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**SUMMARY OF RESULTS FOR ANOMALOUS PHENOMENA IN EXPERIMENTS
ON SOLID STATE REACTIONS ON NOBLE METAL CONTENT**

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and internal date markers

New Energy Times

(Work of J. Champion with Verificatory

Experiments by G. Lin, R. Bhardwaj and Z. Minevski)

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September 1991

Worker: Champion

Method: Thermal

Analysis: X-ray Analog (Analysis of peak variance). Certificate signed by I. Q. Alvaro Garcia Torres (Director)

Increase Detected: 3X for Pt 4x for Au

Absolute Amount in Final Mixture: About 170 ppm Au and 9.6 ppm Pt

Radioactivity: Noted

New Energy Times

TEXAS A&M UNIVERSITY (March 1992 - June 1993)

General Conditions: Radio frequency stimulation, radioactivity after ignition process and chemical and neutron activation analyses.

Workers: The lead worker was Dr. G. Lin, however, Dr. R. Bhardwaj asked to join the project in April 1992. This was encouraged by me because Bhardwaj is a chemist; Lin, a physicist. Zoran Minevski, a graduate student, assisted sometimes. At the beginning, I went into the lab, made mixtures and took samples.

Champion was asked to stay out of the lab during runs as the key point of the A&M test was to be independence. Although he took no physical part in the experiments, he was "around" - in his office, in the corridor and sometimes in the lab to give advice. (He claimed 5 years of intermittently working on the processes concerned.)

Frequent discussions between Bhardwaj, Lin and myself were aimed at thwarting possible intervention by Champion. This included taking samples home at night, etc.

Thermal I: April 30¹

analysis: Bondar Clegg in Ottawa, - metallurgical cupelling. Neutron Activation Analysis in TAMU's reactor (Martha Brown).

Increase? One out of six cupellings showed Au and Pt increase. Au \approx 5x. Ir: \approx 12. Absolute amounts final mixture 3 ppm Ir but in 6th cupelling, Au 148; Pt 167.

Thermal II: May 22, 1992. This was the most studied experiment and gave the most certain results because of the analysis of so many elements by so many methods.

¹Use of the cupel technique, a metallurgical method which involves a porous ceramic in such a way that other constituents are removed into the porous material and any noble metal lies on a surface.

This run is also special for another reason: the results were subject to the full force of MINTEK, the South African Government Lab where the analytical section has probably a greater degree of experience of noble metal analysis than any similar laboratory in the world. Ron Mallet, the Lab Chief, remarked to me in a telephone talk that "South Africa runs on the cupel." Although I did not request it, the South Africans used up to four different methods of analyses and sought seven metals. Later, I wished I had continued to ask them for assistance but it was always quicker to use the ICP machine on campus.

Analysis:

The general method used by the South Africans was ICP-MS but the "4 methods" refers to the pretreatment and extraction (see below).

Chemex used cupelling.

Bondar Clegg used cupelling.

Neutron Activation: Done by Martha Brown at A&M's reactor.

Increases Detected: (1 = Same)

	Ru	Rh	Pd	Ag	Ir	Pt	Au
South Africans (averaged)	1	<2	~10 ⁶	1	1	1	110
Chemex	-	-	~6	-	-	1	170
Bondar Clegg	-	-	50	-	-	30	700
Neutron Activation	-	-	-	-	-	-	~100*

*Assumes original had ~ 4 ppm.

Absolute Amounts: (ppm in final mix)

	Ru	Rh	Pd	Ag	Ir	Pt	Au
South Africans	<1	0.03-1.8	1-2.7	436	0	-	600-986
Chemex (Reno)	-	0.1	0.37-2.07	-	-	0.3	47.9
Bondar Clegg (Ottawa)	-	-	2758 (?)	-	-	160	910
Neutron Activation (TAMU)	-	-	-	-	-	-	408

The Pd result from Bondar Clegg is clearly in error. There is some rough consistency for the Rh. The Au average is 597 ± 400 . Thus, the internal South African results vary over nearly 400 ppm. Mallett did not seem to think this exceptional and wrote to stress the Au result.

Thermal III: Not much work done on this because ICP found no Ag and no Au increases.

However, in the cupel, the final bead was 3-4 times greater for mixtures which contain Hg than for those which do not (possible augmentation of other nuclei?)

