Chapter Four:
What Is A Context?

> *...The methodological horrors [of reflexivity] should not be managed by erecting a hierarchy of knowledges that are variably infected by problems of representation, nor managed by supposing the horrors affect other inquirers' work. So we should not suppose that some sciences escape the problems of representation and reflexivity; nor should we suppose that our own work is invulnerable. There is nothing surprising in these worries. Any interpretive enterprise will confront this version of the hermeneutic circle.* (Simon Schaffer, from "Contextualizing the Canon", 209)

> *The initial stage, the act of conceiving or inventing a theory, seems to me neither to call for logical analysis nor to be susceptible of it ... This latter is concerned not with questions of fact ... but only with questions of justification or validity ...* (Karl Popper, from *The Logic of Scientific Discovery*, 1959)

4.1 General Introduction

In this chapter I argue that the social constructivist invocation to put science in 'context' necessarily implies a logic of justification. Made explicit, this logic of justification provides a basis on which to adjudicate meta-scientific claims. By examining sources and debates within STS, I show how the appeal to context blurs internal and external references, offers an ontological starting point for STS accounts of science, serves as the basis for methodological debate, and refers to values which anchor a notion of scientific objectivity.

This chapter is divided in six sections. Section 4.2 opens with definition of context and then outlines the central argument of this chapter: Social constructivists, in offering interpretations and explanations of scientific practices from a context-based perspective are appealing to a latent form of meta-scientific realism which implies a logic of justification. A context is treated as a real entity which possesses a series of defined, justifiable ontological relationships. That one chooses a specific context in order to interpret or explain a scientific practice implies a series of implied preferences. Made explicit, these preferences cohere, often on the basis of a normative injunction,[1] to a relationship among the context, an identified

[1] A "normative injunction" is an attempt to legislate methodological approaches to a given subject. For example, Fuller (*Philosophy, Rhetoric and the End of Knowledge* Madison: University of Wisconsin Press, 1993) states that the "fundamental mandate" of STS is that "Science *should* be studied as one would any other social phenomenon, which is to say, scientifically..." (my emphasis, 9). Following Fuller's mandate, a context-based perspective can be shown as either giving, or not giving, a coherent account of science. Whether or not the normative injunction is valid is a separate question.

scientific practice, and the world that practice describes. Section 4.3 analyses Kenneth Burke's philosophically and historically-based notion of contextual definition in order to illustrate the paradoxes of locating a definition in relation to a context. Burke's description of the relationship of a specific entity to a whole is also found in discussion of Kuhnian paradigms and sheds light on philosophical problems faced by radical relativists. Section 4.4 begins with another review of the presumed progress and stability of STS as an academic enterprise. The notion of STS progress is wrongly credited to the advancing number of contextual perspectives under which science and technology can be studied. To illustrate the confusing array of references to context, I survey the *Handbook of Science and Technology Studies*. Still, I take the *Handbook* to provide, as described in the introduction, "... an act of imaginative risk taking as of diligent codification" (xi).[2] To focus my analysis of references to context in STS I look, in the opening of section 4.5, at Fuller's distinction between content and context. Fuller explicitly defends a position implicit in much STS analysis – presuppositions are more relevant in explaining scientific outcomes than scientific practice. I then turn to a debate between Mario Biagioli and Simon Schaffer to examine how, from a relativist perspective, one gains access to the historical contexts which affect not only scientists but STS practitioners. If, for example, a necessary condition for performing historical research (in this case) is the existence of a canon of literature, can Biagioli and Schaffer's critiques be seen as a contingency anticipated by the canon rather than access to, and analysis of, the dynamics of the canon itself? Finally, in section 4.6, I examine the most cogent analysis of the affect of context, and context based values, in science – Helen Longino's *Science as Social Knowledge*. Since presuppositions (contextual values and beliefs) affect scientific practice, Longino advocates a form of the ideal speech community in which practitioners assumptions are made explicit and worked out in order to achieve objectivity. Longino's move is to change, philosophically, the grounds of what is considered evidence from observation statements to objects of observation (scientific practice for example). STS practitioners take this move as crucial to the project (in slightly varying forms) of developing a mature post-positivist view of science. Through the examination of the various appeals to context, primarily within STS, I offer the following analytical points:

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[2] The concept of context, while central to the methodological approach of STS, is not reviewed in a systematic way within the literature. The *Handbook* offers a codification of references to context. Roughly, reference to context, in STS, implies epistemological holism and necessarily commits one, I will argue, to a form of meta-scientific realism. I contrast the contextual approaches in the *Handbook* to Longino's (1990) more sophisticated formulation of "contextualism" and "contextual values." Although context in both senses refers to a form of external values, the literature in the *Handbook* shows a curious lack of reflection of appeals to context philosophically entail.

• Contexts are assumed to exist – this presumption is the basis for many STS analyses;

• Contexts can be accessed, and demarcated from, other contexts in which they may be embedded; hence, contexts are not paradigm-bound; [3]

• Since contexts exist, the elements composing them (e.g., historical conditions, social institutions) are taken to exist in some logical and ontological relation to one another;

• Contexts are taken to affect scientific practice – this is the assumption on which social constructivist analyses turn;

• Since contexts affect scientific practice some logical and ontological relation exists among the elements of the context, a given scientific practice, and the world that practice describes;

• Contexts are taken as temporally stable; hence, infinite regress is not an immediate consequence;

• Since contexts exist in a stable, ontological relationship to the scientific practice they interpret or explain, criteria for justifying one context-based perspective over another are necessarily implied;

• Contexts necessarily imply justificatory criteria and, thus, imply a means for adjudicating among contexts and context-based statements. Implied legislative criteria need to be made explicit;

• Since contexts imply a logic of justification independent of the scientific practice being examined (in order to avoid the naturalistic fallacy) meta-scientific evidence consists of observation statements;

• Context-based observation statements can be adjudicated *a priori*;

• A form of meta-scientific realism exists.

4.2 Argument Summary

'Context', in common parlance, refers either to statements proceeding a word or passage, or a set of circumstances or facts surrounding a given situation.[4] Both references appeal to a

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[3] The assumption being that we can get to the "pure suppositions" on which an activity is based. For example, to say that "the world is constructed out of other worlds we take as given" is considered a true statement about the nature of the world. Of course, as a constructivist, one could claim that this statement is nested in another series of presuppositions and so on until an infinite regress follows. A context is taken as a "stopper" of infinite regress and, on a constructivist model, can be seen as the elements which compose a world. Here, social constructivism seems particularly susceptible to the skeptics argument. Assuming a constructivist position, I argue that we can adjudicate among these elements and the worlds they create and, hence, get to the best of all possible worlds – a circle which leads back to the world known through science.

corresponding "big picture" that surrounds an utterance or event. The big picture consists, in part, of additional utterances and events possessing various logical relations to the utterance or event we wish to frame. Putting an utterance or event in context implies that "knowledge about" past assumptions, present circumstances, or future contingencies produces an additional, more accurate, meaning. Context, then, is logically determined at the time of an utterance or event; and as such is held, necessarily, to have some explanatory or interpretive value. The elements composing contexts vary, but the form of inquiry being pursued, and the normative injunctions supporting it, make the appeal to some contexts appropriate and other contexts inappropriate. For instance, a normative injunction accompanies the claim that an utterance or circumstance must be placed in a "proper" context. What a proper context ought to consist of is a matter of negotiation. Generally, that utterances or events occur within contexts is taken as a given. However, determining the elements comprising a context (proper or otherwise), and the relation of utterances or events to those elements is a matter of negotiation and, often, dispute.[5]

The appeal to context illustrates a latent form of meta-scientific realism which is the basis for a philosophy of science and technology studies (STS). To make this point, let me begin with a meta-theoretical distinction. Reference to a set of necessary logical relations among named elements, individual psychological states or social conditions for instance, which leads to a designated result, implies a context. For example, one may postulate that because men, at one time, dominated science, a gender-specific perspective developed. The background assumptions informing male-dominated science led to a concept of nature which, in turn, yielded a form of experimental inquiry which result in biased findings. We may provide evidence for the conclusion that "masculine science" led to a given scientific outcome by referring to the

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[4] Henderson ("Epistemic Competence and Contextualist Epistemology: Why Contextualism is Not Just the Poor Person's Coherentism" *The Journal of Philosophy*. Vol. 41, No 12, December 1994, 627-649) argues for a "contextualist epistemology of empirical knowledge" which lends an account of "perceptual competence" in which perceptual beliefs are articulated in accord with a presupposed background that is taken for granted and cannot, in many cases, be articulated.

[5] Hans Reichenbach's (1938) distinction between a knowledge claim's "context of discovery" and "context of justification" (or validation) can be considered (see Nichols 1980) as an epistemological gradation among recently justified claims and established claims within a research program. The existence, nature and implications of Reichenbach's contexts is disputed. For example, the distinction between the contexts of discovery and justification presupposes a form of progressive epistemic refinement in which the historically later scientist always has the upper hand in legitimating their claims. In this instance, the context of justification implies a Whig history. Given a Kuhnian interpretation, in the absence of a basis for paradigm translation, only by rough, and increasingly irrelevant, equivalence could only apply the same laboratory test to oxygen as dephlogisticated air. Fuller (1988, 7-10) argues that the debate over ideological consequences of the contexts of discovery/justification (such as scientific determinism) can be seen differently as a matter of social negotiation. Of course, social, or rhetorical, negotiation is yet another context in which to place the dispute. Fuller's appeal to other contexts, the social and rhetorical for instance (as well as the historical in the above examples), to reconfigure the debate over discovery/justification is part of a possible infinite regress.

history of science, feminist theories of science, and / or the sociology of scientific knowledge. In so doing, we refer to the contexts, historical, feminist, sociological, which influence, and are influenced by, science. Science, on this account, is a social construction.

