Krivit's LENR Interview for Intelligence Advanced Research Projects Activity

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by Steven B. Krivit
Senior Editor, *New Energy Times*

On Nov. 21, I received a phone call from Avra Michelson, an analyst with MITRE Corp. Michelson explained that MITRE Corp. is a federally funded research and development center that is sanctioned by Congress to work in the public interest exclusively with government. It helps government with some of its hardest systems engineering problems and with its work with the private sector.

Michelson told me that she was doing background research on LENR on behalf of the Intelligence Advanced Research Projects Activity. She invited me to be interviewed as a subject-matter expert on LENR.

"We are asking questions about the field to eminent scientists and experts like yourself who have been active participants in the field," Michelson said. "IARPA's goal is to fund and help accelerate high-risk kinds of research for the intelligence community."

The telephone interview took place on Dec. 1. Michelson's colleague Chrissy Vu also participated in the call.

In a follow-up e-mail, I asked Michelson whether I could expect to see a final report of her project.

"The case studies we are developing on science and technology emergence over the last 30 years are intended for government use. We are not publishing a report," Michelson wrote.

I recommended to Michelson that she speak with Lewis Larsen because he is one of the, if not the, most-consulted experts by the federal government in the LENR field. Larsen, along with co-authors Allan Widom and Yogendra Srivastava, has developed the most promising theory that may explain LENRs.

I also recommended that she speak with Frank Gordon, retired from Space and Naval Warfare Systems Center in San Diego, because of the leadership role he played in one of the most impressive sets of experimental work in the LENR field during the last two decades.

I was surprised to learn that Michelson was not interested in speaking with either expert.

"We are happy to add [their names] to our list. Currently, we are not soliciting any more SMEs in this field, but we are glad to have [these] reference[s] should our situation change," Michelson wrote.

Coincidentally, MITRE Corp. funds the research activities of JASON, an ad-hoc group of elite scientists which advises the government on unusual science and technology topics. Coincidentally, I met with a former chairman of JASON, Richard Garwin, at his apartment in Scarsdale on Nov. 30, 2010, and I provided him with an update to the research in the field.

The following text includes the advance questions Michelson sent me, a few questions Michelson and Vu added during the call, my responses, and a postscript I added.

Preamble (Michelson)

Pre-Read for LENR Subject Matter Experts (SMEs)

Establishing Baseline Judgments for Phase 1 FUSE Test & Evaluation

Thank you for agreeing to meet with us. We appreciate your support. The IARPA FUSE research program is designed to fundamentally advance our understanding of how scientific emergence occurs, how it can be detected, and how it can be measured. The program seeks to develop technology that can characterize, nominate, and provide evidence for emerging scientific capabilities. The goal is to develop technology that complements human analysis and that can be validated by comparing system results to that of a subject-matter expert-judgment baseline.

Toward this end, we would like to elicit your answers, as an eminent editor covering LENR, to questions on emergence that will be posed to the systems developed by performers under contract to IARPA. What we learn will be instrumental in helping us evaluate these research systems – and in this way contribute substantially to our ability to identify and track S&T developments around the world.

The interview will cover: 1) background contextual questions (2) your answers to questions on scientific/technology emergence, and (3) any feedback you might have on the approach.

The questions you will be asked are the same questions we will ask all SMEs who we are interviewing. We want to emphasize that there are no right or wrong answers.

We realize that the questions we are asking may feel somewhat artificial or narrow. But we hope that you will bear with us, as the questions are crafted for a specific purpose – to compare responses from the automated systems being developed with IARPA funding to those of baseline judgments provided by SMEs like yourself.

Part 1: BACKGROUND

Q1 - Michelson: What is your scientific discipline?

A1 - Krivit: Science investigation and analysis.

Q2 - Michelson: What is your background? [question added during interview]

A2 - Krivit: I have spent the last 11 years investigating, analyzing and writing about the subject of low-energy nuclear reactions. My applicable background comes from the experience that I have accumulated from speaking with the experts - scientists and researchers - throughout this time. It's been school through experience. Before that, I had a career in information technology as a network specialist. I have a bachelor's degree in business administration, and I studied industrial design for two years before that.

