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• **ABSTRACT**

Science in the mass media is usually interpreted in terms of traditional, linear, 'dissemination and translation' models of science communication. Using the cold fusion saga that began in 1989, this paper argues that communication among scientists uses many media, which interact in complex ways. A more appropriate model for modern science must account for the permeable boundaries between formal publications, preprints, electronic computer networks, fax machines, mass media presentations and other forums for scientific discussions. The new model must account for the paradox that increased communication activity may be associated with instability rather than stability, at least in the preliminary periods of a scientific controversy.

From Fax to Facts: Communication in the Cold Fusion Saga

Bruce V. Lewenstein

When the public saga of cold fusion began in March 1989, the role of the mass media in a scientific controversy quickly became a central issue. From the initial press conference at the University of Utah, through the daily dispatches in newspapers around the world, to the widely quoted labelling of B. Stanley Pons and Martin Fleischmann as suffering from 'incompetence and delusion', the media had a central place. Not only did the media inform the public about the development of a new area of scientific research – 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 and the media provide little guidance for understanding how the media's presence in the debates affected the construction of cold fusion as a research area. Although analysis of science and the media has a long history, that history is largely one in which media coverage of science is looked at in isolation, as though media coverage of

science is simply a matter of disseminating some sort of 'simplified' science. This leads to concern with issues of accuracy, balance, sensationalism and relevance to the public.¹ The inadequacies of this model have been identified by a variety of researchers. Shinn and Whitley, editing a volume on 'expository science', point to the essential similarities among all discourse that involves science, a topic that has been taken up in studies of the rhetoric of science.² Dornan, analyzing the content and production of early studies in science journalism, points to the progressive scientific ideology underlying the field, a point that Hilgartner expands in his analysis of how scientists use popularizations in political ways to defend their institutional grounds.³

Indeed, if one tries to analyze the coverage of cold fusion using the traditional concerns, one finds that the coverage was very much like most science journalism: generally accurate, at least by some measures; generally balanced, at least as journalists would define that term; not especially sensationalistic; and only sometimes directed towards issues relevant to the public.⁴ That analysis, however, is distinctly unsatisfying, for two reasons. First, if cold fusion was so much like everything else, why was the media's role experienced as something very different? And second, given that the experience was different, what can we learn about science by studying the media's role in cold fusion? This paper addresses these questions.

Traditional Science Communication Models

Although the critiques of traditional studies of science and the media have provided much insight into better ways of understanding the role of the media, they do not address the underlying problem with traditional studies: that they are based on an outdated model of science communication.

In the late 1940s, researchers in a variety of fields were attempting to create comprehensive theories of what we now call 'communication'. Two of the most influential publications came from the political scientist Harold Lasswell and the telephone engineer Claude Shannon. Lasswell's model was expressed in words:

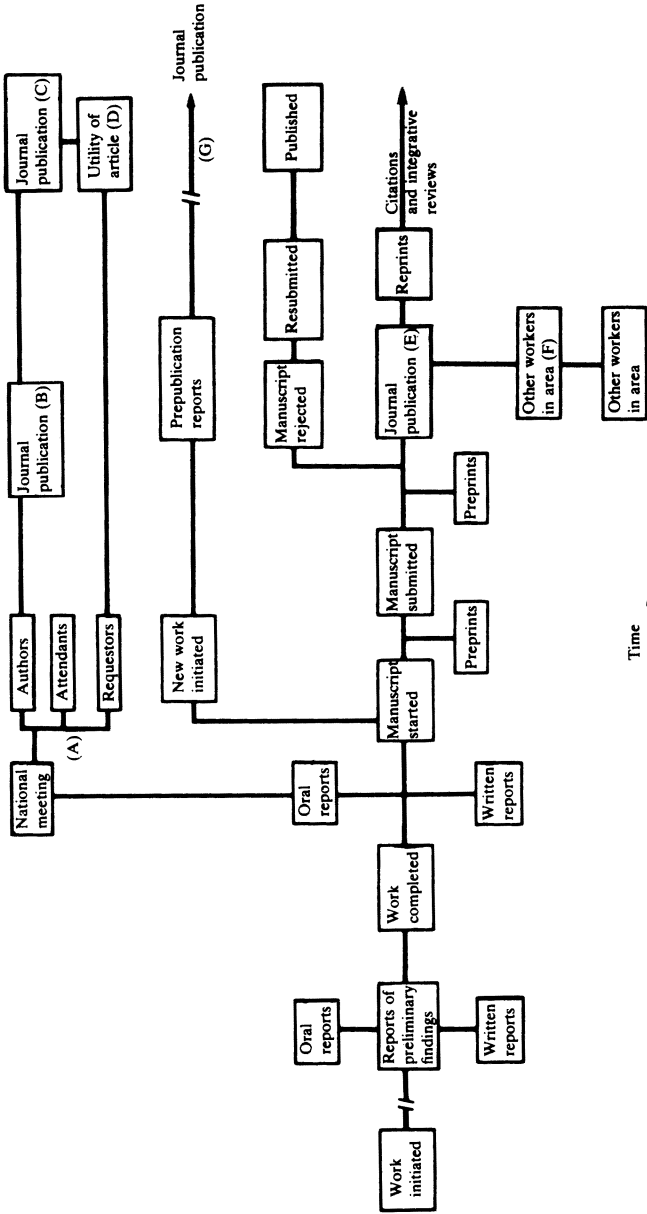
Who
Says What
In Which Channel
To Whom
With What Effect?⁵

Shannon's model was based on his work at Bell Laboratories in signal processing; it was a highly mathematical theory with 23 theorems, and was essentially unintelligible to anyone not versed in the technical aspects of electronic information processing. But when the model was published, it came with an interpretive essay by Warren Weaver, the applied mathematician who had become an executive at the Rockefeller Foundation. Weaver's essay, by pointing to the general application of Shannon's theory, opened up for researchers key theoretical concepts like 'source', 'message', 'channel' and 'receiver'.⁶

Around 1960, those terms were incorporated into a widely distributed model known as the 'SMCR' model.⁷ That model is often cited as 'the' description of what communication is like. It is a linear, uni-directional model; it implies that information is carried from one source to one receiver, without any feedback. In the 35-odd years since the development of the SMCR model, communication theorists have been creating more sophisticated models at the rate of about one a year (one introductory freshman communication text includes 31 theories), and contemporary researchers know – at least nominally – that they need to treat communication as an interactive process.⁸ But during the 1960s, when sociologists and others began looking at science communication systematically, the uni-directional, non-feedback model of communication was the most readily available theory.

