Chapter Five

The Study of Science and Technology:
What Went Wrong?

The constructivist paradigm that is the very core of STS, according to some theorists, is considered moribund, dead, or a grave error by others. **(Sal Restivo,**
"The Theory Landscape in Science Studies: Sociological Traditions" [1]**)**

When the Four Dogmas have been thoroughly absorbed, so that younger scholars start from conclusions as if they were gospel, then enterprises of real peculiarity can be launched.
(Philip Kitcher "A Plea For Science Studies" La Recherché Forum www. larecherche.fr/FOR/SOKAL/kitcher.html)

5. 1 General Introduction

This chapter examines current criticisms of STS, made most profoundly by Philip Kitcher, briefly look at the notion of a "postconstructivist" STS, as proposed by David Hess (1997), then summarizes the arguments forwarded by this dissertation integrated with proposals for the basis for future STS inquiry.

Section 5.3 gives a detailed analysis of the reform agenda offered by Philip Kitcher (1998).[2] Kitcher's critique can be seen as an update of the Larry Laudan's (1981) attack on David Bloor's Strong Programme. Much of what Kitcher proposes has been put into practice by Peter Galison who serves as an example of what I call the "middle way" in STS. The "middle way" combines a form of scientific realism informed by the findings of the sociology and history of science. What Kitcher proposes is that social and historical forces are at work in science but do not have the radical consequences that the constructivists have suggested. In section 5.4 I look at David Hess' *Science Studies: An Advanced Introduction* (1997) which gives an overview of the state of affairs in STS and ends with proposals for a "new" role for sociologists of science in helping to decide scientific controversies. A "postconstructivist" STS appears to be a move "back to the future." Hess echoes the need for a useful STS which can intervene in matters of

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[1] While the disjunction implies that one either accepts constructivism as the "core of STS" or "a grave error", both aspects of Restivo's proposition are true. I find Restivo's use of 'paradigm' in this instances highly suggestive of the effect of Kuhnianism on STS as argued in Chapter 3. (Sal Restivo, "The Theory Landscape in Science Studies: Sociological Traditions") in the *Handbook of Science and Technology Studies* (**Jasanoff,** Markle, Petersen and Pinch (eds.) Thousand Oaks: Sage, 1995, 95-110).

[2] All references in this chapter to Philip Kitcher are to "A Plea For Science Studies" *La Recherché Forum* www.larecherche.fr/FOR/SOKAL/kitcher.html.

public policy. However, the theoretical assumptions informing the role of sociologists as objective third person arbitrators in scientific disputes is decidedly constructivist. Finally, in section 5.5 I offer an argument summary of the dissertation and map out what I take to be the structure of meta-scientific claims. I argue that the unique relationship of meta-scientific claims to the practice of science and the world that practice describes entails a unique epistemological structure.

5.2 Argument Summary

While criticism is what's bred in the bone of STS, the firestorm over the "Sokal Hoax", the resulting "science wars" and, more obliquely, a form of millennialism has turned disciplinary self-reflection into a high art. The "arrival" of STS has been heralded in a series of books published two years apart Steve Fuller's *Philosophy, Rhetoric and the End of Knowledge: The Coming of Science and Technology Studies* (1993), Jasnaoff, Markle, Petersen and Pinch (editors) *Handbook of Science and Technology Studies* (1995), and David Hess' *Science Studies: An Advanced Introduction* (1997). These books are sociological indicators that STS has reached enough cohesion to be identified, criticized and, hopefully, changed.

Reviewing Paul Gross and Norman Levitt's *Higher Superstition* (1994) for *Nature*, Donald Kennedy, the former president of Stanford, warned: "... I hope the authors have set the stage for a serious engagement with the issue in the academy - but about that I have some misgivings. It would be a terrible disappointment if *Higher Superstition* simply propelled us into a noisy struggle with some scientists on one side and some members of the academic left on the other." Over the last four years, however, Kennedy's misgivings have been realized. The "Sokal Hoax" (*Social Text* Spring/Summer 1996, *Lingua Franca* May/June 1996, 62-64), Gross and Levitt's follow up, edited volume (*The Flight From Science and Reason*, 1997), countless articles in the popular and academic press, and extended electronic diatribes ring out that STS is in, as the Kuhnian irony suggests, a crisis stage. Philip Kitcher assesses the crisis in STS and suggests a way out of the social constructivist paradigm which weds scientific realism to the findings of sociology and history of science. The "socio-historic" cluster supports a less idealized notion of science than had been perpetuated by logical positivists. Science is fundamentally a social activity. What remains in question is the way in which social aspects of science are manifest in the production of scientific knowledge. Kitcher points out: "[In] the history of science studies ... scientists were [in the 1960s] conceived as logically omniscient, beings whose work was shaped by what occurred within the lab. Since the 1970s ... scientists

have been conceived of as brain-dead from the moment they enter the laboratory to moment at which they leave." Philosophically, the move in STS is from a strict form of positivism to a strict form of social constructivism. Neither of these views, in themselves, are adequate. This chapter offers grounds for a philosophical synthesis which is necessary for the development of STS.

5.3 What Went Wrong?

"A Plea for Science Studies" [3], Philip Kitcher's salient critique of contemporary STS, caused a firestorm of discussion on electronic lists in science studies. While I have offered an extensive footnote (see Chapter One) to Kitcher's stinging, but playful, retort to reflexive reactions to the early sociology of scientific knowledge, I want to contrast his description of the dilemmas, and possible solutions, in STS with my own proposals.

