Experiment M4 — Record of Discussion with Francis Tanzella

By Steven B. Krivit
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Summary
On Nov. 2, 2023, Steven B. Krivit (*New Energy Times*) and Francis Tanzella (SRI International, retired) began a discussion in Tanzella's private laboratory in Belmont, CA. They discussed experiment M4, performed in 1994 at SRI International and first reported in 1998. The experiment was sponsored by the Electric Power Research Institute (EPRI). Follow-up discussions between Krivit and Tanzella took place by e-mail through Nov. 16, 2023.

Question #1
Steven B. Krivit: To your knowledge, have I ever publicly questioned or cast doubt on the validity or accuracy of any helium measurements (as standalone measurements, rather than as part of a ratio) performed in LENR systems at SRI International?

Francis Tanzella: No

Question #2
SK: Were you an author of this report? "Development of Energy Production Systems from Heat Produced in Deuterated Metals - Energy Production Processes in Deuterated Metals, Volume 1, TR-107843-V1, June 1998" [1]

FT: Yes

Question #3
SK: Were you involved in any of the research in this report?

FT: Yes

Question #4
SK: Were you involved in experiment M4, discussed in this report?

FT: *[Replaced by revised response.]* I was the main lab person.

FT: I oversaw the lab technicians operating the experiment.

Question #5
SK: Were you involved in taking any heat measurements for M4?

FT: Mike McKubre did the heat analysis back in those days.

Question #6

SK: Were you involved in taking any helium measurements for M4?

FT: *[Replaced by revised response.]* Yes, I ran the mass spec.

FT: I may have collected some of the He samples. The He measurements were made at the U.S. Bureau of Mines in Amarillo, TX.

Question #7

SK: Page #3-221 of the EPRI report shows three helium measurements taken for M4: at 669.4 hours, 810.2 hours, and 1172.7 hours. Page #3-222 of the report shows a helium measurement taken at 1407.7 hours. *To your knowledge, were any other helium samples taken for M4?*

FT: No. I suspect that we did not want to disturb the system any more than necessary.

Question #8

SK: Page 3-224 of report states that a heat burst occurred associated with helium sample #4, measured at 1407 hrs. The graph on page 3-178 shows that heat events took place between 1300 and 1400 hours. However, the report states on page 3-224 that "the mass flow stopped" during this time and "the calorimeter was at significant remove from its steady state ... the thermal baseline was not well established ... during the two periods of current oscillations, the calorimeter was apparently endothermic, by as much as 100 mW." *Therefore, would any scientifically credible heat/helium ratio, associated with the fourth helium sample, have been possible?*

FT: *[Replaced by revised response.]* We could not calculate a ratio. The excess power was not noted. We did not report it because the heat measurement was not defensible.

FT: *[Replaced by revised response.]* We could not calculate or report an instantaneous ratio since the excess power was not noted. However, the [He]/energy ratio was calculable because that required using the integrated energy measured to date.

SK: On page 3-222 of the EPRI report, it states that excess power was observed during the third current ramp. (See my graph below) It describes the associated period from 530 hours to 668 hours in which two heat bursts were measured. It states that the energy integrated from the excess power in that period was 82 kJ. The report shows that energy value calculated against the number of helium atoms measured in Sample 1 (noted in the graph below as S1M). The measured amount of helium-4, compared against the 82 kJ of heat, was lower than the amount of helium predicted by the Miles-Bush hypothesis of 24 MeV per helium-4 atom.

On page 3-223 of the EPRI report, it states that helium Sample #2 was taken (noted in the graph below as S2M). This was about 140 hours after Sample #1 was taken. No further heat events had taken place between Sample 1 and Sample 2. Therefore, you and your co-authors made another calculation of the heat/helium-4 ratio, based on Sample #2, but you accounted for the volumetric loss of helium-4 taken in Sample #1. Now, based on Sample #2, the measured amount of helium-4, compared against the 82 kJ of heat, was inexplicably higher than the amount of helium predicted by the Miles-Bush hypothesis of 24 MeV per helium-4 atom.

In this Question #8, my intent is not to discuss the matter of the amount of helium-4 being too low or too high. It is simply intended to illustrate how scientifically credible measurements of heat and helium-4 were determined and reported by you and your co-authors. The values for the proximity of the helium measurements to the ratios predicted by Miles-Bush for those two samples were discussed articulately and supported by mathematical calculations in the 1998 report. We see that the energy component of the ratio was indeed calculated based on integrated energy during the enthalpy from 530h to 668h.

