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Competition Among Scientific Disciplines in Cold Nuclear Fusion Research

The Argument

In the controversy in 1989 over the reported achievement of cold nuclear fusion, parts of the physics and chemistry communities were opposed in both a theoretic and a professional competition. Physicists saw the chemists' announcement as an incursion into territory allocated to their own discipline and strove to restore the interdisciplinary boundaries that had previously held. The events that followed throw light on the manner in which scientists' knowledge claims and metascientific beliefs are affected by their membership of disciplinary communities. In particular, the controversy offers evidence for a constructivist reinterpretation of the "division of nature into levels," which is customarily held to underpin the division of science into disciplines.

1. Cognitive Sociology and Disciplinary Phenomena

Phenomena arising from the division of science into separate disciplines have been extensively studied by historians and sociologists of science, in several waves.

Among the earliest studies were those contributing to "interest theory," or the sociological explanation of scientists' general behavior by appeal to their professional interests. These studies focused heavily on competition among practitioners within single disciplines. Such competition is fueled by the practitioners' knowledge that reward is made available in only limited amount by the professional organizations that govern the discipline. It frequently centers on priority of discovery, or of announcement of discovery, because such priority has come to constitute an important determinant of the distribution of rewards within science.[1]

While competition within individual disciplines received close attention from studies

[1] Sociological studies of competition among practitioners of a discipline are Gaston 1973, 69–93, and Hagstrom 1974. Further remarks on competition and priority disputes are contained in Lamb and Easton 1984, 145–59.

of this kind, cases of competition between disciplines remained less well documented.[2] In this competition, practitioners of a discipline act as a corporate body to promote the perceived interests of their disciplinary community in the face of competing claims or maneuvers by the practitioners of another discipline.[3]

With the rise of cognitive sociology of science, or "sociology of scientific knowledge" in a strong sense, disciplinary phenomena came under attention from a new perspective.[4] Some studies concentrated on the demarcation of the discipline of "science" as a whole from nonscience. This raised appreciation of, among other things, the importance to scientists of "boundary-work," the rhetorical and other strategies to create and maintain a favorable public image for science by contrasting it with other practices (Gieryn 1983; Fuller 1988, 175–89).

Closer to the present paper are those studies in cognitive sociology of the boundaries and interactions between disciplines within science. Such studies share with older sociological approaches the insight that disciplinary communities in science can act as units to further perceived corporate interests, but thereafter the questions they pose deviate from those of earlier workers. In particular, classical sociology of science would have inquired what influence the corporate behavior of disciplines exerts on such things as allocations of funding or career advancement; by contrast, the new approaches investigate the effects of corporate behavior on the content of the knowledge claims of scientists (see for example Fuller 1988, 191–206; 1989, 20–25). This is the project to which I shall here attempt to contribute.

This paper studies some aspects of the controversy, which developed mainly in the United States, provoked by the reported observation of the occurrence of low-temperature nuclear fusion in the spring of 1989. The overt controversy was opened by the first public announcement of an observation on 23 March. It was resolved partially by the Workshop on Cold Fusion Phenomena at Santa Fe in May and to a further degree by the report of the U.S. Department of Energy's expert panel in November. Accordingly, this paper considers mainly events in the period March–November 1989.[5]

The features of the controversy that concern this paper are its disciplinary aspects. I aim to show that the controversy was, in part, a competition between at least some sections of the disciplinary communities of physicists and chemists, and that certain events in the controversy cannot satisfactorily be understood without reference to this disciplinary dimension. For instance, the cause of one of the strands of the controversy is reconstructed in my account as the physics community's apprehension that the chemistry profession had laid claim to the investigation and possible exploitation of

[2] Among "interest-theory" studies of competition between disciplines are Storer 1972, 261–64; Spiegel-Rösing 1974, 20–34; Becher 1989, 141–43.

[3] For evidence that practitioners of a discipline are capable of acting as a corporate body, see Elias, Martins, and Whitley, eds. 1982; Whitley 1984.

[4] For a recent survey of work in cognitive sociology, see Barnes 1990.

[5] Given the importance of chronology in this discussion, references to periodical articles published in 1989 specify the day or month of the issue in which they appeared.

nuclear fusion, a domain that had previously been the preserve of physicists. This disciplinary competition was conducted on two levels: a theoretic level, at which opposing knowledge claims were matched against one another, and a professional level, at which the struggle was for public esteem and financial resources.

This account will advance the following four main theses. First, the participants' responses to the cold fusion claims were colored by the professional interests of the communities of physics and chemistry. Second, in consequence, the claims attracted corporate responses on the part of the participants. Third, the participants' knowledge claims appear to have been prompted in part by their disciplines' roles in the controversy. Fourth, the episode suggests that certain metascientific tenets that are generally held to underlie and ground the division of scientific practice into disciplines are better seen as the outcome of social construction rather than as a reflection of perceived reality. Theses 1–3 are addressed in section 9, while the fourth is discussed in section 10.

I do not claim that disciplinary affiliations were the sole influence on the participants' knowledge claims: the limits beyond which appeal to participants' disciplinary membership cannot hope to explain their behavior will be set out below. Equally, I do not suggest that the disciplinary phenomena of the cold fusion episode are the only noteworthy ones: they are simply the component of the controversy on which I shall here focus.[6]

2. Fusion Physics in 1989

Up to March 1989, research in nuclear fusion had been considered to fall entirely within the domain of the profession of physics. The strategies pursued and the equipment employed in the attempt to achieve controlled nuclear fusion were typical of twentieth-century "big physics," similar to the strategies and machinery used in high-energy particle physics.[7]

The physicists' plans for nuclear fusion involved confining and heating a plasma, an electrically neutral gas of ions and free electrons. Two methods to contain the plasma were being explored: magnetic and inertial confinement. Magnetic confinement was first attempted in the early 1950s and was in 1989 the approach pursued more intensively. In this technique the plasma is heated and compressed while being confined in a magnetic field, inside a toroidal vessel known as a tokamak. The idea of confining the plasma by inertial means was a spin-off from laser research in the mid-1960s and

[6] For instance, another noteworthy aspect of the cold fusion episode is the use of the popular media and other unorthodox channels of communication to disseminate scientific data and claims (see Lewenstein 1990).

[7] The ways in which the origin of fusion research is rooted in various branches of physics are discussed in Hendry 1987. Histories of fusion research up to recent years are Heppenheimer 1984 and Herman 1990. A brief overview of the state of research in high-temperature fusion immediately before the beginning of the cold fusion controversy may be found in Horgan 1989 (February).

was by the 1980s closely allied with military research. In this approach a small, spherical capsule of deuterium and tritium is subjected from all directions to laser radiation; the resulting implosion makes the fuel so dense and hot as to ignite fusion reactions. By 1989 no magnetic or inertial confinement facility had been able to ignite a self-sustaining fusion reaction.[8]

Each of these approaches requires massive works of engineering. To convey the extraordinary scientific efforts involved, it may be sufficient to say that if economically useful fusion is to be achieved by inertial confinement, a pellet of fuel must reach almost 100 million Kelvin and a pressure of several billion atmospheres, conditions typical of the center of the sun (Craxton, McCrory, and Soures 1986, 63).

Because of the magnitude of the engineering tasks, progress in fusion physics depended on a great provision of funds by the state. At the end of the 1970s the U.S. administration attached high priority to the achievement of controlled nuclear fusion, especially in the light of the Arab oil embargo; but the project's perceived importance waned during the 1980s, a decade of high federal budget deficits. Thus by 1989 U.S. funding for fusion research had been cut by nearly 50 percent since its peak in the late 1970s, to just over $500 million per year, of which $350 million was devoted to the magnetic confinement program. In that year, M. Crawford summed up the financial situation facing fusion physicists as follows:

> With the federal budget under pressure, it will be increasingly difficult to persuade Congress to pump more resources into a program that is not expected to make significant contributions to the nation's electrical grid before the middle of the next century (Crawford 1989 [14 April], 138).

In addition, competing teams of investigators within the physics community in the United States had toward the end of the decade begun to disagree over how the dwindling resources should be spent. There was for instance some degree of competition for funds between teams pursuing magnetic and inertial confinement. In this conflict, the magnetic confinement program appeared to fare worse: during the five years up to 1989, Congress had kept its annual funding constant in monetary terms, and at the end of the 1980s the Department of Energy resolved to transfer some funds from the magnetic to the inertial confinement program.[9]

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[8] For an account of techniques of magnetic confinement, see Conn 1983; Stacey 1984, 1–19. For an account of those of inertial confinement, see Craxton, McCrory, and Soures 1986.

[9] The state of U.S. funding for high-temperature fusion research in 1989, and issues arising therefrom, are described in Crawford 1988; Horgan 1989 (February), 15; and Crawford 1989 (14 April). U.S. funding of fusion research is further discussed in Lindley 1989 (25 May); Lindley 1989 (12 October); *Nature* 1989 (16 November). Political and organizational aspects of the U.S. magnetic-confinement fusion program in earlier periods are examined in Bromberg 1982a and 1982b.

