# Unanswered Questions About SRI Experiment M4

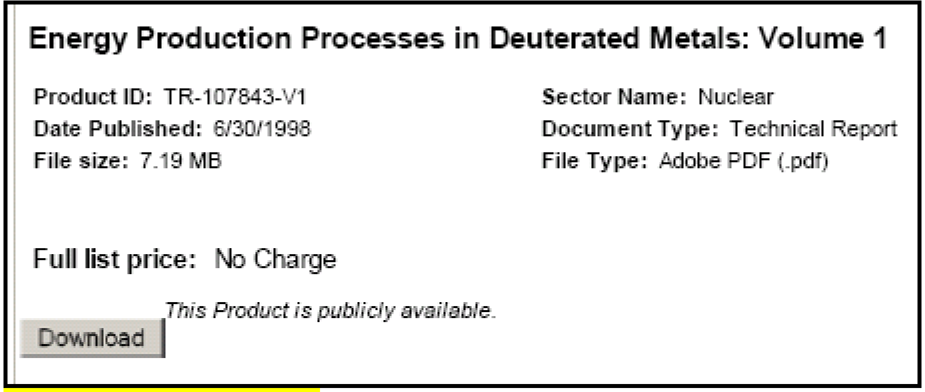
by Steve B. Krivit
March 23, 2010

This is a reference document that follows the Jan. 29, 2010, New Energy Times Issue 34.

The following papers are cited in this document:

**Reference 1**
Development of Energy Production Systems from Heat Produced in Deuterated Metals - Energy Production Processes in Deuterated Metals, Volume 1, TR-107843-V1, Thomas Passell (Project Manager), Michael McKubre, Steven Crouch-Baker, A. Hauser, N. Jevtic, S.I. Smedley, Francis Tanzella, M. Williams, S. Wing (Principal Investigators), B. Bush, F. McMohon, M. Srinivasan, A. Wark, D. Warren (Non-SRI Contributors,) June 1998

EPRI normally makes such reports available free to their members and, I believe, at $2,500 to the public. I do not know when EPRI made this report available free, but here is a screen shot of its current listing:



**Energy Production Processes in Deuterated Metals: Volume 1**

**Product ID:** TR-107843-V1
**Date Published:** 6/30/1998
**File size:** 7.19 MB

**Sector Name:** Nuclear
**Document Type:** Technical Report
**File Type:** Adobe PDF (.pdf)

**Full list price:** No Charge

*This Product is publicly available.*

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**Reference 2**
Michael McKubre, Francis Tanzella, Paolo Tripodi and Peter Hagelstein, "The Emergence of a Coherent Explanation for Anomalies Observed in D/Pd and H/Pd Systems; Evidence for 4He and 3He Production." 8th International Conference on Cold Fusion. 2000. Lerici (La Spezia), Italy: Italian Physical Society, Bologna, Italy.

I do not have a printed version of the ICCF-8 proceedings; however, I have no reason to believe that the version at the LENR-CANR library is materially different from the published version.

**Reference 3**
Peter Hagelstein, Michael McKubre, David Nagel, Talbot Chubb, Randy Hekman, "New Physical Effects In Metal Deuterides."

This is the paper submitted to the U.S. Department of Energy, intended to be a summary paper of LENR for the 2004 U.S. Department of Energy LENR Review. The paper has been widely distributed and may have been available on the DoE Web site at one time.

**Theory Background:**
Ref. 1 uses a theoretical prediction of D+D --> 4He + 22.4 MeV (lattice)
Ref. 2 uses a theoretical prediction of D+D --> 4He + 23.82 MeV (heat)
Ref. 3 uses a theoretical prediction of D+D --> 4He + 23.82 MeV (heat)

For the sake of this discussion, I consider the numerical difference between the heat values insignificant.

**Helium Samples/Measurements:**
M4 took place between July and October 1994. During these months, four separate helium samples were taken, and their concentration in ppm was measured.

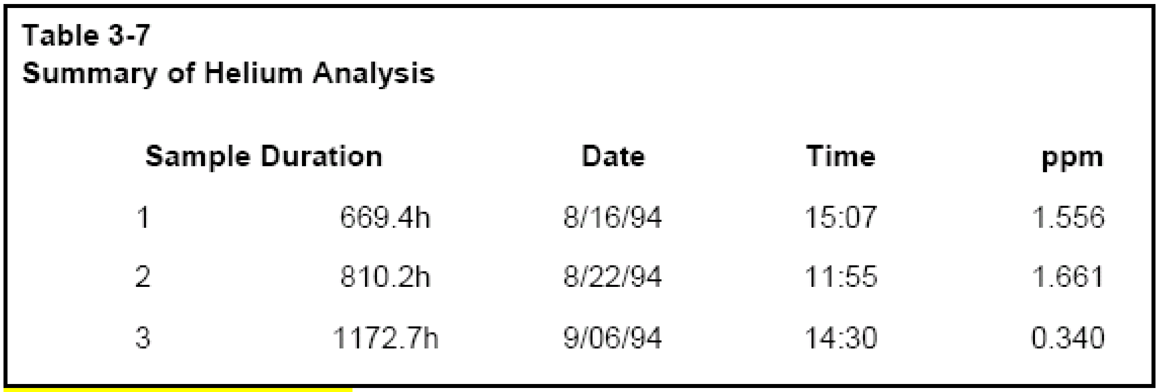
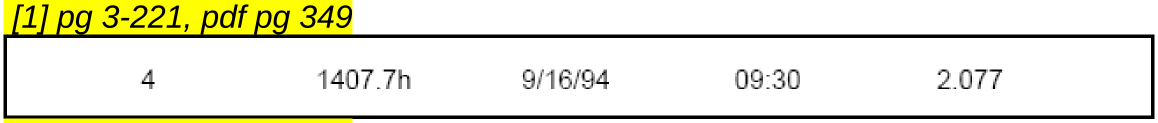


