EXPERIMENT AS RHETORIC IN THE COLD FUSION CONTROVERSY

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(ABSTRACT)

An examination of the role of experiment in the cold fusion controversy is offered. It is argued that experimental results served as rhetorical tools in the service of actors in the controversy. Discourse analysis informed by actor-network theory is employed to analyze the verbal and textual discourse of scientists involved in the construction of experimentally-based scientific knowledge. The practices actors employed to structure their discourse for rhetorical effectiveness are investigated. I conclude that if experiments are to retain their traditional role as arbiters of knowledge claims, the unit of analysis pertaining to "experiment" must be broadened to include not only experimental practices within the laboratory, but also the multitude of practices scientists perform outside the laboratory walls in order to increase the likelihood that their knowledge claims will be adopted by their disciplinary matrix.

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CHAPTER ONE

Overview of the Controversy

On March 23, 1989, two chemists, Stanley Pons and Martin Fleischmann, made a remarkable announcement. They claimed to have created a sustained room-temperature fusion reaction in a palladium electrode sitting in a jar of heavy water. This process had the potential to offer a new energy supply in the form of nuclear fusion. It also had the potential to render obsolete the work of many physicists whose efforts to achieve nuclear fusion had cost billions of tax dollars. The nuclear fusion researchers at Princeton University and elsewhere had the most to lose in terms of vested financial and academic interests, yet at first (according to inter-office memos and other personal correspondence), their attitude seemed to be "lets wait and see." Also, early initial replication efforts were supplemented by remarks to the effect that "we're skeptical but hopeful, we're keeping an open mind."

Unfortunately, it did not take very long for things to turn nasty. For instance, one prominent physicist described Pons and Fleischman as "suffering from delusion" and their work as "shlock science." At the Baltimore American Physics Society meeting, a panel of physicists resorted to voting on the reality of the cold fusion phenomena. This action was later recalled as "science by drawing straws" by Scientist A,

an electrochemist who served as a postdoc under Pons at the University of Utah during the initial cold fusion experiments.[1] By the time of the Baltimore American Physical Society meeting and thereafter (May 1989), the controversy had become rather heated.

Following the initial announcement by Pons and Fleischmann, many laboratories immediately began trying to replicate their results. The success of these replication efforts varied. While some laboratories announced calorimetric evidence of excess heat output and emission of neutrons as confirmation of <u>some</u> sort of nuclear fusion process, other labs reported little or no similar evidence. Two labs, Texas A&M and Georgia Tech, initially announced confirmation, only to reverse their decision a few weeks later. In addition, some labs claimed to have replicated the results claimed by Pons and Fleischman, but offered explanations that did not entail nuclear processes. The combination of a) varying results among different labs, b) fluctuating claims from within particular labs and c) the interpretative flexibility of experimental results available to the actors, does not lend itself to a straightforward

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[1] The comment by Scientist A took place at a Virginia Tech seminar on cold fusion held February 15, 1990. Scientist A had recently completed a post-doctoral position at the University of Utah and was at the time of the seminar a faculty member at Virginia Tech. An analysis of an interview I conducted with Scientist A appears in chapter three.

interpretation of the role of experiment in the controversy as an arbiter of conflicting claims about nature. In this thesis, it is proposed that experimental results served as rhetorical tools. Drawing on recent work in actor-network theory, I argue that actors employed experimental results as part of their arsenal of rhetorical strategies marshalled to extend their networks.

Ostensibly, the controversy centered around the reality of nuclear fusion induced without the orthodox methods involving extremely high temperatures. The confirmation of the reality of cold nuclear fusion centered around the replicability of the experimental results achieved, and the capacity to explain those results with the accepted theories of nuclear physics.[2] One must keep in mind however, that the problems the controversy centered around were not a given but could perhaps more properly be construed as a constructed product that may be accounted for in terms of actor-network theory.[3]

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[2] New theories were also constructed in light of the experimental results, most notably by MIT's Hagelstein (1989a, 1989b, 1989c, 1989d). Although this is itself an interesting and important part of the cold fusion story to date, this thesis focuses primarily upon the experimental practices of the actors involved.

