

This document contains correspondence with the past and present editors of the *Scientific American*. We think the *Scientific American* has made many unwarranted, ignorant attacks against cold fusion. Some of these attacks have been so extreme, they border on the bizarre. In the exchange of letters from 1991 (shown below), the editor of the *Scientific American* declared that any subject “not fully understood by science” is pathological.

This version uploaded May 22, 2003.

## **Appeal to readers: spread the word and help bring about a rebirth of interest in cold fusion**

December 14, 2002

We are gratified that so many readers have downloaded the technical papers in our library. Since we moved to this ISP on October 7, over 12,300 copies of papers have been downloaded, and we have had 6,700 visits. \* Naturally, not all readers agree the papers prove that LENR (cold fusion) exists. Indeed, some of the papers in our library conclude that LENR is an experimental error. We strive to present all views. But the widespread interest demonstrates that many people are open minded, interested in the subject, and willing to look carefully at the evidence. These papers are not easy to read. This is not a subject a person can master in a few days or make a snap judgment about after reading one or two papers. We are pleased to see how many people are taking the trouble to learn more, and to make an informed, scientific judgment. We are confident that given a fair, objective hearing in the traditions of academic science, LENR will be accepted, and research will once again be funded in the United States.

We appeal to our readers to help bring about the rebirth of interest and research in this field. Here is what you can do:

Tell your friends about this site. Encourage others to have a look. Our readers must be doing this already, because the dramatic growth in traffic here has all been due to word of mouth recommendations. Please keep it up! We cannot afford to advertise, and LENR is seldom mentioned in the mainstream media.

Write to scientific journals and decision makers you feel may be open minded and willing to take a second look. We suggest that a non-confrontational but firm, factual, objective tone will be most persuasive. Do not oversell the likelihood that cold fusion will solve the energy crisis in the near future.

Please feel free to send us copies of your letters, and any response. Our address is: editors (at sign) LENR-CANR.org. The attached letter by Edmund Storms to the editors of *Scientific American* is a good model. Unfortunately, the editors of this magazine may not be good targets,

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\* Note: By December 14, 2003, a year later, over 340,000 papers were downloaded.

because they have gone far out on a limb opposing cold fusion. They, along with the leaders of the American Physical Society and the Department of Energy, have made their institutions into bastions of opposition to cold fusion. Rather than writing to such people, it might be more fruitful to approach those who have taken no public position, or those you know personally are open minded and curious. There is not much point to contacting nuclear theorists, because most of them are convinced they can tell *a priori* -- based on theory alone -- that LENR is impossible, and they are convinced the experiments must all be in error. This is a violation of the scientific method: theory can never overrule replicated experiments. Unfortunately these researchers seem to have lost touch with this fundamental principle. It might be more fruitful to contact electrochemists and others who are more accustomed to dealing with difficult, hard-to-reproduce experiments.

Editors  
LENR-CANR.org

Editors  
Scientific American  
415 Madison Ave.  
New York, NY 10017  
editors@sciam.com

Dear Sir:

Your analysis in the December issue about why science is neither respected nor understood by the general public I found to be very much to the point. Those occasions when science accepts claims that are later found to be false clearly give science a black mark. Unfortunately, in an effort to avoid such embarrassment, science also rejects claims that are later found to be true. I ask you, 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?

I could offer many examples of how good ideas have been rejected in the past, but I would like you to consider one very important claim that now has almost universal rejection, yet is supported by a growing body of data. As a scientist, I was trained to judge the reality of nature from good data based on replicated experiments. Yet, I find that the scientific community increasingly bases what is real on the opinions of a few respected journals and academics using theoretical arguments, regardless what is being discovered by other scientists operating in the real world. How is the general public expected to respect science when it does not follow its own stated rules of evidence?

The discovery I would like to use as an example of this double standard is what is called LENR or low energy nuclear reactions. This has also been given the very inaccurate name of cold fusion, a name that now causes rejection and ridicule. This ridicule comes from people who have no understanding about what is now known, yet their opinions are accepted as fact. Is this the way science is supposed to operate?

If you wish to be true to your stated wish to make science more respected, I suggest you educate yourself about this important phenomenon by reading information available at [www.lenr-canr.org](http://www.lenr-canr.org). There you will find over a thousand publications that support the reality of such anomalous nuclear reactions, as well as several reviews in full text that answer important questions raised by skeptics in the past. Serious scientists rejected "cold fusion" in the past for good reason. These reasons no longer apply. If science cannot correct a past rejection, then what good is the scientific method? Can anyone respect a scientist who cannot change his/her mind after being presented with better data?

Respectfully,

Edmund Storms, Ph.D.

The editor, John Rennie, responded with an e-mail:

From: "John Rennie" <jrennie@sciam.com>  
To: <editors@lenr-canr.org>  
Cc: <mstaker@arl.army.mil>  
Sent: Friday, April 25, 2003 11:49 AM  
Subject: from Scientific American

(Via editors of LENR-CANR.org)

Dear Dr. Storms:

Thank you for your email proposing that Scientific American reevaluate the status of LENR-CANR research and consider publishing an article on the subject. As you suggested, I did look over a number of the offerings at [www.lenr-canr.org](http://www.lenr-canr.org). Unfortunately, I still don't see evidence in those papers, or in the mainstream physics literature, that LENR-CANR has achieved any significantly new level of credibility in the eyes of the general physics community. The site does point to a large number of publications that ostensibly offer evidence of the phenomenon, but sheer numbers of papers is not sufficiently compelling-- as I'm sure you know, even the creationists can point to thousands of "publications" and "scientists" seemingly supporting their position.