**Table 3-7**
**Summary of Helium Analysis**

| Sample | Duration | Date | Time | ppm |
|---|---|---|---|---|
| 1 | 669.4h | 8/16/94 | 15:07 | 1.556 |
| 2 | 810.2h | 8/22/94 | 11:55 | 1.661 |
| 3 | 1172.7h | 9/06/94 | 14:30 | 0.340 |

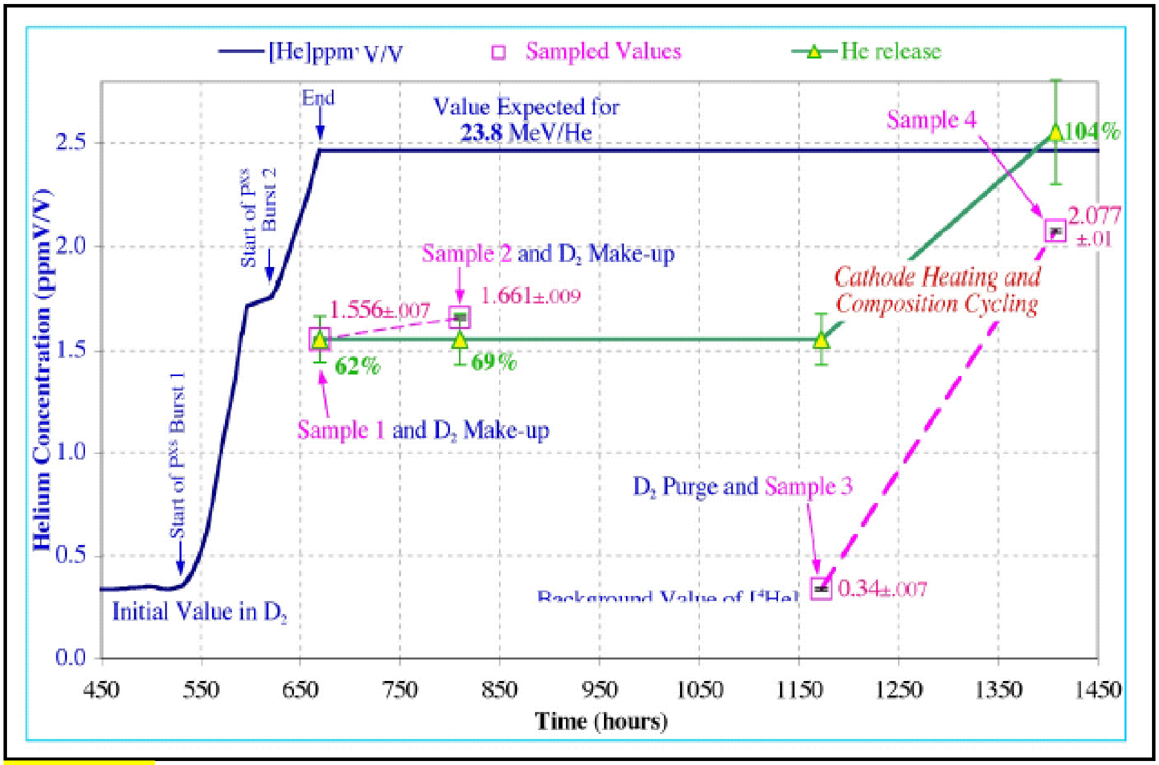
*[1] pg 3-221, pdf pg 349*



| | | | | |
|---|---|---|---|---|
| 4 | 1407.7h | 9/16/94 | 09:30 | 2.077 |

*[1] pg 3-222, pdf pg 350*

We know that the four samples/measurements from [1] are the same physical samples/measurements used in [2] and [3] because the authors displayed the same ppm values on page 9 of [3].