Thermal IV: In the other experiments described to date the starting mixtures had been typified by those published by Champion. A typical mixture in his book runs: Carbon 300g; KNO₃ 900g; S 80g, SiO₂ 120g; FeSO₄ 100g; Cd 30g; Hg₂Cl₂ 100g; Pb₃O₄ 50 g; Ag 5 g and CaO 30g.

In Thermal IV, this was changed and an ore known to contain 4.34 ppm of Ag and 1.46 ppm of Au was used.

Analysis: ICP and visual

Increases? Visible Au bead from cupelling, 47.3 mg in weight.

Absolute Amounts: Referred to whole mixture. 340 ppm (1700 ppm with respect to ore). No other elements were sought.

Thermal V: June 7, 1992. This was an attempt to repeat Thermal II but only with Au analysis.

Analysis: Fire assay and ICP, Neutron Activation.

Increase Detected: Yes.

Absolute Amount Referred to Total Initial Powder: 178 ppm (ICP). No other metals were sought.

63.5 - 65.1 by Neutron Activation Analysis.

TAMU RESULTS FROM JUNE 8 TO 28 OCTOBER 1992

This period is reported in detail in the tabular summary in the parallel report given by Drs. Lin and Bhardwaj.

The very interesting and positive results were then followed by a series of 7 results which gave no (or very little) signs of anomalous metal production. Among points of interest: In Thermal VI, the effect of runs with and without (respectively) Hg was tried (No result).

During this period the Chemex Lab in Reno and the ICP at Texas A&M were those most used.

In Run 10 (July 12) a tiny Au increase to 0.3 ppm was seen.

Run 11 on 21 August 1992 was used for radiation experiments and not analyzed for Au (see below).

Tiny amounts of Au (0.1 ppm) were seen in some of these runs but on 6 January 1993 using Patel's formula (Mid State Recycling) 5.15 g of a silvery bead was recovered and yielded 0.45 mg of Au (perhaps significant).

No other elements were examined in this series.

WORK CARRIED OUT AT TAMU WITH R. A. MONTI (19-26 Feb. 1993)

Monti worked with Dr. Bhardwaj and carried out Experiments 23-27.

The details of these experiments are all given in the Lin-Bhardwaj report. The experiments failed to show Au. No other elements (or radiation) were sought.

RADIATION WORK CARRIED OUT AT TEXAS A&M

It was a general finding of the so-called "successful" results that a weak β radiation could be detected for ~ 50 hours after the firing.

Quantitative records of this were made on April 2, April 21, April 23, April 25 (No Hg) and April 27, 1992 (No Hg).

Several plots are shown in the report by Lin and Bhardwaj. The data are poor in quality. One log plot did give 18.3 hrs (cf Pt¹⁹⁷) and coincided with Champion's report of what Engelhardt had found.

The rest of the data was not inconsistent with the result but poorer. If Hg was removed the counts were less and the slope much less but this result is not definitive.

Prof. J. Natowitz suggested an attempt to measure γ activity and Dr. Lin worked with Henry Randolph from the DOE Savannah River Lab to attempt to establish whether there was much difficulty in obtaining a suitable γ counter. On August 21, 1992, an increase in counts from 30 cpm to 65 cpm was observed by Dr. Schmidt in the Cyclotron Lab but there was no γ spectrum and Lin thought the result insignificant.

On 27 October in Run 11b, Dr. Lin again tried with help from Savannah River. No result.

Very weak β radiation was again observed on 28 October 1992 and on November 3 ($\tau = 20-22$ hrs).

THE RADIO FREQUENCY ACTIVATION WORK AT TAMU:

Champion early writings, whereby he had introduced the Philadelphia Project, were oriented to the radio frequency activation and his idea that one could store tiny quantities and eventually bring about nuclear change. This, of course, would offend normal nuclear theory but R. Brightsen, with his α particle model of the nucleus is acceptive.

Ten experiments were carried out with the R. F. apparatus (for which the big panels full of expensive electronic equipment had been bought). Nothing convincing occurred. However, there are two entries in my notes which say that 100 μg of Ag (in another instant 100 μg of Au) resulted from irradiating Hg.

Were there impurities?

EXPERIMENTS MADE AT THE MIDSTATE RECYCLING COMPANY: (September 1, 1992)

Champion left Texas A&M in June 1992 (i.e., he was a guest worker for about 11 weeks).

He sometimes returned for brief periods and called asking (e.g.) questions about analysis and separation.

He worked at Mid State Recycling in Rosemont, IL for a few weeks in collaboration with R. A. Monti.