Social constructivists hold that the subject matter of scientific research is wholly or partly constructed by the background theoretical assumptions of identified communities. These background assumptions are the "contexts" in which science participates. Absent a normative injunction as to what a context-based account ought to cover, any constructivist account can be trumped as incomplete. Given the holist perspective constructivism entails, one may always counter that another context exists which renders a more adequate explanation, or interpretation, of a scientific event. To counter infinite regress, constructivists must bracket, and take as sufficient, a context-based explanation of a scientific event.[6] By putting a scientific event in context, social constructivists are obligated to a coherence form of justification. Forced either to accept a normative injunction as to what constitutes a proper contextual account, or to bracket a scientific event in context without the problems of infinite regress or incoherence, the constructivists must implicitly accept a logic of justification. Therefore, if one assumes social constructivism without putting science 'in context', then one cannot access the supposed mechanisms of social construction. However, if one assumes science takes place in context, then a justificatory logic is implied *a priori*. As a result, claims about science 'in context' can be adjudicated at the level of meta-science.

As argued in the previous chapter, meta-level evaluative criteria, like adherence to methodological rules, is subsumed to the process of social construction itself. This move parallels Thomas Kuhn's (1962) notion of paradigm-based science. In science and technology studies (STS), social constructivism acts as a governing paradigm in ways similar to Kuhn's account of normal science activity. Social constructivism, strictly conceived, makes all propositions regarding scientific practice thoroughly contingent. If all science is social, and all accounts about science are social, then the epistemic value of a particular constructivist account depends on all other minimally related, continuous, acts of construction.

In order to have a stable ontological platform from which to offer an account of scientific practice, social constructivists invoke the 'contexts' in which science participates. Abstracted from disciple-based perspectives, found, for example, in history, sociology or philosophy, a context refers either to statements proceeding a word or passage, or a set of

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[6] I include here reflexive contexts (as conceived in the sociology of scientific knowledge) which, even with the express purpose of destabilizing traditional hierarchies and opposition relationships, must suspend the reflexive process in order for expression to occur.

circumstances or facts surrounding a given situation. Social constructivists assume that contexts exist and that they effect the conduct of science. However, if consistent, social constructivists must insist that contexts themselves are socially constructed. Without privileging their own act of construction, the background beliefs and assumptions which affect a scientific event cannot be fully known as the process of construction is eternally fluid. Background assumptions, then, can only be identified within a given context. However, the appeal to context entails a form of holism which leads to an infinite regress. On a strict constructivist account, all the contexts in which science participates – historical, economic, rhetorical, social, *ad infinitum* – affect scientific practice. The appeal to a privileged context assigns an ontological status to the relations defined in a social constructivist account: That these relations do exist, or ought to exist, leads to a form of justification.

4.3 Contextual Definition

Kenneth Burke (1969, 24-26) offers a cogent historical account of "contextual definition" and accompanying paradoxes: "... [H]ere the intrinsic and the extrinsic can change places. To tell what a thing is you place it in terms of something else. This idea of locating, or placing, is implicit in our very word for definition itself: to *define*, or *determine* a thing, is to mark its boundaries, hence to use terms that possess implicitly at least, contextual reference (original emphasis 24). The initial paradox, then, is even if we wish to know the thing "in itself" we immediately refer to external categories (i.e., "thing in itself") or definitions. To illustrate, Burke refers to the expression "a man of substance." In determining if someone possesses "substance" or "standing" we take up a consideration of, among other things, character traits, economic resources and social status. Following Aristotle, however, we see Burke's pun – substance, or being, is to be considered "in itself." A man, a rock, a tree could be a substance capable of being considered intrinsically.

Contrasting Spinoza with Aristotle, Burke describes thinking contextually as defining a single object within a total context – an entity in relation to the universal whole. On Burke's account, Spinoza "explicitly held" that to define a thing in terms of context we "... must define it in terms of what it is not" (25). As such, definition becomes a matter of negation. The paradox here is, in Burke's words, "all determination is negation" or more bluntly "every positive is a negative" (25). In sum, contextual definition, Burke states:

> ... might also be called "positional," or "geometric," "or definition by location." The embarrassments are often revealed with particular clarity when a thinker has moved to a high level of generalization, as when motivational matters are discussed in terms of "heredity and environment," or "man and nature," or "mind and matter," or "mechanism and teleology," where each of the paired terms is the other's context in the universe of discourse. To define or locate "man" in terms of "nature," for instance is to "dissolve" man "into" nature. Hence, the more thorough one is in carrying out his enterprise the more surely he opens himself to the charge of failing to discuss man "in himself." Historians who deal with art in terms of its background are continually suffering from the paradox of contextual definition, as their opponents accuse them of slighting the work of art in its esthetic aspects; and on the other hand, critics who would center their attention upon the work in itself must wince when it is made apparent that their inquires, in ignoring contextual reference, frustrate our desire to see the products of artistic action treated in terms of the scene-act, scene-agent, and agent-act ratios. (26)

If we want to know the substance of a thing, we tend to move effortlessly between intrinsic and extrinsic references. We simultaneously appeal to knowledge of the intrinsic nature of a thing, and to knowledge of the relational nature of a thing. For Spinoza, to see a thing as contextually defined was to see it in terms of the absolute and of the necessary; the appeal to context was an appeal to everything. Defined contextually, each object is bounded by the other things that surround it. By negating contextual elements, one begins to see a designated term with respect to what is necessary or substantive. In pairing terms such as "science and society" or "science and religion", we posit a relationship which allows us to define terms with respect to one another. However, in establishing this relationship we not only imply a justification of the way in which these terms speak to one another, we tend to see equivalencies in paired terms.

STS, in offering a higher level of generalization (to paraphrase Burke), defines science in the context of society; ultimately, science is dissolved into society. Dissolving the boundaries of science serves the postmodern dicta of deconstructing science to make it permeable to a prescribed ideological critique.[7] Additionally, a distinction can be made between science as a

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[7] Mark Lilla ("The Politics of Jacques Derrida" *New York Review of Books* Vol. 45, No. 11, June 25, 1998, 36-41) identifies Derrida's unique influence within the United States as "...first introduced into literary criticism, now circulate in the alien environment of academic postmodernism, which is a loosely structured constellation of ephemeral disciplines like cultural studies, feminist studies, gay and lesbian studies, science studies and postcolonial theory. Academic postmodernism is nothing if not syncretic, which makes it difficult to understand or to even describe. It borrows notions freely from the (translated) works of Derrida, Michel Foucault, Gilles Deleuze, Jean-François Lyotard, Jean Baudrillard, Julia Kristeva – and as if that were not enough, also seeks inspiration from Walter Benjamin, Theodore Adorno, and other figures from the German Frankfurt School. Given the impossibility of imposing any logical order on ideas as dissimilar as these, postmodernism is long on attitude and short on argument. What appears to hold it together is the conviction that promoting these very different thinkers somehow contributes to a shared emancipatory political end, which remains conveniently ill-defined" (36). The theory-wing (High Church) of STS displays all of the

social construction and science as participating in a social context. If one holds that science is a social construction, then, following Aristotle, one can know science in itself. As previously argued, this view follows from what Fuller (1992) calls Kuhn's conception of science as an "... Aristotelian picture of science-as-organism ..." (1992, 270-271). Here again, social constructivists, following Kuhn's lead, fold all objects of study into the constructivist paradigm. On strict grounds, social constructivists argue that since science is social like other institutions, and we are social beings, we can come to know science as we know other social institutions. Given the absence of conceptual boundaries, the translation problems, over which positivists obsessed, disappeared. On Burke's description, the extrinsic – "the social" –becomes intrinsic to science, and inasmuch as we can know society, we can know science.

To avoid the reflexive problems and infinite regress, however, some constructivists appeal to context in itself. The contexts delimiting science are separate from, but contain the intrinsic elements of, science. In, for example, the paired terms of science and society, STS practitioners easily move from the broader context of society to science. However, society is bracketed as a term which can be separately identified and which, in a given analysis, does not entail another context. The attempt is to stabilize the movement one way – from extrinsic to intrinsic reference. To avoid the reflexive problem inherent in social constructivism, Harry Collins (1981, 216; Collins and Pinch 1982, 190) has argued that patterns of explanation applied by sociologists to science, need not be applicable to sociology itself. From example, to see science as socially constructed does not reflexively entail that sociology is socially constructed. Still, the appeal to context runs deeper in STS. Putting aside the reflexive problem, context remains "outside" of science. However, the problem to be avoided is not a reflexive rendering of sociology, rather a scientific reduction of sociology. Society can be contextually defined in terms of science. In STS, contextual definition serves a dual purpose; to dissolve science into society and to privilege "proper" forms of social construction.

