Fax to Facts:
Cold Fusion and the History of Science Information

Bruce V. Lewenstein

Abstract

Debates about the management of scientific information have traditionally assumed that information is generated first in scientific laboratories or field sites, becomes "real" or stable when it appears in peer-reviewed journals, and is then further disseminated through textbooks, encyclopedias, trade journals, government reports, mass media stories, and the like. Classic texts—from the proceedings of the 1948 Royal Society Scientific Information Conference and the 1958 International Conference on Scientific Information, through such modern texts as the annual proceedings of ASIS meetings—focus on how to classify and retrieve "real" scientific information, that is, how to retrieve laboratory and field reports that have been produced for and vetted by the peer review system. In this paper I use the cold fusion saga of the late 1980s and early 1990s to suggest that communication among scientists uses many more media than traditionally have been assumed. This particular historical episode suggests that we need to develop new models of the science information process, ones that account for permeable boundaries between formal publications, preprints, electronic computer networks, fax machines, mass media presentations, and other forums for scientific discussions.

Cold Fusion and the History of Science Information

Science has often served as the impetus for the analysis and improvement of information systems, perhaps symbolized best by Watson Davis. Trained as an engineer, for much of his career, Davis ran Science Service, a news bureau that provided science information to the public. He also helped create the international science fair system for youth. His experience in trying to stay on top of the burgeoning flow of specialized science information in the first half of this century led him to look for new ways of managing information, and he claimed to have coined the term *microfilm*. More important, he was a founder of the American Documentation Institute, forerunner of the American Society of Information Science (Lewenstein, 1988).

Another key figure in the general field of information science has been Eugene Garfield. While most scientists know him through the products of the company he founded and built, the Institute for Scientific Information (ISI), *information* scientists see beyond the specific ISI products to the fundamental insights into information management that he provided through his creation of citation indexing. The entire bibliometric field owes its origins to Garfield (Garfield, 1955, 1977–93).

Even today, as ASIS conferences devote special sessions to the challenges of managing information on the World Wide Web, such science-based topics as health provide most of the case studies.

But much of the work on information systems has drawn artificially sharp distinctions between primary science information—that is, original reports of specific research projects—and secondary information, such as media reports, textbooks, and government reports. (The Web is clearly an exception but a very recent one.) The key challenge in the field has often been seen as trying to serve both the producers and the users of primary information (who are often, of course, the same people). So, for example, if one looks up *science information* in the library, one finds lots of work on the management of peer-reviewed journals, preprint systems, and the like. Key founding texts in the field, such as reports of the Royal Society's Scientific Information Conference of 1948 and of the National Academy of Science's International Conference on Scientific Information a decade later, as well as NAS's 1969 report on scientific and technical communication, focus on the information use of primary scientific researchers. Key chapters have such titles as "Explorations on the Information Seeking Style of Researchers" or "Primary Communications," with sections on meetings, preprints, serials, and translations (National Academy of Sciences, 1959; Committee on Scientific and Technical Communication, 1969; Royal Society, 1948). Many of the theoretical models developed during this period, especially ideas about "invisible colleges" and reward systems and the like, focus on issues of information management within the world of primary scientific research (Crane, 1972; Hagstrom, 1965).

While that tradition of research has certainly reflected the reality of what most scientists mostly do on a day-to-day basis, it has presumed linear models of both science and communication. That is, the research has focused on the communication patterns within scientific research communities as if information is created there and then flows in a single direction, out to textbooks, industry, government, and the general public. Linear patterns have a long history in communication research; perhaps the best known is what we now call the "source-message-channel-receiver" model, first presented by the telephone engineer Claude Shannon and the mathematician Warren Weaver in 1948 (Shannon & Weaver, 1949). But today communication researchers consider such linear models to be outmoded. They suggest instead that we should focus on the interaction of multiple sources of information and on the way that meanings are shaped by the interactions.

In this paper I want to suggest that we need to reconceptualize scientific information systems in the same way; that is, we need to develop new models for science information systems that capture the complexity of communication interactions that shape science. To illustrate the need for new models, I will use the cold fusion saga that began (in a public way) in 1989.

The Cold Fusion Saga and Traditional Science Communication Models

The problem of relying on traditional models of science communication appears as soon as one tries to make sense of the cold fusion saga. From the moment of the initial press conference at the University of Utah on 23 March 1989, through the daily dispatches in newspapers around the world, to the widely quoted labeling of B. Stanley Pons and Martin Fleischmann as suffering from "incompetence and delusion," the mass media had a central place in the development of the science (Lewenstein, 1995b). Not only did the media inform the public about the development of a new area of scientific research, but for many scientists, the media also provided the forum for primary dissemination of technical information on a fast-moving research front. Unfortunately, traditional studies of science information provide little guidance for understanding how the media's presence in the debates affected the construction of cold fusion as a research area. Studies of the media's role have focused on issues of accuracy, balance, sensationalism, and relevance to the public. The inadequacies of this approach have been identified by a variety of researchers, who point to the essential similarities among all discourse that involves science. They also point to the assumption that science is only about "progress," only about a closer approximation to Truth, that underlies most analysis of science journalism; little research on science journalism looks at issues of trust, institutional authority, or other aspects of the social context of science (Dornan, 1988, 1990; Friedman, Dunwoody, & Rogers, 1986; Hilgartner, 1990; Krieghbaum, 1963, 1967; Nelkin, 1985; Shinn & Whitley, 1985).

