

The Production Of Helium In Cold Fusion Experiments

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ABSTRACT

It is now known that cold fusion effects are produced only by certain palladium materials made under special conditions. Most palladium materials will never produce any excess heat, and no helium production will be observed. The palladium used in our first six months of cold fusion experiments in 1989 at the China Lake Navy laboratory never produced any measurable cold fusion effects. Therefore, our first China Lake result were listed with CalTech, MIT, Harwell and other groups reporting no excess heat effects in the DOE-ERAB report issued in November 1989. However, later research using special palladium made by Johnson-Matthey produced excess heat in every China Lake D₂O-LiOD electrolysis experiment. Further experiments showed a correlation of the excess heat with helium-4 production. Two additional sets of experiments over several years at China Lake verified these measurements. This correlation of excess heat and helium-4 production has now been verified by cold fusion studies at several other laboratories. Theoretical calculations show that the amounts of helium-4 appearing in the electrolysis gas stream are in the parts-per-billion (ppb) range. The experimental amounts of helium-4 in our experiments show agreement with the theoretical amounts. The helium-4 detection limit of 1 ppm (1000 ppb) reported by CalTech and MIT was far too insensitive for such measurements. Very large excess powers leading to the boiling of the electrolyte would be required in electrochemical cold fusion experiments to even reach the CalTech or MIT helium-4 detection limit of 1000 ppb helium-4 in the electrolysis gas stream.

Introduction

My research on cold fusion at the China Lake Navy laboratory (Naval Air Warfare Center Weapons Division, NAWCWD) began on the first weekend following the announcement on March 23, 1989 by Martin Fleischman and Stanley Pons. It was six months later (September 1989) before our group detected any sign of excess heat production. By then, research reports from CalTech, MIT, and Harwell had given cold fusion a triple whammy of rejection. Scientists often resorted to ridicule to discredit cold fusion, and some were even saying that Fleischmann and Pons had committed scientific fraud.

Most palladium sources do not produce any cold fusion effects [1]. The palladium made by Johnson-Matthey (J-M) under special conditions specified by Fleischmann was not made available until later in 1989. I was likely one of the first recipients of this special palladium material when I received my order from Johnson-Matthey of a 6 mm diameter palladium rod in September of 1989. Our first reports of excess heat came from repeated use of the same two sections of this J-M palladium rod [1-3]. However, our final verification of these excess heat results came late in 1989, thus China Lake was listed with CalTech, MIT, Harwell and other groups reporting no excess heat effects in the November 1989 DOE-ERAB report [4].

These same two J-M Pd rods were later used in our first set of experiments (1990) showing helium-4 production correlated with our excess heat (enthalpy) results [5-7]. Two later

sets of experiments at China Lake using more accurate helium measurements, including the use of metal flasks for gas samples, confirmed our first set of measurements [8]. Following our initial research in 1990-1991 on correlated heat and helium-4 production, other cold fusion research groups reported evidence for helium-4 production [9]. This report, however, will focus mainly on the research of the author at NAWCWD in China Lake, California during the years 1990 to 1995 [1,8].

1. First Set of Heat Helium Measurements (1990)

The proponents of cold fusion were being largely drowned out by cold fusion critics by 1990. In fact, the first International Cold Fusion Conference (ICCF-1) was held March 28-31, 1990 in Salt Lake City, Utah. I found this to be a very unusual scientific conference with a mix of cold fusion proponents, many critics, and the press. Most presentations were followed by unusual ridicule by critics in the question period with comments such as “All this sounds like something from Alice in Wonderland”. Two valid questions by critics, however, were: “Where are the Neutrons?” and “Where is the Ash?”. If the cold fusion reactions were the same as hot fusion reactions, as most critics erroneously thought, then the amounts of excess power being reported (0.1 to 5 W) would have produced a deadly number of neutrons (more than 10^{10} neutrons per second). Also, if there were a fusion reaction in the palladium-deuterium (Pd-D) system, then there should appear a fusion product – sometimes incorrectly referred to as ash. Some researchers, such as Bockris and Storms, were reporting tritium as a product, but the amounts were far too small to explain the excess enthalpy. The reported production of neutrons in cold fusion experiments was even smaller (about 10^{-7} of the tritium).