The industrial design program has enabled me to bring some skills from long ago that are applicable in this field. For example, I learned some basic physics from that course of study as well as materials and processes, which is intrinsic to this field.

Q3 - Michelson: How and when did you become involved with the science of LENR?

A3 - Krivit: It's a long story, but I'm going to start out by giving you a really short answer, and let's go from there to see how much more you want.

I started, I believe, in February 2000, when I learned that there was a new documentary video about the subject. I was fascinated to learn that the subject was still alive and kicking, and I contacted the producer of the video. His name was Gene Mallove. I wanted to know more, and he was very gracious to tell me all about it. I asked if I could come visit his laboratory in New Hampshire, and that's how and when it all started for me.

Q3.1 - Michelson: And then it became a life focus? [question added during interview]

A3.1 - Krivit: Yes. So the slightly more expanded story is that I continued from there. It was a curiosity, I would say, until August 2003. I was a computer geek at the time, so naturally I went onto the Web to try to learn more, and the most

startling thing that I found was that there were two sets of 180-degree opinions about the subject. It was absolutely polarized. Some people said the entire subject was wrong; some people said the entire subject was right. I had the sense that that couldn't be like that, that there had to be some middle ground. So I started very slowly looking into it, and, at the time, all I knew about journalism was what I remembered in my 10th-grade journalism class, and that is — yes, seriously a skill from high school actually did apply to real life — I remembered that, if you really want to get the facts you have to get firsthand, first-person sources. You cannot even go to books. You have to go to where the books went to. I actually avoided reading books initially. I avoided reading other people's written work. I made it a practice to go speak to the principals and the original sources who were active in the field and who had been active in the field. Each time I learned something new, I put it up on the *[New Energy Times]* Web site, and that was the beginning, the very rough early beginning of the *New Energy Times* online magazine.

I didn't come to a conclusion about any significant reality of the field until August 2003. That's when I went to the international conference in Cambridge, [Massachusetts], and I met more than 100 people who were active in the field from, I think it was, about 12 different countries. I realized that there was no way that all of this research, all of these people could be deluding themselves, could be involved in a coordinated plan to deceive. The other thing was that there was consistency in the type of data that was being reported. To me, it seemed statistically improbable that this could have been just coincidental. At that point, I was convinced there was something there, that it meant something and that it was important. I made a commitment to put my full attention to the field.

Q4 - Michelson: What is your definition of LENR?

A4 - Krivit: LENRs are weak interactions and neutron-capture processes that occur in nanometer-to-micron-scale regions on surfaces in condensed matter at room temperature. Although nuclear, LENRs are not based on fission or any kind of fusion, both of which primarily involve the strong interaction. LENRs produce highly energetic nuclear reactions and elemental transmutations but do so without strong prompt radiation or long-lived radioactive waste.

Q4.1 - Vu: What is your definition of "cold fusion"? [question added during interview; written response provided to MITRE Corp. the following day]

Cold fusion is a concept, unsupported by evidence but promoted by some people, that describes their belief that deuterons or protons can overcome high Coulomb barriers and engage in charged-particle fusion reactions at room temperature.

Q5 - Michelson: What has been your role in the development of LENR as an emerging science?

A5 - Krivit: It was in 2003 when I began a serious interest. Initially, my work started with an electronically published report I wrote, which led to a book in 2004. Since then, through my publications, international presentations and Web presence, I have assisted in bringing this field from general obscurity to widespread recognition and awareness.

I have been helping the public, mainstream media, academic institutions, industry and governments to understand and distinguish between the facts and the fallacies of LENR. To the best of my ability, I have consistently held a very hard line against poor or sometimes, as I have found, dishonest science. And I've done this regardless of whether the relevant matters involved apparent proponents or opponents of the science.