Please forgive me for stating such an elementary point, but one cannot calculate a value of energy without the component of time or power. Yesterday, you said that, for helium Sample #4, "the excess power was not noted." Indeed, as far as I can tell, excess power measurements were not reported in the EPRI report in the time period preceding Sample #4. *That being the case, do you see any scientifically credible way that the expected amount of helium-4 associated with Sample #4, based on the published data, was possible to determine?*

FT: *[Replaced by revised response.]* On page 3-224 paragraph it states “the calorimeter was at significant removed (sic) from its steady state for long periods of time (10-20% of the between sample period).”, meaning that the energy measurement was subject to up to a 20% error. That also means that the ratio calculated was subject to that same error. That is still acceptable as scientifically credible since it was called out in the report.

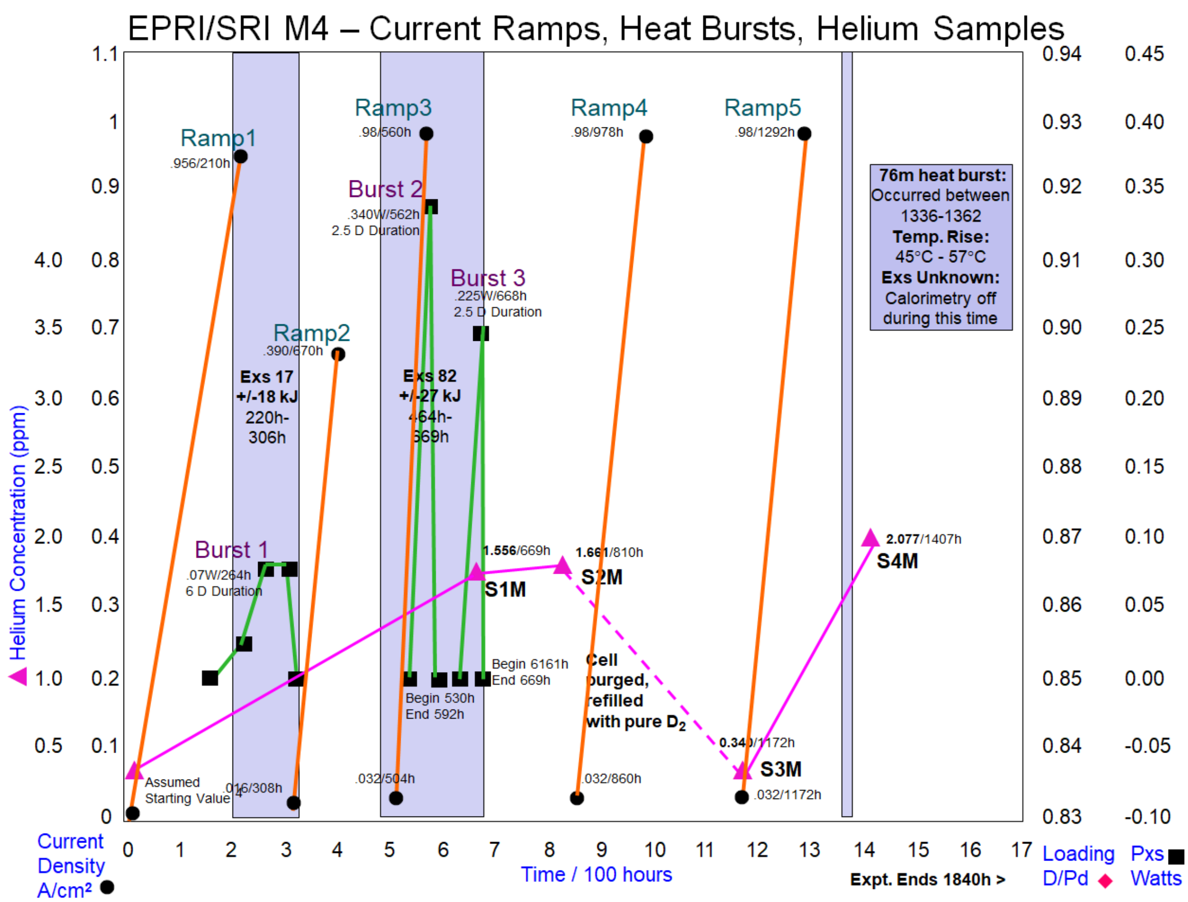
SK: You're not using the right term yet, though it could be my fault because of my initial imprecise wording. The value under discussion, "104%," is not a ratio. It is a value that shows proximity of the amount of helium-4 expected according to the hypothetical ratio of 24 MeV heat per helium-4 atom. That being the case, if excess power was not noted (as you said and as the report concurs), then integrated excess energy could not possibly have been calculated. Without the integrated energy, the 104% value could not possibly have been calculated. Moreover, among the extant published documents on M4, *no such published calculation exists*. *Nevertheless, I understand that you have elected to go on record asserting that the 104% value could have been determined in a scientifically credible way.*

