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# **The History Of The Discovery Of Transmutation At Texas A&M University**

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## **BACKGROUND**

Until 1989 I had been a publisher of high temperature physical chemistry, electrochemical and environmental research papers. I was a physical chemist and my contact with nuclear chemistry was only in using it in some tracer techniques.

The Fleischmann and Pons announcement of March 1989 was of interest partly because of its radical nature, but also because I had known Martin Fleischmann since his days as a student at the Imperial College of Science and Technology in London.

I had easy access to Fleischmann and I therefore could instruct my co-workers (about 20 at the time) about the technique used in the Fleischmann and Pons work. It was around three weeks before we were able to detect strong concentrations of tritium in the solution after prolonged electrolysis and thus prove, (1) for the first time, that the speculation which Fleischmann and Pons had made about their excess heat was indeed correct and that a nuclear reaction was occurring at or in an electrode in the cold.

We continued to work on the new nuclear phenomena, in 1991 discovering excess He<sup>4</sup> in our palladium cathodes.

Later in 1991 I received a phone call from a Joseph Champion. He told me that the long initiation times which I had recorded in my papers could be avoided and that he could “turn on”

what he described as “a radioactive gas” in less than one hour. Champion invited me to visit his laboratory (which was in a trailer on the grounds of the University of Tennessee). I could operate his apparatus and see for myself.

I asked Dr. Ramesh Kainthla and Mr. Omo Velez, senior researchers in my laboratory, to visit Champion and see if his statements could be confirmed. Upon return to Texas A&M, they told me that Champion had left them alone in the so-called laboratory, pointed to the apparatus, gave them a few instructions about turning it on, and left them alone. They measured 40 percent of the excess heat that he had said was obtainable.

Earlier (1990) I had received a letter from a Roberto Monti. He complained that he had not got a reply from writing to Fleischmann and Pons and he decided to write to me instead. His letter concerned a theory which he said would easily rationalize the synthesis of tritium from deuterium and moreover indicate conditions under which many elements in the Periodic Table could be transmuted to neighboring elements in the cold.

On reading the letter, I thought that the writer must be elderly and out of touch with theoretical chemistry, for what he said was clearly impossible, indeed, it sounded like a claim to alchemy. I replied, humoring the writer and promised to meet with him in the forthcoming Cold Fusion meeting in Como in Italy (1991). When I met Monti I was astonished to find that he seemed to be a bright normal person about 40 years old and speaking in a vigorous and seemingly informed manner. He had an established position in an Italian Research Institute in Bologna. His emphasis was not on the reactions which Fleischmann and Pons had carried out but on his own work where he claimed that he was able to carry out transmutational reactions. In spite of my impression that Monti was a normal scientist, I still regarded transmutation with extreme scepticism.

## **THE FUNDING OF TRANSMUTATION WORK**

### **AT TEXAS A&M UNIVERSITY**

The next step was initiated by another phone call from Joseph Champion and this time he said that he had been looking for money so that he could bring his work to the University. His work, - which he now claimed was metal-metal transmutation similar to Monti's (entirely independent) claim was at a stage whereby it needed independent confirmation. He had been working at a Mexican university for some years. He named a professor there with whom he had been collaborating.

He had been to Merrill Lynch and asked if they knew of a client who had money to invest in a speculative venture and thus obtained the name of Mr. William Telander. I invited Champion to come to Texas A&M University and describe the work he had been doing.

Champion turned out to be a big chap and looked more like a football player than a scientist. He had owned a laboratory in Houston, Texas, testing and repairing electronic equipment. He had a degree in electronics and no qualifications in chemistry or physics. He also told of experiments he had done relevant to a Hydrogen Economy. He had ideas about a process for the desalinization of sea water.

Champion said that he had come across a method by which transmutation could be carried out. The method originated from a person called Keller who had lived in a small town in Washington State. Keller had worked after WWII with two colleagues and found he could make gold in ounce quantities. Troy Becker, one of Keller's colleagues, had been imprisoned on the allegation of making a false deposition. On release, he again had set up a laboratory to re-establish his work. However, a visit from the FBI told him he was not to research the production of gold, - if he did he would again be imprisoned.

Champion had taken a description of the process given him by Keller to the University of Gjanamantu in Mexico because producing gold in the United States was apparently unlawful. He had had some success. I asked and obtained his laboratory books. They were difficult to decipher being partly in Spanish. I called the Professor at the Mexican University. He seemed reluctant to

describe his collaboration with Champion. The experiments had been done elsewhere and Champion had come to him only for the analysis. He had received powders with “before” and “after” written on the containing bottles. He certainly had found noble metals in the “after” powders and none in the “before.” He was skeptical, - he had no evidence that the change was due to some process of Champion’s.

Champion said, – in this first interview in my room at Texas A&M – that he had other ideas about how to bring about transmutation. It was not necessary to carry out the “thermal method” (later on called the explosion or impact method) which originated in Keller’s laboratory. He then produced a folder which contained a minor thesis which he asked me to read. It was supposed to be the theoretical basis to an alternate, original method. But this was ideas only and had never been tried out.

I studied the document. It contained much mathematical detail. However, I did not understand where Champion was coming from. He relied on the fact that certain nuclei had quadrupole moments with frequencies in the range of chemical frequencies ( $\sim 10^{14}$  c.p.s.) By subjecting such material to radiation in the range of the quadrupole frequencies, he thought he would be able to obtain transmuted material, new species. Thus, the incident frequencies from instruments producing fields having frequencies overlapping those in the nucleus would cause the nuclei to absorb energy and this would build up to amounts which would cause nuclear fission to occur.<sup>1</sup>

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<sup>1</sup> Much later, around 1995, I came into contact with the Russian nuclear physicist, Kucherov, working at ENECO, a company in Salt Lake City which had originally been formed to continue the work of Fleischmann and Pons and in which I had brought shares at the foundation.. He had published on nuclear reactions in the cold before he left Russia and now proposed (indeed) to carry out transmutational reactions by a method which seemed to me to be remarkably similar to Champion’s. Of course, Kucherov had made a more detailed theory than that which Champion showed me. He sought to obtain the activating energy not from instruments producing electromagnetic fields (as Champion) but from the frequencies of hydrides which existed after saturation of Pd and other metals with H or D. I heard Kucherov proposing this at the Cold Fusion Meeting in Vancouver but I do not know of any realization of the work at ENECO (though I know it was tried out).

The meeting with Champion was then followed after some weeks by the arrival of Mr. William Telander, he who had shown interest in having Champion's proposals tested out. He seemed to be a genial person, self-confident and relaxed. He lived in the Napa Valley in California and had inherited from his mother a chain of restaurants which he had sold.<sup>2</sup> This was the source of his wealth which he had invested mainly in Europe.

Telander stated that he had an office in Zurich in Switzerland and I asked for its telephone number (I called it several times but was told that Telander was "on travel").

I explained to Telander the University system in respect to gifts. The donor had to assert that the gift was a free gift which Texas A&M could spend in any way it wanted. However, it was legal for the donor to state a preference as to how the money would be spent. The University, of course, respected the wishes of the donors in the hope that more support would come.

Eventually \$100,000 came out of the conversation and as Mr. Telander seemed so relaxed and so genial I promptly suggested \$200,000. He said "Fine."

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<sup>2</sup> Later, in discussing possible support with an Investor in Boston, I mentioned Telander and he exclaimed "oh, the restaurant man." But my wife was skeptical of Mr. Telander's story because his shoes and watch were of a quality less than that expected for a wealthy man. We drove to College Station Airport one evening - but found a private jet described by Telander was indeed parked there.

What we had to do then was to introduce the idea to the University. I went to my boss, Dr. Michael Hall, and told him about this “peculiar approach.” We laughed about the ridiculousness of the idea of Transmutation in 1992 but nevertheless, I convinced Dr. Hall that he should give support in the acceptance of the gift which we simply called “inorganic reactions” because I wanted to be able to apply the gift widely.

Gifts at Texas A&M are dealt with through an agency which is separate from the agency which deals with the government grants. Gifts involving research have to be supported by the head of the department involved, and then finally the proposal goes to a Dean (in this case Dean Kemp) who accepts or rejects the funding.

There is advantage in funding work in this way rather than through the channels traveled by the government grants because the overhead on the “gifts” is less (e.g., 5 percent instead of 33 percent). However, the donor cannot have any control over the use of the funds.

