

History of attempts to publish a paper

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My 2004 paper, reviewing recent cold fusion claim, has been rejected (without sending it to referees and without offering any criticism) by editors of seven journals:

- 1) Physics Today, USA
- 2) American Scientist, USA
- 3) Scientific American, USA
- 4) Nature, UK
- 5) New Scientist, UK
- 6) The Physics Teacher, USA
- 7) Science, USA

Please read the rejected article:

Recent cold fusion claims: are they valid?

<http://blake.montclair.edu/~kowalski/cf/152summary.html>

1) Here is how my paper was introduced to the editor of one of the above journals. Other accompanied letters were similar.

“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 that, I hope, can be published in Scientific American. The title is “Recent cold fusion claims: are they valid?” It is not a paper defending cold fusion claims; it is a paper describing them, no matter what one is inclined to think. Scientifically literate readers are likely to appreciate my short summary of recent claims made by cold fusion researchers.

Some of these claims, such as turning Sr into Mo, or Cs into Pr, without stellar temperatures, are even more extraordinary than the claims made by Pons and Fleischmann. The strange thing is that authors of such reports seem to be reputable scientists associated with prestigious universities and laboratories. Is it a matter of fraud? Is it a matter of self-deception, or incompetence? Is it a matter of progressive degeneration due to the isolation of the field from mainstream science? My article does not try to answer these questions; its purpose is to present a summary of what has been

recently reported without taking sides. The subject is interesting no matter what the final verdict of the second DOE evaluation will be.

Like many other science teachers, I am in no position to verify validity of hard-to-accept claims in a specialized laboratory. That is why, as suggested in the concluding section, a new evaluation of cold fusion claims, by an appointed panel of experts, is highly desirable. In writing this 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, which resulted in the attached manuscript.”

2) In rejecting my paper the editor of Physics Today wrote: “ 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.”

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?

3) And here how the editor of American Scientist responded to my submission: “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.”

4) Responding to the above I wrote:

“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 piece” (see below), were attached.

5) SEEK NOT THE GOLDEN EGG, SEEK THE GOOSE

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 focus on essential scientific questions and not on practical applications which are far away, at best. Promising too much, and too early, was one of the mistakes made fifteen years ago. In my opinion the six most important scientific questions are:

- 1) Are unexpected neutrons, protons, tritons and alpha particles emitted (at low rates) in some CF experiments?
- 2) Is generation of heat, in some CF experiments, linearly correlated with the accumulation of ^4He at the rate of 24 MeV per atom of ^4He ?
- 3) Have highly unusual isotopic ratios been observed among the elements found in some CF systems?
- 4) Have radioactive isotopes been produced in some CF systems?
- 5) Has transmutation of elements occurred in some CF setups?

6) Are the ways of validating scientific findings in the areas of CF research consistent with accepted methodologies in other areas of science?

I think that a positive answer to even one of these six questions should be sufficient to justify an official declaration that "cold fusion, in light of recent data, should be treated as a legitimate area of research." The normal peer review mechanisms will then be used to separate valid claims from wishful thinking.

6) In a subsequent reply I wrote: " ... 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 pejorative 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 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 contacts one of them to write a longer review paper of the journal. Steven Jones, Martin Fleicshmann and George Miley were among the scientists I selected. I did not hear from Dr. Reid again. Will she accept my "short piece?" Probably not.

7) The manuscript was submitted to Scientific American. Here is the reply that came after a long delay: “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.”

8) I then later tried to publish the paper in Nature. Instead of sending the article to them I decided to follow the presubmission path. The most impressive part of the path was that the negative reply came about ten hours later. The process of presubmission consists of filling two text boxes on their web site. The first box was for the letter about my article; I wrote essentially the same as what I wrote to other editors. The second box was for the first paragraph of my paper, and for the references used in it. The reply was short and clear:

“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.”

9) I then tried another UK journal, New Scientist. But they never responded. After waiting about a month the article was submitted to **The Physics Teacher**, a journal in which several of my teacher-oriented review papers were published in the past. In submitting the article I wrote:

“Dear Dr. Mamola: As you probably remember, the manuscript on Cold Fusion that I submitted about two years ago was rejected by your reviewers. My letter to the editor, however, was published last summer. I was pleased by this. The topic, as you know (see the "DOE WARMS to Cold Fusion" article in last April issue of Physics Today), is likely to be of great interest in the near future. With this in mind I wrote a **new article** on Cold Fusion and I hope that you will be able to publish it next fall. As you will see, I am simply describing controversial claims, I am not defending them. An extensive list of references is provided for those teachers who might wish to familiarize themselves with recent papers. The length is 3302 words, including 37 references. If necessary I can shorten the article, and reduce the list of references. But I prefer not to do this because I believe that everything is important.

. . . I am still undecided about validity of cold fusion claims but I think that they should be known to physics teachers. Unfortunately, most teachers are not familiar with experimental data gathered in the last ten years. The pending evaluation of the field by the DOE is likely to be publicized in the media; this will lead to student interest and questions. Hopefully, my paper will help teachers deal with the renewed interest in the ‘forbidden field.’”

More that a month later I received the following rejection:

“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.”

10) My immediate reply was: “Dear Dr. Mamola: Was my manuscript examined by referees? I would very much like to see what they had to say about its content. Thanks in advance.” This message has not yet been answered. Will I see the referee’s comments? Probably not. Will the pending DOE review end the unhealthy feud about cold fusion? Will it result in elimination of administrative barriers (such as rejection of articles without the peer review process)? What motivates defenders of the status quo? Who benefits from it? Yes these questions belong to the realm of social sciences. But that does not mean they should remain unanswered.

The reply from Dr. Mamola came much later than I expected. He wrote “Dear Professor Kowalski, My apologies for the delay in responding to your email. I have been out of the office for several weeks. To answer your question, the manuscript was reviewed by our editorial staff. We consulted with one of our referees but did not ask for a formal review, believing it would be premature at this point. Sincerely, Karl C. Mamola Editor.”

11) The manuscript was then submitted to the Editor in Chief of Science, Donald Kennedy. Here is the reply received next day, Saturday afternoon: “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.” Hmm, very efficient; they had only couple of hours on Friday to read the manuscript. It was rejected because the topic is not appropriate. Why is it not appropriate? Aren’t the described claims scientific?