FT: I think we are talking past each other because of semantics. On second thought, you should quote the conclusions on pages 3-228 and 3-229. The result is inconclusive, but since it was stated so in the text, I consider it to be scientifically credible. That means that the 104% may or may not be a relevant number. Again, since this was an internal EPRI report and not a published paper, I don’t think we should be wasting any more time on it.

SK: In order to avoid semantics or ambiguities, let's be very specific. You wrote: "since it was stated so in the text." What was "stated so" in the text? Certainly the text stated no value for Sample #4, let alone a value of 104%. The conclusions on pages 3-228 and 3-229 say nothing about any possible value for Sample #4. The most relevant statement in the conclusion is "We cannot rule out the possibility that helium-4 was sourced during the period between samples 3 and 4." So maybe there was heat correlated to the helium-4 and maybe there wasn't. The report explains on page 3-224 that mass flow of the calorimeter was off during the heat burst. Maybe McKubre, by a method that neither of us knows about, by details that have never been published, found some way to derive and calculate the 104% value. Or maybe he just made it up. *Are you sure that you want to go on record saying that you consider the 104% value published by McKubre "scientifically credible?"*

FT: The conclusions state that the authors cannot prove that the 104% is accurate. As such, the text has allowed that the calculation based on the 4th sample cannot be trusted to be accurate. Such statements of truth make the report, if not all of the results within, credible.

SK: *[In order to bring the discussion to a close, I did not reply further to FT]* The value of 104%, for helium Sample #4, did not exist in the 1998 EPRI report. No value for helium Sample #4 existed in the 1998 EPRI report. Thus, it is impossible, as Tanzella wrote, "that the conclusions [of the 1998 EPRI report] state that the authors cannot prove that the 104% is accurate."



Graph by S.B. Krivit based on data mined from EPRI Report TR-107843-V1

Furthermore, on page 3-201 of the 1998 EPRI report, the authors said that excess power and energy in M4 could be determined only in the heat burst associated with Ramp 3, and not with any heating event after the cell had been purged. Yes, a heating event did occur and yes, there was associated helium-4. But the heat was not measured calorimetrically.

> *Excess Power*. In a total of 14 current ramps in experiment M1-M4, excess power was observed on only 3 occasions: the first ramp of M1, and the first and third ramps of M4. In two instances, the amount|of excess power observed was very small, not much larger than the determined accuracy of the calorimeter (~ 0.2-0.4%). Only in the case of M4 ramp 3 was excess power and energy observed with sufficient resolution to perform further analysis and draw conclusions about the causes and conditions.

Page 3-201 excerpt from EPRI Report TR-107843-V1

Question #9

SK: *[Replaced by revised question.]* On March 5, 2007, at the APS meeting, Denver CO, McKubre displayed a slide showing a heat/helium ratio of 104% at 1407 hours. [2] *Considering the problems with the heat measurement just described, was the **ratio** of 104%, associated with sample #4, displayed at 1407 hours, scientifically credible?*

FT: *[Replaced by revised response.]* The heat measurement was accurate because it was integrated over time. The percentage must be an integral.