I noticed that the LENR-CANR site, on its page appealing to readers to "help spread the word" about the phenomenon, described Scientific American as having "gone far out on a limb opposing cold fusion. They, along with the leaders of the American Physical Society and the Department of Energy, have made their institutions into bastions of opposition to cold fusion." We at SciAm don't feel that we've gone out on a limb in criticizing cold fusion harshly in the past. As for our being a bastion of opposition to it, I don't think we have an intractably committed position. If LENR-CANR can be demonstrated satisfactorily for acceptance by the physics mainstream, we would be more than happy to publish more favorable articles about it. Your problem starts with establishing more credibility in their eyes, not ours.

Sincerely,

John Rennie

--

John Rennie, editor in chief  
Scientific American  
415 Madison Ave.  
New York, NY 10017  
tel: 212-451-8813  
fax: 212-755-1976  
jrennie@sciam.com

## Comments by Jed Rothwell, and Further Correspondence

Edmund Storms made a suggestion to Rennie:

I would like to suggest the following. The process called LENR has now acquired a level of understanding that warrants its serious reexamination. In addition, the clean energy this process promises is essential in light of the present situation with respect to our increasingly limited oil supply and its location. Most scientists who might support the reality of the LENR process have very little knowledge about what is known, in spite of over 77,000 papers that have now been down loaded from the website. If you agree that these conclusions are even partially true, I suggest *Scientific American* publish an article about the subject coauthored by a skeptic and a believer, wherein the reality of the phenomenon would be debated. The magazine would not have to come out on either side of the issue, but be an honest broker of information about an increasingly important subject.

Rennie rejected this plan. I told Rennie I planned to upload his message here, at [LENR-CANR.org](http://LENR-CANR.org), along with a letter from the previous editor, Jonathan Piel, shown below. I sent him the earlier correspondence and asked whether he still agreed with Piel's position.

Cold Fusion Research Advocates  
2060 Peachtree Industrial Court, Suite 312-F  
Chamblee, Georgia 30341

Phone: 404-451-9890 Fax: 404-458-2404

November 22, 1991

Dr. Jonathan Piel, Editor  
Scientific American, Inc.  
415 Madison Avenue  
New York, NY 10017

Dear Dr. Piel,

John Rennie believes that Cold Fusion has been discredited ("The Ig Nobel Prizes," December 1991, page 26). He is incorrect. Cold fusion has been verified by hundreds of workers in over 100 world class laboratories, including: Los Alamos National Laboratory, Oak Ridge National Laboratory, Naval Weapons Center at China Lake, Naval Research Laboratory, Naval Ocean Systems Center, Texas A&M, BARC India, Hokkaido University and National Institute for (hot) Fusion Science in Nagoya.

Japan is particularly active in this field. Every major Japanese university has formed a Cold Fusion research group, and these groups are represented at major physics meetings like Japan Nuclear Energy Conference and the ISEM-Nagoya IEEE meeting next month (see attached).

Dr. Eugene Mallove and I are circulating the enclosed Petition calling on Congress to restore funding to cold fusion. In one month, over 170 scientists, engineers and concerned citizens signed, including: Nobel Laureate Dr. Julian Schwinger; from M.I.T. Drs. Kolm, Labitt and Rafuse; Dr. Hawkins of Smithsonian and Boston U. (retired), noted hot fusion scientist Dr. Mayer; Dr. Fritz Will, and representatives from all of the National U.S. labs listed above. Overseas signatories include Dr. Ikegami, head of Japan's largest program at the National Institute, Dr. Srinivasan, head of India's major program, and many of the top workers from Italy.

We think that Cold Fusion is, at least, a major scientific revolution. Furthermore, we feel that it may well become a practical source of limitless, pollution free energy.

You may not be persuaded by the evidence, but as a journal of science, you should report that hundreds of credible, sincere scientists are working intensively to understand cold fusion. You should not ignore or belittle these people. Your job is to report the news objectively, not to filter or control it. Cold fusion is newsworthy if for no other reason than because large numbers of respected, high caliber scientists believe it is real, as you see from this Petition.

Sincerely,

Jed Rothwell

**Cold Fusion Research Advocates**  
2060 Peachtree Industrial Court, Suite 312-F  
Chamblee, Georgia 30341

Phone: 404-451-9890 Fax: 404-458-2404

December 8, 1991

Dr. Jonathan Piel, Editor  
Scientific American, Inc.  
415 Madison Avenue  
New York, NY 10017

Dear Dr. Piel,

Thank you for your brief, enlightening note. I shall copy it to all the other petition signatories.

One thing puzzles me. Langmuir lists six characteristics of 'pathological science,' but our statement that the "mechanism is not fully understood" is not among them:

1. Causative agents barely observable
2. Effect remains close to the limit of detectability
3. Claims of great accuracy
4. Fantastic theories contrary to experience suggested
5. Criticisms met by ad hoc excuses
6. The ratio of supporters to critics rises to 50% then falls

Which of these six do you have in mind, number four? We claim there is no accepted theory; surely that is not the same as claiming a fantastic theory. If you consider the lack of theory a hallmark of pathological science, are you expanding Langmuir's list? You condemn everything from high temperature superconductivity to human cognition, since these phenomena are not fully understood yet.