3. The Chemists' Report

These disagreements internal to the community of physics were in March 1989 swept into insignificance by the announcement by a team of chemists that they had achieved electrochemically induced nuclear fusion at room temperature on a laboratory bench with apparatus costing a few thousand dollars.[10] To comprehend the degree to which the announced results were alien to the research programs that had been pursued by the community of physics, it is helpful to review some details of the claims made by the chemists.

The first claim of the observation of cold nuclear fusion was advanced by M. Fleischmann and S. Pons, members of the Department of Chemistry of the University of Utah.[11] They reported that they had placed in a container of heavy water two electrodes, one of palladium to act as the cathode (negative) and one of platinum serving as the anode (positive terminal). They dissolved lithium hydroxide in the water to provide ions to carry a current between the electrodes. By electrolysis, the heavy water separated into oxygen, which collected at the platinum electrode, and deuterium, which accumulated at the palladium electrode. Palladium has a great capacity to absorb hydrogen, so the concentration of deuterium gradually built up to a high level within the cathode. Pons estimated that during the experimental runs the electrodes in his cells contained two or more deuterium atoms for every palladium atom (cited in Pool 1989 [31 March], 1662).

Fleischmann and Pons reported that, once the concentration of deuterium had built up to a sufficient level, the palladium electrode began to give off heat. They said that it gave off an amount of thermal energy considerably greater than the amount of electrical energy put in as current: in their best-performing cells, Pons said, a 4.5-watt thermal output was generated by a 1-watt input (cited ibid.). In another measurement, Fleischmann and Pons reported that they had evolved heat in excess of their input energy at a rate of more than 10 watts per cubic centimeter of palladium for longer than 120 hours. The cell in one of the experiments of Fleischmann and Pons generated so much heat that "a substantial portion of the cathode fused (melting point 1554°C), part of it vaporized, and the cell and contents and a part of the fume cupboard housing the experiment were destroyed" (Fleischmann, Pons, and Hawkins 1989 [10 April], 305).

[10] Among chronicles of the cold fusion controversy are Peat 1989; Close 1990; Mallove 1991. In languages other than English, see for example Belloni 1989; Briand and Froment 1990. A chronology of events is in Lewenstein 1991, 4–28. Many documents relating to the controversy are preserved at the Cornell Cold Fusion Archive, collection no. 4451, Department of Manuscripts and University Archives, Cornell University Library.

[11] Fleischmann and Pons first announced their observations in a press conference in Salt Lake City on 23 March; the earliest newspaper report is Cookson 1989 (23 March). The first scientific paper of the University of Utah group is Fleischmann, Pons, and Hawkins 1989 (10 April); see also their 1989 (10 May). A discussion of the technical details of the experiments of the University of Utah group, as well as of those of Jones to be mentioned below, is contained in Levi 1989 (June). The term "cold fusion" had previously been applied to the phenomenon — quite separate from that which Fleischmann and Pons claimed to have observed — of muon-induced nuclear fusion; on this, see Rafelski and Jones 1987.

Such a heat output is so large that it was hard to imagine what besides nuclear processes could be its source. Accordingly, Fleischmann and Pons explained these observations by the hypothesis that nuclear fusion of deuterium was taking place within the palladium electrode.

However, heat generation on its own is not considered evidence sufficient to demonstrate that nuclear fusion has occurred. There are two common modes of deuterium-deuterium fusion, which occur with roughly equal frequencies, liberating the energies shown (here expressed in mega-electronvolts):

$$H^2+H^2 \rightarrow He^3+n \ (3.27 \text{ MeV})$$
$$H^2+H^2 \rightarrow H^3+p \ (4.03 \text{ MeV})$$

The neutrons released in the first mode have an energy of 2.45 MeV, and their capture by hydrogen nuclei — such as those present in the water bath that in the University of Utah experiment surrounded the electrolytic cell — yields gamma rays with an energy of 2.22 MeV. To support their contention that deuterium-deuterium fusion was occurring in their cells, Fleischmann and Pons had therefore tested for production of H^3, for a neutron flux, and for gamma-ray emissions.

Fleischmann and Pons claimed they had detected the emission from the container of some neutrons with energy of 2.45 MeV, and of 2.22 MeV gamma rays. Qualitatively, these observations would have tended to support the suggestion that nuclear fusion had occurred. However, Fleischmann and Pons' heat flux seemed greatly out of proportion to the neutron flux which they reported. They claimed the observation of a neutron flux of 10^4 $cm^{-3}s^{-1}$; but if deuterium-deuterium fusion were responsible for the heat production and if half the fusion reactions produced neutrons, one would expect on established theory a neutron flux of the order of 10^{13} $cm^{-3}s^{-1}$. Indeed, to generate by standard deuterium fusion as much heat as the University of Utah group claimed would have produced a neutron flux lethal to any researchers in the laboratory. Fleischmann and Pons seemed to conclude from the discrepancy that they had discovered a hitherto unknown fusion reaction. The shortfall in the neutron flux was a puzzle, some implications of which will be mentioned in the next section.

The suggestion that nuclear fusion could be initiated by the processes utilized by Fleischmann and Pons was, on established physical theory, very surprising. That theory predicts that very high temperatures — such as may be achieved in a tokamak — are necessary for fusion to occur. The grounds for this prediction are, in brief, the following. For fusion to take place, deuterium nuclei must approach each other very closely. However, because of their positive electrostatic charge, such nuclei are generally separated by distances too great for fusion to occur with appreciable probability. At high temperature, the nuclei's velocities are sufficiently great for them to undergo close collisions with one another despite their electrostatic repulsion. At high enough temperatures, the collisions are so close and frequent that a self-sustaining fusion reaction is initiated. To explain why such temperatures were seemingly not necessary for fusion to occur in their apparatus, Fleischmann and Pons conjectured that for some reason the deuterium nuclei inside the palladium did not repel each other as strongly as they did in free space, and thus approached each other sufficiently closely for the probability of their fusing to become reasonably high.

A short while after the press conference in which Fleischmann and Pons broadcast their claims, a team of physicists and chemists led by S. E. Jones of Brigham Young University, at Provo, also in Utah, announced that they had made similar observations of electrochemically induced low-temperature nuclear fusion.[12] They stated that, upon passing an electric current through palladium or titanium electrodes immersed in an electrolyte of deuterated water and various metal salts, they had detected the generation of some excess heat and of a small but significant flux of neutrons. They suggested that fusion of deuterons within the metal lattice may have been the explanation.

While the University of Utah group came to be perceived by physicists as outsiders who had made an undesirable incursion into their territory, the group at Brigham Young never prompted this reaction. There are two reasons for this difference. First, despite the fact that both groups relied on data from electrolytic cell experiments, which are typical of electrochemistry, the Brigham Young group included some who described themselves as physicists rather than chemists, and could therefore not easily be regarded by other physicists as outsiders. Second, there were various discrepancies between the technical claims of the University of Utah and Brigham Young groups, which made the former far more threatening to some physicists' perceived interests than the latter. The crucial difference in this regard was that the two groups disagreed over the amount of excess heat they had detected: the Brigham Young group claimed to have generated much less heat than that reported by the University of Utah group. If the results of Fleischmann and Pons had been confirmed, it is at least plausible that they would have allowed substantial progress toward the commercial exploitation of nuclear fusion; on the other hand, if the effects claimed by Jones were to be verified, they would be of a magnitude sufficient to answer some longstanding geophysical and astronomical questions — concerning, for instance, the relative abundance of He^3 and He^4 in the earth's crust — but would be too small to permit electricity generation at commercially efficient rates. The emphasis of the two papers is correspondingly different: while the paper of Fleischmann and Pons explicitly addresses the issue of commercial energy generation, that of Jones appears much more concerned with the implications of his findings for geophysics.[13] Jones was later to reiterate that he saw cold nuclear fusion "as an interesting piece of physics, not as a technology for energy production" (reported in Lindley 1989 [4 May]). Because of the difference between the degree of potential technological impact of the two reports, and the consequent difference between the emphasis the two groups gave to their claims, most of the physicists' and chemists' subsequent interest, and the physicists' animosity, was directed at the claims of the University of Utah group rather than at those of the Brigham Young group.

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[12] The results claimed by the Brigham Young group are contained in Jones et al. 1989 (27 April).

[13] Details of the paths by which the two groups came to conceive of their experiments are contained in Pool 1989 (7 April) and 1989 (28 April).

The news from the two groups excited immense interest in low-temperature fusion, both in the scientific community and in the general press. Many teams in laboratories world-wide set about attempting to replicate the electrolytic cell experiments; over the next few months confidence in the reality of cold fusion rose and fell.[14]

4. Chemistry: Anomaly and Opportunity

Among the salient features of the University of Utah group's cold fusion claims was the fact that these had been advanced not by physicists working along the lines enshrined by the fusion physics establishment but by chemists who had proceeded along lines suggested by electrochemistry. This circumstance was reflected in differences in the ways in which the communities of chemistry and physics responded to the claims.