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symptoms Lilla mentions; the most profound being the goal of destabilizing (or deconstructing) the hierarchy of science in the margins in order to achieve democratic participation in science policy-making. However, the practice-wing (Low Church) of STS has offered more concrete means for democratic participation than Lilla suggests especially at the level of public meetings and the production of scientific, technical and government documents.

4.4 'Context' In STS

Stories regarding the development of Science and Technology Studies (STS) appear to describe, at once, either increased theoretical sophistication (Fuller 1993a, 1-33) or an orchestrated methodological maelstrom (Bowden 1995, 64). In either instance, the theme of disciplinary progress anchors the narrative.[8] The move from studying science's internal history to studying science "... as one would any other social phenomenon (see Fuller's normative injunction, 1993, 9) is hailed as lending STS the necessary empirical basis for "studying science scientifically" (see Fuller 1993b, Chapter 1). In part as a consequence, STS has "matured" into a field with a "stable adult personality." Posited as a result of the sociological turn in STS has been a form of reflexive awareness. Finding methodological advantage out of Thomas Kuhn's description of the constraints of paradigm-bound research, the "advance" of STS research is putting science in context. On Kuhn's account normal science is the process of paradigm articulation. A necessary condition of studying science scientifically, from an STS perspective, is to superimpose disciplinary-based contexts onto the object of research. Supposedly bundled to these contexts is, to a greater or lesser extent, access to the conditions of social and historical change affecting science – which scientists do not possess. However, (as argued in Chapter 2) studying science scientifically on the constructivist model has essentially devolved into doing internalist sociology of science – STS acting as governed by the social constructivist paradigm. While STS may have "organized" under social constructivism, it is hard to see what development has occurred as a result.

A division exists between logical empiricist and historical approaches to science.[9] Social constructivism owes much to Kuhn's historicism. Kuhn's appeal to the historical and social contexts in which science occurs in order to provide evidence explaining scientific progress countered the positivist notion that scientific hypotheses could be confirmed by a rule-based assessment within the context of justification. Kuhn successfully made the case for the social dimension of science as relevant to the production of scientific knowledge. However, the notion of context, as it pertains to the historical and social dimensions of science, rests uneasily in STS.

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[8] Whether STS constitutes an academic discipline is an item of considerable debate. Still, in order to make coherent references, STS is referred to as a field, discipline, or part of an inter- or multidisciplinary matrix. Inasmuch as STS occupies a place in academia, and depends on disciplinary sponsorship, calling STS a discipline acts as a descriptive shorthand.

[9] See, for example, Wesley Salmon's "Rationality and Objectivity in Science or Tom Kuhn Meets Tom Bayes" in C Wade Savage, ed. Scientific Theories, vol. 14, *Minnesota Studies in the Philosophy of Science* (Minneapolis: University of Minnesota Press, 1990), 175-204.

Academic postmodernism, with which the theory-wing of STS is identified, supplies the lens through which the science/society relationship is viewed. Science and technology are taken to be the most prominent expression of knowledge production. Of course, on the postmodern view, the dominance of science and technology, particularly in the twentieth century, has come to suppress minority voices, or push them to the margins. Appeals to context seek to "decenter" the hierarchies empowering western science and, thereby, deconstruct the traditional relationship between nature and science. Science "in context" is science placed in a web of social and linguistic relations. "Properly" conceiving of science as *nothing more than* social relations permits "less powerful" academic disciplines (the humanities and social sciences in research universities for instance) to reveal inconsistencies between word and deed in science. Still, the problem of contextualizing science is analogous to a crippling problem found in postmodernism illustrated by John Searle.[10] Derrida, in criticizing the belief that language was a transparent medium – a "logocentrism" – had to follow the linguistic principles which led him to find hierarchies in discourse. If all language is ambiguous and interpretations of an utterance cannot be exhausted or privileged, how does one use that language to claim that language cannot make unambiguous claims? Derrida dismissed such paradoxes as irrelevant and part of his campaign to deconstruct western philosophy. However, given its profound ambiguity, deconstruction is not a platform from which one can mount an effective political agenda. Analogously, in showing that science is just another ideology, STS practitioners have to follow an ideology (social constructivism) which reveals the absence of an epistemological privilege. If all ideologies are the same, and interpretation of science cannot be privileged, then how can one assert one interpretation of science over another? Thus, on what grounds can one assert whether or not science is an ideology just like any other? The answer to this dilemma is to find an "out" – the appeal to context.

In STS, the first move toward contextualism is credited, perhaps wrongly, to Kuhn. Kuhn's emphasis on the historical and social contexts which science inhabited revealed a picture different than the one drawn by the positivists. In outlining a structure for scientific progress, Kuhn was thought to have revealed something that was true about science. The "historiographic revolution" was to evaluate science "in its own time." However, the lesson drawn by STS practitioners was that any field could engage in science studies and, as a result,

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[10] John Searle "The World Turned Upside Down" *The New York Review of Books* Vol. 30, No. 16, October 23, 1983, 74-79.

had the power both to deconstruct the scientific image and to reconstitute their field as a science. Consequently, science could be put into the context of any field of academic endeavor. However, in criticizing the belief that science was a strictly rational, incrementally progressive, institution, STS practitioners had to privilege their own form of inquiry. Known as the "reflexive problem" in STS, practitioners, particularly in the area of the sociology of scientific knowledge (SSK), had to apply the same form of postmodern inquiry to their knowledge about science. Moreover, the problem of context leads to an infinite regress. If we place science in, say, a sociological context, and conclude science is socially constructed, then the social is privileged as an interpretive or explanatory medium. If, for the sake of methodological consistency, we say that both science and SSK are socially constructed, then we privilege the reflexive awareness which provides that perspective. In order to stop the regress of context, a stopping point must be assigned and privileged:

> You're going to have even more reflexive problems, if you like, because for the purposes of this study you're going to have to treat the sociology of scientific knowledge as completely constructed, and having no kind of reality at all, with it kind of being socially negotiated. And yet, boy, you're going to have to assume that your findings have got some objectivity, have an out-there-ness. That's going to be even harder to pull off than the tricks I do! (H.M. Collins, from Ashmore, 1989, 24)

The history of STS shows that contextualism acts a normative imperative which trades on a series of metaphysical and ontological ambiguities to gain epistemic currency. Placing science "in context" suggests a new perspective unavailable to scientists, and denied to, as a methodological consequence, internalist historians. Knowledge of the contexts identified with science, then, supposedly leads to a more accurate rendering of its practice. I argue that disciplinary contexts simply bracket an understanding of a science and, therefore, cannot provide the holistic or synthetic perspective sought by STS practitioners. Gary Bowden rightly suggests that the "... thread holding the fabric of STS together is the notion that S&T [science and technology] must be viewed *in context* (original emphasis)" (75). Still, the legislative framework of appropriate contextualist approaches remains discipline-based and latent at the level of meta-science. Debates over specific historical, sociological or philosophical approaches rarely go beyond criticisms of the image of science and technology promoted in a given interpretation. The image of a mature STS goes hand-in-hand with the number, and implied succession of, context-based analyses. By simply multiplying the number of contexts in which science and technology can be studied, STS is said to mature. Moreover, the addition of a

particular context is considered to trump the contexts previously offered by supplying a more holistic position. For example, the addition of sociologically-based interpretations of science (e.g., feminist approaches), is understood, on its face, to be more comprehensive and methodologically sophisticated than interpretations of science which are not sociologically-based. Contextualism, then, uncritically assumes that the addition of interpretive and explanatory avenues will yield a more detailed picture of science and technology. The nature of contextualism begs the question of whether knowledge about science and technology is necessarily unique, reflexive, comprehensive and cumulative.

4.4.1 Context as Used in the *Handbook of Science and Technology Studies*

'Context', in STS, often acts as a free floating designation. Putting science "in context" was, in the beginning of STS, a pedagogical move to show apprentice scientists their obligations to society. David Edge (1995, 8-9) refers to the U.K. Committee on Manpower Resources, which, in 1968, promoted the British government's belief that science education should be broadened. In this instance, students were to become acquainted with the relationship among science, technology, industry, government and education. The emphasis on producing a "well-balanced output of such graduates" was to invite interdisciplinary innovation and collaboration. According to Edge, "... the pedagogical context created by such moves gave considerable scope for the incorporation of STS scholarship (especially the humanistic insight of SSK and its derivatives) in to the syllabuses of budding scientists and technologists" (9). At roughly the same time, Edge mentions, the Netherlands, by law, had incorporated the principles of "social responsibility" into all technical training. In the United States, putting science in context was seen as a matter of public outreach which was a duty of the research university.

Edge uses the sense of context in two ways. In one sense "science in context" is science as seen in relation to other social institutions – government and education for example. In another sense, context refers to a pedagogical atmosphere in which interdisciplinary cooperation was the natural result of examining the relation of science to various social organs.

