When Nuclear Is Not Enough:
A Tangled Tale of Two Experiments

By Steven B. Krivit
and the New Energy Times Team

July 30, 2010
Updated: Dec. 12, 2012

This updated version includes additional text shown in these red boxes. Additionally, some text has been edited for clarity.

In a 1994 science experiment, four – and only four – helium samples were 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
4	1407.7h	9/16/94	09:30	2.077

Image from EPRI TR-107843-V1 *pg 3-221, pdf pg 349*

Image from EPRI TR-107843-V1 *pg 3-222, pdf pg 350*

An assumption that bears on this entire matter is that there are no other significant nuclear products and emissions in LENRs except heat and 4He. This assumption is false. At the time when McKubre wrote the various reports that are referenced in this investigation, he was searching for a direct correlation between evolved heat and produced 4He in LENRs. He thought, and was challenged by critics, that such correlation would prove “cold fusion” as real.

The experiments took place at SRI International, in Menlo Park, California.

The experiment series was called “M4.”

These measurements eventually became known by "cold fusion" people, mostly the ones in the United States, as the best evidence for D+D “cold fusion.”

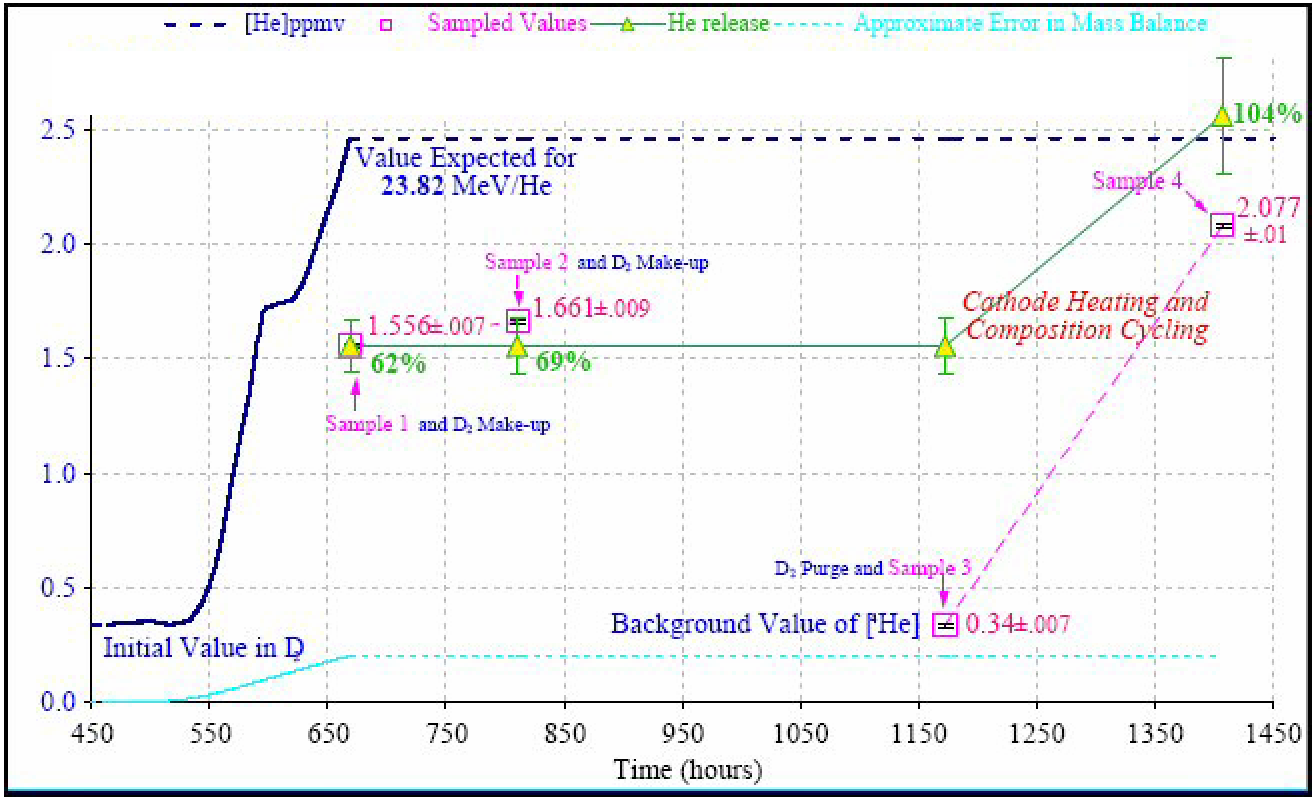


Image from McKubre, ICCF-10, 2003

Almost identical to Hagelstein, McKubre et al., Department of Energy Review, 2004

Since 1989, researchers have been trying to figure out the riddle of “cold fusion.” Some think it’s a new kind of fusion. Others think it’s definitely nuclear, just not fusion - maybe an electroweak interaction. They call it Low-Energy Nuclear Reaction (LENR) research.

This is the story of how one of the most prominent researchers in the field, Dr. Michael McKubre, made multiple inexplicable changes to data and to interpreted values from M4 during a 10-year period.

His objective seems to have been to help his colleague, friend and MIT professor Peter Hagelstein, who claims to have a theory that explains “cold fusion.”

Hagelstein was claiming a D+D -> 4He “cold fusion” from the mid-1990s to at least 2012.

Krivit/Winocur, 2004 “Rebirth of Cold Fusion”, NPR, Gellerman, Nov 10, 2005

Things had been going well in the 1990s. McKubre was the director of the Energy Research Center at SRI. The SRI team conducted outstanding research, some of the best in the field.

By 1998, McKubre's group had reported very strong evidence for nuclear energy and nuclear products – from chemical cells.

That, alone, is an important achievement because, since the 19th century, people haven't thought this was possible.

Some of SRI's work, including the helium observations, was published in June 1998 by the Electric Power Research Institute (TR-107843-V1).

According to an SRI staff member, the Energy Research Center program ended in the late 1990s when EPRI stopped funding it.

Only one little problem:

While the ink was still wet on the EPRI report, McKubre's group did another experiment: a replication of inventor Lester Case's LENR work.

The replication showed evidence for excess heat and helium (31 MeV/4He) that was produced at the same time.

But the data didn't fit Hagelstein's cold fusion theory, which unambiguously predicted D+D -> 4He + ~24 MeV/4He heat and no other nuclear products.

But McKubre made the data fit.

McKubre 2000

Let's begin looking at the M4 helium measurements. They're a little hard to see so I put arrows next to them.

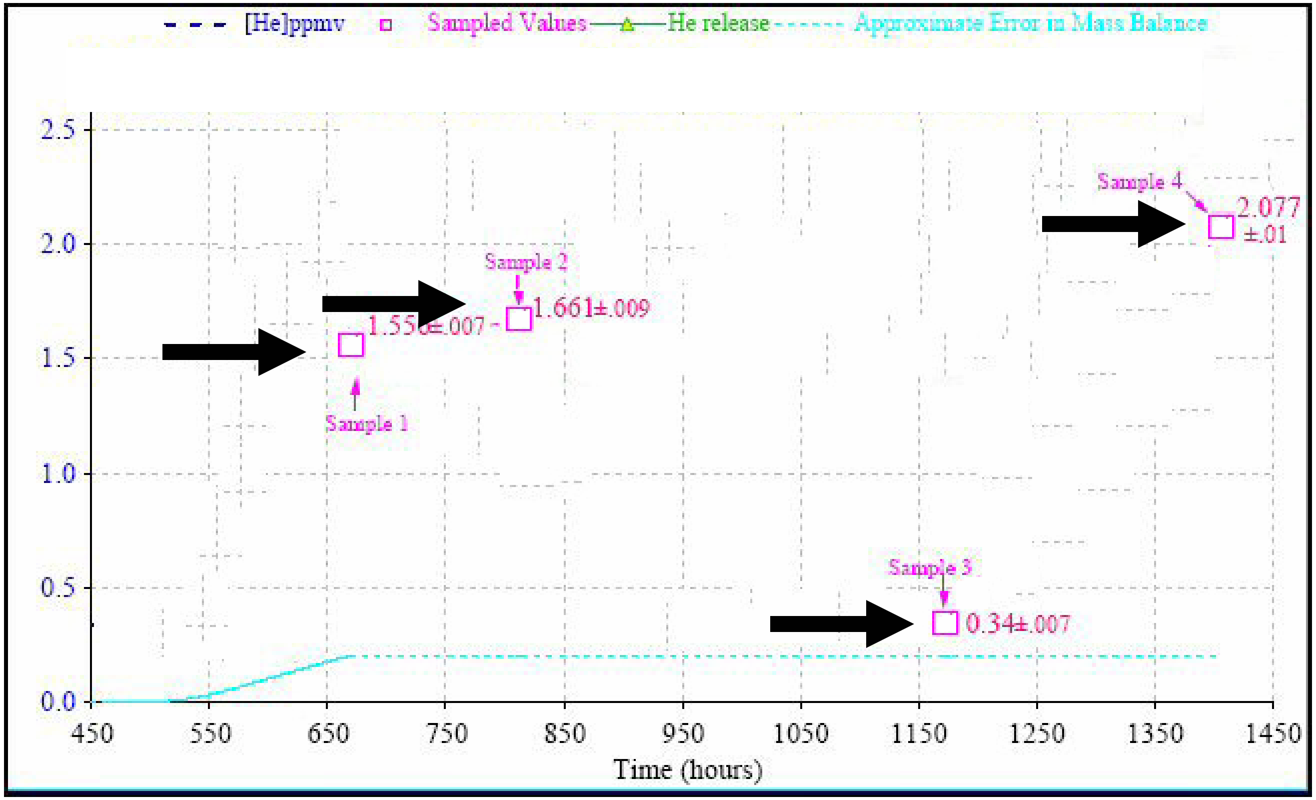


Image from McKubre, ICCF-10, 2003. Bold black arrows added by SBK.

We'll highlight them so we can watch them closely when they move.

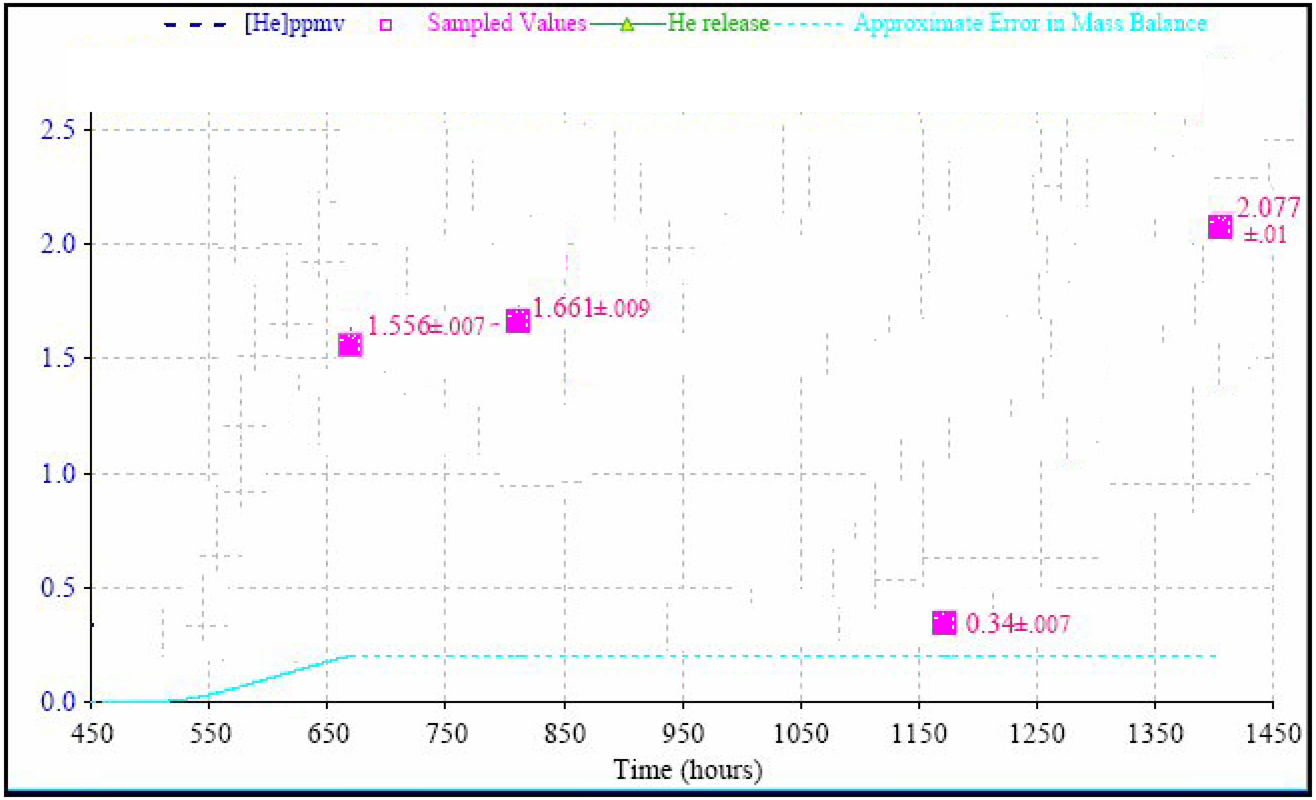


Image from McKubre, ICCF-10, 2003. Squares filled by SBK.

Remember that these are the actual measured helium-4 samples, as my labels show.

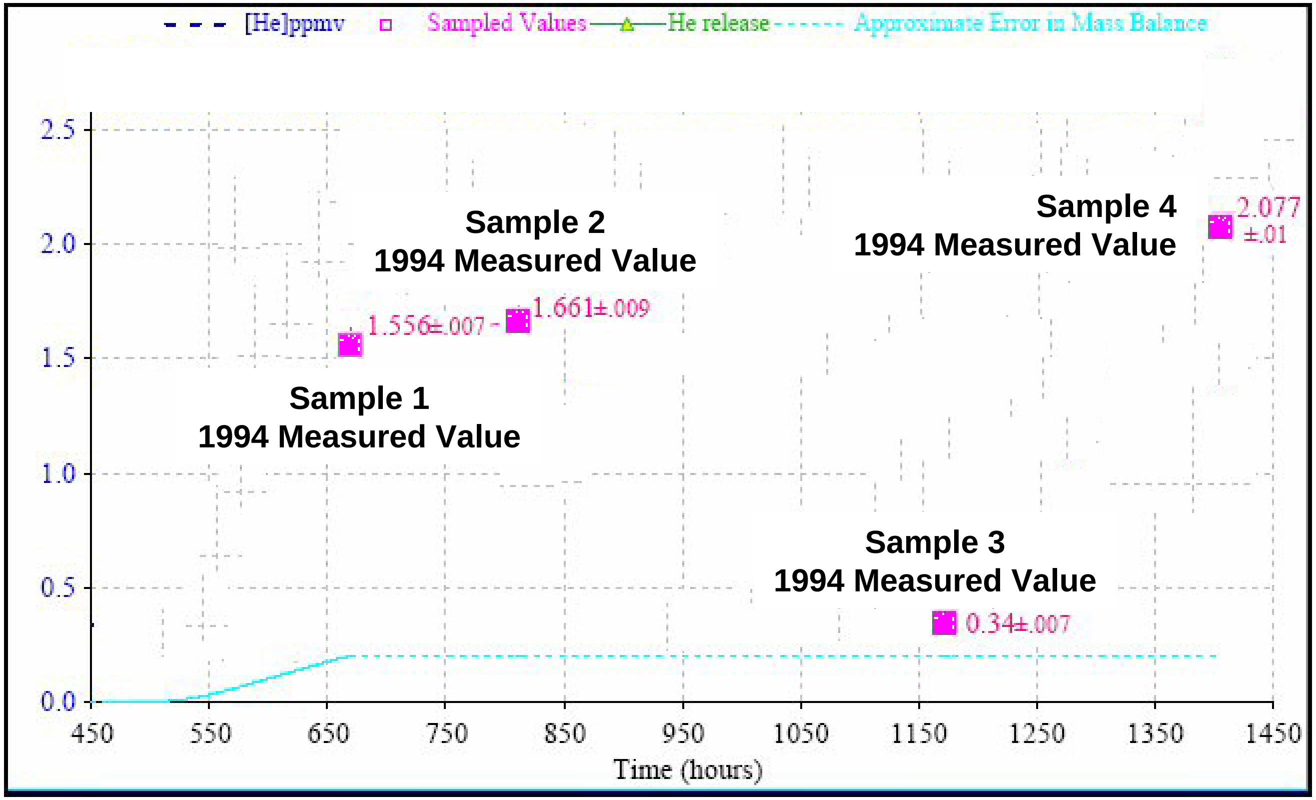


Image from McKubre, ICCF-10, 2003. Bold black text added by SBK.

But we've got to simplify this, so let's call them S1, S2, S3 and S4.

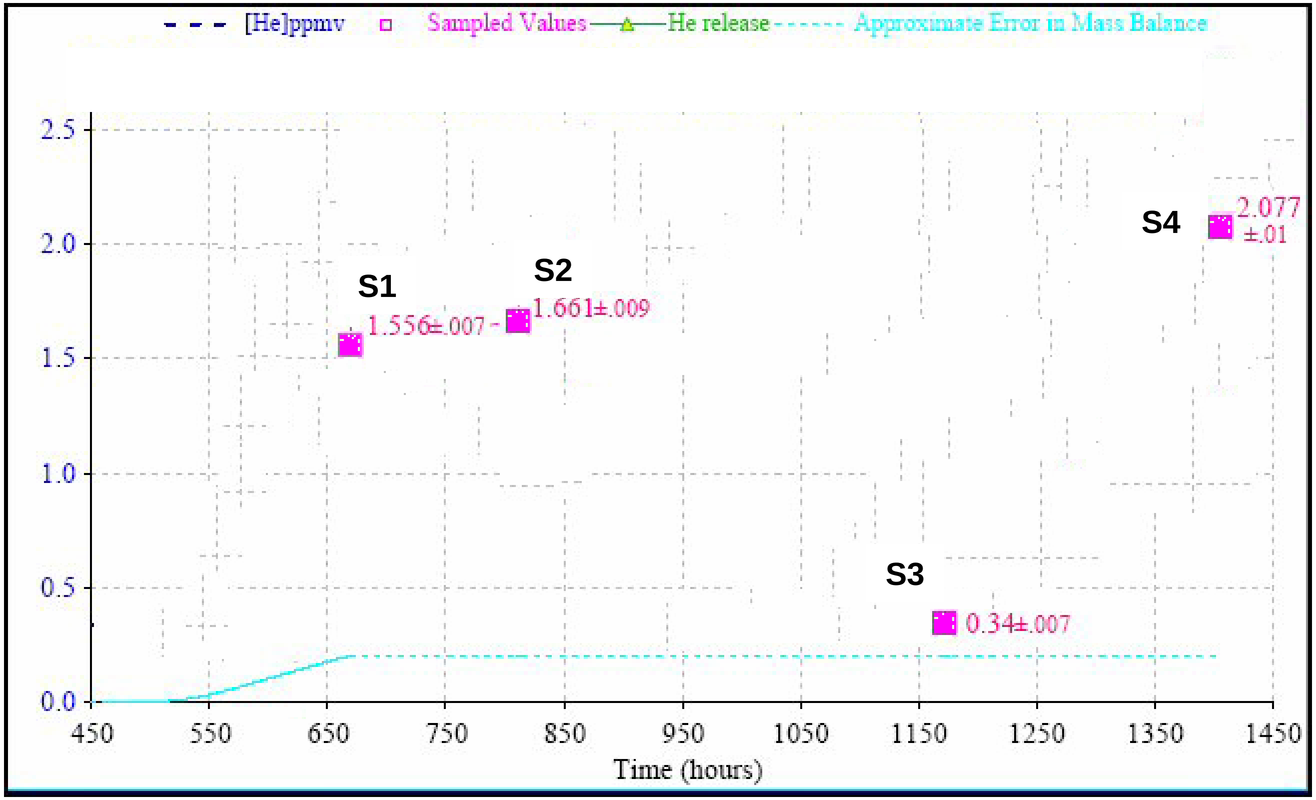


Image from McKubre, ICCF-10, 2003. Bold black text added by SBK.

One of the major questions the researchers on this project asked was,

“How close do these measurements of helium and heat come to the idea of D+D ‘cold fusion’?”

The ~24 MeV evidence of “cold fusion” was imagined by many researchers, but it was never observed.

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

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

Image from EPRI TR-107843-V1 *pg 3-222, pdf pg 350*

The SRI researchers’ first helium sample, taken after they observed a significant energy burst, showed 1.556 ppm of helium.

But concentration of helium in ppm by itself doesn’t tell us anything meaningful.

It tells us nothing about how close the experiment came to the prediction of ~24 MeV based on Hagelstein's theory of "cold fusion."

We have to first find out the concentration of helium in relation to the volume of the cell.

Researchers at SRI, one of the most respected labs in the United States, calculated the helium concentration (ppm) in the cell at the time of each sample in relation to the cell volume as follows:

then from 82.45 kJ we expect

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

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

V = Volume of cell plus manifold $\approx 250 \text{ cm}^3$

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

$= 2.30 \times 10^{16}$

thus

Δppm = 3.42 ppm

Δppm = 3.42 ppm

Δppm = 3.42 ppm

Image from EPRI TR-107843-V1 *pg 3-222, pdf pg 350*

We also have to know how much energy was produced - that is, how much heat the researchers measured calorimetrically.

The next slide shows that they had four heat bursts. The signal on the first burst wasn’t too strong, and they didn’t get a good measurement on the fourth burst, but the second and third bursts were strong and well-measured.

The green lines show the heat bursts that were measured during the experiment.