There was a pause of about six weeks between Mr. Telander’s letter formalizing the gift and the University’s acceptance. Eventually, the Dean concerned said yes and we could go ahead.

### **CHAMPION’S ELECTROMAGNETIC EXCITATION EXPERIMENTS**

Mr. Telander put up Champion as the man who would do the laboratory work under my supervision and he was accepted by the University as a “guest worker” in spite of the fact that he didn’t have a degree in Chemistry. There then arrived a big computer programmed to give information on the nuclear properties of any element. Champion sought the frequency of the quadrupole oscillations which took place in certain nuclei.

The other apparatus was an electrolysis cell in which the material Champion hoped to transmute was in the form of an electrode, opposite which, on either side, were the radiating plaques from which he sought to stimulate the transmutational reactions.

It was about three weeks before we got everything going. In the following weeks we were subject to claims from Champion who would come out of his laboratory (where he was working

alone) and claim in an excited voice that he had a precipitate and this could be what he was looking for - new species.

We took samples of these precipitates for x-ray analysis and on one occasion seemed to see what resembled internuclear distances for gold. However, it did not replicate and I finally, after perhaps 1- 2 months of trying, stepped in and told Champion - it does not work!

Joseph Champion was frank in agreeing that he hadn't got anything out of his electromagnetic stimulation method. It is noteworthy that he had been left alone in this laboratory, the door was usually shut and had he wished to perpetrate a fraud, it would have been a most easy thing for him to have put something into the solution which he could have claimed arose from transmutation.

Mr. Champion and his wife were living in a hotel, at Mr. Telander's expense (no salary). When he admitted that he had not been able to make his method work, it would have been possible, - indeed expected, - of Mr. Telander to say, "Well, I told you nobody would believe it, - now you yourself don't believe it so that's that. Get out."

When he admitted the electromagnetic stimulation method did not work, Champion stated that he had a method which he knew would work because it had worked in the University in Mexico. This was what we later called the Explosion Method and which I later called the Impact Method.

### **THE IMPACT METHOD**

Champion's outline of the Impact Method was simple. One took certain quantities (these are detailed below) of lead chloride and mercurous chloride, mixed them with potassium nitrate and graphite powder, - an explosive mixture, - put the mixture in a coffee pot and set off a mild explosion by means of a propane flame. There were fumes, so the experiments were done in a fume hood. The temperature rose shortly for a few seconds and could be measured by means of an optical pyrometer. I had used these in early work in London and retained some sense of color

and temperature. I guesstimated the temperature of the reacting mass to reach  $1000^{\circ}\text{C}$  for a few seconds but it is possible that there were sites within the powder where the temperature could have reached much higher.

According to the method Champion brought from Keller, one then had to wait three days, after the explosion had occurred. There was a certain backing for the fact that in the mixtures a decay of some nuclear process was taking place. (Figures 1 and 2). The lifetime of this decay corresponded to an isotope of platinum and we shall see below that there is some independent verification of the existence of this intermediate.



Radiation  
April 21, 1992

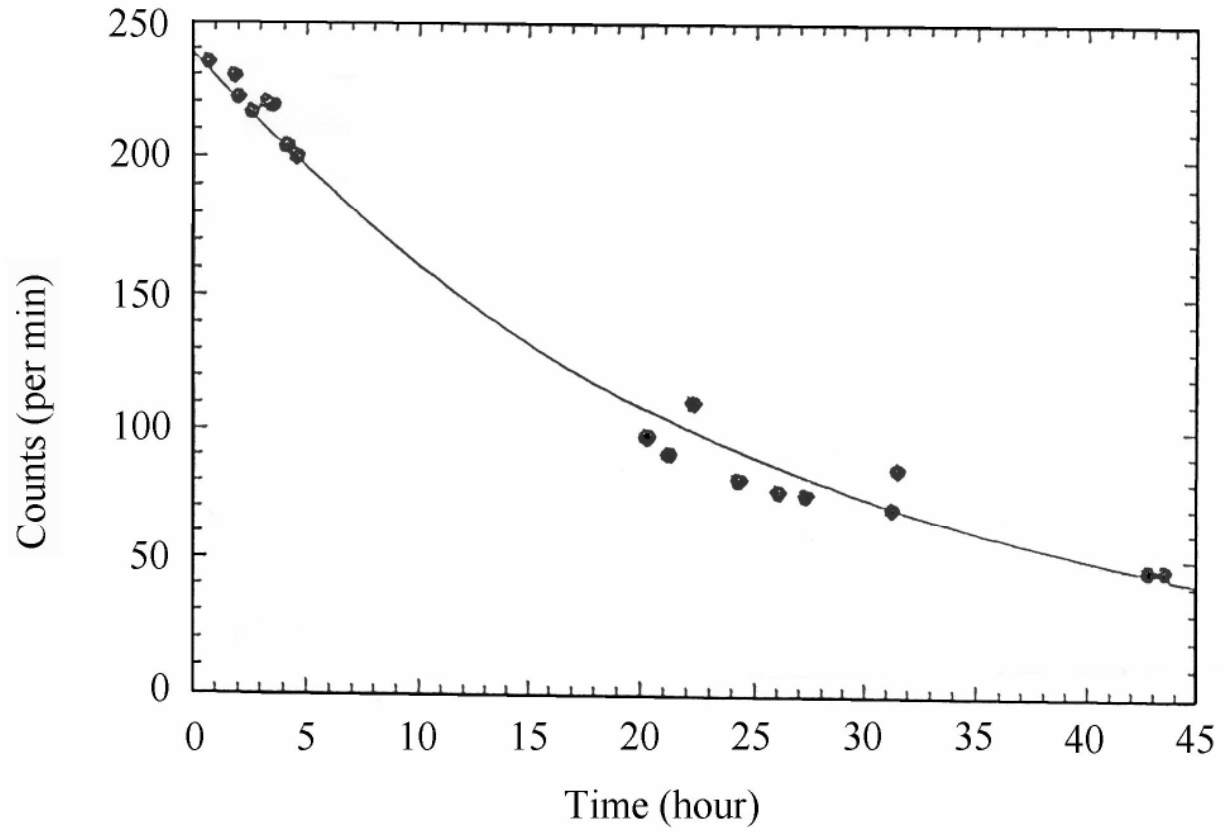


Fig 1  $\beta$  Radiation from a preliminary run of April 21, 1992 (negligible nobles).

Thermal 13  
December 9, 1992

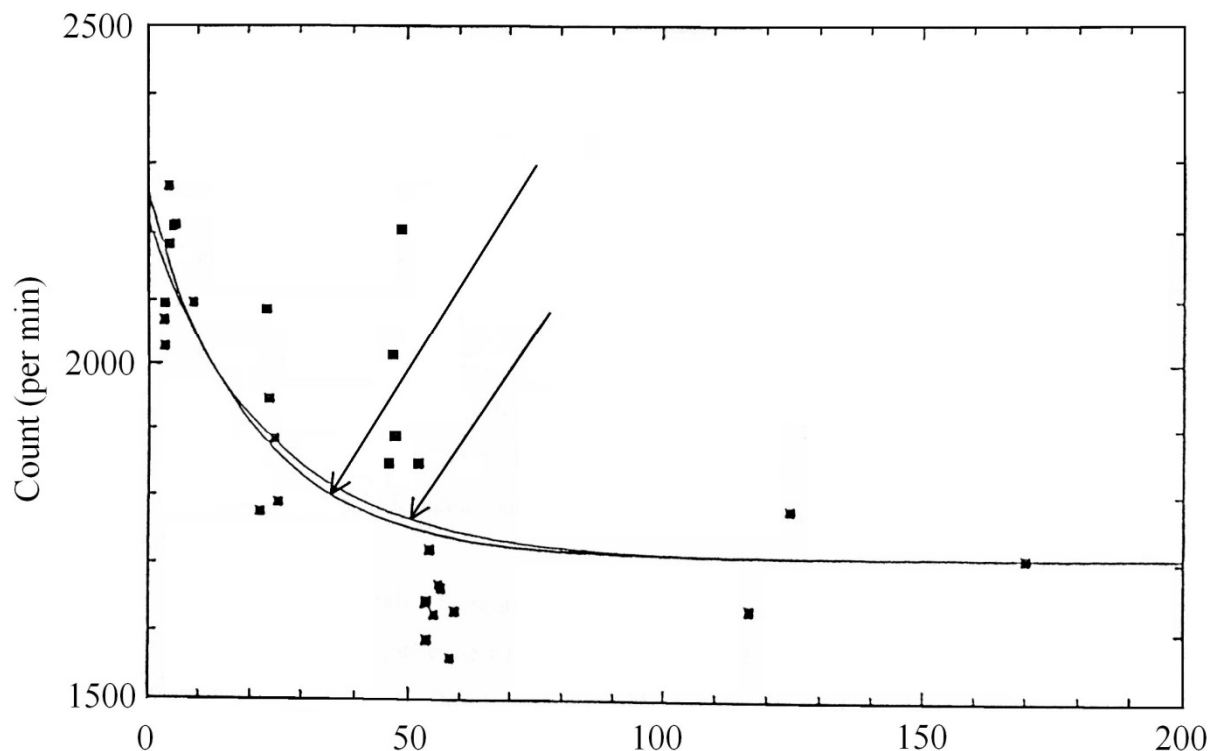


Fig 2  $\beta$  Radiation from later run, December 9, 1992, (negligible nobles).