In a theoretical sense, Callon (1995, 53) states: "...[C]ontext cannot be dissociated from scientific content unless we put the translations that define it between brackets" (53). Here, Callon is arguing for a theory of translation between statements produced in a scientific network (in Latour's sense) which does not require translation of those statements to the world "out there" – "the social" and "the scientific" can only be artificially separated. In articulating the notion of an "actant" (any entity endowed with the ability to act), Callon mentions that within

laboratories "... social groups interested in scientific production are also being formed – groups that make up the famous *social context*" (my emphasis, 54). Analyzing the initial development of nuclear weapons, Callon describes Roosevelt's varying opinions and the creation of "social contexts" which include numerous actants (or "entities") not exhausted by a list containing "electrons and chromatographers, the president of the United States and Einstein, physicians with their assistants ..." (54). To invoke a corpuscular theory, actants make up social groups which are brought together in networks which, in turn, compose a social context.

Gary Bowden (1995) notes the influence of American research universities (see Chapter 2) and engineering discourse on early STS; specifically the attempt "... to substitute a humanistic discourse about technology for the standard engineering discourse involving problem definition and solution ... Engineering discourse sees ... a solution because the 'problem' has been defined in a bounded manner that extracts it from the wider context and establishes the criteria that a solution must meet ..." (70) Citing founders of technology studies such as Ellul and Mumford, Bowden notes a shift in early STS away from the engineering problem solving model: "When the issue is understood within context, the engineer has not solved the problem but has made [a] choice ... In short, this viewpoint attempt to replace the authority of technical solution with human values and the related issues of ethics and political choice"(70).

Bowden speaks of a "wider context" beyond the bounds of a defined engineering (in this instance) problem. This context includes, among other things, ethics and politics. While Bowden rightly points out the perils of short-sighted solutions to which he contrasts the efficacy of solutions based on a holist model. An engineering solution must account for a ever-wider series of context-based contingencies in order to be comprehensive or ethical. Implicit in Bowden's account is that a superior "technical" solution is necessarily social. "To summarize, the formative experiences of STS's youth provide a clear sense of a field organized around the varying historical and cross-cultural manifestation of the relationship between *social context* and the process, cultures, and institutions involved with understanding, manipulating, and using nature." (my emphasis, 71) The use of "social context" as distinct from "scientific culture" plays on a common tension in many STS accounts. Science is social, but "the social" is distinct from science. A clear demarcation exists between the social within science, and the social outside of science. As a result:

> In social studies *of* science, the focus is upon a substantive topic: science and technology. In this case the analytic resources of the social sciences are applied to science and technology to understand something about that topic. Social studies *in* science concerns itself with doing "methodologically proper" social

> science. What counts as methodologically proper depends on the particular issue being raised (e.g., reflexivity) and the methodologically proper resolution for dealing with that issue (e.g., new literary forms). Research is done on science and technology not to understand something about the topic itself but to exemplify the operation of methodologically correct social science within a particular domain. As such, it fails to meet a seemingly trivial criteria, which is that it be about S&T. (original emphasis, 73-74)

As a "scholarly enterprise" STS has "come of age." Yet the methodologically "stable adult personality" of STS, apparently, sustains a "pandemonium of competing methodologies" and contains three forms of explanation; "topic focused, issue focus and combined focus" [11] By emphasizing explanation, Bowden concedes that discipline-specific techniques, from participant observation to statistical and textual analysis, are quite diverse and raise questions as to their application to "the topic of science and technology." More specifically, identifying bibliometrics as an exception, data collection and analysis in STS relies on traditional disciplinary approaches. The contribution of STS, Bowden claims, is placing discipline specific methods, and their subsequent explanations, in the light of different disciplinary contexts. As a result, methodological and explanatory deficiencies are revealed. These "inclusive" approaches drive STS's theoretical development. Still the impact of "inclusive methodology" runs various courses. Citing laboratory studies as an example, Bowden suggests while scientific practice proved tractable to sociological analysis, the same emphasis, particularly in the internalist history of science, may have hindered theory building. Specifically, Bowden offers that "... emphasis upon lab studies within runs the danger of providing methodological warrant to ... excessively micro-level studies that may inadvertently revive certain elements of historicism" (66). Here, Bowden has in mind Frederick Holmes' (1992) defense of biography as a still fertile methodological ground in the history of science. Bowden's final reason for centering on explanation in STS is that methodological choice reveals a researcher's disciplinary vision for STS, or, perhaps more accurately, one's disciplinary training before entering STS.

Briefly, the "topic focused" explanatory method in STS is best exemplified by Merton's *The Sociology of Science* (1973). The characteristics of topic-focused research include a primary focus on a substantive topic, emphasis on data collection and analysis, and little emphasis on explanation. Inherent in the discipline-specific approach adopted by the researchers are

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[11] Gary Bowden "Coming of Age in STS: Some Methodological Musings" *Handbook of Science and Technology Studies* Jasanoff et al. (eds.) Thousand Oaks: Sage 1995.

explanatory structures which, for the most part, remain unexplored. “Issue focused” methods emphasize theoretical issues applied to methodological practices, primarily within STS, and is exemplified by the work of Gilbert and Mulkay (1984), Woolgar (1988), and Ashmore (1989). “Combined focus methods” emphasize the need for a unique approach to a given, unique topic. An interesting example of this approach, one may argue, is found in Latour’s *Science in Action* (1987). Latour articulates “rules” for properly understanding science and technology not codified in articles and textbooks. Bowden rightly point out that like appropriate technology, which facilities a particular societal vision, choosing an appropriate research method presumes a particular vision of STS. The question lies, however, in how one determines, describes, and legislates an appropriate vision for STS.

Bowden rightly captures the demarcation between STS and non-STS. While Bowden finds that methodological concerns like reflexivity are important, they are not about science. That “social contexts” are, on the surface, about science requires similar demarcation criteria. Subsequently, if “social context” includes ethics and politics, then one must show how identified ethical and political concerns are about science. If the further implication is that ethical and political concerns “construct” nature, a significant relationship must be demonstrated among nature, scientific practice(s) and the social context. The necessity of constituent disciplines’ methods to the study of science is echoed in much of the STS literature (e.g, Hess 1997, Fuller 1993). However, while necessary, disciplinary resources to the study of science have not been seen as sufficient. The holism paradox following from studying science from the disciplinary perspectives is that any discipline, and perspective resulting from it, can be applied to the study of science. Matters of interpretive relevance and explanatory coherence are not addressed.

In describing whether one can determine if consensus exists in science, Gieryn (1995) offers that “...[C]onsenus is a contextually contingent product of scientists’ variable interpretative procedures, which means that, for Kuhn to conclude analytically that consensus exists in a research community at a designated time, he must ignore potentially wide discrepancies in a scientists’ own sense of the degree and kind of consensus they supposed share” (404). Applied reflexively, Gieryn must, however, conclude analytically that contexts exist in which one can determine the contingency of Kuhn’s claim, as well as the contingency of scientists claims. The metaphysical assumption is that contexts exist in which a broader set of circumstances exist for a designated time. Gieryn moves effortlessly from a designation of context as an interpretive medium, at the level of scientists, and as an explanatory medium, at the level of meta-analysis. As a result, whether scientific consensus exists is a matter of

interpretation; that consensus is a product of "variable interpretive procedures" serves as an explanation.

Evelyn Fox Keller (1995), providing an overview of work in gender and science for the *Handbook of Science and Technology Studies* asks that historians of science provide "... a deep and thoroughgoing contextualization of the analyses that were initially undertaken by feminists in such broad strokes ... surely, it is time for an integration of gender questions into this admirably careful [i.e., the work of Daston, Dear, Galison and Porter] and context-sensitive historiographical work" (91). Keller refers to the sense of context familiar in STS – the historical context. Yet Keller admits: "...[C]ontext is a big word, and it points in many different directions, a fact that the generic notion of historical specificity, even with the addition of gender, does not always capture. It may, for instance, fail to do justice to the need to attend to specificities of local social (e.g., national, ethnic, and racial) interests" (91). Although the ambiguous reference ('it') requires an interpretive leap, for Keller, a "proper" sense of context can be captured in, for instance, historical works which attend to certain criteria including race, class and gender. Keller's sense of context is normative. While taking gender into account is a necessary condition of context-sensitive historiographic work, it is not a sufficient condition. For Keller, proper historiography must include an account of "specific local interests." In offering Donna Haraway's work as an example, Keller argues for the position that gender cannot be understood apart from "the politics of race and class" – thus invoking the categorical hat trick at the root of postmodern analysis. "Haraway's very method precludes telling, or even hoping or even hoping for one coherent story, and many readers may be left feeling a bit too destabilized. But her reach toward a postmodern historiography has not only provided a new model for working with "gender" in science, it also suggests model of a politically oriented analysis of science" (92).

Keller leaves us with the following conundrum: A proper history of science ought to be thorough. To be thorough an historical account of science must be context-sensitive. To be context-sensitive, an historical account should account for the influence of gender on scientific development. However, if historians account for gender, they must consider factors of race and class. Moreover, concepts of race, class and gender are intertwined. However, since the historian can never fully account for the dynamics of race, class and gender, their account will neither be thorough nor, as a result, context-sensitive. Consequently, one cannot hope to offer a consistent historiographical account of science. This consequence is a by-product of postmodernity. The historian, then, is trapped by a normative injunction which is impossible to fill. Accounting for context, or being context-sensitive, acts as an epistemological ideal. At the pragmatic level,

any account of science can be portrayed as not taking context fully into account and, on Haraway's definition, being incoherent. Further, Keller adds: "... complementing Haraway's emphasis on fractures and partiality ... [I] plea for integration" (92). Metaphorically, Keller's vision of integration is the public high school cafeteria in which students are integrated in name only. As the number of context-sensitive perspectives is perpetually underdetermined, integration into a coherent whole is impossible unless legislated *post hoc* – yet another feature of postmodernity.