[3] See Michel Callon (1986) on the practice of "problematisation", whereby an actor is enrolled in networks by having the problems it deems relevant be defined such that they extend the former actor-network.

Objectives

This thesis explicates the nature of the practices among the actors which led toward establishing some degree of consensus in the controversy. I show that experiments by themselves did not serve to adjudicate conflicting claims. It would be a mistake, however, to conclude that experiments play no significant role in scientific consensus formation. On the contrary, I argue that experimental results serve as very significant rhetorical arrows in the quiver of scientists' discourse. I therefore investigate the practices actors employed to structure their discourse--both in experimental reports and other discursive contexts (e.g. interviews)--in order to render that discourse rhetorically effective. The reader should keep in mind that my thesis argues that we construe experimental reports in the cold fusion controversy as rhetoric, although my means of supporting this argument are primarily based upon examining rhetorical aspects of experimental reports.[4]

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[4] See Latour's (1987) portrayal of the role of experimental literature in the progression "From Weaker to Stronger Rhetoric" that proceeds from experimental reports to the laboratories whence they originated to the data readings of the laboratory machines. Latour argues that the rhetorical power of the reports arises from the way in which they strategically juxtapose the instrumental readings produced in the laboratory.

The greatest obstacles to conceptualizing experimental reports as rhetoric are (i) preconceived notions of "rhetoric" as derogatory, as somehow a devious and underhanded means of communication, and (ii) traditional notions of experiment as gaining access to some trans-social, objective reality. In response to (i), I maintain that the conception of experiment as rhetoric is legitimated by the power of experimental reports to persuade and influence future scientific practice. It is the interpretation of rhetoric as a persuasive, not as a deviant, form of discourse that informs this thesis. The objection from (ii) would account for the persuasive power of experiment precisely because experiments do access objective nature. I counter that the perception that experiment gains such objective access is one of the reasons that experimental discourse enjoys its rhetorical power. The perception of objectivity, however, can arise from means other than experiments producing results that in fact correspond with nature.[5]

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[5] See Shapin and Schaffer's (1985) historical account of the various means appropriated in the seventeenth century in order to induce epistemological and metaphysical reconceptualizations for legitimating experimentation as the dominant scientific methodology.

Relevance of this Thesis to Science and Technology Studies

An analysis of experiment as rhetoric in the cold fusion controversy offers two benefits to Science and Technology Studies. First, by focusing upon an ongoing controversy, it demonstrates that STS can be relevant to the analysis of current events in science. Consider the recent naturalistic turn in science studies; analysts have come to realize the advantage, indeed, the necessity, of basing normative statements on the information provided from the empirical social history of science. In order for the science policy maker to make the most out of her move from the description of the conduct of science to normative claims about science, she should have at her disposal some insights into actor practices that are not always forthcoming from available, traditional descriptions of scientific activity. Hopefully, my explication of experiment as rhetoric in the cold fusion controversy constitutes a more sophisticated "is" from which a science policy maker might make a better informed "ought."

Second, the fact that the controversy had achieved little or no consensus at the time when my research began, allowed me to follow Latour's (1987) maxim to study science "in action." An analysis of an ongoing scientific controversy affords certain advantages to those interested in the nature of scientific change that are unavailable, or harder to come by, through an analysis of a long past episode in the history

of science. As Collins (1975) notes, the study of controversies in science that are studied before consensus is established offers the science analyst a greater opportunity to explicate the nature of the social practices involved in fact construction than does the study of controversy after consensus is achieved. At that later point, actors typically offer reconstructions of events that appeal to ideal standards of rationality and the reality of discovered entities in order to legitimate their past practices.[6] In sum, this study of an ongoing controversy offers a look at actor practices involved in the construction of science before those practices are rationally reconstructed and the products of contingent social negotiation are retrospectively reified as "nature." In order to see the utility of investigating actors' practices to insure the rhetorical effectiveness of their experiments, consider Gieryn's (1982) statement of what he deems to be the constitutive question of the sociology of science: "How does the institution of science establish and maintain its `cognitive authority'?" Although Gieryn has been criticized for pretensions to isolating <u>the</u> constitutive question[7], his question is undeniably important. Indeed, what seems to frame

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[6] For a discussion and criticism of "the scientist's account", see Pickering (1984).

