered if, had we left these cells more than the 500 hours whether perhaps they might have all have given tritium eventually. [3]

During this early stage, a suppressive element was introduced by the journalist Taubes. He came to see us for the first time in the middle of Packham’s time with me and my assumption was that he was a genuine seeker of the truth and I let him see everything we had including notebooks and discussed with him with 100% openness the various plus and minuses of the work. On this occasion, he behaved normally making notes and exposing his Dictaphone to what I was saying, etc.

I later learned that he had visited Texas A&M a second time without seeing me and that he had gone to London to check on what I had said about the background of my student Nigel Packham. He interviewed Nigel’s parents and claimed, quite misleadingly, that Nigel had never been a graduate student at Imperial College (on learning this, we immediately obtained by fax

[2] We had been warned about the long switch-on times by Fleischmann and so we continued electrolyses up to 500 hours (about 3 weeks). When others said at first that they could not reproduce our results, we thought that the most likely reason was that they had not electrolyzed past one day.

[3] The need for some weeks of preliminary electrolysis in certain current density ranges was not realized by most people who tried, and failed, to observe anomalous heat and tritium.

from Imperial College Packham's registration papers for the graduate program in the Electrical Engineering School).

Finally, Taubes made a third visit in which he had a totally different attitude. Now he became extremely aggressive and told me that the result on tritium had been falsified by the student Nigel Packham!!! He said that other workers, particularly those in Charles Martin's group) had not been 3 able to observe tritium so it must be that Packham had falsified his results because he wanted to impress me and get his Ph.D. more quickly.

I remained calm upon this extraordinary attack. One of the things that Taubes said was that I had some part in it because I wanted to increase my funding which (he surmised) would be the result of such a remarkable claim. I showed Taubes the record of all my grants (eleven at the time) and illustrated that I had plenty of funding for research, though, of course, I could always do with some more.

In particular, I told Taubes that he should go and talk to Nigel Packham alone and he would doubtless be able to see the actual notebooks in which Packham had put down his readings when he got the successful results.

Taubes did this. Afterwards, Nigel Packham strode into my room and exclaimed: "this man wants blood".

It seemed that Taubes had threatened Packham after he had talked to him for some time. He had told him that he should "confess" to having put the tritium in the solution from a tritiated water bottle. According to Packham, he said that if Packham confessed to this right now on his Dictaphone, he would not publish anything about it until he wrote a book about the whole myth of cold fusion. Packham would then have 6-9 months to find a job. But if Packham was not willing to confess there and then, he (Taubes) would publish the next morning an article in the New York Times stating that fraud was going on in my laboratory and was being committed by Nigel Packham. In such a case, Packham would have to got as far as Albania to find a job!

This, of course, was an extremely serious threat, - indeed, for an academic, it is difficult to image a worse one. In spite of Taubes attitude, I invited him to lunch at my club and Taubes talked there about his life, writing exposés of wicked scientists in "Discover" magazine, and writing scripts for films in Hollywood. He said that he had written a book attacking a famous Nobel Laureate who worked in Switzerland. He then swiftly departed to get his article in the "New York Times", etc. My attitude at this time was rather shoulder shrugging for I knew what we had done and there seemed to be little that he could do that was truthful, although I understood that he could damage us with false accusations.

No article appeared the next day in the "New York Times", but a little while later, I had a call from London, and the speaker was John Maddox.

Many scientists know Maddox's name because he was for many years (including the year I am talking of, 1990, the Editor of Nature). In a cultured English voice, he quietly said that a paper had been received stating that fraud was being perpetrated in my Laboratory. Had I anything to say? I told him in the same reserved tones that this was not so. The work referred to was being carried out by an English graduate student from Imperial College called Nigel Packham (and also other students and post doctorals) and that although we had been the first to publish a paper on tritium production in 1989, there were now several groups who had found the same.

I asked Maddox in particular if he would forward me the article for my comment and he agreed that he would fax it and that I would get it the next morning.

It isn't pleasant to go home in the evening and think that you are going to be accused of fraud the next day in the most famous scientific publication of all and I did not have a very good night. When I approached my office the next morning, I expected (because London is six hours ahead of Texas) to find the article sprawling out of the fax machine. I opened the door with caution and glanced and then stared at the machine: there was nothing which had come through. I waited until 4:00 p.m. in London and then called Maddox to see what was happening. His secretary said that he was with the lawyers and could not be disturbed.

I called back an hour later and was told that "Dr. Maddox will call you soon". Finally, I did get a call from Maddox and his attitude had changed markedly. The phrase "like a pricked balloon" comes to mind and he said in a resigned voice that "We have put the article on the back burner" because of lawyer's objections to its publication. This was quite a Relief! At the end of this second conversation with Maddox, he said hesitantly: "you say there are others who find tritium." I replied affirmatively, mentioned four names and sent him references and a report.

The next thing that happened was quite a shock. At this time, the groups working on cold fusion at Texas A&M University (one in Chemical Engineering associated with the Thermodynamics Research Center; two groups in the Chemistry Department; a group in the Center for Electrochemical Studies in Hydrogen; and a group in the Nuclear Science Division) met once every two weeks to compare results. At one of these conferences, Professor Kevin Wolf made a startling announcement. Before I get to the subject of his announcement, I would like to tell you a little bit more about Kevin Wolf and his association with us before relating the remarkable things which he said at this meeting.

First of all, Kevin Wolf (who died in 1997) was a well known nuclear chemist and was very well respected. He had plenty of money from the support of his work from the Department of Energy and other sources. As he had been chosen by EPRI to be the recipient of the greatest amount of money they were putting into Texas A&M, he was obviously well respected there, too.

Kevin Wolf had played some part in our work because we were not nuclear chemists and when we began to get tritium (and Tom Schneider at EPRI had given us $27,000 to buy a scintillation apparatus to measure tritium above the EPRI money in our grant), we clearly needed someone who knew nuclear chemistry. One of the suspicions that people had in the early days with tritium was that "it was coming from somewhere else" because the creation of tritium in the cold was regarded as impossible and our results were so unexpected and new that people were insistent upon saying things like "you must have got some impurity in your laboratory" or "there is tritium coming through the ventilation system".

This was not so absurd that it seems because tritium was being used in the department in other ways. It was not out of the question that there could be some tritium, at a very low rate coming through in the ventilation from other labs using tritium.

Kevin Wolf was knowledgeable in relevant techniques and he worked with Nigel Packham and Jeff Wass, another graduate student who was associated with the very early work on cold fusion in my laboratory, really to "take to pieces" the Lab in which the work was being done. For example, testing the curtains and hangings, looking at the floor covering and testing everything they could to find out if the tritium could be coming from elsewhere. In short, Wolf seemed to be very helpful.

Kevin Wolf (who worked outside the Chemistry Department) used to walk down our corridor every day (he said it was to collect his mail). He often talked with Nigel Packham and Jeff Wass, - knew their work intimately, - as we shall later see.

All this is the preliminary to the stunning announcement which Wolf made in one of the joint meetings which we all attended, - namely that an article was to be published in "Science" two weeks hence, and this article would be on our work on tritium!!! He said it would be a lengthy article written by Taubes and would contain the essence of all that Taubes had found out. The major point of the article would be to remonstrate with the administration of Texas A&M for the mistake they had made in allowing us to go ahead with fraudulent and ridiculous work in saying that we had got tritium, for it was well known that this could only be done in a nuclear reaction.

This was, indeed, a Shock for two reasons. The first was for the fact of an academic joining with a journalist secretly to bring odium upon the colleagues whom visited every day without telling us at all of what now appeared to have been the true sin of the visits he made to us.

The second reason was that Wolf himself had detected tritium in high concentrations and had presented a paper at the first International Conference on Cold Fusion in which he had claimed to have produced tritium. [4]

---

[4] Wolf later withdrew his results of 1990. He said they must have been due to impurities of tritium which existed in the palladium before electrolysis. Thus, the early results which we published on tritium had been got with palladium which had been recovered from jewelry and other sources: it was old. We bought this palladium because it was cheaper! At an earlier stage in nuclear laboratories, palladium had been used as a filter for the purification of deuterium and it was thought that the tritium present naturally in deuterium had perhaps settled at special points in the palladium and that is what we have been seeing.

Later on, this work was entirely shot down by Fritz Will and Cedynska. They did two things to destroy Kevin Wolf's attempt to explain our tritium as an artifact. On the one hand, they examined a large number of pieces of palladium which had not been subject to electrolysis and found that none of them contained any tritium at all. On the other hand, they examined Kevin Wolf's method of analyzing tritium in the palladium and they found faults in it. He had relied on a colorimetric method and not distilled the material firstly. This is an interesting example of an entirely erroneous claim giving rise to great effect. Thus, the New York Time Science correspondent promptly published an article concerning Wolf's "discovery" (i.e. tritium reported by us was spurious). As with the Science Article by Taubes, there was no retraction later when plenty of reports of tritium formation came through, and no one thought to explain that there was no basis to the attack by Wolf.

At the same time that Wolf announced that there would be this article in "Science" magazine by Taubes denouncing us and the whole College of Science, he sent me copies of letters which he had apparently been secretly writing both to David Worledge, who was the program manager at EPRI in charge of our work, telling Worledge that our work was fraudulent. He had some reasoning for this which revealed further duplicity.