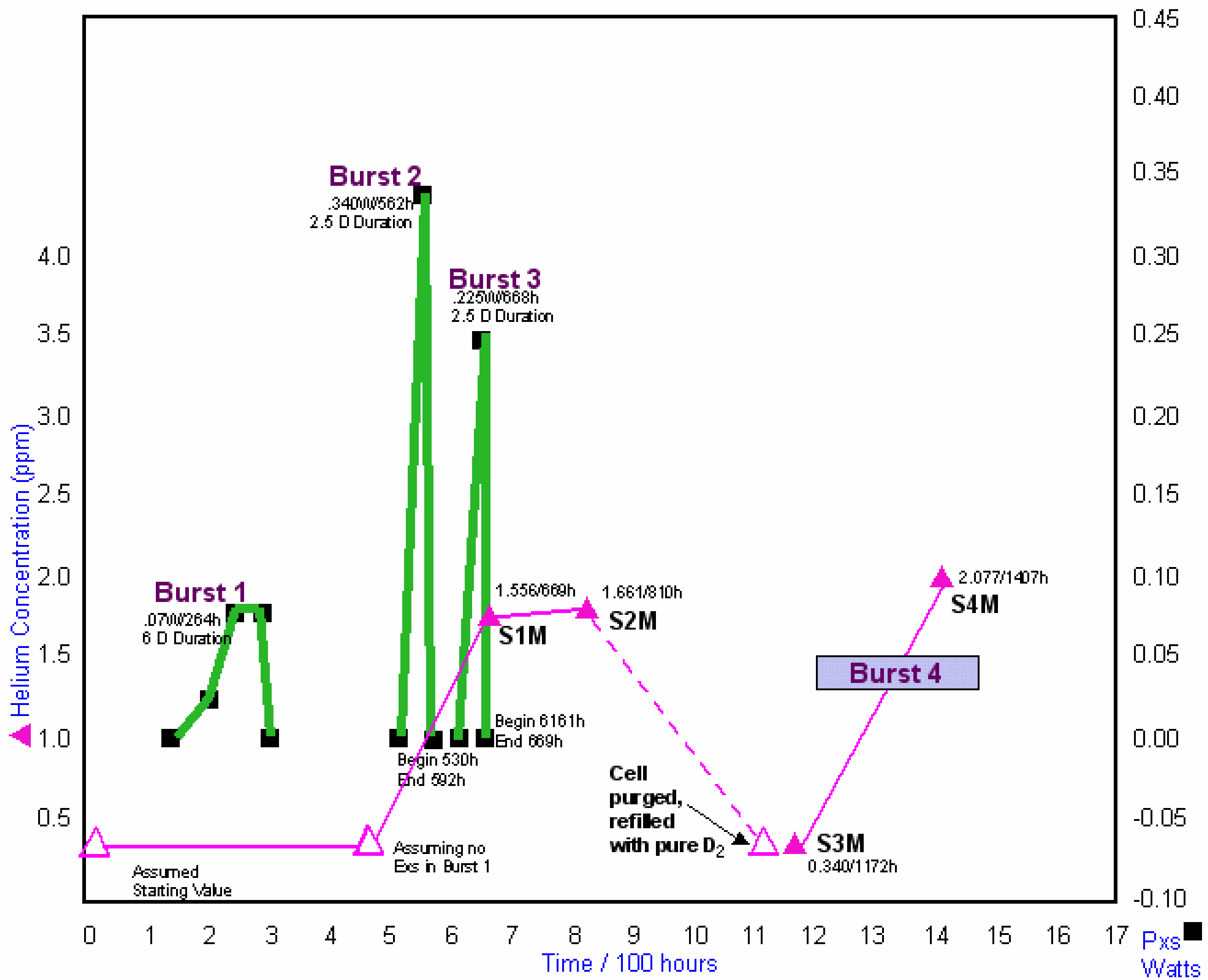


Image by SBK based on data and image from EPRI TR-107843-V1. See slide presentation for NET #34.

Once the researchers knew how much heat was produced in the experiment, they could back-calculate how much helium they should have seen based on the D-D “cold fusion” prediction of ~24 MeV per 4He atom.

So they end up with two sets of numbers:

1. predicted (or expected) helium concentration
2. measured helium concentration

In the text of their technical report shown on the next slide, they show a ratio of measured/predicted helium concentration as a percentage.

Their first helium sample, which measured 1.556 ppm, comes out to **41%** of the amount they predicted, 3.76 ppm.

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

Image from EPRI TR-107843-V1 *pg 3-222, pdf pg 350*

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

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

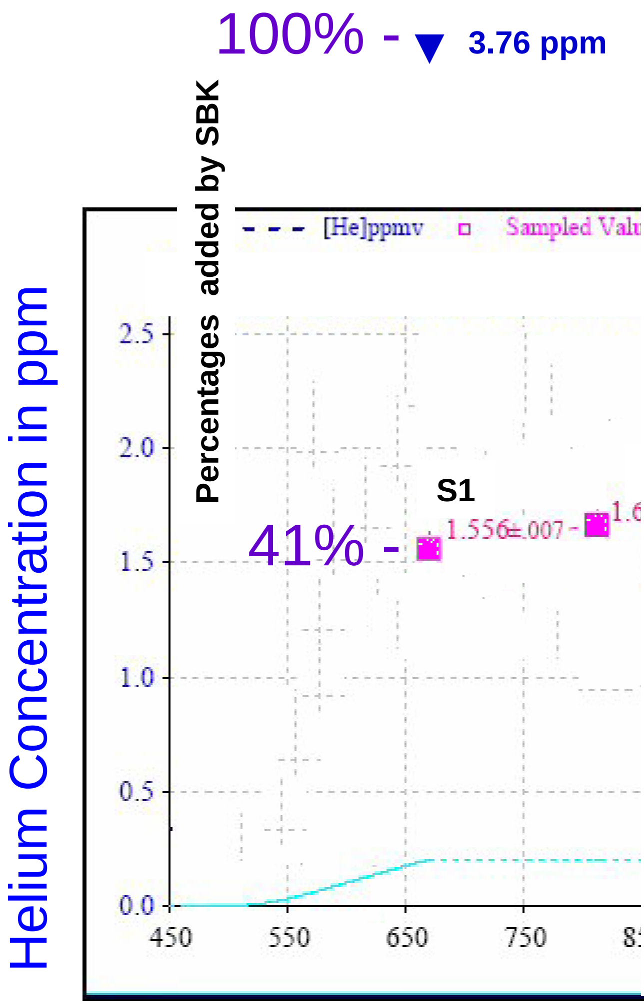
Image from EPRI TR-107843-V1 *pg 3-223, pdf pg 351*

Now that we know both the

- predicted (or expected) helium concentration
- measured helium concentration

we can plot them on the graph.

Remember that the relationship between measured and predicted is a different scale from the ppm concentration measurements.



They told us in the report that their measured value (1.556 ppm) was only 41% of their predicted value (3.76 ppm).

Therefore, we can identify the 1.556 point as 41% and the 3.76 point (which lands above the top of this graph) as 100%.

The 100% point is a baseline and we will identify it with a blue triangle.

Image from McKubre, ICCF-10, 2003. Bold black arrows and text added by SBK. Purple scale and Y-axis label added by SBK.

This slide is new to this version and was added for clarity.

S1 Prediction as Presented in 1998

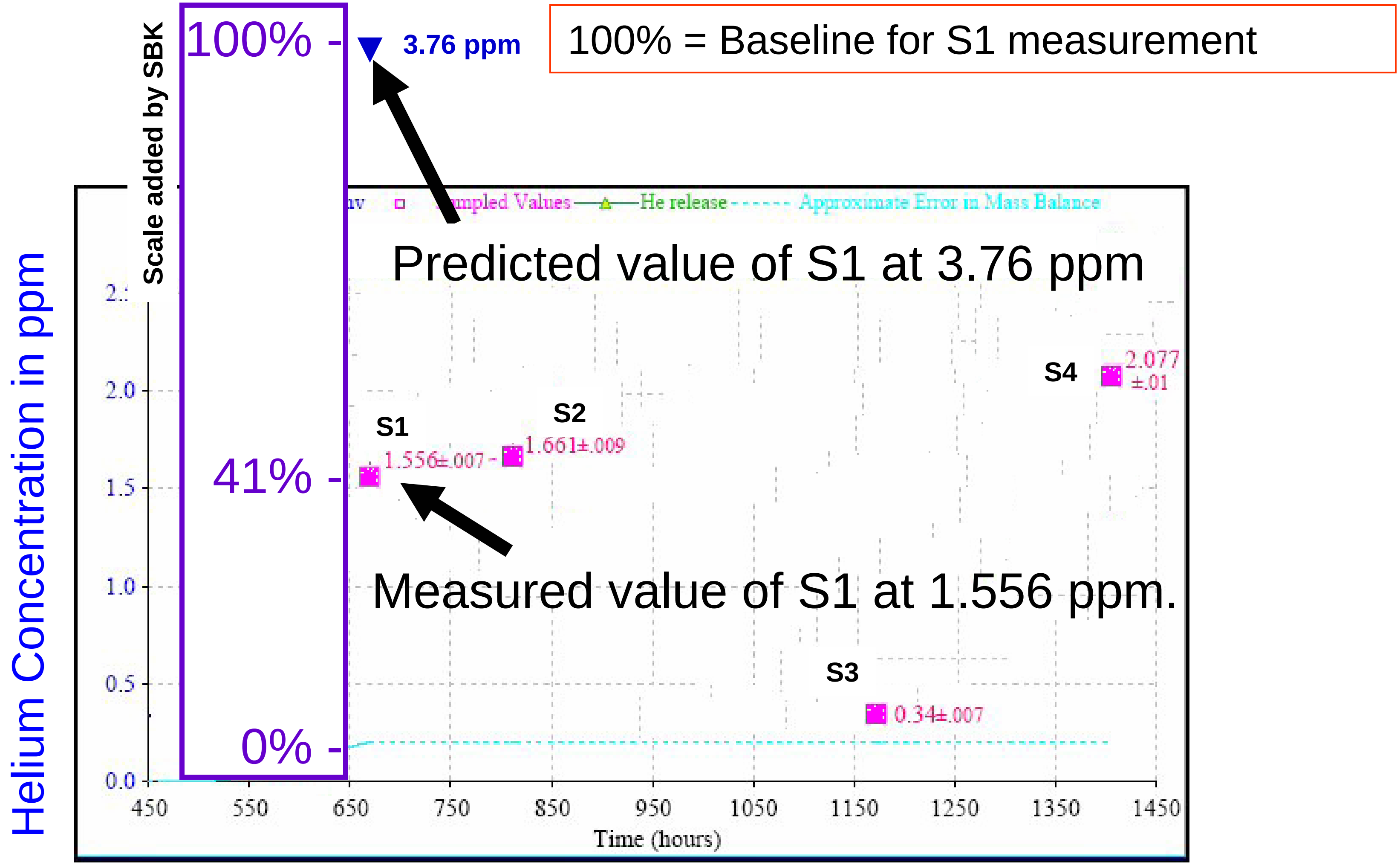


Image from McKubre, ICCF-10, 2003. Bold black arrows and text added by SBK. Purple scale and Y-axis label added by SBK.

The points for the 1.556 ppm measurement, the 100% baseline, and zero, are now enclosed in their own scale (purple line).

During the next week of experiment M4, SRI researchers did some electrochemical things to the cell and took a second helium sample.

We don't know why they decided to take the second sample at the time they did. Maybe they thought more helium might appear.

Here are their calculations for the helium concentration in the second sample. Note that the value is specific to that sample, not a cumulative amount of helium.

Sample 2. The gas sampled at 669h (Sample 1) had 1.556 ppm ^4He. 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 ^4He in Sample 2.

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

$$= 1.13 \text{ ppm}$$

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

Image from EPRI TR-107843-V1 *pg 3-223, pdf pg 351*

But now, instead of having too little helium to match the prediction of their “cold fusion” theory, they had too much helium!

Let’s do the math:

$$1.66/1.13 = \textcolor{blue}{147 \text{ percent}}$$

Remember that the volume is different now because of the amount of gas they removed in the first sample.

So the relationship between measured and predicted helium concentrations of sample 2 is only about sample 2; it has nothing to do with sample 1.

S2 Prediction as Presented in 1998

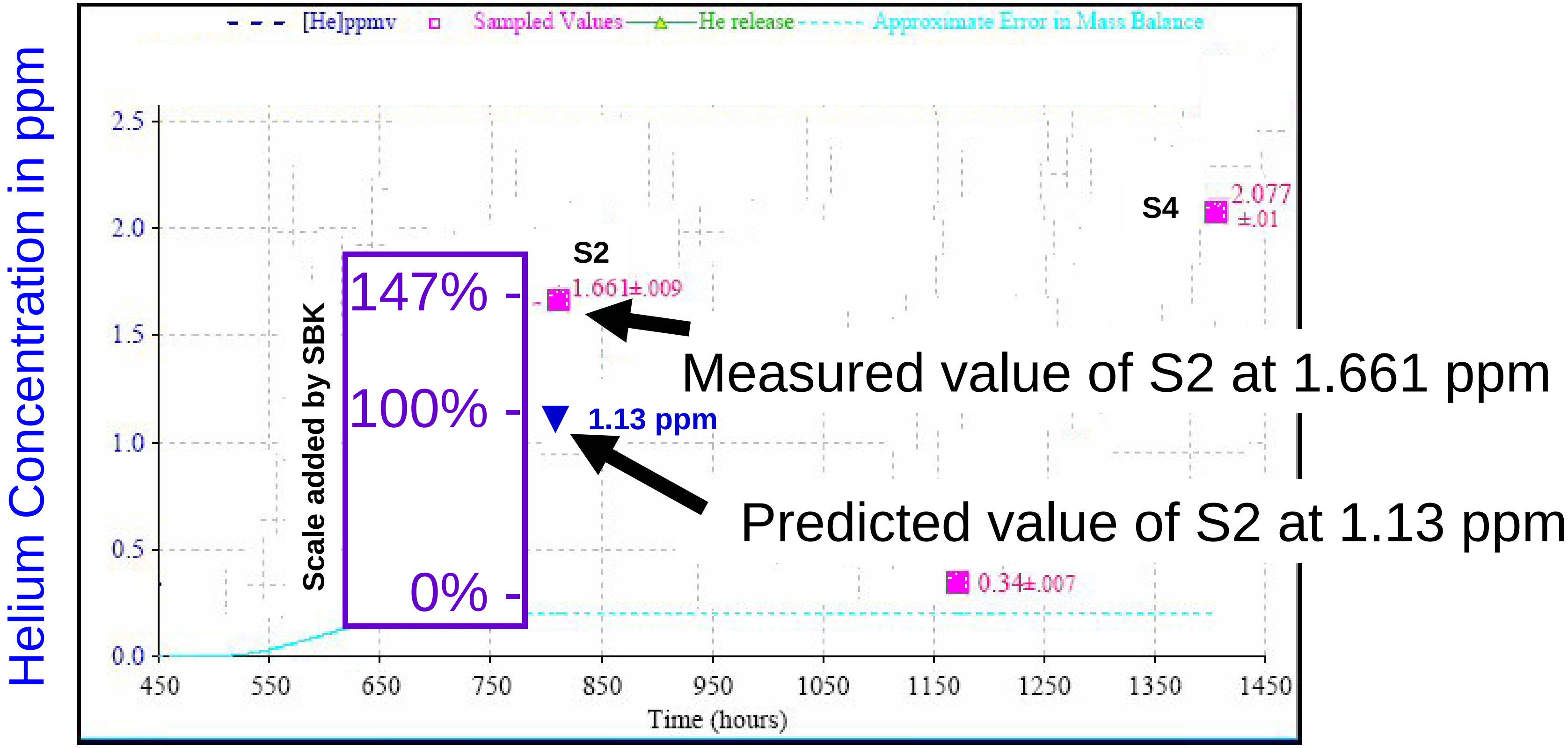


Image from McKubre, ICCF-10, 2003. Bold black arrows and text added by SBK.
Purple scale, Y-axis label, blue triangle and blue text added by SBK.

For sample S2, the 100% "baseline" is at 1.13 ppm

Now move the clock forward two years to 2000. We’re at the Eighth International Conference on Cold Fusion. The place: Lerici, Italy.

Michael McKubre presents a collection of experimental results showing very clear evidence of nuclear-scale heat and the nuclear product helium, produced in the experiment at the same time.

He’s shown that these experiments are creating nuclear energy and products from chemical cells.

By itself, this is revolutionary.

Only one problem:

His best experiment, a rigorously performed and carefully measured Case replication*, doesn't show the correct amount of helium predicted by Hagelstein's D-D "cold fusion" theory.

The Case replication experiment was performed in 1998, four years after M4 took place.

And the Case experiment shows 31 MeV heat / 4He atom.

Not 24 MeV.

* A deuterium gas-phase, activated carbon and palladium-black experimental system which is very different from an aqueous liquid deuterium Fleischmann-Pons electrolytic chemical cell.

McKubre 2000

This is the part of the story that gets tricky.

McKubre explained why the results from the 1998 Case replication don't agree with Hagelstein's D-D "cold fusion" theory.

Strangely, he suggests that something (we'll get to that) his group did back in 1994 can explain how the 1998 Case replication experiment "confirms" Hagelstein's D-D "cold fusion" theory.

Never mind the fact that the two experiments are completely different, one with deuterium gas, the other a liquid electrolytic system.

Now we get into

The “Cold Fusion” Helium Retention Hypothesis

McKubre said in 2000 that he figured out why the 1994 M4 experiment produced too little helium to prove Hagelstein’s D-D “cold fusion” theory.

(Pay no attention to the fact that the second sample from M4 produced *too much* helium.)

McKubre makes an assumption, or rather, he claims “evidence of sequestered helium” in his 2000 paper.

McKubre 2000

He didn’t come up with this idea out of the thin air. Take a look at the 1998 EPRI report. This idea was one of four that his group imagined could explain the source of the helium.

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

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

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

Image from EPRI TR-107843-V1 *pg 3-224, pdf pg 352*

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

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

Image from EPRI TR-107843-V1 *pg 3-225, pdf pg 353*

The conclusion listed five possibilities. Among them was that maybe the helium did not “hide out” but was created, in fact, after sample 3.

Conclusions

1. We cannot rule out the possibility that ^4He 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 ^4He on the assumption that

   a. the system is helium leak tight, and

Image from EPRI TR-107843-V1 *pg 3-228, pdf pg 356*

Notice that they didn't test the helium retention idea. They explicitly said that it "must be tested" before making any "definitive statements."

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 ^4He, and diffusional in-leakage, while possible, would be very hard to account for quantitatively.

4. The possibility of ^4He hide-out and slow emergence into the gas phase must be tested by experiment. This applies to both the ^4He thought to be produced by reaction [1] and to an initial inventory of ^4He in the LiOD and PTFE, due to equilibration with the ambient.

5. Definitive statements will be difficult to make about ^4He production in this or future experiments unless or until it is measured at several times the ambient background level.

Image from EPRI TR-107843-V1 *pg 3-229, pdf pg 357*

Let's review: In 1998, SRI reported measurements of two helium samples (pink squares) and the predictions of what those measurements should have been (blue triangles).

▼ 100% (1998P)

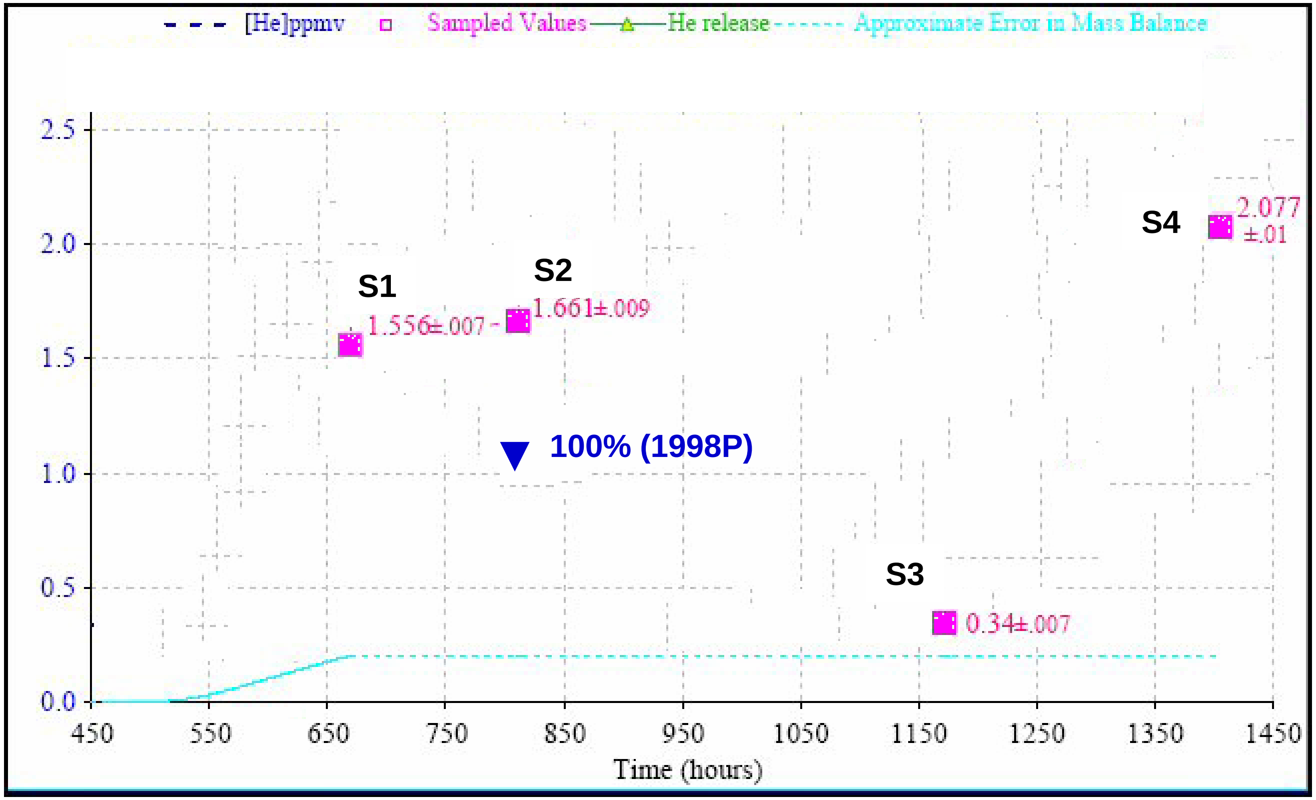


Image from McKubre, ICCF-10, 2003. Bold black text added by SBK. Blue triangle and blue text added by SBK.

We spoke about their idea of helium retention, which they still needed to test.

There are two more samples we need to talk about.

After the researchers took sample 2, they didn't see any more heat.