My early activity with the field was based on a natural level of beginner's ignorance. I lacked the skills and knowledge early on to be decisively critical, but I was extremely enthusiastic. These early communications of mine were welcomed by most members of the field. Then I developed greater scientific expertise and understanding of the field, just as a matter of course. As I dug deeper in the research, I also found some of its biggest flaws, and I then had the ability to analyze them. Much to the surprise and dismay of some of the outspoken members of the field and their fans, I also reported those findings.

I have provided subject-matter expertise to NASA Langley Research Center, NASA Marshall Space Flight Center, Diligence Business Intelligence, Strategic Business Insights, Sandia National Laboratories Technical Library, Department of Energy National Energy Technology Laboratory Library, Government of India Department of Atomic Energy, Netherlands Study Center for Technology Trends, Stanford University Department of Materials Science, Johns Hopkins University, Princeton University, University of California Los Angeles and many of my colleagues in the media who lack specialized expertise in LENR.

Q6 - Michelson: Who have you worked most closely with in this field? In this country? Abroad? Government? Industry? Academia?

A6 - Krivit: Nobody in particular, everyone in general. I have worked independently. My attention does tend to shift from time to time based on a particular [subtopic] that I might be exploring in depth.

Q7 - Michelson: What do you consider the major turning points in the evolution of cold fusion as a field?

A7 - Krivit: I have identified 14 items that answer this question. Some are distinct, time-specific events, and some of them are phases that span a range of time, but I've listed them more or less chronologically.

1. Bockris and BARC's Tritium - The First Nuclear Evidence
In 1989, within the first few weeks [after the fusion announcement by Stanley Pons and Martin Fleischmann], John O'Mara Bockris, at the time an electrochemist at Texas A&M University, and, independently, Padmanabha Krishnagopala Iyengar and Mahadeva Srinivasan, nuclear physicists at the Bhabha Atomic Research Centre in Trombay, India, discovered tritium in their LENR cells after performing their experiments. These were the very first sets of nuclear evidence in the field. However, there was so much general controversy and confusion at this time that these results - proof of a new nuclear phenomenon - went largely unnoticed.

2. Miles' Helium-4 - The Second Nuclear Evidence
In 1990, Melvin Miles, at the time an electrochemist with the U.S. Navy China Lake laboratory, reported helium-4 production in his LENR experiments. This, too, is evidence of a nuclear reaction. However, his first experiments were performed in glass cells. Critics, citing the normal presence of helium-4 in the air and saying that helium could have leaked through the glass, rejected Miles' initial claim. Miles and, separately, Bockris subsequently performed experiments in stainless steel and again detected significant amounts of helium-4.

3. Hagelstein, McKubre and Piantelli - Independent Recognition That Fusion Was Unlikely
Between 1993 and 1996, several milestones occurred that showed that people understood that the phenomena were not explained by a fusion process.

In 1993, Peter Hagelstein, an associate professor of electrical engineering at MIT, summarized the field and wrote in a review paper that nonfusion, weak-interaction, neutron-based theories "more closely match[ed] the experimental observations."

A year later, in the fall of 1994, Francesco Piantelli, at the time a professor of biophysics at the University of Siena, Sergio Focardi, a professor of physics at the University of Bologna, and Roberto Habel, a professor of physics at the University of Cagliari, performed a set of LENR experiments with nickel and light-hydrogen gas. Light-hydrogen LENR reactions are inexplicable by fusion.

The group obtained one of the most significant [to this day] sets of excess-heat results: One cell produced 38.9 +/-1.5 watts of heat; another produced 23.0 +/-1.3 watts of heat. The cells produced excess power continuously at a slowly increasing rate during that period: the first for 278 days; the second for 319 days. The integrated excess energy was 900 MJ and 600 MJ, respectively. Their work is a milestone in the field, but even though they obtained, scientifically, a phenomenal amount of excess heat, their work never caused a major turning point in the field. In fact, no excess-heat claim has ever convinced any skeptic of the reality of this field.