Being rather familiar with our laboratory (the daily visits), Wolf had no difficulty in removing surreptitiously a test tube of a solution in which we had found tritium. Analyzing this in his own lab, Wolf had found some water in the deuterium oxide solution and this seemed to him to be a good piece of support for the idea that Nigel Packham had put into the solution a significant amount of tritiated water, - and this (rather than tritium from tritium in a nuclear processing plant) was now to be the origin of the tritium.

At the same time, Wolf revealed that he had been secretly writing to the Dean too, and telling him that our work was fraudulent because of the tritiated water which allegedly Packham had added to the solution.

Of course, this grew more serious by the day. Now we were to be attacked publicly by the most read magazine of Science in the United States. It was strongly hinted there that we had fabricated our results and this was being played out in front of the Dean of Science. Presumably, this would go further up the University administration, and become famous because of the publication in "Science".

Because Kevin Wolf had announced the Taubes Science article only just before its publication, we could scarcely do anything about it. We had no time to write a letter to "Science" which would give them information which might have prevented their publication, or at least greatly modified it. I went to see Dean Fackler about this and then learned to my amazement that he had known all about it for some weeks and that Taubes had been talking to him on the telephone. The Vice Dean, Abe Clearfield, also knew of the oncoming article in "Science". Although Clearfield was a colleague of mine in the Department of Chemistry, and I knew John Fackler also collegially, neither of them had thought it necessary to inform me of the oncoming disastrous article. When it finally arrived, it was a very long Feature Article, but it can be summarized quite easily. It said, in effect, "ridiculous work was being carried out in the Chemistry Department and should never have been allowed to go on. The University administration is at fault in allowing this."

The "Science" article was careful not actually to say the work was fraudulent, but much was said in the article as to hint in that direction. In particular, there was a framed inset in which Kevin Wolf was quoted as saying that our work had been sloppy anyway and poorly carried out. What does one do in such circumstances? The article was extremely damaging to me. Its implications were only too clear. Thus, at this time, no one believed that it was possible to produce tritium from deuterium in the cold and my co-workers and I had been claiming to be able to do so, saying that we have replicated the results from time to time and that we had many examples of solutions in which we have found tritium after electrolysis. On the other hand, the article in "Science" excoriated the people at the administration at Texas A&M for weak control.

They should have stopped the work as the so-called discovery was obviously impossible. What were they doing? Going to sleep at the wheel?

My first reaction was to turn to the law. I thought it might be possible to sue “Science” magazine for defamation. I took council of many people, a total of seven. Of the seven, only one advised that I sue. On the other hand, he was a Professor of Law at Temple University in Philadelphia and he had some reasoning whereby he thought that I would easily win the case. It concerned some recent precedent which had just been set up and he thought that this applied to my case. All the other people I consulted, including the man at National Science Foundation who looked into accusations of fraudulent science, advised me not to sue on the ground that the publishers of the article clearly could afford a million dollars in a lawsuit and were well equipped with lawyers anyway: I, on the other hand, would be strained to pay one hundred thousand dollars, the minimum I would have to spend on a lawsuit with one lawyer. It was an impossible situation for me financially, and I should not sue. [5]

I thought that, after the “Science” article had been published, that the best thing to do would be to give a Reply, with all the science I could bring to bear, and meet the statements made by Taubes statement by statement. I called the Editor at “Science” magazine and told him that the article was false in its implication. He was rather cold on the phone and said that he was sorry... I then wrote to Science Magazine and asked if I could have the same space that Taubes had for a reply. They said “No, a detailed reply would not be accepted for publication”. A surprising reason was given as: “The public is interested in fraud, but they are less interested in normal science”. Eventually, I was allowed to publish a one column letter in which I stated the plain facts of our discoveries and that the implication that there was anything experimentally wrong with them was not the case.

At this time, I could not bring the major defense, which was replication by others. It was true that there were already a few confirmation papers on tritium, but I had to wait until 1994 to gather 147 papers which at that time reported tritium productivity from deuterium in the cold. I thought this was enough and gave up counting them.

Several of the people I talked to during the time I had phoned around to ask what I could do about the Taubes article said that all that I should be worried about was my scientific reputation, and this would depend on replication, suit or no suit. People only wanted to know whether I had committed fraud or not. This would be proved by replication of others. This sounded good, but in fact an accusation of fraud spreads like wild fire. When I had collected references to 147 papers reporting the formation of tritium it was too late. By 1994, everyone had concluded I had committed a Fraud. So, the mud slung sticks and cleaning it off is very difficult indeed.

[5] In this and the succeeding case about transmutation, I have learned much. What I have learned applies to all academics and indeed to anybody who is defamed in the press. Unless one is very wealthy (able to spend a few hundred thousand dollars with lease) the person libeled has no protection from the law.

He cannot defend himself for financial reasons. We shall see later that the availability of contingency arrangements with lawyers is an uncertain one, too.

A most interesting story in all this, very damning to "Science" magazine, was the experience of Ed Storms who already had got his own successful results in tritium replication. Independent of any discussion with me, Storms devised a Test which would clearly show whether the results were fraudulent or not. If the tritium had been put in by Nigel Packham, it would be present in an ionic or molecular form and would stay there independent of time. If the results were produced as gaseous DT at the electrode, the absorbed gas would be sparged out by the constantly bubbled D_2 and the tritium concentration would decline with time.

In fact, Storms devised a graph in which he showed the two behaviors and pointed out in his letter to "Science" that the actual results constituted a clear proof that the results which I had obtained (and which indeed did decrease with time) if one continued to bubble D_2 through the solution, indicated that the tritium found in the solution had been formed on the electrode as a gas, partly dissolved in the solution and partly rising into the gas phase.

Much to their discredit, having now got the proof that no fraud had been committed, "Science" magazine refused to publish Storm's article. [6]

One last aspect of the tritium work must be recorded here for in some ways it is the most significant part of it. By 1992 I had observed tritium many times. Now I had been working with an individual called Chien from Seoul, Korea who had himself, independently of my work, observed tritium before he came to work with me.

Therefore, working with Chien was helpful and we obtained on one occasion a very remarkable electrode which went on emitting tritium for several weeks.

After the electrode had emitted tritium for ten days, I thought that it was reliable enough so that I could call colleagues to see it for themselves. The rate of increase of tritium in the solution was such that if one stayed around for an hour, one could make a measurement oneself and then an hour later, detect a significant increase in the tritium concentration. My idea was that I would say to colleagues, (particularly those in the nuclear science division), "Come see for yourselves, you can do a test". The scintillation apparatus was in the next room and I surmised the colleagues would take two samples and stay with the apparatus for one hour so that they could observe no one was adding tritium artificially during the time the tritium was increasing.

I firstly phoned the Director of the Nuclear Science Division [7] and he said that he was just about to go to Germany to carry out some research there, hence, could not come to see the tritium being formed in the cold. Then I phoned another person in the Chemistry Department who was concerned with trace analysis and part of who's work was nuclear in an analytical sense, - and he said that it was his son's birthday and he could not come. I tried two other colleagues and each had an excuse which prevented their coming to see. I realized that no one would come and see the anomalous result.

---

[6] This is a particularly blatant case of suppression by ridicule and accusations of fraud. Ed Storms is an Authority on tritium having worked for some years in the National Tritium Center at the Los Alamos National Laboratory.
[7] Professor J. Natowstz, the Director, was one of the academics at Texas A&M who normally discussed our work in a reasonable scientific way.

At this point, I could not help thinking that there was some eerie similarity to what happened at Texas A&M in respect to tritium when no one would come and look and what happened in the sixteenth century about Galileo and the telescope. One remembers here that Galileo was the inventor of the telescope. When he turned it on the moon, he found that the moon was a rocky place with lots of mountains.

At this time the Church was very much in control of Everything and their view of the moon was that it was the "queen of heaven and perfect". They in no sense wanted to be told that their queen had imperfections. When they were told that there was much structure to be seen, the Clerics (who were said to include some Cardinals) turned away and said they did not want to see. They refused to look into the telescope. Toutes ca change, toutes c'est la meme chose. Four hundred years had not made a difference. [8]

TRANSMUTATION

The work on tritium continued through that of Chien (published in 1992). Already in 1991 we had received a phone call from a technician named Joe Champion. He said that he had read about our work on tritium and wanted to show us that he could start up a heat giving reaction more quickly than the hundreds of hours which our own technique needed. He said he worked on the campus of the University of Tennessee where he had a trailer containing his apparatus and equipment, and if we could send someone to see his experiment, we would be convinced.

At that time, I had an intelligent and able post doc, Dr. Ramesh Kainthla, and also an able worker who was on the way to his Ph.D. in Sophia, Bulgaria, but had come to work with me: Mr. Omo Velev. I asked them to go together and see Champion's work and they came back saying that he had shown them the apparatus, given some instructions and let them find out for themselves what it could do. They had seen excess heat from the machine within an hour of switching it on and what he had promised came essentially true, only 30% less (in terms of heat) than what he had promised.

This seemed impressive, although we were not told how he did it. Later, in March, 1992, he called again to say that he had now found support money and he would like to come and tell us more about his work.