So they flushed the cell and got ready for more electrochemical stuff. Toward the end of the flush, they took sample 3, effectively a background value.

Sample 3 is effectively a background value.

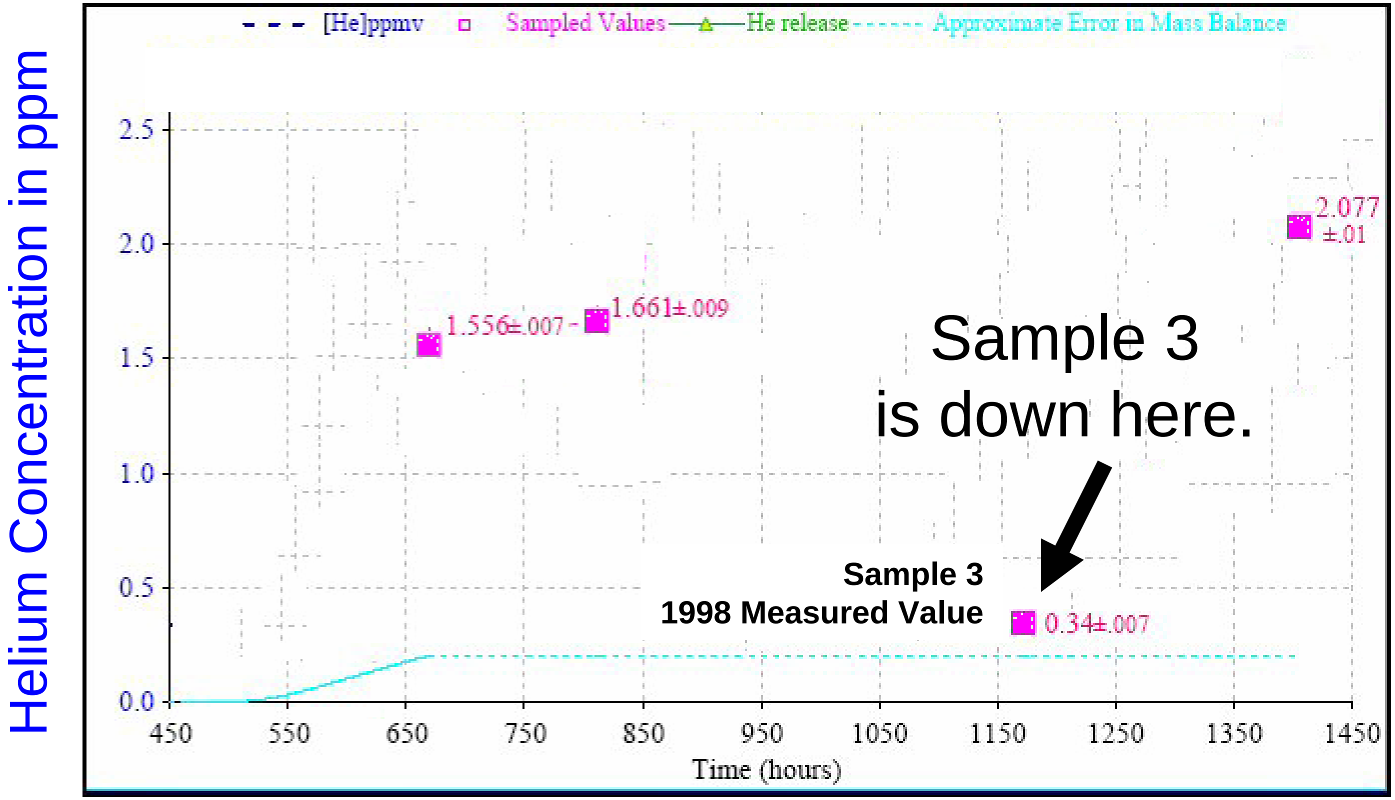


Image from McKubre, ICCF-10, 2003. Bold black arrows and text added by SBK.
Y-axis label added by SBK.

After they flushed the cell and took sample 3, they started doing electrochemical stuff again.

They even had a 76-minute heat burst they called a “mini boil-off,” during which the electrolyte temperature rose from 45C to 57C.

But the mass flow to their calorimeter had stopped during this period, so they weren't able to get an accurate heat measurement.

For a variety of reasons during this period

– a bunch of electrochemical stuff they did,
– lack of a good reading on any possible excess heat,
– confusion about which heat, if any, was responsible,

they didn’t try to calculate a comparison between the measured and predicted values.

Remember that, in the 1998 report, there is no predicted value shown for S4 – not 84%, not 104%, none.

Regardless, in 2000, McKubre somehow came up with a predicted value for S4: 104%. This is the green triangle.

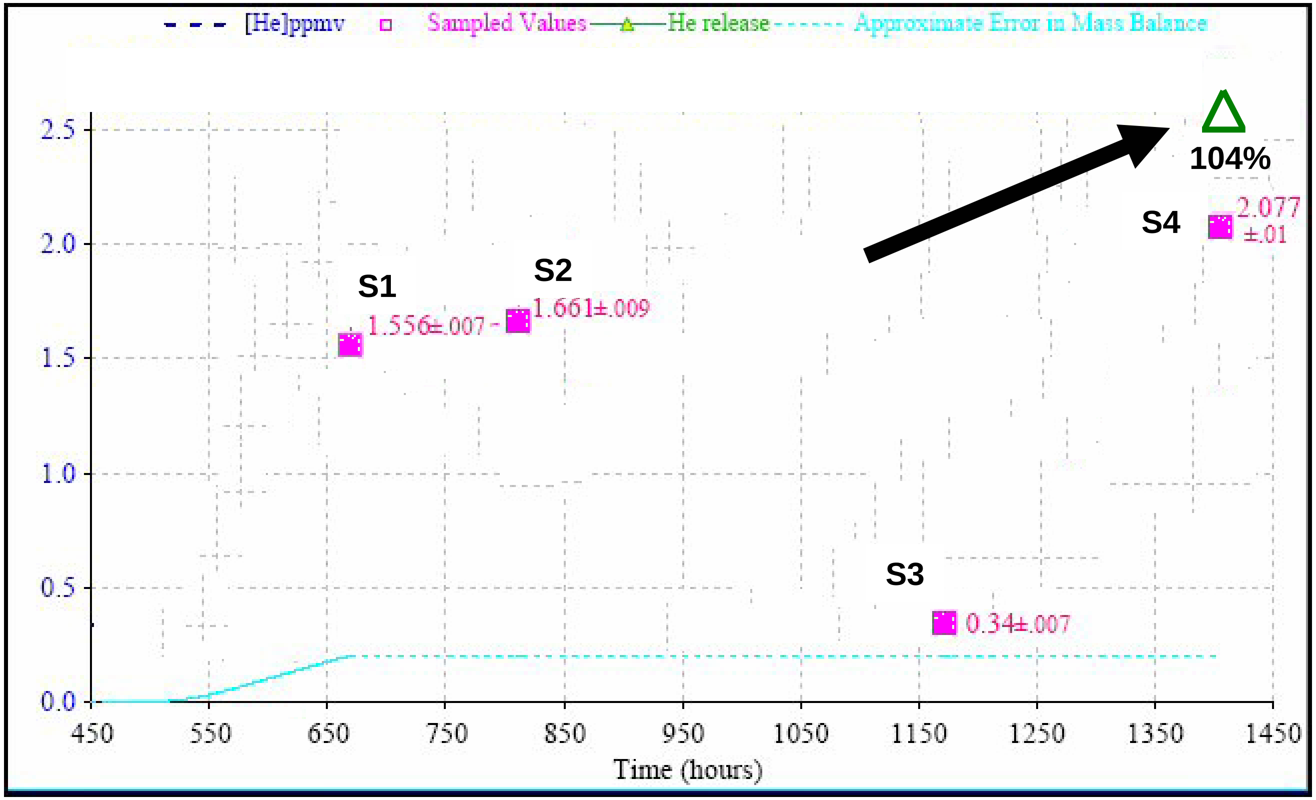


Image from McKubre, ICCF-10, 2003. Bold black arrows and text added by SBK. Green triangle (redrawn) and “104%” (re-written) come from McKubre, ICCF-10, 2003

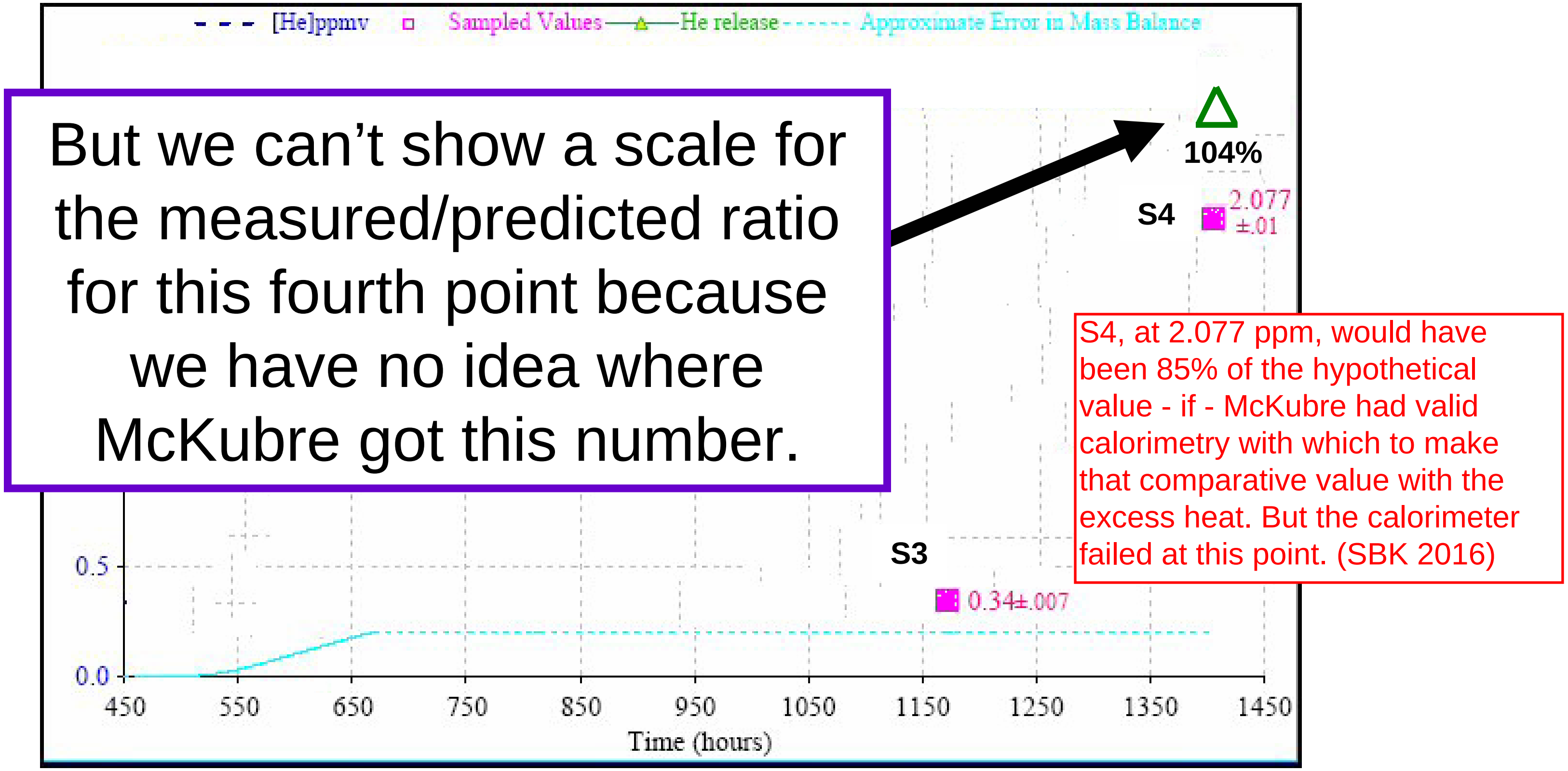


Image from McKubre, ICCF-10, 2003. Purple text box and black arrow added by SBK.

Anyway, 104% of 23.8 MeV comes out to 24.75 MeV, and cold fusion people were happy .

The proof is the 24 MeV!
McKubre nailed it.

- Scott Chubb, 2007

But how was McKubre able, in 2000, to calculate a predicted value of 104 +/- 10% MeV for this fourth point, when he hadn't been able to do so in 1998?

Here is what we know:

– He concluded that the helium measured in sample 4 came from the period before sample 3 was taken.

- He assumed that the fourth heat burst did not produce the helium that appeared between the time they flushed the cell and when they took S4.

– He concluded that helium was somehow hiding out in the cathode.

EPRI TR-107843-V1, McKubre 2000

– He wrote in 2000 that, during the time between sample 3 and sample 4, “the cathode was subjected to an extended period (~200 hours) of compositional and temperature cycling.”

– In a conversation with me, he once gave a simpler description of this procedure:

“shake and bake.”

– He knew somehow, or concluded, that electrochemical processes could somehow implant helium into a metal so it got stuck and that similar electrochemical processes could also release the stuck helium.

I later performed a literature search on McKubre’s helium retention idea and found that it was contradicted by the literature.

EPRI TR-107843-V1, McKubre 2000

Nobody in the LENR field seems to think that helium is created inside the bulk of the cathode. Here is what McKubre's colleague Peter Hagelstein wrote about helium in the bulk:

"It would be very difficult for the helium to diffuse into the bulk, since the associated time scale would be years or decades. Hence, one would not expect to see it in the bulk, and no measurement has indicated it in the bulk."

That’s just about all we know of
how McKubre got the 104% value.

There’s no public record of any
mathematical explanation.

I’ve asked him in writing for an
explanation three times.

Not one reply.

But there's more.

Remember the first two helium points and the predicted values McKubre stated in the 1998 report?

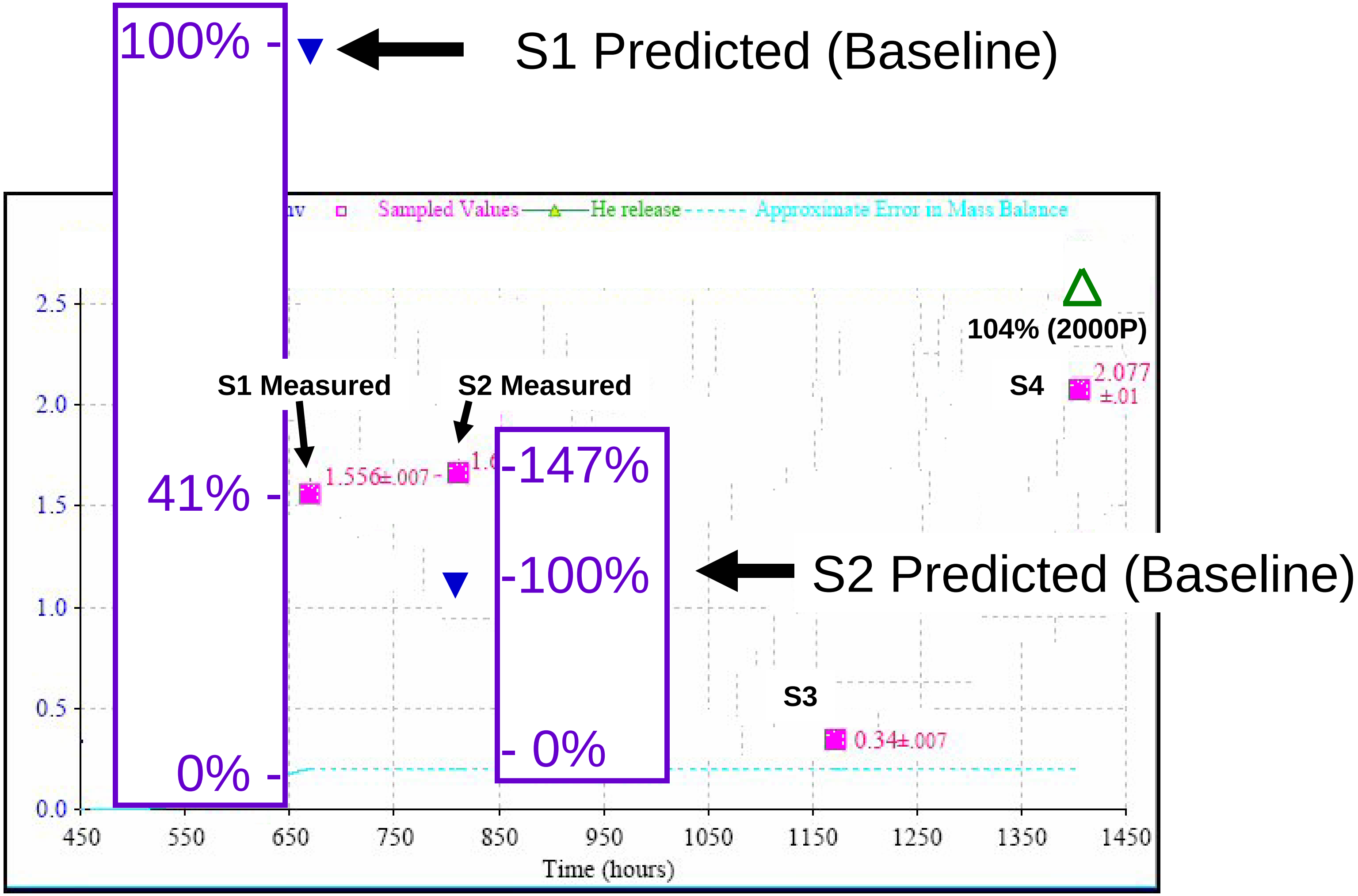
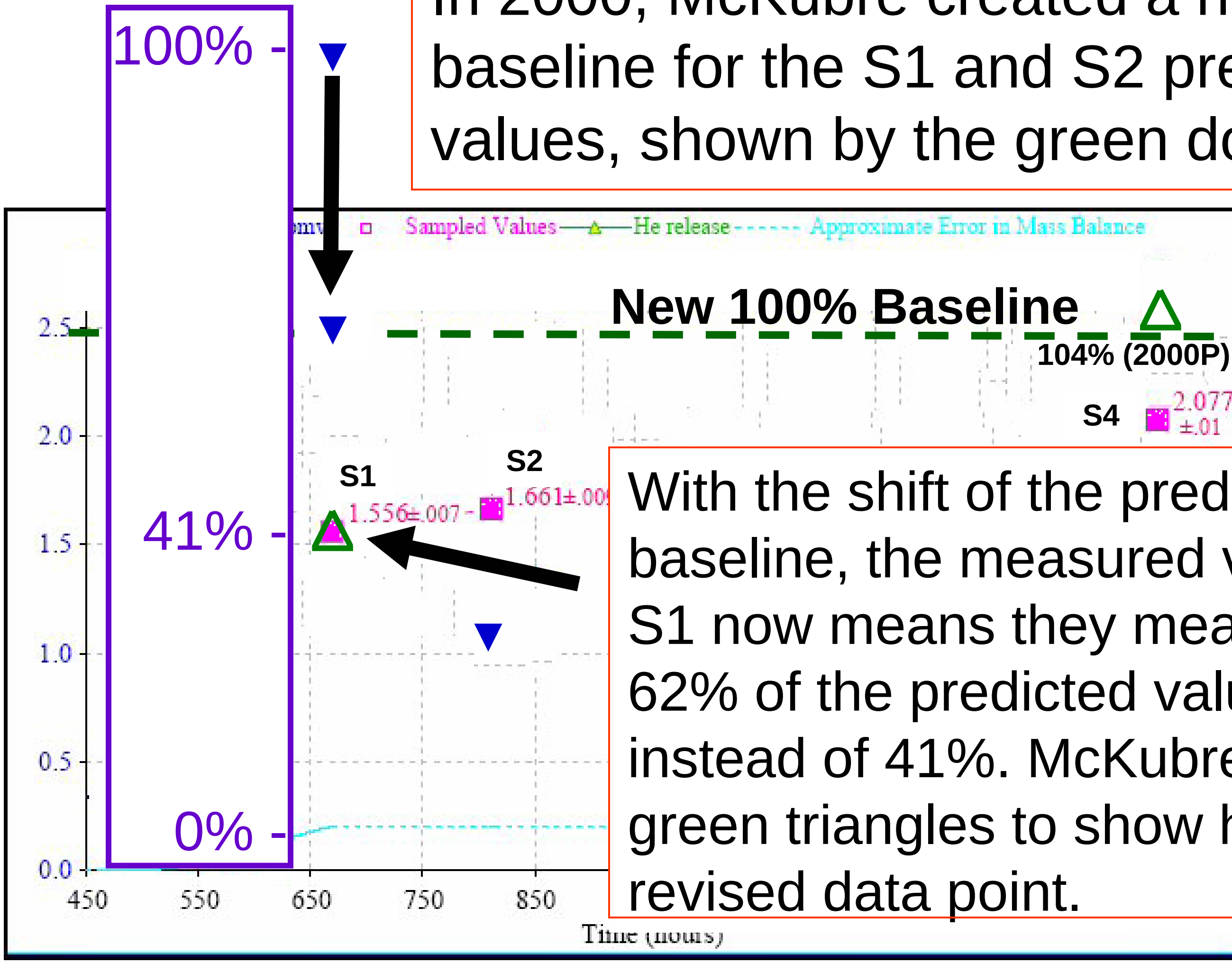


Image from McKubre, ICCF-10, 2003. Purple scale, blue triangle added by SBK.

Something happened to each of them.

Different things happened to each one.