Q7.1 - Michelson: In your turning points, I noticed there is no mention of Fleischmann and Pons. I'm assuming that is because they are the proponents of excess heat. Or theirs is the fusion component. Is that correct? [question added during interview]

A7.1 - Krivit: You'll read in my encyclopedia articles that there were aspects of this field that began in the 1920s. Pons and Fleischmann were not the first to start working on this, but they were the first to have significant results in the subject. They were the first to bring it significantly into the public domain. Pons and Fleischmann's work was an initiation point, not a turning point. In terms of the turning points of the evolution of the field, in terms of significant contributions of Pons and Fleischmann, I do not see a contribution from them to any major turning point beyond their initial introduction of the subject.

Going back to item #3 - In 1996, Michael McKubre, an electrochemist at SRI International, also recognized the distinction between LENR and cold fusion. He appeared on “ABC Nightline” and said that fusion was an unlikely explanation for the heavy-hydrogen LENR research. He said it was definitely not an explanation for the light-hydrogen LENR research.

4. Miley's Five-Peak Elemental Spectrum From Ni-H LENR Research
In 1996, George Miley, at the time a [nuclear engineer] at the University of Illinois, published transmutation data from light-hydrogen LENR experiments. The data displayed a very distinct five-peak spectrum. A guy named Lewis Larsen, a newcomer to the field who had trained as a theoretical biophysicist [and also audited graduate courses in astrophysics,] saw Miley's data, and he recognized that the spectrum was very similar to one he had seen somewhere else in nature, specifically in the field of astrophysics. The LENR spectrum that Miley had corresponded to atomic abundances in the sun and stars and, by association in Larsen's mind, lent credibility to LENRs as real nuclear processes. There was a second point about the recognition of Miley's spectrum. His results were also inexplicable as the result of either deuterium or hydrogen fusion at room temperature.

5. Mizuno's Five-Peak Elemental Spectrum From Pd/D LENR Research
In 1998, Tadahiko Mizuno, a Japanese physicist and director of Hydrogen Engineering Application & Development Corporation, published data from heavy-hydrogen LENR experiments that displayed a similar, distinct five-peak curve. Larsen saw that the Mizuno heavy-hydrogen and Miley light-hydrogen spectra were similar, and he concluded, based on that, that LENRs with heavy hydrogen as well as light hydrogen were caused by the same underlying mechanism. Mizuno's heavy-hydrogen transmutation results were also inconsistent with the hypothesis of "cold fusion." The similarity of the spectra also indicated that the idea, which came later from people like McKubre, that there were "two separate branches" of LENR, one with heavy hydrogen and the other with light hydrogen, was wrong. The similarity of these spectra, each based on a multitude of data

points, indicated that there was no such thing as two branches. The concept of two branches was an artificial designation.

[Cold fusion proponents either did not know about the similar spectra or failed to recognize that it meant that light- and heavy-hydrogen LENRs were caused by the same processes. Regardless, they knew for certain that heavy-element transmutations could not be caused by light-element fusion, so they speculated that light-hydrogen LENRs and heavy-element transmutations were something else, not fusion.]

6. McKubre's Temporal (But Not Quantitative) Relationship Between Helium-4 and Excess Heat

In 1998, McKubre performed a meticulous deuterium gas-phase, activated carbon and palladium-black experiment. It showed an unambiguous rise of helium-4 that occurred simultaneously with a calorimetrically well-measured signal of excess heat. I think that this is still the best experimental evidence that shows the relationship of nuclear heat to nuclear product. There was only one problem: The relationship, the quantitative relationship between the heat and helium-4, was inconsistent with the hypothesis of "cold fusion."

7. Iwamura's Gas Permeation Transmutation Experiments

This research emerged from 1998 to 2003. Yasuhiro Iwamura, a [nuclear engineer] with Mitsubishi Heavy Industries in Japan, began to report meticulously performed experimental work that showed extraordinary evidence of heavy-element LENR transmutations with heavy-hydrogen LENR work. To give you some perspective, one of the observers of the field reported in 2003 that Iwamura's work "seem destined to affect the course of solid state and nuclear science." As predicted, heavy-element LENR transmutation research was taken more seriously in the field at this time. Iwamura's heavy-hydrogen transmutation results were also inconsistent with the hypothesis of "cold fusion."