The procedure was lengthy. The mixtures (compositions, see below) had first to be made and powdered. Then for about an hour one had to kneed it in a pestle and mortar and then shake the powder overnight.

I had been cautious about the way the experiment would be carried out. I was suspicious: the claims seemed outrageous. The likelihood of fraud had to be considered. I reminded myself that Champion and his wife were enjoying hotel life at Telander's expense. It seemed prudent to suspect deception.

Therefore, for the first experiments I donned a white lab coat and goggles, entered the laboratory and took the samples myself. I had tubes with rubber stoppers which contained about

20 grams of the mixtures with which we had started before the explosion, and then corresponding tubes of the mixtures after the explosion, and the three day wait. I ended up with about 12 tubes because I was insistent that several analytical companies should be used. I speculated that wealth might be turned into “strong influence.” But this was not going to be likely if the analysis was done in Australia or Canada or South Africa, and in these countries I had relevant contacts.

I therefore arranged for the analysis to be carried out in a laboratory in Nevada, where a great deal of testing of minerals occurred; a standard analytical laboratory in Ottawa, Canada, the government CSIRO organization in Melbourne, Australia, and, for the second set of experiments, the Institute of Metallurgy in Johannesburg, South Africa (Particularly used to analyzing ores for noble metals).

I decided to exclude Champion from experiments aimed at replication of his impact method. I asked Dr. Guang Lin (an experienced physicist) and Dr. Ramesh Bhardwaj (an experienced chemist), both senior post doctorals working with me on other projects, if they would devote half their time for a few months to working on the “Philadelphia Project.”<sup>3</sup>

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<sup>3</sup> The project was named “The Philadelphia Project” because of the legend (later made into a movie) that during WWII a destroyer was levitated from Philadelphia Navy Yard to that in Boston.

This was thought to be an impossible event, and as the attempt to realize alchemical transmutation looked to be something impossible it seemed a reasonable title for it.

Zoran Minevski, a graduate student, also assisted in the experiments from time to time in the work.

It is important to note the physical arrangements because they bear upon the possibility of fraud. Thus, the laboratories in which my work was carried out lay in a certain corridor in the old Chemistry Building of the Texas A&M University contained five laboratories on one side of the corridor and offices on the other. They were of varied sizes and the arrangements we made during the experiments was that Mr. Champion was not to enter the laboratories at any time. He occupied an office in the corridor while the experiments were ongoing.

However, Mr. Champion certainly had a part in the experiments because Lin and Bhardwaj turned to him rather than me when they wanted advice as to the technique used. My part was supervision. I spent some time each day checking up on what was happening, examining lab book results, having discussions in my room with Champion, Lin, Bhardwaj, and later with Monti (see below). Minevski's contributions were sporadic as he had his thesis work to do.

The preparation of the powder for ignition generally took about one day.

The second day was the critical one in which the actual explosion<sup>4</sup> was carried out and then the resulting mixture was left in the fume hood for three days. During these days samples were taken and tested for radioactivity. In Figures 1 and 2 are some plots we made of the  $\beta$  emission. The half-life came to about 18 hours.

Results of the experiments which were carried out in the first group have been described by Lin and Bockris and the following statement about six experiments corresponds to their account (2).

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<sup>4</sup> Explosion is a big word for what was observed. The mixture in a coffee pot was placed in the fume hood and ignited with a propane torch. There was an audible WOOMP sound and the mixture glowed red hot. About half of it was expelled from the coffee pot.

## Unexpected Elements

Six experiments, named Thermal 1 to Thermal 6, were performed in Texas A&M University from April 30, 1992 to June 15, 1992. The experimental results are summarized briefly in Table 1. Loss of about half of the material during the “explosion” has been considered in the table and the following text.

Table 1. Summary of Experimental Results

<b>Experiments</b>	<b>Main Results Obtained in April - June 1992</b>
Thermal 1	Two times increase of Pt was observed. One fire assay experiment showed the existence of visible Au.
Thermal 2	250-450 ppm of gold present in the product. An increase in Pd was observed, too.
Thermal 3	The weight of precious metal after cupeling from the chemical mixture with Hg was 3-4 times heavier than that without Hg, in the original mixture
Thermal 4	A large amount of gold, about 550 ppm was found.
Thermal 5	The gold concentration in the product was about 178 ppm.
Thermal 6	No gold was found in either experiment (with and without Hg in the raw material).

(1) Thermal 1 experiment was fired on April 30, 1992. The weight of chemicals in the experiment was 1671g, and the chemical composition of mixture is listed below:

C	300 g	(Johnson Matthey, 99.5%)
KNO <sub>3</sub>	900 g	(Baker, 99.2%)
S	80 g	(Spectrum)
SiO <sub>2</sub>	120 g	(EM Science, 60-200 mesh)
FeSO <sub>4</sub>	100 g	(Chempure)
Cd	30 g	(Johnson Matthey, 325 mesh, 99.5%)
Hg <sub>2</sub> Cl <sub>2</sub>	100 g	(Fisher, 99.98%)
PbO	50 g	(Johnson Matthey, 99.99%)
Ag	4.99 g	(Johnson Matthey, 100 mesh, 99.95%)
AgNO <sub>3</sub>	6.2 g	(Johnson Matthey, 99.998%)
Ni	20 g	(Johnson Matthey, Grade I)
Pd	9.78 g	(Engelhart)

Twenty-two grams of the raw chemical mixture (before firing) were sent out for analysis, and 1649 g of the mixture were fired using a propane-oxygen torch. The mixture after ignition burned with a yellow flame and the reaction appeared to die down in 3-4 minutes. The total product after firing was 783.4 g.

Both the raw material and product were sent to Bondar-Clegg in Ottawa and to the CSIRO Laboratories in Melbourne, Australia for analysis. Bondar-Clegg used a fire assay and an ICP method. The CSIRO used ICP and atomic absorption. The remaining product was also

analyzed by our team in Texas A&M University using a fire assay method, neutron activation analysis and absorption spectroscopy.

In some runs precious metals were sought by additional methods, including XPS, EDS, ICP, and mass spectroscopy.

Both the result from Bondar-Clegg and CSIRO showed no gold, and the analysis results by our team with different methods also showed no gold, except for a specific fire assay run where visible gold was found and verified by EDAX, 148 ppm of gold (with respect to the raw material) was observed by ICP measurement. Three pin-head size particles which had the appearance of gold were seen and one tested for Au (x rays).

The results from Bondar-Clegg and from CSIRO showed twice times increase in Pt in the fired product. An x-ray experiment also showed a Pt signal. Neutron activation in Texas A&M showed a small signal for Au and Ir.

(2) Thermal 2 experiment was fired on May 22, 1992. The weight of the chemicals in the Thermal 2 experiment before firing was 1715 g, and the chemical composition are listed on following:

C	300 g	Johnson Matthey, 300 mesh, 99.5%)
KNO <sub>3</sub>	900 g	(Baker, 99.2%)
S	80 g	(Spectrum)
SiO <sub>2</sub>	120 g	(EM Science, 60-200 mesh)
FeSO <sub>4</sub>	100 g	(Chempure)
Cd	20 g	(Johnson Matthey, 325 mesh, 99.5%)
Hg <sub>2</sub> Cl <sub>2</sub>	100 g	(Fisher, 99.98%)
PbO	50 g	(Johnson Matthey, 99.99%)
Ag	4.9 g	(Johnson Matthey, 100 mesh, 99.95%)
CaO	20 g	(Baker, reagent)

There are two differences between Thermal 2 and Thermal 1. The first is that CaO was used in Thermal 2 to replacement of Ni. The second is that Pd was not used.

