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Special Report: Cold Fusion Is Neither

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Table of Contents

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I. Special Report Outline

The articles in this special report are sequentially structured and interconnected; they build on and reference one other. Readers will benefit by reading them in sequence. This outline highlights the major points of each article.

The greatest skepticism about low-energy nuclear reactions in the last decade has come not from mainstream science but from within the field itself, from a "cold fusion" subgroup. The subgroup worked strategically to discredit and suppress the growing experimental and theoretical evidence that disproved "cold fusion" but proved the reality of a new nonfusion, nuclear phenomenon.

1. Taking a New Look at LENR

- Suggests how LENR may lead to a revolution in energy production and storage.
- Provides additional references for deeper technical and scientific information about LENR.

2. Cold Fusion Is Neither

- Explains why some people initially thought LENR was predominately a "cold fusion" process.
- Discusses how the "cold fusion" concept went from hypothesis to assumed fact by members of the "cold fusion" subgroup.
- Reviews how the evidence accumulated to show that LENR was not predominately a fusion process.
- Shows the numerous and substantial distinctions between LENR and the hypothetical "cold fusion."

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

- Summarizes the full investigation, as shown in the corresponding 101-page slide presentation.
- Reviews investigation of a dozen unsubstantiated changes, additions and deletions to an experiment reported by Michael McKubre (SRI International) during a 10-year period.

4. NRL 2008 – The LENR Null Results Laboratory

- Reports how groundbreaking research from Japan in the late 1990s triggered a new wave of interest in LENR transmutation research.
- Reviews attempts by the Naval Research Laboratory to discredit transmutation reports from Arata-Zhang heavy-water LENR experiments.
- Reviews evidence in support of transmutation reports from Arata-Zhang heavy-water LENR experiments.

5. NRL 2009 – The LENR Null Results Laboratory, Again

- Reviews attempts by the Naval Research Laboratory to discredit transmutation claims from Iwamura/Mitsubishi Heavy Industries.
- Presents a step-by-step technical explanation of the Iwamura/Mitsubishi transmutation experiment.

6. Isotopic Anomalies Reveal LENR Insights

- Explains how anomalous isotopic shifts reveal three key insights about LENR transmutations.
- Shows how anomalous isotopic shifts in LENR cast cold water on the D+D "cold fusion" hypothesis.

7. Violante Group Claims LENR Elemental Anomalies Are Contamination

- Reviews efforts by LENR researcher Vittorio Violante, member of the "cold fusion" subgroup, to discredit LENR transmutations.

8. Neutron Capture Is Not the New Cold Fusion

- Reviews efforts by members of the "cold fusion" subgroup to collapse distinction between neutron capture processes and fusion process after they realized that LENR was better explained by neutron processes than by fusion.
- Reviews efforts by members of the "cold fusion" subgroup to discard the nuclear physics definition of fusion once they realized that LENR was better explained by nonfusion processes.

9. Who's Afraid of LENR Transmutations?

- Shows how some LENR researchers attempted to suppress LENR transmutation results because the results disproved their D+D "cold fusion" hypothesis.
- Compares similar five-peak spectra of LENR transmutations in light and heavy water which indicate a similar underlying mechanism, suggestive of a nonfusion process.

10. Fleischmann: It Must Be Neutrons

- Discusses "cold fusion" co-discoverer Martin Fleischmann’s regrets about calling the discovery "fusion."
- Quotes Fleischmann’s affirmation that "fusion has a special meaning in the scientific literature."
- Reports Fleischmann’s concession that the LENR process must involve neutrons, that is, weak interaction nonfusion processes.

11. LENR Weak Interaction Theory – Hagelstein Missed

- Shows that D+D "cold fusion" theorist Peter Hagelstein was on the neutron theory track in the early 1990s but failed to envision a complete neutron-based model.

12. Weak Interactions Are Not Weak Energetically

- Reviews the development of weak-interaction research and its potential for significant energy release.

13. Widom-Larsen Theory Simplified

- Presents simple four-step explanation of the Widom-Larsen theory of LENRs.
- Explains collective effects in LENR.
- Explains how the chemical realm interfaces with the nuclear realm to create low-energy nuclear reactions.

14. Larsen's Vision of LENR Technology

- Discusses Larsen's vision of the potential practical applications of LENR technology.
- Compares potential LENR energy to conventional nuclear fission energy.

15. Development of the Widom-Larsen Theory of LENRs

- Tells Larsen's story of how he and Allan Widom developed their theory of LENRs.

16. Reader's Guide to Larsen Slide Presentations

- Provides a guide to help readers navigate Larsen's first six slide presentations.

17. The Cold Fusion Belief System

- Explains how some LENR researchers failed to distinguish between low-energy nuclear reaction research and "cold fusion."
- Dispels the "cold fusion" myths about helium creation in LENR.
- Shows possible weak-interaction-based nucleosynthetic pathways in LENR.

18. Contempt for Cold Fusion

- Shows how members of the "cold fusion" subgroup continued to support their D+D "cold fusion" paradigm despite the presence of experimental and theoretical evidence to the contrary.
- Explains why mainstream science was justifiably skeptical about "cold fusion."
- Sheds light on why mainstream scientists knew intuitively for two decades that "cold fusion" was problematic yet couldn't specifically identify the problems.
- Shows that some LENR researchers deliberately chose to associate their work with the speculative idea of D+D "cold fusion" instead of representing it as LENR.

19. Two Decades of "Cold Fusion"

- Provides perspective on the last decade of LENR research and history; ties all other articles together.
- Reviews attempts to suppress neutron results, which contradict hypothesis of D+D "cold fusion."
- Reviews attempts to manipulate media and government to promote D+D "cold fusion."

20. Moving Forward

- Contemplates the future of LENR research and possible applications.
- Suggests insights that may be applicable to other cases of potentially revolutionary science.

1. Taking a New Look at LENR

Many people, institutions and organizations around the world are beginning to take a new look at low-energy nuclear reaction research.

Cynthia Lundgren, chief of the electrochemistry branch in the sensors and electron devices directorate of the Army Research Laboratory, is one such person. Lundgren recently invited LENR researchers to come and brief the Army on the subject on June 29, 2010, in Adelphi, Maryland.

Borrowing from the text in the Nov. 13, 2009 Defense Intelligence Agency *Technology Forecast*, she recognized the importance of LENR:

> If nuclear reactions in LENR experiments are real and controllable, DIA assesses that whoever produces the first commercialized LENR power source could revolutionize energy production and storage for the future. The potential applications of this phenomenon, if commercialized, are unlimited. LENR could serve as a power source for batteries that could last for decades, providing power for electricity, sensors, military operations, and other applications in remote areas, including space.

Lundgren set two goals for the LENR workshop: "assess the current state of art and develop a strategy about moving forward." She also wanted to "better understand the following: What are the major (showstopper) questions about LENR for DoD? Are reactions repeatable? What's the metallurgy? What appears to be the shortest route to the greatest benefit for DoD?"

The slide presentations from the LENR researchers provided limited answers to these questions. This *New Energy Times* special report, under development for several months, will provide more in-depth answers to the many questions that readers have on this subject.

This special report was designed to provide the most crucial information to understand the current issues of the broader LENR field and, to some extent, the underlying science of LENR.

For broad reviews of the science, I recommend my peer-reviewed Elsevier encyclopedia articles "Cold Fusion – Precursor to Low-Energy Nuclear Reactions" and "Cold Fusion History." Elsevier permits me to send copies of my chapters to individuals on request.

An additional resource is "A New Look at Low-Energy Nuclear Reaction Research," an invited, peer-reviewed article I wrote (edited by Jan Marwan) for the *Journal of Environmental Monitoring*. There is a peer-reviewed response from Kirk Shanahan, a researcher at the U.S. Department of Energy national laboratory at Savannah River, and an informal response from me to Shanahan. In the future, there may be a formal response to Shanahan from Marwan.

2. Cold Fusion Is Neither

On March 23, 1989, the world witnessed a science discovery that ignited what may very well be the greatest science controversy of the last 100 years.

It began publicly when electrochemists Martin Fleischmann and Stanley Pons, at the University of Utah, announced to the world that they had discovered what became popularly known as "cold fusion."

Since then, an eclectic group of researchers has been following the pair's footsteps in a two-fold struggle: one, to understand the phenomenon; and two, to convince the rest of the world that "cold fusion" is real.

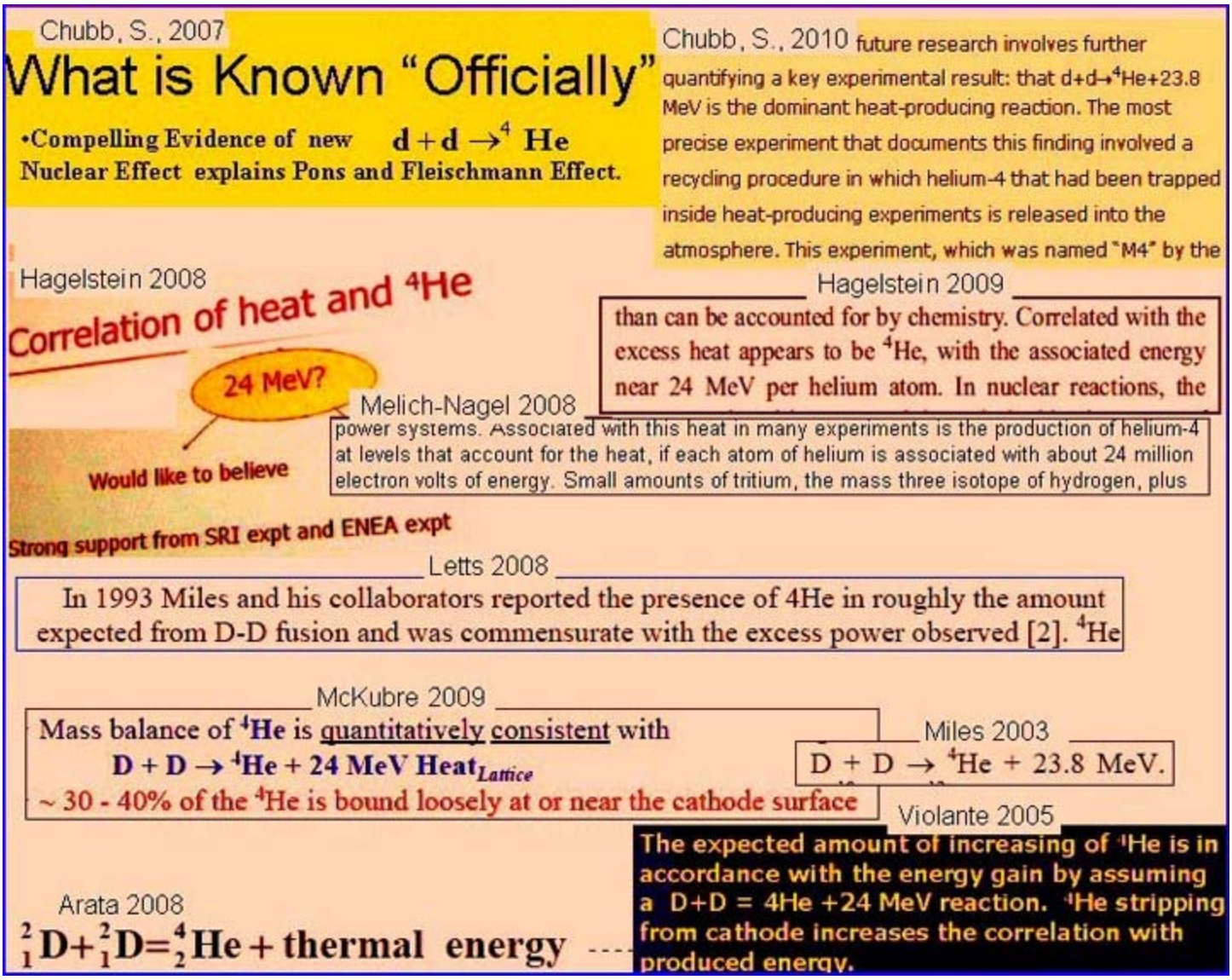
The researchers have claimed that "cold fusion" is the same underlying process as the well-known and accepted thermonuclear phenomenon that appears to occur on the sun, in the stars, and in million-degree experimental test chambers around the world. However, the "cold fusion" researchers have claimed that they can make fusion much more easily and less expensively than the thermonuclear fusion researchers can.

When Fleischmann and Pons first made their discovery, they did not even speculate that their results were caused by the other well-known nuclear process, fission. Their results looked nothing like fission; they were missing key characteristics of fission reactions, and they were not using materials required to create fission reactions.

Fusion was Fleischmann and Pons' best guess because they, as well as most other scientists at that time, were not aware of a third nuclear possibility: weak-interaction processes. Even people who knew about weak interactions had no idea that weak interactions could, in fact, be very energetic, enough to explain the nuclear-scale heat that Fleischmann and Pons observed.

Many "cold fusion" researchers believed that, if deuterium was present in the experiment as an input and helium was present as an output, then by some miracle, nuclear fusion was occurring, even if the idea of room-temperature "cold fusion" contradicted 70 years of experimental and theoretical groundwork. It was as simple as 2+2=4. That is, two deuterons make one helium-4.

Through the 1990s, researchers discussed the "cold fusion" idea in papers and conference presentations, primarily as an hypothesis. Beginning around 2000, however, the character of the discussion shifted. Some LENR researchers began discussing this idea as a fact, not an hypothesis.



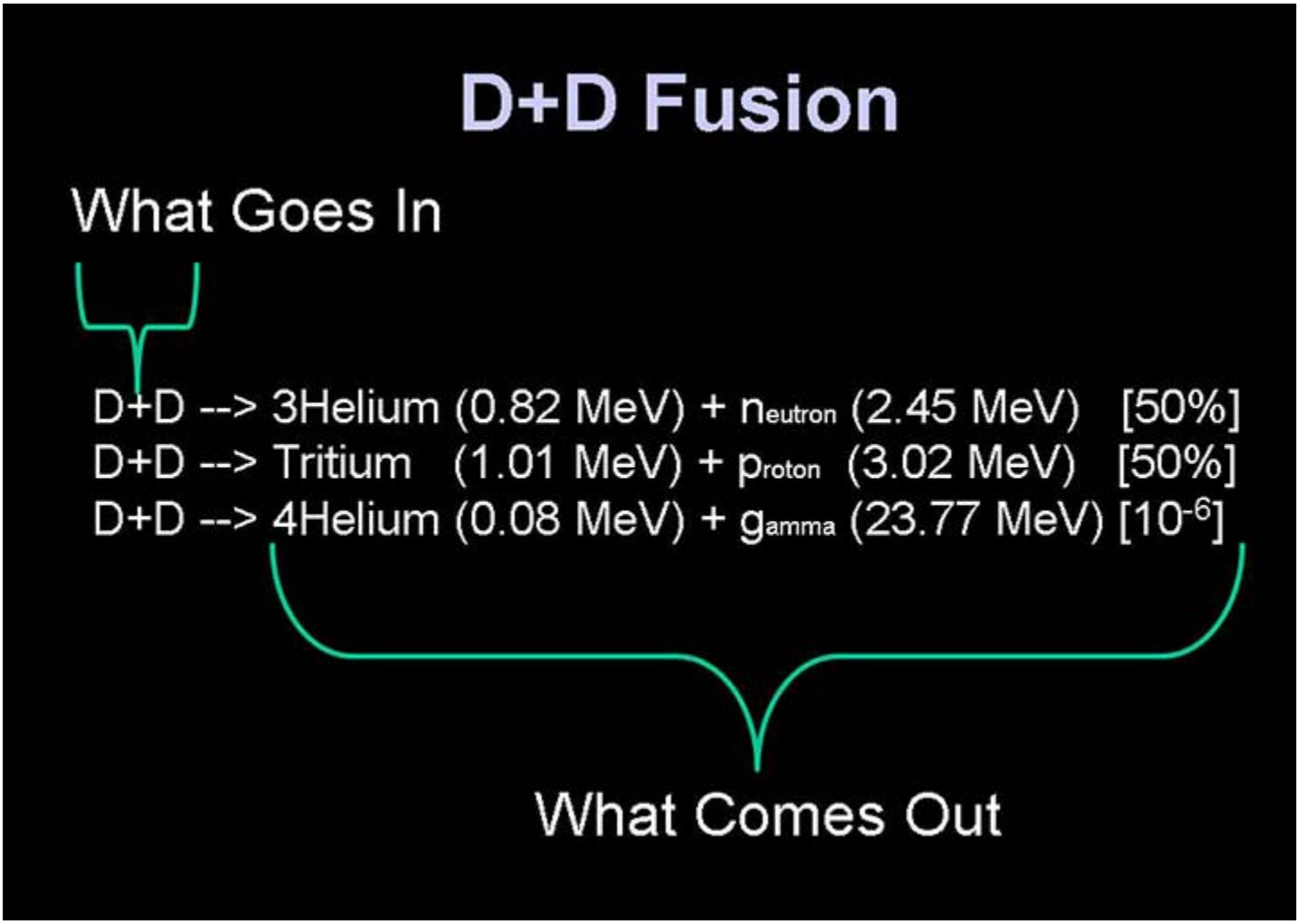
But that little step in the middle where cold nuclear fusion supposedly took place between the two deuterons has always been a problem – a problem that is at the root of some of the most bitter and contentious science debates in the last 100 years.

The logical shift of the D+D "cold fusion" hypothesis to an official fact, however, didn't take place because the researchers made new discoveries. Instead, to get to their new official position, the "cold fusion" researchers invented theories that relied on new physics, invented new untested concepts of metallurgy, were very selective about the data they chose to consider and report as accurate, and, in rare but significant cases, made unscientific changes to data.

Eventually, in 2006, Lewis Larsen and Allan Widom helped the world to see how "cold fusion" could be explained much better by, primarily, weak-interaction processes. Like most new ideas, theirs did not take quickly. In fact, it drew bitter opposition and hostility from researchers who had fixed their minds on "cold fusion."

With hindsight, observers can see that LENR does not look at all like fusion. Only one kind of fusion is and always has been scientifically confirmed: thermonuclear fusion.

When deuterium-deuterium thermonuclear fusion occurs, three sets of reaction products come out.



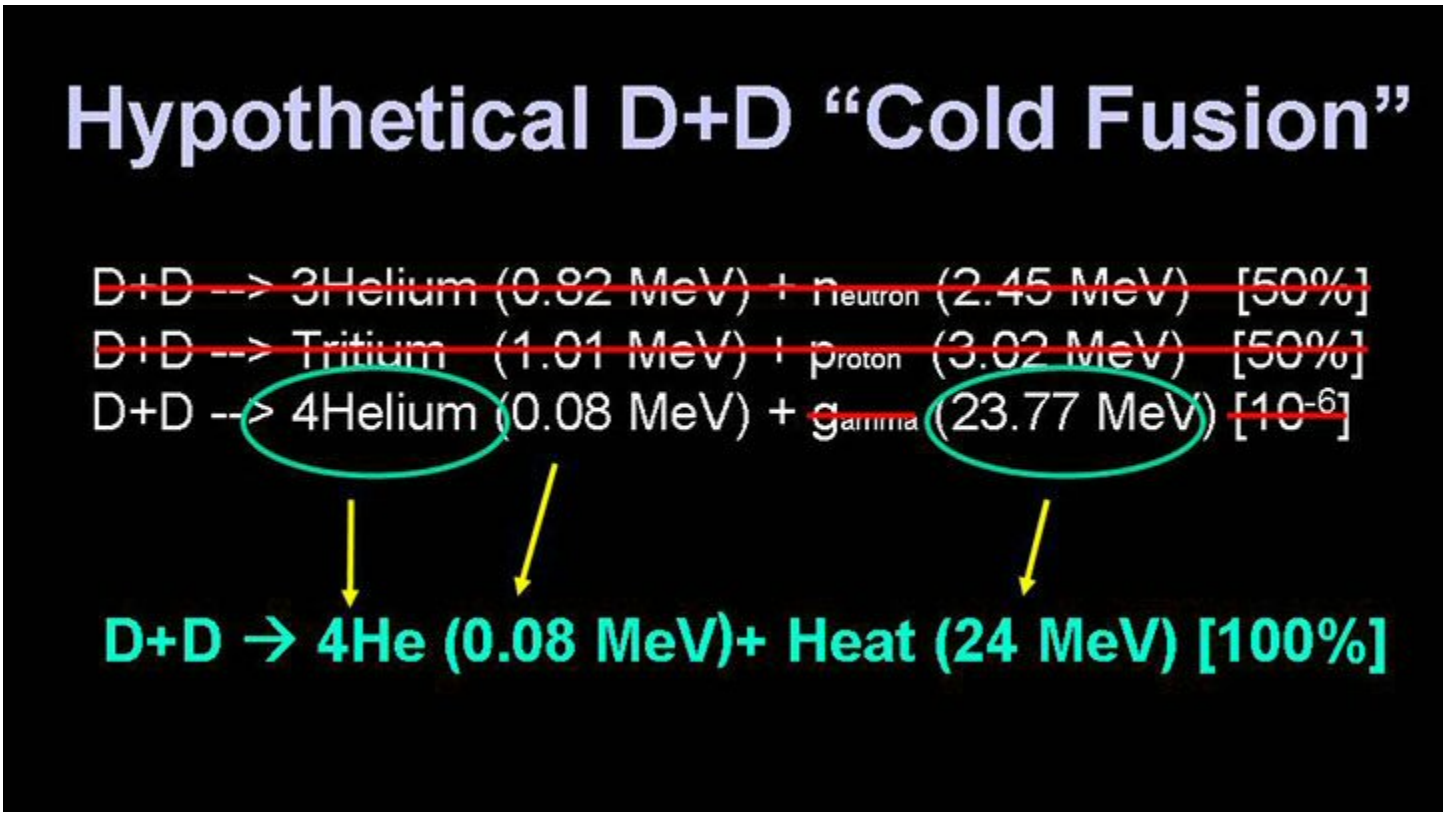
In the first set, or branch, a helium-3 atom and a neutron are produced. This reaction pair makes up about 50 percent of the total reaction products from a given set of fusion reactions. The second set produces a tritium atom and a proton. As with the first branch, these products also make up about 50 percent of the total reaction products. The third branch produces a helium-4 atom along with a gamma ray, but this branch occurs very rarely, about one time in a million.

These relationships are specific and constant. In fusion, all three branches occur, and they all occur with the known probabilities (~50%, ~50% and 10^{-6}), and each branch always has its specific pair of reaction products.

Within each branch, the physics is even more specific. Each product emits precisely known kinetic energy. The entire set of data is very specific and is confirmed by both theory and experiment.

Even "cold fusion" researchers acknowledge that "cold fusion" does not look like fusion as we now know it. Within the field however, there is little argument that LENR produces, among other products, helium-4 and nuclear-scale energy, in the form of heat.

But many LENR researchers took these two phenomena, disregarded all the other LENR phenomena, and asserted for many years that helium-4 and heat are the only products or only main products of LENR. Their hypothetical adaptation of fusion to "cold fusion" looks like this:



Michael McKubre and Vittorio Violante are two of the leading proponents of the "cold fusion" hypothesis. Two months ago, *New Energy Times* published an investigation about the pair's "cold fusion" claims.

The "cold fusion" hypothesis may be on its way to obsolescence, however, because McKubre and Violante, for the first time in perhaps a decade, omitted any reference to this hypothesis as part of their presentations at the March 2010 American Chemical Society national meeting.

The "cold fusion" hypothesis developed from several fundamental assumptions; here are three of the most significant ones.

Assumption #1: Helium-4 as the Sole Product
Early in "cold fusion" history, researchers were not seeing and they are still not seeing amounts of helium-3, neutrons, tritium or protons large enough to explain the excess heat. These were the expected products from deuterium-deuterium nuclear fusion. Eventually they recognized helium-4 as the dominant gaseous product of LENR. Because many "cold fusion" researchers were looking at "cold fusion" through the lens of fusion and because they didn't see the expected nuclear fusion products, they assumed that there were no other nuclear products.

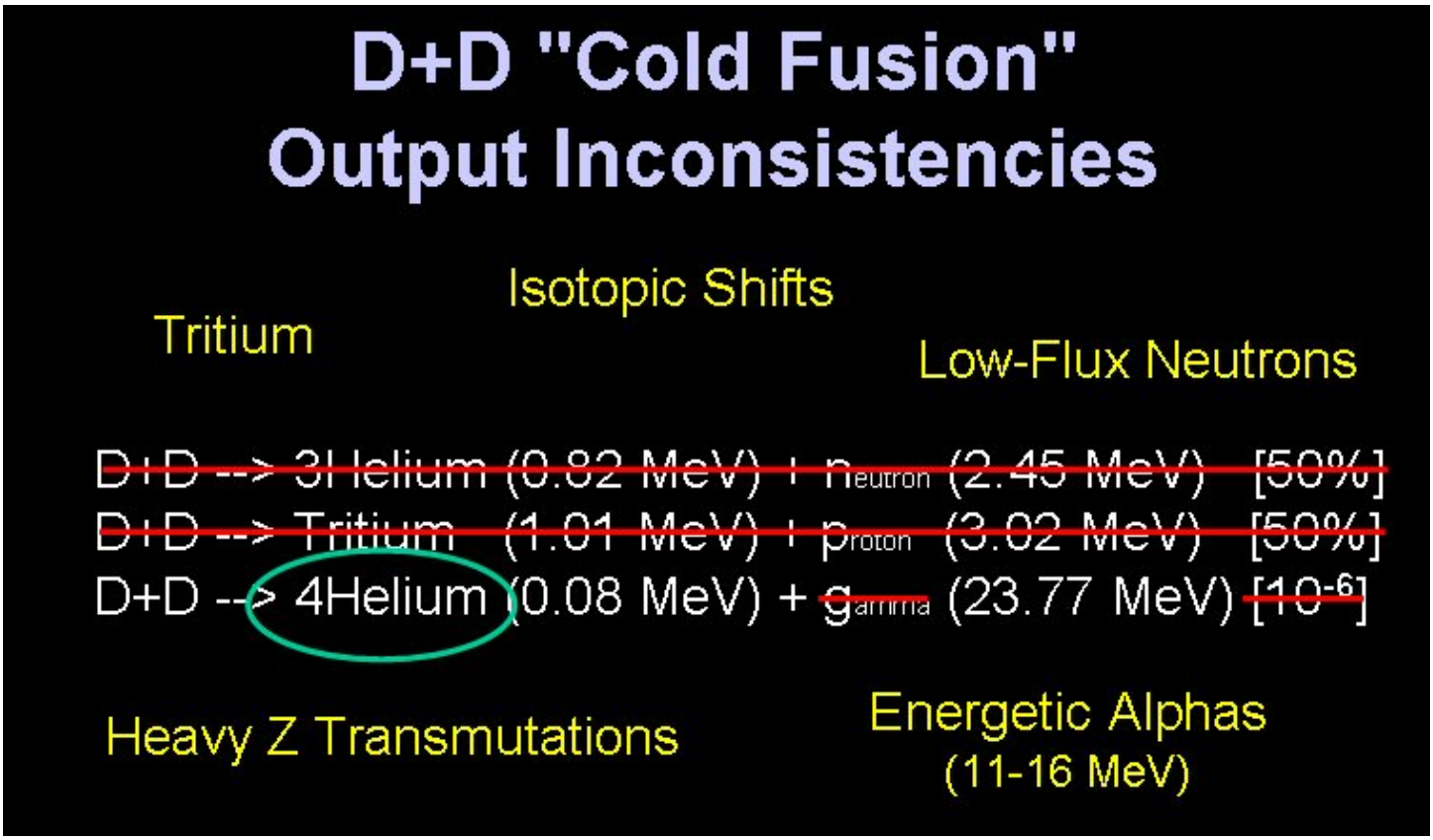
Assumption #2: 24 MeV Heat per Atom of Helium-4 as the Total Energy
Attached to the first assumption of helium-4 is the "cold fusion" researchers' assumption that they should expect about 24 MeV heat per atom of helium-4 as the total energy of the system, because 24 MeV is the mass difference between a pair of deuterons and a helium-4 atom.

Assumption #3: Helium-4 Is Born With an Energy of 20.2 KeV or Less
For Assumption #2 to be valid, the helium-4 atom must be emitted with no more than 20.2 KeV. Otherwise, the math doesn't balance, and "cold fusion" won't look even remotely like the third branch of fusion.

However, these assumptions have fundamental problems:

Problem #1: LENR Products Are Inconsistent With the Hypothesis of D+D "Cold Fusion"

For the "cold fusion" hypothesis to be valid, the researchers had to ignore a multitude of other nuclear products and phenomena that had been reported throughout the 21 years of LENR research. Most researchers in Russia, Italy, France, India and Japan maintained a broader perspective and paid more attention to these other phenomena than did their counterparts in the U.S. The slide below depicts some of the other products and effects of LENR, which are inconsistent with the hypothesis of D+D "cold fusion."

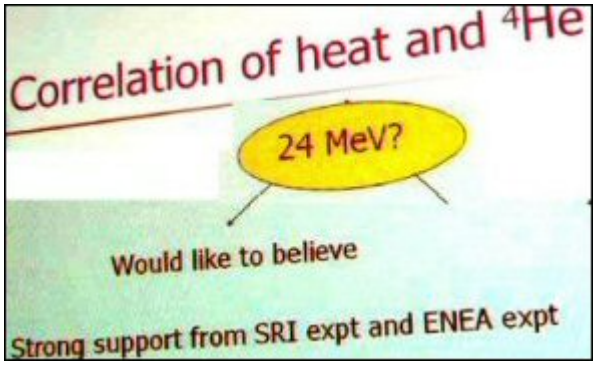


Problem #2: LENR, in General, Doesn't Produce 24 MeV

On March 21, 2010, Peter Hagelstein made the following statement at the American Chemical Society press conference on "cold fusion" about the alleged 24 MeV in LENR experiments:

The evidence in support of helium associated with energy production in the Fleischmann-Pons experiment — that helium-4 is seen in association with excess power — comes from a number of experiments — more than 10 experiments where people have seen that kind of thing — and there's two measurements where the correlation shows a Q-value or an energy per helium-4 of about 24 MeV.

At the 14th International Conference on Condensed Matter Nuclear Science in Washington, D.C., in August 2008, Hagelstein said that he would like to believe in the 24 MeV hypothesis. He specifically mentioned that the SRI International experiment, led by Michael McKubre, and the ENEA-Frascati experiment, led by Vittorio Violante, provided "strong support" for this belief. Thus, we know the two best experiments on which Hagelstein bases his belief in the 24 MeV hypothesis.

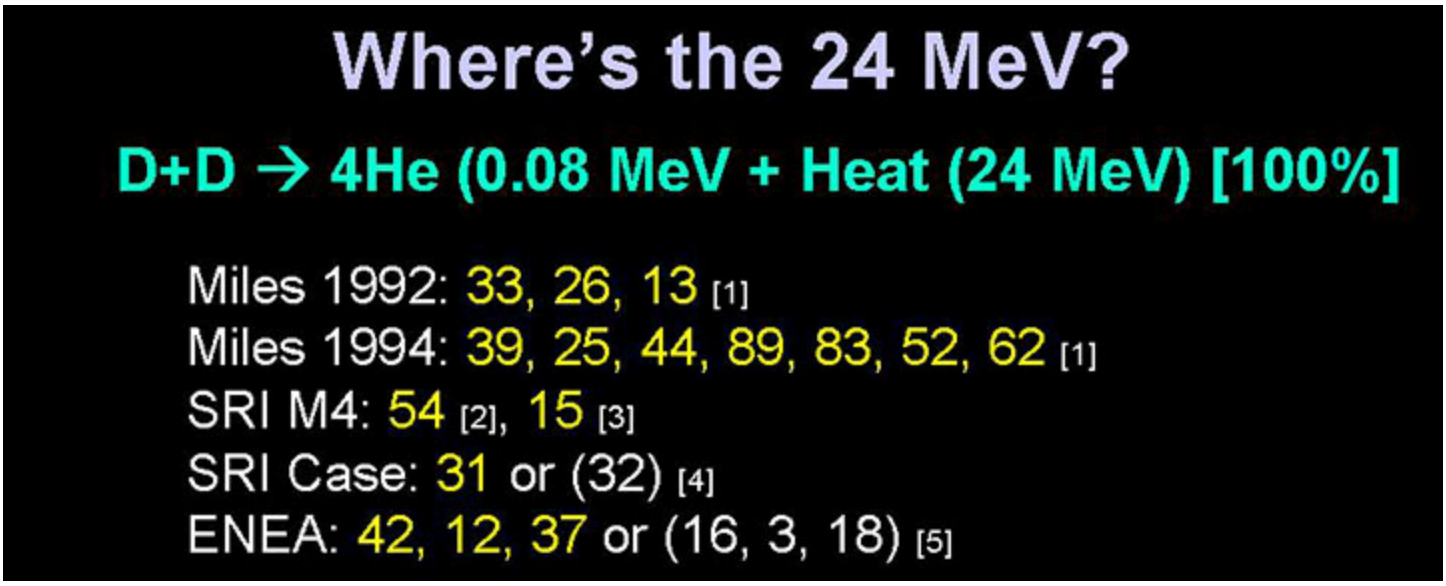


Hagelstein's slide from 14th International Conference on Condensed Matter Nuclear Science in Washington, D.C.

One problem is that, to get the value of 24 MeV that SRI International reported, McKubre made 12 unscientific changes and representations between 2000 and 2009 to data from an experiment that was performed in 1994. See "When Nuclear Is Not Enough: A Tangled Tale of Two Experiments" for full details.

A second problem is that Violante said that he had obtained a 24 MeV value, yet when *New Energy Times* scrutinized his representation of "24 MeV," Violante clearly had not obtained such a value, and he admitted as much after responding to questions from *New Energy Times*.

What does a broader search for "24 MeV" turn up? The following slide lists the most well-known experimental studies:



The values from these five sets of experiments run a wide spectrum, ranging from 12 to 89 MeV.

In contrast with the "cold fusion" claims made by McKubre and Hagelstein in their publications for SRI experiment M4[4,6], the scientific documentation[2,3] does not show a value of 24 MeV but rather values for 54 MeV in one run and 15 MeV in the other.

Similarly, Violante's experiments do not show a value of 24 MeV but values of 42, 12, and 37 MeV, or 16, 3, and 18 MeV, depending on how you read the helium-4 background value.

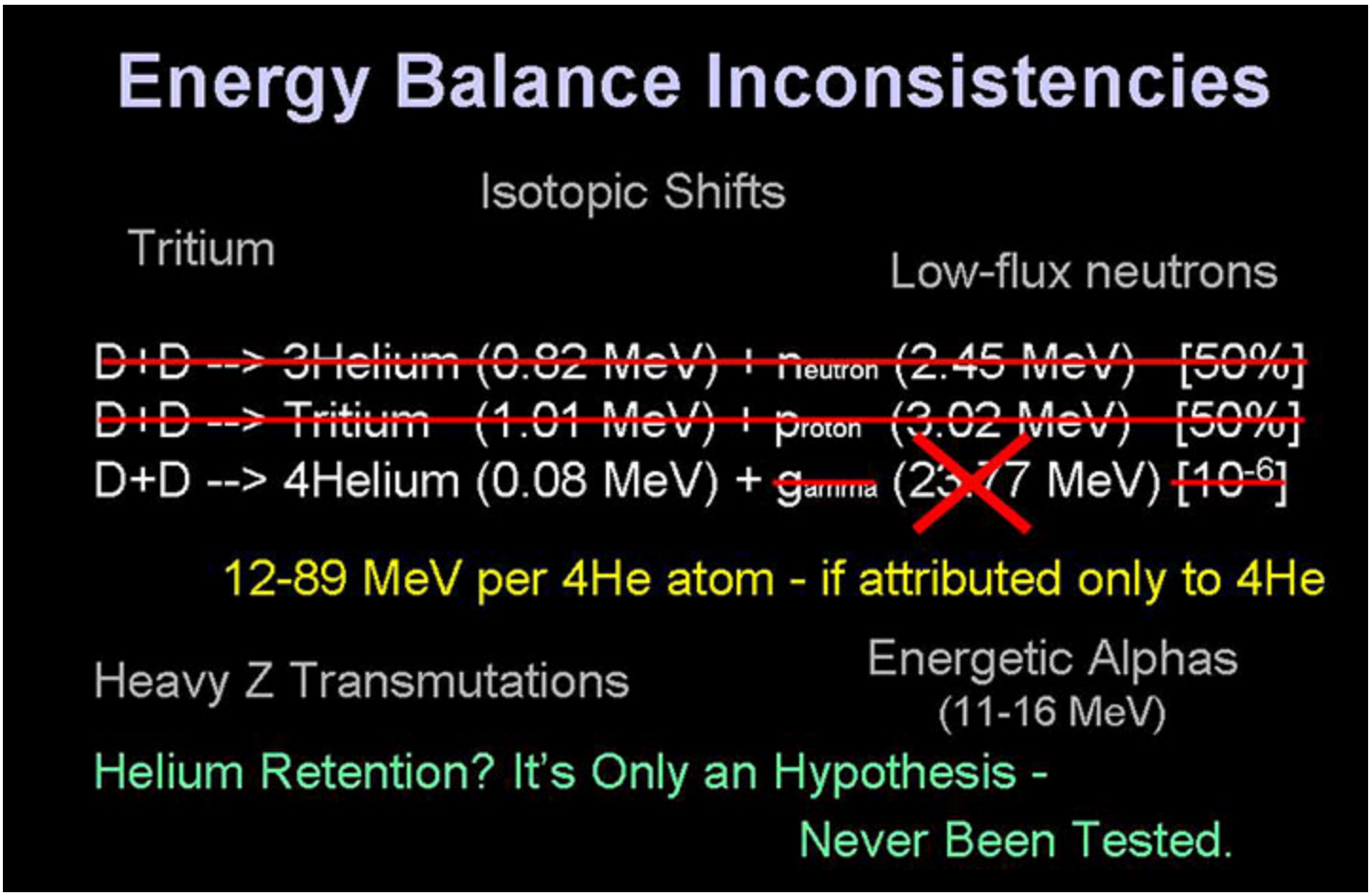
The closest single experimental run to 24 MeV was that of Melvin Miles in 1994, when he worked at the Navy's China Lake laboratory. The average of all those runs gives 56 MeV. However, the average of his 1992 runs gives 24 MeV. As explained in "Bockris' and Miles' Historic Confirmations of LENR-Produced Helium" in this issue of *New Energy Times*, Miles, however, has never reported this value in a paper or presentation.

Problem #3: Energy Balance Inconsistencies

Even if LENR experiments did produce a total of 24 MeV per helium-4 atom, those two phenomena, heat and helium-4, are meaningless. Meaningless? After all this drama and debate about the proximity or not of LENR experiments to 24 MeV?

They are meaningless because there are other nuclear reaction products (and possible inputs) in LENR experiments, with both heavy and light hydrogen, that release nuclear energy.

In other words, **D+D + a, b, c –> 4He + x, y, z**, where a, b and c are other inputs and x, y and z are the other products. Unless researchers know precisely what energy is associated with all nuclear products in the system, attributing a specific amount of energy to any one of the nuclear products in the system is impossible.



Many "cold fusion" proponents have suggested that there are no other energetic nuclear products in these systems. They have also suggested that the other nuclear products occur only in light hydrogen systems, not the deuterium/palladium systems. These are both myths. See "Isotopic Anomalies Reveal LENR Insights" and "Who's Afraid of LENR Transmutations?" in this report.

There are also gross inconsistencies with the products of fusion compared with LENR, as the product ratios in this image show:

Product Inconsistencies

	FUSION	LENR
Neutrons:Tritium	1:1	1:1,000,000
Neutrons:4-Helium	10,000,000:1	1:10,000,000

The ratio of neutrons to tritium in fusion versus LENR is directly opposite, as is the ratio of neutrons to helium-4.

And then there are the inconsistencies of the input materials that are used in fusion compared with LENR.

Input Inconsistencies

	What Goes In
D+D Fusion	Deuterium gas
LENR	Deuterium in heavy water, Deuterium gas, Hydrogen in normal water, Hydrogen gas, Li, C, Pt, Pd, Ti, Ni, Al, W

The input materials to D+D fusion are very simple: deuterium gas. The inputs in LENR systems are a complex and highly variable mixture of materials.

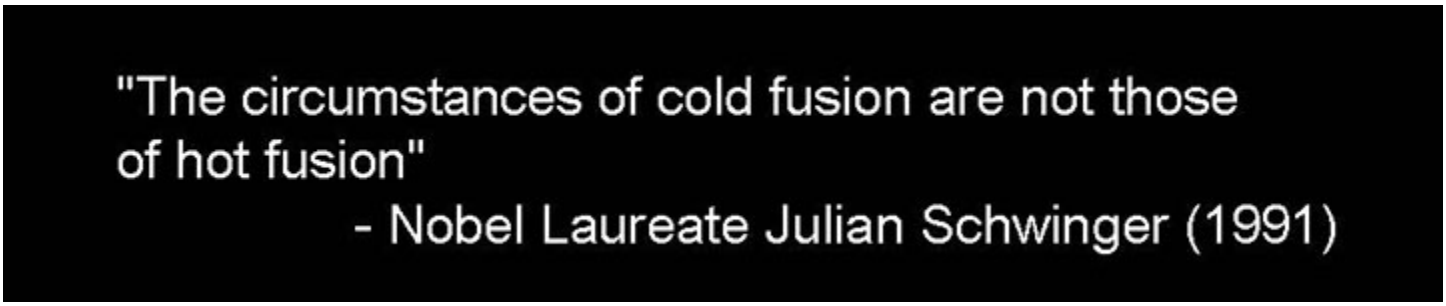
One of the few people who took a more holistic view of LENR was George Miley, a professor at the University of Illinois. Miley's background is in nuclear research, and he earned his Ph.D. in chemical and nuclear engineering.

In 2003, he presented his picture of LENR phenomena, compared with thermonuclear fusion.[7] Miley saw that LENR transmutations and "cold fusion" were part of the same larger picture. Of course, he, like many other researchers, understood that the LENR transmutations could not be explained by "cold fusion."

	Reaction Products	Comments	
D-D Reaction D-D	T + p	Each branch 50% probable "Normal" D-D Fusion	
	He-3 + n		
	He-4 + hυ (heat)	"P-F type Cold Fusion"	LENR
Lattice Reaction p/D + metal	Isolated Isotopes or "fission" product array	"Transmutations"	

Miley's 2003 general comparison of thermonuclear reaction products (first three rows) to LENR reaction products (fourth row).

For many years, the loudest proponents of "cold fusion" have repeated Nobel Laureate Julian Schwinger's mantra in explaining why "cold fusion" doesn't look like fusion.



With the benefit of hindsight and 21 years of scientific research to draw on, it is clear that the circumstances, inputs and products of LENR are not those of fusion: hot, cold or otherwise.

References

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2. EPRI TR-107843-V1, pg 3-223, pdf pg 351; Math: 22.4/41=0.54.

3. EPRI TR-107843-V1 pg 3-223, pdf pg 351; Math: 1.66/1.13 = 147%; 22.4/147=0.15.

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7. Miley, G.H. and Shrestha, P., "Review of Transmutation Reactions in Solids," Proceedings of the Tenth International Conference on Cold Fusion , Cambridge, Mass., (2003).

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

(For a complete, step-by-step account of this investigation, please refer to the accompanying 101-page slide presentation.)

This account documents how numerous scientific data points and values were gradually changed, added and deleted during a 10-year period by electrochemist Michael McKubre at SRI International – all without scientific explanation, most without notification.

McKubre's objective seems to have been to provide support for his colleague, friend and MIT professor Peter Hagelstein, who, after more than 150 attempts, thinks he's figured out a theory for "cold fusion."

The key experiments, called the "M4" series, were performed at SRI International in Menlo Park, Calif., in 1994 and first reported to and published by the Electric Power Research Institute, the sponsor of the project, in 1998.

Beginning in 2000, McKubre and Hagelstein, one of his co-authors, began to change and report the revised data without telling people they had done so. These changes appear to have gone unnoticed until now.

While I was editing and fact-checking a LENR paper submitted for a nuclear energy encyclopedia, I noticed inconsistencies about how M4 had been reported, and I began to investigate.

The extent of manipulation I found was surprising and required thorough investigation. Many of the changes were initially difficult to detect and were buried in, among other documents, a 379-page technical report from EPRI.

The changes are unambiguous and precisely documented in a publicly available paper trail. Only the scientific basis for the changes remains unexplained and ambiguous.

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 1,500 hours**

- **Data point 3 removed**
- **Data point at 500 hours added**
- **Data point at 525 hours added**
- **"Cycling Procedure" changed from 200-hour to 600-hour duration**

As part of McKubre's explanation to support his claim of D+D "cold fusion," he hypothesizes that helium-4 somehow hides out in the palladium cathode as a result of LENR processes. McKubre hypothesizes that helium-4 also somehow releases from the cathode by electrochemical processes.

However, no evidence whatsoever in the precisely detailed EPRI report even suggests that the researchers made attempts to dislodge any helium that might have been hiding out. There is no known public record that McKubre ever tested his hypotheses.

