

RESTORING THE REAL: RETHINKING SOCIAL CONSTRUCTIVIST THEORIES OF SCIENCE

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As we come to recognize the conventional and artifactual status of our forms of knowing, [we realize] that it is ourselves and not reality that is responsible for what we know.
(Stevan Shapin and Simon Schaffer, 1985)

It always seems to me extreme rashness on the part of some when they want to make human abilities the measure of what nature can do.
(Galileo Galilei, *Dialogues Concerning the Two Chief World Systems*, 1632)

Introduction

One of the oldest urges in Western intellectual tradition is to think that reality and truth should coincide, that our knowledge be certified in the end by the structure of reality itself. Modern science, which best embodies this urge, aims at justified knowledge which can faithfully track the contours of the natural world. The logic of science, however, has been severely challenged by a set of doctrines which deny that what we take as scientific facts bear any necessary relation to the causal processes and theoretical entities they claim to describe. Often referred to as 'social construction of science' or 'sociology of scientific knowledge,' these doctrines claim that because science's methods, like any other way of knowing, are wholly relative to a theoretical framework and a world-view, science amounts to a construction, and not a discovery, of reality: we know what we ourselves construct and there can be no warrant that our constructs can progressively come to map the world as it really is. Understood sociologically, the practice of science becomes a matter of conventions, and truth a matter of how we 'garland consensus with authority.' If meaning-conferring power can be imagined as distributed along a triangle whose three sides are the natural world, the individual knower and cultural practices, social constructivism tends to collapse the first two into the third: facts once seen as due to the world's own determination are instead seen as projections upon a much thinner world by the cultural practices of communities of

inquirers. While constructivist approaches to science have revealed how deeply our perceptions, conceptual categories and modes of reasoning are embedded in our social life, they have exacted a heavy price: they have inflated the role of cultural practices in fixing our beliefs to such an extent that the world and the self have virtually disappeared as constraints on the content and logic of our beliefs and cultural **practices**.²

This essay offers a critical exposition of some of the most influential sociological and feminist theories that purport to see the natural world, experimental evidence, scientific facts and objectivity as social constructs.' I will call constructivist any theory of science that includes in its purview 'the very content and nature of scientific knowledge... [and] not just the circumstances surrounding its production.' While the earlier structural-functional tradition of Karl Mannheim and Robert Merton only studied the social conditioning of the agenda of science (the foci of interest and the rate of advance), the newer socio-cultural theories aim to explain the technical *content* of science in terms of social variables (class, gender and/or professional interests, among other things). Although they differ in emphasis, the various schools within the constructivist stream adhere to three tenets. First, what makes a belief true is not correspondence with an element of reality, but its adoption and authentication by the relevant community of **inquirers**.³ Thus, there is no hard and fast philosophical difference between a society's fund of knowledge and the beliefs currently held and disseminated by certified authorities. Second, science is a socially located praxis that creates the reality it describes, rather than a detached description of a pre-existing reality external to its own practice. Science not just describes 'facts', but actually constructs them through the active, culturally and socially situated choices scientists make in the **laboratory**.⁶ Third, the constructivist theories examined here admit of no analytical distinctions between knowledge and society, the cognitive dimension and the socio-cultural dimension: people's *knowledge of the world* and their *organization & life in the world* constitute each other, the two are 'co-produced.'" From the obviously true and undeniable premise that science is done in definite socially located institutions by socialized individuals, constructivist theorists tend to deny any meaningful distinction between what is inside and outside of science and between things natural and social.

We will encounter these assumptions in their strong, moderate and weak versions as we cast a critical look at the major schools of SSK. All through the exercise, I will argue for a recovery of the real through a dialectical, mutually self-correcting relationship between the real world, the knower and **her/his** cultural assumptions, each helping to determine the content and meaning of the other two. Even though I don't present a comprehensive realist theory of science in this essay, I do try to show the desirability and the possibility of a robust (though fallible and non-foundational)

contextual realism which admits the role of culture and social interests in our perceptions, but never loses sight of the discipline the real world imposes on our cultural conventions and interests. Recognizing the social nature of scientific practice does not have to lead us to give up the commonsense idea that a belief is true because the state of affairs to which it refers is in fact the case, and not because it is believed by a community of certified inquirers, as the constructivists would have it.

Political and Philosophical Arguments Against Deconstruction of Science

I decided to undertake this critique *not* because I doubt that sometimes what we accept as an immutable truth about nature is actually socially constructed. There is no question in my mind that scientists can be, and often have been, led by their unconscious biases and conscious material interests to project the existing social order onto the order of nature. Such science-certified 'findings' can, in turn, give socially created differences a gloss of unalterable destiny, sanctioned by the very structure of nature itself. It is a singular achievement of feminist and radical science critics to have drawn attention to the fact that science at times has been used to naturalize unequal and hierarchical social relations of gender, **caste/race**. I share the impulse behind these critiques of science insofar **as** they seek to turn the self-critical rationality of science on science itself. Such an exercise, if combined with a realist epistemology, can identify the blinders of conventions, interests and ideologies so that we can know the truth about the world and our place in it.⁸

It is precisely because I *value* the impulse of emancipatory self-critique that the constructivist science-critics start out with, that I *reject* the position they arrive at, namely, a complete merging and mutual constitution of the social order and the order of knowledge. Once we come to see the very content of natural sciences as not merely conditioned, but constituted by the culturally sanctioned social practices, scientific knowledge becomes a matter of prevailing and ever-changing conventions, with no necessary relation with the natural order and no *critical* relation with the social order. This conflation between the real and our accounts of it jeopardizes the entire project of a progressive critique of ideology, for **as** Frank Farrell puts it:

When reality itself has become a manufactured image...it can no longer make sense to measure our beliefs against how matters really stand. When selves are understood to be cultural artifacts, then the notion of self discovery or self emancipation is a delusion. If rational practices must occur within a nexus of power and taken-for-granted biases, then the goal of coming to have a more objective account of reality and of ethical relations is a foolish one.⁹

Social constructivist doctrines have moved from the critical function of **truth**, to a critique of the very possibility of true knowledge, or as Michele

Barrett puts it, following Foucault, from an analysis of 'the economics of untruth [ideology] to a politics of truth' **itself**.¹⁰ Starting with a suspicion that social interests can sometimes occlude truth, they have concluded that social interests always-already constitute it, and that knowledge whose validity can transcend our local context and interests is in principle impossible. Starting from the materialist insight that science is a socially mediated practice, constructivist theories have come to see logic, evidence, truth and even external reality itself as social, having no content independent of their sociality.

I am especially concerned with the exhaustion of ideology-critique in contemporary feminist and postcolonial writings on science and society. These critics have turned their backs on the earlier science-for-the-people initiatives that used the findings of modern science to critique the false and inadequate knowledge that legitimizes the existing social hierarchies. They have instead turned their critical tools on modern science itself in an attempt to expose science's own unacknowledged social values and ideologies. Having found Western, patriarchal and capitalist assumptions going 'all the way down' into the very logic of modern science, the only alternative these critics can consistently support is that of a multicultural collage of 'ethnosciences' or 'situated knowledges' justified by the world-views and interests of women (for feminists), the non-West (for post-colonials) or non-Western women (for post-colonial and multicultural feminists). But given that neither 'women,' the 'non-West' nor 'non-Western women' make up uniform categories, situated knowledges end up privileging the most hackneyed stereotypes of feminine ways of knowing and the 'wisdom' of **non-Western** traditions over scientific methods of inquiry. What gets lost in this discursive affirmative action is a critical appraisal of these parochial, localized perspectives, many of which are deeply implicated in legitimizing age-old oppressions."

As a one-time biologist and a feminist from a **non-Western** country, I find the neo-traditionalism condoned, tolerated and, indeed, often celebrated by feminist and postcolonial science critics extremely troubling. It may be appropriate at this point to disclose my personal investment in a defence of scientific rationality. I learned to do science as a young woman in India, received a doctorate in molecular biology and later worked as a science writer in close collaboration with science for people movements in India. I found in science the intellectual resources for rationally questioning – and rejecting – many of the Hindu assumptions regarding caste and gender hierarchies. Indeed, it would be no exaggeration to say that training in modern science marked the beginning of humanism and feminism for me. I cannot consent to the radical deconstructionist theories of science for the simple reason that they completely misdescribe the science I did in the lab, and the science I did on the streets as an activist

and a writer in New Delhi, circa mid '70s to the mid '80s. Most progressive intellectuals in the West at the close of the twentieth century have come to see scientific rationality as the 'mantle of those in power, those with **authority**.'¹² But coming from where I come from, I can see the missing half of the dialectic: scientific rationality *also* contains the resources to challenge those in power, those with authority.

The sociological theories of science which see natural science as purely local and context-specific practice contradict the very rationale that made it possible for me to learn modern science while growing up in a small and rather provincial city in **Punjab**. The entire idea of adopting modern scientific education in **non-Western** countries is premised on a belief in the universality or trans-contextuality of scientific **knowledge**.¹³ Social constructivist theories embrace a deep and radical relativism which undercuts all epistemological grounds for transcultural appropriation of the methods, theories and worldview of modern science. These theories reduce the obvious spread of modern science beyond the Western world to an epiphenomenon of the West's imperialism – a sign of the West's cultural hegemony which must be *resisted* in the name of national liberation, cultural survival and the recovery of the civilizational projects interrupted by colonialism. Framing science **as** Western cultural imposition on **non-Western** others undercuts the very rationale of progressive people's science movements in Third World whose primary commitments are not to the nation, but to universal human values of justice and equity; not to cultural survival, but to cultural change that promotes these values; not to a resumption of inherited civilizational projects, but to new futures. These movements are committed, in other words, to an internal critique of local knowledges in the light of the best available and the most humanly accountable values, regardless of the place of **origin**.¹⁴ By treating the best confirmed scientific theories simply **as** local constructs of the West, *no different in their basic logic* than the local cultural constructs of the rest (*i.e.*, pre-scientific folk knowledges), constructivists undercut epistemological grounds for the critique of the latter from the vantage point of the former.

Let me hasten to clarify that by wishing to defend the universality of scientific rationality, I am by no means suggesting that science is insulated from its social context. While science has specialized, knowledge-seeking aims which *differentiate* it from the rest of the society, these goals are met through institutions and practices that are not *separate* from the apparatus of social power, material production and cultural belief. I am not denying that as a hegemonic, state- and corporate-sponsored activity, science is shaped by a society's dominant interests: wrenching scientific knowledge away from hegemonic institutions and making it available to those standing outside the gates was my reason for preferring journalism and activism over doing science. Not for a moment am I denying that scientists

carry their gender, **race/caste** and class biases with them into the lab – as one among a handful of women in a biotechnology lab in an elite technology institute, I am only too painfully aware of the cultural continuity between the lab and the world outside.

But – and this is crucial – I am equally strongly aware of how *fundamentally contra-conventional the content of science can be*. Scientific *practice* is culture- and context-bound, but the content of science is not always so. The prevailing paradigm leads a scientific community to selectively pick out a certain (and not any other) natural **object/phenomenon** from the entirety of the natural order. It is also true that scientists approach this **object/phenomenon** with time- and context-dependent questions, theory-laden methods and rules of evidence. *But the results of scientific inquiry quite often confound the conventions from which we begin. As any self-reflective working scientist can attest, often the methodological conventions, the theoretical postulates and the larger goals of the inquiry are re-evaluated in the light of what we learn about nature starting from our local conventions, goals, and values. Science is simply not so circular and question-begging an affair as social constructivist theories make it out to be, where social conventions determine what we can see and accept as true. Scientific knowledge depends upon social institutions and cultural conventions for its *existence* but not for its *truth*. The truth of a belief, as we shall see, is not a matter of internal relations or coherence within a framework of beliefs, but a matter of the relationship of the belief to something else prior to and independent of the framework. And we can arrive at successively approximate descriptions of this relationship through a constant revision and modification of our conceptual categories and theories in the light of newly discovered features of the world.*

Indeed, it is precisely because the findings of science are a constant threat to the spontaneous consciousness of everyday life sanctified by the authority of culture, that doing and teaching science had a subversive quality in my social milieu. It is the contra-conventional character of science that made it an ally of those of us engaged in an internal critique of some of the inequalitarian elements of our culture. The findings of modern 'Western' science enabled us to show – with empirical evidence that was publicly testable – that *no matter what the consensus of the local community is, no matter what the powers-that-be claim, some social values and some facts of nature that these values are informed by, are wrong and must be rejected as false.*¹⁵ Our project of denaturalizing socially-created, religiously-sanctified inequities (especially of gender and caste) was not different in spirit than that of feminist and other progressive critics of science in the West. But where they see science as an agent of naturalization of social differences, we sought in modern science evidence that the facts of nature were not what they were assumed to be by our community,

and that a different social order, in tune with a different understanding of the natural order, was **possible**.²⁴

Some would – as indeed, many **postmodern/postcolonial** critics of modern science, both in India and the West already do – doubtless think of our attempt to challenge the traditional order from the vantage point of 'Western' science as an act of treason against our natal civilization, and consider us 'internal colonizers' bringing the diverse local narratives under the sway of a eurocentric metanarrative. But as long as we could argue that the **content** of modern science was not 'Western' or Eurocentric in any substantive way, and that it gave us a picture of the natural world that was as true for us in India as it was for the bearer of any culture anywhere on the planet earth, we could defend ourselves against the charges of imperialism. But now, the social constructivist theories that claim that the particular content of scientific knowledge cannot transcend the context of its production have pulled the rug from under our feet. Those of us who believed in science for social revolution are left with no principled defence against the shrill accusations of our cultural nationalists and our fundamentalists, the sophisticated among whom silence us by citing the authority of (the much misunderstood) Kuhn and (only too well understood) Foucault, Rorty and Latour.

So much for the political grounds of my dissatisfaction with social constructivism. My **philosophical** disagreements with it centre on the fact that it denies a **rational self-correction either of scientific knowledge in the laboratory or in the values of the larger culture in the light of evidence from the natural order**. Social constructivism holds that the evidence from nature is never free from contextual values and thus cannot override or contradict the scientists' enculturation. What gets demarcated as 'science' from the other everyday cultural meanings and practices has no necessary or essential features (falsifiability and/or institutional norms that allow for replicability and critique of others' findings) that increase its probability of being a **true(er)** explanation of a phenomenon. The ever-changing ('contingent') boundary between science and non-science, sociologists tell us, is a consequence of rhetorical games through which socially powerful groups draw cultural maps for the rest of us to live by, charting some knowledge that serves their interests in the cultural space marked as 'science.' Because science's claims of approximation to the actual causes and underlying mechanisms of manifest phenomena through an open and dispassionate attempt to falsify all evidence take place through culturally embedded practices, there can not be a progressive march toward truth. Science, like any other cultural practice, is a struggle over institutional accreditation of what is meaningful and true, a struggle that simultaneously stabilizes knowledge and social power.

In this essay I will be concerned with two consequences – namely, **anti-**

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realism and relativism – of the constructivists' attempt to explain scientific rationality *ultimately* in terms of cultural meanings and social power. Anti-realism in constructivist theories does not deny the existence of the real world, but only denies that what we accept as scientific facts necessarily correspond to it. Epistemological relativism claims that, as Martin Hollis and Steven Lukes put it, that 'what counts as good reasons for holding a belief depends upon the context,' or as Karl Popper put it, 'truth is relative to our intellectual background or framework and it may change from one framework to another.'¹⁷ Relativism and anti-realism are intimately related. Larry Laudan, one of the staunchest opponents of relativism, indeed defines relativism as the flip side of the constructivist creed that 'the natural world and such evidence that we have about the world do little or nothing to constrain our beliefs.'¹⁸ When our beliefs are 'liberated' from the constraints of reality, or when the external reality is seen as malleable enough to be moulded into any shape dictated by our conceptual schemes, what is real and true for one social group ceases to be so for another.