The chemistry profession perceived in the claims of cold fusion both a theoretic anomaly and a professional opportunity. The anomaly consisted in the fact that the claimed results appeared to violate well-entrenched principles of physical science; the opportunity was offered by the implications of the fact that chemists appeared to have achieved a notable and lucrative technical goal that physics had set itself, and in this to have surpassed the accomplishments of physicists themselves. In this section I will discuss the chemists' responses, first to the anomaly and then to the opportunity.

The interest of a novel theoretic claim is enhanced if it conflicts with a modest part of the corpus of established science, perhaps showing under which conditions a well-entrenched theory ceases to hold. However, its credibility is eroded if it appears uncompromisingly at odds with large bodies of theory that have generally proved successful on other occasions. As M. Polanyi has noted:

> The professional standards of science . . . must demand that, in order to be taken seriously, an investigation should largely conform to the currently predominant beliefs about the nature of things, while allowing that in order to be original it may to some extent go against these. (Polanyi 1969, 54–55)[15]

The chemists' cold fusion claims appeared vulnerable to the charge of flying in the face of accepted theory in a wholesale manner, rather than merely demonstrating their own novelty by prompting the emendation of accepted theory in certain areas. Other scientists would have regarded this excess of unorthodoxy with satisfaction or anxiety, depending on their attitude toward the claims. Those who, for whatever reason, wished to see the claimed cold fusion results dismissed by the community might have found it helpful to their purposes to depict them as conflicting very severely with

[14] On the attempts at replication, see for example *Nature* 1989 (6 April); *Nature* 1989 (13 April); Pool 1989 (14 April); *Nature* 1989 (20 April); Rich 1989 (20 April); *Nature* 1989 (27 April); Lindley 1989 (11 May); Levi 1989 (June), 18–19.

[15] Some historical studies on the response to anomalous scientific claims are contained in Mauskopf, ed. 1979.

well-entrenched physical theory. On the other hand, those who had formed a commitment to the claims perceived it necessary to show that they could broadly be accommodated within current science. Members of the chemistry profession lay generally within the latter division of the community, and attempted to show how the claims reported by the University of Utah group could be held compatible with well-entrenched physical principles.

The supportive stance of the chemistry community came to light in, for instance, the discussion of the neutron flux measurements reported by the University of Utah group. Among the claims of Fleischmann and Pons, the one which most troubled their supporters was their assertion that the processes taking place within the palladium electrode yielded far fewer neutrons than physical theory predicted for the nuclear fusion reactions they conjectured were occurring. This claim appeared inimical to the chemists' case, in light of the fact that the detection and measurement of neutron fluxes had in other circumstances become established as a standard test to determine whether nuclear fusion had been achieved. It was obvious that, if the University of Utah team persisted in claiming both to have achieved nuclear fusion and to have failed to detect a neutron flux of the expected magnitude, the community's response might be to lend credence to the latter claim and disbelieve the former. The chemists' case therefore awaited some account of the shortfall of the neutron emissions that would appear in harmony with established physical theory.

Such an account was proposed by J. Bockris, a member of the Department of Chemistry at Texas A&M University (reported in Pool 1989 [7 April], 27). If in the interior of the palladium electrode, Bockris suggested, the relative probabilities of the two modes of deuterium-deuterium fusion were for some reason perturbed, and the mode that releases He^3 and a neutron became much less likely than the one resulting in H^3 and a proton, a large number of fusion reactions could take place with the production of only a few neutrons. By means of this account, consistency could be established between the claim that the process taking place within the palladium electrode was a nuclear fusion reaction and the claim that an unexpectedly low neutron flux had been observed.

So much for the chemists' perception of and reaction to the theoretic anomaly. But the anomaly was accompanied by a professional dimension. The professional opportunity that chemists perceived in the cold fusion claims, and a contributory reason why some of them labored to enhance the acceptability of the reports, lay largely in the implications of those reports for the public standing of chemistry.

Traditionally, the science of chemistry has been seen by lay persons as less attractive than other sciences, especially physics. Around the time of the cold fusion controversy, the chemist P. W. Atkins was expressing the frustration felt by him and his colleagues at the popular image of their discipline:

> Is any scientific subject perceived as more inaccessible to the general population than chemistry? Physics is tangible: you can kick it, push it, roll it, feel it, and switch it on and off.... But chemistry?... Which of our common rooms has ever

buzzed with chatter about chemistry? Who knows what question to ask next in a conversation with a chemist *qua* chemist? (Atkins 1990)[16]

The cold fusion reports looked set to transform chemistry's usual state of subordination. Two chemists had apparently paved a cheap and easy route to the goal of commercially exploitable nuclear fusion — for which physicists had been striving for forty years, all the while declaring their need of huge machines costing hundreds of millions of dollars. It seemed that chemists *qua* chemists had answered questions that had evaded everyone else: now chemistry provoked conversation indeed. It was not hard to envisage that, if the reports received confirmation, the standing of the profession of chemistry would rise substantially in the eyes of the public.

The pride of the chemical profession soon became evident. On 19 April the national meeting of the American Chemical Society (ACS) at Dallas held a special forum on cold fusion, attracting 7,000 people in a basketball arena, at which Pons spoke.[17] At the meeting, chemistry was depicted as having generated an elegant and untaxing solution to a problem that had resisted the considerably more lumbering attack of the physicists. Reports speak of an atmosphere of triumphant acclaim for the achievements of chemistry:

> Chemists welcomed the prospect that cold fusion might represent a victory for chemistry over physics. Opening the special session, ACS president Clayton Callis said the goal of fusion as an energy source has remained elusive, and that physicists' efforts at hot fusion using tokamaks and lasers were "apparently too expensive and too ambitious to lead to practical power." To applause from the crowd, he added, "Now it appears that chemists have come to the rescue." (*Nature* 1989 [20 April])

The contrast between the physicists' and the chemists' methods was drawn by appeal to rather effective iconography. R. Pool takes up the story:

> Pons summed up the different approaches of chemists and physicists with a single slide. In an earlier presentation Furth [director of the Princeton Plasma Physics Laboratory] had shown a slide of Princeton's tokamak, a mammoth machine covered with pipes and wires that is still a couple of years away from a break-even fusion reaction. Pons in turn flashed a picture of his own — a simple jury-rigged device in a plastic dishpan that supposedly creates a sustained energy-producing fusion reaction. "This is," he deadpanned, "the U-1 Utah tokamak," and the chemists loved it. (Pool 1989 [21 April, a], 285)[18]

[16] For a similar and nearly simultaneous description of chemistry's public image, see Beall and Berka 1990, 103.

[17] Accounts of the ACS meeting at Dallas are carried in *Nature* 1989 (20 April); Pool 1989 (21 April, a).

[18] A photograph of the University of Utah group's apparatus, standing in a domestic bucket still bearing the manufacturer's label, is reproduced in Pool 1989 (21 April, a), 284. A standard photograph of the Princeton tokamak appeared on the front cover of *Physics Today*, vol. 39, no. 11 (November 1986).

While part of the chemists' satisfaction was probably caused simply by the thought that the profession had at last eclipsed the traditionally more glamorous discipline of physics, considerable financial interests underlay the chemists' maneuvers to secure recognition for their apparent achievements. Immediately after the first announcement of the claims of Fleischmann and Pons, both the U.S. Department of Energy and the State of Utah provided increased funding for their university department.[19] Further government interest in the results became apparent when on 19 April the federal energy secretary, J. D. Watkins, asked his department's Energy Research Advisory Board to draw up a report on the cold fusion claims that would *inter alia* "identify what R&D direction the DOE should pursue to fully understand these phenomena and develop the information that could lead to their practical application."[20]

Congress too seemed minded to upgrade the public standing of the chemistry community, at the expense of that of physics. The state of Utah had laid plans to establish a $100-million National Cold Fusion Institute, attached to the University of Utah (see Pool 1989 [14 April], 144; Lindley 1989 [17 August]). At an early date the state provided $5 million of the amount required, and on 26 April, Fleischmann and Pons, accompanied by officials of the University of Utah, attended a hearing of the Science, Space, and Technology Committee of the House of Representatives to request from Congress a further sum of between $25 and $40 million (see Lindley 1989 [4 May]; Crawford 1989 [5 May]; Goodwin 1989 [December], 44). Fleischmann and Pons told the committee that they were "sure as sure can be" that cold fusion worked (cited in Lindley 1989 [4 May]), and accounts were presented of initial confirmatory results obtained in other laboratories. The committee members talked of "startling possibilities" and of "changing the course of mankind."[21]

The starkest sign that chemistry was winning professional territory previously occupied by physics had in fact been given even before the hearing of the full committee on 26 April. A few weeks earlier — on the urging of Representative R. S. Walker, the senior Republican member of the committee — its subcommittee on energy research and development had resolved to reallocate $5 million from the long-established high-temperature fusion budget to cold fusion research.[22] This decision might have been seen by physicists as well as by chemists as the harbinger of the future enviable standing and influence of the chemistry profession.

