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January 1999

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UNDERSTANDING SCIENTIFIC PRACTICES: CULTURAL STUDIES OF SCIENCE AS A PHILOSOPHICAL PROGRAM

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My subtitle should induce some uneasiness. Perhaps the most striking point of agreement between the recently emergent interdisciplinary field of cultural studies and the mainstream of contemporary anglophone philosophy is that cultural studies does not constitute a philosophical program. Practitioners of cultural studies typically explore the production or emergence of meaning within historically specific and localizable material settings. Cultural studies thus express a resolute historical and social particularism, even when the particular context of meaning-production that they examine is global in scale. Contemporary philosophers, by contrast, still frequently aspire to quite general theories of meaning, thought, knowledge, and action. Cultural studies are usually undertaken from an explicitly politically engaged position; philosophers more frequently aspire to what Thomas Nagel (1986) has called "the view from nowhere." Finally, philosophical theorists have most often cast their projects as allied with or even subsumed by a resolutely naturalistic understanding of the world. As Quine, perhaps the most influential American philosopher of the century, memorably said, "philosophy of science is philosophy enough." No analogous claim could plausibly be made about cultural studies, whose relationship to the natural sciences has been considerably more troubled than that of the "analytic" tradition in philosophy.

Yet I intend that subtitle quite seriously. The theoretical commitments expressed by work in cultural studies, especially cultural studies of science, offer a compelling response to a central question running throughout much of 20th Century philosophy: how does the richly meaningful, normative field of human activity ever successfully connect to "nature," the seemingly semantically inert inexorability of physical regularities or causal processes? What cultural studies can show is that in posing the question in this way, philosophers have displayed a debilitatingly dualist conception of nature and the intentionality of meaningful thought, language, or action. A "dualism" in this sense is "a distinction whose components are [conceived] in terms that make their characteristic relations to one another ultimately unintelligible" (Brandom 1994: 615).

It may go against the grain, however, to look to the politically engaged practice of cultural studies to reconnect this dualism. The more intimate one's engagement with power relations and social criticism, the more remote one may seem to be from the world disclosed by the sciences, and from the material environment within which people act. A long tradition in philosophy, which often still seizes the imagination, proclaims that one only encounters nature in its brute causal unfolding by turning away from human cultural significance and political stakes. Things must be allowed to manifest themselves as they are, were, and always will be, except to the limited extent that our own similarly brute causal powers can effectively rearrange them. Moreover, such an encounter with independent causal capacities may seem to be the

only non-arbitrary constraint upon the inventive and constructive powers of imagination and human cultures.

These intuitions undoubtedly help account for the persistence and even dominance of representationalist construals of intentionality and knowledge. Such accounts begin with the separate constitution of representational content and of the natures or capacities of things represented, and ask how they can ever make appropriate contact. In fact, the most familiar philosophical answers are that they cannot effectively bear on one another. Typically, philosophers undertake either an anti-realist strategy (representations or practices of inquiry do not respond to things as they really are apart from us, but only to their manifestations in experience or social practices), or else a naturalist, realist strategy (representational content is reduced to its causal/functional role already within the natural world). Anti-realists then have trouble accounting for how experiences, actions, and social practices or institutions are accountable to the world, except through the brute, unintelligible resistance of empirical anomalies and practical failures. Realists have similar difficulties understanding how the modally robust causal powers and rigidly designated natural kinds that supposedly determine the content of thoughts, utterances, and actions are normatively binding upon human agents, except through the brute, unintelligible success or failure of some of their projects.

Once the normative and the natural or causal dimensions of the world are thus conceived incommensurably as self-enclosed domains, however, they can never effectively be brought back together. We must understand causality as always already normative, and normativity as always already causally efficacious. It is here I turn to cultural studies; their most important contribution is to insist that meaning emerges within practices, normatively configured interaction with material surroundings. Cultural studies of science play a pivotal role here because of the importance of scientific practices within the contemporary dualism of intentionality and nature. If Descartes' account of mind and body is the paradigmatic dualism, then scientific practices are the pineal gland of contemporary philosophy: the proposed site for the magical reconciliation of what has been conceived irreconcilably. Through the practice of science, supposedly, the brute unintelligiblity of nature becomes intelligible. In surpassing such a dualism, cultural studies of science might restore our ability to recognize and understand the accomplishments of the sciences, without having to praise them for squaring the circle.

Practice talk has in fact been rampant within late twentieth century philosophy, social theory, and science studies. Amidst its proliferation, however, there has been too little attention to divergent uses of the term. Attending to scientific practices will help us only if we understand 'practices' in the right way. My discussion proceeds in four parts. First, I briefly note some pitfalls posed specifically by the extension of the concept of 'practice' to encompass scientific practices. Second, I explore a crucial ambiguity in the concept of 'practices' manifest in recent

critical discussions of the concept by Stephen Turner (1994) and Steve Fuller (1989, 1992). The third part of the paper all too briefly takes up some philosophical implications of the conception of practices that I attribute to cultural studies, a conception whose very possibility Turner and Fuller overlook. Specifically, this conception of practices challenges familiar reifications of language, knowledge, and power, and encourages attention to the temporality of scientific practices and their meaning, justification, and effects. Finally, I respond to Fuller's explicit worry that cultural studies of scientific practices must inevitably be politically and scientifically conservative.

