DOCUMENTATION OF LUDWIK KOWALSKI'S CONTRIBUTIONS TO THE LENR FIELD

A PROJECT OF THE LENR RESEARCH DOCUMENTATION INITIATIVE

THIRD DRAFT REPORT

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December 13, 2021
Revision 1: July 12, 2026

Contents

1. Introduction

Cold fusion (CF) was announced on March 23, 1989, by Dr. Martin Fleischman and Dr. Stanley Pons. The immense potential energy benefits of CF (also referred to as Low Energy Nuclear Reactions, LENR) were immediately recognized. Humankind's need for a source of cheap, clean, inexhaustible, and safe energy seemed to be realized. However, LENR was rejected by mainstream science within a year or so, and it remains highly marginalized to this day. On the other hand, the phenomenon has continued to be rigorously pursued by many investigators in several countries. The mounting evidence for the reality of LENR shows that its potential benefits may yet be realized.

Because it is a pariah science, LENR has attracted relatively few new investigators to the field. Many of the researchers became active in the early months and years after the 1989 announcement. Now 30 years later a significan number of these investigators are leaving the field. The results of their many years of LENR investigation are at risk of being lost, which would be extremely unfortunate not only for the field, but also potentially for humanity.

An initiative is underway to mitigate the risk of loss of research records of LENR investigators. Its objectives are to collect, organize, document, and archive these records. It is being performed by LENRGY, LLC[1], whose President is Dr. Thomas Grimshaw. The LENR Research Documentation Initiative (LRDI) assists researchers in making sure that their efforts are preserved and to keep the records available for additional analysis and interpretation. The LRDI began with a pilot project with Dr. Edmund Storms. It is described in a recent article in Infinite Energy[2] as well as on a dedicated website[3].

[1] LENRGY: LENR Energy – Pursuing the Benefits of Cold Fusion Realization. www.lenrgyllc.com.

[2] Grimshaw, T., 2020. Documenting Cold Fusion Research: Preserving a Vital Asset for Humankind. Infinite Energy, Issue 150, March/April 2020, p. 9-13.

[3] LENR Research Documentation Initiative: Collection, Organization, Description, Archiving of LENR Research Records. www.lenr-documentation.org.

Dr. Ludwik Kowalski was born in Poland in 1931 and received his Doctorate of Physical Sciences from the University of Paris in 1963[4]. Subsequently in 1986 he was awarded the MA degree in Mathematics Education from the Kean College of New Jersey. He immigrated to the US in 1964 and, starting in 1969, held a position in the physics department at Montclair State University for 35 years, retiring in 2004. In 1978 he published a book, Understanding Physics with Dr. Hal Hellman[5].

Dr. Ludwik Kowalski has made significant contributions to the LENR field. When LENR was announced in 1989, he followed the trend of other physicists for several years in not accepting the phenomenon. However, he took a new interest in 2002. His LENR work continued at least until 2017, when he authored an article on the sociological aspects of the LENR's rejection. Most of his contributions are recorded on his "Learn Cold Fusion" (LCF) website[6], which is described in Section 4 below. A photo of Dr. Kowalski with Dr. Martin Fleischmann taken at 2011[7] is shown in Figure 1-1.

[4] Kowalski, L., 1963. Contribution to the Study of the Fission of Heavy Nuclei at High Energy. Theses Presented to the Faculty of Sciences at the University of Paris. Paris, Masson & Co, Editors.

[5] Kowalski, L., and H. Hellman, 1978. Understanding Physics. Belmont, CA, Wadsworth Publishing Co.

[6] https://msuweb.montclair.edu/~kowalskil/cf/.

[7] Kowalski, L., 2012. Cold Fusion Is Not Voodoo Science. Learn Cold Fusion Website, #403, March. http://ludkow.info/cf/403memoir.html.

Figure 1-1.
"Prof. Kowalski on left with Martin Fleischmann.
Taken in 2011 at ICCF-10, Cambridge, Massachusetts.

A project, the Kowalski LENR Research Documentation Project (KLRDP), is being performed under the umbrella of the LRDI. Its objectives are to preserve the record of Dr. Kowalski's LENR contributions. This report of the KLRDP includes a biographical summary, LENR publications and information from websites prepared by Dr. Kowalski. Linda Kowalski is gratefully acknowledged for her vital role in supporting this project.

2. Biographies

Dr. Kowalski was working on a research project at Brookhaven National Laboratory at the time of the LENR announcement in 1989. As noted above, he adopted the predominantly negative view toward LENR along with the mainstream scientific establishment. However, in 2002, he took an active interest as a result of attending a scientific conference in Albuquerque, New Mexico. In 2012, he prepared a summary of his LENR work up to that time[8] (LCF-403). It is entitled "Cold Fusion Is Not Voodoo Science" (shown in the link as "Ludwik's 10 Years with Cold Fusion".) The contents of this document are shown below, and the entire document is in Appendix A1. A slightly revised version of this article is also available online[9].

1) Introduction.
2) What Is Cold Fusion.
3) Meeting a Russian Scientist, Alexander Karabut.
4) Meeting George Miley.
5) Beginning Of Censorship: Also From Miley's Paper.
6) Theories Guide But Experiments Decide.
7) First US Government Investigation.
8) Second US Government Investigation.
9) Excess Heat, Real Or Apparent?
10) Robert Park, A Scientist Writer.
11) Three Professional Biographies.
12) The First CF Conference I Attended.
13) The Editor Of Physics Today Rejected My Letter.
14) Meeting Fleischmann And Jones.
15) New CF Results Reported By Other Researchers.
16) Cooperation With Oriani.
17) Next CMNS Conference: My Main ICCF11 Presentation.
18) My Two Other ICCF11 Presentations.
19) History Of Attempts to Publish.
20) Transmutation Of Radioactive Nuclei--Or An Artifact.
21) The Galileo Project.
22) The Curie Project.
23) Flowcharts: The Last CF Conference I Attended.
24) Andrea Rossi's Unbelievable Claims.
25) What is Next?

[8] Kowalski, L., 2012. Cold Fusion Is Not Voodoo Science. Learn Cold Fusion Website, #403, March. http://ludkow.info/cf/403memoir.html.

[9] https://www.lenr-canr.org/acrobat/KowalskiLcoldfusion.pdf.

An excellent biography is provided on the website of the Hoover Institute. It is in a press release of January 2011 regarding Dr. Kowalski's contribution of his papers to that organization[10]. Wikipedia also has a thorough description of his career. These biographies are included in Appendix A2. Neither of them makes reference to Dr. Kowalski's LENR work.

After Dr. Kowalski's birth in 1931 his family moved from Poland to the Soviet Union in 1932. This move ended in tragedy during Stalin's Purge of 1938, when his father was arrested and imprisoned in a Gulag labor camp. He died there two years later at the age of 36. Dr. Kowalski initially remained a Communist, but later became a staunch anti-Communist. His family story and personal political journey are included in two webpages, which are presented in Appendix B.

[10] The Diaries of Ludwik Kowalski, Former Stalinist, Donated to the Hoover Institution Archives. Press Release, January 3, 2011. https://www.hoover.org/news/diaries-ludwik-kowalski-former-stalinist-donated-hoover-institution-archives.

3. LENR Publications

Jed Rothwell's LENR-CANR.org website[11] is the premier electronic library for the cold fusion field. A search for "Kowalski" on the website resulted in 16 items, which are listed in date order in Table 3-1 The ones that are available have been downloaded for the KLRDP.

Table 3-1. Items Found in Search for "Kowalski" on LENR-CANR.org Website

Year	Description	DL?*
2003	Engvild, K.C. and L. Kowalski. Triple Deuterium Fusion Between Deuterons And The Nuclei Of Lattice Trapped Deuterium Molecules. in Tenth International Conference on Cold Fusion. 2003. Cambridge, MA: LENR-CANR.org.	Y
2003	Kowalski, L. Teachers Debate Cold Fusion. in Tenth International Conference on Cold Fusion. 2003. Cambridge, MA: LENR-CANR.org.	Y
2003	Kowalski, L. The Dilemma Of A Physics Teacher. in Tenth International Conference on Cold Fusion. 2003. Cambridge, MA: LENR-CANR.org.	Y
2004	Kowalski, L., et al. Charged particles from Ti and Pd foils. in Eleventh International Conference on Condensed Matter Nuclear Science. 2004. Marseille, France.	Y
2004	Kowalski, L. History of attempts to publish a paper. in Eleventh International Conference on Condensed Matter Nuclear Science. 2004. Marseille, France.	Y
2004	Kowalski, L., Please Donate ICCF Proceedings To The Niels Bohr Library. 2004.	Y
2004	Kowalski, L. Recent cold fusion claims: are they valid? in Eleventh International Conference on Condensed Matter Nuclear Science. 2004. Marseille, France.	Y
2005	Kowalski, L., et al. New results and an ongoing excess heat controversy. in The 12th International Conference on Condensed Matter Nuclear Science. 2005. Yokohama, Japan.	N
2005	Kowalski, L., et al. Searching for excess heat in a Mizuno-type Cell (PowerPoint slides). in The 12th International Conference on Condensed Matter Nuclear Science. 2005. Yokohama, Japan.	N
2005	Kowalski, L., S. Little, and G. Luce. Searching for excess heat in Mizuno-type plasma electrolysis. in The 12th International Conference on Condensed Matter Nuclear Science. 2005. Yokohama, Japan.	Y
2007	Kowalski, L. On emission of nuclear particles caused by electrolysis. in Proceedings of the 8th International Workshop on Anomalies in Hydrogen / Deuterium Loaded Metals. 2007. Sicily, Italy.	Y
2008	Kowalski, L., Comments on 'The Use of CR-39 in Pd/D Co-deposition Experiments' by P.A. Mosier-Boss, S. Szpak, F.E. Gordon and L.P.G. Forsely, Interpreting SPAWAR-Type Dominant Pits. Eur. Phys. J. Appl. Phys., 2008. 44: p. 291-295.	Y
2008	Kowalski, L. Nuclear or not nuclear: how to decide? in ICCF-14 International Conference on Condensed Matter Nuclear Science. 2008. Washington, DC.	Y
2010	Kowalski, L., Comments on Codeposition Electrolysis Results. J. Condensed Matter Nucl. Sci., 2010. 3: p. 1-3.	Y p7
2011	Driscoll, J., et al., Issues Related to Reproducibility in a CMNS Experiment. J. Condensed Matter Nucl. Sci., 2011. 5: p. 34-41.	Y p40
2012	Kowalski, L., Cold Fusion is not Voodoo Science. 2012, LENR-CANR.org.	Y

* Downloaded for Kowalski LENR Research Documentation Project

[11] LENR-CANR.ORG: A library of papers about cold fusion. https://lenr-canr.org/.

4. "Learn Cold Fusion" (LCF) Website

Dr. Kowalski's major contributions to the LENR are posted on his website, which has 416 articles and essays on the phenomenon. Each topic has its own webpage. It is referred to as part of his "Learn Cold Fusion" (LCF) project, so that name is used in this report. It was started in 2002 while he was still at Montclair State University and continues to be located on the University website[12]. Additions to the website continued for 11 years; the last entry was in September 2013.

The LCF website has on its first page an index to the items with a link ("click") for each one. The first 30 topics in the index are shown below as representative of the webpages, and the entire list of 416 items is in Appendix C.

My motivation? Click to see a short introduction.
0) I am no loger saying "it is voodoo sciece." click
1) Introducing Cold Fusion to students. click
2) A typical "cold-fusion" setup. click
3) Three kinds of Cold fusion. click
4) Short biographies of three Cold Fusion Scientists. click
5) Aberration of the scientific methology. click
6) On dangers of "second hand" publishing. click
7) On Pathological Science (N-rays story). click
8) On Burden of Proof in Science. click
9) Scientific Method in Cold Fusion. click
10) A Russian connection. click
11) Bottom Line. click
12) What do physics teachers think about CF? click
13) More about the Russian Connection. click
14) What is pseudo-scientific in this? click
15) Or what is pseudo-scientific in this? click
16) Here is an example of real pseudo-science. click
17) An Italian connection. click
18) Nobel Prize for "cold fusion?" click
19) A French connection. click
20) Excommunication of heretics? click
21) If it were up to me I would do it. click
22) Another good article summarized. click
23) A Japanese connecion. click

[12] https://msuweb.montclair.edu/~kowalskil/cf/.

24) Three short introductionary tutorials. click
25) A technical tutorial. click
26) Comments on the 1989 ERAB report. click
27) Conspiracy? For what purpose? click
28) Summary of a very impressive paper. click or
29) Another French connection. click
30) New APS ethics guidelines and the CF issue. click

The entire website has also been downloaded for the KLRDP as a PDF file. It consists of over 1800 pages. The full index and contents are presented as an addendum to the LRDI report:

> Grimshaw, T., 2026. Documentation of Ludwik Kowalski's Contributions to the LENR Field. A Project of the LENR Research Documentation Initiative. Addendum to Third Draft Report: Dr. Kowalski's "Learn Cold Fusion" Website. LENRGY, LLC, Austin, Texas. April.

The LCF website is described on one of its webpages that is reached through a link, "My motivation? Click to see a short introduction". This webpage was apparently prepared at about the time the website was initiated. Its contents are shown below[13]:

About my "learn cold fusion" project

In the fall of 2002, to my surprise, I discovered that the field of cold fusion is still active. This happened at the International Conference on Emerging Nuclear Systems (ICENES2002 in Albuquerque, New Mexico). Several papers presented at this conference were devoted to cold fusion topics. Intrigued by the discovery I started reading about recent cold fusion findings and sharing what I learned with other physics teachers. I have been doing this over the Internet using Montclair State University web site

http://blake.montclair.edu/~kowalskil/cf/

What follows is a set of items posted, more or less regularly, on that web site since October of 2002. The items reflect my own process of learning, mostly from articles published by cold fusion researchers. I am still not convinced that excess heat, discovered by Fleischmann and Pons, is real or that nuclear transmutations can occur at ordinary temperatures. But I do think that time is right for the second evaluation of the entire field. I do not believe that extraordinary findings of hundreds of researchers are products of their imagination or fraud. Our scientific establishment should treat cold fusion in the same way in which any other area is treated. Those who study cold fusion do not appear to be pseudo-scientists or con artists. The items on my list are arranged in the order in which they were posted on my web site.

It seems clear Dr. Kowalski's LCF website is important primarily for the benefit of his observations and insights into the cold fusion phenomenon. It is also a valuable historical record of developments in the field during the period he was making observations and writing the articles.

[13] There is a P.S. at the end of the webpage that is less relevant and has not been included.

5. Example Article, LCF #421: Oriani's Death and Comments on NAE

Dr. Kowalski's LCF website covers a variety of topics, but its webpages have a generally consistent format. One example is #421, which was provided by Linda Kowalski and has actually not yet been posted on the LCF website (the last entry is #416). Two topics are covered – the death of prominent cold fusion researcher Richard Oriani and an interview with Dr. Edmund Storms conducted by Peter Gluck. The Storms interview focuses on his concept of the nuclear active environment (NAE) where cold fusion reactions take place. Dr. Kowalski makes comments and proposes questions about the NAE. The contents of the webpage are shown below.

421 Oriani's Death and Comments on NAE

Ludwik Kowalski, Ph.D. (see Wikipedia)
Professor Emeritus, Montclair State University, Montclair, N.J. USA

1) Yesterday (9/3/2015) I learned about the death of Richard Oriani at age 94. The obituary in StarTribune, his local newspaper, can be seen at:

http://www.startribune.com/obituaries/detail/95363/?fullname=richard-a-oriani

My contribution to this formal goodbye is also there.

2) In a private message received today, a colleague quoted Max Planck--"science does progress funeral by funeral." Another CMNS researcher commented: "In this case we are seeing regress, not progress. As I said years ago this is a generational role reversal. Young scientists are conservative while the old, and now dying ones champion new ideas! The world is upside down. ..."
Is the CMNS field progressing or regressing? I do not know how to answer this question. One thing is sure, this area of science, often called "Cold Fusion," is still active.

3) A good example of activity is the "Interview with Dr. Edmund Storms, conducted by Peter Gluck. It was posted on the CMNS forum for active scientists (see the blue italic text below). Dr. Storms is a nuclear chemist with over thirty years of service at Los Alamos National Lab, and now working privately at Kiva Labs. His 2014 book, "The Explanation of Low Energy Nuclear Reaction," describing the field, is commercially available:

http://www.amazon.com/The-Explanation-Energy-Nuclear-Reaction/dp/1892925109/ref=pd_sim_14_1?ie=UTF8&refRID=0W6QXKDRE343BXFS7ZT6&dpSrc=sims&dpST=_AC_UL320_SR216%2C320_

Also see his YouTube presentation at:

https://www.youtube.com/watch?v=qfpdvwaQSnA&noredirect=1

Peter Gluck, PhD in chemical engineering, is a retired technologist who has worked many tens of thousands of hours with matter (chemical industries), energy (new sources of energy) and information (web search). He communicates with the world via the blog EGO OUT.

< http://fqxi.org/community/forum/topic/2015 >

INTERVIEW WITH DR. E. STORMS

Based on a discussion stimulated, in part, by the coming CERN Seminar on D/H loaded palladium , Ed Storms has summarized his answers in this way. It is about the essence of the problems of the field.

"LENR [Low Energy Nuclear Reactions] has two aspects, each of which has to be considered separately. The first question is where in the material does the nuclear reaction take place. In other words, were is the NAE [Nuclear Active Material] located. This means where in space is the NAE located, such as near the surface, and what is unique about the NAE. The LENR reaction CAN NOT take place in the normal lattice structure where it would be subjected to the well known laws [such mutual electric repulsion of positive nuclei] that apply to such structures.

I propose the only place able to support such a nuclear reaction while not being subjected to the known chemical requirements are cracks consisting of two surfaces with a critical gap between them. Before the nature of the nuclear process can be discussed, a NAE must be identified and its existence must be agree to. Failure to do this has resulted in nothing but useless argument with no progress in understanding or causing the phenomenon [of nuclear fusion].

Once the characteristics of the NAE are identified, a mechanism can be proposed to operate in this NAE with characteristics compatible with this environment. Attempts to propose a mechanism without identifying the NAE are doomed to failure. Without knowing the NAE, we are unable to test the characteristics of the nuclear mechanism to see if it can take place in an ordinary material and we are unable to know how to create a potentially active material.

This requirement is so basic, further discussion is pointless unless agreement is achieved.

This is not a normal physics problem where any idea can be made plausible simply by making a few assumptions. The nature of the chemical environment prevents many assumptions. We are proposing to cause a nuclear reaction in ordinary material where none has been seen in spite of enormous effort and none is expected based on well understood theory. A significant change in the material must first take place. This change must be consistent with the known laws of chemistry. Only the creation of cracks meets this requirement.

Once the NAE is identified, the characteristics of the nuclear reaction must be consistent with what is known. Simply proposing behavior based on general physics concepts is useless. For example, the role of perturbed angular correlations, which you suggest, must be considered in the context of the entire proposed reaction. The question means nothing in isolation. Like many proposed mechanisms, the idea cannot be tested because it has no clear relationship to the known behavior of LENR or to the variables known to affect the phenomenon.

This is not a guessing game. We now have a large collection of behavior all models most explain. Why not start by considering models that are consistent with this information?"

4) NAE, in other words, if I understand Storms correctly, is a hypothetical environment in which mutual repulsion of protons is much weaker (and we do not know why) than in the vacuum separating atoms. He is right that cold fusion would take place spontaneously (nearly by definition) in such environments. But is he also right by saying that "attempts to propose a mechanism [of cold fusion] without identifying the NAE are doomed to failure." Probably not. Theoretical scientists have no other options but to use models that have already been validated.

5) Let me mention something else questionable. On one hand Ed states that we know nothing about NAE; on the other hand he claims that NAE can be created "in cracks only." How does he justify this?

6) Is it correct to say that NAE is related to nuclear cold fusion like air, our well-known Flying Active Environment, is related to airplanes on Mars? We know a lot about FAE but we know nothing about NAE.

7) The last paragraph of the interview is profound; it has to do with the essence of scientific methodology. Yes, speculations resulting from arbitrary assumptions belong to mathematics (and to theology), not to physical science, where theories are "made plausible" by reproducible experimental data, as we know.

6. Contributions to the Galileo Project

Dr. Kowalski contributed significantly to the Galileo Project, which was led by Stephen Krivit of the New Energy Institute (NEI)[14]. These contributions are described on the LCF website entries shown below. Copies are included in Appendix D.

319. A Contribution to Galileo Project
320. Phase 2 of Galileo Project
321. Scientific Issues in the Galileo Project.

In March 2007 Dr. Kowalski gave a presentation at the American Physical Society (APS) meeting on the Galileo Project. A copy is included in Appendix D. The title of the presentation is "Our Galileo Project March 2007 Report". The NEI has posted a video of the presentation on YouTube[15]. Information provided with the YouTube posting is shown below.

> 2007 - Ludwik Kowalski (Montclair State Univ.) LENR Presentation at APS
>
> 298 views
>
> Apr 24, 2011.
>
> StevenKrivit
>
> "Our Galileo Project March 2007 Report " presented by Ludwik Kowalski at the American Physical Society March meeting in Denver, Colorado on March 5, 2007.
>
> See New Energy Times magazine issue #21 http://newenergytimes.com/news/2007/N... For more information.

The YouTube presentation, which is about 15 minutes long, has been downloaded as video and audio files for the KLRDP.

[14] Krivit, S., 2007. The Galileo Project: What Worked, What Didn't. Presentation at the 8th International Workshop on Anomalies in Hydrogen- and Deuterium-Loaded Metals. Catania, Sicily, Italy. October.

[15] 2007 - Ludwik Kowalski (Montclair State Univ.) LENR Presentation at APS. https://www.youtube.com/watch?v= VPMjc8C1f8g&list= UL69W83GiHW-w&index=62.

7. 2017 Article on the Philosophy and Sociology of LENR

In 2017 Dr. Kowalski published an important cold fusion paper entitled "Philosophical and Social Aspects of the Cold Fusion Controversy". It was published simultaneously in Polish and English in the Polish journal *Postepy Techniki Jadrowej* (Advances in Nuclear Technology). The cover of the journal issue, February 2017, is shown in Figure 7-1.

Figure 7-1.

Image of the February 2017 Journal Containing Ludwik's Article

A similar paper was presented previously at the *IVe Congres de la Societe de Philosophie des Sciences* in June 2012. And another similar paper was submitted earlier, in April 2012, to the journal Progress in Physics. These two papers are posted on the LCF website as #413 and #409 and are included in Appendix E. The content of the Polish journal article is shown below.

PHILOSOPHICAL AND SOCIAL ASPECTS OF THE COLD FUSION CONTROVERSY

Ludwik Kowalski, Ph.D.
Professor Emeritus, Montclair State University, USA

ABSTRACT

The area of research known as Cold Fusion (CF) has been the arena of a science-and-society feud since 1989, as described by Stephen Ritter in a recent issue of Chemical and Engineering News (1).The conflict is very unusual in terms of duration, the caliber of combatants, and the deviation from basic principles of scientific methodology of validation of claims. The purpose of this article is to comment on methodological mistakes made during the still-ongoing feud among scientists.

1. INTRODUCTION

Scientific methodology (2) refers to the set of norms developed to deal with mistakes and controversies in scientific research. Most mistakes are recognized when new results are discussed with colleagues, or via the peer review process. Occasional errors in published papers are subsequently discovered during replications conducted by other researchers. Scientific results, if valid, wrote J.R, Huizenga, must be reproducible on demand. “When errors are discovered, acknowledged and corrected, the scientific process moves quickly back on track, usually without either notice or comment in the public press." (3) The process, in other words, is expected to be self-corrective.

The so-called "scientific method" is not a list of divine commandments. It is a set of norms described by scientists, and by those who observe their ways of working. The author of this article is a retired nuclear physicist who has observed cold fusion researchers over the last three decades, and participated in several CF conferences and three cooperative replication projects (in which the claimed sensational results were not confirmed). The CF episode is an unusual controversy resulting from a sensational 1989 announcement made by Fleischmann and Pons (F&P). The event (4,5,6,7,8) divided physical scientists into two feuding camps (9,10,11,12,13,14). This is a rare example of a situation in which the expected self-correction of the scientific process was essentially stopped by two formal governmental interventions.

2. ROLE OF ACCEPTED SCIENTIFIC THEORIES

Why is the CF controversy unresolved? Because CF experimental claims are not reproducible on demand, and because they conflict with the generally accepted theory of nuclear reactions. A theory, in this context, is a logical/mathematical structure that agrees with a wide range of already verified experimental data. Scientists know the rule--theories guide but experiments decide. But they are very reluctant to abandon accepted theories. To be reluctant means to insist on additional verification of new experimental results.

Referring to such situations, Huizenga wrote (3): “There are occasionally surprises in science and one must be prepared for them.” Theories are not carved in stone; scientists do not hesitate to modify or reject them when necessary. Rejecting a claim because it conflicts with a theory is not as convincing as rejecting it on the basis of reliable empirical data. In that sense methods of validation of claims in physical and other sciences are similar. Scientific theories are models of objective reality; they are often changed, or modified, when new facts are discovered.

3. LEVELS OF CONFIDENCE IN SCIENTIFIC CLAIMS

A discovered experimental fact is usually presented to the scientific community, to be independently confirmed or refuted. Experimental results are accepted--at a high level of confidence--when they become reproducible on demand. Absence of such reproducibility justifies suspicion of possible errors or fraud. Methods of validation of theories (explanations of facts) are slightly different.

A new scientific theory is also presented to a community of experts, to be independently evaluated. Their level of confidence in a theory depends on the validity of underlying assumptions and on the rigor of quantitative analysis.

But even a most reliable scientific theory, usually called a law, is said to be falsifiable, in principle, when conflict with reproducible-on-demand data becomes undeniable (15). Such unusual conflict could trigger a scientific revolution (16).

To explain something usually means to identify causes and to construct a logically satisfying model of reality. An attempt to explain a fact, or to resolve an apparent logical conflict, usually leads to discoveries of other facts. A classical example was the discovery of planet Neptune, in 1846. A more recent and less widely known example was the discovery of a subatomic particle named neutrino. Experimental data collected in the 1920's showed that beta rays (electrons emitted in radioactive decay) had lower mean energies than expected on the basis of the theoretical $E=mc^2$ formula. Austrian theoretical physicist W. Pauli solved this "logical inconsistency" by suggesting that tiny neutral particles, later named neutrinos, were responsible for the missing energy. His hypothesis was formulated in 1933. Experiments confirming the reality of neutrinos were performed, 23 years later.

4. UNFORTUNATE TERMINOLOGY

The essence of the discovery announced by F&P was "excess heat." Their small electrolytic cell generated more thermal energy than the amount of energy supplied to it. Trying to establish priority, under pressure from University of Utah administration, the scientists announced their results at a sensational press conference (March 23, 1989). They wanted to study the CF phenomenon for another year or so but were forced to prematurely announce the discovery.

The unfortunate term "cold fusion" was imposed on them (17). Why unfortunate? Because it created the unjustified impression that cold fusion is similar to the well known hot fusion, except that it takes place at much lower temperatures. This conflicted with what had already been known--the probability of nuclear fusion of two heavy hydrogen ions is negligible, except at stellar temperatures. It was a mistake to interpret experimental data before the results were recognized as independently reproducible. In fact, F&P had no evidence for the emission of energetic ^1H, ^3H and ^3He products, listed in their first published paper (4). Such evidence appeared only five years later (22). The only thing they knew was that the measured excess heat could not be attributed to a known chemical reaction. Claiming that the measured excess heat was due to a nuclear process was premature. The adjective "non-chemical" does not automatically translate into "nuclear."

Suppose the discovery had not been named cold fusion; suppose it had been named “anomalous electrolysis.” Such a report would not have led to a sensational press conference; it would have been made in the form of an ordinary peer review publication. Only electrochemists would have been aware of the claim; they would have tried to either confirm or refute it. The issue of “how to explain the heat” would have been addressed later, if the reported phenomenon were recognized as reproducible-on-demand. But that is not what happened. Instead of focusing on experimental data (in the area in which F&P were recognized authorities) most critics focused on the disagreements with the suggested theory. Interpretational mistakes were quickly recognized and this contributed to the skepticism toward the experimental data. Using unconfirmed data to justify the nuclear origin of excess heat, by F&P, was inconsistent with the prevailing norms of scientific methodology. A more recent case of violation of scientific norms, by a CF researcher, Andrea Rossi, is described in (14).

5. TWO US GOVERNMENT INVESTIGATIONS OF CF

The significance of CF, if real, was immediately recognized. Some believed that ongoing research on high-temperature fusion, costing billions of dollars, should be stopped to promote research on CF. Others concluded, also prematurely, that such a move would be opposed by “vested interests” of mainstream scientists. Responding to such considerations, the US government quickly ordered a formal investigation.

A panel of scientists, named Energy Research Advisory Board (ERAB), and headed by John Huizenga, was formed to investigate CF in 1989. The final report (18), submitted to the DOE several months later, negatively interfered with the normal development of the field. Modest financial support for additional CF research, by the DOE, NSF, and other agencies, was practically stopped after the report was published.

It is interesting that only one of the ERAB's six conclusions referred to CF experiments; the remaining five conclusions were about anticipated practical uses of CF, and about various aspects of the suggested interpretation of results. Instead of focusing on reality of excess heat, critics focused on the fact that the hypothesis was not consistent with what was known about hot nuclear fusion. The same observation can be made about the six ERAB recommendations. Only one of them referred to possible experimental mistakes. It is clear that the ERAB observations were based mostly on ”theoretical grounds,” not on independently performed experiments. The unfortunate governmental intervention had one serious and unprecedented consequence--editors of some scientific journals started rejecting manuscripts written by CF scientists, bypassing peer review (19).

The second DOE investigation (20) of CF was announced in March 2004, nearly 15 years after the first. A group of 18 experts was selected to review a very significant CF claim--the correlation between excess heat and generation of helium (21,22).

But the DOE experts were not asked to replicate correlation experiments; they were asked to read the report submitted by five CF scientists (20), and to vote on whether or not the evidence for the claims was conclusive. Such a way of dealing with a controversy was not consistent with the scientific method.

Ideologically and politically motivated rejections of scientific claims are not new. Giordano Bruno and Galileo Galilei are well known examples. Lysenkovism--Stalin's discrimination of geneticists--is a more recent illustration. And cybernetics, in the Soviet book entitled “Short Philosophical Dictionary,” was defined as “bourgeois pseudo-science serving American imperialism.” Ludwik Kowalski accepted this kind of "truth" as a communist student in Poland (23). What can be done to make sure that similar discrimination will not be used in the US, to impose "the truth" about evolution, stem cell research, etc.?

6. CONCLUSION: MORE QUESTIONS THAN ANSWERS

Long-lasting controversies about scientific discoveries are not new. Alfred Wegener's theory of continental drift is a good illustration. Mainstream geologists rejected experimental data supporting his now-accepted theory for half a century. The CF controversy, however, seems to be different both in terms of governmental involvement and in the caliber of adversaries on both sides of the divide. Huizenga and Fleischmann were indisputable leaders in nuclear science and electrochemistry. Most leading CF researchers are PhD-level scientists. The same is true for many scientists who reject CF claims.

The long-lasting CF episode is a social situation in which the self-correcting process of scientific development was not allowed to evolve. To what extent was this due to extreme difficulties in making progress in the new area, rather than to negative effects of competition, greed, jealousy, and other "human nature" factors? Such unanswered questions (24) are worth addressing in the context of debates about science and society.

One thing is undeniable; the world is still waiting for the first reproducible-on-demand demonstration of a nuclear process resulting from a chemical process. No progress is possible when reported experimental data cannot be reliably replicated in other laboratories. Considering potential CF benefits, and relatively low costs of research in this area, the DOE should have helped to resolve the controversy, one way or another, in a well-equipped national laboratory, during the second investigation. But it failed to do so. How can such a policy be explained? Why is CF research allowed to stagnate without financial support? These questions also belong to debates about science and society. Will the past 25 years be recognized as the painful beginning of a new paradigm (16), or will this period be known as pseudoscientific (24) ? How can the persistence of the CF controversy be explained?

7. OPTIONAL ADDENDUM: THREE INTERESTING BIOGRAPHIES

The discovery of CF was made nearly simultaneously by three highly competent scientists: Martin Fleischmann, Stanley Pons (both chemists) and Steven Jones (physicist). Excerpts from their biographies, taken from Mallove's book (25), are shown below.

Martin Fleischmann was born March 29, 1927, in Karlsbad, Czechoslovakia, to Jewish parents. The family came to England to avoid persecution by the Nazis. Martin went to high school in Sussex, England during the war, attended

Imperial College in London after the war (1947-1950), and later distinguished himself by achieving at age forty the professorial Chair in Electrochemistry at the University of Southampton.

Since 1986, Fleischmann has been a Fellow of the Royal Society, an honor given only to the most distinguished of scientists. The author of over 200 scientific papers -- a number of them with Pons as collaborator -- and a number of portions of textbooks, Fleischmann won the Royal Society of Chemistry's medal for Electrochemistry and Thermodynamics in 1979. He was president of the International Society of Electrochemistry (1970-1972). In 1985 he was awarded the Palladium Medal by the U.S. Electrochemical Society.

Stanley Pons, who is almost young enough to be Fleischmann's son, was born in 1943 in the small town of Valdese in the North Carolina foothills Pons's Italian Protestant ancestors had fled religious persecution in the old world... He was drawn to the world of chemistry as a child, as many youngsters had also been, encouraged by parent-bestowed chemistry sets and the like.Pons attended Wake Forest University in Winston-Salem, North Carolina, graduating in 1965, and began advanced studies at the University of Michigan at Ann Arbor. But with his doctorate almost in hand in 1967, he left school to work in his family's businesses. Eventually, his love for chemistry drew him back to active science. With the encouragement of faculty at University of Southampton in England, he entered its graduate program in chemistry and received his Ph.D. there in 1978. Martin Fleischmann was one of his professors. ... Pons came to the University of Utah in 1983 as an associate professor, becoming a full professor in 1986, and Chairman of the Department in 1988. He has authored or coauthored over 150 scientific publications.