According to Kitcher: "The root of the problem [in STS] is some bad philosophy that has been strikingly influential in contemporary history and sociology of science (and, occasionally, in some contemporary philosophy of science). There are several ideas that have been dramatically overinterpreted ..." These four ideas include the theory-ladeness of observation, the underdetermination of theory by evidence, the variety of held beliefs by scientists, and actors categories and the writing of history. Kitcher argues these four beliefs correspond to the four dogmas (which correspond to the four tenets of the Strong Programme in the Sociology of Scientific Knowledge) of STS:

(1) There is no truth save social acceptance;

(2) No system of belief is constrained by reason or reality, no system of beliefs is privileged;

(3) There shall be no asymmetries in explanation of truth or falsehood, society or nature;

(4) Honor must always be given to the actor's categories.

Let us examine each of these in turn.

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[3] **Philip Kitcher "A Plea For Science Studies"** *La Recherché Forum* www.larecherche.fr/FOR/SOKAL/kitcher.html

5.3.1 Social Acceptance

One can readily concede, as Longino's (1990) analysis of contextual values shows, that we enter a mediated world. Perception, language, experience and countless background phenomena shape our understanding of nature. However, as the "framework relativists" (Galison uses this terms in reference to Kuhn and Carnap) noted, our categories and concepts do not fully determine the nature of experience. As I have suggested in analyzing meta-scientific claims, the way in which we "shape" or "construct" the world is open to empirical investigation, justification, and revision. Further, on the social constructivist account that one takes empirical principles as a starting point is equally, or more, valid than taking other principles as given. As I have suggest, the skeptics argument can be readily deployed against empiricists and constructivists alike. To live by the normative injunction – one ought not to privilege any representation – is to die by the same normative injunction. Social constructivists either fall prey to infinite regress or the skeptics argument. If the extension of arguments regarding the theory-ladeness of observation leads to the dogma that social acceptance is the only truth, then social constructivists must step up to the plate regarding the theory-ladeness of their observation. While reflexive theorists note the obvious possibility of constructions being constructed, the "solution' is plugging the postmodern holes by the incorporation of ever more sophisticated "crisis discourse." The game, as Collins and Yearly (1992), argue is one of "epistemological chicken":

> ... following the lead of the relativists, each new fashion in SSK has been more epistemological daring, the reflexivists coming closest to self-destruction. Each group has made the same mistake at first; they have become so enamored of the power of their negative levers on the existing structures as to believe they rest on bedrock. But this is not the case. Though each level can prick misplaced epistemological pretensions, they stand in the same relationship to each other as parallel culture; no level has priority and each is a flimsy building on the plain (308).

The game, Collins and Yearly conclude, turns out to be essentially conservative given its "poverty of method" and ineffectiveness as all but a rhetorical target. [4] I counter the game is conservative, on a constructivist account, because it is necessarily foundational. To be able to

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[4] I note the use of the adjective "conservative" being appended regularly to describe the political effect of radical relativism and postmodernism. Fuller calls Kuhn's effect conservative in normalizing the science/society relationship and effectively pulling the plug on early STS political activism.

deconstruct the deconstruction, one must hold the object of the deconstruction "still" and recognize the result. When a community holds the successful result of an act of deconstruction as a state of affairs about the (or a) world, the result is fundamentally operationalized as being real. For example if a postmodern theorist argues that, from a designated methodological perspective, they have deconstructed science, an effect is shown. The designated perspective, feminist theory, literary criticism, cultural studies, acts as an ontological context. This context grounds a given interpretation or explanation of science. The importance of identifying the role contexts play in STS is to provide a bootstrap mechanism to circumvent the intent of radical constructivist and deconstructionist practice.

The intent of deconstructive (and reflexive) practice is to hold philosophy, and politics, in suspension. The same might be said as true of social constructivism. If pulled into the game, as some scientists have become (Gross, Levitt, Weinberg and Wolpert among others), some short-term gains can be had (book sales, political muckraking), but the overall effect is to stall a project. This tactic, clothed in the presumed effects of the "theory-ladeness" of observation in science, has trapped STS. Since STS no longer acts as a progressive venture, empirically, methodologically or politically, the conservatives have "won."

5.3.2 No Privilege

The second dogma – no system of belief is constrained by reason or reality, no system of beliefs is privileged – results from a zealous interpretation of the underdetermination thesis. As commonly defined in the philosophy of science, neither the truth nor the falsity of any scientific theory is completely determined by evidence. The conclusion drawn from this observation by Kuhn, and, Strong Programme sociologists especially, was that theory choice depended on sociological factors. [5]

Interpretation of the Duhem-Quine thesis by, most prominently, Mary Hesse and David Bloor, put social factors and processes in rough opposition to logic and reasoning in determining theory choice. Laudan (1981, 1982, 1990) and Buchdahl (1982) have helped lead the philosophical charge against Bloor – particularly in attacks on this tenet of the Strong

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[5] Larry Laudan's "Demystifying Underdetermination" (C. Wade Savage, ed. *Scientific Theories*, Vol. 14, Minnesota Studies in the Philosophy of Science Minneapolis: University of Minnesota Press, 1990, 267-297) attacks the conclusion that underdetermination thesis necessarily implies bringing social factors into plays to explain theory choice. While Laudan's arguments regarding deductive and ampliative versions of the Underdeterminiation thesis need not be rehearsed here, Bloor's claims that new evidence poses neither necessary nor sufficient conditions for rational scientists to change their beliefs is laid to rest.

Programme. To amplify points regarding the structure of meta-scientific claims, I turn to an exchange between Bloor and Buchdahl.

Bloor in "Durkheim and Mauss Revisited: Classification and the Sociology of Knowledge" (*Studies in the History and Philosophy of Science* 13, 1982, 267-297) offers the following claim:

> What is it that can then account for the known stability of our explicit theoretical knowledge? For the sociologist the answer is simple.
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> Such stability as there is in a system of knowledge come entirely from the collective decisions of its creators and users. That is to say: from the requirement that certain laws and classifications be kept intact, and all adjustments and alterations be carried out elsewhere ... We need not assume a protected law of classification is singled out because of any intrinsic properties like truth, self-evidence or plausibility. Of course, such properties will be imputed to them, but this will be a justification for the special treatment rather than the cause of it.