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[19] For the university's moves to secure increased funding, see Dolan 1989 (31 March); *Nature* 1989 (6 April); *Nature* 1989 (20 April). For background information on the perceived implications for the state of Utah of the cold fusion claims, see *The Economist* 1989 (22 April). On business interest in cold fusion, see Dolan 1989 (31 March); Pool 1989 (21 July); Holden 1989 (15 September).

[20] Watkins' charge letter is reproduced in ERAB 1989 (November), 39, where the phrase quoted appears. On the establishment of the expert panel, see Crawford 1989 (5 May); Goodwin 1989 (December), 44.

[21] Skepticism about the possibilities of commercially exploiting cold fusion was expressed in Crawford 1989 (28 April).

[22] On the decision of the Energy Research and Development Subcommittee, see Goodwin 1989 (December), 44. The reallocation of funds was however reversed at a later date by the full committee.

5. Physics: Anomaly and Threat

The view of the community of physicists — which mirrored that of the chemists — was that, as a result of the cold fusion claims, they faced a theoretic anomaly and a professional threat. The anomaly for the physicists, as for the chemists, consisted in the fact that the results reported by the University of Utah group conflicted with current physical theory; the threat was posed by the fact that these anomalous results had been reported by chemists rather than by physicists, and that this development appeared to constitute an encroachment into the physicists' own subject matter and foreshadow harm to the standing and interests of the physics profession. The chemists had performed an incursion not only in so far as their topic of research had been nuclear fusion, but also in as much as some of their data-gathering techniques (relying on neutron counts and gamma-ray spectra) were alien to standard electrochemistry. The physics community expended great effort in neutralizing both the anomaly and the threat.

Of course, not every physicist regarded the chemists' announcement as a professional threat to the same degree. If low-temperature fusion had been demonstrated possible, this would clearly have had a greater professional impact on those physicists working on the conventional approaches to fusion than on, say, solid-state physicists or cosmologists. While the life's work of the latter would remain almost completely untouched, that of the former would probably be suppressed. Consequently, the need to oppose chemistry in the disciplinary competition was felt not by the entire physics community, but only — or more urgently — by those subdisciplines that had interests in fusion. Chief among these was the subdiscipline of plasma physics; other subdisciplines hardly entered the controversy. Indeed, some theoretical physicists seemed disposed to entertain the chemists' claims, at least to the extent of sketching new theories to explain how fusion might occur at low temperatures. For instance, P. L. Hagelstein, a theoretical physicist at MIT, assuming the genuine occurrence of nuclear fusion, attempted to explain it as the effect of so-called coherent nuclear reactions (Hagelstein 1990; see also Mallove 1991, 118–30).

Even so, the developments in cold fusion were seen as a threat by many more physicists than those directly engaged in plasma or laser research. After all, when a "big physics" project is curtailed, many "little physics" projects — those lying in areas of the discipline that though apparently distant are tributaries of the major effort — suffer too. The effect of cuts in funding imposed in a subdiscipline possessing such intricate relations to other areas of the discipline as does fusion physics would thus undoubtedly be felt over a wide front of subdisciplines of physics.

Those physicists who became involved in the controversy reacted to the theoretic anomaly through two main strategies.

One consisted in attempting to establish that measurements on electrolytic cells performed using equipment and procedures closely similar or considered superior to those described by the University of Utah team failed to detect evidence for nuclear fusion that was either convincing or of the magnitude reported. Groups reported that experiments using calorimeters of various designs, several neutron and gamma-ray detection techniques, and electrodes in a wide range of materials failed to uncover evidence to support the claims of cold fusion advanced by the University of Utah team. They tended further to suggest that the chemists' claims of the occurrence of nuclear fusion might have been prompted by spurious effects such as noise from neutron counters, variations in the cosmic-ray background, calibration errors in calorimeters, or the variable electrolytic enrichment of tritium.

For instance, a team based at the Plasma Fusion Center of MIT argued that the observations reported by Fleischmann and Pons did not amount to an observation of gamma rays of the energy consistent with the occurrence of nuclear fusion (Petrasso et al. 1989 [18 May]).[23] This was a weighty criticism, which many onlookers found persuasive. Other notable contributions of this kind were published by S. E. Koonin and M. Nauenberg, members of the Institute for Theoretical Physics, University of California at Santa Barbara, and by a group working at the A. W. Wright Nuclear Structure Laboratory, Yale University (see, respectively, Koonin and Nauenberg 1989 [29 June] and Gai et al. 1989 [6 July]). Yet another appeared in November, from a group based at the Harwell Laboratory of the U.K. Atomic Energy Authority (Williams et al. 1989 [23 November]). Two similar contributions appeared in *Physical Review Letters*, a periodical of great prestige in the physics community (Ziegler et al. 1989 [19 June]; Price et al. 1989 [30 October]).

The second strategy by which physicists responded to the theoretic anomaly consisted in attempting to demonstrate that, even if the electrolytic cells of the University of Utah group indeed released energy to the extent reported, such emissions could be explained satisfactorily by appeal to processes already known to physics. If so, there was no need to conjecture the occurrence of a novel phenomenon of electrochemically induced nuclear fusion. For instance, J. S. Cohen and J. D. Davies suggested that the phenomena reported by the University of Utah group could be a manifestation not of a new kind of low-temperature fusion the discovery of which was owed to chemists, but of fusion of the kind already known to physicists, which in the case at hand was initiated by the high temperatures associated with the strong electric fields at crystal fractures (Cohen and Davies 1989 [27 April], 1989 [30 November]).[24] Similarly, J. C. Jackson suggested that the energy release was due not to fusion of deuterium but rather to "a chain reaction involving radiative capture, by palladium nuclei, of neutrons produced by photo-disintegration of deuterons" (Jackson 1989 [1 June]).[25]

These two strategies were deployed to best effect at the meeting in Baltimore on

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[23] This paper sparked a brief exchange on gamma-ray detection techniques: see Fleischmann et al. 1989 (29 June); Petrasso et al. 1989 (29 June). For more on the gamma-ray controversy, see Close 1990, esp. 163–70.

[24] Cohen is in the Theoretical Division of the Los Alamos National Laboratory; Davies at the School of Physics and Space Research, University of Birmingham, U.K.

[25] Jackson is at the Department of Applied Mathematics and Theoretical Physics, University of Cambridge, U.K. Sundry other explanations of the cold fusion reports are described in Pool 1989 (21 April, b).

1 May of the American Physical Society (APS). There, Fleischmann and Pons' central claim, that heat is produced in their electrolytic cells in amounts too large to be explained by purely chemical processes, was rejected by several physicists who spoke — among them C. A. Barnes of the California Institute of Technology, who ascribed the reported energy surplus to poor calorimetry and an inadequate accounting of the data. The consensus of the APS meeting appeared to be that the cold fusion experiments were beset by many sources of potential error, and that all the evidence for energy generation that had emerged could equally well be explained by appeal to phenomena other than electrochemically induced nuclear fusion.[26]

If the physicists' response to the claims of the achievement of cold fusion had consisted solely of the interventions reported above, it might be judged not to have exceeded the bounds of scientists' typical reaction to a heterodox and not fully confirmed experimental claim, and would probably fail to establish the occurrence of an interdisciplinary competition. However, the reactions of physicists carried a component that went beyond the effort to reduce the theoretic anomaly to normality, and demonstrates their appreciation of the disciplinary issues underlying the theoretic controversy.

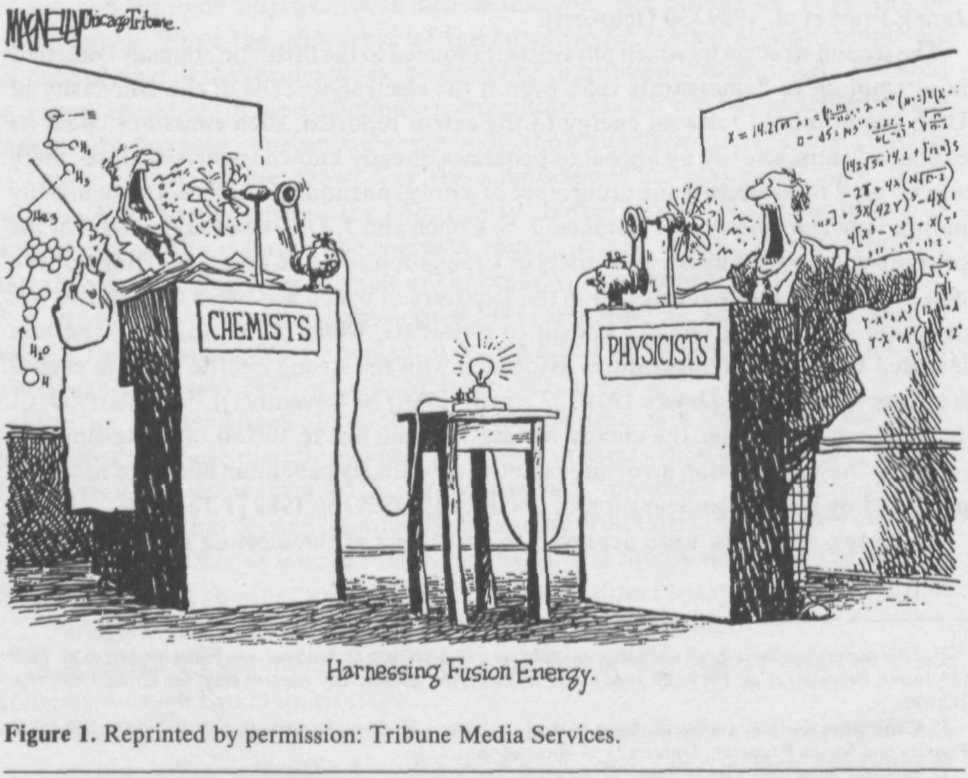

Figure 1. Reprinted by permission: Tribune Media Services.