Steven Jones was well known to physicists and the hot fusion community, which gave him a credibility that Fleischmann and Pons could not match. Born in 1949, he was raised a Mormon, with all that his religion's outlook and demanding codes of conduct implies. Jones was a missionary in Europe for the Church of Latter-Day Saints and pursues his science with religious fervor, almost literally. His University stationery bears witness, inscribed as it is with the Brigham Young University motto, "The Glory of God Is Intelligence."

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8. Project Management and Future Opportunities

The methods used in the MLRDP are based on general LRDI procedures that are modified to meet the specific requirements of individual LENR investigators[16]. The project is being performed according to accepted project management practices[17]. As noted above, the overall LRDI procedure is set forth in a recent article in Infinite Energy[18].

The KLRDP began with a phone call with Linda Kowalski in August 2020. Memos were then used to document a number of websites relevant to the Project. They are listed in Table 9-1. Ms. Kowalski also sent by email the contents of the memos dated August 31 and September 1, 2020.

The primary opportunity for future progress in the KLRDP is to analyze, interpret and integrate his LENR work in a comprehensive report on his research, insights (LENR explanation) and activities in the field. The materials documented in this report would be the source for a future project with these objectives. As noted in a previous section, Dr. Kowalski's papers and website articles are a resource not only for his LENR work but also for the events taking place in the field generally at that time.

A visit to Dr. Kowalski and Linda is not advisable during the COVID-19 pandemic, but may become possible in the future. A site visit May "turn up" additional records of his LENR research. For example, there may be more articles that have not yet been added to the LCF website.

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Table 9-1.

List of Memos for the Ludwik Kowalski LENR Research Documentation Project

8/23/2020	"Learn Cold Fusion" Website: Essays on Cold Fusion Topics
8/24/2020	"Kowalski" Listings on LENR-CANR.org
8/25/2020	Ludwik Kowalski Collection at Hoover Institution, Stanford University
8/26/2020	Ludwik Kowalski Photos and Biographies
8/27/2020	"Learn Cold Fusion" Website: Full Collection of Essays on Cold Fusion Topics
8/28/2020	Kowalski on New Energy Times YouTube
8/29/2020	Ludwik Kowalski Photos and Biographies
8/30/2020	Ludkow Website
8/31/2020	Cold Fusion Article by Ludwik Published in 2017
9/1/2020	Ludwik Article "421 Oriani's Death and Comments on NAE"
9/2/2020	Ludwik Article: "Cold Fusion Is Not Voodoo Science"
9/11/2020	List of Memos for the Kowalski LENR Research Documentation Project

Appendix A. Biographical Information

A1. Summary of LENR Activities (LCF #403)

COLD FUSION IS NOT VOODOO SCIENCE

Ludwik Kowalski (about the author)

Montclair State University, USA, March 2012

Table of Contents (for this item only)

This website contains other cold fusion items.

Click to see the list of links to other items

1) Introduction

As a retiree I participate in a memoir-writing workshop for senior citizens, directed by Lucile Lichtblau. About eight of us meet each month to read and discuss our compositions. My first two memoirs were based on WWII events; the third was about Cold Fusion (CF). What is CF? I am not ready to answer this question at this point; the answer will emerge from subsequent chapters. For the time being let me say that CF is a highly controversial field of physical science research. It is also known as Condensed Matter Nuclear Science (CMNS), and Low Energy Nuclear Reactions (LENR). These acronyms might be useful to those who are impatient and want to start Googling for answers immediately. These three names will be used interchangeably below.

The controversy started in March 1989, when two university professors in Salt Lake City announced a totally unexpected discovery. Some people think that this was the greatest fiasco of the last century; others believe that this discovery was an important step toward future technology of pollution-free nuclear energy. My short memoir was read at one of our monthly meetings. But it was not well received. Most participants were confused by technical terms. I was advised to focus the essay on personal experience, rather than on science. That is what I did. But what started as a short essay turned into a book.

Seeking a model of clear writing about the topic, I consulted the book "Cold Fusion And The Future." The author Jed Rothwell is a friend. He wrote: "many nightmare problems that seem beyond any present solution, such as global warming, invasive species, and providing clean drinking water and sanitation to billions of poor people, may be remedied with cold fusion combined with other technologies. The future might be better than you think." Jed is not a professional scientist. But he knows enough science to describe it clearly to lay people. His book is freely available online.

Yes, abundant and pollution-free energy would make life on earth better for billions of people, especially in underdeveloped countries. Unfortunately, the world is still waiting for a reproducible-on-demand demonstration of a CMNS effect. The essence of the CF controversy is whether or not a chemical process can trigger a nuclear reaction. Most scientists think that this is impossible. But a small fraction of them, perhaps 100 people worldwide, including myself, continue to conduct experiments whose purpose is to investigate LENR effects.

2) What Is Cold Fusion?

The best way to start explaining CMNS is to refer to so-called "hot fusion," a process in which two atomic nuclei of hydrogen fuse at temperatures exceeding several million degrees. This process generates thermal energy (heat) in hydrogen bombs, and in stars. In the last five decades numerous attempts have been made to turn a hydrogen bomb explosion into a "slowly burning" controllable process. This line of technological research, costing tens of billions of dollars, has not yet produced anything of practical use.

Fusion of atomic nuclei has been studied by physicists since 1930s. We know that such fusion is only possible at extremely high temperatures. Its probability at ordinary temperatures-- that is

below ten thousand degrees or so--is practically zero, due to mutual electric repulsion of atomic nuclei.

That is why physicists were so surprised, in 1989, when two chemists, Fleischmann and Pons (F&P), announced the discovery of cold fusion, presumably similar to hot fusion but taking place at room temperature. The announcement made at the University of Utah press conference created a lot of excitement. The cover page of the Business Week magazine was "Miracle or Mistake: Fusion in a Bottle." Similarly, Times magazine's front page question was "Fusion or Illusion?" Newsweek's front page was also devoted to cold fusion; the title was "The Race for Fusion." F&P had no evidence that measured heat was due to a nuclear process; that was only their assumption.

At that time I participated in a research project at Brookhaven National Laboratory. My work had nothing to do with nuclear fusion. But we debated the F&P's discovery constantly. A chart on a wall was updated each morning, showing how many teams of scientists, worldwide, confirmed the announced discovery and how many reported negative results. Similar debates were taking place in other labs, as I learned later. Most of them were focused on the "theoretical impossibility" of CF, rather than on possible experimental errors. Needless to say, I followed the debates with great interest. But, like most scientists, I came to the conclusion (in 1992) that the F&P claim was not justified.

My renewed interest in CMNS was rather coincidental. In the summer of 2002, I went to a scientific conference in Albuquerque, NM, to hear what nuclear scientists had to say about new ways of dealing with radioactive waste produced in existing nuclear reactors. My wife joined me after the conference. We rented a car and had a wonderful week in New Mexico. Naturally, we stayed in Santa Fe--how could one skip this wonderful place. We also visited the WWII museum in Los Alamos, and Alamogordo, site of the first nuclear explosion, several months before Hiroshima

and Nagasaki. But the conference had a most remarkable and totally unexpected consequence for me--the renewal of interest in cold fusion (CF).

Why unexpected? Because the conference was not about CF. Several reports, however, were devoted to CF topics. I listened to them very carefully and talked with scientists who described new results. I was very impressed by their credentials, and by the fact that they were debating experiments, not interpretations. That is why I decided to get re-acquainted with developments in the field, 13 years after the controversy started. The purpose of this book is to describe my CMNS-related activities since the Albuquerque conference. I have met many interesting scientists, attended four international CF conferences, participated in several research projects and published several papers in that field. This has been chronologically recorded, more or less regularly, at a dedicated website:

http://pages.csam.montclair.edu/~kowalski/cf/

Some items at this website are more technical than others. But most of them can be comprehended by people who studied physics and chemistry in high school.The readers of this book are also expected to be familiar with elementary science.

3) Meeting a Russian Scientist, Alexander Karabut

The scientist who impressed me the most in Albuquerque was Alexander Borisovich Karabut, from Russia. F&P, as mentioned in Chapter 2, had no evidence that their excess heat was due to a nuclear process. They suspected that excess heat was nuclear because it was too large to be due to a known chemical reaction. Karabut, and his team, by contrast, reported not only excess heat, generated at the rate of about 9 watts, for 120 hours, but also the presence of several nuclear effects. This was a revelation to me.

The team spent ten years studying nuclear processes associated with generation of excess heat at ordinary temperarures. His talk at Albuquerque was the summary of findings; some of them had been reported as early as 1990. As a Russian speaker I was able to help the author improve his presentation in English, a language in which he is far from fluent. We talked about his paper before it was formally presented, and we discussed it afterwards. What I heard in Russian was much clearer than in his English text. The link to my translation is:

http://csam.montclair.edu/~kowalski/cf/13karabut.html

One of the effects, reportedly observed by Russians, was emission of high-energy alpha particles from the palladium foil saturated with heavy hydrogen. This effect alone, if independently confirmed by other scientists, would be sufficient to validate the idea that a nuclear process can be triggered by a chemical process at low temperature. Why had no one tried to replicate experiments described by Karabut? That question still puzzles me. Whose moral obligation was to it to verify such extraordinary results? The most obvious people were other CMNS experimentalists. But each of them worked on his or her own project, usually without any financial support. Furthermore, confirming a discovery made by someone else is not as rewarding as being recognized as the discoverer of something unknown and important.

I can only imagine how Karabut's discoveries would have been treated in Stalin's USSR. The Academy of Sciences would at once have organized several replications. Confirmation of results would turn Karabut into a famous scientist. Rebuttal of the results, on the other hand, would have led to immediate disqualification, or much worse. But that is not what happed in post-USSR Russia. The scientific establishment, associated with the Academy of Sciences, declared Karabut a pseudo-scientist. This was not based on new experimental results; it was based on theoretical grounds--the reported facts conflicted with the already-accepted theory of hot fusion reactions.

The main accuser, according to Karabut, was the Academician E.P. Kruglakov, the author of "The Highwaymen of

Science." This book, published in 2001, is indeed very interesting; Karabut sent it to me, after returning to Moscow. The Russian scientific establishment, according to Dr. Karabut, considers

cold fusion to be voodoo science. In fact, Dr. Kruglakov heads the "Commission to Oppose Pseudo-Science and Falsifications in Scientific Research." I had no idea that such a commission had been created by Russian Academy of Sciences. The book ranks cold fusion at the same level as N rays (a well known case of either fraud or self-delusion in France), astrology, extrasensory perception, and magic. Karabut hinted that the antagonism against CMNS in Russia has more to do with the competition for very limited financial support than with objectivity.

I can also imagine how our own government would have reacted to CMNS discoveries published by Russian scientists during the Cold War. The DOE (Department of Energy) would have quickly organized several replications, in order not to be left behind. Successful replications would probably have been classified and additional research would have been sponsored by the DOE, at various laboratories. Refutations, on the other hand, would provide evidence that results reported by Karabut should not be taken seriously. It is interesting that a book similar to Kruglakov's was published in the US: "Voodoo Science: The Road From Foolishness to Fraud" by a physicist, Dr. Robert Park.

4) Meeting George Miley

Dr. George H. Miley, a chemical and nuclear engineering professor from the University of Illinois, was another scientist I met at Albuquerque. His conference paper made me aware of how much I had missed since I had stopped paying attention to the CMNS field. A very impressive summary of Miley's professional accomplishments can be found in Wikipedia. Before 1989 he was a hot fusion researcher; afterward he became a CF researcher as well. In 1990 he published a paper, in cooperation with another researcher, about production of chemical elements in thin layers of metallic films saturated with hydrogen. They wrote: " the Ni film [removed from our experimental cell] was found to contain Fe, Ag, Cu, Mg and Cr." Concentration of these elements, after the experiment, was found to be much higher than before. If confirmed this would be undeniable evidence for nuclear reactions taking place at low temperatures.

But the most interesting Miley paper, as far as I am concerned, was published in 2002, the year I met him. The title was "Some Personal Reflections on Scientific Ethics and the Cold Fusion 'Episode' ". Unfortunately, he did not tell me about this paper in Albuquerque. I read it several years later; it is now available online. The link is:

http://www.lenr-canr.org/acrobat/MileyGHsomeperson.pdf

Most papers written by CMNS researchers are devoted to scientific and technical topics, as one can verify by going to the online library at

www.lenr-canr.org

That library, by the way, was created by Jed Rothwell. George Miley's long paper is devoted to political aspects of the controversy. Here is a brief summary. My encounter with CF, he wrote, "was at the initial congressional hearing in Washington D.C. on the topic. I was selected to

provide input from a fusion researcher known for innovative research who might comment on CF from a 'neutral position'. Thus, I was 'squeezed' into the testimony order between the originators of the field, Pons and Fleischmann, and a strong opponent of CF, Harold Furth, the then director of the Princeton Plasma Physics [hot fusion] Laboratory."

In his talk Miley speculated about possible future CF developments. After the hearing, he said that "a CIA agent caught me in the hall and warned that someone like myself with a 'Q clearance' should not publicly air such sensitive speculation. As it turns out, my speculation had some validity. Describing other 1989 meetings, Miley wrote that "almost 'carnival atmosphere' was created by the combination of reporters, entrepreneurs, garage inventors, curious on-lookers, politicians, financial brokers, and scientists at the initial Los Alamos National Laboratory [...]

Then there was the 'famous' NSF-EPRI meeting in Washington DC where the NSF ended up withdrawing 'official' sponsorship at the last moment due to the swing in opinion against CF. Despite this controversy, Edward Teller [known as the father of the first hydrogen bomb] attended this meeting in a wheel chair (due to a recent operation) and provided a guiding example of an open scientific mind by freely entering the discussion. Instead of ruling CF out due to lack of theoretical explanation, he suggested that a new particle, dubbed “meshuganon,” would be needed (and might actually exist) to explain the observations reported by Pons and Fleischmann. [...].

Looking back, he continues, "the CF field has caused grief for many key persons who became 'too' strongly involved. Pons and Fleischmann left the US for France [...]; the President of the University of Utah was forced to resign as a result of issues raised about CF funding procedures [...], John Bockris at Texas A&M, was bombarded with University-appointed investigating committees and, as a 'crowning blow' was forced off-campus with the second

International Meeting on Low Energy Nuclear Reactions that he hosted. Gene Mallove found it necessary to step down from his scientific information post at MIT following publication of his book, Fire from Ice. Peter Hagelstein faced a hostile promotion committee at MIT after his early theoretical work on CF; [...] Why should such intense controversy and drastic personal repercussions develop over a scientific field?

Certainly the unconventional manner in which Pons and Fleischmann introduced CF by announcing it to the press initiated the controversy which eventually polarized the field into camps of 'believers' and 'non-believers'. The fundamental reason behind this emotional approach to CF was, in my view, the tremendous impact that CF, if proven true, could have. Consequently, the vast amount of money and the prestige at stake brought out the 'best' and the 'worst' in people. [...]

5) Beginning Of Censorship: Also From Miley's Paper

The paper from which I am quoting is worth reading in its entirety. It is a rather unique testimony from an open-minded researcher and editor, caught in the middle of the 1989 CF controversy. In one section he writes: " Another criticism of my editorial policy on CF has been

that since I have done research on the topic, I must be biased in favor of it. It's true that I have had papers in most ICCF meetings, starting from the original LANL meeting in Santa Fe.

This criticism, in my view, amounts to a double standard. My initial selection as FT's [FusionTechnology] editor, and the other two journals, was based on my recognized research on fusion, lasers, and plasma physics.

This track record was assumed to provide me with better insight into the technical content of the papers, and allow me to select top reviewers. In universities, teaching and research are well recognized as reinforcing each other. The same is certainly true for editing and research. Why wouldn't the same be true for CF? [...]

In conclusion, the issue of whether my FT position, as opposed to Nature's closed-door policy, is proper for a scientific journal must be left to the reader. The question to be answered, in my opinion, is which policy will advance science best in the long run? To rephrase the question, we might ask if the publications in FT have communicated new scientific information or have they mislead readers? [...]"

In March of 2012, George Miley was again at the center of the CMNS controversy. This time he appeared as a designer of something very practical--a CF nuclear battery. Chemical batteries do not last long enough to power electrical equipment in a spaceship. That is why nuclear batteries, containing highly radioactive plutonium, have been used in long-lasting NASA missions. Miley is developing a battery based on a CF effect. Such battery, using non-radioactiv and less expensive hydrogen and palladium instead of plutonium, would indeed be highly desirable. A brief announcement of his invention can be found at:

http://nextbigfuture.com/2012/03/george-miley-upcoming-presentation-game.htm

6) Theories Guide But Experiments Decide

Scientific methodology of validation of claims, a set of rules developed to deal with difficulties, mistakes and controversies is well known. Most scientific mistakes are recognized when new results are discussed with colleagues, or via the peer review process. Occasional errors in published papers are subsequently discovered during replications conducted by other scientists. Our results, if valid, wrote one scientist, John Huizenga, must be reproducible on demand. "When errors are discovered, acknowledged and corrected, the scientific process moves quickly back on track, usually without either notice or comment in the public press." The scientific process, in other words, is self-corrective. The process might be slow but it works, more often than not.

Why is the CMNS controversy unresolved since its beginning in 1989? Because the claims are still not reproducible on demand, and because experimental results conflict with the accepted theory of nuclear fusion. A theory, in this context, is not just a hypothesis, it is a logical structure that is known to agree with a wide range of already verified experimental data. Scientists know

the rule--theories guide but experiments decide. But they are very reluctant to abandon accepted theories. To be reluctant means to insist on additional verifications of new experimental results.

Referring to such situations, Huizenga wrote: "There are occasionally surprises in science and one must be prepared for them." Theories are not carved in stone; scientists do not hesitate to modify or reject theories when necessary.

Rejecting a highly reproducible experimental result "on theoretical grounds," which is quite common, is not consistent with scientific methodology. But that is exactly what often happens when CMNS claims are criticized.

John Huizenga, one of recognised leaders of the field known as Nuclear Chemistry, was a senior colleague, when I was a post-doctoral researcher at Columbia University. He often visited us and I had the priviledge of discussing topics of common interest with him. His book about CMNS, "Cold Fusion: The Scientific Fiasco of the Century," persuaded me that F&P claims were not valid. This was nearly ten years before the 2002 Albuquerque conference.

My comments on the process of scientific development in general, and on the CMNS field in particular, can be found in Chapter 23.

7) First US Government_Investigation

The significance of CF, if real, was immediately recognized. Some believed that ongoing research on high-temperature fusion, costing billions of dollars, should be stopped to promote research on CF. Others concluded, also prematurely, that such a move would be opposed by “vested interests” of mainstream scientists. Responding to such considerations, the US government quickly ordered a formal investigation. A panel of scientists, named ERAB (Energy Research Advisory Board), and headed by John Huizenga, was formed to investigate CF in 1989. The final report, submitted to the DOE several months later, interfered with the normal development of the field.

I was later disappointed to learn that ERAB scientists investigating the CF claims were not personally involved in replications of experiments. Conclusions and recommendations from their report, based on visits to several laboratories rather than participation in experiments, are summarized in a paper I published recently

http://www.ptep-online.com/index_files/2012/PP-29-L2.PDF

Only one of their six conclusions refered to CF experiments; the remaining five conclusions were about anticipated practical uses of CF, and about various aspects of the suggested interpretation of results. Instead of focusing on reality of excess heat, critics focused on the fact that the hypothesis was not consistent with what was known about hot nuclear fusion. The same observation can be made about the six ERAB recommendations. Only one of them referred

to possible experimental mistakes. It is clear that the ERAB observations were based mostly on ”theoretical grounds,” and not on identified laboratory mistakes. Support for CF research in the US practically stopped in 1989.

Another result of the first DOE investigation, as described by Miley, was that editors of some scientific journals started rejecting manuscripts writen by CF scientists, bypassing peer review. This kind of discrimination, directed against PhD-level scientists, is totally inconsistent with scientific norms. Illustrations of such descrimination are to be found in chapters 17 and 18.

Inserted on 5/24/2012

Here is message about CF that I posted last night at our university forum.

"As shown in < http://www.ptep-online.com/index_files/2012/PP-29-L2.PDF> , the first two DOE investigations of CF claims failed to focus on what was the most important--promotion of experiments designed to verify reality of excess heat. The DOE refuted F&P claims on the "theoretical basis," that is on the conflict between the reported fact and the hot fusion theory.

What would I do if I were appointed to organize the first DOE investigation, in 1989? Familiar with Fleischmann's undeniable reputation I would offer him a generous research grant, for two or three years, to continue CF research. I do not think that F&P's motivation was anything else but desire to be recognized as discoverers of something new and important. They would probably welcome the grant and cooperated with the government. Unwillingness to cooperate (in order to protect secrets, pending patent applications, etc.) would show that the claim is commercial and does not justify the grant."

Knowing that the government grant was accepted, I would find two knowledgeable electrochemists and made them responsible for cooperation with F@P. The purpose of cooperation would be to help them to clearly describe the protocol, and to publish it in a peer reviewed journal. A year or two later (if the controversy were still not resolved via the normal self-correcting process) I would select ten other reputable electrochemists (Energy Research Advisory Board), working in government laboratories, and asked them to replicate the F&P experiment, using the published protocol. The formal report, prepared by ERAB chairman, would be mostly based on available experimental data, not on the fact that results conflict with our model of thermo-nuclear reactions.

P.S. Yes, I know that no one will ask me for something like this. It is only a speculation, based on past mistakes." Responding on the forum a prominant CF researcher wrote:

"Of course Ludwik, any one who was interested in benefitting from LENR would have treated the situation the way you describe. The people at the DOE are not stupid or ignorant. They know how to do a proper review. The fact that the DOE did not act this way reveals that they were not interested in finding out the truth. They wanted the possibility of LENR being real to be eliminated. This phenomenon is a threat to all that the DOE values. When the DOE wants a program to be supported, they work very hard and ruthlessly to see that this happens. They did just the opposite with respect to LENR. This was a result of well considered policy. That is why an attempt to contact Sec. Chu or anyone in the DOE who has authority will not change their approach. They want LENR to fail for obvious reasons. They just don't want to appear to reject the idea, so they created these sham reviews. "

Some one could say that this is a conspiracy theory. Saying "a reasonable hypothesis," rather than "a theory" would probably be more appropriate. A formal theory should be based on specific assumptions--for example, already accepted theories or data--and a logical derivation of the final statement.

8) Second US Government_Investigation

The second DOE investigation of CF was announced in March 2004, nearly 15 years after the first one. The six most important scientific questions, based on new experimental CMNS claims, were:

(a) Is it true that unexpected protons, tritons, and alpha particles are emitted in some CF experiments?

(b) Is it true that generation of heat in some CF experiments is linearly correlated with the accumulation of 4He, and that the rate of generation of excess heat is close to the expected value of the 24 MeV per atom of 4He?

(c) Is it true that highly unusual isotopic ratios have been observed among the reaction products?

(d) Is it true that radioactive isotopes have been found among reaction products?

(e) Is it true that transmutation of elements has occurred?

(f) Is the research methodology of CF scientists the same as that used by other scientists? In other words, is it consistent with the generally accepted norms of the so-called "scientific method?

A positive answer to even one of these questions would be sufficient to justify an official declaration that cold fusion, in light of recent data, should be treated as a legitimate area of research. But only the (b) question was addressed by the selected referees. They were asked to review the available evidence of correlation between the reported excess heat and production of fusion products. One third of these referees stated that the evidence for such correlation was conclusive. That was not sufficient; the attitude of the scientific establishment toward cold fusion research did not change.

Scientific disagreements are not supposed to be resolved by voting. Why was the reconized methodology of validation of claims--theories guide but experiments decide--not followed by the DOE-appointed scientists? Why did “rejections on theoretical grounds” prevail? The only answer I have is that scientists are not ideal; competition among them, as among people in other social groups, has both positive and negative effects.

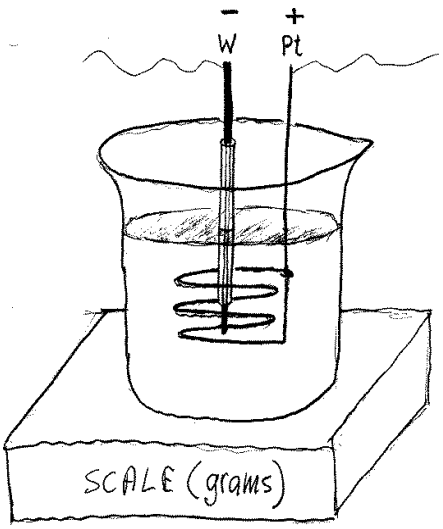
Cold fusion will certainly be viewed as an interesting episode in the history of science, regardless of verdicts about validity of numerous CMNS claims. More specifically, the long-lasting CF episode will be remembered as a social situation in which the self-correcting process of scientific development was not allowed to flourish. To what extent was this due to extreme difficulties in

making progress in the new area (without financial support from the DOE, NSF, etc.), rather than to negative effects of competition, greed, jealousy, and other "human nature" factors?

9) Excess Heat, Real Or Apparent?

The 2005 scientific conference in Japan (ICCF12), in which I participated, was devoted to CF. One French scientist, Pierre Clauson, described a high voltage electrolysis process in which excess heat was said to be generated at the rate of about 100 watts. After some hesitation I decided to verify this claim, with a colleague from Texas, Scott Little. The experiment turned out to be more difficult than I expected; we were not able to confirm or refute reality of excess heat.

But in a subsequent 2007 experiment, conducted with Richard Slaughter in Colorado, and Pierre who came to guide us, the excess heat was apparently detected. Why do I say "apparently"? Because the wattmeter we used, brought by Pierre from Paris, was later found to be inappropriate for our setup. The excess heat turned out to be zero when a more sophisticated wattmeter was used (in Paris, by Pierre and his partners.)



The diagram of our cell is shown above. Here is how the setup was described in my later conference presentation: "[... Our] cell operating under such conditions is shown in Figure 1. The cathode was a tungsten rod while the anode was a large platinum wire spiral, or a platinized niobium cylinder. The electrolyte was potassium carbonate (K2CO3)

dissolved in distilled water. The concentration was 20 grams per liter. Decomposition of water, at high current, was so intense that yellow glow discharge and arcing was taking place in the layer of gas-plasma surrounding the cathode."

The scale supporting the cell was used to measure the amount of water evaporated in each experiment. That allowed us to calculate the amount of heat generated. Instruments used to measure electric energy are not shown in the figure.

Let me mention that Clauson's results presented at the conference were a confirmation of similar results reported by a Japanese scientist, Tadahiko Mizuno, two years earlier. In 1997 Mizuno wrote a book entitled "Nuclear Transmutations: The Reality Of Cold Fusion." It was translated into English by Jed Rothwell, our mutual CMNS friend. Detecting excess heat generated at the rate of 100 W was rather unprecedented; most often measured rates, in F&P type of cells, are 1 W and less. Before going to the conference in Japan--during a sabbatical leave-- I made an arrangement for working with Mizuno. But the plan had to be canceled, due to some administrative complications.

Instead of working with Mizuno I was able to work with another top Japanese scientist, J. Kasagi. But this was not a CF project.

Many people reported generation of excess heat. But non of them claimed that excess heat experiments are reproducible on demand. The situation facing researchers in this area is well described by Mike McKubre, at a cold fusion conference-ICCF15 in Rome. The link to his report is

www.enea.it/it/produzione-scientifica/pdf-volumi/introduction-iccf15-proceedings-2.pdf

Mike is an electrochemist. I know him personally and I have no doubt that he is honest. In fact, he is a hero, in my view. How many people would be willing to continue studying CF for more that two decades under showers of insults.

I think that he is motivated by the noble desire to help society. Unfortunately, I did not attend ICCF15 in Rome in 2009. But I did participate in, and contributed to, four other conferences (USA-2003, France-2004, Japan-2005 and USA-2006). My impression was, and still is, that most CF researchers (not all) are like Mike, and that their methodology of validation is scientific.

Great scientists I have met, including Joliot-Curie, who introduced me to research, would agree. The difficulties described by Mike are real. What do I mean by showers of insults? I will explain this in a later chapter. Download Mike's paper and read it carefully; scientists among you will probably agree that there is nothing unscientific in it.

Speaking about irreproducibility I often refer to the following personal experience. I was heating milk in a microwave oven for two minutes. Then I removed the cup and inserted a cold spoon into it. At that moment I observed sudden “explosive boiling;” It was a clear indication that superheated milk was created. I tried to reproduce this next day, and several days later, using the same oven, the same cup, the same amount of milk, etc. but without success. Does it mean that my observation was not real? I do not think so. It only means that some important factors, perhaps the air temperature or pressure were different in subsequent experiments. Or perhaps the rate and angle at which the spoon was inserted into the cup were not exactly the same. Cold fusion phenomena seem to depend on factors which are hard to identify.

10) Robert Park, A Scientist Writer

George Miley's observations can be contrasted with those of Robert Park, whose book "Voodo Science: The Road from Foolishness to Fraud," published in 2000, has already been mentioned. Referring to this book, one reviewer wrote: ,"Professor Park does more than debunk, he crucifies. You'll never again waste time or your money on

astrologers, quantum healers, homeopaths, spoon benders, perpetual motion merchants, or alien-abduction fantasists." But isn't CF different from the above? I don't exclude the possibility that some CMNS claims may have been fraudulent; con artists are naturally attracted to scientific controversies, as illustrated in:

http://csam.montclair.edu/~kowalski/cf/16voodoo.html

George Miley told me about the 9th International Connference on Cold Fusion, ICCF9. It took place in China, where many new discoveries were presented and discussed. The next conference, ICCF10, was to take place in Cambridge, Massachusetts and I decided to attend. One of the organizers of this conference told me that Robert Park was personally invited but decided not to come. That was an insult, in my opinion. What I would do in his place? I would welcome the chance to meet the authors of questionable claims, to discuss controversial topics and to ask for evidence. Dr. Park's refusal to participate disqualified him in my eyes. Those who accuse others of being pseudo-scientsts should have the courage to face their opponents.

In a review of Park's book at: http://home.netcom.com/~storms2/park.html Edmund Stoirms wrote:

[But to Park] the explanation becomes more important than the observation. Because this particular explanation

can not be believed, the observation must also be rejected. Thus, a major flaw in modern science is revealed - a Theory is more important than an Observation. The behavior of nature is not real unless it can be explained, especially using conventional concepts. This flaw in logic is at the heart of the book and provides an explanation for rejection of these and other subjects by many scientists.

New discoveries always conflict with some dearly held belief. This conflict when used to reject the claims, prevents new discoveries from being explored and properly explained. This is not to say that all 'strange' ideas are correct or

that all have a new and worthwhile explanation. Clearly, some should be rejected as being caused by obvious error, fraud, or simple insanity. The problem comes in deciding how much time and resource should be devoted to a search for an explanation and how the resulting facts should be evaluated. If science is to clean up its act, this defect in

the approach scientists use needs to be addressed. A clear and extensive discussion of this general problem can be found in the book "Revolution in Science" by J. Bernard Cohen (1985) or "Forbidden Science" by Richard Milton (1994)."

Robert Park is not alone in his bias against CF scientists on the basis of authoritarian pronouncements made by DOE investigators. He apparently does not need to hear the CF

reports, or perform his own experiments. Fortunately, such an attitude does not prevail among mainstream scientists, but it is common. Aggressive discrimination against CMNS reminds me of something else. Long ago, when I was a communist student in Poland, I believed that genetics was pseudoscience. That was the official party line, supporting Lysenko's teaching. The same was true about cybernetics; it was defined for us as "bourgeois pseudo-science serving American imperialism." Naturally, no one is takes such statements seriousey today, even in Russia. But disagreeing with them could have been dangerous, when Stalin was alive.

11) Three Professional Biographies

These short professional biographies of Fleischmann, Pons (chemists) and Jones (physicist) appear on pages 46-49 of

E.F. Mallove's book : "Fire from Ice; Searching for Truth Behind the Cold Fusion Furror," John Wiley & Sons, New York, 1991. I strongly recommend this book.

Martin Fleischmann, now a naturalized British subject, was born March 29, 1927. [...] Since 1986, Fleischmann has been a Fellow of the Royal Society, an honor given only to the most distinguished of scientists. The author of over 200 scientific papers [...], Fleischmann won the Royal Society of Chemistry's medal for Electrochemistry and Thermodynamics in 1979. He was president of the International Society of Electrochemistry (1970-1972). In 1985 he was awarded the Palladium Medal by the U.S. Electrochemical Society.

Stanley Pons, born in 1943, attended Wake Forest University in Winston-Salem, North Carolina, graduating in 1965, and began advanced studies at the University of Michigan at Ann Arbor. But with his doctorate almost in hand in 1967, he, the eldest of three brothers, left school to work in his father's prosperous textile mills and to manage a family restaurant in North Palm Beach, Florida. Eventually, his love for chemistry drew him back to active science.

With the encouragement of faculty at University of Southampton in England, he entered its graduate program in chemistry and received his Ph.D. there in 1978. Martin Fleischmann was one of his professors. After being on the faculty at Oakland University in Rochester, Michigan, and the University of Alberta in Edmonton, Pons came to the University of Utah in 1983 as an associate professor, becoming a full professor in 1986, and Chairman of the Department in 1988. He has authored or coauthored over 150 scientific publications.

Steven Jones was well known to physicists and the hot fusion community, which gave him a credibility that Fleischmann and Pons could not match. That Jones came out with a dissimilar but closely related item of cold fusion news at about the same time, ironically, may have boosted the credibility of Fleischmann and Pons in their claims. But there was initial confusion about what Jones was asserting, because of his well-known earlier work on cold fusion of a different sort -- the concept called muon-catalyzed fusion.

