

**MIT/LANR Colloquium**

March 21-23, 2014

**Post-Missouri Priorities for LANR**

**Charles G Beaudette\***

[Optional]

In scientific circles LANR research remains, mistakenly, a pariah science carried on by charlatan operators. Its recent public establishment at the University of Missouri in Columbia, Missouri, was achieved by the narrowly limited and forceful presentation of excess heat data to display the existence of a heat energy source previously unknown to science. This heat-data argument must be used exclusively for the next U.S. Department of Energy review. Also, with continued acceptance, the American environmental movement can be expected to emerge as a strong antagonist of LANR. [87]

\* cgbeau@gwi.net; South Bristol, Maine, USA.

April 4, 2014 (5:06pm)

At the Fleischmann and Pons (F&P) announcement in Utah - now so many years ago - Chase Peterson, then President of the University, set forth a correct methodology of response for the scientific community. Several years of experimental work, he pointed out, would be required to evaluate the claims to be made that afternoon. This proved a reasonable estimate for the time it actually took to confirm that excess heat was real, about five years. After that, Professor Hagelstein would open his talks on LANR theory with the statement, "I believe excess heat is real."

Seared in our memory are the events that took place just five weeks later, at a meeting of the American Physical Society in Baltimore, when two scientists from the California Institute of Technology remonstrated aggressively against these claims. They insisted that the evaluation of the F&P experiment was now complete: currently available Hydrogen-fusion theory and data did not allow the possibility of fusion, and bad calorimetry mortally damaged the claim of excess heat. The emphatic use of slander was not beneath them: the Utah chemists were declared to be delusional. One of the Caltech scientists took the trouble to hold several press conferences just to be sure that their charge was well nailed down for public consumption. As might be expected, neither Fleischmann nor Pons, nor their associates had the skill, or zeal to respond forcefully to such an onslaught of vituperation.

Unfortunately, sixteen months would pass before publication of a detailed, fifty-five page exposition of the experiment and its calorimetry, which publication the scientific community met with a resounding silence. Even today that paper is rarely referenced. I ask this community, in your research papers, always reference the original experiment - Journal of Electroanalytical Chemistry: April 10, 1989; May 10, 1989; and July 25, 1990.

By the fall of 2003, the scientific community once again displayed its obtuse attitude towards LANR. The New Hampshire MIT club asked me to bring my Power Point show to one of their monthly meetings. How they picked up the idea to contact me I don't know, but when I showed up the late Dr. Mort Goulder, one of the club's senior members, and an MIT physics alumnus class of 1946, glommed onto my right arm. He took the chair next to me at dinner and had endless questions to ask.

Goulder held a doctorate in physics and had been an officer of Sander's Associates and Raytheon Corp. He was well known and highly respected within the science community at MIT and in associated industries of the area. In semi-retirement, besides running a consulting service, he was a philanthropic patron of MIT's physics department.

After the presentation, Goulder offered a proposal. He would undertake a visit to the people at MIT's physics department to see if he could interest them in the evidence for excess heat energy. I delivered to him several copies of my book and a binder with twenty or so papers on the most revealing experiments of the previous fourteen years.

In late September of 2003, I received a letter from Goulder reporting on his visit. He had met with the then head of the physics department, Professor Mark Kastner, and several of his top theoreticians and experimentalists.

Goulder was unable to interest them in the subject. In his letter, he gave the five reasons they expressed for avoiding the topic. He listed these according to the physics department's order of importance:

- (1) It is theoretically impossible;
- (2) The government has spent millions chasing this "wild goose" to no avail;
- (3) Those who report positive results have little credibility in the scientific community;
- (4) The only constructive comment was that there was some unknown exothermic chemical reaction taking place, but it was not fusion;
- (5) They also mentioned that no one had run a control experiment with H<sub>2</sub>O instead of D<sub>2</sub>O.

Most startling was Goulder's closing bid. Before departing the meeting, he offered to contribute funding for a graduate student to try to understand what was going on in the field - there were no takers. To borrow one of Robert V. Duncan's favorite expressions, the MIT physics department was "letting others do their thinking for them" on at least three of the five reasons they gave for not taking an interest in this field.

This dismissive attitude toward LANR perpetuates from year to year, and it gets alluded to regularly by those newspaper and magazine writers who call themselves science reporters. This field will eventually require a substantial degree of public financing, so public attitudes toward it need nurturing. This makes DOE the ultimate target. Unfortunately, the science reporter to a large extent is intermediary between the science and the public. It is important to notice how the science reporter works.

The salaried ones in America, whose job title is science reporter, are a small group, with maybe fifty to one hundred in full time employment. They do not have the scientific education or the background of experience that comes from a career practice in science. So the question may fairly be asked, How do they manage to make a career of reporting with a high order of accuracy (cold fusion notwithstanding) concerning new developments in various fields of science?

April 4, 2014 (5:06pm)

Their means are ancient. They ingratiate themselves into the company of leading scientists, especially those who are authors of popular books. When something new comes up the reporter gets his story from them. He is thereby enabled to express himself with precise statements on complex issues. The scientist, in turn, gets to see his name placed in national publications as a contributor to some profound topic.