I--Initial Clarifications

A first crucial mistake to avoid is reading the concept of scientific practices in terms of a distinction between theory and practice. This mistake is especially tempting because one of the most prominent uses of the term 'practices' in science studies has been to emphasize the autonomy of experimental and instrumental practices from theoretical determination. In contrast to the widespread insistence upon the theory-ladenness of observation and methodology which emerged from the post-empiricism of the 1960's and 1970's, it has since frequently been argued that "experiment has a life of its own," and similarly that the development and use of scientific instruments cannot be fully accounted for in terms of their supposed theoretical presuppositions.[1] The target of these criticisms, however, was an overarching conception of theory as representation. Thus, an important complement to the rediscovery of experimental practices has been to recognize that theorizing is also misconstrued as the development of static representational structures. Scientific theory is better understood in terms of theoretical practices: modeling particular situations or domains; articulating, extending, and reconciling those models and their constituent concepts and techniques; and connecting theoretical models to experimental systems. Such a conception of theorizing diverges both from the classical sense of theoria and more recent analyses of theories as axiomatic or model-theoretic systems.[2]

A second mistake would be to understand the turn to practices as an account of the individual or collective doings of scientific researchers, as distinct from the material setting of what they do. Such an interpretation would take 'practices' as an alternative account of scientific subjectivity. Instead of understanding

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[1] Among the most prominent contributors to this experimentalist philosophy and historiography of scientific practices are Hacking 1983, Ackermann 1985, Franklin 1986, Galison 1987, Gooding 1990, and Gooding, Pinch, and Schaffer 1989.

[2] Useful introductions to understanding theorizing as practices include Hacking 1983, ch. 12, Cartwright 1983, Rouse 1987, pp. 30-40, 80-95, 111-19, Rouse 1996 127-33, 166-78, 224-33, and Krieger 1992.

knowing subjects as disembodied minds representing the world, the idea would be to take them as embodied agents acting upon or within it. This idea is attractive, and up to a point, not mistaken. But it founders when one asks whether the doings of such agents are intelligible and coherent if considered apart from their material setting. When understood as mental representation, subjectivity might have a kind of theoretical autonomy from the world it represents; we have can grasp what it would be to (purport to) represent in the absence of any object or situation successfully representd. Similarly, it might seem plausible to speak of socially instituted practical norms as swinging relatively free from their material instantiation.[3] The same cannot be said of practical activity. To open a door or pour a liquid in the absence of the door or the liquid can only be construed as pretending to act, performing some other action (reaching for a door or tipping an empty bottle, each of which engages a different material setting rather than none), or utterly failing to act at all in the requisite way.[4] Elsewhere (Rouse 1996, ch. 5), I have said more about how to begin the analysis of practices, including scientific practices, with the relational complex of materially situated activity rather than with agents' activities or their material setting considered apart from one another. For now, we should think of scientific practices as practical configurations of the world rather than as agents' performances abstracted from them.

A final danger to avoid at the outset is the construal of scientific practices as clearly bounded and closed off from an extra-scientific "context." Undoubtedly, we can identify more or less distinctively scientific practices by their relatively dense

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[3] Thus, Brandom 1994 and Haugeland 1998 take greeting practices as paradigm cases of socially instituted practices whose correctness or incorrectness depends solely upon what the relevant community accepts as a greeting correctly performed. In the end, however, I think that the difference between such "socially instituted" practices, and the "constitutive" practices that are normatively accountable to the world (as practically configured), is at most a difference of degree in the directness of their normative entanglement with their worldly "surroundings."

[4] I speak of "utter" failure, because there are all sorts of ways in which action can fail to be successfully completed (the door might be locked, missing a handle, too heavy, or too symbolically portentous for me to open it), but in the absence of an appropriate material setting, the action cannot even get started in a way that would allow for a failure of completion. By the end of the book, we will realize that this is true even for speech acts. If my vocalizations or other performances are not appropriately connected (perhaps by complicated inferential networks) to a suitable, publicly accessible material setting, it cannot be contentful or referentially indicative.

interconnections with other scientific practices, and by the issues and stakes that arise through those particular interconnections. Nevertheless, the traffic in all directions across whatever boundaries can be thus demarcated will be too heavy to allow for any significant autonomy of a domain of scientific practices.[5] The sciences depend upon their "context" not only for funding and material resources; recruits and auxiliary personnel; institutional location in universities, academies, corporations, bureaucracies, or foundations; social norms, cultural forms, and bodily disciplines; but also for much of what is ultimately at issue or at stake in scientific practices. On this last point, for example, it is instructive to ask how the sciences have come to be so concerned with origins, foundations, secrets, or laws.[6] Moreover, the extension of scientific practices and achievements into new settings cannot always be dismissed as merely the subsequent "application" of antecedently intelligible achievements. The intentionality of scientific practices is often only fulfilled by such extensions beyond the laboratory and outside the field. How, after all, does the manipulation of successive generations of inbred Drosophila come to be about genetics, or the incorporation of radioactive amino acids in rat liver slices come to be about protein synthesis? The spatiotemporal extension of scientific practices and materials beyond the laboratory is often the vindication of the semantic extension of scientific concepts.[7]

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[5] The reverse is also true, that is, that what is left of the social or cultural "world" apart from scientific practices is not autonomously intelligible. It is a truism to note the importance of scientific practices and achievements within the "modern" world, but this truism is often neglected in practice. If as a thought experiment, for example, one were to conceive the image of the social and cultural world constructable by an imaginary overlay of the social science and humanities curricula of most Western universities, one of the more striking divergences between this image and the world it aspires to make intelligible would likely be a seriously truncated view of the natural sciences as sociocultural and material practices.