When the widespread investigation of the social structure and functions of the scientific community began in the 1960s, researchers developed the idea that 'communication' is a fundamental part of science, especially the open communication of scientific ideas. They demonstrated that scientists receive rewards for publishing their ideas and findings in the scientific literature, and that those rewards are part of what creates a community of researchers.⁹ John Ziman, in one of his earliest examinations of the scientific community, defined science as 'public knowledge'.¹⁰ In this context, thorough empirical studies of science communication were conducted from the late 1960s through the 1970s. Run by

FIGURE 1
The Formal Scientific Communication System, as Defined in the 1970s



Source: reprinted with permission from William D. Garvey, *Communication: The Essence of Science – Facilitating Information Exchange among Librarians, Scientists, Engineers and Students* (Oxford & New York: Pergamon Press, 1979), 169.

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).¹¹ Although Garvey and his colleagues did not cite the communication literature directly, the model that they produced is clearly compatible with the linear, dissemination-oriented SMCR model that had emerged in communication studies.

During this time, very little theoretical work was being done on science and the mass media. Most studies were descriptive analyses of how science gets covered by newspapers and magazines, as well as prescriptions for how to 'improve' media coverage of science. These studies, emerging largely from journalism schools and working science journalists, were deeply influenced by the rapid growth of science writing as a sub-discipline within journalism.¹² The studies acknowledged no theoretical difficulties in the standard definition of science writing – disseminating scientific information from the scientific community to the general public. The 'problem' to be solved was the same one that had concerned telephone engineer Claude Shannon: how to avoid distortion as information was transmitted from a source across a channel to a receiver. The studies of accuracy, balance and sensationalism that have traditionally occupied the interest of science journalism scholars are fundamentally shaped by a concern with identifying and resolving the problem of distortion.¹³

However, beginning in the mid-1970s, sociologists of science began to react against the notion that scientific knowledge could only be studied as a privileged type of knowledge. Instead, building on the ideas of Jerry Ravetz, David Bloor and others, sociologists and anthropologists began to examine the everyday practices of scientists as they produced knowledge.¹⁴ As a result of the new turn in science studies that these researchers inaugurated, some sociologists and historians of science began to question the idea that science is 'created' in one sphere and then disseminated in another sphere, 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.¹⁵ 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.

The most recent and fruitful presentation in this new tradition is sociologist Stephen Hilgartner's analysis of the controversy over the relationship between diet and cancer. Hilgartner carefully examines the specific texts that scientists and others prepared as they argued whether diet and cancer are linked – and if they are, the degree to which diet can contribute to cancer. He shows that information changes form depending on the audience and purpose to which the information is addressed.¹⁶ Hilgartner's analysis is based, in part, on a model of science communication that is fundamentally different from the uni-directional dissemination model. Although still linear, Hilgartner's model is explicitly bi-directional. Equally important, there is no distinction between 'science' at one end and 'popularization' at the other end. He talks about 'upstream' and 'downstream', but in the text of his article he carefully shows that it is theoretically impossible to define a point at which science becomes popularization.¹⁷

How does all this relate to cold fusion? 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 provide a history of cold fusion that integrates media reports into the overall communication patterns that shaped the cold fusion saga. In so doing, I show that the media's role in cold fusion can only be understood by reconceptualizing our idea of what science communication is.

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 like this:

This is the CBS Evening News, Dan Rather reporting. Good evening. What may be a tremendous scientific advance tonight in attempts to create and harness the almost limitless, clean power of the sun. Instead of splitting atoms, as is done in nuclear power plants today, fusing atoms. Some of the world's leading researchers have spent decades, and millions of dollars, trying to achieve this in the laboratory. Scientists in Utah tonight believe they have taken a big step forward in a little test tube. CBS News correspondent John Blackstone begins our coverage of what may have been achieved, and what it may mean to you.

[Pictures of experiments; John Blackstone talking]: If what is happening in these test tubes is indeed controlled nuclear fusion, as the scientists who set up this experiment believe, then this laboratory may one day be known as the birthplace of cheap, clean, and abundant energy. . . .¹⁸

Some scientists and members of the public had even been able to read stories in the *Wall Street Journal* and *Financial Times* (of London), both of which ran reports on the morning before the press conference, indicating that an announcement would be forthcoming that scientists 'have achieved "a sustained thermonuclear reaction at room temperature"'.¹⁹ The *Wall Street Journal's* coverage was especially important, because on 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 presence was probably the stimulus that caused Pons and Fleischmann to go public when they did.²⁰

In the six years following that announcement, cold fusion research went through roughly four distinct periods (Figure 2).²¹ The first period, lasting about two months, appeared to many participants and observers as utter chaos (in the everyday, non-specialist sense of that word). Claims and counter-claims changed almost daily, special cold fusion sessions were attached to regular scientific meetings on an *ad hoc* basis, 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 autumn 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

FIGURE 2
Major Points in the Cold Fusion Saga Timeline, 1989–96

23 March 1989: Public Announcement

April–May: *Media and scientific chaos*

- 1989 : 12 April ACS/Dallas
- : 26 April Congress hearings
- : 1 May APS/Baltimore
- : 23 May Sante Fe

Summer–Fall: *Growing stability*

- 1989 : 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*

- 1990 : 29 March 1st NCFI CF Conference
- : 15 June *Science* charges fraud
- : 22 October BYU conference on Anomalous Effects

1991–96 : *Ongoing work, 2 separate strands*

- : January 91 Pons resigns from University of Utah
- : Spring 91 Close and Mallove books
- : June 91 2nd CF conference, Italy
- : January 92 Riley killed at SRI
- : October 92 3rd CF conference, Japan
- : December 93 4th CF conference, Hawaii
- : April 95 5th CF conference, Monaco
- : Fall 96 6th CF conference, Japan

and, for the most part, public and scientific interest in the topic died off.