When reality is 'thinned out' and not allowed to constrain our beliefs, social relations end up carrying the entire weight of justification of our beliefs. Obviously, no ensemble of social relations can be deemed irrational – for the very fact that they allow societies to sustain themselves confers upon them a 'natural rationality.'¹⁹ It follows that all beliefs are as rational and true as the social relations that undergird them. The problem of rationality for sociologists and anthropologists of science is not sorting out mere beliefs from beliefs that are justified to hold by virtue of the traditional epistemic virtues of truth, rationality, success or *progressiveness*.²⁰ Sociologists of science believe that all these virtues are honorifics that a community confers on some beliefs *after* they have been accepted by a community of scientists and the rest of the society through a process that can be fully explained in terms of sociological and cultural variables (e.g. class and gender interests). *Constructivist theories of science, in other words, prohibit us from using correspondence truth as a regulative ideal of our inquiries into the nature of knowledge in different societies and different historical contexts.* All one can justifiably do is to study the empirical conditions under which different communities of inquirers accredit (or de-accredit) their respective beliefs. Without any evaluation of beliefs' correspondence with any given *object/phenomenon*, we are forced into a relativist position.

I will contend that anti-realism and relativism are two sides of one basic philosophical fallacy which, following Roy Bhaskar, I call the 'epistemic fallacy.'²¹ or, following Philip Kitcher, IRA or 'Inaccessibility of Reality Argument.'²² The epistemic fallacy consists of assuming that our socially derived conventions have ontological consequences, or that *how we know determines (or at least, crucially shapes, or delimits) what exists.* The

widespread tendency to derive conclusions about reality from our representations of it follows from the basic philosophical assumption that underlies all social constructivist and postmodern thought, namely, that all the reality we can ever really get at is the reality that is internal to our system of representation. Our representations thus constitute reality for us and what falls outside of our representations is relegated to the Kantian noumena, things-in-themselves, which cannot be known.

Moreover, because IRA is supposed to apply equally to social reality as well as natural reality, constructivists believe that there is no philosophical difference between natural and social sciences, and that both are equally **interpretive**.²³ One logical conclusion of this equivalence is that, as John Searle correctly surmises, for constructivists, natural reality (for example, a mountain) is socially constructed in a way that, say, money is socially constructed." In both cases, it is our representations that confer the particular status to a physical entity: our representations make the object real for us.

There are various versions of the epistemic fallacy and IRA, but all lead to the same result: thinning out and disempowering reality in relation to socially-situated knowers. Very many features of the world once seen as a result of the world's own determination are seen as projections upon a much thinner world by the powers of the subject. This thinning out and contraction of the world is recommended by constructivists as liberatory, for they seem to believe that it gives human subjects more power to change what they take to be reality, by changing their conceptual schemes and discursive practices. The political dead-ends that such prioritization of discourse has led the academic left into are by now well **understood**.²⁴

As a scientist, I am also concerned with how the thinning and **disempowering** of reality urged by social constructionists completely belies the basic assumptions of natural scientists. A realist ontology, as Roy Bhaskar argues, is presupposed by the social activity of **science**.²⁵ A belief in the existence of a law-abiding and comprehensible world, made up of material things and structures forms a kind of background which gives meaning to science. Any activity properly deemed science seeks to get nearer to the truth of the underlying causes and structures of the manifest phenomena and thus provide an account of what's going on behind the phenomena that we experience.

Most working scientists would agree with Gross and Levitt, a **biologist-mathematician** duo who have recently taken issues with constructivism, that 'science is, above all, a reality driven **enterprise**.'²⁷ As the outpouring of working scientists' critique of constructivism that followed the recent hoax by the physicist Alan Sokal indicates, most agree with Sokal's statement that 'there is a real world; its properties are not merely social constructions; facts and evidence do **matter**.'²⁸ Indeed, to tell a physicist

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that laws of nature are not explanations of natural phenomena but projections on nature of our social concepts is like 'telling a tiger stalking prey that all flesh is grass,' as Steven **Weinberg** put it.²⁹ It is scandalous how feminist and sociological critics of science have never paused to wonder why most working scientists fail to recognize the science they do in the picture of science that emerges in constructivist accounts. I am not suggesting that all science critique *must* obtain a seal of approval from working scientists – there may well be aspects of their work (especially science's social history) that scientists may not be aware of, or may not immediately recognize. But at the same time, surely something is amiss in a critique that assumes the actors to be so deluded as to consistently confuse their own constructions for facts of **nature**.³⁰

The realism that I'll defend aspires to capture the robust realism of most natural scientists. I will argue that scientists find out things about the world which are independent of human cognition; they advance true statements, use concepts that conform to natural divisions and develop schemata that capture objective dependencies. One of the most rigorously argued defences of such a realism has been recently provided by Philip **Kitcher** whose definition of science I will follow: science aims to 'produce structured accounts of causal structures of the world, by delineating the pre-existing natural kinds and uncovering the mechanisms that underlie causal dependencies.'" Such realism rejects 'deflationary realism,' a recent favourite in science studies circles, which grants the existence of entities described by science, but does not accept that successful scientific theories are progressively truer accounts of these **entities**.³² The great virtue of the realist philosophers whose work I will use is that their account of realism does not require an appeal to some ideal, a priori notion of rationality involving either a semantic relationship between our words and the world, or a god-like transcendence of the social context. The realization that we can only access the world through our cultural and social categories by no means vitiates knowing the world in a manner that can transcend our cultural and social categories.

The Uncritical Naturalism of Social Constructivism: The Trap of Epistemic Fallacy

Paradoxically, the seeds of social constructivism were contained in the demise of logical positivism. Once the distinctions between pure observation and theory, and between the context of discovery and the context of justification became impossible to maintain – as it was after the critiques of positivism by realists like Karl Popper and historicists like Thomas Kuhn – it became impossible to maintain that we can know the world as it is and mirror it in our theories. Conventionalism can be understood as one

answer to the scepticism that resulted: we know, constructionists say, what we ourselves construct. We make 'facts' and project them onto nature, for nature itself is too generous and can live with any number of contradictory explanations. It cannot be any other way, for there is no pristine, **theory-free** data to judge our theories against. Knowledge is inescapably a construction and constructivist science scholars claimed to be 'only' working out the consequences of this human condition. It is for this reason constructivists take great umbrage at their critics who read them as attacking science by reducing its rationality to social relations. In this section, I will argue that while **constructivist** theories have forced scientists and philosophers of science to rethink their mirror-of-nature descriptions of science and experimental evidence, they have exacted an unacceptable price by denying any normative distinction between knowledge and belief.

There were undoubtedly good historical reason – the Nazis' denouncement of 'Jewish science' for one – that led the logical positivists to try to keep the social-psychological genesis of ideas separate from questions of their **validity**.³¹ But for the next generation of science scholars, whose formative experience was the Vietnam War and the civil rights struggles, the separation between context and content of science lost emotional and intellectual appeal. They came to see science as deeply implicated in **inegalitarian** social theories that legitimated the exclusion of women and racial minorities and supported an imperialistic military industrial complex. This generation experienced the separation of social context from the logic of science as an ideological justification of what Jeffrey Alexander calls 'absent reason' – that is, reason located outside the concrete, everyday life of embodied human beings as they go about making sense of their lives. Because human motives, aspirations and biases were not admitted into the logic and methods of science, the objective knowledge that positivists celebrated was experienced by their heirs as alienating.

Alexander suggests that post-positivist social theory can be understood as a search for 'positive reason' which views reason from **within** the totality of lived life with all its sensuality, its conflicts and its here-and-now goals. In philosophy of science, this search for positive reason started with Thomas Kuhn's *The Structure of Scientific Revolutions*. The individualistic perspective of a knower dramatized by Descartes as a solitary being contemplating the indubitability of his consciousness could not be sustained in view of Kuhn's historical analysis of how scientists absorb ideas from their predecessors and are epistemically dependent on them. After Kuhn, scholars of science became more concerned with the actual processes of science, and it came to be widely accepted that, as Hilary Kornblith put it, 'questions about how we **actually** arrive at our beliefs are relevant to how we ought to arrive at our **beliefs**.'³² By offering to study the

everyday work of scientists, sociologists and anthropologists of science claim to hold the key to questions about rationality which were earlier considered a matter of **philosophical** speculation alone.

Post-Kuhnian science studies, in other words, naturalize reason: scientific reason is seen not as some special and privileged style of thought that must be protected from all other aspects of life in society, but as inseparable from the general spirit of the times that informs the ensemble of social institutions and interests. Naturalists treat science as a part of our interactions with nature and with our peers – a process which can be understood without any *a priori* assumptions regarding the certainty of our consciousness or our *sense*.³⁶ While the naturalistic turn has had a salutary effect of emphasizing the materiality and historicity of scientific rationality, it has opened the door to radical historicism as well. As Thomas Nagel suggests, there have been two responses to the historicization of scientific rationality, 'to recognize the limitations that inevitably come from occupying a particular position in the history of a culture.. or to convert these into **non-limitations** by embracing a historicism which says there is no truth except what is internal to a particular historical **stand-point**.'³⁷

The realist naturalism of Roy Bhaskar, Philip Kitcher, Ronald Giere, Dudley Shapere and Richard Boyd that will be defended in this essay represents the first response, which admits the contextuality of knowledge but tries to show that is not an insurmountable problem in getting to the truth about a context-independent reality. The second alternative is followed by the social constructivist critics who play the sceptic. The crucial difference between the two approaches consists in this. A realist can accept the social nature of science without, at the same time, denying that science is a special kind of an institution with a special function: namely, to discover truth about the natural **world**.³⁸ A realist sociologist then goes on to appraise the social relations and norms that operate in the institutions of learning in terms of whether or not they generate reliable knowledge most of the time. Constructivists, on the other hand, are **anti-essentialist** about truth, reality and aims of science. They believe that different groups of people at different times and places will light up the world differently, see different aspects of the mind-independent world as real, and come to define what is **true** about their particular view of reality differently. On this view, there is no one *a priori* criterion of truth that serves as the goal of all inquiry, but rather knowledge is simply whatever emerges when 'men operate in the interest of **prediction and control**, shaped and particularized by their **situation**.'³⁹

So far, a realist sociologist may agree and demarcate science in terms of the norms that regulate scientific institutions instead of universal and unchanging philosophical first principles. That, indeed, was the approach

of Robert Merton who demarcated science from non-science in terms of institutionalized norms of *communism, universalism, disinterestedness* and *organized scepticism* that help to detect and correct the errors introduced by ideologies, interests and political interest.⁴¹ But the post-Mertonian constructivists are anti-essentialist about these (or any other) norms, and hold that institutional practices and norms of science are themselves open to varying interpretations contingent on the distribution of power (e.g., gender, class and nationality) that prevails in the rest of the society. Thus, they see the social activity of science as no different from the play of power in other institutions of society. For the constructivists, the social can provide no more of a ground for truth than the philosophical, for the social itself is always in the process of being produced simultaneously with knowledge; or, as Shapin and Schaffer put it, 'the solution to the problem of knowledge is embedded within practical solutions to the problem of social order.'⁴² The entire project of demarcating science from non-science is superseded in favour of showing continuities between the two. The boundary is not between truth and falsehood but between what socially powerful groups provisionally (depending on the historical context) decide to label as truth, or as Foucault put it, 'the problem is not in drawing the line between that in a discourse which falls under the category of *scientificity* or truth and that which comes under some other category... but in seeing historically how effects of truth are produced within discourses that are themselves neither true nor false.'⁴³

The 'Strong Programme' (SP) of the new sociology of scientific knowledge (SSK) inaugurated by David Bloor at the University of Edinburgh in 1976 aspired to extend the scope of sociology to the very 'content and nature of scientific knowledge,' as the opening sentence of his influential *Knowledge and Social Imagery* declared. Bloor accused his predecessors of a 'lack of nerve and will' for treating 'science as a special case' and assuming scientific rationality, validity and objectivity to be 'absolute and transcendent.' Instead, Bloor called for treating *all* knowledge, regardless of its validity or objectivity, 'purely as a natural phenomenon,' *amenable to the same type of explanation*. In other words, if sociological factors were evoked to explain those beliefs which were not rational, then the SP required that social factors be used to explain rational beliefs as well. Any prior distinction between rational and non-rational was declared to be an unscientific value judgment on the part of the sociologist. The strong programme's hyper-rationalist emphasis on value neutrality and even-handedness was not unlike, in Roy Bhaskar's words, 'an undercover agent who works on both sides of the fence... playing the game of reason to undermine the authority of reason.'⁴⁴

The SP set the ground for all other social and cultural critiques of science. Even as they try to distance themselves from the relativism of the

SP, all important feminist critiques of science, including the works of Evelyn Fox-Keller, Sandra Harding and Helen Longino, accept the basic idea behind the SP that the very content of science requires a social explanation. The so-called postcolonial critics make the same move and, often without directly citing the constructivist works, simply assume that science is a Western and imperialist way of knowing the world and for that reason, lacks any universal purchase. Indeed, it is only after they accepted the assumption that the identity of the knower makes a difference not just to the questions asked but to the answers given as well, that the radical science critics could justify the demand for 'different sciences for different people,' which has gradually replaced the earlier radical agenda of 'science for all the people.'

It is only their prior commitment to the defence of 'difference,' that can explain how self-identified radicals can continue to overlook the normative anemia of constructivist views of knowledge. Theories inspired by the SP refuse to distinguish between justified beliefs (*episteme*) and mere beliefs (*doxa*) and treat all knowledge simply as 'what people take to be knowledge... without regard to whether the beliefs are true or false.'" The reasons that, say, a tribe believes in a shaman to cure a certain illness are equally rational for the tribe, as the reasons why someone in a different society may believe in, say, a neurosurgeon. It isn't as if better or worse correspondence with nature makes one belief more rational than the other: believers in both cases actually see correspondence with *what they take to be* nature. There is simply no tribunal higher than the culture and custom of the members of a particular group in deciding what is rational to believe in, or as Barry Barnes describes his 'tolerant' theory of rationality:

Different bodies of natural knowledge carry conviction in much the same way... All of them alike are made credible to reasonable human beings by contingent aspects of their context. It is not that some are sustained by reasons and other by causes, or that some are accepted because they correspond to reality and others despite their lack of such correspondence. Rather it is that every body of accepted belief carries conviction as the established account of reality employed by a culture or community . . . Those beliefs that count as knowledge are those sustained by custom. (emphasis added)

This seemingly egalitarian and tolerant view of knowledge is, however, hopelessly inadequate for critique of inherited knowledge 'sustained by custom,' for it is incapable of drawing a distinction between what is current in the society and what is genuine knowledge. It leaves no room for the possibility that a community may find some beliefs credible even though there is not sufficient warrant for them, or in other words, some beliefs of a community could be false and irrationally held. It is this inability to demarcate warranted beliefs from accepted beliefs that makes constructivist theories normatively anemic.

The normative anemia of social constructivist theories is not unrelated

to their anti-realism: the two spring together from the epistemic fallacy that afflicts all varieties of constructivism to various degrees. Epistemic fallacy, recall, is the chief mechanism through which meaning-conferring powers move from the world to our discourses: the properties of the world are supposedly delimited from an unstructured substrate by the knowers' culturally given categories. The real difference between constructivists and realists is not that the former affirm and the latter deny the presence of these culturally sanctioned conventions in the practice of science. The difference is that the constructivists affirm, while the realists deny, that 'in a relevant sense, social conventions in science determine the causal structures of the phenomena scientists study.'⁴⁶ In other words, while the realists admit the conventional nature of scientific knowledge, they hold these conventions to be 'ontologically innocent,' while the constructivists see them as 'world constituting.' Thus, for the constructivists, there is no contradiction between saying that the real world constrains our knowledge and still holding that the knowledge is constitutively social, for the socially constructed reality is as real as any real thing can ever be. We cannot ever know the world as it is, and the only world we can ever know is the world we grasp through our conventions. The tolerant views of rationality espoused by constructivists crucially depend upon giving our discourses the meaning conferring power, for otherwise one would have to admit that the structure of reality itself decides which account is more rational to hold and that some beliefs are false and irrational. Thus the collapse of what it is into how we know it is of fundamental importance to all varieties of constructivism.