[6] Mario Biagioli's (1993) discussion of Galileo's articulation of a new role as a courtly natural philosopher shows this point especially clearly by the constraints that emerged within absolutist court culture upon what could be at stake in natural philosophical controversies. Subsequent discussion of the significance of origins, foundations, secrets, and laws can be found in Haraway 1989, Keller 1992, and Cartwright 1994.

[7] This general issue of the intentionality of experimental systems is discussed in Rouse 1987, ch. 4, 7, and 1996, part II. The examples of Drosophila genetics and in vitro protein synthesis are extensively considered in Kohler 1994 and Rheinberger 1997 respectively, and the latter also extends

Scientific practices are often construed to be apart from any surrounding "culture," and even free from culture,[8] but such construals are simply not adequate to the rich and complex phenomena of scientific work.

II--Practices as Normative

With these clarifications in mind, we can now turn to an ambiguity in the concept of 'practices' that emerges from Turner's and Fuller's critical discussions of practice theories. An important philosophical rationale for practice talk comes from Wittgenstein's discussion of rule-following and Heidegger's account of understanding and interpretation.[9] The ability to follow a rule or to make an assertion seems to presuppose an unarticulated understanding-in-practice in order to halt regresses of explicit interpretation and justification. If understanding requires recourse to rules, after all, how does one correctly understand a rule?. But Turner objects to any attempt to avoid such regresses by situating explicit understanding against a background of practices, arguing that the appeal to practices to account for regularities, continuities, and commonalities in social life is pseudo-explanatory. According to Turner, the inference from common behavior to its supposedly underlying source in shared presuppositions or practices cannot be justified, the causal powers of practices are inevitably mysterious, and the transmission or reproduction of practices over time and from one practitioner to another cannot be accounted for. Fuller's criticism echoes Turner's objections to practice talk as pseudo-explanatory, and adds a political dimension: he argues that recourse to the Geisteswissenschaftliche interpretation of tacit understanding is deeply conservative and anti-democratic, an argument that is buttressed at least ad hominem by reflection upon the political commitments of Heidegger and Wittgenstein, philosophical ancestors of the practice industry.[10]

I take Turner's criticisms very seriously, but argue that he has misunderstood their significance. Turner concludes from his arguments that practice talk is altogether bankrupt in philosophy and social theory. His discussion instead reveals a fundamental ambiguity among uses of the term 'practices', between practices conceived as regularities and a normative conception of practices.

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earlier discussions of the general issue.

[8] Traweek (1988) interprets the self-understanding of the physicists she studied as their belonging to "the culture of no culture."

[9] Wittgenstein 1953; Heidegger 1962.

[10] Turner himself (1989) has argued that the ad hominem consideration of Heidegger's politics must be taken as a quite serious prima facie problem for any conception of practices that draws extensively upon Heidegger's work.

Turner only identifies one side of the ambiguous usage. I think his arguments are indeed telling objections to the conception of 'practices' (as regularities) that he actually addresses, but the upshot of the argument is instead to highlight the importance of conceiving practices normatively. My first task, then, is to consider the differences between conceiving practices as regularities, and conceiving them normatively. These differences concern what practices are, how they become evident, the significance of language within practices, and the sense in which practices are "social."

So what are practices? The question arises with some force, because of the diversity of things sometimes denoted by the term. Turner notes that 'practice' is variously interchangeable with 'tradition', 'tacit knowledge', 'paradigm', 'presupposition', and much more. Practices are sometimes regarded as tacit propositional attitudes and sometimes as inarticulable competences or performances. In either case, he claims, the concept of practices is typically invoked to explain continuities or commonalities among the activities of social groups. Turner argues that to do the explanatory work thus attributed to them, practices must be objectively identifiable regularities. If they are presupposed propositional commitments, they must have some "psychological reality"; if they are practical competences, they must have some causal efficacy. In either case, moreover, the content or pattern embodied in a practice must be transmissable in ways that would preserve its identity across practitioners. Turner ultimately rejects any explanatory appeal to social practices just because of the allegedly intractable difficulties in justifying the psychological reality, causal efficacy, or transmissable identity of any regularities "underlying" more readily manifest human activities.

Turner, however, fails to recognize the possibility of an alternative conception of a 'practice', in which something belongs to a practice if what it does is appropriately regarded as answerable to norms of correct or incorrect practice. Not all constituents of practices do the same things or presuppose the same beliefs, but some of them are subject to correction for performances that are inappropriate or otherwise incorrect. Of course, not all improprieties are actually corrected or sanctioned. So the differential responses that would signify the incorrectness of some performances are themselves normative practices. It is always possible that such chains of proprieties come to an end in some kind of objective regularity. But, as Brandom has noted, "we can envisage a situation in which every social practice of [a] community has as its generating response a performance which must be in accord with another social practice" (1979, 189-90). Such a network of practices need not be identifiable as regularities of action or belief, even as a whole. Brandom therefore argues that the difference between regularities and norms should itself be regarded normatively, that is, as a distinction between those patterns appropriately explained in causal terms, and those appropriately understood as subject to interpretation and normative

response.