When he came (a tall and heavy man looking more like a football player than a scientist) he seemed to be very shy and diffident. He spoke with a slight stammer and told us that he had been working for 2 or 3 years on the material that he was to recount, but that he needed some independent verification. He then described work which was effectively a story of transmutation, at that time a matter for ridicule in scientific circles. The essence of his case was that one could calculate what frequencies of electromagnetic radiation one would have to impose upon a material so that it would undergo a nuclear transmutation. His claim was that he worked only

[8] There was an internal investigation of the tritium results by a Committee of three professors, one of whom was Professor Natowitz. I was interviewed by this Committee who must have talked to others, above all, Packham and Wolf. The result was that no fraud was detected. The results, this Committee declared, must have been due to instrumental error!

with materials, the nuclei of which had a quadrupole moment for this brought them into the range of the chemical types of frequencies which his machine produced and therefore the nucleus would absorb energy provided by the magnetic and electric fields his equipment provided and, eventually, if the amount of energy absorbed were big enough, transmutation would occur. He presented us with a number of calculations in a report.

The next thing was that a Mr. William Telander arrived. He gave the impression of a genial relaxed, wealthy Californian. His story was that he had inherited a restaurant chain from his mother. [9]

Telander said that he distrusted the United States as an investment country because the government pried into everything and he had taken the money he had got by sale of his mother's restaurant chain to Europe and had various interests in Belgium, Germany, Russia and China. He had an office in Switzerland.

I phoned the office in Switzerland and it did exist, although I was told on the two occasions that I phoned, that "Mr. Telander is traveling."

Anyway, on the occasion of Telander's visit he offered $100,000 for us to test Champion's claims and I got him in the ensuing conversation to up the offer to $200,000 to spend on whatever we like within the general area of "inorganic reactions". He said that he was intrigued by Champion's claims which Champion said had been verified not only in Tennessee but in some work which he had done at the University of Guanajuato in Mexico.

I phoned the scientist in Mexico with whom Champion said he had collaborated.

Prof. Garcia gave a partial confirmation of what Champion had said. He had not really collaborated with Champion but Champion had brought him samples which had been produced elsewhere. One set of samples was labelled "untreated" and the second, "treated." The "treated" samples did contain some traces of gold and some other noble metals and were radioactive. However, he made the point that he had no idea where these samples had come from and whether the radioactivity was indeed due to some kind of process or had been put there. He seemed negative and hesitant about the whole thing and he made me suspicious.

When Telander finished his presentation, I explained that we had become interested in this kind of work in the course of investigation of the Fleischmann-Pons work (i.e., tritium production) and would like to do it. I told him the best way to fund the work was to approach the Development Foundation of the University and make a <u>gift</u>. The advantage of the gift was that the overhead was only 5%, - a management fee, - whereas if he went via the Research Foundation, the overhead was much higher, 30-40%. There was, of course, a catch: by going to the Research Foundation, he could make a contract to carry out a definite program of research, whereas if he gave the University a gift, the University could determine what they wanted to do with the money. It would be within the contract that the gift would not be used to fund his

[9] I later came across some independent verification of this in talking to a businessman-scientist in Massachusetts. I tried to get him interested in supporting work on transmutation and mentioned Telander's involvement, whereupon he said: "Oh yes, - the restaurant man."

research at all. I pointed out to him, however, that in practice the gift path went well because he could write a n entirely legal letter to the University in which he donated the money, saying that it could be used by the University in whatever way they wanted. Then, there would be a clause which begins: “However, . . . , and he could state that he would like the money to be used in the support of the work of J. O’M. Bockris, etc.

The University would not be likely to use the money in any way except that desired by the donor because they wanted the sponsor to give more money in a second phase. Therefore, it was reasonably safe to give the money in this way and have his research done with 95% (rather than 65%) of the money being used for his purposes (although legally, the University could do what it wanted with the money).

I introduced Telander to the head of the Development Foundation on the second visit and he spoke with this man. I tactfully left them alone for the meeting and was told later that the offer had been duly noted and we would be told later whether it had been accepted by the University or not. I went down to see the Head of my department and told him of the gift coming in and the fact that it was for strange work (which I outlined) but I thought that a general designation of “investigations into inorganic reactions” would be true and could cover it.

The eventual authority who accepted the money was a certain dean, Dean Kemp, and we will hear more about him later. It took the University several weeks to consider Mr. Telander’s offer Although he was traveling around in his private jet and wasn’t much in College Station, Telander did visit us on another occasion and finally sent one of the lawyers with whom he seemed to be in frequent contact to see the people at Texas A&M and ask them whether they were going to accept the gift or not. Finally, the result came through, - they would accept, we had the money.

Our first reaction to Joseph Champion within the laboratory was that he was an oddball type and an inventor. Mr. Telander had sent a large amount of specialized electronic equipment to accompany Champion and this was duly moved into the laboratory. Discussions with Champion showed that he needed an electrochemical cell to couple up to his electronics and we had plenty of those, so we provided him with one and the usual ancillary equipment and he connected up his machine and proceeded to carry out experiments. The machine produced pulses of a band width and frequency which he could control, and put in a beat frequency mode. Champion seemed to have a list of quadrupole moments of certain elements and the characteristics of nuclei and had a large data base in a computer so that for a given nucleus he could find out the details of the nuclear properties and appropriate frequencies which would interact with the quadrupole moments of the target atom.

Then, he set to work with a solution of ions which he said he would transmute. We got the impression that he was trying ideas which he had not earlier examined.

This first period of Champion’s work with us lasted about 6 weeks. We were extremely skeptical that he would get anything out of it and left him entirely alone in the laboratory, - and in fact, we treated him like a post doc (he was registered in the University as a “guest worker”).

During this six weeks there were occasions in which we thought that there had been some success. Some solids did seem to be deposited and were subject to x-ray and other types of analysis. It seemed that there was indeed a hint of gold being produced but it wouldn't repeat and so we gave it up.

In view of what happened later, I think it is important to note that in this period of unsuccessful work, Champion had freedom to cheat if he had wanted to. We had no control over what he did.

At the time, Mr. Telander was paying Champion's living costs in a hotel. Champion was thus risking his likelihood in admitting the failure of his work. He didn't know whether Mr. Telander would dismiss him on the spot and go off elsewhere. In fact, however, he retained Telander's interest by saying that he had used another method to carry out the work which had proved successful at the University of Guanajuato in Mexico. He called the new method the explosion method (I later called it the impact method because I found a Russian group who in 1998 had found nuclear change to occur after they had subjected their samples to explosions).

We went ahead with Champion's impact method as Telander had asked that this could be independently verified and I therefore asked the post docs, Dr. Lin and Dr. Bhardwaj, to work half time on this work. What this meant was that they would work for 3-4 weeks on the Champion work and then go back to their own work for 3-4 weeks, etc.

It would not be appropriate here to describe the impact method in great detail but a rough outline of it is that there were initial starting mixtures designated by Champion which typically contained cheap materials like lead chloride and merucurous chloride together with carbon powder and potassium nitrate. Sometimes extra things were added, such as sulphur and silica, but the carbon and potassium nitrate and the cheap metal chlorides were always there.

The mixture was put in a tin which in fact had earlier contained coffee and this tin was put in a protective crucible, placed within a fume hood, and set alight. This was done with a taper and led to a muffled explosion, produced much fumes (sometimes from a sulphur constituent), which were duly removed by the hood.

I approached the crucible and looked at it just after the explosion several times. Much earlier in my career I had experience with high temperatures optical pyrometry. Based on this experience, it seemed to me that the mixture in the pot just after explosion indicated a temperature which might approach 1,000° and certainly would be more than 800.

According to Champion's instructions, - which came from his earlier work in Mexico, - the crucible then had to be left for 2-3 days before it was to be analyzed. During this time, we applied a Geiger counter to the mixture and there was no doubt about it: radioactivity was present. However, our measurements were done in a crude way. We simply held a Geiger counter at a fixed distance from the crucible and made the count over ~ 24 hrs.

It is important to define the set up. Telander had insisted that one of the offices in the corridor be occupied by Champion or by a secretary. There was also a lawyer who was present sporadically and he was there presumably with the idea that patent claims could be written out whenever a positive result was obtained.

The experiments were lengthy. The carbon had to be ground fine, materials had to be obtained and ground fine and all had to be mixed for three days. The actual impact experiment itself, which occurred with a "woof" sound was a matter of a few minutes and then the cooling for 3 days where nothing much could be done except the radioactivity measured.

An exciting thing happened in the early days of the Bhardwaj-Lin experiments and encouraged us greatly. We made a plot of the Geiger counter readings (numbers per second) as a function of time. We found a log N_1 was proportionate with time.

This is the expected plot one gets in determining the half life of radioactive materials and it was exciting to find that the half life measured corresponded to that for platinum 197. This had been predicted by Champion earlier. He said that platinum 197 was an intermediate in going from a mercury to gold. This seemed interesting though it wasn't clear to me why mercury, - which is element 80 should go back to platinum first is element 78 and then onto gold at element 79, but at any rate we were eager to see something measurable and the result seemed promising.

At this point, - after the first runs had been carried out by Bhardwaj and Lin, we had to analyze the material which Champion claimed would now contain noble metals. I was anxious to do this in such a way that it could not be faked and I didn't want Champion or anyone outside my research group to have any hand in it. [10] In the first instance, I therefore packaged some of the material myself and sent it to four analysts, to some friends of mine in Australia, in the Government Research Organization there; one to a Canadian analytical organization; one to an organization we had identified in Nevada which specialized in analyzing mineral deposits; and one sample we kept in the University and examined it by atomic absorption spectroscopy and an analyses offered by the local nuclear reaction staff.