The fundamental problem appears to be that traditional studies of science and the media are based on an outdated model of science information. During the 1960s, when sociologists and others developed the idea that "communication" is a fundamental part of science, the unidirectional, nonfeedback model of communication suggested by Shannon and Weaver was the most readily available theory (Berlo, 1960; Merton, 1973; Hagstrom, 1965; Cole & Cole, 1973; Ziman, 1968). It was in this context that thorough empirical studies of science communication were conducted from the late 1960s through the 1970s. Run by the psychologist William Garvey and a number of colleagues and informed by citation analysis, these studies provided a detailed description of the formal publication processes that scientific ideas go through as they move from the laboratory or blackboard into the realm of fixed and stable knowledge (Figure 1) (Nelson & Pollack, 1970; Garvey, 1979). Although Garvey and his colleagues did not cite the communication literature directly, the model they produced is clearly compatible with the linear dissemination-oriented SMCR model that had emerged in communication studies.

Beginning in the mid-1970s, sociologists of science began to react against the notion that scientific knowledge could be studied only as a privileged type of knowledge. Instead sociologists and anthropologists began to examine the everyday practices of scientists as they produced knowledge, and they questioned the idea that science is "created" in one sphere and then

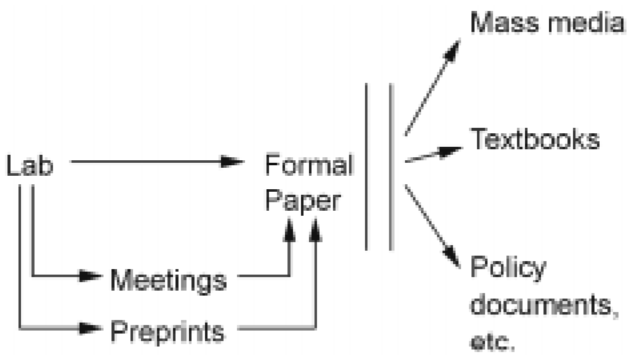

Figure 1. The formal communication system, as defined in the 1970s. Based on a diagram in William D. Garvey, Communication: The Essence of Science—Facilitating Information Exchange among Librarians, Scientists, Engineers and Students *(Oxford/New York: Pergamon Press, 1979), p. 169.*

disseminated in another, with distortion being an inevitable accompaniment of the dissemination. Instead researchers began to talk about expository science, emphasizing the way in which scientific information is shaped by the various audiences to which it can be addressed. At its core this new tradition argues that scientific knowledge does not exist in any abstract form but takes on shape and meaning only when it is expressed in specific contexts and addressed to specific audiences. According to this argument, a technical paper presented at a small workshop is no more "science" than is a multimedia extravaganza presented on an IMAX screen or at Disney World's EPCOT Center. Both are attempts to use rhetoric to present understandings of the natural world to particular audiences (Barnes, 1974; Bloor, 1976; Mulkay, 1979; Ravetz, 1971; Latour & Woolgar, 1979; Shinn & Whitley, 1985).

How does this newer view of the creation of science relate to cold fusion and the history of science information? It suggests that the reason analyses of cold fusion that look only at media coverage are unsatisfying is that they are based on an improper, or at least incomplete, understanding of the communication contexts in which the media reports appear. Thus, in what follows, I will provide a history of cold fusion that integrates media reports into the overall communication patterns that shaped the cold fusion saga. By doing so, I will show that the media's role in cold fusion can be understood only by reconceptualizing our models of science information flow.

The Public History of Cold Fusion

The public history of cold fusion began on 23 March 1989, when B. Stanley Pons and Martin Fleischmann announced at the University of Utah that they had found a way to produce nuclear fusion at room temperature in a small, relatively simple apparatus. Both the public and most other scientists first learned of Pons and Fleischmann's work through the mass media, by hearing breathless, excited reports on television and the radio (Cornell Cold Fusion Archive [CCFA], 1989a, March 23). Some scientists and members of the public had already read stories in the *Wall Street Journal* and *Financial Times* (of London), which both ran stories on the morning before the press conference (Bishop, 1989, March 23; CCFA, 1989c, March 23). The *Wall Street Journal*'s coverage was especially important because the next day it identified Steven Jones, the competitor at Brigham Young University who was doing work similar to Pons and Fleischmann's and whose activities were probably the stimulus that caused Pons and Fleischmann to go public when they did (Bishop, 1989, 24 March).

In the decade since that announcement cold fusion has gone through roughly four distinct periods (Figure 2) (Lewenstein & Baur, 1991; Lewenstein, 1992; Close, 1991; Huizenga, 1992; Mallove, 1991; Taubes, 1993). The first period, lasting about two months, appeared to many participants and observers as utter chaos (in the everyday, nonspecialist sense of that word). Claims and counterclaims changed almost daily; special cold fusion sessions were attached ad hoc to regular scientific meetings; stories with new and conflicting information appeared in newspapers, on the radio, on television, and on a newly created computer bulletin board. In the second period, through the summer and fall of 1989, much of the chaos disappeared, and the nature of the claims became clearer. Several special panels devoted to cold fusion issued reports; researchers identified topics of interest to them in the field; and for the most part public and scientific interest in the topic died off. The history since 1993 is less well covered, but can be followed on computer bulletin boards like the USENET newsgroup sci.physics.fusion and the Web sites http://www.mv.com/ipusers/zeropoint/ and http://world.std.com/~mica/cft.html. Eugene Mallove also publishes a cold fusion magazine, *Infinite Energy*, which contains much information on the continuing work by cold fusion believers.

