COLD FUSION IS NOT VOODOO SCIENCE

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1) Introduction

As a retiree, born in 1931, 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 this 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 reported LENR effects. Being a nuclear physicist I have been passionate about this field for the last ten years. That is why I decided to write about what I know and think.

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

That is why physicists were so surprised 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, Time 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 temperatures. 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:

http://pages.csam.montclair.edu/~kowalski/cf/403miley.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:

http://lenr-canr.org

That library, by the way, was created by Jed Rothwell and Edmund Storms. George Miley's long paper is devoted to political aspects of the controversy. Here is a brief summary. His 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:

> ***. . . an 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, Miley continues:

> ***[T]he 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. [...]

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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? [...]"***

Rejections of submitted manuscripts by editors of some leading scientific journals—without sending them to referees—amounts to harmful censorship. That is not what editors are expected to do, according to norms of scientific methodology of validation. Descriptions of several cases of rejections, and additional comments, can be found in Chapters 13 and 19. Normal development of science would be impossible without the peer review process. How can one disagree with Miley that university teaching and research reinforce each other? He was indeed an ideal person for the task.

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 that 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 hot 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 privilege 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

Only one of their 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," 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 written by CF scientists, bypassing peer review. This kind of discrimination, directed against PhD-level scientists, is totally inconsistent with scientific norms. Illustrations of such discrimination are to be found in chapters 17 and 18.

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:

1. Is it true that unexpected protons, tritons, and alpha particles are emitted in some CF experiments?

2. 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?

3. Is it true that highly unusual isotopic ratios have been observed among the reaction products?

4. Is it true that radioactive isotopes have been found among reaction products?

5. Is it true that transmutation of elements has occurred?

6. 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 in Fig. 1. Here is how the setup was described in my later conference presentation:

> ***A schematic diagram of a cell operating under such conditions is shown in Figure 1. The cathode is a tungsten rod while the anode is a large platinum wire spiral, or a platinized niobium cylinder. The electrolyte, in experiments in which I participated, was potassium carbonate (K_2CO_3) dissolved in distilled water. The concentration was 20 grams per liter. Decomposition of water, at high current, becomes so intense that yellow glow discharge and arcing can take 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.

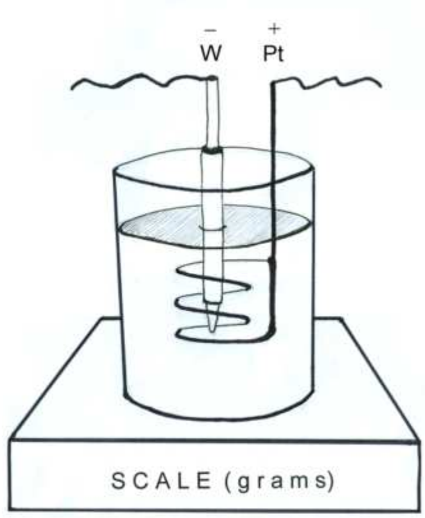

Figure 1

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 none 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 than 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 *Voodoo 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 Conference 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-scientists should have the courage to face their opponents.

In a review of Park's book at: http://home.netcom.com/~storms2/park.html Edmund Storms 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 seriously 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 Furor*, 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 wandering if you can 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 two colliding hydrogen ions. Experimental data certainly do not support such an idea.

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.***

The 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?

My letter has not been published; the editor rejected it without explanation. Why was it not sent to referees? Why was I deprived benefits of their comments? I was probably not the only teacher confused by the CF controversy. 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. Other histories of rejections are described in Chapter 19.

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 on the right in 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 molybdenum on the other side of the foil. The number of atoms of strontium that disappeared was about the same as the number of atoms of molybdenum detected on the other side of the foil.

The provided interpretation was that atoms of strontium are turned into atoms of molybdenum. Instruments used in these 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 possibility of molybdenum 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, 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 particles on the experimental chip was much higher that on the control chip. But the third experiment’s results 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 titanium 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, are 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, outcomes 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 Chapter 19, 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 strontium into molybdenum, or cesium into protactinum, 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

This is what I sent to Dr. Reid:

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:

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

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

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

Have radioactive isotopes been produced in some CF systems?

Has transmutation of elements occurred in some CF setups?

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.

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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 Fleischmann 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, nor 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 initial 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 available 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 attributed 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 scientific 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.