Julian Schwinger, a Nobel laureate, suggested at ICCF-1 the possibility of a D+H fusion reaction that produces only helium-3 as a product and no neutrons [10]. Because of this, I considered measurements for helium-3 in my next experiments, but the mass spectrometer at China Lake was designed for only larger molecules made by organic chemists. However, later in 1990, Ben Bush called to discuss both a possible temporary position at China Lake and my cold fusion results. He held a temporary position at the University of Texas in Austin, and the instrument there could measure helium-3 at small quantities. We worked out details in following telephone conversations about how to collect gas samples and ship them to Texas for both helium-3 and helium-4 measurements by their mass spectrometry expert. My next two experiments, fortunately, produced unusually large excess power effects for our first set of correlated heat and helium measurements [5-7].

These helium results were first published as a preliminary note [5], then in the ICCF-2 Proceedings [6], and eventually as a detailed publication [7]. There was no detectable helium-3, but there was evidence for helium-4 correlated with the excess enthalpy. I had never met Ben Bush and decided to code the gas samples with the birthdays of my family members. My own measurements of excess power were recorded in permanent laboratory notebooks before the samples were sent to Texas for analysis. These were single blind

tests because Dr. Bush did not know how much, if any, excess power was being produced when a gas sample was collected. I am glad, in retrospect, that this was done because I later learned that Dr. Bush was gung-ho on proving cold fusion was correct. Scientists must always leave it completely up to experimental results to answer important scientific questions. It seems to me, on the other hand, that scientists at MIT and CalTech in 1989 were focused only on proving that cold fusion was wrong. There was a “Wake for Cold Fusion” held at MIT at 4 p.m. on June 16, 1989¹ even before their cold fusion experiments were completed [11].

When all results for this study were in (early 1991), I thought about how this research could be published quickly as a preliminary note. All research, except for the helium measurements, was done at China Lake. However, critics of cold fusion were prominent in 1991, and any publication from China Lake had to be first cleared by several management levels. This publication could be held up or even rejected for publication by Navy personnel at China Lake. As a solution, I had this manuscript submitted by Bush and Lagowski at the University of Texas where they were listed as the first authors. A few months later, Dr. Ronald L. Derr, Head of the Research Department at China Lake, admonished me for the publication of this work from China Lake in this manner. However, Dr. Derr, along with my Branch Head, Dr. Richard A. Hollins, were among the few supporters of my cold fusion research at NAWCWD in 1991. Many others thought that such work damaged the reputation of this Navy laboratory.

2. Analysis of the First Set of Helium Measurements.

Neither Ben Bush nor I really knew how much helium should be produced in my experiments by a fusion reaction, but my quick calculations showed that it might be quite small because of its dilution by the electrolysis gases. Recently, I have found an easier and accurate method to calculate the amount of helium-4 theoretically expected from the experimental measurements of excess power. It is known that D+D fusion to form helium-4 produces 2.6173712×10^{11} helium-4 atoms per second per watt of excess power. This is based on the fact that each D+D fusion event produces 23.846478 MeV of energy per helium atom from Einstein’s $E = \Delta mc^2$ equation. Multiplying the number of atoms per second per watt by the experimental excess power in watts gives the rate of helium-4 production in atoms per second.