In the third period, lasting throughout 1990, the sharp division between skeptics (or nonbelievers) and believers (as they were frequently labeled) became more prominent. On the first anniversary of the announcement the scientific journal *Nature*, home of the most prominent skeptics, published a scathing critical analysis of the situation in Pons and Fleischmann's own laboratory (Salamon et al., 1990). That same week believers gathered in Salt Lake City for the first Annual Cold Fusion Conference, sponsored by the Utah-funded National Cold Fusion Institute (Will, 1990). Later in the year the journal *Science* published a news article that came very close to accusing some cold fusion researchers of fraud (Taubes, 1990). In October, trying to tread a middle ground between belief and skepticism, Steven Jones organized a conference on "anomalous effects in deuterium/solid systems" at Brigham Young University; Pons and Fleischmann did not attend.

At the beginning of 1991 the division between skeptics and believers was vividly represented by the publication of two books on cold fusion with diametrically opposite evaluations of the state of the research field. Physicist Frank Close's *Too Hot to Handle* avoided accusing Pons and Fleischmann of fraud only by leaving open the possibility of sloppy incompetence, whereas Eugene Mallove's *Fire from Ice* predicted that cold fusion–powered home heaters were just around the corner. After that, in the fourth period, the two sides continued on their way, largely ignoring each other's critiques. Although a few skeptics (including Close, nuclear chemist John Huizenga, and nuclear physicist Douglas Morrison) continued to speak out against what they saw as the fraud and error of cold fusion supporters, most critics had long abandoned the field. Supporters, on the other hand, continued to meet: In addition to various regional gatherings, international meetings were held in Como, Italy (1991); Nagoya, Japan (1992); Maui, Hawaii (1993); Monte Carlo (1995); Hokkaido, Japan (1997); and Vancouver, British Columbia (1998). In essence, a new social group—a scientific subspecialty—had been created.

Communication and Chaos

In the first period chaos reigned. More accurately, information passed so quickly and permeably among multiple sources and multiple media that many participants recalled in interviews the sense of being completely inundated by information, without being able to judge the relative value of individual pieces of news or gossip. The interchangeability of media is particularly noticeable when we look at a basic information issue: how people heard about cold fusion. For example, then-MIT science writer Eugene Mallove, only a few months after the original announcement, could not recall whether he was in his office and his boss called him or he was out of the office and his boss told him when he checked in for the day (CCFA, 1989, November 8). Steve Koonin, a theoretical physicist at Caltech who was visiting Santa Barbara for a year, recalled who told him about cold fusion, but he did not remember whether the information came by electronic mail or telephone (CCFA, 1989, November 16). These confusions suggest that we need to be careful about focusing too closely on any one communication channel, without recognizing that users of those channels may not distinguish among them very carefully. Given the well-known phenomenon that people can recall precisely the circumstances in which they heard dramatic news, these examples may be anomalies. Other cold fusion participants recall with greater certainty how they heard of the new claims.

Another aspect of the complex flow of information was the degree to which various communication media began interacting within a day of the original press conference. In one example a science correspondent for National Public Radio used electronic mail to get interpretation of information that he had documented via audiotape. (The sci.physics bulletin board is one of thousands of bulletin boards available through the USENET

Cold Fusion Saga, 1989–1998

23 March 1989: *Public announcement*	
April–May 1989: *Media and scientific chaos*	
12 April	ACS/Dallas
26 April	U.S. Congress hearings
1 May	APS/Baltimore
23 May	Santa Fe conference
Summer–Fall 1989: *Growing stability*	
15 June	Harwell rejection
13 July	Interim DOE/ERAB report
15 October	NSF/EPRI panel
12 November	Final DOE/ERAB report
1990: *Consolidation of positions*	
29 March	1st NCFI CF Conference
15 June	*Science* charges fraud
22 October	BYU conference on anomalous effects
1991–1998: *Ongoing work, two separate strands*	
January 1991	Pons resigns from University of Utah
Spring 1991	Close & Mallove books
June 1991	2nd CF conference, Italy
January 1992	Riley killed at SRI
October 1992	3rd CF conference, Japan
December 1993	4th CF conference, Hawaii
April 1995	5th CF conference, Monaco
Fall 1996	6th CF conference, Japan
Spring 1998	7th CF conference, Vancouver

Figure 2. Major points in the cold fusion saga timeline.

computer network, a worldwide collection of "newsgroups" used in the early 1990s by at least 1.4 million people. The Internet has, of course, grown rapidly since then.) For other observers videotape was more important; researchers phoned the University of Utah, requesting copies of a videotape showing the press conference, or watched copies of the television shows that had run extensive stories on the announcement, despite the fact that the level of detail in these programs was not high (CCFA, 1989b, March 23). Some researchers turned to the actual press release for more information, but they did not find much: "In the experiment, electrochemical techniques are used to fuse some of the components of heavy water, which contains deuterium and occurs naturally in sea water" (CCFA, Press Release, Fogle folder, 1989).

Instead researchers found themselves turning the mass media into a source for technical data: "We used photographs from the *L[os] A[ngeles] Times* of Pons holding the cell, and you could see pretty well how it was made," said Michael Sailor, a Caltech postdoctoral student in electrochemistry. "We used Pons's finger for a scale. Gordon [Miskelly, another postdoc] figured his hand was about equal-sized, so he scaled it to his own finger." Another Caltech student brought in the videotapes. "We looked at them to find out what the readings on their thermistors were, where the electrodes were, and how they were doing their electrochemistry," said Nathan Lewis, professor of electrochemistry at Caltech (Smith, 1989).