8. McKubre's Claim of Proof of Cold Fusion

In 2000, McKubre presented an experimental claim, experiment #M4, that produced proof of “cold fusion.” His claim became known among “cold fusion” proponents as proof that deuterons could overcome the Coulomb barrier at room temperature. This matter came to light 10 years later. On Jan. 29, 2010, *New Energy Times* published an exhaustive and detailed investigation of McKubre's M4 claims. *New Energy Times* found that M4 was actually performed in 1994. *New Energy Times* also found that, starting with McKubre's presentation in 2000, McKubre began to retroactively manipulate and fabricate data that was associated with M4. He did so without presenting scientific support and without disclosing his changes to the public or his sponsor, the Electric Power Research Institute. *New Energy Times* provided McKubre with multiple opportunities to respond to the investigation. He did not respond.

9. Letts and Cravens' Laser Triggering Reveals Surface Plasmons
In the summer of 2003, Dennis Letts and Dennis Cravens, independent LENR researchers, performed experiments using low-power laser triggering. Their work tipped off Larsen and, independently, a researcher named Vittorio Violante, an Italian experimentalist, that surface plasmon polaritons were a key aspect of LENR.

10. Cold Fusion Proponents Pitch Department of Energy
In 2004, McKubre, Hagelstein, David J. Nagel, Talbot Chubb, Randy Hekman, Graham Hubler and Michael Melich proposed that the Department of Energy fund "cold fusion" research. The proposal referenced the M4 data and stated that "this value remains the most accurately determined in this field." There is no evidence that any of the co-authors was aware at the time that the data for M4 had been manipulated and fabricated.

The proposers failed to present the heavy-element transmutation work of Iwamura, Miley or Mizuno to the Department of Energy. [Hekman has since changed his perspective on LENRs. He has "lost confidence that the mechanism is explained by either fission or fusion, but [he is] much more convinced that the Widom-Larsen theory better explains the phenomena."]

The second look by the Department of Energy into "cold fusion" brought the subject into wider awareness, but the proposers' work was not sufficient to convince the Department of Energy to fund "cold fusion" research. There is no evidence that the Department of Energy was aware at the time that the data for M4 had been manipulated and fabricated.

11. Widom and Larsen Publish Ultra-Low-Momentum Neutron Theory of LENRs
On May 2, 2005, Larsen, with the help of Allan Widom, a condensed-matter physicist at Northeastern University, published a pre-print of their seminal paper "Ultra-Low-Momentum Neutron-Catalyzed Nuclear Reactions on Metallic Hydride Surfaces." Before their publication, at least half a dozen researchers had preceded Widom and Larsen with ideas of how weak interactions and neutron-capture processes could explain LENRs. But all of the other proposed ideas were either vague and incomplete or nobody had a complete set of beginning-to-end processes [that could explain LENRs] until Widom and Larsen presented theirs. Larsen was the first to see the big picture. He saw the relationship with solar abundances, and he saw the relationships among the diverse data. He worked with Widom, then later also with Yogendra Srivasatva, to develop the theory more fully.

12. Federal Government Begins to Take LENR Seriously
On Dec. 12 and 13, 2006, the Defense Threat Reduction Agency sponsored a meeting on LENR. Widom and Larsen were the only LENR theorists to speak there. The subsequent reception of the Widom-Larsen theory throughout the

federal government was strong. The most hostile and vocal opponent of the field up to that date, Robert Park, a former spokesman for the American Physical Society, also spoke at the meeting and conceded that the new field represented legitimate science. Park's concession also appeared in *Chemistry World* a few months later. The LENR presentations given at this DTRA meeting, based on my observations, appeared to trigger a new, although quiet, wave of interest by the federal government, which could partially explain your call today.