The rate of electrolysis gases produced (D_2+O_2) per second is given by

$$\text{Molecules/s} = (0.75 I/F) N_A \tag{1}$$

where I is the cell current in Amps, F is the Faraday constant, and N_A is Avogadro’s

¹The flyer for this “Wake” at MIT ridiculed cold fusion with statements like “Black Armbands Optional” and “Sponsored by the Center for Contrived Fantasies”.

number. Note that the electrolysis reaction for one Faraday written as $0.5 \text{ D}_2\text{O} \rightarrow 0.5 \text{ D}_2 + 0.25 \text{ O}_2$ produces 0.75 moles of $\text{D}_2 + \text{O}_2$ gases. The largest excess power in the first set of helium-4 measurements was 0.52 W at a cell current of 0.660 A. Therefore, the theoretical rate of helium-4 production divided by the rate of the $\text{D}_2 + \text{O}_2$ molecules produced by the electrolysis gives a ratio (R) for helium-4 atoms to $\text{D}_2 + \text{O}_2$ molecules as shown by Equation 2.

$$R = \frac{(2.617 \times 10^{11} \text{ He-4 atoms/s W})(0.52 \text{ W})}{[(0.75)(0.660 \text{ A})/(96,485 \text{ A.s/mol})] (6.022 \times 10^{23} \text{ D}_2 + \text{O}_2 \text{ molecules/mol})} \quad (2)$$

This calculation yields $R = 44.0 \times 10^{-9}$ or 44.0 parts per billion (ppb) of helium-4 atoms. This is the theoretical concentration of helium-4 present in the electrolysis gases for this experiment if no helium-4 remains trapped in the palladium. Normally, about half of this theoretical amounts of helium-4 is experimentally measured in the electrolysis gas.

The first set (1990) of our China Lake results are shown in Table 1. The theoretical amount of helium-4 expected (ppb) based on the measured excess power and the cell current is also listed. This is compared with the 1990 mass spectrometry results from the University of Texas in terms of large, medium, small or no observed helium-4 peaks. The dates for the gas sample collections are also listed. Two similar calorimeters (A,B) were run simultaneously, in series, in the same water bath controlled to $\pm 0.01^\circ\text{C}$ [1-3].

Table 1. Results for the 1990 China Lake Experiments.

Sample	$P_x(\text{W})$	Theoretical He-4 (ppb) ^c	Measured He-4
12/14/90-A	0.52 ^a	44.0	Large Peak
10/21/90-B	0.46	48.7	Large Peak
12/17/90-A	0.40	42.4	Medium Peak
11/25/90-B	0.36	38.1	Large Peak
11/20/90-A	0.24	25.4	Medium Peak
11/27/90-A	0.22	23.3	Large Peak
10/30/90-B	0.17	18.0	Small Peak
10/30/90-A	0.14	14.8	Small Peak
10/17/90-A	0.07	7.4	No Peak
12/17//90-B	0.29 ^b	30.7 ^b	No Peak

^aI = 0.660 A. For all others I = 0.528 A

^bCalorimetric Error Due to Low D_2O Solution Level

^cThe University of Texas Detection Limit was about 5 ppb He-4 Based on Table 1

The theoretical helium-4 amounts generally follow the peak size reported experimentally for helium-4 except for the one sample where there was an apparent calorimetric error. Also, theoretical amounts of helium-4 vary only by a factor of three between the large and small peaks. Previous estimates [6-8] of the number of helium-4 atoms in these flasks were in error because the rate of helium production is directly proportional to the excess power. Finally, the detection limit for helium-4 measured at the University of Texas was about 5 ppb based on Table 1. This is in line with the ± 1.1 ppb experimental error reported later by the U.S. Bureau of Mines laboratory in Amarillo, Texas [8]. The rate for atmospheric helium diffusing into these glass flasks was later measured to be 0.18 ppb/day, thus 28 days of flask storage would be needed to reach the 5 ppb detection limit. No correlation was found for the helium-4 amounts and the flask storage times [6,7]. Six control experiments using the same glass flasks and H₂O+LiOH electrolysis produced no excess enthalpy at China Lake and no helium-4 was measured at the University of Texas [5-8].

Secondary experiments were also conducted for these heat-production cells. Dental films within the calorimeter was used to test for any ionizing radiation, and gold and indium foils were used to test for any activation due to neutrons. These dental films were clearly exposed by radiation in both calorimetric cells A and B [6,7]. A nearby Geiger counter also recorded unusually high activity during this time period. No activation of the gold or indium foils were observed, hence the average neutron flux was estimated to be less than 10^5 neutron per second. Similar dental film studies in the H₂O+LiOH controls gave no film exposure and no other indications of radiation [6,7].

