

EXPERIMENT AS RHETORIC IN THE COLD FUSION CONTROVERSY

by

Garrit Thomas Curfs

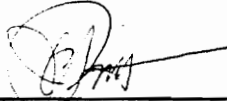
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APPROVED:



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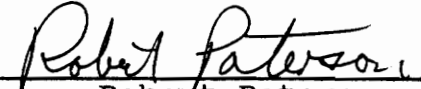
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(ABSTRACT)

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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Table of Contents

ABSTRACT	ii
ACKNOWLEDGEMENTS	iii
CHAPTER ONE:	
Overview of the Controversy	1
Objectives	4
Relevance to STS	6
Methodology	9
CHAPTER TWO:	
Methodology Elaborated	15
Discourse Analysis	17
Background to Actor-Network Theory: Kuhn, Constructivism and Practice	22
Actor-Network Theory	34
CHAPTER THREE:	
Analysis of Cold Fusion Documents	50
Shaping the Rhetoric	52
CHAPTER FOUR:	
Conclusions	72
Where Do Experiments End?	72
Critical Distance from Actor-Network Theory	77
Areas for Further Research	89
BIBLIOGRAPHY	91
VITA	100

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 some 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

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

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 "problematization", 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

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

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 the constitutive question⁷, his question is undeniably important. Indeed, what seems to frame

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 Social Studies of Science 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 knowledge⁹, 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

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

(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-semiotics (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.

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 results¹. 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-

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.

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

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 Mulkey 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 Mulkey'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

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).

The inclusive program of Gilbert and Mulkay would have those who purport to offer primarily a descriptive account of science--historians and sociologists--to keep in mind that the accounts offered by scientists may not be accepted as offering direct access to the reality reconstructed by such accounts. The inclusive program requires that we include the

supplementary goal of describing and documenting the various repertoires and interpretive devices used by participants and, perhaps, of trying to explain how different repertoires and devices come to be adopted in different social settings and in different historical periods.⁷

On the other hand, the "restrictive" program Shapin identifies in Gilbert and Mulkay's work would have us restrict our analyses to only scientists' discourse. Mulkay argues

My formulation does not present scientific discourse as just another topic to be covered in this area. The analysis of discourse is being presented as an alternative to the more traditional concern with describing and explaining action and belief...[The] traditional objective of describing and explaining what really happened has been abandoned and replaced with an attempt to describe the recurrent forms of discourse.⁸ It is from the formulation of the restrictive program that

⁷ Gilbert and Mulkay, "Experiments Are the Key: Participants' Histories and Historians' Histories of Science" in Isis, volume 75 (1984), number 276, p.125.

⁸ Mulkay, "Action and Belief or Scientific Discourse?", pp. 163, 170, quoted by Shapin (1984, p.128).

Shapin derives his two criticisms. First, he notes that Mulkey et al. offer no means of returning from their restrictive program back to description and explanation of science, and that moreover, no logical way to do so exists without abandoning the restrictive program.⁹ In abandoning "why-questions" for "how-questions," the restrictive program of discourse analysis relinquishes an explanatory role for the sociology of scientific knowledge.¹⁰ In the course of accomplishing this more moderate task, the discourse analyst "is no longer required to go beyond the data."¹¹ This leads Shapin to his second critique: Gilbert and Mulkey assume the discourse analyst is engaged in an analytic practice that requires no interpretation. In the course of charting the patterns of scientists' discursive repertoires, Gilbert and Mulkey assume these repertoires are self-evident and present themselves to the blank slate of the analyst's mind. Shapin, as well as Collins (1983), point out that the selection and

⁹ Shapin (1984, p.128).

¹⁰ Similarly, Fuhrman and Oehler (1986), emphasize in their critique that the "restrictive" program of discourse analysis would redirect sociological research toward uninteresting goals, namely, that of providing a sociological understanding of texts alone, instead of a sociological understanding of beliefs and knowledge.

¹¹ Gilbert and Mulkey, "What is the Ultimate Question?", pp.310, 314-315, quoted in Shapin (1984, p.129).

categorization of scientists' discourse is itself a constitutively interpretative practice.

Both criticisms of discourse analysis are overcome in the aim and methodology of this thesis. The discourse analysis offered is explicitly informed by actor-network theory; it is therefore divorced from any pretensions of constituting an interpretation-free analysis. The aim of the thesis is to argue for a role of experiment in the cold fusion controversy, namely, experiment as rhetoric. This entails something more than describing the rhetorical aspects of experimental discourse; I make the more normative claim that the role of experiment should be understood through its rhetorical--that is, persuasive--function in influencing the practices of the actors involved. The texts are not products to be analyzed as ends in themselves, but rather as instruments of persuasion that served to extend or constrict actor-networks in the controversy.¹²

¹² For more on this point, see Latour (1987, p.61)

**Background to Actor-Network Theory:
Kuhn, Constructivism and Practice**

Kuhn

The traditional view in philosophy of science holds that experimental data serve as ultimate arbiter over conflicting knowledge claims in science. This view is actually a vestige of the logical positivists' representation of the justification of scientific theories. Theories were conceived as being capable of being reconstructed as axiomatic structures from which observational consequences could be deduced. Since properly performed experiments would produce the objective observational consequences, experiments were held to confirm or falsify theoretical claims. Furthermore, since the correspondence of theories to the observational world could only be made through controlled experiments, experimental data from so-called "crucial experiments" served as the ultimate arbiter over conflicting theoretical claims.

Early on, Duhem (1954) argued that the theories of theoretical physics are underdetermined by the empirical evidence gained through experiment. Since the observational consequences of theoretical hypotheses need to be interpreted by another chunk of theory, no theoretical hypothesis by itself has any observational consequences. It follows that the hypotheses of theoretical physics are impossible to

evaluate by direct observation, since observation takes place through theory - laden instruments. Therefore, judgements as to exactly which chunk of theory are confirmed or disconfirmed are necessarily inconclusive.

In "Two Dogmas of Empiricism"¹³ Quine pushed Duhem's thesis further and showed that all observations are theory-laden, not just those of theoretical physics. The belief that direct, unmediated observation was possible, Quine argued, presupposed a distinction between analytic and synthetic statements. Analytic statements are statements that are necessarily true in and of themselves, i.e. logical propositions. Synthetic statements are statements about the world, and are true only if what they say about the world is true. For example, "the car is red" is a synthetic statement, and is true only if in fact the car is red. What Quine did was to jettison the distinction between the analytic and the synthetic, and thereby render all our observations theory-laden. Quine's argument may be summarized as follows. "Analytic" and "synthetic" designate types of statements. Whether or not a statement is designated "analytic" depends on the extent to which the predicates of the statement may be interchanged without changing the truth value of the statement. For example, in the statement, "All bachelors are

¹³ Reprinted in Quine, From a Logical Point of View, chapter two.

unmarried men," one may interchange "bachelors" and "unmarried men" without changing the meaning of the statement. Quine argued that such substitutability is in fact a consequence of the conventions of the language that are in effect at a given point in history. Such conventions change through history. The conventions in effect at a given point in time are matters of fact to be ascertained empirically, and therefore the claim of interchangeability of words in an analytic statement itself constitutes a synthetic claim. The argument that analytic statements are relative to the language conventions of a historical context, and therefore synthetic, renders problematic the notion of a clear distinction between analytic and synthetic statements. As noted, this distinction is presupposed by the idea that theory-neutral observations serve to adjudicating conflicting theoretical claims.

A common example used to demonstrate the theory-laden nature of our observations are the "duck-rabbit" illustrations popularized by Hanson (1958). The lines on the page forming the illustration can be interpreted as either a duck or a rabbit, and the viewer undergoes a "gestalt-switch" in interpreting the lines first as one, then the other. Such examples serve to illustrate that we always bring a certain amount of background theory with us every time we observe--even directly--empirical phenomena. Kuhn (1970) offers a model of science free of the problematic theory/world

correspondence principles of the positivists. Crucial to Kuhn's model is the concept of a paradigm. Although Kuhn (1970) offered a vague and sometimes misleading concept of a paradigm, Kuhn (1977) defines a paradigm as a disciplinary matrix which includes, among other things, shared exemplars. On this reading, which informs this thesis, a scientific theory is represented by a symbolic generalization, which are "those expressions, deployed without question by the group, which can readily be cast in some logical form like $f = ma$ or $I = V/R$. They are the formal, or the readily formalizable, components of the disciplinary matrix."¹⁴ In addition, Kuhn states

No one will question that the members of a scientific community do routinely deploy expressions like these in their work, that they ordinarily do so without felt need for special justification, and that they are seldom challenged at such points by other members of the group. That behavior is important, for without a shared commitment to a set of symbolic generalizations, logic and mathematics could not routinely be applied in the community's work.¹⁵

¹⁴ Kuhn (1977, p.297). Kuhn uses a different symbolic generalization in this quote that includes the Greek letter "phi" that is not available on my keyboard. The alternative symbolic generalizations, $f = ma$ and $V = IR$, though taken out of context, do not alter the meaning of the quoted passage, especially since he uses them as examples of symbolic generalizations in the same article.

¹⁵ Kuhn (1977, p.298). Emphasis added, in order to draw attention to the relation between the later Kuhn and social constructivists' emphasis on practice, as elaborated below.