The traditional models of science communication, by focusing on peer-reviewed publications, assume that scientists work with stable, certain information. But the cold fusion saga, like so many controversies, opens up the inner workings of science and lets us see the daily workings of science in greater detail. As in any fast-moving area of science, researchers lacked access to a fixed, stable piece of information (a preprint or published article); so many scientists began exchanging rumors, newspaper articles, and so on. Faxed copies of newspaper articles from distant countries, accompanied by handwritten comments on the article or on other developments, soon circulated widely (CCFA, Manos folder). The combination of newspaper, fax, and interpersonal communication all shaped the meaning of any one particular piece of information. Attempting to sort out the impact of each component would do injustice to the complex context of communication.

After the first week scientists and reporters began to receive preprints and then reprints of various technical articles (CCFA, Preprints folders). Not only did researchers need to acquire, read, and process the information in each of these texts, they also had to compare them—especially the differences between the early manuscripts and the final published articles. Although the process of sorting out the differences and making judgments about the multiple texts would eventually lead to greater stability of information, many researchers recalled in interviews that the need to first resolve *which* version someone was talking about contributed to the sense of chaos or instability.

An important issue concerning access soon emerged: Different people had different levels of access to information. By the time preprints and publications began to get wide circulation in late April, some people had had access to them for almost a month. For example, Richard Garwin, a physicist at IBM, had been asked by *Nature* to referee both a manuscript from Fleischmann and Pons and the Jones manuscript around the first of the month. In late April, Garwin's own summary of a one-day cold fusion conference in Erice, Sicily, appeared in *Nature*, concluding that "large heat release from fusion at room temperature would be a multidimensional revolution. I bet against its confirmation." But while this summary appears, in the text, to be based solely on the presentations at Erice, and while Garwin was careful not to cite his privileged access to the original manuscripts, the extra several weeks he had to consider information undoubtedly shaped his analysis. (In addition, of course, Garwin [1989] knew that the information to which he had access was direct from the main protagonists rather than filtered through mass media reports or other communication media.)

To understand the importance of Garwin's privileged access, recall that his article was one of the first to reach print. Not until the following issue of *Nature* was Jones's article published, along with commentaries by several other scientists. Readers no doubt made some judgments about the relative importance of information in speculative letters, Garwin's meeting report, and Jones's complete article. Communication theory suggests, however, that those judgments are extremely complex and not likely to be directly related to "objective" measures of the relative importance assigned to each publication. People take in lots of information, filter it in various ways, and base their judgments on a range of issues running from salience and importance through time of day and state of hunger. In the case of cold fusion readers had to judge the value of suggestions published by prominent scientists (Nobel laureate Linus Pauling published a letter early on, for example) versus letters from physicists and chemists in Utah (who, to outsiders, might be presumed to have more detailed local knowledge). Theory suggests that each reader would make a different judgment, based on completely contingent factors. No model attempting to predict the value of different types of communications works (Bryant & Street, 1988; Dervin, 1989).

Another factor that made it more difficult for researchers and others trying to make stable judgments about cold fusion was the presence of new or unusual patterns of information flow. Some members of the media, for example, agreed to serve as brokers in the information exchange among scientists. Those activities went beyond merely passing around copies of papers and negotiating access to information. Sometimes reporters acted explicitly as mediators among scientific sources. David Ansley, a reporter for the *San Jose Mercury News*, recalled that:

> At one point, I called up [University of Utah vice president for research James] Brophy and said "Look, this is making no sense. You say that all it takes is the simple description and that other researchers ought to be able to duplicate it. . . . [But] here are the questions they're asking me. Can you answer any of these questions?" And he would give me the answers. I would call [the researchers] back, and they would say "That's so simplistic. That's just not enough. We need X, Y, and Z. The way he's describing that doesn't do us any good." I'd call [Brophy] back, and he'd say, "No, really, that's how it works. It's that simple." (CCFA, 1989, November 18; see also CCFA 1989, August 11 & July 12)

For those people following the rapidly expanding electronic bulletin boards, the mix of media also applied. By the beginning of April a separate newsgroup, completely devoted to cold fusion, called "alt.fusion" was created. Early messages ranged from personal summaries of a seminar given by Fleischmann at CERN, to brief snippets announcing that "CBS News is reporting that the Pons-Fleischmann experiment has been reproduced in Hungary," to speculations about the potential impact of cold fusion on oil prices and the world economy (CCFA, e-mail file).

Thus, no matter where researchers and others trying to find stable information turned to stay informed, the barrage of conflicting material about cold fusion led to what the media frequently called "fusion confusion." The sense of instability caused by frequently changing judgments was reflected in newspaper coverage. At the *Los Angeles Times*, experienced science writer Lee Dye wrote on 19 April that Pons and Fleischmann were receiving a "flood of support"; two days later he said that "evidence continued to mount in support of the controversial experiment." Yet just two days after that, on 23 April, he began a story by noting that "scientists at major research institutions throughout the country are growing increasingly frustrated over their inability to replicate a supposedly simple experiment" (Dye, 1989, 19, 21, 23 April).

To get a sense of the instability, consider what might have happened over just two days. On the evening of Monday, 1 May 1989, a parade of speakers at the American Physical Society meeting in Baltimore ridiculed cold fusion. Strong critiques were made of various experiments from which scientists had claimed positive results. Theoretical calculations were presented to show that Fleischmann and Pons's claims violated the predictions of nuclear theory by nearly forty orders of magnitude. At a press conference eight of nine researchers voted against the likelihood that cold fusion would prove to exist. The sense that Fleischmann and Pons had made absolutely elementary mistakes and that cold fusion could be rejected out of hand was captured by one physicist who wrote a piece of doggerel to criticize the temperature measurements of a colleague:

> Tens of millions of dollars at stake, dear brother,
> Because some scientists put a thermometer
> At one place and not another.