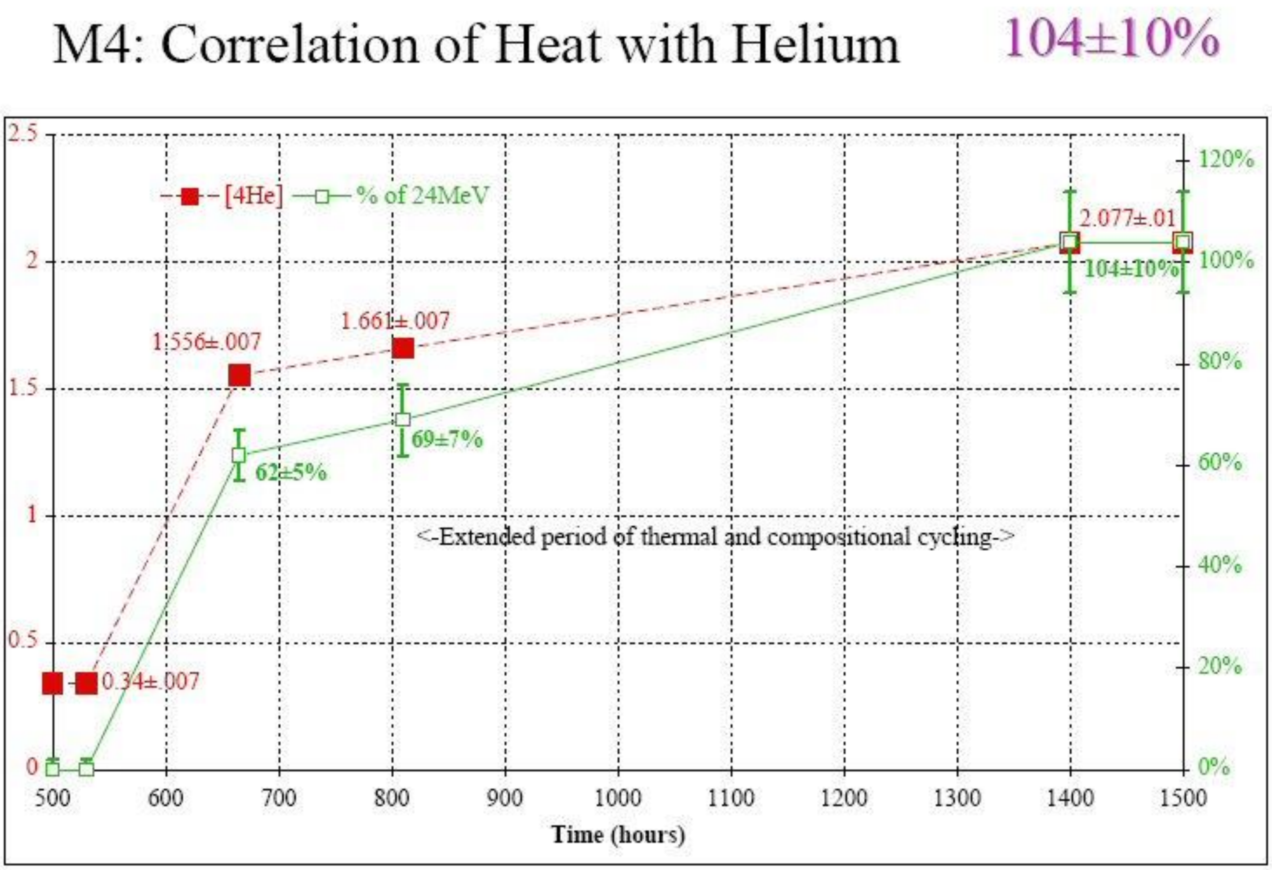
FT: *[Replaced by revised response.]* The excess energy measurement was accurate because it was integrated over time. The percentage must be an integral.

SK: There was an error in my question. I am revising it accordingly. On March 5, 2007, at the APS meeting, Denver CO, McKubre displayed a slide showing a helium measurement of 104%, relative to the predicted heat/helium ratio, at 1407 hours. [2] *Considering the problems with the heat measurement just described, was the **value** of 104%, associated with sample #4, displayed at 1407 hours, scientifically credible?*

FT *[Replaced by revised response.]* As stated above, this calculation was subject to up to 20% error.

SK: No such calculation could have been performed given the lack of published power and energy values for Sample #4. There is no record of any such published calculation for Sample #4. *Nevertheless, I understand that you have elected to go on record implying that "this calculation," which produced the 104% value for Sample #4, was somehow performed and therefore was scientifically credible.*

FT: See the answer to Question #8.



Displayed by Michael McKubre on March 5, 2007, at the APS meeting, Denver CO [2]

Question #10

SK: In the same graph, McKubre displayed another ***value*** ~~ratio~~ of 104 percent at 1500 hours, even though no helium measurement had been taken at 1500 hours. *Was that data point scientifically credible?* [*Correction*: "**ratio**" replaced with "**value**".]