Such a collapse has disastrous consequences for the concept of truth. All varieties of constructivism urge science to divorce truth from the world as it exists, and marry it to what we may believe about the world.⁴⁷ Such a scenario where truth and reality are made internal to the social context will leave both science and society impoverished, and the worst victims will be precisely those who the constructivists want to stand up for: the dominated groups, people on the margins, especially those in the Third World, who need the findings of modern science to question some of the inegalitarian ideas of their own cultures. Truth, understood more traditionally as a degree of fit between *what is said* and *what is* requires the most strenuous defence by all those interested in justice.

The Increasingly Radical 'Symmetry': From the Strong Programme to Actor Networks

'Since the mid-1970s, each new variant of SSK has tended to be a little more radical than the one before. Each new variant has stood longer on the relativist road,' according to Collins and Yearley.⁴⁸ What has made the

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successive variants of SSK ever more 'radical' is their growing degree of 'symmetry.'

Symmetry is a key tenet of David Bloor's 'strong programme' (SP) for sociology of scientific knowledge. It requires sociologists to treat correct and false beliefs symmetrically as caused by sociological factors. While the original idea behind symmetry (as espoused by the Edinburgh school scholars including **Bloor**, Barnes, Collins and Shapin) made truth and falsity equally amenable to social explanation, the more recent idea of symmetry (the Paris School, associated mainly with Bruno **Latour** and Michel **Callon**) makes nature and society symmetrical, that is, if one wants to explain beliefs about nature in terms of social conventions, they also have to explain social conventions in terms of beliefs about nature. Neither the social nor the natural can be taken to be given a priori.

The work of the Edinburgh School is premised on a basic **anti-essentialist** assumption that nothing necessary or essential distinguishes science from any other social activity – 'science *is* social relations,' as a well-known slogan in SSK goes.²¹ Thus the beliefs scientists come to hold must be explained in the same terms as are used to explain the consensus between social actors in any other social institution. The first, the **causality** tenet of the strong programme demands that sociologists treat knowledge 'purely as a natural phenomenon' and 'study the conditions which bring it about' with the methods of natural sciences, which are held up as an exemplar of value-neutral **inquiry**.²² One consequence of mimicking a scientific value-neutrality is that the SP admonishes sociologists to remain **impartial** (the second tenet) between true or false, rational or irrational, successful or unsuccessful knowledge, and to seek **symmetrical** (third tenet) explanations for true and false beliefs: for to seek a different explanation for true belief would amount to allowing one's value judgment to affect one's **analysis**.²³ The sociologists' task is to dispassionately examine how different social interests and cultural conventions determine where the boundary between true and false knowledge gets drawn in different societies and in different historical periods. Finally, because the SP seeks to scientize the sociological study of science, sociologists are obliged to **reflexively** (the fourth tenet) apply the tenets of the SP to their own activities as well.

The symmetry tenet of the SP is the most controversial. It states that sociology of scientific knowledge 'would be symmetrical in its style of explanation. The same type of cause would explain, say, true and false beliefs.' That is: whatever causal mechanism we find useful for explaining in naturalist terms how someone came to have a belief, we should invoke it regardless of whether we think the belief in question is true, false, rational or irrational. The idea that the truth, rationality or pragmatic success of an idea is irrelevant to why one should hold a belief is expressed

more succinctly by Barry Barnes:

What matters is that we recognize the sociological equivalence of different knowledge claims. We will doubtless continue to evaluate beliefs differentially ourselves, but such evaluations must be recognized as having no relevance to the task of sociological explanation; as a methodological principle, we must not allow our evaluation of beliefs to determine what form of sociological accounts we put forward to explain them.⁵²

The symmetry principle is meant to challenge the immunity the traditional philosophers of science (the 'teleologists,' as Bloor calls them) and pioneers of sociology of knowledge (Marx to some extent, but especially Mannheim and Merton) had granted to natural sciences. According to these theorists, the findings of natural science were to be explained by rationality – that is, reasons based on evidence and logic which 'glowed by their own light,' and needed no further explanation. Bloor castigates this asymmetry in favour of science as akin to treating science as sacred, and the rest of the social life of politics and power as **profane**. By demanding that truth or rationality be irrelevant to how we explain belief, Bloor is denying the legitimacy of this distinction and claiming that science be dragged down into the dirt, so to speak, with all the rest of social life. And since it is patently the case that not all aspects of social life and beliefs (myth for instance) can be explained in terms of reasons, but require **social-cultural** causes, the symmetry principle requires that scientific beliefs be explained in terms of social-cultural causes as well – to do otherwise will amount to violating the scientific neutrality that the symmetry principle wishes to bring to SSK. The corollary is that **reasons** that a scientist might cite for arriving at a belief are not to be accepted as the final explanation, but a **sociologist** must provide a **social-cultural cause** for a scientist accepting any given evidence as reasons for his or her theories. Reasons are mental states and not material causes, and sociologists like all good scientists, must strive to reduce the former to the latter without regard to the truth or falsity of a belief. This, in short, is the mandate of the SP.

Apart from initiating a flurry of historical studies trying to show causal connections (or at least congruence) between scientific theories and social **interests**,⁵³ the symmetry tenet **has been** embraced by feminist and other radical epistemologists who champion science from the standpoint of women, racial minorities and non-western cultures. There are two reasons for the attraction the symmetry thesis holds for all 'liberatory epistemologists.'⁵⁴ First, the demand that all beliefs – good or bad, true or false – be explained sociologically gives these critics an opening to argue that social biases are not eliminable by following the norms of good science, but instead structure all knowledge including, in Sandra **Harding's** words, 'the very best beliefs any culture has arrived at or could in principle **discover**.'⁵⁵ Thus, Harding argues that if class, race and gender are called upon to explain the social beliefs of 'health profiteers, the Ku **Klux** Klan or rapists'

then it is safe to assume that race, class and gender have 'probably shaped the 'empirically supported,' 'confirmed by evidence' results of our fine research projects as well.'⁵⁸ Secondly, it is only when social values are seen not as just conditioning the context of discovery, but structuring the cognitive norms for deciding what constitutes appropriate experiment, data and evidence etc., can it be claimed that science done by different social groups (women, working classes, non-western people) will 'look different' (Sandra Harding's phrase) in its very content.⁵⁹

It is only on accepting these assumptions which were first formalized by the SP that the feminist and postcolonial science critics could justify their call for jettisoning the traditional view of objectivity as value-neutrality in favour of doing science explicitly and self-consciously as feminists and/or non-Western scientists: because our social context causes our all beliefs, including our best validated scientific beliefs, we should choose the 'correct' social arrangements in order to maximize the truth of our beliefs. Once knowledge is seen as an effect of social causes, social location ceases to be an unwanted source of bias but becomes a 'resource' for more (or 'stronger') objective knowledge. The problem with these radical epistemologies is not their feminism and/or anti-imperialism. The problem instead lies in the assumption that science will 'look different' and possess a 'stronger objectivity' when done from these political perspectives. I will examine the calls for socialization of objectivity in a later section; at this point, I am more interested in showing that disclaimers notwithstanding? science critics who accept the symmetrical explanation of truth and false claims alike, cannot escape the rather strong epistemological relativism that follows from it.

It is true that unlike the classic relativism of truth, the relativistic programme of the SP does not assume that all knowledge claims are equally true or equally false against their particular contextual web of beliefs and standards. Instead, the SP posits an equivalence or symmetry not between the *veracity* of all beliefs, but 'only' with respect to the *'causes of their credibility.'*⁶⁰ Yet in the end, the SP supports a judgmental relativism no less earnest – a relativism that claims that there is no way to distinguish between beliefs held for good reasons of evidence and logic and any other beliefs held, say, for reasons of custom and habit.

In their attempt to naturalize knowledge and to study it with the methodology of science, the proponents of the SP eschew all ready-made, philosophical criteria of truth. In the interest of scientific (i.e., value-free) study of science, they even claim to set aside their own evaluation of truth or falsity of the belief in question. Thus, Bloor treats the symmetry tenet more like a methodological injunction: 'all beliefs are to be explained in the same general way regardless of how they are evaluated.'⁶⁰ The next step toward epistemological relativism is taken when the strong programmers

deflate the conception of truth by redefining it *instrumentally* as a 'conventional instrument for coping with and adapting to our **environment**'⁶¹; and *epistemologically* as depending not upon a statement's 'correspondence with reality but the correspondence of the theory with itself ... that is, for interpreting experience for internal consistency with the **theory**.'⁶² A very similar idea was expressed by Barry Barnes' defence of natural rationality which is to be evaluated not in terms of how it relates to reality but to the 'objectives and interests a society possesses by the virtue of its historical **development**.'⁶³ And again:

Knowledge cannot be understood as more than the product of men operating in terms of an interest in prediction and control shaped by the particularities of their situation ... Wherever men deploy their cultural resources to authentic tasks of explanation and investigation indicated by their interest, what they produce deserves the name of knowledge.⁶⁴

All beliefs produced in the course of 'men' operating on the world to ensure survival and sustenance are supposed to stand at par with each other. It is not, as Barnes affirms more recently, that some carry more conviction because of greater correspondence with nature but all of them 'alike are made credible to reasonable human beings by contingent aspects of their **context**.'⁶⁵ This is the 'liberal **sensibility**'⁶⁶ toward knowledge that is affirmed by other scholars associated with the Edinburgh school.

So far, the SP has only disabled correspondence truth, but not relativized it: one could argue that different groups find different beliefs more or less credible in their own cultural frameworks, but still hold that only one of them is supported by better evidence and is more rational to hold – that is, given more empirical investigation and arguments derived from already established science, one will emerge as warranted knowledge. In an influential paper, Barnes and Bloor embrace an earnest relativism of truth by making *evidence and reasoning themselves internal to the social context*. Recall that the symmetry thesis had enjoined the sociologist to disregard truth or falsity but only search for causes of a belief's *credibility*. But then, Barnes and Bloor take a further step and announce that there is no sharp distinction *between credibility and validity*. They declare that for the relativist, 'there is no sense attached to the idea that some standards of beliefs are really rational as distinct from merely locally accepted as **such**.'⁶⁷ The 'real rationality' they wish to deny is what is traditionally understood by validity based upon evidence and reason. Barnes and Bloor's claim is that 'evidencing reasons themselves are contingent and socially variable, and what counts as evidencing reason in one context will be seen as evidence for quite a different conclusion in another context [because] something is only evidence for something else when set in context of assumptions which give it **meaning**.'⁶⁸ If this is so – that is, if some data and some other is taken as evidence for some hypothesis on the basis of social convention – then it follows that what is considered valid

may turn out to be invalid in other historical or transcultural circumstances where other contextual assumptions prevail.

By internalizing the very criteria of demarcating the true and the false, science and myth, fact and superstition to the social context of inquiry, the SP goes beyond historicism – and Mamism – and joins the postmodern agenda.²⁵ Mam's historicism, for instance, recognized the situatedness of all human practice, including science, but also held that historically located practice revealed scientific laws that could be discovered once and for all. The warrant for these laws did not change through history. Postmodernism understood most broadly, and the strong sociologists of science, see the standards of justification of beliefs themselves as co-produced with the specific regimes of power. Because both postmodernism and the SP allow no super-cultural norms of rationality, no special tribunals set apart from the sites where inquiry is practised or no perspective-independent rules of evidence, they leave no grounds for preferring one form of rationality to another. Thus we end up with a monism that berates the rationalist dualism between truth and falsity, and fits in very snugly with the postmodernist move away from the economy of untruth to the politics of truth, that is, from a critique of ideology to a critique of truth itself.

Moving from symmetry to 'radical' symmetry are the Paris School followers of Bruno **Latour**. **Latour** finds Bloor's tenet of symmetry rather asymmetric for it uses society to explain nature, but takes society as given prior to nature. **Latour** wants to correct this asymmetry through his 'radical symmetry' and proposes to explain 'nature and society in the same terms.'²⁶ He announces his opposition to all dualisms, including those that juxtapose the rational and the irrational, belief and knowledge *and* the social and the natural. To that end, he proposes a 'counter-Copernican revolution that forces the two poles, Nature and Society to shift to the centre and fuse together [because] we do not make Society, any more than we make Nature, and their opposition is no longer necessary.'"²⁷

But what co-produces both nature and society? The short answer is power. Social actors do not confront Nature and Society separately but in a seamless web. This web is made up not of discrete entities called humans, non-humans, machines, facts, science and society impacting on each other but of 'heterogeneous associations' or networks of all of these. And given **Latour's** counter-Copernican revolution, *any* element of the network – from the Anthrax bacteria that Pasteur isolated to the instruments in the lab – can be seen as an actor with its own interests. Reality and truth get defined in a war of strength between actor networks."²⁸ Scientists simply have more rhetorical and material resources, and consequently get to create alliances large enough that nobody can question their power within their domain.²⁹

This is a decidedly surrealistic picture of science and society and it faces

considerable resistance from those committed to the SP.¹⁴ Contrary to some who find signs of realism in Latour's inclusion of actants from nature in his networks, I believe that Latour's conflation of nature and society represents one of the purest expressions of 'superidealism' (Bhaskar's term) which denies the intransitivity of nature, i.e., that external reality is prior to and independent of all intentionality and human activity in the sense that the world would remain unaffected even if there were no one to represent it. Marx, whose name constructionists (although not Latour, who is more given to discourse theories and semiotics) often invoke to support their claim for radical construction of the 'sensuous world' through labour, allowed the primacy of nature before human practice.¹⁵ Latour's counter-Copernican revolution, in contrast, denies all distinctions between the ontology of the world and our social relations by a fiat.

Social Construction of Nature

Reality itself does not constrain rule use, even when the rules are those for the proper application of empirical concepts or natural kind terms. Reality will tolerate alternative descriptions without protest. We may say what we will of it, and it will not disagree. Sociologists of knowledge rightly reject realist epistemology that empowers reality.¹⁶

Barry Barnes' words capture the kind of anti-realism – or what Barnes himself prefers to call 'single-barrel realism' – that has become the dominant view of constructivist studies of science. So taken-for-granted is the idea of silent compliance of nature with our descriptions of it that in most science studies scholarship it serves as a background assumption requiring no further justification. Nature is disposed off rather easily with a set of scare quotes like in 'the world out there' as if it was somehow naive, or worse, to believe that our theories tell us about the theory-independent world. In this section I will discuss and critique two sets of argument most commonly used to bracket reality, or at least to put it within scare quotes, signifying irony and unreality. The first set leads to nominalism that claims that the way we categorize the entities of the natural world are human and cultural creations with no necessary relation to any essential features of the material entities. The second set includes arguments adapted from Gaston Bachelard, a French philosopher who asked how experiments which crucially depend on human labour can lead to knowledge about a human-independent reality.

Nominalism first. While Barnes and his fellow strong programmers pride themselves in being 'realists' and chastise the adherents of radical symmetry for their idealism, they are more accurately described as 'fig leaf realists'¹⁷: that is, while they are willing to admit the world exists independently of our concepts and conventions, they give it a diminishingly small role in the way we categorize it and what we believe about it. They instead

adhere to a doctrine they call 'finitism,' supposedly the 'most important single idea in the sociological vision of knowledge.'" **Finitism** holds that the relation between the finite number of our existing examples of things and the indefinite number of things that we shall encounter in the future is indeterminate and underdetermined by nature, but depends upon how we decide to develop it. Communities of knowers in different cultures, or in the same culture at different times, do indeed group, order and pattern objects of nature according to perceived similarities and differences, but the sensory input from nature can be classified in any number of possible ways which parallel a community's social relations: as Bloor puts it following Durkheim and Mauss, 'classification of things reproduces the classification of men.' Moreover, the theory of **finitism** holds that every occasion on which a concept is applied to an object in the world, it must be accounted for by reference to specific, local contingencies, which are none other than utility for the purposes of 'justification, legitimation and social persuasion.' In all of this, we are told again and again, nature does not mind what conceptual frame we put on it: it remains 'silent,' 'indifferent' and 'tolerant' in the face of alternative accounts of it. In other words, our accounts do refer to the reality, but the strength of that reference and its future extension is determined by habit and custom.²⁸

If one pauses to think about it, **finitism** is a startlingly radical idea. It claims that our social conventions (which ultimately serve the function of control and dominance) reach all the way down to the very names we give to things. The furniture of the world comes without labels and in an infinite profusion of variety. It is we who determine what belongs with what other objects. The labels we decide to put on things, furthermore, can be explained 'entirely from the collective decisions of [their] creators and users.'²⁹ The order of the world is not given but made by us for the sake of, among other things, controlling our fellow beings.