The results of the first run were disappointing. One had to take into account, of course, that considerable amounts of the material were evaporated in the explosion so that the weight of the initial material in the crucible had to be found out and then the weight after the explosion; and finally, the concentration of any noble metals (analyzed in different ways by the various companies) had to be carried out as a fraction of the mass of material. The results of the first experiment showed a negligible change in, i.e., the experiment did <u>not</u> verify Champion's claims.

Failure of this first experiment, using the method said to have been verified in Mexico, wasn't good for Mr. Champion's credibility. We tried again.

Champion's role in all this was that of an advisor. He talked to Bhardwaj and Lin freely and indeed we had frequent conferences in my office at which detailed discussions of the methods carried out in the experiment took place.

[10] Champion was forbidden any part of the experimental work during the verification period but occupied the Telander office in the corridor and was immediately available for discussion.

We now carried out several experiments successively over the course of April, May and June of 1992, and here remarkable results were observed, which of course, were regarded as being very controversial: for we did indeed find noble metals present as Champion had predicted. There were several aspects of these results and they were as follows:

1. The metals found were gold, ruthenium, rhodium, and platinum. The gold was always dominant and the maximum concentration we found was around 300 ppm. The other materials were much less and in the region of 10 ppm and sometimes less than this but above the error limits of the methods (±1 ppm) and so we counted them. Including the time of return of materials sent for analysis, which generally took about two weeks, each experiment took three to four weeks. The three successful runs occupied the period April, May and June.

2. The analyses by the various analytical organizations were not well in agreement and sometimes there were differences of as much as 50%. However, qualitatively there was no doubt about the fact that in these three experiments using Champion's impact method, we produced noble metals. We had always a before and after concentration measured by the analytical people and it seemed that the basic result: production of 100's of ppm of gold and lesser amounts of other noble metals) was secure.

3. The best analysis, in respect to detail and thoroughness, was that carried out by the National institute of Metallurgy in South Africa. The organization might have been expected to obtain the most reliable result because of the importance at that time of noble metal deposits particularly gold and platinum in the South African economy. The National Institute of Metallurgy in Johannesburg, was used to dealing with such analyses and they gave us two methods of analysis which both worked out to give about the same result.

When Mr. Telander heard all about this, he was not pleased! He was totally unaware of the anomalous nature of the claims we were making. Although he had come to us with the attitude that he was a disinterested wealthy man who would like to find out if there was truth in an unlikely claim, he rapidly became a very interested business man when we reported that noble metals could be produced. In this role, he was dissatisfied: 100 ppm works out to be around 0.01% of the mixture and it would only have satisfied Mr. Telander had we been able to produce actual visible pieces of metal (on some occasions we could see tiny specks of something gold in color which did turn out upon analysis to be gold), but the actual amount of these yellow specks must have been in the milligram range.

By August of 1992 Mr. Telander announced that he did not want to continue work at Texas A&M because of the ridiculously small amounts of noble metals we were obtaining. He would move to a commercial laboratory in Chicago and there the work would be done on a "proper scale". This made no difference to the $200,000 he had given to the University and we were able to continue using it in other researches (see below). In September, 1992, therefore, Champion left our laboratory with a positive feeling. He had come in April, 1992, and he left in September, and although there had been ups and downs, particularly, the failure of the electromagnetic method, his claims had been verified, although the amounts obtained were miniscule. [11]

In spite of the very dubious nature of Champion's testimony, our own results seemed to be sound enough and indeed there has been since then one verification claimed by a French worker called Cau who reported in 1996 that he had replicated Champion's experiment in Paris and obtained gold. There were also the Russian workers who reported at the Vancouver Meeting of the ICCF in 1998 that they had used the "impact method" (i.e., an explosion) and found that they changed the ratio of the isotopes in cesium, i.e., a nuclear change.

RESULTS OF KEVIN WOLF

The transmutation results obtained by Bhardwaj and Lin, upon the instructions given by Champion, were obtained between April and July, 1992. In October, 1992, there was a Cold Fusion meeting in Nagoya, Japan, and at this meeting the rumor was that Kevin Wolf had got some remarkable results which were transmutational. It was said that he had examined a palladium electrode in the usual way, evolving deuterium thereon, and found that the electrode, after several weeks of electrolysis, whereupon the Pd would be saturated with D_2 was radioactive.

Analysis of the radioactivity by gamma ray spectroscopy at Los Alamos (Thomas Claytor) had revealed that there were now many new metals present. This, of course, would be primary evidence for transmutation and indeed because of the radioactivity and directness of the measurement,- as well of the experience of the worker, - perhaps the best of all the evidence for transmutation.

However, the result had been obtained in September, clearly after the first impact method experiments in May had shown new materials obtained from lead and mercury compounds. On returning from the meeting in Japan, I urged my coworkers to get back to more work on the impact method, and pointed out to them that it was necessity to redouble their efforts because of Kevin Wolf's results. They re-began in December, 1992.

---

[11] Mr. Champion's veracity was proved to be low and we quickly became very critical of it because of two areas where we tested him out. His claim to have been successful in the earlier days was to some extent verified by Prof. Garcia at the University of Guanajuato, and by lab notebooks he produced (partly in Spanish). However, when we asked him for further witness of how much he produced, he gave us someone to talk to on the telephone and I got the impression that this was a weak and uneducated technician. Whereas I had found Prof. Garcia convincing, I found the technician Champion offered me most unconvincing "Thus, he said that he had seen actual visible amounts of gold produced. I asked: "How much?" He said, "a small bar." Of course, there was absolutely no evidence for this kind of amount whatsoever.

Correspondingly, Champion stupidly claimed a graduate degree from an American University and this failed to pass certification when I called the University.

However, much to our surprise and chagrin, when we returned to the transmutation work, [12] the amounts of gold found were within the limits of error of the method.

In the Christmas vacation of 1992, about eight runs were carried out by Dr. Ramesh Bhardwaj [13]13, - to try to recover the results we had gotten in the summer, but gained no anomalous noble metals. [14] By the time we got to February, 1993, I was convinced we had to withdraw the support we had given in the summer for the impact method. It did not work in any regular way. I wrote a letter to the lawyer with whom I had been most associated and the dealings with the sponsor, Mr. William Telander, saying that we could not repeat the results.

In any case, shortly after this, the work with the support of Telander had to stop because of alleged irregularities in his funding and we were left to continue our transmutation work with other funds. The main thing we did was to work on the carbon to iron reaction with the help of Dr. Sundaresan, from BARC in Bombay, India, and we were able to obtain a small amount of iron, but the amount was well above the level which corresponded to the tiny amounts of impurity iron remaining in the spectroscopically pure carbon. There was also a dependence on O, - no iron was produced in its absence. In the publication we did a scheme of nuclear transmutation which was consistent with our work and with the heat evolved.

For about seven months, we continued our scientific work on cold fusion reactions, and then quite suddenly a letter appeared in the local newspaper, the Eagle, written by a former student of mine, Dawn Wakefield.

At this time, we had not done any transmutation work involving the impact method (the nearest experiment we did which could be called alchemical) for six months. However, Dr. Wakefield accused us of a heinous crime, doing medieval (i.e., alchemical) work in a State University. The letter was written in an inflammatory tone, as though some moral offense had been committed.

The Dawn Wakefield letter stirred a hornet's nest. The next step was a call from a Joseph Weiss, a reporter from the Dallas Morning News (a newspaper sometimes said to have a tilt

---

[12] What had occurred in the period August - December, 1992, was that Bhardwaj and Lin had returned to their usual work. However, in this time, we sought apparatus for γ ray analysis because Prof. J. Natowitz at the Cyclotron Institute had pointed out to us that some γ ray emission should have accompanied the β emission we had found.

[13] Why had we been able to produce noble metals by the Impact Method in the Summer of '92 but the experiments could not be reproduced by Bhardwaj in December? It is important to note that Bhardwaj himself (a man of great rectitude and high moral character) had ben repelled by certain aspects of behavior (alcoholic consumption; tales of reduction) which he had seen and heard at the table when Telander was in town and invited Bhardwaj and Lin to dine with him and Champion, together with a number of ladies. One has to ask, for example, if the now impatient Bhardwaj had wasted the prescribed three days before analysis.

It must be recalled that this was 1992. I had only Wolf's rumored results as support. Had I known of what I was to find with Minevski, and of the results of Miley at Illinois and Mizuno in Hokkaido, and all the transmutation work which has been published, I might have tried further.

[14] The reason why eight experiments could be done in 4 weeks was that we dropped verificatory outside analysis and simply looked for gold, analyzed in our own lab.

against Texas A&M). Mr. Weiss pointed out that the letter from Dawn Wakefield had made him inquire about things. He knew of Joe Champion and the grant of $200,000 which had been interrupted by actions of the California SEC, etc. He wanted an interview.

I could see trouble coming out of this if it resulted in a hostile article in the Dallas Morning News and I consulted my Department Head who finally obtained a response at the Vice President level that I <u>should</u> give an interview. I planned it for a certain Saturday morning, in order to give us plenty of time.

