



A Nonhumanist Disposition: On Performativity, Practical Ontology, and Intervention

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How to turn an opposition into a possible matter of contrast? Obviously, this is not only a question of goodwill. My guess is that we may do so through the experimental extension of the specific risks that singularize each position. Giving a chance for contrasts to be created where oppositions rule implies producing a middle ground but not a medium or average mitigating differences. It should be a middle ground for testing, in order that the contrasts evolve not from tamed differences but from creatively redefined ones.¹

Science and technology studies (STS) today can be viewed as a relatively stable enterprise, with its own conferences, journals, professional organizations, and graduate programs. With Bruno Latour, one could talk about the black-boxing of STS—however, characterizing a study as “in STS” does not determine its features very predictably because the field of inquiry is heterogeneous as regards the assumptions, theories, institutional affiliations, methods, approaches, goals, and interests of its practitioners; Michael Lynch and Kathleen Jordan’s term “translucent box” is probably more fitting.²

1. Isabelle Stengers, “Beyond Conversation: The Risks of Peace,” in *Process and Difference: Between Cosmological and Poststructuralist Postmodernisms*, ed. Catherine Keller and Anne Daniell (Albany: SUNY Press, 2002), pp. 236–237.

2. Kathleen Jordan and Michael Lynch, “The Sociology of a Genetic Engineering Technique: Ritual and Rationality in the Performance of the ‘Plasmid Prep,’” in *The Right Tools for the Job: At Work in Twentieth-Century Life Science*, ed. Adele E. Clarke and Joan H. Fujimura (Princeton, N.J.: Princeton University Press, 1992), p. 107. See also Jeffrey L. Ramsey, “On Refusing to Become an Epistemologically Black Box: Instruments in

Undoubtedly, this diversity and differentiation make any one description of STS problematic. As has been argued, disciplinary “looseness” is often an asset, rather than a weakness, for developing disciplines and practices,³ which are anyway inevitably shaped in the historically contingent interactions between multiple kinds of actors with different agendas and aspirations. The gesture of presenting one’s study as a “strong case” by emphasizing the coherent theoretical basis on which it is built thus reproduces an idea with which STS itself has regularly taken issue: that the unified position of a scientific community is necessarily a measure of the epistemological merit of that community and therefore conveys, or at least ought to convey, additional credibility to the statements of its members.⁴

Rather than unification, several debates that may be observed within and around STS bear resemblance to what Barbara Herrnstein Smith has called the microdynamics of incommensurability. Their familiar frustrations are exemplified as follows: “‘You can’t argue with these people,’ says one. ‘They don’t play by the rules; they challenge every word you say.’ ‘It’s like talking to a brick wall,’ says the other. ‘They don’t hear a word you say; they keep repeating the same arguments.’”⁵ Smith proposes that such stalemates are often not due

Chemical Kinetics during the 1920s and ’30s,” *Studies in History and Philosophy of Science* 23:2 (1992): 284.

3. See, for example, Adele E. Clarke and Joan Fujimura, “Introduction: What Tools? Which Jobs? Why Right?” in Clarke and Fujimura, *Right Tools for the Job* (above, n. 2), chap. 1.

4. It can be argued, for instance, that unification indicates merely that the discipline is in a stable state of “normal science,” as in Thomas Kuhn, *The Structure of Scientific Revolutions*, 2nd ed. (Chicago: University of Chicago Press, 1970); or, more critically, that conceptual unification brings its own invisibilities. For the case of molecular biology, see Richard C. Lewontin, *Biology as Ideology: The Doctrine of DNA* (New York: Harper Perennial, 1992); for socio-biology, see Susan Oyama, *The Ontogeny of Information: Developmental Systems and Evolution*, 2nd ed. (Durham, N.C.: Duke University Press, 2000).

5. Barbara Herrnstein Smith, *Belief and Resistance: Dynamics of Contemporary Intellectual Controversy* (Cambridge, Mass.: Harvard University Press, 1997), p. xii. John Searle argues that “once you state the claims and arguments of the antirealists out in the open, naked and undisguised, they tend to look fairly ridiculous” (John Searle, *The Construction of Reality* [New York: Free Press, 1995], p. 158). Smith elaborates on the way in which such ridiculousness is produced: “[It] commonly involves in practice reducing some set of more or less heterogeneous accounts, analyses, and arguments to some one-line ‘core idea’ framed in the current idiom of analytic philosophy; plucking away that idea not only from the texts in which it, or, rather its actual counterparts, are articulated and elaborated, but also from the historical and institutional contexts in relation to which those counterparts have their intellectual significance; assigning to that boiled-down, plucked-away, dubiously paraphrased core idea the name of some classic ‘ism,’ the ‘strong’ and ‘weak’ versions of which are duly distinguished, and sub-

to simple misunderstanding or to differences in vocabulary, but are rather symptomatic of “systematically interrelated divergences of conceptualization, that emerge at every level and operate across an entire intellectual domain.”⁶

Within and around STS, such divergences display themselves with especial vigor in relation to recently emerging agendas revolving around such notions as performativity and practical ontology, and the activation of objects as *nonhuman actors*. Put together, these shifts in the understanding of the content and stakes of STS can be seen as characterizing a “posthumanist” or “nonhumanist” disposition—one that has been particularly (but not exclusively) inspired by poststructuralism and pragmatism. Criticisms of various aspects of such a disposition have been expressed particularly in terms of normative worries, and accusations of political abdication. In view of such criticism, and coming with a keen interest in the constructive possibilities of nonhumanist STS, I believe that an important task lies in articulating the considerable potential, in terms of both alternative conceptualizations and interventions, that such studies may hold.

Instead of claiming, in the following, to represent STS as a whole, I draw upon a range of ideas from such writers as Donna Haraway, Andrew Pickering, and Bruno Latour, which have various affinities with poststructuralists such as Gilles Deleuze, Michel Foucault, Michel Serres, and Jean-François Lyotard. These people are not talking about precisely the same issues, and they are not saying exactly the same things; it is not, therefore, my interest to try to integrate all of them in a common, eclectic framework. Instead, I take my cue from the philosopher of science Isabelle Stengers who, in the epigraph above, wonders how to turn differences—or, indeed, antagonistic oppositions—into productive intellectual contrasts, and suggests that a solution could be found in experimentally processing sets of positions through each other. Productivity would be created at the middle ground, where no position would be able to silence any other, and where differences would therefore have to be respected, rather than made to disappear either by force or by consensus (which is often a more invidious force, since it is rarely recognized as such).

jecting the thoroughly processed product of these operations to a show of logical analysis that concludes . . . with a demonstration of its . . . counterintuitiveness, unintelligibility, incoherence, and/or decisive self-refutation” (Barbara Herrnstein Smith, “Reply to an Analytic Philosopher,” *South Atlantic Quarterly* 101:1 [2002]: 234).

6. Smith, *Belief and Resistance* (above, n. 5), p. 131.

The latter qualification is important, because it emphasizes that *tolerance* of alternative perspectives is not necessarily, and certainly not always, the ideal. Specifically, it means that in the following I am not prevented from noting and, indeed, going some lengths to stress non-humanist disagreements with a number of positions within STS and more broadly. For I will imagine that it is only by painstakingly working to clarify differences, rather than glossing them in the name of a pluralistic “goodwill,” that a serious evaluation of the possibilities, limits, and implications of nonhumanist STS-studies can be attempted.