3. Experimental Measurement of Helium-4 Diffusion

One of the main questions raised by our first report in 1991 of the correlation between the excess heat and helium-4 production in our experiments [5-7] was the possible diffusion of helium-4 from the atmosphere into our glass collection flasks. This was certainly possible, but would the rate of such diffusion be fast enough to affect our results? I addressed this question in my presentation at ICCF-2 in Como, Italy where I suggested that since D₂ also diffuses through glass, then the much greater outward diffusion of deuterium gas across the flask surface in the opposite direction might impede the small flow of atmospheric helium-4 into the flask. Experimental measurements of the rate of helium diffusion into these same glass flasks later answered these important questions. The rate of atmospheric helium-4 flowing into our glass flasks was too slow to have affected our first report on the heat/helium-4 correlations. These experiments also showed that large amounts of hydrogen or deuterium in the flask somewhat slow the rate of helium diffusion into the flask. Theoretical calculations using $q = KP/d$ gave good agreement with the experiment measurements [1,5-7] where q is the permeation rate, K is the permeability for Pyrex Glass, P is the partial pressure of atmospheric helium-4 and d is the glass thickness ($d = 0.18$ cm and $A = 314$ cm² for our typical glass flask).

The results for eight experimental measurements of the helium-4 diffusion rate into the same glass flasks used in our experiments are presented in Table 2.

Table 2. Experimental Measurements of Helium-4 Diffusion into the Glass Flasks used at China Lake

Conditions	Laboratory ^a	He-4 Atoms/Day	Ppb/Day ^b
Theoretical	$q=KP/d$	2.6×10^{12}	0.23
N ₂ Fill	HFO	2.6×10^{12}	0.23
N ₂ Fill	HFO	3.4×10^{12}	0.30
N ₂ Fill	RI	3.7×10^{12}	0.32
D ₂ O+O ₂ Fill ^c	RI	$1.82 \pm 0.01 \times 10^{12}$	0.160
D ₂ +O ₂ Fill ^d	RI	$2.10 \pm 0.02 \times 10^{12}$	0.184
D ₂ +O ₂ Fill ^e	RI	$2.31 \pm 0.01 \times 10^{12}$	0.202
H ₂ Fill ^f	RI	$1.51 \pm 0.11 \times 10^{12}$	0.132
Vacuum ^f	RI	$2.09 \pm 0.04 \times 10^{12}$	0.183

^aHFO (Helium Field Operations, Amarillo, Texas)

RI (Rockwell International, Canoga Park, California)

^bBased on 1.141×10^{22} D₂+O₂ Molecules per Flask

^cGlass Flask #5

^dGlass Flask #3

^eGlass Flask #4

^fBoth Experiments Used Glass Flask #2

For our experimental condition of flasks filled with D₂+O₂, the mean helium-4 diffusion rate is 0.182 ± 0.021 ppb/day. Thus, it would take a flask storage time of 28 days to just reach the helium-4 detection limit of about 5 ppb (see Table 1). The theoretical 44.0 ppb in Table 1 would require a flask storage time of 242 days to reach this amount of helium-4. Because of the large excess power measured, the flask storage time was not a factor for the results in Table 1. Also, the flasks filled with N₂ had larger experimental rates for helium-4 diffusion than the flasks filled with the D₂+O₂ electrolysis gases. The various flasks had somewhat different values for helium-4 diffusion because it was unlikely that any two flasks would be exactly the same. Furthermore, filament tape was used on each Pyrex round-bottom flask to help prevent breakage during shipments. However, the measured helium-4 diffusion using the same glass flask in Table 2 for both a H₂ fill and a vacuum show a significant slower diffusion rate for helium-4 for the flask filled with hydrogen [7]. The outward diffusion of D₂ or H₂ across the glass surface apparently does slow the inward diffusion of atmospheric helium-4.