Turner's own arguments against the integrity of practices conceived as regularities ironically often point toward such a normative conception of practices. Turner argues, for example, that Marcel Mauss's identification of distinctively French and American ways of walking could not easily be captured in terms of "culture-free causal categories," in part because "one might acquire the 'same' [external] walk by mimicking or by a kind of training which corrected various untutored walks--and corrected them in different respects--to produce a walk which is externally the same" (1994: 22). Moreover, Turner claimed, the description of practices typically depends upon classification schemes that presuppose acquaintance with other practices: such descriptions are only identifiable by contrast to other local, cultural expectations with which they conflict (1994: 24). They are only identifiable as practices at all against a background of other practices, and thus can never be reduced to objective regularities. Turner still fails to grasp a normative conception of practices as a genuine alternative, however; what other practice theorists would regard as normative responsiveness, he dismisses as merely an instrumentalist appeal to regularities (1994: 37).

Turner objects to such an instrumentalist conception of the appeal to regularities of social practice because it "fails to connect the stuff of thought to the world of cause and substance, ... [leaving] no basis for using our past understandings or interpretations to warrant future interpretations" (1994: 37). But this objection only makes sense if the domain of practices is conceived too narrowly, in two respects. On the one hand, it presumes that the "world of cause and substance" is somehow distinct from the "world" of meaningful practices, the "social world," such that the two are in need of reconnection. It also presumes, more subtly, that practices are distinct from linguistic representation: practices are ontologically suspect, whereas linguistic meaning and reference are not. I turn first to the relation between 'practice' and language.

Practice theorists have often been ambivalent about the significance of language for practices, and vice versa. On the one hand, unarticulated or even inarticulable practices are frequently contrasted to explicit assertions or rules. On the other hand, the domain of practice is often extended to incorporate conceptual or linguistic practices, perhaps even as the paradigm case of practices. Shared conceptual schemes or presuppositions are often the focus of practice talk. Once we recognize the difference between conceiving practices as regularities or normatively, however, we can see a fundamental distinction among two conceptions of linguistic (or discursive) practices. Those who identify practices with regularities (including shared beliefs or conceptual schemes) typically situate language outside the domain of practices; shared practices may account for particular beliefs or conceptual schemes that are expressible within language, but linguistic intentionality itself is then conceived in terms of a representational semantics instead of a pragmatics of discursive

practices. By contrast, a normative conception of practices is best understood as a general conception of intentionality. Brandom (1994) has most explicitly worked out such a conception of intentionality as altogether pragmatic, but once we are clear about the distinction, Heidegger and Wittgenstein, and more recently, Donald Davidson, are best understood as conceiving of intentionality as pragmatically normative normative rather than representational.

Such a pragmatic account of language, and of intentionality more generally, understands language dynamically, without reifying meanings, reference, or shared languages. Shared meanings or beliefs are not the preexisting facts that would explain the possibility of communication, but the norms presumptively invoked in the course of interpreting someone or something as communicative.[11] Only by interpreting a speaker as mostly making sense (within a field of linguistic and other practical proprieties that enable me to make sense of myself as making sense) can I acknowledge her activity (or my own) as discursive, and thus as linguistic. Note that this characteristic feature of Davidson's and Brandom's interpretive semantics (namely that truth, meaning, language, and other semantic categories can only be explicated via interpretation in an unanalyzed home language[12]) is precisely the

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[11] 'Interpretation' (or as Turner too narrowly construes it, "attributing assumptions") is here understood in a thoroughly pragmatic way, as adopting a practical attitude rather than offering an explicit account: I "interpret" someone as communicative simply by listening and responding in appropriate ways. A useful parallel is Heidegger's (1962) discussion of interpretation (Auslegung), in which one interprets something as a hammer by hammering with it; no explicit attribution of properties or meanings is required. An interpreter can, of course, try to make explicit the practical attitudes adopted in making sense of another speaker or agent, but such explications always come to an end in further unexplicated proprieties: this is what we do. This Wittgensteinian point is often misunderstood by taking "what we do" as a behavioral regularity, but that cannot be right in the light of Wittgenstein's discussion of rule-following. We appeal to "what we do" precisely to halt the regress of explications of a rule (whether the rule is supposed to govern the interpreted performances, or is only a regularity the interpreter takes them to exhibit). Such a regress cannot end in another regularity, but only in a propriety (to adapt an example from Samuel Wheeler, we should understand "this is what we do" in the sense of "we don't hit other children, do we?"). "What we do" always includes further practices of correcting deviant practice.

[12] The phrase "relative to an unanalyzed home (or background) language" is the canonical characterization of a point common to Quine, Davidson, Rorty, and Brandom. Strictly

feature of practices that most vexed Turner as unacceptably instrumentalist. Turner complains that,

> The assumptions [one] attributes to [another] are identifiable as assumptions only because [one] is in a position to make a specific comparison [to one's own understanding of the "same" situation]. Starting from a different comparison... would produce different misunderstandings, and different assumptions would need to be attributed.... Such 'assumptions', then, are not natural facts, but hypotheses that solve specific comparative problems. (1994: 33-34; emphasis mine)

But for Davidson, Brandom, Wittgenstein or Heidegger, linguistic practices are non-natural in precisely this sense.