On the contrary, a record in the EPRI report shows that the hypothesis *should be tested*. All facts suggest that McKubre's helium retention concept is an idea that went from hypothesis to fact without any scientific confirmation. And a *New Energy Times* survey of related scientific reports of helium in metals indicates that that McKubre's hypothesis would represent novel behavior of helium in metal. The following table summarizes our survey:

Helium in Metals Analysis		SBK 2010
Behavior in Metals at or Near STP	**Hydrogen**	**Helium**
Permeates (Diffuses) Through Intact and Defect-Free Metal	Yes	No
Permeates (Diffuses) Through Defects and Grain Boundaries	Yes	Yes
Soluble (Dissolves) in Metal	Yes	No

New Energy Times asked McKubre three times, in writing, for an explanation about some of the changes he made in the 12-year period. He has not responded.

On March 21, 2010, at a press conference during the American Chemical Society meeting in San Francisco, *New Energy Times* again asked McKubre about some of the changes. Not only did he fail to provide information that explained a scientific basis for these changes, but he also said he reported "a correction" to EPRI. That, too, failed to materialize.

When *New Energy Times* fact-checked with EPRI later that week, the EPRI representative contacted the two program managers who were directly involved in the project and stated to *New Energy Times* that it had no record of any correction.

McKubre also implied that the public should disregard the information in the 1998 EPRI report (which reveals McKubre's contradictions) even though the EPRI report states that it is a "corporate document that should be cited in the literature."

The day after the press conference, however, McKubre gave his scheduled talk at ACS, but the slides about experiment M4, perhaps for the first time in 10 years, were missing. Maybe he was making a retraction.

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.

Helium cannot be produced by ordinary chemistry. That's a fact. The helium production observed in M4 and SRI's other experiments 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 changes in McKubre's reporting of M4 seem to have begun when McKubre realized that one of his other rigorous experiments showed evidence that did not strongly support Hagelstein's theory.

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

4. NRL 2008—The LENR Null Results Laboratory

Introduction

David Kidwell
Photo: Edward Wall

According to research scientist David Kidwell and his colleagues at the Naval Research Laboratory (NRL), researchers who have made claims of LENR transmutations throughout the last two decades should be "embarrassed" about making such claims.

Kidwell's reasoning is that their results are caused by sources of contamination rather than LENR transmutations. Kidwell is one of several researchers at Washington, D.C.-based NRL who have been particularly well-funded in recent years to perform LENR research. Their group falls under the command of Bhakta Rath, associate director of research and head of the materials science and component technology directorate at NRL. Despite their abundant funding, they have had few LENR publications, and they have claimed no positive LENR results in nearly two decades.

This article begins with a brief background on LENR transmutation, then reviews NRL/Kidwell's attempt to discredit LENR transmutation at the 14th International Conference on Condensed Matter Nuclear Science in August 2008.

A Few Words on Transmutation Terminology

Let's begin with a discussion about terminology. Readers likely have one of two perspectives.

Readers who view the change of deuterium and other materials in LENR experiments to helium-4 as a fusion process likely view the LENR transmutation experiments, which involve elements across the periodic table, as the result of some other, perhaps-unknown nuclear process.

Readers who view the change of materials to helium-4 as a transmutation reaction, perhaps caused by a neutron-catalyzed process, likely consider the distinction made by the first group of readers artificial and irrelevant. In other words, the whole field of LENR research comprises transmutation reactions of one sort or another. Perhaps it has very little to do with nuclear fusion, though it does have to do with other nuclear processes.

This article does not attempt to ascertain which view is more accurate. However, it assumes that "transmutation" experiments may involve changes to or from any elements heavier than the very lightest of the elements, hydrogen and helium, for example.

This article also does not explain the difference between the time-honored understanding and textbook definition of "nuclear fusion" and "neutron capture." That is discussed in "Neutron Capture Is Not the New Cold Fusion."

Localized Perspectives on LENR Transmutation Research

Many readers from the United States appear to see this subject from an entirely different perspective than readers from Russia, France, India, Japan and Ukraine, for example.

Many American LENR researchers believe that LENR transmutation is less important and less credible than the subset of experiments with deuterium-palladium and heavy water that reportedly produce heat and helium as their primary if not exclusive products.

In Russia, for example, the title of the local regional LENR conference series, "Russian Conference on Cold Nuclear Transmutation of Chemical Elements and Ball Lightning," reveals a broader view of the subject matter.

One of the most prominent LENR researchers in China, Xing Zhong Li, professor emeritus of Tsinghua University, who was the chairman of the Ninth International Conference on Cold Fusion, recently completed a transmutation experiment that replicated and confirmed a significant Japanese transmutation experiment.

Although Li was not able to attend and present his work at the American Chemical Society national meeting in San Francisco, California in March 2010, his abstract is informative.

"Nuclear transmutation was discovered on the surface of palladium film using scanning electron microscopy," Li wrote. "SEM analysis revealed that new elements (Cu, Zn, Si, etc.) were detected in the permeation area, but there were no such elements in the original palladium film or in the ring-shape area where no permeation happened."

However, reports of transmutation research at the two most recent ICCF conferences, ICCF-15 and ICCF-14, are difficult to find. ICCF originally meant International Conference on Cold Fusion. About 10 years ago, researchers in the field changed the meaning of ICCF to International Conference on Condensed Matter but kept the ICCF acronym, presumably because of their attachment to the concept of "cold fusion."

The ICCF-14 conference organizers initially did not include transmutations as part of the conference agenda, adding it only after several people protested. During ICCF-14, those organizers gained political control of the subsequent ICCF-15 and thus were able to continue a similar but less-obvious strategy to downplay transmutation research.

For a glimpse of the broad interest (outside the U.S.) in LENR transmutations, you have to go back to ICCF-13 in Russia (2007), ICCF-12 in Japan (2005) and ICCF-11 in France (2004).

One of the most comprehensive references on the worldwide research in LENR transmutations is a review presented at ICCF-10 in 2003 by George Miley, professor at University of Illinois.[1]

The French journal *Science et Vie* published a useful article on the topic in May 2004.

We cannot attempt to cover the broad extent of LENR transmutation research in this article. We direct readers to the Conferences page at the *New Energy Times* Web site.

Talbot Chubb's Review of Mitsubishi/Iwamura LENR Transmutations Claims

The rare strong enthusiasm about transmutation from Americans was evident after ICCF-10 in Cambridge (2003), in Talbot Chubb's Sept. 12 and Oct. 13, 2003, "News Flashes."

Chubb, now retired, is no newcomer to nuclear physics. Highlights of his long career in science include work on the Manhattan Project during World War II at Oak Ridge National Laboratory.

In his News Flashes, Chubb wrote about the experiments pioneered by LENR researcher Yasuhiro Iwamura, of Mitsubishi Heavy Industries, and the confirmatory replications at Osaka University from a group led by Akito Takahashi. In the late 1990s, Iwamura developed an innovative experiment to show LENR transmutations in a deuterium gas environment.

"The Osaka and Mitsubishi studies provide solid evidence that deuteron or alpha-addition nuclear reactions can be made to reproducibly occur on solid metal at a temperature below that of boiling water," Chubb wrote.

The essential part of the experiment is a substrate containing layers of palladium and calcium oxide. Atoms from the source element are placed on the surface. Deuterium gas is passed through the substrate. No energy, aside from the flowing deuterium and a small heater, is applied to the experiment. When Iwamura added cesium on the surface of a palladium complex, praseodymium gradually emerged on the surface and cesium gradually decreased after the palladium complex was subjected to deuterium gas permeation. The cesium ⇒ praseodymium change is one of several pairs of elemental transmutations he has demonstrated.

Chubb knew that the Iwamura work could not be explained easily by the "cold fusion" hypothesis, but he sensed that it was, in fact, part of a larger picture.

"The transmutations are not cold fusion results *per se*," Chubb wrote, "but are likely part of the same 'active deuterium' physics that is responsible for deuteron cold fusion."

Even though Chubb could not see a theoretical explanation for the Iwamura-type transmutations through the lens of his "cold fusion" perspective, he saw something profound:

> The new discoveries remind one of the beginnings of neutron-capture physics. In 1932 Chadwick discovered the neutron. His neutrons were produced by the impact of alpha particles on beryllium. Within a few years a large number of previously non-existing types of nuclei were synthesized by exposure of various

target elements to neutron irradiation. During these neutron-absorption studies uranium fission was discovered and the new element plutonium was synthesized. By the end of 1942 the first controlled nuclear reactor was already in operation. A nuclear power plant was generating electricity in 1955.

NRL/Kidwell's Attempt to Discredit LENR Transmutation at ICCF-14

Let's start with the scientific aspects of Kidwell's 2008 presentation at ICCF-14 in Washington, D.C., Aug. 10-15, 2008.

His first 20 slides explain the intricacies of performing elemental trace analysis, his specialty. According to Kidwell, LENR researchers too easily fool themselves into thinking that they have observed nuclear transmutations when they are, in fact, seeing only contamination.

At about 10 minutes into his presentation (click for video), on slide 21, he refers to "Arata experiments" that show a three- to five-times increase of silver inside a palladium cathode after a LENR experiment. These measurements were performed by neutron activation analysis (NAA) at the University of Texas, Austin.

Kidwell was not sure about the exact source of the materials, as he told *New Energy Times* on Oct. 8, 2008. He thought they had come from SRI International and were from an Arata-Zhang replication experiment. He received them from Thomas Passell, a retired program manager for Electric Power Research Institute.

A cross-check of two of Passell's papers confirm that Passell had received the samples from Arata (not SRI) and given the samples to NRL around 2003 so NRL could determine the isotopic abundances of silver.[2,3] Kidwell said he had no contact with Arata.

At ICCF-14, Kidwell reported that he had performed a "bulk analysis" on the material and found 13 times more silver in the post-electrolysis sample than in the virgin sample, even more than was registered by the NAA at the University of Texas.

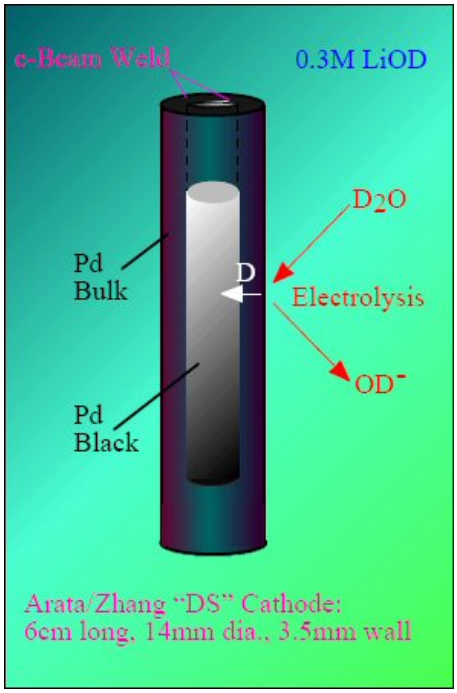
In this case, "bulk analysis" does not necessarily distinguish between the inner structure and the outer surfaces because the entire sample is vaporized; the inner structure and outer surfaces are averaged together.

"We showed, yes, there's about 13 times more silver in the material that underwent this Arata experiment than there was in the starting material," Kidwell said. "However, the isotopic ratio is normal, absolutely normal, no difference whatsoever between that and natural silver. How could that be? Where did the silver come from?"

This is potentially a key point: Researchers generally agree that, if measured isotopic ratios from an experiment for a given element are identical or nearly identical to ratios found in natural abundances, then the isotopes found in that experiment likely are from some kind of natural occurrence rather than, for example, a LENR transmutation.

For reference, Kidwell states in his paper that the natural ratio for silver's two isotopes is 52:48 (Ag107/Ag109). However, we have to take on faith Kidwell's claim that the ratio of the test sample is "absolutely normal," because his slides, recorded talk, and paper do not present his measurements for the Ag107/Ag109 post-electrolysis ratio.

Assuming the ratio is "absolutely normal," this still leaves the very significant mystery of how the silver content grew 13-fold. Kidwell is not without an answer, sort of. He speculates that trace amounts of silver present in the hollowed-out palladium cathode segregate and preferentially migrate from and through the walls of the cathode and contaminate the palladium-black powder inside the container during the experiment.



Arata-Zhang Double-Structure Cathode[4]

"Well, as it turns out, palladium is known to have silver added as a hardening agent," Kidwell said. "It's contained in this container, which has deuterium pumping through it.

"Some of that silver can migrate from container walls into the material on the inside, and of course it's going to be natural isotopic silver because it was natural isotopic silver on the outside of the container. We actually were not able to analyze the [actual] container. [Instead,] we were given part of [another] container [inaudible], and it actually contained about 0.008 percent silver."

Once Kidwell states the few scientific facts that he was able to obtain, he ties his hypothetical scenario together with a pair of pure guesses.

"I'm not sure that's sufficient," Kidwell said, "but once we analyzed the remaining container, I'm reasonably certain it had a higher concentration of silver."

First, he guesses about the amount of silver contaminate sufficient to explain the post-experiment silver. Second, he guesses that the container from the experiment had a higher concentration than the container he measured. His guesses suggest that his process may be based more on disbelief than data.

Failure to Test Actual Container

Kidwell's hypothesis has more problems. First, Kidwell states that the silver is "contained in this container," giving the impression that he found silver in the double-structure cathode used in the experiment. He did not. He tested a different container.

Kidwell's explanation requires that we take on faith that the original container (cathode), which he did not test, had at least 0.008 percent silver.

Failure to Test Whether 0.008 Percent is Sufficient

Kidwell's hypothesis requires us to take on faith that 0.008 percent silver, assuming that it or more was in the actual container, was a sufficient quantity of silver to cause a 13-fold increase of silver inside the center of the container, assuming silver can preferentially migrate through the palladium cathode.

Failure to Test Preferential Migration Speculation

Kidwell tested only for the presence of silver. He did not test to see whether silver, if present in sufficient amounts, can do as he speculates – preferentially migrate through the walls of this double-structure palladium cathode – in this specific experimental configuration.

Does solid silver segregate from palladium and migrate preferentially through solid palladium under these conditions? We do not know because Kidwell provides no facts to support his speculation.

Failure to Test Preferential Migration Against Gas Pressure Speculation

Next problem: Kidwell talks about the pressurized "container which has deuterium pumping through it."

Readers unfamiliar with the experiment might assume that Kidwell is suggesting that external pressure on the container (cathode) forced the silver in the container walls to migrate inward and contaminate the palladium-black in the center core of the container.

However, there is no external pressurization; the double-structure cathode is encumbered merely by the heavy water in which it sits. The heavy water undergoes electrolysis, and the evolved deuterium easily permeates the palladium over time.

Also, the pressure comes from inside the cathode, as Passell explained in his 2000 paper.

"Once the palladium powder is placed inside the core of the cathode," Passell wrote, "it had to be electron-beam welded because the deuterium pressure could reach extremely high

values during electrolysis. In fact, some cathodes failed in a burst mode during electrolysis due to this pressure if not properly welded."

Kidwell might have known of the internal pressure, too. On Oct. 8, 2008, he wrote to *New Energy Times* and explained that the palladium-black samples from the experiment, as well as virgin material, were sent to him "blind," but he could observe differences between them.

"You could guess which was which by the powder size," Kidwell wrote. "The starting material was very fine, whereas the final material had conglomerated as if it was heated. Nano palladium will fuse at about 1/3 of the melting temperature, or about 500C."

Kidwell's attempt to explain the silver as something other than the result of LENR transmutation has more problems.

Failure to Disclose Previous NAA Results

Kidwell did not tell the audience at ICCF-14 and readers of his paper the rest of the story. NAA was performed on this palladium-black sample at the University of Texas, Austin, research reactor at the request of Passell. Passell is an expert in nuclear chemistry; he studied under Dr. Glenn T. Seaborg at the University of California, Berkeley.

Passell confirms, in his 2003 paper, that NAA does show a 2.87-fold increase in silver; he says that NAA, however, is not an optimal method to characterize isotopic ratios of silver.

Here are a few more things Passell reported from the two University of Texas NAA analyses of the Arata-Zhang palladium-black samples[2,3] from the same batch that Kidwell analyzed. Kidwell did not mention these facts:

- 8% increase in Pd110/108 ratio in sample B
- 2.87 times the silver-109 content in sample B
- 1.66 times the cobalt-59 content in sample B
- 15.2 times the zinc-64 content in sample B
- 0.6 times the gold-197 content in sample B
- 0.37 times (decrease) the iridium content in sample B
- 1.62 times the lithium-7/lithium-6 ratio in sample B
- 8 times the iridium content (PPM by weight) in sample A*
- 0.4 times (decrease) the iridium content by weight from sample B
- 6 times the iridium content (PPM by weight) in sample C
- 5.5 times the gold content by weight from sample A
- 0.1 times the gold content by weight from sample B
- 0.7 times the gold content by weight from sample C
- 24% increase in Pd-110/Pd-102 ratio relative to virgin palladium from sample A
- 6% increase in Pd-110/Pd-102 ratio relative to virgin palladium from sample B
- 21% increase in Pd-110/Pd-102 ratio relative to virgin palladium from sample C

In his 2000 paper, Passell reminds readers that the typical skeptical dismissal of LENR transmutation from electrolytic experiments, by cathodic deposition, cannot be used to explain away these results.

"The conventional explanation for such increases is the cathodic deposition of electrolyte impurities on the cathode surface," Passell writes. "However, all these samples were protected from the electrolyte inside the gas-pressure-tight hollow core of the cylindrical cathode."

Passell writes that researchers in the Department of Physics at Tsinghua University, Beijing, China, using a completely different type of LENR experiment, independently reported a similar "severe alteration" of the zinc isotopic ratio.[5]

Non-Technical Aspects of Kidwell's Presentation

Now let's look at the non-technical aspects of Kidwell's presentation.

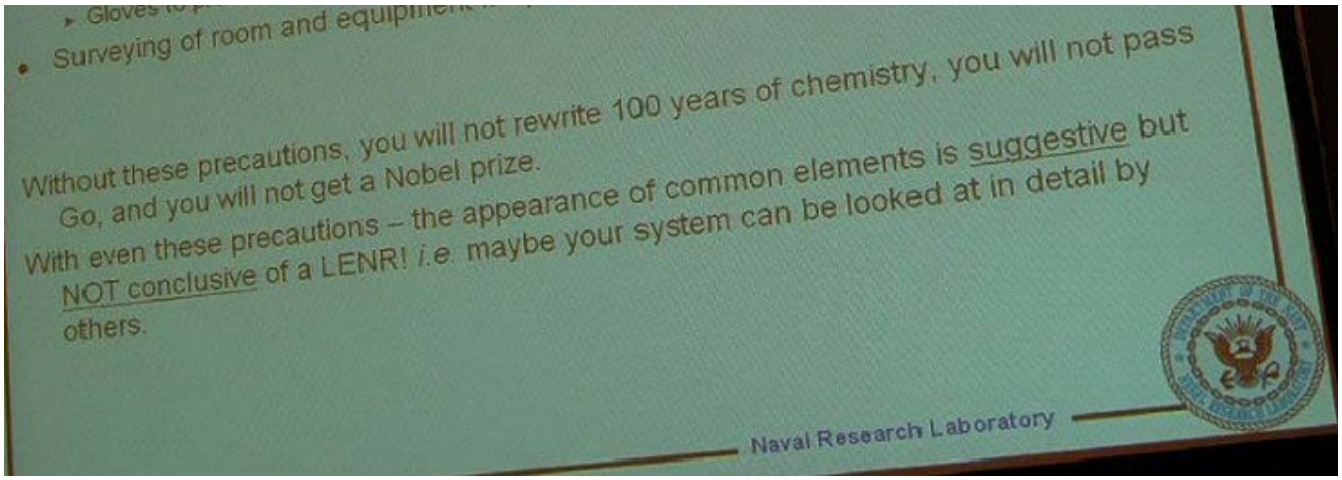
Starting with slide 26, Kidwell explains to the audience how careful and proficient he is when he performs his trace analyses. He appears to be an expert, in precise command of his subject matter. He appears to be well-positioned to provide an authoritative opinion on trace analysis.

Kidwell suggests that LENR transmutation claims in general suffer weaknesses that reduce their credibility.

According to Kidwell, the transmutation claims of LENR researchers in general can be explained by their ignorance and carelessness. He also suggests that they are fooling themselves, and by implication, fooling others.

"If you have what you think you're making all over your room, do you really have it?" Kidwell said. "Or are you just fooling yourself from some random event?

"Without all these precautions, I wouldn't go talk to my colleagues. You're not going to rewrite 100 years of chemistry, you're not going to pass go and you're not going to win a Nobel Prize! With these cautions, you might go talk to your colleagues and say, 'Something unusual occurred.' But without them, I would just kind of be embarrassed."



Kidwell's 2008 Admonishment to LENR Researchers
(Source: Kidwell's ICCF-14 slides)

Readers can watch Kidwell's performance on video.

At the end of Kidwell's talk (21:10 in the full video), LENR researcher John Dash asked Kidwell an interesting question about the weakness of inductively coupled plasma mass spectrometry (ICP-MS), specifically when used for surface analysis of LENR experiments.

"I think it's generally agreed that LENR is a surface effect," Dash said.

Kidwell nodded in agreement.

"ICP-MS is going to dilute whatever surface reaction you have," Dash continued.

Before Dash could ask his question, Kidwell explained that, in his procedure, he performs two analyses, a bulk analysis and a surface analysis, and "actually compares both of them."

A comparison between a bulk analysis and surface analysis won't resolve the problem that Dash was getting to. LENR is primarily a surface effect, and Kidwell washes the surface of his samples before analyzing them with ICP-MS. That won't fix the problem.

Kidwell defends his approach by stating that he does only a "limited etching" and removes only "about one to 10 micrograms per gram of palladium."

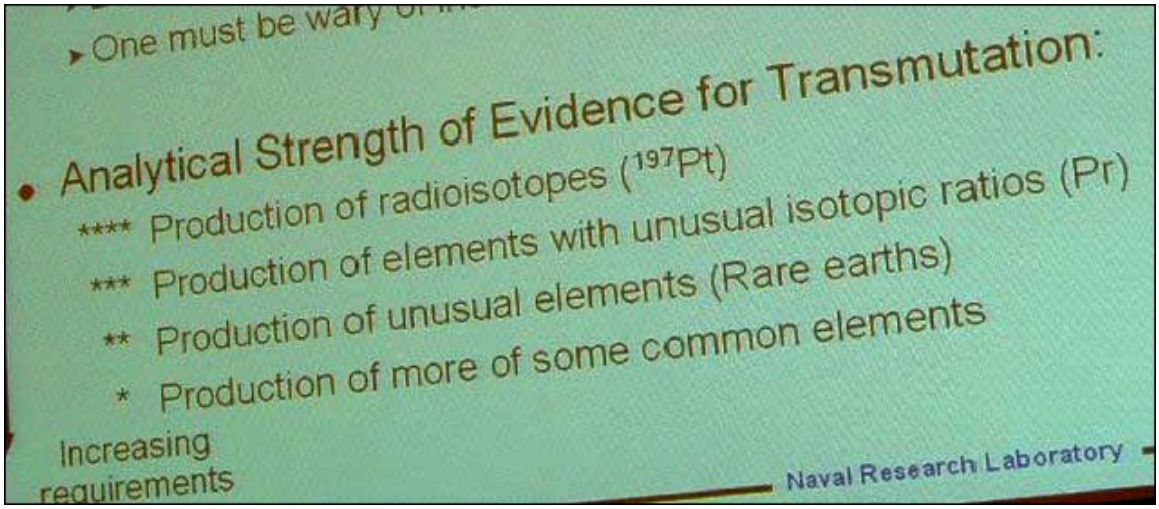
Dash reminds Kidwell of another weakness with his approach. Dash states that "hot spots," craters and ejecta, which are well-known LENR phenomena, are highly localized. Dash tells Kidwell that looking for transmutations on LENR cathodes can be like looking for a needle in a haystack.

Dash says that the process of doing surface analysis for LENR "needs to be looked at very carefully."

Summary of Weaknesses in Kidwell's Explanation of Silver "Contamination"

1. University of Texas NAA confirms anomalous presence of silver.
2. Kidwell independently confirms anomalous presence of silver using alternate analytical method.
3. Kidwell fails to disclose his measurements for the Ag107/Ag109 post-electrolysis ratio.
4. Kidwell does not explain why silver atoms should preferentially segregate and migrate from a homogeneous solid solution of 0.008 percent silver in palladium to the surface of a palladium cathode.
5. Kidwell fails to provide any references for similar tests of silver atoms preferentially segregating and migrating through solid palladium.
6. Kidwell fails to test or cite any literature that supports his speculation that silver atoms can migrate inward, against a high outward pressure.
7. Kidwell fails to test for silver content in the cathode wall.
8. University of Texas NAA shows at least 11 isotopic anomalies in material used in that same experiment.

Using Kidwell's own scale, "Analytical Strength of Evidence for Transmutation," the Arata-Zhang experiment produced very strong evidence for transmutation.



Kidwell's "Analytical Strength of Evidence for Transmutation" Scale
(Source: Kidwell's ICCF-14 slides)

Based on the above facts, LENR researchers seem, in fact, to have rewritten 100 years of chemistry and may be on track for Nobel prizes.

The facts against Kidwell's contamination hypothesis far outnumber the facts in support of his hypothesis.

Which brings us to the next logical question. Who was Kidwell's intended audience? Was it, for example, Bill Wilson, program manager for the Defense Threat Reduction Agency, who was sitting in the ICCF-14 audience? Wilson has been making decisions to federally fund LENR research in the last few years.

Or was the audience the LENR researchers themselves? And why? *New Energy Times* does not have answers to these questions. The only thing we know for sure is that, as Kidwell told us, he was invited to speak at ICCF-14 by Michael Melich, a former NRL laboratory head who is now at the Naval Postgraduate School and seems to have a way of controlling LENR research at NRL. (See "NRL 2009—The LENR Null Results Laboratory, Again" in this report.)

In his 2008 presentation, Kidwell also briefly introduced some speculation about praseodymium, which has been a major component of LENR transmutation claims by Japanese researchers. Audience members would have to wait a year to see the relationship between Kidwell's remark about trace elements "all over your room" and praseodymium.

* 2000 paper says Indium in Table 1. This is an error; it should be Iridium.

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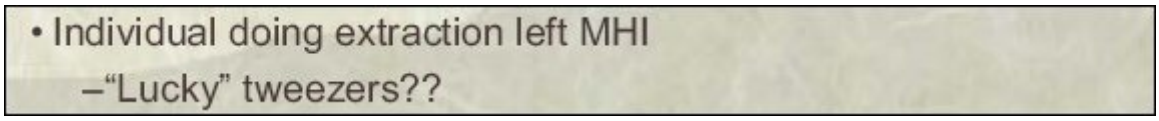
5. NRL 2009—The LENR Null Results Laboratory, Again

Introduction

According to research scientist David Kidwell and his colleagues at the Naval Research Laboratory, the transmutation claims made by LENR researchers at Mitsubishi Heavy Industries in Japan may be the result of an MHI researcher who is no longer employed there who contaminated the experiment with "lucky tweezers."

NRL/Kidwell's Attempt at ICCF-15 to Discredit LENR Transmutation

In his presentation in Rome at ICCF-15, Kidwell attempted to convince the audience that the Mitsubishi/Iwamura observation of a growth of praseodymium was the result of one of two hypothetical scenarios. In the first of Kidwell's scenarios, an ex-Mitsubishi researcher used "'lucky' tweezers" contaminated with praseodymium that fooled the Mitsubishi researchers into thinking they had observed LENR transmutation. Kidwell did not explain how the praseodymium got on the tweezers. Kidwell effectively suggested the possibility of intentional spiking – that is, science fraud.

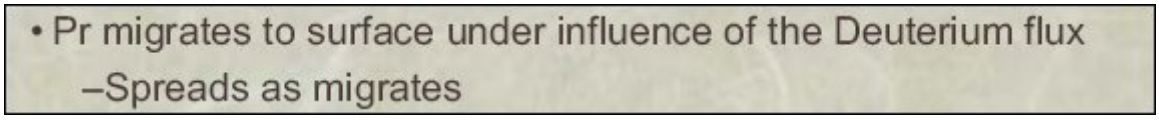


(Source: Kidwell's ICCF-15 Slides)

In his second two-part hypothetical scenario, the praseodymium was first deposited on the multilayered surface while it was in the mass balance and subsequently became buried beneath the fabricated layers. The second part of this scenario is that, during the experiment, the praseodymium preferentially migrated up to the surface.

Kidwell did not explain how, when or why the praseodymium allegedly got into the mass balance in all of the experiments that Mitsubishi ran within the last decade and why such contamination occurred only when they were weighing the experimental samples, not the control samples. Kidwell did, however, find that praseodymium was on the walls of the mass balance when he was at the Mitsubishi lab in Japan on the last day of his 10-day site visit.

According to Kidwell, this second scenario explains why the researchers failed to detect the praseodymium at the beginning of the experiment and were fooled into thinking they had created the praseodymium from LENR transmutation.



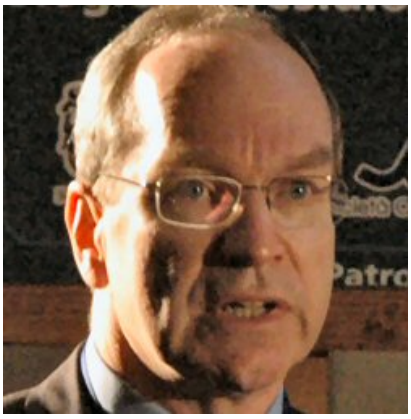
(Source: Kidwell's ICCF-15 Slides)

If Kidwell had any clear evidence for either one of theses scenarios, he wouldn't need to speculate.

Failure to Replicate

Before Kidwell developed his scenarios, an NRL team had attempted to replicate the Mitsubishi LENR transmutation experiment and results. According to another NRL researcher, Kenneth Grabowski, NRL failed to replicate the Mitsubishi results despite—allegedly—precisely replicating the experiment. The multiyear replication program by NRL is rumored to have cost millions. In addition to receiving funding from NRL, the project also received funding from the Defense Advanced Research Projects Agency.

The NRL team was directly managed by Grabowski, controlled by Michael Melich* (a former NRL laboratory head who is now at the Naval Postgraduate School), and funded under the authority of Graham Hubler, head of the Sensor Materials Branch.



Kenneth Grabowski
Photo: S. B. Krivit

Michael Melich
Photo: W. Collis

Graham Hubler
Photo: S. B. Krivit

Other participants were NRL researchers Kidwell, David Knies, Catalina Cetina and Carmine Carosella.

After NRL failed to replicate the results, it collaborated with Mitsubishi to perform replications. NRL again came back empty-handed: null results. It was then – "on a whim," according to Kidwell – that he decided to perform an "environmental survey" in the Mitsubishi laboratory. Kidwell swipes about 25 locations around the lab, comes back to the U.S. and analyzes his swipes. He finds high levels of praseodymium from the swipes he took from inside the mass balance at the Mitsubishi lab.

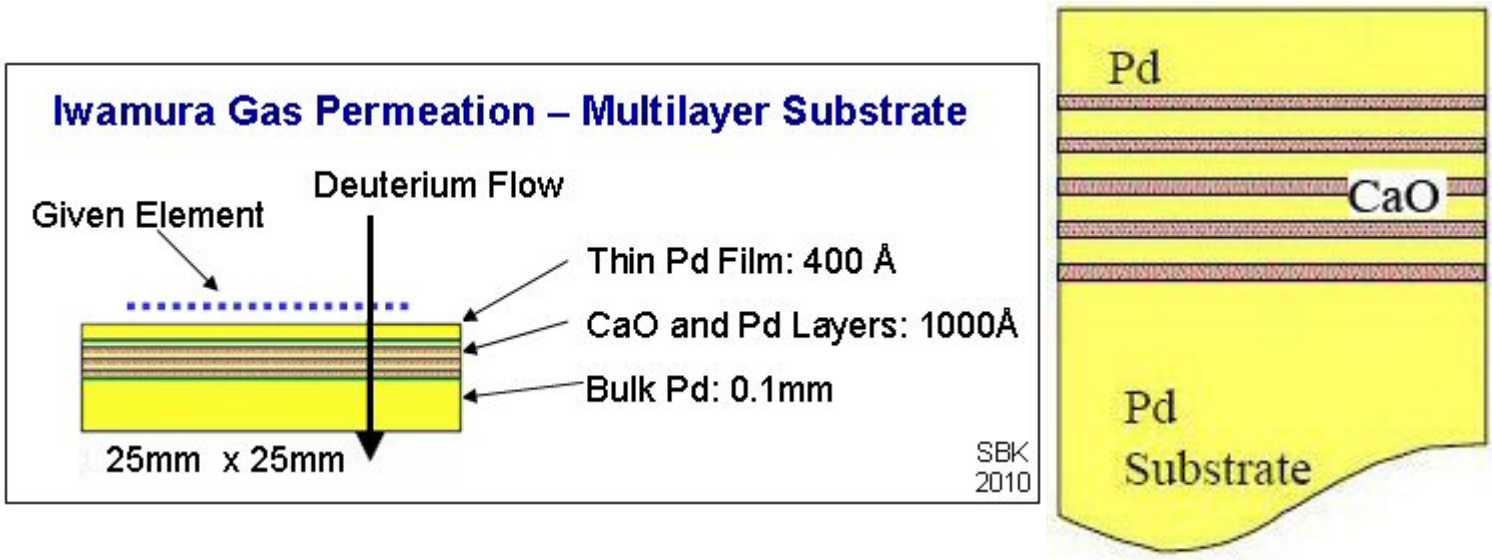
The smoking gun of environmental contamination, right? Wrong.

First, let's go back to school and learn some of the nuts and bolts of the Iwamura experiment.

The Mitsubishi/Iwamura LENR Gas Permeation Experiment

Readers who are familiar with the Iwamura experiment may wish to skip this section and proceed directly to the section "Assessing the Credibility of NRL/Kidwell's Attempt to Discredit the Mitsubishi Work."

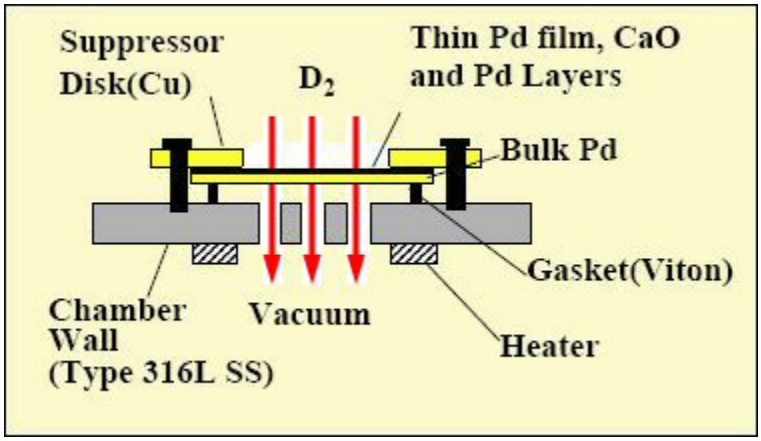
The essential part of the experiment is a multilayered substrate of palladium and calcium oxide. On the surface of the substrate, the Mitsubishi researchers place atoms from a given element. Deuterium gas is passed through the substrate. As the experiment progresses, the given element is seen to decrease in quantity, and an element which was not present before the experiment slowly appears on the surface and increases in quantity.

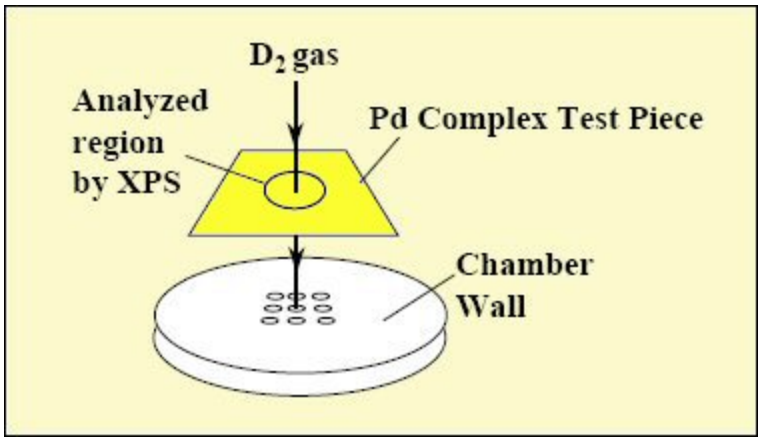


Left: Conceptual view of multilayered substrate.
Right: Detailed view of multilayer fabrication—Five layers each of Pd followed by CaO, finished with final layer of Pd.

Image credit: Mitsubishi Heavy Industries

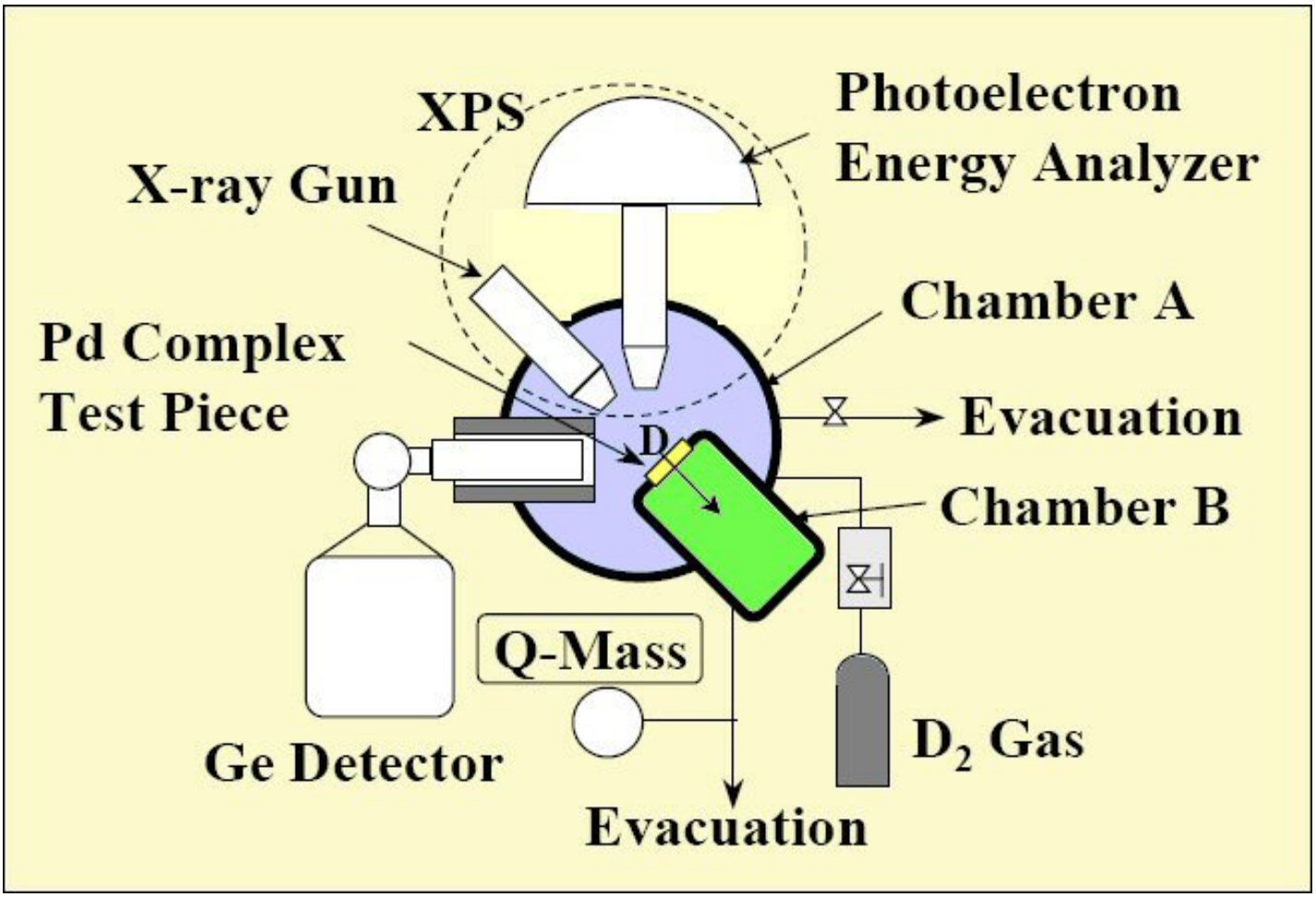
The multilayered substrate is assembled and mounted on top of a set of small holes through which the D2 passes.



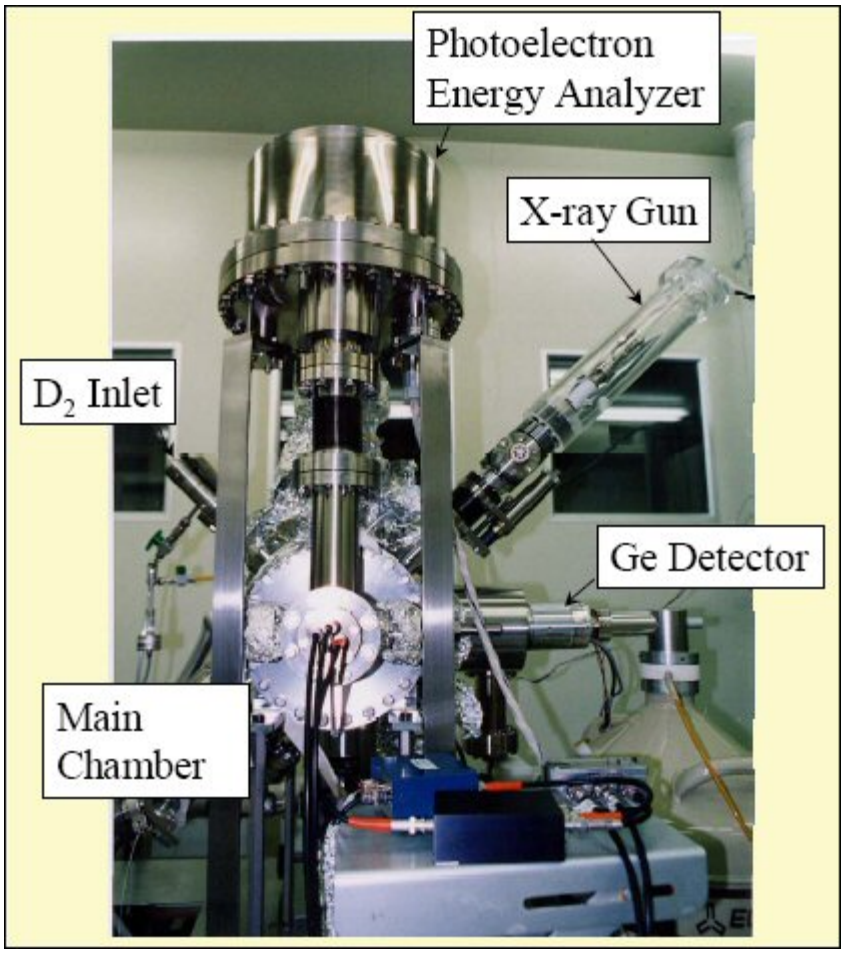


Multilayered Substrate Assembly
Image credits: Mitsubishi Heavy Industries

The multilayered substrate assembly (yellow) is mounted at the intersection of two evacuated chambers: Chamber A (blue) and Chamber B (green).



Deuterium Gas Permeation Apparatus Schematic
Image credit: Mitsubishi Heavy Industries



Deuterium Gas Permeation Apparatus
Photo credit: Mitsubishi Heavy Industries

How It Works

Chambers A and B are evacuated, then chamber A is filled with D2 gas and Chamber B is evacuated to $3x10^{-8}$ Torr with a turbomolecular pump. Over a period of a week or two, depending on the target materials, the Mitsubishi researchers observe a gradual increase in the number of target atoms, for example praseodymium, as well as the simultaneous gradual decrease of given atoms, for example cesium.

The researchers observe atomic changes with X-ray photoelectron spectroscopy (XPS) in real time, while the experiment is in process and sealed. They explain the advantage of this method in their 2002 peer-reviewed journal article.[1]

"Almost all the other researchers in this field are conducting experiments involving the electrolysis of D2O or H2O with metal as a cathode and they usually analyze the transmuted products on the cathode metal by taking it out of the experimental apparatus after electrolysis," the researchers write. "Our experimental method described above is superior in that it discriminates the products from contamination because we analyze the products in vacuum during the experiments without moving the products."