FT: We can assume that the **value** ~~ratio~~ at 1500 is the same as the **value** ~~ratio~~ at 1400 since that is excess energy (integrated excess power) and no more excess power was seen. We do not believe that any He should have been created or lost during this time. [*Correction*: "ratio" replaced with "value".]

SK: This graph depicts data points using two kinds of squares. The solid red squares are intended to represent helium-4 measurements in parts per million. The four numerical values shown; 0.34, 1.556, 1.66, and 2.077 are listed (and are the only such values listed for M4) in the EPRI report. The hollow green squares are intended to represent how close the helium-4 values come to the values expected from the Miles-Bush hypothesis of 24 MeV per helium-4 atom. A value of 100% would mean exactly 24 MeV/4He.

At 1400 and 1500, we see hollow green squares superimposed on top of solid red squares. These squares are depicted as what one would ordinarily assume to be data points; actual measurements. We know that no helium-4 measurement was performed at 1500. *Is the apparent data point at 1500, in* *your professional experience, scientifically credible?*

FT: We have already shown that the cell is not subject to He in-leakage or out-leakage, so assuming the He concentration did not change over that time period is scientifically credible.

SK: I understand that you have elected to go on record saying that a visual representation of a data point that was *assumed rather than measured*, and which furthermore was *not disclosed as assumed*, is scientifically credible.

Question #11

SK: *[Replaced by revised question]* In the same graph, McKubre displayed a **value** ~~ratio~~ of 0 percent at 500 hours and another at about 525 hours, even though no helium measurement had been taken at those times. Were those data points for heat/helium **values** ~~ratios~~ scientifically credible?

FT: We probably also measured helium at that time but didn't report them in the final report.

SK: In the same graph, McKubre displayed helium-4 values at 500 hours and another at about 525 hours, even though no helium measurements had been taken at those times. Were those data points for helium scientifically credible? *(I have revised this question to be more precise regarding the helium measurement, not the helium ratio.)*

FT: *[Replaced by revised response.]* We measured the helium concentration in the starting D_2 gas at those times but didn't tabulate them in the final report. It was in the text. The measurement at 1172.7 hours was of the same starting gas and such is scientifically credible.

SK: On page 3-222 of the EPRI report, it states that "Given an assumed starting concentration of 4He = 0.34." The text in the final report indicates that starting concentration was assumed and not measured. *As such were the two data points for the helium at 500 and 525 hours scientifically credible?*

FT: *[Tanzella did not provide a response at this time.]*

SK: I understand that you have elected to go on record saying that you measured the helium concentration at 500 and 525 hours even though there is no record of such measurements and even though you told me on Nov. 3 that you knew of no other helium measurements besides the four reported in the EPRI paper.

FT: This D_2 cylinder was the same that Bush used in our lab. We assumed that the He concentration in that same cylinder did not change between when Bush measured it and when we used it. Hence, that is a scientifically credible assumption.

Question #12

SK: *[Replaced by revised question]* In the EPRI report, on page 3-223, it states that the helium sample #2 "contained 1.66 ppm, 0.53 ppm more than expected." The APS 2007 graph calculates the ratio for this same helium measurement as 69% of the expected amount of helium. *Do you know of any scientifically credible reason why, at any time, let alone 13 years after the experiment was performed, this value could have changed from 147% of the ratio expected to 69% of the ratio expected, based on the Miles-Bush 24 MeV/4He hypothesis?*

FT: *[Replaced by revised response.]* It was created in the time period between helium sample #1 and helium sample #2, if "expected" refers to the starting gas. The text is not clear whether "expected" refers to the starting gas or that predicted by Miles' results.

SK: I see now that I had an error in my question the first time. The value of 147% was not the ratio, but rather, the amount of helium-4 with respect to amount of helium-4 that would be equal to 100% of the 24 MeV/4He atom ratio proposed by Miles-Bush. I will rephrase my question more accurately:

In the EPRI report, on page 3-223, it states that the helium sample #2 "contained 1.66 ppm, 0.53 ppm more [helium-4] than expected" based on the Miles-Bush 24 MeV/4He hypothesis. Thus, the amount of helium-4 measured in Sample 2 was 147% of that proposed by Miles-Bush. The APS 2007 graph shows the amount for this same helium measurement as 69% of the expected amount of helium. *Do you know of any scientifically credible explanation as to how this value could have changed from 147% of the expected amount to 69% of the expected amount?*

FT: I do not know what data was used to create the APS slides, and as such cannot answer that question.