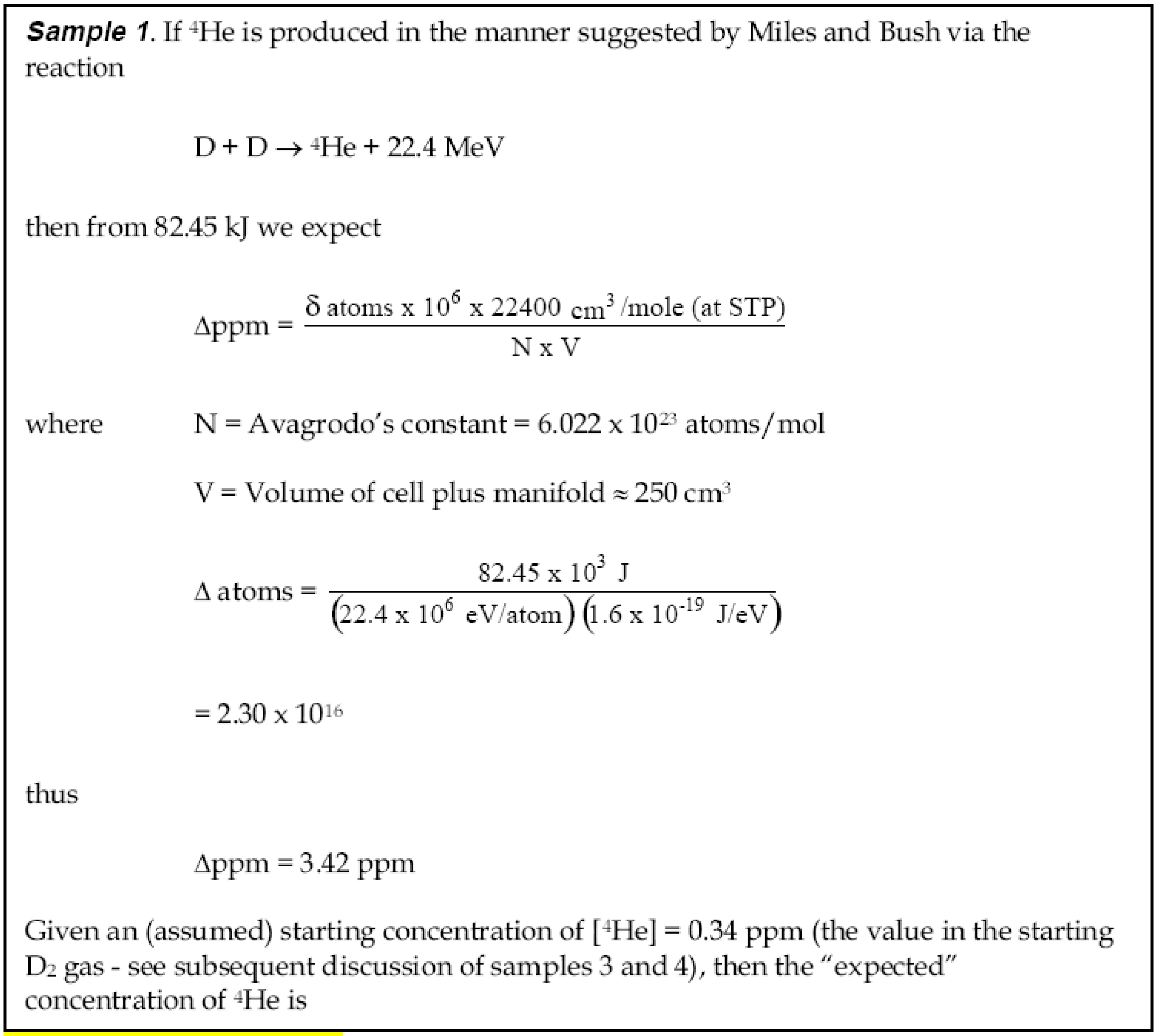

[He]ppm V/V
Sampled Values
He release
Helium Concentration (ppmV/V)
Time (hours)
0.0
0.5
1.0
1.5
2.0
2.5
450
550
650
750
850
950
1050
1150
1250
1350
1450
End
Value Expected for
23.8 MeV/He
Start of $P^{xs}$ Burst 1
Start of $P^{xs}$ Burst 2
Initial Value in $D_2$
1.556±.007
1.661±.009
Sample 2 and $D_2$ Make-up
62%
69%
Sample 1 and $D_2$ Make-up
D₂ Purge and Sample 3
Background Value of [⁴He]
0.34±.007
Cathode Heating and
Composition Cycling
Sample 4
104%
2.077
±.01

[3] page 9

## Questions

### SAMPLE 1

Shown below are the calculations for sample 1 of the predicted number of atoms (based on cell volume and measured ppm) of helium, in relation to the measured heat, assuming a ~24 MeV reaction.



***Sample 1***. If $^4$He is produced in the manner suggested by Miles and Bush via the reaction

$$D + D \rightarrow {}^4He + 22.4\ MeV$$

then from 82.45 kJ we expect

$$\Delta ppm = \frac{\delta\ atoms \times 10^6 \times 22400\ cm^3/mole\ (at\ STP)}{N \times V}$$

where N = Avagrodo's constant = $6.022 \times 10^{23}$ atoms/mol

V = Volume of cell plus manifold ≈ 250 $cm^3$

$$\Delta\ atoms = \frac{82.45 \times 10^3\ J}{\left(22.4 \times 10^6\ eV/atom\right)\left(1.6 \times 10^{-19}\ J/eV\right)}$$

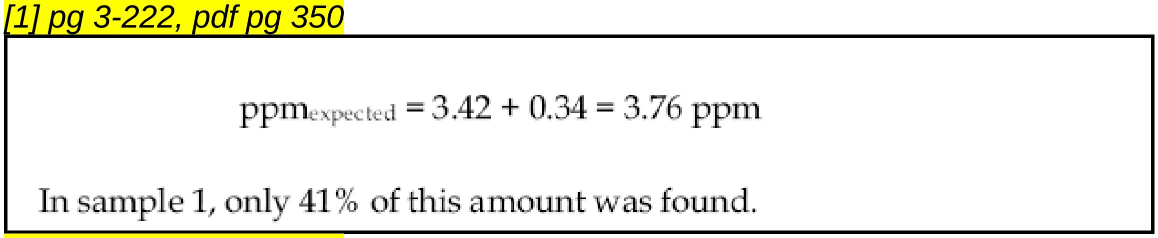
$$= 2.30 \times 10^{16}$$

thus

$$\Delta ppm = 3.42\ ppm$$

Given an (assumed) starting concentration of [$^4$He] = 0.34 ppm (the value in the starting $D_2$ gas - see subsequent discussion of samples 3 and 4), then the "expected" concentration of $^4$He is

[1] pg 3-222, pdf pg 350

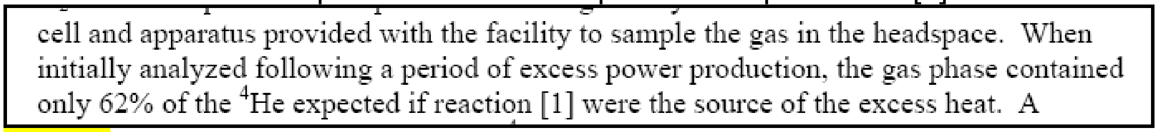


$$ppm_{expected} = 3.42 + 0.34 = 3.76\ ppm$$

In sample 1, only 41% of this amount was found.