13. The War Against LENR

The next time span is from 2007 to now. The recognized American leaders of the LENR field, nearly all of whom had been fighting the battle for recognition of "cold fusion" and fighting for their own personal redemption as a result of sticking their necks out in this field, failed to distinguish and detach the valid LENR experimental research from the theory that deuterons or protons were somehow overcoming the Coulomb barrier at room temperature. They responded to the idea brought forward by newcomers Widom and Larsen with hostility and pathological skepticism. The recognized American leaders of the LENR field also distributed incorrect and negative personal information about Larsen. They also took steps to discredit experimental research that supported the Widom-Larsen theory.

In August 2008, recognized American leaders in the LENR field produced an international conference for LENRs in Washington, D.C., and systematically marginalized heavy-element LENR transmutation research from the conference. Around this time, some of these American participants also coordinated with the Naval Research Laboratory in efforts that appeared intended to discredit heavy-element LENR transmutation research.

On Aug. 20, 2008, at the American Chemical Society national meeting, I presented, for the first time publicly, a comprehensive meta-analysis of experimental phenomena observed in LENRs that individually and collectively disproved the hypothesis of "cold fusion." [My presentation was unchallenged by people in the audience after I spoke, and it has remained unchallenged in the scientific arena. When I spoke at the ACS meeting, two prominent "cold fusion" theorists, Akito Takahashi and Xing Zhong Li, were present for my talk. Peter Hagelstein showed up late and missed my talk, and Michael McKubre failed to appear. He canceled his scheduled talk the week before the conference.]

In 2009, some of the American participants in the field coordinated with the Defense Intelligence Agency and systematically marginalized the theoretical work of Widom and Larsen. The DIA coordinator for the project, Beverly Barnhart, said to me in a phone call after the report published on Nov. 13, 2009, "How could there be anything to Widom-Larsen, when everybody - I mean everybody I spoke to - told me that it was wrong?" Barnhart did not attempt to contact Larsen or Widom.

Robert Park's public concession was selectively ignored by the American participants in the field, even by David Nagel, who also attended the 2006 DTRA meeting at which Park conceded. Why was Park's concession ignored? Because Park [was able to see the distinction. He] dismissed the idea of "cold fusion," but he supported the idea of LENR.

Thus, for the second time in "cold fusion" history, scientists worked collaboratively and unprofessionally to interfere with progress and to block a new idea: the idea of weak interactions and neutron-capture process. The Widom-Larsen theory - right, wrong or perhaps somewhere in between - threatened the prevailing idea, since 1989, of deuterons overcoming the Coulomb barrier at room temperature, "cold fusion."

Part II: EMERGENCE QUESTIONS (Michelson)

We'd like to ask you six questions (each with 2 parts) that we are using to test performer systems. For each question we would like you to

- Share with us broadly your thinking about each question
- Answer YES, NO, or DON'T KNOW over six time periods for "part b" of each of the questions

Q1a - Michelson: Can you help us understand how a community of practice evolved over time among scientists researching LENR? A 'CoP' typically refers to the coalescing of investigators to research, develop, apply, or promote a domain or to otherwise contribute to the body of knowledge about a domain.

A1a - Krivit: In short, it involves several factors, including sub-specialties of research, complementary theoretical explorations, national versus localized collegiate relationships, business partnerships and funding opportunities.

A meeting place and opportunity for interchange has existed primarily through the field's various specialized conferences.

Q1b - Michelson: Did a LENR community of practice exist during each of the six time periods?

A1b - Krivit: [Yes, a community of practice existed from 1989 onward.]

Q2a - Michelson: Can you help us understand whether there were debates in the scientific community as LENR evolved as a science? And if so, the nature/subject of those debates?

"Debates" typically encompass (a) conflicting viewpoints on issues, (b) open and unresolved questions regarding approaches, methods, results, etc., or (c)

conflicting viewpoints on the fundamental merits, usefulness, novelty, etc., of the domain.

Such debates may arise not only internal to the community of investigators who are contributing to a domain, but also external to it, e.g., within prevailing communities whose paradigms are challenged by the domain.

A2a - Krivit: There are three phases.