In what follows, I concentrate in particular on the shift from a representational to a performative idiom for analyzing science and technology, and the closely related switch from an interest in epistemology to a concern with understanding how ontologies are shaped in action. I present first an analysis of what such changes entail in terms of theory, but this question quickly leads to a consideration of what they imply in terms of practice, politics, and intervention. I broach this issue by engaging with a cultural critique of actor-network theory (ANT). I sum up by considering what one may and may not hope for from nonhumanist STS-studies in “practical” terms.

What follows may be seen as an experiment in the expansion of relevant resources for STS-theorizing. Rather than presenting a history of increasing theoretical sophistication—for example from Mannheim and Merton to Latour and Haraway, via Kuhn and Feyerabend, Bloor and Barnes, and numerous other important figures⁷—I offer here a kind of theoretical assemblage, drawing selectively on multiple figures without, I hope, unnecessarily disfiguring them in the process.

7. But some references are due to the canonical literature. See, for example, Karl Mannheim, *Essays on the Sociology of Knowledge* (New York: Oxford University Press, 1952), and Richard K. Merton, *Social Theory and Social Structure* (New York: Free Press, 1968), for classical expositions in the sociology of knowledge. See also Karl Popper, *The Logic of Scientific Discovery* (London: Hutchinson, 1968), chaps. xvii–xviii, and Imre Lakatos, *Proofs and Refutations: The Logic of Mathematical Discovery* (Cambridge: Cambridge University Press, 1976), for classical texts in philosophy of science. The “countertradition” in the history and philosophy of science is most famously exemplified by Kuhn, *Structure* (above, n. 4), and Paul Feyerabend, *Against Method* (London: Verso, 1993). Ludwik Fleck, *Genesis and Development of a Scientific Fact* (1935; Chicago: University of Chicago Press, 1979), was an important precursor of these developments. The strong program in the sociology of scientific knowledge (SSK) radicalized many of these insights; see, e.g., David Bloor, *Knowledge and Social Imagery* (Chicago: University of Chicago Press, 1976). Later came laboratory studies, such as Bruno Latour and Steve Woolgar, *Laboratory Life: The Construction of Scientific Facts* (1979; Princeton, N.J.: Princeton University Press, 1986), and Andrew Pickering, *Constructing Quarks: A Sociological History of Particle Physics* (Chicago: University of Chicago Press, 1984); the empirical program of relativism (EPOR): Harry Collins, *Changing Order: Replication and Induction in Scientific Practice* (1985; Chicago: University of Chicago Press, 1992); reflexivity: Mal-

The "Practice-Turn" in Science Studies

As indicated by the name "science and technology studies," researchers in this field have been interested in closely investigating differing aspects and relationships in the extremely broad sphere of contemporary life that has to do with science and technology.⁸ They have done so from numerous perspectives and with numerous ap-

colm Ashmore, *The Reflexive Thesis: Writing Sociology of Scientific Knowledge* (Chicago: University of Chicago Press, 1989), and Steve Woolgar, *Science: The Very Idea* (Chichester: Ellis Horwood, 1988); actor-network theory: Michel Callon, "Some Elements of a Sociology of Translation: Domestication of the Scallops and Fishermen of St. Brieuc Bay," in *Power, Action, and Belief: A New Sociology of Knowledge*, ed. John Law (London: Routledge and Keagan Paul, 1986), pp. 196–233, and Bruno Latour, *Science in Action: How to Follow Scientists and Engineers through Society* (Cambridge, Mass.: Harvard University Press, 1987); symbolic interactionist studies in science: Adele E. Clarke, *Disciplining Reproduction: Modernity, American Life Sciences, and "The Problems of Sex"* (Berkeley: University of California Press, 1998), and Joan Fujimura, *Crafting Science: A Sociohistory of the Quest for the Genetics of Cancer* (Cambridge, Mass.: Harvard University Press, 1996); cultural anthropological studies of science: Sharon Traweek, *Beamtimes and Lifetimes: The World of High Energy Physicists* (Cambridge, Mass.: Harvard University Press, 1988); newer philosophical approaches: Ian Hacking, *Representing and Intervening: Introductory Topics in the Philosophy of the Natural Sciences* (Cambridge: Cambridge University Press, 1983), and Joseph Rouse, *Knowledge and Power: Toward a Political Philosophy of Science* (Ithaca, N.Y.: Cornell University Press, 1987); and feminist analyses of science: Anne Fausto-Sterling, *Myths of Gender: Biological Theories about Women and Men* (New York: Basic Books, 1992), and Donna Haraway, *Primate Visions: Gender, Race and Nature in the World of Modern Science* (New York/London: Routledge, 1989).

8. References can be multiplied indefinitely: Madeleine Akrich, "A Gazogene in Costa Rica: An Experiment in Techno-Sociology," in *Technological Choices: Transformation in Material Cultures since the Neolithic*, ed. Pierre Lemonnier (London: Taylor and Francis, 1996), pp. 289–337; Karen Barad, "Reconceiving Scientific Literacy as Agential Literacy: Or, Learning How to Intra-Act Responsibly within the World," in *Doing Science + Culture: How Cultural and Interdisciplinary Studies Are Changing the Way We Look at Science and Medicine*, ed. Roddey Reid and Sharon Traweek (New York/London: Routledge, 2000), pp. 221–259; Andrew Barry, *Political Machines: Governing a Technological Society* (London/New York: Athlone Press, 2001); Geoffrey C. Bowker and Susan Leigh Star, *Sorting Things Out: Classification and Its Consequences* (Cambridge, Mass.: MIT Press, 1999); Charis Cussins, "Ontological Choreography: Agency for Women Patients in an Infertility Clinic," in *Differences in Medicine: Unraveling Practices, Techniques, and Bodies*, ed. Marc Berg and Annemarie Mol (Durham, N.C.: Duke University Press, 1998), pp. 166–202; Steven Epstein, *Impure Science: Aids, Activism, and the Politics of Knowledge* (Berkeley: University of California Press, 1996); Donna Haraway, *Modest_Witness@Second_Millennium.FemaleMan©_Meets_OncoMouse™: Feminism and Technoscience* (New York: Routledge, 1997); Corinne P. Hayden, "A Biodiversity Sampler for the Millennium," in *Reproducing Reproduction: Kinship, Power, and Technological Innovation*, ed. Sarah Franklin and Helena Ragoné (Philadelphia: University of Pennsylvania Press, 1998), pp. 173–207; Casper Bruun Jensen and Randi Markussen, "Mårup Church and the Politics of Hybridization: On Complexities of Choice," *Social Studies of Science* 31:6 (2001): 795–820; Annemarie Mol, *The Body Multiple: Ontology in Medical Practice* (Durham,

proaches, as can be ascertained in helpful survey articles.⁹ One fairly generally acknowledged change that has occurred in these studies over the last decades is from focusing exclusively on the content of science¹⁰ to focusing on the intertwining of content with multiple practical and material aspects of *laboratory life*.¹¹

Obviously, the practical and material *effects* of scientific knowledge—the technologies whose invention it has enabled, and the many applications it has made possible—have been regularly taken as important indications of the superiority of Western science over all other systems of inquiry.¹² However, the practice-turn in recent sociocultural inquiries into science and technology refers to a quite different phenomenon, which is in an important sense corrective to

N.C.: Duke University Press, 2002); Andrew Pickering, *The Mangle of Practice: Time, Agency, and Science* (Chicago: University of Chicago Press, 1995); Hans-Jörg Rheinberger, *Toward a History of Epistemic Things: Synthesizing Proteins in the Test Tube* (Stanford: Stanford University Press, 1997); Helen Verran, *Science and an African Logic* (Chicago: University of Chicago Press, 2001).