[1] pg 3-223, pdf pg 351

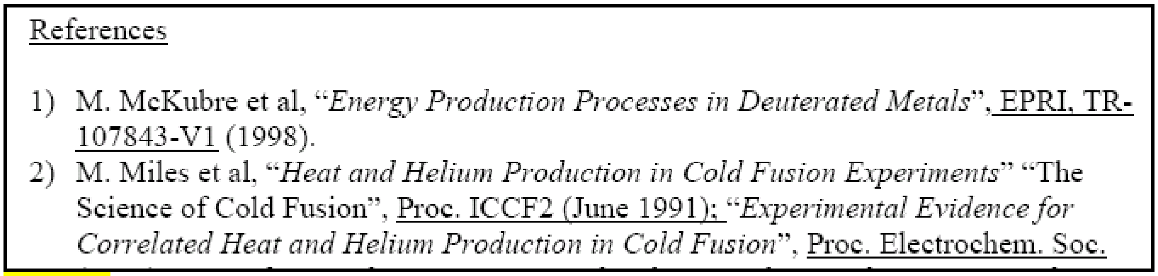
Shown below is the representation of sample 1 as 62 percent in [2]:



cell and apparatus provided with the facility to sample the gas in the headspace. When initially analyzed following a period of excess power production, the gas phase contained only 62% of the $^4$He expected if reaction [1] were the source of the excess heat. A

*[2] pg 6*

In [2], they cited the EPRI report as the source of the original data. Note that no mention of an erratum is listed.

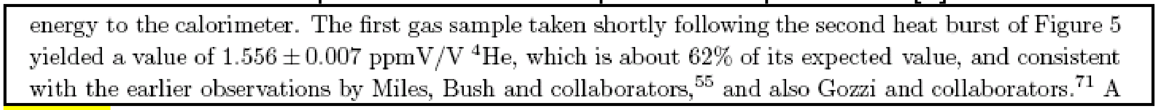


References

1) M. McKubre et al, "*Energy Production Processes in Deuterated Metals*", EPRI, TR-107843-V1 (1998).
2) M. Miles et al, "*Heat and Helium Production in Cold Fusion Experiments*" "The Science of Cold Fusion", Proc. ICCF2 (June 1991); "*Experimental Evidence for Correlated Heat and Helium Production in Cold Fusion*", Proc. Electrochem. Soc.

*[2] pg 9*

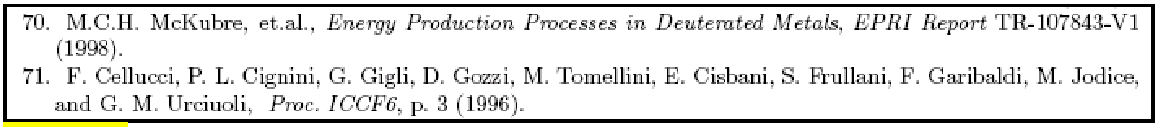
Shown below is the representation of sample 1 as 62 percent in [3]:



energy to the calorimeter. The first gas sample taken shortly following the second heat burst of Figure 5 yielded a value of 1.556 ± 0.007 ppmV/V $^4$He, which is about 62% of its expected value, and consistent with the earlier observations by Miles, Bush and collaborators,[55] and also Gozzi and collaborators.[71] A

*[3] pg 9*

In [3], they cited the EPRI report as the source of the original data. Note that no mention of an erratum is listed.



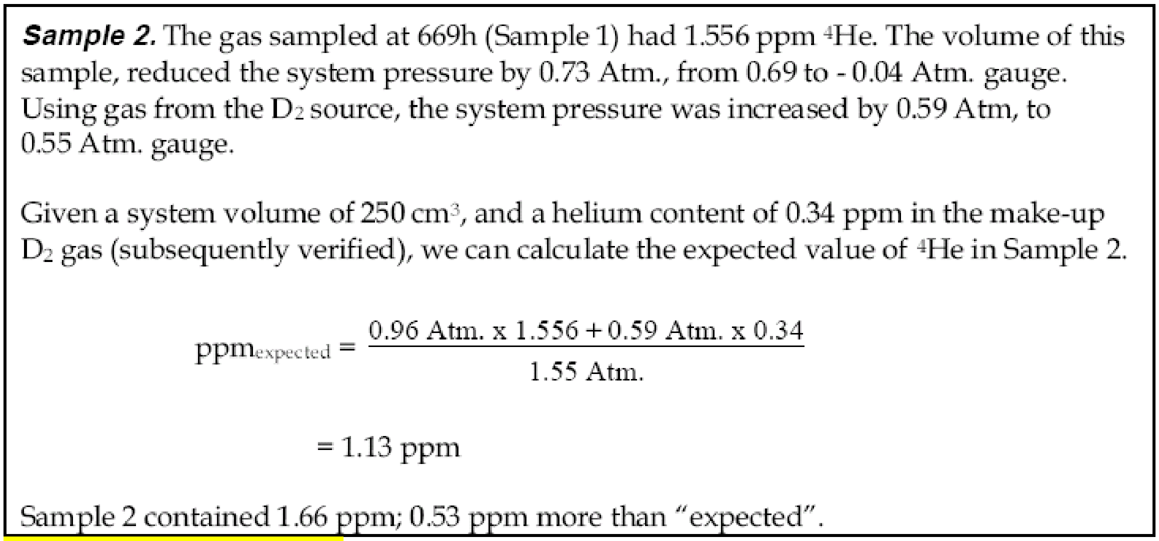
70. M.C.H. McKubre, et.al., *Energy Production Processes in Deuterated Metals, EPRI Report* TR-107843-V1 (1998).
71. F. Cellucci, P. L. Cignini, G. Gigli, D. Gozzi, M. Tomellini, E. Cisbani, S. Frullani, F. Garibaldi, M. Jodice, and G. M. Urciuoli, *Proc. ICCF6*, p. 3 (1996).