Certain methodological approaches such as the study of controversies and laboratory studies led to, according to Karin Knorr-Cetina (1995), "... a thoroughgoing sociological contextualization of science ..." (141). Laboratory studies appeal to context-based social identity to show, in an increasingly elaborate and comprehensive manner, that science is socially constructed. A "social context" affords a stable ontology in which all laboratory activities support a linear form of construction.[12] Knorr-Cetina adds that during controversies "... knowledge is deconstructed by practitioners themselves, and, as it comes apart, analysts can examine the functioning of the standards that are normally thought to hold it together and the contextual influences that inform that opponents and their work" (op. cit. 2, 163). Of course, the social actors are fully unaware that they are involved in the process of deconstructing their own work. Moreover, the study of controversies examined "... how internal scientific standards and experimental evidence fail to provide for scientists' beliefs ... and how the beliefs and knowledge claims of the scientists are influenced by their social context" (141). Knorr-Cetina draws a general equivalence between the context of justification and controversy studies, and the context of discovery and laboratory studies: the former dealing with "finished" science the latter dealing with "unfinished" science. In both instances, however, Knorr-Cetina wants to show that a social context is at play; although "raw" social construction can be seen, apparently, in looking at science behind the scenes – in the laboratory.

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[12] Social construction is, by connotation, a doggedly progressive pursuit. Scientists never socially (or autonomously) deconstruct (or destroy) a designated aspect of science. Trading, in part, on the myth of autonomous scientific and technological progress, scientists, on a constructivist account, are always "building" in the lab. Scientists are never, to extend the metaphor, involved in the singular exercise of "tearing down", "rehabilitating" or "demolishing" – all of these processes, if recognized as such, are subsumed under the process of constructing. If scientists succeed in opposing an accepted theory or observation, constructivists tend to either cast the practice as part of a larger building process, or, given normative injunction, find an appropriate similar description (e.g., the feminist objection to Latour's "agonistic field"). The concept of a social construction is not fined grained enough to capture anything other than the researcher's prior or post interpretation of constructivism itself.

> The method used in laboratory studies, ethnography (participant observation) with discourse analysis components, has become something of a contemporary equivalent of the historical case study method that became popular in the wake of Kuhn ... Constructionism is one of the major, perhaps the major, outcome of laboratory studies ... the origin of its emphatic use in STS lies in the attempt of students of laboratories to come to grips with their observational record of the "made" and accomplished character of technical effects (141).

Knorr-Cetina has it exactly wrong. Constructivism is not the outcome of laboratory studies; constructivism is a separate interpretive tool permitting ethnographers to see, and place significance on, the incongruities between word and deed in science. That science is "socially constructed" is not coextensive to activities being conducted concurrently in the laboratory. Initially, the ethnographer sees duct work on the laboratory roof, a refrigerator with racks of samples, or a secretary typing at her desk (see Latour and Woolgar 1979, 93-103). These sites are not the locales for social construction until interpreted as such. "The construction of scientific facts, in particular, is a process of generating *texts* whose fate (status, value, utility, facticity) depends on their subsequent interpretation" (original emphasis, Latour and Woolgar, 1979, 273). Given the impossibility of a naïve observation, to suggest that "a" social construction is observed, then documented as such, is theoretical bad faith.

In coming to grips with the process of construction, and its implied success in science, analysts taking their cues from laboratory life studies have succeeded in replicating their own disciplinary interests. As a result, scientists have been characterized as master rhetoricians (see Gross 1996), Machiavellian networkers (see Latour 1987), government and/or corporate pawns, (see Dickson 1988), oppressors of women (see Fox Keller 1985), classic entrepreneurs (see Etzkowitz 1989) and artifact traders (see Galison 1989). Seen as (in Latour's words) "more social" than other social activities, science is always contextual – especially if that context has an outward manifestation:

> There is another sense of construction that warrants special attention within the contexts of laboratory studies. This is that construction appears to be, always, local construction. As Rouse (1987) put it, paraphrasing Heidegger, "Science must be understood as a concerned dwelling in the midst of a work-world ready-to-hand, rather than a decontextualized cognition of isolated things" (p. 108) (156).

By placing laboratory science in an ethnographic context science is shown, not surprisingly, to be constructed locally in the laboratory. Knorr-Cetina's reasoning succeeds in reproducing the naturalistic fallacy – science is shown to be socially constructed; a view which is established

by pointing to a causal relation to the social construction of science in the laboratory. 'Social constructivism' presumes the instantiation of local laboratory science. The notion of 'social construction' has no correspondence outside of the presupposition that science is socially constructed. Taken a step further, Knorr-Cetinia draws a even tighter circle: Science is the product of local construction and the referents of local constructions are always the laboratory itself. The designation of 'local construction' does not extend beyond the coordinates of time and place provided by the ethnographer. While Knorr-Centina's observation that science is always the product of local construction has the air of a meta-scientific claim, the elemental and variable epistemic aspects of the construction are not addressed. [13]

Bruce Lewenstein (1995) argues that ignorance of the "... activities and contexts in which the public gains access to scientific knowledge" (356) contributes to the ill-defined relationship between science and the media. In reviewing the literature, Lewenstein suggests that a pattern occurs in which "... particular national or cultural contexts shape public communication of science and technology" (356). For example, examining socialist uses of science, American popular culture, European history and 1960s social movements show how "particular contexts shaped presentations" of public communication of science. Research into the public communication of scientific knowledge shows, according to Brian Wynne, "... that people always encounter 'science' imbued with social interests of some kind ..." (376) As a result, the public understanding of scientific knowledge is always viewed from within a social context. Working-class women, for instance, "... reject dominant medical views of menstruation because the whole medical-scientific construction represents it as ... 'failed reproduction'" (376). Here, class status is the social context which gives, or robs, "properly designated" women the savvy to reject a thoroughly dubious proposition. That the "whole" medical community has constructed, intentionally or otherwise, the idea that "menstruation as failed reproduction" is

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[13] Knorr-Cetina acknowledges as much: " With respect to science, the finding is complicated by the fact that, within the old logic, if the particular specifications of a laboratory must be taken into account in interpreting its results, then it becomes possible to challenge the results as artifacts: Its properties are qualities of the setting rather than of the natural objects themselves ... Because it would be difficult to argue that all the local choices implicated in fact constructions are 'nothing but' insignificant variations without impact on the properties of the results obtained, a new logic is needed ... Local configurations breed the specific advantages and opportunities that, when they are structured into a scientific object, may make it more successful in the wider *context*. In this sense laboratories are like environmental niches ..." (my emphasis, 157). Moving from the metaphor of building to the metaphor of evolution does little in the way of establishing a new logic; rather Knorr-Cetina shifts the ground to a natural process while relying on the same notion of conceptual progression. Moreover, by appealing to the infinite regress of wider contexts, Knorr-Cetina may be able to show how local constructions work on one level, and then lose that utility in a wider context. However, I agree with the need for a need for a new logic – one which adjudicates the truth and efficacy of constructivist claims.

a counter-intuitive claim; that class status is the context in which the truth is necessarily known is an emblematic, though also dubious, constructivist proposition.

Examining the reception of computers in society by gender, Paul Edwards (1995) claims that people:

> ... always encounter technology from a particular context and develop their understanding from there. If they first meet a computer in a course, they are likely to be introduced to them in a theoretical model that emphasizes their abstract properties and their electronic functioning. If they meet computers in an office, they may understand them as word processors or spreadsheet calculators. In every context they will be surrounded by a sort of envelope of other people's talk, writing attitudes, images and feeling about them (281).

The neophyte computer user encounters the technology within the context of gender, within the context of school and/or office and within the contexts of various expressions regarding computers. Edwards' appeal to ever wider contexts is common – and, ultimately because of infinite regress, meaningless. Context, in Edwards sense, substitutes as a truism meant to emphasize that technology is not neutral but has meaning. To resort to an ironic truism that escapes Edwards, understanding a computer as do scientists who "... tend to think of [them] abstractly as Turing machines ..." (281) is to put a technology in context. Edwards' implication, however, is that contexts are evaluative. If one "meets" a computer in a classroom where the instructor insists on a model of autonomous, gender-indifferent technological progress, then there is a misapprehension of the meaning of context, or of the "true" context of technology – which is neither autonomous nor gender-indifferent.

The reference to context in STS is an attempt to refine "brute" social construction. To claim that science is socially constructed just like every other institution is meaningless – and false. The trick is to show which groups are doing the constructing, in what ways, and where, the construction is taking place, and on what occasions science is socially constructed. Consequently, a context is a medium of construction. Putting science in context reproduces, on the meta-level, a system of authority and defined relationships which, ideologically, social constructivism claims to oppose.