4. Second set of Helium Measurements (1991-1992)

Unfortunately, our 6 mm diameter palladium rods from Johnson-Matthey were cut up for helium-4 analysis, and it took nearly a year to find another palladium electrode that produced excess heat². This was a 1.0 mm diameter J-M wire, and the excess power was small due to the much smaller palladium volume used (0.020 cm³ vs. 0.34 cm³). However, Rockwell International provided significantly more accurate helium-4 measurement with a reported error of only ± 0.09 ppb [1,8]. Brian Oliver, who performed these studies, was recognized as a world expert for helium-4 measurements. The helium-4 measurements were carried out over a period of more than 100 days, thus the helium-4 results could be accurately extrapolated back to the time of the gas samples collection [8]. This eliminated any effect due to the diffusion of atmospheric helium-4 into the glass flasks. These were double blind experiments because neither Rockwell International nor the China Lake laboratory knew the results for both the excess power and helium measurements until this study was completed and all results were reported to a third party.

The experimental and theoretical results of this set of experiments in 1991-1992 are presented in Table 3.

Table 3. Results for the Second Set of Experiments (1991-1992)

Sample	P _x (W)	Theoretical He-4 (ppb)	Experimental He-4 (ppb) ^c
12/30/91-B	0.100 ^a	10.65	11.74
12/30/91-A	0.050 ^a	5.33	9.20
01/03/92-B	0.020 ^b	2.24	8.50

^aI = 0.525 A

^bI = 0.500 A

^cReported Rockwell error was equivalent to ± 0.09 ppb

There is considerable information contained in this accurate helium-4 analysis by Rockwell International that supports a D+D fusion reaction producing helium-4 and 23.85 MeV of energy per helium-4 atom. First, Rockwell reported their results as the measured number of helium-4 atoms in each of the 500 mL collection flasks at the time of collection. These numbers were 1.34×10^{14} , 1.05×10^{14} , and 0.97×10^{14} helium atoms per 500 mL [8,12]. The reported error (standard deviation) by Rockwell was only $\pm 0.01 \times 10^{14}$ helium-4 per 500 mL. Therefore, there is a 29 σ effect between the two highest numbers and a 37 σ effect between the highest and lowest numbers. Except perhaps for the cold fusion field, any measurements that produce even 5 σ effects are considered to be very significant by the scientific community. Note that the numbers reported by Rockwell are also in the correct order for the excess power measured (Table 2) for this double-blind experiment.

²If one finds palladium electrodes that produce large excess power effects, hang onto them! Also, do not use them for H₂O controls.

The number of helium-4 atoms per 500 mL can be converted to ppb, as used in Table 3, by calculating the total number of gas molecules contained in the flask. From the Ideal Gas Equation, this number is $(PV/RT)N_A$ or 1.141×10^{22} molecules for our laboratory condition during the flask collection time ($P=0.92105$ atm, $V=0.500$ L, and $T=296.15$ K). In terms of ppb, the Rockwell reported error of $\pm 0.01 \times 10^{14}$ helium-4 atoms per 500 mL becomes about ± 0.09 ppb.

Later experiments using metal collection flasks established that the background helium-4 in our collection system was 5.1×10^{13} atoms per 500 mL or 4.5 ppb [1,8]. Based on theoretical calculations, the diffusion of helium-4 into our collection system was not due to any glass components, but rather due to the use of thick rubber vacuum tubing to make the connections to the collection flask and oil bubbler. We kept our calorimetric system and gas collection system at China Lake exactly the same for several years for the purpose of making comparisons between experiments done at different times. The correction for this background helium-4 actually helped to bring the Rockwell helium-4 measurements closer to theoretical values based on the D+D fusion reaction to form helium-4. This is shown in Table 4.