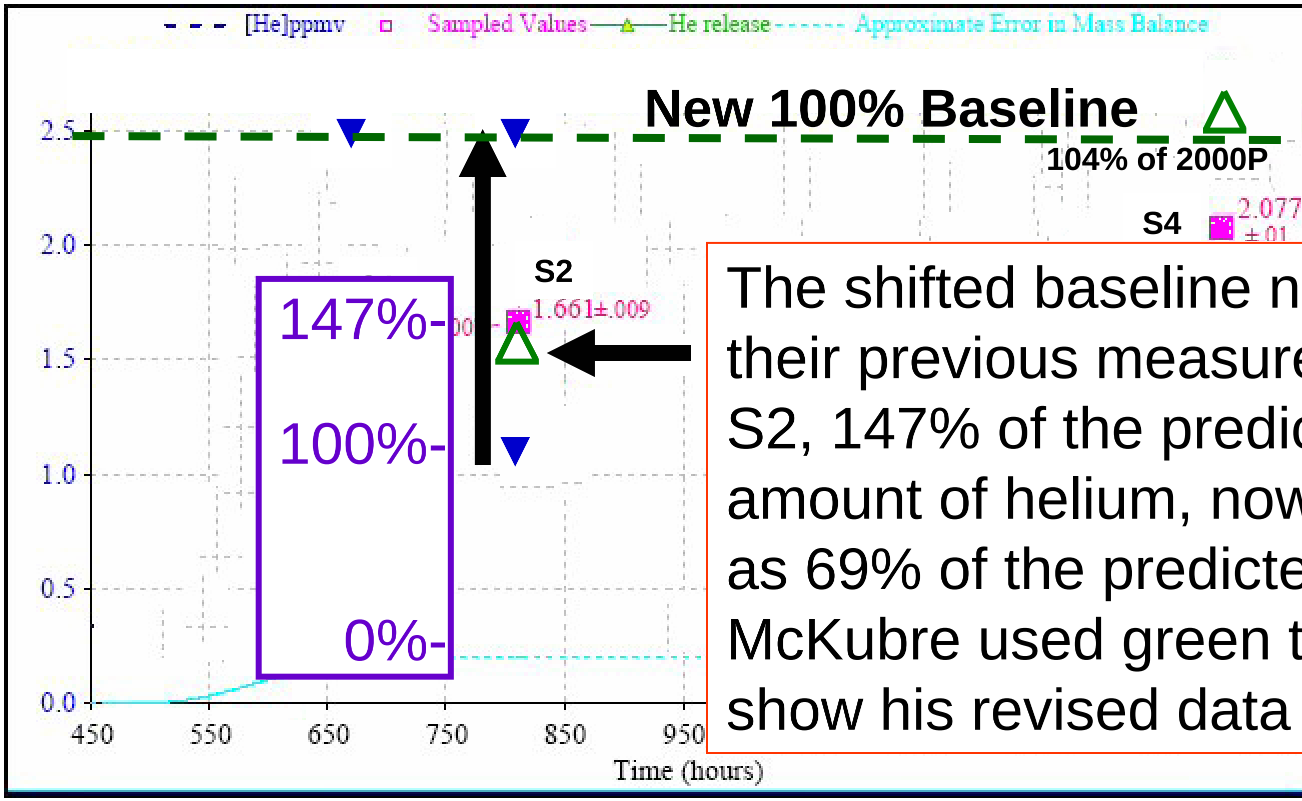
In 2000, McKubre created a new single baseline for the S1 and S2 predicted values, shown by the green dotted line.



With the shift of the predicted baseline, the measured value of S1 now means they measured 62% of the predicted value instead of 41%. McKubre used green triangles to show his revised data point.

Image from McKubre, ICCF-10, 2003. Purple scale, blue triangle added by SBK.
Green triangles (redrawn) and “104%” (re-written) come from McKubre, ICCF-10, 2003

You can’t create a single baseline for both values, they require individual scales and unique baselines for each sample.



The shifted baseline now makes their previous measurement for S2, 147% of the predicted amount of helium, now read only as 69% of the predicted value. McKubre used green triangles to show his revised data point.

Image from McKubre, ICCF-10, 2003. Bold black arrow and text added by SBK.
Purple scale, blue triangle added by SBK.

By shifting where “100%” is supposed to be, McKubre brings new meaning to the ppm values.

He shifted the theoretical baseline.

These shifts occur without any explanation, mathematical or otherwise.

There is NO explanation on record for these shifts.

McKubre discarded the 1998 predicted values, shown by the blue triangles, so let's clean things up and delete them here too.

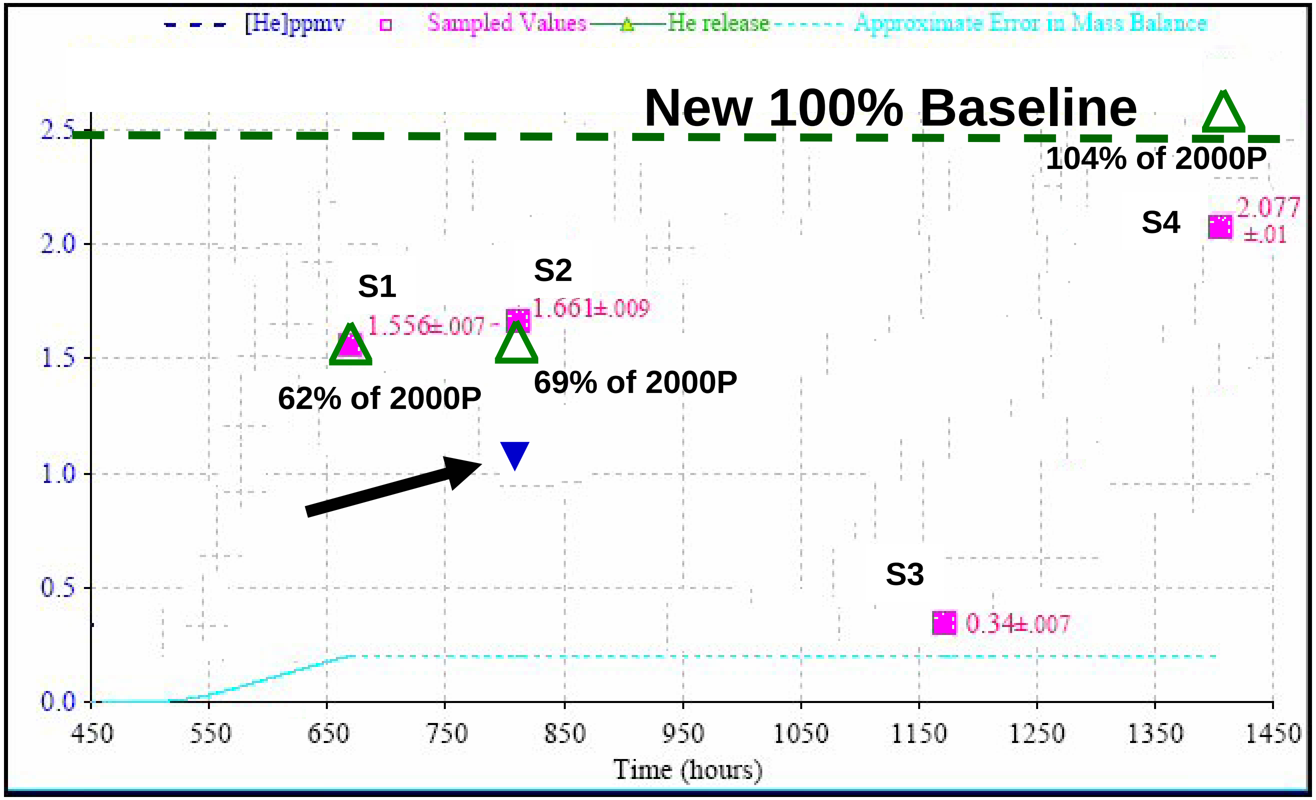


Image from McKubre, ICCF-10, 2003. Green triangles (redrawn) and bold black numerical values (re-written) come from McKubre, ICCF-10, 2003

Now only the new baseline and the 2000 stuff remains.

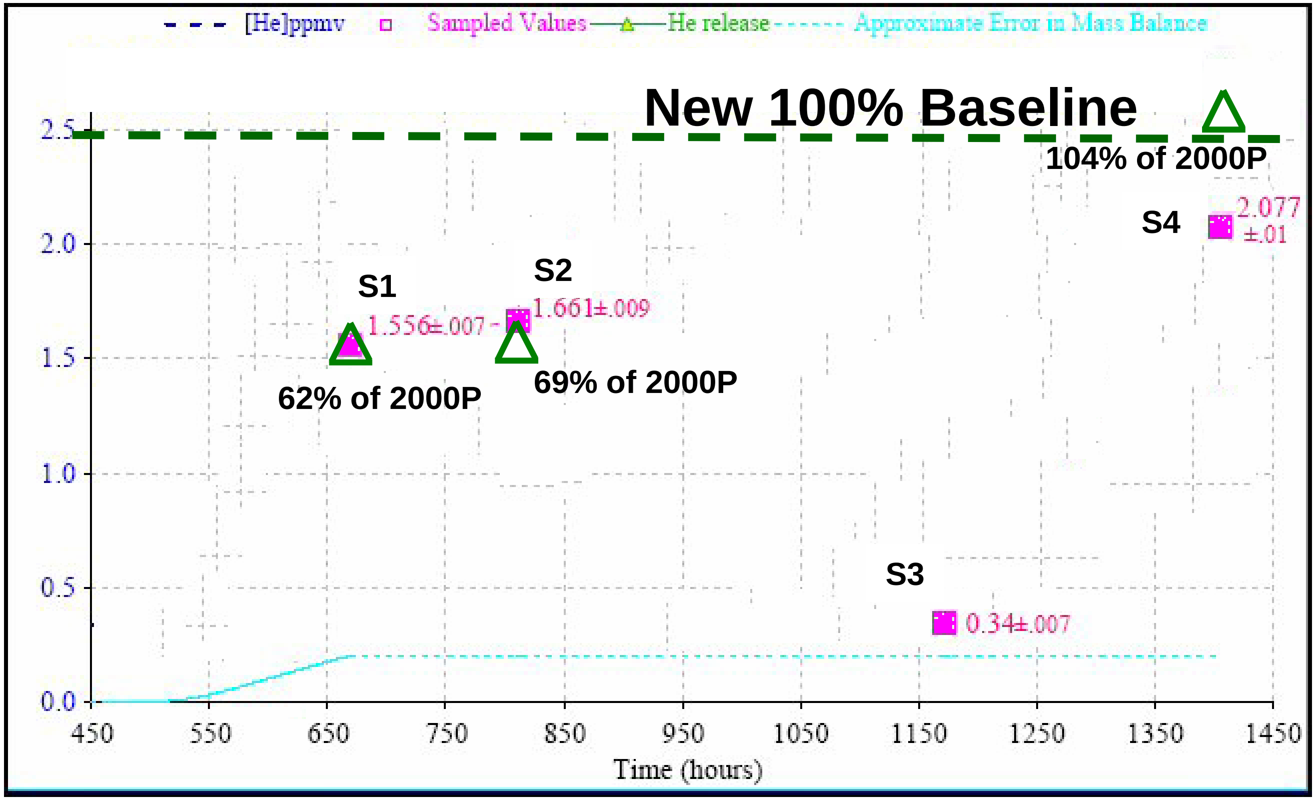


Image from McKubre, ICCF-10, 2003. Green triangles (redrawn) and bold black numerical values (re-written) come from McKubre, ICCF-10, 2003

Then McKubre adds a new “predicted” point above sample 3. His logic is that, even though the measurement was 0.34, helium was “hiding” in the Pd and it should have been 1.556

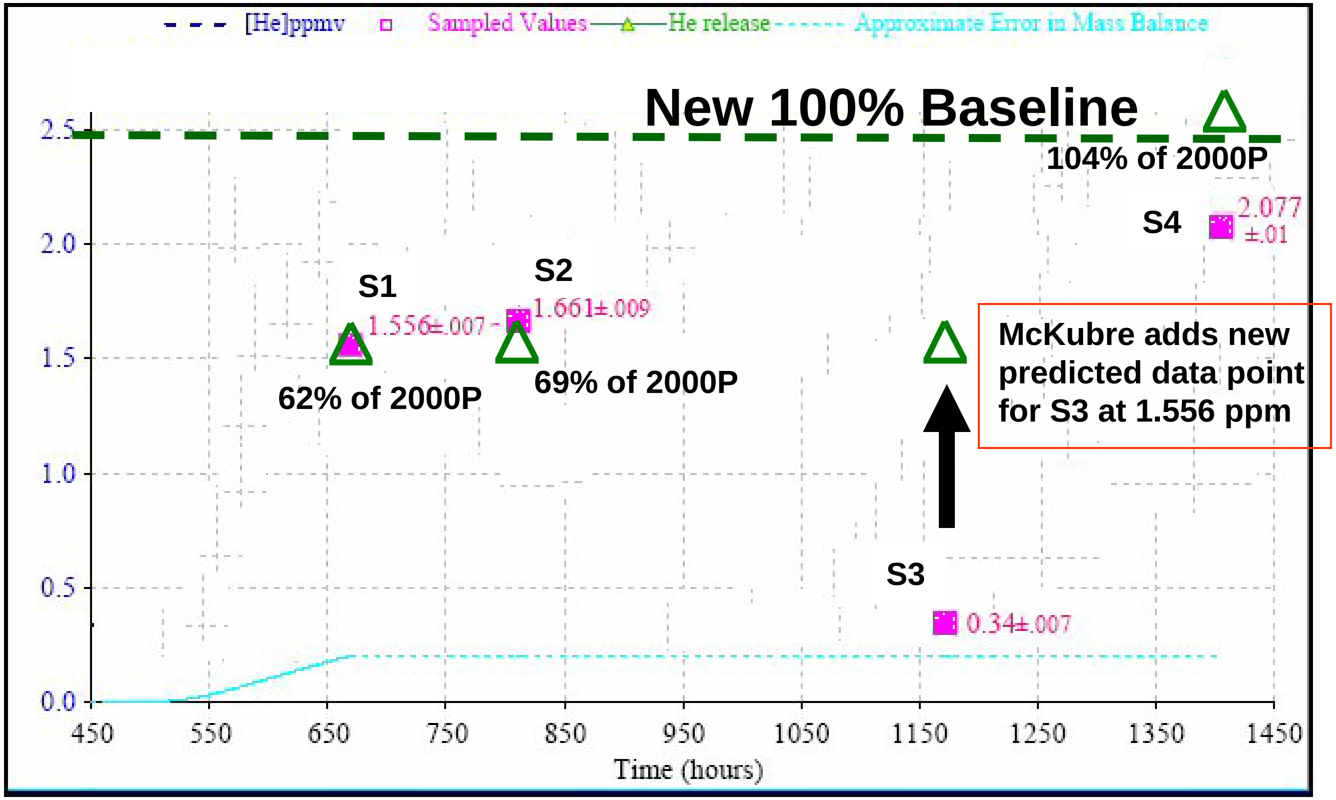


Image from McKubre, ICCF-10, 2003. Bold black arrow added by SBK. Green triangles (redrawn) and bold black numerical values (re-written) come from McKubre, ICCF-10, 2003

And he draws a green line to tie them all together in a nice-looking, coherent curve.

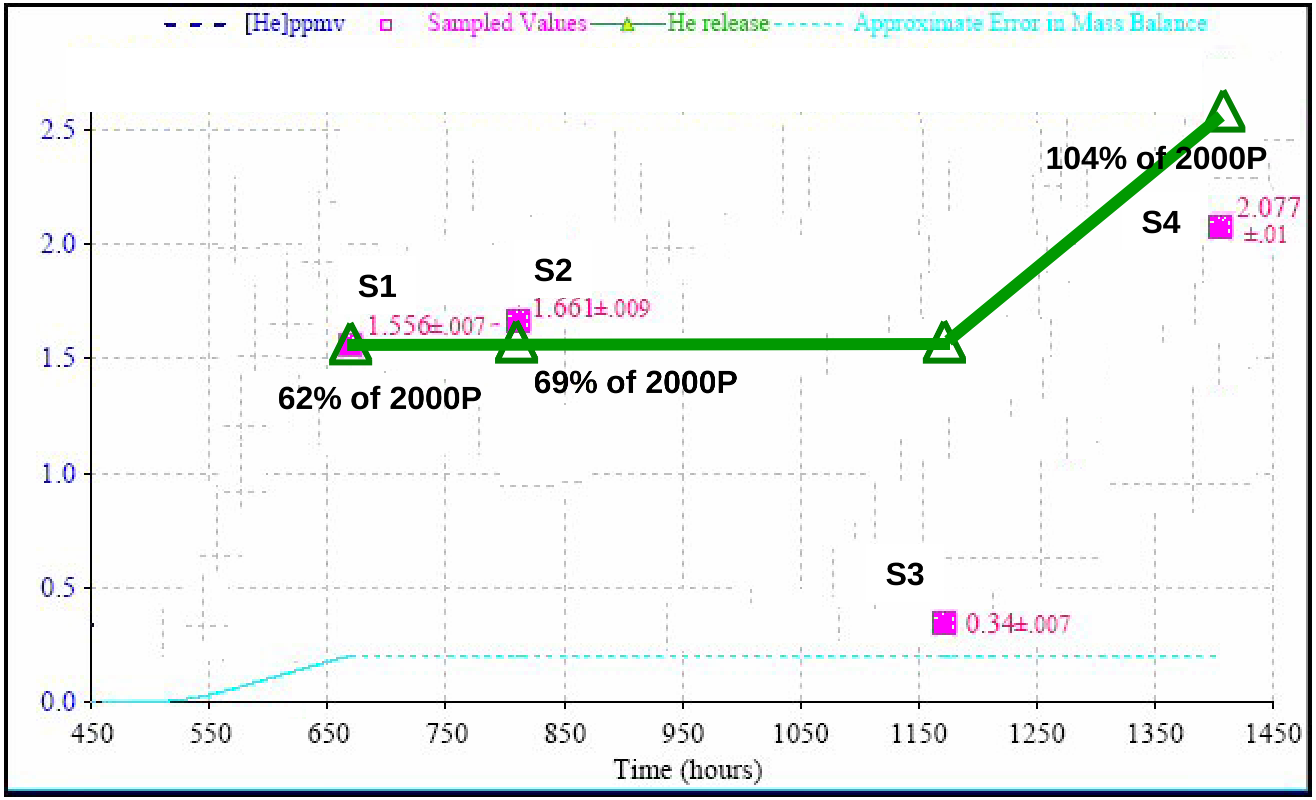


Image from McKubre, ICCF-10, 2003. Bold black arrow added by SBK. Green triangles and line (redrawn) and bold black numerical values (re-written) come from McKubre, ICCF-10, 2003

We still have no idea how he calculated S4 as "104%"

McKubre adds a solid blue line which accurately reflects the predicted cumulative helium formation based on the observed heat bursts #2 and #3.

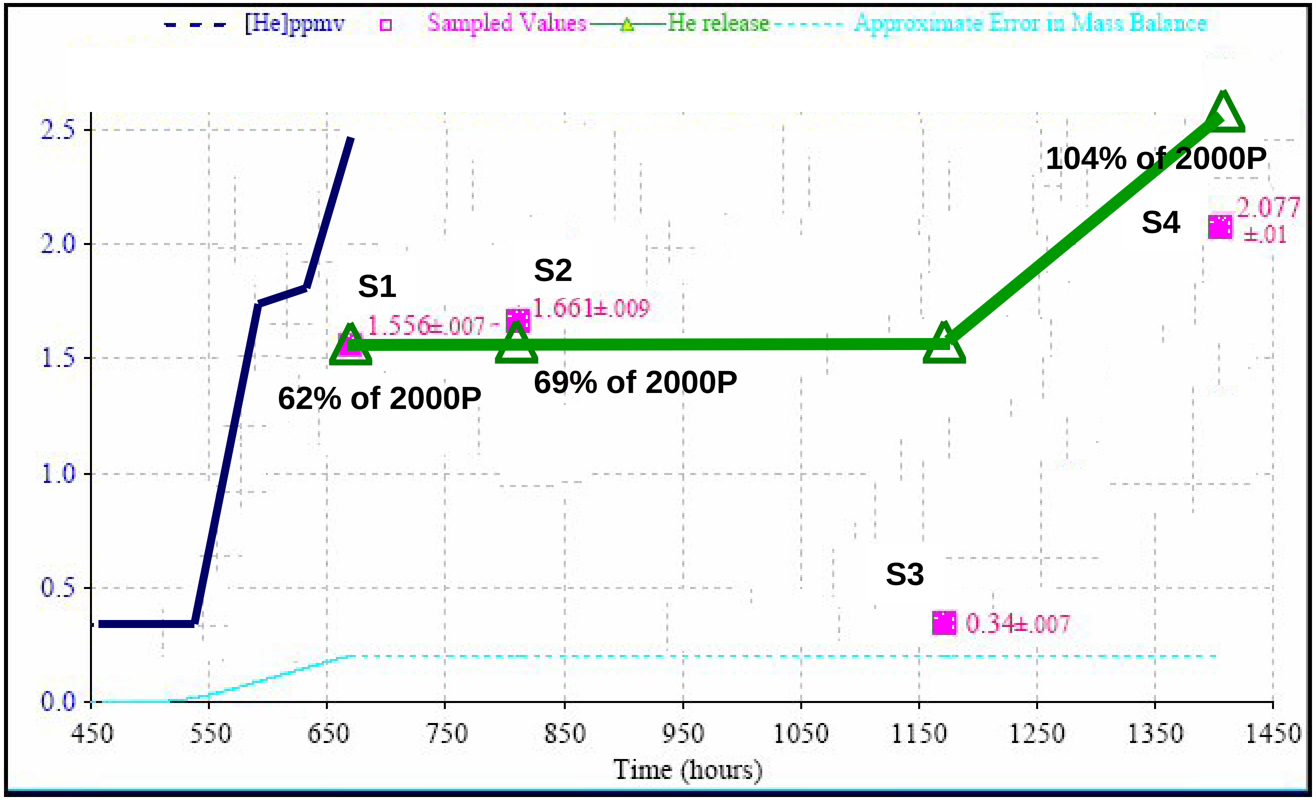


Image from McKubre, ICCF-10, 2003. Bold black arrow added by SBK. Green triangles, green line and blue line (redrawn) and bold black numerical values (re-written) come from McKubre, ICCF-10, 2003

The dotted blue line is supposed to represent the predicted value of helium given a ~24 MeV/4He reaction.