[26] Accounts of the APS meeting at Baltimore are contained in Lindley 1989 (4 May) and Waldrop 1989 (5 May).

Coloring physicists' consideration of the cold fusion claims was the conspicuous fact that the report of an anomalous physical phenomenon had been issued by a member of a profession that had seemingly entered into competition with that of physics. This circumstance ensured that, in responding to the report, the physics community had in mind not only the need to ascertain its credibility, but also the danger that consequences regrettable for the profession would ensue from the assessment that the report was indeed well founded. The physics community feared that the reports of cold fusion would foreclose physics research in fusion, or at least prompt a transfer of funds from that research to investigation of electrochemically induced fusion, the area newly inaugurated by chemists. Crawford captured the reasoning which underlay this anxiety: "The recent media hoopla surrounding claims that fusion has been achieved by an entirely different approach may focus more attention on the huge costs of the magnetic fusion program" (Crawford 1989 [14 April], 138).

The physicists' fears had already acquired considerable justification in their eyes from the proceedings of the Congressional Science, Space, and Technology Committee, described in the previous section. Physical scientists from established fusion laboratories who testified before the committee had urged Congress to delay investing heavily in the effort until the cold fusion claims had received firm experimental verification — preferably, the physicists may have intended, until fusion physicists had incorporated investigation of the phenomenon and exploitation of its applications into their own research programs. However, this advice had not prevented members of the committee from appearing seduced by the reports, nor one of its subcommittees from allocating to cold fusion research projects money that had been subtracted from physics. D. Lindley suggests that "a certain amount of enmity" had been generated among physicists by the proceedings and decision of the congressional committee (Lindley 1989 [4 May]).

As a result, the reaction of the physics community to the cold fusion reports was directed not only at combating the theoretic anomaly but also at reestablishing the ascendancy of the physics profession over the domain of fusion phenomena and their application in energy generation. This second aim was pursued in part by casting doubt on the technical competence of chemists to investigate fusion phenomena. For instance, the discussion of the cold fusion reports at the APS meeting in Baltimore was marked by a contempt for the work of Fleischmann and Pons that seems unjustified by the theoretic proceedings. Koonin for one was reported to have said: "We're suffering from the incompetence and delusions of Professors Pons and Fleischmann. . . . The experiment is just wrong" (Waldrop 1989 [5 May]). It was reported that "loud applause greeted the remark" (Lindley 1989 [4 May]). Personal animosity of this order is somewhat unusual at meetings of such bodies as the APS and would not have been appropriate had the proceedings consisted merely of the dispassionate scrutiny of novel theoretic claims.

The findings of this section and the previous one suggest that the incidence of disciplinary phenomena calls for some elaboration on the notion of "anomaly," now widespread in the philosophy of science. In the usage of T. S. Kuhn, one of the originators of the notion, an anomaly is a theoretic problem that cannot be given a solution within the "style" of current scientific work and is too conspicuous to be ignored. Typically, perception of an anomaly leads one faction of the community to the conclusion that wide-ranging changes in its conceptual framework are necessary (Kuhn 1962, 52–65). This characterization of anomalies fails to allow for the fact that different disciplinary communities may attribute different discipline-inspired implications to one and the same theoretic anomaly and therefore regard and react to it in different ways. Thus while the communities of physics and chemistry in the cold fusion episode faced the same theoretic anomaly (i.e., the demand to explain reported experimental results that violated accepted physical theory), their perceptions of and reactions to this anomaly differed. These differences are due to the disciplinary dimensions of the reported results. It may therefore in certain cases be necessary to qualify talk of "an anomaly" with reference to the disciplinary community whose reaction to the anomaly is at issue.

6. Further Components of the Chemists' Reactions

The chemists' typical initial response to their colleagues' claim to have observed nuclear fusion was analyzed in section 4. However, the chemists also interacted with physicists during the controversy, and the physicists' response to the claims elicited further reactions from chemists.

This section describes two further kinds of responses shown by chemists. The first was a reaction of persistent hopeful support of the cold fusion claims in the face of the criticism aired by physicists at the meeting of the APS and elsewhere. This accorded with the typical reactions that chemists had given at their Dallas meeting. The chemists' second kind of response, on the other hand, was skeptical of the cold fusion claims and showed chemists adopting stances similar to those of physicists described in section 5. Investigating these further reactions will enable us better to grasp the complexity of the influences acting on the participants in scientific controversies of this kind.

Responses of the first kind came into evidence at a meeting of the Electrochemical Society, which mirrored the partisan corporate stance of the physics profession under the aegis of the APS. Because of Fleischmann and Pons' membership of the electrochemistry subdiscipline and because of the typically electrochemical techniques employed in their cold fusion experiments, the Electrochemical Society was the professional organization whose members associated most closely with the "chemists' party" in the controversy. One might thus expect it to have striven to provide a forum in which the persuasiveness of the chemists' claims could be nurtured and the physicists' opposition rebuffed.

This indeed the society did, at a special session on cold fusion held at its meeting in Los Angeles on 8 May. That the organization's very intentions were supportive of the chemists' claims is evident from the fact that it had invited contributions to this session from "research groups who have verified the initial reports" of cold fusion, but had made no mention of groups being welcomed who had gathered disconfirmatory results.[27] The partisan and corporate outlook was reflected equally in the participants in the meeting. One of them, H. A. O. Hill, told later of the frame of mind in which many arrived at the venue:

> Those of us who had followed the story were hoping that the new evidence would be more convincing and quell the rising chorus of criticism from physicists. . . . However, the reports from the American Physical Society were just coming in and the news was not good — the criticisms seemed to have more substance. (Hill 1989 [July])

The session's ten speakers included Fleischmann and Pons, and most of the others were at least well-disposed to their claims. Reports were presented, for instance, of observations that electrolytic cells filled — as had been those of the University of Utah group — with deuterated water had produced heat in excess of that measured in control cells filled with ordinary water; this of course constituted support for the claim that fusion had been achieved in the former. Overall, the proceedings of the session were reported to be "both more sympathetic and more hopeful" over the cold fusion claims than the APS meeting had been (Lindley 1989 [11 May]).

Reactions of the second kind were perceptible in the chemistry community from an early stage in the cold fusion controversy. Not every chemist supported the suggestion that Fleischmann and Pons had observed nuclear fusion. As grounds for their skepticism, they adduced the same contentions that became common among physicists and were outlined in the previous section.

Some chemists suggested that, because of their poor experimental technique, there was nothing in the University of Utah group's reports which compelled the belief that they had indeed observed anything unusual. For instance, N. S. Lewis, a chemist at the California Institute of Technology, suggested that the University of Utah group had kept unacceptably imprecise records of energy inputs and outputs, and that their cell might not have evolved excess heat at all. He collaborated with Barnes and other physicists at Caltech in drawing up one of the influential reports critical of the University of Utah group's claims (Lewis et al. 1989 [17 August]).[28]

Others, while conceding that Fleischmann and Pons may have accurately recorded excess heat output, suggested that this had been due to previously known, and nonnuclear, chemical phenomena. The chemist L. Pauling was among these (Pauling 1989 [11 May]).

The dissent of some chemists from their colleagues' stance of support toward the cold fusion claims tells us something more about the strength of disciplinary affiliations in scientific controversies. The contention that the knowledge claims of the members

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[27] The terms of the invitation issued for the meeting of the Electrochemical Society are reported in Pool and Heppenheimer 1989 (12 May).

[28] For more on Lewis' important role in the controversy, see Close 1990, 192–214.

of a discipline were affected by their corporate interests cites no more than a tendency operating on the individuals, albeit an important tendency with distinctive features. This tendency was superimposed on many other influences on the formation of knowledge claims, pulling in various directions; in some participants, the disciplinary-related influences could well have been completely swamped by other considerations. It is consistent with positing the operation of this tendency that there should have been some chemists, even electrochemists, who disbelieved the cold fusion reports, as well as a few physicists, even fusion physicists, who extended credence to them.