Buchdahl [6] took exception to the notion that "stability" is a result that comes "entirely" from the decisions of scientists to protect a "certain laws and classifications" from refutation. Obviously, Buchdahl concluded, the truth of the law, and evidence supporting it, must have something to do with its stability. If a law were true, evidence would exist in its favor, scientists would accept the law based on the evidence, hence the law would remain "stable." Bloor's response:

> The fact is, however, that with no change whatsoever in their evidential basis, system of belief can be and have been destabilized. Conversely they can be and have been stable in the face of rapidly changing and highly problematic inputs from experience. So the stability of a belief is a prerogative of its users.

Laudan's charge (1990) is that Bloor's conclusion does not follow from the premises. Thus, while scientists have changed their theoretical beliefs without the benefits of new evidence, and while scientists have held their theoretical beliefs despite contrary evidence, it does not follow "stability of a belief is a prerogative of its users." Laudan points out that if one grants that in some cases evidence has not caused scientists to change, or hold, their beliefs, one cannot conclude that evidence never causes beliefs to be changed or be held. Kitcher (1998) goes a step

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[6] "Editorial Response to David Bloor," Studies in History and Philosophy of Science 13 (1982), 299-304. See also, "A Reply to Gerd Buchdahl ," *Studies in History and Philosophy of Science* 13 (1982), 305-311

further and points to the bizarre conclusion drawn from Quine's logical argument that seem to show that "...society can have no bearing on what scientists accept."

As I have argued, primarily in Chapter Four, let us assume the social constructivist position and Bloor's characterization of underdetermination and consider the consequences.

Given the social constructivists naturalistic pretensions, one can argue, on Bloor's account, that the stability of a constructivist account comes from collective decisions from its creators and users. Initially, one wonders about the social role of critics in the negotiation process; although Bloor suggests, with respect to science, that "all adjustments and alterations be carried out elsewhere." In the process of creating a system of knowledge, Bloor does not specifically address the theoretical commitments of critics. However, one can assume that if they participate in the negotiation process, they opt out at a certain stage and the knowledge system is established. However, excluding critics in the initial negotiation process provides stability at the cost of ideological like-mindedness. I make this point to show the particular vulnerability social constructivism faces regarding the skeptics argument, and, as a consequence, to suggest that a necessary condition for epistemological stability, on Bloor's own model, is something more than social negotiation.

If social negotiation is exclusive, or rule governed in some way, then social interaction, broadly conceived, underdetermines the creation of stable knowledge. Is, for example, Bloor suggesting that any group at any point, can stabilize a knowledge claim? Bloor's sociology, of course, is much more sophisticated. Once can conclude, then, that only certain groups, on given socio-historic occasions can forward and stabilize knowledge claims. Further, the kind of knowledge claims which can be forwarded is dictated by circumstance. Since not all contingent social circumstances enter the process of negotiation, and not all possible knowledge claims can be forwarded, a logic of justification is implied. Although we "need not assume a protected law of classification is singled out because of any intrinsic properties..." these properties, like truth or plausibility, are a necessary precondition for social negotiation to take place. Bloor's point is that the epistemological status of scientific claims is assigned after the process of negotiation. If we accept Bloor's premise, it does not address the *a priori* truth that constructivists assume – social negotiation produces knowledge. The truth of this meta-scientific claim is assumed by Bloor and is not necessarily the product of social negotiation. However, if one assumes that social negotiation produces the truth that social negotiation produces scientific laws, an infinite regress follows. Therefore, the truth of the meta-scientific claims that social

negotiation produces scientific knowledge is assumed *a priori*.[7] Thus, just as one can assume that evidence does not or cannot, rationally determine theory choice, one can assume that the absence of a rational choice (which acts as evidence) supplies evidence to support social constructivist theory. While Bloor may admit that sociological theory is as unstable as a given scientific counterpart, the principle of charity is assumed if, perhaps, unwarranted.

5.3.3 Explanatory Symmetry

The third dogma – there shall be no asymmetries in explanation of truth or falsehood, society or nature – follows from a rather mundane point: Scientists disagree. The nature of scientific controversy, so science studies practitioners have discovered, is that the winner is not determined by a direct appeal to evidence obtained through experiment. Since, as constructivists hold, we cannot access the world directly in the first place, different social factors must lead to competing beliefs. Again, as Kitcher points out, constructivists fall back on a caricature of science. The straw man being attacked is that a scientist *only* holds a set of beliefs based on observations about the natural world. If the scientists admits to a range of factors for holding a set of beliefs, strict rationality is thrown out and the observation is given no explanatory role, or is subsumed to the process of social construction. Kitcher makes the obvious, but needed, sociological point that reasoning during a scientific controversy is complex. During debates people can hold true and false beliefs at the same time. Scientific rationality takes time to build. People who become involved in scientific controversies have different experiences, and different cognitive and social tools from which to draw. These differences lead to the formation of different beliefs. Moreover, observations are filtered through these sets of beliefs.

What Bloor does not make room for in the symmetry thesis is the assumption that these differences as explanatory in themselves. An example of "good STS" which is at the center of

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[7] The role of ontological "givens" is the subject of much discussion in the literature of neo-Kantian constructivist (see, for example, Nelson Goodman *Ways of Worldmaking* (Hassocks, Sussex Harvester Press, 1978). Kitcher ("A Plea for Science Studies" *La Recherché Forum* www.larecherche.fr/ FOR/ SOKAL/ kitcher.html) makes an interesting point regarding the rhetorical character of the debate that follows in trying to determine whether access to the natural world is direct or mediated: " If the invitation is to throw all our beliefs away, start from scratch, and justify the claim that the objects about which we form perceptual beliefs are as we represent them, then one cannot offer our contemporary blend of physics, physiology and psychology to advance the kind of picture of perception I have sketched. But neither can the champions of Science Studies offer any rival picture, even one that makes use of screen, veils or cave walls ... If the constructivist reminds us that we haven't shown on the basis of some set of principles that are prior to the deliverances of empirical science that our scientific opinions are reliable, the right response is to confess we haven't. There is no such set of principles that will do that job, but, by the same token, no set of principles that will establish a constructivist picture. The only way to separate out the contribution of our histories of learning from our observations is to call on some parts of science in the way I have proposed."