A number of special cases of such symbolic generalizations exist that bear a "family resemblance" to one another and are embodied in exemplars. Exemplars consist of the words, objects and actions which make up a concrete scientific achievement. For example, Galileo's incline plane experiments led to his finding that a ball rolling down an incline acquires just enough velocity to return it to the same vertical height on a second incline of any slope. This concrete scientific achievement constitutes an exemplar, since others used it as a model for their own work.¹⁶ This exemplar includes words (e.g. "ball," "incline" and "slope"), objects (e.g. balls, inclines with varying slopes) and actions (e.g. lifting the ball to the top of the incline and releasing it). Conceiving of theories as symbolic generalizations embodied in exemplars bestows the following advantage over conceiving of theories as axiomatic systems of universal generalizations from which observation sentences are deduced. As noted, the positivists run into trouble over the issue of the correspondence between the vocabulary of the theory and its non-linguistic correlates in the physical world. Kuhn's conception of theories as symbolic generalizations whose

¹⁶ See Kuhn's account of others who used Galileo's exemplar. For example, Bernoulli, who based a solution to a "rate of flow" problem in liquids on Galileo's pendulum work, which in turn bears a family resemblance to the inclined plane. See Kuhn (1977, pp.305-306).

special cases are embodied in exemplars overcomes the requirement for correspondence rules, since objects in the physical world are part of the exemplar.

Constructivism and Practice

A recurring argument in recent sociological work is that what our culture deems scientific knowledge is constructed, not discovered.¹⁷ Contemporary sociologists of scientific knowledge cite Kuhn as their motivation to focus on scientists' practices. That is, the theories and postulated entities described in scientific texts do not represent an objective, trans-social natural reality, rather, they are constructed through a multitude of extremely complex social practices.

Constructivists have in effect argued that the relevant domain of science studies research be transferred from what goes on in scientists' heads to what scientists do. Whereas traditional approaches analyzed the cognitive processes

¹⁷ Such theses appear most frequently in the sociology of science, for example, Latour and Woolgar (1979), Knorr-Cetina (1981), Pickering (1984). A much earlier constructivist account may be found in Fleck (1979), originally published in 1935. The philosopher Bas Van Fraassen (1980) has also argued that scientific knowledge is constructed, not discovered, as a main tenet of his program he calls "constructive empiricism". Van Frassen would maintain cognitive construction, however, not social construction. The analytic framework adopted below, actor-network theory, overcomes this dichotomy.

ostensibly resulting from studying science texts, constructivists base their analyses upon the manifest activities of scientists, that is, their practices. These practices include not only the standard, positivistic descriptions of scientific activity, such as observation and measurement but also judgement¹⁸, and negotiation¹⁹.

Constructivist literature spells out the necessity of focusing upon actor practices by detailing the shortcomings of rules delivered apriori. Here the constructivists owe a debt to some of the arguments of Wittgenstein, which run essentially as follows. For any rule that can be stated, the application of that rule is underdetermined by the rule itself. An additional rule, that is, a rule of application, needs to be granted. However, it is through practices that actors gain this knowledge, which points to the irreducibly social nature of science that must be accounted for in any normative pronouncements concerning the conduct of science. Critics of the social construction of scientific knowledge miss this point when they argue that constructivists reduce

¹⁸ See Pickering (1984) and Giere (1988) for discussions of judgement in scientific practice.

¹⁹ See Gilbert and Mulkay (1984), Pickering (1984) and Collins (1975).

cognitive activity to "mere" social practices.²⁰

It is through the empirical study of actor practices that knowledge of the social nature of science may be constructed.²¹ As an influential example of such an empirical study, consider Latour and Woolgar (1986).²² L&W trace the synthetic development of thyrotropin releasing hormone (TRH) through a two year period of in situ observations of scientists' practices in the laboratory. Instead of discovering the chemical structure of TRH and analyzing it, the scientists discovered it by synthesizing the substance. The problem of analysis is a consequence of the fact that TRH is so hard to come by. One hypothalamus secretes a minute amount of it, perhaps 20 X (10)⁻⁹ grams.

L&W argue that the criteria of identity for the substance was contingent upon the assay system chosen. This choice was itself an open matter. In fact, at the end of two years, the

²⁰ See Bloor "The Sociology of Reasons: Or Why 'Epistemic Factors' Are Really Social Factors'", in Brown (ed.) The Rationality of Science: The Sociological Turn. See also Gilbert and Mulkay (1981) for an argument that Popper's falsification rules are open to different interpretations in different contexts. This interpretive flexibility renders Popper's rules ineffective as useful prescriptions, while allowing scientists to use them as rhetorical appeals to legitimate or criticize various practices.

²¹ Constructivists are reflexively sophisticated enough to realize their own account may only be instrumentally legitimate.

²² Hereafter L&W.

chemical formula that was agreed by the scientists to represent TRH was in fact the result of practices contingent upon decisions; these decisions were themselves contingent upon past practices.

Importantly, L&W do not deny that scientists arrived at the fact of the matter. "We do not wish to say that facts do not exist or that there is no such thing as reality. In this simple sense our position is not relativist."²³ L&W report numerous facts, but those facts are the historical product of scientists' practices. There is a substance, TRH, secreted in minute amounts by the hypothalamus, and whose structure is that of a tripeptide, a string of three amino acids, or pyroGlu-His-Pro-NH₂. L&W would maintain that although this is a fact, it became a fact. They emphasize

"reality" cannot be used to explain why a statement becomes a fact, since it is only after it has become a fact that the effect of reality is obtained. This is the case whether the reality effect is cast in terms of "objectivity" or "out there-ness." It is because the controversy settles, that a statement splits into an entity and a statement about an entity; such a split never precedes the resolution of controversy.

One consequence of recent constructivist literature (see also Knorr-Cetina, 1981) is a picture of science that is, in a word, messy. Scientists' activities are often structured

²³ Latour and Woolgar (1986), pp.180, 182

by locally contingent opportunities arising within the given constraints of a particular research community. For example, problems may be chosen on the basis of available methodologies, which are in turn dictated by the available material resources and experimental skills of the practitioners. These and other local contingencies would seem to reduce the role of universal methodological rules as a means of accounting for laboratory practices. The methodology presented in an experimental report originating in Laboratory A could, therefore, severely underdetermine the practices undertaken to replicate the results in Laboratory B, where different resources and skills were available.

This "open-ended" aspect of scientific practice has led Harry Collins (1985) to formulate what he calls "the experimenter's regress." According to Collins, experimenters who offer conflicting or incompatible claims enter a regress that could in principle continue indefinitely. The "experimenter's regress" runs as follows. The claims of experimenter A may be contested by experimenter B. However, A may claim that B's results are the product of a flawed methodology. B may counter with the identical argument. In cases such as cold fusion, where the reality of the phenomena is not yet ascertained, the competence of the experimental practices is contingent upon the experimental results. At the same time, what count as legitimate experimental results

depend upon the adequacy of the experimental practices. Thus, the definition of what counts as a good cold fusion experiment, and the resolution of whether or not cold fusion exists, are congruent processes. The experimenter's regress as described by Collins casts aspersions on the notion that the controversy may be brought to closure by experimental results alone. This is one reason that the results, as well as the practices, reported in articles pertaining to cold fusion are analyzed in this thesis as rhetorical instruments of the actors involved.

Although constructivism provides useful insights on the nature of scientific practice, constructivist literature manifests the following two shortcomings. First, constructivist writers like Pickering (1984) continue to invoke interests as an explanatory gloss to account for scientific action and hence the content of scientific knowledge. Explanations like Pickering's entail a certain reification of actor practices into social "facts." Summoning interests in this way presupposes a distinction between the social and the cognitive. On this view, the "cognitive" scientific knowledge figures as a reflection of the social interests of the scientists; that is, actors' scientific knowledge corresponds to a set of social interests. This presents problems similar to the ones the positivists encountered in their notions of correspondence principles

between axiomatic-deductive theories and objects in the world.

Second, social constructivists usually focus their analyses upon actor practices that take place within the laboratory. Too often, they neglect scientists' interactions with funding agencies, journal editors, and others in the wider society. Their analyses tend to end just when things get interesting, namely, at that point where it might be asked what the scientific community at large does with the knowledge that has been shown to be the constructed product of contingent social practices. More importantly, by ending analyses in the laboratory, constructivists leave themselves open to criticisms that rely upon the traditional distinction between contexts of justification and contexts of discovery. Critics who invoke this distinction against the constructivists may accept that social factors take place in the context of discovery, but not in the context of justification, where the knowledge claim must measure up to universal criteria of epistemic standards, that are deemed to be independent of social contingency.

Actor-Network Theory

In the 1980s, a group of sociologists centered in Paris began to articulate a research program that took the best of constructivism and attempted to move beyond it. Bruno Latour (1987, 1986, 1983), Michel Callon (1986, 1982) and John Law (1986, 1982) are foremost among this group. Their research program has come to be known as "actor-network theory."

Like constructivists, these sociologists maintain the epistemological position that scientific knowledge is the product of contingent social practices, rather than the result of following a uniquely rational methodology that is universally applicable. Also like constructivists, actor-network theory advocates maintain an anti-realist ontological position: Nature does not intercede to adjudicate conflicting knowledge claims, rather, nature is the product that emerges once controversies settle (Latour, 1987).

Actor-network theory departs from constructivism in one important way. Whereas constructivists are anti-realist with respect to science, some are, as we have seen, social realists with respect to actor interests. They invoke interests to account for the decisions and other practices of scientists. Actor-network theorists, however, are anti-realist across the board. Interests, like nature, are the result of controversy settlement, and therefore cannot be used to explain why controversies settle (Latour, 1987). Constructivist

explanations that look to causes in the surrounding society are inadequate since the development of science and technology cannot be understood without investigating the simultaneous reconstruction of the social contexts of which they form a part (Callon, 1986). Actor-network theory views science and technology, "techno-science" as Latour (1987) puts it in order to overcome notions of a clear distinction between the two, as the "loci of strategic action in which existing scientific cum social relations are worked upon in order to produce, in one and the same movement, both new knowledge and novel social actions."²⁴ As an analytic framework, actor-network theory seeks to overcome misleading dichotomies, such as the distinction between external and internal, between science and technology, and between "technoscience" and society.