SK: I understand that you no not have sufficient information to determine whether the change of the 147% value to 69% was scientifically credible.

FT: Since I had nothing to do with the preparation of the APS slides, you must ask Mike McKubre about that slide.

Question #13

SK: The EPRI report states that after the second helium sample was taken, the cell was purged and refilled with pure deuterium gas. After that procedure, at 1172 hours, helium sample #3 was taken. It was measured at 0.340 ppm, consistent with the expected baseline after purging and refilling. In the 2007 presentation, at 1172 hours, no data point exists. *Do you consider this omission scientifically credible?*

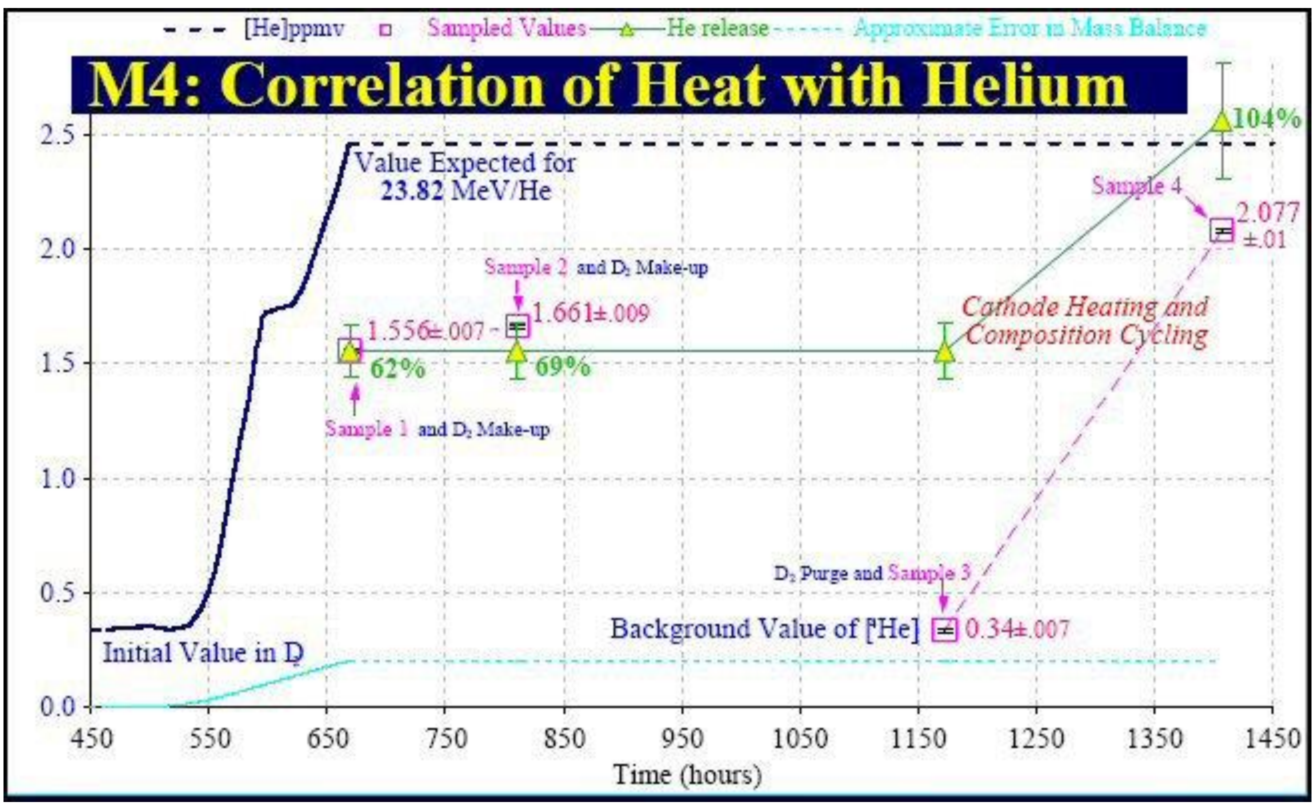
FT: *[Replaced by revised response.]* It was a baseline measurement and it was deliberately not shown because it would have been confusing as it was not part of the continuation of the experiment. Experiment M4 should be explained better in another paper.