Just before starting the permeation, with the chambers sealed, they test to make sure that no other elements are on the surface of the multilayered substrate except the given

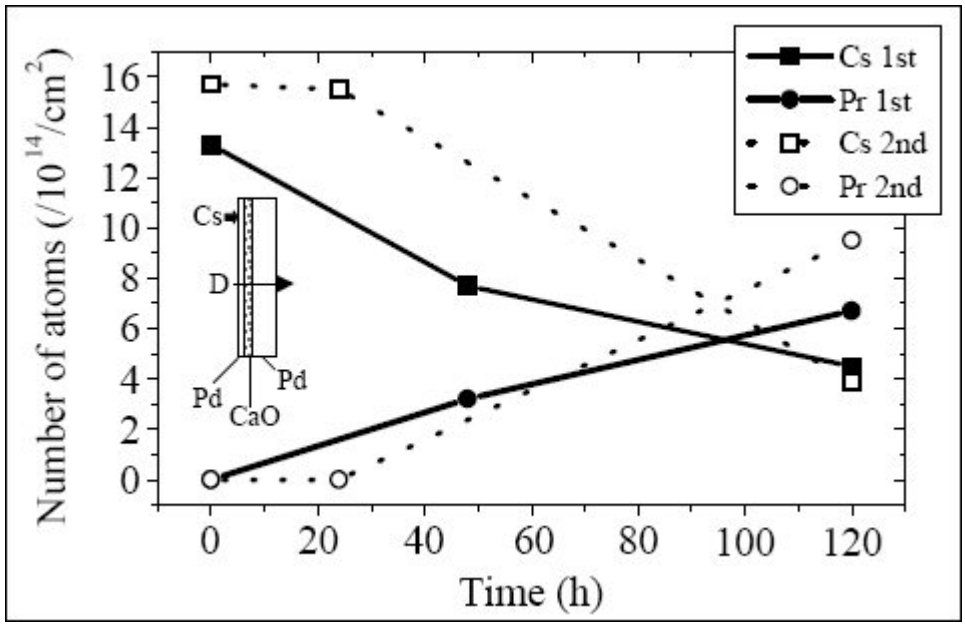
element (for example, cesium or strontium) and the palladium and calcium oxide that is present on the substrate.

In the work with cesium ⇒ praseodymium, for example, no praseodymium is present on the surface of the multilayered substrate at the beginning of the experiment, as shown by the *in-situ* XPS measurements.

Once the experiment begins, the Mitsubishi researchers stop the deuterium gas flow periodically to perform the XPS measurements. They run the deuterium gas permeation from two days to a week, then evacuate the deuterium from chamber A and perform the XPS analysis on the surface of the palladium substrate. They usually do this a few times over the course of the experiment to observe the time dependency of the given as well as newly created elements.

Some of Mitsubishi's Results and Analytical Methods

Below is an example of some of the Mitsubishi results, reported in Mitsubishi's 2002 paper, that shows two pairs of measurements: The first pair was measured at time-zero, at 49 hours and at 120 hours.

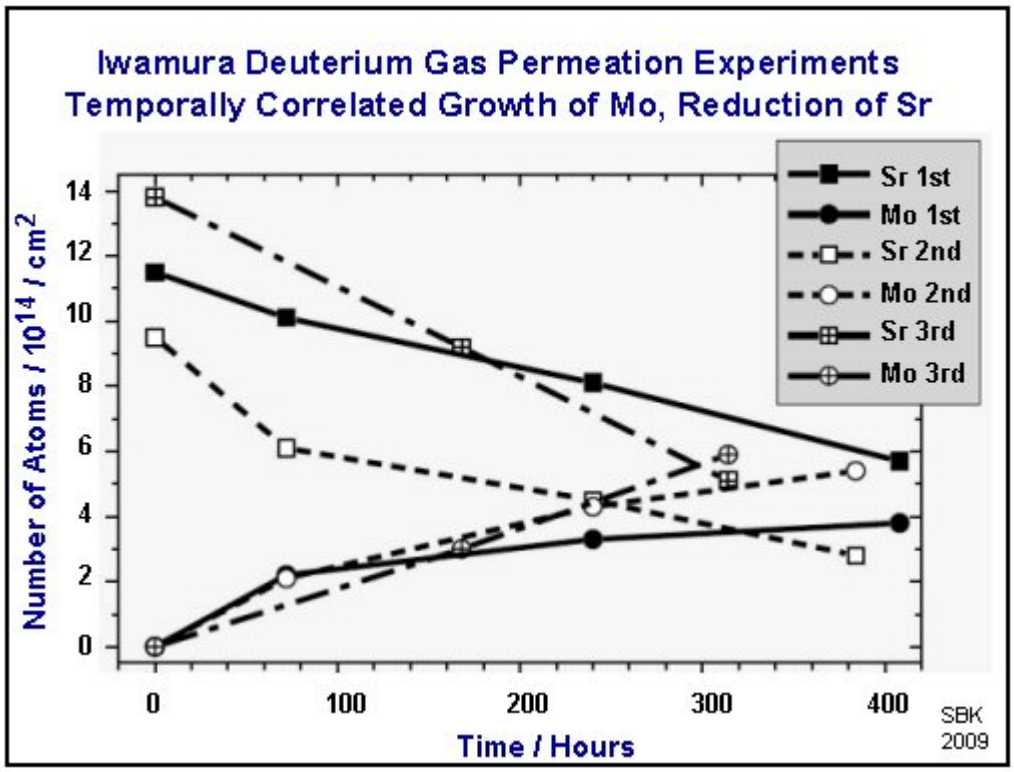


Two experimental runs showing temporally correlated gradual decrease of cesium and increase of praseodymium
Image credit: Mitsubishi Heavy Industries

They have repeated this type of observation many times, with several pairs of elements:

133Cs ⇒ 141Pr (addition of 4 deuterons)
88Sr ⇒ 96Mo (addition of 4 deuterons)
137Ba ⇒ 149Sm (addition of 4 deuterons)
44Ca ⇒ 48Ti (addition of 2 deuterons)

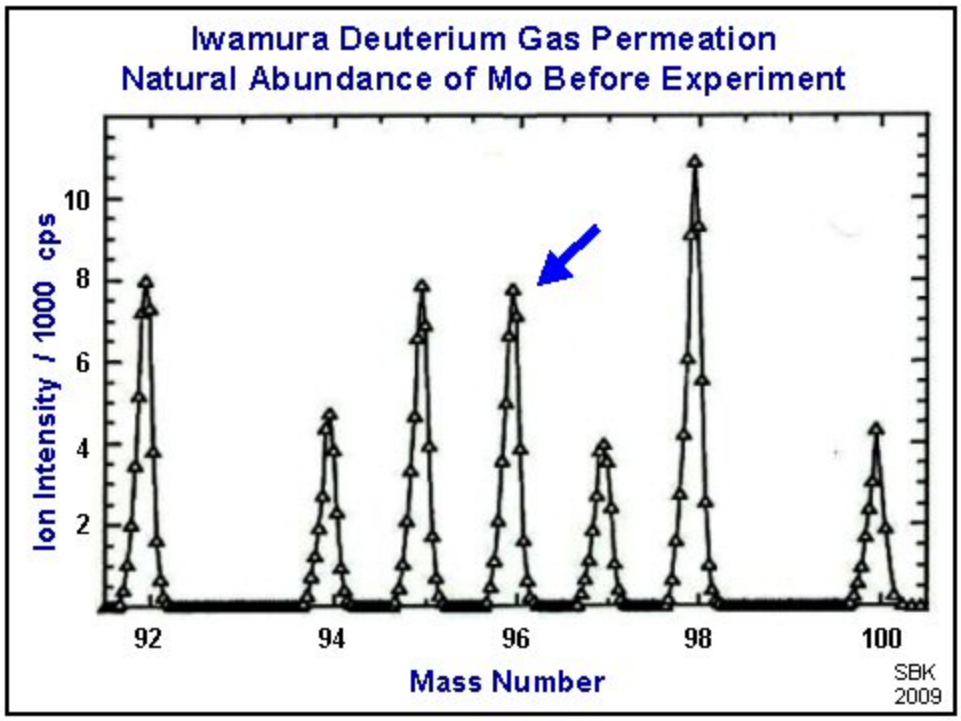
The gradual increase of one element and the temporally correlated gradual decrease of another element are consistent features of their experiment. As shown below, their XPS data create a similar pattern among three sets of experimental runs. Again, the gradual, temporal signature of growth and reduction is crystal clear.

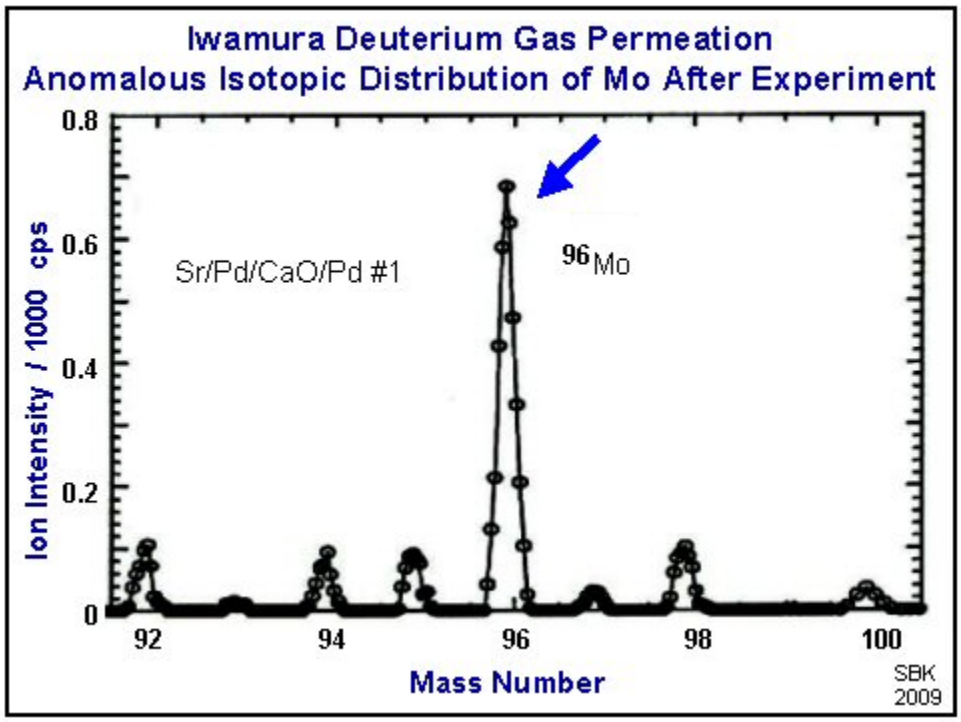


The researchers went further than just the *in-situ* XPS measurements. The stable transmutation products provided permanent evidence, unlike the ephemeral excess-heat from Fleischmann-Pons experiments. Mitsubishi sent its samples out for a secondary analysis to Probion Analysis, Inc., an independent laboratory in France, for secondary ion mass spectrometry (SIMS).

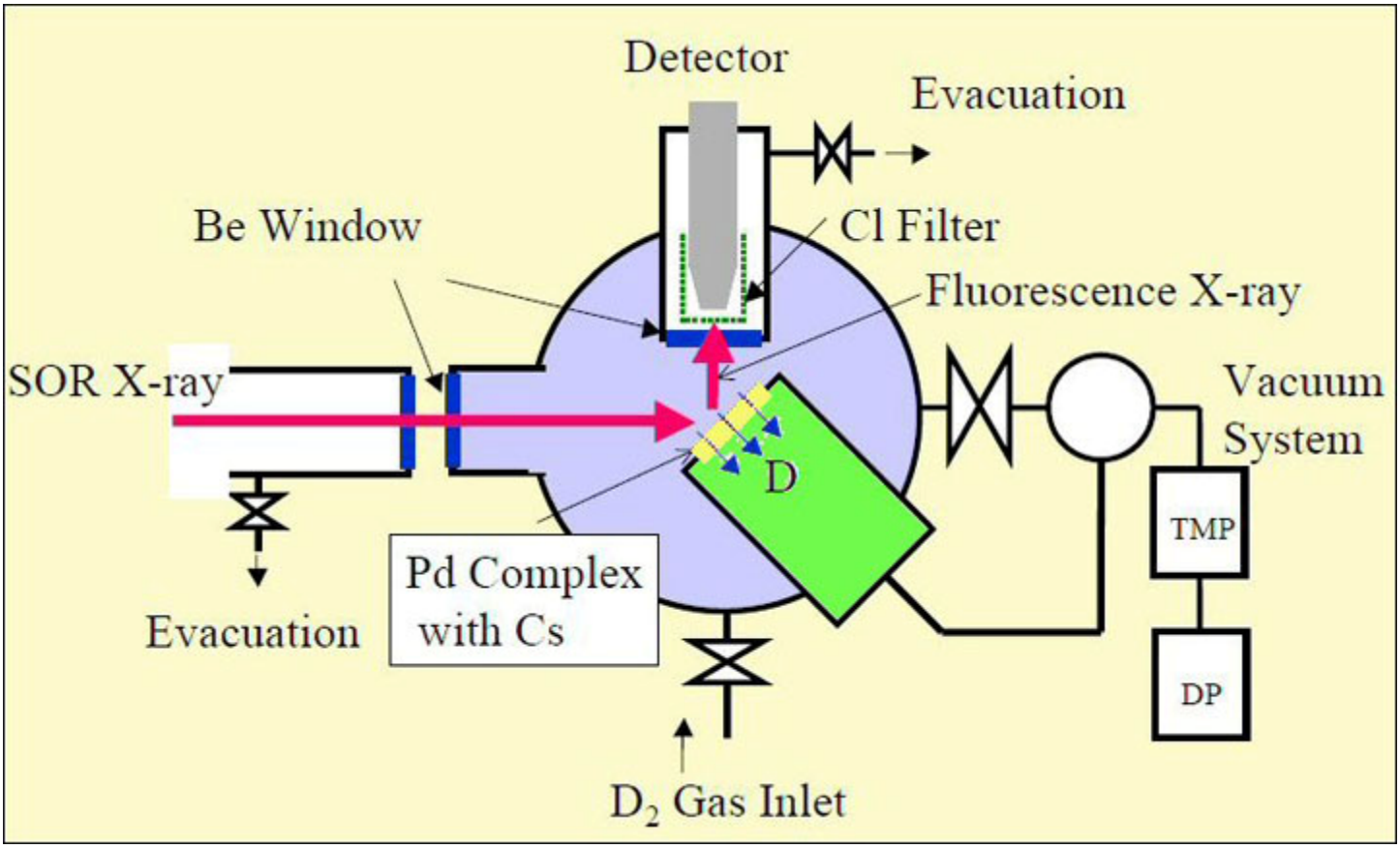
The Mitsubishi researchers explained SIMS: "[SIMS is] a surface analysis technique in which energetic primary ions impact the surface and generate secondary ions, which are subsequently mass-separated and detected. SIMS is capable of analyzing all the elements with isotopic discrimination."

In their set of experiments with strontium as the given element and molybdenum as the target element, they saw very clear results of apparent LENR transmutation: unexpected isotopic abundances. The differences are shown below.

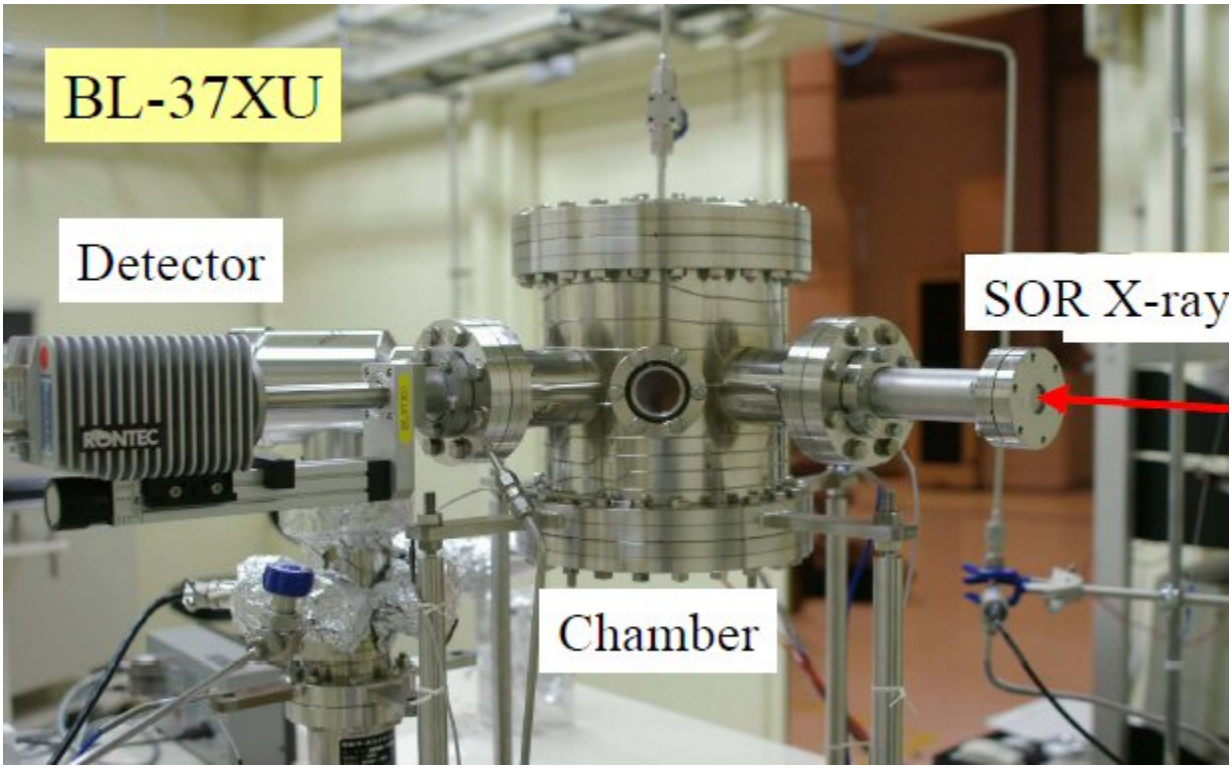




The Mitsubishi researchers went further still. They brought their experimental apparatus to the Spring-8 synchrotron radiation facility in Harima, Japan, and used Synchrotron Orbit Radiation X-Ray (SORX) analysis with yet a third method to confirm the transmutation process, again, *in situ*. Here is the schematic, again showing the multilayered substrate assembly (yellow), chamber A (blue) and chamber B (green).



SORX Apparatus Schematic
Image credit: Mitsubishi Heavy Industries



SORX Apparatus Schematic
Photo credit: Mitsubishi Heavy Industries

Assessing the Credibility of NRL/Kidwell's Attempt to Discredit the Mitsubishi Work

Now it is time to scrutinize Kidwell's hypothetical scenarios in which he attempts to explain away the Mitsubishi LENR transmutation results. Considering the well-controlled and meticulously designed experiment, how, according to Kidwell, is it possible that the experiment got "contaminated"? Let's first look at the "Lucky Tweezers" scenario.

In this scenario, an ex-Mitsubishi researcher used "lucky tweezers" contaminated with praseodymium when extracting the sample after the experiment, and this is what fooled the Mitsubishi researchers into thinking they had observed LENR transmutation.

Aside from the unanswered and potentially volatile question of how praseodymium might have gotten onto the lucky tweezers, this scenario has two major problems. One is that, during the experiment, praseodymium gradually increases over time. The second is that, in a nearly linear relationship, cesium decreases gradually during the course of the experiment.

Now consider Kidwell's second scenario.

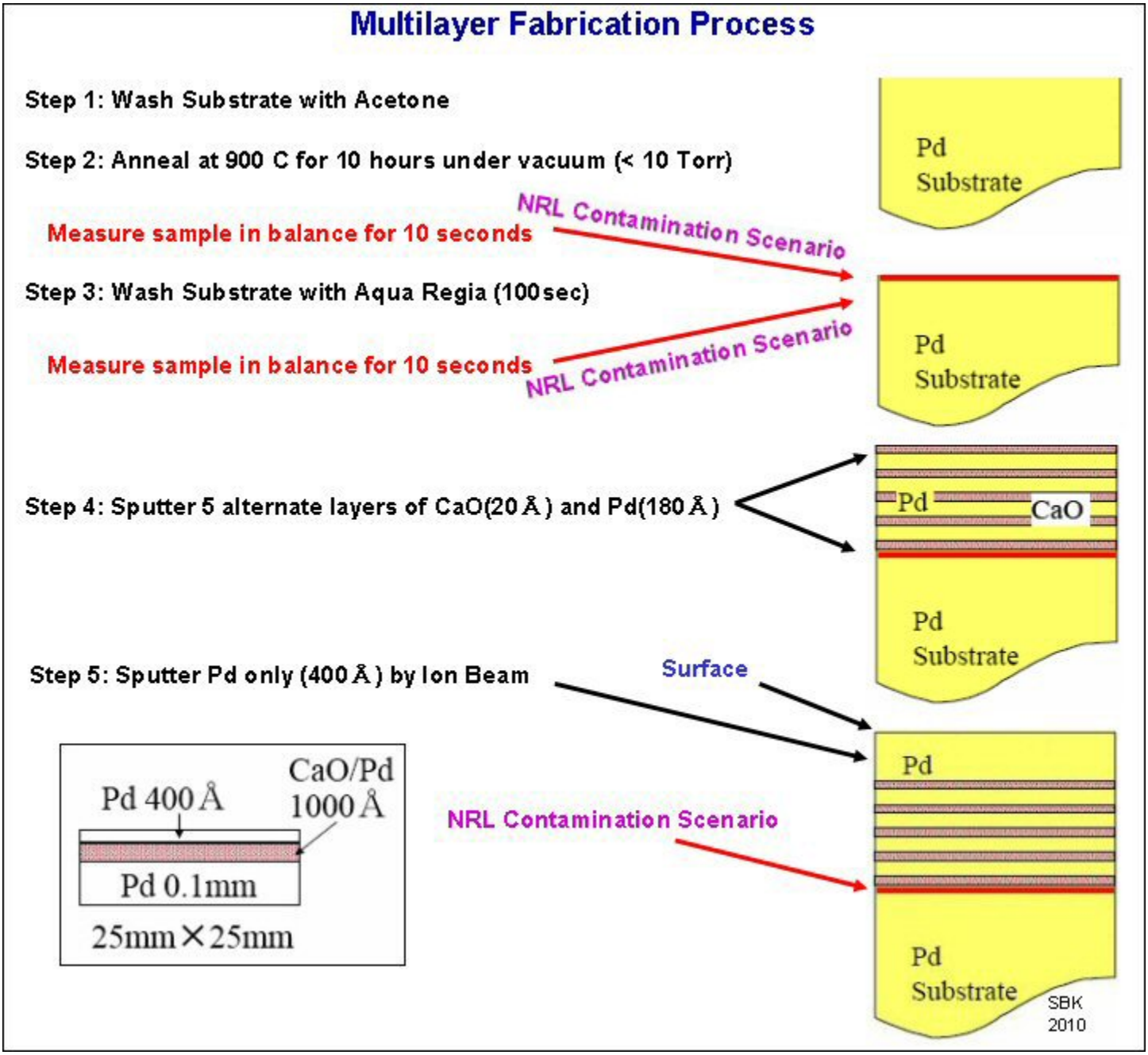
The crucial scene takes place in the balance used to measure the mass of the substrate before it enters the chambers. We're talking about a device that looks like this:

Scientific Mass Balance

This scenario requires that traces of praseodymium were present inside the mass balance at the time of the sample fabrication. It requires that praseodymium fall from the wall of the balance onto the substrate during the build. In Kidwell's words, "contamination from dust in balance contaminates the interior of the multi-layer structure with small particles of praseodymium."

Exactly how does Kidwell explain that praseodymium from the balance contaminated the interior of the multilayered substrate? He doesn't. We have Iwamura to thank for providing a precise explanation of Kidwell's scenario.

The following *New Energy Times* diagram is based on Iwamura's diagram presented at ICCF-15. As Iwamura explained, if the praseodymium contaminated the multilayered substrate in the balance, it would have to fall or be placed on top of the palladium substrate and below the layers of palladium and CaO which are sputtered on in step four. The substrate is put into the balance twice during the fabrication process. The following diagram displays these steps in detail (click image to enlarge).



Once fabricated, the substrate looks like this, as viewed through a field emission transmission electron microscope.

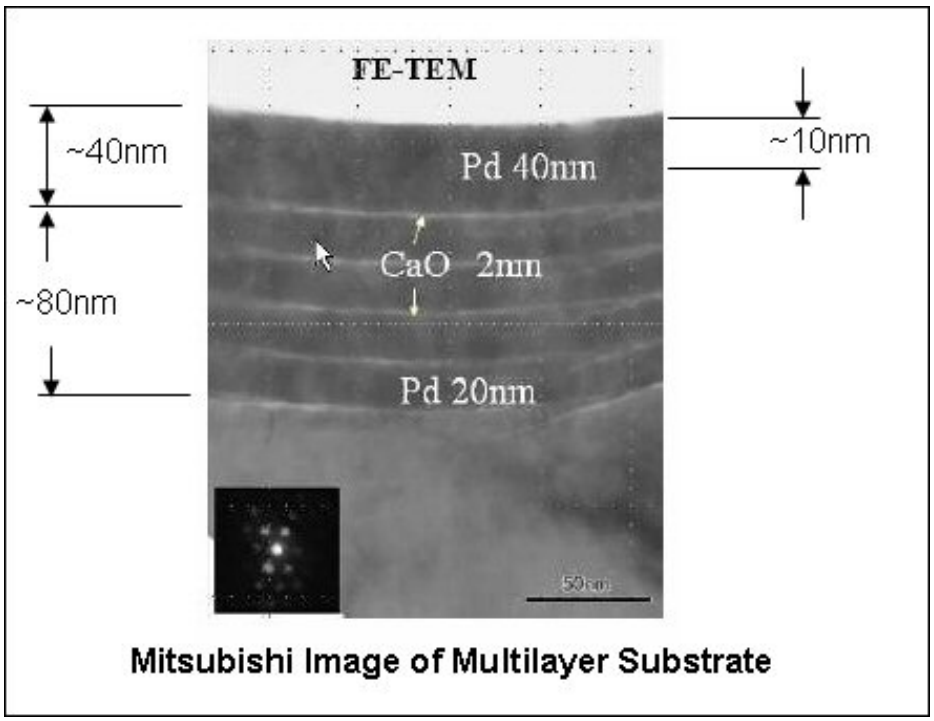
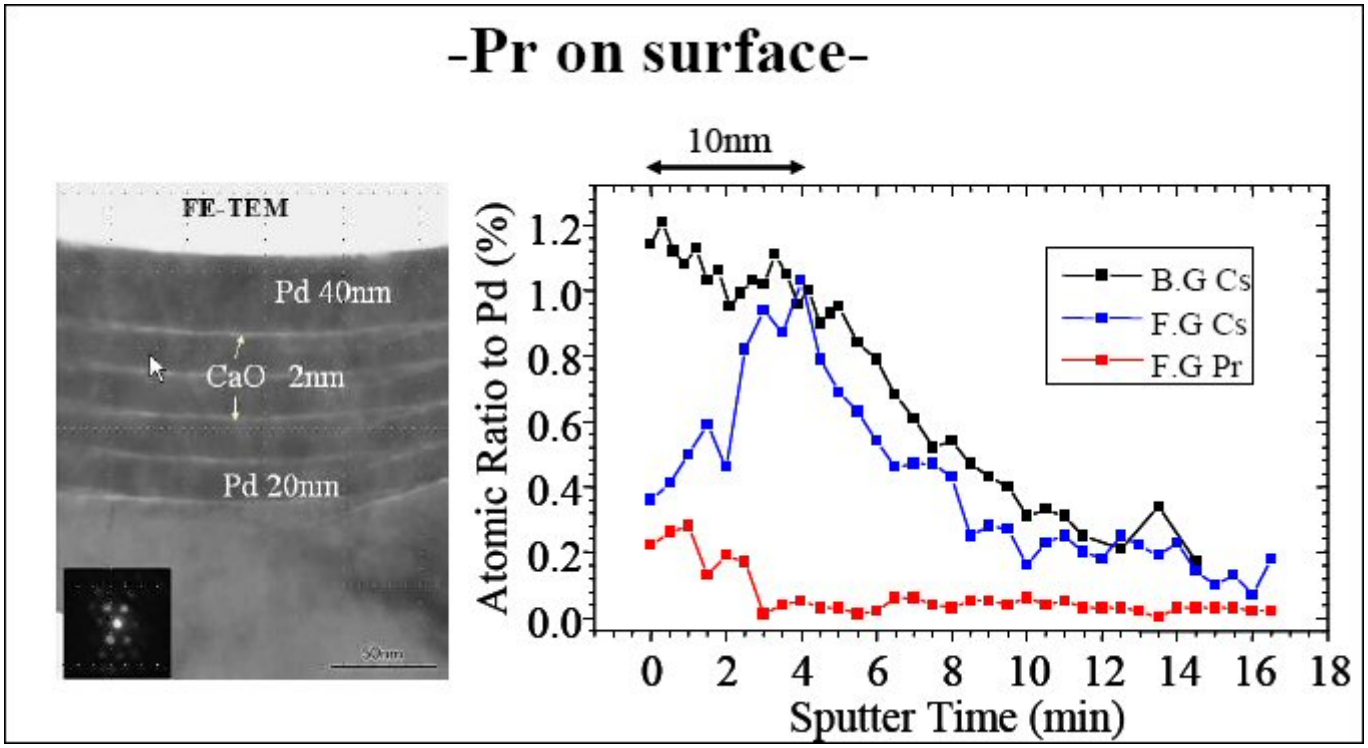


Image of Multilayered Substrate
Photo credit: Mitsubishi Heavy Industries

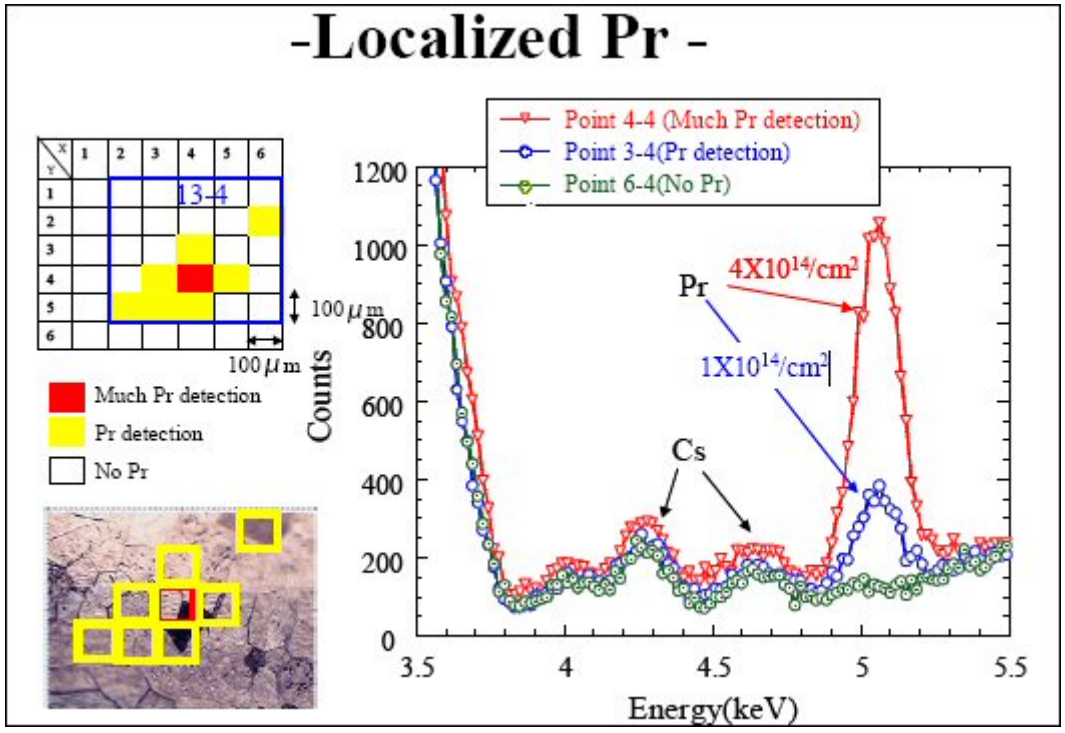
Now, one more major step needs to occur in the Kidwell scenario. The praseodymium must come up through the multiple layers and reach the surface of the multilayered substrate. And the praseodymium must rise from the palladium substrate while the deuterium gas is flowing down into and through the substrate. Similarly, Kidwell had speculated that, in the Arata-Zhang experiment, solid silver preferentially migrated through solid palladium in the opposite direction of the gas pressure (see "NRL 2008-The LENR Null Results Laboratory" in this report).

Assuming that Kidwell can explain how praseodymium can flow in the opposite direction of the deuterium gas, what is the evidence that praseodymium, in fact, exists below the surface? According to Iwamura's tests, praseodymium does not appear in the post-experiment sample below 10nm from the surface. This means that either there is no praseodymium below the surface, or, if there was, it would have had to travel about 100nm up to the surface – all of it.



Depth Profile of Praseodymium on Surface
Image credit: Mitsubishi Heavy Industries

Iwamura also showed that the praseodymium appears in high concentrations in localized spots on the surface. This finding contradicts Kidwell's speculation that "praseodymium migrates to [the] surface under [the] influence of the deuterium flux [and] spreads as [it] migrates."



Topographical Profile of Praseodymium on Surface
Image credit: Mitsubishi Heavy Industries

Iwamura made several other points in his rebuttal to Kidwell (Audio, Slides, Transcript), including the most fundamental for any science experiment, the results of Mitsubishi's control experiments.

"For foreground experiments, we have positive praseodymium," Iwamura said. "But if we have only cesium and no [deuterium gas] permeation, we never get praseodymium. Also, [if we] only use [deuterium gas] permeation without cesium, we have no praseodymium for six cases ... So I can't understand why, if praseodymium originated from the balance contamination, then praseodymium should have been detected every time."

After Kidwell's public presentation in Rome, *New Energy Times* asked Kidwell whether he could explain the temporally correlated decrease of cesium with any of his hypothetical scenarios. He started to respond, but Grabowski interrupted the conversation, instructed Kidwell to stop talking and referred *New Energy Times* to the Navy's office of public affairs.

Summary of Scientific Problems With the NRL/Kidwell Hypothetical Scenarios

1. There are multiple replications of the Iwamura-type experiment at other Japanese and Chinese laboratories.
2. Mitsubishi/Iwamura have demonstrated the effect with a variety of element pairs, not just cesium and praseodymium.
3. NRL/Kidwell's first hypothetical scenario fails to explain the gradual increase of praseodymium.
4. NRL/Kidwell's second hypothetical scenario fails to suggest an explanation for the simultaneous and gradual decrease of cesium.
5. Mitsubishi/Iwamura have shown isotopic shifts.
6. Mitsubishi/Iwamura have used three analytical methods.
7. The "contamination dust" would have to have fallen or be placed on all of the substrates during two 10-second periods for each substrate.
8. The praseodymium "contamination" would have to flow in the opposite direction of the gas flow. Electrochemical cathodic deposition is not even a question; there is no electrolysis in this system.
9. No praseodymium is present in the completed samples below 10nm from the surface.
10. The praseodymium "contamination" flow would have to travel about 100nm up to the surface.
11. All of the praseodymium "contamination" would have to travel to the surface.
12. The praseodymium "contamination" flow would have to rise to the surface and either remain localized through the journey or re-concentrate on the surface into small, localized spots.
13. Mitsubishi/Iwamura's control experiments show what is expected from a control, each and every time.

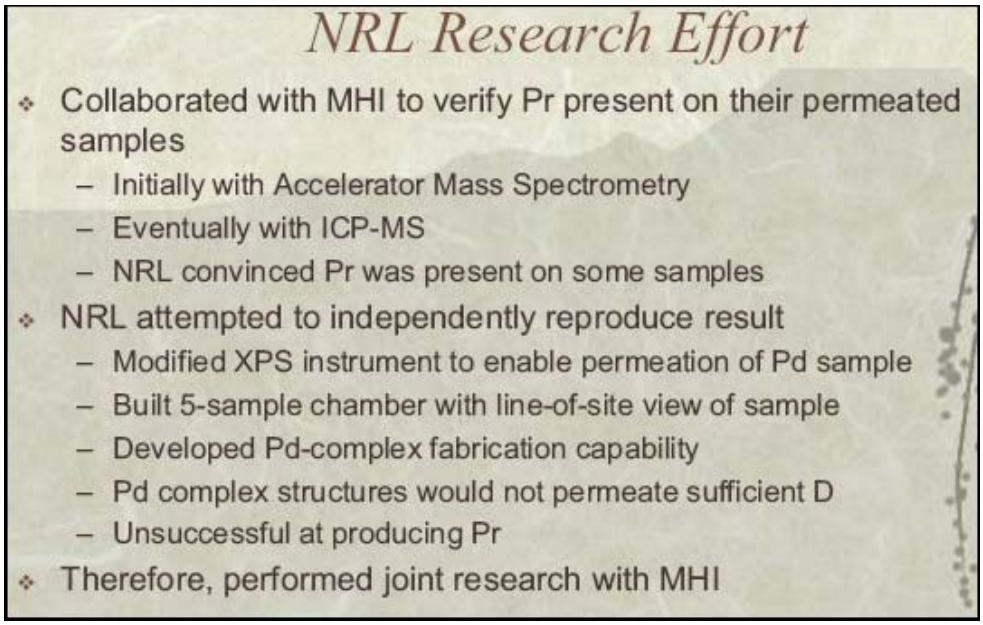
The NRL-Mitsubishi Collaboration

In preparation for this article, *New Energy Times* sent queries to Hubler, Grabowski and Iwamura to confirm details of the collaboration timeline. Only Iwamura responded. Below, we compare Iwamura's timeline with a slide from Grabowski's presentation in October 2008 in Rome showing his reporting of key events in the collaboration.

Timeline According to Iwamura

2002	Grabowski first visits Mitsubishi lab.
2002-2003	NRL attempts to independently replicate Mitsubishi and fails to find praseodymium.
April 2003	Joint research begins. Mitsubishi provides post-experiment samples, NRL confirms praseodymium.
May 2005	Phase 2 of collaboration begins. Grabowski + two visit Mitsubishi, learn and observe successful experiment.
> May 2005	Takasago, Toray and NRL labs confirm successful praseodymium results.
Fall 2005	Kidwell attempts and fails to detect praseodymium from other portion of May 2005 sample.
July 2006	Grabowski, Kidwell + one visit Mitsubishi and observe experiment; null results obtained.
July 19, 2006	Kidwell performs environmental survey.
> July 2006	Grabowski informs Iwamura of "environmental contamination."

Key Events According to Grabowski



(Source: Grabowski's ICCF-15 Slides)

Iwamura explained to *New Energy Times* that origins of the replication project began in 2002 when Grabowski first visited the Mitsubishi lab.

Sometime in 2002 or 2003, according to Iwamura, NRL attempted to replicate the Mitsubishi experiment on its own. After much expended effort and time, as Grabowski claimed, the researchers failed to obtain positive results.

"In April 2003, the collaboration started under an agreement," Iwamura wrote. "We provided our samples, and they detected praseodymium from our sample."

That is correct. NRL confirmed the presence of praseodymium from a Mitsubishi LENR transmutation sample. Grabowski also states this in the slide shown above.

In May 2005, according to Iwamura, the second phase of collaboration began.

"Grabowski and two other NRL members visited us and learned how to conduct the experiment," Iwamura wrote. "NRL members and we performed experiments with them. We obtained praseodymium from permeated samples. Kidwell did not come to Japan at this time."

Iwamura told *New Energy Times* that Mitsubishi sent the samples to two Japanese labs for outside testing; they confirmed the presence of praseodymium. After that, Mitsubishi sent the samples to NRL, and Kidwell also confirmed the presence of praseodymium by inductively coupled plasma mass spectrometry (ICP-MS), as Iwamura was told by Grabowski.

A November 2005 e-mail to *New Energy Times* from Chase Peterson, with whom Melich maintains regular contact, also confirmed the positive result at Mitsubishi.

"Mike Melich confirms the confirmatory work in two labs in this country of the transmutation work that came out of Japan," Peterson wrote.

Sometime in the fall of 2005, according to Iwamura, Kidwell attempted and failed to detect praseodymium from the other portion of the May 2005 sample. The May 2005 sample had been sectioned into multiple parts. Conflict developed.

"Kidwell insisted that it was difficult to think that praseodymium would appear on one part but not from the other part," Iwamura wrote. "I insisted that it was caused by non-uniformity of praseodymium production; it is not unnatural that praseodymium appears on one part but not from another part."

The following year, from July 10 to July 19, 2006, according to Iwamura, Kidwell, Grabowski and one other person from NRL visited the Mitsubishi lab and observed experiments. Grabowski and the other person stayed an extra day.

"To my great surprise," Iwamura wrote, "on the 19th, Kidwell suddenly requested permission from me to perform environmental swipes. Making swipes was not part of our plan, but I permitted him to perform an environmental survey as I have confidence that our lab is clean. Kidwell wiped everywhere and took the swipe papers.

"After he came back to NRL, Kidwell performed ICP-MS analysis, and Grabowski informed us that our balance was contaminated by praseodymium. It was unbelievable, for we have never used praseodymium as a reagent in our clean room!"

Kidwell's slide #14 states that he performed the environmental survey on a "whim." Considering the difficulties NRL had in obtaining positive results, the fact that Kidwell fully expected to find contamination (that's what he said in his talk) and the fact that the main thrust of Kidwell's 2008 ICCF-14 presentation was about the propensity for environmental contamination, Kidwell's suggestion that his environmental survey was not planned is surprising.

After Mitsubishi heard that Kidwell had found praseodymium in the inner wall of its balance, Mitsubishi also performed swipes and sent them out for analysis in Japan. They, too, came back positive. Mitsubishi promptly removed the contaminated balance from its clean room and brought in a new one.

"We investigated when and why our balance was contaminated," Iwamura wrote, "but it is still unclear how our balance got contaminated. Anyway, even if the inner wall of the balance was contaminated by praseodymium, the experimental results cannot be explained by the praseodymium contamination."

New Energy Times had attempted to get this story for several years, but both NRL and MHI kept a tight lid on it until now.

In August 2008, *New Energy Times* spoke with Iwamura and asked whether he could shed light on both NRL's confirmation of the Mitsubishi result and NRL's attempt to perform its own replication.

"They didn't do a precise replication," Iwamura said. "They didn't have suitable equipment."

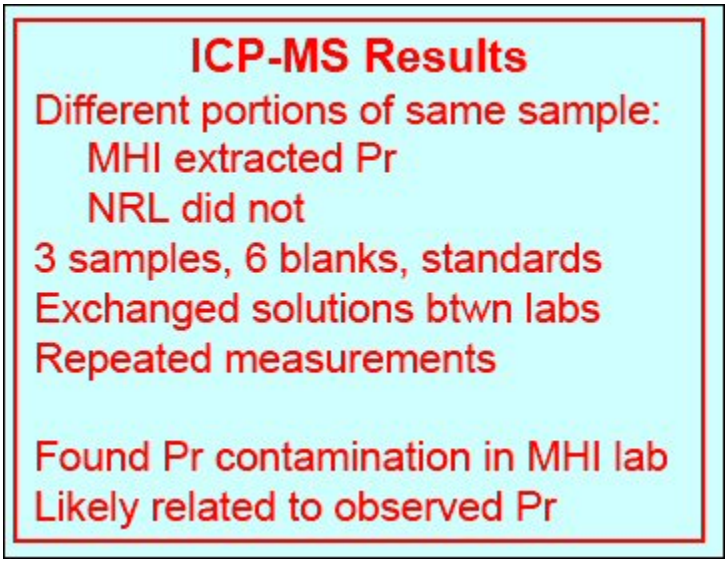
Iwamura declined to provide explicit details at that time.

"They stopped the work," Iwamura said.

It was only at the October 2009 ICCF-15 conference in Rome, Italy, that both sides began to present their sides of the story more fully.

In Rome, NRL was given 22 minutes and an official slot on the program to explain its side. Iwamura was provided five minutes between NRL and the next speaker to provide his rebuttal; Iwamura's rebuttal was not listed on the official program. The session chairman openly encouraged the conference leaders to include Iwamura's slides in the proceedings. They did not immediately respond.

In Grabowski's presentation at the Army Research Laboratory LENR workshop on June 29, 2010, in Adelphi, Maryland, he told the Army that NRL found nothing and that Mitsubishi's results were the result of environmental contamination.



Kidwell "Goes the Extra Mile"

Last but not least in this international science drama are the comments from longtime LENR critic Kirk Shanahan, a research scientist with the defense programs technology section at the Savannah River National Laboratory. Shanahan did not attend the ICCF-15 meeting in Rome. He informed the Wikipedia editors in September 2009 that "contamination [is] the cause of 'new' heavy metals."

"In what may be another excellent effort," Shanahan wrote, "D. Kidwell and coworkers have 'gone the extra mile' and discovered that the claimed production of praseodymium in deuterium flow through palladium membrane experiments may well be due to contamination."

* Three people, Yasuhiro Iwamura, Graham Hubler, and Chase Peterson, former president of the University of Utah, identified Michael Melich as the person in charge of the NRL Mitsubishi replication project.

Reference

1. Iwamura, Y., Sakano, M., and Itoh, T., "Elemental Analysis of Pd Complexes: Effects of D2 Gas Permeation," *Japanese Journal of Applied Physics A*, Vol. 41, p. 4642, (2002)

6. Isotopic Anomalies Reveal LENR Insights

Few people in the LENR field seem to have recognized the three key insights revealed by the anomalous isotopic shifts observed in LENR experiments.

The first insight is that, short of radioactive isotopes, anomalous isotopic shifts provide the most convincing evidence of nuclear reactions in the LENR field.

The second insight is that they suggest the possible levels of energy involved in the reactions.

The third insight is that they suggest the likely or, alternatively, unlikely mechanisms that may be responsible for these reactions.

LENR evolved from an honest and courageous inquiry at the leading edge of scientific exploration. In 1989, Martin Fleischmann and Stanley Pons' best evidence was excess heat. They are world-class electrochemists, and calorimetry was their expertise. With custom-built tools, they made excess-heat measurements that were nearly unparalleled in precision. But excess heat did not carry the day in 1989 when the pair announced their discovery, because calorimetry meant nothing to the nuclear world.