With this background, we can now turn to Turner's worries about the connection between practices and "the world of cause and substance." Turner clearly takes practice theory to be objectionably anti-realist (either instrumentalist or social constructivist) about the causal powers and susceptibilities of practices. But, as I have argued on several occasions,[13] a normative conception of linguistic and other practices challenges the shared commitments of realists and anti-realists alike to a representationalist semantics. The attitudes and responses that identify a practice (including a discursive practice) are only contentful amidst ongoing intra-actions with the world.[14] We interpret utterances by making sense of what is said when, i.e., on which occasions, in what worldly circumstances. Thus, for Davidson (1984), we interpret utterances via prior acquaintance with their truth conditions; for Heidegger, interpretation is an aspect of being-in-the-world. To ask how our representations can ever get a foothold in the world is to presume, erroneously, that we can ever make or understand representations without already having a foothold in the world.[15]

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speaking, however, such views ought to hold no place for any determinate concept of a language, as Davidson (1986) came to recognize, for discursive practice always outruns any previously stable linguistic structure. The background to interpretation must therefore instead be unexplicated discursive practices, which Brandom (1994) models as scorekeeping practices.

[13] Rouse 1987, ch. 5; 1991; 1996, introduction and ch. 7-8.

[14] I follow Barad (1996) in adopting the term 'intra-action' as a substitute for 'interaction', to avoid the connotation that the things that interact have a determinate identity and character prior to or apart from their interactions.

[15] The classic philosophical source for this criticism of representationalism, of course, is the Introduction to Hegel's Phenomenology of Spirit (1972): the representationalism underlying both realism and antirealism is precisely what Hegel called "the fear of error that reveals itself as fear of the

Thus, if we take seriously a normative conception of practices, we must recognize that there is no such thing as "the social world" (or the "natural world") except as reified abstractions from the world.[16] The meanings, agency, institutions, or forms of life with which social constructivists would explain how nature becomes manifest to us are themselves senseless apart from those manifestations; they cannot be an independent explanans. But supposedly natural kinds and their causal capacities only acquire their constitutive counterfactual import from their normative application ceteris paribus within scientific practices of theoretical modeling and experimental manipulation.

III--Philosophical Consequences

So far, we have explored the distinction of a normative conception of practices from mere regularities of social life. I now want to consider, in a similarly programmatic way, some possible consequences of attending to scientific practices understood normatively, instead of as regularities of behavior or belief.[17]

One topic that becomes central when we take scientific practices as normative is their temporality.[18] Postempiricist philosophy of science has recognized the importance of historical changes in the methods, concepts, and standards of the sciences, whether construed as sharp, "revolutionary" breaks or as more gradual, reticulated developments. A central preoccupation of the philosophical responses to Kuhn and Feyerabend has been to understand scientific change in ways that do not render it rationally unintelligible (i.e., unintelligible as rational). But such interpretations of scientific change are not yet an adequate conception of the temporality of scientific practices. If one took scientific communities to share specific beliefs, values, conceptual contents, or activities, then it would make sense to ask how those communities and their commitments change over time. But such conceptions presume that there is an already determinate

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truth" (par. 74).

[16] Brandom (1994) still identifies practical proprieties as instituted within social practices, but in a sense he describes as an "I-Thou" model of social interaction rather than "I-We." It makes more sense to drop the term 'social', which inevitably has connotations of supraindividual entities. But neither should we think of Brandom's theory as one of individual intentionality. Who or what counts as an agent with "original intentionality" is itself a normative question, not a factual one.

[17] A more detailed discussion of some of these issues can be found in Part II of Rouse 1996.

[18] The temporality of scientific practices has also been a central concern in recent work by Andrew Pickering (1995) and Hans-Jorg Rheinberger (1994, 1997).

character to the community itself, and to the shared commitments that define its boundaries. Such determinacy cannot be presumed once scientific practices are conceived normatively, for such practices are constitutively temporally extended. What the practices are now depends in part upon how their normative force is interpreted in ongoing practice, that is, by how practitioners and their surroundings will or would respond to continuities or variations in what is done. But those future responses are not yet determined. The present content of scientific practices is thus subject to reinterpretation and semantic drift. For example, Rheinberger concludes from his historical study of the intertwining of research on oncogenesis, virology and the functional structure of cells that, "the virus of 1950 [depicted by Keith Porter's electron microscopy] must be seen as the condition of possibility for looking at [Peyton] Rous's [1910] agent as that which it had not been: the future virus" (1994:77). This point is ontological rather than epistemic: present semantic content is comparable to whether a goal in soccer is the game-winning goal, in being not yet settled by any facts. Understanding practices normatively helps us see why this is so: what a practice is, including what counts as an instance of the practice, is bound up with its significance, i.e., with what is at issue and at stake in the practice, to whom or what it matters, and hence with how the practice is appropriately or perspicuously described. The temporality of scientific practices is what Francois Jacob described as "a machine for making the future," (Rheinberger 1994) and hence their constitutive openness to genuine future disclosure cannot be adequately conceived in terms of scientific change.