Much of the difficulty that ensued between Fleischmann and Pons on one side and Jones on the other -- a friction that has now lessened considerably -- can be understood in part from a chasm

of personality differences. [...] Jones pursues his science with religious fervor, almost literally. His University stationery bears witness, inscribed as it is with the Brigham Young University motto, 'The Glory of God Is Intelligence.' " Yes, Jones' research was focused on nuclear reactions, not on excess heat. He was fully aware that excess heat associated with reactions he was studying

was too small to measure. The discovery of excess heat was announced by F&P only.

In my opinion that discovery, after being verified several times, should have been announced, more or less, in this way: 'we know that the excess heat is produced in our cells but we have no idea what process is responsible for it.' The approach would be --'let us agree on facts before discussing conceivable interpretations.'

Some think that discovering an experimental fact without giving some kind of explanation is not a scientific event. I cannot agree with this. Electric batteries, for example, invented in the early 1800s by Alexander Volta (Italy), became valuable long before their operation was theoretically explained. The same can be said about the accidental discovery of X rays, in 1895 by Wilhelm Roentgen (Germany). And who would believe that radioactivity of uranium, discovered in 1897 by Henry Bequerel (France), would lead to atomic bombs and to electric power plants, half a century later?

Will CF lead mankind to new technologies in the next century? It's possible but not certain.

12) The First CF Conference I Attended

One year after the Albuquerque conference I attended the conference devoted to CMNS topics only. Such conferences have been taking place each year, in different countries, since 1990. This was the tenth one (ICCF10); it took place in Cambridge, USA. I did not come to the conference empty handed. But my presentation, entitled "The Dilemma of a Physics Teacher," had nothing to do with specific CMNS claims; it was about how to deal with CF claims in classroom situations.

In the first half of my talk I said: "This presentation is dedicated to a high school chemistry student who sent me an e-mail message last spring. She wrote: 'Help! My name is Maggie Johnson and I am a sophomore at Saratoga High School. In my chemistry class, I am doing a project on Cold Fusion. I was looking on the Internet for websites on Cold Fusion, and I came across links to your Cold Fusion items. I was wondering if you could give me some advice or information?'

A year ago I would have replied that cold fusion is pseudoscience. But I am no longer comfortable with this kind of reply. Why am I not comfortable? My first opinion was based on Huizenga's famous ERAB report. I knew the author personally and I respected him. His criticism of cold fusion was convincing because it was based on the idea that cold fusion is a thermonuclear reaction between only two colliding hydrogen ions. Experimental data certainly

do not support such an idea. [... They seem to indicate that a much larger number of atoms, in a crytal, participate in each CF event]

Two other factors helped to discredit the cold fusion field in many minds: (a) the claim that experiments in this area are extremely simple, and (b) that practical applications are going to be possible very soon. Again, I do not know who the authors of such claims were. Those who criticize cold fusion today, Park in the US and Kruglyakov in Russia, essentially repeat Huizenga's arguments. What was convincing in 1989 is no longer convincing today. Why do they ignore generation of helium? Why do they ignore more sophisticated calorimetry? Why do they ignore unnatural isotopic ratios? Why are they not at this conference listening to presentation of new data and defending their own ideas? That is another set of questions that I am not able to answer. Ignoring experimental data is not an acceptable method of addressing a scientific controversy. I am still not convinced that cold fusion is real. But I no longer say that it is voodoo science.

Why am I still puzzled? Because everything I know about nuclear science goes against the idea that nuclear reactions can be induced by chemical processes at ordinary temperatures. I wish I had a chance to personally participate in experiments generating extraordinary results. But, like most teachers, I have no access to a sophisticated laboratory which would be needed to verify accumulation of helium and heavier reaction products. I read about such phenomena and I am impressed. But I would be more comfortable if the reported results were examined and officially confirmed by an appointed panel of open-minded experts.

I am also puzzled by the fact that hundreds of sophisticated research scientists exploring cold fusion over the past 13 years have not yet developed a reliable demo for teachers; windows of opportunity did exist in several countries.

Teachers need experiments that can be performed with simple instruments available in colleges and universities, such as Geiger counters and gamma ray spectrometers. Excess heat generated at a rate of about one watt is not convincing unless one is able to deal quantitatively with all possible chemical reactions taking place in the apparatus. I am not a chemist.

Reproducible generation of excess heat at the level of twenty watts, or higher, for a long period of time, would be much more convincing to a physics teacher, especially if it could be correlated with emission of nuclear particles or gamma rays. Even a 70% reproducible demo would be useful; teachers know that some experiments, for example in electrostatics, do not succeed when humidity is too high. Cold fusion seems to depend on factors which have not yet been identified. Abnormal isotopic ratios, reported by many independent researchers, are extremely convincing but a typical teacher can not verify such data. [...]

I am optimistic that the cold fusion controversy will be resolved, one way or another. The optimism is based on the following quotation from what John Huizenga, the author of the ERAB report, wrote in 1989. 'The scientific process is self-corrective. This unique attribute sets science apart from most other activities. The scientific process may on some occasions move slowly, sometimes even along a circuitous path. The significant characteristic of the scientific method,

however, is that in the end it can be relied upon to sort out the valid experimental results from background noise and error.'

And here is another quote from the panel of appointed scientists responsible for the first national investigation of cold fusion. 'The Panel recommends against the establishment of special programs or research centers to develop cold fusion. However, there remain unresolved issues which may have interesting implications. The Panel is, therefore, sympathetic toward modest support for carefully focused and cooperative experiments within the present funding system.' Sympathetic attitude toward unresolved issues is worth emphasizing.

What will the verdict of history be? Sooner or later, perhaps in 50 years, the cold fusion puzzle will be resolved (like the 'puzzle of cybernetics,' or the 'puzzle of genetics,' both in USSR). Only two outcomes are possible: (a) CF phenomena will finally be confirmed or (b) CF phenomena will not be confirmed. In each case one will have to deal with important social issues. Suppose that CF is confirmed. Then one would have to explain causes of a long-lasting conflict between scientists and administration. Suppose that CF is not confirmed. Then one would have to explain a phenomenon of massive self-deception involving hundreds of top scientists in many countries. In either case you will be recognized as participants of an important and unique event in the history of science.

Keep working to clarify the most intriguing scientific and social puzzle of the 20th century. I am certainly not the only physics teacher waiting for a consensus on cold fusion. Keep submitting good papers to traditional refereed journals, such as Physical Review, etc. Do not be discouraged by frequently unjustified rejections of your papers. Document such rejections and make them known to mainstream scientists. Deplorable confrontations with overly-bureaucratic editors should also be exposed. Take advantage of the new electronic journal devoted to cold fusion. Dissociate yourself from voodoo scientists and openly criticize them. Keep bringing cold fusion topics to scientific conferences devoted to areas overlapping with your activities. My own interest in cold fusion was reawakened at such a conference one year ago. Try to seek contacts with students, and with the general public. But focus on puzzling scientific results; it is too early to speculate about practical applications."

13) The Editor Of Physics Today Rejected My Letter

Discrimination against CF manuscripts, by editors of major scientific journals, has been described by George Miley, in Chapter 5 above. Let me describe my own experience with this kind of unjustified bias. It is also a long quote from my

ICCF10 talk. In the second half of that presentation I said:

"About half a year ago I wrote a letter to the editor of Physics Today. In that letter I described my own dilemma, as a teacher, in dealing with cold fusion, and asked for help. Why was my short letter rejected? Why was I not allowed to see what the referees wrote about it? Ironically, that letter was triggered by the article entitled 'New American Physical Society's Ethics Guidelines.' That article by Jim Dawson was published in the January 2003 issue of Physics

Today." Why "ironically? Because what they did was an example of behavior inconsistent with the article they published.

"I welcomed the new guidelines and asked how a physics teacher can make sense of 'cold fusion?' Was the research conducted in that area, in the last ten years, a 'departure from the expected norms of scientific conduct'? Did it 'lead other scientists along fruitless paths?' I see no evidence that the data were 'fabricated.' As a physics teacher I am confused by the situation. Some say CF was 'a fiasco' while others say it was an 'important discovery.' How should teachers address this topic in the context of 'public affairs between science and society,' or in the context of discussing 'institutional support for new ideas and innovations?'

After waiting several months I sent an e-mail message asking about the status of my letter. On Thursday, June 12, 2003, I received a reply from Letters Editor at Physics Today. He wrote: "So far, I have a split decision on the possible publication of your letter. I expect soon to have a tie-breaking input from a third reviewer. I will let you know as soon as I have a firm decision. Thank you for your patience." On Thursday, July 3 Dr. Hanna wrote "We have completed our review of your letter commenting on the APS ethics guidelines story in our January 2003 issue. Our decision, after some valuable discussion, is not to publish your letter. Thank you for writing and for your interest in Physics Today."

Unhappy about this I wrote "I would very much appreciate if you could send me the reports of the referees evaluating my letter to the editor." The immediate reply was: "Please let me explain. I know that scientists who submit articles to peer-reviewed scientific journals expect reviewers to give them a critique of their letters. Physics Today is not, in the strictest sense, a peer-reviewed scientific journal; it is, instead, a special-interest magazine for physicists. Generally, my reviewers are staff writers and editors (all physicists) who may give me little more than 'thumbs up' or 'thumbs down' on a letter. As a rule, we do not give out the specific comments of the reviewers, because we consider them to be internal business. Thank you for your inquiry. I hope my explanation has helped."

That was the end of our correspondence. Why was it not obvious to them that my letter to the editor was not about specific CMNS claims. I hoped that readers of the journal will comment on my 'special-interest" plea, advising me how to resolve the dilemma. That what Letters To The Editor are for. I often benefit from reading that section each month. Why did the editor decide to deprive me of this valuable resource? How can this censorship be justified in the context of 'New American Physical Society's Ethics Guidelines'? Something was not right. My experience was consistent with George Miley's observations.

14) Meeting Fleischmann And Jones

I was lucky to personally meet Martin Fleischmann and Steven Jones, whose research triggered the CMNS controversy. Fleischmann's presentation at ICCF10 (the first CF conference I attended) was based on what happened in the past. I was surprised to hear that he was motivated by profound theoretical consideration, and by results of experiments performed half a century

earlier. At the end of the talk he said: "I believe that the work carried out thus far amply illustrates that there is a new richly varied field of research waiting to be explored." Jones made three presentations based on work in progress. Fleischmann was kind enough to allow me to be photographed in with him. He is wearing glasses on the photo shown below.

Jones and his coworkers presented two papers devoted to detection of nuclear projectiles from titanium foils saturated with heavy hydrogen. The first presentation was devoted to energetic protons, the second to emission of energetic neutrons. Instruments they used were similar to those I used in my post-doctoral studies. That is why I approached Jones and asked for numerous details. At the end of this conversation Jones invited me to visit his laboratory at Brigham Young University. I visited him several months later and he gave me a sample of titanium to investigate at home. I hoped to discover delayed emission of protons and alpha particles.

15) New CF Results Reported By Other Researchers

The most impressive ICCF10 presentations, from my point of view, were those of Yasuhiro Iwamura, from Japan, and Dennis Letts, from Texas. Iwamura and his coworkers diffused heavy hydrogen through a thin palladium foil. The surface where hydrogen entered was covered with a chemical element Sr. The experiment lasted 400 hours. During that time the amount of strontium was progressively decreasing. This was accompanied by the appearance of the element Mn (molybdenum) on the other side of the foil. The number of atoms of Sr that disappeared was about the same as the number of atoms of Mo detected on the other side of the foil.

The provided interpretation was that atoms of strontium are turned into atoms of molybdenum. Instuments used in this investigations were totally unknown to me. But I was fully aware of the significance of the reported results. It was alchemy--a change of one element into another. The

obvious question, which I did not ask, was about a possibilty of Mo contamination--how do you know that the detected molybdenum was not originally present in the setup?

Anticipating such questions, Iwamura provided an answer. The isotopic composition of molybdenum was very different from that found in nature.

Alchemy, by the way, is known to be impossible unless nuclear reactions are involved. Production of plutonium from uranium, to make atomic bombs, is an example of "nuclear alchemy." Turning atoms of one element into another, by means of nuclear reactions, is usually called transmutation. Even turning mercury into gold is now possible; but one gram of gold produced via transmutations would cost more than one billion dollars. Presence of transmutation products in a reproducible-on-demand CMNS process would be a very strong indication that nuclear reactions do indeed take place at a low temperature.

Letts' setup was much less sophisticated than that of the Japanese scientists. It was an F&P type of experiment, designed to demonstrate production of excess heat. What impressed me was the fact that results--generation of excess

heat at the rate of about one watt--became reproducible when the cell was irradiated with a beam of laser light. Absence of reproducibility was the Achilles' heel for this kind of experiment.

16) Cooperation With Oriani

I was also impressed by Richard Oriani's ICCF10 presentation. He reported emission of alpha particles during electrolysis. These particles were detected in a plastic material known as CR-39. Most eyeglasses are now made from that transparent material. It turns out that an alpha particle, stopped in CR-39, creates an invisible track. Such tracks become microscopically visible after the material is chemically processed. I learned about this method of detection of nuclear particles in Europe, about four years before coming to the US. But the material I used was natural mica, not CR-39.

Let me digress and describe how I became one of the first Europeans to use mica to detect fission fragments (not alpha particles). I was working on my Ph.D. project, in Orsay, near Paris. At that time I was already an expert in using several kinds of detectors of nuclear particles. One afternoon I received a telephone call from our librarian. She told me that she had an American guest interested in fission. "Please come and take him with you," she said, "I am too busy with other things." That is how I met John Walker, who invented the method of detection of fission fragments with common mica. He wanted to demonstrate the method to people who might be interested. I had everything he needed and two days later we observed tracks of fission fragments.

I did not use mica in my dissertation project but I used it in the US, when I became a postdoc at Columbia University, several years later. My most important scientific contribution to nuclear physics, during that time, was made by using mica detectors. CR-39 is used in the same way as mica, except that turning invisible tracks into visible tracks is slightly different. Learning how to

detect alpha particles with CR-39 presented no difficulty to me; I mastered that skill very quickly. I was lucky to be invited to Oriani's laboratory, at the University of Minnesota. Two of our replications of his results, which had been reported at the conference, turned out to be highly successful.

After each replication we examined the CR-39 chip removed from the electrolytic cell and a control chip that was not in the cell during the experiment. The second chip was chemically etched in exactly the same way as the first one.

Then pits on each chip were counted under the microscope. The control chip was used to measure the unavoidable background, such as alpha particles from cosmic rays, radon, etc. In each case tracks on the control chip were much less numerous than tracks on the experimental chip. The next step for me was to perform similar experiments independently. To be sure that my cell was exactly the same as his I ordered it to be made in the shop in which Oriani cells were made. I returned home and started experimenting immediately.

Unfortunately, my results were not as reproducible as our results in Minneapolis. In the first experiment I saw nothing except the natural background, on both chips. In the second experiment the number of observed alpha particls on the experimental chip was much higher that on the control chip. But the third experiment's result were essentially the same as the first one. Subsequent experimental results were also not reproducible. In other words, I was not able to either confirm or refute the results described by Oriani. This, however, was not the end of the story; I will return to this topic on Chapter 22. Results from my cooperation with Letts and Jones are described in the next chapter.

17) Next CMNS Conference: My Three Presentations

The next CF conference, ICCF11, took place in Marseille, France. This time I did have some experimental results to share, plus two 'philosophically-oriented' presentations. Jones, Letts and Cravens were coauthors of my experimental

report, entitled "Charged Particles from Ti and Pd Foils." Referring to my cooperation with Jones I said that his Ti foil was "sandwiched between two CR-39 detectors for the period of 55 days. [...] The number of tracks on the experimental chip was 225; the number of tracks on the control chip was 132. Such results, if generated by a Geiger counter, for example, could be used as evidence of nuclear particles being emitted from the foil."

Then I explained why the difference, 225-132=92 was not sufficiently large to draw the same conclusion from our CR-39 experiment. Additional experiments, I said, will be performed. Unfortunately, such experiments were not performed; Jones was apparently distracted by other matters. According to Wikipedia, "he retired on October 20, 2006 with the status of Professor Emeritus." Fortunately, the low-numbers-of-counts problem did not exist in my analysis of palladium foils from Texas. Here is how my cooperation with Letts was presented in the conference report:

"[... L.K.] asked for a chance to look at a possible 'nuclear signature.' Three palladium cathodes: Pd-613, Pd-616, and Pd-615 were sent to L.K. and he exposed them to the CR-39 detectors. [...] The tracks were counted, under the microscope. The results were: (a) about 500,000 tracks on the two detectors sandwiching the Pd-613 cathode, (b) about 11,000 tracks on two detectors sandwiching the Pd-616 cathode, and (c) no tracks above the background on the detectors sandwiching the Pd-615 cathode. Only then was L.K. informed that the Pd-613 generated an unusually

high amount of excess heat, the Pd-616 generated much less excess heat, and Pd-615 generated no excess heat at all. He was also informed that all three cathodes were cut from the same sheet of pure palladium, and that the electrolyte from the Pd-13 was known to be contaminated with uranium."

In other words, the huge number of tracks from the Pd-613 was most likely due to uranium. The Pd-616 and Pd-615 results, on the other hand, were highly significant. They demonstrated a correlation between the amount of excess heat generated and the number of alpha particles produced. [.] In the same presentation I said that cooperation with Letts

also did not have a happy ending. To establish a correlation between the excess heat and emission of alpha particles one needs more than two samples. But new samples were not sent to me. In 2005, alarmed by the situation, I sent the following e-mail message to Dennis Letts.

"I am going to galley proof our Marseilles presentation. This puts me in an awkward situation. If I were reporting on my own work I would add a short paragraph, something like this: 'No additional experiments were conducted to confirm observations made 6 months ago. The unexpected delay is due to [.]' or something like this. But in this case I

was only a messenger; you are the real player. A reader is likely to be interested in the current status of our investigation. I think that it is not right to report positive results only and keep negative results hidden. Do you agree? [ ]," The following reply was received several hours later.

"No additional experiments were conducted to confirm observations made 6 months ago. The unexpected delay is due to the fact that experiments seldom work on a schedule. The calorimeter had to be modified slightly to re-store design stability and precision. Also, we have not observed laser-triggered excess power since August 2003. Of course I agree [with your last statement] - since changing metals at the end of 2004, my success rate has been zero. This is compared to a success rate of 87% during the years of 2000-2004. Other than changing Palladium stock, I don't know what has caused the sudden loss of the laser effect. Experiments have been conducted in a high quality calorimeter, in a moderate quality calorimeter (my Avanti) and on the open bench. The laser effect has not re-appeared under any of the above calorimetric conditions. Experiments are being conducted now to re-establish the laser effect or to explain why it stopped working. You may use this information in any way you wish, including an addendum.

With regard to reporting negative results, consider this: Cravens and Letts discovered the laser effect in September 2000 and reported the positive results publicly in August 2003. We spent 3

years testing the credibility of our result before reporting publicly. We anticipate behaving in a consistent manner now - we have negative results but we're not in a rush to report until we're sure that we have negative results and try to provide some reasons why the results are negative. I believe that reporting results formally by 2007 will be consistent with our previous work and should not be considered 'keeping negative results hidden'.,"

I am sure that neither Letts nor Cravens are trying to hide negative results. Like most CMNS researchers, they are honestly following scientific methodology of validation of claims. But, as explained by McKubre, outomes of experimental results often depend on unknown parameters. That was a good illustration. No new samples were ever

sent to me.

What a coincidence! While I am describing this past cooperation Dennis Letts has just announced (March 2012) the development of a single mathematical equation that is able to accurately reproduce results from 40 experiments he conducted with Cravens and Hagelstein in 2007-2008. Additional experiments will soon be performed to test theoretical predictions. Is this going to lead to a great step forward? I hope so.

18) My Two Other ICCF11 Presentations

Those who attend scientific conferences know that some contributions are presented as posters. This is unavoidable when time is limited. My non-experimental presentations appeared as posters; both were published in the Conference proceedings. The first, entitled "Recent Cold Fusion Claims: Are They Valid," can be read online at:

http://pages.csam.montclair.edu/~kowalski/cf/152summary.html

It was a manuscript--a review of the CMNS field--with 37 references. I wrote it for publication in a mainstream scientific journal. Unfortunately, that manuscript was rejected by editors who received it. The second poster, as shown in the next Chapter, described my personal experience with the process of rejection of a CF paper.

19) History Of Attempts to Publish

My unsuccessful attempt to publish a letter to the Editor of one journal--Physics Today--has already been described in Chapter 13. What follows is a description of other unsuccessful attempts. Knowing that the second DOE review of the CMNS field was approaching, I summarized what I had learned about CF. This took the form of a manuscript that I wanted to publish, for the benefit of people inerested in science and technology. The title of the article was "Recent Cold Fusion Claims: Are They Valid?" Seven journals to which my manuscript was submitted were:

Physics Today, USA American Scientist, USA Scientific American, USA Nature, UK

New Scientist, UK

The Physics Teacher, USA Science, USA

a) The Cover Letter

Each submission had essentially the same cover letter. In that letter I wrote: "I am sure that you are aware of the DOE move to review the cold fusion field, as reported in The New York Times (3/25/04). Attached is a review article which, I hope, can be published in [your journal]. The title is 'Recent cold fusion claims: are they valid?' It is not a paper defending cold fusion claims; it is a paper describing them, no matter what one is inclined to think. Scientifically literate readers are likely to appreciate my short summary of recent claims made by cold fusion researchers.

Some of these claims, such as turning Sr into Mo, or Cs into Pr, without stellar temperatures, are even more extraordinary than the claims made by Pons and Fleischmann. The strange thing is that authors of such reports seem

to be reputable scientists associated with prestigious universities and laboratories. Is it a matter of fraud? Is it a matter of self-deception, or incompetence? Is it a matter of progressive degeneration due to the isolation of the field from mainstream science? My article does not try to answer these questions; its purpose is to present a summary of what has been recently reported without taking sides. The subject is interesting no matter what the final verdict of the second DOE evaluation will be.

Like many other science teachers, I am in no position to verify validity of hard-to-accept claims in a specialized laboratory. That is why, as suggested in the concluding section, a new evaluation of cold fusion claims, by an appointed panel of experts, is highly desirable. In writing the review I was not aware of the pending DOE investigation. I deliberately avoided references to social aspects, which are interesting but highly controversial. I am a physics teacher at Montclair State University. Studying cold fusion was my 2003/2004 sabbatical project."

b) Reply From the Editor Of Physics Today

," Dear Dr. Kowalski: We received your article submission titled, "Recent Cold Fusion Claims: Are They Valid?," and appreciate your sending it to Physics Today. After reviewing it, however, we have concluded that it does not meet our editorial needs. Thank you for your interest in Physics Today. Sincerely, Stephen G. Benka Editor-in-Chief."

c) My Comment

That is it. Not a single word about the content of the article. How can the phrase ,"does not meet our editorial needs," be interpreted? Why was the article not sent to referees? They do publish many field summaries each year. Why was my summary not given the same chance to be reviewed by experts? Was I writing about sociology, poetry, business or something else unconnected to physics? Are recent cold fusion claims described in the article already widely known to most physicists? Was my description of these claims erroneous? Was the article rejected because of its style, its limited scope, or its disregard for ethical standards?

d) Reply From the Editor Of American Scientist

"Dear Dr. Kowalski: Yes, we've received your original manuscript and the follow-up. I'm afraid we're not always able to acknowledge receipt immediately. I try to give a prospective author an idea of whether we'll be able to consider a manuscript, and sometimes it takes a little time to determine that. We have certain basic criteria for submissions.

When a submission does not meet those criteria, I prefer to say that it cannot be considered rather than simply acknowledge receipt.

In the case of this submission, I'm unsure. We publish feature-length articles and commentaries based on original published research. The authors of American Scientist articles are the people who have done the work and therefore are in a position to survey their own field. I don't actually have evidence (in the form of cited publications or a c.v.) that you have done original research on the topic you propose to write about.

If you would like to publish a short commentary, we do have a department with different criteria, called "Macroscope." This is where we publish short essays conveying a scientist's point of view on a matter of personal or professional interest to scientists and engineers. The maximum word count is 1,500. If you would like us to consider publishing your piece in a short form, please let me know, and I'll share it with my colleagues and let you know the response. Sincerely, Rosalind Reid Editor, American Scientist."

e) My Reply:

"Dear Dr. Reid: Thank you for your prompt reply. I understand your hesitation. Protecting readers of American Scientist from people who are not qualified to write about science should be one of your tasks. To help you decide here is a little summary about myself.

I am an experimental nuclear physicist (Ph.D., 1963) with a large number of publications (mostly as coauthor) in that field. The attached abbreviated list of publications, spanning four decades, makes it clear that my teaching

commitment has not prevented me from active participation in nuclear physics research. Like most scientists, I accepted the 1989 verdict about cold fusion. And you are correct, I have no publications about cold fusion. My new interest in this field was triggered in October 2002. I attended a nuclear conference in New Mexico and heard several scientists talking about cold fusion research. It was the beginning of my sabbatical year. The paper I submitted is the product of that work.

I hope your hesitation will not prevent you from sending my article to competent and unbiased reviewers. Please let me know what your decision will be. Meanwhile I would like to follow your suggestion about writing a short commentary on the anticipated review of cold fusion by the DOE; see the attached file. Thank you for your consideration. Sincerely yours, Ludwik Kowalski.

A list of my selected publications, and a file containing my ,"short Golden Egg piece," (see below), were attached.

f) Seek Not The Golden Egg, Seek The Goose (attached file)

According to a recent article in The New York Times (3/25/2004) the US Department of Energy (DOE) is going to review the field of cold fusion this year. This is a significant event; the controversial field of cold fusion (CF) has often been called pseudoscience. If it were up to me I would suggest that the panel of DOE scientists focuses on essential scientific questions and not on practical applications which are far away, at best. Promising too much, and too early, was one of the mistakes made fifteen years ago. In my opinion the six most important scientific questions are:

1) Are unexpected neutrons, protons, tritons and alpha particles emitted (at low rates) in some CF experiments?

2) Is generation of heat, in some CF experiments, linearly correlated with the accumulation of 4He at the rate of 24 MeV per atom of 4He?

3) Have highly unusual isotopic ratios been observed among the elements found in some CF systems?

4) Have radioactive isotopes been produced in some CF systems?

5) Has transmutation of elements occurred in some CF setups?

6) Are the ways of validating scientific findings in the areas of CF research consistent with accepted methodologies in other areas of science? I think that a positive answer to even one of these six questions should be sufficient to justify an official declaration that ,"cold fusion, in light of recent data, should be treated as a legitimate area of research.," The normal peer review mechanisms will then be used to separate valid claims from wishful thinking.

g) After Waiting Several Days I Sent This Addendum

"I already mentioned two reasons making such review urgent: the 15th anniversary of the Utah announcement and the pending DOE investigation. In my opinion, by publishing my paper, or a review written by somebody else, you will contribute to something desirable. Nobody is happy with the unhealthy feud between a group of well motivated researchers and official representatives of 'mainstream science.' Most people are passive but those who do take extreme positions often use highly perjorative adjectives, such as 'pathological', stubborn, misguided, and fraudulent.' Please do not miss an opportunity to contribute to ending this unnecessary feud. I would be happy to give you names and addresses of top people in five main areas of cold fusion.

So now you have several excuses for bending a rule of your editorial policy. They are: a) the anniversary, b) the pending DOE investigation, c) my paper is a review describing (very objectively, and without accusations of any kind, as you probably noticed) several very different areas of a broad field, d) my background as an active nuclear physicist, and e) my unpublished research in two areas of cold fusion. You are certainly aware how difficult it is to publish cold fusion research papers in important scientific journals. Will the situation change after the pending DOE investigation of cold fusion? I hope so. Please help to contribute to this cause.

If you decide to approach Fleischmann, be aware that he is an electrochemist; I do not consider him to be an expert in nuclear physics. This became clear in 1989 and contributed heavily to the

cold fusion controversy. One can only imagine what would happen if Fleischmann and Pons, who are chemists, refused to participate_in the infamous press release, organized by the administrators of the University of Utah, and decided to work with Steven Jones, who is a physicist. A year or two later they would publish a peer reviewed paper and [...] But I refuse to speculate; my goal is to heal the wound by focusing on purely scientific topics and by ignoring stupid things people said or wrote before.

Please help me. I think that cold fusion, no matter what the final verdict will be, is a highly significant episode in the history of science. Let your journal be a part of that history."

I also gave Dr. Reid names and e-mail addresses of five people (who are certainly much more knowledgeable than myself) suggested that she contact one of them to write a longer review paper for the journal. Steven Jones, Martin Fleicshmann and George Miley were among the scientists I selected. I did not hear from Dr. Reid again. Will she accept my ,"Golden Goose" item? Probably not.

h) Reply From The Editor of Scientific American

,"Dr. Kowalski: Thank you for your offer to contribute to SCIENTIFIC AMERICAN. After much consideration, I regret to say that the piece you propose is not suited to our somewhat limited editorial needs. We appreciate your interest in SCIENTIFIC AMERICAN. Regards, Jacob Lasky Editorial Administrator.,"

i) Reply From The Editor of Nature

,"Thank you for your inquiry about submitting your paper entitled 'Cold fusion 15 years later' to Nature. I regret that the paper that you describe seems unlikely to prove suitable for publication in Nature, and we accordingly suggest that you pursue publication elsewhere. I am sorry that we cannot respond more positively on this occasion. Yours sincerely Dr Karen Southwell, Senior Editor.,"

j) My Comment

I was aware, from browsing their web site, that the rate of acceptance in Nature is about 1 out of 10. On that basis I should have expected a rejection. Frustrated that my timely review of the Cold Fusion field is being delayed I decided to send it to another UK journal, New Scientist. But they never responded.

k) Reply From The Editor Of The Physics Teacher

, "Dear Professor Kowalski: We have reviewed your manuscript ,"Cold Fusion 15 Years Later," in the light of the recent Physics Today article 'DOE Warms to Cold Fusion.' While a paper in TPT on this subject may be warranted, we do not believe there is any great urgency to publish one immediately. After all, according to the Physics Today piece, DOE Deputy Director Decker says that their 'review of cold fusion will begin in the next month or so [that was back in April]' and it 'won't take a long time--it's a matter of weeks or months.'

We believe that it would be premature to publish a cold fusion paper in TPT before the results of the DOE review are announced. Were we to do so, a follow-up piece would almost certainly be

required later, regardless of how that review turns out, and we don't feel that two papers on the subject are warranted. We will consider your paper again (along with any revisions induced by the DOE report) after the report is made public.,"

l) My Reply

"Dear Dr. Mamola": Was my manuscript examined by referees? I would very much like to see what they had to say about its content. Thank you in advance.," This message has not been answered. Will I ever see the referee's comments? Probably not.

m) Reply From The Editor of Science

"I've consulted with our editorial staff in the physical sciences. Unfortunately, we don't think this topic is an appropriate one for review in Science at this time. Thanks for thinking of Science. Sincerely yours. Donald Kennedy."

n) My Comment

Hmm, it was rejected on the basis of the topic, not on the basis of the content. George Miley was right; the editors of most journals put this topic on their blacklist. The scientific methodology of validation of claims, made by recognized experts, does not count anymore.

I can now say that I have had personal experience with peculiar aspects of CMNS area: irreproducibility of experimental results and censorship imposed by editors of journals.

20) Transmutation Of Radioactive Nuclei--Or An Artifact.

As mentioned in the Introduction, I came to the Albuquerque conference to learn about a claim made by a physicist who was not a CMNS researcher. He was discussing practicality of the idea of destroying radioactive materials by bombarding them with high-energy neutrons. That topic was at the center of my attention earlier, when I was on sabbatical leave in France. I had a chance to participate in the collection of experimental data on production of energetic neutrons by using energetic protons, from a large accelerator.

What a coincidence! One of the CF researchers I met at Albuquerque, a university professor, claimed that he was able to reduce radioactivity of uranium via a CMNS process. I definitely wanted to participate in the next phase of his experiment. But the scientist was not enthusiastic about this idea, claiming that it might create patent-related complications. Fortunately, I later learned that another CF researcher, Hal Fox from Salt Lake City, UT, made a similar claim, destroying radioactive thorium via a CMNS process. This time my suggestion that we should replicate the experiment together, in Hal's laboratory, was welcome.

Their high voltage electrolysis cell was similar to that shown in Chapter 9. The essential difference was that the cathode was made from zirconium, not tungsten, and that the substance dissolved in water was a thorium salt. To prepare myself for this task I read the paper by Hal Fox and Dr. Shangxian Jin very carefully. It had been published in Journal of New Energy. Hal was

more an organizer and businessman than a scientist. But his younger co-worker was a highly qualified nuclear physicist.

It occurred to me, while reading their paper, that the destruction of thorium might have been an illusion, resulting from not taking under account one possible effect. I described this to Hal, before coming to Salt Lake. He agreed that the effect I suggested should be investigated. That is what we did during one week in 2003, in his Salt Lake City laboratory.

The experiment was performed twice, first more or less as described in their 1998 paper and then in the way I suggested. The first experiment consisted of the following four steps:

Step 1: A small amount of radioactive thorium salt was dissolved in water, in an open glass jar.

Step 2: A sophisticated detector placed outside the jar was used to measure gamma rays emitted by thorium. The result was close to 20,000 counts.

Step 3: An electric current was passed through the salt solution in the jar, a process claimed to be responsible for the destruction of radioactivity. After about 30 minutes the current was turned off. The immersed zirconium cathode delivering electricity to the solution was partially decomposed during this step; metallic fragments could be seen at the bottom of the jar.

Step 4: Our detector was placed in the same position as before and thorium radioactivity was again measured for two hours. This time the number of counts was close to 10,000. In other words, the final count was one half of the intial count. In that sense the replication of the 1998 experiment was very successful.