Twenty-one years have passed, many good books have been written and many good research studies have been performed. Calorimetry still means nothing to the nuclear world.

First, some basics to bring all readers up to speed.

Every chemical element has a specific number of protons in its nucleus. The number of protons defines the element's atomic number. The element helium, for example, always has two protons in its nucleus. Its atomic number is two.

A finer gradation further defines most elements. These gradations are dictated by the number of neutrons in their nucleus. Some elements have just a few gradations; others have many gradations. These gradations are called "isotopes." The isotopes are identified by number, based on the total number of protons and the total number of neutrons in the nucleus. "Mass number" is another term that represents the total number of protons and neutrons in the nucleus of an atom. For example, helium has two isotopes. The first helium isotope has two protons and one neutron. This is called helium-3 (also written ^{3}He, He3, or He-3). The second helium isotope has two protons and two neutrons. This is called helium-4. Carbon-12 has six protons and six neutrons.

Two letters are often used as shorthand to refer to an element's atomic number and its isotope. The atomic number is denoted by "Z," and the mass number is denoted by "A."

Palladium has six stable (that is, not radioactive) isotopes in nature. The key point is that, as a natural element, palladium always occurs with six stable isotopes, no matter where on earth it is found. Each of these isotopes always occurs as a specific percentage, an abundance, of the total amount of palladium in a given sample. Thus, the six stable

palladium isotopes and proportions are ^{102}Pd (1.02%), ^{104}Pd (11.14%), ^{105}Pd (22.33%), ^{106}Pd (27.33%), ^{108}Pd (26.46%), and ^{110}Pd (11.72%).

These isotopic ratios are effectively benchmarks. Isotopes from samples can be identified qualitatively and measured quantitatively and compared with the natural abundance. For example, the natural ratio of Pd108/Pd110 is 2.26. If researchers find that this ratio in a test sample is significantly different from the benchmark, they know something anomalous has occurred.

The strength of this benchmark is that, in general, the only known cause for anomalous isotopic ratios is nuclear reactions. Ordinary chemical reactions cannot change isotopic ratios.

With the exception of nuclear reactions, there are virtually no conventional explanations for anomalous isotopic ratios.

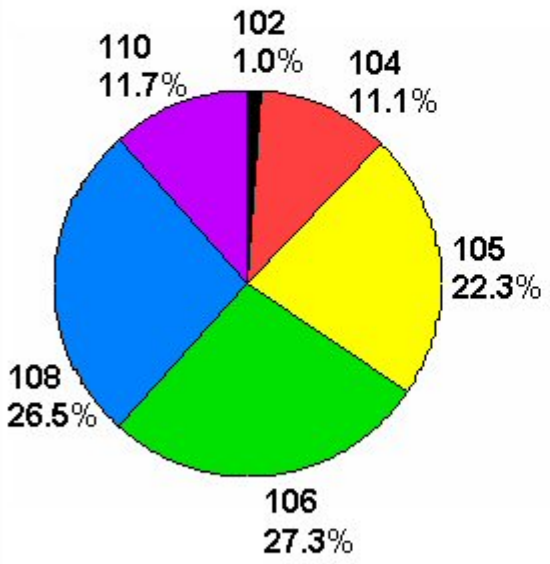
In this article, we will examine several LENR experiments that reported anomalous isotopic distributions. Each of the experiments has several features in common, but some of them, based on the original research, provide specific vantage points from which to observe the key insights of nuclear evidence, energy production, and possible mechanisms.

Evidence of LENR Nuclear Transmutation

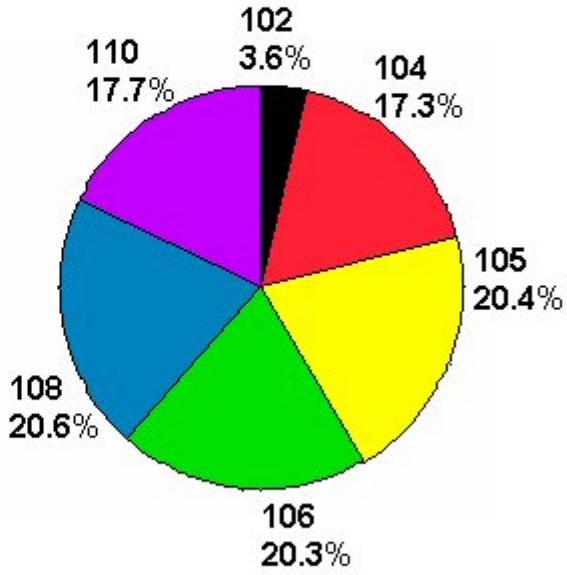
For example, the first image below graphically represents the changes to the palladium isotopic ratios that took place as the result of a heavy-water LENR electrolysis experiment performed by researcher Tadahiko Mizuno in 1991.[1] A variety of significant changes is evident. The second image below, from the same experiment, also shows a significant anomalous shift in the isotopes of chromium.

Pd Isotopic Profiles of Surface by SIMS of Excess Heat-Producing Pd Rod in LiOD (1991)

Before Electrolysis



After Electrolysis



Mizuno, Tadahiko, "Isotopic Changes of Elements Caused by Various Conditions of Electrolysis," American Chemical Society, March 2009

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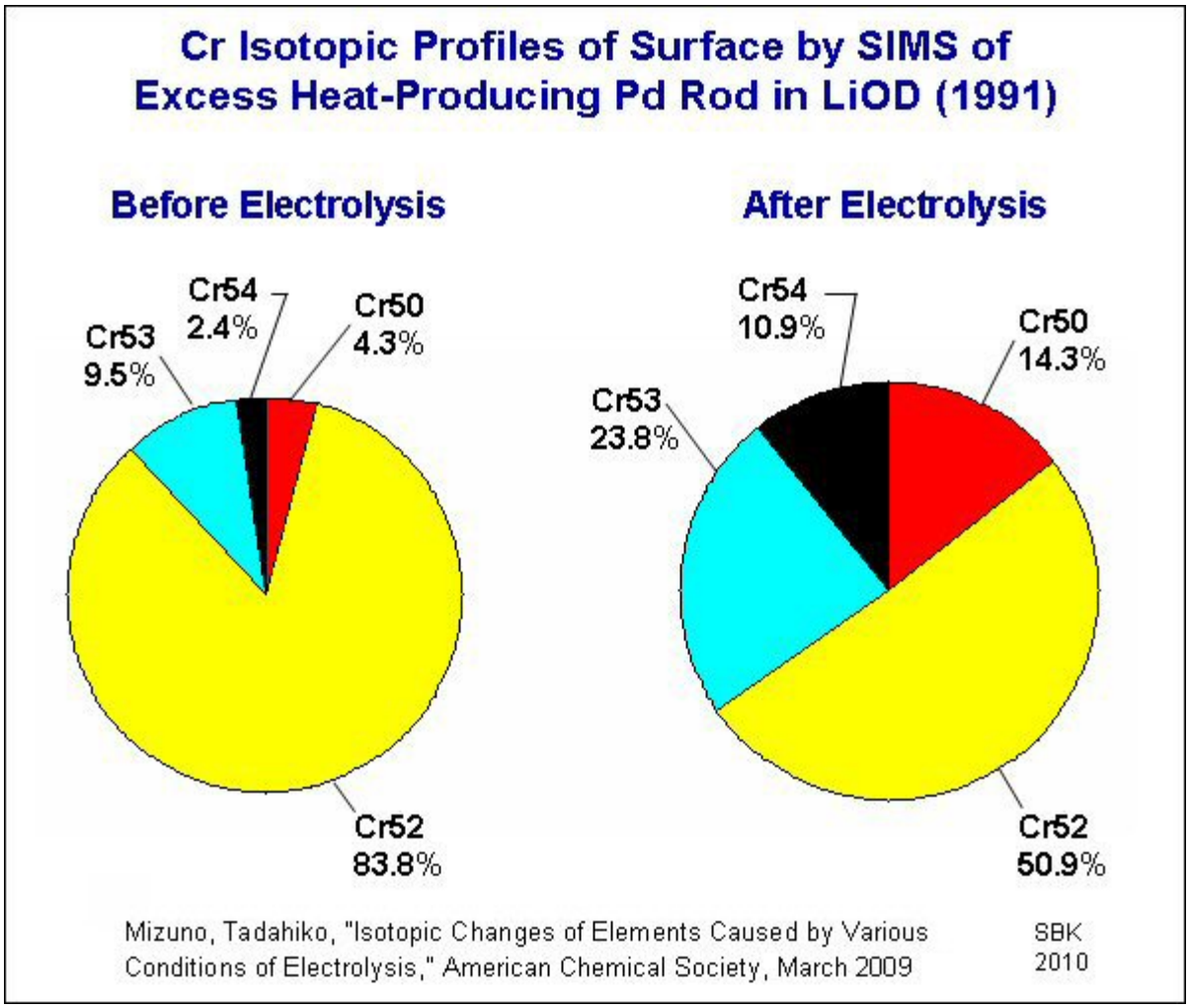


The transmutations of palladium and chromium are only a few of the changes Mizuno observed in this 1991 experiment. In addition to using SIMS (secondary ion mass spectrometry), Mizuno used AES (auger electron spectroscopy), EPMA (electron probe micro-analyzer), EDX (energy-dispersive X-ray spectroscopy) and GDS (Glow Discharge spectrometry).

"Many elements were found and detected on the palladium surface and confirmed using several different analytical methods," Mizuno wrote. "These are apparently reaction products: several elements ranging from hydrogen to lead with mass numbers up to 208.

"The isotopic abundance of selected elements detected after long-term electrolysis was found to be drastically different from the natural isotopic abundance. This phenomenon was confirmed eight times with good reproducibility. All sources of contamination have been carefully eliminated by repeated pretreatments of the sample and the electrolysis system."[1]

The transmutations observed in this experiment came from a Fleischmann-Pons-type heavy-water experiment, not a light-water experiment. This crucial point will be discussed later in this article.

Energy Production From LENR Nuclear Transmutation

In November 1999, the Electric Power Research Institute published a significant "technical progress" report that has been hidden for 11 years. At some point when EPRI developed its Web portal, it began to place many documents online. In September 2009, this writer surveyed the available LENR papers on the site, but for TP-108743, EPRI had released only the abstract. The abstract itself was intriguing.

Because EPRI had apparently placed all its other LENR papers online for free public access, *New Energy Times* asked EPRI to waive the significant but standard nonmember fee of $2,500 for this report. After several requests in the fall of 2009, EPRI declined.

Later, in January 2010, after this writer had finished a meeting at SRI International regarding the SRI "M4" experiment, he drove to EPRI's headquarters, just 2 miles away in Palo Alto, Calif., to try again to gain complimentary access to the report. Several days after a friendly meeting with an EPRI representative, EPRI notified *New Energy Times* that the report was online. Oddly, EPRI included a cover page stating that the report had been made public on Oct. 22, 2009.

One of the project managers who wrote the report is Thomas Passell, a former EPRI program manager who helped to fund millions of dollars in LENR research in the 1990s. A significant portion of those funds went to projects directed by electrochemist Michael McKubre, the director of the former SRI Energy Research Center, a program which continued until the late 1990s. Passell is an expert in nuclear chemistry; he studied under Dr. Glenn T. Seaborg at the University of California, Berkeley.

As a nuclear chemist, Passell naturally appreciated the significance of isotopic abundances and their possible shifts. In the same way that Passell co-managed many of his LENR projects, Passell co-managed this one with EPRI project manager Albert Machiels. Passell and Machiels used what may be the most rigorous detection tool for both quantitative and qualitative elemental as well as isotopic analysis: neutron activation analysis (NAA).

Samples of a material are placed in a nuclear reactor for a brief time and "activated" with neutrons. After the samples are removed from the reactor, the emitted gamma rays are precisely measured and used to characterize the material. It is a standard and well-trusted device in nuclear physics research.

Passell and Machiels collaborated with principal investigators Ben Bush and Joseph Lagowski in the Chemistry Department at the University of Texas, Austin. They, in turn, collaborated with other researchers in the Nuclear Engineering Department at the University of Texas.

There is at least one more significant player in this picture: Stanley Pons. Martin Fleischmann may be in this story, as well, but that fact is unconfirmed.

The samples – sections of palladium cathode rods – that were tested in the University of Texas reactor came from an experiment conducted by Pons in the laboratories of the Toyota-sponsored Institut Minoru de Recherche Avancée in Nice, France. Exactly when Pons conducted this experiment is unknown. The experiment may have taken place in the mid-

1990s, because Passell cites similar work by Pons, T. Roulette and J. Roulette published in October 1996.[2]

Lots of Transmutation Anomalies

Like the Mizuno experiment, the University of Texas analysis shows a wide variety of transmutations. The researchers reported 4 times the amount of cobalt (Co), 5.4 times the amount of chromium (Cr), 2 times the amount of cesium (Cs), 1.3 times the amount of europium (Eu), 56 times the amount of iron (Fe) and 11 times the amount of zinc (Zn) that is found in the virgin material.

Table 2-1 Trace Elements in Electrolyzed and Virgin Pd

ELEMENT		ELECTROLYZED Pd	VIRGIN Pd	RATIO
Cerium	(Ce)	<5 ppm	<5 ppm	NA
Cobalt	(Co)	2 ppm	<0.5 ppm	>4
Chromium	(Cr)	27 ppm	< 5 ppm	>5.4
Cesium	(Cs)	14 ppm	<7 ppm	>2
Europium	(Eu)	0.04 ppm	0.03 ppm	1.3
Iron	(Fe)	13870 ppm	247 ppm	56
Hafnium	(Hf)	<0.5 ppm	<0.4 ppm	NA
Rubidium	(Rb)	<20 ppm	<20 ppm	NA
Selenium	(Se)	< 3 ppm	<3 ppm	NA
Zinc	(Zn)	60 ppm	5 ppm	12

Electrolytic experiments, as opposed to gas experiments, are often but not necessarily easily critiqued for the possibility that rogue elements from the electrolytic solution may deposit on the cathode. However, some of these elements reported here represent rather large concentrations, and thus the credibility of such critiques wears thin.

However, the nearly indisputable smoking gun is the anomalous isotopic ratio of palladium-108 to palladium-110. The EPRI report said, "Pd-108 was depleted in the active sample relative to the virgin material by an apparent 28% with the one sigma error limits extending from 7% to 49%." *New Energy Times* knows of no conventional explanation for this shift.

Lots of Heat

According to the researchers, the Pons experiment produced lots of heat. The researchers who prepared the EPRI report did not know what type of nuclear process to attribute the reactions to. They speculated, based on their understanding of nuclear chemistry, "that the excess power episodes observed with the cathode integrated over the time of the episodes must have totaled 160 kilojoules." They also had information from Pons about similar experiments for comparison.

"Pons of IMRA volunteered a cathode that had experienced such episodes of excess heat well above the required levels of several hundred kilojoules," the researchers wrote. "It was this cathode and its virgin counterpart that were analyzed in this study."

The researchers were not given the excess-heat data from Pons, but they back-calculated the minimum amount of energy release based on the facts they obtained from the NAA along with their knowledge of nuclear binding energy. They based their interpolation on the most conservative estimate of depleted Pd-110 atoms (7%), and from this they extrapolated an amount of energy in the same ballpark as that which Pons had reported by calorimetry.

Here is how Passell and Machiels explained their calculations:

> If we take the 7% number as the value, this implies a loss of 2.3E18 atoms of Pd-108. At 10 MeV per atom lost, this amounts to 3.6 megajoules for the sample, and extrapolating to the total cathode assuming homogeneity gives 163 megajoules of excess heat. Of course, total homogeneity is not likely in the electrochemical cell. The total excess heat generated by this cathode has not been made available to us as yet. To get 163 megajoules of excess heat would require an episode with an excess power of 10 watts for 4,527 hours, or about 0.5 years. The conclusion we must draw is that homogeneity is unlikely for excess-heat episodes or that our measurement of Pd-108 depletion is in error. However, it should be noted that Roulette, Roulette, and Pons[2] report one cell giving a total net excess heat of 294 megajoules and another yielding 102 megajoules.

Insights Into Possible Nuclear Mechanisms

Nowhere in the body of the text of TP-108743 does the word "fusion" appear. The researchers did not even consider "cold fusion" as a possible explanation. As they explained, the closest they could come to any sort of explanation was fission:

> The transition elements Cr, Co, Fe, and Zn are possible fission products of Pd, for which the energy available from the mass change of about 20 MeV per fission or 10 MeV per fission product atom assuming binary fission. This is the only nuclear process we have imagined capable of producing these elements from elements in the cell. The Cs increase observed may be from the 0.1 Molar LiOD electrolyte, it being an alkali element similar to lithium.

At the time, the researchers almost certainly had no idea of the possibility of real neutrons being generated in the LENR cell, and through weak interaction processes, neutrons might have been created and then captured by nearby nuclei. The depletion of Pd-108 compared to Pd-110 can be explained simply by the capture of two neutrons.

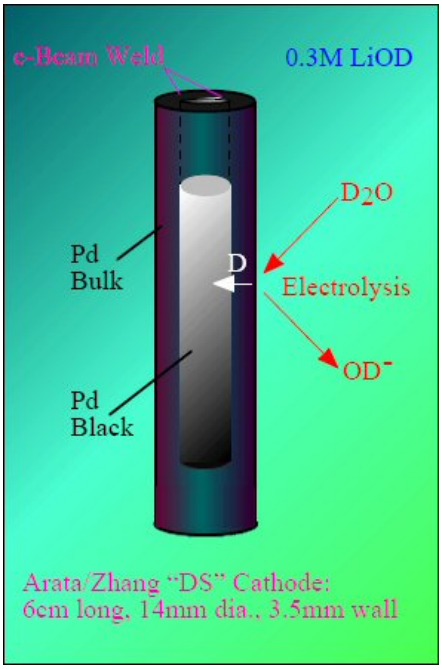
As with the Mizuno experiment, these samples came from a Fleischmann-Pons heavy-water experiment, not a light-water experiment.

Gas Experiments Overcome Electrolytic Impurity Concerns

Passell was a man on a mission; his search was for rigorous nuclear evidence for LENR, and he found it. He knew that some people wondered whether impurities from electrolytic solutions were partly responsible for the reported LENR transmutations (but not the isotopic shifts).

He realized that the double-structure Arata-Zhang electrolysis experiment provided a unique opportunity over conventional electrolytic LENR cells that would make it difficult for honest skeptics to dismiss the findings. He explained this in two papers:

> Focusing on material exposed only to gaseous deuterium has the advantage that conventional electrochemical cathodic deposition from electrolyte impurities or extracts from anodes can be eliminated as a possible cause of observed changes.[4]
>
> All these samples were protected from the electrolyte inside the gas-pressure-tight hollow core of the cylindrical cathode.[3]



Arata-Zhang Double-Structure Cathode[4]
Image Credit: SRI International

Again, Passell used the services of the University of Texas, Austin, and performed NAA on the palladium-black powder from inside the core of the cathode.

The most striking thing he found was 6.6 to 14.4 times the zinc-64 isotope over the virgin palladium.

Passell was given three samples (A, B and C) from post-electrolysis experiments and one virgin sample (D).

Neutron Activation Analysis Results for Impurities in Palladium Exposed to Gaseous Deuterium

SAMPLE	Zn-64 Content PPM by Weight	Zn-64 Ratio Relative * To Unused (Virgin) Palladium Sample D (One Sigma)	Iridium * Content PPM by Weight	Gold Content PPM by Weight	Pd-110/Pd-102 Relative to the Ratio Observed in Unused Pd i.e., Sample D (One Sigma)
ARATA A	50	6.6 (1.6)	4.1	55	1.24 (0.11)
ARATA B	121	14.4 (3.2)	0.2	11	1.06 (0.06)
ARATA C	58	8.3 (2.1)	3.1	17	1.21 (0.11)
ARATA D (VIRGIN)	8	1.0 (DNA)	0.5	10	1.00 (DNA)

* Typographical errors fixed

The table above, from Passell and Russ George's 2000 paper,[3] shows the following anomalies, in addition to the increase of zinc-64 over virgin palladium:

- 7-15 times the zinc-64 by weight
- 6.6-14.4 times the zinc-64 isotope over the virgin palladium
- 8 times the iridium content by weight from sample A
- 0.4 times (decrease) the iridium content by weight from sample B
- 6 times the iridium content by weight from sample C
- 5.5 times the gold content by weight from sample A
- 0.1 times the gold content by weight from sample B
- 0.7 times the gold content by weight from sample C
- 24% increase in Pd-110/Pd-102 ratio over virgin palladium from sample A
- 6% increase in Pd-110/Pd-102 ratio over virgin palladium from sample B
- 21% increase in Pd-110/Pd-102 ratio over virgin palladium from sample C

The fact that, for whatever reason, with the exception of zinc growth, sample B did not show the same performance characteristics as samples A and C with iridium, gold and Pd110/Pd-102 is striking.

(Note: The unretouched "Table 1" from the PDF has two typographical errors. The header for column three was originally Za-64; this should be Zn-64. The header for column four was originally Indium; this should be Iridium.)

Several years later, Passell had arranged for further NAA studies on these same samples analyzing the isotopic ratio anomalies more extensively.[5]

Summary of Results

Sample Name	Pd110/Pd108 Atomic Ratio (One Sigma)	Ag109/Pd110 Atomic Ratio (X 10^6)	Co59/Pd110 Atomic Ratio (X 10^6)	Zn64/Pd110 Atomic Ratio (X10^6)	Au197/Pd110 Atomic Ratio (X10^6)	Ir191/Pd110 Atomic Ratio (X10^6)	Li7/Li6 Atomic Ratio (One Sigma)
Pd-D (Virgin)	Exactly 1 By Definition	53	227.5	83	19.4	2.26	13.6(1.0) Nat. Li=12.5
Pd-A (Active)	1.037(.008)	663 A/D=12.5	286 A/D=1.26	516 A/D=6.22	89.4 A/D=4.61	17.8 A/D=7.88	14.5(0.3) A/D=1.07(.08)
Pd-B (Active)	1.089(.008)	152 B/D=2.87	377.5 B/D=!.66	1259 B/D=15.2	20.6 B/D=1.06	0.84 B/D=0.37	22(1.4) B/D=1.62(.16)
Pd-C (Active)	1.014(.009)	488 C/D=9.21	730 C/D=3.21	608 C/D=7.33	24.1 C/D=1.24	1.5 C/D=0.66	16.2(0.1) C/D=1.19(.09)

The table above shows the following anomalies compared to virgin palladium:

- 8% increase in Pd110/108 ratio in sample B
- 2.87 times the silver content in sample B
- 1.66 times the cobalt content in sample B
- 15.2 times the zinc content in sample B
- 0.6 times the gold content in sample B
- 0.37 times (decrease) the iridium content in sample B
- 1.62 times the lithium-7/lithium-6 ratio in sample B

Passell then attempts to speculate on the nuclear binding energy that would be released as a result of these nuclear products.

The precise amount of excess heat produced by the cathodes in which the powdered palladium was contained has not yet been made available. Arata and Zhang's published work shows data from similar cathodes which produced about 30 to 40 megajoules of excess heat over the most active two-month period of their electrolysis.[6] If one assumes that some nuclear process produced each excess zinc-64 atom at about 10 MeV per atom, that some 12 grams of powdered palladium was contained in each cathode hollow core, and that our sample of 5 to 15 milligrams was a representative sample of the full 12 grams present, then one obtains expected excess heat of 20 Megajoules.

The only heat-producing nuclear reaction capable of producing zinc-64 is the fission of palladium isotopes or proton capture in impurity copper. Although some copper impurity is undoubtedly present, it is not readily measured by NAA under these circumstances, and even if present, it is unlikely to be as large as several hundred PPM.

Again, Passell almost certainly had no idea of the possibility of real neutrons being generated in the LENR cell and, through primarily weak interaction processes, producing such transmutations.

In his 2000 paper, Passell also cited independent LENR work from a group led by Xing Zhong Li, in the Physics Department at Tsinghua University in China. Passell said that the group used a "completely different type of experiment" yet reported a similar anomalous isotopic shift with zinc.[7]

Li told *New Energy Times* that during the period from 1989 to 1998, the Tsinghua group performed a lot of work on LENR transmutation studies. They confirmed that nuclear transmutation appeared in both Pd-H and Pd-D systems.

"The most severe alteration showed an enhancement of the Zn-68 to Zn-64 ratio by 20% relative to the ratio for natural zinc," Passell wrote. "Their unexposed palladium showed no zinc present."[3]

In writing this article, we did not attempt to conduct a survey of anomalous isotopic shifts in LENR systems. We looked primarily at the work managed by Passell. Most likely, there are many other reports of similar isotopic studies.

Cold Water on "Cold Fusion"

Consider the following history: As early as 1989, Pons was open-minded and receptive to light-water heat, despite the fact that it disproves the "cold fusion" hypothesis, but Fleischmann had dismissed the credibility of light-water heat. In the mid- to late-1990s, Pons clearly was interested in transmutations because he gave a sample to Passell for analysis.

Fleischmann had been reluctant to acknowledge LENR transmutations, which also contradict the "cold fusion" hypothesis. He finally and only reluctantly conceded the reality of LENR transmutations in 2004. Almost certainly, as of November 1999, Pons knew absolutely that "cold fusion" was not the predominant, if any, mechanism responsible for the excess heat in the Fleischmann-Pons experiments.

Passell was sensitive to the potential impact of these findings, as well:

> Probably the greatest revelation in this work is the possibility that trace elements may be significant participants in nuclear reactions in solids such as palladium so that focusing entirely on D+D fusion is not necessarily the only path forward in understanding these phenomena.

More Revelations

Additional revelations from this body of work dispel several myths about LENR and suggest the following:

1. Heavy-water (D/Pd) systems do produce transmutations.
2. LENR transmutations likely release significant amounts of nuclear binding energy.
3. Separate and discrete mechanisms are not necessarily responsible for the phenomena observed in the heavy-water (D/Pd), light-water (H/Pd) and nickel-hydrogen LENR systems.

The studies reviewed in this article cast great doubt on the claimed certainty by researchers and project managers such as Michael McKubre (SRI International), Peter Hagelstein (MIT), Michael Melich (Naval Postgraduate School) and Vittorio Violante (ENEA Frascati) who have made great efforts to measure heat and helium-4 in Fleischmann-Pons experiments or to promote such claims. Unless they make equally painstaking and sincere efforts to perform elemental and isotopic analyses, their claims of identification of a definitive energy balance between excess heat and helium-4 are meaningless.

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7. Violante Group Claims LENR Elemental Anomalies Are Contamination

In other articles in this *New Energy Times* special report, we reviewed the research efforts of people such as Thomas Passell, Xing Zhong Li, and Yasuhiro Iwamura to search for, identify and characterize possible low-energy nuclear transmutations. We reviewed their precise, thoughtful and clear investigations and powerful results.

In this article, we review an effort by a group led by LENR researcher Vittorio Violante at ENEA Frascati. Violante is part of the LENR consortium of Michael McKubre (SRI International), Peter Hagelstein (MIT), the Naval Research Laboratory in Washington, D.C. and the former Energetics Technologies laboratory in Israel. This consortium has claimed for at least a decade that the products of LENR are excess heat and helium-4 - and nothing else: that is, no neutrons, no energetic alpha particles, and no transmutations.

As they reported in October 2009, Violante's group tested and found anomalous elements after a LENR electrolysis experiment. However, the researchers attributed the anomalous elements, all 12 of them, to a variety of ordinary contaminants. They even attributed some of the elements to multiple sources of contamination. The authors stated that "all the procedures and the experiments [were] performed in a class-100 clean room."

The group presented this work at the October 2009 ICCF-15 conference in Rome, hosted by ENEA Frascati. Violante is considered a prominent researcher in the LENR field; he was the chair of the ICCF-15 conference. He was elected to that position in a private meeting by, among others, McKubre, Hagelstein and ICCF-14 chairman Michael Melich. Violante also had been invited by these same Americans to talk at the 2004 Department of Energy LENR Review. *New Energy Times* reported on his participation and his puzzling claims there in our January 29, 2010, issue.

In the work reported in October, Violante's group performed a light-water electrolysis experiment using a variety of cathode films, palladium, nickel and copper. The main thrust of this paper is the researchers' claim that all 12 of the anomalous elementals they observed in the experiment have conventional explanations – that is, no transmutations, no anomalous isotopic shifts.

The Violante researchers used neutron activation analysis, without a doubt the most rigorous tool for the job of characterizing many elements. However, the search for anomalous isotopic abundance, for which NAA is also superbly qualified, is strikingly absent from their presentation.

Ambiguous Pre- and Post-Experiment Comparison

A significant problem with their presentation is the way the six pages of data are presented. Only page 13 appears to have control (or baseline) data. Nowhere in the presentation are direct comparisons between elemental analysis before and after the experiment. This

weakness makes careful scrutiny of the presentation difficult without replotting the given values in a new table.

Incomplete Comparative Data

Another problem is that, although quantities of several new elements from post-electrolysis are shown, no counterparts for the measurements of those elements appear on the page for the "blank" electrodes. This complicates knowing what the pre-electrolysis levels were, unless the reader assumes that such elements were initially undetected.

Measured Anomalies

Without performing an exhaustive secondary analysis, let us assume that the anomalous elements the authors reported in their conclusion are well-supported by data and well-differentiated from pre-electrolysis conditions.

The authors report anomalous levels of gold, iridium, chromium, hafnium, molybdenum, tungsten, zinc, silver, tantalum, cobalt, iron and vanadium.

Five of these elements – gold, iridium, cobalt, silver and zinc – were also detected in anomalous amounts in NAA studies by Arata-Zhang in an entirely different and independent experiment and laboratory. Furthermore, the Arata-Zhang experiment was performed with heavy water; this Violante group experiment was performed in light water. (See "Isotopic Anomalies Reveal LENR Insights" in this special report.)

A Defective Bridge

However, in the Violante group's work, the researchers use a defective logical bridge to connect the data and the conclusions.

On one side of this logical bridge are meticulous details of data. On the other side are a few brief sentences claiming that the source of these measured anomalies is contamination.

But the presentation provides almost no detail about how the researchers performed tests which supported their conclusions. The only information that suggests anything about their judgment process is that they tested their lab gloves and paper using NAA. Presuming that they wore gloves and used lab paper for their control experiments as well, this should be irrelevant, or would be irrelevant had they shown a direct pre- and post-experiment comparison.

The Conclusions

The researchers account for the anomalous presence of gold and iridium by "erosion from the platinum anode." It's certainly possible; electrochemistry is messy. But what did their control experiment show? We don't know; they didn't tell us.

They account for the anomalous presence of chromium, hafnium, molybdenum, tungsten and zinc by "mold during the etching process." Exactly how did mold contribute to this

contamination? In a class 100 clean room? And did this mold affect only the test experiments, not the control experiments? Again, we are left in the dark.

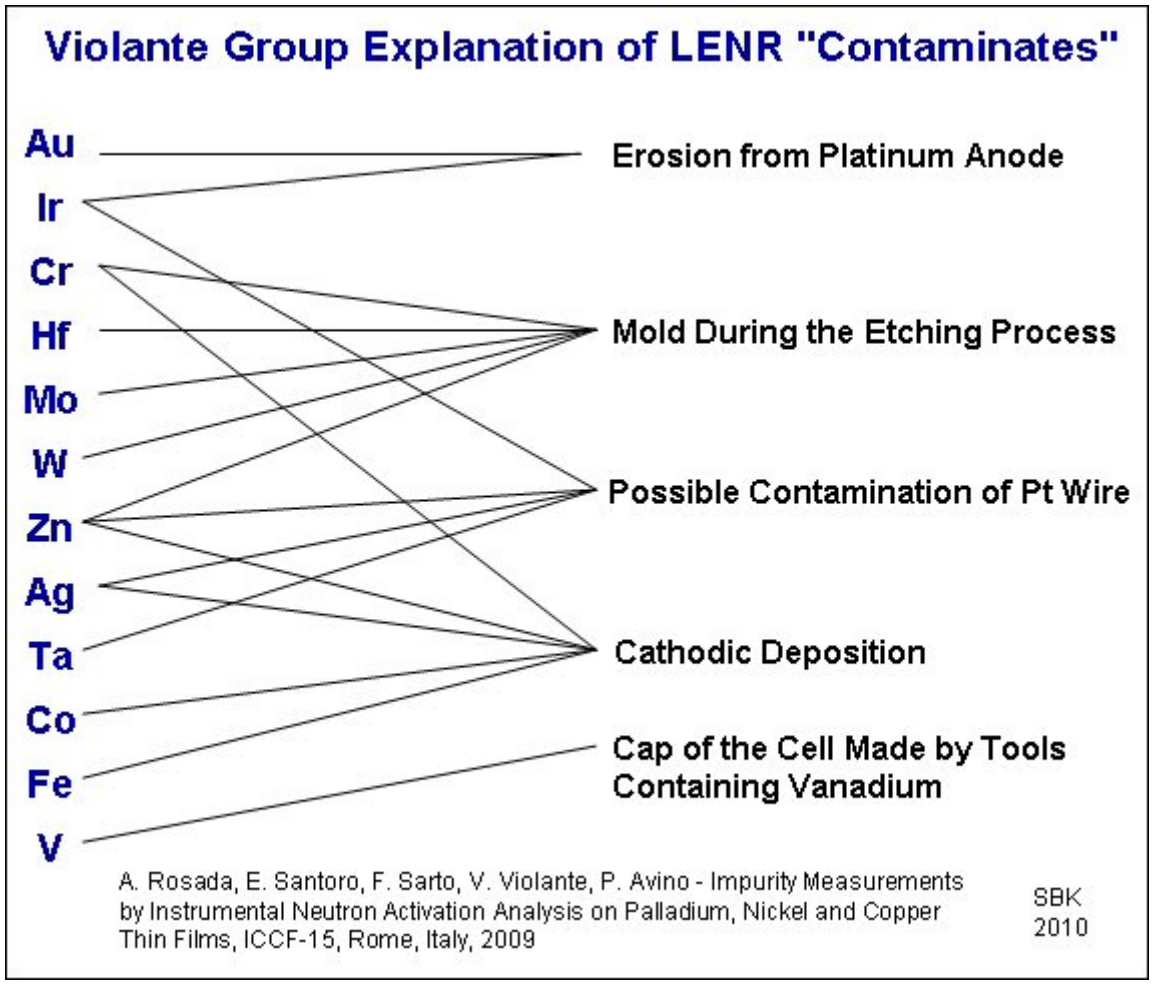
For the anomalous presence of silver, iridium, tantalum and zinc, they claim "possible contamination of Pt wire." The same questions prevail: How did they determine this possibility, and what happened with the controls?

For the anomalous presence of silver, cobalt, chromium, iron and zinc, they claim electrochemical "cathodic deposition." This is certainly possible if enough of these elements were present in the starting electrolyte, but that is very hard to tell based on the information given.

The skepticism inherent in the electrochemical cathodic deposition speculation is, of course, not new. Some people have offered this suggestion for two decades as an explanation of LENR transmutation.

Last is the explanation for high levels of vanadium. This, they claim, came from the "cap of the cell [that] was made by tools containing vanadium."

A cross-reference map will help readers visualize which elements, according to the Violante group, were caused by the respective form of contamination. The group claims that many elements came from multiple sources of contamination. The group did not explain what percent of each of the elements came from each of the claimed contamination sources.



8. Neutron Capture Is Not the New Cold Fusion

Recently, a misunderstanding has arisen—only within the LENR field—over the terms "neutron capture" and "nuclear fusion." Some people are suggesting that "neutron capture" processes are a form of "nuclear fusion."

The problem surfaced in May 2009 when some "cold fusion" researchers attempted to convince others that the time-honored and well-recognized concept of neutron capture processes could accurately be called fusion. Concurrently, they attempted to convince others that their 21-year war for recognition and justification of their claim of D+D cold nuclear fusion was on the way to victory by their suggestion and subsequent newly revised interpretation of the concept of nuclear fusion.

> **They abandoned both the nuclear physics definition of neutron capture and the nuclear physics definition of fusion.**

Here are the *New Energy Times* concise definitions of the two concepts, derived from the Oxford Dictionary of Physics:

> **Neutron Capture** involves **a single particle**, such as a neutron, with **no electric charge** entering **a nucleus**.
>
> **Nuclear Fusion** involves **two nuclei** having **like-charges** that overcome electromagnetic forces (the Coulomb barrier).

Origin of the Confusion

Nearly two decades ago, when neutron capture processes were more popular in the LENR field, there was no confusion about the distinction between neutron capture and nuclear fusion. (See "LENR Weak Interaction Theory—Hagelstein Missed" in this special report.)

As Peter Hagelstein, associate professor of electrical engineering at MIT, wrote in 1990: "No Coulomb repulsion occurs for virtual neutrons." Of course, the same logic applies to real neutrons.

Hal Fox, editor of *Fusion Facts* wrote in 1991: "Theorists like [Frederick] Mayer and Hagelstein, and Professor Yang of Hunan Normal University ... believe that what is occurring is a neutron transfer, not the fusion of two nuclei."

Three factors precipitated the recent confusion.

The first is the lack of progress on the part of LENR theorists and experimentalists in producing viable theories and rigorous evidence for LENR as a D+D "cold fusion" process.

The second factor is the growing recognition, since 2005, of the Widom-Larsen ultra-low-momentum neutron-catalyzed theory. These two factors brought things to a head for the LENR researchers who had supported the idea of D+D "cold fusion."

The third factor grew out of 2008 research published by the Navy SPAWAR group. For perhaps for the first time in at least a decade, researchers claimed experimental evidence of neutrons.[1] The SPAWAR claim is, thus far, not supported by conventional electronic detectors and characterization of neutron energies is based on interpolation. They are instead interpolated from spatial measurements of particle tracks in solid-state nuclear track detectors.

However, *New Energy Times* is not aware of anyone who has suggested a conventional mechanism that explains how these particle tracks appear to traverse right through the detectors. We are not aware of any attempt to explain these events as anything other than neutrons.

Sporadic, low fluxes of neutrons, of course, were rigorously measured and reported in the field long ago, for example by researchers at Bhabha Atomic Research Centre in India. However, the flux of detectable neutrons is in no way commensurate with the excess heat that is measured in LENR cells or the flux that would occur as the result of thermonuclear fusion. Instead, the neutrons are presently useful for, among other things, confirmation of the reality of nuclear processes in LENR.

Following the neutron publication by this well-respected Navy laboratory, the American Chemical Society disseminated a press release about it on March 23, 2009. That, along with an ACS press conference, triggered a worldwide wave of interest in "cold fusion," possibly unprecedented since 1989.

Dueling Dictionaries

Nobel prize-winner Brian Josephson, a D+D "cold fusion" advocate, and Lewis Larsen, co-developer of the Widom-Larsen "not-fusion" theory, fought a fierce war of words on a CBS-TV blog. Josephson was advocating that the definition of fusion "to join or become combined," from "the dictionary" (he didn't specify which one), include neutron capture.

Larsen, however, advocated the definition of "nuclear fusion" from the Oxford Dictionary of Physics.

The disagreement spilled over into the members-only CMNS e-mail list for LENR researchers. Other theorists began to follow the neutron capture lead.

On April 26, theorist Andrew Meulenberg announced to the CMNS list his "Modest Proposal" for new mechanisms to explain D+D "cold fusion." Meulenberg proposed that "energetic electrons within the nuclear region enhance the probability of one of the protons becoming a neutron."

On May 19, Russian researcher Fangil Gareev announced to the CMNS list his "New Mechanism of Cold Fusion Reactions," in which an electron somehow reacts with a deuteron and produces a new imaginary particle, dineutroneum, as well as a phonon. Gareev's concept was akin to the second step in the Widom-Larsen process. Gareev told *New Energy Times* that his colleague Yuri Ratis had developed a mathematical model for the theory and had published it in Russian, though he did not state where.

A week earlier, on May 11, Josephson reiterated his definition of fusion to the members of the CMNS list.

"I hope people will agree with me that blandly coming out with the statements such as 'fusion is not involved' should be considered bad practice," Josephson wrote.

American theorist John Fisher, who has developed a neutron capture model of LENR, corrected Josephson and SRI International electrochemist Michael McKubre, who had also entered the fray.

"The process could be called 'fusion' in Mike's sense," Fisher wrote, "but this would not be acceptable to physicists who already have settled on `neutron capture.'"

McKubre replied to Fisher.

"[I] don't see any need why we should be frightened off by physicists' jargon usage of the word fusion," McKubre wrote. "The high energy and particle physicists don't own the word in our context. In the future we will."

McKubre followed that post with another generic definition of the word fusion and a message to his colleagues in the field.

> Folks, I have a contribution about definition.
>
> It is true that a number of our physics colleagues are strongly opposed to the phrase 'cold fusion,' but I have always thought that this is because they have internalized an over-restricted (and over-possessive) use of 'fusion.'
>
> As the dictionary states, this is a shorthand for nuclear fusion, but it seems to have been restricted even further to mean (in their minds exclusively to mean) the pairwise fusion of light isotopes – specifically Li and D – by a particular mechanism. I find this to be a limited jargon usage at the point of banality, and I wonder why we bother to argue.
>
> First, look at the definition: 'The process or result of joining two or more things together to form a single entity.' I submit that D + D –> 4He qualifies (whereas pairwise collisional hot fusion via the normal branches might not. ...). I also submit that John's neutron addition reactions also qualify as fusion. We are certainly not bound to use the jargon of particle physics and should not! We are free to use the English language (I suspect this definition is similar in most), without apology or deference, so long as we use it correctly.
>
> A more subtle point is the presumption of mechanism. In thermodynamics, one is concerned with initial and final states only! The pathway (mechanism) is just important for rate predictions (and we are a long way from that). The fusion 'purists' (with their corrupt definition) want ownership of the products and process. However, to re-coin a phrase, 'the circumstances of cold fusion are not those of hot fusion.' Whatever reaction we are studying could (I would argue

must) undergo a very large number of steps involving a very, very large number of participant species. But for definition, this is not important. Is the final state the result of 'joining two or more things together'? If yes, then I submit it is fusion. If it is spontaneous, then it is exothermic. If it is exothermic, then it consumes mass.

–Mike

P.S. One reason the critics are concerned about naming is to get us involved in a trap of circular reasoning that proceeds something like this. If it is fusion, then it is 'our' (hot) fusion, then you must see 'our' rates and products, which you don't, so it is not. You can all see the flaw, and by now I would hope we could ignore this scarecrow made of very poor straw.

As an interesting side note, in 1996 McKubre thought it unlikely that fusion was occurring in his heavy-water cells and certainly not in the light-water cells.

LENR researcher Dennis Cravens commented next on the CMNS list:

I am not sure what the definition of fusion is, but if we put deuterium into the system and get helium out, we have increased the [atomic number]. The process of how that is done doesn't seem to be important to the definition. There may be many ways to achieve it. The net effect is we have increased the [atomic number] and the mass. Sure seems like fusion to me. To me, fusion [is] just producing a product with higher [atomic number] and mass than the reactants.

New Energy Times received another interesting idea about the concept of fusion from a LENR researcher whom we agreed to quote anonymously:

> In my view, if palladium absorbs a neutron, it becomes unstable. It then gives off an electron and becomes silver. Is this fusion or transmutation? The silver nucleus has one more proton than the palladium atom from which it was produced. So we can say that the neutron fused into the palladium nucleus, causing it to transmute to silver.

Another response on the topic came from LENR-CANR librarian Jed Rothwell:

> I think you are silly for saying it may not be fusion. Of course it's fusion! Heat + Helium production = Fusion. Case closed. I have no idea how those deuterons fuse, but I am sure they do. I cannot judge. However, I am sure that any process that converts deuterium to helium and produces heat is fusion, by definition.

"Cold fusion" proponents may elect to adopt a nonscientific interpretation of the word "fusion" rather than the nuclear physics interpretation. However, by doing so, they risk associating LENR not with nuclear physics, but with automobiles, ice cream and hairstyles.

Reference

1. Mosier-Boss, P.A., Szpak, S., Gordon, F.E., and Forsley, L.P.G., "Triple Tracks in CR-39 as the Result of Pd–D Co-deposition: Evidence of Energetic Neutrons," *Naturwissenschaften*, Vol. 96, pp. 135-142, (Oct. 1, 2008)

9. Who's Afraid of LENR Transmutations?

Was LENR transmutation suppressed by U.S. LENR researchers who were enamored of D+D "cold fusion" because transmutation disproved their hypothesis? Or was it judiciously omitted for other, perhaps more prudent, reasons?

In 2007, Mahadeva Srinivasan, former associate director of the physics group at the Bhabha Atomic Research Centre in India, invited SRI International electrochemist Michael McKubre and me to come to India on a two-week lecture tour. Our trip was partially sponsored by the Indian government.

On our first stop, at the International Conference on Systemics, Cybernetics and Informatics in Hyderabad, organized by Ethirajan Rajan, McKubre, Srinivasan and I gave lectures on LENR.

McKubre was first. His talk, as requested and advertised, was to be a broad review of the LENR field. After his talk, Srinivasan was the first to ask a question.

"Mike, thank you for the great presentation," Srinivasan said, "but why didn't you say anything about transmutations?"