*[3] pg 27*

**Question 1: Because there is no reference to any erratum and there is no mathematical explanation provided in [2] or [3], how did the first value, measured in 1994 and published by EPRI in 1998 as 41 percent, change to 62 percent?**

## SAMPLE 2

Shown below are the calculations for sample 2 of the predicted number of atoms (based on cell volume and measured ppm) of helium, in relationship to the measured heat, assuming a ~24 MeV reaction.



***Sample 2.*** The gas sampled at 669h (Sample 1) had 1.556 ppm $^4$He. The volume of this sample, reduced the system pressure by 0.73 Atm., from 0.69 to - 0.04 Atm. gauge. Using gas from the $D_2$ source, the system pressure was increased by 0.59 Atm, to 0.55 Atm. gauge.

Given a system volume of 250 cm$^3$, and a helium content of 0.34 ppm in the make-up $D_2$ gas (subsequently verified), we can calculate the expected value of $^4$He in Sample 2.

$$\text{ppm}_{\text{expected}} = \frac{0.96 \text{ Atm. x } 1.556 + 0.59 \text{ Atm. x } 0.34}{1.55 \text{ Atm.}}$$

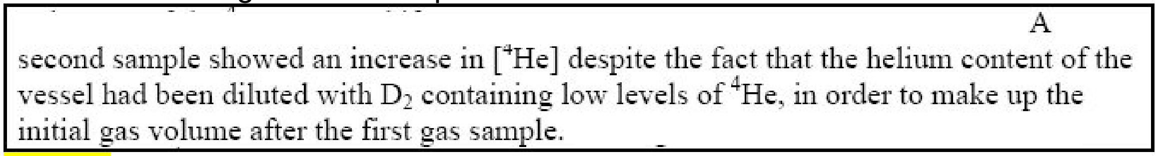
$$= 1.13 \text{ ppm}$$

Sample 2 contained 1.66 ppm; 0.53 ppm more than "expected".

*[1] pg 3-223, pdf pg 351*

The percent of measured/expected helium is 1.66/1.13 = 147 percent.

Shown below is the representation of sample 2 in [2]. Note that they did not provide a numerical value; they merely stated that the concentration had increased. Note that they did not state that the sample was not only an increase but also much higher than the predicted value - not a trivial difference.

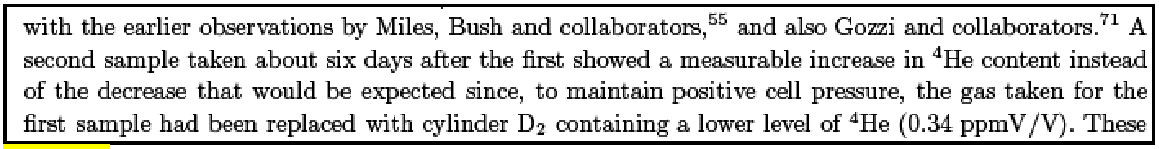


A

second sample showed an increase in [$^4$He] despite the fact that the helium content of the vessel had been diluted with $D_2$ containing low levels of $^4$He, in order to make up the initial gas volume after the first gas sample.

*[2] pg 6*

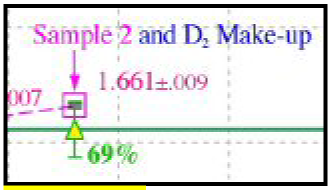
Shown below is the representation of that sample 2 in [3]. Again, they did not list a numerical value in the text, but they did show the value of 1.661 in [3] on page 9.



with the earlier observations by Miles, Bush and collaborators,[55] and also Gozzi and collaborators.[71] A second sample taken about six days after the first showed a measurable increase in $^4$He content instead of the decrease that would be expected since, to maintain positive cell pressure, the gas taken for the first sample had been replaced with cylinder $D_2$ containing a lower level of $^4$He (0.34 ppmV/V). These

*[3] pg 9*

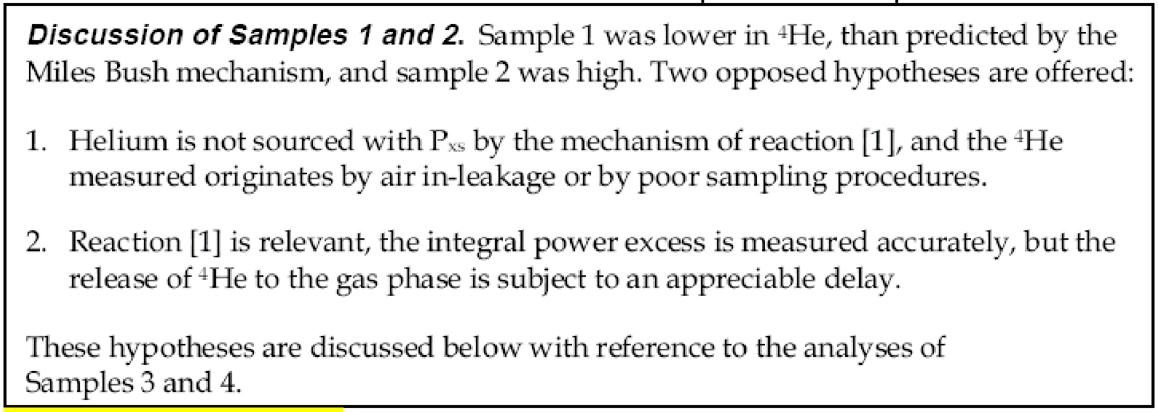
Shown below is the interpreted value for sample 2 of 69 percent:



*[3] page 9*

**Question 2: Because there is no reference to any erratum and there is no mathematical explanation provided in [3], how did the second value, measured in 1994 and published by EPRI in 1998 with a value that equals 147 percent, change to 69 percent?**

Shown below is the discussion of samples 1 and 2 from [1]. The authors did not know how to attribute the differences in both samples from the predicted value.