Why was the experiment performed in an open jar and not in a pressurized container, as in the original experiment? Because we believed that the setup could eventually be used by teachers to demonstrate a CMNS effect. Jars are widely avalable in school laboratories. And they are safer than sealed pressurized containers. Let me now describe our second experiment. The open jar and all instruments were the same as in the first one. The underlying idea of counting gamma rays before and after Step 3 was also the same. But the way of counting was modified, according to my suggestion.

The salt was first placed on top of the detector and gamma rays were counted for ten minutes. The number of counts was close to 100,000. This was not surprising, considering the very short distance between the source of radioactivity and the detector. Then the salt was dissolved in water and the electric current was passed through the solution for 30 minutes, as in the Step 3 of our first experiment.

Next we heated the jar and allowed all the water to evaporate. The thorium salt remained in the jar, forming a white precipitate, mixed with metallic debris from the partially destroyed zirconium rod. The deposit was collected and placed on top of the detector. Gamma radiation was again measured, for ten minutes. Suppose that the original claim--destruction of 50% of thorium by electric current--was correct. In that case the final count would be about 50,000, one half of the initial count. Our final count, however, was close to 90,000, a strong indication that the fraction of destroyed thorium was much lower than 50%.

Results from the two experiments allowed us to reach the following conclusions: (a) The final count was indeed reduced by a factor of two, as originally reported by Fox and Jin. (b) The reported reduction of count, however, was not due to destruction of atoms of radioactive thorium; it was due to the suspected artifact, redistribution of thorium in the jar, during Step 3. (c) The approximately 10% reduction of counts was attribued to thorium escaping with steam during the process, or deposited on the inner walls of the jar. Hal did not participate in this experiment personally. But he took the remaining precipitates and sent them to a chemical laboratory.

This episode put me in a rather delicate situation. Being a scientist, I wanted to publish the result, perhaps in the same journal in which the original claim appeared. But it was not my experiment and I decided to do nothing more. Hal had generously invited me (I stayed in his home, met his family, etc.) and the task of publishing the new result should have been on his shoulders. He said that he would wait for the result of chemical analysis. Did the results of that analysis reveal presence of transmutation products, expected by Hal? I do not know.

This was the first successful CMNS experiment in which I personally participated. The term success does not mean finding what was suspected. In the context of a scientic exploration to succeed means to obtain a clear yes-or-no answer--in this case about destruction of thorium atoms. I would be equally happy, or probably happier, if the last count were close to 50,000 rather than close to 100,000. That would be a strong indication that about 50% of thorium was indeed destroyed (rather than redistributed) during the process. Two other successful CF experiments, in which I participated, are described in the next two chapters.

21) The Galileo Project

In November 2006 Steven Krivit, editor of the online magazine New Energy Times, started recruiting researchers for what he called The Galileo Project. They were to replicate experimental results obtained in the US Navy laboratory in San Diego, known as SPAWAR. A team of scientists from that laboratory, headed by electrochemist Pamela Boss, he wrote: " produced something unique in the 17-year history of the scientific drama historically known as cold fusion: simple, portable, highly repeatable, unambiguous, and permanent physical evidence of nuclear events using detectors that have a long track record of reliability and acceptance among nuclear physicists."

The task was to follow the SPAWAR protocol and detect alpha particles by using CR-39 detectors. Naturally I was

one of the six people who immediately agreed to participate. Scott Little, with whom I had worked in Texas, also became one of the independent participants. Oriani refused to join because, in his opinion, the SPAWAR CR-39 pits were due to corrosion rather than to alpha particles. I was well prepared for this task. Working at Montclair State University, with a student helper, I was able to replicate SPAWAR results in about three weeks. The Winter Meeting of the

American Chemical Society was approaching and I knew that Pamela Boss was scheduled to talk about the original SPAWAR results.

That prompted me to sign up for the conference. Fortunately, my "last minute" application was accepted. They gave me a time slot to present the results, at the same session. The meeting was in Denver and I was able to be there just on time. Photos on my slides were practically the same as on those shown by the SPAWAR team. That was the happy part of my presentation; I could see smiles on faces of SPAWAR researchers.

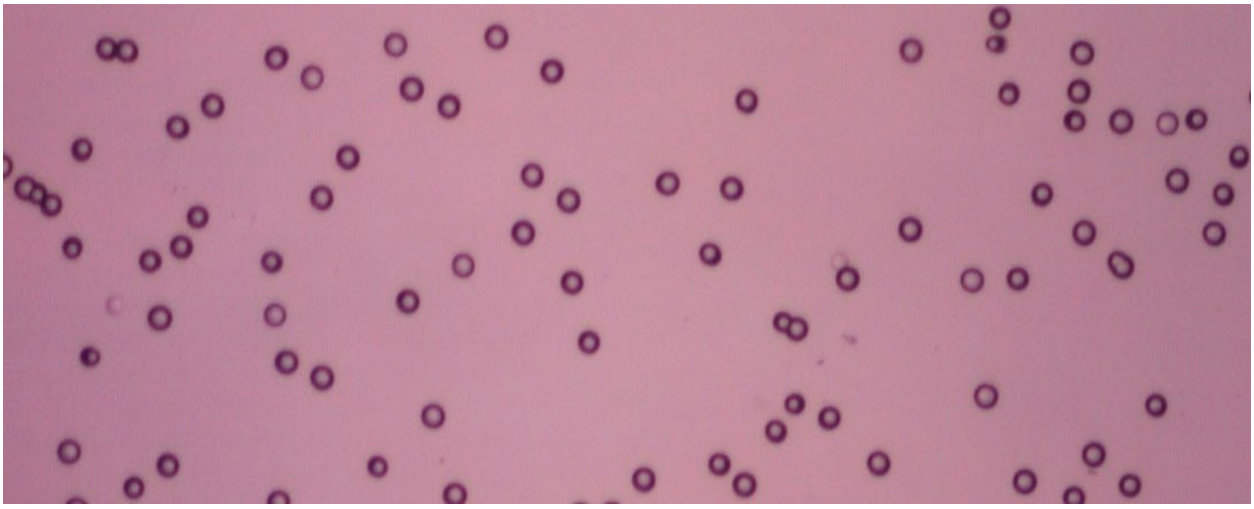
But the ending was not happy; my conclusion was similar to Oriani's--the pits were not due to alpha particles. But I am not a corrosion expert, like Oriani. I said that the pits might have been due to much larger nuclear projectiles, such as fission fragments. To reach this conclusion I measured sizes of typical SPAWAR-like pits and compared them with sizes of pits due to particles from my alpha radioactive source. Friendly personal relations with Pamela deteriorated rapidly after the conference.

But we debated the issue on pages of European Physics Journal. Fortunately, its open-minded editor does send CF-related submissions to referees. The experiment was successful, a clear "yes" answer was obtained to the replication-of-experimental- results question. The disagreement was about the interpretation of results, not about the results themselves. I respect SPAWAR scientists; their numerous publications are certainly up to high standards. But R. Park, and others like him, probably continue calling them pseudo-scientists, without any justification. That is very unfortunate.

22) The Curie Project

My cooperation with Richard Oriani, described in Chapter 16, was not successful; the answer about reproducibility of results was neither "yes" nor "no." That was not a pleasant situation. But our cooperation was resumed after he changed the procedure (covering experimenta CR-39 chips with thin foils of Mylar). He submitted a paper with a new impressive results to a prestigious US journal, Physical Review. That manuscript was rejected but results were presented at our 2008 conference (ICCF14), in Washington, DC. His paper prompted me to make another attempt to replicate new data.

My 20 consecutive experiments, lasting three days each, were performed on the 27th floor of an apartment building, rather than in a private home with granite walls. The probability of being exposed to radon at this location was relatively low, especially in my study, where the window was kept slightly open. Additional precautions were made to minimize exposure of CR-39 to radon. Chips ready to be used were kept in distilled water, not in air. Etching of CR-39 chips and counting of pits, on the other hand, was done at the university. The photo below shows typical circular tracks of alpha particles in CR-39, as seen through a microscope.



The mean track density reported by Oriani was 122 per square centimeter; it was significantly higher than his measured background. My mean density, from 17 experiments, was only 16 per square centimeter; it was not significantly different from my measured background. Total elimination of background is not possible, due to radon, cosmic rays, and other contaminants.

Track densities on three of my experimental chips turned out to be very much higher than those reported by Oriani. My results, in other words, were not in agreemen with those reported by Richard. The exceptional results from these three experiments were attributerd to contamination. In a subsequently published paper I wrote that "attempts to identify contaminants were unsuccessful. Alpha radioactive substances such as uranium, thorium and radium are known to be present in our environment. One nano-gram of radium, for example, emits 37 alpha particles per second. Atmospheric testing of nuclear weapons in the 1960s contributed to contamination of our environment with long-lasting alpha-radioactive isotopes."

No one knew about my undeniable disagreement with Oriani's published results. Before sharing my data with others I summarized his results and asked, using the Internet, for people interested in replicating them, in the same way SPAWAR results were replicated in the Galileo Project. Three individuals expressed interest. One was a young engineer I had met in Denver, after the ACS conference (Jeff Driscoll). Two others (Mike Horton and Pete Lohstreter) were high school physics teachers.

Our cooperation, called the Curie Project, was very successful. Working independently, and unaware of each other's results, each experimentalist came to the same conclusion--measured track densities were not significantly different from those due to the measured background. The results were subsequently published in the Journal of Condensed Matter Nuclear Science, a peer-reviewed journal of the International CMNS Society. It can be downloaded as

<www.iscmns.org/CMNS/JCMNS-Vol5.pdf>.

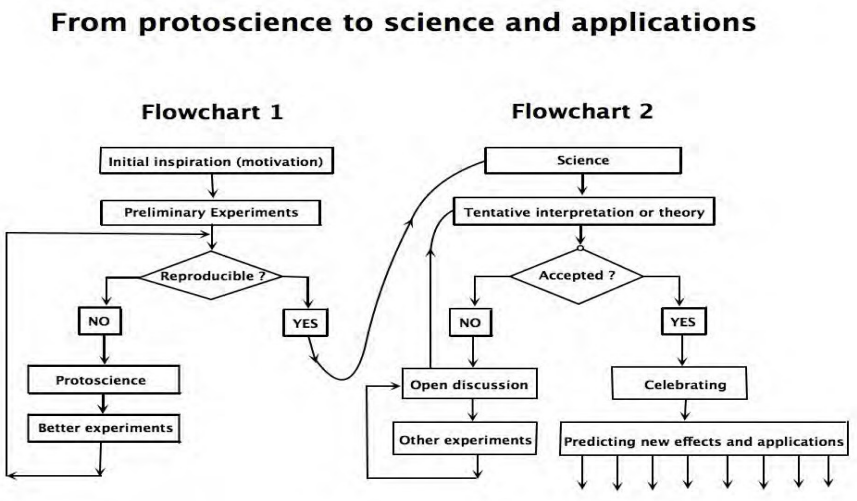
Numerous details and illustrations are in this short article (pages 34 to 41). Would I volunteer to participate in another CF project? Probably not. But I will continue paying attention on future claims, as illustrated in the next chapter. My most recent publication in this area is the letter to the editor of Progress in Physics. The title is " Social Aspects of Cold Fusion: 23 Years Later." The link is:

<www.ptep-online.com/index_files/2012/PP-29-L2.PDF>

In the concluding section I wrote: "The CF controversy is unprecedented in terms of its duration, intensity, and caliber of adversaries on both sides of the divide. Huizenga and Fleischmann were indisputable leaders in nuclear science and electrochemistry. CMNS researchers are mostly also Ph.D. level scientists. The same is true for those scientists who believe that the announced discovery of CF was a “scientific fiasco”. We are still waiting for at least one reproducible-on-demand demonstration of a nuclear effect resulting from a chemical (atomic) process. In the case of CF the self-correcting process of scientific development emphasized by Huizenga has not worked. This fiasco seems to be due to the fact that scientists appointed to investigate CF claims did not follow [the welll established] rules of scientific methodology."

23) Flowcharts: The Last CF Conference I Attended

CMNS conferences take place each year. The next one will be in South Korea (summer 2012). But the last CF conference in which I participated took place in 2008, in Washington DC. The title of my presentation was "Nuclear Or Not Nuclear: How To Decide?



The above diagram was part of my presentation. It illustrates a typical experimental discovery process. What triggers the process (see the top of the left flowchart) is irrelevant. A subsequent experiment might or might not be reproducible. Non-reproducible results, in my opinion, belong to protoscience, not science. But they may become science, sooner or later. The next task is to

interpret (understand) the result. The scientific community might or might not agree on a proposed explanation (theory).

This typically leads to other debates, and to calls for additional experimental data, as shown in the diagram. Practical applications might or might not emerge immediately, after a discovery is recognized as valid. But a valid discovery does become part of science, like each little stone is part of a magnificant cathedral. Practical applications usually

result from sets of many discoveries, including those made long ago.

CMNS claims, I said, still belong to protoscience. "It is unfortunate that, except for The Galileo Project, researchers work in isolation from each other. This is understandable, each resercher does what matches his/her expertise and limited resources. This kind of work was going on for 19 years. [...] The task of turning protoscience into accepted science is still waiting for us. How to approach this difficult task and how to proceed more effectvely? In my opinion, well-focused cooperative investigations, as in The Galileo Project, are likely to be more productve, in the next two or three years, than uncoordinated efforts of many individuals."

What is the main difference between hot fusion and cold fusion communities? In both cases the goal was to build a device whose energy output exceeds the energy input, without consuming chemical fuel. The hot fusion community has been trying to achieve the "break-even" point for five decades and it knows exactly why reaching it is so difficult. The cold fusion community, on the other hand, started by experimenting with break-even devices without understanding what was going on and why. What is the probability that something profoundly new will emerge from hot fusion? It is much lower than from cold fusion, in my opinion. In any case turning hydrogen bombs into candles is not going to be any easier than turning swords into plowshares.

24) Andrea Rossi's Unbelievable Claims

As mentioned at the end of Chapter 2, I have a website devoted to CF--a cross between a logbook and a diary describing participation in CMNS activities. Writing this book would be much more demanding if this free online resource were not available to me. Another resource was a private Internet list for CMNS researchers. On February 26, 2010, the following patent description was posted on our list: "The patent applicant is engineer Andrea Rossi, owner of a small company, employing 2-5 people. In the patent he claims that 'A practical embodiment of the inventive apparatus, installed on October 16, 2007, is at present perfectly operating 24 hours per day, and provides an amount of heat sufficient to heat the factory of the Company EON of via Carlo Ragazzi 18, at Bondeno (Province of Ferrara).' (Italy). This suggests that power output is at least tens of kilowatts!"

This is a reasonable estimate. Excess heat generated at the rate of tens of thousands of watts, for several months, was indeed a sensational claim. That would be equivalent to burning several tons of coal. Most excess heat demonstrations generate excess heat at the rate of one watt, or less, for much shorter durations.

Several people commented on the above announcement. I responded. "A suggestion was made, two days ago, that someone should visit the place where spectacular results are available on demand. The visitor would either confirm or refute what has been reported. I do not think that an outsider would be able to evaluate the setup. What is needed is a blueprint and a detailed protocol. Following the protocol a team of competent researchers would try to build the device from scratch and to measure excess heat with their own instruments. Only team members should be allowed to enter the room in which the device is being constructed.

Yes, I am thinking about the possibility of fraud. Fraudulent people, such as identity thieves and those who solicit profitable partnerships by email, do exist. Someone replied: "I strongly support your suggestion to organize a group to replicate the Rossi patent and I offer myself to assist. I have a modest laboratory and am well equipped to undertake gas absorption experiments."

Unfortunately, things did not develop along the path I suggested. What happened was a demonstration, on January 14, 2011, at Bologna University in Italy. It was followed by a press conference, etc., as photographically illustrated at:

http://www.journal-of-nuclear-physics.com/

The Bologna demonstration could have been more effective than it was, without revealing the nature of the secret catalyst. Rossi could have provided the blueprint of the apparatus to a trusted authority, for example, an Italian government laboratory, asking them to manufacture his simple device. They could have brought it to the University of Bologna and allowed Rossi to place the secret fuel (nickel powder mixed with something else) into the cylinder. He would not have been allowed to do anything else to the apparatus. That would eliminate any suspicion of a hidden energy source somewhere within the apparatus.

This, however, would not have eliminated another possible suspicion--that a chemical fuel was mixed with nickel. But suppose the powder supplied by Rossi is weighed, both before and after the experiment. Suppose the change in weight is negligible, in comparison with what it would have been if a suspected chemical fuel were present. That would rule out a possibility of the chemical-fuel fraud.

Generation of a huge amount of excess heat was not the only claim made by Rossi. He also wrote that 30% of nickel was transformed into copper, during six months of operation of his 12 kW reactor, that the radiation level was negligible, etc. Such claims were in conflict with everything I knew about nuclear physics. After becoming aware of this I decided to publish a paper that even undergraduate physics students would be able to understand. Unable to find a publisher I posted the paper at my website. This was in April of 2011. The article, entitled "Rossi reactors--reality or fiction," was subsequently published, in Progress in Physics (January 2012). The link is:

http://www.ptep-online.com/index_files/2012/PP-28-07.PDF

Rossi does not want to be involved in discussing physics--he is an inventor, not a scientist, he keeps emphasizing. He believes that the validity of his discovery will be confirmed by a large number of satisfied customers. At one time (January 2012) he stated that he had found two

customers for his 1000 kW power plants. But their identity has not been revealed. I agree with him that a large number happy users will be a convincing argument. But I would not advise anyone to invest in his "secret technology" at this time. The possibility that Rossi has discovered something totally unknown is real but the probability of it is very low, in my opinion. I wish him well. We do need alternative sources of pollution-free energy.

"Is Andrea Rossi the world's greatest inventor since Nikola Tesla and the savior of mankind, or is he one of the worst scoundrels of the year? It's very difficult to say at this time, but the question really is that basic. There are those who would like to tread some middle ground on the topic, but there is no middle ground; it's either one way or the other. The mystery remains, and we have no way of knowing for sure which is the truth. The good news is that, given a little bit of time and patience, the answer to this question will be clear. Meanwhile this is indeed such an incredibly fun and interesting story to watch unfold." I am quoting John Ratcliff, the author of an online article "Andera Rossi: Sinner or Saint?," published on January 21, 2012. <www.examiner.com>.

25) What Is Next?

The process of sharing what I know and think about CMNS will consist of three steps. The first step was to address mainstream scientists. This has been accomplished in a short Letter To The Editor of "Problems In Physics", published in January 2012 (see the link in Chapter 22). The second step was to address generally-educated people; this book is written for them. The third step will be to address philosophers of science, at a congerss in Montreal (June 2012). My paper, entitled "Cold Fusion 23 Years Later: Social And Philosophical Aspects Of That Controversy," has been accepted. What follows are excerpts from slides I am now preparing.

Excerpt 1:

(a) Mathematics, in my opinion, is much closer to theology than to science.

(b) Validation of claims in theology is based on acceptance of initial axioms (self-evident truth) and on logical consistency. The only way to justify the rejection of a claim (either in theology or in mathematics) is to find a logical error in the derivation.

(c) In science--both physical and social--claims are not based on logic only; in the final analysis they are based on experimental data. Mathematics is formalized logic; it is not science.

Excerpt 2:

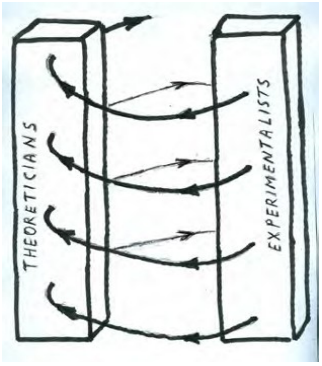
Reproducibility on demand is an important scientific requirement. But suppose a reproducible result conflicts with an existing theory. What should be rejected, the experimental result or the theory?

Excerpt 3:

Basic scientific assumptions, derived from philosophy, are: (a) reality is objective, (b) humans have the capacity to perceive reality accurately, and (c) rational explanations of phenomena in our material world are useful.

Excerpt 4:

"Theoreticians guide while experimentalists decide? Yes, but theories are based on verified results from experiments and observations. The chicken and the egg dilemma? Not really. Why not? Because the process of accumulation of scientific knowledge is not circular; it is spiral, as illustrated below:



Excerpt 5:

In 1942 Robert Merton described CUDOS, the prevailing Norms of Science. In this acronym,

C is for communalism (discoveries are not private property, they belong to all scientists),

U is for universalism (principles of validation of claims are universal, not subject-specific),

D is for disinterestedness (primary motivation for scientists is not money; it is love of truth), and

OS is for organized scrutiny (skepticism is very useful).

Excerpt 6:

A leading CMNS researcher Edmund Storms once asked this question: "Which is the greater threat to science and mankind, accepting a claim that can have no possible benefit or rejecting a claim that can have great benefit?

This question was addressed to editors of scientific papers who often deprive CMNS researchers of the peer review process.

Excerpt 7: (a) Why are scientific investigations usually more effective than investigations in any other field? This is due to the so-called "scientific method," a set of rules developed to deal with difficulties, especially with mistakes and controversies. (b) Most scientific mistakes are recognized when new results are discussed with colleagues, or via the peer review process. (c) Depriving PhD-level scientists of the peer review process is a crime against science.

ABOUT THE AUTHOR

Ludwik Kowalski, born in 1931 in Warsaw, Poland, became a nuclear physicist. The link below will lead to additional details about his unusual life, in the USSR, Poland, France and the USA. CLICK to see additional details

A2. Biographies on Wikipedia and by the Hoover Institute

From Wikipedia, the free encyclopedia

Ludwik Kowalski

Born: 1931, Poland

Alma mater: Warsaw University of Technology, Curie Institute, Warsaw

Occupation: scientist, professor

Ludwik Kowalski (born 1931) is a Polish-American physicist, anti-communist author, and professor emeritus of Montclair State University in Montclair, New Jersey.

Life

Kowalski was born in Poland. His parents, members of the Polish Communist Party (the precursor of Polish United Workers' Party), decided to emigrate to the Soviet Union in 1932. In Moscow, they continued in their careers as an engineer and nurse. His father was arrested in the night during the Great Purge in 1938 as an "enemy of the people," never to be seen again by his family. Kowalski and his mother lost their home and lived briefly in the polyclinic where his mother worked, then moved to the Dedenievo settlement, some 30 miles north of Moscow, where his mother worked in a nursing home. Kowalski joined the Red Pioneers in elementary school. He learned that his father had died in a Siberian camp some two years later, aged 36.

In June 1941, when Nazi Germany attacked the USSR, the Red Army retreated from Dedenievo, but the German army halted in nearby Jachroma (Yakhroma). However, the Germans bombed the settlement, killing many residents. Kowalski survived this period in the basement of an old church where peasants had stored vegetables from collective farms. In 1943, Kowalski's mother became a nurse in a Polish orphanage near Moscow, while Kowalski relearned Polish in the attached elementary school.

In 1946, the orphanage moved to Warsaw and merged into the existing Nasz Dom orphanage. His mother worked there as a nurse, and Kowalski attended a progressive gymnasium (high school). His mother re-joined the Polish Communist Party, and he joined the Union of Polish Youth. Kowalski then attended the Warsaw Polytechnic Institute (now the Warsaw University of Technology) and then also joined the Party.

As an engineer, Kowalski specialized in electro-medicine. He learned how to design electrical instruments for doctors. He obtained a graduate degree based on research at Warsaw's Radium Institute (now the Curie Institute, Warsaw). In 1956, his application to work in a nuclear laboratory in the Soviet Union was rejected, and he traveled to Paris, where his father's sister lived. He hoped to be able to pursue doctoral studies in France. A written recommendation from Cezary Pawlowski, director of the Radium Institute and a former assistant of Marie Curie, helped him join the laboratory of Joliot-Curie, where he worked for the next seven years. In 1963, having obtained his doctorate in Physics from the Sorbonne, he returned to Poland to work in an academic research laboratory.

In 1964, Kowalski attended a scientific conference in the US—and stayed on, becoming a research associate with Professor Jack Miller in Columbia University's Chemistry department. In 1969, he began a 35-year career as a professor and researcher of Physics at Montclair State University. Since retiring in 2004, he has continued to work in two areas of personal interest—conducting Physics experiments and writing about experience under the Communism of the Soviet Union from an anti-communism viewpoint.

Works

Kowalski has authored and co-authored nearly 100 scholarly papers on Physics, a textbook on Physics, and two books on Stalinism.

In January 2011, Hoover Institution archived his original notebook diaries (in Polish), letters, and personal documents and photographs.

His papers can be found online. His presentations can be found online.

One of his memoirs is an autobiography based on a diary he kept between 1946 and 2004 (in the USSR, Poland, France, and the USA) and illustrates his evolution from one extreme to another - from devoted Stalinist to active anti-communist:

"Stalin's Hell on Earth". Wasteland Press. 2008. pp.. ISBN 978-1-60047-232-9.

Diary of a Former Communist: Thoughts, Feelings, Reality (memoirs online) (2010)

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"Ludwik Kowalski". Academia.edu. n.d. Retrieved 9 October 2010.

"Ludwik Kowalski Publications". Ludwik Kowalski. Retrieved 10 October 2010.

"Ludwik Kowalski Presentations". Ludwik Kowalski. Retrieved 10 October 2010.

External links

Official site.

"Ludwik Kowalski Papers, 1946-2011". Hoover Institute. 3 January 2011. Retrieved 9 March 2013.

Ludwik Kowalski Biography from Hoover Institute Announcement

The Diaries of Ludwik Kowalski, Former Stalinist, Donated to the Hoover Institution Archives

Monday, January 3, 2011

Ludwik Kowalski, a retired physics professor at Montclair State University in New Jersey, has an extraordinary biography. He was born in 1931 in Warsaw to a Jewish family. Shortly after his birth, his naively idealistic parents, deceived by Soviet propaganda, moved to the Soviet Union. In 1938, Ludwik's father, an engineer, along with tens of thousands of Polish Communists and ethnic Poles, was arrested on false charges and sent to the GULAG. He died of exhaustion in the Kolyma gold mines at the age of thirty-six. Ludwik spent most of his childhood in Moscow, receiving a thoroughly Stalinist education; he and his mother returned to Poland a few months after the end of the war. He completed his secondary and university education in Warsaw, followed by graduate studies in France, from 1957 to 1962; he received a doctorate from the Institute of Nuclear Physics in Orsay. After a brief visit to Poland, the young scientist was invited to a scientific conference in the United States. That 1964 visit led to a research position at Columbia University and his immigration to the United States.

His father's tragic fate had not shaken Kowalski's blind faith in the Soviet system, and he became a dedicated young Communist while living in Poland. In secondary school in Poland, he began keeping a diary, in which he recorded his reactions to developments in his private life, with observations on major developments on the national and international scene. Kowalski continued to write his diary through much of his life, though his notes from the 1950s

are most extensive and interesting as a source on Polish society, education and culture during the early years of the communist regime.

The diary also provides a record of the author's gradual intellectual de-Sovietization and the search for his own identity. Kowalski's reactions to the death of Stalin, the revelations of the Twentieth Congress of the Soviet Communist Party, the Hungarian revolution, and the Polish October 1956 are important landmarks in the young scholar's personal liberation, a process that was very private and took decades to complete. As a university professor and a scientist, he concentrated on his teaching and research and did not reveal his complete political metamorphosis until he retired from academia in 2004. Since that time, he has written two books: Hell on Earth: Brutality and Violence under the Stalinist Regime (2008) and Tyranny to Freedom: Diary of a Former Stalinist (2009). Both books were published by Wasteland Press in Shelbyville, Kentucky.

The Ludwik Kowalski Papers available in the Hoover Institution Archives include the original notebook diaries (in Polish), hundreds of letters, and personal documents and photographs.

Source:

https://www.hoover.org/news/diaries-ludwik-kowalski-former-stalinist-donated-hoover-institution-archives#:~:text=Ludwik%20Kowalski%2C%20a%20retired%20physics%20professor%20at%20Montclair,by%20Soviet%20propaganda%2C%20moved%20to%20the%20Soviet%20Union.

Appendix B. Changing Views on Communism

Dr. Kowalski's family (Figure B-1) belonged to the Polish Communist Party. In 1932 his parents, Marek and Halina Kowalski, responded to the lure (propaganda) of Communism in the Soviet Union and moved to Moscow.

Figure 8-1.

Ludwik with His Parents, Marek and Halina Kowalski

Unfortunately, the move to the Soviet Union did not end well for the family. Marek was imprisoned during Stalin's Purge of 1938 and died in a Gulag camp two years later at the age of 36. Ludwik and his mother eventually returned to Poland, where he continued to belong to the Communist Party. Sometime later he changed his mind about Communism, or at least Stalinism.

He authored two books on these subjects, “Hell on Earth, Brutality and Violence under the Stalinist Regime” (2008) and “Tyranny to Freedom, Diary of a Former Stalinist” (2009).

Dr. Kowalski has developed eight “projects” on a separate website from his LCF website[19]. One of the projects (webpages) is entitled “Introduction: Stalin’s Hell on Earth”[20]. It is dedicated to his father, Marek Kowalski. It includes links to online chapters of his 2008 book. Another webpage is entitled “Introduction: Diary of a Former Communist”[21] and is dedicated to his mother, Halina Kowalski. It has links to online chapters of his 2009 book.

B1. Introduction: Stalin’s Hell on Earth (Dedicated to Marek Kowalski)

Introduction: Stalin's Hell on Earth

by Ludwik Kowalski

In memory of my father, Marek (Mark Isakovich) Kowalski, a naive idealist deceived by propaganda.

He was one of millions of victims of Stalinism.

The idea of writing about Stalinism sprang from an accidental encounter. In July of 2000, while vacationing in Alaska, I noticed a plaque with the name of the Russian town Magadan in an

[19] www.ludkow.info.

[20] http://ludkow.info/father2/.

[21] http://ludkow.info/diary/.

Anchorage souvenir store. That name had been engraved in my memory since 1939, when I was eight years old, living in Russia. The address: "Kolyma, Magadan, Buchta Nagayevo" was where my father, arrested one year earlier, had died in a concentration camp at the age of 36.

Not too many Americans know that the number of lives lost in Stalin's death camps in the Magadan area (between 1930 and 1960) was comparable to the number of those wasted in Hitler's Auschwitz. My wife and I purchased the plaque. Oval in shape, it has a white carving of a jumping reindeer against a black background. The young lady who sold us the plaque happened to be from Magadan. She told us that "it is a rapidly growing industrial town. It has a great future. Buchta Nagayevo now has tall buildings and discotheques." At that point I told her why I was interested.

She immediately knew what I was referring to and expressed sympathy by giving me a celebratory T-shirt on which "Magadan 1939-1994 Anniversary" was printed. She also told us that Magadan was a sister city of Anchorage, and suggested I contact City Hall for details. There I found a description of Magadan as a modern town which used to be a "peaceful village" at the shore of the Sea of Okhotsk. The horrible Stalinist history of the town was not mentioned. This disturbed me.

One week later we were in Fairbanks, where The University of Alaska library has a large archive of materials (books, maps, interviews, photographs, etc.) devoted to the polar regions. I quickly found books about Magadan, some in English, others in Russian. This book is based on materials found in Fairbanks, and later in other libraries. An inner voice told me that I had to share what I learned, in the name of the victims of Stalinism. I called my compilation "Alaska Notes." My fellow professors at Montclair State University, in New Jersey, were the first to read Alaska Notes.

A discussion that followed guided me toward the second part of this book. Starting with the Chapter entitled, Gruesome Soviet Ideology, it provides additional testimonies and focuses on the ideology of the Soviet state of proletarian dictatorship. That ideology must be studied to understand the tragic history of the Soviet Union. Referring to the prevailing attitude toward Stalinism, Vladimir Putin said, on May 5, 2005, "people in Russia say that those who do not regret the collapse of the Soviet Union have no heart, and those that do regret it have no brain." I can understand this. Is it not ironic that the President of Russia had been a Soviet KGB officer. Equally ironic is the fact that the collapse of the Soviet Union resulted from initiatives of M. Gorbachev and B. Yeltsin - both high officials of the Soviet Communist Party. They knew what was going on.

B2. Introduction: Diary of a Former Communist (Dedicated to Halina Kowalski)

Introduction: Diary of a Former Communist.

by Ludwik Kowalski

In memory of my heroic mother, Halina Kowalska

This personal history traces my evolution from a dedicated Stalinist in Poland to an active anti-Stalinist in the USA. Romantic affairs and scientific preoccupations are not totally ignored. The story is based mostly on diaries spanning the years between 1946 and 2008, stored in an old green metal trunk. In late 2009 I finally decided to open it. Up to then, I had never re-read the diaries. The total volume of the diaries was approximately three cubic feet.

One thing became clear as soon as I started reading. Translating everything made no sense, considering poor composition, numerous repetitions, too many details. But I began to see my life more clearly, and decided there was enough substance to be of interest to others. Fortunately, my wife Linda helped me with the task of editing, as I translated from Polish to English. “Are you going to be embarrassed to read descriptions of episodes from my sexual life?” I asked before we started. The answer was “we are senior citizens now.” Description of my unusual life is likely to generate more questions than I can possibly answer. Answering them would be easier if I were a historian or a sociologist.

Here is my story in a nutshell. Born in 1931 in Poland, I spent my early childhood, up to age 15, in the Soviet Union. During that time my idealistic father became a victim of the Stalinist regime; like millions of others, he was arrested and sent to die in Siberia. My mother and I returned to Poland after the end of WWII. That is where my undergraduate and graduate education were completed. In 1957 I went to France for postgraduate studies. After returning to Poland in 1963 with a Ph.D. in Nuclear Physics, I was invited to a scientific conference in the US, and became a research associate at Columbia University. My teaching career began in 1969. After retiring in 2004, I wrote “Hell on Earth: Brutality and Violence Under The Stalinist Regime,” a short and easy-to-read book for those Americans who know very little about Soviet history. The printed versions of that book, published in 2008, and the current book, published in 2009, are available at amazon.com. Reviews are also available at that site.