"My friends in the cold fusion community chastise me very severely, but I'll say it again since we're all friends," McKubre said. "I know what to do with heat; I know how to use heat. In the transmutation business, I don't know what to do with the ability to turn expensive elements into cheap ones. I don't have a use for that.

"Now, in turns of a demonstration of a nuclear effect, I think it's useful, and other people have done that. But my interest is a little bit beyond demonstrating that there is a new nuclear effect here. I know that there's a new nuclear effect here. What I'm interested in is,

What is it good for?"

Turn the clock back to 2004. The place: Marseilles, France. The event: the 11th International Conference on Condensed Matter Nuclear Science. As is traditional, several researchers summarized their thoughts about the conference. Talbot Chubb, a D+D "cold fusion" theorist, was among these.

"Well, I agree that the heat production is a primary concern," Chubb said, "and I think that the transmutation and the observations of flakes, or whatever, producing energetic particles are part of the picture, but the real core is the heat. The other things are teaching, are experiments that teach us something about what the process is going on. But I think the heat observations are really going to make the difference." (Audio)

Can Engineering Precede Understanding?

McKubre's and Chubb's statements have a common theme: Obtaining practical levels of LENR excess heat should be the most important focus of the field.

This is a dubious strategy for two reasons. First, LENR researchers, in general, neither understand the logical mechanism responsible for the excess heat nor have precise physical control of excess-heat generating experiments. All they know are the minimum conditions required to create excess heat.

Second, unless they get lucky enough to gain full control of the heat-generating process, any incremental progress they make with excess heat will do exactly what excess heat has done politically and technologically for the field throughout the last 21 years: almost nothing.

Chubb's comment is profound. He comes very close to recognizing the significance of transmutations – that is, how they might enable LENR researchers to learn what makes LENR tick.

It is remotely possible that a trial-and-error experimental approach may lead to practical LENR excess heat before the logical mechanism is understood. But for 21 years, it hasn't.

The fact that discoverers Martin Fleischmann and Stanley Pons had better success in 1989 than perhaps the most well-funded group since then – the consortium of SRI International, ENEA Frascati, MIT and NRL – reveals the harsh reality of the approach taken by this consortium. Ironically, the federal group group—U.S. Navy SPAWAR Pacific—which has not been a key part of this consortium has made some of the most dramatic progress in LENR research.

The SPAWAR group has been expanding the LENR knowledge base consistently with leading-edge research and publishing in peer-reviewed journals for two decades. The group has only received limited amounts of internal funding.

How many papers reporting new LENR research have SRI International and NRL published in the last decade? To our knowledge, none. The only thing the SPAWAR and NRL groups have in common is that they are both part of the U.S. Navy. They are worlds apart in their

culture, politics, financial streams and, at least as far as LENR, their effectiveness.

Fleischmann Reluctantly Accepts Transmutations

At the 2004 ICCF-11 conference, Fleischmann, sometimes referred to as the grandfather of the field, indicated what may be his first public acceptance of the reality of LENR transmutations.

"This conference is notable for the results of transmutation experiments which have been presented, and I would agree with Francesco [Celani] that these are now much more solid," Fleischmann said. "This is an aspect of the field which I viewed with intense skepticism originally. We, ourselves, had seen some transmutations, but we realized that to produce a publishable result from that was something which we just didn't have the resources for.

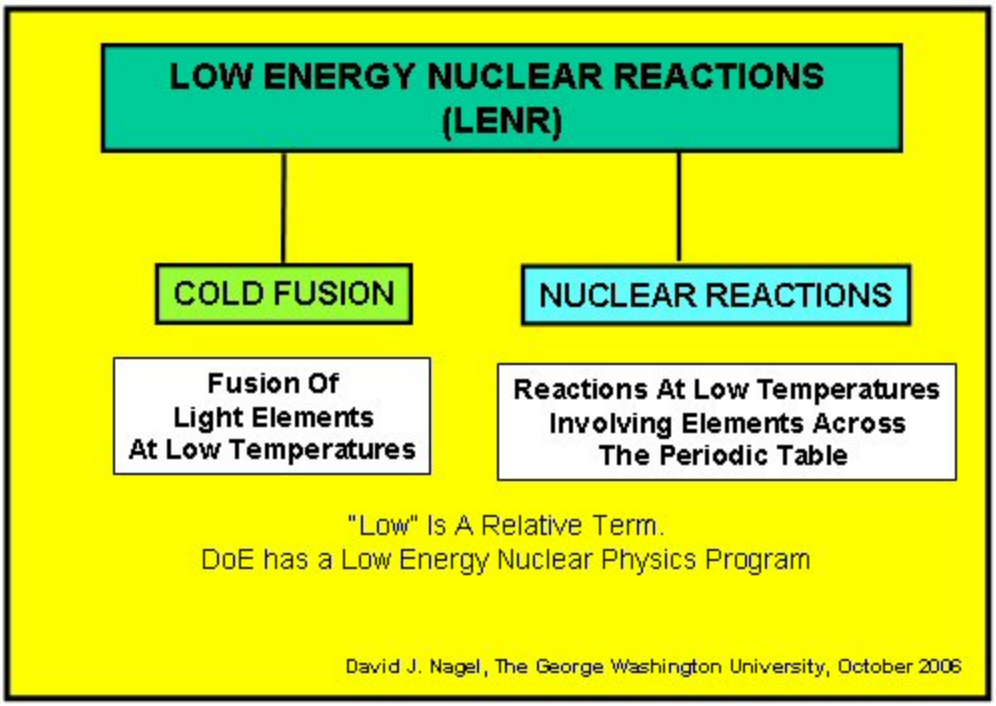
"As far as the theory is concerned, we have to recognize that people will circle 'round their patch of knowledge. This doesn't mean it's true, but they will circle around their center of expertise, and we have to just wait for another few years and see which of these approaches will survive. The key point is that the transmutation experiments now seem quite believable." (Audio)

An Artificial Distinction

As the reality of LENR transmutations began to sink in among the LENR researchers, some of them glimpsed the potential significance of the transmutation observations. That is, they realized that the transmutation results could not be explained by the D+D "cold fusion" hypothesis. What could this mean? That LENR excess heat and helium-4 were not the result of D+D "cold fusion"? Or instead, that LENR transmutations were the result of a completely different process?

An artificial distinction evolved to fit "excess heat and helium-4" into the D+D "cold fusion" category and "transmutations" into another category.

In 2006, David Nagel, the chairman of the ICCF-14 conference, depicted this distinction as follows:



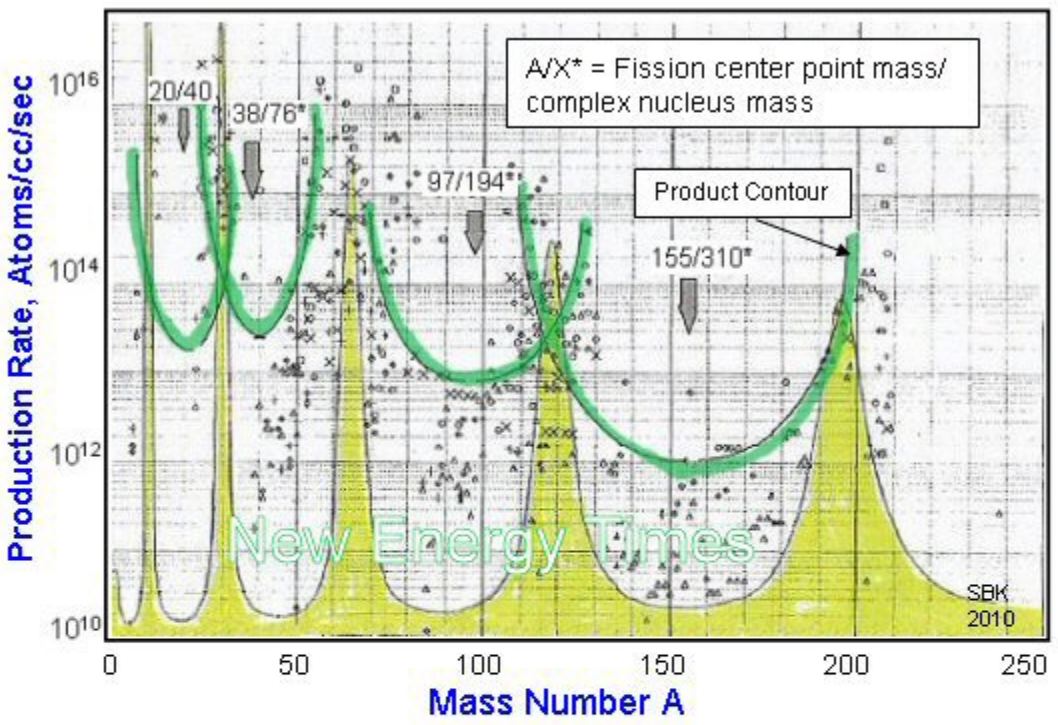
Three years earlier, George Miley, a pioneer in light-water LENR research, distinguished the two groups this way:

	Reaction Products	Comments	
D-D Reaction D-D	T + p	Each branch 50% probable	
	He-3 + n	"Normal" D-D Fusion	
	He-4 + hυ (heat)	"P-F type Cold Fusion"	LENR
Lattice Reaction p/D + metal	Isolated Isotopes or "fission" product array	"Transmutations"	

But some people were aware of data that suggested similarities between the two groups.

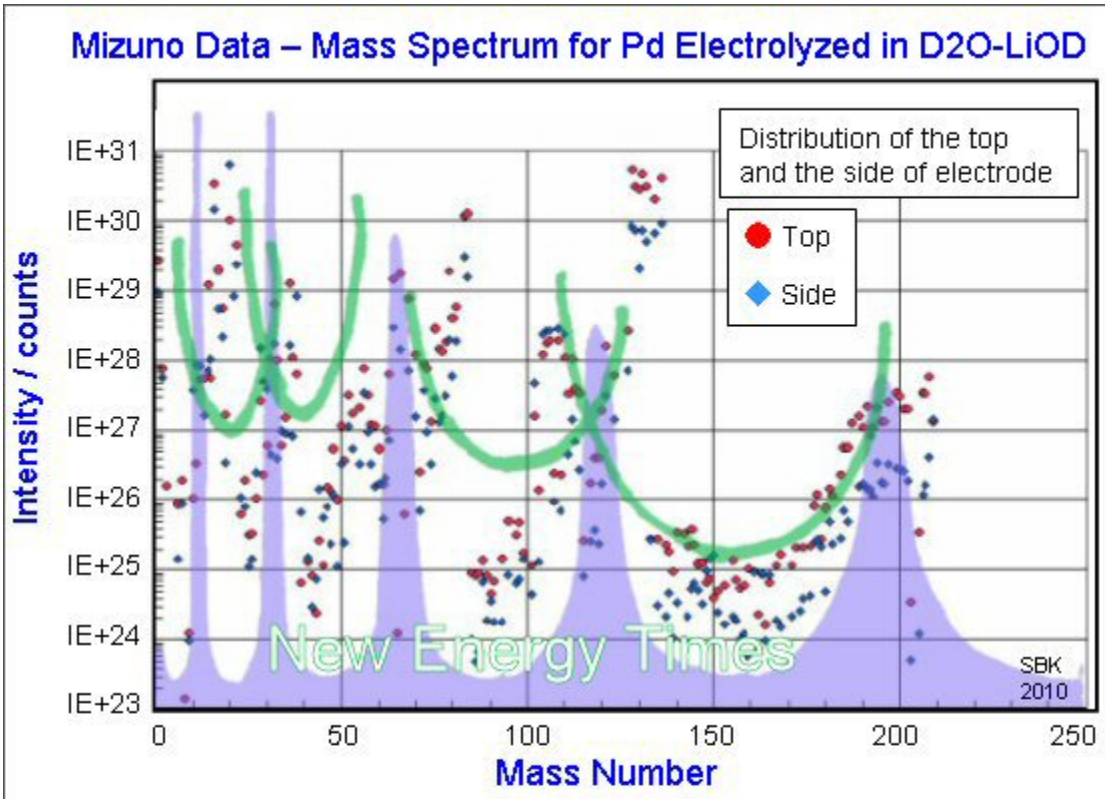
For example, during one of McKubre's talks in India, he mentioned (but did not show the following images) the heavy-water work of Japanese LENR researcher Tadahiko Mizuno and the light-water work of Miley. He said there was a relationship between the two.

"Mizuno and Miley – you can overlap [their data] and see the same thing," McKubre said.



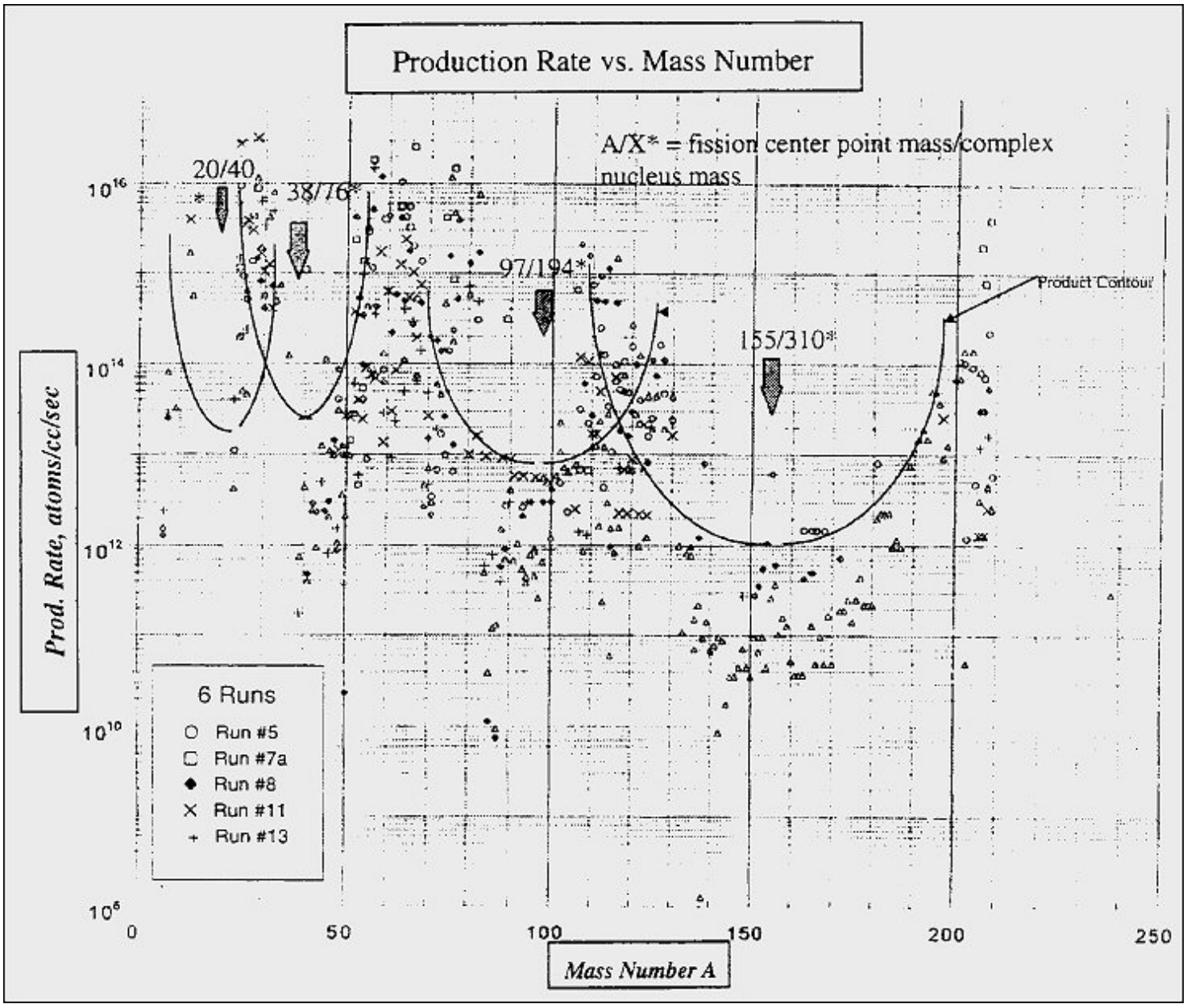
Upper 5-peak peak curve drawn by Miley in 1996 based on 6 experimental runs of transmutation yields from Ni-H LENR systems [1]. Lower curve, shaded in yellow, drawn by Larsen in 2000 with no fitting, based on Widom-Larsen ultra-low momentum neutron absorption model. [2]

1. Miley, G.H, and Patterson, James, "Nuclear Transmutations in Thin-Film Nickel Coatings Undergoing Electrolysis," Journal of New Energy, Vol. 1(3), pg. 5, (1996)
2. Larsen, Lewis, Feb. 7, 2009 slides

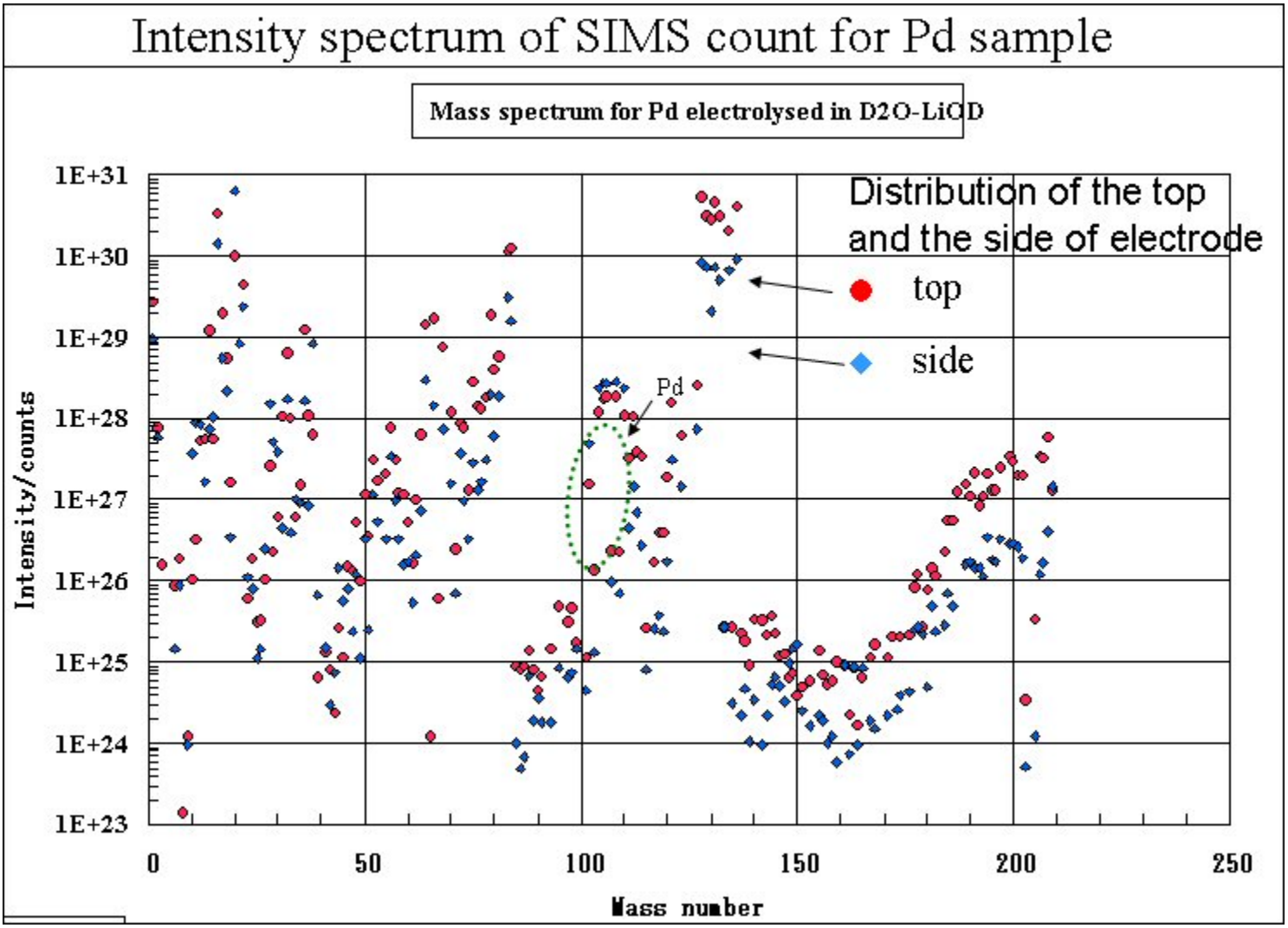


Mass spectrum of SIMS measurement for Pd sample surface, which generated excess heat, after electrolysis in 1991. Confirmatory tests also with EDX, AES, EPMA, ICP-MASS. Two areas of surface were analyzed and show similar spectra [1]. Upper 5-peak peak green curve is an overlay from Miley experiment, drawn by Miley for his data in 1996 [2]. Lower curve, shaded in purple, drawn by Larsen in 2000 with no fitting, based on Widom-Larsen ultra-low momentum neutron absorption model. [3]

1. Mizuno, Tadahiko, "Isotopic Changes of Elements Caused by Various Conditions of Electrolysis," American Chemical Society, March 2009
2. Miley, G.H, and Patterson, James, "Nuclear Transmutations in Thin-Film Nickel Coatings Undergoing Electrolysis," Journal of New Energy, Vol. 1(3), pg. 5, (1996)
3. Larsen, Lewis, Feb. 7, 2009 slides



Source: George H. Miley, "Possible Evidence of Anomalous Energy Effects in H/D-Loaded Solids-
Low Energy Nuclear Reactions (LENRs)," *Journal of New Energy*, 2, No. 3-4, pp.6-13, (1997).



Source: Tadahiko Mizuno, 2009 ACS, Salt Lake City, Utah

As it turns out, the researchers who focused on deuterium-palladium systems (heavy-water) gave life to the latter philosophy – two different sets of phenomena – though the distinction was artificial. They obtained extensive though not complete compliance from the researchers who focused on either hydrogen-palladium systems (light water) or nickel-hydrogen systems.

A few years ago, I asked Miley whether he was planning to report any new light-water results. He said "no," and he quickly walked away.

One light-water researcher who no longer attends the ICCF conferences told *New Energy Times* recently, "The heavy-water researchers were livid with us because they were using light-water as their control."

In the last decade and a half, the heavy-water researchers got their way: In many cases, they sufficiently ostracized the light-water researchers from participating in the ICCF conferences. In other cases, they intimidated those who stayed in the game, discouraging them from making any direct claims that might conflict with the party line.

The cold fusion faction's platform was that heavy-water experiments produce heat and helium-4 but not transmutations and light-water experiments produce transmutations but not excess heat and helium-4.

The logical separation between the light-water work and heavy-water work as being two distinct mechanisms, particularly in light of the heavy-element transmutations in heavy water, appears to be incorrect.

Why the Resistance?

What is the reason for the resistance to accepting transmutation at room temperature by the people who easily accept the hypothesis of "fusion" at room temperature? Perhaps it is based on the fear of being associated with medieval alchemy, which was marked as pathological science by modern chemistry. Is this fear based on reality or substance?

Why did the American "cold-fusion" faction omit LENR transmutations when its members were invited to give a comprehensive view of the broad subject of worldwide LENR research to the Department of Energy in 2004? Were they justified in omitting this topic because the Department of Energy looked at LENR transmutation with greater skepticism than it did D+D "cold fusion"?

Based on the fact that DoE had invited Lewis Larsen to speak on LENR as well as on LENR transmutations half a year earlier than the "cold fusion" faction's presentation to the DoE, the faction's fear, if such was the case, was unjustified. Larsen's DoE presentation reflected his confidence about LENR transmutations.

"You can argue about excess-heat measurements and ponder near-absence of 'normal' nuclear products, but transmutation experiments involving LENRs are irrefutable," Larsen said.

Perhaps the resistance to accepting LENR transmutation by the "cold-fusion" faction is based on their members' fear that transmutation disproves the hypothesis of "cold fusion."

Look at what the faction tried to do at ICCF-14, under the direction of organizers Nagel, Melich and McKubre. Despite the fact that LENR transmutation was Talbot Chubb's "big news" at ICCF-10 in 2003, accepted as part of the field by the "cold fusion grandfather" at ICCF-11 in 2004, and represented well in ICCF-12 and ICCF-13, it was initially omitted from the conference agenda in 2008 at ICCF-14, thanks to the "cold fusion" faction.

Instead of setting the agenda to include the full breadth of LENR research, the faction promoted the idea that excess heat and helium-4 from heavy-water electrolysis experiments, denoted as the Fleischmann-Pons experiment (FPE), was the primary focus of the field.

When I noticed this omission of transmutation from the ICCF-14 conference agenda, I asked the conference organizers, through the CMNS e-mail list, for an explanation. I received vague and incomplete responses. After seeing the less-than-satisfactory response and interest by organizers, Miley, a LENR researcher who has been a pioneer in LENR transmutation research, addressed Nagel, also through the CMNS e-mail list:

"I feel it is unfortunate that the focus of the meeting on the FPE seems to ignore transmutations which were not envisioned by these great pioneers and are not explained by the traditional D+D fusion theories," Miley wrote. "Is the transmutation effect relevant to the FPE? Or vice-versa? This area was separated earlier from the presentations at the last DoE review, and that, in my opinion, weakened the case there. I hate to see the field break up into separate areas, but events seem to be moving in that direction."

Bob Smith, an assistant to the ICCF-14 organizers, commented next.

"As far as why they want to keep it to the FPE, ... my opinion is that they want to minimize the competing effects of getting excess heat and turning it into power," Smith wrote.

Smith must have meant "effects that compete with the FPE," because, when the final agenda was published, FPE was given center stage on Monday and Tuesday of the conference. This was significant because the organizers promoted the FPE as the most important aspect of the conference on their Web site and they gave free conference access to the media on Monday and Tuesday.

Had any members of the mainstream media chosen to attend and report on the conference (none did) and had they chosen to attend past Tuesday, they would have had to pay to attend those days on which transmutation was mentioned.

Actions by Melich also helped to keep some Russian transmutation researchers away from the conference. Melich and Nagel blamed the U.S. Department of State for the failure of many of the Russians to obtain American visas. Melich told *New Energy Times* that the U.S. consulate in Moscow gave Melich one set of instructions that caused the logistical failure, but the information provided to *New Energy Times* by Christofer Van Bebber, vice consul for the U.S. Embassy in Moscow, contradicted the information provided by Melich.

Coming back to Smith's e-mail to the CMNS list, he then quoted McKubre.

"Mike McKubre," Smith wrote, "has said, 'Studies of the FPE keeps the community together and working toward a useful goal of producing a substantial heat source.' I think that Talbot Chubb, Yoshiaki Arata, and others would agree that studying the FPE is very worthwhile, as the baseline technology of interest."

I thought the idea of "keeping the community together" was profound and fodder for another possible essay on groupthink.

People who have never attended an ICCF conference should realize how McKubre is regarded within the field. He is the one asked to emcee ceremonies and speak at the conclusions of conferences. He's good at it; he is eloquent, witty, and knowledgeable and has a delightful English accent. He exudes confidence and authority and commanded the respect of LENR audiences at ICCF-14.

For example, when McKubre said to the audience, "I'd like you all to rise to your feet and give Dave [Nagel] and Mike [Melich] a round of applause," you can bet they stood up.

In this context, if Martin Fleischmann is the "grandfather" of "cold fusion," perhaps McKubre

could be called its "king."

When I saw the strong political efforts to keep transmutation from the ICCF-14 conference, I called to chat with George Miley. He spoke about organizing a separate mini-conference either before or after the ICCF-14 conference.

Once word got around of that possibility, Nagel announced to the CMNS list that the "ICCF-14 Web site ... has been modified to make more explicit that transmutations are a subject of interest for the coming conference, as they have been at past conferences."

So was there a transmutation session at ICCF-14? Yes, an hour and a half was dedicated to the transmutation session, the same amount of time dedicated to presenting compilations of country-by-country "cold fusion" history.

However, after the official close of the conference on Friday, about 70 people packed into a separate room for a LENR transmutation workshop that was not officially included or recorded for the scientific record as part of ICCF-14.

The Importance of Transmutations

Readers of this Special Report by now realize the significance of LENR transmutations, how they may help all LENR researchers learn about every type of experiment and effect, be it the Fleischmann-Pons effect, the Iwamura effect, the Letts-Cravens effect, and so on.

Mizuno also provided additional insight into the ability to turn expensive or cheap elements into other elements at the American Chemical Society meeting in Salt Lake City on March 22, 2009.

"If the transmutation mechanism can be understood," Mizuno said, "it may then be possible to control the reaction and perhaps produce macroscopic quantities of rare elements by this method. In the distant future, industrial-scale production of rare elements might become possible, and this would help alleviate material shortages worldwide."[1]

On June 25, 2009, Larsen released an edgy but precise analysis of his perspective.[2]

- Measurements of transmutation products, so-called “nuclear ash,” if reliably observed at the conclusion of an LENR experiment, are important because they indicate that new chemical elements have somehow been produced and/or isotopic ratios of some elements previously present have been significantly altered.
- Accurate detection and analysis of whatever types of products may be produced during an LENR experiment can potentially allow one to determine exactly which type(s) of nuclear process(es) occurred and the reaction(s) that created the products.
- Since 1989, most “cold fusion” researchers have focused primarily on the Holy Grail of creating macroscopic LENR devices that can produce substantial fluxes of calorimetrically measured excess heat.

- Absent a usable theory of LENRs and a detailed understanding of nanoscale device physics, achieving success with such an approach is at best a random proposition. It is a bit like trying to fabricate modern microprocessor chips with submicron feature sizes on silicon dies using machinists' T-squares, rulers and scribes rather than using advanced lithography and CMOS process technologies.
- Even when substantial macroscopic excess heat is achieved in a 1 cm^2 device, heat as the sole metric of success provides little or no insight into underlying mechanisms of heat production or what one might do to improve the quantity and duration of heat output in future devices.
- For example, exhaustive detection/identification of all nuclear reaction products to whatever extent possible is crucial technical information.
- Unguided, random Edisonian exploration of LENRs' vast physics and materials parameter space is very likely responsible for the lack of readily reproducible experimental results and limited R&D progress that have characterized the field of LENRs for the past 20 years.

References

1. Mizuno, T., "Isotopic changes of elements caused by various conditions of electrolysis," presented at American Chemical Society, Salt Lake City, March 22, 2009

2. Larsen, L., June 25, 2009, Technical Overview

10. Fleischmann: It Must Be Neutrons

Telephone Interview Conducted June 3, 2009 (audio)

Steven B. Krivit: When the *60 Minutes* program aired about a month or two ago, they had a clip of you saying something about regrets, and I think you said you regret calling it fusion. I was curious about that. Can you tell me more?

Martin Fleischmann: Well, fusion has a special meaning in the scientific literature – hot fusion – and perhaps it was a mistake to call this process fusion. It should have been called a nuclear effect, you see.

Calling it fusion gave people the opportunity to say, "No, it isn't fusion." It isn't like that. If it had been called a nuclear effect, they couldn't have related it back to the fusion process, as such.

SK: How about from your own perspective? I remember reading something [from about] a week or two before the [1989] public announcement. I think you wrote to David Williams at Harwell, "The neutron-to-tritium ratio doesn't match. I'm very concerned." I think you said in that message or another message around that time, "What else could it be?" It seems like that was your best guess, and it seemed to me that you felt like that was a reasonable guess to make at the time. Was that?

MF: Yes

SK: That's a fair statement?

MF: Yes.

SK: I suppose you probably had no idea what the reaction was going to be like.

MF: No. It seemed to me that calling it fusion drew attention to the type of process which it could be, you see. It seemed reasonable to call it that at that time.

SK: I suppose there was nothing else, to your awareness, from which to categorize it?

MF: No, it was a type of process to which one could refer.

SK: Yes, certainly. Well, 20 years later, now it seems like that distinction is much easier to see. I've seen other ideas that relate to neutron-related processes that could be – not perhaps as simple and direct as D+D > 4He – but other more-complex processes, perhaps other alternative pathways to getting to heat and helium.

MF: Yes, it seems reasonable to have called it that, but perhaps one shouldn't have called it that.

SK: Yeah, that seems understandable. I was wondering whether you had a chance to catch wind of the ideas in the last few years about neutron-catalyzed reactions?

MF: Yes, it must be. You know, the neutron is not very strongly bound in deuterium so maybe there is some substance to those thoughts.

11. LENR Weak Interaction Theory—Hagelstein Missed

Since 1989, MIT professor Peter Hagelstein has been struggling to find a viable mechanism to explain LENR experiments.

Among LENR researchers, Hagelstein has been a driving force and icon in the quest for the holy grail of energy: the mythical idea of "cold fusion."

An applied physicist, as he calls himself (his degrees are in electrical engineering and computer science), he thought he had figured out the mechanism for "cold fusion" three weeks after the 1989 University of Utah fusion press conference.

By 2005, he had tried more than 150 models in an attempt to explain LENR.

But he didn't always pursue the "cold fusion" hypothesis.

Back in 1993, he recognized that both fusion and nonfusion theories were viable approaches. Here's an excerpt from his ICCF-3 trip report[1]:

> The theories may initially be divided up into two general categories; those involving (modified) fusion mechanisms, and those not involving fusion mechanisms. Papers considering fusion mechanisms face the two basic problems of (1) arranging to get nuclei close enough together to fuse, and (2) possibly modifying the fusion reaction profiles. We first consider papers describing theories based on fusion mechanisms.

Hagelstein thought nonfusion, weak interaction, neutron-based theories "more closely match[ed] the experimental observations."

> A number of theorists, including myself, have gone away from fusion reaction mechanisms. The motivation for this is to avoid the coulomb barrier (if possible) and to find reactions with signatures that hopefully more closely match the experimental observations. Each new non-fusion approach carries with it specific problems and issues that are associated with the specific reaction mechanism. Aside from this, any new approach must also arrange itself to be consistent with physical law, observations in this and other fields, and must presumably be functioning in a manner not previously expected (lest it would have been found earlier). We describe such contributions below.

He was very straightforward and did not attempt to use sophistry to co-opt non-fusion approaches as fusion. In the next paragraph, Hagelstein explains his interest in the nonfusion, weak interaction, neutron-based hypothesis.

Electron capture on a deuteron would lead to two virtual neutrons; if it could be arranged for the virtual neutrons to be in proximity with neighboring nuclei, then further reactions could occur. This approach was described in two abstracts by J. Yang of the Dept. of Physics, Hunan Normal University of China.[84,85] Yang proposes that the two neutrons form a stable dineutron that reacts with deuterium to make tritium and a free neutron, and with ^{105}Pd to make ^{106}Pd and a free neutron.

I consider this general approach to be one of the basic non-fusion approaches that actually begins to try to address the coulomb barrier problem. Once the electron capture occurs, the coulomb barrier is gone, potentially leading to the possibility of something happening near room temperature. One difficulty involved in this approach are that the electron capture is mediated by the weak interaction, which really is very weak, making it hard to obtain significant reaction rates. A second difficulty is that virtual neutrons do not generally wander more than fermis away from their point of origin, making it difficult for a virtual neutron to reach another nucleus to interact.

Even as early as 1990[3], Hagelstein was seriously considering weak-interaction processes that would create (virtual) neutrons.

THEORY-HAGELSTEIN

Peter L. Hagelstein, (MIT), "Status Report on Coherent Fusion Theory", *Proceedings of The First Annual Conference on Cold Fusion,* March 28-31, 1990, University of Utah Research Park, Salt Lake City, Utah.

ABSTRACT

Nuclear reaction which may exhibit coherent effect have been studied as a candidate explanation for cold fusion effects.

An analysis of a general class of two-step coherent reactions involving charged nucleons has been performed, and very small reaction rates are found. This result is due to the small tunneling factors associated with coulomb repulsion.

We are investigating two-step coherent reactions which begin through weak interaction mediated electron capture, which in hydrogen isotopes would produce off shell (virtual) neutrons. No coulomb repulsion occurs for virtual neutrons. Virtual neutron capture by deuterons would yield tritium, and virtual neutron capture by protons by a factor of 5000 on a per nucleon basis, and corresponds to a heat-producing reaction. The nuclear reaction energy would be coupled into the electrolysis process, with the final reaction products stationary.

Hagelstein's 1990 theory involved creating virtual neutrons through the weak interaction. Presumably, he was not able to fully articulate the physics and mathematics correctly to develop a complete weak-interaction-based theory. Otherwise, he would have shouted his success from rooftops and claimed priority over Widom and Larsen, which he has not. Instead, there has been bitter antagonism between Larsen and Hagelstein.

If Widom and Larsen end up being correct, Hagelstein, as well as Tadahiko Mizuno, Yasuhiro Iwamura and Mitchell Swartz, can take credit for the good instincts that led them to consider weak interactions and neutrons key to LENR. Images from papers of each of these authors are shown below.

In this case the potential of the entire system acts as a powerful force between electron and proton. In other words, the entire potential is V + V′. With these interactive effects, the following reaction between the proton and the electron occurs, and as a result a neutron is produced:

$$p + e^- \rightarrow n + \text{neutrinos} \quad (15)$$

When this is not a proton but a deuteron, you get this reaction:

$$d + e^- \rightarrow 2n + \text{neutrinos} \quad (16)$$

Compared to many other reactions that have been proposed, this one is markedly better at explaining the observed experimental results. The sporadic production of neutrons, the tritium production at orders of magnitude higher than neutrons, the sudden onset of heat, the dearth of radioactivity, the difference between hydrogen and heavy hydrogen, and finally the anomalous isotope reaction products can all be explained by this. This theory is most effective at explaining the difference between electrolysis with light and heavy hydrogen, and the reaction products from each. The hydrogen electrolysis reaction produces neutrons, which go into the nuclei of the cathode metal atoms, producing heavier elements. When these are unstable, they undergo beta decay to become more stable elements. Depending on how the neutrons enter the nuclei the elements do not always convert to heavier elements, but in some cases nuclear fission occurs until a stable isotope is formed. In this case, a large mass deficit is created, causing huge amounts of heat.

Mizuno weak-interaction inverse beta decay produces neutrons.[3]

Considering all the backgrounds, we propose the EINR model as one of the guiding models to investigate this new phenomenon. Needless to say, it is just an assumption to explain observed experimental results, which contain many problems to be solved and should be improved.

First, the electron capture by a deuteron occurs in deuterated Pd:

$$d + e^- \rightarrow {}^2n + \nu \ . \tag{8}$$

Simultaneously, further neutron capture reactions occur. In the following reactions, only exothermic nuclear reactions are assumed. Basically one di-neutron created at electron capture reacts with surrounding nucleus X, as expressed in the following equation:

$$X_Z^A + {}^2n \rightarrow X_Z^{A+2} \xrightarrow{\beta^-} Y_{Z+1}^{A+2} \ . \tag{9}$$

The tritium production reaction is an example of the equation, which is observed in our experiments[2,3,5]:

$$p + {}^2n \rightarrow t \xrightarrow{\beta^-} \mathrm{He}_2^3 \ . \tag{10}$$

Helium-4 production, noticed as an important nuclear product, is also explained by this EINR model:

$$d + {}^2n \rightarrow \mathrm{H}_1^4 \xrightarrow{\beta^-} \mathrm{He}_2^4 \ . \tag{11}$$

Iwamura weak-interaction inverse beta decay produces neutrons.[4]

INTERPRETATION – POSSIBLE PATHWAY

When the possible pathway mentioned above is considered, it is noted that it might contribute to the excess heat and to other researchers' reported transmutation reactions, through the production of *de novo* deuterons. We suggest a multistep pathway leading to deuteron production with the generation of an excited intermediate state (a virtual deuteron in light water with nickel, and -- as presented elsewhere -- an excited helium-4 nucleus in heavy water with palladium systems). Fig. 3 shows some of the relevant nuclear states.

The first step is the corollary of well-known neutron decomposition. Free neutrons have a theoretical lifetime of ~10 minutes but are observed to decay within circa three minutes via interaction with a proton. Taking the neutron lifetime as 918 seconds, we rewrite the basic beta decay equation, using backwards time, to give a proton and electron opportunity to react to an extremely rare occurance in a fully loaded, one kilomolar electron-loaded, fully-hydrided alloy. This gives a thermal neutron and neutrino.

$$p^+ + e^- \rightarrow n^0 + \nu^e$$

Swartz weak-interaction inverse beta decay produces neutrons.[5]

Neutrons produced by weak-interaction inverse beta decay were even mentioned by Larry A. Hull back on May 15, 1989, in a letter to *Chemical & Engineering News*.

SIR: The detection of helium-4 by the University of Utah group in cold fusion experiments would seem to confirm that fusion reactions have definitely occurred. However, there remains the question of mechanism. I would like to suggest the following electron tunneling reaction in an electrostatic field, which is a version of the $p^+ + e^- \leftrightarrows n$ nuclear reaction:

$$np^+ + e^- \rightarrow nn$$

diffusion of nn to hit np^+

$$nn + np^+ \rightarrow [n_3p^+] \rightarrow e^- + [n_2p_2{}^{++} \equiv {}^4He^{++}]$$

This would essentially be a catalytic fusion process, which would have the advantage that the neutral nn particle could circumvent the electrostatic barrier to $np^+ + np^+$ fusion. Any nn escaping fusion would likely degenerate back to $np^+ + e^-$.

Also, the presence of $Li_7{}^+ + nn$ could

Continued on page 46

Continued from page 3

lead to Be_9 and B_{11} production by similar processes. It will be interesting to learn if the electrodes are stable enough for long-term fusion or whether the electrodes need to be frequently recycled by baking or refinement. I do not know if this process can be scaled up to practical large-scale energy production, but cold fusion may just be the answer the world needs for its greenhouse effect, energy shortages, and environmental pollution problems.

Larry A. Hull
New Sharon, Iowa

By 1998, right about the time when SRI International conducted its Case replication experiment, Hagelstein went back to pursuing fusion-based concepts.

Larsen began looking at LENR in 1997. He traveled to conferences, met many of the researchers and appeared to have amicable relationships with all of them. But apparently nobody knew he was working on a theory until May 2, 2005, when he shocked his colleagues with a pre-print of a nonfusion theory paper that he and his collaborator Allan Widom had submitted to the *European Physics Journal C*.

Although many other people had intuited that weak interactions might create neutrons (Step 2 in the Widom-Larsen theory, or WLT), Larsen and Widom figured out an explicit mechanism (Step 1 in WLT) to explain how weak interactions would form such conditions. They used quantum field theory collective effects that create real, not virtual, neutrons.

Widom and Larsen envisioned the neutrons as significantly less energetic than any previously known neutrons – for example, "ultra-cold neutrons," having an energy less than 10^{-7} eV. The ultra-low-momentum neutrons, as Larsen calls them, have an energy of 10^{-10} eV or less, according to Larsen. He says that the neutrons are born with such low energy because they are formed collectively as opposed to being formed in two- or three-body reactions. To explain "collective effects," he provides the analogy of a flock of geese, versus two or three birds, traveling through a turbulent storm.

The third innovation to the WLT is the gamma to infra-red conversion process, which explains why there are little or no hard gammas emitted.

The theory is difficult for experimenters to test because neutrons with such low kinetic energy will never travel beyond the surface of the cathode and thus will never be directly detectable. However, they may trigger spallation neutrons and thus be responsible for small fluxes of more energetic neutrons, as in the Bhabha Atomic Research Centre experiments.

It is understandable that Widom and Larsen's competitors in the LENR field have been incredulous that WLT could have any, let alone full, viability. Widom and Larsen are outsiders. Widom probably has never set foot into a single ICCF conference. Larsen was previously known only as a business consultant and futurist. According to Barron's, he's had a track record of being dead-on with predictions. Perhaps some people in the LENR field have underestimated Larsen.

What must be excruciatingly annoying to the other LENR theorists, particularly those who have been struggling since 1989 and paid many dues, is that people outside the LENR community, particularly in various branches and agencies of the U.S. government, have taken a liking to WLT.

Months before Peter Hagelstein presented his "cold fusion" theory to the Department of Energy reviewers, Larsen had been invited to present at a Department of Energy/Electric Power Research Institute workshop.

Larsen and his colleagues, in their paper in the American Chemical Society *LENR Sourcebook* (Vol. 2)[6], mention that they have presented their work in a variety of places in Italy and India as well as "various U.S. government departments and agencies in Washington, D.C."

New Energy Times is aware of a few of these meetings. One was a Defense Threat Reduction Agency meeting on Dec. 12, 2006, in Ft. Belvoir, Va. Widom and Larsen were the only two speakers invited to present a theory of LENR.