7. The Appraisal of the Chemists' Claims

In view of the wealth of work in the cognitive sociology of science cited in section 1, an investigation of the disciplinary aspects of scientific practice can carry interest only if it is able to point to some repercussions of them on the processes by which knowledge claims in science are constructed and evaluated.

Some accounts of the cold fusion episode have presented it as an uncontroversial fact that data eventually demonstrated that low-temperature nuclear fusion does not occur, at least in the apparatus used by the University of Utah group. These accounts generally go on to explain the production of data that appeared to support the cold fusion claims as being the result of poor experimentation, especially by the University of Utah chemists.[29]

This assurance that the controversy was terminated by unproblematically "better" data needs to be considered with care. Writing specifically of the cold fusion episode, T. Pinch points out that, if examined in great detail, scientific evidence on all sides of a controversy begins to look less reliable and less unlike grounds adduced in nonscientific controversies (Pinch 1991). If this is so, an account of the termination of a controversy such as this cannot rely on "good data" as a definitive *explanans*. On the contrary, part of the *explanandum* of the episode will be the community's construction of standards of assessment and limits of tolerance for data such that one set of data was invested with apparent reliability and persuasiveness greater than those of another set.[30]

The way in which such standards of assessment and limits of tolerance for data were constructed in the cold fusion episode has, like the beginnings of the controversy, a disciplinary dimension. I argued above that the disciplinary aspects of the cold fusion controversy were initiated — at least in the eyes of the physics community — by an "incursion" of chemists into the terrain of physics, in the sense that chemists had addressed a physics topic and also adopted physics techniques of experiment and data gathering. For instance, the prime sources of evidence for the occurrence of fusion in the University of Utah group's test-tube were calorimetry, neutron counts, and

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[29] Among such accounts are Close 1990; Herman 1990, 228–34. By contrast, Mallove 1991 leaves open the possibility that the University of Utah team was right.

[30] For more on these issues, see Collins 1985.

gamma-ray spectrometry. The latter two sources are the typical tools of physics, especially nuclear physics, not of electrochemistry. The highest-rated neutron counters, the greatest familiarity with gamma-ray energies — in short, the very parameters of data gathering in these areas — were the purview of physicists rather than chemists.[31]

Thus, to the extent that the chemists were relying on these sources of evidence to persuade physicists, other scientists, and political agencies of their achievement of nuclear fusion, they were trading on grounds staked out and regulated by physicists. This impaired the chemists' persuasive power. A scientist's credibility depends to a considerable extent on his or her being perceived to be "at home" in a topic. This evaluative criterion was voiced by F. Close, a physicist:

> To say that the chemists didn't understand what they were doing was not intended as an insult to chemists — the physicists for their part would not have expected to be taken seriously if they had suddenly claimed to be able to measure subtle effects in chemistry. (Close 1990, 158)

Much of the physicists' reaction to the chemists' cold fusion claims consisted in pointing out their distance from home ground. This was all the easier, since every notable participant and onlooker in the controversy was aware that a great deal of the chemists' evidence and evidence-gathering technique was better known to physicists than to the chemists themselves.

Even the University of Utah group showed awareness of this. For instance, Close describes how the chemist's areas of expertise were mapped out in a talk on the cold fusion claims by Pons at Indiana University in April 1989:

> Although he [Pons] was competent and confident with the chemistry, he was very unsure with the physics and wrong on some basic points. It was the performance of an expert chemist who has not had time to brief himself on the physics, reinforcing the impression that while five years may have been spent on the investigation, the physics had been "discovered" only at the last moment and had not yet been assimilated. (Ibid., 146–47)

This effect was all the more evident, given that so much of the controversy was played out in public meetings such as lectures and conferences. In such formats, which area is habitual to the speaker and which lies outside his or her expertise is concealed less easily than in a published paper in which references are aptly deployed.

In view of the fact that those aspects of physics which required mention were not peripheral but central, pertaining to the ways in which the chemists' data had been gathered and processed, it is clear that the appraisal of the chemists' knowledge claims was liable to take place with the chemists at some disadvantage toward the physicists. In this light, it is significant that the disciplinary controversy was terminated in forums that attracted the participation of both physicists and chemists, and where therefore

[31] See Fuller 1988, 191–93, on the ways in which different disciplines "control" different bodies of knowledge.

the chemists' knowledge claims could be appraised by physicists, in an audience of physicists and chemists. In contrast, as sections 5 and 6 indicated, important previous meetings on cold fusion had been held under the auspices of particular disciplinary associations and had therefore generally attracted scientists from one discipline only.

Two interdisciplinary forums were especially influential during 1989 in terminating the controversy. The earlier of these was the Workshop on Cold Fusion Phenomena sponsored jointly by the U.S. Department of Energy and the Los Alamos National Laboratory, held at Santa Fe, New Mexico, from 23 to 25 May; the later was the expert panel established in April by the Department of Energy, which published its report in November.

The conclusions reached by the Santa Fe conference gave satisfaction to the physics profession: while the results presented did not entirely rule out the possibility of low-temperature fusion, they weighed quite heavily against the claim that significant amounts of excess energy can be produced by this process and hence cast serious doubt on the technological applicability of any process that might be occurring.[32] The conference was therefore seemingly marked by the scientific community's declaring that the physics profession had succeeded in neutralizing the crucial parts of both the theoretic anomaly and the professional threat it had faced in March. Wider social institutions gradually took notice of this resolution: in July, for instance, the U.S. administration decided to cease funding investigative research into cold fusion (see Lindley 1989 [20 July]).

The victory of the physics profession was further entrenched by the report of the Energy Research Advisory Board's expert panel (ERAB 1989 [November]).[33] This had been co-chaired by J. R. Huizinga and by N. F. Ramsey, described by the American Institute of Physics journal *Physics Today* as "a leader of the physics community" (Levi 1989 [December], 19).[34] The panel ruled in favor of the physics profession, in the theoretic as well as the professional controversy, as follows.

In the theoretic controversy, the panel subscribed to three conclusions, in increasingly sharp conflict with the chemists' claims. It concluded first that "the experimental results of excess heat from calorimetric cells reported to date do not present convincing evidence that useful sources of energy will result from the phenomena attributed to cold fusion"; second, that "the experiments reported to date do not present convincing evidence to associate the reported anomalous heat with a nuclear process"; and third, that "the present evidence for the discovery of a new nuclear process termed cold fusion is not persuasive" (ERAB 1989 [November], 36–37). These conclusions endorsed the reactions of the physicists aired at the meeting of the APS and elsewhere.

[32] The proceedings of the Santa Fe meeting were reported in Lindley 1989 (1 June); Pool 1989 (2 June); Mallove 1991, 148–70. Abstracts of the papers presented at the conference are to be found in the published program. Neither Fleischmann nor Pons attended the Los Alamos conference.

[33] A critical evaluation of the ERAB report is in Mallove 1991, 176–81.

[34] Ramsey was professor of physics at Harvard University; he had served as president of the APS in 1978–9, and the award to him of a share of the 1989 Nobel Prize in physics was announced during the expert panel's hearings. Huizinga was professor of physics and chemistry at the University of Rochester.

The panel upheld the physicists' case equally in the professional controversy. In this contest, the chemists' corporate interests would plainly have been furthered by a transfer of energy R&D funding from established fusion physics programs to cold fusion research, especially where this was to be conducted in dedicated centers such as the institution planned by the University of Utah. On both counts the panel favored the maintenance of the funding patterns that had prevailed in energy research before the chemists' announcements in March: "The Panel recommends against any special funding for the investigation of phenomena attributed to cold fusion. Hence, we recommend against the establishment of special programs or research centers to develop cold fusion (ibid., 37)." In the panel's opinion, the elucidation of any theoretic questions left unresolved was best served by "modest support for carefully focused and cooperative experiments within the present funding system" (ibid.), which traditionally assigned the investigation of nuclear fusion to the physics profession. The panel thus restored the financial privilege that had been enjoyed in fusion R&D by the physics community up to March, curtailing investigation of a distinctive "chemists' route" to nuclear fusion. The report therefore marks — to the physicists' great advantage — the reestablishment of the preexisting state in both the theoretic and professional controversies that had opposed physicists to chemists.

Once this resolution had been accepted by the majority of participants, the contraposition of the professions of physics and chemistry was erased and their more usual harmony, which they preserve through maintaining distinct the areas of their research, was restored. By July the editor of *Nature* was able to write: "The brief spell in April when it seemed as if cold fusion would permanently divide chemists and physicists has left no trace" (Maddox 1989 [6 July]).

8. A Comparison: The ZETA Episode of 1957–58

This paper argues that some of the principal features of the cold fusion controversy are due to the incidence within it of disciplinary issues. The suggestion is that the controversy consisted in part of a competition between discipline-based factions, and that at least some of the incidents in the controversy would have proceeded differently had the disciplinary issues not been a factor.