B3. Donation to the Hoover Institution

Dr. Kowalski donated his diaries and other papers to the Hoover Institution in January, 2011. A description of the collection is available from the institution's website[22]. The scope and content of Dr. Kowalski's collection is as follows.

> The collection includes the original notebook diaries (in Polish), hundreds of letters, and personal documents and photographs, relating mainly to communism.
>
> Kowalski began keeping a diary in secondary school in Poland. In it he recorded his reactions to developments in his private life, with observations on major developments on the national and international scene. Kowalski continued to write his diary through much of his life, though his notes from the 1950s are most extensive and interesting as a source on Polish society, education and culture during the early years of the communist regime.
>
> The diary also provides a record of the author's gradual intellectual de-Sovietization and the search for his own identity. Kowalski's reactions to the death of Stalin, the revelations of the Twentieth Congress of the Soviet Communist Party, the Hungarian revolution, and the Polish October 1956 are important landmarks in the young scholar's personal liberation, a process that was very private and took decades to complete. As a university professor and a scientist, he concentrated on his teaching and research and did not reveal his complete political metamorphosis until he retired from academia in 2004. Since that time, he has written two books: Hell on Earth: Brutality and Violence under the Stalinist Regime (2008) and Tyranny to Freedom: Diary of a Former Stalinist (2009). Both books were published by Wasteland Press in Shelbyville, Kentucky.

The Hoover Institution Library & Archives is self-described on the website as shown below.

> Founded by Herbert Hoover in 1919, the Hoover Institution Library & Archives are dedicated to documenting war, revolution, and peace in the twentieth and twenty-first centuries. With nearly one million volumes and more than six thousand archival collections from 171 countries, Hoover supports a vibrant community of scholars and a broad public interested in the meaning and role of history.

[22] https://www.hoover.org/news/diaries-ludwik-kowalski-former-stalinist-donated-hoover-institution-archives

Appendix C. Index for the "Learning Cold Fusion" Website

The index to the 416 items on the LCF website is shown in this appendix. The full index and contents have also been published as an addendum to the LRDI report. The citation is as follows:

> Grimshaw, T., 2026. Documentation of Ludwik Kowalski's Contributions to the LENR Field. A Project of the LENR Research Documentation Initiative. Addendum to Third Draft Report: Dr. Kowalski's "Learn Cold Fusion" Website. LENRGY, LLC, Austin, Texas. April.

This website contains other cold fusion items.
Click to see the list of links

Links to "cold fusion" items.

Ludwik Kowalski

My motivation? Click to see a short introduction.

Click here to go to the bottom of this long list

---

0) I am no loger saying "it is woodoo sciece." click
1) Introducing Cold Fusion to students. click
2) A typical "cold-fusion" setup. click
3) Three kinds of Cold fusion. click
4) Short biographies of three Cold Fusion Scientists. click
5) Aberration of the scientific methology. click
6) On dangers of "second hand" publishing. click
7) On Pathological Science (N-rays story). click
8) On Burden of Proof in Science. click
9) Scientific Method in Cold Fusion. click
10) A Russian connection. click
11) Bottom Line. click
12) What do physics teachers think about CF? click
13) More about the Russian Connection. click
14) What is pseudo-scientific in this? click
15) Or what is pseudo-scientific in this? click
16) Here is an example of real pseudo-science. click
17) An Italian connection. click
18) Nobel Prize for "cold fusion?" click
19) A French connection. click
20) Excommunication of heretics? click
21) If it were up to me I would do it. click
22) Another good article summarized. click
23) A Japanese connecion. click
24) Three short introductionary tutorials. click
25) A technical tutorial. click
26) Comments on the 1989 ERAB report. click
27) Conspiracy? For what purpose? click
28) Summary of a very impressive paper. click or
29) Another French connection. click
30) New APS ethics guidelines and the CF issue. click
31) Excess heat for a student lab? Yes, why not. click
32) Pathological science or important observations to share? click
33) How would Richard Feynman react to CF? click
34) My own proposal. click
35) On methodology and on difficulties. click
36) Ethical issues as seen by an active CF researcher. click
37) On coulomb barrier lowering. click
38) Producing radioactive tritium. click

39) Changing isotopic composition. click
40) My cold fusion lecture plan. click
41) Comments from a friend. click
42) More comments.; to publish or not to publish? click
43) One year after the announcment: click
44) Before going to Salt Lake City: click
45) After returning from Salt Lake City: click
46) Charlatans versus scientists: click
47) Catalytic fusion: click
48) Charge Clusters ? click
49) Not accepted by The Physics Teacher: click
50) From the last APS meeting: click
51) US Navy supported cold fusion: click
52) Alchemy in cold fusion: click
53) Another way; role of surface structure: click
54) Criticizing cold fusion: click
55) The smoking gun?: click
56) Technological Con Artistry?: click
57) And what about hydrinos?: click
58) From a debate on another list: click
59) A piece to publish in a newsletter: click
60) Nuclear Alchemy, 1996: click
61) What are the causes of this conflict?: click
62) Cold Fusion was compared with creationism: click
63) Jed's interesting general observations: click
64) Stalin's pseudo-science: click
65) Pseudo-science in Russia today: click
66) Cybernetics as pseudo-science: click
67) Observations made at Texas A&M University: click
68) Two meanings of "impossible:" click
69) Conspiracy to deceive? I do not think so:" click
70) Please help us click
71) A Nobel Laureate about voodoo science click
72) Anecdotal Evidence? click
73) A confirmation of a reproducible excess heat experiment click
74) E. Mallove describes reproducible excess heat experiments click
75) Do not mix science with fiction click
76) Secrecy in cold fusion research click
77) Another evidence of nuclear reactions in "cold fusion" click
78) An older fight for acceptance; the story of Arrhenius click
79) Early beta decay studies compared with cold fusion click
80) Secular theology? click
81) Where are theories of cold fusion? click
82) Speculations of a retired physicist (This unit is being revised by the author) click
83) Disassociate cold fusion from antigravity, hydrinos, etc. click
84) A cold fusion opinion statement of a physics teacher click
85) From a book of a cold fusion researcher in Japan. click
86) Pseudoscience in Russia. click
87) Fighting a straw man. click
88) Rejections of cold fusion papers by editors click
89) Hydrinos again click
90) My talk at the 10th International Cold Fusion Conference click
91) My poster at that conference click
92) Agenda for the preconference cold fusion workshop click

93) Back to stories from Kruglyakov's book click
94) Browsing the Internet click
95) Catalysts in cold fusion? click
96) No gamma rays were found in our experiment click
97) My published letter to the editor of The Physics Teacher click
98) Students demonstrating excess heat from cold fusion click
99) Speeding up radioactive decay? click
100) Documenting a rejection by Physics Today click
101) How excess heat was measured. click
102) A paper by the retired physicist from unit #82. (This unit is being revised by the author) click
103) Students trying to demonstrate excess heat. Is it nuclear? click
104) New alchemy? Yes, indeed. click
105) More about new alchemy experiments. click
106) Why is Norman Ramsey silent today? click
107) Biological alchemy ? click
108) Another experiment for your students ? click
109) A video cassette "Fire from Water" for your students click
110) They need a real leader click
111) Photos of Fleischmann and Jones, August 2003 click
112) The dilemma of a physics teacher. click
113) Unexplained neutrons and protons; recent papers of Steven Jones. click
114) Voices from teachers and students (?) click
115) They need your suppost click
116) A negative evaluation of cold fusion claims click
117) Exposing false claims click
118) New error analysis versus old? click
119) Errors in unison click
120) A Chinese connection click
121) Just Withering from Scientific Neglect click
122) Laser-like X-rays in "cold fusion?" click
123) An important Japanese connection (Iwamura) click
124) How can one doubt that charged particles are real (WAITNING FOR PERMISSION TO SHARE) click
125) An article I want to publish click
126) Reactions or contamination, that is the question click
127) "Water remembers?" This is pseudoscientific click
128) Screening in condensed matter or something else? click
129) Quixotic Fiasco? click
130) Sonofusion becomes acceptable click
131) Cold Fusion History described by Steven Jones click
132) Cold Fusion History described by Martin Fleischmann click
133) Cold Fusion name was dropped click
134) Second evaluation by the DOE decided. How certain is this? click
135) Seek not the golden egg, but the goose click
136) What is cold fusion? click
137) An inventor or a con artist? click
138) Recent Internet messages. click
139) If I were in charge. click
140) Kasagi's papers. click
141) A paper from Dubna, Russia. click
142) In memory of Eugene Mallove. click
143) Questions about science and society. click
144) Catalytic nuclear reactions. click
145) Role of the non-equilibrium. click
146) Scientific or not scientific? click

147) Extract from an old good summary (E. Storms, 2000). click
148) On difficulties communicating. click
149) A message from a young person. click
150) Answers to some of my questions formulated in unit #148. click
151) Richard's simulated debate about excess heat errors. click
152) My review article on current cold fusion claims. click
153) TOO LONG (History of rejections of my review article.) click
154) SHORTER (History of rejections of my review article.) click
155) Storms' tutorial on diffcult cases in calorimetry. click
156) Unexpected charged particls were observed again. click
157) Detecting cold fusion charge particles with CR-39: Comments and questions. click
158) An extract from an interesting MIT article. click
159) Categorization of cold fusion topics. click
160) Radon background or not? (WAITNING FOR PERMISSION TO SHARE) click
161) Josephson's lecture and other comments on cold fusion (mostly from teachers). click
162) An example of a cold fusion claim that makes no sense to me. click
163) Absence of 100% reproducibility: What does it mean? click
164) A case of mutual deception? click
165) A short comment on names and definitions. click
166) Non-scientists in cold fusion? click
167) An unnecessary "open letter?" I think so. click
168) Nucleosythesis in a lab? A Ukrainian connection. click
169) An interesting effect was discovered in Texas click
170) A Swedish connection that became something else. click
171) A lively and informative discussion? I hope so. click
172) Cold fusion being presented to students. click
173) Wikopedia: Philosophical points of view. click
174) What was the origin of excess power? An experiment worth replicating. click
175) According to Mizuno et al. excess power can not possibly be chemical. click
176) Swift nuclear particles from an electrolyte? Check it in a lab. click
177) List of eleven international cold fusion conferences. click
178) Sharing recent messages and comments click
179) A student project. Work in progress. NOT YET POSTED click
180) Please help to preserve cold fusion history. click
181) A new cold fusion book. click
182) Seeing a huge number of cold fusion tracks with my own eyes. click
183) Pictures and numbers. (continuation from the unit #182). click
184) Contamination or very long "life after death?" (continuation from the unit #183). click
185) CR-39 detectors of charged nuclear particles. click
186) Too good to be true? Turning radioactive isotopes into stable isotopes. click
187) Magnetic monopoles in cold fusion, and other claims. click
188) A chemically triggered nuclear process? What else can it be? click
189) About my four attempts to observe a nuclear "cold fusion" effect. click
190) A better generic name for "cold fusion?" click
191) Trying to describe my understanding of Fisher's polyneutrons. click
192) Trying to replicate Oriani's observations in my own cell. An electronic logbook. click
193) Links to another website. click
194) Comments about theories. click
195) A pdf file to share. Click to see my introduction. Then download, if you want. click
196) Open letter to the DOE scientists who investigated recent CANA claims. click
197) My second Oriani effects experiment (the first is described in the unit #192). click
198) Work in progress
199) Nonsense, fraud or very advanced science? click
200) Teachers discussing scientifc methods click

201) Cooperating with a high school student performing excess heat experiments. click
202) Fraudulent claims of a German anthropologist. click
203) On ending the controversy. click
204) An Israeli connection. click
205) A troubling episode. What can be done to prevent such things? click
206) A new Russian report on nuclear alchemy. click
207) Controversial cases in science (from New Scientist). click
208) Haiko's conversation with Martin Fleischmann click
209) An Australian connection. click
210) Making progress toward 100% reproducibility? click
211) Charles Beaudette writes about the DOE report. click
212) Answering four questions. click
213) About the company Energetics Technologies in Israel. click
214) The power of delusion or healthy optimism? click
215) Solar Electricity click
216) Too good to be true click
217) Ukrainian connection again click
218) To do or not to do it? click
219) A workshop at Stevens Institute of Technology. click
220) Upcoming CF workshops and conferences. click
221) Work in progress (Mitch) click
222) The majority of nature's treasures are still hidden. click
223) A spectacular excess heat report from Russia. click
224) A cold fusion colloquium at MIT. click
225) A student essay (WORK IN PROGRESS) click
226) Another attempt to commercialize? click
227) A new version of Fisher's polyneutron theory. click
228) Cars running on water? An old US patent. click
229) A Russian patent of Gnedenko et al. click
230) Translations of two Russian papers. click
231) Gold from carrots. click
232) Free energy and its impact. click
233) More on free energy. click
234) Comments on Ellis' article about laws of complexity. click
235) One year later. click
236) Promises promises. click
237) An MIT professor writes a report on an iESiUSA device shown to him. click
238) What is cold fusion? click
239) Identity theft? Cold fusion claims should be justified scientifically. click
240) Generation of helium in cold fusion. click
241) Questions concerning the protocol described in unit #240 click
242) Now I must deal with two slightly different protocols. click
243) Will sixty letters to the editor be published by Physics Today? click
244) Coulomb barrier depends on the range of nuclear forces. click
245) Avoiding a global disaster. click
246) Manipulating half-lives of radioactive nuclei ? click
247) Can magnetic forces (resulting from rotation) help deuterons to overcome coulomb barriers? click
248) A proposed set of better names for known nuclear anomalies. click
249) Trying to understand a theory explaing Condense Matter Nuclear Science (CMNS) data. click
250) Stanislaw Szpak et al. -- another case of nuclear alchemy. click
251) Fracto-fusion, crack-fusion, Casimir-fusion, van der Waals fusion, hammer-fusion. click
252) An invitation to perform a simple excess heat experiment. click
253) History of Mizuno-type experiments (such as that described in unit #252). click
254) Comments of a theoretical paper of Windom and Larsen. click

255) Progress report and comments. click
256) A possible source of error in some excess heat reports click
257) A difficult to accept statistical protocol of Bass and McKubre click
258) Can systematc errors result from sampling of irrecular waveforms? click
259) The excess heat can be apparent in our next week experiment. click
260) Is that kind of excess heat real or apparent? click
261) How much excess heat ? click
262) Common hydrogen (H2O) verus heavy hydrogen (D2O). click
263) Fraudulent schemes are probably as old as civilization. click
264) Measuring electric energy. click
265) Another Italian connection. click
266) Scared, reassured and scared again. click
267) Excess heat not confirmed in our Texas experiment. click
268) With an apology to Dr. Dean Sinclair click
269) Analytical methods used in CMNS (condense matter nuclear science) research. click
270) Colorado experiments also fail to confirm excess heat. click
271) Another Colorado experiment. click
272) No excess heat from Mizuno-type experiments. click
273) Mircobial Transmutations at ICCF12 click
274) Scientific Fraud ? An article in Washington Post and comments it generated. click
275) Kasagi and excess fusion cross sections at low energies. click
276) Low counts statistics (not finished?) click
277) An outburst of messages. click
278) New tabletop fusion devices: is it hot fusion or not? click
279) Fraudulant financial manipulations ? click
280) No courtesy of replying from Yale Scientific. click
281) All reliable results should be reported. Hiding negative results is not scientific. click
282) Velikovsky's speculations. click
283) Trying to be a moderator at the ISCMNS meeting. click
284) Hydrinos versus CMNS click
285) Our private correspondence before the Colorado-2 experiment. click
286) An exciting Colorado2 experiment and comments over the Internet. click
287) Social aspects of our controvery that started 17 years ago. Work in progress click
288) Voices from a restricted list for CMNS researchers. click
289) Another Russian connection? click
290) Unexpected comments from some subscribers of the restricted CMNS discussion list. click
291) Yes, these experiments are dangerous, but . . . click
292) Why is this kind of discrimination legal? click
293) Pathological science? click
294) A historical overview of cold fusion. click
295) Chiropractic also had to fight for recognition. click
296) About the origin of Mizuno-type excess heat. click
297) Too much sociology? click
298) Nuclear alchemy in CMNS. click
299) Randy Mills and his new chemistry. click
300) Preliminary Colorado2 results. click
301) Colorado2 results are now much less certain. click
302) Alarming numbers and comments. click
303) Well known reactions or something else? click
304) Researchers discussing excess energy. click
305) Science versus protoscience. click
306) How to restrict a Google search to one server? click
307) Archive of private correspondence about Mizuno-type experiments click
308) Steven Jones plus an expecting new book about CMNS click

309) Researchers speculate about NAE (nuclear active environment) click
310) Alchemy versus CMNS; waiting for the proverbial "proof in the pudding." click
311) Reifenschweiler Effect (introducing an expected essay) click
312) My old speculation about another kind of beta decay click
313) Are oil companies responsible for conspiring against CMNS? click
314) Will this be the first simple and truly reprodicible-on-demand demo? click
315) A new phenomenon or a wrong interpretation of experimental data? click
316) A new paradigm at the next stage! Why not? click
317) About CR39 and other things click
318) Theories, metatheories and philosophy click
319) Our Phase 1 of The Galileo Project experiment click
320) Our first steps in Phase 2 of The Galileo Project click
321) My rejected publication + references click
322) Rutherford-Bohr model being questioned. click
323) This publication was not rejected; it was withdrawn. click
324) Additional validation of our claim (made in unit #319). click
325) More about SPAWAR results. click
326) Online logbook of an experiment (continuation of unit #320) click
327) Online logbook of the next PACA experiment (continuation of unit #326) click
328) Srategy and scientific methodology: Recent comments and observations. click
329) Continuation of after item 327; the online logbook. Experiment #5. click
330) Trusting authotities in science click
331) An illustration of propagation of errors via calibration. click
332) Sonofusion is also struggling for recognition. click
333) Oriani's paper that was rejected by Phys Rev C without sending it referees. click
334) For an item devoted to an ongoing Canada project (to be shown to me). still waiting
335) A draft of my Catania 2007 workshop paper. click
336) Catania 2007 paper as submitted, after the workshop. click
337) Catania 2007 paper on nuclear radiation inside a glow discharge cell. click
338) Voices from an interesting discussion about theories. click
339) Three body orbiting: macroscopic and submicroscopic. click
340) Speeding up radioactive decay: why is it not used to destroy radioactive waste? click
341) Bazhutov's search for erzions and enions click
342) Two speculative messages from theoretically-oriented people click
343) Work in progress click
344) A new book about cold fusion (plus 4 recent messages from the CMNS list). click
345) My own comments on the new book about cold fusion. click
346) Calibration of CR-39 and other useful data. click
347) About a new cold fusion paper published in a European mainstream physics journal. click
348) Replying to a student interested in cold fusion click
349) Modeling CR-39 tracks click
350) What is it, unexplained alpha particles or something else? click
351) High voltage electrolysis experiments (updates) click
352) Ludwik's paper for the next Cold Fusion conference (Washington DC, August, 2008) click
353) After the Cold Fusion conference (notes and reflections) click
354) Excess-heat cell of John Dash. click
355) Neutrons ? click
356) 20th anniversary is approaching click
357) Discussing SPAWAR interpretation in a mainstream refereed journal. click
358) Summary of Ludwik's CMNS projects click
359) SPAWAR high energy neutrons (plus other things) click
360) CBS broadcased a unit about cold fusion click
361) SPAWAR triple tracks click
362) Curie Project click

363) About Alchemy and CMNS click
364) Do polyneutrons explain CMNS? click
365) My shot-in-a-dark experiment click
366) Cold Nuclear Fusion: Does it exist? A recent review by a Russian scientist click
367) Discussing theories click
368) The Curie Project (a difficult start) click
369) History of my CR-39 cooperation with Oriani click
370) Spawar new results and new interpretation click
371) Physics Teachers discuss our energy options (not a cold fusion item) click
372) Technical information about CR-39, mylar, etc. click
373) The Curie Project (update) click
374) New Scientist thread (NOT READY discussing cold fusion) click
375) Results from The Curie Project (NOT READY. To be shown after results are published) click
376) Arata-type experiments click
377) Scientific method click
378) Destruction of radioactivity by cavitation or a false alarm? click
379) My paper (comments about SPAWAR results) was rejected by a mainstream journal. click
380) Destruction of radioactivity by cavitation or a false alarm? click
381) Free proceeding from the 4th cold fusion conference (ICCF4) click
382) Four most important cognitive terms to discuss scientific validations. click
383) Other sets of CR-39 results click
384) Loose ends: The debate is going on. click
385) More speculations. click
386) Integrity or hypocrisy (on the Physics Today web site)? click
387) Voices from the private discussion list for researchers.? click
388) A patent for a spectacular energy amplifier click
389) This article might be a joke. click
390) Another set of spectacular claims. But the two papers are poorly written. click
391) Topic to be assigned click
392) A potentiall damaging episode click
393) Rejections of CF manuscripts click
394) Draft of the Montreal article click
395) A new SPAWAR paper (emission of high enegy neutrons). click
396) What is new in March 2012 ? click
397) Ludwik's first Progress in Physics article (about Rossi) to download. click
398) Ludwik's second Progress in Physics article (Social aspects of CF) to download. click
399) Spectacular claims of Andrea Rossi click
400) Why no follow-up investigations? click
401) Curie Project and SPAWAR project (July 2011). click
402) Bacterial transmutations (to download). click
403) Ludwik's 10 Years With Cold Fusion: A Memoir. click
404) Cold fusion is not the same as hot fusion. click
405) AmoTerra Process Destroying Radioactive Waste Again (see Unit 186). click
406) History of the biological alchemy controversy. click
407) Our Curie Project did not confirm this CF claim. click
408) Rossi's claims conflict with traditional nuclear physics. click
409) Social aspects of the cold fusion controversy. click
410) Cold Fusion Energy Levels. click
411) Interesting Fall 2012 messages (production of He4). click
412) NAE again; Storm's summary click
413) Philosophical and Social Aspects click
414) Sample of interesting posts click
415) Another Cold Fusion conference is approaching click
416) Discussing reproducibility click

Return to the top of this list of items.

This website contains other cold fusion items.
Click to see the list of links

========== Comments will be appreciated
kowalskil@mail.montclair.edu

Appendix D. Galileo Project: LCF Articles and Presentation

D1. Articles on LCF: #319, #320 and #321

I promised not to post this report before the item shown below was published in The New Energy Times by Steven Krivit.

http://newenergytimes.com/news/2007/NET21.htm

I hope that experiments now in progress will bring more clarity into the SPAWAR effect discovered by Stanislaw Szpak et al. They have been studying several codeposition effects for more than ten years. But their use of CR-39 detectors is relatively recent.

This website contains other cold fusion items.
Click to see the list of links

319) A contribution to Galileo Project

Ludwik Kowalski; 2/25/2006
Department of Mathematical Sciences
Montclair State University, Upper Montclair, NJ, 07043

1) Introduction

As described in the unit #314, I volunteered to be part of the The Galileo Project (TGP) organized by Steven Krivit. Before reading beyond this introduction, please go to

http://newenergytimes.com/news/2006/NET19.htm

and click the link to item #7, entitled "Extraordinary Evidence." You also should read item #8; I do not want to repeat what has already been described on the above webpage. Read these two items carefully and then read what is below. This unit is about our attempt to replicate one of the recent SPAWAR experiments. The study was conducted together with Michael Stolarz, a physics major at Montclair State University, and with two colleagues -- Brian Scanlan and Simon Young. Michael will present a separate paper at the Sigma Xi student research conference. Richard Oriani, who lives in Minneapolis, was our adviser. It is important to emphasize that several papers about nuclear processes triggered by chemical processes in electrolytic cells have already been published by the SPAWAR team in professional journals (see references in 1). The experiment we attempted to replicate has also been published (6).

The heavy-water electrolyte we used was identical to that described in (1). The time table for the electrolysis (see below) was also very similar. We were able to replicate SPAWAR results that were presented, by Frank Gordon (2). But our interpretation of results is different from that offered by SPAWAR team. The diameters of dominant dark pits, on our CR-39 detector, were found to be too large to be attributed to to alpha particles, or to smaller nuclear projectiles.

Inserted on 3/3/07--> After finishing writing this unit, three days ago, I decided to make a presentation at the upcoming APS meeting. I wrote to the session chairman, Scott Chubb, that I have something important to share and would be happy to take place of someone who does not show up. He offered me a time slot and suggested that I write an abstract and distribute it at the conference. Here is my abstract"

Title: Nuclear or not nuclear? That is the question.
Authors: Ludwik Kowalski, Brian Scanlan and Simon Young.

Text: An experiment described by Frank Gordon, last summer (1), has been successfully replicated at Montclair State University. Copious pits were observed on the CR-39 chip that was in contact with the silver cathode wire in the magnetic field. No such pits were observed on the identical chip from the cell that was not in magnetic field. The two cells were in series with each other during the electrolysis. Diameters of large post-electrolysis pits have been compared with diameters of pits due to alpha particles from a radioactive source. On the basis of this analysis, and on the basis calibration curves reported by Roussetski et al. (2), we conclude that large pits on our CR-39 chip, from one experiment, cannot possibly be attributed to alpha particles, or to less massive nuclear projectiles.

References:
1) F. Gordon et al, at the Naval Science & Technology Partnership conference hosted by the National Defense Industrial Association and the Office of Naval Research (Washington, DC, August 2, 2006).) See items 7 and 8 in
2) A.S. Roussetski, A.G. Lipson and V.P. Andreanov; "Nuclear emission from titanium hydride/deuteride induced by powerful picisecond laser beam." Proceedings of the 10th international conference on cold fusion, pages 559-566.

2) Experimental Method

Two identical electrolytic cells were connected to power supply, as shown in Figure 1. The little boxes, at the upper left corner represent integrated circuits designed to maintain constant currents. To change the current we simply had to replace one box by another. The voltage of the power supply was sufficiently high (up to 50 V) to support this mode of operation. This inexpensive method of keeping the current constant was introduced to us by Dr. Winthrop Williams. The experiment would not be possible without his help in dealing with numerous practical problems.

- -

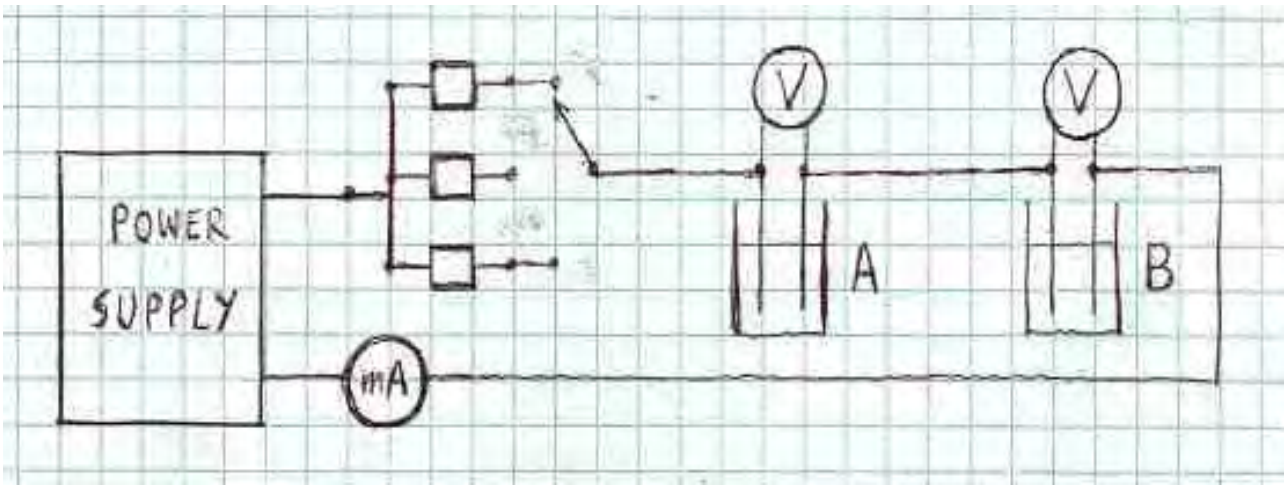

FIGURE 1
Our electric circuit contained nine integrated circuits (LM334 for lower currents and LM317T for higher currents). Only three of them are show in this diagram. A third cell was connected in series with A and B. It is not shown in this figure because it was part of another experiment (to be described in another unit).

- -

The two electrolytic cells, like those described in (1), were made from acrylic. Each has a wire-made platinum anode and a wire-made silver cathode, as shown in Figure 2. A CR-39 detector of nuclear particles was mechanically applied to the silver wire of each cell.

The only difference between the two cells was that one (A) was placed in a magnetic field crated by two very strong

neodymium magnets while the other (B) was far away from any source of magnetic field higher than terrestrial.

- -

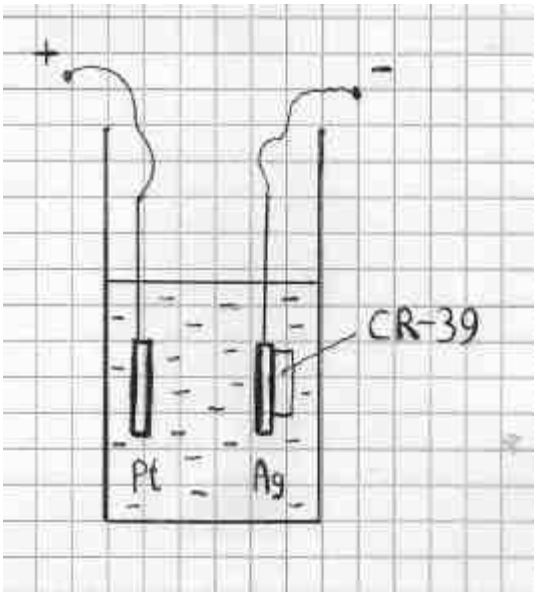

FIGURE 2
The distance between the anode and the cathode was approximately 15 mm. The height of the acrylic cell was 80 mm. The direction of the magnetic field lines (in cell A only) was from the anode toward the cathode. The magnetic field near the cathode was close to 0.2 T.

- -

The following time table was used during the electrolysis:

Current 0.11 mA, 24 hours
Current 0.21 mA, 24 hours
Current 0.56 mA, 12 days, till the electrolyte changed from brown to transparent
Current 1.0 mA, 24 hours
Current 5.0 mA, 24 hours
Current 10.1 mA, 24 hours
Current 26.9 mA, 24 hours
Current 55 mA, 24 hours
Current 127 mA, 24 hours

Heavy water had to be added to cells in order to compensate for what was lost due to evaporation and electrolysis. Increases in electric current were accompanied by increases of potentials across the cells. The potentials at the beginning of the electrolysis were 0.96 volts on each cell; the potentials at the end of the electrolysis were close to 8 volts. (At 1 mA they were close to 1.6 V and at 50 mA they were close to 5.5 V).

After the electrolysis the CR-39 chips were removed and visually examine. The opalescencent lines were seen on the chip from the cell A, more or less where the cathode wires were. No such lines were seen on the chip from the cell B. Another detail is worth mentioning. Before the electrolysis the cathodes in both cells were in contact with detectors along the entire length. After the electrolysis the cathode wire in cell B was still in contact with the detector along the entire length. But the cathode wire in the cell A bulged away from the detector, except near the edges. The chips ere etched together for 3 hours in the standard NaOH electrolyte (6.25 N and 68 C), rinsed and examined under the

microscope. The CR-39 chip removed form the cell A had several areas in which numerous pits were observed, as illustrated in Figure 3. In that respect our results are in very good agreement with results reported by the SPAWAR team. The CR-39 chip removed from the cell B had no areas covered with such pits. That observation is also consistent with what was reported by the SPAWAR team. We agree with them that the magnetic field plays a role in the formation of pits. The mechanism of this effect is not clear at this time.

- -

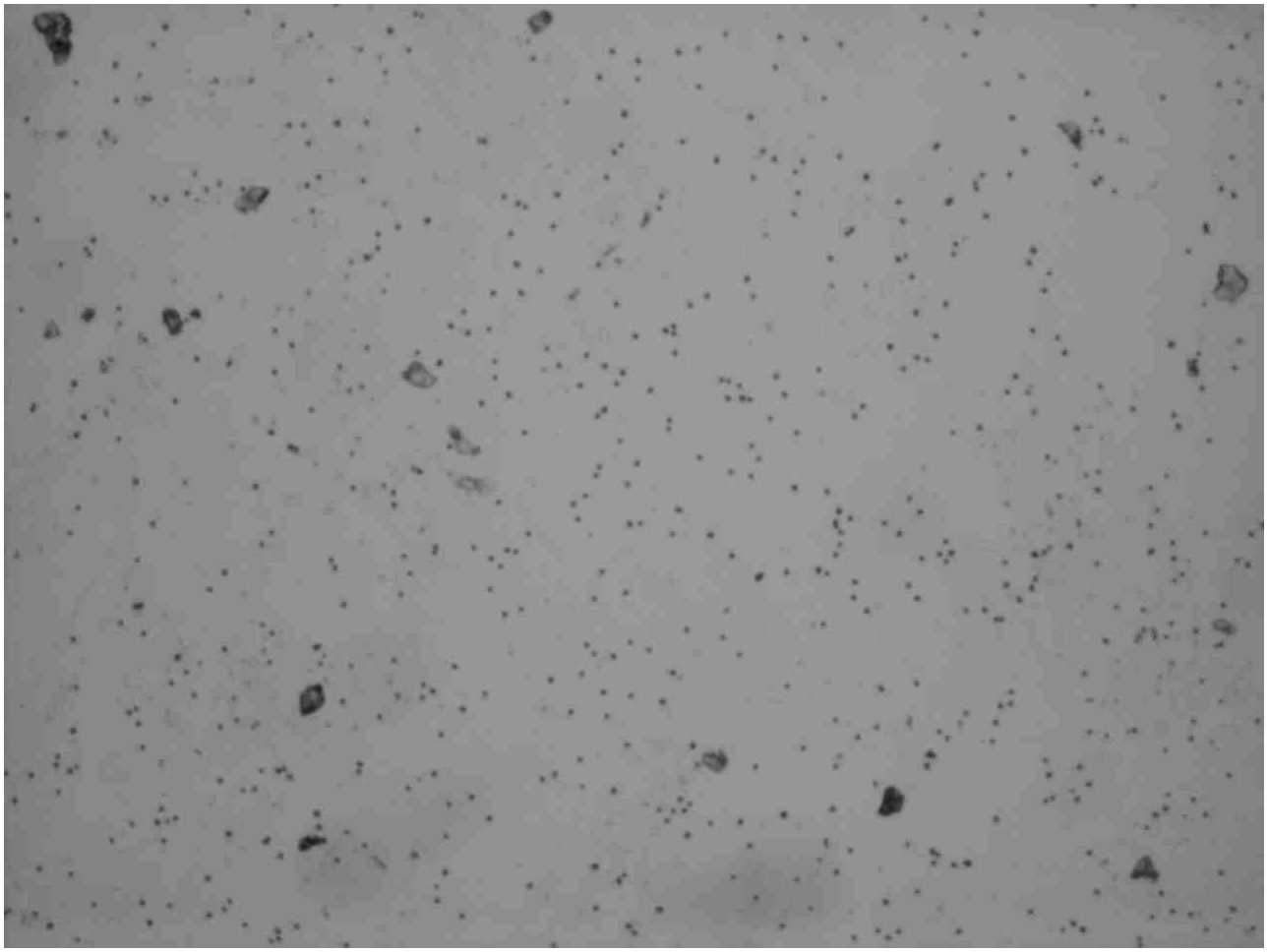

FIGURE 3A
Dot-like pits due to alpha particles from our Am-241 source at magnificatin 40. Surface defects or dust can also be seen. The area on this low magnification photo is 1.3 by 0.9 mm.