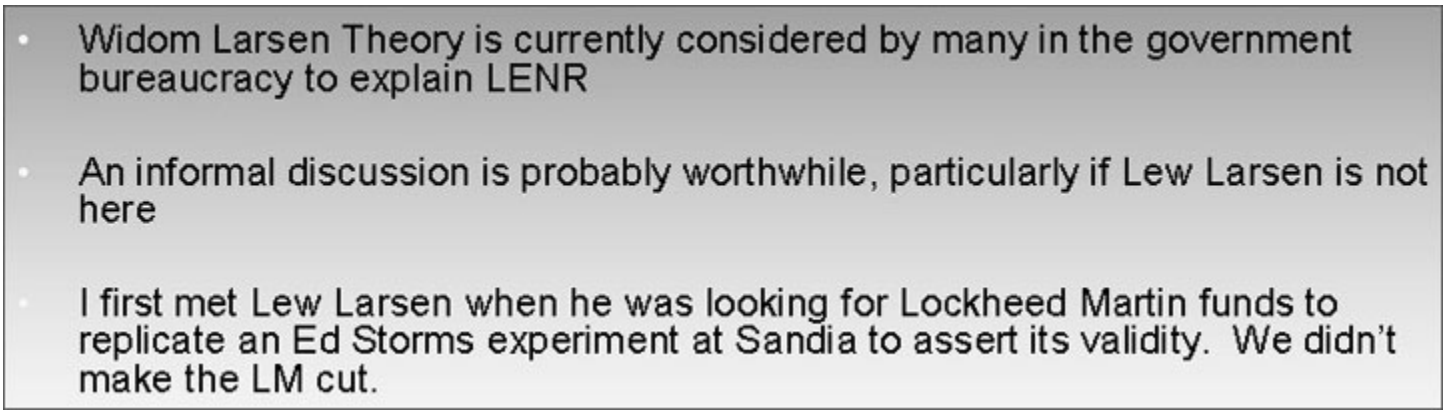
Neither Widom or Larsen, however, was invited to speak at a Defense Intelligence Agency workshop meeting on Aug. 4-5, 2009, at SPAWAR San Diego. Hagelstein, on the other hand, was invited and attended. Francis Tanzella, Michael McKubre, Mitchell Swartz, Pamela Mosier-Boss and Lawrence P.G. Forsley were also invited and attended.

Pat McDaniel (University of New Mexico, retired from Sandia) presented an analysis of the WLT at this meeting. McDaniel was concerned that the WLT was wrong but, more

important, that many people in the federal government are mistakenly accepting that it explains LENR.

New Energy Times spoke with McDaniel and two of the organizers after the meeting. They all knew in advance that Larsen had not been invited. And rather than cite Widom-Larsen's published theory in the DIA report, the authors of the report included only an obscure reference (#43) to unrelated work performed by Srivastava and Widom, without Larsen.

Such meetings help government officials understand science. This meeting did include some debate among the people who attended, according to one of the attendees. However, excluding Widom and Larsen from the opportunity to speak on their own behalf at this closed government meeting while including a critical review of their research by a third party was unprofessional.



Slide 2, bullets 1-3 from Pat McDaniel's Aug. 4, 2009 presentation

On the other hand, when the Army Research Laboratory held a workshop on June 29, in Adelphi, Maryland, Widom and Larsen were given a chance to discuss their theory. For unexplained reasons, however, Widom, who was scheduled to give the presentation, did not show up.

Hagelstein spoke at ARL about several theoretical aspects of LENR, but he did not present a theory that explains LENR.

Like Hagelstein, Yeong Kim, of Purdue, presented some mathematical ideas at ARL that he says can explain D+D "cold fusion." Kim finished his slide presentation with a cartoon depicting scientists at a chalkboard. Scribbled between two groups of calculations are the words "Then A Miracle Occurs." Kim's slide would be funny if it were not true.

Interestingly, Steven Koonin, then with Caltech and now Under Secretary for Science at the Department of Energy, and his colleague Michael Nauenberg with University of California, Santa Cruz, also had an idea—apparently intuitive—just two weeks after the Fleischmann-Pons announcement that preceded the Widom-Larsen heavy-mass electron idea.

On April 7, 1989, Koonin and Nauenberg—who is experienced in particle physics, condensed matter physics and astrophysics—electronically circulated a pre-print of their theoretical idea of "cold fusion." They postulated that local electrons with substantially enhanced masses would lower the barrier to fusion with screening. Apparently they only

considered strong interactions rather than an electron + proton (or electron + deuteron) weak interaction.

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12. Weak Interactions Are Not Weak Energetically

From the perspective of physics, four fundamental forces exist in our world: gravity, electromagnetism, strong interactions and weak interactions.

From birth, we learned about gravity; it became obvious the moment we tried to walk for the first time. Eventually, we were old enough to open the refrigerator door. It required a bit of a tug. And when we closed it, there was a bit of a snap when the door came within a few millimeters of the refrigerator frame. For those of us who liked to take things apart, we found that magnets lined both the door and the frame. Thus, we learned about electromagnetism.

People who went on to become nuclear engineers and physicists learned about strong-force interactions; these are the fundamental forces which enable nuclear fusion to occur or, conversely, they inhibit nuclear fission from occurring.

Weak interactions, though, are relatively new to science. Around 1968, physicists Sheldon Glashow, Abdus Salam and Steven Weinberg developed a broad theory of weak interactions at high energies – that is, weak-energy theory applied to high-energy nuclear research experiments. They won a Nobel Prize for their work in 1979.

But experimental verification of weak interactions didn't occur until the 1980s. Much of the related work took place at CERN, the European Organization for Nuclear Research. Carlo Rubbia, for example, former president of ENEA, the Italian National Agency for New Technologies, Energy and the Environment, won a Nobel Prize for related weak-interaction work in 1984.

When "cold fusion" came along in 1989, people were confused. It clearly wasn't strong-interaction fission. And it certainly seemed like a strange variant of thermonuclear fusion, also a predominately strong-interaction process.

Very few people who looked at "cold fusion" early on – proponents and skeptics alike – knew about weak interactions. Those people who did have some familiarity with weak interactions didn't consider that these processes might be fundamentally responsible for LENR phenomena. Before Allan Widom and Lewis Larsen released the pre-print of their LENR theory in 2005, most people thought that weak interactions had no capacity for significant energy release.

Larsen describes weak interactions simply as "any type of nuclear process that either emits or absorbs a neutrino," for example, beta decays or beta-delayed alpha decays. Weak interactions are not necessarily weak energetically, as he wrote in 2008 for the U.K. Institute of Science in Society[1]:

In the aftermath of World War II, Enrico Fermi’s beloved weak interactions became somewhat neglected. It was looked upon more as a scientific curiosity of theoretical interest with no practical applications. After all, every physicist and chemist “knows” that a) radioactive beta decay rates are mainly low-energy and, b) being random, they cannot be controlled; and hence c) they are useless for power generation applications.

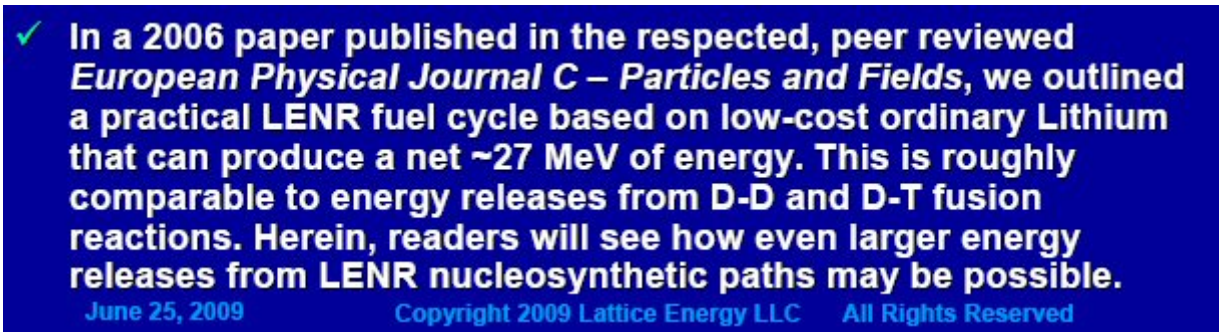
Also, no one considered the possibility of creating neutrons directly via the weak interaction; there just didn't seem to be any reasonable way to get weak interaction rates high enough to be useful. The Widom-Larsen theory of LENRs and hundreds of credible experiments have demonstrated otherwise.

Widom and Larsen applied the existing electro-weak theory of the Standard Model and added many-body collective effects.

Larsen explains that, "contrary to common belief, weak-interaction LENRs are not necessarily weak in terms of the total amount of energy released."

Widom and Larsen provide an example of a series of ultra-low-momentum neutron-catalyzed LENR processes that start with lithium, in which weak interactions are very important[2]:

Lithium-6 + 2 neutrons ⇒ 2 helium-4 + beta particle + neutrino + 26.9 MeV
This particular series can release about the same amount of energy as fusion reactions without creating any energetic neutrons, hard gamma radiation, or hot radioactive isotopes. While some of the 26.9 MeV in excess nuclear binding energy released is certainly lost to the neutrino, much of it remains in the kinetic energy of the two helium atoms (alpha particles) and beta particle.
Local solid matter is heated-up by the impacts of the alpha and beta particles; and heavy-mass electrons also convert any locally produced hard gamma or X-rays directly into infrared heat.



Source: June 25, 2009 Slide Presentation by Lewis Larsen

In another article he wrote for the Institute of Science in Society[3], Larsen explains the potential energy yield from LENR. According to the Widom-Larsen theory, fuels used in LENR have the capacity to release several thousand times more energy than gasoline does. The following text is a slightly edited version of what Larsen wrote in the article:

> The calculation of energy yield for a prospective LENR target fuel is straightforward. It involves two steps. The first is to decide which "base fuel" (hydrogen or deuterium) to use for producing LENR neutrons that can then react with a prospective target fuel. This choice determines the energy "cost" of the necessary neutrons. The second step is to calculate the nuclear [binding] energy released by a particular target fuel when it subsequently absorbs (captures) the

neutron(s).

It “costs” a minimum of 0.78 MeV of input energy to make a heavy-mass electron (e*) that has enough additional effective mass-energy to successfully react with a given “base fuel” such as hydrogen H = (p) or deuterium D = (p,n) to produce the ultra-low-momentum (ULM) neutrons (n). The required input energy that is “pumped” into the collective plasmon polariton electrons found on all metallic hydride surfaces can be in the form of an externally imposed electric current, laser pulse, injection of energy into a local magnetic field, pressure gradient across a metallic hydride “membrane,” and so on.

The minimum input energy “cost” to make a heavy electron (e*) is exactly the same, 0.78 MeV, regardless if H or D is used. While D is a much more expensive “base fuel” than is ordinary hydrogen, it is still an attractive option for many commercial LENR applications because it produces two LENR ultra-low-momentum (ULM) neutrons for the input energy “cost” of one[4], so the cost per ULM neutron is just 0.39 MeV for D instead of 0.78 MeV for H.

If, upon absorption of neutrons by a given target fuel atom, subsequent reactions release somewhat more (losses to neutrinos must be factored in) than 0.78 MeV (using H) or 0.39 MeV (using D) per neutron absorbed, then the LENR fuel burn-up process exceeds breakeven. That is, you are then ahead of the game, at least as far as the overall energetics is concerned.

The energy range of individual beta decays extends from keVs up to about 20 MeV for certain neutron-rich isotopes[5]; many potential LENR target fuel beta decay processes are well-above the breakeven point. The multi-step LENR reaction series is outlined in our 2006 paper.[1]

For a high-level overview of the full Widom-Larsen theory, please see "Widom-Larsen Theory Simplified" in this special report.

Widom and Larsen are not the only LENR theorists familiar with the potential power of weak interactions.

Xing Zhong Li, professor emeritus from Tsinghua University in China, with a long career in nuclear physics behind him, stated as much in a paper about his own theory, a resonant tunneling model.[6]

"The weak interaction does not mean a weak power source," Li wrote. "Even if the lifetime is as long as 10^4 seconds (~3 hours), a cubic centimeter of palladium might produce megawatts of thermal power. Its power density is higher than that of the fuel rod in a fast fission reactor."

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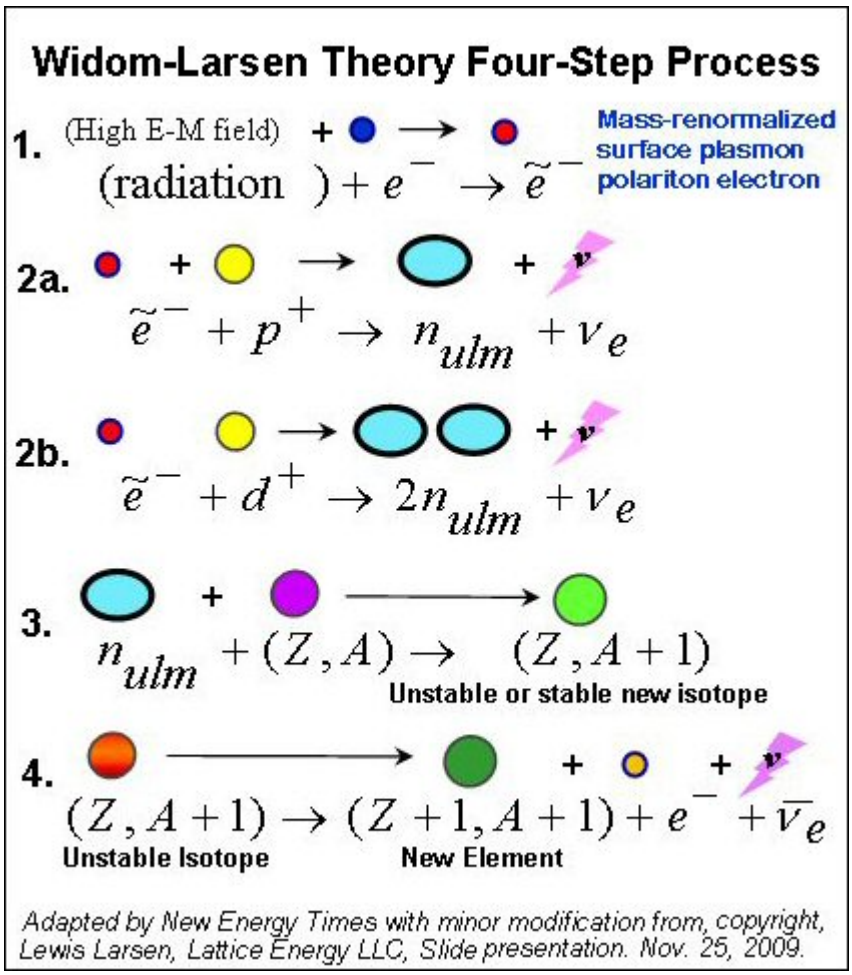
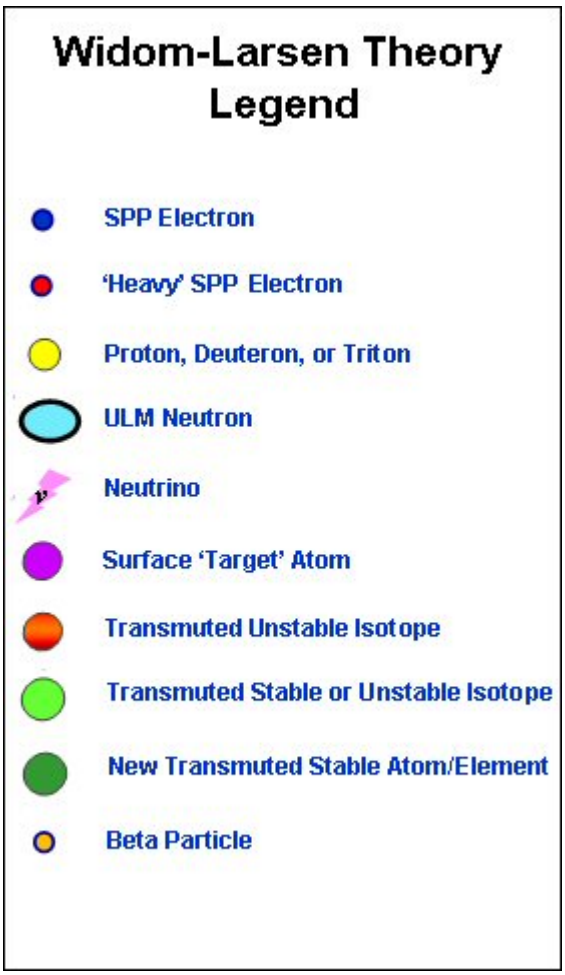
13. Widom-Larsen Theory Simplified

For several years, Lewis Larsen has been prolific in his written communications about his and Allan Widom's theory of ultra-low-momentum, neutron-catalyzed, low-energy nuclear reactions. Most of Larsen's science papers, news articles, and slide presentations are listed on the *New Energy Times* Widom-Larsen Theory Portal.

This article will present the simplest possible overview of the Widom-Larsen theory of LENRs. In order to keep this article as concise as possible, I will begin it at a level that assumes readers have some basic understanding of nuclear physics. This article will also go relatively quickly from simplicity to a highly complex overview of the potential implications and applications of not only the Widom-Larsen theory but also of LENR.

The Widom-Larsen ultra-low-momentum neutron-catalyzed theory of LENRs involves four fundamental steps, as shown graphically in Larsen's image below:





Summaries of the four steps follow:

1. Creation of Heavy Electrons
Electromagnetic radiation in LENR cells, along with collective effects, creates a heavy surface plasmon polariton (SPP) electron from a sea of SPP electrons.

2. Creation of ULM Neutrons
An electron and a proton combine, through inverse beta decay, into an ultra-low-momentum (ULM) neutron and a neutrino.

3. Capture of ULM Neutrons
That ULM neutron is captured by a nearby nucleus, producing, through a chain of nuclear reactions, either a new, stable isotope or an isotope unstable to beta decay.

A free neutron outside of an atomic nucleus is unstable to beta decay; it has a half-life of approximately 13 minutes and decays into a proton, an electron and a neutrino.

4. Beta Decay Creation of New Elements and Isotopes
When an unstable nucleus beta-decays, a neutron inside the nucleus decays into a proton, an energetic electron and a neutrino. The energetic electron released in a beta decay exits the nucleus and is detected as a beta particle. Because the number of protons in that nucleus has gone up by one, the atomic number has increased, creating a different element and transmutation product.

In the graphic above, step 2 is listed twice: 2a depicts a normal hydrogen reaction, 2b depicts the same reaction with heavy hydrogen. All steps except the third are weak-interaction processes. Step 3, neutron capture, is a strong interaction but not a nuclear fusion process. (See "Neutron Capture Is Not the New Cold Fusion" in this special report.)

Given that the fundamental basis for the Widom-Larsen theory is weak-interaction neutron creation and subsequent neutron-catalyzed nuclear reactions, rather than the fusing of deuterons, the Coulomb barrier problem that exists with fusion is irrelevant in this four-step process.

The most unusual and by far the most significant part of the Widom-Larsen process is step 1, the creation of the heavy electrons. Whereas many researchers in the past two decades have speculated on a generalized concept of an inverse beta decay that would produce either a real or virtual neutron, Widom and Larsen propose a specific mechanism that leads to the production of real ultra-low-momentum neutrons.

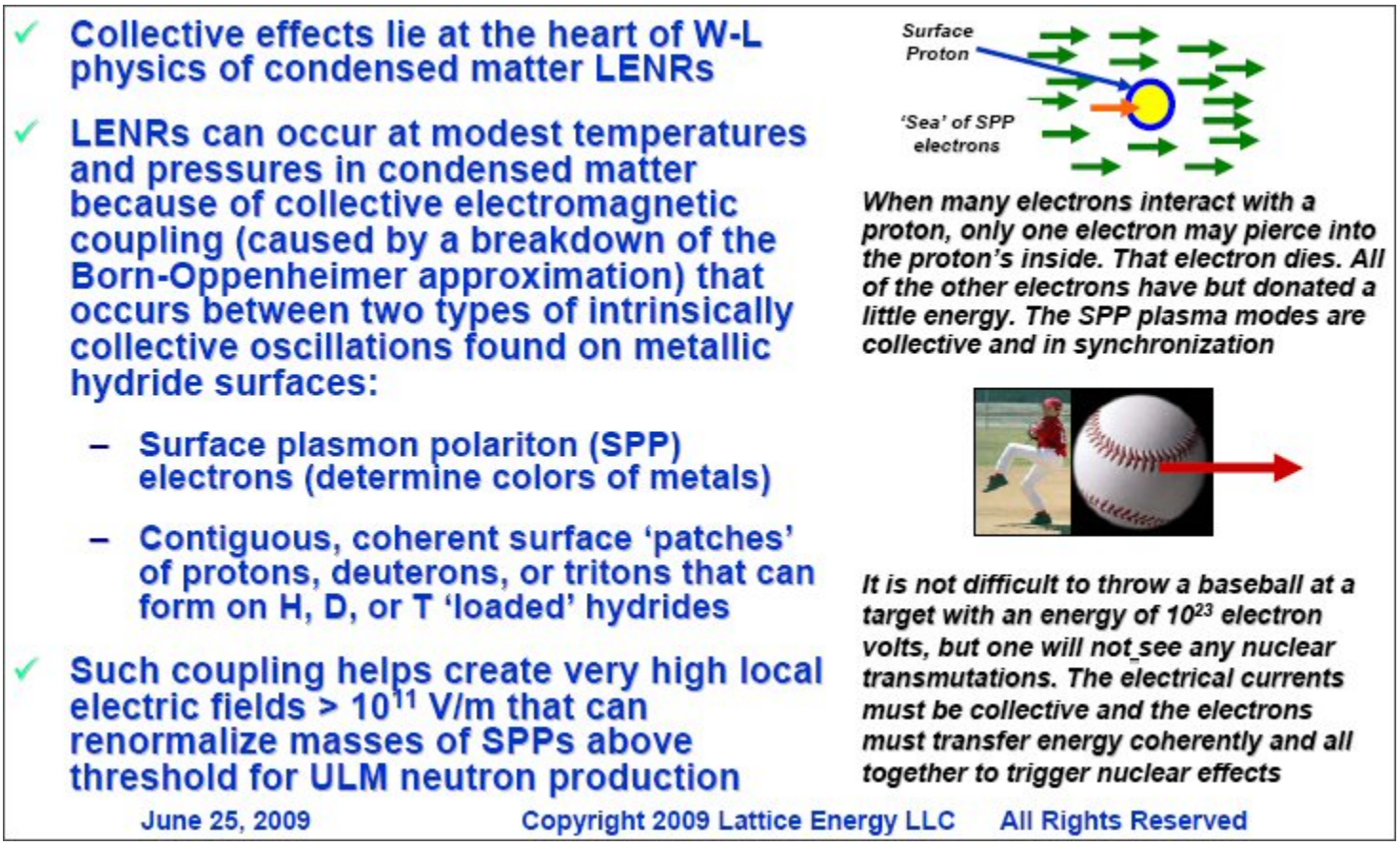
Larsen also provides a more technical explanation of the four steps in his slides:

1. E-M radiation on metallic hydride surface increases mass of surface plasmon electrons
2. Heavy-mass surface plasmon polariton electrons react directly with surface protons (p^+) or deuterons (d^+) to produce ultra low momentum (ULM) neutrons (n_{ulm} or $2n_{ulm}$, respectively) and an electron neutrino (ν_e)
3. Ultra low momentum neutrons (n_{ulm}) are captured by nearby atomic nuclei (Z, A) representing some element with charge (Z) and atomic mass (A). ULM neutron absorption produces a heavier-mass isotope (Z, $A+1$) via transmutation. This new isotope (Z, $A+1$) may itself be a stable or unstable, which will perforce eventually decay
4. Many unstable isotopes β^- decay, producing: transmuted element with increased charge ($Z+1$), ~ same mass ($A+1$) as 'parent' nucleus; β^- particle (e^-); and an antineutrino

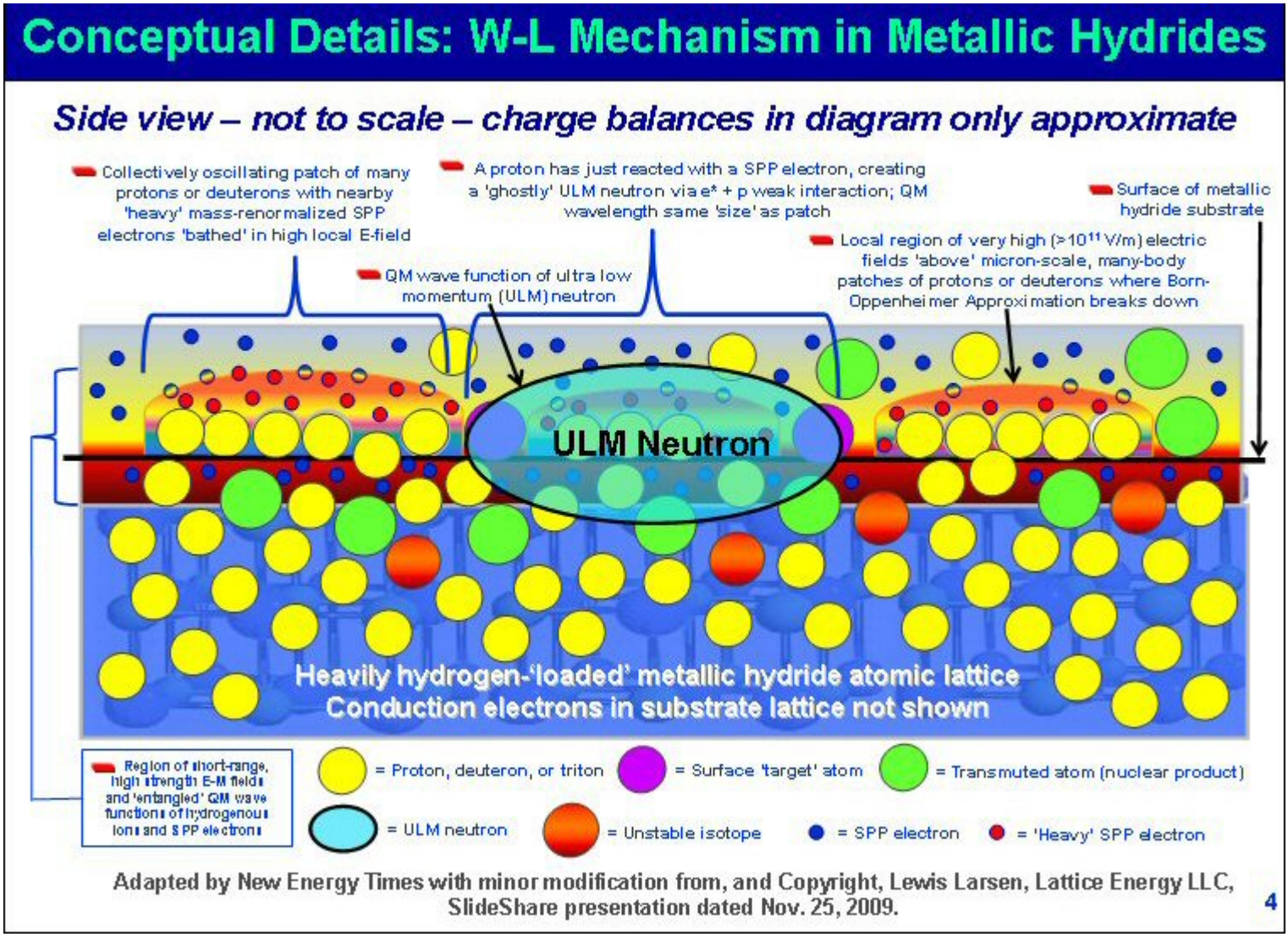
At the heart of the mechanism for step one is a new understanding of the behavior of collective effects in LENR.

As a metaphor, Larsen discusses how a flock of geese can travel through turbulent storms while two or three geese alone would not get through. The geese take advantage of the collective effects of, among other things, their group aerodynamics.

In a similar way, Larsen envisions a sea of surface plasmon polariton electrons working collectively on the surface of metallic hydrides in LENRs to create heavy SPP electrons. His slide below provides more detail.



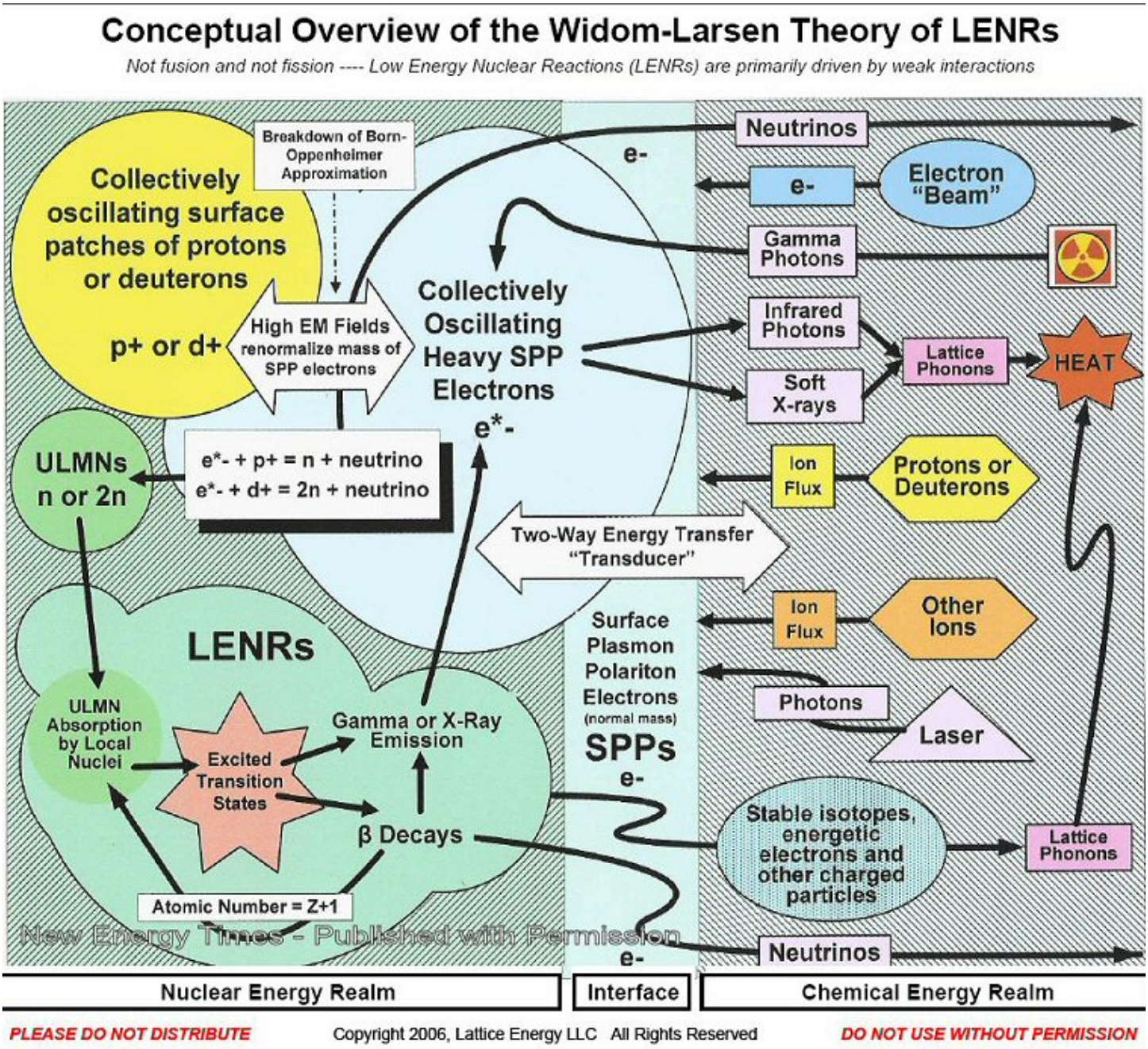
Larsen also created a graphical representation of the collective effects working at the atomic scale. (Click the image for a larger version of the slide.)



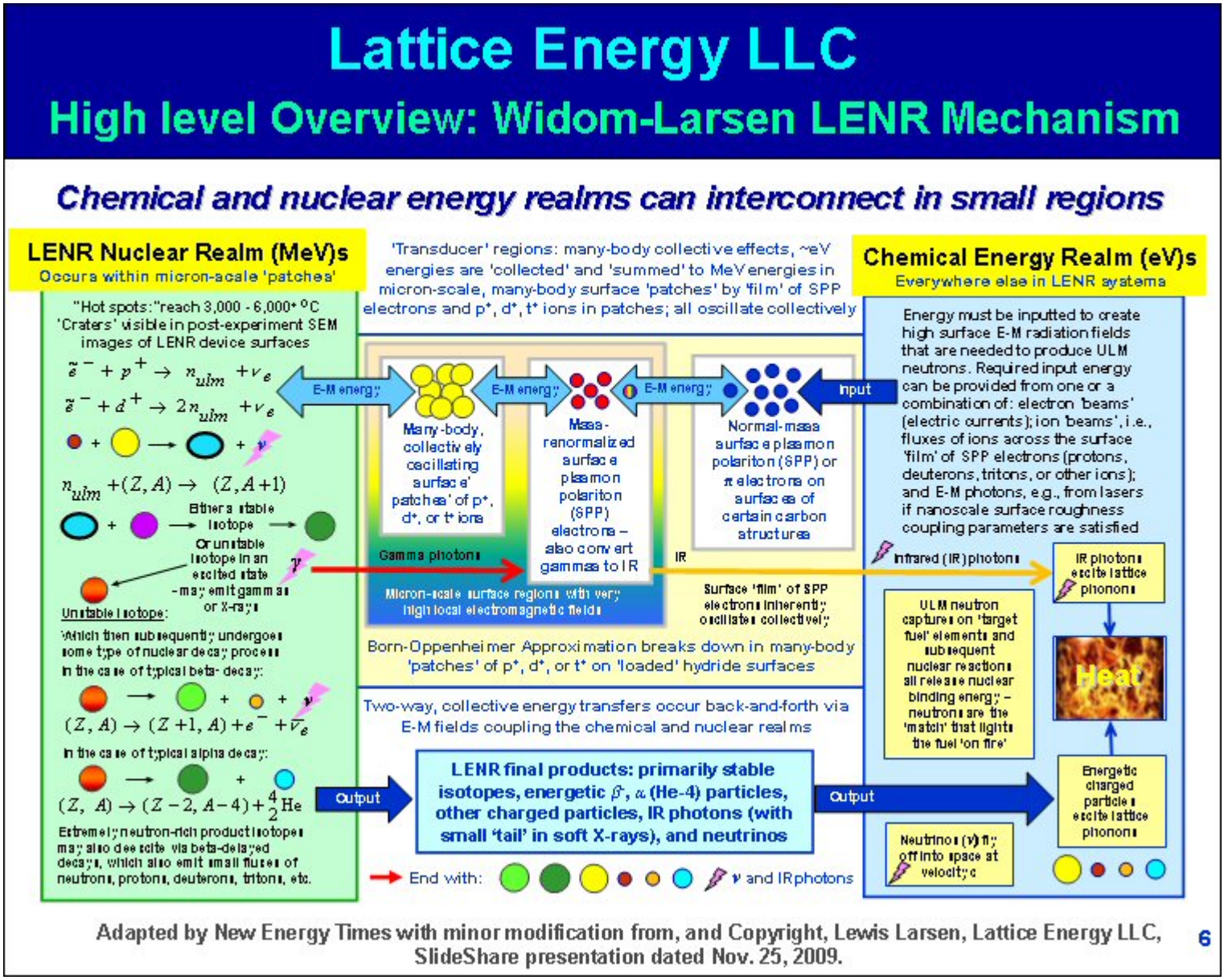
Now let's broaden the picture and take a wider view of LENR from Larsen's perspective. In 2006, Larsen created a visual diagram, "Conceptual Overview," of LENR. In this single slide, he shows the wide variety of inputs, outputs and processes in LENR systems.

The four-step mechanism, according to the theory, can explain the production of most of the LENR phenomena that have been reported for the last two decades: helium-4, helium-3, tritium, heavy element transmutations and much more.

Most important, the slide shows how the chemical realm interfaces with the nuclear realm. In essence, his graphic explains why chemists, surprisingly, were able to create nuclear reactions in ordinary, room-temperature environments. (Click on the image below for a larger version.)



In 2009, Larsen refined his "Conceptual Overview" into a more-detailed "High-Level Overview," shown below. However, the 2006 graphic still has a distinct advantage in that it is more graphical than textual and provides easier comprehension for visually oriented readers. (Click on the image below for a larger version.)

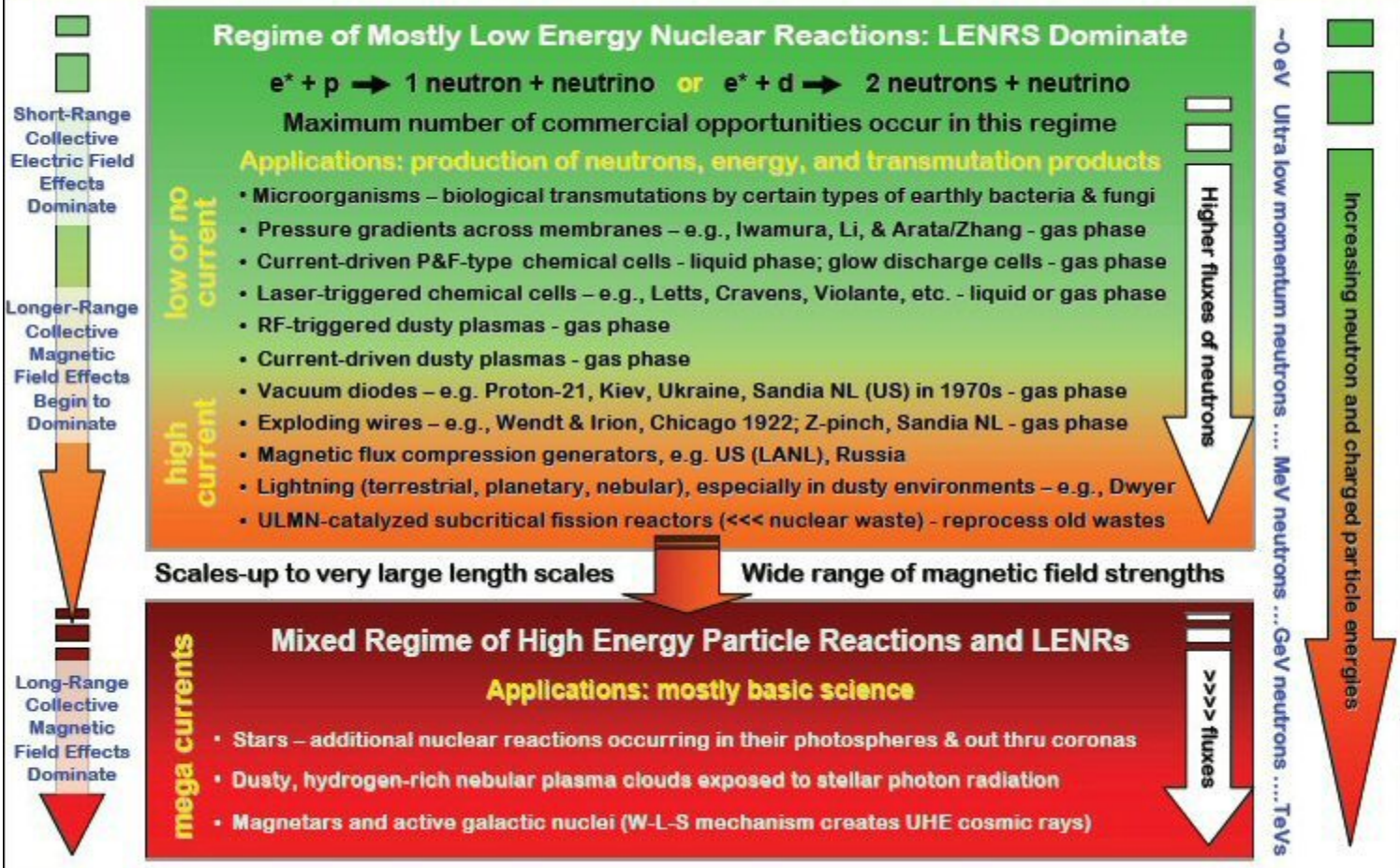


According to the Widom-Larsen theory, no large fluxes of high-energy neutrons will be emitted from LENR cells – exactly what has been observed for two decades. Nearly all the ULM neutrons are absorbed locally because they do not have enough energy to travel beyond the immediate reaction site.

The theory also proposes that lethal photon radiation (gamma radiation), normally associated with strong interactions, is internally converted into more-benign infrared (heat) radiation by electromagnetic interactions with heavy electrons. Again, for two decades, researchers have seen little or no gamma emissions from LENR experiments.

Last, for readers who are ready for an even larger picture of the potential domain and application of weak interactions coupled with collective effects, Larsen provides this mind-boggling image showing applications that range from micro-organisms all the way to stars. (Click the image below for a larger version.)

W-L and Srivastava – Standard Model + collective effects



14. Larsen's Vision of LENR Technology

Lewis Larsen, co-developer with Allan Widom of the Widom-Larsen theory of LENRs, has a vision for the potential of low-energy nuclear reactions.

When Larsen began looking into LENR in the mid-1990s, he noticed that, scientifically, it had very attractive features. Whereas conventional nuclear fission technology has complex hurdles to overcome with radiation emissions, massive and costly shielding and containment structures, not to mention waste disposal requirements, LENRs have virtually none of these problems.

Thermonuclear fusion technology, whenever it becomes technically and economically feasible, is expected to have some of the same problems as fission but to a lesser degree.

The key difference is that, unlike fission and fusion, which principally release energy through strong interactions, LENRs release energy through a mixture of weak and strong interactions, along with a mechanism that suppresses gamma radiation by converting it to infrared photons. (For more information on weak interactions, see *New Energy Times* article "Weak Interactions Are Not Weak Energetically.")

The strong interaction processes in fission and fusion are also responsible for the creation of the waste material with long-lived radioactive isotopes that presents environmentally sound storage challenges.

Larsen wrote six articles for the Institute of Science in Society between November 2008 and January 2009 that are excellent entry points for the layperson.

The articles explain Larsen's vision of the potential practical applications of LENR technology. They also provide excellent comparisons between LENR and conventional nuclear fission energy. The articles can be accessed free online at the following Web addresses:

1. *Low Energy Nuclear Reactions for Green Energy* – Nov. 13, 2008
2. *Widom-Larsen Theory Explains Low Energy Nuclear Reactions & Why They Are Safe and Green* – Dec. 4, 2008
3. *Portable and Distributed Power Generation from LENRs* – Dec. 10, 2008
4. *LENRs for Nuclear Waste Disposal* – Dec. 11, 2008
5. *Safe, Less Costly Nuclear Reactor Decommissioning and More* – Jan. 26, 2009
6. *LENRs Replacing Coal for Distributed Democratized Power* –Jan. 27, 2009

Larsen has also been producing a series of highly technical slide presentations since January 2009. *New Energy Times* lists these on our Widom-Larsen Theory Portal. In his Jan. 30, 2009, slide presentation, however, he provides a non-technical perspective on his views of early practical development of LENR technology.

Larsen characterizes early LENR products as "small, long-lived power generation systems." Based on his understanding of the potential of LENR, Larsen envisions several phases.

He foresees the first phase as follows:

- Initial products with custom form-factors will target high performance, mission critical applications in military and civilian markets that are not price-sensitive to cost of power source. For example, military and police emergency radios, small portable electronic devices, and small stand-alone, off-grid distributed stationary power generation systems. Production in low to medium unit volumes.
- Power output of market-entry systems will probably be in the 10s of Watts for battery-like devices to several kilowatts for stationary systems, all having duty cycles of at least 500-1,000 hours at full output capacity. This would provide greater than 10 times the performance of chemical batteries.

Larsen sees the second phase as follows:

- Over time, manufacturing cost curve drops, similar to development in microprocessors, memory chip, personal computer and cell phone technology.
- Production in this phase is characterized by larger unit volume and much more price-sensitive market applications as the manufacturing experience accumulates and costs drop. For example, battery-like power sources for commodity mobile phones.
- High-volume market applications for LENR-based battery-like form factors could vastly reduce manufacturing costs of LENR power generation systems. Continuing economies of scale ride down the cost curve, displacing competing technologies.

Over time, Larsen sees a third phase in which the total output capacity of LENR power sources increases, further disrupting and displacing prevailing technologies:

- As manufacturing costs drop and applications proliferate, total electrical power output of LENR-based power generation systems could be scaled up dramatically, ultimately reaching 100s of kilowatts — enough to power some types of motor vehicles, aircraft (Unmanned Aerial Vehicles), and smaller commercial buildings.
- Similar to the market penetration of personal computers and mobile phones, use of progressively less expensive, LENR-based distributed power systems could spread rapidly worldwide, especially to rural areas where an electrical power grid is either absent, uneconomic, or unreliable. LENR-based power devices could also eventually displace internal combustion engines.

Larsen envisions LENR technology improving the quality of life for billions of people.

15. Development of the Widom-Larsen Theory of LENRs

With this article, *New Energy Times* reveals the history of the Widom-Larsen theory of low-energy nuclear reactions.

Both Allan Widom, a condensed matter physicist with Northeastern University, and Lewis Larsen, president and chief executive officer of Lattice Energy LLC, have been reluctant in the past to speak with the media about the development of their theory.

Widom has declined repeated phone and e-mail requests from *New Energy Times*, but Larsen has agreed to speak with us.

He has an undergraduate degree in biochemistry and a master's degree in business from the University of Chicago. As an undergraduate, he audited courses in astrophysics under Nobel Prize-winning physicist Subrahmanyan Chandrasekhar.

Larsen enrolled in a doctoral program in theoretical biophysics at the University of Miami until a block grant from the Atomic Energy Commission was terminated. He had finished his coursework and was working on his dissertation. Later on, he worked as a financial and securities analyst and was cited by Barron's as a "futurist" with a "dead-on prediction" about a "coming technology revolution."