From this period there is a letter from Don Patel (who collaborated with Monti and Champion, Document 48). He reports a "button" or "bead" which had 1% Au and 0.02% Pd. Another button had much less. There is insufficient data to calculate the significance of this or to refer it to the ppm of original mixture scale used earlier.

EXPERIMENTS AT THE REAL DEL MONTE ORGANIZATION (A MINING HOUSE)

e.g. Nov. 17, 1992:

The results of one run were sent to me. I can in no way support the validity of the results done entirely outside my control. (Document 54)

Analysis: Classical metallurgical analysis with cupelling.

Increases Claimed:

Ag	Au	Pd
9	4	> 10

Absolute Concentrations Claimed (ppm):

	Ag	Pd	Au
Before	72	0	1.1
After Process	694	12.5	4.2

"Large Scale":

This experiment (according to the report involved 218 kg not 1-2 kg as earlier²)

If the claim is to be believed, 2-3 grams of Pd could have been produced.

PERIOD OF WORK AFTER CHAMPION'S INCARCERATION²

Champion's arrest² (Dec. 11, 1992) shortly after the rather good claim is used by him to support a conspiracy theory to the violent end of his work aimed at synthesizing expensive metals cheaply (cf. General Electric, Diamond Production).

Champion has continued without pause to turn out memoranda, and alleged experimental results!

How can a man awaiting trial do this? In the following statements I rely on Roger Briggs, a former associate of Telander's, who has befriended the incarcerated Champion. The essence is that Champion instructs technicians paid by Briggs to carry out runs. He also claims a new technique.

²He is accused of illicitly obtaining ~ \$80,000 in 1982. He has a credible story of semi-innocence.

I have told him that I shall reactivate an interest in his work where he shows me an ounce of a noble metal verifiably made synthetically.

CAN LOW LEVEL (~100PPM) TRANSMUTATION OF Pb AND Hg BE MADE TO OCCUR?

The results given can be summarized:

(1) When one carries out a Thermal Procedure (essentially, explosive heating to ~ 1000°C, 2 days cooling, etc.), various new materials are claimed to turn up at amounts which increase in the amounts originally present by 10-100 times and gives absolute concentration of 10-100 ppm.

In a detailed warning to the inexperienced Briggs, I have pointed out various pitfalls of this work, i.e., the possibility of concentrating small amounts into the final cupel and etc.

However, my present opinion is that the increases observed here were real. The analysis is just about $\pm 100\%$ (among the methods) but in view of the importance of the result if it is true, that should not diminish acceptance.

(2) The radioactivity has been seized upon by the scientists with whom I have discussed this work - as much more important than whether there is a noble metal synthesis.

Dr. Lin is dismissive of the radiation because a weak (but quantitatively different) radiation remains after removal of the Hg (Pb is present).

I myself (who have so often heard the Geiger counter switch on after an ignition and be silent before) think the balance is in favor of the radiation being a real effect. It was reported by Garcia before Champion came to A&M.

(3) The radio frequency approach does not work.

DISCUSSION

It goes without saying that the experiments, results of which are recorded here involve an unexpected hypothesis. It extends the proven work on nuclear reactions under solid state confinement where the production of T near the surface of deuterated Pd is now a commonplace result to the idea that ultra low energy nuclear changes are possible with heavier elements.

Let the disturbing factors which shield us from acceptance of the applicability of this idea be brought out.

(1) FEAR

The common image that research scientists live for the New, - is true so long as the new concerns the advancing frontier and not a revision of earlier principles. If changes in fundamentals is proposed, - experimental results which demand that there is a sense of unease among scientists. Really understanding what is happening, takes a long time. It is easier to suppose there must have been experimental error or even FRAUD.

For those who blithely cross the Barrier, ridicule and then odium will be applied.

Hence, there is a justified FEAR in putting out an idea which flies in the face of the ruling theory. It is a paralyzed fear, often, and leads to an attack with emotional intensity in the facts presented, hoping they will go away.

This has all been seen with the nuclear reactions involving D reaction dissolved in solids ("Cold Fusion"). Various hints of Champion's alleged findings have been available for 30-40 years.

So, any Discussion of these strange (and very uncertain) results must be done with the understanding that scientists may not behave with objective calm in considering them.

(2) Extraordinary Externals

When Mr. Telander gave his gift to Texas A&M (April 1992), and introduced Joseph Champion as an inventor who had something to show us, it seemed that Telander was a genial millionaire, an American Success Boy, and Champion an earnest and erudite technician with a record of dabbling in far-out experiments.³

One year later, Telander had been accused by the SEC of the misappropriation of investment funds, and Champion was in jail, on a dormant charge from an offense from 1986.