In the third period, lasting throughout 1990, the sharp division between ‘sceptics’ (or non-believers) and ‘believers’ (as they were frequently labelled) became more prominent. On the first anniversary of the announcement, the scientific journal *Nature*, home of the most prominent sceptics, published a scathing critical analysis of the situation in Pons and Fleischmann’s own laboratory.²² That same week, believers gathered in Salt Lake City for the first Annual Cold Fusion Conference, sponsored by the State of Utah-funded National Cold Fusion Institute.²³ Later in the year, the journal *Science* published a news article that came very close to accusing some cold fusion researchers of fraud.²⁴ In October, trying to tread a middle ground between belief and scepticism,

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 sceptics and believers was vividly presented 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*, which avoided accusing Pons and Fleischmann of fraud only by leaving open the possibility of sloppy incompetence, and Eugene Mallove's *Fire From Ice*, which predicted cold-fusion-powered home heaters just around the corner.²⁵ After that, in the fourth period, the two sides continued on their way, largely ignoring each other's critiques. Although a few sceptics (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, annual meetings were held in Como, Italy (1991); Nagoya, Japan (1992); Maui, Hawaii (1993); and Monte Carlo (1995); and scheduled for Japan (1996). 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 you look at a basic communication 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 whether he was out of the office, and his boss told him when he checked in for the day.²⁶ Steve Koonin, a theoretical physicist at Caltech who was visiting Santa Barbara for a year, recalled who told him about cold fusion, but did not remember whether the information came by electronic mail or telephone.²⁷

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.

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. Richard Harris, a science correspondent for National Public Radio (a nationally-syndicated radio service run by a non-profit, partially government-funded corporation) posted a message on the sci.physics electronic bulletin board available through the Internet:²⁸

>Newsgroups: sci.physics
 Subject: Test-tube fusion details
 Message-ID: <1318@sri-arpa.ARPA>
 Date: 24 Mar 89 19:41:59 GMT
 Lines: 28

>From: Richard Harris . . .

Here are some details from the University of Utah press conference yesterday. Fleischmann said the team had attained sustained fusion for hundreds of hours, producing heat at the rate of greater than 20 watts/cu-cm of electrode. He said the heat once melted the palladium electrode (M.P.1554 C). He did not state how much electric current was being applied at the time. . . . If you can interpret these 'findings', I would appreciate hearing from you. I am science correspondent for National Public Radio. The information in this message comes directly from the press conference, which I have taped.

Richard Harris²⁹

Again, notice the interaction among media: a radio reporter is using electronic mail to get interpretation of information that he has documented via audiotape.

For many observers, videotape was more important than audiotape. Across the country, researchers phoned the University of Utah, requesting copies of a videotape showing the press conference, or watching copies of the television shows that had run extensive stories on the announcement, such as the 'CBS Evening News' or the 'MacNeil-Lehrer News Hour'. The level of detail was not great. Here is Pons at the press conference:

The experiment we have accomplished has been described in the news release which you have, and I'll just give you a brief synopsis. Basically, we have established a sustained nuclear fusion reaction by means which are considerably simpler than conventional techniques. Deuterium, which is a component of heavy water, is driven into a metal rod . . . exactly like the one I have in my

hand here, to such an extent that fusion between these components, these deuterons in heavy water, are fused to form . . . a new atom. And with this process there is a considerable release of energy, and we have demonstrated that this can be sustained on its own. In other words, more energy is coming out than we're putting in.³⁰

When reporters and other researchers did turn to the press release for more information, 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'.³¹

Instead, they found themselves reading newspapers and watching the videotape again, 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' 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.³²

Lacking the traditional access to a fixed, stable piece of information (a preprint or published article), many scientists began exchanging rumours, newspaper articles, and so on. Once again, the interaction of multiple forms of communication can be seen in a faxed copy of a Japanese article received at the Princeton Plasma Physics Laboratory (PPPL). Along one side of the article (which is in Japanese), someone had written in English:

4/1/89. Yomiuri Shimbun (Japanese biggest news paper). Tokyo Agriculture & Engineering University announced 'They [Koganoi-shi] re-produced the Utah experiments'. They measured heat, gamma and tritium. Prof. Koyama.

The fax was received on 1 April 1989 in Princeton by Dennis Manos, a physicist who had taken informal control of the PPPL's attempts to reproduce the experiments. Manos, in turn, passed the fax on to PPPL director Harold Furth, with the brief notation 'Harold – More Claims – D. Manos'.³³ A newspaper, a fax and various interpersonal communication connections all shaped the meaning of this 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. Among the first to appear were the manuscript of the original Fleischmann and Pons article, which had been accepted by the *Journal of Electroanalytical Chemistry and Interfacial Electrochemistry (JEC)*, and the manuscript for an article submitted by Jones and his colleagues to *Nature*. In mid-April, a 'true' preprint (photocopy of journal pages) of the Fleischmann and Pons article was released by *JEC* at the American Chemical Society's meeting in Dallas, along with a full page of errata – including the addition of a third author, graduate student Marvin Hawkins. The Jones article appeared in *Nature* in late April.³⁴

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 (this was especially true of the Jones article, as the published version had been stripped of much of the geophysical rationale that had motivated the research). Those differences raised questions ranging from what constituted quality science – 'If you got a paper like that from an undergraduate, you would give it an F', David Bailey, a physicist at the University of Toronto, told reporters asking about the Fleischmann and Pons manuscript – through questions of interpretation and judgement about relevant data.³⁵ Frequent questions in those days involved the role, if any, of control experiments; the treatments, if any, that electrodes had undergone; the make-up of the electrolytes in the experiment; and other details of the experimental protocols, both of the Fleischmann-Pons and Jones experiments and of the attempted replications at myriad institutions. Although the process of sorting out the differences and making judgements 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 one was talking about contributed to the sense of chaos or instability.