Although the total weight of the chemical mixture 1715 g only 1655 g was used for the firing. The weight of the product was 849 g.



Both the raw material and product were sent to three labs (Bondar-Clegg in Ottawa, Chemex in Nevada, and Mintek in South Africa) for analysis. The remaining products were treated by means of a fire assay method and analyzed in our laboratory in a similar way as used in Thermal 1.

Chemex used a fire assay - ICP technique. The gold composition had increased from 0.3 ppm (with respect to the raw material) to greater than the detection limit (100 ppm) in the product. Bondar-Clegg used a fire assay - ICP technique. The gold concentration increased from 0.12 ppm to 450 ppm (with respect to the raw material) in the product. Mintek used four different methods to analyze the samples (see table below). The gold concentration increased from 4 ppm in the raw material to 420 ppm. An interesting feature is that an increase of Pd was observed in all three analyses. The increases of Pd were from 0.5 ppm to 1.3 ppm by Bondar-Clegg, from 0.4 to 2.1 ppm by Chemex, and from 0.3 ppm to 1.3 ppm by Mintek.

The analytical results in our laboratory were the following. Three sets of experiments were performed. The first set used 126 g of product, and 0.4 ppm (with respect to the raw material) of gold was detected. The second set use 131 g of product power, 253 ppm gold was detected. The third set used 481 g of product, 240 ppm of gold was obtained.

(3) The next experiment, Thermal 3, used a component of mineral sand instead of pure chemicals. The mineral (Mineral 1) contained no (i.e., <0.1 ppm) gold and silver. The total weight of chemicals in the Thermal 3 experiment before firing was 770 g, and the chemical composition was the following:

Mineral 1	100 g	(Action Mining)
PbO	20 g	(Johnson Matthey, 99.99%)
C	150 g	(Johnson Matthey, 300 mesh, 99.5%)
KNO <sub>3</sub>	450 g	(Baker, 99.2%)
S	30 g	(Spectrum)
Hg <sub>2</sub> Cl <sub>2</sub>	20 g	(Fisher, 99.8%)

Thermal 3 was fired on May 27, 1992. On the same day, another comparison experiment was fired, which had the same chemical composition but contained no mineral.

Thirty grams of Thermal 3 product and a comparison product were treated. The fire assay treatment from Thermal 3 products contained 3 mg of precious metal (50 ppm), compared to 0.8 mg of bead (13 ppm) from a comparison sample.

No further analysis of the Thermal 3 product was performed.

(4) Thermal 4 had the same chemical composition as Thermal 3 except that the mineral sand 2 contains 1.6 ppm of Au and 4.8 pm of Ag. The experiment was fired on May 30<sup>th</sup>, 1992.

The total weight of the mixture was 770 g. The product powder weighed 360 g. Part of the product, 100 g, was treated by means of the fire assay method. Visible gold beads about 47.3 mg, was obtained, which was equivalent to 1700 ppm of gold (with respect to the mineral sand).

(5) Thermal 5 was a repetition of the Thermal experiment 2. The total weight of the chemical matrix was 1615 grams. The weight of the homogenized powder after ignition was 841 g. The fire assay - ICP method gave 178 ppm of gold (with respect to the raw material).

(6) Thermal 6 experiment had two independent parts. The first part was the same as Thermal 4, and the second part containing all the chemicals but no  $\text{Hg}_2\text{Cl}_2$ . The two parts were done in the same experimental conditions. Both parts were fired on June 8, 1992.

However, the fire assay method gave no gold in either part of Thermal 6 experiments.

The rest of this paper does not originate in the paper of Lin and Bockris. (4)

### **ANALYSES**

The Nevada group used cupeling, a metallurgical method which attempts to isolate the actual metal concerned from the ceramic crucible in which it is formed. Secondly, normal chemical (wet) analysis. Thirdly, spectroscopic analysis. A neutron activation analysis carried out in the reactor group at Texas A&M.

Other methods were also used. For example, at the South African National Institute of Metallurgy used mass spectroscopic - ICP analysis. (Cf. results from the U.S. Company, see below) as well as other methods were used. At Texas A&M we used ICP and fire assay but also neutron activation analysis. Occasionally, x ray and electron dispersive analysis were used.

TABLE 2 - Analysis of Thermal 2

(Analysis carried out at the South African National Institute of Metallurgy).

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

There was some indication of a small amount of other noble metals, particularly in the South African National Institute of Metallurgy's results and these analyses are particularly significant because workers at this Institute analyzed gold in ores down to 0.1 ppm.

In the sixth experiment described, gold was found in five. In one, visible quantities of gold were found (tiny pinheads). About 0-01 percent Au is found in South African ores used commercially, but even 1 gram per ton (1 ppm) is found to be of interest.

The fruitful experiments were carried out between April 30<sup>th</sup> and June 15 in 1992. Thus, the average experiment took around a week. There were sometimes pauses due to apparatus breakdowns, the three day wait together with the times used in sending and receiving samples from Analyzing Organizations used in some experiments.

### **FURTHER WORK**

The work paused in June of 1992 but it was continued in an irregular way through February 15<sup>th</sup>, 1993. Between December 2 and January the 15<sup>th</sup> in >93, Dr. Bhardwaj worked alone and carried out 11 runs<sup>5</sup>.

In the meantime and from about midsummer of >92 to Christmas of >92, the transmutation-oriented work was slowed by two factors.

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<sup>5</sup> 2 days per experiment! Dr. Bhardwaj was then working alone during the Christmas Vacation. In these experiments, Bhardwaj looked only for gold and used local spectroscopic methods in the University so that pauses for sending and receiving the analyses were avoided. He appears not to have used the three-day pause as in the technique used by Champion.

Lin and Bhardwaj had to return to their official projects and continued working on them to catch up with the time they had taken off in contributions to the Philadelphia Project. However, there was another reason for the delay in further experiments to seek noble metals. We were interested more in the mechanism of what seemed to be happening than whether we got noble metals or not. We sought a meeting with Professor Joseph Natowitz of the Cyclotron Institute at Texas A&M University, an eminent nuclear chemist. Natowitz told us that one of the things we should be observing if the transmutation which we claimed took place, was  $\gamma$  radiation. We therefore set out on a quest to detect gamma radiation in the fired product, but there we fell upon hard times because although people had been helpful in lending us nuclear detection equipment up to that point, when it came to help with the gamma experiments, little was forthcoming. Eventually we did obtain a gamma detector from outside the University but detected only trace indicating gamma radiation from the products which - in some cases, - did give  $\beta$  radiation.

The later experiments carried out by Dr. Bhardwaj, were negative and the question arises as to why the original experiments, were fruitful, could not be repeated.

The cause may be the absence of the three day pause, but in my opinion there may have been a psychological component.

Dr. Bhardwaj is a serious and studious man, and it is likely that he was offended by the secular ways of Telander and Champion. Thus, Mr. Telander, when in College Station, was fond of holding dinners which often were prolonged, wine being drunk copiously, and many people being invited, including Bhardwaj and Lin, but also some women friends of Mr. Telander. Dr. Bhardwaj saw these people in their enjoyment of alcohol, during which it is reasonable to suppose that some of the conversation would be deemed unfitting for the studious person. Bhardwaj may have thought that nothing claimed by Champion and the funder of the work we were testing could be true.

Much later, Bhardwaj told us that he had reported details of the work to an FBI agent who had visited all concerned and who of course, held the opinion that the whole thing was a fraud. By December 1992 Bhardwaj may well have thought it embarrassing to have signed off

on five experiments indicating noble metal formation from cheap metals and was not displeased when the later results (without the 3 day wait) were unsuccessful.<sup>6</sup>

### **CONFIRMATORY WORK**

There are only three independent workers which, to a degree, confirm the impact method as causing a nuclear change and this is the work of Filimov and Kobets (11) who presented their work at the Cold Fusion meeting in Vancouver (1998) where they showed that by causing an explosive compression of the powder, new isotopes could be found. Cau (12) reported a confirmation. A report from a U.S. company (see below) offers some support.