4.5 Social Epistemology and Context

Steve Fuller argues that "...what is theoretically worth preserving from the history of sociology (as well as other disciplines) are the contexts, not necessarily the concepts, of our

predecessors." Illustrating his position, Fuller appeals to the dynamics of translation in which the formal equivalent of an utterance corresponds to specific content. The dynamic equivalent of an utterance corresponds to the context "... of the original even if that means providing substantially new content." (1998, 1) [14] Fuller goes on to articulate the hermeneutic activity surrounding Plato's *Republic* in which one can prefer a translation of Plato's actual utterances to understand his arguments. Still, one can choose to examine the circumstance which gave rise to his work in the context of Athenian social and political order. While translation offers little more than mindless repetition, Fuller argues that contextualism uncovers, in this instance, the "absolute presuppositions" (a phrase credited to Robin Collingwood) governing Plato's work. Putting the *Republic* in context shows Plato as a "social agent" whose relationship to a "... specific audience and circumstances offers guidance on the terms ..." (3) for currently evaluating and extending his ideas. In this instance, the socio-historic conditions under which the *Republic* was produced act as a transcendental category; a necessary, accessible, universal set of conditions from which a plausible exegesis of the text can follow. For the practitioner of social epistemology "... normative contextualism ... defines the frame of mind within which to address pressing social and sociological problems." Embedded within a given context, then, is a map lending the proper normative orientation that leads to a correct textual interpretation. Leaving aside problems of circularity, Fuller faces questions of determinism. The thrust of Fuller's analysis is to recast the structure / agency debates[15] in sociology in terms of the social contexts in which they were developed. Fuller suggests: "Specifically, in a given social context, the forces of 'structure' (i.e.. the so-called voice of order and the past) and 'agency' (i.e. the so-called voice of change and the future) are determined less by the content of their views, which draw largely on the same cultural resources, than on the difference in their access to those resources as they compete to define a common future." (17) The point of Fuller's analysis is to show how, or if, one can deploy the past in prescribing the future direction of science. Kuhn, Fuller points out, implied that great scientists could break the historical legacies which bind "normal" scientific research. He counters that scientific change turns on access to past events as they are circumscribed within social contexts.

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[14] Steve Fuller "A Social Epistemology of the Structure-Agency Craze: From Content to Context" in *What Is Social Theory?* Alan Sica (ed.) Blackwell, 1998.

[15] Roughly speaking, the structure / agency debate in sociology turns on which category to privilege in a given sociological account. The emphasis on structure is associated with the more "objective" (Fuller's term) or material facets of social life. The emphasis on agency is associated with the more "subjective" or personal facets of social life. Fuller's objects to the trans- cultural and historical aspects of these categories – the function of social structure or agency acting as ideal types in sociological explanations.

What follows from Fuller's thesis is we have a fact of the matter regarding what conditions give rise to a specific scientific event. Context is treated as a realist category. Moreover, context is viewed as much less ambiguous than content. For example, science and scientists, at least in Kuhnian terms, remain "pathologically" unreflexive. However, social epistemologists, by unpacking the socio-historic contexts surrounding of a scientific event can determine which exigencies provide the proper context which gives rise to a scientific event. Fuller's concern is with the way one uses the past in sociology, and by extension, other disciplines, to construct a possible future:

> Whereas some social theorists may pronounce magisterially (a la Giddens) that partisans of structure and agency are like blind men groping on opposite sides of the same elephant, and others may urge more provocatively (a la Kuhn) that partisans of the two perspectives are locked in mortal combat, I would propose a standpoint along more classical lines. Specifically, in a given social context, the forces of "structure" (i.e., the so-called voice of order and the past) and agency (i.e., the so-called voice of change and the future) are determined less by the content of their view, which draw largely on the same cultural resources, then on the difference in their access to those resources as they compete to define a common future. In short, people become vehicles of change when they fail to see themselves in the most probable future because others have already appropriated what had been their common past (17).

To illustrate Fuller's distinction between content and context, one can look at ways in which legal documents are interpreted. Supreme Court Justice Antonin Scalia defends a position termed "textualism."[16] For Scalia, the theory of textualism requires courts to avoid referring to legislative history (e.g., committee reports, legislative drafts, debate transcripts) when interpreting an ambiguous statute. One must determine statutory meaning only by referring to ordinary language usage, to accepted rules for interpreting texts (e.g., close reading), and to passed legislation. To support his position, Scalia appropriates Ronald Dworkin's concept of "semantic intention" in order to get around the sticky problem of trying to determine authorial intent. Semantic intention, for Scalia, puts the emphasis on what a text would reasonably be understood to mean rather than what the author(s) intended to mean. In interpreting the Constitution, Scalia looks for the original meaning of the statute, not the framers' intent. While Scalia's recommendations run afoul of necessary forms of historical interpretation, textualism is evocative of the notion of content Fuller uses. Perhaps more profound is the notion

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[16] See Antonin Scalia *A Matter of Interpretation: Federal Courts and the Law* (Princeton, NJ: Princeton University Press, 1998). See also Robert Post "Justice for Scalia" *The New York Review of Books* Vol. 45, No. 10, (June 11, 1998), 57-62.

of legislating an activity by primarily referring to a context. One can come to know science, STS practitioners argue, by analyzing the contexts in which it participates. Fuller's argument proposes a way to use the past to set, in this instance, intellectual policy. What remains in the general discussion of context are legislating which ones are appropriate to apply to achieve a desired end. Further, and by way of illustration, the desired ends must be articulated in order to rule out reference to content as a potential context.

4.5.1 Contingentism and the Canon

Mario Biagioli (1996, 189-206) and Simon Schaffer (1996, 207-230) debate the function of, and access to, the social and historical contexts of science from a relativist perspective. Biagioli 's methodological recommendations follow a critique leveled by Fuller (1992) against Thomas Kuhn; namely, that historians ignore the affect that social processes have on their own work. Biagoloi's solution, contingentism, is yet another charge into the breech of reflexivity. Contingentism is an attempt to "locate" historians and sociologists of science and ".... mak[e] explicit the partiality of their perspective on scientific changes and practices" (195). [17] Subsequently, historians must understand, and account for, the processes by which current disciplinary standards and conventions have emerged. In response, Schaffer suggests a social history of the canonization of texts is "... required to compliment a history of knowledge production" (208). Schaffer also sees efficacy in juxtaposing cannonizations "...within our own cultures and canonizations at moments of cross-cultural contact" (211). In part, the question motivating Schaffer is: "What would happen if historian of science engaged in the enterprise of explaining how canonical texts became classics, rather than simply contextualizing those texts" (208). Schaffer ups the ante slightly in not taking a context as simply a new matrix into which historical texts (in this case) are placed. Rather, the process of canonizing texts itself is the target of investigation. Whereas Biagioli wants to "locate", and make explicit, the contexts that historians of science employ, Schaffer wants to investigate how a context (the canon) is constructed.

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[17] Much of the concern with "locating" historians' sensibilities, like reflexive practice in SSK, exhibits an trend toward the individual historian (or sociologist) as the story. Biagioli, in this instance, moves the study of science away from science and toward the motives of individual historians. The presumed importance of historians and sociologists to the development of science seems highly exaggerated and magnified in social constructivism. Absent is an explanation of how a given historical account, by a "located" historians leads to a given scientific result. Again, with Biagioloi's contingentism, we see an example of the constructivists treating all influences on science, except science itself, as undifferentiated.

Biagioli takes the defense of "present-centered" history [18] (tabbed "neo-whiggism") and sociologists "bracketing-off" of reflexivity as:

> ... possible indicators of the current socioinstitutional predicament of science studies and of its practitioners' cultural and professional identities. I believe that if, *properly contextualized*, the issue raised by the reflexivity debate in sociology and by the limits of the historians' ability to understand the past "on its own terms" do not need to deepen the anxieties of relativist historians and sociologists about the contingent status of their disciplines ... Because of its methodological sophistication, some of the recent work on scientific discoveries offers a good example of historians' routine effacing of their own frame of reference. These studies argue that the scientists historical narrative about their discoveries are rooted in and help stabilize the very closure of the debates through which those discoveries had become legitimized (and then passed into history) as such. In short those narrative are constitutive of (and constituted by) the scientific facts that are about. However, while the historians *contextualize* the scientists' own account of discoveries by relating them to the dynamics of debates and the structure of the communities or networks involved in them, the historians' narratives are not usually presented as being affected by comparable processes. (my emphasis, 189 -190).

Biagioli 's "proper contextualization" of the reflexive problem, contingentism, is an attempt to account for reciprocal affect between science and history of science: "As historians of science, we are not only located in the present but we are also tied to the current state of scientific knowledge and to our institutional predicament and culture as our ultimate frames of reference for historical interpretations. However ... today's scientific knowledge cannot be said to be the best possible in any general sense" (201). Biagioli defends the historian's pronouncement after the fact that, for example, a given scientist was right if, by that pronouncement, the historian means:

> ... So-and So's scientific tribes happened to be successful and that we happen to belong to a culture that is genealogically connected to that tribe. True, So-and-So survived also because his or her worldview was "good" needs to be understood within this genealogical framework. Had the socionatural environment been different, things might have gone differently, and we might have been writing not only histories of a different science, but also from a very different point of view (202)

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[18] As evidence of his assertion, Biagioli (1996, 189-191, see also 471-477) cites articles two decades apart ranging from Stocking (1965), Hull (1979), and Wilson and Ashplant (1988).