And Caltech's Koonin was widely quoted when he said that "we are suffering from the incompetence and possible delusion of Professors Pons and Fleischmann" (Associated Press, 1989; Browne, 1989; CCFA, audiotapes and videotapes; CCFA, 1989, May 22).

On the following day, Tuesday, 2 May, MIT researchers led by Richard Petrasso submitted to *Nature* a major article questioning the gamma-ray spectrums presented by Fleischmann and Pons as evidence of nuclear reaction products. (Petrasso's article included a gamma-ray spectrum taken off a television broadcast; this may be the first time a piece of scientific evidence has carried a citation to "KSL-TV in Utah." This unusual reference highlights interactions between media that information analysts have not previously noticed [Petrasso, Chen, Wenzel, Parker, Li, & Fiore, 1989].)

Yet that week *Time* and *Newsweek* issued their 8 May 1989 magazines. Both chose to feature cold fusion on the cover. Though the headlines included some skepticism (*Time*'s was "Fusion or Illusion: How Two Obscure Chemists Stirred Excitement—and Outrage—in the Scientific World"), the effect was to present cold fusion as a potential energy savior to millions of people around the world. A reader had to contrast the weekly news magazines, which by their writing style foster a sense of authoritativeness, with the reports of the APS meeting appearing in their daily newspapers. Especially for readers who depended on brief stories in local papers or television broadcasts, the news magazine stories might well have had more impact. And so, while journalists and researchers who had attended the APS meeting decided that consensus—or a stable judgment—was becoming clear, researchers not physically present in Baltimore, and certainly the general public, still faced highly unstable information.

The period of instability ran through the end of May, when the Department of Energy sponsored a three-day meeting devoted to cold fusion in Santa Fe, New Mexico. By the end of the meeting many of the four hundred participants were still undecided about the reality of cold fusion effects, but they were much clearer about how to go about testing the claims of Pons and Fleischmann, Jones, and the others who had now entered the fray. As *Science* magazine said in its headline, it was the "End of Act I" (Pool, 1989).

The Growth of Stability and Consolidation

Although the cold fusion drama continued after the intermission that (metaphorically) followed the Santa Fe meeting, the mass media for the most part did not come back to the show. The peak of media coverage of cold fusion occurred during the excitement of the mid-April period (Figure 3), when fresh reports appeared daily and Pons was cheered by seven thousand chemists at the American Chemical Society meeting in Dallas. A dramatic drop in coverage came after the APS meeting in Baltimore; many reporters said in interviews that the apparent consensus among scientists meant that a stable judgment had appeared and they could turn their attention to new issues. And following the Santa Fe meeting coverage dropped even more (Lewenstein, 1992; University Microfilms, Inc.).

With the drop in media coverage, the number of communication channels involved in cold fusion dropped dramatically. Without the mass media to carry information from one channel to another, the intermixing of other communication media also dropped, suggesting that, as we think about a more complex model of science communication, we need to give the mass media a catalyzing role in creating complexity.

The drop in media coverage, however, does *not* imply that cold fusion itself died out after May 1989. Indeed, there is significant evidence to show that cold fusion research remained robust for months after the Santa Fe meeting, even among the harshest skeptics. Reports of the Santa Fe meeting were circulated by electronic mail, then printed out and circulated even further on paper (CCFA, Weisz folder). The Department of Energy had created a special panel to investigate cold fusion. That panel met for the first time at the Santa Fe meeting, then conducted a series of meetings and site visits over the summer. When the Energy Research Advisory Board (ERAB), as the DOE panel was known, issued an interim report in mid-July, press coverage labeled the report a devastating blow to cold fusion. And while the report certainly was not friendly to cold fusion, it explicitly acknowledged the need for further research (CCFA, 1989, July 13).

The ERAB panel's report was part of the emerging consensus during the summer. About the same time a Brookhaven National Laboratory researcher presented a paper titled "Cold Fusion: Myth or Reality?" He concluded that "Cold fusion will *not* be our next power source," but that "there *do* appear to be some interesting physical effects to be pursued" (CCFA, Brookhaven National Laboratory). In the meantime the state of Utah had allocated about $5 million to a new National Cold Fusion Institute in the University of Utah's research park, and experiments there were being conducted with the advice of Pons and Fleischmann.

During the fall continuing discussions among the many participants took place at meetings and via the traditional forms of scientific communication, especially preprints and papers. A two-day cold fusion meeting was held at Varenna, Italy. The National Science Foundation and the Electric Power Research Institute (funded by the electric utility industry) jointly sponsored a three-day meeting in Washington in October. The ERAB panel issued its final negative report in November. But the results of these various meetings and panels were also distributed electronically and via fax and telephone into a growing cold fusion underground. Douglas Morrison, a CERN physicist who was one of the first and most persistent to tag cold fusion as pathological science, distributed an irregular *Cold Fusion Newsletter* via electronic mail, and copies were posted to the sci.physics.fusion newsgroup (which had, by now, superseded the alt.fusion newsgroup) as well (CCFA, Morrison newsletter folder).

Thus, by the end of 1989, the cold fusion saga had become stable. Mass media coverage of cold fusion (including news reports in the science trade press, such as the news sections of *Nature* and *Science*) dropped essentially to zero by the fall and remained there except for brief flurries caused by anniversaries of the original announcement or by accusations of fraud that have periodically appeared. Meanwhile the number of articles appearing in the technical refereed literature had climbed steadily and by the end of 1989 consistently averaged nearly twenty articles per month. Electronic newsgroup volume was also about to settle into a pattern and by early 1990 averaged about seventy messages per month.