9. See Finn Olesen, "Konstruktive Studier af Videnskab og Virkelighed: Fra Sociologi til Kulturforskning," *Philosophia* 25:3-4 (1996): 9-44; Joseph Rouse, "What Are Cultural Studies of Scientific Knowledge?" *Configurations* 1 (1993): 1-22; Sharon Traweek, "An Introduction to Cultural and Social Studies of Sciences and Technology," *Culture, Medicine, and Psychiatry* 17 (1993): 3-25.

10. The sociological focus on the content of science was initiated, not least, by the Edinburgh sociologists David Bloor and Barry Barnes, and had two targets. Against classical sociology of knowledge, as represented by Mannheim or Merton, SSK aimed to show that the content of scientific knowledge was, indeed, amenable to sociological analysis, and that the classical distinction between *external factors* (which sociologists could study) and *internal factors* (which they could not) did not hold. Against classical epistemology, they aimed to show that what was regularly characterized as internal to science was in fact influenced by the putatively external, and that the classical distinction between the *context of discovery* (which might be messy) and the *context of justification* (in which the mess was removed and the logical core of discovery was elucidated) could not be upheld.

11. See, e.g., David Gooding, Trevor Pinch, and Simon Schaffer, eds., *The Uses of Experiment: Studies in the Natural Sciences* (Cambridge: Cambridge University Press, 1989); Andrew Pickering, ed., *Science as Practice and Culture* (Chicago: University of Chicago Press, 1992); Theodore R. Schatzki, Karin Knorr-Cetina, and Eike Von Savigny, eds., *The Practice Turn in Contemporary Theory* (New York/London: Routledge, 2001).

12. As case-studies has indicated, however, the situation is often the reverse; that is, technological possibility and change often drive scientific inquiry. See, e.g., Thomas P. Hughes, *Networks of Power: Electric Supply Systems in the U.S., England, and Germany, 1880-1930* (Baltimore: Johns Hopkins University Press, 1983); Wiebe E. Bijker, Thomas P. Hughes, and Trevor Pinch, eds., *The Social Construction of Technological Systems: New Directions in the Sociology and History of Technology* (Cambridge, Mass.: MIT Press, 1989). For a view of science as one "indigenous knowledge system," see Verran, *Science and an African Logic* (above, n. 8); Helen Verran, *Science and the Dreaming* (Chicago: University of Chicago Press, forthcoming).

the received view: that is, that if it works it must be true. For STS has focused not only on material effects as they are *enabled by* scientific ideas, but also on the material, practical, and institutional aspects as they *participate* in the construction of scientific content. This change of emphasis, which has been driven by empirical inquiries into the way science is practically carried out, has been of consequence for discussions about epistemology and ontology.

Challenging Epistemology

Epistemology is generally seen to concern itself with investigating the foundations of certain knowledge. This inquiry has been almost universally premised on the idea of a split between the ideal and the concrete, and has prioritized the abstract capabilities of the mind over the inadequacies of the body. Scientific ideas are generated in the interaction with obdurate materials with unknown qualities, and a prominent concern of epistemology has been with *purifying* science from the many biases that could potentially invalidate its knowledge in this interaction. Epistemology thereby tries to establish an ideal relationship between the level of scientific ideas and the level of their practical validation and application, and in this project it has consistently prioritized theory over practice.¹³ In contemporary epistemology this purification has been typically managed by invocation of the scientific method, which, if properly applied, has been seen as the guarantee of knowledge-claims. In recent years claims pertaining to the absoluteness or universality of such knowledge have been toned down somewhat, and often the emphasis is now on securing the least-fallible knowledge—but, then, the claim to be able to (unequivocally) determine what is least fallible in itself continues to rely on the idea of an external standard.¹⁴

13. Finn Olesen argues that “the still dominating Anglo-American realism debate by and large maintains a series of traditional themes, not least related to Cartesian notions of objectivity and logical truth” (Finn Olesen, “Videnskabelig Realism og Eksperimentel Praksis,” *Philosophia* 22:1–2 (1993): 54; my translation). He further suggests that “it is not unfair to claim that Anglo-American philosophy until around 1980 was theoretical philosophy, or, in the words of David Baird, ‘philosophy of scientific theory’” (ibid., p. 162; my translation). Ian Hacking specifies some recent epistemological concerns: “It is a theory of knowledge about familiar facts and events; it includes the theory of sense perception, of grounds for belief, and the analysis of ‘I know that *p*’” (Ian Hacking, *Historical Ontology* [Cambridge, Mass.: Harvard University Press, 2002], p. 87).

14. Stengers notes that Popper’s “starting point is logic’s inability to give an account of scientific knowledge” (Isabelle Stengers, *The Invention of Modern Science* [Minneapolis: University of Minnesota Press, 2000], p. 45); yet, “Popper’s staging is undertaken in order to lead to a perspective that conserves the ideal of a pure science and the correlative definition of an ‘external milieu’ as impure” (ibid., p. 47).

The classical epistemological ambition is regularly presented as a defense against the contamination of knowledge-claims, for instance by the partisanship or local provincialism of their producers. The analytic philosopher Paul Boghossian, in a recent polemic against constructivism in general and Barbara Herrnstein Smith in particular (one, that, incidentally, vividly illustrates Smith's analysis of the microdynamics of incommensurability), offers the following description:

What matters to epistemology are three things: first, the claim that only some considerations can genuinely justify a belief, namely, those that bear on its *truth*; second, a substantive conception of the sorts of considerations that qualify for this normative status—observational evidence and logic, for example, but not a person's political commitments; and finally, the claim that we do sometimes believe something because there are considerations that justify it and not as a result of some other cause, such as *because* it would serve our interests to do so.¹⁵

Another recent example is afforded by John Searle's *Construction of Social Reality*, which has less interest in defending epistemology per se,¹⁶ yet leaves no doubt about the undiminished importance of such classical notions as evidence, objectivity, reality, and truth:

Having knowledge consists in having true representations for which we can give certain sorts of justification or evidence. Knowledge is thus by definition objective in the epistemic sense, because the criteria for knowledge are not arbitrary, and they are impersonal.¹⁷

Undoubtedly the understanding of what exactly counts as proper evidence, objectivity, and truth varies between analytic philosophers, including Boghossian and Searle, as do, therefore, interpretations of what the scientific method would consist in, and what it would mean for it to be properly applied.¹⁸ Certainly, analytic philosophers would also contend that these divergences are substantial.

However, what remains in the background of these debates is the assumption that (unreconstructed) notions of evidence, objectivity,

15. Paul Boghossian, "Constructivist and Relativist Conceptions of Knowledge in Contemporary (Anti-)Epistemology: A Reply to Barbara Herrnstein Smith," *South Atlantic Quarterly* 101:1 (2002): 218.

16. Searle, *Construction of Reality* (above, n. 5), p. 173.

17. *Ibid.*, p. 151.