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<sup>6</sup> In fact, Bhardwaj's final position on the summer experiments had become negative during Christmas 1992 and some emotion had added to the caution he normally showed in his work. Thus, I recall his bursting unannounced into my office around February 1993. He threw a series of Lab Notes on the floor, saying angrily, "It doesn't work. See!"

## THE SUCCESSFUL RESULTS OF SUMMER 1992

Thus, experiments, - in which due care was taken for the three-day pause, - did show an enhancement of the gold in three of them in concentrations of 100 -550 ppm and there was evidence of small concentrations of other noble metals in another. The first two sets of results were confirmed by multiple and different methods of analysis carried out by various organizations. These organizations agreed in their analysis qualitatively, but differed quantitatively by up to 50 percent.

There is a tradition among older mining engineers that if one explodes an ore there is an order of magnitude increase in the noble metals extracted from the ore. However, whether this is a transmutational effect or an effect of “shaking” of metals out of the ores, is not clear. Two of the four successful experiments were carried out with pure materials (no ores) showed that it seems unlikely that this explanation would be the basis of what we observed.

There were repercussions of this work and it was attacked later in the local press in College Station and in Newsweek as a fraud. (Interestingly, when, later, I had multiple confirmation of metal to metal transmutation, the local newspaper did not find confirmation of the discovery it had pilloried to be interesting enough to report.

It seems fair to claim that in the summer of 1992 at Texas A&M University and in particular on May the 22<sup>nd</sup> of 1992, the obtaining of noble metals from mixtures of cheap materials was observed. One cannot say “established”, because this means that several groups would have to get the same kind of result.<sup>7</sup> As of 2003, what is now being called “The Monti Method” is said to have been confirmed in Italy and in Taiwan. The emphasis now is of course in nuclear waste remediation.

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<sup>7</sup> Much information on the details of related experiments is available in my archives.



One piece of information which tends to support the contention that noble metals were indeed transmuted from simple metals arose as follows: In June 1992 Telander and Champion took the process to a well-known company in the United States which deals only in noble metals production. The company agreed to carry out experiments themselves which would confirm or deny the validity of what had been claimed.

I was lecturing in Australia at the time, but I was in contact by telephone with the people in College Station and learned that a week before the time I am just about to report, the instructions for the technique were given to the Research Director at the company concerned.

Now, I phoned this man from Auckland, New Zealand and asked him about the degree of success that he had had in testing out the experiments. He was very enthusiastic and said that it was remarkable,- his researchers had succeeded using mass spectroscopy to identify the intermediate isotope which were present during the wait period. He appeared to be satisfied that the process worked.

When I returned to the USA, however, I got a phone call from this man. On this occasion he said that this whole business of transmutation was nonsense and I should never report that there had been any verification of it by his company. I interpreted this as a political reaction of a Company which lived by processing precious metals made from low grade ores.

### **UNIVERSITY'S REACTION**

The reaction of the University to the Philadelphia Project was at first silence. I had told the head of the Department of Chemistry what we were trying to do and kept in touch with him by occasional talks. He had, of course, expressed high skepticism and made the reasonable condition that if we did obtain anything which could be published, it should first of all be replicated by another group.

However, in November 1993, the quiet time was broken by the publication of a letter which a former employee of the Research Foundation at Texas A&M (let go by that

organization) in the local newspaper, expressing disgust that “alchemy” was being practiced in a State university in 1993.

Whether the employee actually believed that medieval alchemy (rather than a modern attempt to replicate it) was being practiced seems doubtful. She had been the assistant to a Dean of Science who had set himself up as a critic of the work. It is noteworthy that the letter was published seven months after the work had stopped.

The letter gave rise to much war-like drum beating. There were articles in the national press which hinted broadly that I, too, must be involved in a fraud. I consulted a lawyer and he told me that I should not deny allegations because my ability to sue those libeling me would disappear were I to become a public figure.<sup>8</sup>

Another matter which lead to the University’s negative reaction arose from the fact that Mr. Telander invited Lin and me to visit Mexico City to present the work to a group of technical journalists. It was a 9:00 a.m.. meeting in a hotel in which there were maybe 20 journalists present. Telander, Champion, Lin, and I gave brief, - five minutes, - presentations of the work that we had done and, e.g., I said that if metal to metal transmutation were confirmed, it would be a major innovative step in nuclear science and have consequences for the theory of the nucleus.

The meeting lasted about one hour (there was discussion) and then Lin and I returned to College Station. The Mexican press reported this meeting widely and the fact that I had made a statement about transmutation and that I was a professor at Texas A&M University was featured. This caused Dean Kemp, - the Dean who accepted the \$200,000 which Mr. Telander had donated, - to make a formal accusation against me for “misconduct in research.” There was a kind of trial within the University in which my judges were four distinguished professors. I hired a lawyer and he took much trouble to study the case and provide me with a booklet

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<sup>8</sup> Thus, in U.S. law, public figures, e.g., well-known politicians, can be libeled, but individuals are subject to the laws of libel.

showing the plus and minus of my case, what I might be asked and what would be appropriate answers to give and etc.

The accusations against me were that I had “conspired” with William Telander to obscure the intention of the research related to be the Philadelphia Project. I was accused of exaggerating the research findings.

The defense was based upon letters which had been exchanged between William Telander, Joe Champion, Michael Hall (the head of the Department of Chemistry) and me. One letter by Telander refers to the focal point of the research being “on the single phenomenon which has implications in the accelerated production of precious metals.” This document, - which clearly stated the object of the work, - had been faxed to the Department Chairman on April the 16<sup>th</sup>, 1992, and it must have been therefore seen by Michael Hall. It has the official stamp of the Provost and Vice Provost of the University dated April the 20<sup>th</sup>, 1992, indicating that it had been seen by these officials.

There were other documents between myself and the head of the department and I made clear to Dr. Hall that we were examining Champion’s alleged process. Dr. Duwayne Anderson, the Vice President in charge of research at that time came to watch the firing of one of the experiments. A memo dated August the 10<sup>th</sup>, 1992 from me to Dr. Hall informed him that we had produced more than 100 mgs of precious metals in some experiments.

The four distinguished professors which were set to investigate my handling of this research had done their job well (examined 1000 documents!) and used voice enhancement machinery to hear all elements of a conversation which took place between a reporter of the Dallas Morning News who came to investigate the affair, Dean Kemp and myself. This was an interview which took several hours.

One of the determining documents which the distinguished professors brought out was a rough draft in my own handwriting of a letter which I had written in a New York Hotel. It was a letter to Mr. Telander telling him he must not exaggerate the significance of the success we had had, that it was not commercial grade as yet, and etc. How the committee got hold of this letter

draft, I don't know, but I found that a number of things were disappearing from my offices in the University at that time. I suppose people came at night and removed them and my letter draft to Mr. Telander must have been among them. This letter showed that I did not exaggerate the research findings but on the contrary pulled back Mr. Telander who indeed did want to hint that the artificial production of commercial amounts of gold had been would be possible.<sup>9</sup>

On January the 31<sup>st</sup>, 1994, there was a letter of "complete exoneration" from the Committee of distinguished professors who had tried me. I was congratulated by a few people in the Chemistry Department, but a group in the Inorganic Division of the Department of Chemistry felt that the inquiry had not been broad enough and my true sin was that I had not exposed the work to peer review in an established journal (to attempt this would, of course, been useless because no one would have agreed that the results were possible).

The latter group did not give up and a two year persecution of me began. It was based on the implied threat of an ad hoc Committee formed to investigate whether I had done anything which would justify firing me (recall that I was a tenured Distinguished Professor). I have told the principal story of this investigation in a paper (10). At any rate, finally in May 1995 I received a rather cold letter from the Acting Provost saying that the Committee had decided (after a year of meetings) that I had done nothing outside the Rule Book of the University and that a "change of personnel" was not contemplated.

### **LATER EXPERIMENTS**

I have made clear that the successful experiments we did here were largely done during the summer of 1992, in April - June. In September, Joseph Champion and his wife left College Station and shortly after were started work in a Chicago facility. Their objective was to scale up the process up.

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<sup>9</sup> Whether the process could ever lead to commercialization is something which needs examination (and above all replication). The South African mines work with one part per million in the ore and we were getting hundreds of parts per million so that it is possible that our work could lead to something practical. (Cf. Keller's claims).