He told *New Energy Times* about his early involvement in LENR.

"In 1997, I was running a technology consulting company," Larsen said, "and one of our clients asked, 'Are there any wild cards in energy?' We were working in energy and information management and control systems.

"I remembered the cold fusion controversy from 1989, so I went looking for a scientist who was with a major university, had a decent reputation in thermonuclear fusion research and who was working in LENR. Through the early Internet search engines, I found George Miley at the University of Illinois at Urbana-Champaign.

"When I saw the 1996 Miley transmutation research I remembered an elemental abundances chart I had seen 30 years earlier when I was studying astrophysics," Larsen said. "I recognized similar peaks and abundances, and began to suspect that LENRs were neutron-catalyzed reactions, just like similar processes in stars."

Despite his fascination, he approached the work cautiously, and in 1998, he sought the independent opinions of two top nuclear physicists in the U.S. He asked them whether Miley's data were accurate and appeared to reflect a genuine nuclear process. Both said yes.

Around 1998, Larsen saw a very similar five-peak elemental spectrum from Japanese LENR researcher Tadahiko Mizuno (see "Who's Afraid of LENR Transmutations?" in this special report).

"Miley's transmutation experiments and Mizuno's showed different relative sizes of the peaks in different parts of the mass spectrum due to different seed elements initially present in the electrolyte," Larsen said. "However, the most amazing thing was that Mizuno's experiments were with heavy water and Miley's were with light water.

"At that point, I knew the coincidence could not be just accidental. I knew then that the heavy-water and light-water results were part of the same phenomenon.

"I also knew that neutrons were the key; I had a conceptual understanding of the theory worked out by then but not the precise details of how the neutrons were formed."

Larsen started Lattice Energy LLC in 2000 and conceived the idea of ultra-low-momentum neutrons.

"I realized they had to be ultra-low-momentum neutrons," Larsen said, "because researchers saw the results of transmutations but they never saw the neutrons, aside from, possibly, spallation neutrons. I also knew there had to be some kind of gamma conversion mechanism. Somehow, the electrons were suppressing the gamma radiation."

In May 2001, he received his first seed funding, and during Thanksgiving week that year, he had his first telephone meeting with government scientists. This was the first step in his outreach to several national laboratories.

Larsen said that he and Widom have participated in numerous private federal government meetings and briefings. He said that federal agencies have been interested in the theory for a variety of reasons. However, he has not received federal funding.

"They sucked all kinds of information out of us, but they never gave us a dime and never proposed any significant funding," Larsen said.

The next insight for Larsen came in 2002. In June, Japanese LENR researcher Yasuhiro Iwamura, with Mitsubishi Heavy Industries, published new research showing LENR transmutations.

Several years earlier, Iwamura had filed a U.S. patent and hypothesized how a neutron-based mechanism might be responsible for the LENR transmutations, but Iwamura did not have a specific mechanism to account for the formation of the neutrons.

Larsen also learned that Mizuno had recognized the potential viability of weak interactions, as Mizuno published in his 1997 book.

"Sometime around July 2002," Larsen said, "Miley had just returned from the ICCF-9 conference in China, and he showed me a copy of the Iwamura paper. He asked me if I believed it and if I thought the data was correct. I responded, 'Yes, I'm very confident,' without explaining to Miley why I was so confident.

"By that time, I had already performed my independent due diligence on Miley's transmutation data. He thought his five-peak transmutation spectrum was the result of a fission process."

In the summer of 2003, before the ICCF-10 conference, Larsen learned about the successful laser triggering and gold surface treatment reported by U.S. LENR researchers Dennis Cravens and Dennis Letts. He also heard claims that the Letts-Cravens effect had been replicated by LENR researchers Edmund Storms and Michael McKubre.

"I then knew LENR had to be primarily a surface effect and not a bulk reaction. I knew that electron capture (electron + proton) played a key role in producing neutrons and neutrinos," Larsen said. "I knew that surface plasmons were key because gold loves to form surface plasmons. This helped me to identify which electrons were participating with the protons/deuterons to make neutrons via a collective weak interaction.

"I had all the concepts put together, but I needed an academic collaborator who was well-published and who had the physics and calculation skills necessary to help me complete the development of the theory. I hadn't done calculations like this for many years."

"Around March or April 2004," he said, "I began to look for a theoretical physicist with strong experience in many-body collective effects, quantum electrodynamics and condensed-matter physics. I knew that these were the required disciplines, but they are a rare combination. Eventually – it took me a while – I found Widom. Coincidentally, he was also a close personal friend of Giuliano Preparata, a well-known LENR theorist.

"Widom had had no involvement or interest in LENR at the time. He was skeptical but was willing to look at the experimental evidence and consider my theoretical concepts. Together, we reviewed hundreds of papers dating back many decades, all the way back to 1922; it took us about six months.

"We found that the important work had happened outside the U.S. The researchers overseas had much more open minds. Together, we worked out the remaining details of the physical and mathematical mechanisms."

At the same time, Larsen learned that someone else also seemed to be closing in on the idea.

"In October 2004, I was sitting in the audience at the ICCF-11 conference in Marseille, France, and all of a sudden I heard Vittorio Violante [ENEA Frascati] start talking about surface plasmons," Larsen said. "I nearly had a heart attack. I thought Violante was going to publish the details of the mechanism before we did."

On May 2, 2005, Larsen and Widom placed their concept in the public domain by uploading the pre-print of their first paper to the arXive server. They decided that Widom would be the senior author. The first paper published on March 9, 2006, in *European Physical Journal C – Particles and Fields*.

The pair submitted several other papers for publication. They later brought in Yogendra N. Srivastava for his expertise in collective magnetic phenomena and Standard Model high-energy particle physics.

They placed their "primer" paper on the arXive server in 2008. It was peer-reviewed, accepted and published by the American Chemical Society in 2009. An expanded version of

the paper has been accepted for publication in 2010 by *Pramana Journal of Physics*, a refereed publication of the Indian Academy of Sciences.

Larsen, Widom and Srivastava ceased active collaboration in October 2008. Larsen then developed and extended the theory to carbon fullerenes and aromatic rings.

"Ironically," Larsen said, "many reviewers take a quick look at our work and think we are proposing 'cold fusion.' Of course, nothing could be further from the truth."

16. Reader's Guide to Larsen Slide Presentations

Lewis Larsen, co-developer of the Widom-Larsen ultra-low-momentum neutron (ULMN) theory of LENRs, has been prolific in his Web-based slide presentations. I have reviewed his first six presentations and present below a very brief, subjective guide to help readers navigate efficiently. Larsen has not identified sequence numbers for his slides (we have enumerated them in our listing); nor has he given concise titles for his slides. This review, as well as our Web portal, should help readers navigate Larsen's slides.

In many cases, his later presentations include information repeated from earlier presentations or repeated with improved text and graphics. For this reason, new readers are advised to begin with Slide Presentation #4.

Slide Presentation #1, Jan. 30, 2009 (19 pages)

- Is relatively low-tech, not too heavy on science.
- Provides a business research and development perspective.

Slide Presentation #2, Feb. 07, 2009 (6 pages)

- Introduces general concept of nucleosynthesis; however, technical explanations of ULMN are covered better in presentation #3.
- Introduces nucleosynthesis in stars. Nucleosynthesis in LENR is covered in depth in presentation #5.

Slide Presentation #3, Feb. 14, 2009 (24 pages)

- Covers some historical background of Widom-Larsen theory and LENR field.
- Discusses D+D "cold fusion" theory presented by Peter Hagelstein to Department of Energy in 2004.
- Discusses 2004 Department of Energy LENR report.
- Slide #20 is unique; content does not appear in any of Larsen's other slides. Provides single-page visual overview of Larsen's conceptual model of LENRs.
- Slides #21 and #22 are unique; content does not appear in any of Larsen's other slides. These slides portray how LENR relates to broad spectrum of new and existing science.

Slide Presentation #4, June 25, 2009 (78 pages)

- Is a massive, encyclopedic presentation. It provides not only a valuable educational tool for Widom-Larsen theory but also a tool to better understand weak interactions in general.
- Slides #14-26 provide deep technical explanation of Widom-Larsen theory.
- Slide #32 dispels the myth that the only way helium-4 can be produced in LENR is through D+D "cold fusion."
- Slide #38 gives perspective on distinct signatures of fission missing from LENRs.

Slide Presentation #5, Sept. 3, 2009 (65 pages)

- Is another very scientifically dense, heavy technical presentation.
- Slides #10-18 go deeply into nucleosynthesis, introduce graphical display of diverse array of some of the possible weak interactions in LENR.
- Slides #19-37 examine SRI International replication of Lester Case experiment and mystery of disappearing helium-4.
- Slide #23 reveals danger of taking too narrow a view of data, omitting larger context.

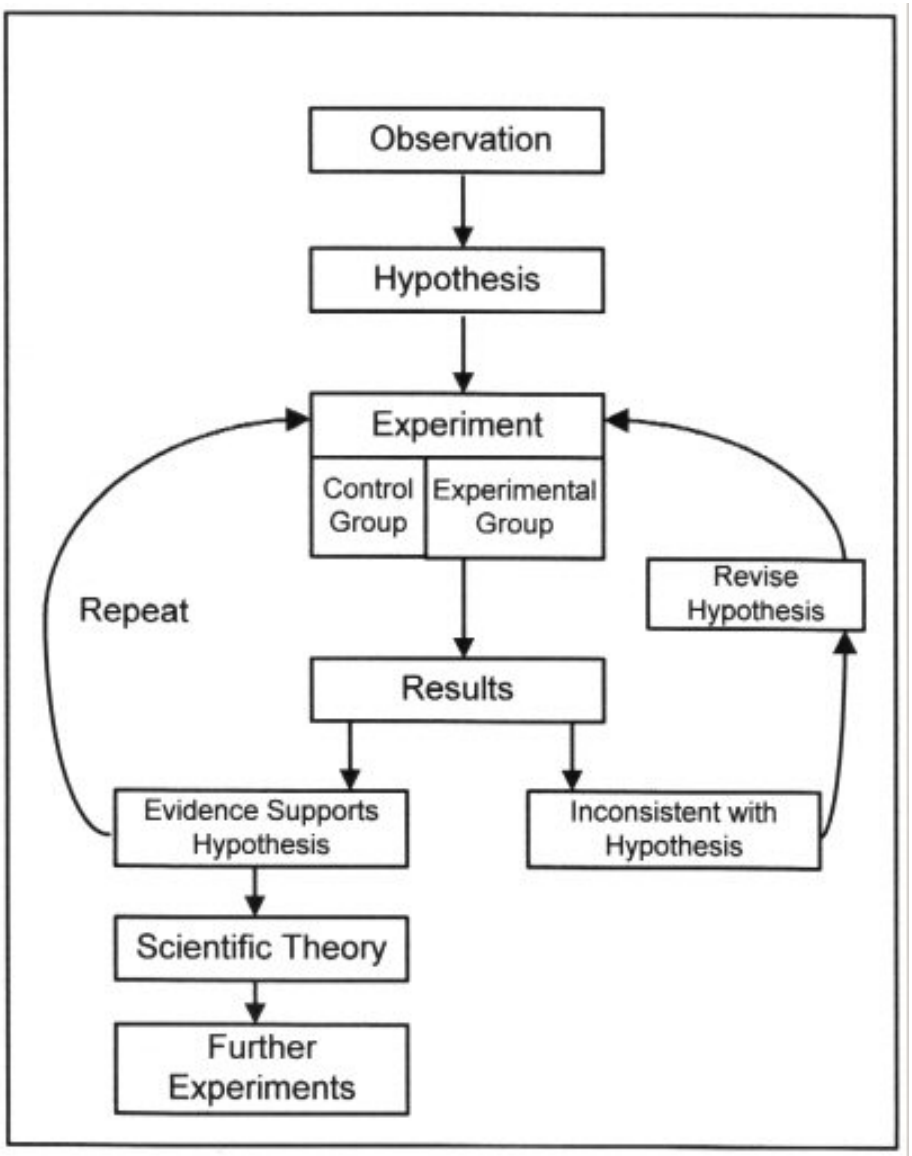
Slide Presentation #6, Nov. 25, 2009 (61 pages)

- Slide #16 introduces best (so far) visual representation of four-step WL process.
- Slide #17 introduces excellent visual representation of collective effects in LENR.
- Slide #18 introduces high-level overview of WL process. It is similar in concept to slide #20 from presentation #3.
- Slides #26-40 discuss Tadahiko Mizuno's LENR experiments with phenanthrene.

17. The Cold Fusion Belief System

For two decades, "cold fusion" researchers have at times been accused of being "true believers." Has this been a fair critique?

The first step in the scientific process begins with "observation," which leads to "hypothesis." It does not begin with "belief"; nor is "belief" part of the process.



Scientific Process (Method.) Diagram provided courtesy of William S. Gaud, Professor Emeritus, Department of Biological Sciences, Ph.D. University of North Carolina, based on an original diagram by Wendy Gorman.

The D+D "cold fusion" belief system can best be understood by a quote from LENR-CANR.org librarian and former computer programmer Jed Rothwell to *New Energy Times* on Dec. 29, 2009.

"In the case of cold fusion," Rothwell wrote, "we know the reaction does not happen without deuterium and a lattice, and we know it produces helium. The only thing in the

system that can convert to helium is deuterium, and the ratio of heat to helium is close to that of plasma fusion, so that has to be it."

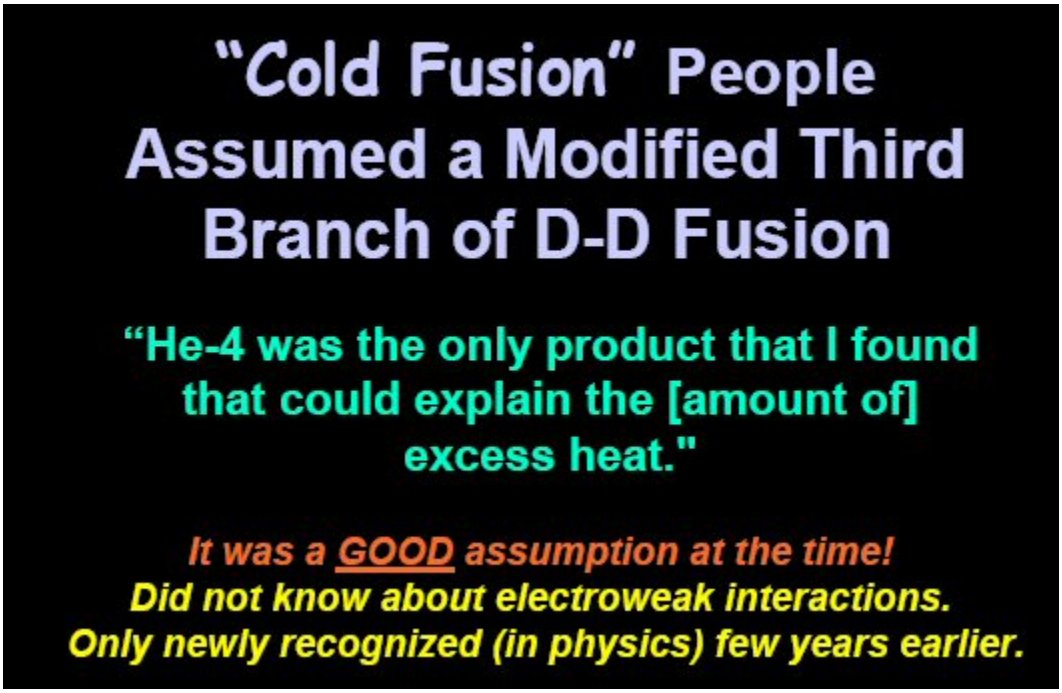
To be fair, "cold fusion" discoverer Stanley Pons, as early as April 1989, suggested that the amount of helium-4 produced in his and his colleague Martin Fleischmann's fusion experiment agreed roughly with the thermonuclear fusion branch which releases 24 MeV of energy through gamma radiation.

But that was 21 years ago: long before the many experiments attempting to confirm the possible relationship between heat and helium-4; long before the experiments revealed a very broad energy range (12-89 MeV); long before *New Energy Times* exposed the problems with SRI electrochemist Michael McKubre's "M4" experiment.

Still, Rothwell can be faulted only for unquestionably repeating the D+D "cold fusion" mantra from MIT electrical engineering professor Peter Hagelstein, who repeated the assertion of 24 MeV as recently as March 21, 2010.

In the text he provided for the American Chemical Society's "cold fusion" press release, Hagelstein stated that, "in the experiments, a large amount of energy is produced and He-4 is observed, with about 24 MeV of energy measured per helium atom detected."

Melvin Miles, a meticulous electrochemist and as straight a shooter as scientists come, told me that "helium-4 was the only product that [he] found that could explain the [amount of] excess heat."

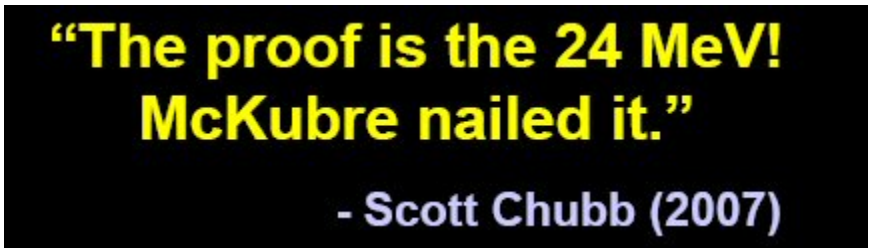


Slide #3 from Krivit presentation prepared for but not presented at American Chemical Society meeting in San Francisco, Calif.

And it was indeed a good assumption at the time, because few people, particularly electrochemists, had any idea about electroweak interactions.

The 24 MeV Myth

For a while, for some people, D+D "cold fusion" seemed to be real, based on the so-called correlation of 24 MeV of heat to helium-4 and the neglect of all other nuclear products in the LENR cell.



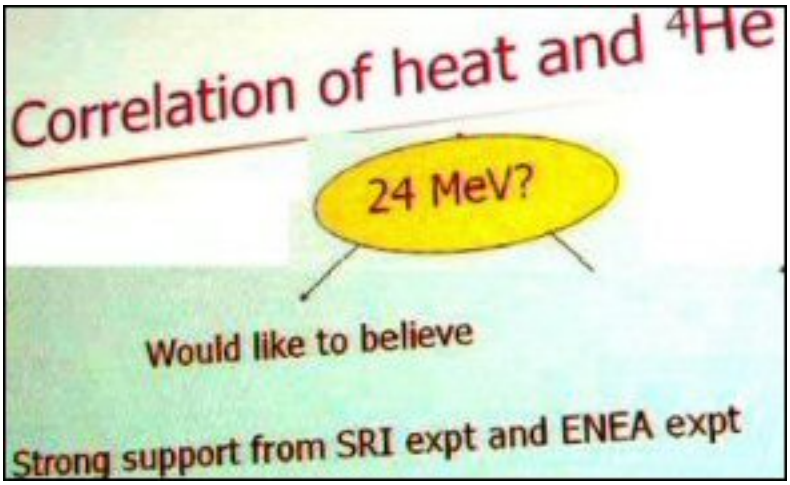
Scott Chubb told *New Energy Times* that McKubre proved "cold fusion."

On Jan. 29, I published *New Energy Times #34* and exposed the flaws in McKubre's 24 MeV claim. McKubre's only response was to inform my board of directors that I had made serious errors, misrepresentations and omissions although he failed to identify a single example.

Two months later, on March 21, despite the exposure of the flaws with the 24 MeV claim, Hagelstein spoke at the American Chemical Society "cold fusion" press conference about 24 MeV as if nothing had changed.

"[There are] a number of experiments – more than 10 experiments where people have seen that kind of thing – and there's two measurements where the correlation shows a Q-value or an energy per helium-4 of about 24 MeV," Hagelstein said.

Two years earlier, in August 2008, he told the ICCF-14 audience that he would "like to believe" that the energy release was 24 MeV.



Slide from Hagelstein presentation at ICCF-14 in Washington, D.C.

In October 2008, *New Energy Times* published an extensive review of the 24 MeV belief, held by Hagelstein and several other LENR researchers. Hagelstein did not admit his belief in 24 MeV publicly after that.

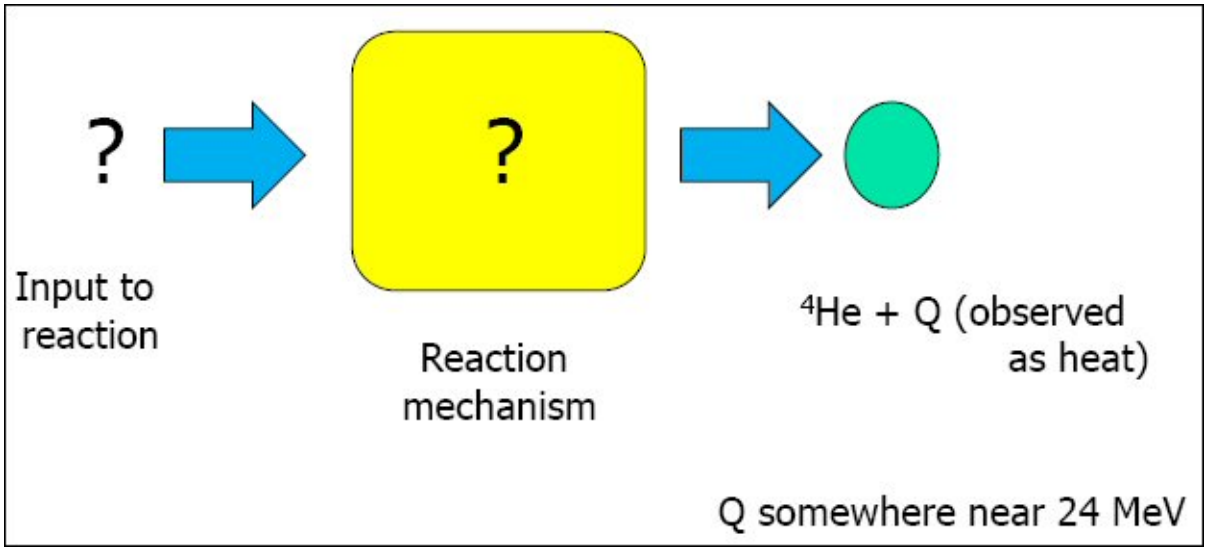
On March 21, I had the opportunity to ask McKubre face to face for an explanation of his claims of 24 MeV. He gave his response in a live Webcasted press conference hosted by the American Chemical Society. The details of this matter are covered in depth in "When Nuclear Is Not Enough: A Tangled Tale of Two Experiments" in this special report.

A day later, on March 22, McKubre's proof disappeared silently at 4:29 p.m. when he concluded his presentation at the American Chemical Society meeting in San Francisco, Calif. For the first time in a decade, McKubre failed to mention his D+D "cold fusion" 24 MeV/4He atom claim, let alone the idea of heat and helium-4 correlation.

McKubre's colleague Vittorio Violante, of ENEA Frascati, also presented at ACS in San Francisco. Violante allegedly had independently replicated and confirmed the 24 MeV claim just two months before the 2004 Department of Energy LENR review organized by McKubre and his "cold fusion" faction colleagues. Violante's ACS presentation also, for the first time in six years, didn't mention the D+D "cold fusion" 24 MeV.

When McKubre spoke at the June 29 Army Research Laboratory workshop on LENR, the 24 MeV claim was not mentioned, defended or otherwise explained.

When Hagelstein spoke at ARL, he adjusted his representation of the 24 MeV from "about 24 MeV" to "somewhere near 24 MeV." It is still questionable whether the 12-89 MeV reported by *New Energy Times* is "somewhere near" 24 MeV.



Hagelstein slide #3 from Army Research Labs LENR Workshop, June 29, 2010

Observation or Belief?

We return to the scientific process and use it in an example to distinguish between observation and belief.

Sometime in March 2010, D+D "cold fusion" theorist and technical editor of *Infinite Energy* magazine Scott Chubb published the following comment, first on the *Infinite Energy* Web site:

A second avenue for future research involves further quantifying a key experimental result: that D+D 4He + 23.8 MeV is the dominant heat-producing reaction. The most precise experiment that documents this finding involved a recycling procedure in which helium-4 that had been trapped inside heat-producing experiments is released into the atmosphere. This experiment, which was named "M4" by the SRI group who conducted it, was performed in 1994. "M4" should either be repeated or more precise measurements should be performed, using other systems potentially involving recycling techniques, to test its conclusions.

Chubb knew that *New Energy Times* had exposed the weaknesses in McKubre's "M4" experiment; he knew the other reported 24 MeV claims also had problems. Yet he spoke about 24 MeV as if it were a scientific fact.

I wrote to Chubb and asked him why he perpetuated this myth. He responded with a long list of possible explanations of why McKubre's data failed to show what he and McKubre had initially claimed. I was surprised. Chubb had forgotten the first step in the scientific process: observation. The point was that the 24 MeV had not been observed. I asked him how he accounted for this fact.

"It is not that the helium-4 23.8 MeV reaction must be right all the time," Chubb wrote, "But there is a finite probability that it is, and in quantum mechanics, when a finite probability exists for something, it must take place, at some level."

After 21 years of searching, after the evidence showed that LENR energy release could easily fall between 12 MeV and 89 MeV per helium-4 atom, Chubb was still asserting the hypothesis of 24 MeV. After 21 years of evidence showing a wide variety of other nuclear products with LENR cells, Chubb was still asserting D+D "cold fusion."

I criticized him for failing to distinguish between fact and fantasy.

"The verdict is still out about the 24 MeV reaction," Chubb wrote. "To say otherwise ignores the error bars, the results, etc. ... This is not fantasy."

The experimental evidence includes rigorously performed and honestly reported data. There is no observation of 24 MeV. This became clear to me. Chubb's continued hypothesis of D+D "cold fusion" in the absence of factual observations was belief.

Chubb is not alone is his failure to distinguish between science and belief. David Nagel, who chaired the ICCF-14 conference in 2008, suggests that "science and religion can be viewed as interrelated."

The Helium-4 Myth

Let us return to part of Rothwell's comment: "The only thing in the system that can convert to helium is deuterium."

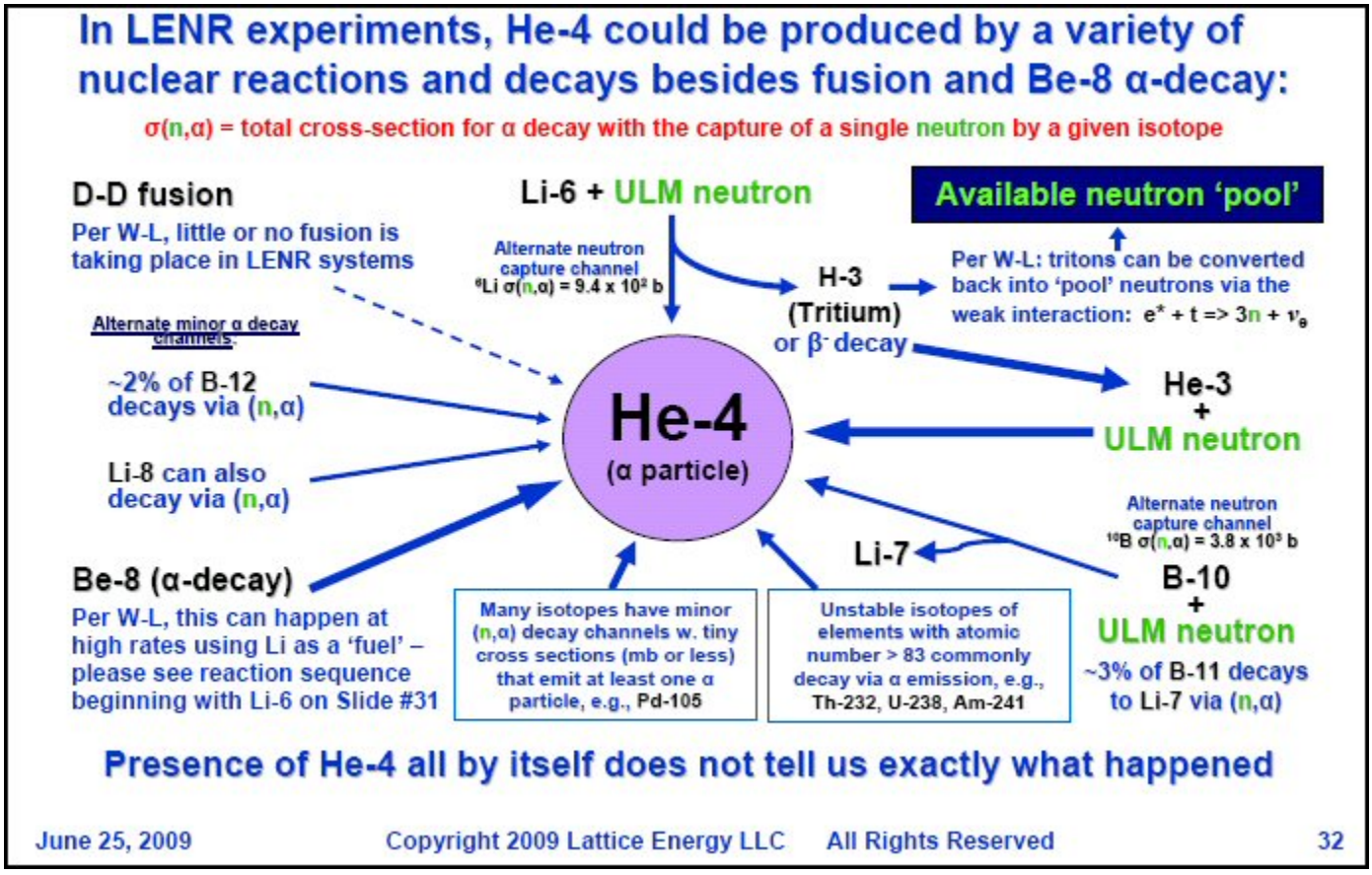
On April 3, I had an e-mail exchange with Bob Bass, a mathematician and another D+D "cold fusion" theorist.

Bass was adamant that D+D "cold fusion" was adequately supported by observation and justified by the observations of helium-4, because the third branch of thermonuclear fusion also produced helium-4.

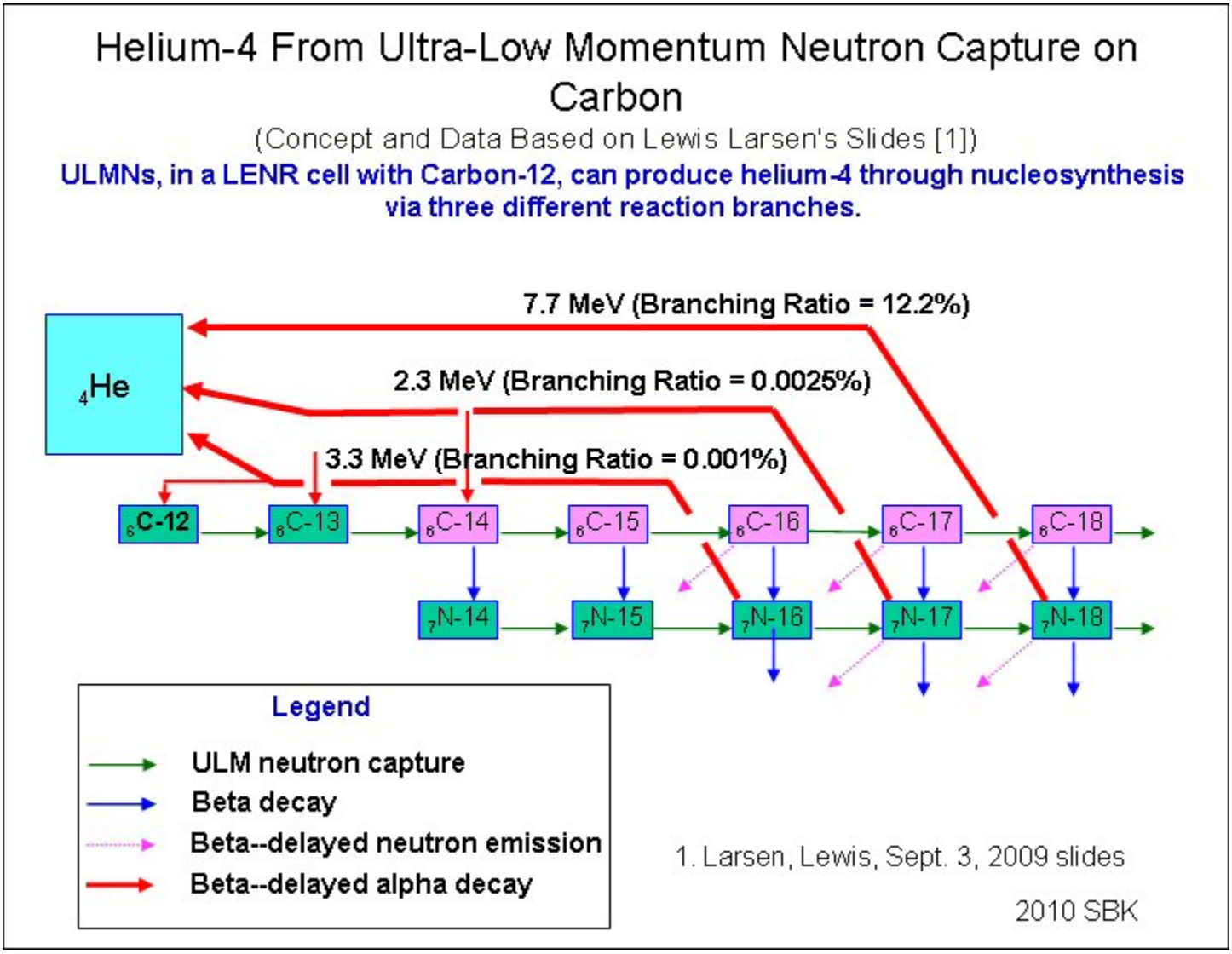
"I have some credibility in hot fusion," Bass wrote, "because my patent-expired Topolotron has been written up in the leading books on the subject (by Tom Dolan and by J. Reece Roth) as a 'promising idea never actually tried.' And enough is known about nuclear physics to assure me that only a finite number of nuclear reactions are even possible in either hot or 'cold fusion.' "

I wrote back to Bass and let him know that, according to another theorist, Lewis Larsen, nuclear physics provides innumerable ways to create helium in LENR experiments.

For example, I showed him slide #32 from Larsen's June 25, 2009, slide presentation.



In Larsen's Sept. 3, 2009, presentation slide #5, he goes deeply into nucleosynthesis and introduces a graphical display of a diverse array of some of the possible weak interactions he sees in LENR. The following *New Energy Times* slide is a simplification that depicts, for example, a beta-delayed alpha decay that can produce helium-4 with a 7.7 MeV release of nuclear binding energy.



Bass did not respond to my e-mail, and I have not heard from him since.

18. Contempt for Cold Fusion

At nearly every "cold fusion" conference I have attended, particularly the recent ones, the unofficial representatives of the field have waxed polemic about how unfairly the field continues to be treated.

The field was certainly treated unfairly in its earlier years. But the current rejection of "cold fusion" papers by some journals and the lack of interest from some government sponsors and corporations have little to do with the past. The assertion by some "cold fusion" proponents that the 2004 Department of Energy reviewers were too dishonest, ignorant or prejudiced to give "cold fusion" a fair shake is also largely incorrect.

The rules of the game have been clear from Day One: If you want to claim "fusion," then you need hard experimental evidence for *fusion* and, better yet, a mathematically supportable theoretical model to back you up.

Almost no LENR experimentalists that I know of have been forced to affiliate their work with any form of fusion. The only exception to this of which I am aware is a paper that was being peer-reviewed by a "cold fusion" theorist who would not approve the paper without the inclusion of his suggestions for a speculative mechanism.

A Belief System

Electrochemists Martin Fleischmann and Stanley Pons did not go public with their idea of "n-fusion" in 1989 based merely on a finite probability that fusion may have been taking place in their electrochemical cells.

They did predict a "one in a billion chance" that packing deuterium into palladium would cause a nuclear reaction. But they didn't bring their idea to the science community based on this theoretical (im)probability or a belief; they brought it forward because five years of experimental work revealed to them direct observations of some of nature's unexplained mysteries.

March, April and May 1989 were among the most confusing, chaotic and frustrating times of science history. This confusion revealed much unprofessional behavior on the part of Fleischmann and Pons' critics. Fleischmann and Pons and the University of Utah administrators who handled the matter made mistakes. Blame for the 1989 chaos is shared equally among the discoverers, the administrators and their critics.

When the field was still in its infancy in 1989, and an extensive database of experimental information did not exist, labeling the discovery a possible "cold fusion" was asking for trouble from the prevailing fusion researchers, but it was not wrong. Asserting that "cold fusion" was a certainty, however, was wrong. A few early LENR researchers, John Bockris (Texas A&M University) and Robert Huggins (Stanford) for example, resisted the trend of their "cold fusion" peers.

On May 26, 1989, the *Wall Street Journal* said Bockris and Huggins "steadfastly refused to speculate on what is producing the excess heat." By the time the First Annual Conference

on Cold Fusion took place March 28-31, 1990, Huggins and Bockris' attempt to remain circumspect became moot; "cold fusion" had not only established its place in the lexicon but also become the *de facto* identifier for the work and its researchers.

By the year 2000, a significant experimental database had accumulated. The data increasingly disconfirmed that "cold fusion" was real. Conversely, the data increasingly confirmed that the body of observed phenomena represents new nuclear effects.

The results were inconsistent with the "cold fusion" hypothesis, and it was time to revise the hypothesis. However, many experimentalists and theorists failed to change with the times. They remained wedded to their original hypothesis of "cold fusion."

When Lewis Larsen and Allan Widom released their "nonfusion" theory of low-energy nuclear reactions on May 2, 2005, the expansive experimental database contradicting "cold fusion" was joined by a rigorous theoretical model that also contradicted "cold fusion."

From this point, the actions of people who chose to continue pursuing a "cold fusion" hypothesis in the absence of supportive empirical observations increasingly justified the label of "true believers."

Their actions, coupled with the fact that "cold fusion" was so unlikely to begin with, perpetuated the disregard of the science establishment for the field.

Most scientists and scientific institutions that had a reputation to uphold would not have any visible affiliation with the field by authorizing research or publishing papers. How it was identified or labeled – condensed matter nuclear science or low-energy nuclear reactions – made little difference to such institutions.

The term LENR is certainly more scientific and respectable, but the trepidation toward the field continues. This is the main reason, according to several LENR sources, that top management at the Naval Research Laboratory is hesitant to see its people put in sincere efforts to obtain positive results and, if found, report them.

Miss Atomic Bomb Speaks Her Mind

One of the failures of "cold fusion" proponents is their refusal to hear what their critics are saying. However, in the last decade, very few critics have paid any attention to the field.

One of the few to effectively express outrage at those who have perpetuated "cold fusion" is a blogger who goes by the names "Miss Atomic Bomb" and "Nuclear Kelly." Kelly has a doctorate in applied physics and a bachelor's degree in engineering physics and is a postdoc in low energy nuclear astrophysics.

In a June 10, 2008, blog post, Kelly referred to "stellar nucleosynthesis," which might be expected because she is studying astrophysics. A year later, Lewis Larsen explained just such a relationship between LENR and stellar nucleosynthesis in his slide presentations. Kelly's main point is her annoyance over the reckless use of the word "fusion" by some scientists.

Graphical avatar used by blogger Nuclear Kelly

> If nobody truly understands it, what right do any of those researchers – or you, for that matter – have to call it cold fusion? You don't know the mechanisms, you don't understand the results, and therefore you can't just call it what you like.
>
> That's as presumptuous as me saying that, outside of any consistent, reliable and physically feasible mechanism, my thesis data alone (though it is reproducible and purely experimental) constitute some vague and unexplainable "stellar nucleosynthesis." I can't, in all good conscience, make that leap outside of the proper (i.e., theoretical) context. My results would be published as standalone. No matter how many times I was able to, experimentally, repeat the one or two measurements I made, that wouldn't give me license to blithely create some far-fetched and, as of yet, unphysical and inexplicable concept which I see as producing the entire thing.
>
> Do your experiments, come up with theoretical explanations, combine the two into some coherent and logical whole with reproducible results and predictive power, and then maybe you can give it a name, keeping in mind both what it is and what it is not. Whatever it is, it's a subject still very much in its infancy, and there is absolutely no reason to hock it as a proven technology.

Kelly provides two valuable insights for LENR experimentalists: 1) Realize why the rest of the world views you and your work with disrespect when you call it "cold fusion," and 2)

continue to do your good experimental work but leave out your speculations about mechanism.

American Chemical Society

I asked electrochemist Jan Marwan, the organizer of the New Energy Technologies symposium at the March 2010 American Chemical Society meeting, why he pitched "cold fusion" to the ACS news service, the public relations group for ACS. The service is run by journalists Michael Woods and Michael Bernstein, who are not scientists, let alone specialists in LENR. To a great degree, they depend on what their member scientists tell them.

Here is my brief exchange with Marwan at the ACS "cold fusion" press conference:

"Considering the mainstream view of 'cold fusion' and the strong evidence for LENR but weak evidence for 'cold fusion,' isn't promoting this field as 'cold fusion' just about worst thing you can do to gain respect for the field?" I asked Marwan.

"I don't know what you mean about weak evidence for cold fusion," Marwan said. "What brings you to this opinion?"

It was almost as if Marwan had forgotten what he and I wrote in his preface and my introduction to our *Low-Energy Nuclear Reactions and New Energy Technologies Sourcebook (Vol. 2)*, he didn't listen to any of my presentations at his ACS symposiums, he didn't read my article "The Decoupling of Cold Fusion From LENR," and he didn't read my series of articles on the 24 MeV belief.

American Physical Society

Now let's look at "cold fusion" from the perspective of someone at the American Physical Society.

After the ACS "cold fusion" press conference, James Riordon, the head of American Physical Society media relations, wrote a blog post titled "Chemists Taken In by Cold Fusion ... AGAIN!" He ended with a plea for the ACS to let go of "cold fusion" "until one of them makes a working battery or something."

Of course, technological applications and science research are two different things. What is crucial about Riordon's comment and his perspective about "cold fusion" is this: It looks bogus. This is where Riordon and I agree. And chances are that his view is shared by a dominant portion of the scientific world. I wrote *The Rebirth of Cold Fusion* after only about a year of muckraking in "cold fusion." I knew very little about science at the time. It's six years later. I've been studying LENR and science nearly every day since then.

This is Riordon's big and legitimate problem (if I'm wrong, I will certainly ask him to correct me): He doesn't have the time or motivation, let alone access, to dig deeply enough into "cold fusion" to find out why it's not real and why LENR, without the presumption of fusion, might be real.

Charles Petit, veteran journalist and lead tracker for the Knight Science Journalism Tracker, also was annoyed that the ACS was promoting "cold fusion."

I responded to Petit's blog post on May 15, 2009.

> Many smart scientists sense that there is something wrong with the picture of "cold fusion." They have sensed this for 20 years. But they can't quite put their finger on what is amiss.
>
> They are frustrated with the fact that "cold fusion" is so resilient as a topic of interest. They are puzzled as to why anybody gives low-energy nuclear reaction research (the more appropriate name, and no this is not mere semantics) a second glance and how there could possibly be any validity to the research.
>
> In the media, the more experienced science journalists are puzzled as to why the less-experienced science journalists give "cold fusion" more than a shred of credibility. The frustration often comes out as pejorative characterizations and emotional rants.
>
> Many smart scientists don't have the interest and patience to dig into "cold fusion" to find out what the problem is. Or maybe they've tried but they run into dead ends. Same for the more experienced science journalists.
>
> I came into this field nine years ago as an agnostic. After my initial investigation, I came to accept that much of the reported experimental phenomena was real. Soon after, I naively accepted the hypothesis that two positively charged

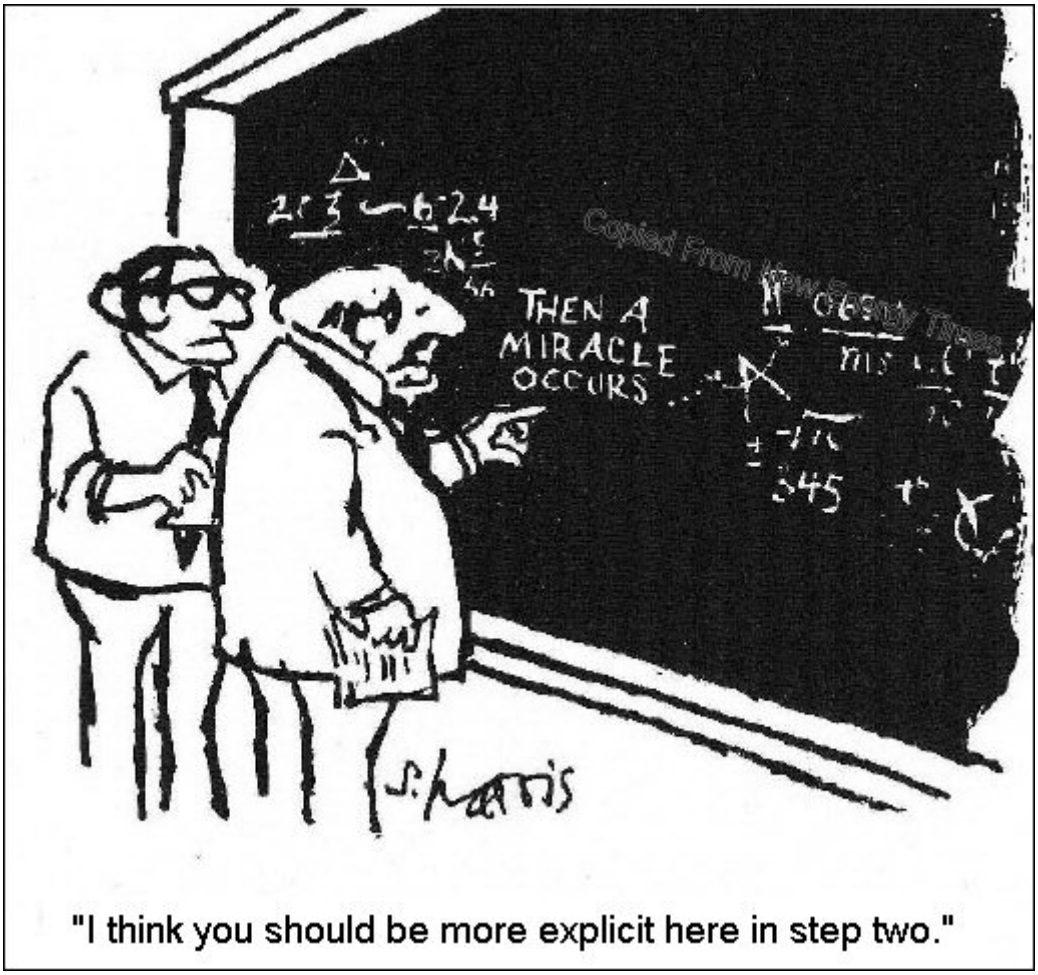
deuterium nuclei were magically overcoming the Coulomb barrier at room temperature and pressure.