18. For alternative descriptions, see Peter Galison and David J. Stump, eds., *The Disunity of Science: Boundaries, Contexts, and Power* (Stanford: Stanford University Press, 1996).

reality, and truth cannot be done without—not, at least, without inviting epistemological and quite possibly moral catastrophe. The challenge posed to classical epistemologists by STS-research has therefore been much more severe than internal epistemological quarrels.¹⁹ For in insisting on the participation of practical and material effects in the production of knowledge, these studies have problematized virtually all the key distinctions and relations in epistemology—notably, between knowledge and power and between (scientific) ideas and their (technical) concretizations. By doing so they have ineluctably challenged the central epistemological ambition to guarantee the possibility of formulating true (in the sense of reliably decontextualized) statements about the world. This *challenge of constructivism* is of wide-ranging ramifications for the conceptualization of science, technology, society, and their interrelationships.

Principles of Symmetry

These ramifications are themselves variably reviewed depending upon the strand of STS of one's adherence (such as, for instance, standpoint feminism, sociology of scientific knowledge, social epistemology, symbolic interactionism, cultural anthropological studies of science, or actor-network theory). Many of these studies would in principle agree with the famous symmetry doctrine formulated by the sociologist of science David Bloor, which proposes that statements that we take to be true and statements that we take to be false

19. Although the radical implications of this challenge are sometimes underestimated by commentators. In a recent example, the philosopher of science Helen Longino works strenuously to reconcile classical philosophy of science with STS, claiming that both sides unwittingly believe in what she calls "the rational-social dichotomy" (Helen Longino, *The Fate of Knowledge* [Princeton: Princeton University Press, 2002], p. 77). After "disassembling" this dichotomy, Longino suggests that philosophers should indeed be able to appreciate the practice focus of STS, while STS-scholars should acknowledge the existence of cognitive features of the kinds that philosophers like to evaluate. Arguably, however, Longino's disassembling and subsequent reconciliation of positions are premised on a number of ideas that are themselves at stake in the debates she proposes to mediate. Her solution is not likely to convert many STS-scholars, since it can be viewed as *reasserting* the idea that it is relevant, or, indeed, possible, to distinguish between social and cognitive "factors." This is precisely the idea that STS-scholars have worked hard to "disassemble" (and reassemble). See Andrew Pickering, "From Science as Knowledge to Science as Practice," in Pickering, *Science as Practice* (above, n. 11), pp. 14–15, n. 11. Longino's text thus figures as a lucid example of what Barbara Herrnstein Smith has termed "equivocation"—that is, the move to reconcile what may be viewed as irreconcilable or incommensurable positions: Barbara Herrnstein Smith, "Cutting-Edge Equivocation: Conceptual Moves and Rhetorical Strategies in Contemporary Anti-Epistemology," *South Atlantic Quarterly* 101:1 (2002): 187–212.

should be accounted for with the same set of explanatory devices.²⁰ But what is viewed as following from this doctrine is highly variable.

In the “chicken debate” between sociologists of science Harry Collins and Steven Yearley, reflexivist Steve Woolgar, and actor-network theorists Bruno Latour and Michel Callon, the former formulated one important version of the consequences of adopting a constructivist stance in the exploration of science:²¹ Collins and Yearley suggested a dualistic model, referred to as *meta-alternation*, in order to account for how one could take seriously both the realist findings of scientific research and the constructivist findings of science studies research. Alternation (without the “meta”) referred to the idea that the sociologist (“promiscuously”) “develops the ability to switch between different frames of reference,” viewing each setting as displaying but “one set of beliefs among many.”²² Yet, according to Collins and Yearley, this is a merely analytical stance, for one cannot, presumably, be a constructivist in everyday life:

In spite of this achievement, all of us, however sophisticated, can switch to modes of knowing that allow us to catch buses and hold mortgages. We all engage as a matter of fact in which we might call “meta-alternation.”²³

According to Collins and Yearley, we necessarily cease to be social constructivists the moment we enter a bus, because we cannot doubt that it will safely carry us home from work. While social constructivism as *intellectual* endeavor is aligned with doubt and illusion (its job being to “pull the veil from science”), one must be a realist in *everyday* life, and *especially so* with respect to objects of scientific inquiry.

20. Bloor, *Knowledge and Social Imagery* (above, n. 7), p. 243. Thus he formulated a methodical response to the situation, characterized as lamentable by B. F. Skinner in 1953, that “we have not gone all the way. We regard the common man as the product of his environment; yet we reserve the right to give personal credit to great men for their achievements. . . . We want to believe that right-minded men are moved by valid principles even though we are willing to regard wrong-minded men as victims of erroneous propaganda. Backwards peoples may be the fault of a poor culture, but we want to regard the elite as something more than the product of a good culture. . . . We dismiss those who disagree with us as victims of ignorance, but we regard the promotion of our own religious beliefs as something more than the arrangement of a particular environment” (B. F. Skinner, *Science and Human Behavior* [New York: Macmillan, 1953], pp. 8–9). Skinner thought this symptomatic of a transitional cultural period, but if this is the case the transition is still under way.

21. The “chicken debate” is named after an article by Collins and Yearley: Harry Collins and Steven Yearley, “Epistemological Chicken,” in Pickering, *Science as Practice* (above, n. 11), pp. 301–326.

22. *Ibid.*, p. 301.

23. *Ibid.*, p. 302.

The stance of meta-alternation was defined in order to discredit the actor-network idea that nonhumans should be “granted agency.” It enabled Collins and Yearley to query how one could *really* acquire knowledge of nonhuman capacities, and it suggested that one could not, except by becoming a scientist:

In any case, the complicity of the scallops (or whatever), if it is to play a part in accounts of this sort, ought to be properly recorded. How is the complicity of scallops to be measured? There is only one way we know of measuring the complicity of scallops, and that is by appropriate scientific research. If we are really to enter scallop behavior into our explanatory equations, then Callon must demonstrate his scientific credentials. He must show that he has a firm grip on the nature of scallops. There is not the slightest reason for us to accept his opinions on the nature of scallops if he is any less a scallop expert than the researchers he describes. In fact, we readers would prefer him to be *more* of a scallop expert than the others if he is to speak authoritatively on the subject.²⁴

In matters scientific, we have no better bet than taking at face value the pronouncements of experts since they, not we, are the specialists in their respective areas. All this sounds quite humble. However, when it comes to explaining the means of achieving agreement on scientific matters, authority should be deferred to the sociologist of science, since this is his area of expertise. This model thus gives to the scientist with the one hand the epistemic authority (realism about nonhumans), and seeks to remove it with the other (constructivism about scientists' realism about nonhumans). On the one hand, the epistemological realist position of science is granted—but it is then doubled by the position of the sociologist, who is able to really point to how realism is the result of the open and negotiable work of scientists (but notably *not* the open and negotiable work of natural entities). Although Collins and Yearley conclude that “of course we cannot claim epistemological authority either . . . we can only compete on even terms for our share of the world with all the usual weapons,”²⁵ this share takes on significant proportions, as the final say in epistemic matters is conferred back to the sociologist who really knows how science works.

Similar double-movements of endorsing radical (anti)epistemological principles but forgetting about their implications at the time when they would reflexively apply to oneself—a strategy that Barbara Herrnstein Smith has referred to as “cutting-edge equivoca-

24. Ibid., p. 316.

25. Ibid., p. 324.

tion²⁶—are found in a number of STS-studies that claim to offer specific kinds of political leverage in their engagements with scientific practices, such as the ability to *criticize* or *resist* the status quo.²⁷ So where to go from there?