Actor-network theory includes material elements as well as human players as entities of an actor-network. The actor who speaks or acts with the support of, or on behalf of, an actor-network is also a part of the actor-network. In addition, she is also a network, as well a point or node within a network. Hence the term actor-network.

According to actor-network theory, an important element necessary to comprehend the nature of an actor world is that of simplification. Actor worlds are composed of a complex,

²⁴ Michel Callon, John Law, and Arie Rip, Mapping the Dynamics of Science and Technology, p.8.

heterogenous mixture of entities. "The notion of simplification is used to account for the reduction of an infinitely complex world by means of translation."²⁵

No privileged epistemic viewpoint exists whereby the analyst may legitimately assign a causal hierarchy to the entities of an actor world.²⁶ Indeed, the assumption that such a hierarchy may in fact be constructed, or does in fact exist, is but the outcome of innumerable past practices, themselves contingent upon the outcome of a vast host of contingent, context-dependent negotiations. We may understand Latour and Woolgar's (1986), and Latour's (1987) portrayal of scientists reconstructing the entities of actor-worlds into causal hierarchies corresponding to objective, trans-social "nature" in order to continue to appropriate the institutional and material resources required to maintain their cognitive authority in our society (Gieryn, 1982).

Actor-network theory offers an exciting new way to build on the insights of constructivism. It allows the analysis to continue beyond the laboratory walls--the end point of most constructivist analysis. It also avoids the reification of the social and its inherent social/cognitive distinction

²⁵ Callon in Callon et. al. (1986, p.29), and see especially pp. 30-31, 49-50.

²⁶ Callon et al., p.23. See also Fuller (1988, intro).

criticized above in the attempt to account for all scientific practice by appeal to scientists' "interests."

Specifics of Actor-Network Theory

In actor-network theory, an actor-network consists of an interrelated set of entities. These entities include not only the human agents and institutions of traditional sociology, but non-human entities as well. The actor-network is "the context which gives each entity its significance and defines its limitations."²⁷ It does this by associating the entity with others that exist within the network. It is the interaction of these entities that make up the mechanisms whereby actor-networks are extended. Because an actor-network is a network of simplified entities (which are themselves other networks), the operations that lead to changes in the composition and functioning of an actor-network are extremely complex.²⁸ Nevertheless, the interactions among actor-networks may be simplified and described in order to facilitate the analysis of the dynamics of actor-networks. We begin by examining three key words and the practices they refer to: "enrollment," "translation" and "problematization."

²⁷ Callon, "The Sociology of an Actor-Network", p.30.

²⁸ *ibid.*, p.31.

First, enrollment designates the practice whereby actor-networks define and distribute roles for other network entities (including but not limited to human actors) to play, in order to extend the former actor-networks. "It should be noted that roles are not fixed and pre-established, and neither are they necessarily successfully imposed upon others."²⁹

How does this enrollment take place? Most commonly, through a process of translation. Callon depicts translation as consisting of three components: the translator-spokesman, a geography of obligatory passage points, and displacement. The translator spokesman "speaks" on behalf of the entities he constitutes. "The translator expresses their desires, their secret thoughts, their interests, their mechanisms of operation. This is the most general way of expressing it, for what is true for human entities, whether they be collective or individual, is also true for the other elements that constitute an actor-world."³⁰ By "speaking" for the entities, the translator spokesman "establishes their characteristics, determines the identity and regulates their behaviour and evolution."³¹

²⁹ Callon et al. (eds.), (1986), p. xvi

³⁰ Callon, "The Sociology of an Actor-Network", p.25.

³¹ *ibid.*

Successful translation is difficult, since entities are not easily translated. The task of the translator spokesman is to define the identities of entities that may resist such a definition, ".whether they are fuel cells, catalysts, users, or industrial firms, translated entities could in theory follow other routes or be brought into other projects. They could in other words escape the logic of the actor-network into which they have been enlisted."³²

The second component of translation is necessary because the success of a translation can never be taken for granted. Through obligatory points of passage, the actor-network renders itself indispensable. Probably the most common obligatory passage point discussed in science studies is the laboratory. This is the case because laboratories are composed of such a heterogenous array of entities. This insures that they form a passage point in a vast number of actor-networks. According to Latour (1983), this is no accident, since laboratories must of necessity transport their contexts to other places so that the practices effective inside the laboratory may be successfully reproduced elsewhere. Thus, it is not so much that the laboratories are a part of so many actor-networks, as that the laboratories have extended their networks into every facet of society.

³² *ibid.*, p.33

The third component of translations are displacements, in a literal sense. "The translator-spokesman and the translator-strategist who impose certain itineraries are bringing about movement. Some link is necessary to make entities accept a certain spokesman, and certain points of passage."³³ For example, "entities are converted into inscriptions: reports, memoranda, documents, survey results, scientific papers. These are sent out and received back, acted upon and reacted to...translation cannot be effective, i.e. lead to stable constructions, if it is not anchored to such movements, to physical and social displacements."

Finally, a common form of translation consists of "problematization," which ties closely to the obligatory points of passage component of translation. In effect, one actor-network (more precisely, the translator-spokesman of that actor-network) states (not necessarily verbally) "If you want to solve your problem, you must first solve this problem. We have the means necessary, you are obligated to solve this problem in order to solve your problem." In this manner, a displacement occurs, for the actor-network is obliged to detour through the "problematization" as outlined by the translator-spokesman.

³³ *ibid.*, p.27

In sum, enrollment requires translation, which in turn requires that actors are displaced, which in turn is often accomplished by positing an equivalence between two problems. The translator "must make the displacement either desirable or unavoidable from the standpoint of the entity being enrolled."³⁴

Enrollment through Interestment

John Law (1986) describes the writing practices actors undertake in order to interest potential readers. Here, "interest" is to be understood as it is used by sociologists, i.e. as a placeholder for an entity that serves to maintain or increase the well-being of an actor, whether she consciously realizes it or not. It should not be interpreted simply as the invoked curiosity or overt desire of an actor. For example, a student has interests in good grades (an example of the former definition) even though she may not personally be interested in acquiring good grades (an example of the latter). Thus, when an actor attempts to interest an audience, her practices are to be construed as an attempt to make her audience share the same goals and values. As Law puts it, "The reader has to be sucked into the series of

³⁴ Callon et al., (1986), p.105.

translations that will, if skepticism is pressed to its ultimate extreme, lead back into the laboratory."³⁵

According to Law, interesting a reader involves both identifying the reader and shaping the text by arraying the appropriate forces that will translate or enroll that reader. The two are interrelated because what count as "appropriate" words are a function of the characteristics of the reader.³⁶ A set of forces that might operate upon one reader will naturally leave another quite unmoved.³⁷ Both practices took place in the cold fusion controversy. What is more, these practices often occurred simultaneously. That is, actors have a particular audience in mind when they shape their reports (through juxtapositioning interesting words) and simultaneously realize that which audience they have a chance to interest depends upon the shaping of the text. This symbiotic relationship between targeting audiences and shaping texts should be kept in mind during the following analysis, even though the two may occasionally be separated for purposes of my own presentation.

Law argues that a number of heterogenous elements are brought together in the scientific text. Law argues that this

³⁵ John Law, "The Heterogeneity of Texts" in Callon et. al. (1986), p.67.

³⁶ Law, "The Heterogeneity of Texts", p.69.

³⁷ *ibid.*

is in fact a primary way scientific texts acquire the power they do, in their juxtaposition and homogenization of diverse and unrelated entities. Law discusses the practice whereby actors achieve a "funnel of interests" in their texts.³⁸ According to Law, the text gains the attention of a potential reader through beginning with statements of the widest possible interest, that may be connected to the more specific interests related in the text below. Through a series of displacements, the text forces a reader who is interested in a general problem to first become interested in the solution to a specific problem reported in the text. The "funnel of interests" may be thought of as the textual means of "problematization." "In order to add to the solidity of the funnel of interests, and to push the reader along to the conclusion that particular experiments have relevance far beyond the time and place that they were undertaken, heterogeneous elements are regularly assembled by the authors of scientific texts. The text both constitutes and indexes

³⁸ John Law, "The Heterogeneity of Texts" in Callon et al. (eds.) Mapping the Dynamics of Science, pp. 67-81. See also Michel Callon and John Law, "On Interests and their Transformation", in Social Studies of Science, vol.12, 615-625.

a mixture of "scientific," "social," "economic" and "organizational" forces."³⁹

In the next chapter, I analyze some cold fusion research articles in order to reveal the rhetorical strategies of actors who marshal heterogeneous forces in order to maximize the persuasiveness, and hence insure the success, of their articles.

Black Boxes

Within actor-networks, "facts" that are taken for granted are termed "black boxes." A black box is "a way of talking of the simplified points that are linked together in an actor-network. A simplified entity that is nevertheless also a network in its own right."⁴⁰ Black boxes may be thought of as those commitments--often embodied in instruments--which may be taken for granted, no longer questioned, whose contingent factors of construction have been forgotten. "A black box contains that which no longer needs to be reconsidered, those things whose contents have become a matter of indifference. The more elements one can place in

³⁹ John Law, "The Heterogeneity of Texts" in Callon et al. (eds.) 1986, p.79.