Kitcher's analysis in Rudwick's *The Great Devonian Controversy*. **Geologists eventually came to a consensus on the ordering of strata, with two exceptions. On the symmetry thesis, we are entreated to determine why all of the actor's did not join accept the consensus finding. The implied presumption on the part of constructivists is that hidden in objections to epistemological consensus is an unexplored truth abut the social nature of science. However, difference in judgment and in sets of scientific belief simply be attributable to differences in experience and perception. Still, according to dogma, we must not privilege any sets of beliefs –even if they are right.**

5.3.4 Actor's Categories

The fourth dogma – honor must always be given to the actor's categories – turns on the historical imperative that one ought not employ concepts and categories that were not contemporaneously available to the actors. Didactically, the point of respecting actor's categories is to provide an accurate sense of intellectual possibility. To know what the actor's thought and believed is key to understanding their actions. However, as Kitcher suggests, a purist conception of only employing actor's categories is intellectually disingenuous (especially if one remains a constructivist) and denies possibilities for other forms of historical explanation and judgment. According to Kitcher (1998):

> Once we recognize that trying to suspend some current beliefs can be valuable in giving us insight into the situations as they appeared to the participants and that not suspending those beliefs can be important in leading us to recognize (from the outside) their problems and success, we can give the historians' totem its precise due.

All of the dogmas Kitcher delineates stand as normative methodological injunctions in STS.

As I have pointed out earlier in referring to Fuller (1998), the move to subsume the study of science to constructivist doctrine results in taking particular methodological approaches (i.e., Bloor's four tenets, Latour's network theory) and applying them universally. The tension in what Fuller calls "contextualist boilerplate" (assuming all is context and context is all) and what I have referred to as the "appeal to context" is the move from a general constructivist ontology to a framework ontology bracketed by a designated context. [8] By designating a context,

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[8] In the case of mindlessly generalized contextualism, there would seem to be what logicians call a 'pragmatic paradox' in which the content of what one says is contradicted by the fact that one is saying it. In STS, given

constructivists attempt to carve out stable ontological frameworks. The easiest way to do this is legislatively. For example, to assert that no set of beliefs is to be privileged runs directly counter to the assertion of a context-based perspective from which to offer a particular account of science. Of course, denying the invocation of a privileged set of beliefs applies only to the ascribed rationality of scientists; until we insist on the reflexive application of the symmetry thesis to the STS researcher. Therefore, the move to legislate STS methodological approaches is to establish a "given" and provide the resulting construction.

5.3.5 Good STS?

What does good STS look like if we accept Kitcher's criticism? "Science studies ought to respond to two clusters of phenomena. Its systematic danger is to emphasize the themes in one cluster and slight those in the other even though both should be uncontroversial." The two clusters are the "Realist-Rationalist" cluster and the "Socio-Historical" cluster. In brief summary, the realist-rationalist cluster consists of five propositions: 1) scientific research is progressive; 2) entities described in scientific research exist independently of our theorizing about them; 3) scientific claims can be refuted; 4) science depends on evidence and disputes are settled by appeals to reason and evidence; and 5) as reason and evidence progress so too do the ways we learn about the world. Kitcher essentially repeats the doctrine of scientific realism as outlined by Boyd (1983) among others. The consequences of scientific realism, for epistemology, would be a form of naturalism. STS practitioners, too, (in the case of Fuller 1993) have mandated a form of social science naturalism; whereas Kitcher and Boyd look to the natural sciences. Avoiding the naturalist fallacy, [9] Laudan (1990)[10] has argued for a "normative naturalism" – to wit:

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the tendency to become "paradigm-happy", if something works in one case, the empirical program of relativism, for example, it is presumed to work for all cases. Harry Collins, for instance, may be against any kind of universal theory of science, but is all for a universal method for studying science. Another part of the reason, which you one see in the way people pick up actor-network theory, is that it feeds into the way one demonstrates research success in the grant culture – i.e., actor-network has been applied to millions of different cases, ergo it will probably work for the one for which I want the grant.

[9] Here, I offer the general definition of the naturalistic fallacy as the idea that if something is natural, it is normatively acceptable. The expression comes from G.E. Moore, who criticized Mill for allegedly basing his ethical views on the natural dispositions of people, when in fact (so Moore thought) ethics was about ideal forms of action and sensibility. Applied to science studies, the naturalistic fallacy suggests that just because there are certain localized ways of accounting for scientific activity, it does not follow that is necessarily the best context in which to situate an understanding of science.

[10] See also Gerald Doppelt "The Naturalist Conception of Methodological Standards in Science" (1-19) Jarrett Leplin "Renormalizing Naturalism" (20-33) and Alex Rosenberg "Normative Naturalism and the Role of Philosophy (34-43) in *Philosophy of Science* Vol. 57, No. 1, 1990.