From Epistemology and Representation to Practical Ontology and Performativity

In the view here presented it will be suggested that the challenge of constructivism is intimately bound up with what I will lay out as the move from an epistemological approach to one focusing on practical ontology, and from a representational to a performative idiom in the understanding of science. The gist of this change can be nicely summarized in a formulation of Bruno Latour, “Essence is Existence and Existence is Action,”²⁸ but its philosophical history can be traced, at least in some of their interpretations, to Greek philosophers such as Heraclitus and Lucretius, to Leibniz and Spinoza, to Friedrich Nietzsche and A. N. Whitehead, and, unsurprisingly, to different radical thinkers in recent French philosophy who, in effect, have worked to reinterpret several of the above-mentioned: I think here in particular of Gilles Deleuze, Michel Foucault, and Michel Serres.²⁹

Essence is Existence. This is a claim that denies the purity of the ideal and refers all there is, in the first instance, to the material world. *Existence is Action.* But what exists? We do not know, at least not comprehensively, or not yet.³⁰ But what this formulation sug-

26. Smith, “Cutting-Edge Equivocation” (above, n. 19).

27. By no means exclusive examples include Hans Radder, “Normative Reflexions on Constructivist Approaches to Science and Technology,” *Social Studies of Science* 22:1 (1992): 141–173; Longino, *Fate of Knowledge* (above, n. 19); Langdon Winner, “Upon Opening the Blackbox and Finding It Empty: Social Constructivism and the Philosophy of Science,” *Science, Technology, and Human Values* 18:3 (1993): 362–378.

28. Bruno Latour, *Pandora’s Hope: Essays on the Reality of Science Studies* (Cambridge, Mass.: Harvard University Press, 1999), p. 179.

29. For specific discussion of Lucretius and Heraclitus, see Michel Serres, *Hermes: Literature, Science, Philosophy* (Baltimore: Johns Hopkins University Press, 1982); Michel Serres, *The Birth of Physics* (New York: Clinamen Press, 2000). For Leibniz, see Gilles Deleuze, *The Fold: Leibniz and the Baroque* (London: Athlone Press, 1993). For Spinoza, see Gilles Deleuze, *Expressionism in Philosophy: Spinoza* (New York: Zone Books, 1990); Warren Montag and Ted Stolze, eds., *The New Spinoza* (Minneapolis: University of Minnesota Press, 1997). For Nietzsche, see Gilles Deleuze, *Nietzsche and Philosophy* (London: Athlone Press, 1983); Michel Foucault, “Nietzsche, Genealogy, History,” in *The Foucault Reader*, ed. Paul Rabinow (New York: Pantheon, 1984), pp. 76–100; idem, “What Is Enlightenment,” *ibid.*, pp. 32–50.

30. This is one of the key insights of Spinoza, according to Deleuze, *Expressionism in Philosophy* (above, n. 29). See also Benedict de Spinoza, *Ethics* (London: Dent, 1959).

gests is that we can try to find out; for, often enough, action and activity are empirically observable. Not, however, as something *simply* out there. For as scientists well know, it is only through an organized and coordinated effort, using multiple machines and devices as mediators, that different entities become able to reliably “express themselves.” Such “expressive displays” enabled by humans can be characterized as “events” because of their unforeseeable character.³¹ The implication of this view is that novel aspects of the world (in the shape, for instance, of new effects, particles, or phenomena) can be articulated in the laboratory only because of the constellation of the particular forces that constitute the given experiment by which they are shaped and through which they emerge. As such articulation takes place at the *intersection* of the (sets of) forces that we regularly categorize as “observers,” “instruments,” and “the natural world,” the distinctions among these forces and the properties that go with them *themselves* become problematic and are turned into topics for investigation, rather than taken for granted as resources in the investigation of science and technology.³²

I have been talking here, in somewhat unusual terms, about Michel Callon and Bruno Latour’s proposed generalization of the doctrine of symmetry.³³ This extension suggests that while it is true that all beliefs should be treated symmetrically, as Bloor suggested, STS should also develop models and concepts to account for the fact that there are large differences between the *effects* of beliefs. In Callon and Latour’s model, the *stabilization* (legitimation, institutionalization) of some set of beliefs and practices rather than others is crucially dependent on the successful *delegation* of actions and responsibilities

31. This makes events literally “just happen,” when understood in a real-time framework rather than retrospectively: Pickering, *Mangle of Practice* (above, n. 8). It also introduces another vantage point from which to pinpoint the difference between non-humanist and epistemological understandings of science, as Stengers indicates: “If we take seriously the description of stories belonging as much to the history of the science as to contemporary practices, and particularly the controversies aroused by any new proposition, we are obliged to conclude that the criteria of scientificity or objectivity that should allow these controversies to be settled *did not preexist them*, but are on the contrary a major issue in discussions between scientists. And this situation has not been changed at all by philosophers of science and the criteria that they propose” (Isabelle Stengers, *Power and Invention: Situating Science* [Minneapolis: University of Minnesota Press, 1997], p. 81; emphasis in original).

32. This is to restate the premise of generalized symmetry as outlined in Callon, *Some Elements* (above, n. 7), and discussed in detail in Latour, *Science in Action* (above, n. 7).

33. Callon, *Some Elements* (above, n. 7); Michel Callon and Bruno Latour, “Don’t Throw the Baby out with the Bath School! A Reply to Collins and Yearley,” in Pickering, *Science as Practice* (above, n. 11), pp. 343–369.

to nonhuman actors, and their consequent practical redefinitions or *translations*. This move is related to, if not identical with, Andrew Pickering's suggested emphasis on the performative dimension of science. As he explains, "the representational idiom casts science as, above all, an activity that maps, mirrors, or corresponds to how the world really is."³⁴ But, Pickering continues, "there is quite another way of thinking about science," one that starts from the idea that the world is *doing things*, and is therefore, first of all, full of agency; this idea is the starting point for a performative analysis of scientific practice, "in which science is regarded as a field of powers, capacities, and performances, situated in machinic captures of material agency."³⁵

The bringing of nonhuman, material agents into the analytical picture is a move whose consequences I will engage in what follows.

Complexity

Following the doctrine of symmetry, it becomes possible to study technoscientific practices while taking seriously, but not investing too much in, the self-descriptions of their practitioners. For some time researchers in cultural studies, cultural anthropology, and qualitative sociology have told sophisticated stories about the artful work needed to successfully integrate the many different and sometimes contradictory exigencies of stabilizing social identities in multiple social formations.³⁶ In recent years these stories have been extended to also cover the social formations involved in technoscientific production, and this has been one important strand in the depurification of contemporary understandings of science. But what happens when nonhumans are added to the collectives to be described?

Perhaps a first experience is one of increased complexity. Indeed, in some research in cultural studies and STS the delight in making

34. Pickering, *Mangle of Practice* (above, n. 8), p. 5.

35. *Ibid.*, p. 7. I have been encouraged by a reviewer to clarify the relationship between these ideas and Austin's well-known distinction from *How to Do Things with Words*. Austin stated that, as opposed to constative utterances, performatives "do not 'describe' or 'report' or constate anything at all, are not 'true' or 'false'" (John L. Austin, *How to Do Things with Words* [Cambridge, Mass.: Harvard University Press, 1962], p. 5); instead, they constitute an act, as when the "I do" pronounced by the relevant actors during a marriage ceremony effects the marriage. Austin's entire discussion concerns *words*, while *things* are of little concern. This paper considers a broad range of analytical implications of taking the performances of materialities seriously, as in Austin's own brief example from Lecture IX: "Thus intimidation may be achieved by waving a stick or pointing a gun. Even in the cases of convincing, persuading, getting to obey and getting to believe, we may achieve this response non-verbally" (*ibid.*, pp. 117–118).