***Discussion of Samples 1 and 2.*** Sample 1 was lower in $^4$He, than predicted by the Miles Bush mechanism, and sample 2 was high. Two opposed hypotheses are offered:

1. Helium is not sourced with $P_{xs}$ by the mechanism of reaction [1], and the $^4$He measured originates by air in-leakage or by poor sampling procedures.

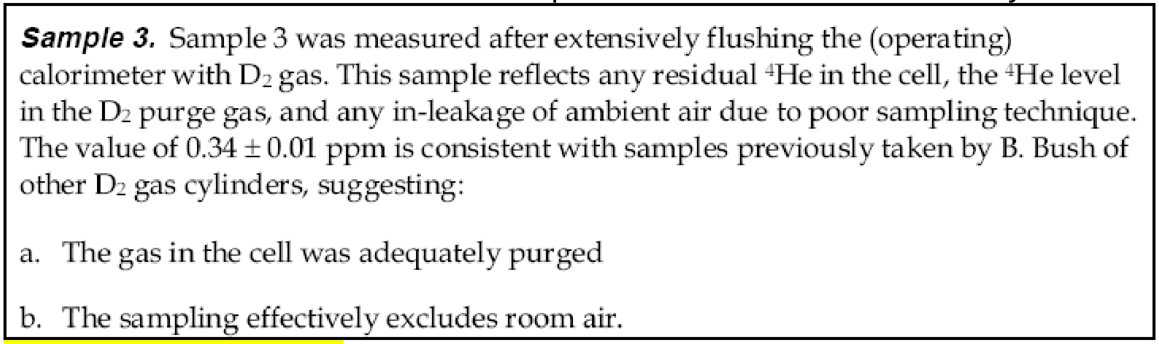
2. Reaction [1] is relevant, the integral power excess is measured accurately, but the release of $^4$He to the gas phase is subject to an appreciable delay.

These hypotheses are discussed below with reference to the analyses of Samples 3 and 4.

*[1] pg 3-223, pdf pg 351*

## SAMPLE 3

Shown below is information about sample 3. No discussion is necessary.



***Sample 3.*** Sample 3 was measured after extensively flushing the (operating) calorimeter with $D_2$ gas. This sample reflects any residual $^4$He in the cell, the $^4$He level in the $D_2$ purge gas, and any in-leakage of ambient air due to poor sampling technique. The value of 0.34 ± 0.01 ppm is consistent with samples previously taken by B. Bush of other $D_2$ gas cylinders, suggesting:

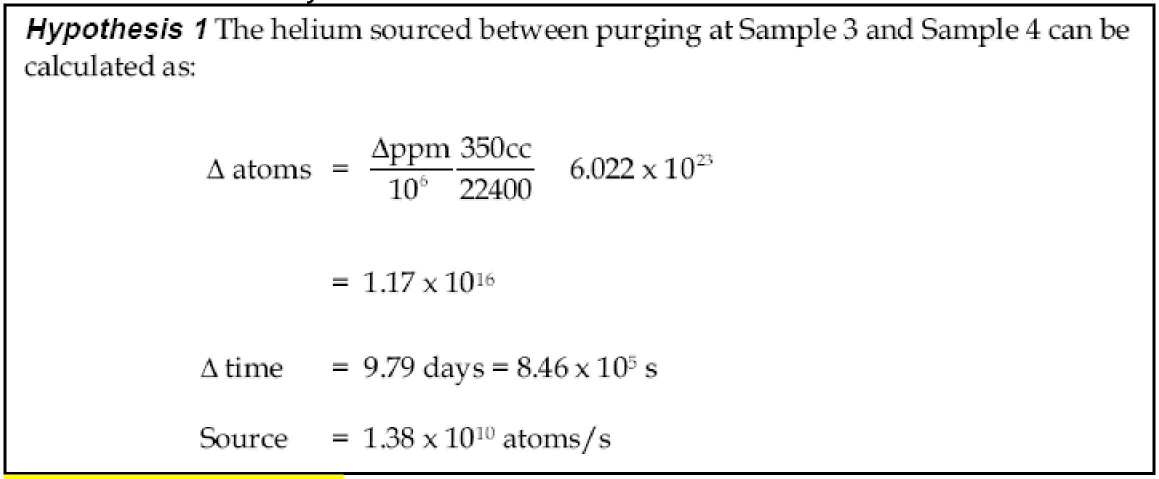
a. The gas in the cell was adequately purged

b. The sampling effectively excludes room air.

*[1] pg 3-223, pdf pg 351*

# SAMPLE 4

In the EPRI report [1], sample 4 is discussed in great detail from pg 3-223 pdf / pg. 351 through pg. 3-228 pdf / pg. 356. In no place in the report did the authors discuss an "expected" value for this helium sample. All they knew for certain was how much helium they measured.