An even more radical nominalism follows from the work of another group of scholars who take the metaphor of social construction of reality to apply equally to social objects (like our classification schemes, above) and to *material objects themselves*. Most of these scholars are associated with the participant-observer or 'lab-study' tradition and include well-known works by Knorr-Cetina, Latour and Woolgar. This brand of neo-Kantian nominalism, represented here by the work of Knorr Cetina, goes on these lines: nature is undifferentiated, featureless and 'malleable,' and one cannot assume the existence of natural kinds in this infinite, malleable, constantly changing complexity. Scientists bring bits and pieces of nature into the laboratory to 'delimit' the specific entities (including the unobservables like electrons, genes, etc.) that they claim exist in it. The process of delimiting the actual objects of nature is cultural and social because 'what pre-exists before scientifically delimited objects are

culturally delimited objects, those humans pick out and encounter and deal with in everyday **life**,⁸¹ or as she puts it elsewhere, in the laboratory, scientists 'align' the natural order with the social order by selectively noticing and 'reconfiguring' aspects of the world depending upon their own location in time and place. Thus, **Knorr-Cetina** insists that we take the metaphor of 'manufacture of knowledge seriously,' and see science as a 'way of world-making... [science] secretes an unending stream of entities and relations that make up the 'world.' .. this known world is a cultural object, a world identified and embodied in our language and our **practices**.'⁸² In other words, our conceptual schemes and conventions are not ontologically innocent, but have world-constituting powers.

Knorr Cetina's basic idea that 'this known world is a cultural object' is quite widespread among radical critics of science, even though it may not always be couched in the same theoretical terms. Various standpoint epistemologies that seek to empower specific groups of knowers begin with an attempt to loosen the grip of the given over our conceptual schemes, in order that knowers can freely exercise the totality of cultural and other **subjective/interpretive** resources that they may **have**.⁸³ That the contents of the real world are cultural creations appears in many guises in feminist writings on science. Harding, echoing Donna Haraway, ascribes intentionality to nature because 'nature-as-object-of-human knowledge never comes to us naked, but only as *already constituted in thought*...' and simulates an intentional **being**.⁸⁴ Likewise, Fox-Keller sees good science as 'the science that brings the material world in closer conformity with the stories and expectations that a particular 'we' bring with us as scientists embedded in particular cultural, economic and political frames.'" The growing post-colonial literature on science is defining new groups of 'particular we' who should define nature according to our own metaphysical categories. This route leads to 'ethnoscience' which end up affirming cultural essences of civilizations, often in ways that benefit the local elite more than the people.

Notice, in all these theories there is a deep-seated anthropocentrism at work. The direction of determination is from human subjectivity to the world: the limits of our culturally embedded beliefs are taken as the limits of the knowable world. For something to exist, we have to be able to **say** that it exists and as a result there is a tendency to think of nature itself as 'already constituted in thought.' It is true that as language using animals, we grasp the world only through language, but what we say is often **about** a language-independent state of affairs. Not recognizing the distinction between our concepts and the objects that exist independent of these concepts is far from empowering, for the simple reason that the aspects of reality that have not been realized yet in our discourses continue to impact on our lives.

Nominalist theories in both their Humean (Barnes and Bloor's

'finitism') and Kantian (Knorr-Cetina's 'delimiting') guises claim that there are no natural kinds: nature is mute and does not dictate our categories. The natural reaction of the scientist in me is: nature is far from mute. It tells us when we get it wrong. A child dying of a misdiagnosed disease, a bridge not holding up, rains not following prayers – all of these are nature's ways of talking to us, telling us to revise our classification and to check our rules. And in natural science, the scientist in me wants to affirm, we have figured out a way to learn more systematically and efficiently from experience so that we constantly correct the conceptual framework we started out with, and learn to distinguish natural kinds from social kinds. It is precisely this article of faith of working scientists – that science improves our abilities to listen to nature's messages – that is denied by the fore-mentioned scholars and constructivists in general. The most advanced laboratory science is no different from, say extra sensory perception, for in principle, we can only know the world by imposing our prior beliefs which are enmeshed in our 'form of life.'

But the obvious progress in scientific knowledge over the last three centuries shows that it is simply not the case that modern science is trapped by the existing web of beliefs, continuous with our everyday common sense or cultural habits. As Ernest Gellner suggests, the success of modern science in 'insisting upon treating like cases in a like manner ... in bringing about a marked 'rationalization' in our attitude to nature' shows that there may have been a 'diminution of that conceptual opportunism which allows the classification of things to be at the service of too many and too varied a set of social ends.'⁸⁶ What is even more remarkable is that not only have we learned to perceive and classify the regularities in the world but we have also begun to understand the processes and entities responsible for these regularities. As many philosophers, notably Wesley Salmon and Ernan McMullin have argued, science has moved beyond description to explanation: it can not-only tell us what but also why. Science has moved to an 'ontic conception' of explanation in which it is increasingly able to open the black boxes of nature to reveal the underlying causal mechanisms at work. To take a couple of trivial examples, we understand very well the atomic structure on the basis of which we classify the elements in the periodic table; we know what makes for a perception of a colour. Our classification schemes are not only able to group objects into the natural kinds but can explain why they constitute a natural kind."

Furthermore, the idea that we have as great a measure of freedom in applying our rules as social constructivists would have it, is blind to all the accumulated knowledge of evolutionary biology. As Giere argues, the constructivists assume 'a Humean view that there is no natural connection between two impressions. The only connections are those we impose.' But such a view, Giere argues is 'rooted in pre-Darwinian empiricism.'⁸⁷ Humans,

being intelligent, talking primates, can at least be assumed to be capable of the kind of 'fine discriminations in among objects in our environment without the benefit of social conventions' that other animals can accomplish. Thus Giere points out well-known facts about some universal categories (colours, e.g.) that all human beings, regardless of culture are capable of distinguishing. It appears that at least for some perceptual judgments, the fact of widespread agreement does not require a social explanation and the explanation of evolutionary biology is sufficient.

To turn now very briefly to arguments from the artificiality of the phenomena produced in the laboratory, **Gaston Bachelard** insisted on the crucial significance of the human labour that goes into conducting experiments and asked: how can experiments tell us anything about a human-independent reality, when they are not a part of nature but produced in the laboratory by very specific and intricate work? This question forms the basis of many ethnographic studies of laboratory work. Using participant-observation techniques Knorr Cetina tries to deconstruct the artificiality and the constructedness of the entire laboratory and claims that a laboratory is a site of action from which 'nature' is as much as possible excluded rather than included.' This, she explains, is a sign of the **power** of the lab to 'enculture natural objects. The laboratory subjects natural conditions to a social **overhaul**.'⁸⁹

A very similar logic is evident in the story that **Latour and Woolgar (L&W)** tell of the Nobel Prize winning discovery of Thyrotropin Release Factor (TRF), a **peptide** hormone which regulates the production of thyrotropin by the thyroid gland. From their participant-observer study of how Roger **Guillemin** (the co-discoverer of TRF) and his colleagues went about standardizing a test for the putative TRF, L&W conclude a. that the bioassay was chosen as a result of social negotiations; and b. 'without the bioassay, TRF could not be said to **exist**.'⁹⁰

In a trivial sense, it is true that without the bioassay scientists would not be able to assert that the test chemical is actually TRF. But if the claim is the stronger one that 'none of the phenomenon 'about which' participants talk could exist without' the material arrangements in the lab (including the bioassay) and that 'the phenomenon are thoroughly constituted by the material setting of the lab,'" then they are obviously equating the very existence of a phenomenon (TRF) with the scientists' ability to isolate and identify it in the lab. Moreover, their second premise that the bioassay is adopted as a result of social negotiation and is a matter of 'interpretive flexibility' simply shows a lack of appreciation of how there are auxiliary theories for checking the suitability of a given bioassay.

There is in fact a parsimonious explanation that can reconcile local human labor that goes into an experiment with an experiment's ability to tell us something about the entities in the world outside the lab. First, the

very fact that an experimental result is *reproducible* shows that it is not produced by the work done in any particular local setting." *The human labour that goes into an experiment only actualizes the potential and structures already existing in nature.* As Bhaskar elaborates, the very significance of experiments lies in the fact that they tell us about mechanisms, structures and systems of relations that persist in the object of study even outside the lab when it is not being experimented upon. A strict localism on the lines of Bachelard assumes that our transitive knowledge /practice is the same as the intransitive dimension and exhausts it. Bhaskar correctly points out, experiments 'do not produce its intransitive objects of investigation but only the conditions for their **identification**.'⁹³

Social Construction of Experiments

How much 'interpretive flexibility' scientists enjoy in accepting or rejecting experimental evidence is one of the most hotly debated aspects of sociological theories of science.⁹⁴ In this section I'll examine the sociological study of experiments inspired by Harry Collins' Empirical Program of Relativism (EPOR).⁹⁵ I will concentrate on Collins' analysis of gravity waves in order to explicate the kind of reasoning sociologists employ to argue that the criteria for distinguishing confirming evidence of a theory **depend**, in the final instance, not on what the evidence tells us about the phenomenon in question, but on sociologically maintained consensus. Collins' *Changing Order* and other classics in the EPOR tradition have been subjected to severe critiques, leading to newer works in the study of experiments which admit more constraints by nature. Yet, these classics are important, for they are frequently cited as having 'shown' that interpretation of experiments is driven by social interests.

Harry Collins took the radical core of the SP, namely, the tenets of impartiality and symmetry, to a logical conclusion and declared that 'we must treat the natural world as though it in no way constrains what is believed to be.'⁹⁶ Starting from this 'methodological relativism,' Collins goes on to study the debate over the existence of gravity waves and concludes that experimental evidence is not sufficient to decide theory choice. Scientific controversies and disputes cannot be resolved by experiments, because the outcome of experiments is itself decided by tacit knowledge and conventions shared by the community of scientists led by a core group of elite scientists. Collins raises the problem of the 'experimenter's regress,' which goes as follows: Suppose evidence determined theory choice. But evidence consists of replicated experimental results. How do we know that the experiment is successfully replicated? When we can be sure of the competence of the experimenter. But the only way experimenters can demonstrate their competence is by replicating experiments.

Thus, in Collins' study of gravity waves (g-waves), how do we know we have a good g-wave detector? By successful detection of g-waves. But how do we know that these are g-waves that we have detected? Because they have been detected by an instrument which a community of scientists thinks detects g-waves.

But controversies do get settled. What breaks the regress? Scientists' prior beliefs that are entrenched in their 'form of life', answers Collins. If a community of scientists already believes in the phenomenon, they will regard confirming experiments as competent, and if they don't, they will regard disconfirming experiments as incompetent. And how does the community come to a consensus about their belief in the phenomenon and the competence of the experiment? Collins believes that a 'core set' of scientists 'funnels all of their competing ambitions and alliances' and certifies whether or not the crucial experiment is competently performed. The involvement of elite scientists in settling a controversy is not a sign of 'bad science,' Collins argues, but is a necessary feature of experimental work: because experiments cannot be evaluated on a criterion independent of the outcome, scientists have no option but to employ social negotiations to resolve the controversy.

Collins' work adds grist to the constructivist mill that science is not a reality-driven enterprise but, in the final instance, a convention-driven one: we choose theories based upon the 'multiple entrenchment' of concepts and rules in our social conventions. If experiments themselves are resolved ultimately by conventions underlying the tacit knowledge of scientists, it is these conventions that make scientists see correlation and causation in nature. Appropriately, Collins offers sociological solution to Hume's problem of induction:

we perceive regularity and order because any perception of irregularity in an institutionalized rule is translated by ourselves and others as a fault in the perceiver ... it is not the regularity of the world that imposes itself on the world but the regularity of our institutionalized beliefs that imposes itself on the world. We adjust our minds until we perceive no fault in normality. It is why our perceptual ships stay in their bottles.

There is a high degree of consensus between Collins and others on this view of experiments in science as an extension of our web of our institutionalized beliefs. Shapin and Schaffer's celebrated defence of Hobbes, for instance, is premised upon their assumption that the debate between Boyle and Hobbes was not only about alternate conceptions of knowledge and appropriate knowledge-seeking practices, but ultimately involved different ideas of the entire social order, including the relation between laity, the intellectuals and the authorities. Likewise, Andrew Pickering's study of quarks is meant to show that high-energy physicists accepted the theory of weak neutral current because 'they could see how to ply their trade more profitably in a world in which neutral current was **real.**'¹⁷ This so-called

'opportunism in context' is a rather cynical and economic interpretation of scientists' behaviour which has them investing their expertise in areas promising highest returns.

One fallout of this convention-driven view of science is that practicing scientists end up looking like 'reality dupes' who naively believe that their theories are picking out aspects of the real world. But, as Roth and Barrett point out, the anti-realism of the EPOR scholars is not a *conclusion* they reach from their case studies, but rather a *precondition* for their 'phenomenological epoche' (i.e., suspension of natural attitude) regarding scientists' own accounts of doing science. That is the whole point of Collins' famous injunction to constructivists to proceed as if reality does not matter (even though scientists think it does). By bracketing reality, the sociological method is supposed to give factors *other* than the alleged nature of the physical world a chance to be seen at work. It may be the case that the best explanation of why scientists chose one theory over another is to be obtained by omitting references to reality but, as Roth and Barrett correctly point out, it may be that explanation might, at least in some cases, also require reference to reality. The point is that EPOR *sets up rules of the game such that the reality is ruled out by a methodological fiat*. The signature conclusion of EPOR scholars on realism runs as follows:

SSK offers strong empirical evidence that if our beliefs about controversial features of the world are a consequence of the way the world is, this is not evident during passages of discovery and proof. An account which rests on orderly interaction with the world can be provided only after retrospective reconstruction.¹⁰⁰

So here we have Collins offering us evidence to the effect that evidence doesn't matter!

All irony aside, I find both of the above cited 'conclusions' (assumptions?) of EPOR – that science is, in the final instance, convention driven, and that the reality is a retrospective 'upshot' of scientific practices – extremely troubling. The views of these sociologists go completely against all that made laboratory work exciting and worth the effort. But there are good reasons to refuse Collins' conclusions. For one, because Collins completely ignores the perfectly rational reasons (wrong calculations, computer errors) why the experiment in question had to be rejected." Furthermore, Collins' premise for experimenter's regress is plain wrong. Experimenters' regress does not exist. It does not exist for this reason: *the reliability of an instrument (or a technique) can be established by connecting its performance to procedures that can be validated by a set of background assumptions and laws that are independent of the claim under test*. Thus it is simply not the case that the test of the g-wave detector depends upon the existence of g-waves, or the existence of g-waves is 'coextensive with' the availability of a g-wave detector. We have a large number of independently justified beliefs – indeed, Einstein's theory of

general relativity itself – that predict the presence of g-waves and allow us to hypothesize their nature, and how, in principle they can be detected. Likewise, there are other networks of physical theories that justify the design, functioning, sensitivity etc. of the detector. It is encouraging that the more recent work recognizes the independence of experiments from the theory under **test**.¹⁰²

Social Construction of Facts

My Lord, facts are like cows. If you look at them in the face long enough, they generally run away.¹⁰³

In the studies reviewed above, constructivists try to stare away the materiality and independence of nature, and experimental evidence thereof, into the interests, ideologies and the prevailing commonsense of scientists and the societies they belong to. The social and the real become inseparable and our knowledge of the real – what we takes as facts – becomes a projection of the social on the real. In this section, I will describe and critique the near total permeability constructivists allow between the social and the real at all times in the evolution of scientific facts. I will argue instead that science is not a seamless web between the outside and the inside, the social context of inquiry is *not* inseparable from the logic of inquiry. Rather, *science separates itself from conventional wisdom by repeatedly testing and revising the wisdom of the conventions in the light of what it learns about the world*. It is this dialectic between the background assumptions and the real world that is independent of these assumptions that makes science a distinctive way of knowing the world. It is this dialectic that allows nature to have a say in our construction of facts.