I strongly disagree with your last statement. Your coverage of Cold Fusion has been inaccurate and incomplete. You have made a catastrophic misjudgment, and your continuing refusal to reexamine the experimental evidence is nothing but close-minded unscientific obstinacy.

Sincerely,

Jed Rothwell

Rennie responded:

Date: 21 May 2003 16:52:50 -0400  
From: John Rennie <jrennie@sciam.com>  
Subject: Re: LENR-CANR.org  
To: Jed Rothwell <JedRothwell@mindspring.com>  
CC: Ed Storms <Storms2@ix.netcom.com>  
X-Mailer: QuickMail Pro 3.1 (Mac)  
Reply-To: John Rennie <jrennie@sciam.com>

Mr. Rothwell:

I'm not sure how you think letters from my long-gone predecessor will demonstrate "how irrational" I am, but you do what you feel is appropriate.

Moreover, I notice that although you called Jonathan Piel's decision "a catastrophic misjudgment" almost a dozen ago, the scientific mainstream would still side with him. Not bad as catastrophes go.

Your message is full of fresh vitriol but I'll repeat what I've said previously: it does you no good to curse Scientific American because the people you need to convince about the scientific credibility of cold fusion aren't journalists. They're professional physicists who review submissions for respectable technical journals. If you can convince mainstream scientists that LENR-CANR is real and significant, magazines like Scientific American will drop into line.

Good luck,

John Rennie

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John Rennie, editor in chief  
Scientific American  
415 Madison Ave.  
New York, NY 10017  
tel: 212-451-8813  
fax: 212-755-1976  
jrennie@sciam.com

My response:

You wrote:

I'm not sure how you think letters from my long-gone predecessor will demonstrate "how irrational" I am, but you do what you feel is appropriate.

I mean to establish how irrational your publication is, as an institution. Please note that you authored the article I referred to in the first letter. I presume you still agree with that article? If you have retracted I will note that fact in the document on LENR-CANR.org.

Moreover, I notice that although you called Jonathan Piel's decision "a catastrophic misjudgment" almost a dozen ago, the scientific mainstream would still side with him. Not bad as catastrophes go.

It resembles the *Scientific American's* previous record set in 1906, when you continued to dismiss & ridicule the Wright brothers three years after Kitty Hawk. You went along with the crowd then and refused to look at the evidence, just as you are doing now. (See attached.)

If you can convince mainstream scientists that LENR-CANR is real and significant, magazines like *Scientific American* will drop into line.

You can see from the publications in our Library that researchers did that years ago. The Japanese Journal of Applied Physics, for example, is considered the most prestigious journal in Japan. It devoted an issue to cold fusion and it has published several articles recently, including definitive work by Iwamura et al. (Mitsubishi). Cold fusion is attacked in the U.S. and the U.K., but not elsewhere.

Your message is full of fresh vitriol . . .

You are incorrect. Perhaps you are suffering from cognitive dissonance. I am mainly amused, and reconciled to your views. I only hope to make use of them to push you out of the way -- politically speaking! (I am a timid person who would never physically push anyone.) Some people in this field, such as the late Julian Schwinger, were upset and filled with vitriol. He wrote:

"The pressure for conformity is enormous. I have experienced it in editors rejection of submitted papers, based on venomous criticism of anonymous referees. The replacement of impartial reviewing by censorship will be the death of science."

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

You may think this issue is dead, but many people disagree. Readers have downloaded 110,000 papers from our website since we opened it on October 7, 2002. We are now distributing ~10,000 per week, or roughly one-third of your newsstand distribution, I believe. The Internet has given us a voice, despite your ridicule.

- Jed Rothwell

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"If such sensational and tremendously important experiments are being conducted in a not very remote part of the country, on a subject in which everyone feels the most profound interest, is it possible to believe that the enterprising American reporter, who, it is well known, comes down the chimney when the door is locked in his face even when he has to scale a fifteen-story skyscraper to do so would not have ascertained all about them and published them broadcast long ago?"

- The Wright Aeroplane and its Fabled Performances, Scientific American, January 13, 1906

Rennie responded promptly, in a remarkably open and forthright tone:

Subject: Re: LENR-CANR.org  
To: Jed Rothwell <JedRothwell@mindspring.com>  
CC: Ed Storms <Storms2@ix.netcom.com>

Mr. Rothwell:

If so much of the scientific community outside the U.S. and U.K. is supportive of LENR-CANR, it hardly seems necessary for you to try so hard to enlist Scientific American to publicize your cause. It is odd, though, that although we have editions and well-respected scientific contacts all around the world, I have never heard any of them request an article making the case for the phenomenon.

Perhaps you will say that you are not looking to enlist Scientific American, much as you say that your last message was not vitriolic (I must have been misled by your announced intention to ridicule us.) But then why bother to write to us at all? Leave us to suffer the ignominy of our catastrophic misjudgments without warning.

It's funny that you would cite the case of the Wright Brothers to show how misguided Scientific American has been in the past. Actually, Scientific American covered the development of aviation long before the Wrights' 1903 flight; I believe we had photos of the Wrights and their glider experiments in 1902. We enthusiastically published all about the attempts to develop heavier-than-air craft by the Wrights' contemporaries, both before and after the Kitty Hawk flights. So you couldn't honestly argue that Scientific American didn't believe in the possibility of airplanes--to the contrary, we saw planes as a technical challenge that somebody would eventually crack.