[7] See the response(s) to Gieryn (1982) by Collins, Mulkay, and others in <u>Social Studies of Science</u> vol. 12 #2.

the issues in many recent science studies debates is the nature of the mechanisms (e.g. social or cognitive) that are most responsible for maintaining the cognitive authority of science.

Scientists in our culture employ a multitude of rhetorical tools to maintain their cognitive authority. Some rhetorical tools are stronger than others. Experimental reports are among the strongest, if not the strongest. The strength of experimental reports as rhetorical tools lies in a) the resources an opponent needs to master (labs, access to journals, professional reputations, "expertise") to challenge the claims of an experimental report, and in b) the flexibility open to the authors of experimental reports to sidestep criticism even after such resources have been mobilized (i.e. Collins' (1985, chapters 4 & 5) experimenter's regress). Conceptualizing experimental reports as rhetoric lends itself to analyzing an important aspect of such reports: they are persuasive. Experimental reports influence the practices of other scientists either a) by obliging them to cite or somehow acknowledge the reported claims, or b) by promoting practices on the part of scientists desiring to contest or replicate reported claims.

The persuasive power of experimental reports deserves analysis because not all scientists possess the resources (time or materials) necessary to replicate the results claimed

by particular groups of scientists. The ability of a tiny fraction of the scientific community to replicate and report confirmational results must usually suffice in order for the claims to become "blackboxed" as knowledge. Thereafter, even when replication efforts fail, the methodology of the scientist whose replication efforts failed is considered somehow flawed, if it even gets that far. Usually, our hapless replicator will move on to something else.[8]

Methodology

Despite a tremendous growth in science studies literature detailing empirical evidence for a sociology of scientific knowledge[9], some philosophers are often still obstinate in their refusal to advance beyond apriori claims that relativist implications of such studies are untenable and represent a pernicious challenge to the image of science as rational activity par excellence.[10] Explanations of science by

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[8] See Kuhn (1970) on his exposition of normal science wherein a "scientific failure" becomes a "scientist's failure".

[9] See Shapin (1982) for a survey of empirical case studies. See also Collins (1981) for an extensive bibliography of relativist/constructivist literature in the sociology of scientific knowledge.

[10] Such objections may be found in Laudan's (1981) review of Collins' empirical program of relativism, and in Nola's (1989) review of Siegal's Relativism Refuted [in British Journal of Philosophy of Science, vol. 40 #3 (Sept 1989). Ian Hacking concludes in his review of Franklin

philosophers are often still grounded in articulating the nature of scientific rationality in terms of individual scientists' cognition.

For example, Ron Giere (1988) offers a cognitive account of science that downplays the role of social factors. But Giere's emphasis on the cognitive over the social is clearly misguided. Giere argues that the cognitive capacities of scientists may be accounted for by the neuro-physiological structures developed through the evolution of the species. He points out that since our ability to represent the world through models is an ability shared by rats in mazes, that this cognitive capacity has been "hard-wired" in us. Articulating this cognitive process--representing nature through scientific theories construed as models--is therefore our best bet for explaining science. Giere's explanation fails because it presupposes something unique in modern man's neurophysiology vis a vis pre-modern man's, since science, broadly construed, did not exist before the seventeenth century, the medieval period, or the Greeks, depending on the definition of science invoked. The point is, going back even

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(1986)--wherein Bayesian philosophy is marshalled to explicate a traditional role for experiment in science--that "all is well" in experimental science. Likewise, Newton-Smith (1981) claims that in the face of empirical sociology of scientific knowledge, "the rationalist can still sleep well at night."

as far as the Greeks, we have no evidence that anything has changed in the physiological make-up of homo-sapiens sufficient to account for the advent of science. Since pre-modern and modern neuro-physiology (and hence basic cognitive functions) are the same, why should we think that science can be explained by pointing to cognitive features? In other words, how can we account for tremendous change by pointing to a constant? Surely, the tremendous social upheavals that have transpired since any of the possible "beginning of science" periods and the present are the appropriate variables to legitimately invoke to account for the rise of science. Furthermore, and for similar reasons, variable social factors today serve to explain contemporary science.