Table 4. Results For the Second Set of Experiments With Corrections For the Background Helium-4 (4.5 ppb)

P_X (W)	Theoretical He-4 (ppb)	Corrected He-4 (ppb)	He-4/sW ^c	MeV/He-4 ^d
0.100 ^a	10.65	7.24	1.8×10^{11}	35
0.050 ^a	5.33	4.70	2.3×10^{11}	27
0.020 ^b	2.24	4.00	4.7×10^{11}	13

^aI = 0.525 A

^bI = 0.500 A

^cTheoretical Value: 2.617×10^{11} He-4/sW

^dTheoretical Value: 23.85 MeV/He-4

The corrected helium-4 measurements by Rockwell are reasonably close to expected values based on the D+D fusion reaction to form helium-4 as the main product. Only the results for an excess power of 0.020 W suggests a problem because the corrected experimental value (4.00 ppb He-4) is larger than the theoretical value (2.24 ppb He-4). This is not unexpected because 0.020 W is near the measuring limit for the calorimeter used. The correct experimental excess power may have been closer to 0.040 W³. Also, the rate of work done by the generated electrolysis gases (P_w) was not considered. This alone would add another 0.010 W to give 0.030 W for the excess power. This small P_w term is less important for higher excess power measurements.

³Using 0.040 W gives 2.4×10^{11} He-4/sW and 25 MeV/He-4.

An example of the experimental calculation of He-atoms per Ws (or J) is presented in Equation 3 for the measured excess power of 0.100 W (I = 0.525 A).

$$\frac{(1.34 \times 10^{14} - 0.51 \times 10^{14}) \text{ He atoms/500 mL}}{(4644 \text{ s/500 mL})(0.100 \text{ W})} = 1.8 \times 10^{11} \text{ He atoms/J} \quad (3)$$

where 4644 seconds is the time required to generate 500 mL of D₂+O₂ electrolysis gases at a cell current of 0.525 A.

The value for MeV per helium-4 atom readily follows as shown by Equation 4.

$$[(1.8 \times 10^{11} \text{ He-4/J})(1.602 \times 10^{-19} \text{ J/eV})]^{-1} = 35 \text{ MeV/He-4} \quad (4)$$

A mean value for the three experiments in Table 3 yields 25±11 MeV/He-4. Omitting the smallest excess power measured gives 30.5±5.0 MeV/He-4. The results given in Table 3 are reasonable considering the rather small excess power measured. This was probably due to the small volume of the palladium electrode (0.020 cm³). Typical excess power for the Pd/D system is about 1.0 W/cm³ of palladium for our current densities used [13]. The experimental corrected values for helium-4 compared to the theoretical amounts in Table 3 are 68% and 88% for the two largest values for excess power. There would likely be a smaller percent of helium-4 trapped in the palladium for the two small volume cathodes used.

5. An Analysis of the Third Set of Helium Measurements (1993-1994)

Many cold fusion critics refused to accept the correlation of excess heat and helium-4 production in our experiments because of the diffusion of atmospheric helium into glass containers. Therefore, metal flasks were used in place of glass flasks to collect gas samples from our experiments for helium analysis. The use of these metal flasks prevented the diffusion of atmospheric helium into the flasks after they were sealed. Even the flasks valves were modified to provide a metal seal by using a nickel gasket. All other components of the cells, gas lines, and oil bubblers remained the same in order to relate these new measurements to the previous measurements using glass flasks [1]. However, it was difficult to get the large excess power effects observed in our first set of measurements that used the special 6 mm J-M palladium rods. The helium-4 analyses for these experiments using the new metal flasks were performed by the U.S. Bureau of Mines laboratory at Amarillo, Texas. This was another laboratory with special skills in making such measurements. By this time, we were using four similar calorimeters (A,B,C,D) in two different water baths for calorimetric studies.

Table 5 presents helium-4 results for seven experiments that produced small excess power effects. The theoretical calculated amounts expected for helium-4 are also presented.

Measurements in similar experiments where no excess power was measured gave a background level of 4.5 ± 0.5 ppb (5.1×10^{13} He-4 atoms) for our system [1].