> ... epistemology can both discharge its traditional normative role and nonetheless claim a sensitivity to empirical evidence ... normative naturalists hold that the best methods for inquiry are those which produce the most impressive results ... the naturalist uses the simple method of induction to 'bootstrap' his way to more subtle and demanding rules of evaluation which, in their turn, become the license for subsequent and yet more highly refined rules and standards .. The normative naturalist is unfazed by – if anything welcomes – the much-heralded collapse of foundationalism; for he sees in the capacity of "scientized" philosophy to correct itself the dispensability of other, "higher" form of grounding (44, 58)

The role that Laudan sets out for epistemology generally, is the role I have attempted to suggest for STS in describing the implied function of meta-science; namely, that meta-scientific claims, of the kind made in STS about the nature of scientific practice, rely on the invocation of context (a set of realist categories) to bootstrap to a higher rule of evaluation. [11] Folded into the social constructivist paradigm, the structrue of meta-scientific claims is not apparent but can be discerned from the appeal to context.

In brief summary, the socio-historical cluster consists of four propositions: 1) Human beings, possessed of cognitive limitations, form complex socio-historical groups and perform scientific research; 2) Scientists have historically and socially based preconceptions; 3) Social structure in science affects theoretical debates; and 4) Social structure affects the questions posed and answers accepted in science.

Kitcher argues that these two clusters need to be considered in tandem. Put broadly, the position advocated is that scientific realism does not rule out a study of the social and historical factors which affects science. Where STS has "gone bad" is in emphasizing the social-historic cluster over the realist-rationalist cluster. For Kitcher, critics such as Gross and Levitt have rightly identified the shortcomings of STS, and damn with faint praise the work Shapin and Schaffer, Longino and Fox Keller, but tend to paint with a broad sociological brush. As a result, *all* STS appears to be driven by the common motive of demystifying science and undermining its authority.[12]

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[11] Rosenberg (1990, 37) describes the criteria to which Laudan is committed as a normative naturalist which I have argued are brought into play by the appeal to context: "... Laudan is committed to assessing this epistemology in the same way scientific theories are assessed. But it is pretty clear that assessing this epistemology against standards drawn from this epistemology is going to be pretty inconclusive. So, there have got to be other criteria, coherence, simplicity, predictive fertility, explanatory power, that an epistemology, like a scientific theory, must meet, and it must meet them, not because they are intrinsic goals of science, but because they are instrumental ones, instrumental to the goal of attaining knowledge" (42-43).

[12] In a brief sociological defense of Gross and Levitt, and if Fuller's (1998) account of Kuhn's effect on the field is true, then STS does present itself like a paradigm-based activity in which the practitioners have

Kitcher points to examples of work which wed the elements of the two clusters: Martin Rudwick's *The Great Devonian Controversy,* **Helen Longino's** *Science as Social Knowledge*, **and** **Shapin and Schaffer's** *Leviathan and the Air-Pump* **. Taken as a methodological exemplars,** Kitcher sees Longino as providing a service by carefully showing the way in which objectivity is achieved socially, and the way in which subgroups within science have contributed to the current state of epistemic affairs. In sum:

If we are to understand the complexities of the relationship between science and other social institutions, we are going to need rich descriptions of the particular instances and some parts of the sociology of science (as currently pursued) as well as the style of history that Shapin and Schaffer exemplify may aid our attempt to paint a more general picture. Similarly, Longino's thoughtful discussion of the ways in which values may, surface in scientific research can help us to formulate sharply questions that have been too long neglected in studies of science. Once we see the importance of accommodating two sets of themes, the Realist-Rationalist and the Socio-Historical cluster, its clear that even works trumpeting the hegemony of the social can serve as parts of an eventual synthesis.

The approach Kitcher advocates forms a "cluster" as well which I would designate "middle way" approaches which turn on a broad form of realism. The "concession" on the part of scientific realists is that the production of scientific knowledge is shaped by social and historical forces. However, objectivity and scientific truths about the world are not threatened when we see science as social. Kitcher points to Longino's work (with which he disagrees) to show a way in which objective knowledge remains the product of science even if at the end of a social process. Peter Galison echoes this idea.

5.3.6 The "Middle Way"

Peter Galison (1996, 118-157) ends his analysis of the complex program of nuclear-weapons building during and after World War II with a "provocative" methodological lesson (157). "Contextualized, practiced based" analyses of scientific development will remedy the inaccurate characterizations perpetuated by the progression of "framework relativists" – Carnap, Kuhn, and "recent scholars" (such as Barnes and Bloor) – whose normative analytical orientation holds that one ought to study science as a compilation of discreet, disunified objects of inquiry. Neither "universalism, physicalism, nor reductionism", proclaims Galison,

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reached consensus and share working premises. Of course, one would assume that Gross and Levitt would wish to avoid the self-same charges they level at STS practitioner's characterizations of science.

adequately capture scientific inquiry's apparent chaos. Rather science is best understood as a cluster of common skills initiating local research. These skills, coordinated in a "trading zone", transform diverse activities, disciplines, and languages into techniques (in this case Monte Carlo simulations) for manipulating representations of nature. By weaving seemingly unrelated practices together, scientists maintain the continuity of their inquiry. The ability to create order, through local transactions, from heterogeneous scientific commitments, mitigates the fragmentation of ontological frameworks and paradigms postulated by Carnap and Kuhn. The local coordination of scientific subcultures, Galison argues, speaks to a "middle way"; a plausible alternative to the unity of science movement and its relativist offspring.

Galison's work is emblematic of the current emphasis in science studies on the local production of scientific knowledge. However, the mechanisms used for locating and distributing local knowledge remain in dispute. Galison, for example, takes exception to the work of Rouse (1996, 398-416) who postulates that knowledge, like power, remains diffuse through the body politic – being both everywhere and nowhere. Still, both Galison and Rouse hold out for a form of epistemic realism. Rouse claims that "truth matters" (416), further:

> ...truth is often fiercely contested. And if one cannot stand outside that contest to assess it from a neutral standpoint, this does not mean that all claims to truth can be put forward on an equal basis. Knowledge claims are historically, socially, and materially situated in contexts that govern what can be intelligibly and seriously asserted, and how much or what kind of argument is necessary to support it. But such epistemic contexts are always in flux; their boundaries and configurations are continually challenged and partially reconstructed, as epistemic alignments shift ... To recognize this interconnection is not to devalue knowledge or science for political purposes, but to take seriously the stakes in struggles for knowledge and truth, and to place epistemology and philosophy squarely in their midst.