- -

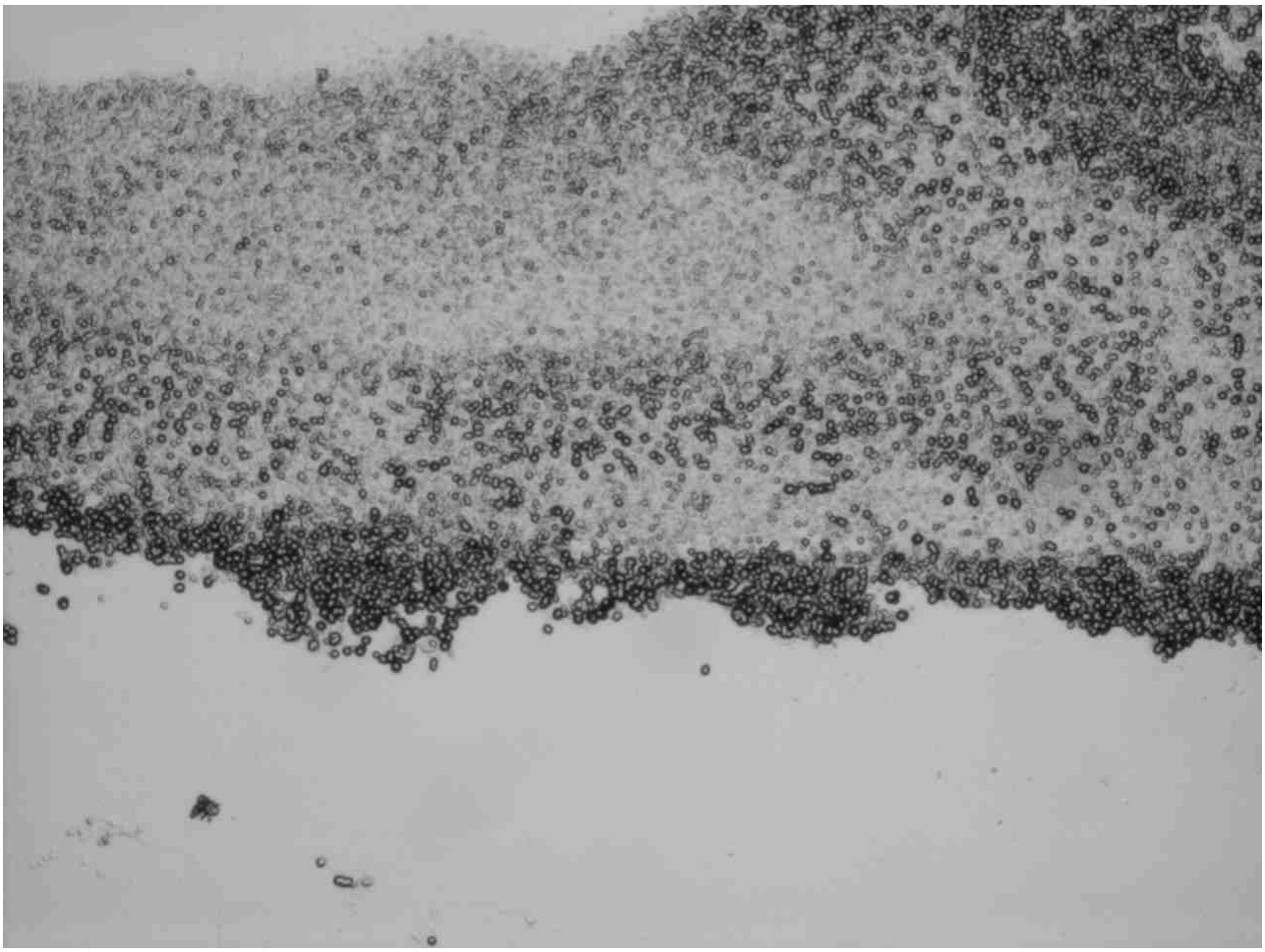

FIGURE 3B
Piits on the post-electrolysis chip from the cell A. Etching conditions and magnification were the same as for the photo on Figure 3A. The area on this low magnification photo is 1.3 by 0.9 mm. Next four figures should be used to compare diameters of post-electrolysis pits with diameters of pits due to alpha particles.

The claim of nuclear origin of pits, made by SPAWAR team, was based on comparisons of observed pits with pits that were known to be due to alpha particles. We followed the same approach. A fresh CR-39 chip was irradiated with alpha particles from an 241 Am source. It was etched together with the post-electrolysis chips. Figure 4 shows pits due to alpha particles, under the microscopic magnification of 200. Figure 5 shows pits on the chip removed from the cell A, under the same magnification. It is clear that the post-electrolysis pits are larger than pits due to alpha particles. The same conclusion can be reached by comparing pits in Figures 6 and 7. They were taken under the magnification of 400.

- -

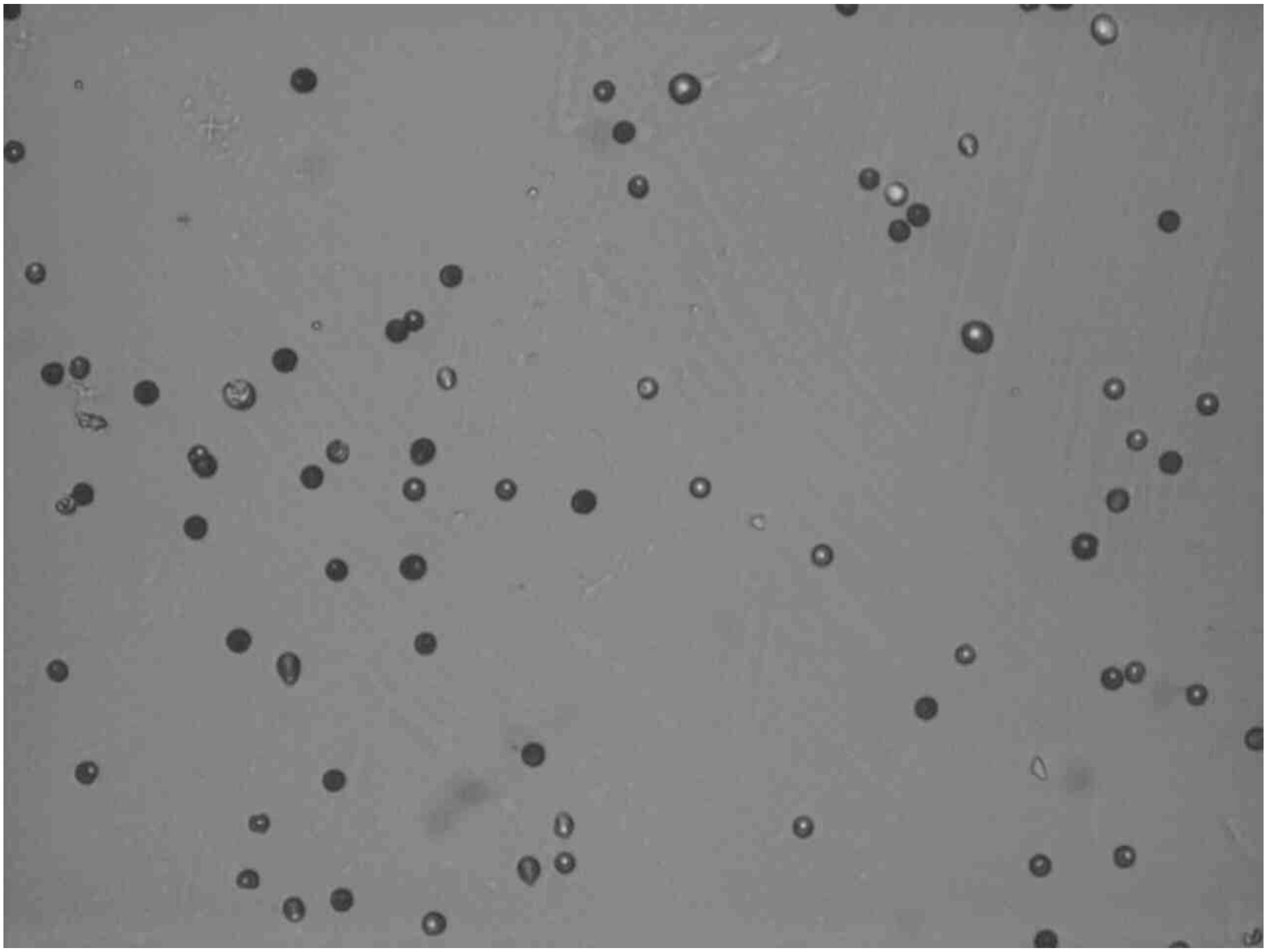

FIGURE 4
Pits which are known to be tracks of alpha particles, under magnification of 200

- -

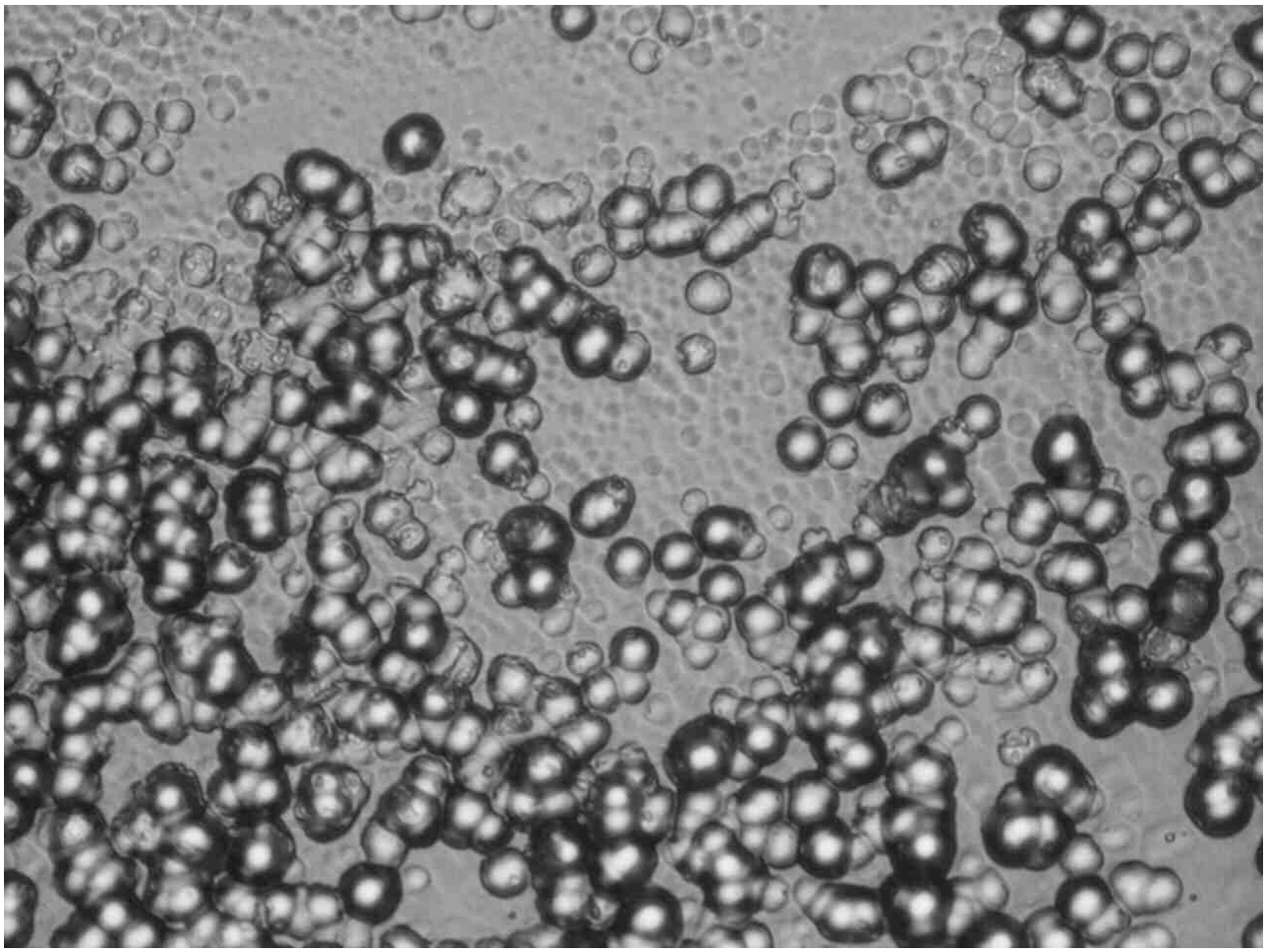

FIGURE 5
Pits on the post-electrolysis chip from the cell A. Etching conditions and magnification were the same as for the photo on Figure 4. Note that diameters of faint pits are about the same, and smaller, than diameters of dark pits in Figure 4. Large dark pits can't be due to alpha particles, or lighter projectiles.

- -

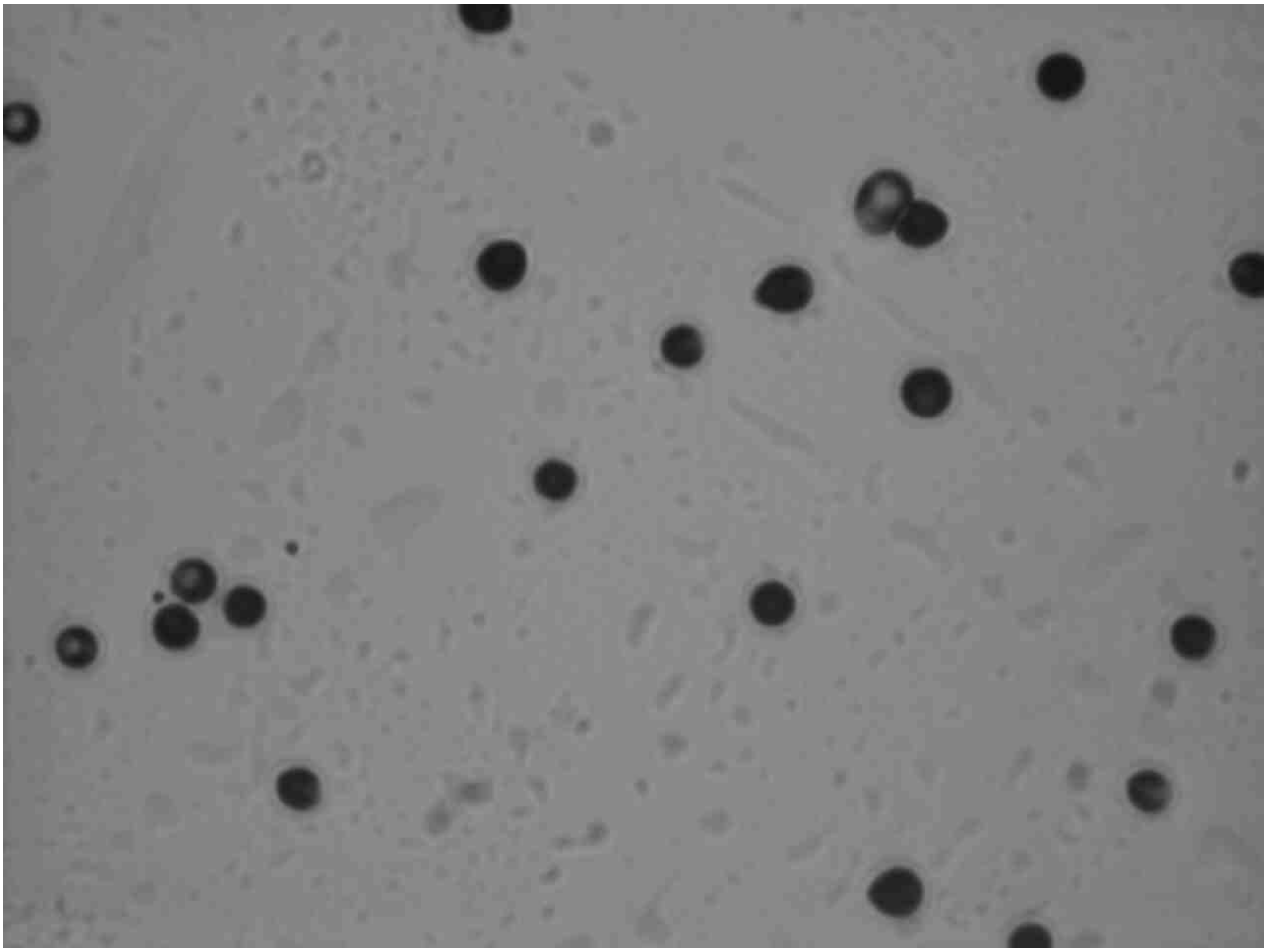

FIGURE 6
Pits which are known to be tracks of alpha particles, under magnification of 400

- -

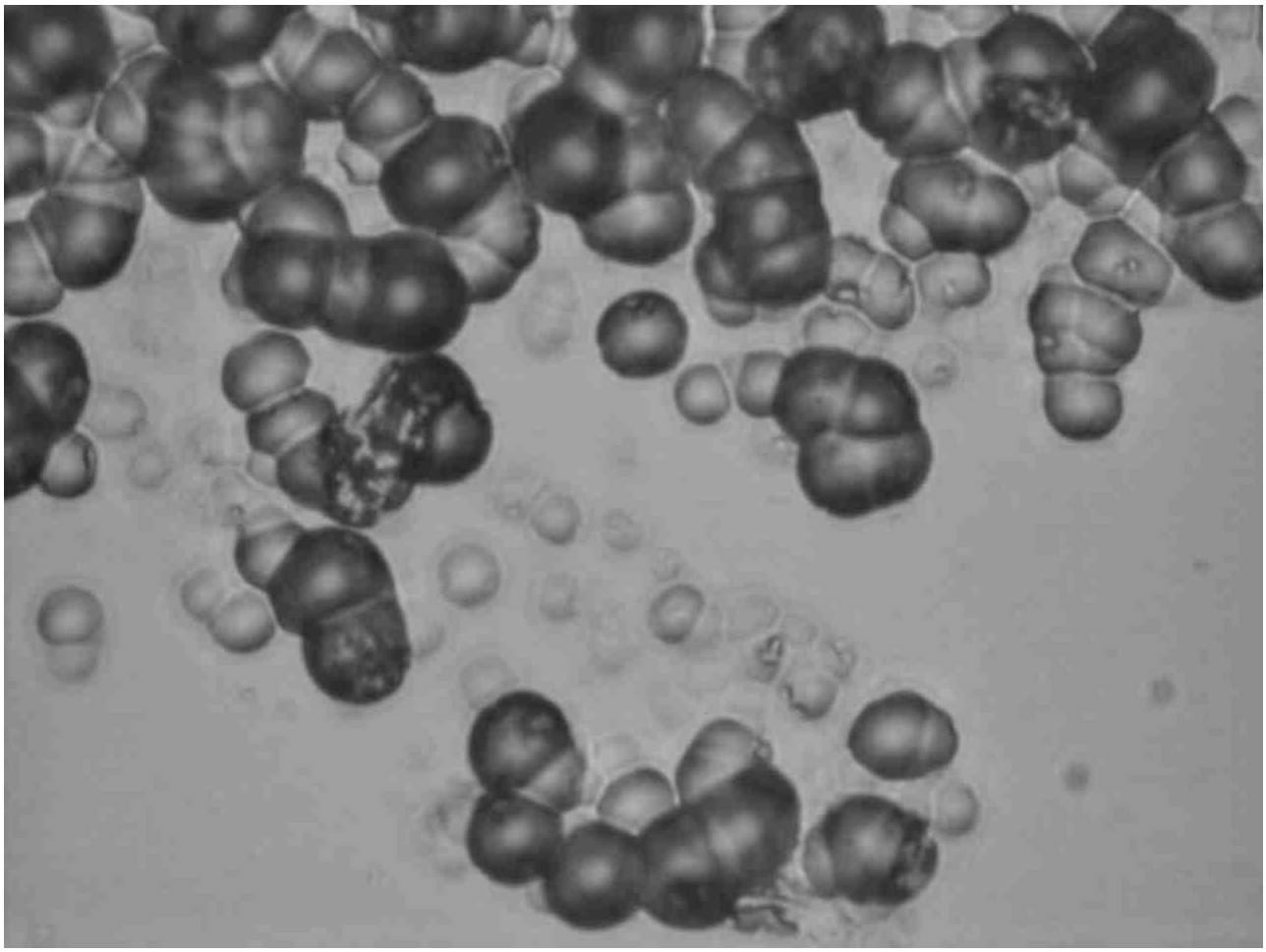

FIGURE 7
Pits on the post-electrolysis chip from the cell A. Etching conditions and magnification were the same as for the photo on Figure 6. Note that diameters of faint pits are about the same, and smaller, than diameters of dark pits in Figure 6. Large dark pits can't be due to alpha particles, or lighter projectiles.

- -

Use of CR-39 detectors in air is common (3). But their prolonged use in the electrolyte, initiated by SPAWAR team, is new. A separate test was necessary to verify that prolonged exposure of CR-39 to the electrolyte does not interfere with its ability to detect alpha particles. A small piece of CR-39 was cut from the chip A, after the electrolysis, and exposed to alpha particles. It was then etched and examined under the microscope. Presence of familiar tracks reassured us that ability to detect particles was not lost during the electrolysis. Furthermore, the thickness of the post-electrolysis chip A, after etching, was compared with the thickens of the blank chip that was etched with it. The difference, if any was, smaller than 5 microns. This additional test was necessary to rule out a possibility that exposure to the electrolyte changed the bulk etching speed.

Absence of pits on the post-electrolysis chip B was a strong indication that the pit-formation process was strongly influenced by the magnetic field. That fact reminded us Marie Curie's attempts to influence radioactivity by magnetic fields. These attempts, like those undertaken by other investigators, were not successful. Magnetic fields do influence some atomic processes but their ability to either slow down or to speed up emission of alpha particles (or protons) from atomic nuclei has never been demonstrated.

3) Discussion

Differences between ^{241}Am pits and pits found on the post-electrolysis chips are not limited to sizes (typical alpha

particles tracks are about 2.5 times smaller than typical post-electrolysis pits). Another difference becomes obvious when one tries to refocus the microscope under a larger magnification, such as 200 or 400. Isolated post-electrolysis pits seem to be more shallow than pits produced by alpha particles. Most post-electrolysis chips appear so close to each other that the individual boundaries are no longer identifiable. The areas covered by overlapping pits are whitish, as if they were affected by some kind of chemical corrosion.

Let us assume that individually identifiable post-electrolysis pits are due to nuclear particles, as claimed by the SPAWAR team (6), and not to a chemical, thermal or mechancal effect. What kind of particles can produce pits shown in Figure 3? To answer this question we will use results of measurements made by Roussetski et al. (4). These Russian scientists exposed CR-39 chips to protons and alpha particles of various energies. Then they etched the chips, measured the diameters and summarized the results in the form of a very useful graph. The smallest diameter (5 microns) was for protons of 3 MeV, the largest diameter (12 microns) was for alpha particles of 2 MeV. The etching in the NaOH (6N, 70 C) took 7 hours.

Our etching conditions were quite different, as specified in section 2. For that reason we decided to modify the original graph slightly, as shown in Figure 8. Our diameters are expressed in arbitrary units named prots. One prot, by definition, is the diameter due to 3 MeV protons, no matter how many microns it is under different etching conditions. The advantage of that representation is obvious; it decouples essential information (relative sizes of pits) from the laboratory-specific details about etching. The modified graph is likely to be valid, at least approximately, when etching conditions are not exactly the same as those used in (4).

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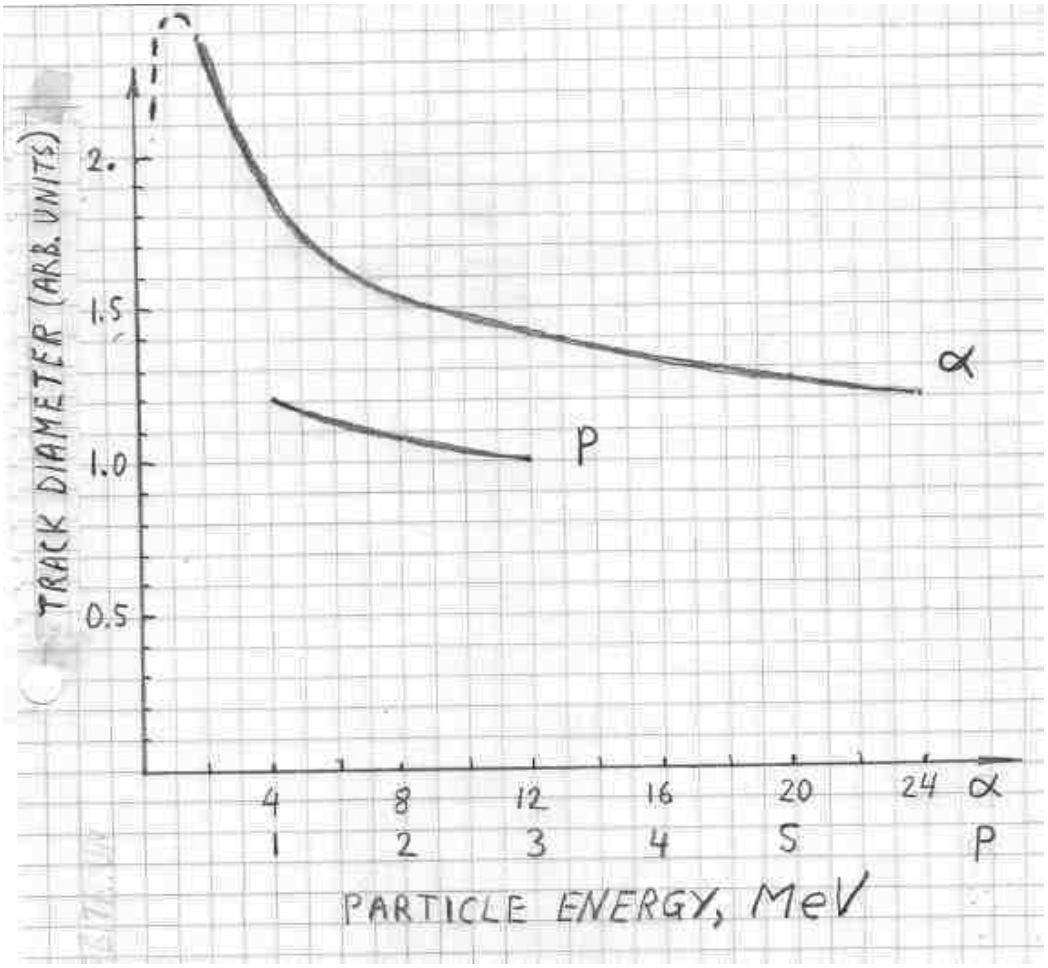

FIGURE 8
Relative diameters of tracks due to alpha particles and protons of various energies. The dashed line, at the upper left

corner, was added to indicate that alpha particles cannot possibly produce tracks larger than about 2.5 prots. This expectation is based on the well known fact that alpha particles at energies below 2 MeV tend to capture electrons when they slow down. The dE/dx values do not increase when E becomes sammler than about 2 MeV.

- -

According to Figure 8, the mean diameter of alpha particles from ^{241}Am (~ 5 MeV) is 1.7 prot. According to Figures 6 and 7, individually identifiable post-electrolysis diameters are about 2.5 times larger than those due to ^{241}Am. In other words, their average pit diameters are 1.7*2.5=4.5 prots This at once rules out a possibility that the post-electrolysis pits are due to protons or to neutrons; diameters of tracks due to these particles would be smaller than 1.5 ports. It also rules out a possibility that poste-electrolysis chips are due to alpha particles; diameters of these would be smaller than 2.5 prot. The only available avenue, for defending nuclear origin of post-electrolysis chips, it to assume that they are caused by nuclear projectiles that are much heavier than alpha particles. Fission fragments from a californium source, for example, were shown (5) to produce pits that were about 2.5 times larger than those due to alpha particles. But that does not mean that any large pit on the CR-39 surface is a track of a massive nuclear projectile. An additional evidence would be needed to defend such claim.

According to (6), "the density of tracks registered by a CR-39 detector was found to be of a magnitude that provides indisputable evidence of their nuclear origin." Our analysis of diameters of pits does not support this interpretation, unless only massive particles are responsible for the large pits shown in Figures 5 and 7. A more likely explanation is that post-electolysis pits are due to a non-nuclear process taking place near the cathode. This kind of interpretation is supported by presence of glossy regions on the chip from the cell A, visible before the chip was etched (see section 2). Prolonged exposures of CR-39 to alpha particles, undertaken to produce overlapping pits, did produce foggy (but not glossy) areas on chips, but such areas could be seen only after etching. One might speculate, for example, that hydroxyl ions, produced by reduction of water, somehow concentrated near the CR-39 surface, attacked the plastic.

Inserted on 4/6/07:
Let me comment on the four pictures at the bottom of:

http://newenergytimes.com/Reports/Krivit-KowalskiCR-39Discussion.htm

The photos were made by SPAWAR researchers. The right two clearly confirm what I was saying about relative sizes of pits. Pits on the post-electrolysis chips are about 2.5 times larger than pits due to our alpha particles, under identical etching conditions. The left two pictures show that for SPAWAR chips the ratio was close to 1.7. This lower ratio also supports my hypothesis. It implies that a typical diameter of promenent pits is 1.7*1.7=2.9 prots. This is larger than the maximum for alpha partcles, as indicated in Fifure 8.

External fields:
The most puzzling fact, discovered by the SPAWAR team (6), and confirmed by our experiment, is that the unexplained pits are produced only when magnetic field is present. Suppose the post-electrolysis pits are due to a non-nuclear process of some kind. In that case the role played by magnetic field is also far from being clear. As far as we know, magnetic fields do not drastically change rates of chemical reactions. If this is true then something new, and totally unexpected, has been discovered by the SPAWAR team. The magnetic field effect is interesting and worth investigating, even if pits are not of nuclear origin. The magnetic field effect, by the way, is a very good indication that observed pits are not due to a familiar process, such as radioactive contamination in the cell or an ordinary chemical attack.

The effect of the electric field, reported in (2), is even more puzzling. The difference of potential of 6000 volts, applied to parallel copper plates situated outside the cell with acrylic walls, should make a negligible contribution to the electric field inside a cell with the electrolyte. This is due to the very high resistivity of acrylic in comparison with the low resistivity of the electrolyte. And yet, the removal of an extremely small electric field stops a process by which track-looking pits are formed on the CR-39 detector. This effect is also interesting and worth investigating.

Additional reflections. (Inserted on 3/16/07):
Why do many scientists reject the idea that a nuclear process can be triggered by a chemical process? Because this idea conflicts with the atomic model they have already accepted. That model, progressively evolved during the 20th century, makes sense out of a large number of experimental facts. It states that chemical reactions are changes at the level of the outermost electrons orbiting around atomic nuclei. Nuclear reactions, on the other hand, are changes occurring in tiny central regions of atoms. Nuclear force bounding protons and neutrons in these tiny regions have very short ranges; they have no effect on orbiting electrons. Coulomb forces between electrons and protons are negligibly small in comparison with nuclear forces. That is why electronic rearrangements (chemical changes) are generally expected to have negligible effects on what is going on inside atomic nuclei.

In research, and in teaching, we progress from what is already known toward what remains hidden. New models of reality are refinements of models that are known to be consistent with experimental facts. That is why many scientists say that cold fusion claims are extraordinary. They feel that one has to be extraordinary reserved toward such claims. To "be reserved," however, does not mean to "reject." It means to ask many more questions than in the case of claims that are consistent with accepted models.

It is important to emphasize that that SPAWAR scientists, like many other cold fusion researchers, are experimentalists. They observe what happens and try to interpret it in terms of familiar concepts. Our photos (shown in Figures 3, 5 and 7) are very similar to their photos (shown in reference 2). But we do not know if large pits on their post-electrolysis chips are also considerably larger than pits on their chips exposed to alpha particles (under identical etching conditions). The disagreement is about the idea that large post-electrolysis chips can be attributed to alpha particles, and lighter nuclear projectiles. Fortunately, experiments seem to be reproducible. In that situation the disagreement can easily be turned into an agreement, one way or another. It becomes a simple matter of remeasuring, or repeating the experiment. Will the clear picture emerge before the next week anniversary of cold fusion? I hope so.