The first phase is 1989-1993. The initial problem is that nuclear experts had never known of any kind of nuclear energy that did not produce commensurate levels of dangerous radioactive emissions. Few people at this time were aware of weak interactions, let alone the possibility that weak interactions could lead to high reaction rates. So, for most scientists, the claimed results were inexplicable according to what they knew at the time.

Nuclear physicists couldn't conceive of a way that deuterons could penetrate or overcome the Coulomb barrier at room temperature. Some people, like Hagelstein, tried to come up with explanations for this, but they all relied on imaginary physics.

From the experimental side, the field suffered early on from "experimenter's regress," which is explained by author Harry Collins: "When the normal criterion - successful outcome - is not available, scientists disagree about which experiments are competently done."

When the field emerged in 1989, there was a lot of initial opposition. Many people in science academia responded to it unprofessionally and with outright hostility. Some of these opponents lacked the courage to consider something so radically new and potentially disruptive; some lacked imagination. On a psychological level, it threatened their fundamental understanding of physics. On a practical level, it threatened their stature and funding. It threatened to make their textbooks and coursework obsolete. There were also some other opponents who were researchers who attempted to replicate the initial claim but failed and then may have felt embarrassed and frustrated and then became angry.

The second phase is 1993-2004. During this period, the field was largely neglected by mainstream science and mainstream media. To a great degree, although the researchers would certainly have liked to receive more financial support, I think they were happy to be left alone. However, significant misinformation which occurred from the onset of the field was never corrected in the broader public awareness [during this time]. But that started changing as of [the publication of] Charles Beaudette's *Excess Heat & Why Cold Fusion Research Prevailed* in 2000 and Steven B. Krivit and Nadine Winocur's *The Rebirth of Cold Fusion* in 2004. These books began to help correct some of the historical record.

The third phase starts in 2005, when Widom and Larsen came out with their theory, and has continued to the present. During this phase, the field has been experiencing bitter factionalism between two groups. One group is people who maintain their belief in "cold fusion" or, if not [in name], at least the idea of deuterons somehow overcoming the Coulomb barrier. Sometimes, they seem to have loyalty only to the name of "cold fusion." [Often, many of these proponents defend either the concept or the term "cold fusion," much like adherents to a religion defend their right to their beliefs.]

The other group of people, whom you don't hear much about, recognizes low-energy nuclear reactions as real, but they don't presume or assert that it's a fusion mechanism.

Q2a.1 - Michelson: So did the debates start in 1989 with the dawn of the field? [question added during interview]

A2a.1 - Krivit: There are two phases of debate. The first debate is about [whether the entire set of phenomena was real]; this began in 1989. The second debate is about whether it [was real but not] fusion; that started in 2005.

Let me add one more thing. That [second] debate has been suppressed, to the point that you're not clearly aware of it.

Q2a.2 - Michelson: What we are looking for are responses to these questions from the perspective of LENR, condensed matter nuclear science, cold fusion as a single field. [question added during interview]

A2a.2 - Krivit: You seem to think that there is unification in the field. This is wrong, but it's not your fault. There is a myth that has been portrayed, and at one time, I was a participant in propagating this myth. There is no unification of this field any longer.

There was up until 2005, when this serious idea came back: the idea of weak interactions. This doesn't have to do just with Widom-Larsen. They may have the best [approach]. The idea has to do with the concept of weak interactions or neutron-capture processes, whether it's Widom-Larsen or somebody else, but they broke the field into two different domains.

Q2a.3 - Michelson: So what you're saying is that, starting in 2005, we have another thread. [question added during interview]

A2a.3 - Krivit: Yes, you're getting it. There was a very significant distinction and fissure that occurred in 2005.

Q2b - Michelson: Were there debates within the scientific community about LENR during each of the six time periods?

A2b - Krivit: [Yes, there were debates from 1989 on.]

Q3a - Michelson: Can you help us understand what kind of infrastructure was required to conduct LENR research?

“Infrastructure” typically refers to equipment, computational resources, access to hardware, software or well-defined algorithms, etc., that are required to effectively explore a line of scientific or technical inquiry.