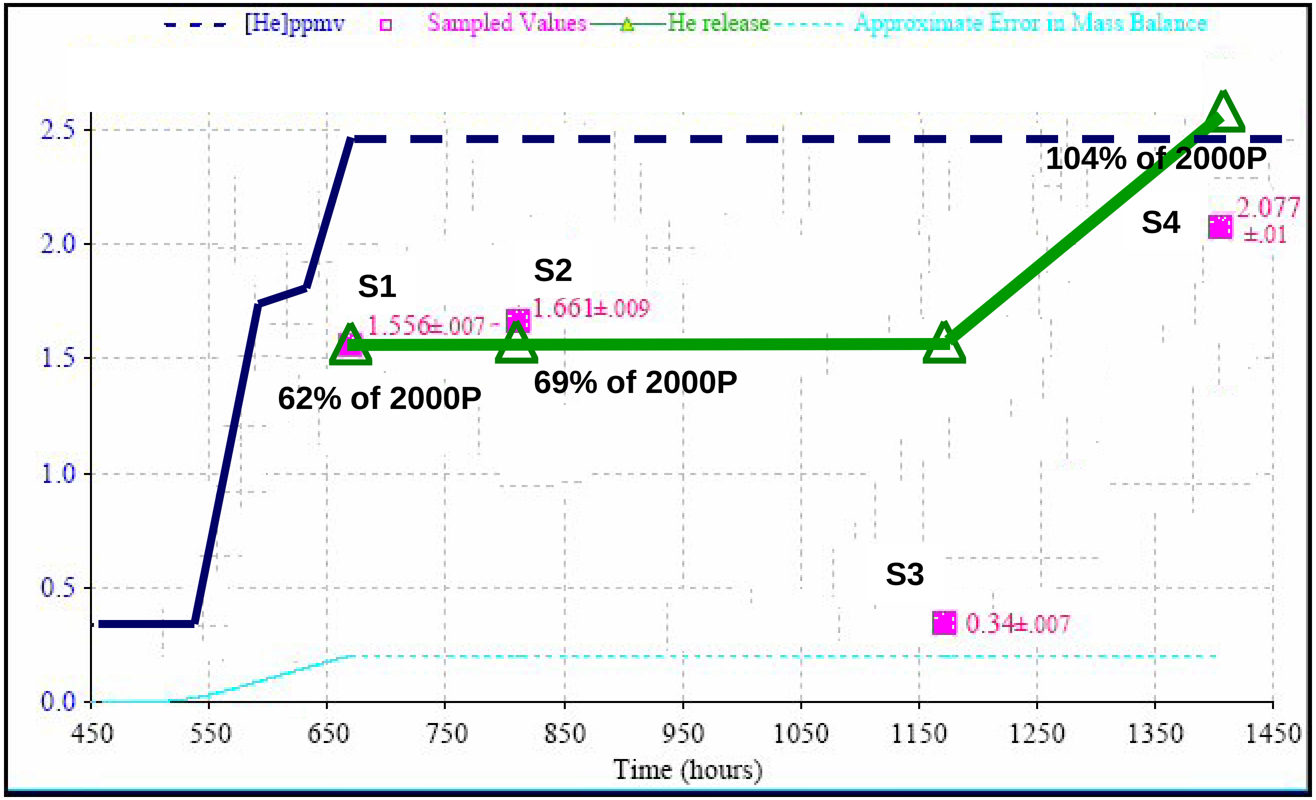


Image from McKubre, ICCF-10, 2003. Bold black arrow added by SBK. Green triangles, green line and blue line (redrawn) and bold black numerical values (re-written) come from McKubre, ICCF-10, 2003

McKubre implies that once the heat burst #3 completes at 668h, no more heat or 4He is produced. That is why his dotted blue line plateaus.

To get his data to meet his predicted value, McKubre said the missing helium “got stuck.” He claims that his ~200-hour “shake and bake” process, rather than the fourth heat burst, released the “missing” helium.

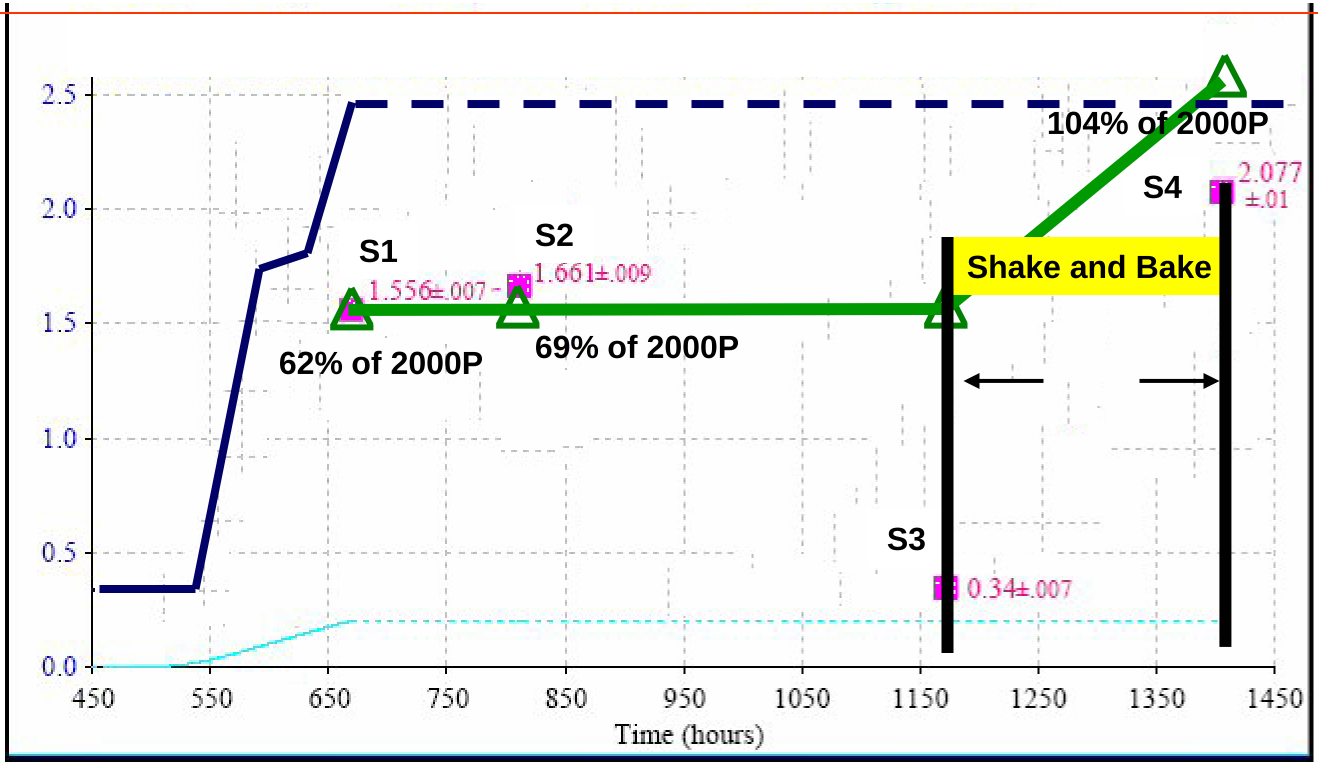


Image from McKubre, ICCF-10, 2003. Bold black arrow added by SBK. Green triangles, green line and blue line (redrawn) and bold black numerical values (re-written) come from McKubre, ICCF-10, 2003. “Shake and Bake” section added by SBK based on McKubre, ICCF-10, 2003

Our reconstruction of the graph McKubre presented to ICCF-10 and the Department of Energy is now complete.

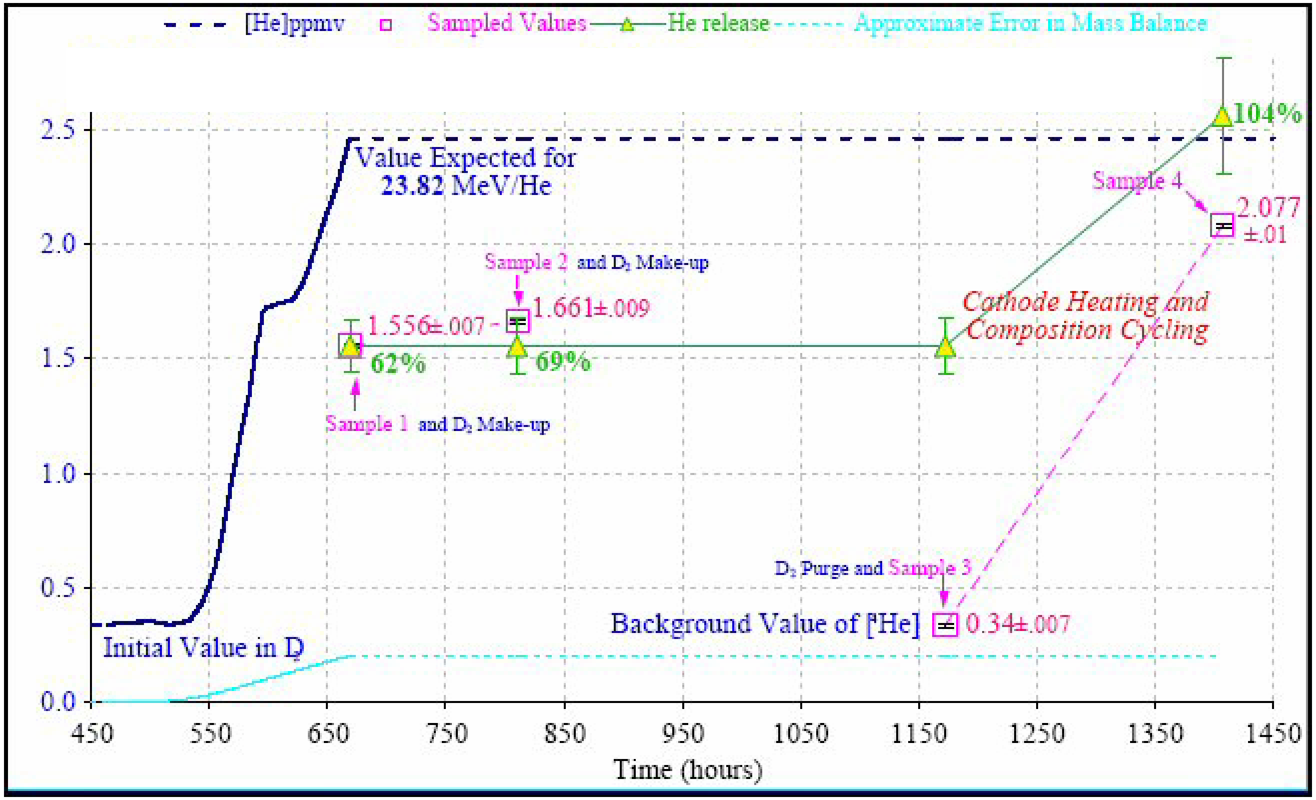


Image from McKubre, ICCF-10, 2003

But there are still more changes.

Three years later, at the 2007 APS conference, McKubre changed the data even more. Now, 13 years after the data was taken, the curves change again and new data points appear.

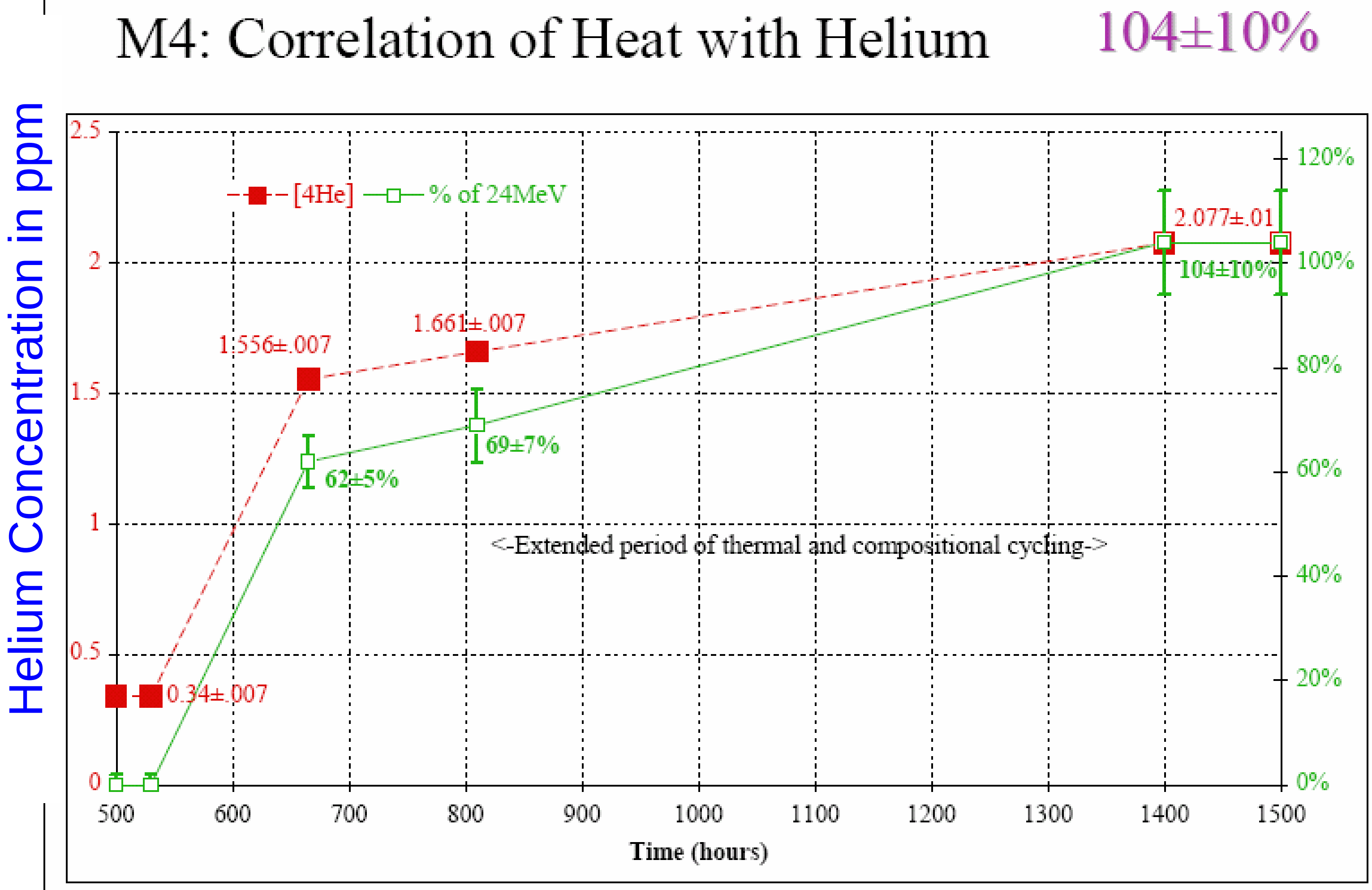


Image from McKubre, APS, 2007, Y-axis label added by SBK.

Let’s take it one step at a time and look at just the stuff in red, which is supposed to represent real experimental measurements.

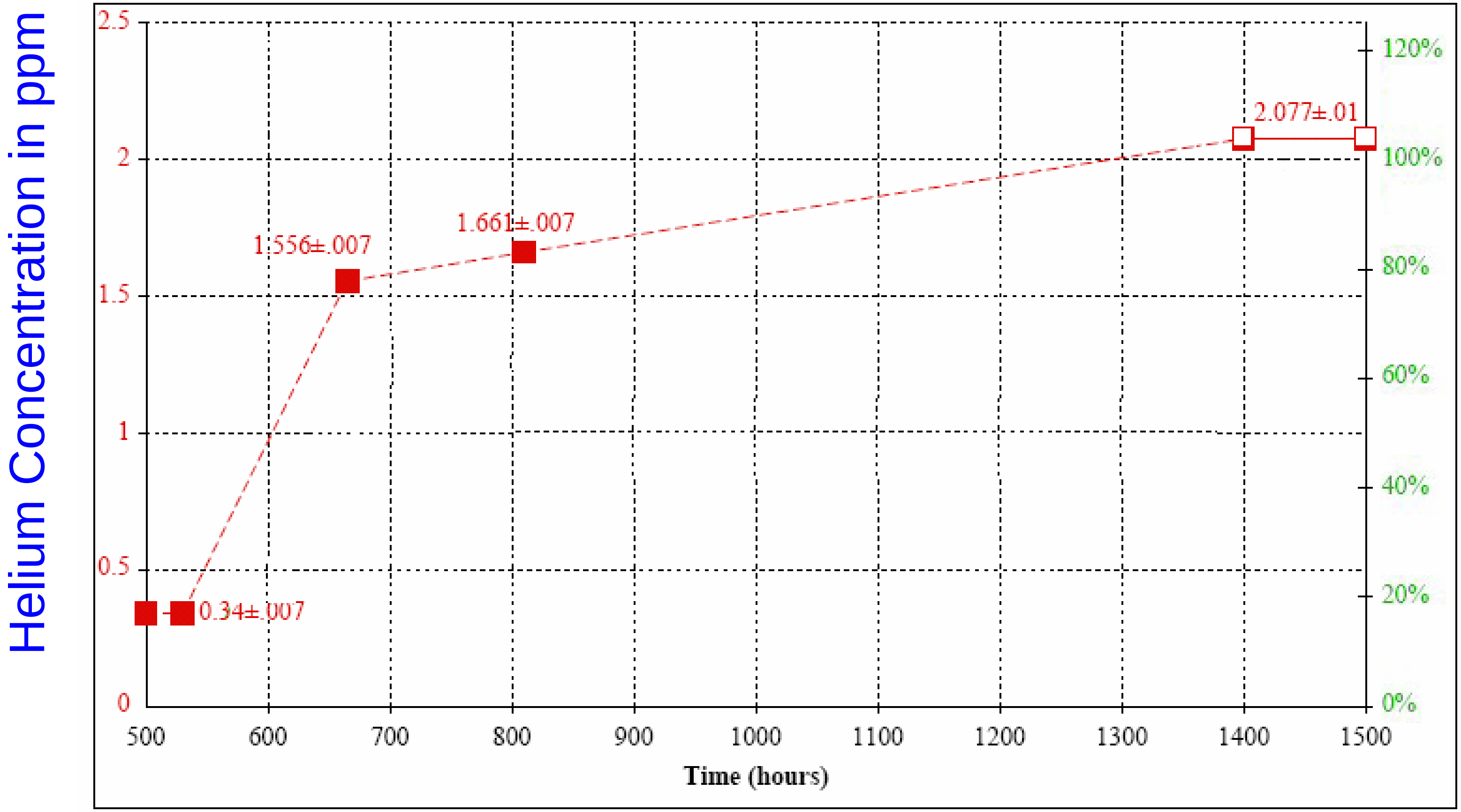


Image from McKubre, APS, 2007, Y-axis label added by SBK.

Let's look at sample 4 first. This is easy to spot from its concentration (2.077ppm) and its time (1407h).

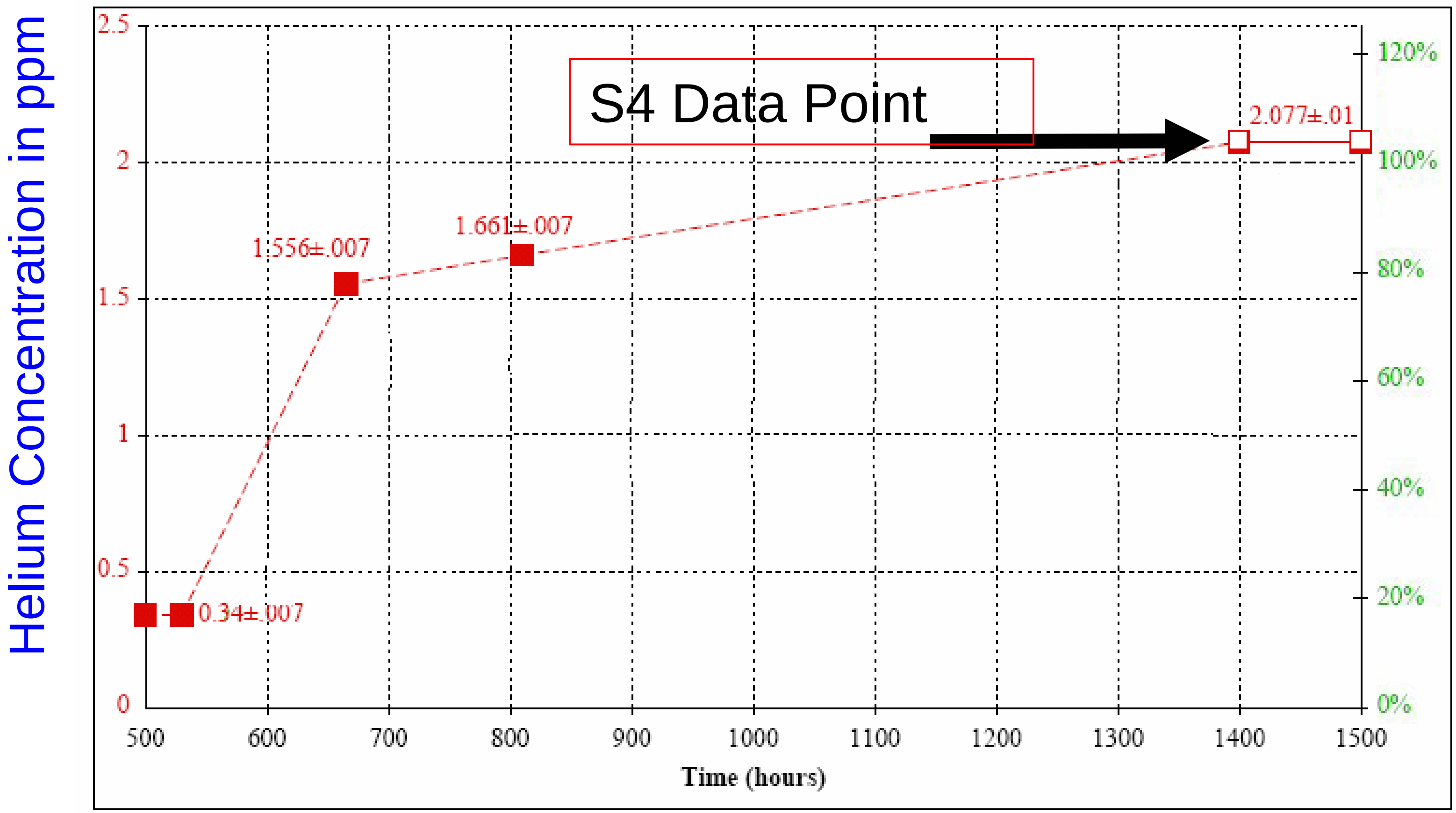


Image from McKubre, APS, 2007, Y-axis label and bold black arrow added by SBK.