Biagioli 's contingentism emphasizes that science, and history of science, could have turned out other than it did. Realists, on the surface, would agree with this proposition. Implicit in Biagioli 's historical contingency, however, is not the idea that science could have turned out other than it did because scientists more accurately represented the natural world – rather that different social conditions (broadly conceived) could have determined different winners and losers. A proper context, then, excludes natural science. Further, everything regarding scientific knowledge production is contingent except the contingent nature of science. Contingentism, like contextualism, implies a normative justification: Explanation or interpretations of scientific development ought not include traditional contexts.

Much of Biagioli's argument for contingentism rests on the seeming absence of historians' awareness of their own historical presence – a charge also leveled against the arch-historian Thomas Kuhn (see Fuller 1992). Since *Structure*, according to Biagioli, presumably established both a "higher epistemological status" (191) for history and the ability to understand science historically, then the features of historical interpretation "... should have received extensive attentions by historians." (191) Central to the production and understanding of historians' interpretations, Biagioli insists, is their "location" in space and time. Reflexive knowledge of this type would be "partial", given a broader context, but would not be "arbitrary." Here, Biagioli tries to avoid the omnipotent posture found in the forms of relativism preferred by many STS practitioners; that of an interpreter who studies science as it happens, objectively, by being everywhere and understanding everyone (Biagioli, following Haraway, calls this a "god trick"). Latour's *Science in Action* (1987) serves as a case in point. Relativists fail to attend to themselves as " ... constituencies operating in allegedly legitimate and legitimizing institutions such as universities." (195) Of course, the location of social scientific research, in which a student body carries the implied task of representing the general population, is the frequent topic of meta-analysis. However, Biagioli 's point is that the omnipotent postures found in academic social science, legitimates the practices as "scientific" which is critical to continued institutional survival. Emphasizing the local production of knowledge, Biagioli argues that local practices become instantiated because they survive professionally. Successful interaction, however, define between a group and the world keep a belief system intact. A view's coherence is a measure of "contingent effectiveness" as locally defined. Thus:

> ... A scientific tribe ontologizes its worldview not simply because it does not have access to alternatives, but also because such a world view embodies (by the very fact of having survived) the result of the successful interaction

> between that tribe and the environment (both natural and social) with which it happened to interact ... although the knowledge of that tribe reflects a "success story" in the sense that it indicates that the tribe has survived a more or less long interaction with its environment, the quality of this knowledge cannot be assessed according to any general and external parameter" (198-199).

Biagioli appears to strike a logical relationship between the creation of a scientific worldview and the absence of "alternatives." Seemingly, a necessary condition for operationalizing theoretical entities into a coherent worldview is a limited access to alternatives. Again, Fuller attributes a similar argument to Kuhn. In teaching history of science to apprentice scientists, Kuhn: "... focused the student's attention on only the salient aspects of scientific episodes that were necessary for recognizing and resolving paradigmatic tension. Here we find the Orwellian historical perspective that enabled the students in Kuhn's course to acquire the kind of "understanding" of science that [James Bryant] Conant sought" (1992, 273-274) Pedagogically Kuhn's focus, Fuller claims, cashes out in a way to use history to promote a given form of research: "... one philosophical influence that is missing is the one that continues to inform didactic macrohistories ... a perspective that confers epistemic privilege on "being later" without presuming that what one now sees is better than before ... it prods the audience to convert a usable past into a viable future" (274)[19] Part of Biagioli's task is to redirect the didactic purpose of history. In short Biagioli argues that historical change leads to scientific change. Historians who tell the story of these changes and attribute causes are as much a part of the historical narrative as the science and scientists they study. To locate the force of change on historians themselves, one would have an account of scientific change. History, then, is constitutive, interpretive and explanatory on Biagioli's notion – and tightly circular.

Simon Schaffer shifts the ground somewhat in examining disciplinary canons to contextualize history. "The canon of a discipline is the corpus of exemplary texts that provide a standard of that discipline. The canon of the human sciences provides resources for any currently possible historiography of the human sciences" (207). By contextualizing canons Schaffer suggests that one can determine a field's "political agenda." A field, and, seemingly by extension, an individual can come to self-understanding by reflecting on the contingent nature of how canons are formed and how they shape present inquirer positions – which seem anything but contingent. While the canon, for Schaffer, is a material expression of practitioners' intents,

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[19] Fuller names Philip Morowski's *More Heat Than Light* (Cambridge, UK, 1989) as an example of a didactic marcohistory which documents the decline of neoclassical economic theory and offers a new theory of economic value out of that account. See page 274 footnote 118 in "Being There With Thomas Kuhn: A Parable for Postmodern Times" *History and Theory* Volume 31, No. 3, 241-275, 1992.

things could have turned out otherwise. As an example of how the canon is deployed Schaffer examines the use of, and dispute over, Emile Durkheim's work in supporting the research of David Bloor and Joseph Ben-David. Ben-David argued that the strong program misapplied Durkheim's work and it did not support sociological relativism. Schaffer quotes Ben-David that by "'becoming interpreters of particularistic, context-dependent epistemologies,' sociologists of knowledge were acting like philosophers rather than social scientists" (215-216). Ben-David's charge was that Bloor was abusing the canon and subverting the purpose of the discipline of sociology. Schaffer implies that Ben-David's charge is groundless if absent reflection on how the canon was formed in the first place. The canon is not "given" rather produced and used by a community that changes over time. However, the leap from understanding the social construction of a canon to legislating how texts are subsequently used needs to be established. That disciplinary canons possess a contingent authority is clear. Once a particular aspect of canon formation is known, however, does not, without argument, suggest the way canonical texts should be deployed. For example, Schaffer points out that Ben-David treats Durkheim much like a religious text which must be appropriately used.

The service SSK performs, Schaffer argues, is making "the tacit presuppositions of a culture become explicit. An emphasis on the disunity of scientific fields, and on the historical construction of incommensurability between these fields, indicates that much scientific work requires the management of these liminal encounters" (222-223). The trick in deconstructing canons appears to be to produce a different master narrative in which, ironically, the canon of postmodern techniques is suitably deployed. Schaffer points to the work of Edmund Said and Martin Bernal as scholars who have appropriately (i.e., reflexively) analyzed canon formation. For example: "In his attempt to displace the canonical 'Aryan model' of the origin of Greek culture with a version of the 'ancient model,' which recognized and documented the role of Egypt in ancient Greece, Bernal deliberately foregrounds his own marginality to the specialist disciplines of classical archaeology and Hellenistics" (225). Schaffer attributes the "force" of Bernal's argument "... on the detailed historical analysis of the cultural and political resources upon which the Aryan model drew ..." (225). Apparently, Bernal succeeds in reproducing, on Schaffer's account, what is a forceful historical argument by deploying accepted canonical techniques of history. Moreover, the extent to which Bernal's account is considered successful by Schaffer depends on the recognition availed by disciplinary criteria. What remains in question is how much the narrative of the canon scripts Bernal's response, and

Scahffer's subsequent assessment. That Bernal, for example, by "proper" contextualization makes an end-run around the western canon is not explained. For Schaffer to hold that a culture's canon is necessarily non-reflexive, but is successfully duplicitous in oppressing, in Bernal's case, another culture is inconsistent. The notion behind the cultural canon is that "the powerful" intentionally construct, out of a series of contingent propositions, a set of behaviors that, if followed, offer reward and, if violated, offer sanction. What inspires the critique of the canon is the mundane observation that it could have been otherwise. True, canon formation is a social process, but hardly arbitrary. As well, that canon formation assumes, and achieves, a foresee historical outcome, absent self-reflective critique is simply Kuhnian paradigm formation on a larger scale. Thus, the counter-argument to Schaffer is that the brand of STS he practices is so infused with the Kuhnian canon that all that historians succeed in doing, in contextualizing cultural and disciplinary cannons, is reproduce the Kuhnian historical narrative.

4.6 Contextual Values

'Contextual values' are the background beliefs and assumptions individual, and groups of, scientists carry which help determine what is taken to be confirming evidence for a theory or hypothesis. While varying more or less from scientist to scientist, the relevance of these values to confirmation is communally determined. During the process of determining whether an observation confirms an hypothesis, background assumptions are made more or less explicit. The scientific community sifts through these assumptions in order to reach an objective conclusion.

Helen Longino's process of analyzing contextual values appropriates much of Habermas's communicative pragmatics found in *Knowledge and Human Interests*. (see Longino 1990, 197-202 and Habermas 1971). Longino takes Habermas as proposing that "... knowledge is constituted by fundamental cognitive human interests and that different kinds of knowledge are constituted by different kinds of interest" (197). In the "empirical-analytic sciences" the ruling interest is the "technical control over objectified processes" (198). A theory is justified if it promotes the interest of technical control. Put in Longino's terms, a background assumption of the sciences is to extend "technical control"; as manifest in the origins of early modern science and currently understood and practiced. Scientific knowledge, then, is constituted, in part, by this controlling interest. However, the intersubjective interests of individual scientists also

come into play. Once scientists' interpretations of a given event are made known, then the ground is provided, on Habermas' account, for the clarification of metatheoretical problems. For Longino, "...the necessary engagement of intersubjectivity and a multivocal community of scientists in the resolution of theoretical disputes and not just metatheoretical ones" (199). At stake in this difference is the locus at which contextual values are constitutive of science. An account of the "technical control over objectified interests" in science is incomplete, for Habermas, and points to the need for a discussion of intersubjective interests. If this discussion is metatheoretical, objects Longino, it may not be seen as constitutive of scientific knowledge. Longino wants to avoid putting subjective interests off to one side thereby making interpretation a necessary condition for constituting scientific knowledge. In locating where intersubjective interest comes into play in science, Longino wants to get to a notion of truth. The relation between a consensus achieved by scientists in an ideal speech situation and what is true is unclear on Habermas's conception. What happens, for example, when consensus changes? Does the truth, say, of a theory, change as result? Does a lack of consensus suggest that there is no truth of the matter? Longino ends the list of fairly standard objections to Habermas' ideal speech situation in suggesting that will consensus within a community may be a necessary criteria for truth, it may not be sufficient.