⁴⁰ Callon et al. (1986), p.xvi.

black boxes--modes of thoughts, habits, forces and objects--the broader the construction one can raise."⁴¹

We need to consider the force black boxes lend to actors who appropriate them in their (the actors) efforts to extend the actor-networks in which they play a role. Likewise, black boxes present difficulties for the extension of an actor-network when that extension would in some way oppose a black box. In principle, any black box may be overcome. However, this usually entails that the actor employ resources beyond the reach of her actor-network.⁴² Therefore, actors usually choose to enroll such black boxes in their efforts to extend their actor-networks.

Black boxes play an important role in the translation of actor-networks by other actor-networks. We have here perhaps most conspicuously the rhetorical usefulness of a blackbox: one cannot deny, without pain of ridicule, marginalization or other sanctions the "truth" represented by a black box⁴³. The extent to which an actor-network is able to enroll a number of the "blackest" black boxes can be used therefore as a measure of the likelihood of success in translating the

⁴¹ Callon and Latour (1981, p.285).

⁴² See Latour (1987, chptrs. 1-3) for a depiction of the hardships an actor must undergo in order to "deconstruct" such a black box.

⁴³ See Latour, (1987, Chptrs. 1-3)

interests and hence enrolling another actor-network or group of actor-networks.

The cold fusion story is a story of Pons and Fleishman's efforts to construct a black box of deuterium-palladium nuclear fusion through the translation and enrollment of other actor-networks. These efforts, both by Pons and Fleischman, as well as their allies and opponents, were marked by the attempt to enroll pre-existing black boxes. My analysis in the next chapter focuses upon some of these efforts of Pons and Fleischman, their allies, and other actor-networks that became their allies, as well as other actor-networks that did not. The success or failure of actor-networks to extend themselves was largely contingent upon the strength of the rhetoric they were able to construct; this strength depended in large part upon the number and "blackness" of the black boxes enrolled.

The following chapter provides a glimpse of the innumerable entities that were appropriated and enrolled in the experimental reports. Through media articles, experimental reports, preprints, referee reports, interview quotes, and other sources the multitude of heterogenous entities which actors structured in order to form persuasive experimental reports are enumerated. Palladium rods, scientists, funding agents, companies seeking to capitalize on recently translated interests (for example, Marshall

Products, a company selling palladium wire to would-be replicators), neutrons, heavy water, calorimeters, voltage readers, fax machines, journals, data printouts--all needed to be enrolled, establishing and maintaining the appropriate links in experimental reports once those reports left the laboratories, in order for the authors to build an actor-world.

In managing the multitude of entities in order to construct persuasive experimental reports and extend their actor-networks, Law (1986) argues that scientists face two problems. First, they must choose their audience. Second, once the audience is chosen, the text must be appropriately shaped or structured.

As to the first problem, the scientist must include as part of her decision-making strategy a consideration of the interests of a prospective audience. The audience must possess interests that will predispose them to taking the text seriously, not to mention considering it at all. Two approaches are available to the author of a paper who seeks to ensure that her audience will be interested. The first approach entails making sure that the audience shares the same research interests as the author. The second approach would see our author attempting to translate the interests of prospective audiences.

The first approach can be implemented by making sure the audience has published results of similar experiments, e.g. those seeking to confirm or disconfirm the same theory or previously published experimental results. Alternatively, the author may aim experimental results at theoreticians she expects may be eager for either confirmation of previously published theories, or for fresh data from which to theorize from.

The second approach would see our author attempting to translate the interests of prospective audiences. Rather than formulating the text so that the audience sees its interests as identical with the author's, the author employing this approach would situate her findings in such a way that the audience would perceive them as relevant to the author's interests as well as the interests of her audience.

I offer examples of both approaches. The first approach is most easily recognized outside the text itself, when actors negotiate which journals to submit their work to. My evidence of such negotiation consists of various correspondence between scientists and between scientists and journals. The second approach appears most strikingly in the text itself.

Before I begin analyzing cold fusion documents, let me emphasize that in combining actor-network theory and the recent tradition of discourse analysis to the cold fusion controversy, it should be clear that actor-network theory's

capacity for explicating scientific practice need not heuristically preclude other approaches. Rather, actor-network theory's importance lies in the resources it brings to the science studies analyst sensitive to the fact that, to borrow from Harry Collins (1976), "Ideas cannot, in any case, be held responsible in a simplistic manner for the ways in which they are taken up and the uses to which they are put in diverse historical circumstances."⁴⁴

⁴⁴ Harry Collins (1976), p.430.

CHAPTER THREE

Analysis of Cold Fusion Documents

The claim under investigation here is that the depiction of laboratory practices in sections labelled "experiment" or "methods" in experimental reports serves as a powerful rhetorical device. The logical coherence of these sections enables the authors to increase their chances of enrolling others, since the procedures are outlined in such a way as to lead the potential "enrollee" to assume that the results would inevitably follow as long as the procedures were repeated in the manner prescribed by the report. The implicit guarantee could ostensibly serve as a motivating catalyst for enrollment by facilitating the interessment of the latter by the authors of the report.

In this chapter, I note the rhetorical shaping of a few select reports¹. I will then trace some developments of the subsequent fate of these reports in the hands of others. This is in keeping with the actor-network theory argument that facts are a function of the strength of the associations between juxtaposed entities, and the strength of these associations can only be gauged by their durability in the

¹ Pons and Fleischman (1989) Jones et al. (1989), Two experimental reports allied with cold fusion, one against.

hands of others. In the following chapter I discuss how a number of actor practices may be interpreted as efforts to ensure the success of their constructed claims, in order to show the value of reconceptualizing experiments so as to include practices outside the laboratory as well.

In this chapter I also analyze a scientist's rhetoric in an interview I conducted. This analysis serves two purposes. First, the scientist simultaneously reveals and rhetorically appeals to a number of the values in the culture of experimental science. Second, the insights derived from this analysis of verbal rhetoric shed further insights on the textual rhetoric of the preliminary note on cold fusion experiments published by Pons and Fleischman.² The various forms of rhetoric are analyzed in the same order in which they were originally produced during the course of the controversy.

I begin with an analysis of the interview in which a scientist reconstructs the laboratory practices that led to the publication of the initial preliminary note by Pons and Fleischman. Aspects of this preliminary note are then examined, as well as the paper by "co-discoverer" Jones et al. An early critical reply by Kreysa et al. (1989) follows, which demonstrates the open-ended nature of experimental life and the role of Duhemian underdetermination in the

² Pons and Fleischman (1989).

experimenters regress as explicated by Collins (1985). Finally, the textual rhetoric of two parties sympathetic to cold fusion claims are analyzed in the light of actor-network theory.

Shaping the Rhetoric

I begin by analyzing an interview I conducted with Scientist A, who held a post-doctoral research position at the University of Utah at the time of the initial press conference and the Preliminary note (Fleischman and Pons (1989)) issued in the Journal of Electroanalytic Chemistry. I intend to reveal rhetorical strategies involved in shaping the published version of the text through a comparison of this scientist's account of laboratory practices with the published version of the text. In addition, scientist A employs a number of rhetorical strategies in the course of the interview that manifest the epistemic values appealed to within the culture of experimental scientists.

Scientist A was responsible for the day-to-day operations of the initial cold fusion experiments before the results became public. In his own words,

I was Stan's [Pons] hands, basically, in the laboratory. I was running the experiments and collecting the data, making sure things were okay and fixing--initially, I did everything...[later] once we got a lot of experiments going, we had some other people working on the project, and I still

did those initial things...I made sure things were running smoothly, made sure things were getting done.

When asked to elaborate on what exactly was entailed by "making sure things were running smoothly," Scientist A replied:

The instruments were not designed to run for three or four months, which was required of them to run these experiments, so consequently we had...the equipment periodically broke down, in which case it needed to be fixed. We tried to watch the instruments for...there are tell-tale signs that something is going wrong and sometimes you can catch it beforehand and switch instruments in and out to make sure that the experiment continues...sometimes the instruments developed a lot of noise over a period of time, which was bad.

Scientist A describes some aspects of experimental procedure, in the course of detailing why "noise" presented the problem it did.

We were essentially applying a constant current between two electrodes, and it was essential that the current reading remained steady, and if you had a lot of noise it wasn't that way. How you...the excess was calculated by knowing the current and the cell voltage...so current is constant, you just have to measure cell voltage. So if the current wasn't constant because it was noisy then that led to errors in the measurements.

On a number of occasions, Scientist A began his discourse in a first or second person narrative, but then

immediately altered the format to an objective, third person perspective. For example, in the passage above, the scientist begins the second sentence in the second person "How you...", catches himself, and immediately shifts discourse back to objective third person perspective. How might we account for these actions?

Such impromptu revisions of discourse reveal the extent to which objectivity is entrenched as a value in the culture of experimental scientists. In turn, objectivity as an epistemic value in the culture of experimentalists accounts for the rhetorical strength of appeals experimentalists make to the objectivity of their methods.

However, we understand from Polanyi (1958) and Collins (1974), the important role tacit knowledge plays in scientific practice. The skills required to actually accomplish the laboratory practices are in fact part of a subjective, "personal" knowledge (Polanyi, 1958) acquired through the disciplinary-specific enculturation of the experimental scientist.³

When asked to discuss the skills required to perform the cold fusion experiments, Scientist A claimed the experiments themselves were easy and it was rather the managing of the

³ See also Kuhn (1970, chapters 3 through 5) on the training of scientists, as well as Barker (1989).

experiments that required special skill:

The experiments themselves were very simple, the computer was controlling it. But, there's a lot involved in the management of the experiment that we had to take care of day to day...the computer made all of the measurements, and we did all the bookkeeping and day to day operations.