36. See contributions to Franklin and Ragoné, *Reproducing Reproduction* (above, n. 8).

complexity visible seems to overshadow the question of what productive differences such redescription could render pertinent. Another perspective would view the notion of complexity as a “lure for feeling,” with the capability of generating new, different, and, perhaps, harder questions for us to answer about sciences and society. This is the proposal of Isabelle Stengers:

As for the notion of complexity, it sets out *problems*—we don’t know a priori what “*sum of parts*” means—and this problem implies that we cannot treat, under the pretext that they have the same “*parts*,” all the “*sums*” according to the same general method.³⁷

In her suggestion, noticing complexity is the mere beginning of the process of understanding and transforming relations between the sciences and society. It is a necessary beginning, however, because relevant questions regarding a given situation can be formulated only if the situation has first been decomposed into enough divergent elements to prevent its simple evaluation.

Power and Coalitions

Today, most technological-social innovations affect things in much more varied modes than those anticipated by our questions, and thus create a gap between “things,” as they are implicated in it, and their scientific representation.³⁸

Surely a lesson that has been well learned in the past century is that scientific innovations are effective in a multiplicity of ways, only a small number of which are anticipated.³⁹ Stengers is not the first, or only, scholar to point to that fact. In the domain of the “human sciences,” for instance, Michel Foucault analyzed and described the multiple sociopolitical effects, for better or worse, of the invention of modern medicine, psychiatry, and criminology.⁴⁰ In this work he has prefigured, as well as functioned as a tremendous inspi-

37. Stengers, *Power and Invention* (above, n. 31), pp. 12–13 (emphasis in original).

38. Stengers, *Invention of Modern Science* (above, n. 14), p. 158.

39. See, e.g., Donald Mackenzie, *Inventing Accuracy: A Historical Sociology of Nuclear Missile Guidance* (Cambridge, Mass.: MIT Press, 1990); Charles Perrow, *Normal Accidents: Living with High-Risk Technologies* (Princeton, N.J.: Princeton University Press, 1999); Kim Fortun, *Advocacy after Bhopal: Environmentalism, Disaster, New Global Orders* (Chicago: University of Chicago Press, 2001).

40. See, e.g., Michel Foucault, *The Birth of the Clinic: An Archaeology of Medical Perception* (London: Tavistock, 1973); idem, *Discipline and Punish: Birth of the Prison* (London: Penguin Books, 1991); Nikolas Rose, *Governing the Soul: The Shaping of the Private Self* (London/New York: Free Association Books, 1989).

ration for, research in STS and numerous other disciplines. In order to take up the challenge of Isabelle Stengers—to respond inventively to the fact that nonhuman actors are increasingly brought to bear on our lives in ways we not only do not, but probably *cannot* anticipate—some of Foucault’s sociopolitical ideas could prove useful.

Foucault’s political thinking was concerned with, among other things, how to “cut off the king’s head”⁴¹—by which he meant constructing a mode of political analysis that would not primarily be organized about the classical themes of sovereignty and law, and that would consequently not have to imagine that power comes “from above.”⁴² Instead, he was interested in the “play of power” as instantiated in myriad microprocesses throughout the social field. In different analyses Foucault showed how sets of practices were slowly and painstakingly composed, not least through the stabilization of specific discourses, even though no common interests between their constituents existed prior to their engagement. Power (as efficacy of action) seemed thus not to be inherent in some actors (and not others), but rather to be always in the making. By considering sociopolitics from this transformative viewpoint—as having to do with the composition of coalitions out of heterogeneous elements, rather than as having to do with stable formations with specific predefined interests and powers—Foucault could view the construction of disciplinary and institutional matrices as “intentional but non-subjective,” rather than as enforced by the presumptively powerful.⁴³

This descriptive conclusion, based in historical analysis, also functioned as a practical heuristics in Foucault’s own political engagements. Since, in his phrase, “power is everywhere” but is inherently unstable and transformative (as it is shaped in the ongoing interactions between practices, discourses, institutions, etc.), in order to resist specific functions of power one must become *as flexible as it is*. In this case, what could be termed roughly as a coalition-based approach to political action makes sense, precisely because it is based on constellations of people that temporarily take shape around a

41. Michel Foucault, *Power*, ed. James Faubion (New York: New Press, 2001), p. 122.

42. A very similar move is made in Bruno Latour, “The Powers of Association,” in idem, *Power, Action, and Belief: A New Sociology of Knowledge* (London: Routledge and Keagan Paul, 1986), pp. 264–280. Here, he distinguishes between power understood as the transmission of a substance (the diffusion model) and power understood as an effect of the successful work to stabilize associations among a set of actors (the translation model).

43. See Michel Foucault, *The History of Sexuality: An Introduction* (New York: Random House, 1990), chap. 2. For a discussion of emergent interests, see also Stengers, *Power and Invention* (above, n. 31), pp. 83–84.

cause of concern.⁴⁴ As no overall political program needs to be constructed for there to be resistance, no heavy bureaucratic apparatus *mimicking power* would be needed to *oppose it*.⁴⁵

The Foucauldian (and Nietzschean) conception of power and the conditions for political efficacy has been theoretically developed in recent political theory by scholars such as Ernesto Laclau and Chantal Mouffe, in their influential *Hegemony and Socialist Strategy*; Wendy Brown, in *States of Injury: Power and Freedom in Late Modernity and Politics Out of History*; and Judith Butler, in *Excitable Speech: A Politics of the Performative*.⁴⁶ It has also been investigated in STS through case studies, such as Steven Epstein's *Impure Science* on gay and lesbian activism, which shaped and redefined AIDS-research; Andrew Barry's work on ecological activism; and Brian Wynne's writing on sheep farmers' controversies with scientists.⁴⁷

44. For example, Foucault temporarily participated in organizations such as *Mouvement pour la Liberté de l'Avortement* (Movement for Freedom of Abortion): see Foucault, *Power* (above, n. 41), p. 423; and the *Groupe d'Information sur les Prisons*, which worked for reforms of the penal system: *ibid.*, pp. 418–422.

45. In a related move, Stengers has discussed issues of power in science. She contrasts minority and majority science: "I mean by minority not a part of the population which is not, but could and tries to become, the majority, but active minorities who do not dream of obtaining for themselves the power of a majority. Like Félix Guattari, I dream about multiple connections among minorities, so that each of them would become able to work out its own singularity through the creation of alliances, not in isolation, and so that each individual would be simultaneously part of many minorities. Science, as I love it, is also a minority movement in a way" (Isabelle Stengers, "From Describing Falling Bodies to Understanding People: What Is Scientific Objectivity? and Metamorphoses of Science: Feminism and Shifts of Paradigm," Working Paper in the *Gender-Nature-Culture Feminist Research Network Series* [Odense, Denmark: Odense University Press, 1994], p. 41). Whereas minority science is seen by her as "in league with power: *the invention of the power to confer on things the power of conferring on the experimenter the power of speaking in their name*" (Stengers, *Power and Invention*, [above, n. 31], p. 165; emphasis in original), majority science merely *mimes* this inventive capacity "with the systematic production of beings constrained to 'obey' the apparatus that will allow them to be quantified (such as the all-too-famous rats and pigeons of the experimental psychology laboratories). 'In the name of science,' innumerable animals have been vivisected, decerebrated, and tortured in order to produce 'objective' data" (Stengers, *Invention of Modern Science* [above, n. 14], pp. 22–23).