The validity of these claims could be tested by examining, by way of comparison, a different scientific controversy — one that paralleled the cold fusion episode as closely as possible but lacked its disciplinary aspects. This other episode, if such could be found, would take the role of a null or base-line comparison: its investigation would indicate which aspects of the cold fusion story were indeed due to its having consisted in part of a controversy between different disciplinary factions, and which are on the contrary shared also by controversies lacking its disciplinary dimension.

This purpose may be served by an episode in nuclear research in 1957–58. It closely resembles the cold fusion episode in several respects. For instance, it was sparked by the reported achievement of controlled nuclear fusion. However, it departs from the

1989 episode in that all its participants belonged to the physics community; it therefore lacked the element of competition between different disciplines that characterized the cold fusion episode. It will be seen that the stances typically taken by the participants in the earlier controversy were significantly different from some of those adopted during the cold fusion episode. I will suggest that these differences are due at least in part to the disciplinary dimensions that the latter episode possessed but the earlier one lacked.

In August 1957 an apparatus known as the Zero Energy Thermonuclear Assembly, or ZETA, entered service at the U.K. Atomic Energy Research Establishment, Harwell. (This same institution's role in the cold fusion episode was mentioned in section 5.) ZETA could be described as an early version of a tokamak: it consisted of a toroidal chamber designed to contain ionized hydrogen at temperatures close to those necessary to provoke nuclear fusion reactions. Almost immediately upon commencing work on this machine, researchers detected a neutron flux from the plasma. They believed that the neutrons had a nuclear origin and concluded from this and certain other observations that in ZETA they had achieved a controlled fusion reaction. They published a paper in *Nature* announcing this achievement under the heading "Controlled Release of Thermonuclear Energy" (Thonemann et al. 1958).

The physics community in both the United Kingdom and the United States, including many highly respected and influential senior scientists, were impressed by the group's results and adhered to the view that a fusion reaction had indeed been achieved. A conference on the apparent findings was held at the Royal Society in London in February 1958 (proceedings reported in Gibson 1958). The presentation of the results was opened by Sir John Cockcroft, the director of the Harwell establishment, and a discussion ensued:

> Sir George Thomson complimented Sir John and the Harwell team on their achievement. He said the evidence that the observed neutrons have a true thermonuclear origin is strong. . . . If the neutrons are not thermonuclear, then the temperature measurements are in error and there must be a non-thermal neutron source which, by chance, gives the neutron yield predicted for the incorrect temperatures; Sir George himself did not believe this. (Ibid., 804–5)

Cockcroft himself said that his team was "90 percent certain" that the neutrons had a thermonuclear origin (cited in Bromberg 1982b, 83). Further work was planned at Harwell to investigate the implications of the results for energy generation.

A short while later, however, a separate group of physicists at Harwell who had not been involved in running ZETA took a closer look at the grounds for their colleagues' claim that the neutrons detected had a thermonuclear origin. They measured the energy spectrum of the neutron flux and found that the distribution of energies was inconsistent with the claim that the flux originated in deuterium-deuterium fusion. They suggested that the neutrons detected were generated rather by a phenomenon unrelated to nuclear fusion, the bombardment of stationary targets within the vessel by a beam of deuterium nuclei accelerated by the electric discharges to which the plasma was subjected in the ZETA experiments (Rose, Taylor, and Wood 1958). The arguments of this second team of physicists appeared unanswerable, and the claim that ZETA had achieved nuclear fusion was retracted by the Harwell laboratory.[35]

The positive analogy between the ZETA and cold fusion episodes is evident. In both, researchers claimed to have achieved nuclear fusion and adduced evidence of a neutron flux among the principal grounds for their claim. A period followed in which the claim appeared to gain credit among some fellow-scientists. In each case, some experimental results were then produced that appeared inconsistent with the claims, and the consensus was formed in the community that no nuclear fusion had indeed been observed. The period of active investigation and discussion of the results was in each case a few months.

The negative analogy is no less striking. In the ZETA episode there is no parallel of the bad-tempered controversy in the professional domain that enlivened the cold fusion episode; the disagreement was confined entirely to the theoretic domain. For instance, the second group of researchers drew from its disconfirmatory results only theoretic consequences and intimated no unfavorable inferences about the experimental prowess of the researchers of the first group, or their competence to work on nuclear fusion. Furthermore, the rebuttal of the ZETA fusion claims appears to have been performed in an atmosphere of amity between the two groups of researchers; indeed, the team which presented the disconfirmatory results recorded that they were "grateful to many members of the ZETA group for their co-operation" (ibid., 1632).

The cold fusion claims of 1989, in contrast, attracted criticism that spilled over from the purely theoretic domain to the professional one. In the 1989 episode, but not in the one of 1957–58, physicists appeared to express not just skepticism at the suggestion that fellow-scientists had achieved nuclear fusion but also contempt for those who advanced such claims. Professional relations between the supporters and the opponents of the fusion claims were in the later episode characterized by competition rather than cooperation. The amity of the ZETA proceedings may be contrasted with the enmity reported by Lindley at the APS meeting toward the chemists who had advanced the cold fusion claims.

It may be that part of the difference in tone is due to the fact that, independently of disciplinary issues, the conduct of scientists was less genteel in the late 1980s than in the late 1950s. Nonetheless, I attribute the differences between the two episodes in part also to the fact that the later episode, but not the earlier one, was marked by a competition in defense of the professional interests of two disciplinary communities. Participants in the earlier episode saw no such interests as being at stake because all were members of the same disciplinary community, that of physics. In that episode, therefore, physicists felt themselves to be under no professional threat from intruders from a discipline in potential competition with theirs and thus reacted solely to the

[35] An account of the expectations aroused by ZETA in the U.S. physics community, of their disappointment by the new results, and of similar experiments in other plasma laboratories is contained in Colgate and Furth 1958, 340–42. Fuller accounts of the ZETA episode in general are contained in Bromberg 1982b, 73–86; Close 1990, 37–44.

theoretic issues raised. The extra features shown by the cold fusion controversy are due in part to the disciplinary issues that underlay it.

9. Knowledge Claims and Disciplinary Membership

In this section, on the basis of the material presented above, three interconnected conclusions will be drawn about the progression of the cold fusion controversy, and to this extent about scientific practice in general. A fourth, broader thesis will be explored in section 10.

1. *The scientific investigation of the cold fusion claims put at stake the corporate interests of parts of the two disciplinary communities of physics and chemistry.* The chemists' claim that they had succeeded in inducing room-temperature nuclear fusion by electrochemical means threw into question important disciplinary arrangements that had not previously been seen as the subject of negotiation within the scientific community. There were probably three sets of interests that the participants in the episode saw to be brought into issue.

The first of these concerned the relative public standing of the physics and chemistry professions. It was professionally rewarding for chemists to announce an apparent success in a field, that of nuclear fusion research, which had appeared to elude a physicists' solution. Correspondingly, physicists experienced professional pique at the chemists' seeming success and acclaim. The second concerned the dispute over the demarcation between the two disciplines: in the physicists' eyes, the chemists had transgressed the disciplinary boundary of physics, purporting to solve a nest of theoretic and technical problems that had long characterized the subdiscipline of fusion physics. The third set of interests was more nakedly financial: physicists discerned the danger that the new developments would cause their profession to lose a large amount of their state and industrial funding, while chemists saw the opportunity of attracting such funding to research programs pursued in university departments of chemistry as well as in dedicated cold fusion research centers.

2. *Knowledge claims affecting disciplinary interests attracted corporate responses on the part of the disciplinary communities involved.* In its response to the cold fusion claims, the scientific community divided substantially along disciplinary lines; roughly speaking, and notwithstanding the qualifications pointed out in sections 5 and 6, chemists tended to support the results claimed, while physicists tended strongly to dispute them. This disciplinary factionalization was noted also by Pool: "It seems that physicists are much less convinced of the reality of Pons' results than are chemists, and fusion physicists are not convinced at all" (Pool 1989 [21 April, a], 284).

The factions collected under the organizational structures of three professional societies — the APS, the ACS, and the Electrochemical Society. Thus the reception accorded to the cold fusion claims by the meetings of the ACS and the Electrochemical Society was quite different from the response to them of the APS; in particular, the first two associations sponsored and encouraged the presentation of results tending to confirm the cold fusion claims, while the third supplied a forum and an ambience to discredit the claimed results.