Here again we see the invocation of context as a stable, reflexive, epistemological and ontological platform from which to describe situated knowledge. Putting Rouses's reasoning into an STS framework, within the swirl of socio-historical activity epistemological contexts can be asserted and claims forwarded. While "in flux" these contexts are the basis on which knowledge claims can be asserted and adjudicated. Rouse holds that knowledge claims are always contested. And yet, the hedge against immediate skepticism is that the meta-epistemic context is identifiable long enough so that observation-based reports of epistemological and ontological relationships can be challenged.

Rouse (1996, 409) also argues that (borrowing from Latour):

> Knowledge is developed in an agonistic field, and will typically be contested in very specific respects. And it is precisely in those respects that knowledge will be developed and articulated most extensively and precisely. Where there is (possible) resistance, new and more powerful techniques will be sought, more precise and careful measurement will be provided and theoretical models will be refined to eliminate or by pass possible sources of inaccuracy or unrealistic assumption ... Hence, around the specific points where knowledge is resisted, there emerges a whole cluster of new local capabilities an their extension into new contexts.

Here, Rouse is engaged in *a prioi* explanation of the process of legitimating epistemological claims. The "given" is the agonistic field. Within this field arise proper, local contexts for the adjudication of knowledge claims. That the field is agonistic, and that local contexts exist, are realist meta-epistemic claims. What Rouse, and Galison appear to agree on is the need to situate a stable ontological foundation within a relativist/constructivist framework. To turn Galison's phrase, Rouse advocates a type of "framework realism." I wish simply to note the necessity of asserting an epistemological context. Whether, knowledge is "really" developed on an agonistic field I take as a meta-scientific claim to which we can bring questions about coherence, simplicity, explanatory power and the like.

5.4 Postconstructivist STS

Examining scientific controversies, David Hess (1997, 152-159) proposes elements of an "analytical framework in which "... the third party position of the social scientist ... provide[s] an outside evaluation of the scientific merits of different positions on a controversy." This aspect of "Low Church" STS has a rich history and speaks to original disciplinary concerns regarding, science , technology, ethical, social values and grass roots policy making. Hess uses, as the basis for this framework, case study material developed regarding alternative medical treatments for cancer. Through this work, Hess has developed four principles of analysis grounding a new approach. The analysis is "political", "cultural", "evaluative" and "positioned." (153-154)

The political analysis, depending on the structure and subject matter of a controversy, assumes that the dominance of one theory comes at the suppression of an alternative theory. Implied by Hess, and a theme in studies of controversies, is that forgotten in the rush to scientific consensus, were alternatives which deserve a fair hearing. A cultural analysis

consists in looking at power. However, Hess wants to maintain the idea that evidence and rationality are powerful and do shape scientific progression, Still interests theory goes to the interpretation of: "(1) the growth of the autonomy of research cultures that responds with the same internal integrity to ecological changes in the political economy; (2) the role of gender in the construction of consensus knowledge; and (3) the place of transcultural movements in the destabilization of a consensus and associated suppressive apparati" (154). The social scientist, then, would show how changes in political economy, gender specific conceptions of, among other things, power, and alliterative cultural perspective would destabilize consensus. The kinship Hess shows with canonical postmodern cultural studies does promise a way to escape the gravity of the constructivist paradigm. The evaluative aspect of Hess' program leans heavily on a social epistemology. Knowledge claims are presented in a sociological court of law. Still, the case law depends "...on a wide range of sources in the philosophy of science, including the work of feminists as synthesized by Longino" (1954). Finally, that the sociologist would be "positioned" smacks of trying to retain the epistemological high ground. As implied in much STS analysis, individual reflexivity seemingly offers a negotiating stance in which the "third party" can monitor themselves, individuals and institutions involved in the controversy (what Collins and Yearly, 1992, call "meta-alteration"). Thus, when push comes to shove, postconstructivist STS turns to the tacit dimension of anthropological inquiry – not a promising beginning.

5.5 Dissertation Perspectives

> Bruno Latour is astute enough to concede that if the objects of scientific study are mere social constructs, then so too is 'Society', the object of social-scientific investigation. What escapes his notice is that the same thing could be said of networks of social actors: they could be mere social construct cobbled together by social scientists ... Latour, like Karin Knorr-Cetina ... presents us with a false dichotomy: either (1) science is merely a 'description' of an independent Nature that is causally responsible for this description, or (2) science is a set of beliefs about Nature that is socially constructed through networks of professional allegiances among scientists. If to be a social constructivist is simply to be someone who believes that there is more to science than just writing down what one sees, then we are all social constructivists in this weak sense (Schamus, Segeratrale and Jesseph "A Manifesto" *Social Epistemology*, 1992, Vol. 6, No. 3, 244-245)

While Schamus' initial point is well taken, this dissertation pushes the analysis back an additional step. Thus, I analyze the way in which the process of social construction can, in

itself, be conceived as socially constructed in STS. In so doing I look at two aspects of the socio-historic composition of STS:

The first, in Chapter Two, is the development of the American research university as legislated by the Morrill Act of 1862. American STS is uniquely and inexorably linked with the development of state research institutions. Ideally conceived, the mission of these universities was to democratize higher education. Analysis of congressional debate at the time of the legislation's passage suggests great concern with securing a relationship among agricultural research, technology, economic development and federal and state infrastructure during a time of national crisis. Access to higher education provided a trained workforce. Roughly a century later, STS would ideologically ape the land-grant mission – with science substituting for education as a whole. Although adopting the rhetoric of democratization, the institutional survival of American STS depended, in part, on supporting the land-grant mission. My contention is that social constructivism, as the theoretical basis for American STS, is prefigured by its institutional setting – social constructivism is a construct of the American research infrastructure. Consequently, constructivism supports the scientific, technological and economic alliance by helping to dissipate social criticism. The programmatic bickering within STS, the Sokal Hoax and the effect of "science wars" offer evidence to suggest the activist social agenda was effectively hamstrung by social constructivism owing parentage to the American research university