46. Ernesto Laclau and Chantal Mouffe, *Hegemony and Socialist Strategy: Towards a Radical Democratic Politics* (London/New York: Verso, 1985); Wendy Brown, *States of Injury: Power and Freedom in Late Modernity* (Princeton, N.J.: Princeton University Press, 1995); *idem*, *Politics out of History* (Princeton, N.J.: Princeton University Press, 2001); Judith Butler, *Excitable Speech: A Politics of the Performative* (New York/London: Routledge, 1997).

47. Epstein, *Impure Science* (above, n. 8); Barry, *Political Machines* (above, n. 8); Brian Wynne, "May the Sheep Safely Graze? A Reflexive View of the Expert-Lay Knowledge Divide," in *Risk, Environment and Modernity: Towards a New Ecology*, ed. Scott Lash, Bro-

Coordinating Science and Society

In the above examples I have focused on coalitions seemingly external to normal science that formed in order to resist specific kinds of development, but this should not be viewed simply as a matter of “outside forces” trying to “influence” what is (or ought to be) “internal” to science—for one reason for redescribing scientific controversies in the language of shaping and transforming coalitions is precisely to point to the fact that society is *never* external to science. On the contrary, the success of activists in challenging and redefining, for example, AIDS research can be seen as *one more bridge* being built between the presumed inside and outside of science. If this is a (partial) success story it is for the double reason that the articulation of parts of the AIDS research community with the agenda of activists *simultaneously* improved the situation of both communities: it enabled the scientists to do better science *and* improved the lives and the chances of survival for people with AIDS.⁴⁸

However, successfully linking these “outsides” and “insides” is a difficult task, because the sets of practices, relevancies, and interests of those who would need to cooperate are often vastly divergent. As the articulation of a novel entity in a laboratory can be described as an *event*, so can the (partial) success of the AIDS-coalition in redefining the relationship between science and the public, and for the same reason: that it required a unique constellation of forces that, far from being *given*, had to be painstakingly *constructed* in order to “express itself” effectively. Such technoscientific events can be characterized as experiments in democracy when they succeed in conferring on all interested parties the capacity of expressing a viewpoint, without trying to predetermine what it is, or what the consequences of it should be.⁴⁹

nislav Szerszynski, and Brian Wynne (London: Sage, 1996), pp. 44–84. See also Diana B. Dutton, *Worse than the Disease: Pitfalls of Medical Progress* (Cambridge: Cambridge University Press, 1988).

48. This discussion links up with Stengers’s suggestion that there is an inherent link between science and democracy: “In fact, as soon as one puts aside the classical division of responsibilities, which gives the sciences and their experts the task of ‘informing’ politics, of telling it ‘what it is’ and deciding what it ‘must be,’ one comes face to face with the inseparability of principle between the ‘democratic’ quality of the process of political decision and the ‘rational’ quality of the expert controversy. . . . This double quality depends on the way in which the production of expertise will be provoked on the part of all those, scientific or not, who are or could be interested in a decision” (Stengers, *Invention of Modern Science* [above, n. 14], p. 160).

49. For a discussion of Danish citizen-participation projects as experiments in democracy, see Casper Bruun Jensen, “Citizen Projects and Consensus Building at the Danish Board of Technology: On Experiments in Democracy,” *Acta Sociologica* 48 (forthcoming).

STS-studies in various instantiations have been interested in analyzing how the creative potential of scientific practices is opened up precisely to the extent that ideas, programs, or techniques have had to be shared among what proto-STS scholar Ludwig Fleck referred to as different thought collectives with different thought styles.⁵⁰ Scholars such as Leigh Star and James Griesemer have shown not only that the coordination of complicated scientific efforts is *not dependent* on homogeneous interests among participants, but also that actors' disparate goals may actually enable scientific success.⁵¹ They demonstrate that material entities can function as *boundary objects* that give *just enough* structure to the interactions between members from heterogeneous social worlds (or thought collectives) to enable the successful coordination of their efforts, yet do not try to impose on members the *same* definition of the work they are trying to accomplish. In their view, then, it is the mediation of nonhuman actors that allows members of different social worlds to productively articulate their similarities and dissimilarities.⁵²

I started off the previous section by invoking Stengers's proposition that there is a gap between things as they become effective in the world and things as they are mostly scientifically represented. This led me into discussing coalitions as a means of engaging sciences, with the purpose of narrowing the gap between expectation and effect under specific circumstances—such as those of AIDS research. Interestingly, this can be seen as a way of coming full circle back to the traditional philosophical question of how to address questions of predictability and reliability in scientific research from a completely nontraditional angle—notably, one that does not rely on a notion of universality to function successfully. Finally, I discussed how the actual work of coordinating coalitions, of building bridges between different social worlds or thought collectives, could itself be seen as a possibly inventive endeavor, and I pointed to nonhuman actors as important mediators and facilitators in the process. I would like now to reconnect this discussion to the initial consideration of epistemology and practical ontology.

50. Fleck, *Genesis and Development* (above, n. 7). See also Barbara Herrnstein Smith, "Netting Truth," *PMLA* 115:5 (1999): 1089–1095.

51. Susan Leigh Star and James R. Griesemer, "Institutional Ecology, 'Translation,' and Boundary Objects: Amateurs and Professionals in Berkeley's Museum of Vertebrate Zoology, 1907–1939," *Social Studies of Science* 19 (1989): 387–420.

52. Ilana Löwy has suggested that this idea can also apply to boundary concepts: Ilana Löwy, "The Strength of Loose Concepts: Boundary Concepts, Federative Experimental Strategies and Disciplinary Growth: The Case of Immunology," *History of Science* 30 (1992): 371–396.

The Collapse of Epistemology into Ontology

Stressing the intertwinement of human and nonhuman actors in science challenges traditional epistemology because activities such as observing or representing are not seen as distinct from intervening or constructing; rather, they are viewed as specific *ways of* intervening and constructing.⁵³ In this view, epistemology *collapses into* ontology and the sciences are reformulated as practical activities aimed at (re)building the world by adding new elements with new capabilities and new relationships to it. Knowing (and thinking about knowing) are turned into *particular styles and methods* for connecting and cooperating with specific actors (human and otherwise)—thus shaping reality, or *doing* practical ontology. Scholars such as Anemarie Mol, Hans-Jörg Rheinberger, Charis [Cussins] Thompson, and Helen Verran have detailed this occurrence in various settings,⁵⁴ and Latour has formulated the important implication: “There is no primary quality, no scientist can be reductionist, disciplines can only *add* to the world and almost never subtract from it.”⁵⁵

Interestingly, this idea is almost as foreign to social constructivism as it is to epistemology because its focus is on the eventful *reconfiguration* of reality, taking place in laboratories and elsewhere, rather than on the replacement of naturalist *explanations* of science with social or cultural ones.