Adequately accounting for the significance of scientific practices and its temporality requires dynamic accounts of language, knowledge, and power, or so I argued in Engaging Science. We can now see why dynamic or non-reified conceptions of language, knowledge, and power are mutually implicated. In criticizing Turner, I already pointed toward a conception of language as discursive practices, that is, as dynamic interactions among speakers and their surroundings. Linguistic practices are mediated not by conventional meanings, languages, or beliefs, but by partially shared situations, which have a history. One consequence of recognizing their dynamics highlights the importance of tropes, whose contrast class is not "literal" meanings, but familiar or uncontested uses.[19] Within the sciences, models (including mathematical, verbal, physical, pictorial or schematic, and experimental models) are especially important examples of tropes. We should think of models as simulacra rather than representations. The crucial difference is that 'representation' too often denotes a semantic content that intervenes between knowers and the world, whereas simulacra are just more things in the world, with a multiplicity of relations to other things. What makes them models,

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[19] The loci classici for such a conception of metaphor are Davidson 1984 and Wheeler 1991.

with an intentional relation to what they are models of, is their being taken up in practices, ongoing patterns of use that are answerable to norms of correctness.[20]

This constitutive role for proprieties and sanctions in linguistic interaction already shows the indispensability of concepts of 'power' and 'resistance' for understanding language. Whether an unfamiliar way of speaking about or dealing with a situation is taken as an innovation, a mistake, a curiosity, an irony, or a humdrum variation on the familiar depends crucially upon asymmetries of power among those who encounter it. Yet the recognition of models as simulacra extends the interconnection of meaning and power beyond the immediate relation between speakers and their interpreters. To see why this is so, consider a question sometimes asked rhetorically about meaning: how could merely representing things differently possibly have a causal influence on them? A similar question about simulacra cannot have the same rhetorical effect: simulacra are transformations of the world, and more significantly, they transform the available possibilities for acting, what it is intelligible to do. They do so both by materially enabling some activities and obstructing others, and also by changing the situation such that some possible actions or roles lose their point, while others acquire new significance.[21] Indeed, the most important philosophical role for the concept of power is to express how actions materially transform the world in ways that normatively reconfigure what can be at stake in subsequent actions.

So far, I have sketched how considerations of power might become relevant to conceptions of language and meaning as emergent from the normativity of discursive practices. If such an account were construed in terms of a reified conception of power, the result would be a reduction of meaning to rhetorical force, and language to a technology of persuasion. Such a move not unknown within science studies. Reducing meaning or significance to rhetorical or material effects is nevertheless a fundamental mistake. The mistake can be avoided if we also conceive of power dynamically, not as a regularity of social life, a thing possessed or exercised by dominant agents, but as a situated and temporally extended relationship among agents and their surroundings.

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[20] This concept of simulacra is discussed in Rouse 1996, ch. 8. The contrasting sense of 'representation' is indifferent to whether representations are conceived as thoughts accessible to individual minds, or as concepts, conceptual frameworks, languages, or forms of life shared by social groups.

[21] The sense in which scientific practices transform the available possibilities for human action is more extensively discussed in Rouse 1987, ch. 6-7. That discussion should be connected to my more detailed subsequent account (Rouse 1996, ch. 8) of both theoretical modeling and experimental systems as simulacra rather than representations.

Wartenburg (1990) developed a partial model for such a dynamic conception of power. He began by noting how power is mediated by what he calls "social alignments": one agent's actions effectively exercise power over another only to the extent that other agents' actions are appropriately aligned with the actions of the dominant agent. For example, judges exercise power over prisoners only if the actions of bailiffs, guards, appeals courts, and others are aligned in the right way with what the judge does. Power relations are dynamic, because the presence of an alignment, and its effectiveness, depend upon how the alignment is sustained or transformed over time, in response to subordinate agents' efforts to resist or bypass them as well as dominant agents' attempts to utilize, strengthen, or extend them. Power is thus dispersed and deferred across a field of possibilities. Wartenburg's model is only partial, because he mistakenly restricts the mediation of power to social alignments of human agents. A more adequate conception would recognize the materially circumstantial configuration of power relations, such that tools, processes, and physical surroundings more generally all belong to dynamic alignments of dominance, subordination, and resistance. Thus, just as practices should not be reduced to social practices, power should not be reduced to social power.

It may be initially more difficult, however, to grasp how to understand knowing without reifying knowledge. Dynamic accounts of language have helpful precedents in Davidson, Derrida, and Brandom; Foucault likewise makes familiar a dynamics of power. Yet Wartenburg's discussion of dynamic power alignments offers a useful analogue to a dynamic conception of knowing.[22] Power is only effective in enabling or constraining action through dynamic alignments that bring one action to bear upon another. Knowing is likewise only informative through dynamic alignments that enable one thing (a statement, a model, an image, a skillful performance, etc.) to be correctly taken to be about another. Philosophers of science nowadays emphasize the importance of "background knowledge" in establishing inferential relations between hypotheses and evidence, but that concept is too homogeneous and static. Knowing is mediated not just by a "background" of beliefs, but also by models, skills, instruments, standardized materials and phenomena, and situated interactions among knowers, in short, by practices. Moreover, a dynamic account of language as discursive practices obliterates any clear distinction between the representational content of knowledge, and its material or social construction or implementation.