The Wrights were very secretive. Unlike many of their contemporaries, they didn't want to reveal many details about how their aircraft worked because they wanted to maintain a monopoly on certain aviation technologies. My hunch is that my predecessors in 1906 published their article in part as a goad to get the Wrights to reveal more to the world (if only the Wrights had published in peer-reviewed journals, for example....)

So to me the Wrights don't seem like a very good parallel to the case of LENR-CANR. In 1906 Scientific American (along with other publications and inventors) was extremely eager to learn more about what the Wrights had to offer, but the Wrights didn't want to reveal it. Today the LENR-CANR camp wants publicity, but Scientific American (along with most scientists) doesn't find the case very persuasive.

I don't imagine that any of what I just wrote will influence your decisions to draw such parallels, but I include it for your own information.

Anyway, good luck with your ongoing campaign to brush Scientific American aside (politically!). I could use a rest, and I imagine it will be peaceful in the dustbin of history. When I get there, can I have your seat?

Sincerely,

John Rennie

--

John Rennie, editor in chief  
Scientific American  
415 Madison Ave.  
New York, NY 10017  
tel: 212-451-8813  
fax: 212-755-1976  
jrennie@sciam.com

My response, the same day:

You wrote:

If so much of the scientific community outside the U.S. and U.K. is supportive of LENR-CANR, it hardly seems necessary for you to try so hard to enlist Scientific American to publicize your cause.

I am not trying hard. That would be futile. I appreciate the opportunity to communicate with you, and I appreciate your forthright expression of your opinions.

It is odd, though, that although we have editions and well-respected scientific contacts all around the world, I have never heard any of them request an article making the case for the phenomenon.

Schwinger, Fleischmann, Storms and other distinguished scientists told me they gave up trying to communicate with you years ago.

Perhaps you will say that you are not looking to enlist Scientific American, much as you say that your last message was not vitriolic (I must have been misled by your announced intention to ridicule us.)

I am not trying to ridicule you. I am publishing your words verbatim. I would like the world to see exactly what you think, what you have read about cold fusion, what you know about it, and how you formed your opinions. I want our readers to judge you by your own words. Some readers will agree with you. Others may find your statements ridiculous, or infuriating.

(By the way, the record does not have to be perfectly verbatim. If you would like to make changes, or add clarifications or footnotes to your previous notes, please let me know. E-mail is so easy a person sometimes lets mistakes slip through. I myself often use voice input, which produces peculiar, non-human errors!)

But then why bother to write to us at all?

I am writing to you in order to elicit the kind of response you have provided. You have done the field a great service by revealing your honest opinions and the extent of your knowledge.

It's funny that you would cite the case of the Wright Brothers to show how misguided Scientific American has been in the past. Actually, Scientific American covered the development of aviation long before the Wrights' 1903 flight; I believe we had photos of the Wrights and their glider experiments in 1902. We enthusiastically published all about the attempts to develop heavier-than-air craft by the Wrights' contemporaries, both before and after the Kitty Hawk flights. So you couldn't honestly argue that Scientific American didn't believe in the possibility of airplanes--to the contrary, we saw planes as a technical challenge that somebody would eventually crack.

The Wrights were very secretive.

These statements are incorrect. They are based on often-repeated myths. I suggest you review the "The Wright Brothers - An Authorized Biography" by Fred C. Kelly, or look up "Scientific American" in the index in T. Crouch, "The Bishop's Boys" (W. W. Norton, 1989). The claim that the Wrights were "secretive" is particularly risible, as pointed out by Kelly:

Dan Kumler . . . city editor Daily News, in Dayton, recalled in 1940 . . . that many people who had been on interurban cars passing the Huffman field and seen the Wrights in the air used to come to the Daily News office to inquire why there was nothing in the paper about the flights.

Such callers, said Kumler, got to be a nuisance.

And why wasn't there anything in the paper? Kumler was asked.

We just didn't believe it, he said. Of course you remember that the Wrights at that time were terribly secretive.

You mean they were secretive about the fact that they were flying over an open field?

I guess, said Kumler, grinning, after a moments reflection, the truth is that we were just plain dumb.

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

Unlike many of their contemporaries, they didn't want to reveal many details about how their aircraft worked because they wanted to maintain a monopoly on certain aviation technologies.

They were issued a patent in 1906. A patent must reveal all details or it will be ruled invalid. Their patent was upheld.

My hunch is that my predecessors in 1906 published their article in part as a goad to get the Wrights to reveal more to the world (if only the Wrights had published in peer-reviewed journals, for example....)

They did, starting in 1902. Also, reporters who visited them were free to watch flight tests, which were infrequent because flying was so dangerous. On days when no flights took place, reporters and potential investors were show equipment, photographs, a list of 60 local witnesses, and affidavits from a bank

president and other leading citizens of Dayton attesting to the Wright's claims. The *Scientific American* editors did not believe the Wrights because they never bothered to send anyone to Dayton, they never looked at any of this evidence, and they ignored letters from independent observers who had watched the flights.

So to me the Wrights don't seem like a very good parallel to the case of LENR-CANR. In 1906 *Scientific American* (along with other publications and inventors) was extremely eager to learn more about what the Wrights had to offer, but the Wrights didn't want to reveal it. Today the LENR-CANR camp wants publicity, but *Scientific American* (along with most scientists) doesn't find the case very persuasive.

With all due respect, I doubt that you have reviewed the literature closely. As far as I know, you have not published a critical review or any rigorous technical arguments explaining why you think these results are incorrect, nor have you cited any critical review written by someone else.