However, the following passage reveals just how much specialized knowledge was required. This is knowledge that would not be possessed by many non-experimental laymen.

Throughout the whole thing, we...you're electrolyzing heavy water, so that meant that the solution level diminished with time. We had to keep track of the rate which it diminished, and maintain the level by adding solvent. We had to add solvent at the same temperature as the cell was to make sure that the [unintelligible] temperature...we had to keep the spare solvent at a elevated temperature...things of that nature.

From these excerpts we note two examples of the rhetorical strategies employed to appeal to the objective and replicable nature of the laboratory practices. First, the passage reveals the extent of specialized knowledge actually required to gain successful experimental results, although in the previous passage Scientist A downplayed the personal knowledge required to carry out the experiments, thus increasing the perceived likelihood of successful replication by others, since the procedures are of an impersonal nature and universally applicable. Second, the last passage serves

as another example of the rhetorical role played by the actor's distancing himself from the first person perspective as an implicit appeal to objectivity. In this case, the actor corrects himself by moving immediately from a first person to a second person narrative, in his "We...you're..." in the first sentence.

The quote above also points out the extent to which expertise or competence is disciplinary specific. What might appear to the uninitiated as quite elaborate procedural details that must be kept in mind, are considered second-nature to the experimentalist; again, only the managing of the experiment, and the reliance of such management upon computer programming and operation, was considered particularly challenging to this experimenter.

When asked to elaborate the judgmental procedures necessary to determine how much noise was too much noise in the experimental readings, the experimenter replied

Noise was easy...There really was no judgement, as far as that goes. You could look at it, and see there was something wrong.

Thus, what ostensibly appears to an outsider as a situation that requires judgement, appeared self-evident to the experimentalist, who downplays the role of any judgement on the part of actors.

Shapin and Schaffer (1985), following Garfinkel (1967) and subsequent ethnomethodological literature, emphasize the merits of maintaining a stranger's perspective to the culture of experimental life. Such a perspective allows the analyst to question taken-for-granted assumptions held by the actor. This approach serves us well here, for we may thereby question the claimed self-evidentness of the computer-generated information on noise. Specifically, we may suspend our judgement as to whether the actor's account is itself self-evident. We may assume for purposes of analysis that the objective nature of the computer information concerning experimental noise is in fact another appeal to the objectivity of the procedures reconstructed by the actor. For the actor, discounting any role for human judgement as to how much noise constituted too much noise by reference to the objective data generated by the computer is self-evident; for the analyst, this appeal represents another instantiation of the appeal to objectivity as a persuasive and hence rhetorical strategy on the part of the actor.

On the more general subject of judgements concerning the success of individual experiments, scientist A said

As far as judging whether an experiment was going well or not, you could calculate roughly when you would expect from the time that you turned the experiment on when you would expect to see some excess heat being generated. That went as four times the square of the radius of the rod divided

by the diffusion coefficient. We used that as a sort of rule of thumb, and we should have started to see some excess heat, and the calculations were pretty straightforward at that point. If we didn't, then...generally we let it go longer, in case we...well, that four times the square of the radius divided by the diffusion coefficient is kind of a rule of thumb, so...if we did see excess we let it go and if we didn't see excess after a real long period of time then...we generally note that we didn't see excess and we had all the data...

Here, it is important to note that the criterion of success for an experiment was essentially the results obtained. Whether or not the various control factors and sources of experimental error were accounted for was secondary to the actual results obtained. The inscriptions produced took precedence over the logic of the methodology employed. This is brought out clearly in the following passage.

A typical example, and I think this happened on several occasions is, we had one, we had it going for a while and it was a dud, and by a dud, I mean there was no excess. We took out that palladium cathode, put in a new palladium cathode and restarted it without changing the solution and it got [unintelligible]. For some reason, some of the electrodes were duds and others weren't...we had a few electrodes which were really spectacular, and those were the ones that I would imagine are still running now [November 1989].

The scientist's description of practices undertaken when the negative results were obtained reveals an important aspect of the criteria for the success of the experiments. An experiment was judged to be successful if it yielded results

that could serve as evidence for the reality of cold fusion. The somewhat arbitrary insertion of a new electrode does not seem like an action that follows from adhering to a preconceived methodological procedure. The fact that the evaluation of an experiment by the experimenters was based primarily upon the results obtained has a number of important consequences.

First, it supports Collins (1985, chapter 3) arguments concerning the nature of experimenters' negotiations. Second, it is significant that the "duds"--experiments that did not produce excess heat--did not appear in the Preliminary Note published in The Journal of ElectroAnalytic Chemistry, (Pons and Fleischman, 1989). Neither did any methodological aspects concerning the difficulty of managing the experiments. In fact, there is no mention of computers anywhere, although Scientist A reported that managing the experiments "required sophisticated computer programming, and I'm not a sophisticated programmer." The omission of both the negative results and the difficulties encountered in managing the experiments serve to increase the authors' chances for enrolling others by increasing the perceived likelihood of successful replication.

Pons and Fleischmann, in "Electrochemically Induced Nuclear Fusion of Deuterium" (the first experimental report on cold fusion that appeared in Journal of Electroanalytic

Chemistry state in the "discussion" section:

it is necessary to reconsider the quantum mechanics of electrons and deuterons in such host lattices. In particular we must ask: is it possible to achieve a fusion rate of $10^{(-19)} \text{ s}^{(-1)}$ for reactions (v) and (vi) for clusters of deuterons (presumably located in the octahedral lattice positions) at typical energies of 1eV? Experiments on isotopically substituted hydrides of well defined structures might well answer this question.

Here Pons and Fleischman attempt to enroll both experimental and theoretical scientists, and attempt to translate the interests of ditto in a manner that will extend their (P&F's) network. Were they (P&F) effective? It appears they were, judging by the number of publications generated by experimentalists and theoreticians on cold fusion.

Nevertheless, evidence abounds for the interpretive flexibility open to experimenters depicted by Harry Collins (1985). For example, in "Gamma Ray Spectra in the Fleischmann, Pons, Hawkins Experiment" by Petraso et. al., the authors invoke instrumental readings, and more particularly, their interpretations of those instrumental readings, to undermine the claims of Fleischman and Pons. Petraso et. al. dismiss P&F's interpretation and offer as the only plausible explanation that "it is possibly an instrumental artifact unrelated to a Gamma-ray interaction."

For another aspect on the nature of Collins' (1985) experimenters regress, consider the experimental results

reported by the "co-discoverer" of cold fusion, Jones et al. (1989). These claims, though similar to those reported by Pons and Fleischman, were more moderate. As noted in Shapin and Schaffer (1985, pp.65-69), modesty as a rhetorical strategy in experimental reporting has its roots dating back to the onset of experimental science in the seventeenth century. Several opponents of cold fusion argued that Pons and Fleischman (1989) claimed too much in their report, that their claims were too bold. We may contrast this to the rhetoric of both Jones' article, which claimed less "extravagant" results, according to many critics of Pons and Fleischman, as well as the rhetoric of cold fusion opponents and other players, who cited Jones as the more moderate and hence, implicitly or explicitly, the more scientifically legitimate.

An interesting implication to be noticed, and closely related to Collins' experimenter's regress, is that what at first appears as a criterion of legitimacy (i.e. the modesty) of Jones' report cashes out in practice as a means by which replicators are more capable of reproducing cold fusion. That is, the more modest claims of Jones' report were easier to attain in practice; such attainment signified success for the replicators. Since the modest results published in Jones' article increased the probability of success among replicators, we may conjecture that it was at least in part

for this reason that Jones' article was rhetorically lauded as "sober," level-headed and generally more in keeping with the norms of scientific reporting. This indicates an instantiation of actors' practices manipulating normative structures in order to extend their actor-networks, rather than the normative structures constraining the practices of the actors, as Mertonian sociology of science would account for their actions.

Another example of the open-ended nature of scientific practice is found in the near-textbook example of classic Duhemian underdetermination of theory by evidence. This example presents itself in an early critical reply to Pons and Fleischman. Kreysa et al. (1989) state

We have tried to confirm the results of the recent paper by Flieschmann and Pons in this Journal. Although we have, in principle, observed all the phenomena which they reported, additional check experiments have enabled us to explain all our results without assuming any nuclear fusion.⁴

Let us now turn to an experimental article, "Hydrogen-Hydrogen/Deuterium-Deuterium Bonding in Palladium and the Superconducting/electrochemical properties of PdH/PdD" by Johnson and Clougherty. The authors offer a clear example of "funnel of interests" (Callon et.al. 1986) in their first

⁴ Kreysa, G. G. Marx, and W. Plieth, "A Critical Analysis of Electrochemical Nuclear Fusion Experiments" in Journal of Electroanalytic Chemistry, vol. 266, 437-450.

paragraph. Also, the authors offer an intriguing example of translation of interests: they use widespread current interest in cold fusion to further their own agendas, without focusing on cold fusion as their primary topic, but rather their previous interest in superconductivity. These authors state

Recent reports of electrochemically induced "nuclear fusion" of deuterium in palladium (Pd/Dx), including significant heat production, have stimulated much interest among the scientific community, press, and general public. The superconductivity of hydrogen or deuterium dissolved interstitially in palladium (PdHx/PdDx) has also been of long-standing scientific interest, since pure palladium is not a superconductor, the transition temperature (T_c) increases systematically with increasing H/D concentration to $(T_c)_{\max} = 9\text{K}/10\text{K}$, and PdHx exhibits an inverse isotope effect when D is substituted for H. In this communication, we propose a common chemical-bonding basis for superconductivity and electrochemical properties of PdHx/PdDx. Because of its historical precedent, superconductivity in PdHx/PdDx is addressed first. This is followed by a discussion of anomalous electrochemical properties.