I met with the journalist and to my surprise, shortly after the meeting began, a Dean Kemp, - whom I had never met before, - entered my office and said that he would like to be present at the interview. It seemed that he had got to hear of the interview via the Press Relations Department of the University, whose representative was also present It later transpired that Dean Kemp did have a personal interest in what kind of a story should be written up in the Dallas Morning News. It was he who had approved the acceptance of the grant from Mr. William Telander, the broker who had given us the funds. Telander had said that it came from his personal funds (those which originated with the sale of a restaurant chain which he had inherited from his mother). Mr. Telander continued to confirm that this was the case, and it may have been that this was true. However, the amount of money Mr. Telander was finally accused of misappropriating was several million dollars (cf. The $200,000 given to Texas A&M). The difference between stealing and misappropriating arises in the situation that Telander had accepted funds from investors to be put to work in Switzerland where arbitrage schemes on currency fluctuations allow above those of other investment schemes. In fact, - as he claimed, - he had gambled on backing a development which, if successful, would bring higher returns. The illegal part was that he did not get his investors' approval for the change of goals. There is also the discrepancy between the amount given to Texas A&M and the amount misappropriated. Telander claimed he had spent millions at other labs following up the results obtained in Summer 1992 at Texas A&M.

I was totally frank with the journalist, Weiss. The interview was recorded on Dictaphones provided by me, one by journalist Weiss, one by a representative of the University Press Relations unit and lastly one belonging to Dean Kemp, so that four recordings were made. The interview lasted several hours. We talked for 2½ hours before lunch and came back after lunch for about another 1½ hours and Mr. Weiss really got a lot to write about, for I had no reservations in telling him everything I knew about the entire business of the funding and the work that we had done, the results we had got, etc. I gave them to him "straight," pointing out that we did not understand the mechanism of the impact method which had produced tiny amounts of noble metals, that the work on my side sprang out of my verified and published work on the deuterium to tritium reaction; i.e., I had wanted to see if the same kind of nuclear reaction in the cold obtained hydrogen isotopes could also be found with elements of higher atomic number; and that the end of the game, had been disappointing because after the three successful experiments which were astonishing and promising, we found we could not repeat the results although some new anomalous radioactivity had again been observed.

Then, shortly after the journalist had been, and got his tape recording, I was astounded to get a letter from Dr. Kennedy, who was the Vice President in charge of research at Texas A&M,

which said that Dean Michael Kemp had accused me of "misconduct in research". It seemed that he read into the interview things which I had never thought of. During the heady time after we had got good results and before we tried to replicate them, Lin and I had been invited by Mr. Telander to go to Mexico City and make a presentation about the work to a group of science journalists there. I was pleased to do this and both Lin and I spoke for perhaps 5 minutes, each, about the work. I pointed out something which has now been verified plentifully in all parts of the world, that if transmutation in the cold were indeed true, then there would have to be a major revision in the theory of nuclear chemistry, according to which (as seen in 1992) nuclear reactions in the cold are impossible.

Dean Kemp read this statement of mine quite differently and thought that I had been down to Mexico as an advocate of the sponsor, Mr. William Telander. Kemp thought that Telander wanted to commercialize the product of our finding. However, as transmutation in the cold was impossible, - thought Kemp, - any statement that it occurs must be fraudulent and hence constitutes a misconduct in research. Telander wished to deceive the Mexicans and sell a process which was clearly a Nonsense. I, a Distinguished Professor at Texas A&M University, was supporting him.

Of course, this accusation was really worrying and it was backed up shortly afterwards by a remarkable document, which I reproduce here, and which came supposedly from the Distinguished Professors group, - about 25 professors. The "Distinguished Professor" is the highest grade of professor at Texas A&M University. All the Distinguished Professors are world famous in their respective fields, and the purpose of the accompanying document (please read) was to say that anyone who was so crazy as to say that one could get tritium from deuterium in the cold, and then even to say that metals transmute to other metals, including gold, must be certainly scientifically idiotic, but perhaps worse, - a Fraud. It was hinted that I was carrying out a fraud for the sake of Money.

A group of four of my peers (thus, Distinguished Professors) was assembled and I met with them in the building of Texas A&M University containing the office of the General Counsel. It was a very formal inquiry. I gathered six of my collaborators, each of whom had had experience in either cold fusion or transmutation (clearly, both phenomena contributing anomalous nuclear reactions in the cold), and I wanted them to be on hand if the matter was raised, - was it true, that we got tritium, did we get newly created nuclei, etc.?

I had asked the assistance of a lawyer as a consequence of the accusation was made of misconduct in research, and after much hesitation on the part of the University, he was allowed to accompany me into the conference room in which this "trial" was held. The Assistant General Counsel, a vigorous and impressive woman lawyer with whom I had had interaction when the Taubes article came out, was present.

At the beginning of the meeting I asked permission to make a presentation of ten minutes and in this I pointed out two things. Firstly, that there had been some breaking of rules on the part of the University. Thus, one of the rules of the University Policies and Procedures Manual is that no one may speak to reporters and give interviews without permission. An article in Newsweek magazine quoted a spokesperson of the University administration saying "that the work on transmutation was embarrassing the University." This statement to the Press seemed to me to be an act outside the rules of the University Policy and Procedures Manua (for it was not sanctioned by me). My wife had investigated other actions of this Assistant General Counsel, too, and told me that there were some points in which it seemed to her that the Counsel had made moves in connection with the case inconsistent with what she had read in the Policy and Procedures Manual of the University.

Then, I went on to give an account, - as far as was possible to an audience not skilled in nuclear chemistry, - of what we had done. I pointed out that we had repeatedly made tritium from deuterium; that this was undoubtedly a nuclear change in the cold, and we had several publications attesting to this in refereed journals. [15] Thus, it had been a reasonable thing to try to do something similar with the higher elements. We had seemed to succeed, but then after a pause of three months, we could not reproduce the results. As far as all questions referring to Mr. Joe Champion (who later turned out to have had an imbroglio with the law at an earlier stage) and Mr. William Telander, who was now under investigation in respect to whether he had permission from his clients to invest 1% of their money in research at Texas A&M, I could only say that I knew nothing of any misproprieties by Champion or Telander while I collaborated with them. It didn't seem to affect the work which had been carried out by Bhardwaj and Lin.

The four distinguished professors who were "trying" me were genial and pleasant and the whole thing went off, in my opinion and that of my lawyer, very well. There was no opportunity to call in any of the six post docs who had carried out the nuclear work with me.

The result came out after a week or so and it was the best possible that we could have imagined: I was given a "complete exoneration" from the charges. The Distinguished Professors who had tried me gave some account of their work. They had examined more than 1,000 pages of documents. They had taken evidence from four or five people (Dr. Wakefield had been asked to give evidence but had refused), and one thing came out which surprised me. They had used voice enhancement techniques to be sure they understood what had been said between me and the journalist.

One of the pieces of evidence which they quoted was that they had recovered a note, hand written by me, from a hotel in New York City. It was a draft of what had presumably been made into a typed letter later, a specific warning from me to this broker, William Telander, saying that he must not in any way use the successful results we had gotten in Summer, 1992, to imply that there might be some commercial value in the work. What was so interesting about this was that I had forgotten writing this note which was written on the stationery of a New York hotel, although I vaguely remembered it later on. How had they found it? But I think I understood something of that for it was clear that my room had been under surveillance for a long time and that various documents had been stolen from it, presumably by entry into the room (which was, of course, always kept locked) at night with a pass key. It was a wonderful case of a player

[15] Again, I could not, at this time, pull on the hundreds of observations of new tritium (produced in the cold) published in journals in the last 10 years.

defending his goal, but kicking the ball through it himself! ("Hoist by his own petard.") Of course, it was a key point in the trial because as the essence of it was that I was supposed to be encouraging the broker in fraudulent activity, pretending to make gold which could be sold, - the fact that they found this note warning against this very thing among my private papers, - made it difficult continue the case against me.

After the trial, I got on once more with my life of research and teaching and we went another four or five months in peace and quiet, just as we had after the end of the work supported by the now dubious Mr. Telander, but unfortunately, we read one day in the newspapers, that a New Inquiry had begun. This must have been somewhere in June, 1994 (the complete exoneration letter was dated January 31, 1994).

The journalist who wrote the new article in the Eagle, the local newspaper, was frank. He implied that the New Inquiry had been set up to see if any "personnel changes" were needed as a result of the Philadelphia Project. [16]

It was, of course, very difficult to see how this could be done after the letter of complete exoneration, but nevertheless, I understood that a big attempt was being made to do it. I further understood that decisions in secret Political Trials are not always made according to the truth discovered but according to the power exerted. I clearly had Enemies (two of whom revealed themselves later).

The new committee was called an Ad Hoc Committee and when my lawyer inquired of the Assistant General Counsel what was the object of the inquiry, he was told only that the University could investigate whom and what it liked. The inquiry went on and on. After some months, I wrote to the Committee pointing out that it was I who knew more about the Project than anyone and that they could shorten their work by inviting me to one of their meetings and plying me with questions, any one of which I was only too eager to answer. My veracity could be later checked.

I was told later by a member of this committee that the primary mover against me was a professor in the Inorganic Division of the Department of Chemistry, and that, at a meeting with the Dean of Science, this professor had pointed out that he had published more than 1000 papers, whereas I had published only 700! Hence, it seemed to have been implied, his view should count more than mine!

There was no reply my letter asking to be plied with questions. Christmas, 1994, was approaching, and, finally, I thought it would be a good thing to approach the Chairman of the Committee, Dr. Kennedy, with whom I had had a fair talking relationship in the past, and ask him what was going on.