***Hypothesis 1*** The helium sourced between purging at Sample 3 and Sample 4 can be calculated as:

$$\Delta \text{ atoms} = \frac{\Delta\text{ppm}}{10^6}\frac{350\text{cc}}{22400} \quad 6.022 \times 10^{23}$$

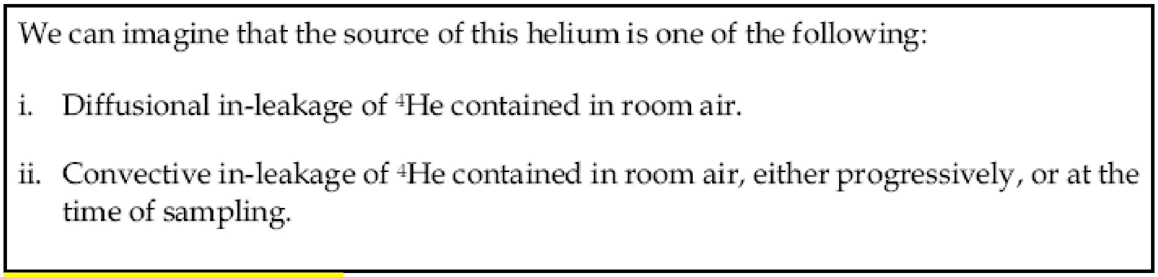
$$= 1.17 \times 10^{16}$$

$$\Delta \text{ time} = 9.79 \text{ days} = 8.46 \times 10^5 \text{ s}$$

$$\text{Source} = 1.38 \times 10^{10} \text{ atoms/s}$$

*[1] pg 3-224, pdf pg 352*

The reason the authors did not discuss an "expected" value for this helium sample is that they had no clear idea what heat, let alone what process, was responsible for the helium. The best they could do was to speculate on four scenarios:

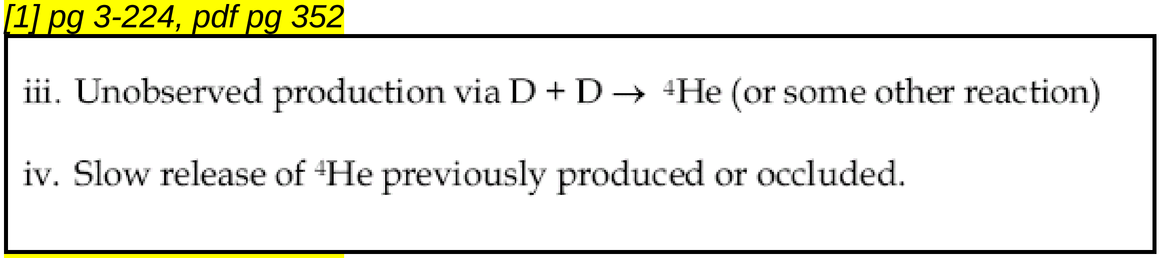


We can imagine that the source of this helium is one of the following:

i. Diffusional in-leakage of $^4$He contained in room air.

ii. Convective in-leakage of $^4$He contained in room air, either progressively, or at the time of sampling.

*[1] pg 3-224, pdf pg 352*



iii. Unobserved production via $D + D \rightarrow {}^4He$ (or some other reaction)

iv. Slow release of $^4$He previously produced or occluded.

*[1] pg 3-225, pdf pg 353*

The authors in [1] did not state where the heat came from for sample 4 and did not provide a value for the relationship between the measured helium versus the expected helium. However, the authors in [2] provided a value of 104 +/- 10 percent.

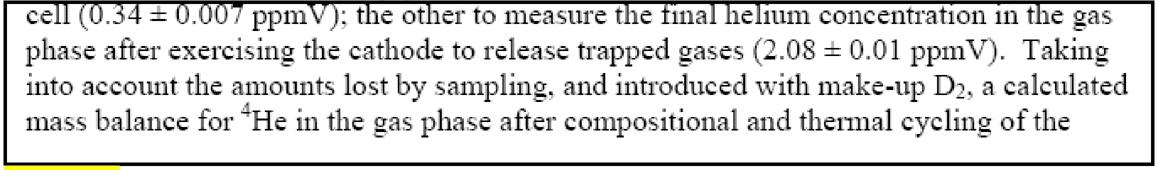
The authors of [2] appear to have applied several factors to obtain the value of 104 +/- 10 percent.

First, they, in contradiction to the authors of [1], assumed as a certainty that the heat bursts that took place before sample 1 were responsible for the generation of helium.

Second, they, in contradiction to the authors of [1], assumed as a certainty that no heat was created in the period between sample 3 and sample 4. Had there been heat, it could have been responsible for the generation of helium.

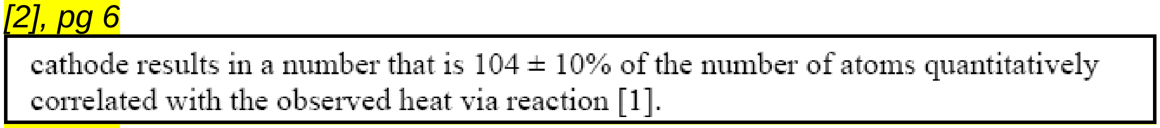
Third, they, in contradiction to the authors of [1], assumed as a certainty that helium had been hiding out in the palladium cathode. They invented a concept called a "helium retention hypothesis," which involves another concept they invented called "cathode heating and compositional cycling."

This is how the authors of [2] explain getting the sample 4 value of 104 percent.



cell (0.34 ± 0.007 ppmV); the other to measure the final helium concentration in the gas phase after exercising the cathode to release trapped gases (2.08 ± 0.01 ppmV). Taking into account the amounts lost by sampling, and introduced with make-up $D_2$, a calculated mass balance for $^4$He in the gas phase after compositional and thermal cycling of the