Note that the authors in the article employ a rhetoric appealing to the cumulative nature of scientific development in their justification of superconductivity's top billing, since superconductivity served as "historical precedent." Do the authors mean to imply that because knowledge of superconductivity predated knowledge (contested or not) of "anomalous electrochemical properties" that the former is therefore a necessary precursor to the latter? So it would

appear; the authors seem to be selling their long-standing interest (superconductivity) as a necessary precursor to a topic of widespread current interest (cold fusion). The authors seek to achieve a translation of interests through the rhetorical characterization of their past research as an obligatory passage point to future cold fusion researchers.

Let us next examine an article submitted by a group of scientists led by Scientist N which serves as an example of the "funnel of interests" described by Law. The abstract for the article by the group led by Scientist N reads

(1) A substantial part (125 points) of the palladium hydrogen potential energy surface showing those aspects most directly related to hydrogen and deuterium absorption in palladium is presented. (2) The surface was obtained via a variational and perturbational configuration interaction scheme (involving a couple of hundred thousand configuration using an ab-initio effective core potential.⁵) (3) The relevance of this potential energy surface for the possible palladium catalyzed fusion phenomena at room temperature is discussed.

Here, the author explicitly funnels the interests of the reader from the general physical phenomena of sentence (1) to the specific relevance of this phenomena in sentence (3). The laboratory practices and results reported in the article may in themselves be uninteresting to all but a few other scientists. Such scant interest alone would never allow the article to be published in a journal. However, the authors

⁵ The closing parenthesis was not printed.

link their practices and results to wider interests. The isolated results of potential energy surface of palladium hydrogen molecules are themselves unlikely to interest many. But these results are "most directly related" to another link, that of hydrogen and deuterium absorption in palladium. The authors juxtapose their findings with this link, forming a new link that makes their laboratory practices relevant to readers already interested in the established link. The authors realize the established link has interested readers in the past (and therefore has the force to interest readers again). This is because the established link is itself the product of past juxtapositions with "palladium catalyzed fusion," which has recently been linked with the potential to solve mankind's future energy needs. The authors accomplish the rhetorical juxtaposition of isolated laboratory results concerning surface potential energies to the solution of humanity's energy problems. This makes their text very interesting indeed. It now possesses the power to withstand encounters with other actor-worlds, and survive intact. Or does it?

The article was submitted to Physical Review Letters, but was not accepted. The article was reviewed by three referees. Only one of the referees was entirely unsympathetic to the piece; another was neutral, while a third was extremely positive. Interestingly, the comments of the referees centered in large part upon the proposed connections to cold

fusion claimed by the authors. An examination of the two referee reports with strong feelings toward the article reveals the extent to which the author succeeded in enrolling support through the juxtaposition of interest-linking words.

The positive referee report stated

(1) This paper presents timely, significant new information on the Pd--H--H potential energy surface, calculated at a usefully realistic level of theory. (2) This potential is highly pertinent to the question of possible Pd-catalyzed fusion. (3) The calculated bond shortening and vibrational softening are tantalizing (not conclusive) suggestions that something interesting may really be going on. (4) The work will likely stimulate further investigations of H--H potentials for non-C2v arrangements in metal matrices. (5) This paper has the topicality, substance and significance to warrant publication in PRL.⁶

This referee has been enrolled, the "interestment"⁷ initiated by the authors has persuaded this reviewer that the article should be published (5) and will stimulate further research along the same lines (6). In other words, the modality of the statements made by the authors has hardened into factual status and constitutes a black box⁸, according to this referee. Notice that the referee takes specific notice of the rhetorical strategies linking the scientists' results to the

⁶ Unnamed referee's report, sent by PRL editor to contributing scientist. Obtained at Cold Fusion Archive, Cornell University.

⁷ John Law, "The Heterogeneity of Texts" in Callon et. al. (1986).

⁸ Latour and Woolgar (1986, ch.2), Latour (1987, ch.1).

wider cold fusion interests. In fact, the referee employs a rhetorical strategy as well, referring to the relevance of cold fusion in cumulatively stronger ways with each succeeding sentence. Sentence (1) refers to the authors results as "timely and significant," sentence (2) as "highly pertinent," while sentence (3) states the results are "tantalizing (not conclusive) suggestions that something interesting may really be going on."

Unfortunately for the authors, the other journal referee was not so persuaded by their rhetoric. According to this referee,

(1) Regardless of whether or not cold fusion actually does occur, this paper does not have much relevance to hydrogen in palladium metal. (2) The results are for a single Pd in the presence of two hydrogens, which is a significantly different situation than H in the bulk: According to the paper, the Pd is in a closed shell (d10)¹ So configuration, while bulk Pd metal is known to have holes in its d band due to hybridization; in fact, the number of d electrons in bulk Pd and Pt are very similar. (3) Since the authors claim that an expansion of the H--H distance occurs only for the case of a closed shell configuration (the open shell groundstate of Pt is said to also cause an expansion of the H--H distance) and since in the bulk Pd is in an open shell configuration, their results are irrelevant to H in Pd metal. (4) Because the actual PdH₂ potential energy surfaces have already been published (refs. 4 & 6 [of original article]), the suitability of the paper for PRL rests solely on its relevance to cold fusion and in particular to H in Pd metal. (5) Since this paper has none such relevance, in my opinion the paper does not satisfy the importance or interest criteria for Physical Review Letters and should be rejected.

In the language of actor-network theory, the authors have failed to enrol this referee, who has not accepted the associations constructed by the authors through their juxtaposition of interesting words. The authors therefore have failed to enrol the journal and its readers; the article will not serve to extend the authors' actor-network by becoming a black box that future papers will be obliged to reference.

But all is not yet lost for the authors. What has been juxtaposed can be rejuxtaposed. The authors can reformulate an alternative account of their practices. Since the one referee is all that stands between the authors' account of their practices and publication in a respected journal, they can try again. Perhaps the referee can yet be persuaded. Perhaps the authors can still enroll the referee and hence the journal as an ally. This will require a somewhat altered rhetorical strategy.

To the referee, the authors reply

(1) Considering that yours was the only negative report (out of three) and that based on it Physical Review Letters is rejecting our paper, I would like to make some precisions, if I may to yours [sic] objections. (2) Due no doubt, to a faulty first version of our paper, you misunderstood [sic] several things. (3) We do not claim that the ground state configuration of Pt is the one responsible for the expansion of the H--H distance, but rather that it is an excited closed shell state (in effect precisely of the same configuration (d10)1So as the Pd ground state) that does so. (4) This is clearly explained in the paper by Poulain et al. whose

reprint I am enclosing here. (5) Therefore, you see, it is not the symmetry of the states that makes the difference but the fact that Pt and Pd have essentially different behaviours towards the H₂ or D₂ molecules, this will hardly be otherwise for bulk palladium and therefore some relevance of our calculations to deuterium in metals must exist. (6) You are right, of course, in saying that an atom is not a perfect model for a solid metal, but considering that a more or less rigorous [sic] solutions of the Schrodinger equation for bulk Pd (such as we obtain for the smaller PdH₂ system) is out of the question, perhaps you should concede that our partial and tentative approach to the problem is not completely meaningless. (7) Obviously our results do not explain or confirm cold fusion (whether or not it actually occurs, as you say) but the latter is certainly an issue of such actuality that it deserves to be addressed by using every reasonable method. (8) Our modest contribution should then be made available to the community.

The authors marshal several rhetorical strategies in order to interest the referee. First, in sentence (4), they refer to Poulain et al., in other words, the authors enlist another ally whose knowledge claims the authors presume the referee considers to be a black box. They attempt to recapture the relevance of their findings for their "calculations to deuterium in metals"(5). They also attempt to recapture their relevance to the other constructed link, the one to cold fusion (7). Finally, the authors argue straightforwardly for the successful extension of their actor-world in (8), through the appeal to Mertonian norm of communism.⁹

⁹ See Merton (1973, chapter 13, pp. 273-275)

The authors also wrote a letter (dated July 3, 1989) to the Editor of Physical Review Letters, summarizing their responses to the objections of the referee, and stating

By answering each and every objection by the reviewers, and by carefully rewriting our manuscript and updating and completing the bibliography, we think we have done all that is necessary for you to reconsider your decision.

I do not know the outcome of the authors' efforts; the final document concerning this matter is a letter dated August 3, 1989 wherein the main author states, "I have not yet received PRL's last word on my paper." In any case, this analysis could be continued, in principle, ad infinitum. But by now, the point is clear: The methodological practices in the laboratory were not solely responsible for the success of the knowledge claims in the report.

In addition, the dialectic maneuvering between players resembles the procedures similar to the responses and replies in any scholarly journal. This serves as further evidence for the erosion of the demarcation of scientific from non-scientific knowledge, especially with regard to the rationalist methodological strictures deemed to be the unique vehicle for the production of knowledge in the natural sciences. The fact that such negotiations are played out "behind the scenes," and not in the pages of the scientific journal itself, does not diminish the role they play in the

production and distribution of scientific knowledge. Instead, the systematic omission of these practices in the journal article maintains the image that scientific knowledge is discovered through the objective application of universally legitimate and available methodological procedures, as opposed to being constructed through the strategic juxtaposition of numerous heterogeneous entities that are designed to be rhetorically effective through their capacity to interest a preselected audience.