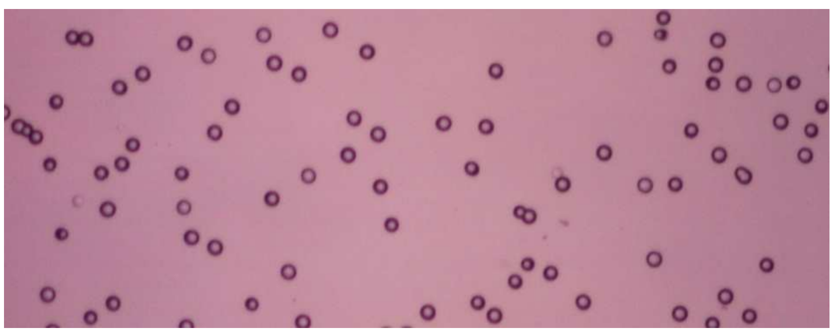
But the ending was not happy; my conclusion was similar to Oriani's — the pits were not due to alpha particles. But I am not a corrosion expert, like Oriani. I said that the pits might have been due to much larger nuclear projectiles, such as fission fragments. To reach this conclusion I measured sizes 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.

We debated the issue on pages of the 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, will 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 experiments with CR-39 chips with thin foils of Mylar). He submitted a paper with 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 agreement with those reported by Richard. The exceptional results from these three experiments were attributed 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:

http://pages.csam.montclair.edu/~kowalski/cf/409social.pdf

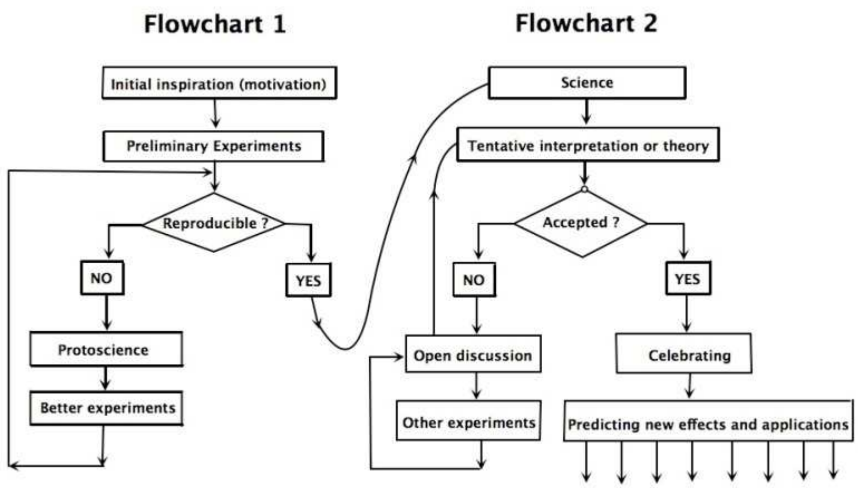
In the concluding section I wrote:

> ***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 well-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?"

From protoscience to science and applications



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 magnificent 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 researcher 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 effectively? In my opinion, well-focused cooperative investigations, as in The Galileo Project, are likely to be more productive, 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://pages.csam.montclair.edu/~kowalski/cf/408rossi.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 congress 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:

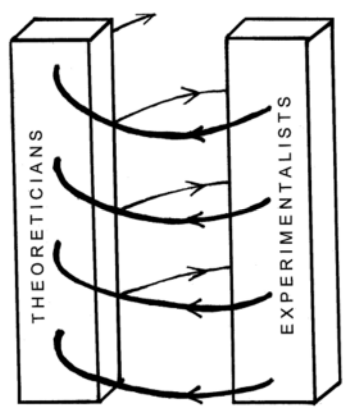
Reproducibility on demand is an important scientific requirement. But suppose a reproducible result conflicts with an existing theory. What should be rejected, the experimental result or the theory?

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 Cold Fusion 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.

===

26) About The Author

Ludwik Kowalski, born in 1931 in Warsaw, was educated in the USSR, Poland, France and the USA. He is a retired nuclear physicist (see Wikipedia). In addition to this short book about Cold Fusion he wrore two ideologically-orinted books. One of them is **Hell on Earth** (link 1 below), another is **his autobiography** (link 2 below).

Writing these free online books was a moral obligation to his parents, to millions of other victims of Stalinism, and to Poland. Link 3 below will take you to a list of all his publications (mostly scientific and pedagogical).

Links:

1) http://csam.montclair.edu/~kowalski/father2/introduction.html
2) http://csam.montclair.edu/~kowalski/life/intro.html
3) http://csam.montclair.edu/~kowalski/LK_publications.html
4) http://csam.montclair.edu/~kowalski/LK_presentations.html
5) http://csam.montclair.edu/~kowalski/my_opeds.html