An important issue here is that different people had different levels of access to information. By the time these 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, his own summary of a one-day cold fusion conference at 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 presentation 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 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.)³⁶

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' article published, along with commentaries by several other scientists. While readers no doubt made some judgements about the relative importance of information in speculative letters, Garwin's meeting report and Jones' complete article, communication theory suggests that those judgements are extremely complex and not likely to be directly related to 'objective' measures of the relative importance to be assigned to each publication. People take in lots of information, filter it in various ways and base their judgements 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 (Nobelist 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 judgement, based on completely contingent factors*. No model attempting to predict the value of different types of communications can work.³⁷

Another factor that made it more difficult for researchers and others trying to make stable judgements 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. David Ansley, a reporter for the *San Jose Mercury News*, recalled that:

I cut a deal with several of these people – with Nate Lewis and with [Stanford University materials scientist Robert] Huggins, and later with some people at SRI [a major consulting firm based near San Jose]. . . . I said, ‘If you’ll let me come watch you do this, I’ll be happy to pass on any information I get on how this sucker works’. So when I got the manuscript [of the Jones paper] I faxed it to Huggins and to Lewis. And then I called them back to ask what they thought of it. . . . Everyone was scrambling for information. The day after we printed the story saying ‘here’s what the Jones manuscript says’, I got a call from SRI. An electrochemist there said, ‘Um, would you send us a copy of that [paper]. I’m sort of unused to asking this, but would you?’ I said, ‘Sure, what do I get in exchange?’ So I ended up talking to a guy there . . . who I used as a source from then on.

Ansley’s activities, and those of other reporters, went beyond merely passing around copies of papers and negotiating access to information. Sometimes, they acted explicitly as mediators among scientific sources:

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’.³⁸

Throughout this period, scientists continued to mix mass media, preprint and direct interpersonal information. On 8 May, for example, researchers at the Princeton lab received a fax from a colleague in Britain, which contained a preprint of a paper to be published in Italian, along with a story from the front page of the *Sunday Times* (London), along with comments about a seminar on cold fusion which the colleague had attended in Varenna.³⁹

For those people following the rapidly expanding electronic bulletin boards, the mix of media also continued. By the beginning of April, a separate ‘newsgroup’ called ‘alt.fusion’ had been created, completely devoted to cold fusion. 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.⁴⁰

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'. Although much of the information that flowed among researchers and through the media was sceptical, the sense of excitement and enthusiasm periodically broke through. On 13 April, NBC science correspondent Robert Bazell reported that:

Many scientists greeted the initial claims of fusion with profound scepticism. Now that scepticism is giving way to excitement at the possibility that this may be real. . . . An increasing number of scientists now agree that this experiment does produce at least small amounts of nuclear fusion, the reaction that powers the sun.

When NBC anchor Tom Brokaw expressed some scepticism of his own (mainly by his tone of voice and facial expression), Bazell admitted that 'it takes a while to go from a little glass in the laboratory to a big reactor'. Unable to keep his enthusiasm in check, however, he gushed, 'But still, if this is true, we'll have a very different energy picture in the world.'⁴¹

The sense of instability caused by frequently changing judgements was reflected in newspaper coverage, as well. 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'.⁴²

To get a sense of the instability, consider what might have happened on a given day: Tuesday, 2 May 1989. The previous night, a parade of speakers at the American Physical Society meeting in Baltimore had ridiculed cold fusion. Strong critiques were made of various experiments that had claimed positive results, and theoretical calculations were presented to show that Fleischmann and Pons' claims violated the predictions of nuclear theory by nearly 40 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'.⁴³ Also on that day, 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. (Highlighting the interaction between media, 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'.)⁴⁴

Yet on the same day, *Time* and *Newsweek* both appeared with cold fusion on the cover. Though the headlines included some scepticism (*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 potential energy saviour to millions of people around the world. A reader had to contrast the weekly newsmagazines, 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 newsmagazine 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 judgement – was becoming clear, researchers not physically present in Baltimore, and certainly the general public, still faced highly unstable information.⁴⁵

The period of instability ran through the end of May, when the US Department of Energy sponsored a three-day meeting devoted to cold fusion in Santa Fe, New Mexico. Intended to be the meeting at which conflicting claims would be resolved, the Santa Fe meeting instead proved to be the first place in which systematic questions could be asked in a context that allowed for full explorations of the various issues that were raised. By the end of meeting, many of the 400 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'.⁴⁶

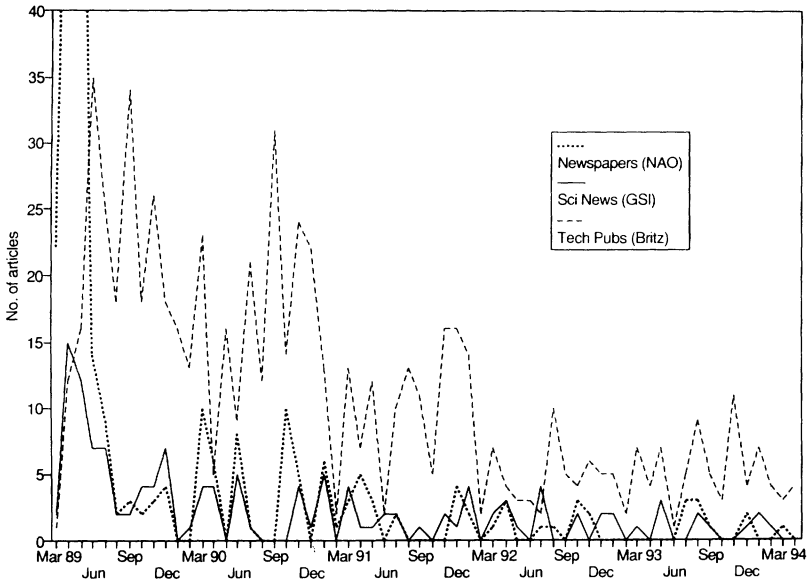
The Growth of Stability and Consolidation

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 7000 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 (especially the presentation by Nathan Lewis, himself a chemist at a physicists' meeting) meant that a stable judgement had appeared and they could turn their attention to new issues. And following the Santa Fe meeting, coverage dropped even more.⁴⁷

With the drop in media coverage, the number of communication channels involved in cold fusion drops dramatically. Not only do the mass media cease to be a major factor in the sharing of information among protagonists in the scientific drama, but without the media to carry information from one channel to another, the intermixing of other communication media also drops. This suggests 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 sceptics. Reports of the Santa Fe meeting were circulated by electronic mail, and then printed out and circulated even further on paper.⁴⁸ 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 ERAB committee (as the DOE panel was known) issued an interim report in mid-July, press coverage labelled the report a devastating blow to cold fusion.