The result is a deflationary conception of knowledge, modeled

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[22] The connection is more than just an analogy. Wartenburg's approach to power is deeply influenced by Foucault, whose reflections on the dynamics of power were introduced as an analytics of power/knowledge. Foucault's work of the 1970's suggests a dynamics of knowing, even though it is less extensively articulated than his correlative conception of power.

on deflationary, prosententialist conceptions of truth.[23] In the latter case, the truth predicate and the capacity to use it are recognized as indispensable to linguistic and epistemic practices, even though no underlying nature of truth unifies or reifies the instances of its appropriate application.[24] A deflationary account of knowledge likewise denies that 'knowledge' or 'scientific knowledge' constitutes a theoretically coherent kind. There are many appropriate ascriptions of 'knowing' within the multifarious practices of assessing, attributing, relying upon, or contesting understanding and justification, but there is no nature of knowledge underlying these ascriptions. This claim has far-reaching consequences: participation in the wholesale legitimation, explanation, or critique of scientific claims to knowledge almost invariably proceeds from a conception of scientific knowledge as a theoretically coherent concept that can be surveyed as a whole. It only makes sense to claim that scientific knowledge as a whole is approximately true, rationally arrived at, socially constructed, or interest-relative if there is such a (kind of) thing.[25] Scientific realists, historical metamethodologists, empiricists, and social constructivists regularly argue for their preferred accounts of the nature or goal of scientific knowledge, but typically and mistakenly take the unity and theoretical integrity of 'scientific knowledge' for

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[23] It is crucial to distinguish two oppositely directed uses of Tarski's schema for a deflationary (or semantic) conception of truth. For Tarski or Hartry Field, the semantic conception shows how 'truth' can be eliminated (at least for formal languages), and thereby rendered innocuous within a thoroughgoing naturalism or physicalism. Reduction, however, is inevitably a two-way street. Along with Davidson, Brandom, and others, I deploy the Tarski schema in the opposite direction, making 'truth' the fundamental semantic category, and dispensing with reifications of meaning (or convention), reference, or shared languages.

[24] The standard technical objections to deflationary conceptions of truth has been their proponents' inability to articulate what to say about the uses of the truth predicate in conditionally embedded, nominalized, or quantified contexts. Brandom's (1994) development of Grover, Camp, and Belnap's (1975) prosentential theory, and its extension to 'refers' as a "proform-forming operator," has resolved the principal technical problems, however, which ought to place a burden of argument upon those who would still call for a more robust and substantive conception of truth.

[25] Rouse (1996, Introduction and Part One) articulates and criticizes the commitments to the "legitimation project" that are necessary to make sense of the disagreements among scientific realists, historical metamethodologists, empiricists, and social constructivists.

granted.

IV--Locating Cultural Studies of Science

Conceiving of practices normatively rather than as composed of underlying social (or natural) regularities, and refusing to reify language, power, and knowledge, thus challenge many of the most familiar philosophical and sociological approaches to science studies. An important aspect of this challenge concerns the theoretical and political "location" of science studies themselves, often discussed under the heading of reflexivity. Philosophers and sociologists alike have typically attempted to achieve a standpoint of "epistemic sovereignty" (Rouse 1994), a theoretical position "outside" or "above" scientific practices from which to establish or undermine their legitimacy and authority once and for all. We can now appreciate the conceptual link between aspiring to epistemic sovereignty, and conceiving scientific practices as regularities. The various participants in the legitimation project hope to characterize the underlying regularities that most fundamentally govern scientific practices, whether these are socially, psychologically, or rationally predetermined goals, or causally efficacious features of the world. By thus ascertaining what science <u>really</u> <u>is</u>, its indispensable nature or goal, they would provide an incontrovertible ground for assessing its successes and failures, and its appropriate place among other human practices and achievements.

If we understand scientific practices normatively, however, no such determining standpoint above or outside of ongoing practice is available. Davidson's and Brandom's semantics offer a useful parallel; they take natural language as its own metalanguage, and ask that we explicate discursive practice from "within." We must likewise recognize "science" to incorporate its own interpretive and critical metapractices, and engage in epistemic and political explication and criticism from within. 'Science' should in this context be construed broadly as the <u>cultures</u> of science, the practical, discursive nexus that incorporates everyone and everything in relation to which 'science' is an intelligible and significant category, however contested.

This insistence that science studies are inescapably "internal" to the culture of science may then raise Fuller's principal worry, that an interpretive engagement with scientific practices is necessarily conservative.[26] Fuller (1992) claims that interpretive engagement with scientific practices must abandon any attempt to hold science accountable to norms not of its own choosing. But this criticism only shows Fuller's commitment to a conception of practices as (social) regularities: he limits "science" to the set of practices already conventionally recognized

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[26] Strictly speaking, this criticism would allow an alternative to a conservative continuity with current practice, namely an arbitrary imposition upon current practice, but this alternative is <u>unjustified</u>, because justification is conceptually linked to continuity with current practices and norms.

as science, its "norms" to the goals and standards to which its practitioners already subscribe, and he therefore concludes that critical perspective can only come from elsewhere. If one recognizes scientific practices as normative, what science or knowledge is is not already determined, but is instead at issue in scientists' and others' practical responsiveness to their circumstances. Various epistemically significant practices are normatively accountable, but such practices also offer competing interpretations of the epistemic and cultural/political norms within which they are situated. The interpretive resources for science studies thus include a multiplicity of sciences and metascientific discourses, together with various marginal and oppositional epistemic practices. Science studies do not come in from the "outside" to settle the differences among these co-existing and competing practices, but are already situated among them and engaged with them.