## **THE CONTRIBUTIONS OF DR. ROBERTO MONTI**

Dr. Monti came over from Italy in May 1992 at Telander's invitation (and my suggestion) and worked with us during the summer experiments in which we got five experiments which contained some results which showed that a chemically assisted nuclear reactions occurred.

Dr. Monti re-emerged, so to speak, in early February 1993 and said that he was certain he could produce new gold. I personally paid his fair to come from Canada to Texas and he arrived in February of 1993. The last experiment which he performed is dated the 27<sup>th</sup> of February, 1993.

Dr. Monti worked in the laboratory in collaboration with Dr. Bhardwaj.<sup>10</sup> However, all in all the result of their collaboration was disappointing. There were indeed blips of gold which we saw in those days and one mixture exhibited radioactivity, but there was nothing that we could claim compared with what we had in the Summer. I therefore wrote to Nancy Meechum, one of Mr. Telander's lawyers, and told her that we had not been able to reproduce the experiments on which we had reported positively at an earlier time.

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<sup>10</sup> In a letter of August 6, 2003, Dr. Bhardwaj disputes my conclusion that in his "final" tests which he made of the impact method, the three-day pause had been neglected. I find the number of runs which Dr. Bhardwaj claims to have made over the Christmas vacation of >92 (11 in my memory; 16 in his) inconsistent with a three-day pause.

## **INTERVENTION OF THE SEC IN CALIFORNIA**

Around March 1993 we were disturbed by alarming news, namely that the SEC in California had accused Mr. William Telander, - of having used money given to him for investment by others in a way which differed from that which he had offered them. Up to this time, we had thought that the financial support from Telander came from his own pocket. However, we now learned that Telander had advertised a scheme in California that he could obtain high interest levels by using the process of arbitrage in Swiss Banks. If one is a person who has a million dollars to invest (and not less) one can go to certain Swiss banks and use currency difference for gain. For example, perhaps (whilst in Zurich) one bought a million dollars worth of gold in Hong Kong where the price was \$300 per ounce whilst the price in London at the time is \$301 dollars per ounce so one could sell the gold bought in Hong Kong in London with a gain of a dollar per ounce of the gold. One can see that with million dollar sums of money to use in this way, one could make more money than by investments available in the USA (for this kind of process can be done repeatedly during a year).

Directly the University knew of the SEC's suspicions, we were told that the grant Telander had given the University could no longer be spent. There was, to my memory, \$48,000 left in the funds at this time and of course we had people hired whose salaries came from this grant.

Luckily the news arrived on a Friday at the end of the month when everyone had been paid. This gave me a month to find money for the researchers I was employing and whom I could no longer pay from the money which Telander<sup>11</sup> had provided. I was able to continue employment of my co-workers because of support of work on Transmutation by ENECO which was a company arising from the original Fleischmann and Pons work in Salt Lake City, and also I was permitted to use a small amount of EPRI money for continuing work on Cold Fusion. (See below).

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<sup>11</sup> Mr. Telander landed in jail. There was a lot of legal language which was involved in the charges against him but it came down to the fact that he promised his investors to use their money in a certain sense and he did not use it in that sense. He was condemned to four years in jail. I have not met him since that time though I did have a telephone conversation with him after he left jail.

As far as Joseph Champion was concerned, I saw him lastly in September 1992. On December 11<sup>th</sup>, 1992, we had a call from one of Telander's secretaries who said "The boss is on the way. He's flying over. He's got big news for you."

We met Telander at his private jet at the airport in College Station, took him back to the laboratory. He said he was dissatisfied with Mr. Champion's handling of money which he had put in an account in Mexico City which could be drawn on jointly by Telander and by Champion. It was of the order of \$100,000. He didn't claim that Champion had done anything illegal with this but that he had withdrawn money unilaterally and that he had spent it on personal living and not new work on transmutation which Telander thought he was going to do. Telander devised a charge against Champion for some unfinished legal business which he had in Arizona. It was to do with the fact that Champion had signed a check on a bank account which he had closed and the check would not be paid. The next we heard of Joseph Champion was that he was in jail in Arizona, - that he had agreed to the charge for a lesser sentence.

Of course the fact that the two people who had worked with us were now both in jail, - though for sins which were disconnected with claims of transmutation technology, - was nevertheless a deadly blow because, of course, the story which spread was that Champion was a fraud and that he had been put in jail for fraudulent attempts to make artificial gold. Presumably, I was to be regarded as a fool who had been deceived by Champion's nonsense.

## THE CONVERSION OF CARBON TO IRON

By mid-1993 I knew that we had converted deuterium to tritium (this had been confirmed in 20 to 30 laboratories by this time).<sup>12</sup> I felt that there was some evidence in four or five experiments, that we did indeed get tiny quantities of gold and noble metals from lead and mercury.

I wanted therefore to continue to probe the transmutation area but I didn't want to do it in experiments connected with gold because it gave rise to such a furor of criticism, - I thought the best thing was to change to another system.

At this time Monti wrote to say that he himself had seen the conversion of carbon to iron.

At first this seemed to me a most unlikely transmutation reaction. However, Monti claimed that not only he had seen this transmutation reaction work but also at the Baba Atomic Research Center in Bombay, India, work was being carried out in which they had seen not only iron but also nickel and cobalt formed from spectroscopically pure carbon. A post doc, Sunderesen, from the Baba Atomic Research Center in Bombay working with me, struck an arc between two spectroscopically pure carbon rods in water and after sparking interruptedly for ten hours, observed small amounts of deposits which contained iron. (4)

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<sup>12</sup> I stopped counting when I reached 47 papers on tritium.



**TABLE III**

**Values of Iron in the Carbon Detritus After Arcing: Series II (4)**

Time (h)	Electrode 1			Electrode 2			Electrode 3		
	Weight of Carbon (mg)	Iron ( $\square$ g)	Iron in Carbon (ppm)	Weight of Carbon (mg)	Iron ( $\square$ g)	Iron in Carbon (ppm)	Weight of Carbon (mg)	Iron ( $\square$ g)	Iron in Carbon (ppm)
1	24.5	3.43	140	30.3	1.4	46.2	25.1	1.94	77.3
3	79.1	9.62	121.6	83.6	22.8	272.7	86.3	15.0	173.8
5	140.9	1.1	8	142.7	4.5	31.8	a	a	a
10	a	a	a	a	a	a	286.1	39.9	139.5

a. Experiments not done.

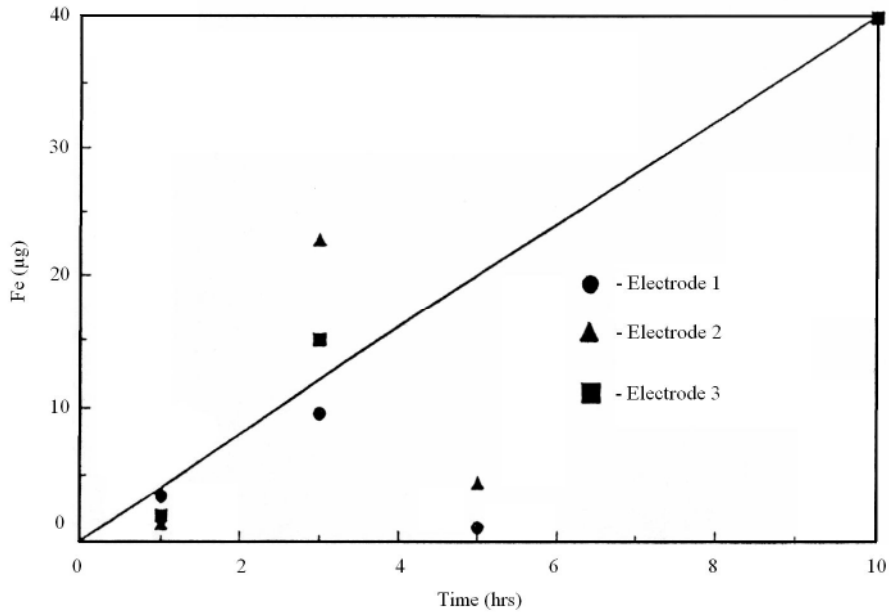
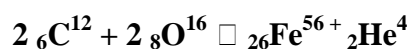


Fig. 3. Effect of time of electrolysis on the formation of iron. (4)

Of course, one had to be sure about the iron content of the carbon rods before oreing and this we did by multiple analyses. There is no doubt that the iron which we analyzed at the bottom of the beaker was much more in quantity than the total iron in the spectroscopically pure rods .