“Readily available” typically means that few, if any, obstacles exist that prevent researchers from acquiring and properly employing said infrastructure.

A3a - Krivit: Chemical lab, gas-handling apparatus, standard electrochemistry hardware including potentiostat, data acquisition hardware and software, data collection and presentation software, a variety of chemicals, heavy water, normal water, light and heavy hydrogen gas, platinum group metals, nickel, other metals, electrolytes, detection and measurement devices (neutron, x-ray, alpha, gamma, helium, tritium), calorimetry systems (isoperibolic, Seebeck, mass flow), glassware, access to metal shop, access to reactors for neutron activation analysis, variety of microscopes and spectroscopes, thin-film fabrication devices, ultra-high vacuum pumps, nanoscale fabrication tools, beam devices, furnaces, technical reference library.

Q3b - Michelson: Was the infrastructure needed to conduct LENR research readily available during the six time periods?

A3b - Krivit: It is difficult to generalize, but assuming the case of well-equipped university or government laboratories, most of the infrastructure existed during most of the time periods. However, nanotech fabrication tools and processes may not have existed during the onset of the field.

Q4a - Michelson: Can you help us understand how a demonstration of a practical application of LENR was or was not achieved -- how LENR research has moved from a concept to a practice – from a concept about what might be possible to a demonstration of feasibility?

A “demonstration of practical application” typically means that a domain has been tangibly realized (implemented, formed, built, etc.) and shown to contribute to solving a problem or satisfying an unmet need.

A4a - Krivit: There are no existing demonstrations of LENR as a practical demonstration or application. The attempts when these were not achieved are too numerous to count.

Q4b - Michelson: Was there a demonstration of a practical (vs. theoretical) application of your domain during these six time periods?

A4b - Krivit: None.

Q5a - Michelson: While the previous question was broad, this question narrows, focusing ONLY on commercial applications. Can you help us understand how commercialization of the domain occurred – if this is indeed the case?

A “demonstration of commercial application” typically means that there is a product offered for sale in the commercial marketplace, or industry is supporting R&D to enable the manufacture and sale of a commercial product.

A5a - Krivit: There are no existing demonstrations of LENR as a commercial application. The attempts when these were not achieved are too numerous to count.

Q5b - Michelson: Was there a demonstration of a commercial application of your domain during these six time periods?

A5b - Krivit: None.

Q6a - Michelson: At the dawn of your domain, was it a completely new innovation, or was it replacing a previous generation of science? Was there an established science that it was an alternative to, or was it novel?

An “alternative to an established science” is typically something relatively new which promises to replace or supplant a known and accepted/applied idea, tool, approach, solution, etc.

A6a - Krivit: It was, effectively, the dawn of a new domain, both scientifically and potentially, hopefully, practically. However, glimpses of it had surfaced as far back as the 1920s. LENR is poised to provide a completely new process, fuel use and form factor for energy and material science applications.

Some people may argue that LENR was poised to replace thermonuclear fusion, but that domain is neither practical nor directly related. Some people may argue that LENR was poised to replace nuclear fission. If it does, it may follow a model similar to how microcomputers replaced many mainframe computers.

Q6b - Michelson: Was your domain considered an alternative to an established technology during these six time periods?

A6b - Krivit: No

Part III: CONCLUDING QUESTIONS (Michelson)

Q1 - Michelson: Were there any publications in languages other than English that significantly contributed to the evolution of your domain?

A1 - Krivit: *Biological Transmutations*, by Louis C. Kervran, translated by Crosby Lockwood $24.95, Beekman Books, Inc., ISBN 0846401959, (June 1998)

Nuclear Transmutation: The Reality of Cold Fusion, by Tadahiko Mizuno $12.00, Infinite Energy Press, Concord, N.H., ISBN 1-892925-00-1, (Dec. 1998)

Q2 - Michelson: Do you have any suggestions of new domains that are currently emerging or of negative examples of emergence -- lines of inquiry that would be considered “false starts” in that the “domains” are unlikely to emerge?

A2 - Krivit: No.