Laudan's (1977) theory of rationality articulates a new form of scientific rationality that implicitly recalls Kuhn's (1970) depiction of science as an essentially problem-solving activity, but explicitly rejects the relativistic implications of Kuhn's arguments that criteria of rational choice between paradigms are internal to the paradigms themselves. Laudan argues that scientists' theory selection should follow his problem-solving algorithm. A scientist is to choose that theory which offers the maximum capacity to solve empirical problems, while simultaneously entailing a minimum of conceptual problems. Scientific rationality, according to Laudan, is maintained when scientists follow his algorithm.

On the other hand, Laudan's arationality principle claims sociological explanations are warranted only in cases where scientists are deemed to have proceeded in ways that fail to measure up to philosophical standards of rationality.

The reluctance to consider sociological approaches to the study of science represents more than an intellectual schism between scholars in academia's ivory towers. Were this the case, philosophers and sociologists could go on espousing the virtues of their respective traditions, and the producers and consumers of scientific knowledge could go about their business with little concern for whether or not a consensus will ever emerge. However, the stakes are somewhat higher than short-term positive reinforcement of academic prowess. The ability to understand, and hence change for the better, the conduct of scientific inquiry in our culture hinges in large part upon the willingness of policy makers and their informants to abandon rationalist prejudices about science and acknowledge the relativist implications emerging from empirical social studies of science. Otherwise, science policy will flounder on the misguided assumptions of science as the sine qua non of rational activity. The danger of such assumptions is that they are liable to propagate policy makers' expectations of super-human rational performance from scientists. These expectations could serve to hinder the realistic advance toward a better society made possible

through the utilization of science policies that are sensitive to both the social forces acting on scientists, and the cognitive limitations possessed by scientists *qua* real human beings.

This being said, the general methodological approach followed in this thesis is a sociological one and may best be understood as *discourse analysis that appropriates the insights of actor-network theory*. Three presuppositions are inherent in this approach. First, following the recent work in discourse analysis (Gilbert and Mulkay, 1984) and socio-semeiotics (Woolgar, 1980, Latour and Bastide, 1986), the statements scientists make are the topic for analysis. They are not regarded as a resource to be accepted verbatim as an explanation or as adjudicating evidence. In the language of the received view in philosophy of science, the statements actors made constitute our *explanandum*, not our *explanans* (Hempel, 1965). Second, a constructivist perspective is maintained throughout. The knowledge claims issued by the scientific community for or against the reality of induced nuclear fusion of deuterium in palladium are to be viewed as the product of labor-intensive and interest-driven construction, not the passive and objective discovery of natural reality. Finally, the view developed here is, in general, opposed to the position in philosophy of science

known as scientific realism.[11] Following the actor-network theorists, scientific claims are construed as statements whose acceptability is determined by the strength of the associations actors construct between juxtaposed entities, not upon a correspondence with nature.

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[11] Scientific realism does not constitute a single school of thought in philosophy of science, as scientific realists argue over fundamentals among themselves as much as they do against anti-realists. I agree with Churchland and Hooker (1985, introduction) that this condition represents a healthy state of affairs in the debate. However, I also agree with Fuller (1989, introduction) that engaging in the debate for purposes of winning the argument has run out of gas, but that the debate nevertheless raises issues important for informing normative pronouncements. For a survey of contemporary realist and anti-realist positions, see Cartwright (1983), Leplin (1984), Churchland and Hooker (1985), van Fraassen (1980 and 1981), and Fuller (1988, chapter 3).