The second, in Chapter Three, is the intellectual foundation of STS – Thomas Kuhn's *The Structure of Scientific Revolutions*. If *Structure* is the founding text of STS, its effect has been to mold STS into a paradigm-based activity. As a result, all of the misapprehensions of the scientific image Kuhn documented, have been visited on STS. Most profoundly, STS lost a sense of its critical mission and went about the Quixotic process of trying to produce enough empirical case studies, as paradigmatic inquiry warrants, to show that science is just another ideology. I have argued that the constructivism thought to radically deconstruct science was anticipated by the Kuhnian narrative. Constructivism became an internal dialect of paradigm-based STS which no one, especially scientists, could understand. Thus, STS was rendered politically ineffective. Here, social constructivism is a construct of Kuhnian science. However, the teleology embedded in Kuhnian history, a product of Jean Piaget's developmental theory and Nelson Goodman's neo-Kantian constructivism, supplies STS practitioners with a way out of the social constructivist paradigm.

As I have argued, Kuhn is generally interpreted as collapsing meta-level discourse regarding truth and rightness to the object level discourse of paradigms. As a result, scientists

were seen as having no rational basis for theory choice. Still, the seeds for meta-level assessment were planted by the kind of neo-Kantian constructivism advocated by Nelson Goodman. The consequence of the "new" riddle of induction (in *Fact, Fiction and Forecast*, **1955**), was to force Goodman into a many worlds ontology. To avoid the consequences of an apparent radical relativism, Goodman would later propose (in *Ways of Worldmaking*, **1978**) that "world versions" could be determined to be "right" or "true."[13] While tricky to navigate, the consequence of Goodman's notion of categories as corresponding to "rightness of fit" offers criteria for preferring one constructed world to another – these include logical consistency and theoretical elegance.[14] The rough equivalence Kuhn would draw to "world versions" would be "images of science" . On Kuhn's account a pedagogical version of science could be right or wrong (and revisable on the meta-level by the appeal to history). The rightness or wrongness of the pedagogical image of science was a matter of fit with scientific practice. History was the arbiter.

Social constructivists saw, in their interpretation of Kuhn's radical relativism, a way to unseat the hegemony of science. However, the appeal of epistemic privilege was too strong. Goodman's "world versions" and Kuhn" images of science" were substituted by appeals to the "contexts" of science. Still, as constructivists followed the Duhem-Quine thesis to its radically relativist conclusion, the epistemic value of context-based perspectives lost connection to the practice of science itself. Criteria for rightness of a meta-scientific perspective became relative on a meta-theory level. Latour's actor-network theory, the empirical program of relativism, reflexivity, discourse analysis, and assorted ethnomethodological programs offered epistemic claims relative to the meta-theoretical perspective they were advocating. Therefore, that a "network" exists, for example, became relative to the positing of actor-network theory – a tightly circular argument.

Many social constructivists, however, did not follow the relativism they espoused into a complete *reductio ad absurdam*. Although as Collins and Yearly(**1992, 323**) point out, actor-network and reflexivist theories have basically jumped in the hole in the road from which there is no escape. The constructivist that do not fall prey to a *reductio ad absurdam* rely on the

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[13] "Willingness to accept countless alternative true or right world-versions does not mean that everything goes, that tall stories are as good as short ones, that truths are no longer distinguished from falsehoods, but only that truth must be otherwise conceived than as correspondence with a ready-made world." (Nelson Goodman *Ways of World Making* Cambridge: Hackett, 1978, 94.)

[14] "...[R]ightness of categorization, which enters into most other varieties of rightness, is a matter of fit with practice; that without the organization, the selection of relevant kinds, effected by evolving tradition. there is no rightness or wrongness of categorization, no validity or invalidity of inductive inference (Nelson Goodman *Ways of World Making* Cambridge: Hackett, 1978, 138.)

appeal to context to provide a stable ontological platform from which to analyze science. By invoking context, I argue constructivists are necessarily committed to a form of meta-scientific realism. On these ground we can adjudicate the efficacy of explanations about science.

Meta-scientific claims are descriptive and normative statements about the conduct of scientific practice, or about how scientific practice ought to be conducted – from various disciplinary and interdisciplinary perspectives. Social constructivists argue that no fact of the matter, right and wrong, exists until closure is brought to an interpretively open situation. Closure constrains what moves can be made to justify future action. Closure also establishes social and linguistic conventions that allows for theory to be put into practice. While open in interpretive theory, the constructivist appeal to context signals ontological closure. In order to avoid a *reductio ad absurdam* and immediate infinite regress, the appeal to context is both a necessary and sufficient condition for providing a constructivist account of science.

The elements which compose a context, historical categories, actor-networks, power, market forces, rhetorical appeals, are taken to act as existing in a causal relation to scientific practice. These contexts can be demarcated from other background activities which affect science. The appeal to contexts, and the knowledge derived from them is seen bearing on, and not just the product of, paradigm-based inquiry. Kuhn's appeal to history is one example. The elements which compose a context are taken to exist in some logical and ontological relation to one another. Taken from those relationships is an interpretation or explanation of a scientific practice. Here, one can assert an initial criterion for preferring one meta-scientific account over another, for example, internal logical consistency of the elements within a designated context.