FIGURE 3
Cold Fusion Publications



Source: Newspaper data (which include book reviews) from 'Newspaper Abstracts OnDisc' CD-ROM database; two data points not shown, in the interests of overall clarity – April 1989, 123 articles, and May 1989, 67 articles. News items and book reviews from science trade magazines (*Nature*, *Science*, *New Scientist*, and so on) taken from 'General Science Index' CD-ROM database. Technical publications taken from Cold Fusion Bibliography distributed via sci.physics.fusion Internet newsgroup by chemist Dieter Britz; data shown include only those for which specific month of publication is identified.

And while the report certainly was not friendly to cold fusion, it explicitly acknowledged the need for further research:

The experiments reported to date do not present convincing evidence that useful sources of energy will result from the phenomena attributed to cold fusion. . . . Hence, no special programs to establish cold fusion research centers or to support new efforts to find cold fusion are justified at the present time. . . . However, there remain unresolved issues and scientifically interesting questions stemming from reported cold fusion efforts.⁴⁹

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'.⁵⁰ 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 established 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.⁵¹

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. At the same time, by the end of 1989, the number of articles appearing in the technical, refereed literature had climbed steadily and now consistently averaged nearly 20 articles per month. Electronic newsgroup volume was also about to settle into a pattern, and by early 1990 averaged about 70 messages per month.

The stability also came because some researchers deliberately chose to avoid 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 established 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, however, he made a conscious decision to disregard information coming from outside the laboratory. His group felt that they should focus on their own experiments, rather than trying to follow every twist and turn the others reported.⁵² 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 logbook and playing racket-ball. 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.⁵³

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 100 experiments performed at the Bhabha Atomic Research Centre in Trombay, Bombay, and the proceedings of the First Annual Conference on Cold Fusion, sponsored in March 1990 by the NCFI.⁵⁴ 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 calorimetry results 'will be noted as a decisive turning point in the history of the affair', said Ernest 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.⁵⁵

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.⁵⁶

The sceptics, 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 sceptics 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' laboratory and had found no evidence of nuclear reaction products.⁵⁷ Sceptics 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, for that showed that the bulk of the papers which had been published actually represented research done in 1989. The volume of research conducted after that was clearly dropping.⁵⁸

The split between sceptics and believers was perhaps best illustrated by the publication in early 1991 of two books we have already noted: 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.⁵⁹ After 1991, cold fusion was essentially completely divided into the two paths of belief and scepticism, 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).

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 (notably Tom Droege, an engineer at Fermilab conducting research in his basement; Steven Jones; Jed Rothwell, a software entrepreneur who had become a strong advocate of cold fusion and who frequently passed on information from contacts in Japan; and Mitchell Swartz, a researcher from near Boston), 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. Droege inspired several stories, as did the international conferences in Japan and Hawaii. But for the most part, the complexity of the cold fusion communication context had died out by the end of 1992.

An Example of Complexity

One of the implications of this history of communication about cold fusion is that the presence of multiple forms of media is associated with problems of information stability. To illustrate the point explicitly, I will track my own knowledge of a particular event in cold fusion: the death of cold fusion researcher Andrew Riley in California in January 1992. (I am not claiming that any cold fusion researchers acquired knowledge in exactly the same way; based on interviews, however, I believe that this example illustrates the process that researchers experienced at other times in the cold fusion saga, especially in the first two periods.) The point of this exercise is to reiterate that the information available to any individual at any given time is of varied quality, age, usefulness and accuracy.

- On 2 January 1992 (Wednesday), Andrew Riley was killed in an explosion at SRI in Menlo Park, California.
- On 3 January 1992 (Thursday), I saw a very brief (one paragraph) wire service story on the accident in my local newspaper.
- On 4 January 1992 (Friday), I heard the end of a National Public Radio Story on the explosion. The portion of the story that I heard suggested that perhaps a faulty pressure valve in the experimental apparatus had caused the explosion.
- I heard no more details (despite checking my e-mail regularly) until about a week later (Monday 13 January), when I tracked down articles from the *Wall Street Journal* and *New York Times* that had appeared in the first day or two after the accident.

These articles ‘again’ suggested that a pressure valve might have been at fault.

- About the same time, reports about the explosion began to appear in the electronic newsgroup. Although those discussions had in fact been posted over a several day period after the explosion, I received them all at once about a week after the explosion because of the method by which I happen to receive the newsgroup. Information on the net is confused, but the pressure valve is again mentioned. (Though I had some doubts about the reliability of information, I nevertheless was left with only the ‘pressure valve’ theory to help make sense of the explosion.)
- On 21 January, I received a copy of *Science* magazine with additional details of the explosion. Written about a week earlier by a reporter in Washington, DC, it too mentioned the pressure valve possibility.
- On 23 January, I received from California several local newspapers that had carried stories about the explosion for several days. There I learned that about two days after the explosion, investigators had explicitly ruled out the faulty pressure valve theory.