The point of departure is again the 'strong programme' of SSK. Recall that the SP had argued that evidence cannot be used to determine whether any particular knowledge of any social group is better grounded in reality than the other because what counts as evidence is itself a matter of cultural convention. The claim that 'something is only evidence for something else when set in context of assumptions which give it **meaning**'¹⁰⁴ plays a prominent role in sociological case studies. These case studies set out to show that scientific theories are accepted because of the congruence between the background assumptions (which serve as reasons for treating an observation as evidence) and the social interests and cultural meanings of the dominant groups at any given time. The SSK literature is replete with case studies 'demonstrating' that the web of background beliefs relevant to science is coterminous with the entire society and includes its religious myths, cultural assumptions and social interests.'" Or as the recent work on 'technototemism' suggests, science can be seen as the 'highest form of totemism' through which the totemic relations between

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social groups and natural world get mapped on to, or inscribed into, scientific facts.'" But this role of culturally embedded background assumptions in construction of scientific facts has been developed most consistently by the feminist critics of science.

Evelyn Fox-Keller, for instance, argues that what counts as knowledge – specifically, 'the kinds of questions one asks and the explanations one finds satisfactory' – depends upon one's prior relationship (more controlling vs. interactionist) with the object of study, which in turn is shaped by the relationship of the knower with other members of the society (especially the mother-child relationship which leads to a more 'dynamic objectivity' among women and a 'static objectivity' among men). Given the metaphoric genderization of science since its beginning in the 17th century and the static objectivity encouraged among men by exclusive parenting by mothers, modern science is permeated with the language of control and domination. Keller ascribes the attraction of metaphors of 'master molecule' that have guided research in molecular biology to a prior commitment to ideas of hierarchy and control that underlie and support patriarchal relations.¹⁰⁷

Helen Longino provides a more analytical argument on very similar lines and tries to show *how* our prior commitments to certain desirable social relations and political ideals ('contextual values'⁷) become a part of the rules scientific reasoning ('constitutional values'). Almost echoing Barnes and Bloor, Longino argues that 'how one determines evidential relevance, why one takes one state of affairs as evidence for one hypothesis rather than for another, depends upon one's beliefs, which we can call background assumptions . . . a state of affairs will only be taken to be evidence that something else is the case in light of some background belief asserting a connection between the two.'¹⁰⁸ Thus if our background assumption is that human behaviour is the result of neonatal exposure to sex-hormones which determines the relevant brain structures (the 'linear model' in Longino's terms), then the data on correlation between sex hormone levels and variability of physiology and sexual behaviour becomes evidence for differential exposure to prenatal hormones. But if our background assumption is that human behaviour is a result of interaction between biology and social factors, (the 'selectionist model' derived from Gerald Edelman's work on neural Darwinism), then the variability of behaviour will be evidence for both biological differences and differences in social conditioning. According to Longino, both models can explain the data correlating hormones and behaviour equally well. Thus it is not reality per se that determines which model is better. Rather, 'in the final analysis commitment to one or another model is strongly influenced by values or other contextual features,'¹⁰⁹ with those interested in human autonomy and expansion of human potentiality (as feminists are)

favouring the selectionist model over the linear.

As a result, Longino argues that search for a feminist epistemology should be replaced by 'doing research as a feminist' which would require that scientists consciously choose those background assumptions/explanatory models that are congruent with their political values, or as Longino puts it, 'scientific practice admits political considerations as relevant constraint on reasoning, which through their influence on reasoning and interpretation shape content.'¹⁰ The 'bottom line' requirement of doing science as a feminist, according to Longino, will be to choose background assumptions which 'reveal or prevent the disappearance of the experiences of women **and/or** reveal or prevent the disappearing of gender ... [that is] prevent the erasure from inquiry of a gradient of power that keeps women in a position of subordination.'¹¹ Feminists, because of their interest in revealing the **gender/power** gradient in knowledge, will prefer explanatory models which privilege heterogeneity, complexity of interaction and decentralization of power over ontological homogeneity (*i.e.* treating difference as aberration), linear cause-and-effect and unidirectional control. These values are equally and simultaneously cognitive and social because they determine what data count as evidence. And if we have to have contextual values to make sense of data, we should choose the 'right' ones and create knowledge that empowers, rather than dominates, human beings. Longino, unlike other less careful critics, does insist that for feminist values to replace the traditional values that guide scientific work, they have to withstand an open and critical scrutiny by the scientific community, that includes feminists and non-feminists. But the basic thrust of Longino's contextual empiricism – that standards or values that guide evidential reasoning are inseparable from the social context and that these standards ought to serve politically progressive ends – resonates well in science studies where the answer to the question 'whose side are you on?' has become the test of the validity of knowledge.

Is it true that political considerations – or more generally contextual values – decide the choice of background assumptions (or models) in the light of which some observation becomes evidence for a hypothesis? Is it true that cognition can never be free of contextual values and political beliefs? Is it true that cognition is not only socially organized and mediated but is actually driven by social logic?

That an observation by itself proves nothing is not a novel insight. The condition of relevance, *i.e.*, the requirement that to count as a reason, a claim must be relevant to the idea to be tested, was among one of the earliest critiques of logical empiricism, most well known among them being Karl Popper. It was recognized by the early critics of empiricism that such relevance is determined by background information and assumptions.

If critical realists like Karl Popper and Dudley **Shapere**, as well as relativists like Kuhn and constructivists like Bloor and Barnes and the feminist scholars of science can agree on the role of background assumptions in establishing the condition of relevance, and yet come to different pictures of science, the difference obviously must lie in *how they define the domain of the background assumptions*.

For the science studies scholars and most feminist science critics, the domain of the background assumptions is a network of all social and cultural forces that shape the common sense of an era: there is no line between what is internal to science and what is an external influence. These scholars work with what **Ernan McMullin** has dubbed 'presumption of unrestricted sociality (PUS).'¹¹² By and large, those who operate with PUS tend to treat background assumptions themselves as givens which don't change in the light of the new knowledge. Reading social values as constraints on scientific reasoning fails to see the other half of the dialectic: the initial common sense and cultural assumptions that lead scientists to seek some kind of evidence themselves get revised in the light of the evidence. Those theorists who collapse the social context of discovery into all the later stages of research tend to see science as a seamless fabric which is stamped forever by the conditions of its origin. They fail to see that all aspects of scientific inquiry are potentially capable of redesign in the light of knowledge derived from the earlier **phase**.¹¹³

Recent developments in many disciplines of science-as-we-know-it (that is, without any radical make-over of either the institutions of science or the larger society) provide ample evidence of a constant revision of background conventions, metaphors and philosophical assumptions about our world and life in it. As new empirical findings have revealed new phenomena and structures at successive levels in matter, living organisms and mind-brain relationships, there are signs of a new synthesis between what has been conventionally called reductionism and holism. Understanding of details of mechanisms at one level have led to theories that seek to understand relations between different levels and see how qualitatively new properties emerge through these interactions. Take the case most often cited as the exemplar of reductionist, controlling and patriarchal thinking – the idea of DNA as the 'master molecule,' or the 'central dogma' of transfer of information from DNA to proteins. Almost from the time that DNA structure was discovered, the attempt to understand how it was replicated and translated into proteins involved an understanding of a concert of enzymes and structures involving the entire cell – a far cry from the image of central control and dominance read into the metaphor of 'master molecule' by the critics. Likewise, there was nothing dogmatic about the central dogma of molecular biology: each step of it was subjected to rigorous empirical **tests**.¹¹⁴ This is not to say that science-as-we-know-it

is free from social interests and influences: the rush to commercialize processes and products of molecular biology has encouraged a race to decode, manipulate and sell genetic information, often at the cost of a deeper understanding of the physiology of the whole organism. But these interests hardly warrant giving the internal logic of molecular biology a gendered gloss.

Or, take the selectionist model of consciousness developed by Gerald Edelman that Longino holds up as a model that feminists should adopt to understand the relative role of hormones and the environment in shaping human behaviour. Edelman's model of neural Darwinism (or the theory of **Neuronal** Groups Selection) developed as a result of cross fertilization of ideas from immunology, where Edelman had already done Nobel Prize-winning work on how antigens select out the 'right' kind of antibody. Edelman saw an analogy between the immune system and the nervous system, for both of them have to distinguish between self and **non-self**. His selectionist model, moreover, does not exactly displace the linear model of hormone action. Its appeal for scientists lies in its ability to make sense of diverse observations of a dozen of different fields from artificial intelligence to memory **research**.¹¹⁵ To be sure, Gerald Edelman holds strong philosophical views which favour free will and human agency and opposes any attempt to reduce consciousness to molecular or physiological terms alone (although he seeks a purely biological explanation of **consciousness**).¹¹⁶ But these philosophical assumptions don't map on to any particular political position, and as far as one can tell from published sources, there is no evidence that Edelman's work was a result of any explicit political allegiances, or that its appeal is limited to a section of the scientific community with any given set of beliefs on gender, race or class. Indeed, there are strong suggestions in Edelman's explication of his philosophical views that he could well have arrived at his strong aversion to reductionism and biological determinism as a result of evidence from modern biology and physics."¹¹⁷

What I want to suggest is that a congruence between facts and values may not *always* be evidence of values constructing facts, but can *also* be read as facts leading to values: the two generally grow and change together, and I see no reason to see contextual values as given and prior to the actual work of science. Moreover, I would argue that the very fact that the **selectionist** model has had the kind of appeal among biologists interested in the development of brain and consciousness shows that the existing scientific institutions are capable of correcting their course and heading out in directions which hold promise for expansion of human abilities – a promise that feminists and other progressives can welcome, as Longino indicates.

One aspect of the give-and-take between our assumptions and the real world in the process of scientific inquiry is that the *domain of background*

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assumptions changes and becomes less social as science matures, and correspondingly, scientists have less choice to pick background assumptions and models that are consonant with their politics. As Dudley Shapere has argued, for a *mature* science, background assumptions are mostly made up of a set of background *scientific* theories, or 'a body of successful and doubt-free beliefs which have been found relevant to the **domain**':¹¹⁸ science becomes more *internalized* in its reasoning process as it grows, and once past its infancy, the external milieu is no longer internal to it. As Kuhn put it, 'compared with other professional and creative pursuits, the practitioners of mature science are effectively insulated from the cultural milieu in which they live their extra-professional lives.'" Science aims at becoming more self-sufficient in the sense that 'the more science learns, the more it becomes able to learn' on the basis of its already existing stock of the best, most confirmed, least doubted beliefs. Therefore, *what decides evidential relevance in mature science is not raw social values but scientifically tested reasons*, and the two gradually get demarcated from each other through a process of 'conceptual bootstrapping,' which involves a constant revision of culturally derived assumptions in the light of empirical evidence generated by the hypotheses which are initially based on the cultural assumptions. The increasing demarcation between the scientifically relevant background assumptions and contextual factors need not be based on any a priori and universal criterion but is itself a product of historical development of a science. The relevance of this dynamic to the question of doing science as a feminist (or as a socialist, as a Third World scientist, or as a Hindu or Islamic scientist) is that as any science matures, background assumptions are *not* up for grabs. After a certain point, the question of the political valence of background assumptions becomes rather meaningless.

Another way to visualize how background assumptions become a part of science – not as raw social conventions which can be changed voluntarily depending upon ones politics, but as a part of the corpus of tested and confirmed results – is to see the growth of science in any domain as a progressive filling out of a crossword puzzle. In this crossword puzzle, as Susan Haack argues, experimental evidence serves as the analogue of the clues, while background information serves as already completed **entries**.¹²⁰ How reasonable a new entry in the puzzle is depends on how well it is supported by the clue and any other already-completed intersecting entries. Once a new entry is accepted, it becomes a part of the background against which other clues are read and new entries made.

Now, it is entirely possible that some particular entry might be put in place without sufficient support from the rest of the solved and unsolved clues for reasons of ideology, political interests, aesthetics or the pressure to complete the puzzle as soon as possible: that is, a scientific claim might

be accepted without sufficient warrant. And it is a legitimate role of the social and humanist critics to investigate and show, *on a case by case basis*, when claims get accepted not because they are warranted by evidence, but because of social interests. But in order to show that, it is necessary to believe that *in principle* how good a piece of evidence is (i.e. warrant) does *not* depend on whether or not it gains acceptance: a statement may be true even if no one believes it at any given time. But if one follows Haack's analogy, it becomes clear that the constructivist critics that we have been examining in this essay collapse warrant of a claim into its acceptability: they argue that it is *because some privileged members of a community of inquirers find a claim acceptable to their prior beliefs that the claim becomes warranted*.

According to Haack, it is due to the strength and distinctiveness of the social and communal nature of science that warrant does *not* get determined by acceptance. Because science is the work of many persons, spanning different generations and different cultures (increasingly so in today's globalized science), scientific knowledge gets accepted through a process of checking and criticism. Here, one would happily agree with the constructivist critics that the more open and inclusive the social institutions of science, the better it would be for science, since inclusion of diverse view points will improve the quality and degree of mutual criticism. Thus the argument for equal opportunity for women and minorities with adequate interest and training, makes not just social but epistemic sense. The more open and inclusive communities of knowers are, the more likely they will be to pass on **true(r)** beliefs rather than falsehoods.

What does *not* make sense is the claim that the gender, race or ideology of the knowers will (or should) make any difference to the assessment of the evidence and the conclusions derived therefrom. The idea that the social location of the knower makes a difference to scientific reasoning does not make sense because, just as someone solving a crossword puzzle is limited by the grid of already-completed entries and the clues, the *scientific community is not at liberty to change any entry at will without destroying the integrity of the puzzle*. In the final instance, this integrity is crucially dependent on the structures and mechanism of the world itself. Only those who would simultaneously deny the integrity and independence of reality would have no qualms in treating the social character of science as necessary *and sufficient* to explain the entire logic of science.

Before I move on to my next – and final – task, namely, a critique of the radical call for socializing objectivity, I will very briefly examine how the adherents of the Paris School explain the social construction of facts. To recapitulate, the Paris School theoreticians, notably Bruno **Latour** and Michel **Callon**, assume a more radical symmetry that sees both nature and society as being produced together as a consequence of the network of

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human and non-human actors.

In this actor network model, a statement becomes a fact if it is inserted in network of other sentences that 'black box' it, or remove it from any further inquiry: or as one face of **Latour's** Janus announces in his famous Science in Action, 'when things hold, they start becoming true.'" A fact is a consequence, and not a cause of our coming to believe in it; treating a statement as a fact, causes it to be a fact.

But what makes statements 'hold'? The strength of the alliances of a sentence with other **actants** in the network which as we have seen before includes, without any ontological distinctions, bits and pieces from nature, culture and society: bacteria, X-rays, hormones and machines (inscription devices), scientists, other scientists, their sponsors and the rest of us. To the extent a scientist succeeds in establishing a strong enough network to the point that no further questions are asked regarding the **actants**, **he/she** succeeds in defining reality and the truth about them. And since the resources required for this network building are so many and so expensive, only elite scientists in elite labs located in elite countries get to define the truth for the rest of us."