The basis for preferring given meta-scientific claims, while consistent with the science being described, must be instrumental with respect to the goal of gaining more accurate knowledge about the way science really works. Subsequently, meta-scientific claims are empirical. However, as Kitcher proposes, the judgments we make about the adequacy of a meta-scientific claims must be drawn on what we know about the socio-historical characteristics of science in relation to scientific realism. Social constructivism begs the question in first positing the affects of the social nature of science and then turning to sociology to affirm these affects.

The logic of meta-scientific claims is established by assuming a realist ontology. As a result, a relationship is implied among, say, an actor-network, a scientific practice and the world that practice describes. For a meta-scientific account to be given, the actor-network, in this case, must be assumed to exist as are the affects of the process of construction within that network. If the actor-network is not assumed to exist, the account immediately reduces to a

reductio ad absurdam. While the internal logic of the theory must be consistent, the activity being explained must be recognizable to the actors involved. Thus, Schmaus (1992, 247) et al.'s rejection criterion is well-taken: "If a scientist does not accept or even recognize our explanation of his or her actions, this is *prima facie* evidence for the inadequacy of the explanation." Further, as the scientists actions are linked to a theoretical framework, a meta-scientific explanation must show a logical connection to the outcome of the practice.

Before science needs to be brought directly into the justificatory framework, however, meta-scientific accounts can be adjudicated on their own grounds. Therefore, observation statements made within a meta-scientific framework (i.e, science is more objective if done by women) must meet epistemological criteria like coherence and explanatory power. A good deal of legislative work can be done *a priori* in taking the realist basis from which meta-scientific claims stem and seeing if they are necessary conditions for scientific practice.

5.6 Future Work

Our knowledge of science has been enriched by STS, and, perhaps, we have come to learn more about what science is not than what it is. We know that science is not *just* a series of logical propositions arbitrated by experimental results. We know that science is not *just* a social construction and like every other ideology. And yet, STS seeks comfort in familiar battles with a known enemy, positivism, in service to an old master, social constructivism. Although the social constructivist paradigm is in crisis, the move to a contingent foundationalism has yet to begin in earnest in STS. Likely, the postmodern hangover will continue to obscure a clear direction for STS. Still, I have argued that STS practitioners need to look no further than their own methodological framework for a solution; the appeal to context belies a form of meta-scientific realism. For STS to put itself on secure epistemological footing, a process of adjudicating knowledge claims must be put in place. From the current STS perspective, science is all things to all researchers. I have suggested a means by which the meta-scientific claims can adjudicated.

Let us consider the following two claims:

1. Science is a set of beliefs about nature that is socially constructed through networks of professional allegiances among scientists.

2. Science is a set of beliefs about nature that is socially constructed through the use of rhetoric in scientific discourse.

I consider both these claims to be meta-scientific: they describes how science is practiced. Taken at face value, however, both claims are epistemologically undifferentiated. In either instance, only one outcome is possible – the social construction of science. Yet, as I have argued, the appeal to context entails meta-scientific realism; thus in the first claim (above) the existence of a "network" is posited and can be identified. In the second claim (above) the existence of a rhetorical effect (that leads to the construction of scientific beliefs) is posited and can be identified. If these contexts are mutually exclusive, inasmuch as they lead to the construction of beliefs about nature, either networks or rhetoric offers a causal explanation. In adjudicating among mutually exclusive context-based claims, the first criterion would be coherence within a given context. One can not hold a network to be a causal explanation in the same way as rhetoric if one accepts a rhetorical context. The relation among existing entities within contexts makes the difference in these claims apparent and relevant.

As these claims posit different entities in a causal relationship to science, then they are, to some degree, exclusive. Of course, one could suggest that rhetoric is a tool used by scientists in a network to construct scientific belief. However, I would argue that within the context of actor-network theory rhetoric implies a distinct set of ontological relationships. Conversely, one could suggest that actor-network theory is a rhetorical strategy used in the construction of scientific beliefs. Again, the implied ontology is unique. Meta-scientific realism, then, is tied to the context which is invoked. Further, the initial criteria for choosing context-based account over another would be the internal coherence and explanatory power. Therefore, we may choose an actor-network account over a rhetorical account based on its comprehensiveness and simplicity. One possible area for future work would be to examine ways in which the elements of one context can be translated into another context. The differences between context-based account have been suppressed to the social constructivist paradigm.

If claims 1 and 2 are not exclusive, we can begin to enumerate which contexts are causal and which are not. The difficulty exists in translating among contexts – a subject for future research. For example, do entities and categories posited in actor-network theory exist in rhetorical theory, or in feminist theory, or in postmodern theory? Although the boundaries dividing these theories is neither hard nor fast, a series of ontological commitments follows from any theory-based explanation. By asserting ontological differences within context-based account of science, I suggest a more sophisticated philosophical approach to the study of science.

The constructivist paradigm has hindered the development of a philosophical imagination in STS. STS can offer a theory of philosophical knowledge in relation to science;

perhaps more directly than traditional epistemology. I have argued that we can adjudicate knowledge claims about science on realist grounds. In so doing, I have first concentrated on the relation of claims about science, context-based claims, to one another. That is, whether social constructivist really believe what they say, then actants, networks, meta-alteration, power, and the like, exist – and they exist in some relation to science. Initially, then, a given meta-scientific claim can be adjudicated *a priori* in logical relation to the context which supports it. However, the early constructivist critiques were seductive and, to some degree, effective. In bringing everything to the level of the social, STS practitioners could avoid the hard meta-philosophical questions of the relation of meta-scientific knowledge to science and the natural world. Once a meta-scientific claim is taken to be logically consistent within a given context, then a logical relation must be made to the scientific practice which is being described. Future work, then, in developing a philosophy of STS is to further develop the logical grounds on which meta-scientific claims can be adjudicated. Once the dead wood has been eliminated, we can turn to the question of if the epistemological claims made in STS and science are justificationally similar.