CHAPTER FOUR

Conclusions, Critical Distance from Actor-Network Theory, and Future Research

In the last chapter we examined some of the discourse--both verbal and textual--through the lenses of actor-network theory, that emerged from the cold fusion controversy. We saw the efforts of actors to enroll others, and some of the instances where experimental reports contributed to this enrollment, through the manner in which experimental discourse translated the interests of actors via linking them to larger, shared interests. This chapter breaks down into three parts. First, I provide evidence for my claim that experiments do not end in the laboratory. Second, in order to gain critical distance from actor-network theory, the episodes are re-examined in the light of other science studies conceptual frameworks. Our assessment of actor-network theory will be based upon what new, if anything, actor-network theory tells us about the practice of science that cannot be accounted for through other analytic frameworks. Third, some areas for further research are outlined.

Where Do Experiments End?

As the preceding chapters suggest, scientists' practices undertaken to construct actor-worlds are by no means limited

to the strategic shaping of their papers. The strategic juxtapositioning of heterogeneous entities in experimental reports constitutes a rhetorically effective means of enrollment, but actors have other means at their disposal to extend their actor-networks. Due in part to the interpretative flexibility available to the audience of any experimental report, the practices actors undertake outside the laboratory can influence the ability of experimental reports to act as a device of enrollment.

Latour and Woolgar (1986) and Latour (1987) argue that a scientific fact begins as a statement whose modality is progressively eliminated once the discursive behavior of actors changes such that they no longer question the status of the statement as fact. In addition, Latour (1983) argues that scientists are capable of interesting those outside the lab by offering solutions to the outsiders' problems. But since the scientists' solutions are replicable only in those situations where the conditions required for their laboratory practices are reproduced, the practices that take place inside the lab must be transported outside the lab into the wider world.

No one has ever seen a laboratory fact move outside unless the lab is first brought to bear on an 'outside' situation and that situation is transformed so that it fits laboratory prescriptions. [This] means that [scientists] will do everything they can to extend to every setting some of the conditions that make possible the reproduction of favorable laboratory practices.¹

Scientists endeavor to extend the conditions and the practices of the laboratory through available networks to contexts outside the laboratory walls.² This insight, combined with both the fact that the actual laboratory events are rationally reconstructed in experimental reports, and that experimental reports are aimed at particular audiences, renders the decision to construe "experiment" as laboratory practices conducted inside the laboratory alone a somewhat arbitrary one. This way of demarcating the end-point of experiment reinforces the notion that laboratory practices are detached from the actor-networks that legitimate those practices and at the same time extend beyond the laboratories.³

¹ Latour (1983, p.166).

² See Latour (1983) for empirical examples of this practice.

³ See also Nickles (1989) and Fuller (1989) on the social contexts of justification.

We must instead look to the interactions between actor-networks that take place after the experimental report leaves the laboratory⁴. In particular, we need to examine the interactions between the authors of a report and the receiving audience. I argue that if experiments are to retain a role in the formation of scientific consensus, the unit of analysis pertaining to "experiment" must be broadened to include not only experimental practices within the lab, but the multitude of practices scientists perform outside the laboratory walls to increase the chances of the success of their experiments, thereby extending the actor-networks they constitute.

Toward this end, I present a few instances of actors' practices undertaken outside their laboratories in order to increase the likelihood that their knowledge claims would be accepted. For example, Pons engaged in just such practices at a number of occasions. Pons gave a lecture at Indiana University at both Bloomington and Indianapolis campuses on April 4, 1989. The press release for the talk stated, "His discovery, announced March 23, makes it much more likely that the world will eventually be able to rely on fusion for a clean, almost inexhaustible source of energy." In addition, Pons also conducted a radio broadcast on "Science Journal" on April 6, transmitting the results of the cold fusion

⁴ See Latour (1987, chapter four).

experiments to a wide public audience.⁵ Pons also spoke to an audience of approximately 7,000 at the American Chemical Society meeting in Dallas. Free copies of the issue of the Journal of Electroanalytical Chemistry and Interfacial Electrochemistry containing the article by Pons and Fleischman⁶ were distributed to journalists and scientists attending the convention. As an example of the attempted enrollment of actors through the translation of interests, consider the following excerpt from a news release concerning the conference by the journal's publishers:

NOTE TO SCIENTISTS: The editors of the JOURNAL OF ELECTROANALYTIC CHEMISTRY AND INTERFACIAL ELECTROCHEMISTRY will be pleased to consider for publication in the journal further papers on nuclear fusion in which electrochemistry plays an important role. The publisher guarantees rapid publication of all papers accepted by the editors.⁷

Pons, Fleischman and Jones also appeared at a press conference conducted in Los Angeles on May 8. Some excerpts from a transcript of the press conference include Fleischman's comments, "I view with total dismay the possibility that existing programs might be affected by a new discovery"

⁵ Science Journal transcript, April 6, 1989, vol.2 #14.

⁶ "Electrochemically induced nuclear fusion of deuterium", Journal of Electroanalytic Chemistry and Interfacial Electrochemistry, vol. 261, no. 2a, pp.301-308.

⁷ News Release, Elsevier Sequoia, Lausanne, Switzerland.

(referring to hot fusion programs), and "It really is difficult to believe that our results can be accounted for in terms of deficiencies in our experimental technique."

Another example of actor practices undertaken outside the lab to reinforce their positions is found in the practices of Project Director of Georgia Tech, Dr. James Mahaffey. Georgia Tech initially offered confirmatory experimental evidence for cold fusion, but then retracted and came up with negative results two weeks later. Mahaffey was therefore placed in a rather delicate rhetorical context when he reconstructed the experimental efforts of his laboratory before various news agents. Mahaffey's answers to questions pertaining to the experiments manifested a number of rhetorical moves designed to maintain credibility, employing both the experimental results and his rationally reconstructed methodology of laboratory practices as rhetorical devices in order to maintain credibility.

Critical Distance From Actor-Network Theory

Actor-network theory has informed the analysis in this thesis. It is now time to take one step back, and make actor-network theory itself the object of analysis. In order to gain some critical distance on actor-network theory, aspects of its approach to studying science will be compared to other analytic frameworks frequently cited in science studies

literature, with occasional reference made to some of the cold fusion events analyzed above and in the preceding chapter.⁸

Three analytic frameworks are compared to actor-network theory. First, aspects of Imre Lakatos' (1978) model of scientific change with its claims for offering a rational reconstruction of science will be addressed. Second, I examine Edinburgh-style interest theory, as offered by Bloor (1976 and 1982), Barnes (1974 and 1977) and Pickering (1984). Finally, the arguments found in the constructivist writings of Latour and Woolgar (1986) and Knorr-Cetina (1981) are offered. Is there anything about actor-network theory that offers the science studies analyst something these other approaches do not? In each case, I will argue that there is.

Lakatos

Let us begin with Lakatos (1978). Lakatos developed a methodology to offer explanations of rational change in the history of science, and simultaneously to offer a prescriptive model for rational progress in science. Lakatos took as his unit of analysis the research program, instead of the single

⁸ A full articulation of the positions addressed is of course impossible within the space of a single chapter. Summaries of the positions, citing key points, will be offered. The reader is advised to consult the references cited.

hypothesis or even the single scientific theory. The components of a research program, according to Lakatos, consist of a hard core, a protective belt of auxiliary hypotheses, and a positive and negative heuristic.

For example, the hard core of Newtonian mechanics consists of four conjectures--the three laws of motion and the law of universal gravitation. These conjectures are protected from refutation by the belt of auxiliary hypotheses. For example, if a planet does not move as predicted, this does not serve to refute the laws of the hard core. Instead, auxiliary hypotheses concerning, for example, atmospheric refraction, the propagation of light in magnetic storms, etc. are marshalled to account for the anomaly. This practice of protecting the hard core from empirical refutation, by redirecting such refutations at the protective belt of auxiliary hypotheses, is what constitutes the negative heuristic. The positive heuristic "consists of a partially articulated set of suggestions or hints on how to change, develop the 'refutable variants' of the research programme, how to modify, sophisticate, the 'refutable' protective belt."⁹ Let us now examine a few aspects of Lakatos' Methodology of Scientific Research Programs that may be compared and contrasted to actor-network theory.

⁹ Lakatos (1978, p.50).

First, in order to compare Lakatos to Actor-network theory, one question that arises is, does cold fusion amount to a Lakatosian research program? Perhaps it should be considered a budding young research program, which Lakatos would admonish us to treat gently, to give it a chance to prove itself. Or do the empirical results claimed by Pons and Fleishman instead constitute a potential refutation of existing research programs, since they contradict accepted theories of physics? An analogy to the debate over creationism will prove useful here. The question to be asked of cold fusion is like the one asked of creationism: Are there any positive results on which to base an autonomous research program, or just negative results from some other research program? Lakatos would claim creationism is parasitic upon evolutionary theory (to the extent that it feeds off gaps in the fossil record and any other anomalies it can uncover in evolutionary theory) but that it has no positive heuristic of its own. Similarly, cold fusion seems to be feeding off those experimental results generated that do not lend themselves to explanation via the accepted theories of nuclear physics. Yet, does cold fusion offer a positive heuristic of its own? What might such a heuristic look like, and how might it differ from the positive heuristics of other research programs seeking to achieve nuclear fusion, aside from the obvious methodological differences? Answers to such questions require

a degree of interpretation on the part of the analyst, and hence the potential for disagreement between analysts. But would not such questions of articulating the components of a Lakatosian research programme need to be answered before the Methodology of Scientific Research Programs could be implemented? It appears one advantage actor-network theory offers over Lakatos is that the components of a research program need not be identified in order to proceed with an actor-network analysis. Indeed, strictly cataloging various entities into the pre-determined Lakatosian components of positive and negative heuristic, hard core, and auxiliary hypotheses runs counter to the "heuristic" of actor-network theory, namely, that of identifying the many heterogenous entities and the associations actors construct between them, and mapping the dynamics of these associations as they encounter other such associations, in order to follow the extensions of actor-networks.