Another island of stability grew out of the efforts of some researchers who deliberately removed themselves from the morass of information in which they found themselves wallowing. David Williams, an electrochemist who led the replication effort at the United Kingdom's Harwell laboratory, had begun his experiments with help from Fleischmann *before* the public announcement. After the announcement, he briefly noted the many conflicting bits of information he heard from other groups attempting replications. Recognizing the confusion this was creating in his own group's work, he made a conscious decision to disregard information coming from outside Harwell. His group felt that they should focus on their own experiments rather than trying to follow every twist and turn that others reported (CCFA, 1990, April 11). Charles Martin, an electrochemist at Texas A&M University who had been among the first apparently to replicate parts of the Pons and Fleischmann experiments, discovered in the early summer of 1989 that he had devoted so much time to cold fusion that he had dropped all other activities—including keeping up his log book and playing racquetball. He, too, made a conscious decision to resume his normal information and working habits—which, of necessity, meant spending less time seeking information and watching for the latest permutations in the cold fusion activities of others (CCFA, 1989, July 17).

As 1990 proceeded, the stable positions consolidated. Review articles and conference proceedings that argued for cold fusion began to appear, such as an Indian summary of one hundred experiments performed at the Bhabha Atomic Research Center in Trombay, Bombay, and the proceedings of the First Annual Conference on Cold Fusion, sponsored in March 1990 by the NCFI (Iyengar & Srinivasan, 1989; Will et al., 1990). Notice that much of this information continued to appear in the "gray literature"—accessible to insiders who were on distribution lists, but not part of the formal peer-reviewed literature system. Out of the NCFI conference came comments indicating the strength of the beliefs of cold fusion supporters: "It is no longer possible to lightly dismiss the reality of cold fusion," said UCLA physicist Julian Schwinger, a 1965 Nobel laureate. Recent calorimetric results "will be noted as a decisive turning point in the history of the affair," said Ernest

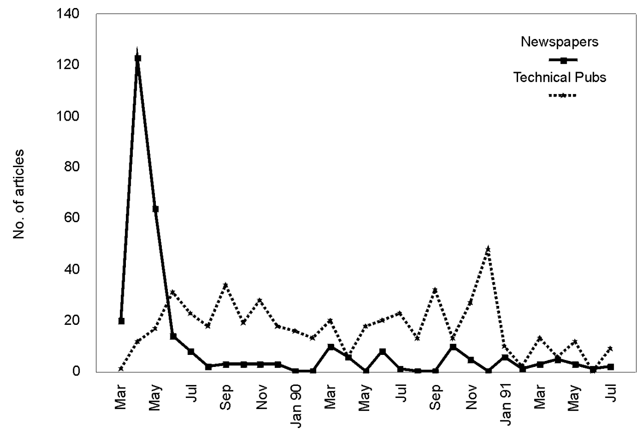

Figure 3. Cold fusion publications. Newspaper data (which include book reviews) from the "Newspaper Abstracts OnDisc" CD-ROM database. Technical publications taken from the Cold Fusion Bibliography distributed via sci.physics.fusion Internet newsgroup by chemist Dieter Britz; data shown include only those items for which the specific month of publication is identified.

Yeager, a Case Western Reserve researcher. "These results cannot be explained by trivial mathematical errors," Yeager continued. And two Los Alamos National Laboratory researchers, speaking to one of the specific issues that bothered many observers, said, "We can put aside the question as to whether the tritium is real." To their satisfaction, it was (Mallove, 1991).

Finally, in mid-1990, Fleischmann and Pons published the major article they had been promising for months, providing in exhaustive detail the calculations they had performed to calculate the excess heat they said they had observed in their cells (Fleischmann, Pons, Anderson, Li, & Hawkins, 1990).

The skeptics, however, were also consolidating their position, and the new article from Fleischmann and Pons contributed to their certainty, since it dealt only with calorimetry, not with measurements of nuclear reaction products. The skeptics pointed to the lack of evidence of nuclear reactions to justify their own decision to ignore further cold fusion claims. They were especially impressed by a paper published in *Nature* on the first anniversary of the original announcement by Michael Salamon, a University of Utah physicist who had been allowed into Pons's laboratory and had found no evidence of nuclear reaction products (Salamon, et al., 1990).

Skeptics could also point to the gradual decrease in the number of publications in the formal refereed literature. The decrease was especially dramatic if one considered the actual date of submission, rather than the date of publication. Submission dates showed that the bulk of published papers actually represented research done in 1989. The volume of research conducted after that was clearly dropping (Lewenstein, 1992).

The split between skeptics and believers was perhaps best illustrated by the publication in early 1991 of two books: physicist Frank Close's *Too Hot to Handle*, an indictment of the methods and procedures followed by Pons and Fleischmann; and science writer Eugene Mallove's *Fire from Ice*, a paean to the possibilities of power created by cold fusion.

After 1991 cold fusion was essentially completely divided into the two paths of belief and skepticism, with few intersections between them. Although a few traditional journals continued to publish cold fusion work (most notably *Fusion Technology*), communication now tended to take place between individuals, in informal meetings or via the "cold fusion underground" of telephone and fax communications. The proceedings of the annual cold fusion meetings were also important sources of information for continuing cold fusion researchers, as were newsletters like *Fusion Facts* (published in Salt Lake City) and magazine's like Mallove's *Infinite Energy*.