In the next few years, I dug in. Three years ago, I began to see what was amiss, and I recognized the problem.

I broke this story first at a session at the American Chemical Society in August 2008. This presentation is available at *New Energy Times*.

Steven B. Krivit
Editor, *New Energy Times*

Let's go back to Atomic Kelly. What does Kelly mean by far-fetched? Sidney Harris hit the nail on the head, with a cartoon that first appeared in *American Scientist* magazine in 1977. It was too early for Harris to be thinking of "cold fusion," but he couldn't have been more prescient.



"Then a Miracle Occurs" Copyright ScienceCartoonsPlus.com

According to people who have scrutinized some of the "cold fusion" theories, this is exactly the problem. At least one key logical bridge is spanned by some sort of miracle. Theorist Yeong Kim referred to this cartoon during his June 29, 2010, presentation at the Army Research Labs LENR workshop.

Hagelstein's Difficulty

Before I discuss my interaction with MIT electrical engineering professor Peter Hagelstein at the ACS "cold fusion" press conference, let me give outsiders some perspective.

Hagelstein brought to LENR researchers the name of his prestigious institution and the recognition he had earned as the X-Ray Laser whiz kid. Because of these associations, many LENR researchers and their fans put Hagelstein on a pedestal.

He was one of several Americans in the field who controlled the field's micropolitics, as a reviewer of journal papers, chairman of the ICCF-10 conference, lead participant in the DoE 2004 review of LENR, and an often-quoted spokesman for the field.

Let's take a close look at my exchange with Hagelstein at the press conference. Here is the video; below that, the transcript:

Steven B. Krivit: Dr. Hagelstein, doesn't the experimental evidence of isotopic shifts in D/Pd systems, because of their energy releases, disprove the idea of D+D "cold fusion" with a total of 24 MeV of heat?

Peter Hagelstein: I'm going to have a tough time understanding and interpreting that particular question. The evidence in support of helium associated with energy production in the Fleischmann-Pons experiment is that helium-4 is seen in association with excess power. It comes from more than 10 experiments where people have seen that kind of thing, and there's two measurements where

the correlation shows a Q-value or an energy per helium-4 of about 24 MeV. About experiments that show transmutation, the question is, Are they similar experiments or not?

For example some of the claims have been made from light-water experiments rather than heavy-water experiments. Except for a small number of measurements produced by my colleague George Miley and the experiments of [John] Dash and the SPAWAR group showing some elemental anomalies, I'm not familiar with evidence that I believe in showing transmutation correlated quantitatively with the energy production.

I don't know that it doesn't exist; I don't know that it does exist. I think the experimental situation at this point is unclear. In any event, I don't believe that the results from such light-water experiments cause one to doubt the results from heavy-water experiments. I don't understand the logic associated with your question.

SBK: That wasn't actually my question. My question was D/Pd systems, with deuterium. I have a report of a neutron activation analysis from a Fleischmann-Pons system: NAA performed at the University of Texas which shows transmutations, isotopic shifts. Are you familiar with this one? It's "Trace Elements Added to Palladium by Electrolysis in Heavy Water." It introduces about 10 MeV.

PH: Who's the author?

SK: Here's your copy.

Readers who have traveled thus far with me in this special report now know the problems with Hagelstein's response:

- There is no "24 MeV" correlation. The two experiments he cites have been exposed by *New Energy Times* to have significant scientific-integrity problems.
- The two people who claimed a correlation "about 24 MeV" effectively retracted the following day after the ACS press conference.
- Even if there were an observed value of 24 MeV, the entire concept of energy/product correlation is meaningless unless you either know precisely what mechanism is responsible or you perform a complete assay of all nuclear products in the experiment.
- The results of LENR transmutations and anomalous isotopic shifts mean other energetic reactions/processes are occurring besides whatever produces the helium-4.
- Hagelstein's speculation about the D/Pd versus H/Pd experiments being similar or not is made irrelevant by the isotopic analysis of the Arata-Zhang and Pons experiment reviewed earlier in this special report.
- Hagelstein's assertion of what he thinks credible is irrelevant. Miley, Dash, SPAWAR and numerous foreign researchers have produced real, permanent, unambiguous nuclear evidence.

- The second part of Hagelstein's assertion is a smokescreen. If you don't know the mechanism, you cannot determine anything about energy/product correlation. And if he did know the mechanism, he hasn't convinced many people.

Now, about the paper I placed in his hands. Is it conceivable that Hagelstein was unaware of the EPRI-sponsored isotopic analysis of the Pons heavy-water cathode? Or that Hagelstein was unaware of the isotopic analysis reported by retired EPRI program manager Thomas Passell at ICCF-10, of which Hagelstein was the chairman? Is it conceivable that Hagelstein, friend and business partner of electrochemist Michael McKubre, and McKubre, friend of Passell, did not know what Passell had found in the Pons cathode?

Or is it conceivable that Hagelstein and McKubre knew about the isotopic anomalies and ignored them or, worse, omitted to report these facts because they cast cold water on "cold fusion"?

Why is the field being represented by scientists who were fudging data they wanted to see and avoiding data they didn't want to see? And how did "cold fusion" gain a second life in the last decade? These questions are answered in the next article in this special report, "Two Decades of 'Cold Fusion.'"

19. Two Decades of "Cold Fusion"

The Birth of "Cold Fusion"

"Cold fusion" was born on March 23, 1989, from the research of electrochemists Martin Fleischmann and Stanley Pons.

It was named in a *Wall Street Journal* article, but it was known by many other names: "The Scientific Fiasco of the Century," "Bad Science" and "Voodoo Science."

When "cold fusion" arrived on the world's doorstep, it was a surprise to all but a handful of people. Throughout the past two decades, it has continued to offer surprises.

The history of "cold fusion" contains a rich collection of real-life examples from which to study science at and beyond the leading edge. The current lessons on "cold fusion" given in academic instructional materials – based on 17-year-old references – are outdated and incomplete. A new wave of scholars is beginning to re-examine the subject with fresh eyes and information and with the much clearer view that hindsight provides.

This article builds on all the previous articles from this special report to offer one perspective on some of the major factors and turning points in the field. Because I have been a direct witness to the last decade and have met nearly all the major players, the second decade of "cold fusion" is most familiar to me.

1989 - Enter "Cold Fusion"

It was all mainstream science could do to suspend disbelief when Fleischmann and Pons appeared on the evening of March 23, 1989, by satellite on the *MacNeil/Lehrer News Hour*.

As has been reported in many books (Krivit and Winocur, Beaudette, Mallove), the 1989 "cold fusion" critics led the public and the scientific world to believe that the idea of "cold fusion" was entirely without merit; this had a profound and lasting effect.

"The anti-cold fusion crowd was equally guilty, if you believe another of the solemn canons: ... Science must be firmly rooted in experiment or observation, unladen with theoretical preconceptions," Caltech professor David Goodstein wrote at the time.

No one has better characterized the devastating effects of these angry scientists at the May 1-2, 1989, American Physical Society meeting than Goodstein.

"Although there were numerous presentations at this session," he wrote, "only two really counted. Steven Koonin and Nathan Lewis, speaking for himself and Charles Barnes – all three from Caltech – executed between them a perfect slam-dunk that cast cold fusion right out of the arena of mainstream science."

Not only was the nascent science delayed as a result of this hostility, but also many of the brilliant men and women who have courageously explored the field have suffered severe consequences as a result. In some cases, the stories are heartbreaking.

Some of the 1989 critics, particularly the theorists of the day, said the idea of "cold fusion" was wrong because it didn't agree with their understanding of fusion. They forgot the scientific process: Experiment trumps theory.

For a short while, the paradigm of "cold fusion" disrupted the paradigm of thermonuclear fusion. The term "hot fusion" became part of the vocabulary only after March 23, 1989.

The next decade brought significant breadth to the understanding of LENR, including a wide spectrum of measured phenomena. However, the field remains, to a certain extent by choice, isolated in its science ghetto.

2003 - LENR Transmutations Replicated in Japan

A significant milestone occurred in 2003. The Mitsubishi Heavy Industries/Iwamura LENR transmutation experiments were replicated at Osaka University. These results elevated LENR transmutation research to a level of unprecedented credibility within the field. Talbot Chubb's ICCF-10 (2003) "News Flash" marked this change well.

"The results presented at this meeting seem destined to affect the course of solid state and nuclear science," Chubb wrote.

A year later, Fleischmann, the "grandfather" of "cold fusion," acknowledged these phenomena.

"I think the key point I would make is the transmutation experiments now seem to be quite believable," Fleischmann said.

2003-2005 - NRL Confirms Mitsubishi LENR Transmutations

Around this time, the Naval Research Laboratory in Washington, D.C. (not to be confused with the SPAWAR laboratory in San Diego) decided to restart its "cold fusion" research. The lab had dabbled in it for a few years at the very beginning.

For starters, NRL requested samples from Mitsubishi's LENR transmutation experiments. NRL's confirmation of the presence of apparently transmuted praseodymium in April 2003 and in May 2005 invigorated its LENR program.

2005 - LENR Theory Breaks From "Cold Fusion" Mainstream

On May 2, 2005, a preprint of a new theoretical model from Lewis Larsen and Allan Widom was released to the arXive physics pre-print server. The Widom-Larsen theory showed a much clearer explanation for most of the phenomena that had been attributed by other people to "cold fusion."

The Widom-Larsen theory was also highly consistent with the transmutation experiments.

This theory said that the underlying explanation for the experimental phenomena had almost nothing to do with fusion, that the key reactions were production of neutrons and beta decays – in other words, weak interactions.

The Widom-Larsen theory broke new ground for the field. It was sufficiently convincing to independent third parties that some of them called it a "viable" theory to explain LENR; other longstanding critics were silent.

These third parties included Richard Garwin of IBM; Dennis Bushnell, chief scientist at NASA Langley Research Center; David Rees, particle physicist with SPAWAR Pacific; and Gregory Greenman, nuclear physicist with Lawrence Livermore National Laboratory.

The history of "cold fusion" had never included such endorsements, active or passive, from people outside the "cold fusion" supporters.

I still do not know whether the Widom-Larsen theory correctly explains LENR, but it seems highly likely to do so. However, even if it turns out to be partially correct, the insight it provides into the understanding of LENR is clear and significant.

Coincidentally, NRL began its second phase of a LENR transmutation research collaboration with Mitsubishi in May 2005. Kenneth Grabowski and two other NRL researchers visited Mitsubishi, where they learned about and observed a successful experiment. They took a sample with them back to NRL and confirmed the successful transmutation into praseodymium.

2005 - NRL Disconfirms Mitsubishi LENR Transmutations

On Sept. 10, 2005, Widom and Larsen released to the pre-print server their second paper, which discussed their gamma suppression mechanism.

Soon after, NRL researcher David Kidwell was invited and/or assigned to work with Graham Hubler's group at NRL on an attempted replication of the Mitsubishi LENR transmutation experiment. From the same May 2005 sample that had given a positive result, Kidwell performed a test on a different spot from that sample.

He reported to NRL and Mitsubishi that the sample failed to show transmuted praseodymium.

I have met Kidwell. I have spoken with other people who have met him. By all appearances, he believes there is no validity to LENR. His discomfort with LENR was so strong that, a year later, in the middle of a Department of Defense briefing on LENR, his sniggering and snickering while one of the LENR presenters was talking caused that presenter to stop in the middle of his presentation, single Kidwell out, and diplomatically suggest that Kidwell reserve his judgments until the session was over.

Why was Kidwell brought in? By whom? For what purpose? My best speculation is that NRL was put in an uncomfortable position by the success of its Naval competitor SPAWAR. The NRL Web site says it is "the corporate research laboratory for the Navy and Marine Corps" and that it has "served the Navy and the nation for over 85 years." People in Washington would ask NRL why its lab has had nothing to show for eight years of LENR research while SPAWAR had two dozen journal papers with positive results.

According to other LENR researchers who have visited the NRL labs, NRL didn't have the immediate skill for most of the LENR research it was attempting. Also, NRL appeared not to have the courage that SPAWAR had. According to some LENR researchers, the motto at NRL is "We would rather be right and second than wrong and first." Thus far in their LENR research, NRL seems to be second as well as wrong.

A retired LENR researcher familiar with NRL and Washington politics pointed out to *New Energy Times* that the only LENR review paper published by NRL omits all reference to the successful LENR results reported by other Navy labs: China Lake and SPAWAR Pacific. According to the retired researcher, the easier option for avoiding embarrassment over NRL's less-than-spectacular LENR research compared to the other Navy labs is for NRL to cast some doubt and uncertainty on the entire subject of LENR.

As LENR pioneer John Bockris once said, "negative results can be obtained without skill and experience."

Was the timing of NRL's actions vis-à-vis the publication of the Widom-Larsen not-fusion theory a coincidence? According to three people, one thing is clear: Michael Melich, retired from NRL and now with the Naval Postgraduate School, was calling the shots, at least in the latter years of NRL's "efforts to replicate" Mitsubishi. And multiple sources have confirmed to *New Energy Times* that Melich's sole interests in LENR were excess heat and helium-4 – that is, the only possible products that might support his belief that D+D "cold fusion" was real.

2006 - Energetic Charged Particles - A Repeatable Experiment

In 2006, something else caused a significant turning point in the field. As I reported on Sept. 10, 2006, scientists at the Navy's San Diego SPAWAR Systems Center achieved a remarkable first in the LENR field: a fully repeatable (by their group) and highly reproducible (by replication groups) LENR experiment. On top of that, this experiment provided hard, permanent evidence of energetic charged particles from low-energy nuclear reactions.

Only one little problem: the multi-MeV energetic charged particles observed in the SPAWAR experiments are inconsistent with the "cold fusion" theory, which for 21 years has presumed that the helium evolved in LENR is born with 0.08 MeV of energy.

For the record, the observation of energetic charged particles in LENR was not new in 2006, but the fact that they were showing up in an experiment that was repeatable and reproducible, and by a government laboratory at that, made these particular LENR phenomena and experiments difficult to refute and impossible to ignore.

2006 – Weak-Interaction Wake-Up Call

When program managers David Nagel and Melich, D+D "cold fusion" theorist Yeong Kim, and Kidwell entered the closed-door Defense Threat Reduction Agency (DTRA) meeting in Ft. Belvoir, Virginia on Dec. 12, they saw Widom and Larsen. The pair had been invited to speak about their theory in front of 75 to 100 top DoD brass. No other LENR theorists spoke at this meeting.

To top it off, "cold fusion" arch-rival Bob Park spoke briefly to the group about LENR and, perhaps for the first time in history, did not say anything negative about LENR. Instead, according to a person who attended the meeting, Park conceded that LENR was "real physics though poorly understood." A few months later, in *Chemistry World*, Park made his first public concession.

When the "cold fusion" supporters later learned that major players in the U.S. government intelligence, defense and energy sectors had quietly evaluated the Widom-Larsen theory and decided that it alone sufficiently explained LENR, they undertook two strategies.

Pete Nanos

Photo credit: Marcio Jose Sanchez / Associated Press

The "cold fusion" faction attempted to marginalize and discredit Widom and Larsen and their theory. And they capitalized on the inroads made by the Widom-Larsen theory, using those newly opened doors to sell "cold fusion" to some people in the government, particularly the DTRA.

I met Bill Wilson of DTRA while he was attending a "cold fusion" session at one of the APS meetings. Wilson clearly spent time learning about LENR, but the decision making about LENR at DTRA appears to have occurred higher up. According to several government sources, Melich was putting in great efforts to lobby George "Pete" Nanos, the associate director of research and development.

According to government sources who spoke to *New Energy Times* on condition of anonymity, Melich made forceful and overt attempts to discredit the SPAWAR claims and convince Nanos that "heat and helium-4" were the most crucial lines of LENR research to investigate.

2007 – Low Fluxes of Neutrons Stimulate Discussion

In March 2007, at the American Physical Society meeting in Denver, Colorado, the SPAWAR group cautiously began to report possible neutron signals. Around this time, the group also submitted its paper to a journal.

The researchers reported low fluxes but clear evidence of neutrons. These findings are inconsistent with D+D "cold fusion," unless, by some chance, someone conceives of a viable process for a primary D+D "cold fusion" scheme that produces tritium *in situ* which in turn leads to secondary DT "cold fusion" reactions. The findings are, however, readily explainable as spallation neutrons by a non-fusion mechanism, for example, Widom-Larsen.

The SPAWAR group's work developed further, and it was replicated at SRI International and the University of California, San Diego. In October 2007, at the 8th International Workshop on Anomalies in Hydrogen/Deuterium-Loaded Metals in Catania, Italy, electrochemist Francis Tanzella reported his replication of the SPAWAR experiment.

"Neutron count above background suggested in at least three experiments," Tanzella said.

In one of these, he observed a neutron signal 14 times greater than background from an electronic detector. Researchers in Russia confirmed this by cross-checking, using a different verification method. According to their mechanical method, the signal was real, though not as strong.

A year later, the SPAWAR group's paper claiming evidence of energetic neutrons published online on Oct. 1, 2008.

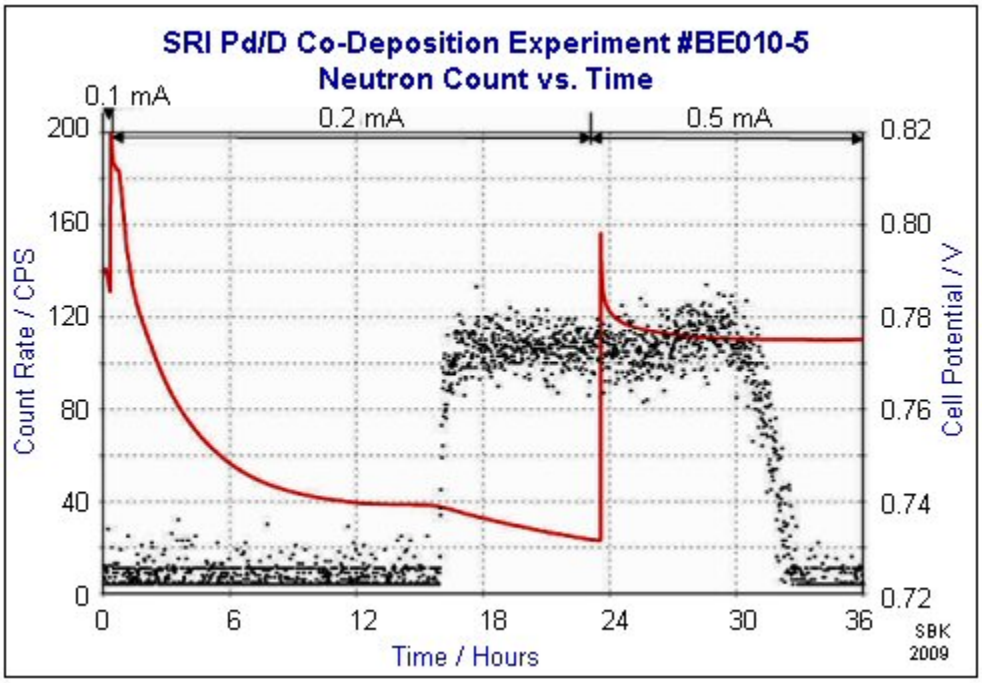
Three months after that, on Jan. 7, 2008, Michael McKubre of SRI International, in a presentation to the leaders of the Bhabha Atomic Research Center in Trombay, India, said there were "no neutrons" in LENR.

How could he have made such a statement when he and Tanzella had just reported neutrons three months earlier? When the largest team to investigate LENR had, in this very same Indian institution, observed neutrons in LENR two decades earlier?

Two days after McKubre's talk at BARC, I spoke at the Indian National Institute for Advanced Studies. I gave my perspective and discussed the variety of observed neutron signals in the recent experiments. The audience included leaders of India's national laboratories as well as some members of its Atomic Energy Commission. I included neutron data reported by SRI International.

I expected some tension after my talk, but I was unprepared for this: McKubre demanded that I discontinue discussing the neutron signals reported from his lab by Tanzella at the Catania conference. A copy of Tanzella's slide presentation is here.

"I don't want you showing Fran's graph with the neutron signal anymore," McKubre said. "It's bullshit. I don't believe it."



I wanted to know whether there was a legitimate problem with the data reported by Tanzella. After a lengthy follow-up discussion with McKubre in which he confirmed the above quotation, I learned that neither McKubre nor Tanzella had issued any form of retraction or correction for the neutron-signal data. Nor did they give me a single indication of any known correction. I also confirmed with McKubre and Tanzella that I had not misrepresented the neutron-signal data seen at SRI. In an e-mail, McKubre stated that he was confident about the electronic detector.

"Fran[cis Tanzella] reported a clear, clean and unusual response of a detector that has worked for us pretty reliably and for which we have developed a level of trust over many years of observation," McKubre wrote.

Months later, in Moscow, Russian researchers Andrei Lipson and Alexei Roussetski analyzed permanently recorded signals from the same experiment using an entirely different detection tool, solid-state nuclear track detectors, and confirmed an unambiguous neutron signal.

2008- Storms and Hagelstein "There are No Neutrons"

Longtime "cold fusion" researcher, advocate and author Edmund Storms objected to discussion about neutrons in LENR. On Oct. 18, 2008, Storms sent the following message to the CMNS e-mail list:

"I'm confused about a discussion based on neutron emission from LENR. All of the published values for the flux of neutrons are near the sensitivity limit of the detector, if any are detected at all. Why are we now finding a flux high enough to warrant discussion? If neutrons are actually being emitted, they will produce all kinds of isotope changes that will result in beta decay. Such beta decay has not been observed, except on a few occasions, even though it would be easy to detect. What is the point of wasting time speculating on such reactions when no evidence exists for significant neutron emission?"

In fact, researchers were finding a flux high enough to warrant discussion.

The SPAWAR group's researchers were seeing nuclear particle tracks on the backside of their solid-state nuclear track detectors inexplicable by anything other than neutrons.

Eleven days after Storms sent his message to the CMNS list, on Oct. 29, 2008, Peter Hagelstein gave a presentation at MIT about "cold fusion." Hagelstein, too, said that there were "no neutrons" in LENR.

Neutrons, whether spallation neutrons (secondary reactions) resulting from ultra-low-momentum neutrons (primary reactions) or from some other unexplained reaction, are inconsistent with the D+D "cold fusion" hypothesis, which McKubre, Hagelstein and Storms have so vigorously defended in this past decade.

April 2009 - *60 Minutes* Showcases "Cold Fusion"

The April 19, 2009, broadcast of CBS Television's "Cold Fusion Is Hot Again," helped get the attention of the Defense Intelligence Agency, as DIA analyst Beverly Barnhart noted in her

Nov. 13, 2009, report. That, in turn, helped to get the attention of the Army Research Laboratory.

CBS producers Sam Hornblower and Denise Cetta decided to focus the program on "excess heat" and promote it as "cold fusion" rather than LENR, without mention of any neutrons, energetic charged particles or alternative explanations for "cold fusion."

They also decided to focus entirely on a single "cold fusion" research company, the former Energetics Technologies laboratory in Israel, to highlight an example of the field. Singling out sources to feature is standard practice for journalism; when we are looking to report good news, we seek the best examples of the subject we can find.

About two years earlier, two of the principals, Irving Dardik and Alison Godfrey, asked me to help them generate publicity so they could better impress their benefactor, Sidney Kimmel, who, according to Godfrey, was losing confidence in Dardik's "SuperWave Fusion" idea. I told them that would not be appropriate for me, and I suggested they hire a publicist.

On May 17, 2008, an entire year before the CBS show aired, Godfrey told me that they were working in some unspecified capacity with *60 Minutes*.

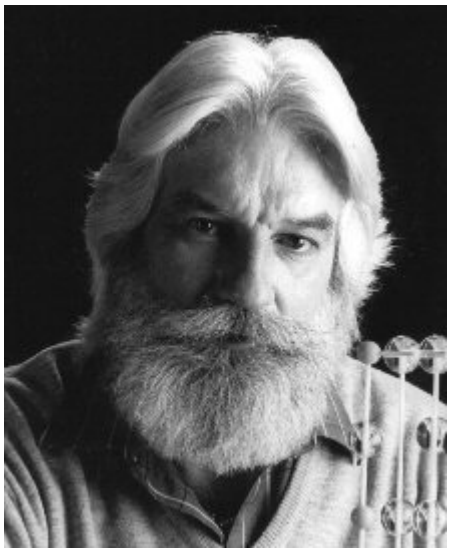
"We are a go with *60 Minutes*. ... The final yes is dependent on two key people getting permission to go on air. It is VERY good news," Godfrey wrote.

In August 2008, at the ICCF-14 conference, I met Rick Kramer, who introduced himself to me as a "media manager." Throughout much of the first two days of the conference, I saw CBS spend a lot of time talking with Kramer, Dardik, Godfrey and the Energetics Technologies researchers. I don't recall seeing CBS speak to any other researchers.

As a postscript to the CBS "Cold Fusion Is Hot Again" show, *New Energy Times* has learned that the Energetics lab, according to a government source who was not authorized to speak on the record, lost its funding and closed its doors sometime in October or November 2009. In April 2010, LENR researcher Akito Takahashi wrote that McKubre told him that the Energetics scientists are moving to the University of Missouri, to a lab under the direction of Rob Duncan, the independent expert hired by *60 Minutes*.

CBS put in great effort to investigate and report on excess heat. The network found highly competent and qualified researchers to feature, and for this, they did a superb job. But did they miss the larger picture?

Did CBS know about the SPAWAR neutron story that later, in March 2009, got worldwide attention in the news media? Yes. Did CBS know about alternative explanations for "cold fusion"? Yes. One of its first hired experts, Paul Michael Grant, contacted me on Dec. 28, 2008, to ask my advice for the show. Grant describes himself as follows: "Visiting Scholar, Applied Physics, Stanford (2005-2008), EPRI Science Fellow (Retired), IBM Research Staff Member Emeritus."


Paul Grant Calls Himself: "Top Science Gun For Hire"

"I've learned that I might be one of the 'talking heads' ... on Sam's upcoming production," Grant wrote. "I'm off to CBS News in a couple of weeks for an interview. I'd like to give you a call sometime soon (like in the next five days!) to get your input on 'what's new and important' in the field of CF. I imagine your 'doesn't look like fusion' has gotten you some interesting responses from your community."

After I had a lengthy phone call with Grant on Dec. 30, 2008, I sent him and Hornblower a follow-up e-mail.

"Do not make the erroneous assumption that, just because LENR proponents have the same goal [of clean energy], they are all fighting [alongside one another]," I wrote. "If you underestimate the factionalism that exists, you will be misled."

My e-mail to Grant continued:

"Why have you and Sam been directed (until now) to only one Navy group, NRL, which, in the last 17 years, has published just one paper on LENR? And it's not even their original work, but a review of the work of others? Not only that, but there are serious omissions in the Hubler paper (Navy China Lake, Navy SPAWAR). ... On the other hand, you've got the SPAWAR group: 20 published papers on LENR, all of their own original research. [The fact] that Denise Cetta is talking with SPAWAR is a very good sign. [The fact] that [you] hadn't been told about the SPAWAR work is a bad sign."

I wrote a follow-up e-mail to Grant, copied to Hornblower and Richard Garwin:

"I remember a conversation I had with Dick about the reproducibility problem a few years ago. I really couldn't argue with Dick about reproducibility and its importance. I asked him for his definition. I remember it clearly: 'It has to happen more often than not.'

"The consortium's paper from the 2008 ACS book shows excess-heat runs that were reproducible. But the '64a and 64b' pair (reported in 2004) were not reproducible in Dick's definition or in mine. Why would *60 Minutes* have any interest in a non-reproducible effect when there are other more-reproducible experiments to consider?

"I encourage you and Sam to look at the big picture. I don't want to see CBS shoot itself in the foot."

Frank Gordon, now retired from SPAWAR, told *New Energy Times* that CBS did, in fact, meet and speak with them.

"[However,] a few days before [the CBS program] aired," Gordon wrote, "I got a call from one of the editors telling me that we would not be part of the story. He gave two reasons: one, that there wasn't enough time to cover both heat and neutrons, and two, that since we wouldn't go on camera, they didn't feel they could say much about neutrons."

A month before the *60 Minutes* program aired, on March 27, the Discovery Channel's *Brink* covered the SPAWAR neutrons story in six minutes without needing SPAWAR to either go on camera or be quoted on the record.

I do not know why CBS made the choices it did. What I do know is that Alison Godfrey, a principal of Energetics Technologies, strong-armed me to prevent me from meeting the CBS crew.

I have always tried my best to maintain friendly relationships with other journalists who have shown an interest in LENR, though I also make sure they know that I won't hesitate to publicly criticize them if they get the story wrong. On the first day of the ICCF-14 meeting in Washington, D.C., I saw a film crew at the back of the meeting hall. At a break, I walked out of the room, and just outside the doors, in the hallway, Godfrey and several Energetics people were standing and chatting.

Hornblower, whom I later got to meet, was there, too, so I offered my card to him. When my arm was stretched out with my card in hand, Godfrey grabbed my upper arm and dragged me about 10 feet away. She told me, angrily, "You'll meet him later!" A few hours later, Godfrey came up to me and told me that she was angry at me for rudely interrupting their meeting.

August 2009 - DIA

In August 2009, Beverly Barnhart, an analyst with the Defense Intelligence Agency, conducted a workshop at SPAWAR Pacific to learn about LENR. I received a copy of her *Technology Forecast* report indirectly on Nov. 13, 2009. I called her up and spoke with her for about two hours. I asked her how the whole idea of a workshop and report came about; I asked her some specific questions about theory, the invited speakers and how the report was written.

I was particularly fascinated by the omission of the Widom-Larsen theory and, for that matter, any mention of weak-interaction ideas. Her report stated that "no one theory currently exists to explain all the observed LENR phenomena." She wrote that some researchers think it could be D+D fusion, other researchers think it could be from "non-nuclear means," and that another possibility was "an intricate combination of fusion and fission."

I had chatted on the phone with Pat McDaniel, who was one of the invited speakers at the workshop. As of last year, McDaniel was a research professor with the University of New Mexico. Before that, he had a career in the U.S. Air Force and Sandia National Laboratory.

McDaniel either took it on himself or was asked to provide a critical review of the Widom-Larsen theory at this meeting. Why? Because in McDaniel's view, "Widom Larsen Theory is currently considered by many [people] in the government bureaucracy to explain LENR." McDaniel does not appear to have experience in particle physics or condensed matter nuclear physics.

Beverly Barnhart

Patrick J. McDaniel

- Widom Larsen Theory is currently considered by many in the government bureaucracy to explain LENR
- An informal discussion is probably worthwhile, particularly if Lew Larsen is not here

First two bullets from second slide of Pat McDaniel DIA presentation at SPAWAR, San Diego.

McDaniel knew that Larsen would not be at the workshop. Hagelstein attended the workshop.

2009 - Hagelstein "There are No Energetic Particles"

The SPAWAR group had reported rigorous, repeatable evidence of energetic charged particles and neutrons. These were inexplicable by any D+D "cold fusion" theory Hagelstein might have had.

In November 2009, Hagelstein submitted a paper for publication with theoretical "constraints" speculating that the energetic charged particles were coming from secondary reactions of D+D "cold fusion."

If he could provide a viable explanation for D+D "cold fusion," he might be able to explain secondary reactions that created the energetic charged particles.

2010 - SPAWAR Group Claims DT Fusion

On July 7, 2010, the SPAWAR group published another paper in its long line of LENR research. However, despite the fact that the SPAWAR group was aware that the Widom-Larsen theory provides a mathematically correct and conventional physics-based explanation for a primary source of LENR neutrons, the group did not list this idea even as a possible explanation for secondary, spallation neutrons.

Instead, the researchers speculated that "the most likely source of neutrons responsible for the triple-tracks is DT fusion inside the Pd lattice."

One problem with their speculation is that tritium is not one of their starting materials. Also of significance is the weak experimental evidence they cite for in-cell tritium. Their entire underlying evidence for tritons is shown in the image below. According to electrochemist Pamela Mosier-Boss, the larger circles are tritons, and the smaller are protons.

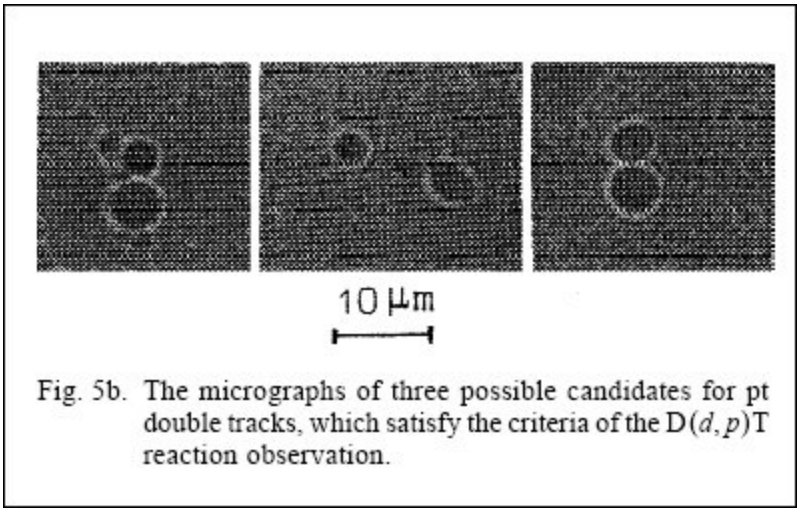


Fig. 5b. The micrographs of three possible candidates for pt double tracks, which satisfy the criteria of the D(d,p)T reaction observation.

Lipson/Roussetski "Possible candidates for proton-tritium double tracks"

The SPAWAR group floated the idea of DT fusion, yet they provided nothing for theoretical support.

The empirical aspects of the research are very strong, they have made excellent progress to answer previous experimental critiques and they have performed notable work in their attempt to characterize the neutron energies, in collaboration with researchers at a Department of Energy laboratory in Santa Barbara, Calif.

Lost Opportunity

I found that LENR researchers who had anything to say about the Widom-Larsen theory, including many of the experimentalists, were hostile and cynical toward it. Many of them were attached to the hypothesis of "cold fusion."

By 2008, some of the remaining active players in the field had been using the term "cold fusion" for so long that perhaps they couldn't mentally separate the hypothesis of "cold fusion" from the hypothesis of a new nuclear effect.

The "we're all in this together" attitude conveyed by McKubre and the "cold fusion" supporters at ICCF conferences was not holding up. The fight for clean energy seemed less important than the fight for intellectual property and intellectual primacy.

The only formal critique of the Widom-Larsen theory from the "cold fusion" supporters that I am aware of is that by Hagelstein. Many "cold fusion" people provided informal critiques to *New Energy Times*, and I believe we published every one.

When the Widom-Larsen momentum failed to abate, both at *New Energy Times* and in the mainstream, the personal attacks against Larsen began. I have seen this pattern frequently in controversial science; when bitter opponents of a new scientific idea fail to impede the growing acceptance of that new idea with scientific argument, they attack the person.

John Burdon Sanderson Haldane, a geneticist and evolutionary biologist born in 1892, also had a perspective on new theories:

> Theories have four stages of acceptance:
>
> i. this is worthless nonsense,
> ii. this is interesting, but perverse,
> iii. this is true, but quite unimportant,
> iv. I always said so.

The attacks have targeted Larsen because he was the originator of the Widom-Larsen theory and he is driving its progress. For a detailed background on the development of the theory, see "Development of the Widom-Larsen Theory of LENRs" in this special report.

Before his and Widom's pre-print appeared on May 2, 2005, the LENR community did not yet know he was working on a nonfusion theory and Larsen seemed to be on friendly terms with most of the LENR researchers.

Factionalism

The LENR paradigm, bolstered by the Widom-Larsen model, fueled an undercurrent of factionalism between people who identified themselves with "cold fusion" and those who did not. This battle has been taking place beneath the surface for several years.

The "cold fusion" faction includes McKubre, Hagelstein, Melich, Nagel, Hubler, Scott Chubb, Talbot Chubb, Storms, Violante, Godfrey, Dardik and their supporters, including Jed Rothwell.

The LENR faction comprises Larsen and Widom, a few other researchers, and their supporters, including me. Most of the Widom-Larsen supporters have steered clear of the conflict and kept a low profile.

Infinite Energy magazine founder and editor Eugene Mallove had seen the beginnings of the "cold fusion" factionalism. He saw that a "mainstream," which had started to ostracize

nonconformists, had developed within the LENR field, much in the same way that mainstream science had ostracized "cold fusion" in 1989.

Much of this factionalism has flown under the radar because the "cold fusion" faction has been acting in more insidious ways than the 1989 "cold fusion" critics.

For example, at the March 2009 ACS meeting in Salt Lake City, Peter Hagelstein of MIT and Scott Chubb of *Infinite Energy* magazine speculated that possible instrument error or some kind of imaginary processes were responsible for reported transmutations of palladium to silver, which contradicts their "cold fusion" theories. Vague speculations are the hallmark of pathological skepticism.

Other people, like NRL researchers David Kidwell and Kenneth Grabowski, have suggested, for example, that Iwamura's transmutations are not real LENR transmutation effects but the result of "contamination" by human hands and "lucky tweezers."

When the "cold fusion" faction saw that it was losing ground to the LENR faction, the former suggested that the distinction was mere semantics. When that didn't fly, the faction said that nonfusion reactions could also be defined as fusion. This fallacy is explained in "Neutron Capture Is Not the New Cold Fusion" in this special report.

As the tensions heightened in 2009 and 2010, one of the less-outspoken "cold fusion" theorists, Xing Zhong Li, asked me on March 16, 2010, to hold back my growing skepticism about "cold fusion."

"CMNS is too weak to be split now," Li wrote.

The leaders of the "cold fusion" faction controlled the 2004 Department of Energy review of LENR. They controlled what went into the 2009 Defense Intelligence Agency report. They controlled the politics and funding for the field at NRL, DTRA and DARPA. They have been among the peers asked to review papers. (The field is so small that anonymity is close to impossible.) They now control the recently "cold fusion"-friendly journal, *Naturwissenschaften*. And they have controlled the last two ICCF conferences.

Control of the conferences means setting the agenda, choosing the scope, deciding on the featured (plenary) speakers, deciding who gets an oral presentation and who gets a board to mount a poster on, and deciding which of the non-plenary speakers get a 10-minute speaking slot, a 15-minute slot or a 20-minute slot.

It also has meant determining which speakers have been sponsored to come to the conferences. One U.S. LENR researcher who supported the "cold fusion" faction's agenda and who wishes to remain anonymous told *New Energy Times* he was paid $4,000 by Nagel and Melich to attend ICCF-14 and write a review paper. Another unprofessional action of the U.S. ICCF-14 organizers was that they informed speakers of the day and time of their talks only 12 days before the conference. This had the most significant impact on international researchers. For most science conferences that draw international attendees, this information is determined months in advance to enable them to schedule their travel plans more precisely rather than having to block out an entire week for a single 10- to 20-minute talk.

Change?

I don't know what will happen at ICCF-16 in India next year. I do know that the Japanese, Chinese and Indians are taking LENR transmutations seriously and SPAWAR is likely to continue its winning streak. If NRL management gets an infusion of courage, it will seek new people in its ranks who will be eager to succeed.

From a nonscientific perspective, nothing is wrong with the ideal of "cold fusion." It represents hope over hopelessness, clean energy over environmental destruction, and abundant energy over scarcity. This ideal has inspired and motivated scientists to persevere, benefactors to sustain and observers to maintain vigil over this new science that may foreshadow unimaginable new technologies.

20. Moving Forward

The Future of LENR Research and Applications

• **Nanotechnology:** Where the most important and most immediate LENR mysteries likely will be (or may have been) solved.
• **Surface Chemistry:** Specifically, surface plasmon polaritons seem to be very important.
• **Transmutations:** Likely to provide expansive windows into understanding LENR.
• **Fully Repeatable Experiments:** Any effect, no matter how seemingly banal, that is fully repeatable provides a powerful platform from which to explore LENR. Using expensive and highly constrained diagnostic devices makes little sense when researchers are not confident the experiment will provide a positive result.
• **Material Pairing:** There appears to be a paired relationship with deuterium-palladium systems and nickel-hydrogen systems. For reasons not clearly known, a reversal of the pairing does not seem to work as effectively.
• **Deuterium/Hydrogen Gas**: Future applications are far more likely to use either a gas phase device or some kind of solid-state device rather than a liquid electrolytic device.
• **Back to the Future**: Some of the most valuable research in LENR may have occurred two decades ago. It is incorrect to assume that the research promoted today is more significant or successful than that performed 20 years ago. Attitudes about LENR research have gone through many phases and sometimes come full circle.
• **Regional Perspectives:** There are strong variations in the international view of LENR. Most countries with active researchers have developed a localized set of perceptions of and preferences for the research.
• **Excess Heat is Only an End Product:** The study and measurement of excess heat as its own goal has limited use. Instead, investigate the conditions, materials and processes which lead to excess heat.
• **Involve the Nuclear Community:** Contributions and guidance from experts in nuclear physics, particle physics and nuclear chemistry will be essential.

Insights for Potentially Revolutionary Science
(Ideas by S.B. Krivit unless otherwise noted)

Scientific Critique

• "Breakthrough science cannot be peer-reviewed." (Sri Kumar Banerjee, chairman of Indian AEC)
• Critical and honest scientific skepticism is essential to the scientific method and to the advancement of scientific progress. Clever skeptics can always find ways to theorize and imagine alternate explanations for anomalies in controversial science. If the critic cannot provide explicit and specific explanations about the hypothetical alternative explanation, the skepticism is pathological, and worthless.
• Proving a negative, or the non-existence of an idea, can be close to impossible. (Labinger and Weininger)

Scientific Replication

● Failure to replicate means failure to replicate.
● People who do not want to successfully replicate an experiment most likely will achieve their goals. (Paraphrased from Fleischmann)
● "Negative results can be obtained without skill and experience." (Bockris)
● There is a crucial distinction between a scientific replication attempt and a scientific result, though they are closely related.
● When a dispute arises about a scientific replication attempt, the replicator's opinion about the accuracy of the replication is no more valid than the originator's opinion about its accuracy.
● There are no completely independent replications; a minimal level of knowledge must transmit between parties. Newer and more difficult replication attempts require greater knowledge transfer; more mature and simpler experiments require less.
● The highest confirmatory power of a scientific replication is not an identical replication. (Collins)

Science Sociology

● Scientific behavior tends to change among people exploring potentially revolutionary science. The high stakes and opportunities, known best by the most knowledgeable players in the field, influence behavior of scientists in unexpected ways. Remain vigilant.
● Scientists and scientific establishments – new or old – are not, white lab coats notwithstanding, as pure as the driven snow. (Scientists are human beings, too.) (Beaudette)
● Science philosophy, specifically science sociology, is in need of overhaul. Scientific skepticism must follow the same rigor as scientific claims. (Baloney Detection Collection)
● University coursework in science sociology, ethics, conduct and research integrity is woefully inadequate.
● In new science, there are no established authorities or experts; the playing field is wide open.
● Science journalists must not forfeit a critical attitude to so-called or self-proclaimed science authorities. The same journalistic discipline should apply as that used in reporting politics, business, crime and society. (Pownall)

Science Philosophy

● Experiment trumps theory, always, no matter how many centuries old a theory may be. (Paraphrased from many people)
● Honest mistakes are essential to science. (Pownall/Popper)
● Science is never wrong, but sometimes people are.