The significance of the critical resources for such engagement with the sciences can be seen by comparison to philosophical appeals to the inferential role of "background knowledge" in justification. In my terms, such appeals to background knowledge implicitly invoke a conception of practices as regularities. But there are not, and need not be, any such shared background commitments within science or the larger culture of science. The "background" to justification is not shared belief, but partially shared situations (which have a history and a futural orientation). We justify actions and interpretations by making sense of a presumptively shared situation. Indeed, on this account, part of what it is to act is simply to understand what one does as (defeasibly) making sense within a meaningful situation. There is thus a continuity between action and the explicit interpretation and justification of action: action takes place within the presumptive space of reason-giving and sense-making, and to act in a particular way is also implicitly to construe one's situation in a way that would make sense of one's action within it. Such interpretive construal has an essentially temporal or narrative dimension, because a (partially) shared situation is its history, including its future: we make sense of what we do by enacting narratives in which what we do has an intelligible place (Rouse 1997, ch. 6). To do science is also to construe its history, while to offer an account of its history (or its supposedly ahistorical nature) is to project particular ways to do science.

The futural dimension of the narrative reconstruction of science is crucial here. Actions and explicit reinterpretations of them do not merely construe the past, but also project a future. Such projections leave room for an explicitly utopian dimension of science studies (and scientific practices!), even while insisting upon the grounding of such utopian vision in its implicit past and present. Donna Haraway's work is especially instructive in this respect: it matters enormously that her critical reconstructions and utopian imaginings are responses to a history "we" share, even if that history needs to be reconfigured for us to recognize it as ours, and as potentially the past of such an imagined future (or

"elsewhere").[27] The critical significance of such narrative reconstructions arises both from their insightful reconfiguration of past and present, and from potential alignments with the activities of others that might help effect a future continuous with what they envision.

Such a conception of critical engagement within the culture of science nevertheless may raise a worry frequently associated with Frankfurt School Critical Theory. The worry is that power may be exercised not only to foreclose the effectiveness of political criticism, but also its rational legitimacy, if what is is permitted to govern the space of reason-giving. Fuller thus appropriately cites Marcuse's emphasis upon negative thinking in articulating his objection to the allegedly conservative character of Geisteswissenschaftliche philosophy and sociology of science.

One indispensable response to this worry is to acknowledge it. There is no guarantee that there will always be adequate resources for the articulation and realization of compelling epistemic and political criticism. Recognition that ongoing political and epistemological work is needed to sustain a space for critical reflection and political transformation is a crucial goad against complacency.[28]

There is also a more substantive response, however. Taking seriously the prospect of an overwhelming ideological hegemony that would foreclose the intelligibility of critical alternatives underestimates the diversity and contestedness of epistemic practices and their political significance. There will always be conflicting interpretations of ascendent scientific disciplines, as well as marginal and alternative ways of knowing, which have at least the potential to support critical perspectives upon dominant practices of justification. What is presently accepted as justification can never be finally secured against alternative interpretations, precisely because there are no self-certifying epistemic foundations immune from criticism. Moreover, the dynamics of linguistic meaning remind us that hegemonic ideologies are open to subversive readings, while social and material alignments of power are not self-maintaining. New forms of power and domination invite counter-alignments. If epistemic and political criticism must always be an intelligible response to past

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[27] Among the works that most clearly display the utopian dimension of her project are Haraway 1997, 1992, 1991 (ch. 8-10), 1989 (ch. 16).

[28] What one might have expected to be a theoretically and politically invigorating freeing of the imagination in the wake of the collapse of the dead weight of "actually existing socialism" in the annis mirabilis of 1989 has instead too often led to triumphant reinvocations of an end to ideology or even to history, countered only by half-hearted hopes to mitigate the worst effects of the inevitably disspiriting recurrence of our present political and theoretical circumstances.

and present practice, we must nevertheless remember that what past and present practice is includes its possible futures, and those have not yet been fully determined. An appropriate response to worries about irresistable power and seamless ideology is not to seek secure grounds for criticism, but to engage the specific forms of domination that trouble us, to articulate imaginative, insightful and effective criticisms of them, and to forge specific alignments and solidarities with others who share (or might come to share) such concerns.

Such a conception of the critical positioning of science studies calls for a thicker conception of reflexivity than has usually been articulated in the science studies literature. Within recent sociology of science, 'reflexivity' often marks a concern for a rhetorical consistency that would expose the construction of the authoritativeness of one's own texts and truth claims to the same critical scrutiny accorded to scientific texts and practices. In the hands of Steve Woolgar (1988) or Malcolm Ashmore (1989), for example, such scrutiny calls for inventive and playful writing that would undermine naive reading of texts as transparently representational. But such a rhetorical conception of reflexivity too easily overlooks that such textual constructions are also actions situated amidst alignments of power and resistance. Reflexivity has moral and political as well as rhetorical and epistemological dimensions: what do these writings and sayings do? to whom do they speak? what other voices and concerns do they acknowledge, make room for, or foreclose? which tendencies and alignments do they reinforce and which do they challenge? above all, to whom are they accountable? These questions arise with considerable force, because science studies as such are not politically or epistemically pre-positioned: critical engagement with scientific practices might variously articulate and reinforce dominant epistemic and political alignments, contribute to or extend oppositional discourses and practices, or shift the field to envision new possibilities. A modest and self-critical attentiveness to our own partiality and situatedness, and to our accountability for what we say and do, are the political responsibility incurred by our own contingent positionings within the culture of science.

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