Electronic conversations about cold fusion continued to take place regularly in the sci.physics.fusion newsgroup and the associated Fusion Digest listserv distributed over the Internet. Until about mid-1992 the newsgroup consisted primarily of interested bystanders commenting on cold fusion. But with the regular contributions of a few active cold fusion researchers or supporters, volume increased somewhat after that. (The growth may have reflected new developments within the cold fusion social community as well as the rapid growth of all Internet-based activities worldwide; exploring those developments, however, is beyond the scope of this article.)

The mass media continued to run an occasional story on cold fusion. But for the most part the complexity of the cold fusion communication context had died out by the end of 1992.

Conclusion

Two major conclusions can be drawn from this history of cold fusion focusing on communication issues.

Communication Complexity

Although traditional models of science communication described a linear process, this article has clearly shown that many forms of scientific communication interacted in the case of cold fusion. A better, nonlinear model might be a circle or a sphere, with all forms of communication leading to each other (Figure 4). Some evidence of mixed forms of communication makes this clear:

- The reliance of some teams on television for depicting experiments that they tried to reproduce.
- The debate on social and moral issues (such as the effect of cold fusion on the world economy) appearing almost solely on the electronic networks, but drawing from data mainly in the mass media.
- The exchange of information among media, such as the NPR reporter who gathered commentary on the Internet or the media commentary that appeared on the Internet.
- The growing sense of excitement after the Jones preprint was distributed via fax and electronic mail, with the excitement infecting the mass media.
- The importance of meetings, both large and small, for setting the tone among multiple media.
- The way in which some researchers changed their opinion of Pons and Fleischmann (generally in the negative direction) after they appeared before a congressional hearing on 26 April and tapes of their appearance were broadcast on C-SPAN.

In this model, the category "mass media" moves toward a central place. As suggested in the text, mass media were not crucial to the ongoing process of cold fusion science. But their presence did contribute to the complexity and instability of information available to researchers at any given time. The mix of all communication media depended on the degree to which *mass* media were involved.

This revised model of the science communication process suggests a resolution to one of our initial problems: how to understand the role of the mass media in science. The answer is do not try—or, at least, do not try without also examining the full communication context. In the cold fusion saga any attempt to understand the role of the mass media must deal with the permeable boundaries that existed between the various forms of communication that were involved. In more general terms the model suggests that to understand science communication, we must explore the complexity of interactions among *all* media.

One can question whether this more complex version of science communication applies to all of science. In the science studies world research on scientific controversies is valued precisely because it highlights points of stress in the system. By that argument a model derived from studies of cold fusion is a plausible candidate for explaining the communication patterns seen in other areas of science. But the role of public discussion of fine details of the scientific process was clearly greater in the cold fusion saga than in most areas of science; conceivably this could bias my description toward greater complexity than normal. Only future studies attempting to apply this description of science communication can resolve this issue.

Preliminary evidence suggests, however, that mass media does indeed influence scientific practice. For example, in a study of research patterns appearing in the *New England Journal of Medicine*, sociologist David Phillips and colleagues showed that those articles that had been brought to the attention of the public by the *New York Times* received an amplified response in the technical scientific literature for years after they initially appeared. Several analyses of recent controversies in geology regarding catastrophes and extinctions have pointed to the media's role in catalyzing technical discussions. A brief analysis of the media's role in the discussion of the possibility that fossil signs of life were found in a Martian meteorite also supports the importance of understanding the interactions of media (Phillips, Kanter, Bednarczyk, & Tastad, 1991; Clemens, 1986; Glen 1994; Lewenstein, 1997).

In perhaps the closest comparison to cold fusion a high degree of complexity occurred in the case of high-temperature superconductivity. In the early months of that field scientists regularly presented data straight out of the laboratory at press conferences and other nontraditional forums. As in the case of cold fusion, researchers from other laboratories had to decide whether to wait for more stable, certain information or to proceed with their own work based on the incomplete information acquired through the media. The media played a role in helping researchers exchange data, though with unclear results on the progress of the research itself (Hazen, 1988; Schechter, 1989; Felt, 1993; Nowotny & Felt, 1997). Superconductivity represents the opposite pole from cold fusion: an unexpected finding that eventually led to the consensus that the phenomenon had been confirmed. Yet it also offers a case in which the model described above seems, to a first approximation, to be applicable.

Despite these suggestive cases, more work is needed to see if the model of complex science communication described above can be applied in other contexts.

Information Stability

One of the most intriguing new questions in information science is the effect of new communication technologies on the process of scientific inquiry (Lewenstein & Heinz, 1992; Harrison & Stephen, 1994; Crawford, Hurd & Weller, 1996). This study suggests that one important issue is the degree to which scientific judgments are based on the stability of information. Cold fusion presents a particularly vivid example of the ways in which judgments changed depending on what information was available. Clearly the nontraditional forms of communication (including electronic mail, electronic bulletin boards, faxes, and news media reporting) were associated with unstable information. But what was their role? Did the presence of new communication contexts create instability? Or were the new contexts—and the vast quantities of material they offered—used precisely because they provided an opportunity to resolve uncertainty and thus create stability more quickly than traditional contexts? There is a correlation, but in what direction is the causation: Does information cause instability, or does instability create a need for information? Is it even possible, given the interactional model of science communication presented above, to specify direction or causality?