But wait. 2.077 used to be down at about 85% of the 2000 predicted value. Now, in 2007, the predicted baseline has shifted again. This means the data point now lands at 104%.

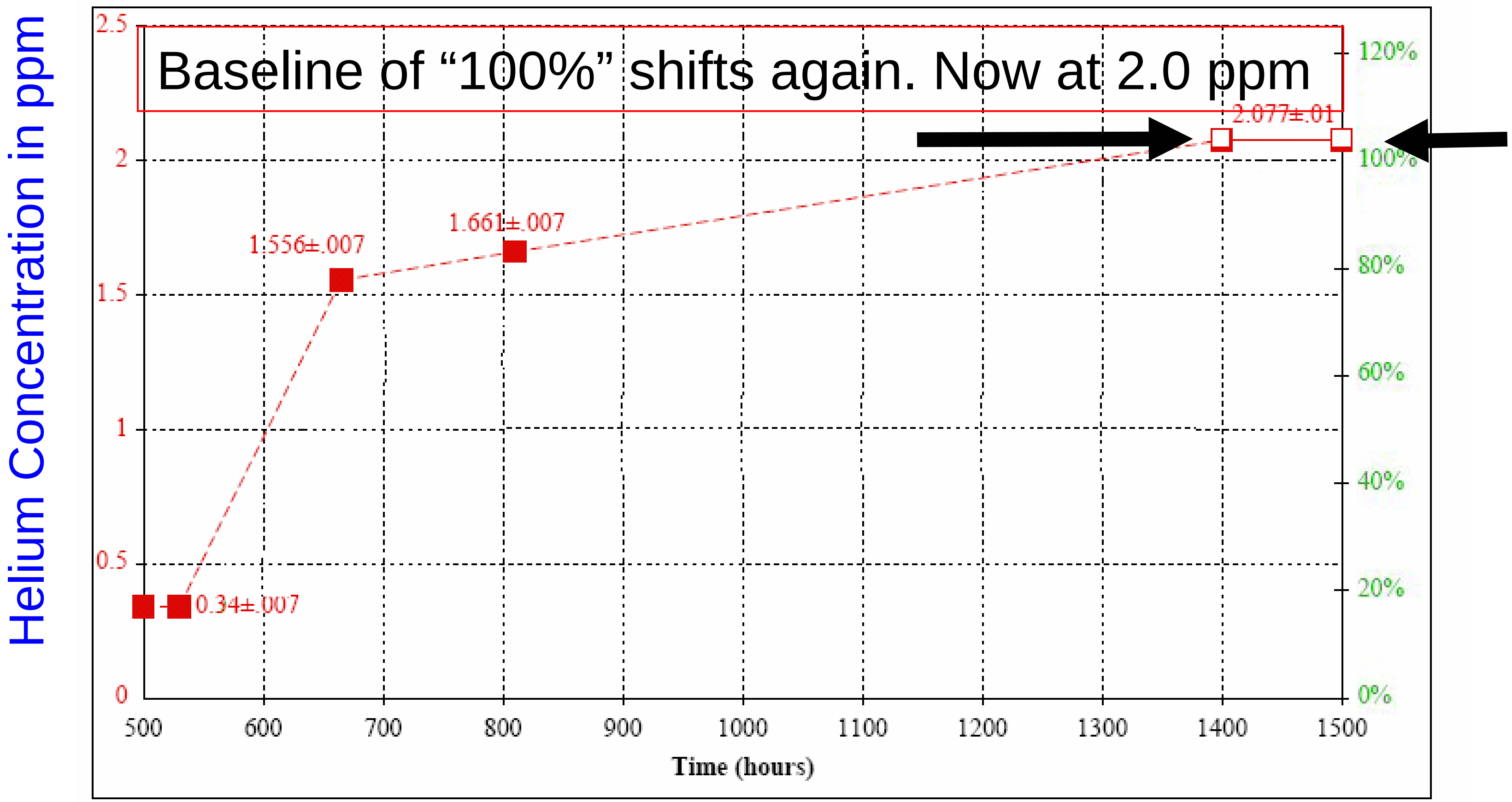


Image from McKubre, APS, 2007, Y-axis label and bold black arrows added by SBK.

Remember? The 2000 baseline was at 2.5 ppm, not 2.0 ppm

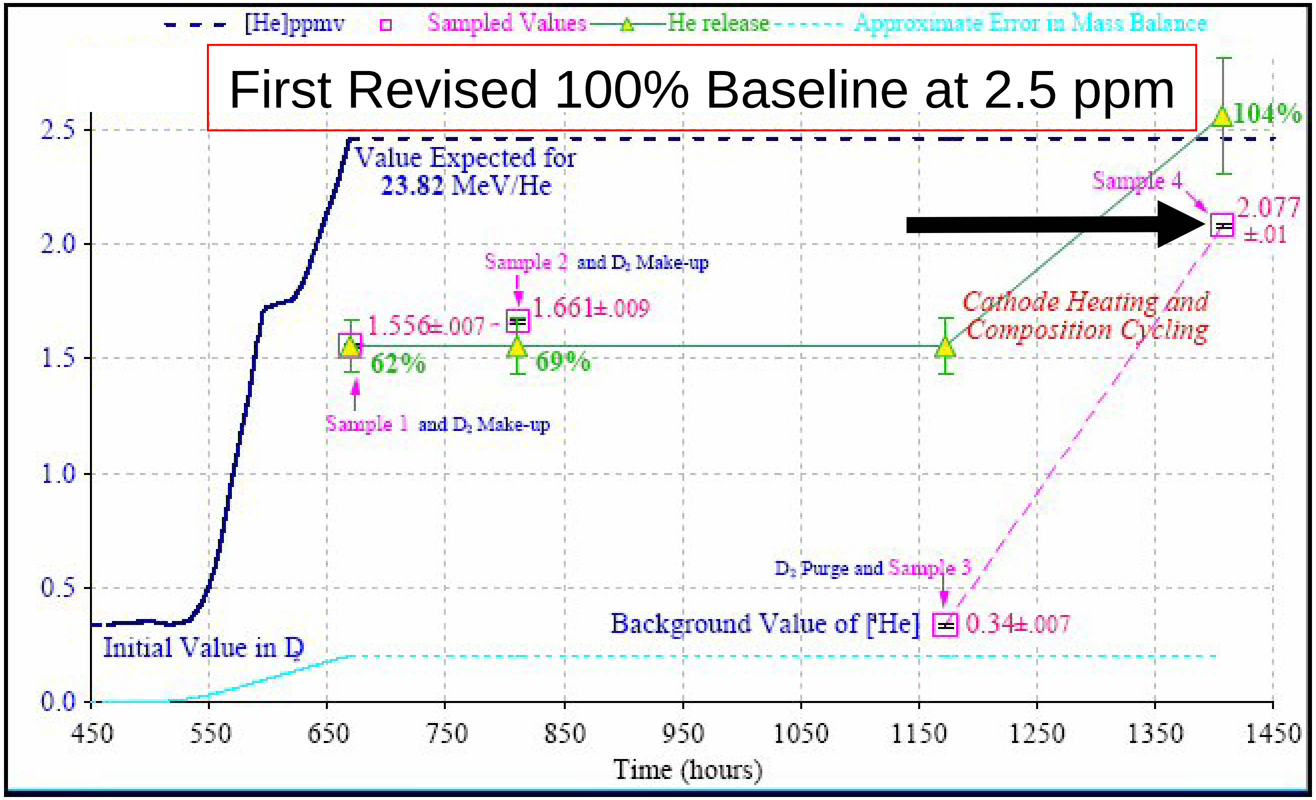


Image from McKubre, ICCF-10, 2003. Bold black arrows added by SBK.

And what's that next to the value for the S4 helium sample? It's a brand new value, added 13 years after the experiment, with no explanation given.

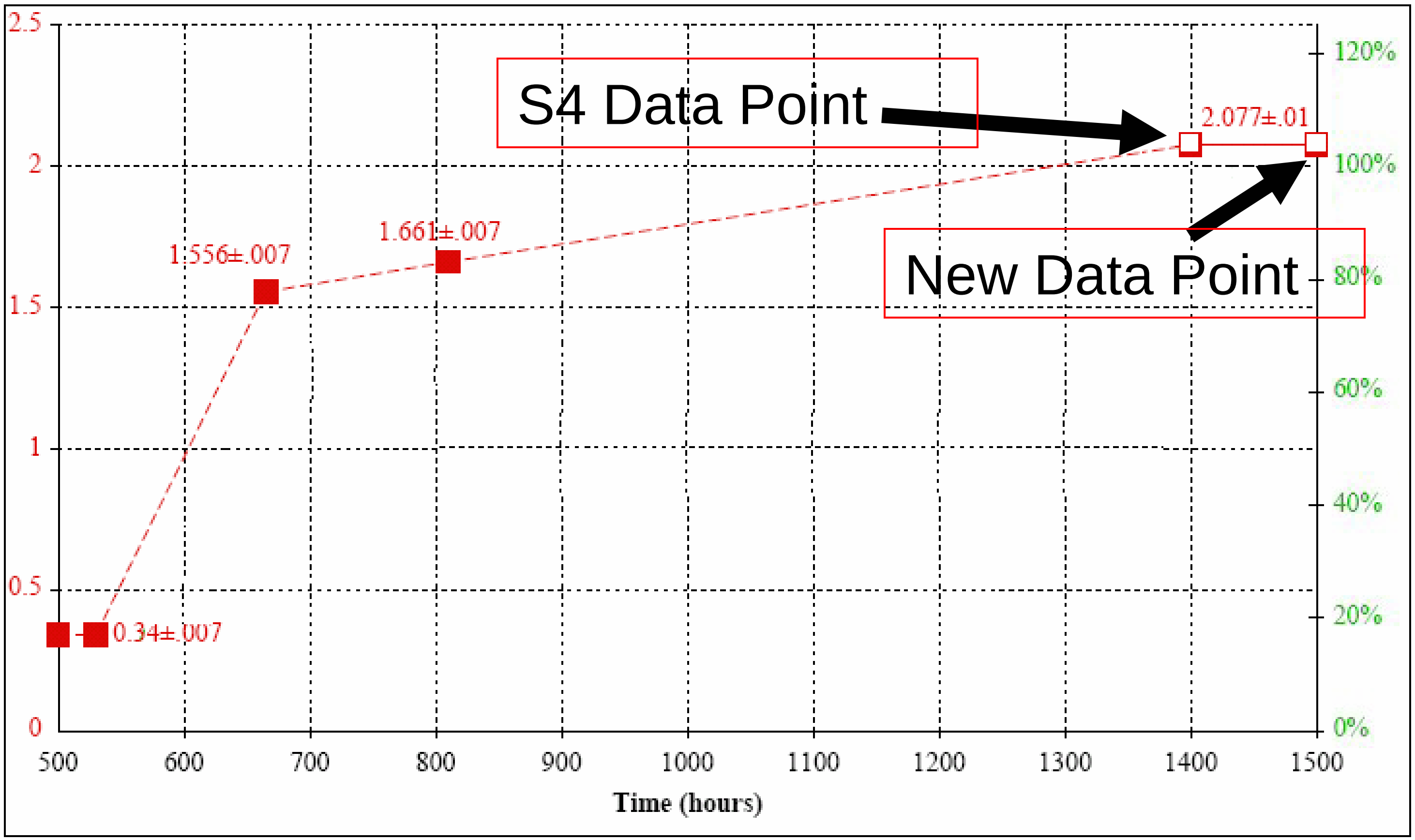


Image from McKubre, APS, 2007, bold black arrow added by SBK.

The actual measured values for samples 1 and 2 haven't changed.

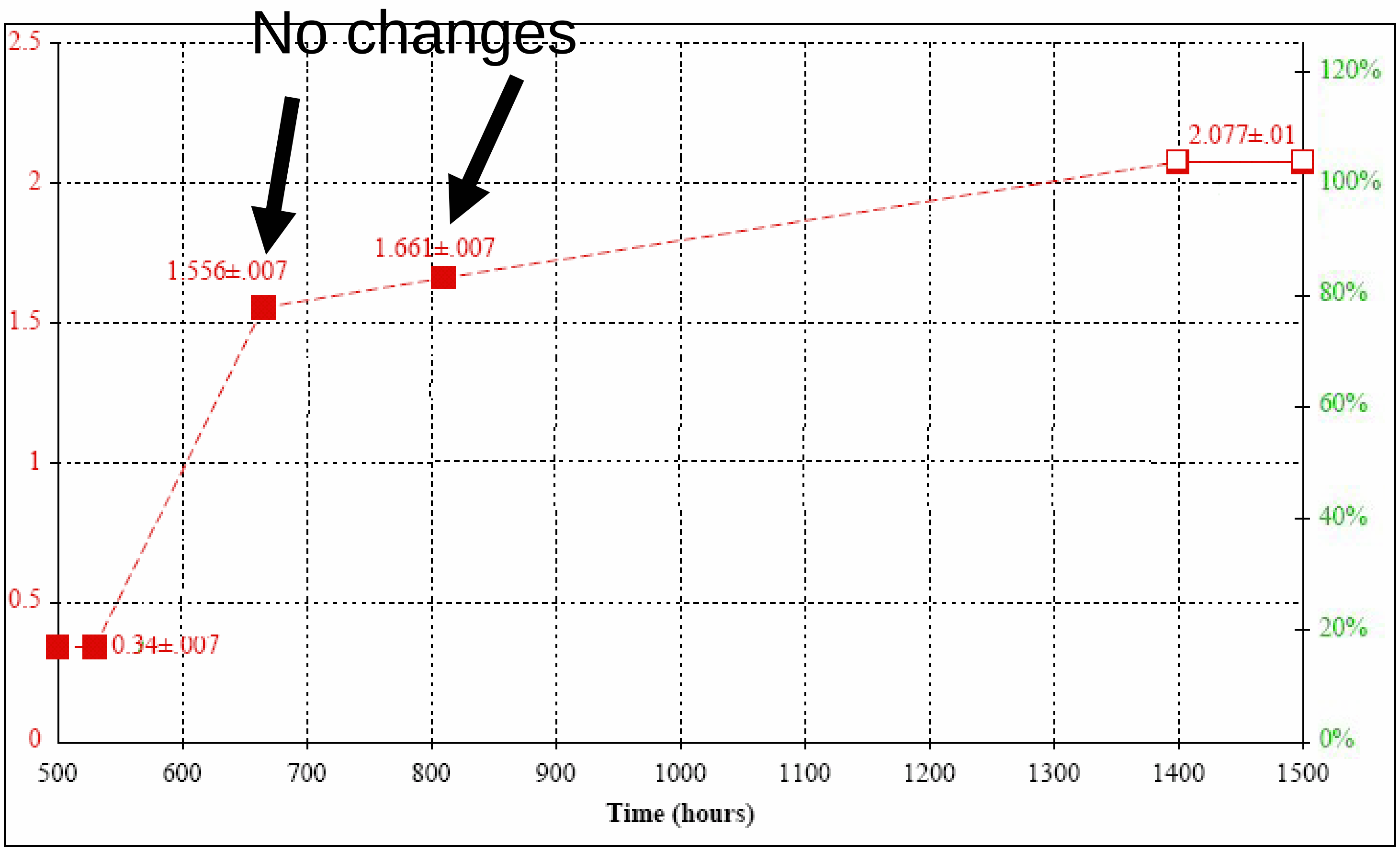
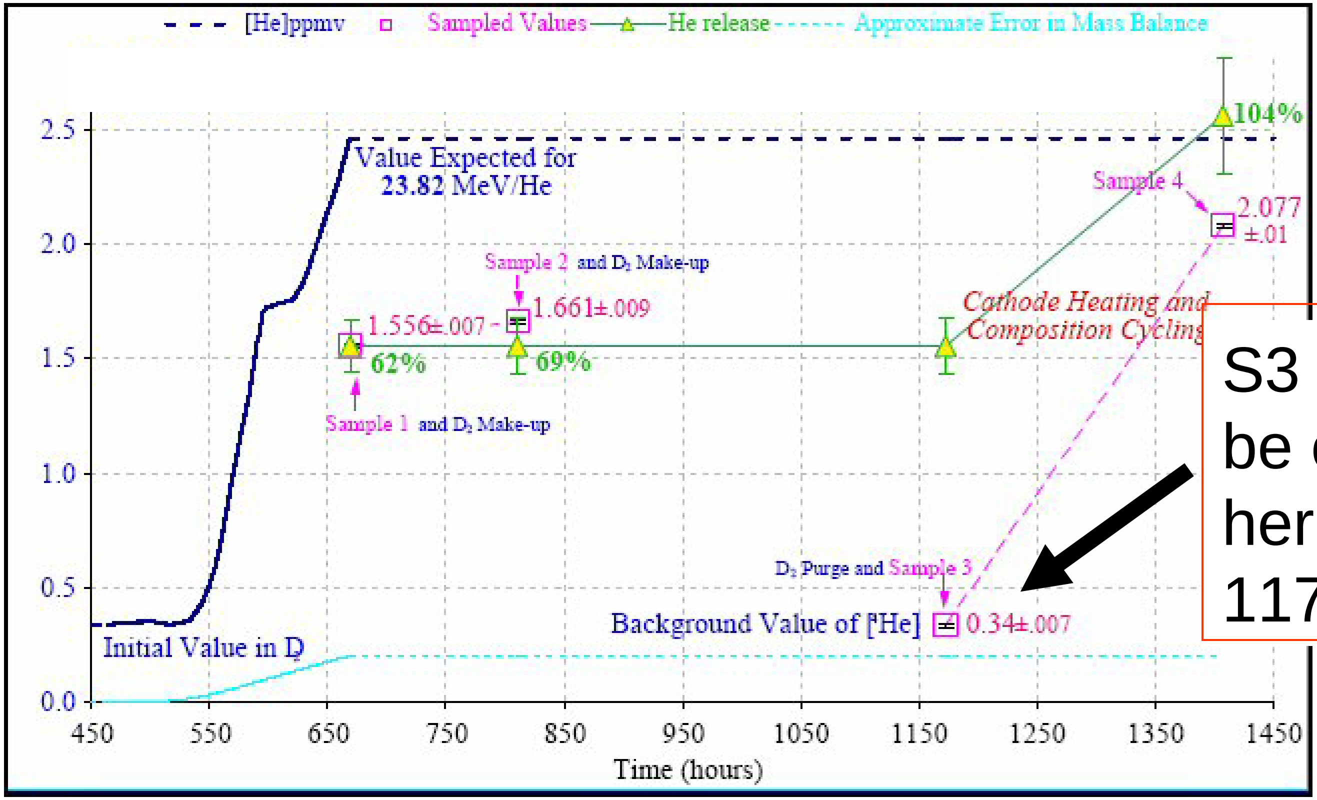


Image from McKubre, APS, 2007, bold black arrows added by SBK.

But sample 3 has now disappeared. Remember that one?



S3 used to be over here, at 1172 hours

Image from McKubre, ICCF-10, 2003. Bold black arrow added by SBK.

Instead, S3 now takes place at 530h instead of 1172h. And there is one new “measured” data point where there was previously only an “assumed initial starting value.”

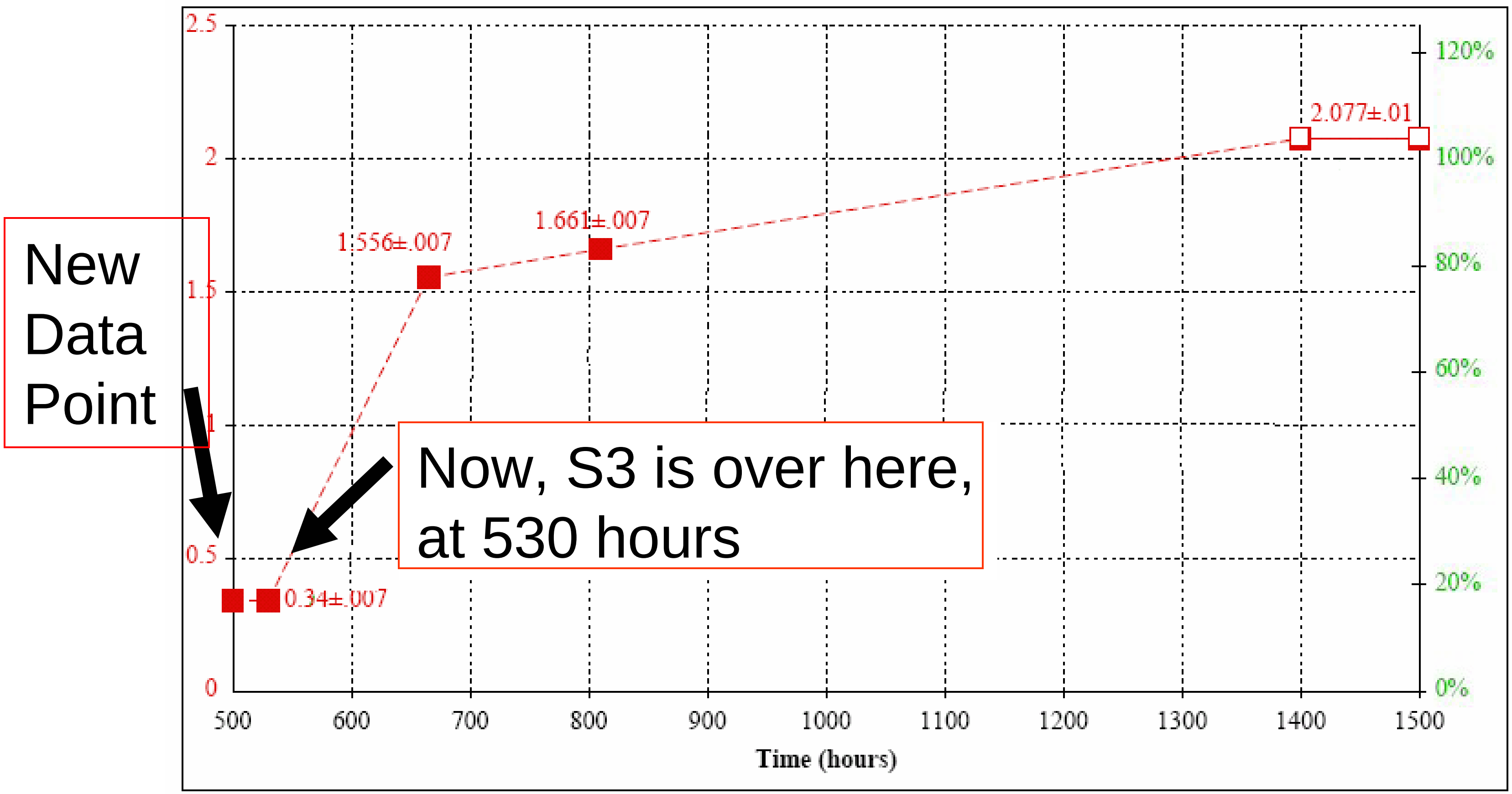


Image from McKubre, APS, 2007, bold black arrows added by SBK.