Writing a review takes months of hard work. As far as I know, neither you nor any other skeptic has published a paper showing errors in any major experiment. Therefore, you are expressing mere opinion, not a debatable or testable argument. Until you cite specific, detailed, original source, peer-reviewed papers, and you point out mistakes in the choice of instruments or techniques, you have no grounds for disbelieving cold fusion. You have no basis to form any opinion, positive or negative. You certainly have no grounds to mock it or dismiss it, as you did in the article I cited earlier.

As far as I know, only one scientist has published a technical paper that attempted to find fault in one set of experiments. I do not think he did a credible job. See:

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

Another negative report, the DoE ERAB report, was published while the first round of replications was getting underway, long before results were published, so it does not count. Also, some of the researchers the ERAB report described contacted the authors and told them the draft document misrepresented their work and failed to report positive results, but the authors ignored them. See:

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

I don't imagine that any of what I just wrote will influence your decisions to draw such parallels, but I include it for your own information.

You have been a great help. Thank you. By "parallel" I assume you mean to the Wright brothers history. I suggest you include information from Crouch. He is the Senior Curator of the Division of Aeronautics at the National Air and Space Museum, so he probably knows more about the Wrights than you do, and much more than I do.

- Jed Rothwell

Rennie responded in considerable detail:

Date: 22 May 2003 12:48:30 -0400  
From: John Rennie <jrennie@sciam.com>  
Subject: Re: LENR-CANR.org  
To: Jed Rothwell <JedRothwell@mindspring.com>  
CC: Ed Storms <Storms2@ix.netcom.com>  
X-Mailer: QuickMail Pro 3.1 (Mac)  
Reply-To: John Rennie <jrennie@sciam.com>

Mr. Rothwell:

Since I think history will show that my remarks hold up better than yours, I won't worry about looking ridiculous. (I must have gotten the misimpression that you wanted to ridicule us from your comment in your first message to me, "I think I will append these letters to our Appeal so that readers will see how irrational you are.")

The gist of your comments about Scientific American and the Wrights is that we didn't believe in their claims to have flown, and we couldn't be bothered looking at the evidence, right?

>The Scientific American editors did not believe the  
>Wright's because they never bothered to send anyone to Dayton, they never  
>looked at any of this evidence, and they ignored letters from independent  
>observers who had watched the flights.

You're mistaken. As I've mentioned previously, Scientific American covered the early work in aviation extensively, and anyone who looks the issues from that era will see just how true that was.

You're most especially wrong in trying to give the impression that Scientific American didn't credit the Wright's claims to have flown. For example, here is what we published in our December 29, 1903 edition, just a couple of weeks after the Kitty Hawk flight:

Scientific American  
December 26, 1903  
LXXXIX, No. 26  
page 486, Col. 3

A Successful Experiment with a Motor-Driven Aeroplane.

On December 17 the Messrs. Orville and Wilbur Wright made some successful experiments at Kitty Hawk, N.C., with an aeroplane propelled by a 16-horsepower, four-cylinder, gasoline motor, and weighing complete more than 700 pounds.

The aeroplane was started from the top of a 100-foot sand dune. After it was pushed off, it at first glided downward near the surface of the incline. Then, as the propellers gained speed, the aeroplane rose steadily in the air to a height of about 60 feet, after which it was driven a distance of some three miles [sic] against a twenty-mile-an-hour wind at a speed of about eight miles an hour. Mr. Wilbur Wright was able to land on a spot he selected, without hurt to himself or the machine. This is a decided step in advance in aerial navigation with aeroplanes, and it is probably due to the increased degree of controllability resulting from the Wright brothers' novel form of horizontal rudder, which is a small guiding aeroplane placed in front of, instead of behind, the aeroplane proper. A well illustrated description of the Wright aeroplane appeared in our February 22, 1902, issue. The present aeroplane has the very large surface of 510 square feet, making its apparent entire controllability all the more remarkable.

What then of the quotation from Scientific American that you cited, which seemed to cast doubt on the Wright's' claims? That was on a rather different matter from 1905 (not 1906 as you said--and of course, before the Wright patents were granted):

Scientific American  
Vol. XCIV  
No. 2  
January 13, 1905  
Page 40

#### The Wright Aeroplane and its Fabled Performance

A Parisian automobile paper recently published a letter from the Wright brothers to Capt. Ferber of the French army, in which statements are made that certainly need some public substantiation from the Wright brothers. In the letter in question it is alleged that on September 26, the Wright motor-driven aeroplane covered a distance of 17.961 kilometers in 18 minutes and 9 seconds, and that its further progress was stopped by lack of gasoline. On September 29 a distance of 19.57 kilometers was covered in 19 minutes and 55 seconds, the gasoline supply again having been exhausted. On September 30 the machine traveled 16 kilometers in 17 minutes and 15 seconds; this time a hot bearing prevented further remarkable progress. Then came some eye-opening records. here they are:

October 3: 24.535 kilometers in 25 minutes and 5 seconds. (Cause of Stoppage, hot bearing.)

October 4: 33.456 kilometers in 33 minutes and 17 seconds. (Cause of stoppage, hot bearing.)

October 5: 38.956 kilometers in 33 minutes and 3 seconds. (Cause of stoppage, exhaustion of gasoline supply.)