Inserted 3/23/07
Our paper was discussed extensively on the private list for CMNS researchers. Replying to one message I wrote:

From past experience (placing CR-39 on a Pu-Be neutron source) I know that most tracks due to neutrons (via collisions with hydrogen nuclei in that material C_{12}-H_{18}-O_7) have diameters that are clearly smaller than diameters due to alpha particles from the Am-241 source. Less than one percent of tracks due to neutrons had diameters comparable to those seen on the chip exposed to Am-241. I attributed these large pits to the (n,alpha) reactions. That is why I said that pits larger than those due to Am-241 (identical magnification and etching) cannot be attributed "to neutrons." Perhaps I should have said "to protons produced by neutrons." I am not denying a possibility that large dark pits on our photos might be due "to carbon or oxygen ions produced by neutrons," as suggested at the APS meeting by Larry.

Let me estimate the number of neutrons that would be responsible for large pits Larry observed on the "back side" of his CR-39 chip. The rule of thumb used by Frank Gordon (private communication during the APS conference) is that only one out of hundred thousand fast neutrons, passing through a CR-39 detector, interacts with with atomic nuclei insid the detector. Taking this for granted, let me estimate the number of neutrons from the number of large pits. To accomplish this I will make the following assumptions (which can easily be modifies). To accomplish this I will make four simplifying assumptions. These arbitrary assumptions can be modified, if necessary.

a) One half out of 100,000 intercepted neutrons is colliding with heavy nuclei in CR-39 producing ions of C and O. The other half is scattered on hydrogen.
b) The mean range of heavy ions in CR-39 is 1 mg/cm^2, which is 8 microns.
c) Larry's large dark pits are due to only 25% of heavy ions produced in the last 8 microns of CR-39.
d) The thickness of the CR-39 chip is 1 mm, which is 1000 microns.

Thus the fraction of ions responsible for Larry's pits is 0.25*8/1000=1/500. In other words, the number of large pits will be 500 times smaller than the number of neutrons interacting with heavy nuclei inside the CR-39 chip. If 100,000 neutrons produce one interaction then 500*100000=5*10^7 neutrons will produce one Larry's pit. For 1000 pits the

number of neutrons would be 5*10 . This is 50 billion. Another conclusion is that large pits should also be observed on the front surface of the CR-39 chip.

This speculation was posted on the private list for CMNS researchers. Here is a comment from one researcher: " If 50 million neutrons were emitted by the cell during the last 48 hours of the run when the current is at its highest value (and presumably making the nuclear reactions happen), the flux onto the CR-39 chip would be 290,000 neutrons/sec. That'd make any neutron detector sing, so to speak. Also, assuming the neutrons are 2.5 Mev, it only takes 20 neutrons/cm^2/sec to make a dose rate of 2.5 mrem/hr (1)....so the CR-39 chip would see a dose rate of ~36 rem/hour (assuming the chip was 1 cm^2). The dose falls off rapidly with distance but, still, such an experiment would be of considerable hazard to the health of bystanders. (1). Radiation Detection and Measurement, Glenn Knoll, 1979, p. 802."

After reading the 2002 paper of J.A. Frenje et al. (Review of Scientific Instruments, vol. 77, July 2002, p 2595) I realized that the assumption (a) above was highly unrealistic. Experimental data show that the number of large pits due to heavy ions represents only a very small fraction of one percent of all pits, even for 14 MeV neutrons. For that reason the 50 million neutrons was underestimated by orders of magnitude. To produce 1000 pits on CR-39, the number of neutrons had to be much larger than 5 billion. Fortunately, a Landauer neutron detector was used in one of The Galileo Project experiments. How many neutrons were actually detected during that experiment? I will answer this question as soon as the outcome is announced.

References:

(1) Stanislaw Szpak · Pamela A Mosier Boss, Charles Young and Frank E. Gordon; "Evidence of nuclear reactions in the Pd lattice." Naturwissenschaften (2005) 00: 1–4 DOI 10.1007/s00114-005-0008-7. he document can be downloaded from the library at http://lenr-canr.org

2) F. Gordon et al, at the Naval Science & Technology Partnership conference hosted by the National Defense Industrial Association and the Office of Naval Research (Washington, DC, August 2, 2006). The document can be downloaded from the library at http://lenr-canr.org

3) D. Nikezic and K.N. Yu, "Formation and growth of tracks in nuclear track materials;" Material Science and Engineering R 46 (2004) p 51-123.

4) A.S. Roussetski, A.G. Lipson and V.P. Andreanov; "Nuclear emission from titanium hydride/deuteride induced by powerful picisecond laser beam."Proceedings of the 10th international conference on cold fusion, pages 559-566. The document can be downloaded from the library at http://lenr-canr.org

(5) D. Paul et al., "An SSNT study of spontaneous fission fragments from the soil gas samples of Bekerswar thermal springs;" Radiation Measurements 33 (2001) p 167-169

6) Stanislaw Szpak, Pamela A Mosier Boss and Frank E. Gordon; "Further evidence of nuclear reactions in the Pd/D lattice." Naturwissenschaften (2007) DOI 10.1007/g00114-007-0221-7. Can be downloaded from the library at http://lenr-canr.org

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320) Phase 2 of Galileo Project

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1) Introduction (written in September of 2007):
CMNS researchers are fully aware that a simple reproducible-on-demand demo of a nuclear effect, such as emission of alpha particles, caused by a chemical process would have profound effect on mainstream scientific community. The accepted point of view is that chemical processes (interactions involving valence electrons in atoms and molecules) are too weak to produce emission of nucleons, or groups of nucleons, from atomic nuclei. Yet, several researchers, such as Lipson, Jones, and Oriani, have been reporting unexpected emission of alpha particles, for many years.

About two years ago I decided to replicate Oriani's effects. This was described in unit #192 at this website. I did observe results reported by Oriani but they were not sufficiently reproducible. Last summer I found out that Richard Oriani has a draft of an unpublished paper in which new results, confirming emission of nuclear particles, are described. The methodology used by him was essentially the same as that described in unit #192. But instead of investigating several effects he decided to focus on one effect -- emission of alpha particles from a cathode, after the end of electrolysis. Thirty seven similar experiments were performed; a typical electrolysis time was 3 days and a typical counting time was 2 days. The mean track density, attributed to nuclear particles, turned out to be 90 tracks per cm^2. The standard deviation was 35; and the lowest result was 40. This was significantly higher than results from 24 control experiments, where the mean track density turned out to be 20. The standard deviation for the control samples was 11 and the lowest value was zero.

2) Inserted on 3/28/07:
Impressed by the above results, I decided to perform similar experiments. But plans changed after The Galileo Project (TGP) was organized by Steve Krivit. The Phase 1 of that project, and our contribution to it, are described in unit #319. When we were busy with the Phase 1 Oriani was performing a new sequence of experiments. The major modification, in comparison with his earlier setup, was a very thin window (5 microns of polypropylene) placed between the O rings. The cathode wire was above that window (in the electrolyte) while the CR-39 detector was below that window (in air). With this setup Oriani was able to record tracks of nuclear particles, presumably alpha particles or protons. They were emitted during the electrolysis, not after the electrolysis. The numbers of tracks on chips below the cathode were about two orders of magnitude higher than on blank chips of the same size. The photo of tracks after one of his experiments is shown in Figure 1 below.

The idea of using thin foils separating CR-39 detectors from the electrolyte has been discussed extensively on a special Internet list for TGP researchers. But Oriani was the first to implement this idea. He was also the first to use the term "PACA detector," for a CR-39 chip "protected against chemical attack." In fact, he invented this term after being convinced that at least some of copious SPAWAR pits resulted from chemical, rather than nuclear, activity.

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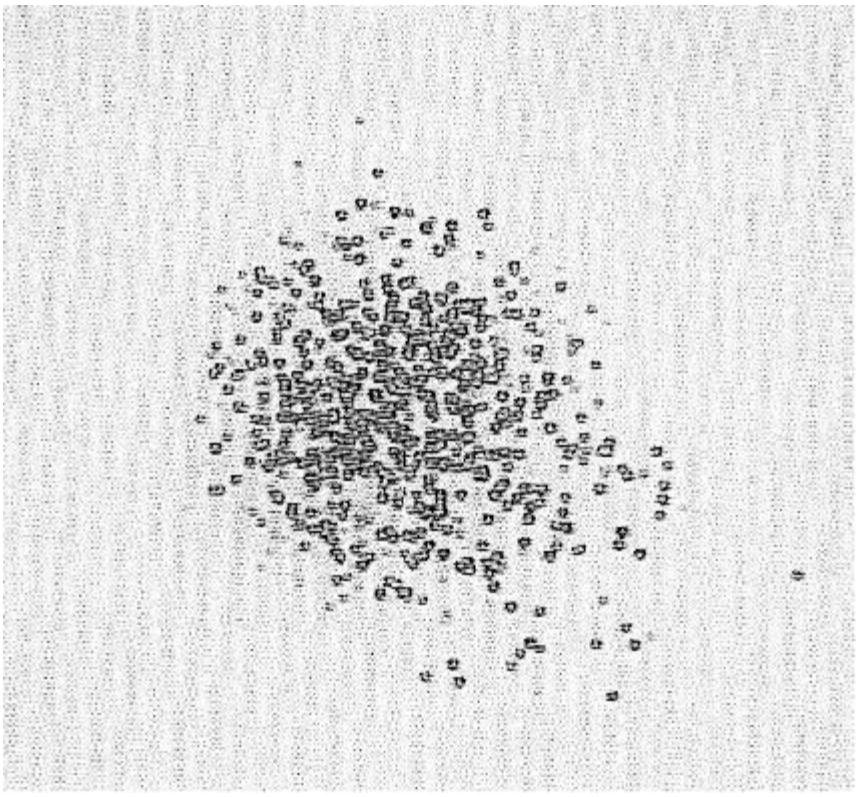

Figure 1
A spectacular shower of Particles reported by Richard Oriani. Magnification was 100; the area on the photo is about 1.5 mm^2." The clustering of tracks is remarkable. In a private message (4/2/07) Richard wrote: "In past work I have compared mean diameters of tracks produced by Am241 and by my experiments when both detector chips are etched at the same time, and have found that the tracks produced in the experiments are a bit smaller than those produced by Am 241. "

- -

Our Phase 1 contribution to TGP project has been described in a paper presented at the recent APS meeting (see unit #319). Proceeding toward the Phase 2 we wanted to replicate new Oriani experiments. What follows was composed from my own notes, and from pieces of information supplied by other people. Numbering of individual sections of this unit corresponds to chronological order, more or less, in which separate pieces were added and modified. I apologize for some repetitions.

3) Experimental method
Our version of Oriani's thin-window cell is shown in Figure 2 below. Note that only one O-ring was used in our first Phase 2 experiment. Oriani, on the other hand, used two O-rings, one on the wet side of the window and one on the dry side. In at least one case he used the same heavy water electrolyte as the SPAWAR team (see unit #319) in other cases he used Li_2SO_4 in light water. Results, he reported, were not very different. He did not use magnets. In our first Phase 2 experiment the electrolyte was also the same as that used by the SPAWAR team. In our second Phase 2 experiment, still being analyzed, we used the same electrolyte as Oriani (2.367 grams of Li_2SO_4 in 100 cc of light water)

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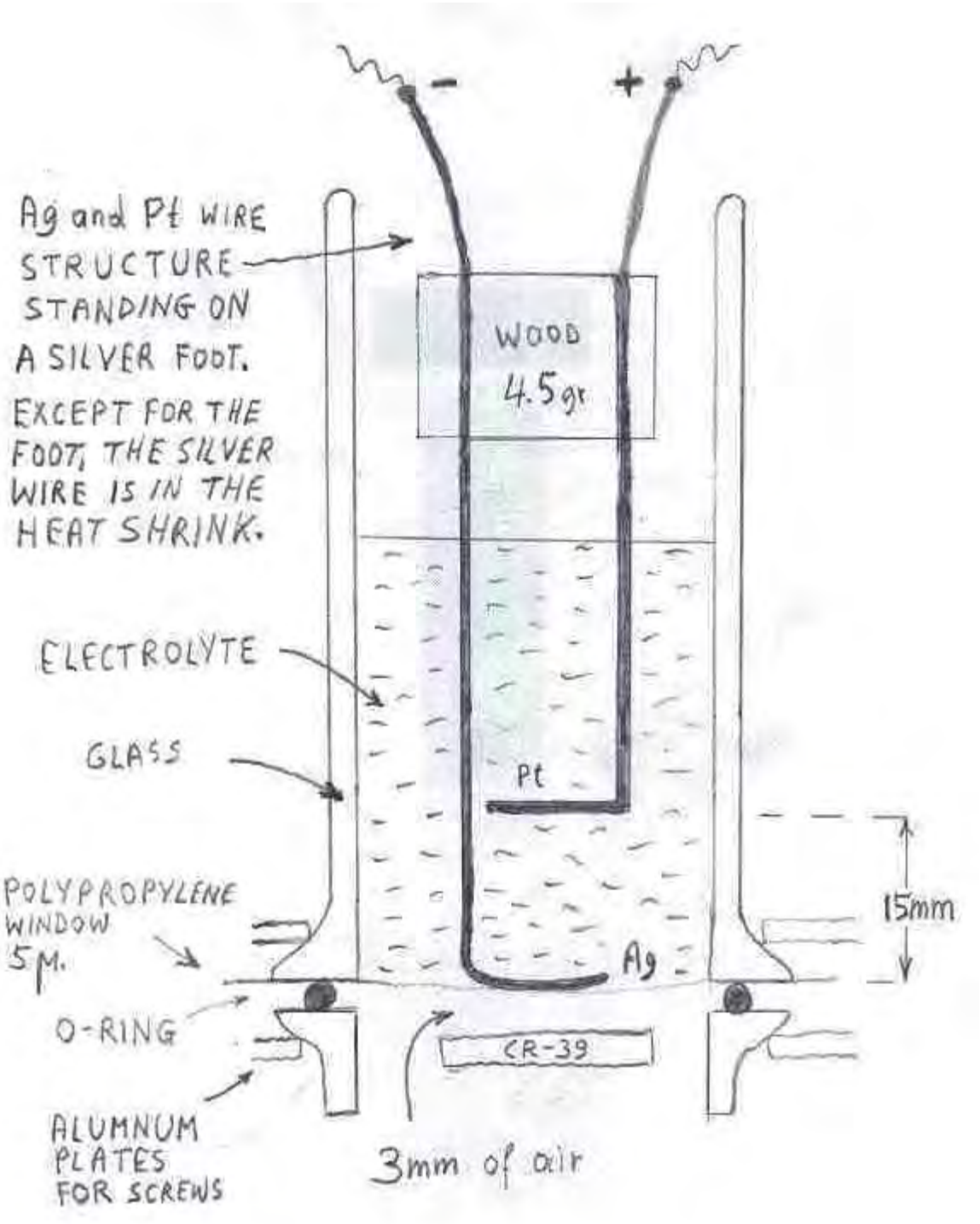

Figure 2
An Oriani-type cell with a thin polypropylene window. The rigid cathode-anode structure is supported by the thin layer of polypropylene.

- -

Contact between the polyethylene window and the silver-wire cathode (see Figure 2) was due to the weight of the cathode-anode structure. The total mass of that structure, in the first experiment, was close to 7 grams. In the second experiment the weight was close to 17 grams -- wood was replaced by a plastic cylinder and an iron nut.

4) Additional experimental details.
The electrolysis time schedule was as described in unit #319, except that the first day (I=0.5 mA) was skipped; the

third cell was inserted into the circuit one day after the first two cells. The CR-39 chip was located below the window. Another CR-39 chip was located next to the glass tube (see Figure 2). The two chips were etched and analyzed, as usual. Here is how the result was described in a message posted on our CR-39 list, on 3/1/07:

"We etched all chips in a fresh NaOH solution for 6 hrs at ~70 C and americium pits confirmed that etching was sufficiently long. But post-electrolysis chips did not show anything significantly larger than the background. Here are details:
a) The area of the chip below the thin window was about 0.95 cm^2. The surface facing up (looking at the cathode wire 3 mm away) recorded 17 track-looking pits.
b) The blank chip, exposed to air, far away from the cell, had the area of 1.6 cm^2. The number of pits one one side was found to be 17 while on the other side it was 10.
c) The area of the chip in contact with the glass tube (in air, looking toward the electrolyte above the anode, the bubbly region) was 1 cm^2. The number of pits on the side facing the tube was 27, the number on the opposite side was 14.

In my opinion the signal should be at least ten times stronger that the noise (number of pits on the blank chip) before it can be taken for granted. I know that three standard deviation is considered to be sufficient, under well defined conditions. But such conditions are not satisfied in our experiments. Oriani's signal/noise ratio was much larger than 10; our results are significantly different from his results. But Richard's experiment was not exactly like our's. Today we started another Oriani-type experiment; it will last about 10 days. The experiment will be supervised by Mike because I will be away. The geometry is similar to our first Phase 2 experiment but the cathode is made from nickel wire and the electrolyte is Li_2SO_4 in light water. "

The cell for our second Phase 2 experiment was essentially the same as in Figure 2 above. But the distance between the anode and a cathode was 24 mm. Here is how our preliminary result was described on 3/15/07 (in a message sent to Oriani, Brian and Simon).

"Oriani 2 experiment ended today. Working with Mike we examined three CR-39 chips that were positioned to detect particles during the electrolysis. Quick examination showed that there might be slightly more tracks on these chips than on the blank chip. But it will certainly not going to be the factor of ten difference (as reported by Oriani) or the factor of 7 (as in a recent SPAWAR experiment, according to Pam). Mike will analyze the chips and will report the actual number of pits. This will be the main part of his paper, to be presented at the Sigma-Xi student research conference. The other part will be comparison of background on CR-39 from three sources. And he will have an appendix explaining CR-39 detectors.

Before specifying details I want to mention an interesting observation. The chip that was below the cathode (originally about 2 mm from it, in air) had a naked-eye-visible circle when we removed it. Contrary to my expectation, the circle was not washed away after 4.5 hours of etching at ~78 C. Microscopic examination (after etching) showed that the circle is not made from track-looking pits; it was a grayish area matching the cathode wire "foot.". What can this circle be? Here is my guess. The polypropylene foil, supporting the weight of the electrolyte, and the weight of the cathode-anode structure, sagged and was actually in contact with the CR-detector. In other words, the cathode "foot" was separated from the CR-39 by only five microns of the plastic window. The circle (imprint of the cathode) was probably due to a thermal (or mechanical) effect. What else can it be?

a) Electrolyte: Li_2SO_4 in distilled H_2O (2.367 grams in 100 cc of water)
b) Distance from the circulat cathode (Ni) and pancake anode (Pt) was 24 mm.
c) Timing: 30 hrs with 10 mA, 72 hrs with 50 mA, 192 hrs with 127 mA and 30 hrs with ~270 mA (at the end, without a current stabilizer).
d) Three CR-39 chips were used. One bolow the polypropylene window (where the circular imprint was discovered), one in the air above the tube (exposed to escaping H_2 and O_2) and one applied to the external side of the cell (at the level below the anode). We also had two blank CR-39 chips and two chips exposed to Am-241. Pits due to americium were well developed."

5) A message from Oriani:

Here is a very interesting message that Oriani posted on the CR-39 list for TGP researchers: "I thought that it may be useful for me to submit for your consideration my results so far from experiments with film-protected CR39 during electrolysis in which the cathode is in close contact with the protecting film. The work so far has been designed only to see if one can get repeatedly more tracks than background level with this procedure. A systematic investigation to try to optimize the production of tracks is still to be done. . . .

I believe that one can conclude that the SPAWAR electrolyte composition in D_2O is not particularly advantageous. One sees also that the formerly used O-ring adds considerably to the tracks/cm^2 seen in an experiment. Additional experiments have shown that the O-ring retains a residual radioactivity after an electrolysis, and presumably so do other portions of the apparatus employed in an electrolysis experiment, as described by Fisher's polyneutron theory. All of the results so far share the characteristics that the track distributions are bi-modal, ie. groups and widely separated tracks, and that there is no discernible correspondence with the position of the cathode wires. There is also no correspondence between the tracks on one side of the detector and the tracks on the other side. These are independent events."

6) Future plans and ideas

After reading the above message, I decided to perform at least one more Oriani-type experiment. But not immediately; I want to learn more from the ongoing Phase 2 experiments of Pamela Mossier Boss et al. And I am not the only one who made such decision. In a private message received this morning Oriani promised to send me some "kindling" for another attempt to observe showers.

Will our result -- prominent pits on post-electrolysis chips are about 2.5 result larger than pits due to alpha particles, under identical etching conditions -- be confirmed by other researchers? So far I have seen only one confirmation (from TGP beta team #2) and not a single negation. This is not enough; hopefully other TGP researchers will soon ether confirm or negate our results. I also want to know if my interpretation of the above result is valid. This section will probably be expanded when new results, and new interpretations, become available. Please do not miss unit #321 which I am posting at the same time. It shows my view of scientific issues raised in The Galileo Project.

7) Polyneutrons

Oriani referred to a theory developed by John Fisher. My understanding of that theory has been described in units #191 and #227. But that was about two years ago. I know that John has been improving his theory constantly. Unfortunately I am not familiar with the curren version. Here is a recent message from John; it was posted on the CMNS list (3/13/07):

"Some researchers have regular success with starting nuclear reactions in their laboratories whereas others, notably those trying for the first time, can wait for weeks or months without success. This may not be the fault of the beginners. Polyneutron theory suggests that a free polyneutron is required to initiate a reaction (just as a free neutron is required to initiate a fission reaction), and that such polyneutrons are emitted by precursor particles in the air and water. The precursor particles are exceedingly rare and it can take considerable time for ignition of the first reaction in a laboratory. After that things get easier. A successful reaction produces large numbers of the required precursors, many of which stick to the experimental equipment or escape and condense on surfaces throughout the laboratory. On subsequent attempts they are available in the equipment, or are desorbed from the laboratory surroundings, enabling much prompter ignition. Hence it may be helpful to seed a start-up experiment with a bit of hardware or electrolyte taken from a successful one. This is equivalent to transporting an ember from a fire to a stack of kindling, rather than waiting for lightning to strike."

That is an interesting observation. Replying to the above I wrote: "Irreproducibility is taken by John as an experimental fact. Guided by that fact he found a theoretical explanation. That seems to be consistent with scientific methodology. But is the explanation valid? Oriani had eight consecutive successes in observing CINA (chemically induced nuclear activity). Our one recent attempt to reproduce it failed. That attempt was made at the same time (February and March 2007) at which we were able to successfully reproduce the SPAWAR experiment. Another attempt to reproduce Oriani's experiment is in progress. If it fails then I will ask Oriani to send us kindling pieces. I suggest that other researchers, using CR-39 and thin foils, do the same. Were we just lucky to replicate the SPAWAR

effect [without any kindling] or Fisher's theory does not apply to SPAWAR effect? . . . "

And here is a message I posted on the same list more recently (3/16.07). "In reading Steve Krivit's summary, just published in the #21 issue of NET, I see a reference to a recent PACA-type experiments (Protected Against Chemical Attack) at SPAWAR. It is an indication that large dominant pits, possibly due to corrosion (term first used by Oriani, if I recall correctly), mask presence of smaller and much less abundant pits due to nuclear projectiles. Oriani's eight consecutive experiments confirmed presence nuclear tracks on PACA chips. SPAWAR team also reports presence of tracks "more than seven times the background." Unfortunately, our (beta 5) results, and preliminary results reproted by beta 2 and beta 6 teams of TGP, seem to be very different in that respect. I hope that the situation will become more clear when other beta teams, and other investigators, share the results of their Phase 2 observations on this list. It would be interesting to know if tracks observed on PACA detectors are also formed only when magnetic and electric fields are present. That would be a highly convincing argument that tracks are not from radioactive contamination.

9) Other things.
This section is a place for an insertion that has nothing to do with science of TGP. I plan to write that insertion after it becomes clear whether or not the SPAWAR effect is an example of a chemically induced nuclear activity. Keep in mind that only one half of the Phase 1 teams reported the results so far. According to our leader, Steve Krivit, the Phase 2 was supposed to be less rigid than Phase 1. It was not to be a project in which each beta team was expected to follow a protocol developed by alpha teams. Each beta team researcher was expected to participate in Phase 2. Let us hope for more reproducible and convincing data from the second phase of The Galileo Project.

Also keep in mind that the SPAWAR claim is not based only on recent discoveries made with CR-39 detectors; it is also based on other observations, as described in numerous papers. References can be found in the 2007 paper of S. Szpak et al.:

<http://lenr-canr.org/acrobat/SzpakSfurtherevi.pdf>

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321) Scientific Issues in The Galileo Project

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Available results from The Galileo Project (TGP), as described in the March issue of this journal, confirmed the presence of copious SPAWAR pits. That is very important; it shows that the effect is reproducible. But is this discovery an example of nuclear activity (emission of nucleons from atomic nuclei) initiated by electrolysis? That question remains unanswered. About 30 researchers, from different laboratories, (two alpha teams, six beta teams and two advisers) continue to address it. I am one of these researchers. Let me outline six pending scientific issues that have emerged from the ongoing investigations..

1) Unconventional use of detectors
CR-39 detectors, used in TGP, are also used in other areas of science and technology. But we used them in the electrolyte, rather than in air. Is it possible that prominent pits, recorded in the electrolyte, are due to chemical corrosion and not to nuclear projectiles? That possibility was raised by the beta 2 team, after examination of their CR-39 detectors.

2) Relative sizes of prominent pits
Absolute sizes of CR-39 pits, due to nuclear particles, are known to depend on the time of etching and on the temperature of the etching solution. But relative sizes of pits, for example, in comparison with those due to alpha particles from an americium source, are likely to be much less dependent on etching parameter. According to two TGP teams (beta 5 and beta 2) diameters of prominent pits are about 2.5 times larger than diameters due to alpha particles (under identical etching conditions). Will this experimental fact be confirmed by other teams? That remains to be seen. My interpretation of this fact was offered at the recent APS meeting. It has also been described at

http://csam.montclair.edu/~kowalski/cf/319galileo.html

Will other scientists agree with me that large SPAWAR pits “cannot possibly be due to alpha particles or protons?” That also remains to be seen.

3) Emission of neutrons:
Two researchers, R. Oriani and L. Forsley, independently reported seeing pits on both sides of thick CR-39 chips. This led to a hypothesis, offered by Forsley, that at least some nuclear particles emitted in the SPAWAR effect are neutrons. Fortunately, two neutron detectors were used in one TGP experiment. Will the analysis of results confirm the hypothesis? That also remains to be seen.

4) PACA detectors:
Suspecting that dominant pits on post-electrolysis chips are due to some kind of corrosion, and expecting to detect less prominent pits due to nuclear projectiles, Richard Oriani started experimenting with PACA detectors. The acronym stands for Protected Against Chemical Attack. In his setup the CR-39 detector is separated from the electrolyte by a thin polypropylene film (5 microns). The wet side of the film is in contact with the cathode while the dry side is in contact with the CR-39 chip (located in air). He performed 8 consecutive experiments with that setup and reported positive results (at least ten times more pits on a PACA detector than on the control detector). Similar results were mentioned by Pamela Boss. Our beta 5 team was not able to replicate such results. This, however, does not necessary

conflict with positive results because experimental conditions were not identical. A confirmation of positive or negative results is likely to emerge when experimental conditions are normalized.

5) Electric and Magnetic fields:
According to the November issue of NET, tracks of nuclear particles are detected only when electrolytic cells are located in external fields, either electric or magnetic. The magnetic field effect was confirmed by our beta 5 team. But, according to the beta 2 team, magnetic field has no effect on the pit formation process. Results from other teams have not yet been announced. The issue remains unresolved.

6) Other nuclear signatures:
It is important to be aware that the SPAWAR claim, about a nuclear effect resulting from a chemical process, was not based only on CR-39 detectors in the electrolyte. It was also based on production of tritium (and other new elements), on generation of characteristic X rays, and on morphological changes on surfaces of cathodes. Hopefully these observations will also be subjected to scrutiny of independent investigations.

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What follows are some useful references, for those who did not follow the short history of The Galileo Project.

1) November 2006, the project is announce by Steve Krivit. See items 7 and 8 in the November issue of New Energy Times. <http://newenergytimes.com/2006/NET19.htm>

2) January 2007, publication of the SPAWAR paper in a refereed journal. Stanislaw Szpak · Pamela A Mossier Boss, and Frank E. Gordon; "Further evidence of nuclear reactions in the Pd/D lattice." Naturwissenschaften (2007) DOI 10.1007/g00114-007-0221-7. Can be downloaded from the library at <http://lenr-canr.org>

3) March 2007, Two Cold Fusion sessions at the meeting of American Physical Society, Denver, Colorado. See item 10 in the March issue of New Energy Times <http://newenergytimes.com/news/2007/NET21.htm>

3a) March 2007, Pamela A. Mossier-Boss, Stanislaw Szpak, Frank Gordon; "Production of High Energy Particles Using the Pd/D Co-deposition Process," Paper presented at the American Physical Society meeting, March 5, 2007, Denver Colorado.

3b) Lawrence P.G. Forsley, Garry Philips, J Khim, Pamela Mossier-Boss, Frank Gordon, Stanislaw Szpak; "Time Resolved, High Resolution Gamma Ray and Integrated Charged and Knock-on Particle Measurements of a Pd:D Co-deposition Cell," Paper presented at the American Physical Society meeting, March 5, 2007, Denver Colorado.

The above note was submitted to New Energy Times today. Unfortunately, the editor of this online journal, Steve Krivit, rejected the note without providing any explanation. SEE CORRECTION BELOW ! ! !

APPENDED ON 12/24/07
Let me clarify what I meant by the term "rejected." Steve Krivit actually suggested what the note should be improved. I decided not to do this because what is shown above seemed to be sufficiently clear. That is why I decided to post this summary note at my website. But the phrase "without any explanation" was wrong. This will be clear to anyone who reads his message (see below). I apologize for this misleading phrase. I also thank Steve Krivit for sending me copies of two messages shown below; they have been deleted from my mailbox long time ago.

Ludwik to Steve (4/6/07):
Hi Steve,
Many days ago you mentioned that the deadline to submit a piece for the next issue of TNT is April 20. With this in mind I am sending you this piece. I hope you will publish it. Your help in improving my style will be appreciated.

Steve to Ludwik (4/11/07):
Dear Dr. Kowalski,
Please attend to all of these required changes if you wish us to publish your letter.

1. You must explicitly refer to text or data that I wrote in my report regarding calibration curves, not what you think I believe.
2. If you wish to critique what I wrote in my March 16 article, you must do so only with what was known at the time, not what you have learned afterwards, because that would become an extension of the discussion, not a direct rebuttal.
3. If you wish to refer to data or results of experiments in your rebuttal you must include only that which has been presented in a legitimate science conference or journal publication.
4. If you want to discuss pit size, you must provide a scaled image, as well as an image of that area which shows the relative size of such pits that support your distinction of "dominant" pits.
5. In your presentation, you stated your conclusion, "analysis of our single experiment does not support the idea that dominant pits, on our CR-39 chips, are tracks of alpha particles, or less massive nuclear projectiles." In your letter of 6 April you stated it differently, "prominent large pits on post-electrolysis chips cannot possibly be due to alpha particle, to directly emitted protons, or to protons ìkickedî by neutrons." Please explain why, in your presentation, you did not state that the pits could be from more massive nuclear particles. Since your 6 April letter uses the absolute characterization, "cannot possibly be," please provide absolute proof that the tracks cannot be caused by protons kicked by neutrons.

Thank you,

Steve Krivit
Editor, New Energy Times

This website contains other cold fusion items.
Click to see the list of links

D2. 2007 Presentation

Our Galileo Project March 2007 report

Ludwik Kowalski
Montclair State University
Montclair, N.J. USA

http://csam.montclair.edu/~kowalski/cf/319galileo.html

"COLD FUSION" WAS A BAD NAME.

It implies one knows what is responsible for:

a) Excess heat,
b) Transmutation of elements,
c) Emission of nuclear projectiles, etc.

I prefer CINA; it stands for chemically induced nuclear activity.

Coauthors: [Names redacted].

Title of this preliminary report:

Nuclear or not nuclear ? That is the question.

http://csam.montclair.edu/~kowalski/cf/

Motivation for this study?

Summer 2006 SPAWAR report, as described by Steven Krivit at

http://newenergytimes.com/news/2006/NET19.htm

A protocol was provided and we used it.

We did what SPAWAR people did and observed the same things.

http://csam.montclair.edu/~kowalski/cf/

What are Cold Fusion researchers expected to do?

They are expected to offer a reproducible-on-demand demo of a nuclear process associated with a chemical process.

That is the name of the game.

Why is there a controversy?

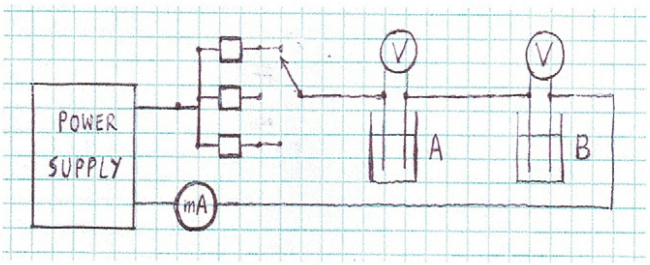
Strong nuclear forces, responsible for emission of nucleons, or groups of nucleons, are believed to be independent of what happens at the level of orbiting atomic electrons.

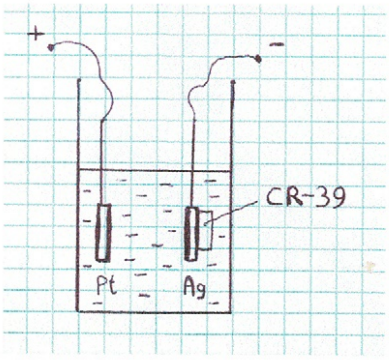
http://csam.montclair.edu/~kowalski/cf/

That was general background.