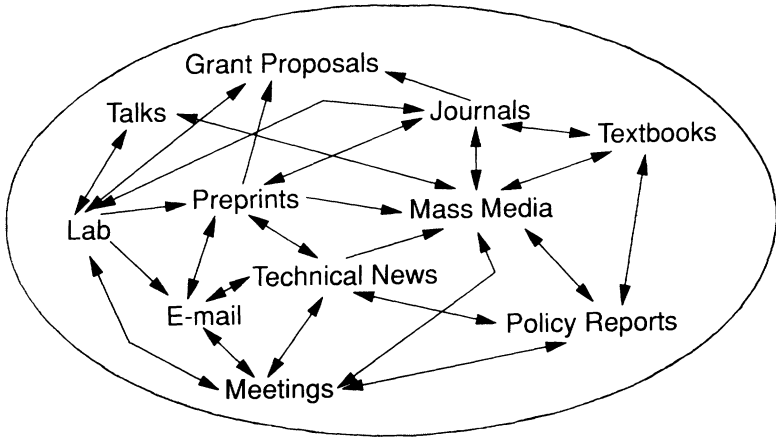
Thus, in the three weeks following the explosion, my own knowledge had gone from almost nothing, to about two and a half weeks of reasonable confidence that the pressure valve had something to do with the explosion, to sudden knowledge *from on the scene* (and, therefore, somewhat more reliable than most of my sources of information) that the pressure valve had been eliminated. That sequence of confusion, misinformation and delayed access to specific, detailed, credible information exemplifies the problem of interacting, unstable information that is at the core of the critique of the traditional model of science communication.⁶⁰

Conclusion

This history of cold fusion focusing on communication issues leads to two major conclusions.

Communication complexity. Although traditional models of science communication described a linear process, this paper has clearly shown that many forms of scientific communication interacted in the case of cold fusion. Indeed, rather than the

FIGURE 4
The Web of Science Communication Contexts



'continuum' of science communication suggested by Hilgartner, it might be better to describe a circle or a sphere, with all forms of communication leading to each other (Figure 4). A few points make this clear:

- The reliance of some teams on television for creating experiments.
- The debate on social and moral issues (such as the effect of cold fusion on the world economy) almost solely on the electronic networks, but drawing from data mainly in the mass media.⁶¹
- The exchange of information among media, such as the NPR reporter who gathered commentary on the net or the media commentary that appeared on the net.
- 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 seeing C-SPAN tapes of the two researchers appearing at a Congressional hearing on 26 April.

In the model, 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 the initial problem about how to understand the role of the mass media in science. The answer is: don't try – or at least, don't try without also examining the full communication context at work. In the cold fusion saga, any attempt to understand the role of the mass media must deal with the permeable boundary that existed between the various forms of communication that were involved. In more general terms, the model suggests that analyzing the role of the mass media in science cannot be accomplished in isolation, but must be an exploration of 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 at least 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 towards greater complexity than normal, and especially toward a more important role for mass media than normal. Only future studies attempting to apply this description of science communication can resolve the issue.

Preliminary evidence suggests, however, that the fundamental point of mass media influence on scientific practice will stand. For example, in a study of research appearing in the *New England Journal of Medicine*, sociologist David Phillips and colleagues showed that articles which had been covered 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.⁶² And, 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 non-traditional forums. As in the case of cold fusion, researchers from other laboratories had to decide whether to wait for more stable, certain information or whether 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 process of the research itself.⁶³ Superconductivity represents the opposite pole to cold fusion: an unexpected finding in which the emergent consensus was 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 science studies is the effect of new communication technologies on the process of scientific inquiry.⁶⁴ This study suggests that one important issue is the degree to which scientific judgements are based on the stability of information. Cold fusion presents a particularly vivid example of the ways in which judgements changed depending on what information was available. Clearly the non-traditional 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 that they offered – used precisely because they provided an opportunity to resolve uncertainty and thus create stability more quickly than traditional contexts? We have 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 we don't yet have enough 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 technologies provided a surfeit of information; that this surfeit led to confusion and complexity; and that only when the surfeit was removed (by the mass media dropping out of the communication context) did the scientific community proceed to more stable

information and stable judgements (both among sceptics 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 stable judgements were formed. More information led to more instability, but also reduced the time until stability was achieved.

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 mishmash of changing data, of varying degrees of reliability, in various states of intelligent presentation. As the model presented above suggests, communication in science is a convoluted, irregular process. The pressure of e-mail and other forms of electronic communication *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.

An alternative scenario, running counter to my argument, would be that as active researchers reached stable judgements they needed less access to multiple communication contexts; according to this argument, stability would precede reduced complexity. However, the evidence available in the cold fusion case seems to me not to support this view. For example, as noted above, researchers such as David Williams actively chose to limit their use of multiple sources of information *in order* to help them achieve stability. Similarly, the dramatic drop in mass media coverage of cold fusion *preceded* the general technical consensus that emerged in the summer of 1989. Thus, while I acknowledge the sketchy nature of the evidence, and urge future researchers to examine this issue more closely, I believe the cold fusion case supports the view that more complex communication contexts lead, at least initially, to information instability. Only when the complexity is reduced can more stable judgements arise.

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

- Greater complexity could change the way in which people are

recruited into the scientific debates, since it changes the premium value of access to information, speed of response, and so on. (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 this happened.)⁶⁵

- 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.⁶⁶

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.

Again, one major alternative is that communication density decreases precisely because stability has been achieved. That is, causality runs the other way – complexity is used to achieve resolution. But, as noted above, in the cold fusion case this explanation does not seem to me to account for the perception of confusion experienced by the participants. It was only by closing off destabilizing channels of information that many researchers found stability.

If my interpretation that complexity leads to instability is correct, it carries implications about the role of new communication technologies. Commentators often talk about fax and e-mail in moral terms, claiming that they improve science, make it better.

But my interpretation suggests an ironic paradox: that fax and e-mail are essentially destabilizing, throwing researchers into greater uncertainty because of their co-existence with more stable forms of information, and because the reception and spread of information is not uniform. Only when fax and e-mail become less active does the science get 'packed down'. To use the moralists' terms, that does not mean that electronic communication is bad. It clearly speeds things up, and in science that may be good. But as science moves from *fax to facts*, it gains speed at the cost – at least temporarily – of stability. From a practical point of view, that would mean that everyone – scientists and analysts alike – needs to work especially hard at maintaining balance.