At one level, this model of making of facts can be read as a Machiavellian description (Latour's own label) of what scientists do. It can't be denied that a good part of scientists' time, effort and resources are taken up by mundane, everyday concerns of the kind that occupy all of us: who to collaborate with? who to trust? whose work to cite? The difference is that for **Latour**, **Woolgar** and other lab studies scholars, scientific reasoning is this mundane reasoning and that nothing 'scientific' happens inside the laboratory. Any talk of reasoning that links the inscriptions to something independent of the inscriptions (TRF, **Pasteur's** anthrax bacilli) are treated as so many post-facto rationalizations.

That power legitimizes ideas is hardly a novel insight. Marx famously argued that the ruling ideas of a society are the ideas of the ruling classes. But Marx left open the possibility that those without power can come to a scientific understanding of the true state of affairs, and armed with it, expose and dethrone the ruling ideas. But if truth by definition is an effect of power, what hope is left for those without power? If there is no truth but that defined by those with better lawyers, as follows from the **actant-network**,¹²³ can the poor ever get justice? **Latour** is well aware of these concerns but treats them as one more example of the traditional sociologists' lack of symmetry between 'might' and 'right': in his radically even-handed world, 'it is necessary not to make any a priori distinction between might and **right**'¹²⁴ (in order to show the might that is implicit in what is accepted as right and reasonable). I fail to see what useful purpose can be served by treating as symmetrical values that should be ethically and politically asymmetric.

Socializing Objectivity

Most radical critiques of science tend to display a set of interesting contradictions. While the critics reject the idea of objectivity as authoritarian or worse, they simultaneously want to claim that doing science as a **feminist/third world woman/working class** will make science *more* objective (or less biased). Moreover, while they have no hesitation in internalizing the very criteria of validity to the social context of any group of knowers, most constructivists simultaneously don't wish to be called relativists. This linking of radical change in social relations with a promise of greater objectivity in knowledge serves to legitimize the critique of science-as-we-know-it, for it casts the critics not as rash rebels against science, but as visionary leaders of a new science and a new society. In this final section, I will examine how the constructivist critics square their radical contextualization of all aspects of science discussed in previous sections with their idea of objective knowledge. I will argue that the resolution they offer ends up distorting those aspects of science which may be most important for a critical self reflexivity of background assumptions that the critics claim to value.

Typically, constructivists argue that while they are epistemic relativists, they are not judgmental relativists: that is, even though they hold that the truth of a belief depends upon the social context of its production, they are not suggesting that all beliefs are equally true or **false**¹²⁵: some social relations and cultural contexts, they claim, lead to better accounts of the world. Given their antipathy to the realist notion of truth as correspondence of our statements with an independently existing world, constructivists operate with a deflated notion of objectivity which, in the final instance, rests on the social arrangements under which scientific inquiry takes place. They typically offer two sociological criteria for objectivity. First, a statement is objective if the social conditions of its production can screen out purely subjective, arbitrary and idiosyncratic beliefs in assessing the relevance of the evidence to the **hypothesis**.¹²⁶ This sociological criterion of demarcation between science and non-science is perfectly valid and was the cornerstone of Robert Merton's sociology of science. But the *raison d'être* of the post-Mertonian sociology of science is to 'show' that scientific institutions and social norms cannot screen out biases and social values that are a part of a culture's world-view, or to put it another way, the very reasoning of science (the constitutive values) cannot function without social values (contextual values). Constructivists cannot define objectivity as a total screening of all bias, for then they have no choice but to say that objectivity is a chimera. The only choice that appears to be available to them is to equate objectivity with elimination of 'bad biases': a statement is deemed objective if it serves desirable social ends. Because value

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freedom is impossible, the constructionist argument goes, we can improve the quality of science by improving the social values that science serves. As Evelyn Keller puts it quite succinctly:

scientific knowledge is value-laden (and inescapably so) just because it is shaped by our choices ... [thus] why should we even think of equating 'good' science with the notion of 'value-free'? Far from being 'value-free,' *good science* is science that effectively facilitates the material realization of particular goals, that does in fact enable us to change the world in particular ways ... In this sense, *good science* typically works to bring the material world in close conformity with the stories and expectations that a particular 'we' bring with us as scientists embedded in particular cultural, economic and political frames.¹⁰⁷

It is this logic that leads most constructivist critics to call for more ideology as a cure for purported ideology in science, with a difference that the 'particular we' whose stories and expectations that science is supposed to justify are on the 'right' side, whatever that means. My discomfort with making political use-value as the criteria of goodness of knowledge stems from this question: what happens when the 'particular we' turn out to be from the 'other side' who can just as easily use the logic of construction to justify reactionary ideas, as is happening today with religious and cultural nationalists in Third World?¹⁰⁸

Yet, an explicit call to admit political ideals into the logic and reasoning of science is one of the most important conclusion and recommendation to emerge from constructivist science studies. This recommendation obviously turns the traditional ideal of value-neutrality on its head. Conventionally, objectivity is understood as a 'stepping back' from one's initial view in order to form a new conception which has the earlier view as an object amenable to a scientific **analysis**. In other words, as Thomas Nagel explains in his classic *The View From Nowhere*, we continually raise our understanding to a new level by forming a new conception that includes a more detached understanding of ourselves, of the world and of the interaction between them. Thus objective understanding is aimed at 'transcending our particular viewpoint and developing an expanded consciousness that takes in the world more fully.'¹⁰⁹ As we detach our understanding of the world from the **contingencies** of the here and now, the traditional view of objectivity holds, we simultaneously succeed in revealing successive layers of reality. Objective truths are therefore seen as more faithful representations of reality.

The constructivist reformulation of objectivity in terms of use value of knowledge leads them to this practical recommendation: when faced with a choice of 'evidencing reasons,' (i.e., background assumptions) pick the ones which allow the inquirers to criticize increasingly deeper layers of the assumptions of their worldviews. As Sal Restivo puts it, those knowledge systems are to be preferred that have a higher 'capacity for criticism,

reflexivity and **meta-inquiry**.¹³⁰ Here Restivo makes common cause with Sandra Harding's 'strong objectivity' and Helen Longino's 'transformative criticism,' which are aimed at revealing the hidden biases against women and other oppressed people that become constitutive of 'good' (i.e., empirically adequate) science. All three assume one, that science-as-it-is is incapable of spotting those biases which the members of the group share, and secondly, there are certain social arrangements which enhance the scientific community's capacity for self critique of their assumptions and biases. Thus the agenda for a deeper and stronger objectivity becomes 'detection of limiting interpretive frameworks and construction of more appropriate frameworks . . . which are consistent with the values and commitments we express in the rest of our **lives**.'¹³¹

This project of active 'detection' of the social and political valence of interpretive frameworks and construction of 'more appropriate frameworks' takes different routes for different sociologists of objectivity. For Restivo and his 'weak programme' associates, the most **objectivity-enhancing** political position is anarchism, while for Harding and Longino, it is feminism. In other words, explicit embrace of these political positions – 'doing science as a feminist,' as Longino exhorts – is justified because these positions offer scientists a better opportunity to question and critique their own taken-for-granted background assumptions. In practice, these proposals translate into striving for a society where individuals can become 'open-ended and self-actualizing epistemic agents' (Restivo), or starting inquiry from the standpoint of marginalized social groups, who presumably can see farther, deeper and clearer than those in the dominant groups (Harding), and organizing scientific institutions such that subjective opinions of no one group dominate and the assumptions of all groups are equally subjected to a 'transformative criticism' (Longino). It is safe to say that all three would agree that as a minimal requirement, scientific community must be maximally inclusive of diverse and conflicting viewpoints and interest, a sentiment expressed well by Longino:

That theory which is the product of the most inclusive scientific community is better, other things being equal, than that which is the product of the most exclusive. It is better not as measured against some independently accessible reality but better as measured against the cognitive needs of a genuinely democratic community.¹³²

There is a weak interpretation of these assorted arguments for a democratized and inclusive science which is totally incontrovertible, and perfectly compatible with a realist epistemology. **This** weak interpretation would limit the appeals for democratization to the process of discovery – that is the choice of problems, setting the agenda etc. Opening the institutions of scientific and technological learning and research to all those interested in science, regardless of social location is undoubtedly good for science and good for the society. The larger the stock of ideas, view points

and experiences, the larger the diversity of questions asked and approaches followed. Such a diversity is needed for compensating for individual biases and idiosyncrasies. Equal access to scientific knowledge is both a political *and* an epistemic good.¹³³ Undoubtedly, scientific institutions should be open and capable of rigorous critique of ideas without regard to position of the scientists in the pecking order, and ideally, institutions of learning should not be at the service of dictators, generals, and profiteers. And undoubtedly, modern scientific institutions need a substantial course correction in this regard.

But science for all the people is hardly the agenda of the kind of science critique that we are examining here. Indeed, for those who see science as inherently ideological, it makes more sense to *insulate* people from modern science rather than ask for equal access to it for all: that is indeed the demand of the so-called 'post-colonial' critics of science who see modern science as a threat to other cultural meanings. Furthermore, as we have already seen, the partisans of stronger objectivity believe that the context of discovery extends seamlessly into all aspects of science, including the processes of justification. Given this assumption, the constructivist argument for greater democracy and pluralism in science has a much stronger interpretation which extends beyond the institutional arrangements of scientific institutions to the very reasoning of science.

Deeper democracy and pluralism in more and more aspects of society is an ideal I share. But I believe that the demand to democratize science, when carried into the realm of justification itself, may end up making science *less* objective, rather than more. However flawed our contemporary scientific institutions are, they nevertheless allow room for scientists to come together (or at least aspire toward coming together) as a community not based on their sex, race or class, but united in a broadly defined goal – finding truths about the natural world. These institutions are based upon an ethic (however imperfectly adhered to) that demands submission of all claims, regardless of the source, to the toughest empirical tests and critique. Constructivists by and large are suspicious of any such profession of unity of ideas and goals and tend to see them as ideological justification of scientists' bid for power. They would like scientists to be explicit about their 'true' interests (determined by personal identities, interests and histories) and give up 'pretensions' of universalism. This lies at the heart of the demand for discursive democracy, wherein scientists should be free to explicitly invoke their identity-based ideologies and assumptions at all steps of scientific reasoning. Such a demand, I am convinced, will destroy the community of knowers united in an ideal of search for truth and replace it with a Hobbesian war of all ideologies against all others.

I believe that the deeper democracy of society that the radical critics of science seek may require that the process of evidential justification in

science *not* be seen as inherently political, and *not* be democratized if democratization means rooting out 'bad' bias (by whose standards?) and inculcating 'good' bias (by whose standards?). If democratization of science requires 'admitting political considerations as relevant constraints on reasoning ... and content' as Longino recommends,¹³⁶ or that science be seen as 'politics by other means,' as Sandra Harding advises¹³⁵ or that science incorporates anarchist values of 'individual liberty, community life and healthy environments' as Sal Restivo urges,¹³⁸ then I am afraid such 'democratization' will end up undercutting the grounds for genuine and deeper democracy in our social relations, because the latter requires reliable knowledge of the structures of the world that exist independently of all partial perspectives.

To insist upon restructuring the social relationships of science – as Restivo, Harding and Longino do – without allowing that science as a social institution may have a social dynamic that is well adapted to its function of generating knowledge of the world, can make science *less self-reflexive* rather than more. The dilemma that these critics face is this: the values they want to promote in scientific practice – deeper critique and reflexivity – may causally depend on social relations based upon an ethos of co-operation tinged with competition. As David Hull's well-known study demonstrates, the apparently self-sewing behaviour of scientists in terms of competition for credit and career advancement helps to ensure a higher degree of self-reflexivity and critique in modern scientific institutions as they have developed in the West.¹³⁷ The question then is, can we afford to radically alter the social relations of science, and yet continue to desire the advantages the peculiar logic of the existing scientific institutions make possible? There is no doubt that inclusion of women, and all other historically excluded social groups will improve the critical reflexivity of science by increasing the variety of viewpoints. But beyond that, making science more consensual may end up closing off venues for critique and growth of knowledge.

Another major problem with letting go of the ideal of objectivity as neutrality is the sheer unpredictability of how knowledge affects politics. Ideas, as postmodern theorists have been at pains to show, don't have essences. Given the extreme variation in social contexts in which scientific knowledge is received, it is hard to tell in advance which ideas are 'progressive.' To take a few examples, the recent findings that homosexuality may have biological basis was received with much consternation in Germany where the gay community remembered the Nazi solution to unwanted genetic characters, while in the US, many gay activists welcomed the findings, feeling that they may alleviate discrimination. Likewise, the ideological correctness of studying racial and gender differences changes with time and place. After years of agitating against raging

hormone theories about women, feminists are now calling for research on how the effect of medication varies with **hormones**.¹³⁸ What political values should the researchers have given consent to at any given time?

Thirdly and finally, despite their valiant efforts to avoid judgmental relativism, I don't think that social theorists of objectivity have given us good reasons why they should not be considered as embracing a variety of village relativism. In the case of Sandra Harding, the superior epistemic standpoint of the oppressed was to protect feminist knowledge from becoming one more kind of knowledge, no better and no worse than any other way of knowing. But Harding has given no convincing evidence to show that marginalized persons possess superior epistemic qualities. It is not clear how her account of knowledge can remain non-relativist in the absence of the epistemic privilege of marginality. Likewise, it is not clear if **Longino's** view of transformative criticism can avoid relativism for the simple reason that what counts as public scrutiny varies from culture to culture, from one community of experts in one field to another and from one historical period to **another**.¹³⁹

In view of these problems with the various calls for science produced from the perspective of the oppressed, I prefer C.S. Pierce's frank confession of belonging to 'that class of scalawags who propose to look the truth in the face, whether doing so be conducive to the interests of the society or not. Moreover, if I ever tackle that excessively difficult question, 'what is for the true interest of society?' I should feel I stood in need of a great deal of help from the science of legitimate **inference**.'¹⁴⁰

Conclusion

In this essay I have tried to show that while science is a socially *manifested* process, it is nevertheless not a socially *grounded* activity: only a dialectical and progressive interaction between the socially situated inquirers with each other *and* with the mind-independent world can explain the content of scientific ideas.

Unlike the constructionist theorists, I believe that social relations do not play a *constitutive* role, although they do play a crucial *facilitating* role in the practice of science. I have argued that social relations of science have to be studied with an eye toward the overall goal of science, that is the production of structured accounts of the world that actually tell us something true about the world. The validity of these accounts, I have further argued, cannot be completely subsumed into the existing social and cultural context, for that would be reductionist and anthropocentric to boot, as it will make 'human abilities the measure of what nature can do,' something the wise **Galileo** cautioned against nearly four centuries ago. Scientific knowledge as a purely and ultimately social knowledge,

furthermore, cannot avoid a non-trivial epistemological relativism, which is antithetical to the cause of justice: for 'without truth, there is no injustice,' only so many different stories.¹⁴¹

In sum, this one-time scientist believes that science is simply too important to be left to those who would only deconstruct it.