The "nuclear versus non nuclear controversy has been going on for 18 years. I am going to talk about our attempt to replicate one particular SPAWAR experiment, and share what I tentatively think about our result.

Electrolyte: $LiCl$ 0.3 M + $PdCl_2$ 0.03 M





WHAT IS CR-39 ?

A plastic material (polycarbonate) from which eyeglasses are made.

It is widely used to detect charged nuclear particles, such as protons and alpha particles. Also to detect neutrons.

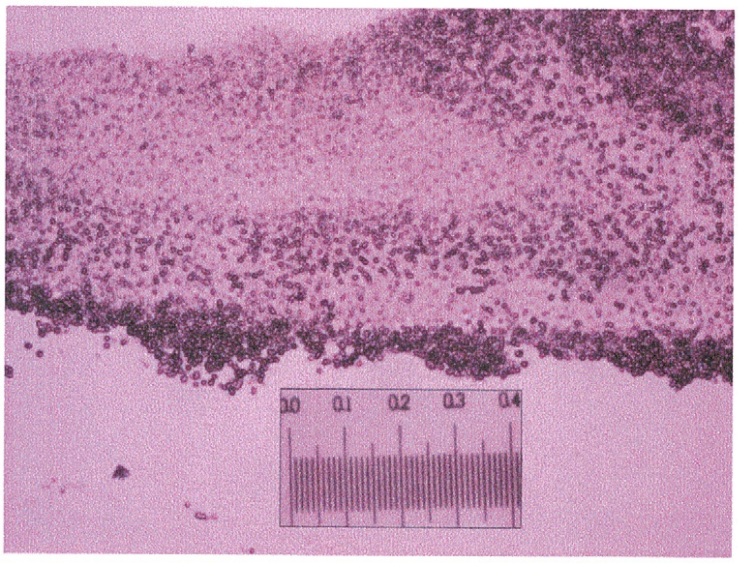
Tracks become microscopically visible after etching in hot NaOH for several hours.

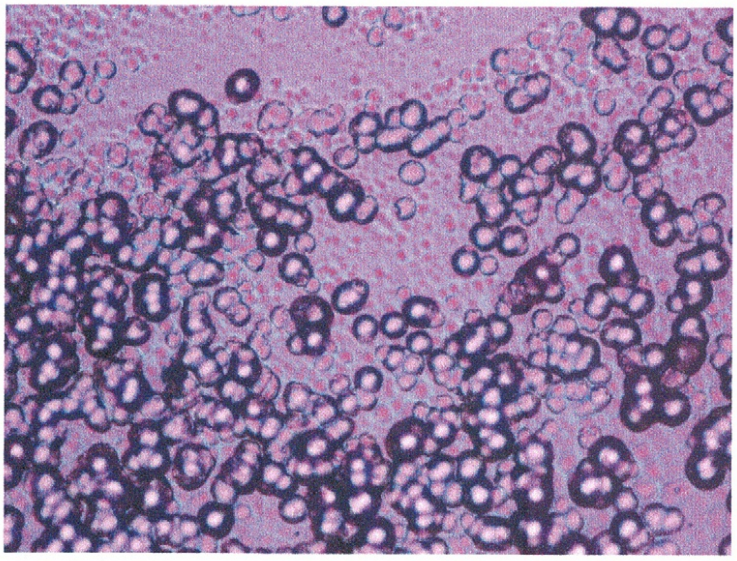
Duration of electrolysis

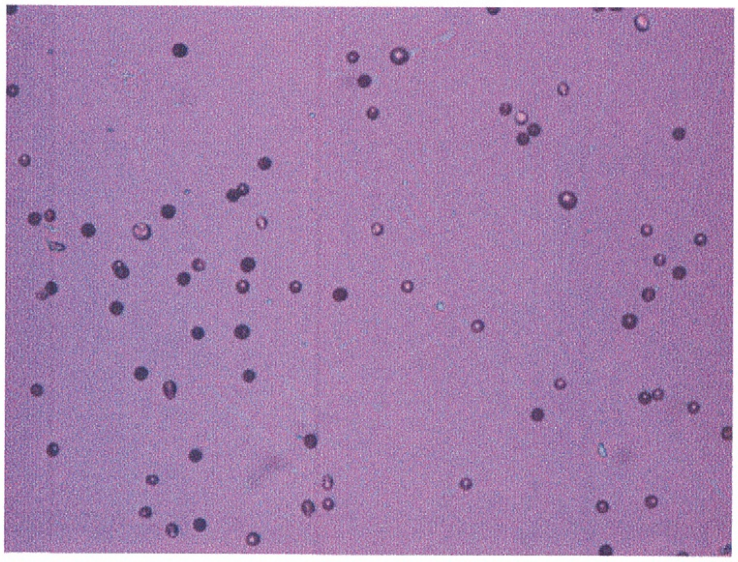
- Current 0.1 mA, 1 day
- Current 0.2 mA, 1 day
- Current 0.6 mA, 12 days, till the electrolyte became transparent

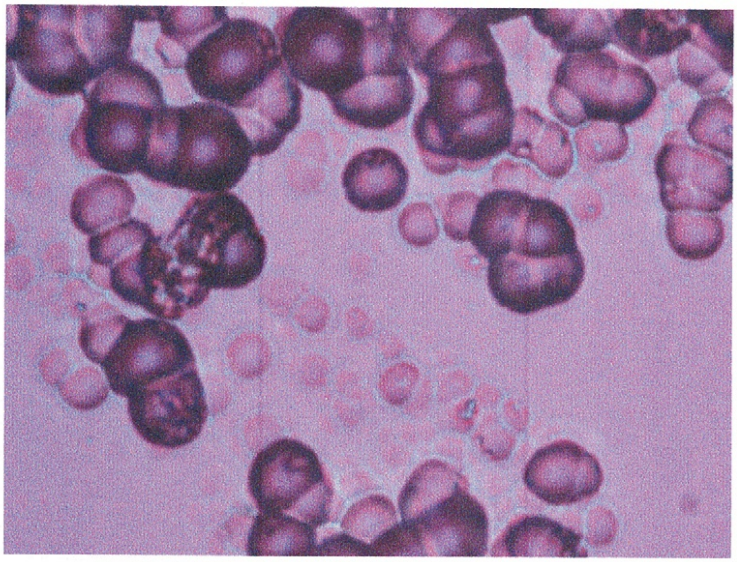
- Current 1.0 mA, 1 day
- Current 5.0 mA, 1 day
- Current 10 mA, 1 day
- Current 27 mA, 1 day
- Current 55 mA, 1 day
- Current 127 mA, 1 day

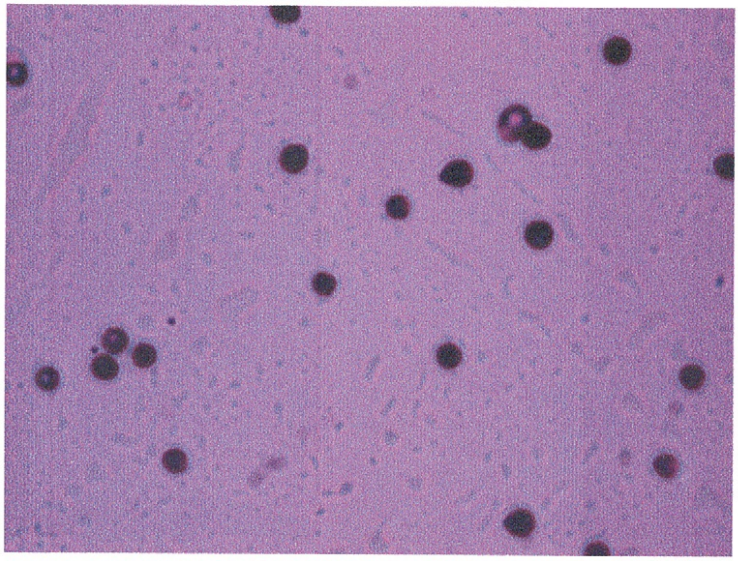
A digital camera picture











A.S. Roussetski et al. Proceedings of the 10th International Conference on Cold Fusion, pages 559 - 566

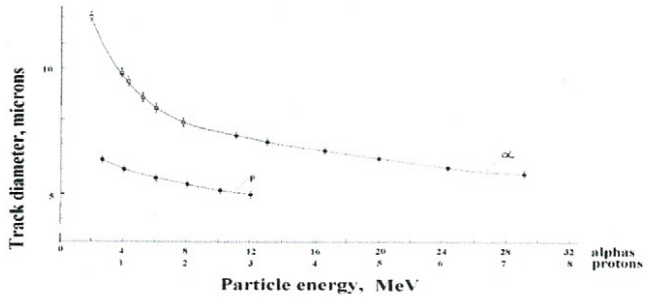
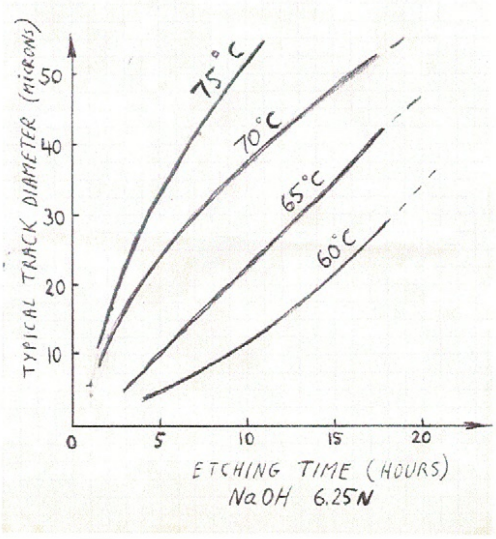
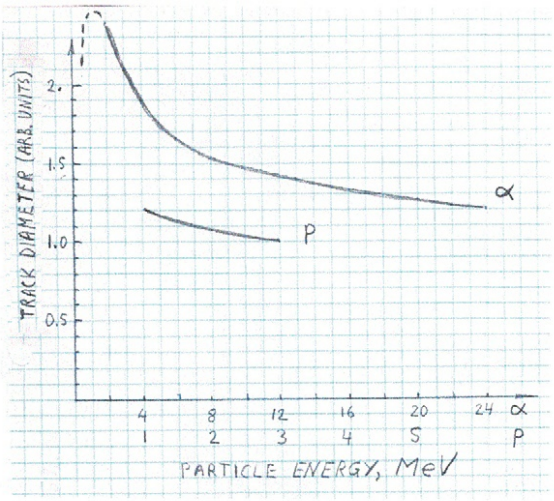


Fig. 2. The calibration dependence of track diameters of alpha-particles and protons on their energy. □ - radioactive alpha-sources, • - particle beams.





Large pits we observed cannot be attributed to alpha particles or protons, or neutrons.

Why not? Because of their diameters:

1.7*2.5 = 4.25 prots

Fission fragments would produce such pits. But I am not claiming that our pits are due to massive nuclear projectiles.

The magnetic field effect

The pits (large and small) were created only in the presence of a magnetic field (from a pair of neodymium magnets). How can this be explained?

We do not know how to answer this puzzling question.

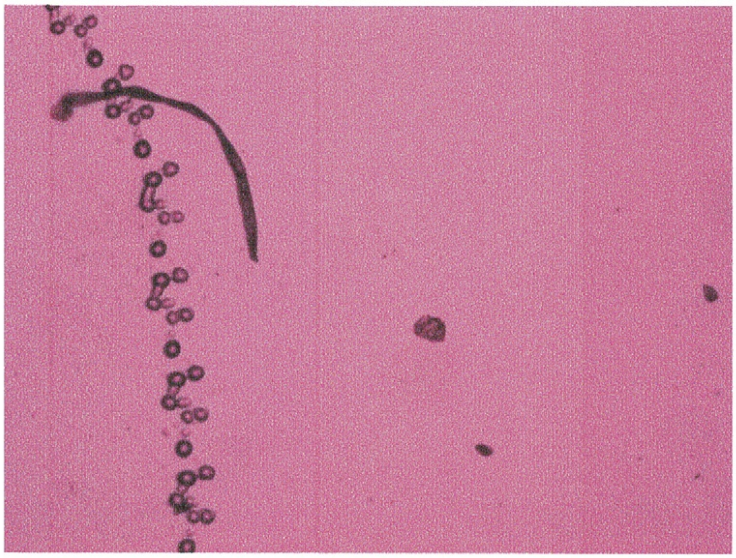
Please be aware that

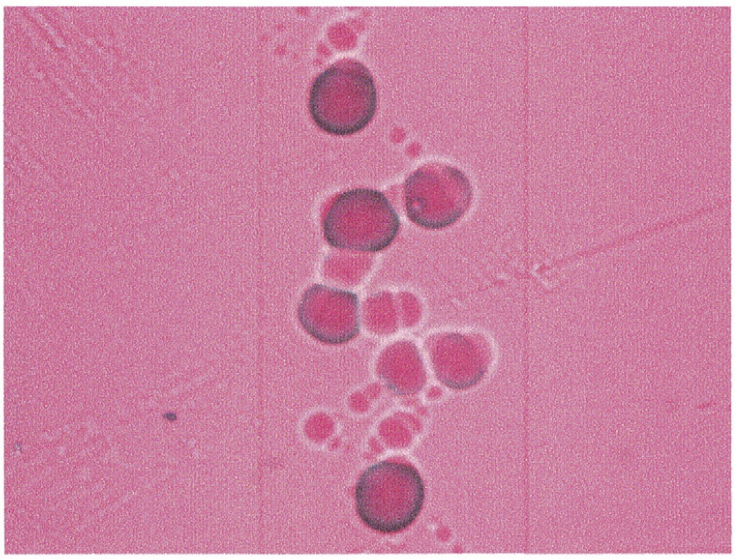
I am reporting on the basis of an experiment that was performed only one time. The SPAWAR researchers, on the other hand, performed numerous experiments, used several different instruments and published several articles in refereed journals in the last decade. At least two of them are recognized authorities in the field of electrochemistry.

We are amateurs in comparison. But we feel that the method of analysis of our CR-39 results is valuable. It might help to deal the basic issue of "nuclear versus not nuclear" origin of CR-39 pits.

Final comments

- It is possible to wrap CR-39 detectors into foils that are thick enough to prevent chemical corrosion but thin enough to transmit alpha particles and protons.
- Such experiments are now in progress, in several laboratories. Results will probably be known in a month or two. Then one will be in a better position to decide whether or not a nuclear process is responsible for large pits on CR-39.
- For the time being the issue should be considered unresolved. It is conceivable that large pits will disappear while small pits will remain. That would be consistent with nuclear origin of some pits.





Appendix E. LCF Entries Related to 2017 Polish Journal Paper

E1. Philosophical and Social Aspects of the Cold Fusion Controversy (LCF #413)

Philosophical and Social Aspects of the Cold Fusion Controversy

Presented at the

IVe Congrès de la Société de Philosophie des Sciences

June 1, 2012, Montreal, Canada

Ludwik Kowalski (see Wikipedia)
Professor Emeritus
Montclair State University, NJ, USA

Abstract

The area of research known as Cold Fusion (CF) is the arena of a science-and-society feud. The conflict is unprecedented in terms of duration, intensity, the caliber of combatants, and the deviation from basic principles of scientific methodology. It is also unprecedented in terms of two formal governmental investigations of the field. The US Department of Energy (DOE) has evaluated the CF claim--a chemical process triggering a nuclear process. The claim was rejected on "theoretical grounds," not by performing better experiments. The self-correcting nature of scientific progress, in both social and natural sciences, depends heavily on peer review. Depriving PhD-level scientists of that process, as practiced by editors of some scientific journals, violates the "New American Physical Society's Ethics Guidelines," described in the January 2003 issue of Physics Today. Such distortions of scientific methodology are worth studying.

1. Introduction

Scientific methodology (1,2) refers to the set of norms developed to deal with mistakes and controversies in scientific research. Most mistakes are recognized when new results are discussed with colleagues, or via the peer review process. Occasional errors in published papers are subsequently discovered during replications conducted by other researchers. Scientific results, if valid, wrote Huizenga, must be reproducible on demand. “When errors are discovered, acknowledged and corrected, the scientific process moves quickly back on track, usually without either notice or comment in the public press (3).” The process, in other words, is expected to be self-corrective.

The so-called "scientific method" is not a list of divine commandments. It is a set of norms described by scientists, and by those who observe their ways of working. The author of this article is a retired nuclear physicist who has observed cold fusion researchers over the last ten years, and participated in several CF conferences and three cooperative replication projects (in which the claimed results were not confirmed). The CF episode is an unusual controversy resulting from a sensational 1989 announcement made by Fleischmann and Pons (F&P). The event (4,5,6,7,8) divided physical scientists into two feuding camps (9,10,11,12,13,14). This is a rare example of a situation in which the expected self-correction of the scientific process was essentially stopped by two formal governmental interventions.

2. Role of Accepted Scientific Theories.

Why is the CF controversy unresolved? Because CF experimental claims are not reproducible on demand, and because they conflict with the generally accepted theory of nuclear reactions. A theory, in this context, is a logical/mathematical structure that agrees with a wide range of already verified experimental data. Scientists know the rule--theories guide but experiments decide. But they are very reluctant to abandon accepted theories. To be reluctant means to insist on additional verification of new experimental results.

Referring to such situations, Huizenga wrote (3): “There are occasionally surprises in science and one must be

prepared for them." Theories are not carved in stone; scientists do not hesitate to modify or reject them when necessary. Rejecting a claim because it conflicts with a theory is not as convincing as rejecting it on the basis of reliable empirical data. In that sense methods of validation of claims in physical and social sciences are similar. Scientific theories are models of objective reality; they are often changed, or modified, when new facts are discovered.

3. Levels of Confidence in Scientific Claims: Data and Explanations.

A discovered experimental fact is usually presented to the scientific community, to be independently confirmed or refuted. Experimental results are accepted--at a high level of confidence--when they become reproducible on demand. Absence of such reproducibility justifies suspicion of possible errors or fraud. Methods of validation of theories (explanations of facts) are slightly different. A new scientific theory is also presented to a community of experts, to be independently evaluated. Their level of confidence in a theory depends on the validity of underlying assumptions and on the rigor of quantitative analysis. But even a most reliable scientific theory, called a law, is said to be falsifiable, in principle, when conflict with reproducible-on-demand data becomes undeniable (15). Such unusual conflict could trigger a scientific revolution (16).

To explain something usually means to identify causes and to construct a logically satisfying model of reality. An attempt to explain a fact, or to resolve an apparent logical conflict, usually leads to discoveries of other facts. A classical example was the discovery of planet Neptune, in 1846. A more recent and less widely known example was the discovery of a subatomic particle named neutrino. Experimental data collected in the 1920's showed that beta rays (electrons emitted in radioactive decay) had lower mean energies than expected on the basis of the theoretical $E=mc^2$ formula. Austrian theoretical physicist W. Pauli solved this "logical inconsistency" by suggesting that tiny neutral particles, later named neutrinos, were responsible for the missing energy. His hypothesis was formulated in 1933. Experiments confirming the reality of neutrinos were performed, 23 years later.

4. Unfortunate Terminology

The essence of the discovery announced by F&P was "excess heat." Their small electrolytic cell generated more thermal energy than the electric energy supplied to it. Trying to establish priority, under pressure from University of Utah administration, the scientists announced their results at a sensational press conference (March 23, 1989). They wanted to study the CF phenomenon for another year or so but were forced to prematurely announce the discovery.

The unfortunate term "cold fusion" was imposed on them (17). Why unfortunate? Because it created the unjustified impression that cold fusion is similar to the well known hot fusion, except that it takes place at much lower temperatures. This conflicted with what had already been known--the probability of nuclear fusion of two heavy hydrogen ions is negligible, except at stellar temperatures. It was a mistake to interpret experimental data before the results were recognized as independently reproducible. In fact, F&P had no evidence for the emission of energetic ^1H, ^3H and ^3He products, listed in their first published paper (4). The only thing they knew was that the measured excess heat could not be attributed to a known chemical reaction. Claiming that the measured excess heat was due to a nuclear process was premature. The adjective "non-chemical" does not automatically translate into "nuclear."

Suppose the discovery had not been named cold fusion; suppose it had been named "anomalous electrolysis." Such a report would not have led to a sensational press conference; it would have been made in the form of an ordinary peer review publication. Only electrochemists would have been aware of the claim; they would have tried to either confirm or refute it. The issue of "how to explain the heat" would have been addressed later, if the reported phenomenon were recognized as reproducible-on-demand. But that is not what happened. Instead of focusing on experimental data (in the area in which F&P were recognized authorities) most critics focused on the disagreements with the suggested theory. Interpretational mistakes were quickly recognized and this contributed to the skepticism toward the experimental data. Using unconfirmed data to justify the nuclear origin of excess heat, by F&P, was inconsistent with the prevailing norms of scientific methodology. A more recent case of violation of scientific norms, by a CF researcher, Andrea Rossi, is described in (14).

5. Two US Government Investigations of CF.

The significance of CF, if real, was immediately recognized. Some believed that ongoing research on high-temperature fusion, costing billions of dollars, should be stopped to promote research on CF. Others concluded, also prematurely, that such a move would be opposed by “vested interests” of mainstream scientists. Responding to such considerations, the US government quickly ordered a formal investigation.

A panel of scientists, named ERAB (Energy Research Advisory Board), and headed by John Huizenga, was formed to investigate CF in 1989. The final report (18), submitted to the DOE several months later, negatively interfered with the normal development of the field. Modest financial support for additional CF research, by the DOE, NSF, and other agencies, was practically stopped after the report was published.

It is interesting that only one of the ERAB's six conclusions referred to CF experiments; the remaining five conclusions were about anticipated practical uses of CF, and about various aspects of the suggested interpretation of results. Instead of focusing on reality of excess heat, critics focused on the fact that the hypothesis was not consistent with what was known about hot nuclear fusion. The same observation can be made about the six ERAB recommendations. Only one of them referred to possible experimental mistakes. It is clear that the ERAB observations were based mostly on ”theoretical grounds,” not on independently performed experiments. The unfortunate governmental intervention had one serious and unprecedented consequence--editors of some scientific journals started rejecting manuscripts written by CF scientists, bypassing peer review (19).

The second DOE investigation (20) of CF was announced in March 2004, nearly 15 years after the first. A group of 18 experts was selected to review new CF claims, such as linear correlation between excess heat and generation of helium. Helium was reported to be produced at the rate of one atom for approximately 24 MeV of released thermal energy. This was consistent with the theoretical $E=mc^2$ formula for the suggested radiation-free D+D->He fusion. But the DOE experts were not asked to perform correlation experiments; they were asked to read the report submitted by five CF scientists (21), and to vote on whether or not the evidence for the claim was conclusive. Such a way of dealing with a controversy was not consistent with the scientific method.

Ideologically and politically motivated rejections of scientific claims are not new. Giordano Bruno and Galileo Galilei are well known examples. Lysenkovism--Stalin's discrimination of geneticists--is a more recent illustration. And cybernetics, in the Soviet book entitled “Short Philosophical Dictionary,” was defined as “bourgeois pseudo-science serving American imperialism.” Ludwik Kowalski accepted this kind of "truth" as a communist student in Poland (22). What can be done to make sure that similar discrimination will not be used in the US, to impose "the truth" about evolution, stem cell research, etc.?

6) Conclusion: More Questions Than Answers

Long-lasting controversies about scientific discoveries are not new. Alfred Wegener's theory of continental drift is a good illustration. Mainstream geologists rejected experimental data supporting his now-accepted theory for half a century. The CF controversy, however, seems to be different both in terms of governmental involvement and in the caliber of adversaries on both sides of the divide. Huizenga and Fleischmann were indisputable leaders in nuclear science and electrochemistry. Most leading CF researchers are PhD-level scientists. The same is true for many scientists who reject CF claims.

The long-lasting CF episode is a social situation in which the self-correcting process of scientific development was not allowed to evolve. To what extent was this due to extreme difficulties in making progress in the new area, rather than to negative effects of competition, greed, jealousy, and other "human nature" factors? Such unanswered questions (23) are worth addressing in the context of debates about science and society.

One thing is undeniable; the world is still waiting for the first reproducible-on-demand demonstration of a nuclear effect correlated with a chemical process. No progress is possible when reported experimental data cannot be reliably replicated in other laboratories. Considering potential CF benefits, and relatively low costs of research in this area, the DOE should have helped to resolve the controversy, one way or another, in a well-equipped national laboratory, during the second investigation. But it failed to do so. How can such a policy be explained? Why is CF research

allowed to stagnate without financial support? These questions also belong to debates about science and society. Will the past 23 years be recognized as the painful beginning of a new paradigm, similar to those described in (16), or will this period be known as a pseudoscientific episode, similar to those described in (24)? How can the persistence of the CF controversy be explained? Were the US government interventions necessary or not?

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E2. Social Aspects of Cold Fusion: 23 Years Later (LCF #409)

LETTERS TO PROGRESS IN PHYSICS

Social Aspects of Cold Fusion: 23 Years Later

Ludwik Kowalski

The field of Cold Fusion, now called Condensed Matter Nuclear Science (CMNS), remains controversial. The original 1989 claim made by M. Fleischmann and S. Pons was that a chemical process in an electrolytic cell could initiate a nuclear reaction–fusion of two deuterium nuclei. More recent CMNS claims, made by experimental scientists, are: emission of charged nuclear projectiles during electrolysis; accumulation of ^{4}He; production of radioactive isotopes; and transmutation of elements. In the US, CMNS claims have been evaluated in two Department of Energy (DOE) investigations, in 1989 and 2004, as summarized in this article. These investigations did not lead to any resolution of the controversy. Scientists and adminstrators are not ideal; competition among them, as among other groups of people, tends to have both positive and negative influences.

1 Introduction

The so-called "scientific methodology", a set of norms developed to deal with difficulties, especially with mistakes and controversies, is well known. Most scientific mistakes are recognized when new results are discussed with colleagues, or via the peer review process. Occasional errors in published papers are subsequently discovered during replications conducted by other researchers. Scientific results, if valid, wrote Huizenga [1], must be reproducible on demand. "When errors are discovered, acknowledged and corrected, the scientific process moves quickly back on track, usually without either notice or comment in the public press." The scientific process, in other words, is self-corrective. The purpose of this presentation is to analyze an ongoing controversy about the so-called "cold fusion" (CF). The author of this article, and three other researchers, tried to verify one recent CF claim – emission of alpha particles during electrolysis. The results were negative, as described in [2]. Critical analysis of some CF claims, as illustrated in [3], can enrich nuclear physics courses, even at the undergraduate level.

Why is the CMNS controversy started in 1989 unresolved? Because CF claims are still not reproducible on demand, and because they conflict with accepted theories. A theory, in this context, is not just a hypothesis, or only a logical/mathematical argument. It is a logical structure that is known to agree with a wide range of already verified experimental data. Researchers know the rule–theories guide but experiments decide. But they are very reluctant to abandon accepted theories. To be reluctant means to insist on additional verifications of new experimental results. Referring to such situations, Huizenga wrote: "There are occasionally surprises in science and one must be prepared for them." Theories are not carved in stone; scientists do not hesitate to modify or reject theories when necessary. Rejecting a highly reproducible experimental result "on theoretical grounds" would not be consistent with scientific methodology. Unlike mathematics, science is based, in the final analysis, on experimental data, not on logical proofs.

2 The Original Claim

It is well known that two hydrogen nuclei can fuse, releasing energy. But this happens only at extremely high temperatures. At ordinary temperatures the probability of the reaction is practically zero, due to the well known coulomb repulsion of positive nuclei. This has been confirmed by reliable experimental data. But two scientists – Steven Jones, a physicist, and Martin Fleischmann, a chemist – independently speculated that this might not always be true. The term CF was introduced by them to identify the claimed fusion of hydrogen nuclei (ionized atoms dissolved in solid metals). The DOE supported Jones' work long before Fleischmann and his colleague Pons (F&P) applied for similar support. That is why the DOE asked Jones to evaluate the new research proposal. He was later accused (by the administration of Utah University) of stealing the idea of CF from F&P. Trying to establish priority, Utah University organized a press conference (March 23, 1989) at which the discovery of generation of nuclear heat in an electrolytic cell was announced to the world. The released heat was declared to be due to fusion of deuterium nuclei – ionized atoms dissolved in palladium. At that time Jones and his co-workers had already authored numerous peer-reviewed articles [4]. But their claim was not excess heat; it was emission of neutrons.

3 The First DOE Investigation

Most scientists immediately rejected claims conflicting with well-known facts and theories. But many attempts to replicate F&P's poorly-described experiments were made. Some attempts were successful (unaccounted heat was generated at rates close to one watt), while others were not [5]. That was the beginning of the controversy. Fleischmann and Pons wanted to study the CF phenomenon for another year or so but

were forced to announce the discovery by the university administrators [6]. They had no evidence that the measured heat was due to a nuclear reaction. The only thing they knew was that it could not be attributed to a known chemical reaction.

Suppose their experimental results had been described without any interpretation, and the phenomenon had been named "anomalous electrolysis". Such a report would not have led to a sensational press conference; it would have been made in the form of an ordinary peer review publication. Only electrochemists would have been aware of the claim; they would have tried to either confirm or refute it. The issue of "how to explain excess heat" would have been addressed later, if the reported phenomenon were confirmed. But that is not what happened. Instead of focusing on experimental data (in the area in which F&P were recognized authorities) most critics focused on the disagreements with the coulomb barrier theory. Interpretational mistakes were quickly recognized and this contributed to the premature skepticism toward their experimental data.

But the significance of CF, if real, was immediately recognized. Some believed that ongoing research on high-temperature fusion, costing billions of dollars, should be stopped to promote research on CF. Others concluded, also prematurely, that such a move would be opposed by "vested interests" of mainstream scientists. Responding to such considerations, the US government quickly ordered a formal investigation. A panel of scientists, named ERAB (Energy Research Advisory Board), and headed by John Huizenga, was formed to investigate CF in 1989. The final report, submitted to the DOE several months later, interfered with the normal development of the field. It should be noted that ERAB scientists investigating the CF claims were not personally involved in replications of experiments. Their report [7], based on visits to several laboratories rather than participation in experiments, can be summarized by the following statements:
Conclusions:

1. There is no evidence that a nuclear process is responsible for excess heat.
2. Lack of experimental reproducibility remains a serious concern.
3. Theoretically predicted fusion products were not found in expected quantities.
4. There is no evidence that CF can be used to produce useful energy.
5. The CF interpretation is not consistent with what is known about hydrogen in metals.
6. The CF interpretation is not consistent with what is known about nuclear phenomena.

Recommendations:

7. We recommend against any extraordinary funding.
8. We recommend modest support for more experiments.
9. We recommend focusing on excess heat and possible errors.
10. We recommend focusing on correlations between fusion products and excess heat.
11. We recommend focusing on the theoretically predicted tritium in electrolytic cells.
12. We recommend focusing on theoretically predicted neutrons.

Note that only one conclusion (item 2) refers to CF experiments. Conclusion 4 is about anticipated practical uses of CF while the remaining four conclusions (1, 3, 5, and 6) are about various aspects of the suggested interpretation of experimental results. Instead of focusing on reality of excess heat critics focused on the fact that the hypothesis was not consistent with what was known about hot nuclear fusion. The same observation can be made about recommendations. Only one of them (item 9) refers to possible errors in experiments. Items 7 and 8 refer to future funding while items 10, 11, and 12 refer to what was expected on the basis of the suggested hot-fusion interpretation. It is clear that the ERAB observations were based mostly on "theoretical grounds,"and not on identified errors in experimental data. Recommendations about future financial support for CF were very important. But they were ignored by the DOE. Support for CF research practically stopped in 1989. Another result of the first DOE investigation was that editors of some scientific journals stopped accepting articles dealing with CF research. Why was the scientific methodology of validation of claims – theories guide but experiments decide – not followed by the DOE-appointed scientists? Why did "rejections on theoretical grounds" prevail?

4 The Second DOE Investigation

The second DOE investigation of CF was announced in March 2004, nearly 15 years after the first one. Links to three online documents related to that investigation – Conference Agenda, Meeting Agenda, and DOE CF Report – can be found in [8]. The six most important scientific questions, based on new experimental claims, were:

a) Is it true that unexpected protons, tritons, and alpha particles are emitted [9, 10] in some CF experiments?
b) Is it true that generation of heat, in some CF experiments, is linearly correlated with the accumulation of ^{4}He and that the rate of generation of excess heat is close to the expected 24 MeV per atom of ^{4}He [9, 11]?
c) Is it true that highly unusual isotopic ratios [9, 12] have been observed among the reaction products?
d) Is it true that radioactive isotopes [9, 13] have been found among reaction products?
e) Is it true that transmutation of elements [10, 14] has occurred?

f) Are the ways of validating of claims made by CF researchers (see conference reports presened at [16, 17, 18]) consistent with accepted methodologies in other areas of science?

A positive answer to even one of these questions would be sufficient to justify an official declaration that cold fusion, in light of recent data, should be treated as a legitimate area of research. But only the (b) question was addressed by the selected referees [8]. They were asked to review the available evidence of correlation between the reported excess heat and production of fusion products. One third of them stated that the evidence for such correlation was conclusive. That was not sufficient; the attitude of the scientific establishment toward cold fusion research did not change.

5 Conclusion

The CF controversy is unprecedented in terms of its duration, intensity, and caliber of adversaries on both sides of the divide. Huizenga and Fleischmann were indisputable leaders in nuclear science and electrochemistry. CMNS researchers are mostly also Ph.D. level scientists. The same is true for those scientists who believe that the announced discovery of CF was a "scientific fiasco". We are still waiting for at least one reproducible-on-demand demonstration of a nuclear effect resulting from a chemical (atomic) process. In the case of CF the self-correcting process of scientific development emphasized by Huizenga has not worked. This fiasco seems to be due to the fact that scientists appointed to investigate CF claims did not follow the rules of scientific methodology.

Submitted on January 30, 2012 / Accepted on February 5, 2012

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