And remember McKubre's "shake and bake" period that supposedly took place from 1172h to 1407h?

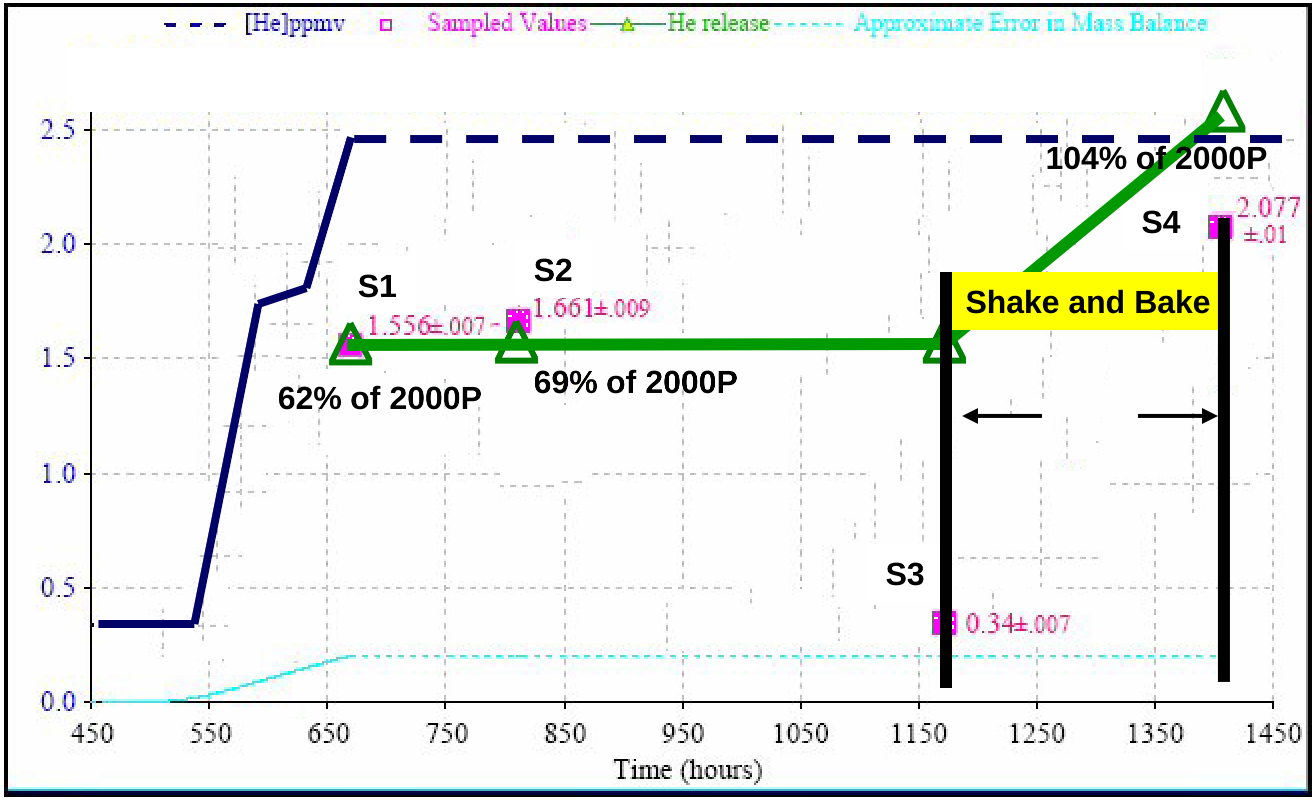


Image from McKubre, ICCF-10, 2003. Bold black arrow added by SBK. Green triangles, green line and blue line (redrawn) and bold black numerical values (re-written) come from McKubre, ICCF-10, 2003. "Shake and Bake" section added by SBK based on McKubre, ICCF-10, 2003

Now, in the 2007 depiction of this 1994 experiment, the 200-hour "shake and bake" becomes a 600-hour "extended period of thermal and compositional cycling."

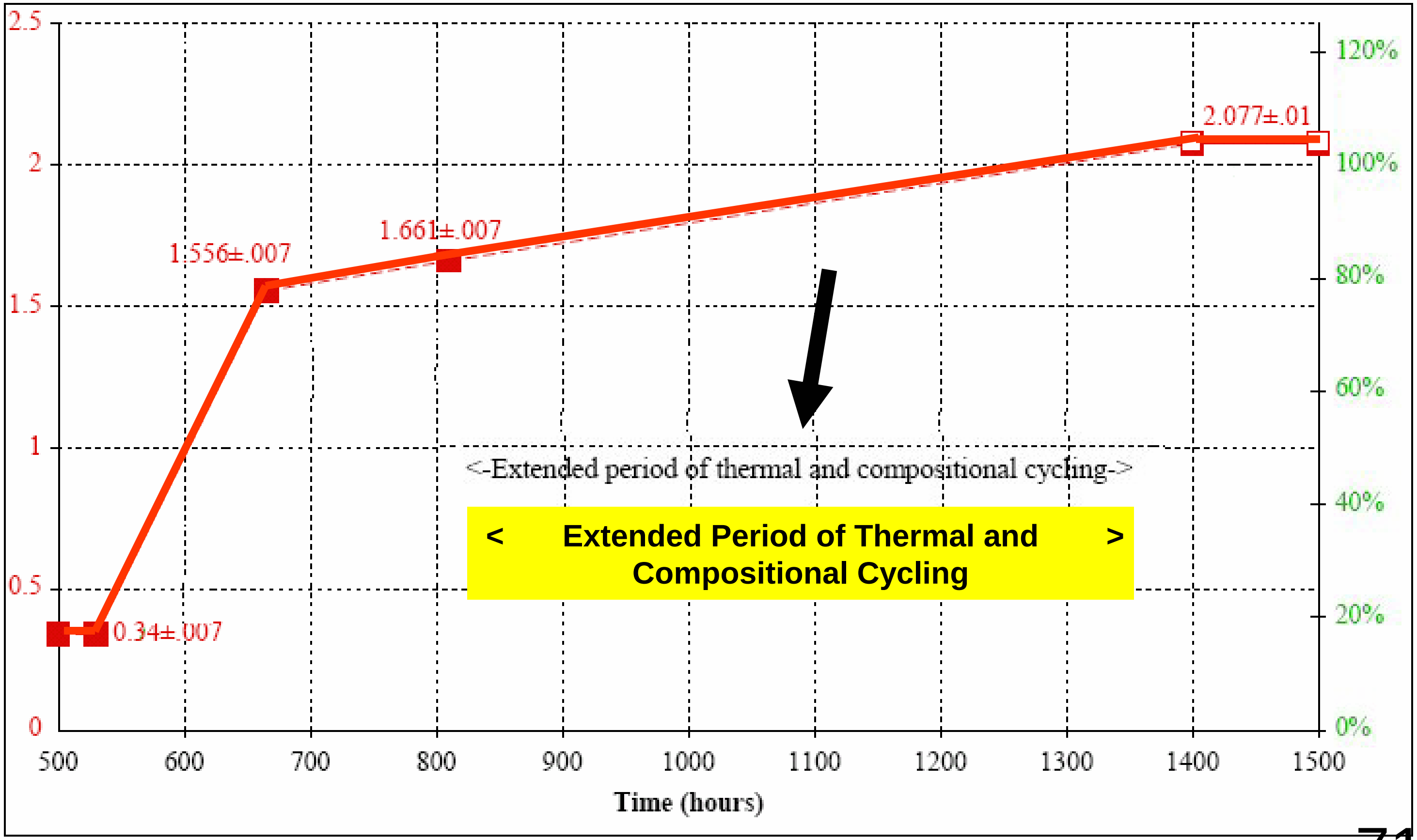


Image from McKubre, APS, 2007, bold black arrow and line added by SBK.

After that, McKubre draws a new green line that shows a remarkably precise theoretical agreement with the "measurements."

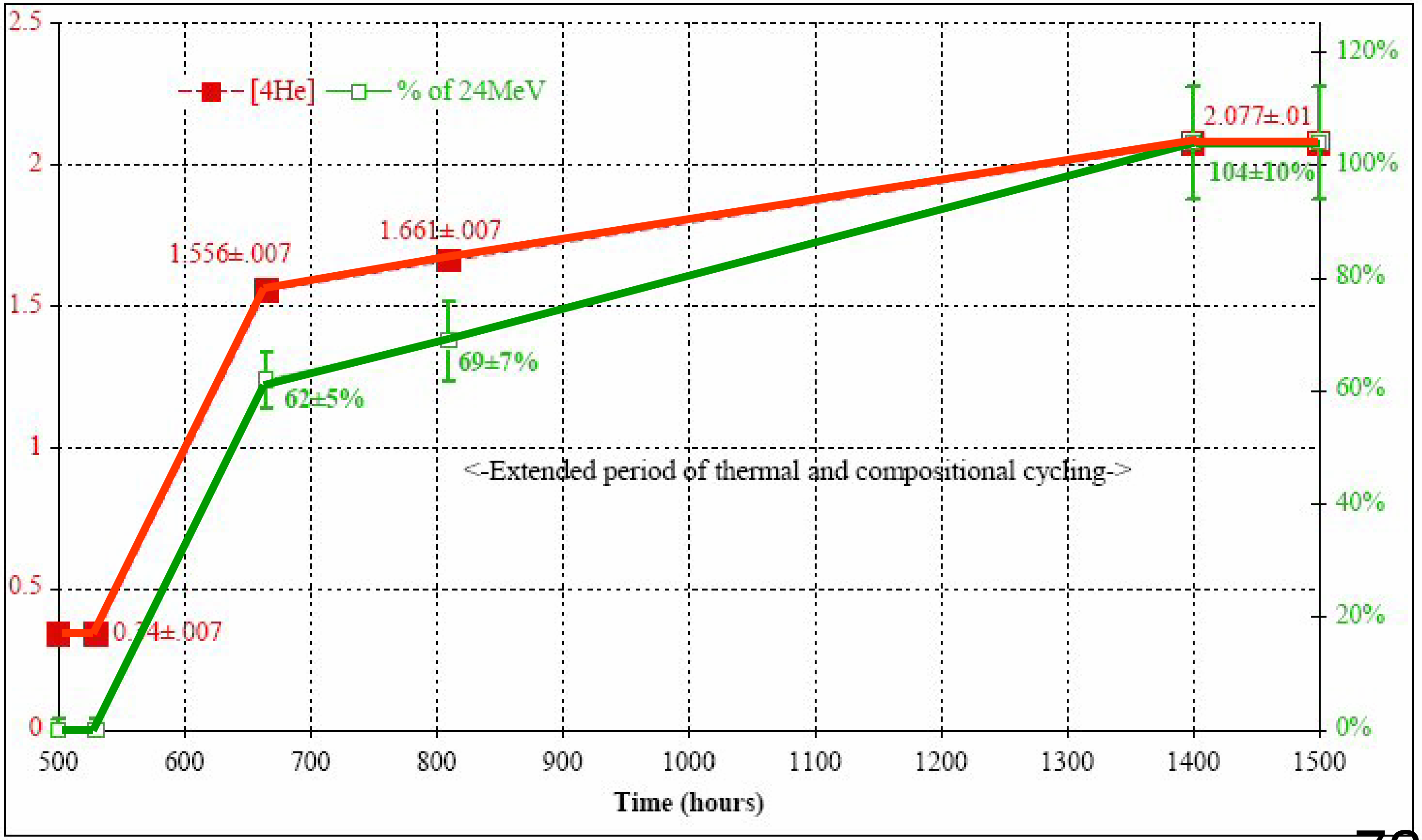


Image from McKubre, APS, 2007

So precise that you can almost lay them on top of each other.

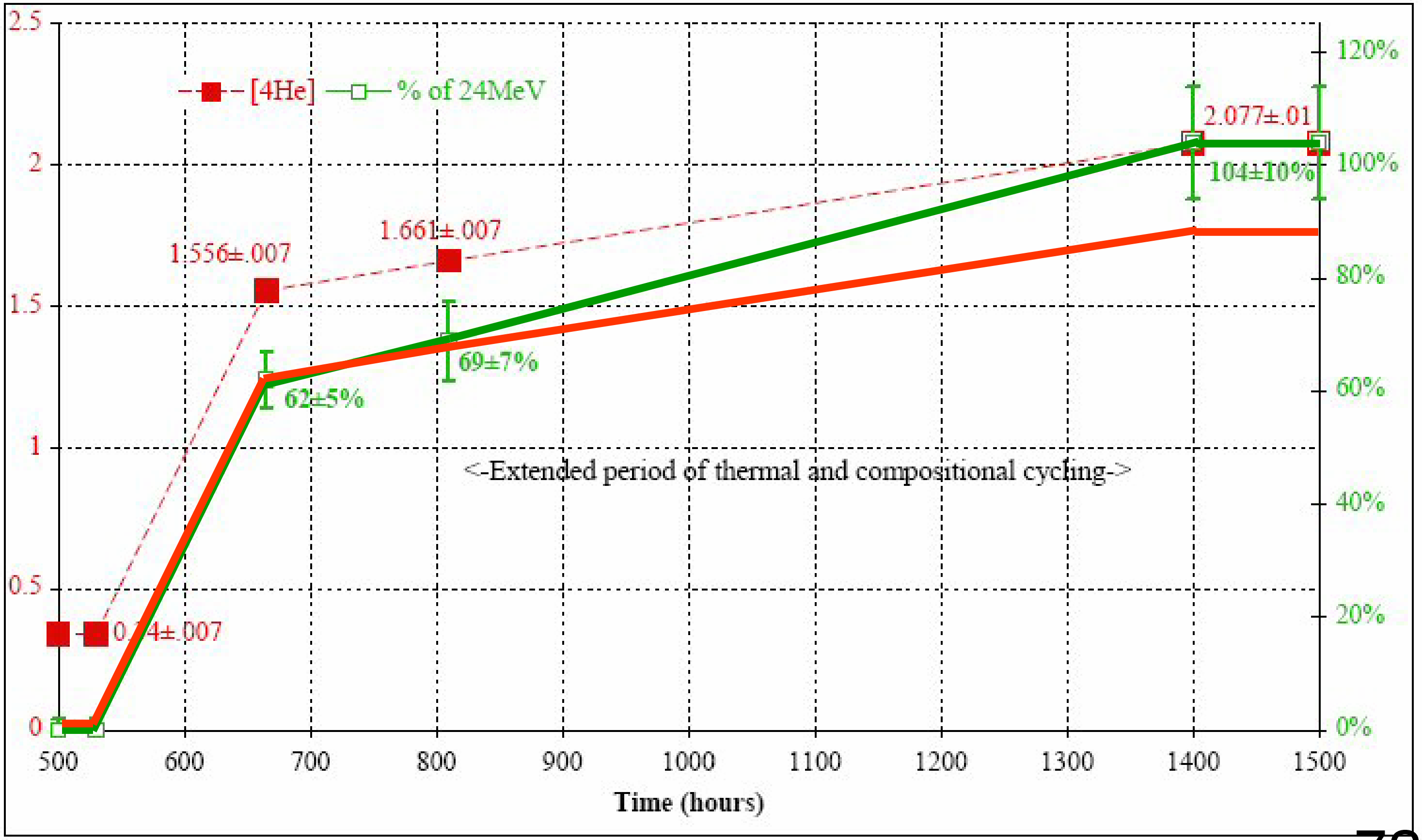


Image from McKubre, APS, 2007, Red line superimposed on green line by SBK.

In October 2009, at ICCF-15 in Rome, McKubre put it all together, including numerical values of “retained” helium.

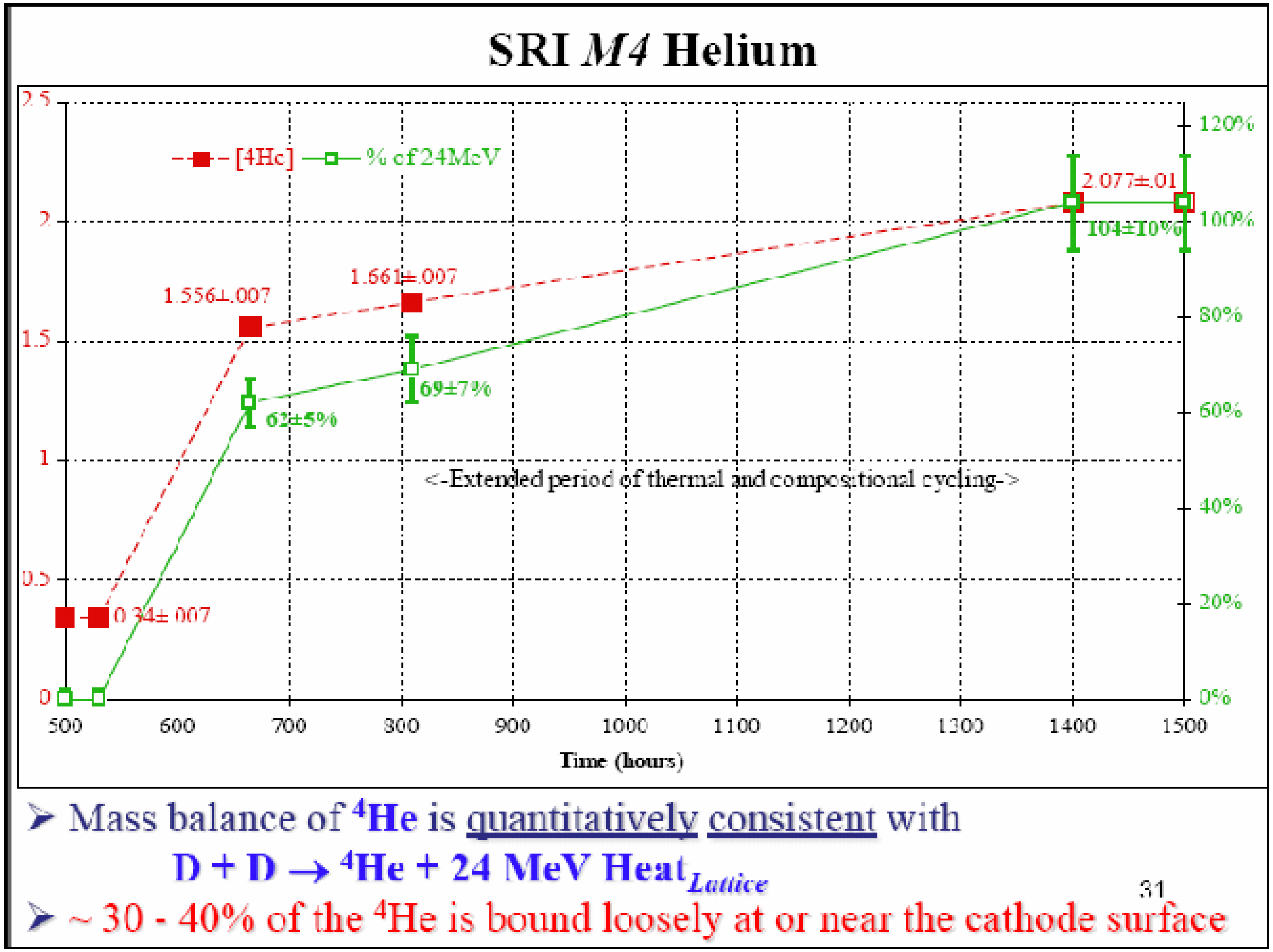


Image from McKubre, ICCF-15, 2009

In December 2009, I was reviewing a paper from a Navy researcher on LENR for publication in a print encyclopedia.

I hadn’t known about any of the changes to M4.

But I did know that Daniele Gozzi, in another LENR experiment, had melted part of his cathode and found no retained helium within his detection limits.

And I knew that John O’Mara Bockris had quickly preserved his cathode after the experiment in liquid nitrogen to prevent helium from outgassing, and then he found helium stuck to the surface or near-surface areas.

A back-and-forth with the Navy author began.

Gozzi 1998 and Erratum, Chien (Bockris), 1992

I told the author that McKubre's helium retention idea seemed to contradict Gozzi and Bockris, and I suggested changes.

The author did not see a contradiction.

So I began to read the papers more closely.

I read McKubre's 2000 paper. I found two short paragraphs about M4 and the helium retention idea.
No graphical or tabular data. Very sketchy details.

Then I looked at the reference. There was only one – EPRI report TR-107843-V1. I cracked it open – 379 pages.

I found the section for M4 fairly quickly.