Table 5. Helium-4 Measurements Using Metal Flasks

Flask/Cell (Date)	P _x (W)	Theoretical He-4 (ppb)	Experimental He-4 (ppb)
3/B (9/13/94)	0.120 ^a	13.4	9.4±1.8
2/A (9/13/94)	0.070 ^a	7.8	7.9±1.7
2/D (5/30/93)	0.060	8.4	6.7±1.1
3/A (5/31/93)	0.055	7.7	9.0±1.1
4/B (5/21/93)	0.040	5.6	9.7±1.1
1/C (5/30/93)	0.040	5.6	7.4±1.1
1/A (7/7/93)	0.30 ^a	3.4	5.4±1.5

^aI = 0.500 A. For all others I = 0.400 A

It should be noted that the largest excess power in Table 4 (0.120 W) was for a palladium-boron rod (0.6 x 2.0 cm) made by Dr. Imam at the Naval Research Laboratory (NRL). We had been testing palladium materials made by NRL for several years, but none had produced a significant excess enthalpy effect. However, seven of eight experiments using Pd-B rods from NRL produced significant excess heat effects before this Navy program on palladium-deuterium systems ended in June of 1995 [1]. Most of the other excess power effects reported in Table 5 were produced by J-M palladium materials. Five experimental values for helium-4 in Table 5 are larger than the theoretical values reported. Assuming that the excess power reported is correct, then this is readily explained by the need to subtract the background of 4.5 ppb from each experimental value. These results are shown in Table 6 along with the electrode volume and the experimental rate of helium-4 production per second per watt of excess power.

Table 6. Background corrections For Helium-4 Measurements Using Meal Flasks

P _x (W)	Corrected He-4 (ppb) ^a	Percent of Theoretical %	Electrode Volume (cm ³)	Helium-4 S.W
0.120	4.9	37	0.57	1.0 x 10 ¹¹
0.070	3.4	43	0.63	1.1 x 10 ¹¹
0.060	2.2	26	0.04	0.7 x 10 ¹¹
0.055	4.5	59	0.51	1.5 x 10 ¹¹
0.040	5.2	93	0.02	2.4 x 10 ¹¹
0.040	2.9	52	0.01	1.4 x 10 ¹¹
0.030	0.9	27	0.29	0.7 x 10 ¹¹

^a4.5 ppb subtracted from reported He-4 measurements

Because of the small amounts of excess power reported in Tables 5 and 6, it is difficult to reach any strong conclusions from the use of metal flasks except that helium-4 production is observed in experiments that produce excess power and no helium-4 production above background is measurable in experiments with no excess power. Furthermore, both the uncorrected and corrected experimental amounts of helium-4 are close to the theoretical amounts expected. Larger excess power, such as in our first set of helium-4 measurements would be needed before more definite conclusions could be made. Perhaps these results suggest that a larger percent of helium-4 is released into the gas phase for the palladium cathodes that have the smaller volume of material.

6. Discussion of China Lake Heat/Helium-4 Results

Some critics claimed that our results must be wrong because the experimentally measured helium-4 is only in the ppb range. However, this manuscript shows that the theoretical amounts of helium-4 for our experiments should be in this ppb range.

Many other critics attribute our heat and helium-4 results to some form of contamination from atmospheric helium-4 normally present in air at 5.22 ppm [12]. Such contamination sources would be random and equally likely to be found in controls or experiments which show no excess enthalpy results. In summary, for all such experiments conducted at NAWCWD (China Lake), 12 out of 12 produced no excess helium-4 when no excess heat was measured and 18 out of 21 experiments gave a correlation between the measurements of excess heat and helium-4. The three failures either had a calorimetric error or involved the use of a different palladium material, i.e. a palladium-cerium alloy that perhaps traps most of the helium-4 produced. An exact statistical treatment that includes all experiments shows that the probability is only one in 750,000 that the China Lake set of heat and helium measurements (33 experiments) could be this well correlated due to random experimental errors [1]. Furthermore, the rate of helium-4 production was always in the appropriate range of 10¹⁰ to 10¹² atoms per second per watt of excess power for D+D fusion or other likely nuclear fusion reactions that produce helium-4 [1,8].