It seems that these alleged experiments were made at Dayton, Ohio, a fairly large town, and that the newspapers of the United States, alert as they are, allowed these sensational performances to escape their notice. When it is considered that Langley never even successfully launched his man-carrying machine, that Langley's experimental model never flew more than a mile, and that Wright's mysterious aeroplane covered a reputed distance of 38 kilometers at the rate of one kilometer a minute, we have the right to exact further information before we place reliance on these French reports. Unfortunately, the Wright brothers are hardly disposed to publish any substantiation or to make public experiments, for reasons best known to themselves. If such sensational and tremendously important experiments are being conducted in a not very remote part of the country, on a subject in which almost everybody feels the most profound interest, is it possible to believe that the enterprising American reporter, who, it is well known, comes down the chimney when the door is locked in his face--even if he has to scale a fifteen-story skyscraper to do so-- would not have ascertained all about them and published them broadcast long ago? Why particularly, as it is further alleged, should the Wrights desire to sell their invention to the French government for a "million" francs. Surely their own is the first to which they would be likely to apply.

We certainly want more light on the subject.

So the skepticism of Scientific American's editors was not about whether the Wrights flew (which the magazine acknowledged years before), it was about whether some of the sensational claims that the Wrights were making about the distances that they had flown were credible, given their financial interests. Not the same at all. If you publish the Scientific American excerpt in full and in context, suddenly it doesn't look so ridiculous, does it?

The editors of Scientific American were right to be skeptical about such poorly documented claims at the time, just as its editors today are right to be skeptical of mountains of cold fusion "evidence" that somehow fail to convince most physicists that the phenomenon is real and significant.

You also wrote:

>With all due respect, I doubt that you have reviewed the literature  
>closely. As far as I know, you have not published a critical review or any  
>rigorous technical arguments explaining why you think these results are  
>incorrect, nor have you cited any critical review written by someone else.  
>Writing a review takes months of hard work. As far as I know, neither you  
>nor any other skeptic has published a paper showing errors in any major  
>experiment. Therefore, you are expressing mere opinion, not a debatable or  
>testable argument. Until you cite specific, detailed, original source,

>peer-reviewed papers, and you point out mistakes in the choice of  
>instruments or techniques, you have no grounds for disbelieving cold  
>fusion. You have no basis to form any opinion, positive or negative. You  
>certainly have no grounds to mock it or dismiss it, as you did in the  
>article I cited earlier.

These arguments seem to form the core of your reasons for thinking that Scientific American is remiss in not writing about LENR-CANR, or in doing so briefly and critically. But they rest on two misconceptions.

The first one is apparently a misconception about how scientific method works. You are claiming that unless we (or, more properly, mainstream physicists) establish a technical basis for disbelieving claims of LENR-CANR, we have no basis for dismissing it. But it is not up to mainstream physicists to disprove LENR-CANR; it is up to LENR-CANR's physicists to come up with convincing proofs. The burden of evidence is on those who wish to establish a new proposition.

The second misconception concerns Scientific American's function. We're journalists here at the magazine, even those of us with scientific credentials. We don't claim to be authorities on physics or any other discipline (for all that there is quite a lot of real expertise built into our staff). For that reason, the scientific points of view we choose to publish are ones that have already been vetted in the technical, peer-reviewed literature and that generally seem to represent a consensus within the scientific community. To do otherwise would mean that we were pretending to be more expert than the actual scientific authorities. That is not what Scientific American's readers want or expect.

(As for whether we're entitled to mock cold fusion...well, sorry if you disagree, but that opinion reflects the consensus of most scientists, too.)

So it really doesn't make a difference to me if LENR-CANR advocates petition me for articles on the subject; I'll put them on the stack of similar requests from the scientific creationists, the global warming deniers the face-on-Mars people, the crypto-archaeologists, and all the others who want publicity and scientific respectability but can't make their case convincingly to the community of scientists. But I'll say this again, too: if LENR-CANR's physicists can convince the mainstream physics community that they've got a credible case and articles to that effect start appearing in major peer-reviewed journals, Scientific American would be glad to write about it.

You and I have been over this several times already in this correspondence, and there's really nothing more to add, so you'll have to excuse me if I bow out of these exchanges hereafter. I can use the time more productively to put out a magazine, and you can use the time more productively to...well, I'm sure there's something. You'll undoubtedly want the last word, and you're welcome to it.

John Rennie

--

John Rennie, editor in chief  
Scientific American  
415 Madison Ave.  
New York, NY 10017  
tel: 212-451-8813  
fax: 212-755-1976  
jrennie@sciam.com

I responded:

You wrote:

Since I think history will show that my remarks hold up better than yours, I won't worry about looking ridiculous. (I must have gotten the misimpression that you wanted to ridicule us from your comment in your first message to me, "I think I will append these letters to our Appeal so that readers will see how irrational you are.")

That is not the same as ridiculing you. I take your point of view seriously, and I mean to expose it in as much detail as you will permit, so that the readers can judge for themselves.

Please note I am quoting your own words, in full. In contrast, you have ridiculed cold fusion, and as far as I know you have not published a single paper authored by any of the cold fusion researchers themselves, describing their own work, in their own words. I have not seen one paragraph, diagram, or data graph taken from any peer-reviewed cold fusion paper in your magazine. You have published only statements made by people who strongly oppose cold fusion, and who apparently have not read the literature, since they have not cited any technical details to bolster their opinions. This seems extreme to me.

The gist of your comments about Scientific American and the Wrights is that we didn't believe in their claims to have flown, and we couldn't be bothered looking at the evidence, right?