I felt on the phone that Dr. Kennedy was reluctant to talk with me but he finally agreed to a meeting, and when we met he told me he couldn't tell me anything! He said that the Committee was doing its work. When I asked him what the result would be, he said he did not know but that

[16] Translated, I took this to mean: "Could they find grounds for firing me?"

he personally was fed up with it all. He then said: "There is a message from the Provost." [17] He has asked me to tell you: "Bockris will not be the only one."

It is difficult to imagine a more chilling message than this within the situation and it obviously meant that they were tending towards firing me, - on what grounds I did not know, for any new charges had been kept secret from me.

I learned later that a principal reason further renewed investigation was that I had obtained results in my research which were clearly impossible and this was causing the University to be ridiculed in the outside world.

At this point I thought it would be a good idea to spend a few thousand dollars more on the lawyer who had helped me in the first Inquiry in which I had been "totally exonerated", and therefore I approached him and we finally agreed that the best thing to do was to take the whole thing to the American Association of University Professors (AAUP).

We wrote 11 pages and described what the University had been doing to me since 1993, and that there had now been two years of virtual persecution, a Trial, the exoneration, then the New Committee, the 11 months of investigation, the refusal to tell me what any charges were, and etc.

The AAUP is a very powerful body within the universities of the United States. It does indeed investigate what it considers to be unjust treatment of professors and it can blackball a University if it finds cause. If a University is blackballed by the AAUP, new faculty of first class quality will be less easy to hire. They want to know why such an august body as the AAUP had blackballed the University concerned. Texas A&M has good reason to be worried about this, for in the 1980's, it had been under a blackball from the AAUP. Perhaps the University didn't want to risk again, a censure. Thus, there had been talk with the Texas representative of the AAUP in which he said that the Association might well send a team of investigators to Texas A&M to find just what in the name of goodness was being done to me.

Anyway, I have no proof that what happened next had anything to do with my letter to the AAUP, but it is interesting, in the light of the chilling message given to me a few months earlier to find that on May 5, 1995, I received a letter from the Acting Provost at that time (an individual called Charles Lee) which said that the 11 month investigation had shown that in no case had I done anything which contravened the Rules and Regulations Manual of the University.

I suppose that this was tantamount to another complete exoneration although the letter was not as warm as that obtained from the first group of professors. I felt it was written regretfully, it seemed to imply that there were no legal grounds on which to convict me, but . . .

One of the most invidious and difficult things to bear in all this horrible business, starting with Dean Kemp's accusation, and not ending (as I shall tell below) with the second exoneration were the social aspects. There were about 65 professors in the very large Chemistry Department

[17] In a University, the President is the head man for relationships with the outside world and the ultimate boss (although there is a chancellor above him), but the faculty looks toward the provost as the boss for faculty and academic affairs.

at Texas A&M and all of them wanted to ignore me for most of the period which covered, about two years. It is true that after the first complete exoneration, two professors came to congratulate me, but I was isolated and indeed my wife felt it more than anybody because she had, of course, a number of faculty wives whom she knew, and found that when she met them in the supermarket, instead of having the usual womanly chat, they turned their backs on her and made off elsewhere.

My wife is a refugee from Hitler and she said that the year she spent in Vienna after the Nazis came, was far less unpleasant and threatening than the isolation and nastiness which she felt in College Station, TX, in 1993-1995.

One would have thought, that now, again, after all that had been done, everything would be alright. But this was not the attitude taken by certain of my colleagues in the Department of Chemistry. It has been suggested that the motivating force for the antipathy was the fear that the discoveries that my colleagues and I had made would be proven and recognized the work. Then, our original contributions would be rated as Discoveries of Great Magnitude, - worthy of that Prize which is the objective of all scientists' dreams. There were at least two professors in Chemistry who thought that they should get this prize and the possibility that it might go instead to a colleague for the much denigrated work seemed an unwelcome thought.

Therefore, having failed in respect to the three [18] official investigations which had been carried out against me, they decided on the only thing they could would be to persuade the head of the department to have me shunned. This, of course, meant that no one was supposed to speak with me.

I didn't understand for a long time this was going on because most of the colleagues had been ignoring me since the Inquiries began in 1993. However, I did notice that whenever I wanted to talk to the Head of the Department, perhaps once every couple of months, he came to my room and did not invite me to come to his. Of course, he was more than 20 years younger than I, but later I realized it was an example of the shunning. He wanted to let no one see that he was talking with me.

My colleagues in the physical chemistry division took no notice of the shunning order which must have gone round unofficially. Thus, in practice it made no effective difference to how I carried out my work. However, it was nevertheless a very considerable act of spite and tended once more to show that at least in the Chemistry Department at Texas A&M University, research results which do not agree with the existing theory, are not to be tolerated.

This was particularly brought out in 1996 when a rather perky young man in the Engineering Department wanted to have a symposium in that department on New Sources of Energy. He was a student bound for the Navy and he was interested in serving in nuclear submarines and eventually, perhaps, become a Captain of one. He had "leader" characteristics, I thought, being reasonably polite but also quite dominating and a trifle arrogant.

[18] Apart from the two described here, there was an Audit of my accounts but this passed with only the comment that I had delayed payment of two bills for three weeks!

In any case, he persuaded the Head of his Department in Engineering (through a Committee of students recommending seminars) to agree to a symposium on New Sources of Energy and invited four Fusion people, of whom I was one, to speak. Of course, the subject he invited me to talk on was Cold Fusion.

Directly the posters appeared saying that this was all to be there was a sharp reaction from certain professors, and they set out to show that it would be impossible for me to speak and, as it would hardly be possible to pick on one speaker (an assault on Academic Freedom), the entire symposium should be cancelled.

I learned from diverse sources that the movement to ban the symposium came from Chemistry but the official version was that "the speakers were not of good quality." The Head of his Department told the Navy student that there would be no lecture theaters available in the Engineering Department for a symposium of this kind and if he tried to hold it in other parts of the University, he would be duly held responsible. He was later duly accused of having illegally used State Property because he communicated with the speakers using an office typewriter. Nevertheless, the Eagle described the matter as though it threw doubt on the student and his symposium.

This student was not to be so easily gunned down and so he went off campus to the local Catholic Church and a certain Father Sis, with whom I had had some exciting theological discussions immediately gave him a hall and the symposium was duly held with the original speakers. [19]

An interesting thing happened in respect to the local newspaper, the Eagle, which had had headlines denigrating my experimental work on transmutation. They sent a photographer to the symposium and presumably, - to be coherent with their former policy, - to ridicule the symposium and show, perhaps, that nobody turned up for it.

However, the photographer went away empty handed, i.e., no photographs because the symposium took place perfectly normally with about 35 people present, including, ironically enough, several members of the Chemistry Department and all was normal. The four lectures were given and discussion duly had, etc. [20] The Eagle fell silent.

So, University Censorship of New Science had been thwarted, but it was yet another example of the fact that Texas A&M University does not want any ground-breaking new research material against the paradigm, in contradiction to material in the books, to be found or presented within the University.

[19] All of whom, except the wind energy speaker, were Ph.D.'s and specialists in various New Energies.
[20] The battle was not over for the Navy student. Accepted for Officer rank while at the University, he found that his acceptance had been withdrawn "on grounds of adverse reports received." I had to write to the relevant Naval Authorities and explain that the student's initiative had poked a hole in a boiling cauldron. I also spoke to the people at the local Navy recruiting office. The student was re-instated.

AN EXCUSE FOR TEXAS A&M?

Since I retired from Texas A&M, several things have happened, one of them which is that I have had time to consider the treatment which was meted out to me at this University. Again, the fact that the so-called cold fusion phenomena has been so much confirmed in various parts of the world (2,000 publications!!!) and that the American Nuclear Society has agreed for the last three years to host sessions on low energy nuclear reactions, all shows that we were right in 1989 with the first scientific measurements of tritium, and again in 1992 with the first published measurements of transmutation among metals. I stopped counting at 174 papers with the tritium confirmation because it seemed there seemed no point in obtaining further confirmation of our pioneering work. Tritium had its day when its finding was primary evidence for nuclear reactions in the cold, but now the barriers to analyzing helium have been overcome and Melvin Miles has shown that helium production is the main product, - and accounts for about ½ the heat, - the production of tritium is no longer of primary importance.

How is it that a University can react so strongly against a Distinguished Professor who obtains new and unprecedented scientific research result? Is not a University the place for this kind of thing? Such fundamental new and disturbing results would never be tolerated in industrial labs. Further, heads of groups in government agencies are not pleased when something unexpected and fundamentally new is discovered because it upsets their plans. Thus, where in the United States, is New Science to be created? Is it not in the universities? Do not the words "academic freedom" mean quintessentially that a man can research on what he likes and publish results according to what he finds? Isn't the fact that he publishes them in refereed journals sufficient for their intended integrity to be confirmed?

All these questions are apparently answered in the negative for Texas A&M University and this is a Tragedy. However, I have tried to look at it from the point of view of the President's office.

First of all, Texas A&M is without doubt, a football school. I mean nothing pejorative in this, but the fact is that when one speaks in Pittsburgh or Boston or Los Angeles about "the Aggies", they are not talking about the Distinguished Professors of the Physics or Chemistry Departments (nor even those in Agriculture), - they are talking about the football team.