We did this with varying times of arcing and there seemed to be an increase of new iron with the time of arching. It depended on the oxygen content. One deoxygenated the solution, the reaction didn't happen and we ended up, - with the help of Dr. Lin, - by suggesting the process of what was the following:



George Miley, the Editor of Fusion Technology at this time had been a doubting spectator on the brink of the work on transmutation. I had a number of discussions with him and his attitude was to be interested but to say that he couldn't publish transmutation work because he would be soon be out of his job as Editor of the Journal.

Gradually, however, Miley's attitude changed and, of course, - as is now well known, - he became a forefront person in research on transmutation reactions. He has published several confirmatory papers himself in which his own ability as a nuclear physicist has come to the fore. He has utilized more advanced methods, particularly determining the isotope abundance frequency of the new material, so that he is now (in the USA) a leader (because of his reputation in nuclear physics) of the new world of transmutation (3).

Finally, therefore, Dr. Miley did accept the paper by Sundaresen and myself.

### **NUCLEAR REACTIONS INSIDE PALLADIUM SATURATED WITH HYDROGEN(5)**

Zoran Minevski was a graduate student when I asked him if he would be willing to work on damage within palladium as part of his Ph.D thesis. He evolved hydrogen on palladium for several weeks and then examined the interior of the palladium at various depth. He removed the

surface by Argon ion bombardment and therefore was able to analyze by means of XPS surface and EDAX (deeper) the substances present inside the electrode.

He varied potential, time, and temperature, and registered damage electron microscopically and by means of Normarski Polarized light microscopy. We settled down to look at new substances observable as electrolysis proceeded. We understood that there would be deposits on the surface from impurities in the solution. We made an analysis of the solution by IPC and found that the new materials on the surface corresponded to those in the solution. We then examined what the EDAX had given us. This method penetrates deeper than XPS and hence avoids registering the surface impurities. Results are given in Table IV.

This was still the days of skepticism in respect to transmutation in general. We asked ourselves whether these new materials had diffused into the palladium from the solution but they would have had to diffuse several hundred angstroms, and this seemed inconsistent with known diffusion coefficients of metals in the cold. We worried about fissures in the electrode but again this would only have brought the same materials deep into the metal as we had seen from the surface impurities, the origin of which we knew.

Finally, therefore, we came to the bold conclusion that Minevski had established that when one saturated palladium with hydrogen one did create new materials therein. (Table IV)

This was a discovery (1993). It resonates with the work of Kucherov in Russia, who reported something similar at the Cold Fusion meeting in Maui in late 1993. Kucherov had not named his “impurities” transmutation. We were prepared for it more than others because of the work we had done in finding tritium and helium; the work we had done on lead and mercury into gold and the observations of new materials from carbon arcing.

The work we did with Minevski (5) was a forerunner of much work done later by George Miley (3) and his group at the University of Illinois and by the people in the University of Hokkaido in Sapporo, Japan (7, 8), by Mizuno, Ohmori and Notoya.

The work by these nuclear physicists was better than the work we had done earlier. They did isotopic abundance analysis on the new materials which we didn't do, but we had made an ICP examination of the solution and this they did not do.

**TABLE IV**

**CONCENTRATIONS OF IMPURITIES(5)**

**(Atomic weight percent found in virgin Pd after three weeks of electrolysis, (EDS))**

	<b>Elements at 1 <math>\mu</math> Depth</b>		
	<b>(Atomic %)</b>		
	<b>Virgin Pd</b>		
<b>Element</b>	<b>Johnson Matthey/ICP</b>	<b>Present Work/EDS</b>	<b>Electrolyzed Pd 3 Weeks/EDS</b>
Mg	<1.0 * 10 <sup>-4</sup>	- *	6.7 $\pm$ 1.0
Ag	<1.0 * 10 <sup>-4</sup>	- *	1.9 $\pm$ 1.0
Si	8.0 * 10 <sup>-4</sup>	- *	10.2 $\pm$ 1.0
Cl	-	- *	3.0 $\pm$ 1.0
K	9.0 * 10 <sup>-4</sup>	- *	1.1 $\pm$ 1.0
Ca	3.5 * 10 <sup>-3</sup>	- *	19.9 $\pm$ 1.0
Ti	<3.0 * 10 <sup>-4</sup>	- *	1.6 $\pm$ 1.0
Fe	<4.0 * 10 <sup>-4</sup>	- *	10.5 $\pm$ 1.0
Cu	4.5 * 10 <sup>-3</sup>	- *	1.9 $\pm$ 1.0
Zn	<4.0 * 10 <sup>-4</sup>	- *	4.2 $\pm$ 1.0
Pt	1.0 * 10 <sup>-2</sup>	- *	7.1 $\pm$ 1.0
Pd	99.80	98.10 $\pm$ 1.0	31.9 $\pm$ 1.0

\* Lower than measuring limit of EDS

Thus, at Texas A&M between 1992 and 1993 we were able to discover metal to metal transmutation, lead and mercury to gold and noble metals, iron from carbon and (widely confirmed) palladium into various metals.

### **JOSEPH CHAMPION'S AND ROBERTO MONTI'S CONTRIBUTIONS**

Joseph Champion is, of course, a black sheep because of years wasted in jail. However, much he owed to Keller, it is true to say that the outburst of work on transmutation in the cold, - which has by now is occurring worldwide, - was triggered by the work we did at Texas A&M in 1992 and that would not have been carried out without Champion's persistence and Telander's curiosity (although cf. Out earlier 1989 finding of tritium from deuterium).

The part played by Roberto Monti was seminal in encouraging me to take up what seemed at the time a most unlikely research project. Monti was a credited physicist and his intellectual influence on me was more than that of the claims of Champion and the funding of Telander.

The early work of Lin, Bhardwaj , Sundaresen, and Minevski must also be recognized. They dived into very rough water and contributed at a time when admission to be working on transmutation attracted ridicule and vitriolic articles in the press.

### **KEVIN WOLF'S WORK**

I went to a Cold Fusion meeting in Nagoya in Japan in late 1992 and at the meeting there I heard about work by Kevin Wolf of the Cyclotron Institute at Texas A&M. The rumor was that he had observed new radioactive isotopes in one of his electrodes after prolonged evolution of deuterium thereon. The difference between the Kevin Wolf work of December 1992 and the work which I did later with Minevski (1993) was that the Kevin Wolf new elements were radioactive whereas ours were not.

The radioactivity of the materials claimed by Kevin Wolf made it easy to analyze and there was a visit by Tom Claytor from Los Alamos to Texas A&M to discuss mechanism with

Kevin Wolf and also work done at Los Alamos, using gamma ray analysis, to identify the new isotopes in Wolf's electrodes, obtained, - it was stated at the time, by electrolysis.

This seemed to be a powerful piece of evidence for transmutation in metals. Indeed, for a time Kevin Wolf's announcement had more effect than ours for Wolf was an established nuclear chemist. Indeed, there was another factor: we were prohibited from publication of our results from the summer 1992 work because Telander had made us sign an undertaking not to publish the work for three years.

Kevin Wolf also decided not to publish his work. He talked of a letter he had from Tom Schneider, his program manager EPRI, in which there were given seven reasons for not presenting the work at a Meeting.

But the reticence of Wolf naturally raised questions and one questioner was Tom Passell, a Program Manager at EPRI. Tom took an interest in Kevin's work and indeed he was the first person to present it in the Cold Fusion meeting in Monte Carlo in 1995. There was, of course, a question about Tom Passell presenting something which had been carried out by someone else. Apparently, he had a legal right to do this as the work belonged to EPRI, the sponsor and, of course, could not be kept secret.

I invited Passell to re-present the work in the first transmutation meeting I held at Texas A&M in 1995.