Given these potential implications, it seems worthwhile to present the argument for complexity leading to instability, and to challenge others to examine the issue with evidence from other areas of scientific inquiry.

• NOTES

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9. Key texts include: Robert K. Merton, *The Sociology of Science: Theoretical and Empirical Investigations*, ed. Norman W. Storer (Chicago, IL: The University of Chicago Press, 1973); Warren Hagstrom, *The Scientific Community* (New York: Basic Books, 1965); Jonathan Cole and Stephen Cole, *Social Stratification in Science* (Chicago, IL: The University of Chicago Press, 1973).

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11. Garvey's work was compiled as William D. Garvey, *Communication:*

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12. Key texts of the period included the two by Kriehbaum, both op. cit. note 1. Historical analyses of this period appear in Bruce V. Lewenstein, 'The Meaning of "Public Understanding of Science" in the United States after World War II', *Public Understanding of Science*, Vol. 1 (1992), 42–68; Dornan (1988), op. cit. note 3; and John Burnham, *How Superstition Won and Science Lost: Popularizing Science and Health in the United States* (New Brunswick, NJ: Rutgers University Press, 1987).

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15. See especially Shinn & Whitley (eds), op. cit. note 2.

16. Hilgartner, op. cit. note 3.

17. Hilgartner goes further, and argues that it is precisely by trying to understand why and how scientists try to identify such a boundary that we can get insight into the political activities that shape scientific knowledge. That aspect of his argument is important, but not necessary for my purposes in this paper.

18. CBS Evening News, 23 March 1989, on 'Vanderbilt TV Archives' videotape, CCFA. Unless otherwise indicated, all sources (including tapes of interviews) are available in the Cornell Cold Fusion Archive, Division of Rare and Manuscript Collections, Kroch Library, Cornell University, Ithaca, NY 14853, USA (hereafter, 'CCFA').

19. Jerry E. Bishop, 'Breakthrough in Fusion May Be Announced', *Wall Street Journal* (23 March 1989), B1. The 'thermonuclear' quotation was taken from the University of Utah's press release itself; '“Simple Experiment” Results in Sustained N-Fusion at Room Temperature for First Time' (press release, 23 March 1989, 'Pamela Fogle' folder, CCFA).

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21. Histories of cold fusion appear in various sources. See especially Bruce V. Lewenstein and Wolfgang Baur, 'A Cold Fusion Chronology', *Journal of Radio-analytical and Nuclear Chemistry*, Vol. 152 (1991), 279–98; Lewenstein, 'Cold Fusion and Hot History', *Osiris*, Vol. 7 (1992), 135–63; Frank Close, *Too Hot to Handle: The Race for Cold Fusion* (London: W.H. Allen; Princeton, NJ: Princeton University Press, 1991); John Huizenga, *Cold Fusion: The Scientific Fiasco of the Century*, revised and updated edition (Oxford, New York & Tokyo: Oxford University Press, 1993); Eugene Mallove, *Fire from Ice: Searching for the Truth Behind the Cold Fusion Furor* (New York: John Wiley, 1991); and Gary Taubes, *Bad Science: The Short Life and Very Hard Times of Cold Fusion* (New York: Random House, 1993).

22. Michael H. Salamon et al., 'Limits on the Emission of Neutrons, Gamma-

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24. Gary Taubes, 'Cold Fusion Conundrum at Texas A&M', *Science*, Vol. 248 (15 June 1990), 1299.

25. Close, op. cit. note 21; Mallove, op. cit. note 21.

26. Interview with Eugene Mallove, 8 November 1989, CCFA.

27. Steven Koonin interview, 16 November 1989, CCFA. 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.

28. The sci.physics bulletin board is one of thousands of bulletin boards available through the USENET computer network, a worldwide collection of 'newsgroups' used in the early 1990s by at least 1.4 million people. A rapidly expanding literature deals with the Internet and issues in 'computer-mediated communication' (CMC); useful introductions to and overviews of the status of the Internet and CMC in the early 1990s can be found in John S. Quarterman, *The Matrix: Computer Networks and Conferencing Systems Worldwide* (Bedford, MA: Digital Press, 1990); Lee Sproull and Sara Kiesler, *Connections: New Ways of Working in the Networked Organization* (Cambridge, MA: MIT Press, 1991); and Ed Krol, *The Whole Internet: User's Guide and Catalog* (Sebastopol, CA: O'Reilly & Associates, Inc., 1992).

29. 890401.sp.electronic file, CCFA.

30. 'Off the Record' videotape, CCFA.

31. 'Simple Experiment', op. cit. note 19.

32. Douglas Smith, 'Quest for Fusion', [*Caltech*]*Engineering and Science* (Summer 1989), 3–15.

33. Dennis Manos folder, CCFA.

34. All of these are in the 'Preprints' folders, CCFA. The Jones article is S.E. Jones et al., 'Observation of Cold Nuclear Fusion in Condensed Matter', *Nature*, Vol. 338 (27 April 1989), 737–40.

35. Ron Dagani, 'Hopes for Cold Fusion Diminish as Ranks of Disbelievers Swell', *Chemical & Engineering News* (22 May 1989), 8–20, on 13.

36. Garwin's referee report is in the 'Richard Garwin' folder, CCFA. His article was Richard Garwin, 'Consensus on Cold Fusion Still Elusive', *Nature*, Vol. 338 (20 April 1989), 616–17.

37. Jennings Bryant and Richard L. Street, Jr, 'From Reactivity to Activity and Action: An Evolving Concept and Weltanschauung in Mass and Interpersonal Communication', in Robert P. Hawkins, John M. Wiemann and Suzanne Pingree (eds), *Advancing Communication Science: Merging Mass and Interpersonal Process* (Newbury Park, CA: Sage, 1988), 162–90; and Brenda Dervin, 'Audience as Listener and Learner, Teacher and Confidante: The Sense Making Approach', in Ronald E. Rice and Charles K. Atkin (eds), *Public Communication Campaigns* (Newbury Park, CA: Sage, 2nd edn, 1989), 67–86.