In those cases where there is serious controversy, the reporter has a choice. He can stand by his source and offer intelligent commentary, or treat the matter as a horse race by reporting who is scoring and who is not.

Recently Dr. M.H. Miles corresponded with a science reporter who had written the Fleischmann obituary for the English weekly journal *Nature* – arguably the foremost scientific journal.

In it, this science reporter pointed to the four experiments that reportedly failed to generate excess heat in the spring of 1989. In a letter to him, Miles responded to this argument by referring to the lack of adequate loading, the now well established requirement of eighty five percent minimum Deuterium loading into Palladium. In particular, Miles pointed to Lewis's paper published in *Nature* that reported on his experiment. It identified a loading of only seventy eight percent.

That reporter dismissed Miles' point out of hand on the argument that Lewis was not now involved in their conversation. Without Lewis present to defend his paper, reference to it was inadequate and unfair. In that reporter's view, the numerical details as reported in the published paper did not serve a definitive purpose. Without the author actually present to answer Miles' point, there was no compelling argument by way of the published article. This inability of science journalists to make use of published research papers is an endemic flaw in current science reporting.

The November 1998 issue of *WIRED* magazine offered an outstanding exception to this pattern. The reporter, Charles Platt, wrote an article on the status of the field of "cold fusion" that, quite simply, reported the published results. This was an unusual and commendable example of competent science reporting.

Where LANR scientists depend upon published papers for their knowledge of the field, those same papers offer no bridge to the mainstream media's science reporters.

One thing the reporters enjoy is chasing after claims of a power production demonstration. That seems to be a subject they believe they can handle by themselves. The New York Times apparently has sent a science reporter to some of the Andrea A. Rossi's E-Cat demonstrations in Italy. But that same newspaper has not mentioned the papers of J.F. Piantelli from the nineteen nineties reporting the strong production of heat energy from which the E-Cat equipment design evolved. So far, little illumination of our work can be expected from science reporters reporting in mainstream publications.

Another topic looms large that will eventually compel attention. A brief glimpse at what the environmental movement might have in store for LANR may serve as a useful warning – a warning that emphasizes the need for caution when anticipating the world of commerce. The environmental movement took nuclear-fission electric generation by surprise in the 1970s. Today the movement is much stronger with enormous public standing and government agency support that would enable it, if it chose, to bring LANR to a stand-still for a second twenty five year interlude. After dealing with DOE, EPA will follow close on.

What argument would the environmental movement use? The argument that it generates radioactivity, not in some distant, fortress-like building, a guarded and fenced, concrete monument, but rather in the home, the vehicle, and the work place. That this radioactivity is minute, short lived, and boxed in, though true, will prove a difficult point to carry. Just note the outcry of possible brain damage from the harmless cell phone.

April 4, 2014 (5:06pm)

And the movement would do this to what purpose? Its purpose would be to limit the amount of energy with which three hundred million polluters can further undertake to ruin the environment by using that energy to build larger and more differentiated houses and by almost continuous traveling about by every means available.

The environmental movement is always somewhat hobbled because it cannot address itself directly to this matter. After all, it loves these three hundred million polluters because they include its sympathizers, ardent supporters, and contributors. But the movement so far has found ways to limit our access to energy by law, by high prices, and for some major sources of energy – such as the Alberta oil sands – by revulsion. This movement will not be easily disarmed.

The field's first successful breakout in America into scientific and professional acceptability is the recent establishment of the Sidney Kimmel Institute for Nuclear Renaissance in the physics and chemistry departments of Missouri University at Columbia. The then Vice-Chancellor for Research, Dr. Duncan, achieved this simply by fortifying himself with data demonstrating excess heat and demanding a search for the understanding of it. Critics on the Missouri campus who suggest there might be error in this data are, proverbially, led to the campus laboratory where *they* can make a major contribution to the field by identifying that error. Using this approach to disinterested parties constitutes a winning strategy but, alas, it requires a campus laboratory.

Recall that the government's November 1989 review of this incipient field contained a strong argument about methodology (expressed in Norman Ramsey's words): "...even a single short but valid cold fusion period would be revolutionary."

It is now ten years since the U.S. Department of Energy, Office of Science, last accomplished a small review of this field. When the time becomes opportune – now it is not – and it is persuaded to a small review our field again, What would constitute a suitable set of review arrangements?

April 4, 2014 (5:06pm)

They should be limited to evidence for the production of anomalous-heat power and energy. If the reviewers are suitably qualified they will necessarily have expertise in thermodynamics and calorimetry, the principle skills appropriate to this much narrowed topic. By concentrating reviewer time and specialty on the production of heat the field would gain new recognition. If DOE will not agree to these terms, then a small DOE review is probably not worth a repeat of previous attempts. Only a much larger scale review could absorb twenty five years of accomplishment.

So the recommended question to be answered by a new review asks,

**Has a single, valid anomalous-heat period occurred?**

If the review answers in the affirmative, science will have officially gained a new source of energy to be explored for its science and, later, for its usefulness to society.

[1985 + 86]

[PRESENTATION END]

FILE END