Of course, these said happenings had a devastating effect upon the project. Anyone who talks of a cheap route to noble metals is suspected of fraud anyway - and now the man who suggested it is in jail!

I respect greatly the scientists (Drs. Lin and Bhardwaj) who worked with me on this. But I do think that it has been difficult for them to remain untouched by Champion's arrest and the freezing of Telander's funds. One did not want to be a part of anything so daring and unconventional as ultra low energy nuclear change anyway - but now the central figures have been violently removed. This increases FEAR over that which comes anyway with intellectual courage.

(3) FRAUD

³It would have helped if Champion had avoided changes to noble metals and used what he later so often discussed, nuclear waste systems.

Whenever Champion was in the building, more than 50% of the experiments showed anomalous concentration changes but when he was completely removed, the results of finding new nuclei were radically less or even zero.

In the case in which the formation of tritium was discovered, the graduate student who did the work was accused by a journalist in Science Magazine of spiking his solution with tritiated water. How much easier, then, to suspect that these surprising results are due to slight of hand.

Secret Fraud is impossible to disprove. Perhaps that is the basis of the concentration changes detected. However:

(1) A topic of discussion between my coworkers and me was about the possibility of fraud. As we were presented with claims which seemed so unlikely, we naturally suspected it.

Champion (as far as we know) never handled the apparatus or equipment in the so-called successful runs.⁴

(2) The wealthy Telander would have rewarded Champion privately had he given him a worthy patent possibility. The concentration had to be orders of magnitude greater for practicality. Why did not Champion "put some more in" and get rewarded?

(3) Although we were so cautious once claims were made, we did not take any special care with Champion when he worked on his radio frequency activation. This failed.

(4) Why did the work go so badly after Champion left? I don't know. Some runs failed while Champion was around. Dr. Bhardwaj (a formal and conservative man) became in

⁴Russ George (see below) has brother who is Director of Research for Kennecott-Copper; which deals in Au extraction. He says that one might introduce a gel like suspension into the rim of the crucible. Possibly. And Pd and Ir, too?

turn critical of the main players; then skeptical of the idea and finally angry and negative so that, in an outburst of fury, he flung his notes on the floor in front of me.

Did this growing repudiation of Champion and Telander interfere with Dr. Bhardwaj's ability to carry out the procedures which went right for the first 8-10 weeks?

(5) Repetition is ^{the} only way to gain certainty. However, the situation has changed since March 1992. During this time a number of authors have reported results which although entirely different in execution from those of Champion, nevertheless indicate ultra low energy nuclear change in solids. Thus:

(i) At the Nagoya Meeting of October 1992, a number of rumors reported that radioactivity had been observed in Pd electrodes used in D_2 evolution from D_2O by K. Wolf; Texas A&M. P. Hagelstein described a visit to EPRI to examine these. T. Claytor (Los Alamos) described the γ ray spectroscopic analysis of Wolf's electrode and said that many new elements had formed therein, as a result of nuclear reactions of D with Pd. Tom Passel from EPRI, formerly Wolf's manager, confirmed this. Concentration $\sim 10^9$ - 10^{10} atoms/cc.

Wolf has been invited to present these results in the Hawaiian Conference. As of this writing, it appears he does not wish to present them as yet.

(ii) Y. Kucherov (with co-authors Karabut and Sammitova) have put out a number of preprints concerning their work in a Moscow Institute. They detect many new nuclei formed in Pd as a result of spark discharge between two Pd rods in D_2 gas. The total concentration is 0.1%.

Some of these results (or part of them) have recently been reproduced by Smullen at MIT in collaboration with Hagelstein.

(iii) Bush and Eagleton have reported that in electrolysis from aqueous solutions containing RbOH, Sr has formed. The MS analysis shows the isotopic ratios to differ from those of natural Sr.

(iv) Stringham, working privately in Palo Alto with sono-luminescent irradiation of Pd in D₂O, has found that not only He⁴ but also several new nuclei form in the Pd, with heat evolution.

Although these experiments in no way replicate those of Champion, yet they all lead to a similar conclusion: that nuclear changes in solids can be made to occur at very low concentrations. Thus, indirect support for Champion's theme is given.

Max date indicator

The Russian work, among others, is to be presented in Maui December 6. Kucherov et al. will propose that reactions between D and Pd are the origin of the heat in what was formerly called "Cold Fusion." Champion has been suggesting this since 1989.