NOTES

1. David Bloor, *Knowledge and Social Imagery*, (Chicago, 1991, 2nd Edition), p. 42.
2. The three-sided relationship between the world, the self and culture is from Frank Farrell's *Subjectivity, Realism and Postmodernism: The Recovery of the World in Recent Philosophy*, (Cambridge, 1994).
3. Sociological theories critiqued in this essay include the Edinburgh School's 'Strong Programme' put forth by David Bloor and supported by Barry Barnes and Steve Shapin among others; Harry Collins' 'Empirical Program of Relativism' and the associated work by Andrew Pickering; the lab studies tradition of Knorr Cetina, Steve Woolgar and Bruno Latour and the actor-network theories of the 'Paris School' led by Bruno Latour and Michel Callon. Feminist critics of science examined in this paper include Evelyn Fox Keller, Sandra Harding and Helen Longino. Attention will be drawn to selected postcolonial science critiques as derivative discourses of sociological and feminist theories of science.
4. See the opening lines of David Bloor's programmatic text, *Knowledge and Social Imagery*. For a clearly laid out distinction between the *structuralist* (associated with Robert Merton) and *constructivist* (post-Mertonian and post-Kuhnian) streams, see Harriet Zuckerman, 'The Sociology of Science,' in Neil Smelser (ed.) *Handbook of Sociology* (California, 1988).
5. Arthur Fine labels this as the 'central doctrine' of constructivism. See his 'Science Made Up: Constructivist Sociology of Science,' in Peter Galison and David Stump (eds.) *The Disunity of Science: Boundaries, Contexts and Power* (Stanford, 1996).
6. See Karin Knorr Cetina's writings for programmatic statements regarding the constructive v. descriptive nature of science, especially her paper in Knorr Cetina and Michael Mulkey (eds) *Science Observed* (California, 1983).
7. Bruno Latour is the best known representative of co-production thesis; see his *Science in Action: How to Follow Scientists and Engineers Through Society* (Cambridge, Mass.: Harvard University Press, 1987). For an extended theorization of coproduction of science and social order as 'techno-totemism' see Devid Hess, *Science and Technology in a Multicultural World: The Cultural Politic of Facts and Artifacts* (New York: Columbia University Press, 1994).
8. Anthony Giddens interprets postmodern critiques of scientific rationality not as superseding modernity, but as a deepening of modernity which turns the self-critical rationality of science on scientific rationality itself. See Giddens, *Consequences of Modernity* (Stanford, 1990), p. 50. See also, John Searle, 'Rationality and Realism, What is at Stake?' *Daedalus*, Vol. 122, 1993.
9. Frank Farrell, *Subjectivity, Realism and Postmodernism*, p. 245. I am not suggesting, however, either that truth will always shine through all socially motivated narratives, or that truth is to be valued only insofar as it promises to dispel particular ideologies. I agree with Andrew Collier that 'as far as the politics of liberation is concerned, the cognitive virtues of objectivity, clarity, logical rigor are also political virtues – even though they may conflict with some short-term political advantage. Good politics itself requires that cognitive practices be conducted according to cognitive criteria...' see Andrew Collier, *Socialist Reasoning: An Inquiry Into the Political Philosophy of Scientific Socialism* (London, 1990), p. 148.

10. Michele Barrett, *The Politics of Truth: From Marx to Foucault* (Stanford, 1991).
11. For a selected sample of the vast and fast-growing literature in support of 'ethno-sciences,' see Sandra Harding, 'Is Science Multi-cultural? Challenges, Resources, Opportunities, Uncertainties.' *Configurations*, Vol. 2, 1994. Ashis Nandy (ed.), *Science, Hegemony and Violence* (Oxford, 1988); Vandana Shiva, *Staying Alive: Women, Ecology and Development* (London, 1989); Stephen Marglin and Frederique Marglin (eds.) *Dominating Knowledge: Development, Culture and Resistance* (Oxford, 1990); Ziauddin Sardar (ed.), *The Revenge of Athena: Science, Exploitation and the Third World* (London, 1988).
- For a critique, see Meera Nanda, 'History is What Hurts: Materialist Feminist Perspectives on Green Revolution and its Ecofeminist Critics,' in Rosemary Hennessy and Chrys Inghram (eds.) *Materialist Feminism: A Reader* (forthcoming). For a defence of science for postcolonial feminism, Meera Nanda, 'The Science Question in Postcolonial Feminism,' *Annals of The New York Academy of Sciences*, vol. 775, 1996. For a defence of popular science movements, Meera Nanda, 'Against Social De(con)struction of Science: Cautionary Tales from The Third World,' in Ellen M. Wood and John B. Foster (eds.) *In Defense of History*, Monthly Review Press, 1997.
12. Sal Restivo *Science, Society and Values: Toward a Sociology of Objectivity* (New Jersey, 1993), p. 55.
13. 'Science, surely, is not limited to national or racial boundaries,' Hans Reichenbach wrote while in exile from Nazi Germany.
14. Science for people movements in Third World countries which look to modern science not just as a means for economic development but also cultural change, operate on a Kantian sense of universal rationality well described by Christopher Norris, 'such is the *sensus communis* as Kant conceived it: the jointly epistemological and evaluative quest for principles of reason that would point beyond the limiting horizon of present, de facto communal belief.' See Noms, 'Truth, Ideology and Local Knowledge: Some Contexts of Postmodern Skepticism,' *Southern Humanities Review*, Vol. 28, No. 2: 109-166, p. 134. For a rigorously argued defence of the project of internal critique of traditions, see Martha Nussbaum and Amartya Sen, 'Internal Criticism and Indian Rationalist Traditions,' in Michael Krausz (ed.), *Relativism: Interpretation and Confrontation* (Notre Dame, Indiana, 1989).
15. Such an objective basis for critique has fallen out of favour with the constructionist and pragmatist critics of science who suggest that we reject a practice not because of its falsity with respect to some actually existing state of affairs, but because of its 'meaning in use' i.e., social effects. The problem is that enculturation habituates people to many injustices, and as a result, often the most odious social practices appear benign in their 'meaning in use.' Besides, the meaning in use differs for different groups of people: the network of cultural ideals, myths and images can make practices that are degrading and harmful to particular groups of individuals appear benign and even necessary for the good of the larger society: clitoridectomy is one example, the practice of untouchability is another.
16. A primary commitment to human values can keep scientism at bay as well: just because we have discovered some facts of nature, it does not follow that we *must* remake society to correspond with them. While we *can* derive our values from facts, that does not *oblige* us to choose only those values that are consonant with facts about the natural world.
17. Martin Hollis and Steve Lukes (eds.) *Rationality and Relativism* (Cambridge, Mass., 1982). Karl Popper's definition of relativism appears in his *The Myth of the Framework: In Defense of Science and Rationality* (New York, 1994). For a review of the variety of anti-realisms and relativisms that appear in the constructionist literature, see Robert Nola, 'Introduction: Some Issues Concerning Relativism and Realism in Science,' in Robert Nola (ed.) *Relativism and Realism in Science* (Dordrecht, 1989).
18. Larry Laudan, *Science and Relativism: Some Key Controversies in the Philosophy of*

- Science* (Chicago, 1990), p. viii. Despite his uncompromising opposition to epistemological relativism, **Laudan** is not a realist.
19. **Barry Bames**, 'Natural Rationality: A Neglected Concept in the Social Sciences,' *Philosophy of Social Sciences*, Vol. 6, 1976.
 20. In his interpretation of the central dogma of constructivism, H.M. Collins explicitly forbids invoking TRASP—that is, truth, rationality, success and progress—for explaining why anyone should hold a belief. Collins, 'What is TRASP?: The Radical Programme as a Methodological Imperative.' *Phil. Soc. Sci.*, 11, 1981.
 21. **Roy Bhaskar**, *A Realist Theory of Science* (Leeds 1975), p. 36.
 22. Philip Kitcher, 'Knowledge, Society and History,' *Canadian Journal of Philosophy*, Vol. 23 (1993).
 23. For a well-stated case for treating natural science as equally interpretive as human sciences, see Joseph Rouse, 'Interpretation in Natural and Human Science,' in David Hiley, James Bohman and Richard Shusterman (eds.) *The Interpretive Turn: Philosophy, Science, Culture* (Ithaca, 1991).
 24. **Searle**, *Construction of Social Reality* (New York, 1995), p. 190. **Searle** of course opposes this interpretation and argues that mountains differ from money in the sense that the former 'do not require the existence of representations as part of the conditions of their normal intelligibility', p. 193.
 25. For a recent work, see Teresa **Ebert**, *Ludic Feminism and After: Postmodernism, Desire and Labor in Late Capitalism* (Michigan, 1996).
 26. Roy Bhaskar, *Realist Theory of Science*, p. 9.
 27. Paul Gross and Norman Levitt, *Higher Superstition: The Academic Left and Its Quarrels with Science* (Baltimore, 1994), p. 234.
 28. With an aim of exposing science critics' rather weak grasp of science and even weaker standards for peer review, Alan **Sokal**, a theoretical physicist at New York University intentionally strung together the most outrageous statements by well-known postmodern theorists and cultural critics, from **Derrida**, **Lacan** and **Latour** to Aronowitz and Haraway to pay exaggerated tribute to how postmodern social theory has shown that the reality physicists study is a social and linguistic construct. *Social Text*, an avant garde journal of cultural critique published the paper (No. 46-47, 1996). Sokal's later explanation for why he perpetrated this hoax makes interesting reading on the gap between scientists and their constructionist critics. See *Lingua Franca*, May/June and July/Aug. 1996. See also Steven Weinberg, 'Sokal's Hoax,' *New York Review of Books*, August 8, 1996.
 29. **Steven Weinberg**, *Dreams of a Final Theory* (New York, 1992), p. 29.
 30. For a rare critique of constructionists for ignoring the view point of natural scientists, see Warren Schmaus et al, 'The Hard Program in the Sociology of Scientific Knowledge: A Manifesto,' *Social Epistemology*, Vol. 6, 1992. These critics believe that 'if a scientist does not accept or even recognize our [sociologists'] explanation of his or her actions, this is prima facie evidence of the inadequacy of the explanation.'
 31. Philip Kitcher, *The Advancement of Science: Science without Legend Objectivity without Illusions* (Oxford University Press, 1993), p. 104. For another work by a kindred spirit, see Peter Kosso, *Reading the Book of Nature: An Introduction to the Philosophy of Science* (Cambridge, UK, 1992). A strong realism that retains the notion of truth as constrained and validated by reality also appears in the writings of Ronald Giere, **Ernan McMullin**, **Dudley Shapere** and Richard Boyd.
 32. For recent works advocating a deflationary realism see, **Sergio Sismondo**, *Science Without Myth: On Construction, Reality and Social Knowledge* (Albany, 1996); Joseph Rouse, *Engaging Science: How to Understand its Practices Philosophically* (Ithaca, 1996).
 33. **Insisting** as Hans Reichenbach did that 'relation of a theory to facts [is] independent of the man who found the theory.' Quoted from Ronald Giere, 'Viewing Science,' *PSA* 1994, Vol. 2. Giere offers a brief but insightful social history of the rise of social

constructivism.

34. **Jeffery** Alexander, *Fin de Siecle Social Theory: Relativism, Reduction and the Problem of Reason* (London, 1995).
35. Hilary Kornblith, 'Introduction: What is Naturalistic Epistemology?' In Hilary Kornblith (ed.) *Naturalizing Epistemology* (Cambridge, Mass, 1985).
36. The logical positivism of the Vienna circle was the leading trend among the **first generation** of philosophers of science and reined supreme until around 1960. The historicists, including Thomas Kuhn, Paul Feyerabend and Stephen Toulmin constituted the zeitgeist of the **second generation**. The **third generation** of science scholars are reacting both to positivism and historicism and 'seeking a dialectical synthesis' of the two. See Werner **Callebaut**, *Taking the Naturalistic Turn: or How Real Philosophy of Science is Done* (Chicago, 1993). For a contrast between traditional and radical sociological naturalism, see Philip **Kitcher**, 'The Naturalists Return,' *The Philosophical Review*, Vol. 10, 1992.
37. **Thomas Nagel**, *The View From Nowhere* (Oxford, 1986), p. 11
38. As Susan Haack puts it, 'science is not *simply* a social institution like banking or fashion industry, but a social institution *engaged in inquiry*, attempting to discover how the world is, to devise explanatory theories that stand up in the face of evidence.' see Haack, 'Toward a Sober Sociology of Science,' *Annals of the New York Academy of Science*, Vol. 775, June 1996. (emphasis in the original) See also, Hilary Kornblith, 'A Conservative Approach to Social Epistemology,' in Frederick Schmitt (ed.) *Socializing Epistemology*, 1994.
39. Barry Barnes, *Interests and the Growth of Knowledge* (London, 1977), p. 24.
40. Robert **Merton**, *The Sociology of Science* (Chicago, 1973).
41. **Steve Shapin** and **Simon Schaffer**, *Leviathan and the Air Pump* (New Jersey, 1985), p. 15.
42. Michel Foucault, 'Truth and Power,' (ed.) *Power/knowledge* (Brighton, 1980), p. 118. For constructivist literature on boundary-work, see Rouse, *Engaging Science* and Gieryn, 'Boundaries of Science,' in *Handbook of STS*.
43. **Roy Bhaskar**, 'Feyerabend and Bachelard: Two Philosophies of Science.' *New Left Review*, 94, 1975.
44. **Barry Barnes** and **David Bloor**, 'Relativism, Rationalism and the Sociology of Knowledge,' in Hollis and Lukes (eds.). *Rationality and Relativism* (Cambridge, Mass. 1982). The phrase 'normative anemia' is borrowed from Joseph Rouse, *Engaging Science*.
45. Barry Barnes, 'Realism, Relativism and Finitism,' in D. Ravenet **al**, *Cognitive Relativism and Social Sciences* (New Brunswick, 1992), p. 133-134.
46. **Richard Boyd**, 'Constructivism, Realism and Philosophical Method,' In John **Earman** (ed.) *Inference, Explanation and Other Philosophical Frustrations: Essays in the Philosophy of Science* (Berkeley, 1992).
47. The new union has many names: pragmatic theory of truth, the coherence theory of truth, the deflationary theory of truth. None of them, as the recent survey by Frederick Schmitt shows, escapes the problem of **relativizing** truth to a system of belief. See his *Truth: A Primer* (Boulder, CO, 1995).
48. H.M. Collins and Steven **Yearley**, 'Epistemological **Chicken**,' in Andrew Pickering (ed.), *Science as Practice and Culture* (Chicago, 1992), p. 303.
49. **Attributed** to Robert Young. Quoted here from Sal Restivo, 'The Theory Landscape in Science Studies,' *Handbook of STS*.
50. **Bloor** urges sociologists of science to 'only . . . proceed as the other sciences proceed, and all will be well.' See his *Knowledge and Social Imagery*, p. 157.
51. **But** as Larry **Laudan** argues, the **SP's** reading of value neutrality of scientific methodology clearly runs against the best established precedents in natural science because natural scientists routinely use different models and causal mechanisms to explain related but **different** phenomena. **Larry Laudan**, 'The Pseudo Science of Science?' *Phil. Soc. Sci.*

Vol. 11 (1981).