38. David Ansley, taped interview, 18 November 1989, CCFA. See also Ivan Amato, taped interview, 11 August 1989, CCFA; and Kelvin Lynn, taped interview, 12 July 1989, CCFA.

39. Enzo Bertolini to D. Mead, 8 May 1989, 'Dennis Manos' folder, CCFA.

40. Messages from John Gilmore and Matt Ginsberg in 890405.af electronic file, CCFA.

41. NBC Nightly News, 13 April 1989, on 'Vanderbilt TV Archives' videotape, CCFA.

42. Lee Dye, 'Stanford Lab Adds Support to Table-Top Fusion Claims', *Los Angeles Times* (19 April 1989), 3, 17; Dye, 'Fusion Scientists Delay Publication of Research', *ibid* (21 April 1989), 3, 28; and Dye, 'Scientists Grow Frustrated Over Fusion Claim', *ibid* (23 April 1989), 1, 43. According to the physicist Frank Close, a series of previously scheduled meetings held around the country on 23–24 April played a major role in creating a social consensus about what had been individual doubts regarding the experiments; see Close, *op. cit.* note 21, 157–61.

43. 'APS Press Conference', audiotapes, CCFA; 'APS, TV news excerpts', videotapes, CCFA; Phillip Schewe to William Havens, 22 May 1989, in 'James Krumhansl [Part 1]' folder, CCFA; Associated Press, 'More Scientists Cool to Fusion Claims', *San Jose Mercury News* (3 May 1989), 7A; and Malcolm W. Browne, 'Fusion Claim is Greeted with Scorn by Physicists', *New York Times* (3 May 1989), A1.

44. Richard Petraso et al., 'Problems with the Gamma-Ray Spectrum in the Fleischmann et al. Experiments', *Nature*, Vol. 339 (18 May 1989), 183–85.

45. *Time* (8 May 1989); *Newsweek* (8 May 1989). Both major newsmagazines are dated at the end of the week in which they appear.

46. Robert Pool, 'Cold Fusion: End of Act I', *Science*, Vol. 245 (2 June 1989), 1039–40.

47. Data from *Newspaper Abstracts Online*, a CD-ROM database produced by University Microfilms, Inc., Ann Arbor, Michigan. For further details, see Lewenstein (1992), *op. cit.* note 21, 155.

48. See, for example, copies circulated at the National Science Foundation, in the 'Diane Weisz' folder, CCFA.

49. Draft interim report of the Cold Fusion Panel to the Energy Research Advisory Board, typescript, 13 July 1989, 'Steven Jones' folder, CCFA.

50. Brookhaven National Laboratory, 'Seminar notes' folder, CCFA.

51. 'Morrison newsletter' folder, CCFA.

52. David Williams, taped interview, 11 April 1990, CCFA.

53. Charles Martin, taped interview, 17 July 1989, CCFA.

54. P.K. Iyengar and M. Srinivasan (eds), *BARC Studies in Cold Fusion* (Trombay, Bombay, India: Bhabha Atomic Research Centre, December 1989) (copy in 'Papers' folder, CCFA); and Will et al. (eds), *op. cit.* note 23.

55. Mallove, *op. cit.* note 21, 210, 226.

56. M. Fleischmann et al., 'Calorimetry of the Palladium-Deuterium-Heavy Water System', *Journal of Electroanalytical Chemistry and Interfacial Electrochemistry*, Vol. 287 (25 July 1990), 293–348.

57. Salamon et al., *op. cit.* note 22.

58. For details, see Lewenstein (1992), *op. cit.* note 21.

59. Close, *op. cit.* note 21, and Mallove, *op. cit.* note 21.

60. A subsequent investigation by SRI indicated that a build-up of pressure in a sealed container was the most probable cause of the explosion; see SRI folder, CCFA.

61. My initial collaborators on the Cold Fusion Archive, Tom Gieryn and William Dougan, were drawn to the case because of its similarities to the 'Internet

worm' case of 1988, when a Cornell graduate student created a piece of software that crippled the Internet for a few days; Gieryn and Dougan studied the construction of an image of the perpetrator via the net.

62. David P. Phillips et al., 'Importance of the Lay Press in the Transmission of Medical Knowledge to the Scientific Community', *New England Journal of Medicine*, Vol. 325 (17 October 1991), 1180–83; Elizabeth S. Clemens, 'Of Asteroids and Dinosaurs: The Role of the Press in the Shaping of Scientific Debate', *Social Studies of Science*, Vol. 16 (1986), 421–56; William Glen (ed.), *The Mass Extinction Debates: How Science Works in a Crisis* (Stanford, CA: Stanford University Press, 1994).

63. Robert M. Hazen, *The Breakthrough* (New York: Summit Books, 1988); Bruce Schechter, *The Path of No Resistance* (New York: Simon & Schuster, 1989); Ulrike Felt, 'Fabricating Scientific Success Stories,' *Public Understanding of Science*, Vol. 2 (1993), 375–90; Helga Nowotny and Ulrike Felt, *High Temperature Superconductivity: A Research Field in the Making* (forthcoming).

64. Bruce V. Lewenstein and Lisa Heinz, *Changing Culture of Research: Processes of Knowledge Transfer*, contractor report to US Congress, Office of Technology Assessment, December 1992; Teresa M. Harrison and Timothy D. Stephen (eds), *Computer Networking and Scholarship in the 21st Century University* (Albany, NY: State University of New York Press, 1994).

65. Bruce V. Lewenstein, 'Do Public Electronic Bulletin Boards Help Create Scientific Knowledge? The Cold Fusion Case', *Science, Technology, & Human Values*, Vol. 20, No. 2 (Spring 1995), 123–49.

66. Marcel C. LaFollette, 'Scientists and the Media: In Search of a Healthier Symbiosis', *The Scientist* (9 July 1990), 13, 15.

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