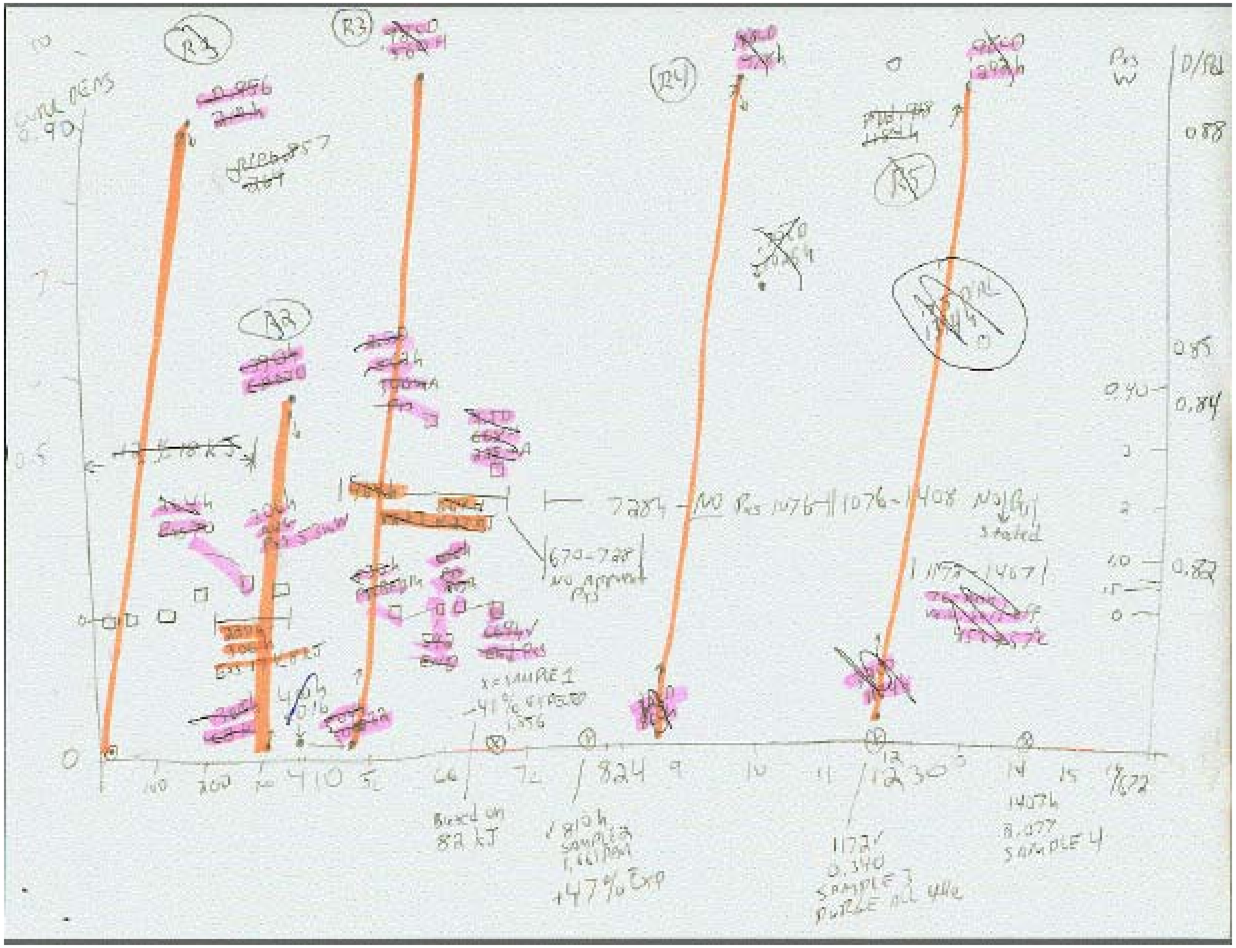
But when I first looked where the value for 104% was supposed to be, it wasn’t there. No value was shown.

Then I looked for the 62% value. It wasn’t there, either. But in the place where it should have been, it said 41%.

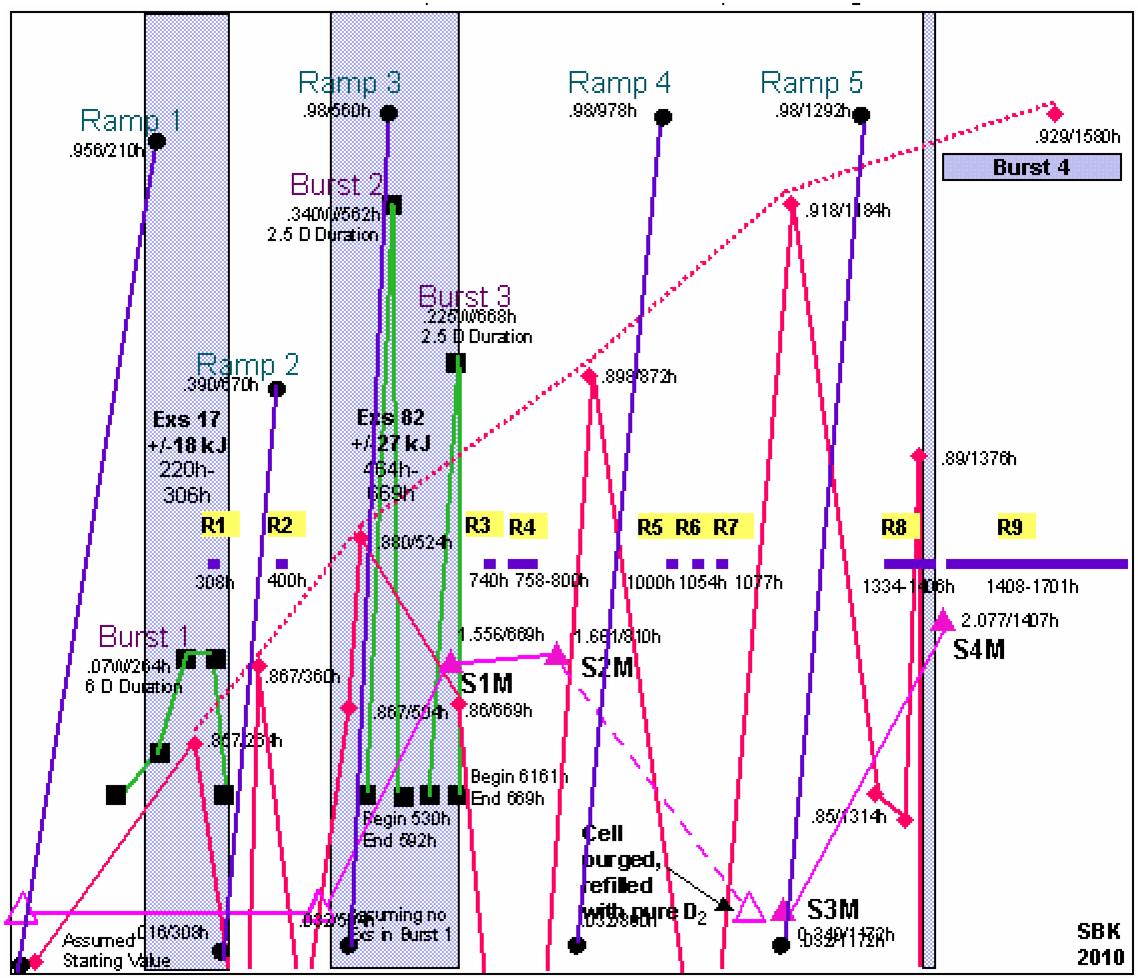
Then I looked for the 69% value. It, too, wasn’t there. Instead, there were values that indicated 147%.

This worried me.

I needed to understand the details. It took several weeks of analysis because all the text and graphs were split up among 40 pages. I also wanted to get the big picture. Here is an image of my initial hand-drawn sketch of the full experiment.



Using the sketch as a base, I made graphs for each of the data sets. I could then see how they all inter-related. This is the result.



I realized that the changes to the data created some potentially serious problems.

I went as far as I could on my own. Then I called up Francis Tanzella at SRI, one of the authors of the 2000 paper.

I asked him whether I could come visit and talk about M4. He was very gracious and helpful and gave me 2½ hours of his time. But I learned very little from him that I didn't already know about M4.

In fact, I pointed out to him two apparent minor errors in the EPRI report.

After I did the best I could to learn about M4, I began asking all the authors on the 2000 paper serious questions.

Their responses (or lack thereof) are published in New Energy Times Issue 34.

On March 21, at the American Chemical Society meeting in San Francisco, during a press conference, I asked McKubre about the changed values (plural) in experiment M4.

“Dr. McKubre, when I was discussing the values, the changed values for SRI experiment M4 with Pam Boss, she told me that Peter Hagelstein explained that he or his colleagues explained this ‘correction.’ Where can I find some documentation about both the exact error as well as the math for the ‘correction’?"

Here is McKubre's response: (Page 1)

"In the preliminary report we issued to the Electric Power Research Institute, which was a report private to [EPRI] that now is public, [it] contained, I think, a value of the mass-balance for helium-4 and heat which was, I think it was, from memory, and this is sixteen years ago, maybe, now, 85 +/- 10 percent.

"When we **recalibrated the volumes** that were involved in determining that mass balance **the value became a more correct value**, it was 105 +/- 10 percent. Now those two values are experimentally the same. I would prefer the lower value since you can't get more product than your reaction produces."

Here is McKubre's response: (Page 2)

"But the correction was observed, reported to the Electric Power Research Institute, which were the sponsors of that work. I also made a comment about it in the conference at Lerici in the year 2000 at ICCF-8 during my presentation. So **the published value, the first published value** is in the conference proceedings and **the first published value contains the correct value** of that mass balance, 105 +/- 10 percent.

"Is that the information you were looking for?"

The next day, McKubre gave his scheduled talk at ACS. (His title slide says he’s the director of the SRI Energy Research Center, but that doesn’t exist anymore.)

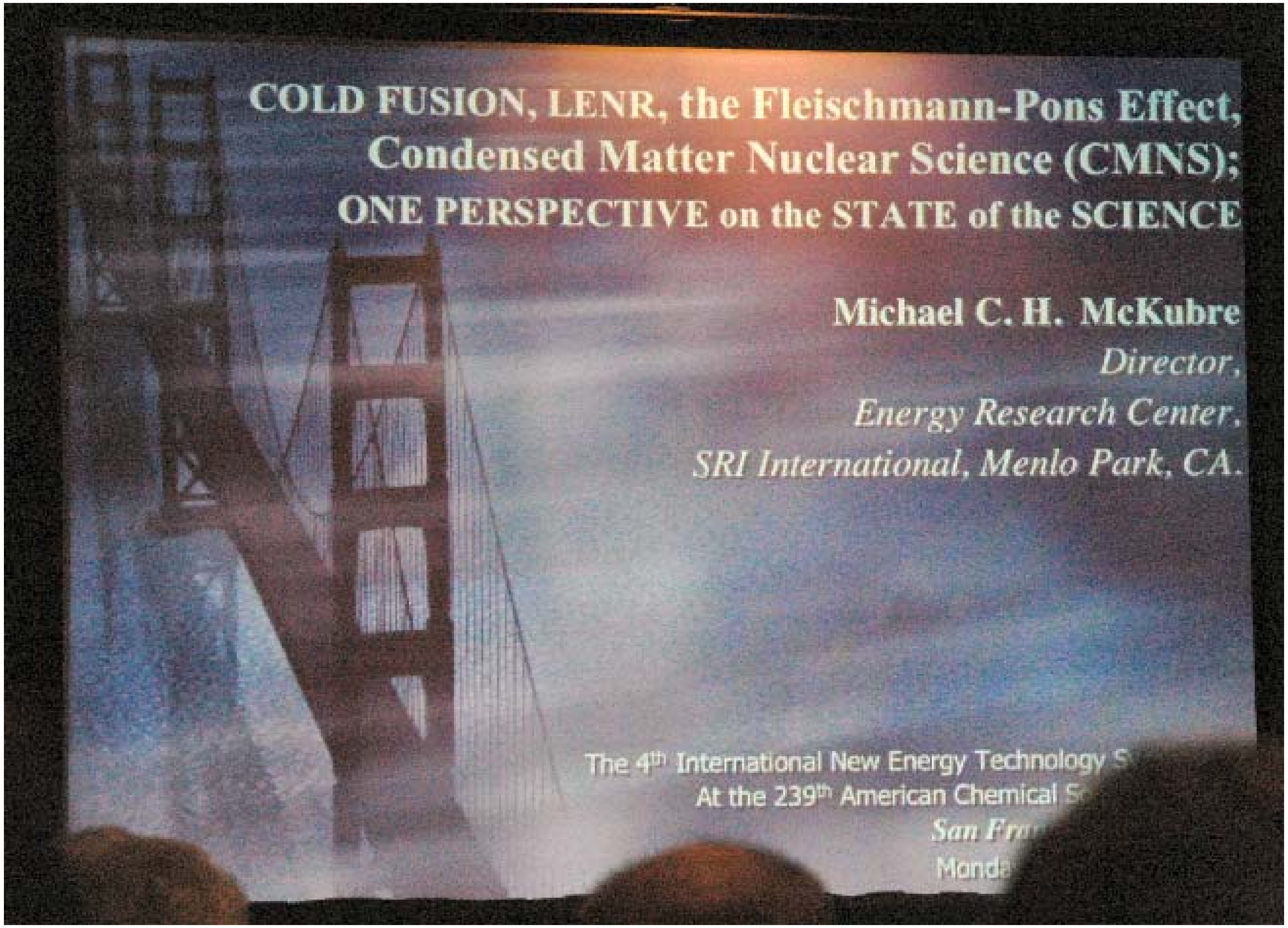


Image from McKubre ACS Presentation, San Francisco, March 22, 2010

He spoke about why critics gave "cold fusion" a hard time and why they treated the field like pathological science.

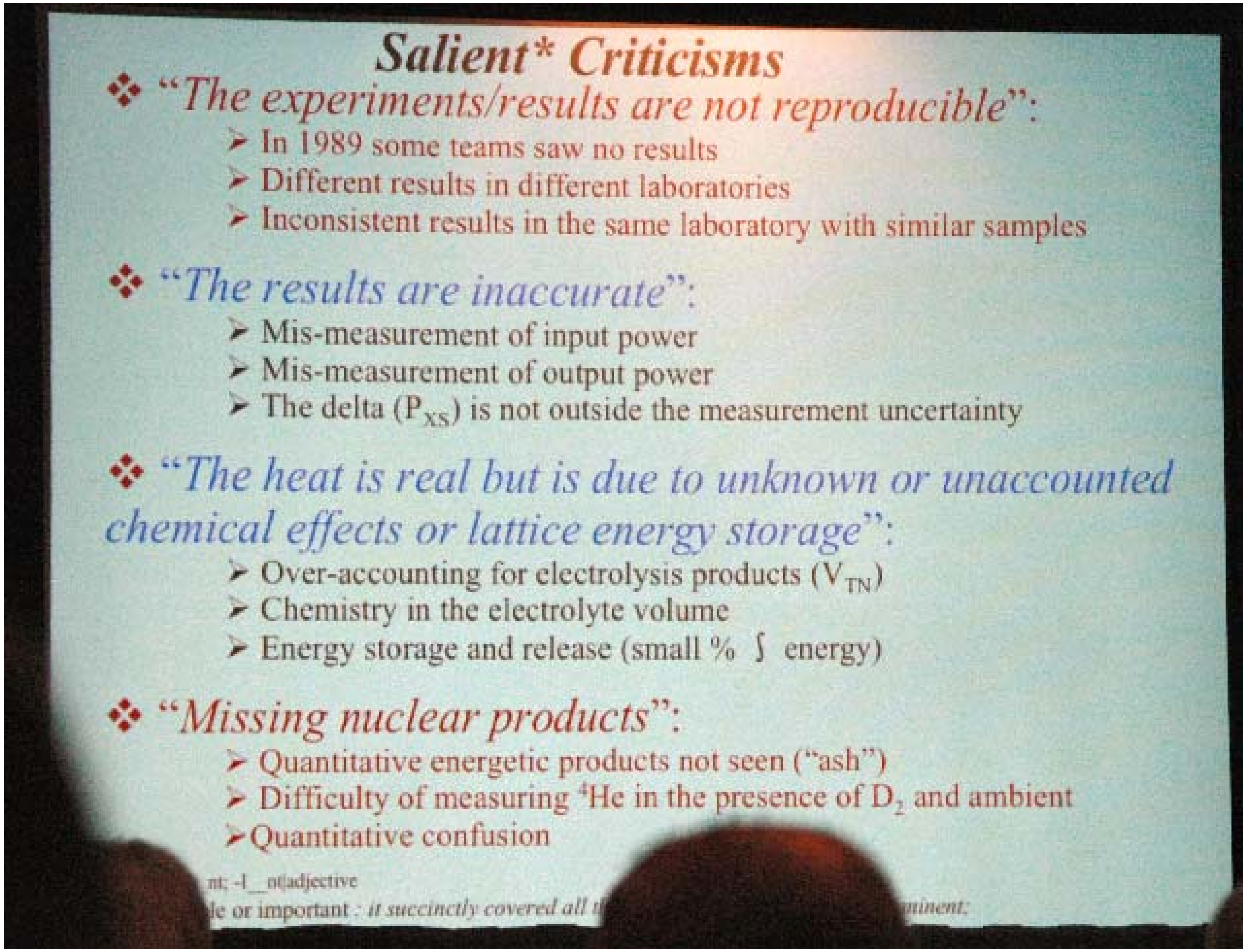


Image from McKubre ACS Presentation, San Francisco, March 22, 2010

Interestingly, perhaps for the first time since 2000, McKubre did not discuss or show any slide about experiment M4, let alone claim that it showed evidence for D+D “cold fusion.”

Only six months earlier, in Rome, he had shown M4 and explained how it matched the prediction of Hagelstein’s “cold fusion” theory. I wondered whether this was his way of making a retraction?

Later that week, I thought I should check on the correction McKubre claimed that he reported to EPRI.

Brian Schimmoller of EPRI answered my inquiry and, to my great surprise, wrote,

"After checking, there is no record in our system of any corrections or errata published for those reports, and the retired project manager tells us that he's not aware of any corrections or errata either."

That project manager was Thomas Passell, who also was at the March 2010 ACS meeting. Schimmoller also contacted Albert Machiels, the other manager on that project. Machiels too, was also not aware of any corrections or errata.

Schimmoller to Krivit, March 30, 2010

Then I thought some more about McKubre’s response during the press conference. He had offered a cursory explanation of how the 84% (85%) value “became” 104% (105%).

“When we recalibrated the volumes that were involved in determining that mass balance, the value became a more correct value.”

I wondered why there was no published, scientific explanation for this change.

I also wondered why the SRI researchers could not measure volumes properly.

How could the EPRI researchers make an erroneous volume measurement that would cause the fourth sample to

disappear,

the value for the first sample to

move down,

and the value for the second sample to

move up?

Then I remembered that McKubre said something else that might have been important:

“The published value, the first published value, is in the conference proceedings and the first published value contains the correct value of that mass balance, 105 +/- 10 percent.”

This was strange. He seemed to be suggesting that one and only one value was reported for the fourth sample, and that’s in his 2000 paper.

That paper devotes barely two paragraphs to M4, has no tabular or graphical data, and refers only the EPRI report.

As I understood it, McKubre and Tanzella received draft copies of the EPRI report in January 1998.

In June, the final report was printed, bound, listed in the EPRI catalog, copyrighted and specifically identified as a

"corporate document that should be cited in the literature."

I wondered whether McKubre was implying that the data had to be published in a journal or conference proceedings to count.

That didn't make much sense, either.

Nothing about McKubre's response provided any confidence in the validity of his reported changes for experiment M4.

If McKubre had a scientific explanation for all these changes, he would want the public to know.

It was a dilemma. What to do?

After all, McKubre has done some of the finest heat and helium work in the field. He has championed the "cold fusion" underdogs against the skeptics and hot fusion cabal.

I wondered whether other researchers in the LENR field could explain McKubre's changes and, if not, whether such actions were tolerated by that community.

I asked Robert Bass, a colleague of McKubre’s, to see whether he knew of, or could learn of, any scientific explanation for McKubre’s changes.

Bass thought I may have been nit-picking, but he asked McKubre, anyway. He said McKubre, however, told him, "I'm not going to waste my time on that."

I asked Melvin Miles whether he could find some scientific explanation for McKubre’s changes. Miles wasn’t interested. He wrote that I was “barking up the wrong tree.”

I asked John Bockris about the M4 changes. Bockris, if anybody, would know what it was like to be wrongly and unfairly accused of being unscientific, I thought. I’ve told his story many times.

Bockris, as McKubre once said, was one of the top five electrochemists in the world. So Bockris was certainly a good source to ask about the M4 changes.

"I knew McKubre since Como, 1991,” Bockris wrote. “His physics is good. I would not think it likely that he would put forward an error. Also, McKubre is a straight shooter, i.e. HONEST."

Maybe, I thought, the types of changes McKubre made are customary and tolerated in mainstream science, too, not just in the controversial field of "cold fusion,“ or LENR.

The sad thing about all this is that McKubre has a substantial collection of rigorous experimental reports for the co-production of excess heat and helium, especially the Case replication.

Helium cannot be produced by ordinary chemistry. That’s a fact. The helium production observed in M4 and Case should have been enough to convince any reasonable mainstream scientist of the reality of LENR. This alone would have been a major achievement.

But inexplicably, McKubre did all these convoluted manipulations and data massaging just to try to prove Hagelstein’s “cold fusion” theory.

The irony of this 10-year saga is that McKubre's many experiments, including the early Fleischmann-Pons replication that was audited by Richard Garwin and Nathan Lewis, stand as valuable contributions to science and the LENR field.

The bizarre changes in McKubre's reporting of M4 seem to have begun when McKubre and Hagelstein realized that the Case experiment showed 31 MeV/4He rather than the Hagelstein prediction of 24 MeV.

Expt. HH: 1993

> Clarification of a possible origin for the apparent ^{4}He deficit in experiments "1" and "2" can be obtained from the results of experiment "3". Approximately 82 kJ of

Image from McKubre, ICCF-8, 2000, pg. 6

Case Expt.: 1998

Expt. M4: 1994

It failed to prove their idea of D+D "cold fusion."

Summary of Changes Between 1998 and 2004

- Invented fourth predicted value, 85% of ~24 MeV
- Invented helium retention principle based on untested hypothesis
- Invented helium extraction procedure
- Shifted theoretical baseline for first sample down by about 40%
- Shifted theoretical baseline for second sample up by about 150%
- Added third data point represented as 1.556ppm when it was measured at 0.34ppm

Summary of Changes Between 2004 and 2007

- Invented data point 4 now shifted from 85% to 104%
- New data point added at 1500 hours
- Data point 3 shifted from 1172 hours to 530 hours
- Data point at 525 hours added
- “Cycling Procedure” changed from 200-hour to 600-hour duration

References

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,) “Development of Energy Production Systems from Heat Produced in Deuterated Metals - Energy Production Processes in Deuterated Metals, Volume 1, TR-107843-V1,” June 1998

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.

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

Web sites with more LENR papers: www.lenr-canr.org and www.newenergytimes.com