The reasons why individual physicists and chemists rallied to the banners of their professional communities in the cold fusion episode probably relate to their perception of the professional interests at stake in the controversy, an account of which was given in the discussion to conclusion 1, above. First, the chemists' sense of professional triumph and the physicists' corresponding feelings of pique probably drove members of both sides to display solidarity with their fellow-professionals. Second, many physicists, especially those working in plasma physics and allied subdisciplines, seemingly experienced an impulse to join forces to repel the chemists' disciplinary intrusion and to reclaim the phenomena of nuclear fusion for the discipline of physics and its established research programs. Third, physicists decided that the restoration of the financial *status quo ante* would best be achieved by a corporate and coordinated effort to invalidate or discredit the chemists' claims. According to the observation of Pool reported above, the depth of disbelief in the claims was greatest among members of the subdiscipline that professionally and financially had most to lose from their confirmation; this adds weight to the interpretation that the participants of the cold fusion episode were aware of and reacted to the professional repercussions of potential resolutions of the disciplinary controversy in which they were engaged.

3. *The knowledge claims of the participants in a disciplinary controversy are molded in part by their disciplines' roles in the controversy.* The difference between the physicists' and the chemists' reactions to the cold fusion claims suggests that members of each profession saw their knowledge claims as, in part, an instrument in the professional controversy. Chemists showed themselves prone to the claim that their fellow-professionals had indeed identified an electrochemical way to nuclear fusion, and it is hard to escape the conclusion that this claim was prompted partly by the thought of the likely professional consequences that would have ensued from the wider scientific community's endorsing it. Equally, at least part of the reason for the physicists' embracing with such alacrity the claim that the chemists were mistaken was their calculation that the scientific community's endorsement of such a conclusion would be professionally beneficial to them. To speak loosely, it appears not that physicists in the controversy were drawn to the professional banners of physics because of their skepticism about the chemists' cold fusion claims, but rather that they were skeptical of the chemists' claims because they had previously enrolled in the professional lists of physics.

10. Disciplines and Levels of Reality

Recent cognitive sociology of science has enjoyed much success in demystifying or reinterpreting "givens" of reality — which in the standard objectivist views are reflected in the findings or in other aspects of science — as the product themselves of social construction. Most attention has hitherto been devoted in the literature to elucidating the way in which substantive scientific facts, the subject matter of individual theories, can be seen in this way as constructed. But the investigation can surely be extended to interpret metascientific states of affairs, which, though perhaps seldom mentioned by individual scientific theories, implicitly underlie broad fields of science.

I conceive of what I have just called metascientific states of affairs as aspects of reality that are presumed to be reflected in metascientific, methodological, or metaphysical knowledge claims, in the same way that scientific states of affairs, or "facts," are taken to be reflected in scientific knowledge claims, or theories. An example of a metascientific state of affairs is the composition of the world by material particles or atoms. The knowledge claims that are subsumed under the heading "atomism" purport to represent or reflect this state of affairs; they are metascientific claims in that, rather than being propounded by an individual theory, they implicitly underlie a variety of substantive scientific theories. On an objectivist interpretation, atomism, if valid, genuinely represents the composition of the world. On a constructivist interpretation, it is rather that atomistic claims are the outcome of a consensus underpinned by decisions about what should count as good evidence or sound argument in metascientific disputes, and the particulate composition of the world in which atomists believe is a projection, or reification, of this consensus.

There are several metascientific tenets that one might seek to reinterpret along these lines. Since this paper has focused on some disciplinary aspects of science, I will pursue this reinterpretative project with reference to a metascientific tenet connected to the division of science in disciplines.

The standard philosophical view about the coexistence of different scientific disciplines holds that each is distinctive and needed because each addresses one particular, circumscribed "domain of phenomena" or "level of reality," and draws its *raison d'être* from so doing. According to this view, physics latches on to one particular, and very basic, level of reality; chemistry and biology (while in some interpretations partially reducible to physics) operate at "higher levels," or describe phenomena "of different domains" from those of physics. It is the differences in the levels or domains of operation of the particular sciences that determine their differences of method or presupposition.

A nineteenth-century proponent of this view was A. Comte, who went so far as to arrange the sciences into a hierarchy reflecting levels of emergence of phenomena.[36] But the notion of levels of reality, to each of which there corresponds a scientific discipline, has proved of enduring popularity. For instance, P. Kitcher writes:

> A sophisticated reductionist ought to allow that, in the current practice of biology, nature is divided into levels which form the proper provinces of areas of biological study: molecular biology, cytology, histology, physiology, and so forth. Each of these sciences can be thought of as using certain language to

[36] The reasoning leading to Comte's classification of the sciences is reproduced in Redondi and Pillai, eds. 1989, 45–50.

> formulate the questions it deems important and as supplying patterns of reasoning for resolving those questions. (Kitcher 1984, 370)

Kitcher writes of the life sciences, but views equivalent to these are regularly applied to other branches, and to science as a whole. Such views hold nature's supposed division into levels to be primary and to be the cause or justification of the division of science into branches, each of which gradually develops particular approaches or techniques to grapple with the distinctive level of reality allotted to it.

By applying the approach of cognitive sociology to disciplinary aspects of science, metascientific tenets concerning levels of reality are, I believe, revealed as the product of construction and projection. In other words, the division of reality into levels, the existence of which is in the objectivist view reflected by the pursuit of separate scientific disciplines, is seen itself to have been constructed by the division of science into disciplines.

According to this new view, what is primary is the socially determined agglomeration of researchers into communities, which will come to be called "scientific disciplines." The formation of these communities is to be explained by, perhaps, the modes in which scientific work forces, the competition of individuals, or reward mechanisms are organized. Each of these communities develops its own methods, approaches, instrumentation, and so on. They apply these resources in, as I. Hacking would put it, "creating" phenomena (1983, 220–32). Some phenomena are created by one community, some by another. A phenomenon that has been created by a community is taken to have been acquired by that community. Occasionally, more than one community may claim a phenomenon, and the resulting conflict will require adjudication.

This process of acquisition of phenomena by communities leads to a partition of phenomena into mutually exclusive sets. The division has a social origin but is thereafter projected or reified into science's subject matter, constructing a division of reality into levels or domains of phenomena. Once it has been attributed primary existence, this division of reality is then routinely used to justify the separation of science into disciplines and the existence of separate disciplinary communities of scientists.[37]

This constructivist view of the metascientific tenet of "levels of reality" has two possible links with the cold fusion controversy. On the one hand, the controversy offers some grounds for holding to the constructivist view of levels just outlined; on the other, this view offers an interpretation of the controversy.

The support that the history of the cold fusion controversy offers to the constructivist view of levels emerges as follows. From the standpoint of electrochemistry, the University of Utah chemists advanced knowledge claims about the topic of nuclear fusion; their knowledge claims on this topic were seen as an intrusion by nuclear physicists. On what grounds was the opposition of the physics community based? It was not based on a perception by physicists that chemists had committed a mistake in

[37] This view of the origins of disciplines is sympathetic to Fuller 1989, 20–25.

believing that they could apply electrochemistry, a discipline developed to address one particular level of reality, to another level: this did not arise as an issue in the controversy. Rather, the physicists' opposition was stimulated by their perception of a threat to the interests of their disciplinary community. In other words, the evidence suggests that, at a time of disciplinary controversy, a discipline commands the loyalty of its members not by appeal to a division of reality into levels in terms of which the areas of competence of different disciplines are supposedly defined, but by appeal to the utilitarian interests of the disciplinary community. In this light it is reasonable to conclude that the interests of different communities are what the separation of science into disciplines is determined by, and the supposed primary division of nature into levels is projected into reality as a reflection of the social division.

In return, the constructivist view of the disciplinary division of science offers an interpretation of the cold fusion controversy. I have argued above that what is primary is the socially determined division of scientists into disciplines, which is thereafter reified into tenets about the division of reality. In this light, the controversy is perhaps best seen as an attempted and failed renegotiation of the former division, the social division. The chemists attempted, as a matter of social arrangements, to bring the topic of nuclear fusion into their own domain of operation, but failed. If the attempt had succeeded, the projected division of reality into levels or domains of phenomena would gradually have come to reflect this reorganization of science at the social level, and common metascientific tenets would have been revised accordingly; given the way in which the events unfolded, there arose no need for this revision.

To close, here is a more speculative generalization of some of the ideas presented in this section. The constructivist view of scientific facts is that they are elaborated and projected into nature by the interactions of social agents. Naturally, there operate in science agents of different kinds: disciplinary communities and research teams are agents different from individual scientists. What is suggested here falls into two theses. The first is that the constructivist view should be interpreted as claiming that the construction of facts is the outcome of the interaction not only of individual scientists but of agents of all the kinds that operate in science. This suggestion is relatively uncontroversial. The second thesis is that the interactions of agents of these different kinds result in the construction of facts of different kinds. For instance, the interaction of individual scientists might contribute to the construction primarily of relatively small-scale scientific facts: after all, this is the sort of fact about which individuals in science, *qua* individuals, typically disagree. By contrast, the interaction of agents of other kinds might contribute primarily to the construction of facts of other kinds, the kinds about which these agents typically disagree in scientific practice. For instance, the interactions of agents constituted by disciplinary communities may, as outlined above, contribute primarily to the construction of metascientific facts about the organization of nature.[38]

[38] I am grateful to an anonymous referee of this journal for helpful criticism of a previous draft of the paper.

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