This all went into the background as the years went on and I assumed that Kevin Wolf had indeed been No. 2 in the observation of metal to metal transmutation. However, Tom Passell had suspicions about Kevin Wolf's work. These were based upon his reports to EPRI, - which were never published, - and it appeared that in them he had described how he got these radio elements in more detail. Passell had come to the conclusion that it was doubtful that Wolf had got them by evolving hydrogen or deuterium on palladium but instead by classical radio chemistry. One would have simply to irradiate the palladium in a certain way and transmutation would indeed take place in a textbook manner. In fact, in 2003, Passell carried out such



radiation and got the very same new radioactive elements as Wolf had seemed to have obtained by means of electrolysis.

Did Wolf intend to deceive or was he bent upon a charade, it being revealed at a time of his choosing that the elements came into existence by classical means? At any rate, at present, Wolf (who died in 1997) is still thought to have been No. 2 in the discovery of transmutation in the cold. Passell will probably not bring to light his recent findings.

### **THE MEETING ON TRANSMUTATION AT TEXAS A&M UNIVERSITY IN 1995**

My colleague Dr. Guang Lin was keen on extending knowledge about workers in other countries who were following us in getting results indicating transmutation in the cold. I went to my boss who at that time was Professor Emile Schweikert. He readily agreed to an international meeting on the subject being held in the Chemistry Department at Texas A&M. I'm glad to say that when he was later criticized for allowing the meeting, he stuck to the truth and agreed that he had indeed sanctioned the meeting.

We came up with a list of invitees and got no refusals to present. The principal people whose papers we finally heard were:

(1) Tom Passell, who presented the work of Wolf as though they were Cold Fusion experiments.

(2) T. Ohmori from the University of Hokkaido in Japan who reported finding iron on the electrode surface during hydrogen evolution. He had analyzed the isotopic distribution of the iron isotopes and found that the  $\text{Fe}^{57}/\text{Fe}^{54}$  was much greater than that of the natural ratio.

(3) John Dash from Portland State University reported experiments which are qualitatively similar to those of Ohmori. He analyzed his palladium cathodes by SEM and determined that after evolving  $\text{H}_2$  on there, silver and cadmium were present in spots on the palladium surface. Large concentrations of gold were also found in dendrites protruding from the platinum.

(4) Kucherov reported experiments which he had carried out in Moscow with Karebut and Savatimova (6). He was cautious in claiming that he had observed transmutation but finally in the end said he could find no other explanation. However, he made no attempt to distinguish the impurities on the surface; from those in the solution and no attempt at isotopic abundance measurements.

(5) Notoya from the University of Hokkaido presented results which suggested that calcium had been formed from potassium during aqueous electrolysis experiments.

(6) Rabzi from the Ukrainian Academy of Sciences presented transmutational results which bore some relationship to those which we had done earlier in collaboration with Champion. For example, he heated rapidly lead (99.5 percent pure) and found that this yielded several different elements including 0.2 percent of gold.

(7) Mizuno applied high current densities to a ceramic at about 500° C and not only produced excess heat but also new materials including Al, Bi, Sn, Gd, and Dy. He measured the isotopic abundance ratios and found them to be significantly different from the natural ones. (Cf. 7, 8).

(8) A contribution from R. Monti described the suppression of the radioactivity of thorium oxide. Monti's method was similar to the method worked on in the summer of 1992. He could reduce the activity from 900 cpm to about 100 cpm in four days of a series of sudden heatings. Possibilities of de-naturing radioactive wastes appears and have been further developed by Monti and separately by Hal Fox (2003).

The meeting at Texas A&M in 1995 was marred by an event which showed the hostility of certain Professors in the Chemistry Department. On the second day of the meeting Professor F. A. Cotton accompanied by two colleagues, approached the meeting, showing anger, and made unpleasant comments calling the participants at the meeting "all kooks".

This was unfortunate because the two people who were standing outside the lecture theater and to whom Professor Cotton apparently addressed his remarks were Professor

Hagelstein from MIT and Dr. Ward, an employee of the Department of Energy, who made a speech at the meeting indicating that he thought the DOE would fund some work of this kind.

I was due to go to Australia directly after the meeting but nevertheless did write a letter to the President of the University complaining about Professor Cotton's interruption.

### **THE MEETING ON TRANSMUTATION IN COLLEGE STATION IN 1996**

Lin and I thought that it might be a good idea to hold a second meeting on transmutation, particularly as interest in the subject seemed to be growing. I once more approached the Department Head, Professor Emile Schweikert again but by now the rules had changed. Our colleagues in Chemistry had decided that requests for such a meeting should pass through a committee which consisted of 12 members of the department.

I duplicated a comprehensive review of cold fusion by Ed Storms which contained more than 100 references and had a copy distributed to each Committee member before the meeting. I made a brief presentation saying that this was new work and that it was going on around the world, there had been a number of confirmations of new nuclear reactions. I hoped that they would allow the new science to be heard. The voting was 12 to 0 against. I called one of the people on the Committee I had known longer than some and asked him what the discussion had been about after I had left the room. He said that everyone on the Committee knew that it was impossible for nuclei to be changed except under conditions of very high energy exchange. Hence, the members had concluded that the work that I wanted to have presented was either a joke or a fraud.

We held the meeting in the local Holiday Inn. Because of the assault made by Professor Cotton and his colleagues on the first meeting we thought that a more violent one might be made in this meeting and therefore hired a deputy from the police department to be present outside the door of the meeting in order to quell any attempt by members of the Chemistry Department to suppress the presentation of new ideas by violence.

The papers of the >96 meeting have been published in the Autumn edition of New Energy of that year. George Miley co-chaired the meeting with me. Monti was present and

attacked one of the speakers with a vigor which was thought to be too strong. Mizuno and Miley presented papers which supported the work Minevski and I had done and thus made the likelihood that transmutation in Pd-H systems is highly probable.

Many members of this second meeting expressed disgust at the refusal of the Chemistry Department Committee to allow them to present their papers in the University, - a clear example of the suppression of New Ideas.

### **TRANSMUTATION IN 2003**

The meetings of 1995 and 1996 at Texas A&M in College Station, Texas were pioneer meetings. Transmutation of metals has spread around the world and is now an accepted part of the so-called Cold Fusion science.

Transmutation work is particularly carried out in Russia and Japan. The most remarkable addition to the transmutation work is that being carried out at Mitsubishi, Inc., where a million dollar laboratory has been built and a number of interesting transmutational results have been confirmed.

There seems to be financial support for an investigation of nuclear waste remediation by an electrochemical approach.

## **CONCLUDING REMARKS**

The work described in this paper is of interest not only for the pioneer character of the results obtained (and the complex steps by which a new field came to be) but also because it is an example of the need researchers have to attack their research with an independent mind, not bound by knowledge of the past. Science has an always moving frontier. It is the duty of all research scientists not only to look ahead but to step over the boundary which denotes the frontier of the time.<sup>13</sup>

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<sup>13</sup> My attitude is not held by all scientists. For example, I received a letter (1993) from a Professor in the Inorganic Section at Texas A&M. I was advised that if I were able to regain credibility after what I had published, then I should be careful to stick to what is in the book in my future researches!

## ACKNOWLEDGMENTS

I firstly thank Roberto Monti for having suggested to me (1990) that transmutation outside the hydrogen isotopes (cf. My publication of 1989 on tritium) might be possible; and Joseph Champion for having brought to my laboratory a method for obtaining transmutation. I thank Mr. William Telander for the support, about \$140,000 of which was used in the research. (The remainder is with the Univerisity). My post docs Bhardwaj and Lin, were courageous in agreeing to devote half their time over about seven months in the investigations. Dr. Bhardwaj worked further for about one month with Dr. Monti in 1993.

Several other students (in particular Nigel Packham, Jeff Wass and Zoran Minevski) worked on the foregoing work on hydrogen isotopes: Some postdoctorals were involved, particular Dr. Ramesh Kainthla and Dr. Dalibor Hodko. Dr. Sunderesen worked on it too but he also worked on the carbon to iron transmutation. All are to be duly thanked.

## **FIGURE**

Fig 1  $\beta$  Radiation from a preliminary run of April 21, 1992 (negligible nobles).

Fig 2  $\beta$  Radiation from later run, December 9, 1992, (negligible nobles).

Fig 3 Iron formed from arching spectroscopically pure carbon rods.

## **TABLES**

- (1) List of results obtained in Summer, >92.
- (2) Detailed results obtained in Thermal 2 by the South African National Institute of Mining.
- (3) New nuclei found in Palladium after three weeks electrolysis evolving H<sub>2</sub>.
- (4) Concentrations of Impurities.

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