Please note those comments were made by Orville Wright, Fred Kelly and Tom Crouch. They are the leading experts in the history of the Wrights. I am merely quoting and citing them.

>The Scientific American editors did not believe the  
>Wright's because they never bothered to send anyone to Dayton, they never  
>looked at any of this evidence, and they ignored letters from independent  
>observers who had watched the flights.

You're mistaken.

Not me. Perhaps Wright et al. were mistaken, but I think they present compelling evidence.

As I've mentioned previously, Scientific American covered the early work in aviation extensively, and anyone who looks the issues from that era will see just how true that was.

You're most especially wrong in trying to give the impression that Scientific American didn't credit the Wright's claims to have flown. For example, here is what we published in our December 29, 1903 edition, just a couple of weeks after the Kitty Hawk flight:

Scientific American  
December 26, 1903  
LXXXIX, No. 26  
page 486, Col. 3

A Successful Experiment with a Motor-Driven Aeroplane.

On December 17 the Messrs. Orville and Wilbur Wright made some successful

experiments at Kitty Hawk, N.C., with an aeroplane . . .

I read this, and so did Wright, Kelly and Crouch. Many other journals and newspapers took note of the Kitty Hawk flights. However, in subsequent issues of these publications, editors and reporters said they doubted the Wrights reports from Kitty Hawk, and they did not believe the reports of flights in Dayton in 1904 and 1905. They claimed the Wrights were being secretive. They did not bother to send reporters to check the facts, or look at the photographs, affidavits and so on. The Scientific American and others accorded similar treatment to Pons and Fleischmann in 1989. First they reported the claims in a reasonable, evenhanded way. Then, six months later, just as much better data was becoming available from replications, they turned against the subject, and refused to publish any news of the replications.

It seems that these alleged experiments were made at Dayton, Ohio, a fairly large town, and that the newspapers of the United States, alert as they are, allowed these sensational performances to escape their notice. . . .

So the skepticism of Scientific American's editors was not about whether the Wrights flew (which the magazine acknowledged years before), it was about whether some of the sensational claims that the Wrights were making about the distances that they had flown were credible, given their financial interests.

This skepticism had no rational basis. Any time after the summer of 1904, the Scientific American editors could have sent a letter or telegram to a technically competent person in Dayton (a stringer), asking him to verify the claims. Many people visited the Wrights on such missions, and the Wrights were unfailingly polite, informative and convincing. The Scientific American was inexcusably negligent not to send someone. It is far more negligent today, because it is unwilling to publish even one paragraph describing important research published by prestigious Japanese journals of physics, by China Lake, Los Alamos, and others.

Not the same at all. If you publish the Scientific American excerpt in full and in context, suddenly it doesn't look so ridiculous, does it?

With all due respect, it looks just as bad to me. Orville Wright himself thought it was outrageous, and I agree with him.

The editors of Scientific American were right to be skeptical about such poorly documented claims at the time, just as its editors today are right to be skeptical of mountains of cold fusion "evidence" that somehow fail to convince most physicists that the phenomenon is real and significant.

This statement is not in evidence. You have not offered any technical critique showing mistakes in Iwamura's paper the Japanese Journal of Applied Physics (JJAP), for example, or any other paper. Perhaps you have such detailed knowledge, but you have not revealed it to me, or to Iwamura. So as far we can tell, you have no business judging or rejecting his work. Waving your hands, putting quotes around the word "evidence," and citing other people's unsupported opinions does not constitute a scientifically valid, rigorous technical rebuttal. If you disagree with Iwamura, you must present your case with as much rigor as the JJAP editors demanded from him.

>fusion. You have no basis to form any opinion, positive or negative. You  
>certainly have no grounds to mock it or dismiss it, as you did in the  
>article I cited earlier.

These arguments seem to form the core of your reasons for thinking that Scientific American is remiss in not writing about LENR-CANR, or in doing so briefly and critically. But they rest on two misconceptions.

The first one is apparently a misconception about how scientific method works. You are claiming that unless we (or, more properly, mainstream physicists) establish a technical basis for disbelieving claims of LENR-CANR, we have no basis for dismissing it. But it is not up to mainstream physicists to disprove LENR-CANR; it is up to LENR-CANR's physicists to come up with convincing proofs. The burden of evidence is on those who wish to establish a new proposition.

I disagree, and so do many distinguished scientists such as Schwinger. We think that all points of view must be held to the same rigorous standards. If you will not give me detailed, specific reasons why you reject experiments, I cannot argue or rebut your point of view. Your views are not falsifiable, and therefore they are outside the domain of science. All arguments must be backed up by detailed knowledge of the experiments, with reference to the established laws of physics. A skeptic should not get a free ride just because he holds the majority view. People who reject cold fusion reject the laws of thermodynamics. They are saying, in effect, the instruments and techniques perfected by J. P. Joule in the 1840s do not work. These same instruments and techniques are used today to prove that cold fusion produces heat beyond the limits of chemistry. (Thermocouples replace mercury thermometers, and computers are used instead of manual logging, but in other respects many calorimeters have not changed.) The skeptical view is a radical reinterpretation of conventional, textbook physics. It should not get a free pass just because one must hold it in order to reject cold fusion.