All of our theoretical calculations for helium-4 production have assumed that the main fusion reaction is $D + D \rightarrow He-4 + 23.8 \text{ MeV}$. However, other fusion reactions producing helium-4 could also be considered such as $D + Li-6 \rightarrow 2 (He-4) + 22.4 \text{ MeV}$ or $D + B-10 \rightarrow 3 (He-4) + 17.9 \text{ MeV}$. Neither of these two possible reactions seem to fit well with our experimental measurements. Both reactions lead to large increases in the theoretical amounts of helium-4 for each experimental measurement of excess power. For example, the $D + B-10$ reaction would increase the theoretical amount of helium-4 by a factor of 3.991. In Table 3, the theoretical amount of helium-4 corresponding to $P_X = 0.100 \text{ W}$ would be 42.50 ppb rather than 10.65 ppb. For likely fusion reactions that produce helium-4, the $D + D$ reaction seems to fit best with our experimental results. Other proposed fusion reactions produce less than 23.8 MeV of energy per helium-4 atom.

At about the same time period of our first heat and helium measurements in 1990, two different theories were proposed that predicted helium-4 as the main cold fusion product and that this helium-4 would be found mostly outside the metal lattice in the electrolysis gas stream. These two independent theories came from Scott and Talbot Chubb [14] and Giuliano Preparata [15]. Both Scott Chubb and Preparata called me shortly after our first publication on correlated excess heat and helium-4 in 1991, and Preparata soon made a visit to my China Lake laboratory. I first met Scott and his uncle, Talbot Chubb, at ICCF-2 in Como, Italy, and our friendship lasted many years. Some of the most boisterous ICCF moments involved loud debates between Scott Chubb and Preparata over their two theories.

7. Related Research By Other Laboratories

There are presently more than fifteen cold fusion groups that have identified helium-4 production in their experiments. A summary for these groups reporting helium-4 has been reported elsewhere by Storms [16]. Publications by Bockris [17], Gozzi [18] and McKubre [19] relate closely to our electrochemical cold fusion studies at China Lake. McKubre and coworkers at SRI report on several different experiments using three different calorimetric methods that gave a strong time correlation between the rates of heat and helium production [19]. Using sealed cells, the helium-4 concentration exceeded that of the room air. These SRI experiments gave a near-quantitative correlation between heat and helium-4 production consistent with the fusion reaction $D + D \rightarrow He-4 + 24 \text{ MeV}$ (lattice). Special methods were used by SRI to remove sequestered helium-4 from the palladium cathode [19]

8. The CalTech and MIT Helium-4 Experiments in 1989

Both CalTech and MIT looked for helium-4 production in the electrolysis gases in their 1989 experiments and reported that there was none [20,21]. However, both institutions also reported that they found no excess enthalpy. We have never observed any helium-4 production in our experiments when there was no measurable excess heat. There were actually some signs of small excess heat in both the CalTech and MIT experiments, but

these were zeroed out either by changing the cell constant or by shifting experimental data points [22,23]. Major calorimetric errors were also present in the Cal Tech and MIT publications [22,23]. Nevertheless, the reported helium-4 detection limit by both CalTech and MIT was one part per million (ppm) or 1000 ppb. By using Equations 1 with $R = 1000$ ppb (1.0×10^{-6}), the excess power would have to be 8.94 W. From Table 1, 1000 ppb helium-4 would require more than 20 times the highest excess power listed for our experiments or about 10 W. With such a large excess power, most calorimetric cells would be driven to boiling just by the fusion energy alone. Such large amounts of excess enthalpy would be very obvious even without the use of calorimetry, but the amounts of helium-4 produced would barely reach the detection limit reported by these two prestigious universities. Why was such a glaring error in the CalTech and MIT results missed by the reviewers for these publications? It seems like almost anything was accepted by major journals, such as Nature and Science, in 1989 if it helped to establish the desired conclusion that reports of cold fusion were not correct.

Acknowledgements

Long term support for my cold fusion research has been received from an anonymous fund at the Denver Foundation through the Dixie Foundation at Dixie State University. An Adjunct faculty position at the University of Laverne and a Visiting Professor at Dixie State University are also acknowledged.

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