One of the higher administration officials at Texas A&M has described to me just how strong the influence of the success of the football team is and what influence it has on the Board of Reagents. First of all, as in other universities, the coach of the teams is reported to receive an income larger than that of the President. The Board of Reagents is the controlling body of the University, and their degree of satisfaction is strongly influenced by the football team. When the Aggies win a game, the donations from rich people to the University increase. But when the Aggies lose, it declines!

The great attention given to football in the University, doesn't help the academic atmosphere.

The second aspect of Texas A&M which has affected what happened to me I think is the militaristic background. By now, only about six percent of the student body are in the officers training corps but it seems that the idea of "command from the top" pervades the atmosphere at Texas A&M and indeed this has come to the fore much more in recent years with a new president who seems to want to have a hand in "controlling" everything and who has caused a decrease in the atmosphere of relaxment on the campus which is necessary to the prosecution of "disinterested inquiry". The recent persecution of a man in the computational department, based upon the fact that he taught some extra mural classes (this led to his eventual firing), is a case in point. [21]

Briefly, the kind of publications which a University of this kind likes are those which confirm the paradigm. Of course, the papers have to be original and have to constitute an advance, for example, as refereed papers published in the Journal of the American Chemical Society. These papers should be a little better than the papers which have been published there before. This will disturb nobody and also not make much difference but it will not scare people and that what leaves everyone smiling and happy.

Texas A&M University, - military history, concentration on football, - should not, however, be criticized too severely for giving in to the requests of the professors who tried to harm me. In spite of all, the final results were favorable to me, the due Academic Process held, although I undoubtedly underwent 2-3 years of totally unjustified persecution. It is worth quoting the situation at Harvard when John Mack published a deep study of what a number of his patients related during hypnosis. The essence of Mack's book, *Abduction*, is to say that the persons concerned passed every test for sanity but claimed they had been abducted and operated on in space vehicles to provide genetic material. Mack was duly investigated, - as I, - but his trials were much shorter (3-4 months) and the result more friendly and encouraging to the goal of basic scientific research, the Establishment of the New.

TWO INTERNATIONAL MEETINGS ON TRANSMUTATIONAL CHEMISTRY

After we had got the results from the work of Sundaresan (carbon to iron) and the results of Minevski (protons plus palladium to numerous new metals within the palladium) we looked around to see if there were others claiming to have results parallel to ours, - mainly results which confirmed the fact that, - indeed, amazingly, - nuclear reactions did occur in the cold within solids. Not only in the Fleischmann-Pons well authenticated case of deuterium and palladium, but also over much wider swathe of systems, evidence began to appear for Low Temperature Nuclear Reactions in Solids. It was Dr. Lin who suggested to me that we should hold an international symposium on these matters and I went to Dr. Emile Schweikert, who is the Head of the Department of Chemistry, and asked him for permission to have the one day symposium held at Texas A&M. He replied, "Of course."

[21] The professor concerned had also "illegally used the University facilities " There are nasty words which can be used to describe activities which, judged away from an atmosphere of fear, would more properly have led to a reprimand.

After much organization, about which the laurels must go largely to Dr. Lin, the symposium was held and it attracted about 85 people, including one from Russia and several from other countries. We had a student interpreter for the Russian who could not speak English.

The symposium went very well and was started off in a rather auspicious way by the EPRI man who spoke about the hidden transmutational results of Kevin Wolf.

Thus, Kevin (who died of a heart attack in 1997) had not wanted to publish the transmutational results he had obtained, and which had definitely established the presence of new radioactive materials in the palladium. After four years, it was decided by Tom Passell, an EPRI manager that this was all too much and as the results belonged legally to EPRI, he concluded that he had every right to bring them out and publish them himself, of course, acknowledging the authorship of Kevin Wolf.

This he did and it was perhaps the high point of the symposium, the opening paper, and certainly set the theme, for this alone seemed to prove that transmutation in the cold did occur in some metallic systems.

The rest of the symposium went well. Tom Ward was present from DOE and he made a speech at the end of the symposium praising it and saying that DOE money might well be available for such efforts "very soon."

However, there was a most untoward incident, which, I fear, confirmed all that we know about Texas A&M University and those who opposed the publication of new material. A professor from the Inorganic Division of the Department of Chemistry, - a small man with a bald head, - approached with two colleagues in the early afternoon. Dr. Ward and another speaker were outside the lecture hall and when the bald man saw what was happening in the lecture theater, he announced in a loud voice that these people were "all gooks." The DOE man took great exception to this and he wrote a letter of protest to the President of the University pointing out that he had come to hear science and wanted to do that, but not to be insulted by an ignorant man who knew nothing about a developing field.

This was immediately taken up by the local newspaper, the Eagle, who talked about ruffled feathers at Texas A&M but seemed to imply, - as it always had during the whole of the publications of my nuclear reaction work there, - that it was something wrong, going on in discussing these new reactions.

I was just about to go to Australia for a three-months period, but managed to get in a comment on the Eagle article on what Prof. Cotton was reported in the article to have said. The essence of my comment to the Eagle (which was not published) was that we were reporting the results of a large number of scientists from various countries and the process of science was to listen and to accept these experimental results, and to see where they led to revisions in the theory.

Another year passed and we got to 1996, and now it was time to consider whether a second symposium should be held. There was enthusiasm for it and so we got on with the organization, sending out requests for speakers, etc. The response was very encouraging and around 100 people registered for the Symposium.

I now approached the Head of the Department again and asked him once more whether we could hold the symposium at Texas A&M University, as in 1995. However, by this time, - as a consequence presumably of the earlier happenings, the Head of the Department had been told (by whom?) that he must submit any such request to a Committee which had been formed during the time between the first and second symposium. The Committee, consisting of about 12 members of the Department of Chemistry, listened to me give a five minute presentation of the symposium and what we would be presenting. I knew, of course, beforehand, that they had heard negative reporting on the subject of the meeting because they learned about it largely through the Eagle newspaper which had regaled them with tales of a gold seeking professor and avoided reporting the object and significance of the work. In order to overcome this I got a review which had just been published by Ed Storms and contained 468 references to work in cold fusion, a substantial number of them in refereed journals. I saw to it that each member of the committee had the review in hand the day before they were to be asked to agree to the symposium. There was, of course, no indication while I was in the room as to what the decision would be. I received a memorandum the next day from the Department Head telling me that the votes had been unanimous in rejecting the symposium. It could not be held in the Chemistry Department. Academic Freedom!

I called one member of the Nuclear Chemistry group in the University and asked him the reason for the unanimous vote against it and he said the following: "They think it is a fraud or a joke."

I think his answer was perfectly true, although he himself (a member of the Committee) had received from me a copy of Nate Hoffman's book which explains the field at a high scientific level. In conventional texts of 1996, it is said that nuclear reactions take place under extreme conditions (e.g., neutron bombardment in nuclear reactors). The Cyclotron Institute itself at Texas A&M was devoted to transmutation, but occurring under extreme conditions in which particles were accelerated to strike other particles and the equipment was valued in the millions of dollars region when what we had been using cost ~ ten thousand dollars. So, it all seemed incredible to the people at the meeting and they voted unanimously that it just could not be and therefore it must be a joke or fraud.

We held the meeting at the local Holiday Inn. It was very successful. Prof. George Miley, who is well respected member of the nuclear community in the United States, - and editor of *Fusion Technology*, - co-chaired the meeting with me and I am glad to say that Prof. Joseph Natowitz attended, - as he had attended the first symposium, also. Prof. Natowitz is the leading nuclear chemist at Texas A&M University and Head of the Cyclotron Institute.

We left plenty of time on the second day of the symposium for free discussion, in fact, two hours of it, and while this discussion went on, I asked Prof. Natowitz publicly, the following question. Had he been Kevin Wolf's boss? He replied in the affirmative. I then asked him why then he had "allowed" the transmutational results of 1992 to remain unpublished for four years. His answer was that they were not reproducible.

At any rate, the two symposia on transmutation at Texas A&M University made a turning point in the attitude of many towards such reactions, and although they are certainly not accepted by the majority of chemists at this time, at least the American Nuclear Society has held for three successive years, sessions in their national meeting on Nuclear Science. For this reason, the continuation of the meetings which took place at Texas A&M would have no more point, - they were meant to start the introduction of the subject into mainstream science and I think they were successful in doing that. With my retirement, it is now George Miley, who "carries the ball" for low temperature nuclear reactions as far as University science is concerned in the United States. He also is the person who has made the connection between the anomalous work which appears in the literature, and nuclear Physics.

Again, one has to criticize Texas A&M. One can take two attitudes, the one saying that "they did not know." But what of the reading of the very low key and scientific presentation of Ed Storms, which they were given to read as preparation for the meeting?

What of the idea of Academic Freedom, of allowing professors to present anything they wish, above all, anything which has already received the sanction of refereed publication?

These are questions which those who look in the further future of Texas A&M University must confront and ask whether the extremely hostile reaction to the pioneering work which has been done on these nuclear reactions in the University will remain a blotch upon the idea of Academic Freedom there for many years to come.

AN AFTERMATH OF THE DISCOVERY OF FORMATION OF TRITIUM IN THE COLD: PACKHAM'S ORAL

It duly came to pass that Nigel Packham, the person who had been the main worker in the initial work of the discovery of tritium in the cold, came to write his Ph.D. thesis. Nigel Packham had been working with me for about two years, on and off, on cold fusion but the subject in which he had started working with me had been entirely different. It was aimed at examining the production of hydrogen from water using bacteria to catalyze the reactions concerned.