In Chapter 3 ("Evidence and Hypothesis") of *Science as Social Knowledge*, (1990) Longino offers the following example:

> Consider someone, myself, coming to believe that an eight-year-old child has the measles, and suppose that I base this belief on the fact that her stomach is cover with red spots. What explains why I come to believe she has the measles rather than, say the moon is blue, is some belief that I have about the relationship between having a red spotted stomach and having the measles in light of which I take her red-spotted stomach to be evidence that the child has measles. Ordinarily we might suppose that the relevant belief is that a red spotted stomach is a symptom of the measles, but it is entirely possible that I should come to believe that she has measles because a crystal ball reader told me that if this child's stomach appeared covered with spots on a given day she would have a disease called measles (41).

The connection drawn between a red spotted stomach and the measles is predicated on background belief. As a result, the evidence seemingly confirms the hypothesis. However, on Longino's account, one's inference, from red-spotted stomach, to the explanation, measles, could have been otherwise given another set of background beliefs. Moreover, what counts as confirming evidence depends on a given description – in this case the color, location and manifestation of the spots.

Longino's move is to consider evidence as a state of affairs rather than as the relation among observation statements. Taking a lesson from Kuhn, Longino collapses the oft-repudiated distinction between the context of discovery and the context of justification. What counts as a confirmation, or as evidence, are the background beliefs and assumptions which determine which hypothesis we consider as being confirmed by which evidence. These criteria could be paradigm contingent. Longino sets as a foil to her view the normative positivist position that subjective factors, including social factors, ought to play no role in confirming an observation. According to Curd and Cover (1998, 242) in presenting "... her contextualist analysis of evidence, Longino is scrupulous about respecting the important distinction between two questions: 'Does this piece of evidence confirm this hypothesis?' and 'Why do scientists take this piece of evidence to confirm this hypothesis'." Chapter 2 of *Science as Social Knowledge* gives Longino's criticism of Hempel's satisfaction criterion and illustrates her belief that philosophical attempts of formal analyses of confirmation are failures.

To avoid portraying science as the random application of individual beliefs, Longino notes the critical role that social organization plays in science. "While the possibility of criticism does not totally eliminate subjective preference either from an individual's or from a community's practice of science, it does provide a means for check its influence of 'scientific knowledge'" (1990, 73). Longino argues that background beliefs may or may not be supported by data, or may be modified by personal interaction within a community. In either case, criticism is "transformative." Here, Longino wants to put in play individual and/or community assumptions as a way to negotiate between the positivist position against unobservables and the anti-empiricist position against "intersubjective reality" (e.g., individual cognition). Therefore, contextual values are both individual and collective and they change as the complex process of science unfolds. Longino's account of objectivity turns on the idea that contextual values exist, must be heard, and then transformed by critical discussion.

> ...[C]ontextulaism ... understands knowledge as the historical product of interactions between contextual factors such as social needs, values, and traditions and practice of inquiry such as observation, experiment and reasoning. This account was defended by appeal to certain features of the formative episode of modern Western science. It is contrasted with the objectivism and scientism associated with logical empiricism and with the self-ratifying internalism of Kuhn (and of Laudan). These three approaches offer distinct perspectives on the relevance of scientific ideas to social, cultural, and political principles, practices and ideals (176-177).

To the degree that Longino depends on a form of ideal speech community to achieve objectivity, her account of contextual values is normative. Further, her account depends on access not only to a shared discourse with which to air one's views, but access to one's own preferences. How well an individual scientist can account for their own background assumptions and preferences is not clear. Moreover, the appropriate forum for expressing, and responding to, criticism supports or denies individual forms of expression. Longino lobbies for reaching consensus by open, rational negotiation. That open, or rational, negotiation exists and leads to consensus, or objectivity, is an empirical question. [20]

Curd and Cover (1998, 244-245) raise the question as to whether Longino's criteria for objectivity are necessary conditions in the strict logical sense. For example, is the objectivity of a scientific enterprise necessarily compromised by the exclusion of women and minorities? Would a scientific enterprise necessarily be more objective if women and minorities were included? Longino's argument that criticism from alternative points of view are a necessary condition for scientific objectivity denies the possibility that scientific objectivity (however defined) can be reached accidentally. Although Longino's point is that intersubjective views, taken as given, can be weeded out in the process of critical discussion, she appeals to a metaphysical notion, and standard, of objectivity. Therefore, an individual or group can hold wrong contextual values, but if the process of transformative criticism works, ideally, objectivity can still be achieved. Longino moves easily from the contextual values held by an individual or community to the context of objectivity. Although objectivity appears to be achieved through the process of transformative criticism, Longino does not appear to want to make another contextual value which could, itself, be transformed through critical discussion. Curd and Cover (1998, 244-245) also bring into question whether Longino's criteria for objectivity are sufficient. To modify this criticism slightly, I would suggest that Longino is forced to consider objectivity meta-scientifically – that is, as a stable context which is not just

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[20] The "Forman Thesis" (1971, 62, 110) – physicists adopted quantum indeterminism in response to the social and cultural condition of Weimar Germany – provides a case illustrating aspects of Longino's position. Forman argues that many German physicists suddenly, after 1918, endorsed indeterminism because "... the scientific context and content ... point inescapably to the conclusion that substantive problems in atomic physics played only a secondary role in the genesis of this acausal persuasion, that the most important factor was the social-intellectual pressure exerted upon the physicists as members of the German academic community." Fuller (1988, 235) objects to Forman's conclusion on the grounds that no evidence is provided that German physicists were responding directly to popular talk. Other difficulties in asserting a cause for the German physicists change of direction are contingent on the scientist' awareness of their position, the social awareness of the larger community inhabiting Weimar Germany, and the subsequent morality, or corruption, of the decision-making process. The question as to whether the German physicists operated in an environment in which their choices were informed and resulted in the desired consequences remains unanswered. The Forman thesis stands as an example, on Longino's account, where transformative criticism is blocked yet German scientists came to adopt an indeterminist interpretation of quantum mechanics.

another contextual value which is "transformed" in the process of criticism. If objectivity is a contextual value, the Longino's definition would run counter to the idea that knowledge of phenomena can exist independently of and individual's and community's belief. If objectivity is just another contextual value, it serves as a distinction without an epistemic difference.

4.7 Conclusion

> At the risk of being boring I repeat: the truth value of a proposition, even a proposition about subjectivity, *is not affected by context*. This is the whole point of science; the whole point of the revolution in thinking that Weber set out to analyze under the heading of "rationality." (my emphasis, Fox 1996, 335)

'Context' is an essential, but epistemologically undifferentiated notion in STS. Social constructivism implies a form of epistemological holism in which scientific practices, and their outcomes, can only be interpreted or explained in relation to a complex set of social relations. Constructivists designate context-based relationships as contingent and offer empirical demonstrations, through case studies, of ways in which a scientific result could arise from any combination of conditions. At the heart of the appeal to context is the assumption that the analyst has access to the contingent circumstances affecting the agent's practice. In science, the ability for the actor to perform meta-level inquiry is bracketed by the paradigm in which they work. As I have previously argued, this condition, if true, is visited, as well, on the STS practitioners bound to the social constructivist paradigm. However, I have argued that meta-level inquiry, which is also available to scientific actors, necessitates a series of *a priori* logical, epistemological and ontological commitments which can be adjudicated on the basis of metaphysical coherence.

Positing that a context exists, one can always counter that a context is, itself, a product of social contingency. I grant this counter-argument on two conditions: 1) The constructivist position is also considered contingent and may exist to support objectionable ends (as I have demonstrated in Chapters Two and Three) and; 2) The relationships posited by a context-based analysis imply a theoretical commitment to their likely existence. For example, if an STS practitioner argues that scientific objectivity is a contingent social construction based on class interests, the assumption that class interests, in this case, more likely explains objectivity must be recognized. I have argued such a relationship is necessarily implied *a priori* as any expression requires some stability in the predicates we use. Further, that we choose class interests as an explanation, as opposed to cheese cake, entails a logic of justification. A

necessary and sufficient condition for demarcating between likely forms of contingent explanation is the existence of a series of meta-scientific relationships among things (race, class, gender for example) thought to exist. Thus, I argue that contingent propositions based on existing contexts assume a set of necessary relations. Meta-scientific claims, then, must exist in a necessary relationship among the contexts from which they are offered, the practice to which they refer, and the world to which that practice refers.