Although there is not yet sufficient clear evidence to answer these questions, I want to present one possible answer, in part to stimulate further discussion. I believe the available evidence suggests that, in the cold fusion case, new communication contexts (including electronic technologies) ensured a surfeit of information; that this surfeit led to confusion and complexity; and that only when the mass media dropped out of the communication context did the scientific community proceed to more stable information and more stable judgments (both among skeptics and believers). At the same time I think that the initial presence of complex, unstable information also created the need to find stability more quickly and thus may have hastened the time when

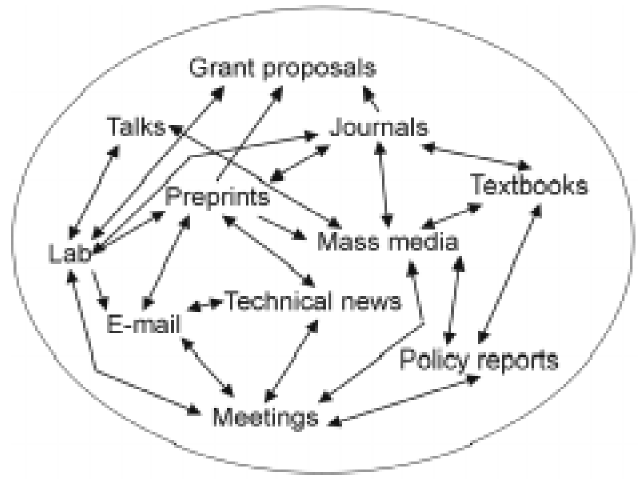

Figure 4. The web of science communication contexts.

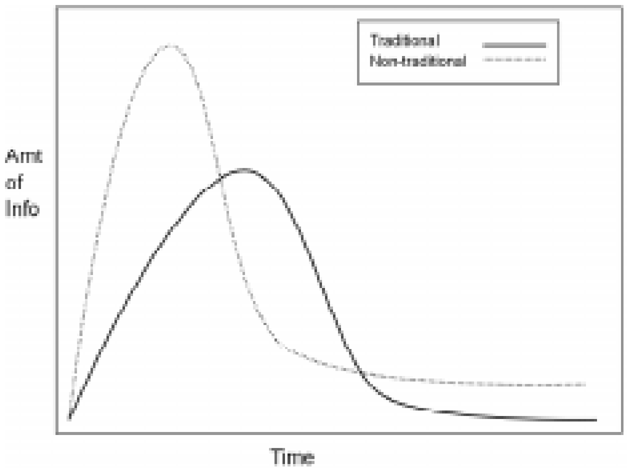

Figure 5. Information stability. The horizontal axis represents time; the vertical axis represents information quantity. The traditional curve, with less information spread out over a greater amount of time, is lower and more "stable." The new curve, with greater information reached initially, shows a thinner, less stable "peak." Notice that the new curve also levels out at a higher level, suggesting that information in nontraditional contexts remains more complex over time, despite reaching a relatively stable level earlier.

stable judgments were formed. More information led to more instability but also reduced the time until stability was achieved. This relationship is illustrated in Figure 5; while the figure can only be suggestive, since it lacks units, it may provide a useful graphical metaphor as we try to develop new models of science communication.

Although the instantaneous nature of modern electronic communication has become a cliché, the speed with which information flowed had an important impact on cold fusion, because many people were trying to make decisions based on a mish-mash of changing data, of varying degrees of reliability, and in various states of intelligent presentation. As the model presented in Figure 4 suggests, information flow in science is a convoluted, irregular process. The pressure of e-mail and other forms of electronic communication (in addition to the presence of the mass media) *added* to the confusion in the cold fusion case. Communication times were shorter, but the communication itself was more complex, chaotic, and intense. Only after information channels were removed, and thus the chances of receiving conflicting or competing information reduced, could stability develop.

What might be the effects of a shorter, more intense communication period in which more unstable information is converted into stable knowledge? Two possibilities exist, which need to be investigated with additional research:

1. Greater complexity could change the way in which people are recruited into the scientific debates, since it changes the premium placed on access to information, speed of response, etc. (Some people, for example, have argued that electronic mail allows the scientific playing field to become more level, since issues of status, age, gender, physical location, and so on do not enter into an electronically mediated discussion in traditional ways. But at least in the cold fusion case, it is not clear that such democratization happened [Lewenstein, 1995a].)
2. Another possible effect is that intense communication periods may make emotion more important: Anyone who uses e-mail regularly has had the sensation of pushing the SEND button and then saying, "Oops, I didn't really want to say that." With an old-fashioned letter, or a game of telephone tag before you reach someone, there is the chance for things to cool down a bit. Emotion, of course, plays no role in the canonical "scientific method." But given the clear findings of science-studies researchers regarding the importance of social interaction in the development of scientific knowledge, we need more research on the role of emotion in scientific communication (LaFollette, 1990).

Although we do not fully understand these effects, one possibility is that the traditional routines of peer review and formal publication will remain important components of the social process of science, because they will serve as ways for information to become more stable than it is in the faster but more ephemeral forms of communication that are a part of everyday scientific life. As the density of communication media falls off, information (and thus knowledge?) becomes more stable because the competing sources of information are not there.

Clearly these possibilities are only speculation, constrained by our lack of clear knowledge of how the science communication process actually works. While the traditional linear models focusing on peer-reviewed literature have provided useful guides for much of the last generation, they are inadequate to explain the complexity of modern scientific communication. We must develop more sophisticated models of science information, both for theoretical reasons and as a guide to the practice of librarians, information scientists, and scientific researchers in the future.

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CCFA. Diane Weisz folder.
CCFA. E-mail file. Messages from John Gilmore and Matt Ginsberg. 890405.af electronic file.
CCFA. Morrison newsletter folder.
CCFA. Preprints folders.
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