52. Barry Barnes, *Interests and The Growth of Knowledge* (London, 1977), p. 25.
53. For a bibliography, see Steve Shapin, 'History of Science and its Sociological Reconstruction,' *History of Science*, 1982, Vol. xx: 157–211.
54. I do not mean to suggest that feminist critiques of science are derived from sociology of science in general, or the SP in particular. The two have independent, although mutually supportive, trajectories, which have only now begun to explicitly come together at some points. Feminist and social-cultural critics of science increasingly use and cite each other's work to support their distinctive agendas. Feminist scholars of science have tried to distance themselves from the relativist implications of the SP and don't seem to have made much use of the Paris School (at least so far.)
55. Sandra Harding, *Whose Science? Whose Knowledge?* (Ithaca, 1991), p. 138.
56. *Ibid.*, p. 12.
57. Helen Longino provides the most rigorously argued case for the necessity of the social for understanding the cognitive processes of science. See her *Science as Social Knowledge* (Princeton, 1990). Longino uses the SSK literature to bolster her case that observation and justificatory reasoning are social; see her 'The Fate of Knowledge in Social Theories of Science,' in Frederick Schmitt (ed.) *Socializing Epistemology*. Likewise, Evelyn Fox-Keller cites the findings of the SP to argue that 'on every level, choices are social even as they are cognitive and technical.' See her *Secrets of Life, Secrets of Death: Essays on Language, Gender and Science* (New York, 1992), p.26.
58. See Knorr-Cetina and Michael Mulkay, 'Introduction,' *Science Observed* (California, 1983). Prominent feminist science critics, especially Sandra Harding, Evelyn Keller and Vandana Shiva simultaneously treat the content of science as constituted by social location (as sanctioned by the SP) and yet deny that they are arguing for just another story from women's lives: rather, they present feminist knowledge to be superior – i.e., more objective – as compared to 'malestream' science.
59. Barnes and Bloor, 'Relativism, Rationalism and the Sociology of Knowledge,' in Hollis and Luke (eds.) 1982, p. 23.
60. David Bloor, *Knowledge and Social Imagery*, p. 158.
61. *Ibid.*, p. 40.
62. *Ibid.*, pp. 38-39.
63. Barry Barnes, *Interests and the Growth of Knowledge*, p. 2.
64. Barry Barnes, 'Natural Rationality: A Neglected Concept in the Social Sciences,' *Phil. Soc. Sci.* Vol. 6, 1976: 115-126., p. 124
65. Barry Barnes, 'Realism, Relativism and Finitism,' in Raven et al, *Cognitive Relativism*.
66. Steve Shapin, *The Social History of Truth: Civility and Science in the Seventeenth Century* (Chicago, 1995), p. 5
67. Barnes and Bloor, in Hollis and Lukes, p. 27.
68. *Ibid.*, p. 29.
69. For a clear statement regarding difference between historicism and postmodernism, see Linda Nicholson's introduction to her anthology, *Feminism/Postmodernism* (New York, 1990), p. 4.
70. Bruno Latour, 'One More Turn After the Social Turn...' in Ernan McMullin (ed.) *The Social Dimension of Science* (Notre Dame, Indiana, 1992). Latour first described his 'actor-network model' in his *Science in Action: How to follow scientists and Engineers through society* (Cambridge, Mass, 1987).
71. *Ibid.*, 'One More Turn . . .', p. 280.
72. Military metaphors abound in Latour's writings. One wonders why those who have seen metaphors as vehicles of masculine values in science have accepted Latour's metaphors so uncritically.
73. Latour's favourite example of this Machiavellian process is Louis Pasteur's work on anthrax: because he had the laboratory, Pasteur could win the anthrax bacillus to his side

- and was able to translate the interest of the farmers in ~~terms~~ that coincided with his own interest in establishing his authority and the interest of the bacillus. In this process, **Latour** argues, both nature and society were changed simultaneously: the society had to accept a new reality of the microbe and had to go through Pasteur's lab to find a solution.
74. See Harry Collins and Steve Yearley, 'Epistemological Chicken,' in Andrew **Pickering** (ed.) *Science as Practice and Culture*. Chicago, 1990. See also, Barry Barnes, 'How Not To Do Sociology of Science,' *Annals of Scholarship*, Vol. 8, 1991.
 75. See especially, *German Ideology* (New York, 1991), p. 62-63.
 76. **Barry Barnes**, 'How Not to! p. 331, emphasis added.
 77. The term 'fig leaf realism' was coined by Michael Devitt to describe 'an idle addition to idealism!' Quoted from Robert Nola, 'Introduction,' to his *Relativism and Realism in Science*, 1989.
 78. **Bloor's** 'Afterword' to the second edition of *Knowledge and Social Imagery*. See also, Barry Barnes, David Bloor and John Henry, *Scientific Knowledge: A Sociological Analysis* (Chicago, 1996).
 79. This summary of **finitism** is from Barnes, 'Realism, Relativism and Finitism,' in Raven et al, 1992, Barnes, 'On the Conventional Character of knowledge and Cognition,' in Knorr Cetina and Michael Mulkey (eds.) *Science Observed* (California, 1983), and David Bloor, 'Durkheim and Mauss Revisited: Classification and the Sociology of Knowledge,' *Stud. Hist. Phil. Sci.* Vol. 13 (4), 1982.
 80. Bloor, *ibid*, p. 280.
 81. Karin **Knorr Cetina**, 'Strong Constructivism – from a Sociologist's Point of View: A Personal Addendum to Sison's Paper.' *Social Studies of Science*, vol. 23, 1993: 555-63. p. 558.
 82. K. Knorr Cetina, 'The Ethnographic Study of Scientific Work: Towards a Constructivist Interpretation of Science,' in Knorr Cetina and Michael Mulkey (eds.) *Science Observed* (California, 1983).
 83. As Frank **Farrell** argues, the contemporary social constructionist nominalism is not very different in its motivation from the medieval nominalism. For medieval theologians, 'a world where things have their proper natures and proper modes of actualization seemed to place undesirable limits on God.' With modernity, human subjectivity took the place of God but retained the anti-realist orientation toward the world. With postmodernism, subjectivity itself has become nominal, further diminishing the role of the real in fixing our beliefs. See, **Farrell**, *Subjectivity, Realism and Postmodernism*.
 84. Harding, *Whose Science?* p. 147 (emphasis added.)
 85. **Evelyn Fox-Keller**, *Secrets of Life*, p. 5. For a recent critical review of **Keller**, see Alan Soble, 'Gender, Objectivity and Realism,' *Monist*, Vol. 77, 1994.
 86. Ernest **Gellner**, 'The Paradox in Paradigms,' *Times Literary Supplement*, April 23, 1982.
 87. See Wesley Salmon, *Four Decades of Scientific Explanation*. Minneapolis, 1989, **Ernan McMullin**, 'Enlarging the Known World,' in Jan Hilgevoord (ed.), *Physics and Our View of the World*, (Cambridge, 1994). Strong arguments for existence of natural kinds based upon 'cluster of properties determined by causal structures of the world' have been provided by Richard Boyd, 'What Realism Implies and What it Does Not,' *Dialectica*, 43 (1989).
 88. Ronald Giere, 'Toward A Unified Theory of Science,' in James **Cushing**, C.F. Delaney and G. Gutting (eds.) *Science and Reality: Recent Work in Philosophy of Science* (Notre Dame, 1984), p. 15.
 89. Knorr Cetina, 'Laboratory Studies' in Handbook of STS, p. 146. Knorr Cetina justifies lab studies thus: 'detailed description deconstructs – not out of an interest in critique but because it cannot but observe the intricate labor that goes into the creation of a solid entity, the countless nonsolid ingredients from which it derives, the confusion and negotiations that lie at its origin . . . ' p. 146.
 90. **Bruno Latour** and Steve **Woolgar**, *Laboratory Life: The Construction of Scientific Facts*,

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- Princeton, 1986 (first edition 1979), p. 64. For an excellent description and critique of **L&W's** project, see Ian Hacking, 'The Participant **Irrealist** at Large in the Laboratory,' *Brit. Jour. of Phil. Sci.* 39: 277-294.
91. **Latour** and **Woolgar**, *ibid.*, p. 64.
92. Hans Radder, 'Science, Realization and Reality: The Fundamental Issues,' *Stud. Hist. Phil. Sci.* Vol. 24(3), 1993.
93. See Roy Bhaskar, 'Feyerabend and Bachelard,' p. 53.
94. For a not so recent but illuminating review, see Ian Hacking, 'Philosophers of Experiment,' *PSA*, 1988.
95. **Harry Collins's** EPOR is also sometimes referred to as the Bath school, **after** the University of Bath, where Collins teaches. EPOR serves as the hub of a network of well-known sociologists in constant traffic with each other and with the strong programme. Included in this tradition are the sociological history of quarks by Andrew Pickering, study of solar neutrinos by Trevor Pinch and the most well-known of all, Steve Shapin and Simon Schaffer's sociological history of the development of experimental method by Robert Boyle.
96. **Roth** and **Barrett**, *ibid.*, James R. Brown, *The Rational and the Social* (New York, 1989); Alan Chalmers, *The Fabrication of Science* (Minneapolis, 1990). For more recent works that admit more constraints by nature see Peter **Galison**, *How Experiments End* (Chicago, 1987), **Allan Franklin**, *Experiment, Right or Wrong* (Cambridge, 1990). Later works by Pickering also move toward recognizing 'resistance' from the world. See Andrew Pickering, 'Objectivity and the Mangle of Practice,' *Annals of Scholarship*, Vol. 8, 1991
97. **Harry Collins**, 'What is TRASP? The Radical Programme as a Methodological Imperative.' *Phil. Soc. Sci.* Vol. 11, 1981.
98. *Ibid.*, p. 148.
99. **Pickering**, quoted from Roth and Barrett, 'Deconstructing Quarks,' p. 594.
100. Collins, *Changing Order*, p. 185.
101. For details, see Gerard Darmon, 'The Asymmetry of Symmetry,' *Social Science Information*, 25(3): 743-755.
102. **Notably**, David Stump, 'From Epistemology and Metaphysics to Concrete Connection,' in Peter **Galison** and David Stump (eds.) *The Disunity of Science: Boundaries, Contexts and Power* (Stanford, 1996) and Peter Kosso, *Reading the Book of Nature*.
103. **Epigraph** from Knorr Cetina, *The Manufacture of Knowledge: An Essay on the Constructivist and Contextual Nature of Science* (Oxford, 1981).
104. Barnes and **Bloor**, 'Relativism, Rationalism and Sociology of Knowledge,' in Hollis and Lukes (eds.), p. 29
105. See David **Bloor**, 'Durkheim and Mauss Revisited,' for his interpretation of Boyle's laws as congruent with his conservative political ideas; **Barry Barnes** and Steve Shapin, *Natural Order*, Beverly Hills, 1979, Steve Shapin, 'History of Science and its Sociological Reconstruction,' *History of Science*, Vol. xx, 1982 for a review.
106. David Hess, *Science and Technology in a Multicultural World: The Cultural Politics of Facts and Artifacts* (Columbia University Press, 1994).
107. Evelyn **Fox-Keller**, *Reflections on Gender and Science* (New Haven, 1985); especially part III. **Keller's** book has the status of a classic among feminist science critics. For a more recent defence of constructionist ideas, see **Keller**, 'Science and Its Critics,' *Academe*, Sept./Oct. 1995.
108. Longino, *Science as Social Knowledge*, p. 43. Longino distinguishes her approach from SSK by distancing her 'contextual empiricism' from **SSK's** **wholist** assumptions which tend to explain theory change in terms of gestalt switches and incommensurability. But see her 'Fate of Knowledge in Social Theories of Science' in Schmitt (ed.), *Socializing Epistemology*, 1994, where she describes her affinities with science studies literature in insisting that cognitive is social.
109. Longino, *Science as Social Knowledge*, p. 189.

110. *Ibid.*, p. 193.
111. Longino enumerates six epistemic and political virtues of 'doing epistemology as a feminist' in her 'In Search of Feminist Epistemology,' *The Monist*, Oct. 1994, Vol. 77. See also her recent 'Gender, Politics and Theoretical Virtues,' *Synthese*, 104: 383-397, 1995, for elaboration of feminist values for **accepting** theories and models.
112. McMullin, Ernan, 'The Rational and the Social in the History of Science,' in J.R. Brown (ed.) *Scientific Rationality: The Sociological Turn* (Dordrecht, 1984). PUS can also stand for the 'presumption of ultimate sociality,' for SSK not only insists that social explanation be sought for all aspects of science but also that ultimately, in the final instance, it is the social that explains the cognitive.
113. See Thomas Nickles, 'Good Science as Bad History: From Order of Knowing to Order of Being,' in Ernan McMullin (ed.) *Social Dimension of Science* (Notre Dame, 1992).
114. See Francis Crick, *What Mad Pursuit* (New York, 1988). See also Paul Gross and Norman Levitt, *Higher Superstition*, p. 141.
115. See Oliver Sacks, 'A New Vision of the Mind,' in John Cornwell (ed.) *Nature's Imagination* (Oxford, 1995). See also Gross and Levitt, *Higher Superstition*, for the compatibility between Edelman's model and the linear cascades of hormone action, p. 147.
116. Gerald Edelman, 'Memory and the Individual Soul: Against Silly Reductionism,' in Cornwell (ed.) *Nature's Imagination*.
117. *Ibid.*, p. 201.
118. Dudley Shapere. *Reason and the Search for Knowledge* (Dordrecht, 1984), p. xxii.
119. Those among constructionists who find Kuhn's ideas congenial to theirs tend to read his work rather selectively, for Kuhn offers one of the sharpest arguments against the presumption of unrestricted sociality. While Kuhn admits that 'early in the development of a new field, social needs and values are a major determinant of the problems on which its practitioners concentrate,' he insists that as the field of inquiry matures, 'the problems on which specialists work are no longer presented by the external society but by an internal challenge. . . and the concepts used to resolve these problems are normally close relatives or those supplied by prior training. . . In short, compared with other professional and creative pursuits, the practitioners of mature science are effectively insulated from the cultural milieu in which they live their extra-professional lives.' *The Essential Tension* (Chicago, 1977), p. 119.
120. Susan Haack, 'Science as Social? – Yes and No,' in Jack Nelson and Lynn Hankinson Nelson (eds.) *A Dialogue on Feminism, Science and Philosophy of Science* (The Netherlands, forthcoming).
121. Latour, *Science in Action*, p. 12.
122. *Ibid.*; and Michel Callon, 'Four Models for the Dynamic of Science,' in Jasonoff et. al (eds.) *Handbook of STS*.
123. Stephen Fuchs and Steven Ward, 'What is Deconstruction, and Where and When does it take place? Making Facts in Science, Building Cases in Law,' *American Sociological Review* Vol. 59, 1994.
124. Bruno Latour, 'Clothing the Naked Truth,' in Hilary Lawson and Lisa Appignanesi (eds.) *Dismantling Truth: Reality in the Postmodern World* (NY, 1989).
125. For a classic statement of the distinction between epistemic and judgmental relativism, see Karin Knorr Cetina and Michael Mulkay's 'Introduction,' *Science Observed*, 1983.
126. See Longino, *Science as Social Knowledge*, p. 62 for a description of this distinction.
127. Keller, *Secrets*, p. 5. (emphasis in the original).
128. Meera Nanda, 'Science Wars in India,' *Dissent*, Winter 1997.
129. Thomas Nagel, *The View from Nowhere*, p. 5.
130. Restivo, *Science, Society and Values*, p. 61.
131. Longino, *Science as Social Knowledge*, p. 191.
132. *Ibid.*, p. 214.

133. For a thoughtful discussion of why an objectivist should welcome pluralism, See Noretta Koertge, 'Ideology, Heuristics and Rationality in the Context of **Discovery**,' in S. French and H. Kamminga (eds.) *Correspondence, Invariance and Heuristics* (The Netherlands, 1993).
134. Longino, *Science as Social Knowledge*, p. 193.
135. Harding, *Whose Science?* p. 10. The complete quote reads: 'Science is politics by other means, and it also generated reliable information about the empirical world. Science is more than politics of course, but it is that.'
136. See Sal Restivo, *Science, Society and Social Values*. Restivo argues for what he calls the 'weak program' in SSK which would aim at not just explaining science but critiquing science and the social arrangements it is constituted with.
137. David Hull, *Science as Process* (Chicago, 1988).
138. **Both** examples are from Noretta Koertge, *op. cit.*
139. Critiques of standpoint are legion. For a critique from a Marxist perspective, see **Bat-Ami Bar On**, 'Marginality and Epistemic Privilege' in Linda **Alcoff** and Elizabeth Potter (eds.), *Feminist Epistemologies* (New York, 1993); Rosemary Hennessy, *Materialist Feminism and the Politics of Discourse* (New York, 1993). For an incisive philosophical critique see Cassandra **Pinnick**, 'Feminist Epistemology: Implications for Philosophy of Science,' *Philosophy of Science*, Vol. 61, 1994.
- For a critique of Longino's transformative criticism, see Sharon **Crasnow**, 'Can Science be Objective? Longino's Science as Social Knowledge', *Hypatia*, Vol. 8, 1993 and Philip **Kitcher**, 'Socializing Knowledge.'
140. Quoted from C.F. Delaney, *Science, Knowledge and Mind: A Study in the Philosophy of C.S. Pierce* (Notre Dame, 1993).
141. Norman **Geras**, *Solidarity in the Conversation of Humankind*, p. 107.