More to the point, if this one-sided standard were put in place generally, progress in science would grind to a halt. Most major breakthroughs in science were opposed by the establishment. It seems that even smart people often instinctively reject innovations. For example, in his autobiography, Charles Townes says Rabi, Kusch, Thomas, Bohr and von Neumann and other important scientists were convinced that the maser would never work. Rabi and Kusch called him in to a meeting and said: "Look, you should stop the work you are doing. It isn't going to work. You know it's not going to work. We know it's not going to work. You are wasting money. Just stop!" - C. Townes, "How the Laser Happened," (Oxford, 1999), p. 65.

The second misconception concerns Scientific American's function. We're journalists here at the magazine, even those of us with scientific credentials.

In that case, I suggest you stop taking sides and ridiculing distinguished scientists. Stop comparing them to creationists. Experimental electrochemistry does not begin to compare to creationism, or to real evolutionary biology, for that matter. Cold fusion is not based on theory or speculation. It is based on high sigma replicated data, from calorimetry, mass spectroscopy, autoradiographs and so on.

We don't claim to be authorities on physics or any other discipline (for all that there is quite a lot of real expertise built into our staff). For that reason, the scientific points of view we choose to publish are ones that have already been vetted in the technical, peer-reviewed literature and that generally seem to represent a consensus within the scientific community.

No consensus exists yet. There has been no debate. The people who oppose cold fusion have not published papers supporting their arguments. Until they publish something, the only point of view you can read about or publish is that of the cold fusion researchers.

To do otherwise would mean that we were pretending to be more expert than the actual scientific authorities.

You seem to be pretending you know more than Melvin Miles or Julian Schwinger, even though you have not published a paper or article about their work!

That is not what Scientific American's readers want or expect.

(As for whether we're entitled to mock cold fusion...well, sorry if you disagree, but that opinion reflects the consensus of most scientists, too.)

As I said, no such consensus exists, or can exist, because the opposition has not published a falsifiable argument yet. Furthermore, most scientists have not read the literature. You cannot balance the opinion of someone like McKubre, Storms or Miles who has worked for decades on electrochemistry, and who has read hundreds of papers, against a physicist in an unrelated field who has read no papers at all, and can cite no evidence in support of his view. General knowledge of science cannot give a person insight into a paper he has not read. Science cannot be done by ESP. It is not a popularity contest, or an election. The only opinions that count are those backed by rigorous research and deep knowledge of the subject.

So it really doesn't make a difference to me if LENR-CANR advocates petition me for articles on the subject; I'll put them on the stack of similar requests from the scientific creationists, the global warming deniers the face-on-Mars people, the crypto-archaeologists, and all the others who want publicity and scientific respectability but can't make their case convincingly to the community of scientists.

The difference is that LENR-CANR scientists have made a convincing case by publishing rigorous papers in some of the world's leading peer-reviewed journals, and these papers are based on long-established thermodynamic principles. You say they have not published, but I have given you a long list of titles proving they have. You can verify the thermodynamics yourself, in any cold fusion paper on calorimetry. Waving your hand and declaring "X is like Y" does not actually make X resemble Y.

- Jed Rothwell

Here is a telling, final note. While composing the above message, I accidentally sent a rough draft. I wrote:

From: Jed Rothwell <JedRothwell@mindspring.com>  
Oops! Please disregard previous message. Still working . . .

Excuse me. I did not mean to send that last message yet. I was still running a spell & grammar check in Microsoft Word, and I wanted to add in an interesting quote from Charles H. Townes. Please disregard that version, especially the embarrassing typos and run-on sentences!

- JR

Rennie responded characteristically:

Date: 22 May 2003 14:37:45 -0400  
From: John Rennie <jrennie@sciam.com>  
Subject: Re: Oops! Please disregard previous message. Still working . . .  
To: Jed Rothwell <JedRothwell@mindspring.com>  
X-Mailer: QuickMail Pro 3.1 (Mac)  
Reply-To: John Rennie <jrennie@sciam.com>

Jed,

Don't worry about it. I'm disregarding all your messages. Bye.

--

John Rennie, editor in chief  
Scientific American

Rennie has personalized the issue, as skeptics often do. He reduces it to a one-on-one, tit-for-tat quarrel. He refuses to address any substantive technical issue, or to muster a falsifiable argument in support of his views. One can only conclude that he has not read any papers, or he cannot understand them. (A reader might suspect that I do not understand them either, but you can find out. You can give me a pass/fail grade by reading some of my own work, such as my review of McKubre.) The problem is not that Rennie disregards me. I have no standing in the scientific community, and there is no reason he should listen to me. The problem is that he disregards Iwamura, Storms, Miles, Schwinger, the editors of the Japanese Journal of Applied Physics and hundreds of other distinguished experts. He cannot distinguish between such people and "creationists, the global warming deniers the face-on-Mars people, the crypto-archaeologists." He is stuck in a time warp, back with Robert Park in 1991. Park, a spokesman for the American Physical Society, described cold fusion in *The Washington Post*:

"If everyone knows it is wrong, why are they doing it? Inept scientists whose reputations would be tarnished, greedy administrators . . ., gullible politicians who had squandered the taxpayers' dollars, lazy journalists . . . -- all had an interest in making it appear that the issue had not been settled. Their easy corruption was one of the most chilling aspects of this sad comedy. To be sure, there are true believers among the cold-fusion acolytes, just as there are sincere scientists who believe in psychokinesis, flying saucers, creationism and the Chicago Cubs... A PhD in science is not inoculation against foolishness -- or mendacity."

After 14 years, the only arguments opponents can muster against cold fusion are vituperative comparisons with creationism.

Jed Rothwell  
May 22, 2003