CHAPTER TWO

Methodology Elaborated

Harry Collins (1983) has elaborated three stages of research on scientific change. The first stage is constituted by empirical evidence of the interpretative flexibility that is open to experimental results[1]. The second stage is concerned with the way flexible interpretations are brought to an end and the way closure sets in. As Collins notes, "The mechanisms of closure have been found to include various rhetorical presentational and institutional devices working within a context of `plausibility' and other conservative forces."[2] Collins (1983) also sketches a model for the third stage, which would relate the mechanisms of closure to the wider social and political structure.

Actor-network theory may be assessed in the light of Collins' second and third stages. Actor-network theory addresses the processes whereby the contingencies of debate are brought to closure by offering an account of scientific practices that relies on notions of actors adopting ever-

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[1] For example, Collins' (1985) "experimenters regress" and the context-dependent deployment of either the "empiricist" or the "contingent" discursive repertoire by scientists (Gilbert and Mulkay, 1984).

[2] Collins (1983), p.96

stronger rhetoric that is forged through the juxtaposition of heterogenous entities. Concerning Collins' third stage, wherein the mechanisms of closure are to be related to the wider social and political structure, actor-network theory is extremely helpful. This is because actor-network theory does not draw apriori dichotomies between social and political structures and science. As Latour (1983, p.168) observed, "Science is politics pursued by other means." The forces of society and politics, and the forces of science, mutually enroll and counter-enroll each other in the effort to extend their respective actor-networks.[3]

This enrollment and counter-enrollment would be most apparent if we would "follow scientists through society" (Latour, 1987). However, since my means for doing so are limited, I will instead focus upon texts. This is not a shortcoming, considering the important role texts play in circulating knowledge claims through actor-networks (Callon et al, 1986). In order to analyze such texts, as well as other instantiations of actors' discourse, I adopt a methodology of discourse analysis that is informed by actor-network theory.

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[3] See Latour (1983) and (1987), Callon (1986).

Discourse Analysis

A recent research program in the sociology of science has developed an alternative methodology for treating the discourse of scientists. Although sociology has traditionally been concerned with accounting for the actions of social groups, until recently, the discursive practices of actors have not been included among those actions. The methodology of discourse analysis maintains that discourse--both verbal and textual--is a topic for analysis, not a resource to be accepted verbatim.[4]

Although discourse analysis offers an important tool for science studies, it is not without its critics. Gieryn (1982) bases his criticisms of discourse analysis on the notion that since discourse and context are distinct, and since context is drawn upon as an analytic resource, discourse analysts are necessarily forced to draw upon extra-discursive practices to formulate their analyses. As McKinlay and Potter (1987) point out, Gieryn's criticism depends on a distinction between discourse and context. However, by drawing this distinction, Gieryn fails to recognize that context is itself constituted by actors' discursive practices. If one views context as an emergent phenomenon, arising out of actor practices, then

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[4] See Gilbert and Mulkay (1980), (1983), (1984a), (1984b), Mulkay, Potter, Yearley (1983), Woolgar (1980).

Gieryn's critique fails.[5] Likewise, Woolgar (1986) contends that critics of discourse analysis rely upon a peculiarly anglo-saxon notion of discourse as distinct from praxis, and that this distinction robs "discourse" of its robust meaning as that which encompasses all human action as communicative action--the meaning originally intended by continental structuralist writers.

Regardless of the notion of "discourse" adopted, Gilbert and Mulkay are mistaken in their claims that discourse analysis is itself a practice that does not require interpretation on the part of the analyst. Furthermore, their contention that the science analyst should consider discourse analysis as analytically prior to all other approaches is mistaken. These two criticisms are offered by Shapin (1984). Shapin distinguishes two programs in Gilbert and Mulkay's formulation of the program of discourse analysis: the "inclusive" and the "restrictive" programs. According to Shapin, the inclusive program is historiographically sound, but the restrictive program suffers numerous shortcomings when its strictures are applied reflexively.[6]

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[5] This perspective on discourse, as noted by McKinlay and Potter (1987, n.33), is fleshed out in Heritage (1984).

[6] For a critique of discourse analysis based upon its inadequacy for dealing with issues of reflexivity, see Fuhrman and Oehler (1986).