Nigel Packham had an earlier background in England partly in biochemistry and therefore he was an ideal worker to use on this sort of topic. Thus, his thesis consisted of two quite different parts, that on the hydrogen production from bacteriological examinations, and that on cold fusion.

I had suspected that Trouble might arise during the Oral of Nigel Packham because of the article in *Science* by Taubes, because of the general feeling in the department that there had been fraud carried out by Packham himself because he was reporting "impossible" Chemistry, and therefore I arranged with the graduate school representative (who is present at all orals for the Ph.D. degree to see that fair play is done) to be ready to remove the Oral from the big lecture theater in which it was being held into my office, if the noise, the potential barracking, or shouting, became too much and disallowed the academic process to be carried out. I had a meeting in my room before the Oral began to discuss procedures, although one member of Packham's committee, Dr. M. Soriaga, did not attend this meeting.

I also had made another precaution to make sure that Packham would have fair play. Two knowledgeable people who had themselves had experience in tritium production in the cold, would be members of Packham's Committee.

Both these individuals were famous electrochemists at the time. The first was Dr. Norman Hackerman, the President Emeritus of Rice University. Dr. Hackerman was not only a well known electrochemist, but he had also been on a trip to Korea and had seen the work being carried out there on tritium production. He had called me when he got back to say that he had seen tritium produced in completely different circumstances from those in which Packham had produced it.

As far as Prof. Yeager is concerned, he was perhaps the most well known Physical Electrochemist in the United States, had been President of the Electrochemical Society, and had many other honors. I knew that he had obtained tritium in cowork with Robert Adzic, although he had chosen not to publish the work, perhaps because he felt that "the atmosphere would not be quite right".

When we got to the lecture theater for the Oral, we found that it was full of people. Usually, these Orals take place in small rooms, and the persons who attend them are just the members of the Committee concerned, namely four people, and the candidate. Legally speaking, there can be other members of the University present and we already knew that it would be likely that this would occur, and that is why we had scheduled the Oral to take place in the large lecture theater.

At the beginning of the Oral, the examiners, Hackerman, Yeager, and two professors in Biochemistry, together with me, the Chairman of the Committee, sat in the front row.

Two other people of note were present, apart from a large number of graduate students, one was the Dean of Science, Dean, John Fackler, of whom we have heard earlier; and the other was Prof. Michael Hall, who was the Head of the Chemistry Department at the time.

Packham began summarizing the whole thesis by talking about his bacteriological work on decomposition of water to give hydrogen. I thought that he was spending a bit too much time on this, - everyone present had come to hear about tritium and not about bacteria decomposing water,- so I interrupted him to ask him to get on with the tritium story, which he did.

When it came to the discussion, I was his Chairman and I had the task of choosing among the many hands held up at question time. Kevin Wolf was present and I favored him because I thought it would be most fair because he had greatly opposed the work, and now was the time to say why. I therefore allowed him a total of eight minutes to question Packham. After Wolf's questioning, I exposed Packham to many other questions and after half an hour was just about to close the discussion when Dr. Soriaga rose to his feet and walked down the aisle with a bunch of papers in his hand. He handed them to Packham and said: "Answer these." Packham stared at the sheaf of papers, each of which contained a question. It was obviously impossible for him to deal with this publicly, - it might have taken a couple of hours. One of the time constraints was that I had to get Hackerman back to Houston by the limousine that I had hired for him, and which was waiting. I went to the Graduate School representative who was sitting in the front row, and said quietly “What now?” He recommended promptly that Packham should be asked to respond to these questions in a written answer which would have to be at the back of his thesis.

I announced the decision and the Oral was then terminated, the big audience left and the Committee remained, including Dr. Hall, who asked if he might be present at the subsequent deliberations. I agreed.

All the members of the Committee were quick to assent upon my questioning that they were satisfied with Packham’s performance and that they thought that his work certainly came up to the standard of the Ph.D. degree. Only one person held out against the work and this was Dr. Soriaga, who said that he could not sign the thesis because the formation of tritium in the cold was impossible.

In a subsequent discussion Dr. Soriaga became rather heated. Dr. Michael Hall then made a seminal suggestion. He thought that if it was undertaken as a part of the acceptance of the thesis, that replies to Soriaga’s many questions should be printed out in the thesis as an Appendix, then would Dr. Soriaga sign? He agreed to do so.

This was the end of the Oral examination and all the people present, except Dr. Hall who was not part of the Committee, signed the official forms which are generally regarded as giving the graduate student his Ph.D. degree.

We went up the stairs to the lecture theater and at the top of the stairs Dr. Hall shook Packham by the hand, and said “Congratulations on your Ph.D.” (Packham was kept outside the lecture theater while the deliberations by the Committee were carried on). He had, of course, become rather anxious because usually these deliberations last ten minutes and ours had lasted more than half an hour.

Now, Hackerman was released to the limousine and I invited Fackler, Hall, and Yeager to come with me to the Plaza Club in Bryan for dinner.

It was a pleasant occasion, redolent with academic emphasis, and a positive one: the academic process had worked satisfactorily. The Oral had been completed in a very controversial area.

Next day, however, to our consternation, everything had changed. One of the conditions which is usually assumed to be sui generis had still to be fulfilled in the awarding of the student a Ph.D. degree. That is the Department Head’s signature. Usually after the Graduate School Committee completes his recommendation, the papers are sent to the Head of the Department and he routinely signs off the thesis and that is that.

Dr. Hall refused to sign off. His handshake to Packham and congratulations had evidently not been meant seriously and he now said that he couldn’t accept the thesis either because it was well known that tritium could not be formed in the cold.

The next few days were a furor of negotiation and discussion and finally the following was worked out, largely, I think, on the suggestion of Kevin Wolf. It was that Packham would rewrite his thesis cutting out all reference to tritium and the thesis had to stand or fall on the basis of the biological work. Packham would be allowed to have an appendix which would consist not of the answers to Soriaga's questions, but containing the papers he had already published (in refereed journals) on the formation of tritium.

This arrangement was agreed to by the biochemical professors who said that the biochemical work that Packham had done was "just enough" for a Ph.D. degree. Now Hall signed the thesis and Packham had his degree.

But it was not quite over because there had been among the audience a woman journalist whom I had met at an earlier meeting of the Society for Scientific Exploration held a year earlier. The Dallas Morning News has been often quoted as a paper in which accounts negative to Texas A&M University sometimes appear.

The journalist's article was in a Sunday edition, and lasted two full, large pages. It described the oral in detail. I was pictured as the "God" professor, suppressing the Discussion and not allowing the poor junior member to speak properly or to ask his questions. Nothing was said about the tension, - in fact, the torture, - put upon a student who has worked for six years on his Ph.D., been congratulated on having it by the Department Head, certified as having it by the Graduate Committee, and having it torn from him in the last moment by the Department Head's overnight change of mind.

One more thing and that is the memorandum which the Department Head sent around to the faculty the next day after the Oral. He promised that no other orals of this type would ever occur He apologized to the junior professor, Dr. Soriaga, whose feelings had been hurt, and said in the note, "You have witnessed the chairmanship of a committee by an autocratic professor. . . . . I sent it to the Smithsonian Museum in Washington, DC, to add to their collection of memorabilia about the Discovery of nuclear reactions in the cold.

Thus, the process of academic freedom at Texas A&M University had been strained, some will say broken, by the suppression of reports on the synthesis of tritium in the cold in Packham's thesis. However, very fortunately, we had been able to publish the work before his Ph.D. degree and so it was out in the public domain and its suppression as a part of Packham's thesis reflected only on Texas A&M University, it did not stop dissemination of news of the Discovery.

WRITTEN BY F. A. COTTON

12-21-93

(See p. 27)

A REQUEST

Professor J. O'M. Bockris' activities since 1989 (the inception of the "cold fusion" embroglio), and particularly recent allegations that he lent his name and that of our university to a fraudulent scheme to promote a bogus engineering enterprise, has brought this university into disrepute. Note that on page 6 of the "Policies and Procedures Regarding Distinguished Professor Appointments" (September, 1993) it is stated that "The Distinguished Professors bring honor and recognition to the University ….." Instead, we believe that Bockris' recent activities has made the terms Texas A&M" and "Aggie" objects of derisive laughter throughout the world among scientists and engineers, not to mention a large segment of the lay public. The "Alchemy" caper is, everywhere, a sure trigger for sniggering at our university. And so it should be. For a trained scientist to claim, or support anyone else's claim, to have transmuted elements is difficult for us to believe and is no more acceptable than to claim to have invented a gravity shield, revived the dead or to be mining green cheese on the moon. We believe it is sheer nonsense, and, in our opinion, could not have been done innocently by one with a lifetime of experience in one of the physical sciences.

In view of the above considerations, we the undersigned Distinguished Professors of Texas A&M University hereby request the Provost to take steps to revoke the title of Distinguished Professor now carried by John O'M. Bockris. We do this because of our belief that Dr. Bockris' alleged disregard of the accepted standards of scholarly and professional behavior has brought great embarrassment upon this university and his colleagues. In our opinion he no longer merits the title of Distinguished Professor.