Consider the examples of Collins' experimenters regress offered in the preceding chapter. On actor-network theory, these practices may be accounted for as actor-networks opposing the strength of the associations constructed by other actor-networks. A Lakatosian might seek to account for such practices in terms of the negative heuristic of the research programme the actors were working under. This rule-based methodological prescription fails because Collins'

experimenter's regress entails that the methodology of critiquing the methodology of others may itself be refuted. In other words, the negative heuristic works only if everyone agrees which heuristic is appropriate when.

As a second point of comparison, consider that Lakatos maintains in effect that during the course of the development of a research program, theoretical entities progress from an instrumental nature to one in which a realist interpretation is warranted. Such shifts in ontological commitment are problematic. First, do all members of a research program undergo such a shift simultaneously? If not, what are the social mechanisms responsible for the gradual shift in commitment through the research programme? Does the belief that a heretofore instrumentally conceived entity now really exists bring about changes in actors' practices, or (more likely) do changes in practices serve as one of the mechanisms responsible for the shift in ontological commitment? How would a Lakatosian account for the changes in practice? Such shifts in ontological commitment are not entailed by A-N theory.

Recall that scientific facts are depicted in actor-network theory as black boxes, statements that have become stable through the strength of the associations of entities they represent and which must be broken in order to challenge their factual status. Thus, it is not that an entity or group

of entities undergoes a transformation from a theoretical instrument to an object of nature. Instead, the number and strength of the associations between actor-networks that constitute the fact have become so great that one has no choice but to become enrolled in the networks that take for granted the reality of the entity in question.

Third, as Fuller (1989) notes, Lakatos can compensate for the multitude of non-rational episodes in the history of science by claiming that science could have progressed more rationally had it followed the prescriptions of MSRP. This neglects "the fact-laden character of values," that "what appears as the optimally rational move from the standpoint of abstract philosophical criteria could turn out to be an implementation nightmare."¹⁰ Rationalist philosophers' prescriptions like Lakatos' neglect the material means of production necessary to produce scientific knowledge, and the various cost-benefit relations that must be taken into account at numerous points throughout a research program. Actor-network theory makes explicit the heterogeneous entities that must be brought together in order to construct scientific facts, and is therefore much more reliable as a real-world picture of science from which normative prescriptions would

¹⁰ Fuller (1989, p.10)

follow that would possess a better chance of actually being implemented.

Edinburgh-style Interest Theory

Bloor (1974) presents four tenets that are to establish the nature of sociological explanation of scientific knowledge. Such explanations should be 1) causal, that is, concerned with the conditions which would bring about belief or states of knowledge. According to strong programmers, however, there will be other types of causes apart from social ones which will cooperate in bringing about belief; 2) impartial with respect to truth and falsity, rationality or irrationality, success or failure. Both sides of these dichotomies will require explanation; symmetrical in its style of explanation. The same types of cause would explain, say, true and false beliefs; 4) reflexive, that is, in principle its patterns of explanation would have to be applicable to sociology itself.

Bloor is quick to emphasize the scientific nature of these theses. These principles "embody the same values which are taken for granted in other scientific disciplines."¹¹ The strong program "possesses a certain kind of moral neutrality, namely the same kind as we have learned to associate with all

¹¹ *ibid.* p.4

the other sciences."¹² Furthermore, "the search for laws and theories in the sociology of science is absolutely identical in its procedure with that of any other science."¹³ The sociologist of scientific knowledge should, according to Bloor, "proceed as the other sciences proceed and all will be well."¹⁴ Strong Programmers usually cite the interests actors hold for a causal explanation of scientific knowledge. That is, underlying social interests, such as class interests, serve to explain why actors chose certain scientific knowledge claims over others.

Roth (1987, chapters 7 & 8) offers an extended critique of the Strong Programme, calling into question especially their notions of causality and their conflation of underdetermination and holism.¹⁵ In addition, sociology of scientific knowledge that appropriates interests in order to account for the knowledge claims actors assent to suffers two other shortcomings.

First, reliance upon interests as explanations is short-circuited once it is recognized that interests are themselves transformed during the course of technoscience controversies.

¹² *ibid.*, p.10

¹³ *ibid.*, p.17

¹⁴ *ibid.*, p.141

¹⁵ See also Arieu (1984) on this point.

Consider the scientists at Georgia Tech, who initially reported confirmation of cold fusion results, then offered disconfirmatory evidence two weeks later. May we account for the actions of Mahaffey and his colleagues following the initial confirmation results (such as public endorsement of cold fusion) in terms of their interests? May we then account for their actions after disconfirmation (such as defending their competence as experimenters) in terms of the same interests? Or would the new actions need to be accounted for in terms of new interests? Had their interests changed from promoting cold fusion to disclaiming it? If so, it would appear that their interests did indeed change as a result of new scientific developments, and actor-network theory would be vindicated. However, it could be argued that their underlying interests of maintaining and enhancing their credibility as scientists never changed, and that their practices in fact changed in order to most effectively protect this underlying interest. But of course this shows not only the ambiguity of interests as an explanatory device, but also that interests appear to gain increasing explanatory power only when we begin to abstract from the actual history of actors' practices.

Second, in order for interests to explain cognitive action, a distinction is presupposed between the social and the cognitive (see Bloor, 1982), in order that the cognitive

may be explained by social interests. In this, the strong programmers follow Durkheim and Mannheim in positing a reflexion theory of sociology of knowledge, whereby cognitive activity "mirrors" society. This distinction may be viewed as a vestige of both early positivism and "vulgar" Marxism, wherein a dichotomy between "objective" social facts and "subjective" individual cognition is maintained in order to "scientifically" explain the latter by the former. Lukacs (1951) argues that these ways of thinking were in fact systematically reinforced aspects of bourgeois rationality. Lukacs argued for a theory of knowledge that did away with a simple representational perspective to one in which the material basis of social relations and socially available knowledge are dialectically related. Lukacs believed that Marx "intended a resolution of the materialism-idealism debate by transcending it to a point where practice may be seen as the union of thought and reality, where our conceptions of the world may be tested in attempts to change it."¹⁶ The dialectic method involved the analysis of history and could accomplish what bourgeois rationalism could not comprehend, that "facts" be reconceptualized in their historical totality. Through this formulation, thought does not reflect existence, the two do not "correspond." The inseparability of the social

¹⁶ See Hamilton (1974), p.49

and the intellectual is clear in Marx:

But even if my activity is a scientific one, etc., an activity that I can seldom perform directly in the company with other men, I am still acting socially since I am acting as a man. Not only the material of my activity--like language itself for the thinker--is given to me as a social product, but also my own existence is social activity; therefore what I individually produce, I produce individually for society, conscious of myself as a social being."¹⁷

The dichotomy between social and cognitive, a vestige of early positivism and systematically reinforced through an exploitative ideology, is maintained by the Strong Program. Actor-network theory, on the other hand, collapses the dichotomies between social and cognitive, and between science and society. "We do not have on the one hand 'knowledge' and the other 'society'. We have many trials of strength through which are revealed which link is solid and which one is weak."¹⁸

Constructivism

Although social constructivism is appropriately sensitive to the negotiations and contingencies of scientific practice, most social constructivist analyses have been limited to

¹⁷ McLellan (1977), p.90.

¹⁸ Latour (1987, p. 200). For more on the connections between Durkheim, the Strong Program and positivism, see Roth (1987) and Curfs (1989).

laboratory studies (Latour and Woolgar, 1986, Knorr-Cetina, 1981). Knorr-Cetina (1982) and Latour and Woolgar (1986, chapter 5) are sensitive to the shortcoming, and hint in directions toward actor-network theory.¹⁹ Actor-network theorists have recognized these shortcomings, and offer a research program that articulates the processes by which micro-actors become macro-actors. Through the enrollment of allies and resources via interestment and problematisation, macro-actors are constructed through long and stable chains of micro-actors.²⁰ Actor-network theorists offer a depiction of actor practices that goes far toward overcoming hurdles presented by micro-macro dichotomies in the sociology of scientific knowledge.

Areas for Further Research

This thesis has focused on experiment as rhetoric, but much more could be said concerning the explanations offered by actors of the experimental results obtained. Two approaches come to mind. First, a rhetorical analysis of the explanations offered, which would focus upon the various

¹⁹ Knorr-Cetina (1982) does so in her prescription that sociologists refocus their attention upon "transepistemic arenas of research."

²⁰ See Latour and Callon (1981) and Latour (1987, chapter four).

forces actors enroll in their explanations. Second, one could follow more traditional approaches in the philosophy of science by applying and/or testing various theories of explanation to the explanations offered by the actors.

In addition, my analysis began with the March 23 press announcement of P&F and the experimental reports following immediately thereafter. In keeping with A-N theory (Latour (1987, introduction) this was in effect a somewhat arbitrary decision on my part. More extensive analysis could begin before the press announcement. References to previous publications in bibliographies of reports that served as the starting point this time around could serve as the first step of additional cold fusion analysis, that is, the explication of the translation and enrollment of Pons and Fleischman.

Finally, useful insights may follow by comparing my argument that experiments do not end in the laboratory, but are also constituted by the practices that actors employ outside the laboratory to augment the likelihood of success for their claims, to other arguments concerning the end of experiments, such as Galison (1987) and the finalization theorists (Schaefer, 1984).

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