SK: I disagree that Sample #3 was "not part of the continuation of the experiment." It was in between Sample #2 and Sample #4. It was also of sufficient importance to be described in the text and in the table in the EPRI report. The 2.077 ppm helium-4 value in Sample #4 is directly characterized by the fact that, 235 hours earlier, helium-4 concentration started from the baseline value. *Thus, with these considerations in mind, was this omission scientifically credible?*

FT: *[Replaced by revised response.]* This is a client report and not subject to peer review. Such reports are not considered preliminary scientific reports. Unfortunately, it looks like those results were not presented in subsequent papers.

SK: I understand that you have elected to go on record defending this omission of Sample #3 as scientifically credible because the report you provided to EPRI was "not subject to peer review" and that "such reports are not considered preliminary scientific reports" and that Sample #3 was "not presented in a subsequent paper."

I would like to point out that the value for Sample #3 was, in fact, presented in a subsequent paper. See the graph below, presented by Michael McKubre et al. to the Department of Energy in 2004. [3] That value is the hollow red square at 1172 hours and 0.34 ppm. That value is also simultaneously represented at 1.556 ppm, a shift by itself which is subject to question.



Published and provided by Michael McKubre as part of the 2004 DOE Review of LENRs [3]

SK: This client report that you claim was "not subject to peer review" was published in 1998 and provided to EPRI's commercial clients. It was later provided free of charge to the public. (See image below and enclosed document.)

Energy Production Processes in Deuterated Metals: Volume 1

Product ID: TR-107843-V1	**Sector Name:** Nuclear
Date Published: 6/30/1998	**Document Type:** Technical Report
File size: 7.19 MB	**File Type:** Adobe PDF (.pdf)

Full list price: No Charge

This Product is publicly available.

Download

Image captured from EPRI Web site (Record of public availability)

FT: See the answer to Question #12.

Question #14

SK: In the 2000 paper, you and your co-authors wrote that "When initially analyzed following a period of excess power production, the gas phase contained only 62% of the 4He expected if reaction #1 were the source of the excess heat." In the 1998 EPRI report, you and your co-authors described that value as 41% rather than 62%. No calculations were provided in 2000. [4] *Can you defend the 2000 revision as scientifically credible?*

FT: I didn't perform any of these calculations. You will need to ask Mike McKubre. Generally, a proceedings paper is considered more authoritative than a client report.

SK: I understand that you no not have sufficient information to defend whether, in two reports you co-authored, the change of the value of 41% to 62% was scientifically credible.

Question #15

SK: In the 2000 paper, you and your co-authors provided numerical values for helium Samples #1 (62%), #3 (0.34 ppm), and #4 (2.08 ppm). You provided no numerical value for Sample #2. You simply wrote "A second sample showed an increase in helium-4." The fact that the helium was in excess of the amount expected weighs against the working hypothesis. *Therefore, was this omission consistent with good scientific practice?*

FT: A proceedings paper will always contain less results than a client report. See the answer to Q #14.

SK: I understand that you imply this omission was scientifically acceptable.

Question #16

SK: Are you aware of any error regarding M4 that was ever found and reported to EPRI?

FT: To the best of my knowledge, there were no further written reports regarding M4 sent to any one in writing. That is not unusual when dealing with client reports, unless the experiment was redone. The M series of experiments were never reproduced as we moved on to different He-heat experiments, which gave better results.

FT SUMMARY NOTE: Some of these answers are speculation based on reading of the report about experiments performed almost 3 decades ago. To be conclusive, I would need access to the original notebooks, which is not possible as I am no longer an SRI employee. The notebooks are archived in SRI's library.

REFERENCES:

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. Huaser, 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)

2. Michael McKubre, "Cold Fusion at SRI An 18 Year Retrospective and Brief Prospective," presented at the APS meeting, Denver CO, March 5, 2007

3. Peter Hagelstein, Michael McKubre, David Nagel, Talbot Chubb, Randy Hekman, "New Physical Effects In Metal Deuterides," Submitted to the 2004 U.S. Department of Energy LENR Review, (2004)

4. 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. Lerici (La Spezia), Italy: Italian Physical Society, Bologna, Italy, (2000)