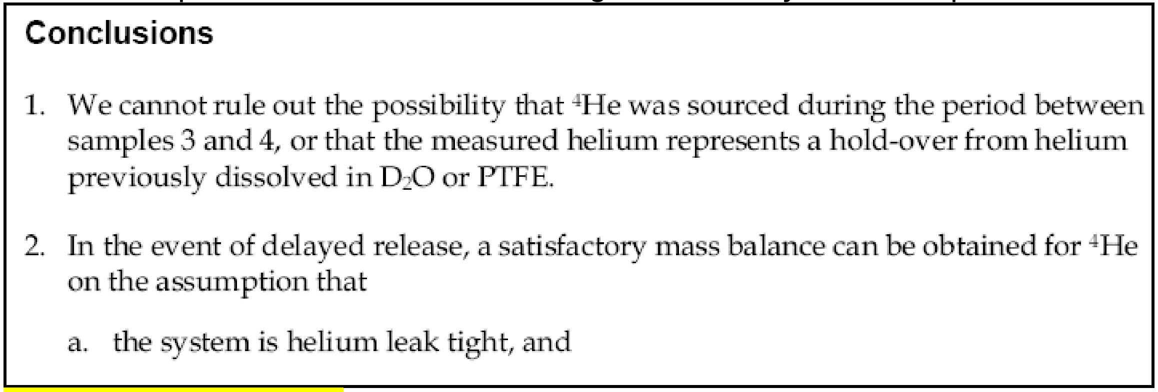
*[2], pg 6*



cathode results in a number that is 104 ± 10% of the number of atoms quantitatively correlated with the observed heat via reaction [1].

*[2], pg 7*

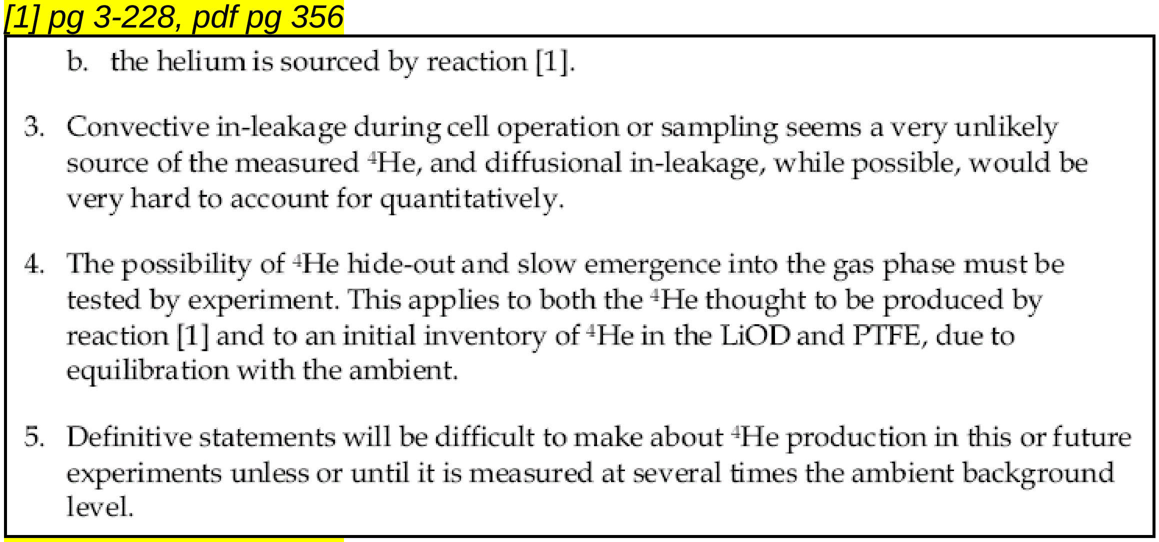
The conclusions of the M4 section of [1] list five points. Chief among these is #4, which states that the helium retention hypothesis must be tested by experiment. This means that the researchers did not test this hypothesis at the time they wrote this report, let alone before or during the time they ran the experiment.



### Conclusions

1. We cannot rule out the possibility that $^4$He was sourced during the period between samples 3 and 4, or that the measured helium represents a hold-over from helium previously dissolved in $D_2O$ or PTFE.
2. In the event of delayed release, a satisfactory mass balance can be obtained for $^4$He on the assumption that
   a. the system is helium leak tight, and

*[1] pg 3-228, pdf pg 356*



   b. the helium is sourced by reaction [1].

3. Convective in-leakage during cell operation or sampling seems a very unlikely source of the measured $^4$He, and diffusional in-leakage, while possible, would be very hard to account for quantitatively.
4. The possibility of $^4$He hide-out and slow emergence into the gas phase must be tested by experiment. This applies to both the $^4$He thought to be produced by reaction [1] and to an initial inventory of $^4$He in the LiOD and PTFE, due to equilibration with the ambient.
5. Definitive statements will be difficult to make about $^4$He production in this or future experiments unless or until it is measured at several times the ambient background level.

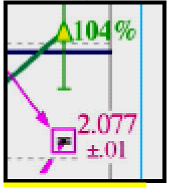
*[1] pg 3-229, pdf pg 357*

**Question 3: How is it possible, as stated in [2] and [3], that the researchers conducting experiment M4 intentionally used a "cathode heating and compositional cycling" process to "release trapped helium" when they effectively stated in 1998 that they had not done so?**

**Question 4: How is it possible that the authors of [2] and [3] knew with certainty that the heat which was responsible for the generation of helium in sample 4 was from the heat bursts that took place before sample 1?**

**Question 5: How is it possible that the authors of [2] and [3] knew with certainty that no heat from the period between sample 3 and sample 4 was responsible for the generation of helium sample 4?**

Additionally, the authors of [3] indicated the fourth sample at a graphical location of 84 percent but showed it (also) graphically and numerically as 104 percent.



*[3] page 9*

**Question 6: Even if we assume that a correction was provided to EPRI that shows a correction between the 84 percent mass balance value and the 104 percent mass balance value, how can this possibly explain the fact that, in the EPRI report [1], there was no mention of any value or calculation for a mass balance for sample 4?**

**Question 7: Because helium concentrations in all four samples were measured separately and helium atoms were calculated independently based on the cell volumes and conditions at each sampling time, is it correct to represent, as McKubre did, the fourth value as an accumulation of helium from all four samples?**