The move, suggested in Callon and Latour’s notion of generalized symmetry, of treating the sociality and the naturalness of the sciences as equally troublesome, thus opens up a space for viewing the sciences as vehicles for the construction of many different socionatural entities—for a multinaturalism replacing both (traditional, realist) mononaturalism and multiculturalism.⁵⁶ This position clearly

53. A point stressed, for instance, in Hacking’s tellingly entitled *Representing and Intervening* (above, n. 7).

54. Cussins, “Ontological Choreography” (above, n. 8); Mol, *Body Multiple* (above, n. 8); Verran, *Science and the Dreaming* (above, n. 12); Rheinberger, *Toward a History of Epistemic Things* (above, n. 8).

55. Bruno Latour, “How to Talk about the Body? The Normative Dimension of Science Studies,” *Body and Society* 10:2 (2004): 226.

56. See Eduardo Viveiros de Castro, *From the Enemy’s Point of View* (Chicago: University of Chicago Press, 1992); Bruno Latour, “Whose Cosmos, Which Cosmopolitics?” *Common Knowledge* 10:3 (2004): 450–462. Perhaps we might characterize this situation as also opening the possibility for the construction of many “psycho-natural” entities. The suggestion is Barbara Herrnstein Smith’s, and goes against the grain of recent work in STS, which has taken seriously the “moratorium” on psychological explanations suggested in Latour, *Science in Action* (above, n. 7). Recent suggestions are offered by

leaves no room for the epistemological aspiration to define a method for the generation of objective knowledge within a discipline, much less in general, because knowledge is constructed precisely at the intersection of the many different agencies concretely interacting in the world. This condition of specificity, of course, does not prevent technoscientific constructions from becoming consequential on much vaster scales "in the hands of later users."⁵⁷

The "Northwest Passage," Orders, and Their Others

Michel Serres has provided a nice analogy for knowledge-production by comparing it to the navigation of the "Northwest passage."⁵⁸ Traveling the Northwest passage has many hazards such as wild tempests and drifting ice-floes, but if these perilous conditions can be successfully navigated, novelty awaits at the end of the journey in the shape of a new (and unexpected) environment.

In spite of the connotations of scientific heroism that this image may evoke, I do not think glorification is the main point Serres is trying to make. It is possible instead to make some different observations. First, Serres, as Whitehead before him, presents a view of science as an adventurous journey in which curiosity and *jouissance* figure as important components. But it is a journey with specific risks and dangers. This is not a very esoteric observation, but one to which people as diverse as Cumbrian sheep-farmers and ACT-Up AIDS-activists can testify. Since we really *do not know* how the landscape we encounter at the end of our journeys will look, this poses to scientists the important challenge of learning how to become responsible for all the entities of our making that, nevertheless, we cannot master.⁵⁹ Likewise, it poses to our societies and institutions the challenge of learning how to respond innovatively to the world-building activities of scientists and others.

Steven D. Brown and Paul Stenner, *Psychology without Foundations* (London: Sage, forthcoming); Vinciane Despret, *Our Emotional Make-Up: Ethnopsychology and Selfhood* (New York: Other Press, 2004); Edwin Hutchins, *Cognition in the Wild* (Cambridge, Mass.: MIT Press, 1995); Smith, *Belief and Resistance* (above, n. 5), chap. 4.

57. Please refer to note 39. The quotation is from Latour, *Science in Action* (above, n. 7), p. 29.

58. The Northwest passage refers to the much-sought-for polar passage that would connect the Atlantic Ocean with the Pacific. Michel Serres uses the term as an analogy for the construction of connections between seemingly nonrelated cultural and scientific ideas and events: see Michel Serres and Bruno Latour, *Conversations on Science, Culture, and Time* (Ann Arbor: University of Michigan Press, 1995), pp. 104, 152.

59. Isabelle Stengers, "God's Heart and the Stuff of Life," *Pli* 9 (2000): 86-118 (Special Issue on "Parallel Processes"), quotation on p. 100.

Two additional points can be made. Throughout the Serresian sea-journey, disorder and uncertainty reign, and multiple contingencies must be handled to prevent the ship from perishing. Only at the *end of the trip* does it make sense to credit the traveler with the courage and rationality necessary for its completion.⁶⁰ But the ordering thus achieved also comes with a price, which is often forgotten: that the traveler is *still somewhere* (albeit somewhere *new*) and for that reason *not everywhere else*. Scientific orderings (practical, institutional, conceptual) thus always come packaged with their own disorders—their forgotten questions, irrelevancies, impracticalities, and other invisibilities—which Marc Berg and Stefan Timmermans have called “orders and their others,” and which Adrian Cussins has pointed to in his theory of cognitive trails.⁶¹

STS and Cultural Critique

Scholars of widely diverging political and intellectual observation have taken issue with a broad array of the claims made here, because of their presumed implications.⁶² From politically motivated scholars, such criticism is often focused on what they take to be the quietistic properties of these positions; that is, they see the unwillingness to formulate clear-cut normative positions (which is to say, the insistence on symmetry) as leading to complicit conservatism.⁶³ Here I want to dwell on some of the sources and consequences of

60. The point is emphasized to provocative effect in Latour, *Science in Action* (above, n. 7), but it has a long history. Louis Menand describes the following as a key tenet of classical American pragmatism: “In the end, you will do what you believe is ‘right,’ but ‘rightness’ will be, in effect, the compliment you give to the outcome of your deliberations. Though it is always in view while you are thinking, ‘what is right’ is something that appears in its complete form at the end, not the beginning, of your deliberation” (Louis Menand, *The Metaphysical Club* [New York: Farrar, Strauss, and Giroux, 2001], p. 352).

61. Marc Berg and Stefan Timmermans, “Orders and Their Others: On the Constitution of Universalities in Medical Work,” *Configurations* 8 (2000): 31–61; Adrian Cussins, “Content, Embodiment, and Objectivity: The Theory of Cognitive Trails,” *Mind* 101 (1992): 651–688.

62. See above, n. 27, for examples. More elegant analyses, in my opinion, include Nick Lee and Steven D. Brown, “Otherness and the Actor-Network: The Undiscovered Continent,” *American Behavioral Scientist* 37:6 (1994): 772–790; Marilyn Strathern, *Property, Substance, and Effect: Anthropological Essays on Persons and Things* (London: Athlone Press, 1999), chap. 5.

63. At this point it is worth remembering that the *powers that be* look rather different from the perspectives here discussed than from traditional perspectives in philosophy and social theory. Unsurprisingly, these differences become consequential for how one might want to engage them.

this kind of interpretation, and I proceed to do so by engaging with a recent commentary by the cultural critic and deconstructionist Sande Cohen, which has had wide circulation as it was printed in the *Science Studies Reader*.⁶⁴ In the following comments, I want to stress primarily how Cohen operates a set of classical critical gestures in his analysis.

Sande Cohen attacks the “language suppression” of Bruno Latour, by which he refers to Latour’s putative inattentiveness to the troping of his own discourse, and the imperialism of his rhetoric.⁶⁵ Attentiveness to language is surely not a bad thing, whereas its opposite might well be. Also, deconstructive strategies have been integrated into STS-studies with increasing sophistication in recent years. My purpose here is therefore not to repeat Latour’s worn attack on deconstruction. In his comments on Latour’s nonhumanism, however, Cohen relies on a number of surprisingly undeconstructed political tropes to achieve his critical effects. I believe that this incongruence accounts for the significant failure of his piece to seriously engage these ideas. More importantly, this is a failure that is shared with many other critical—deconstructive or otherwise—analyses of post- or nonhumanist studies. Let us see how it is played out in Cohen’s text.

In Cohen’s analysis, Latour’s refusal to make use of the classical breaks posited, for example, in history (e.g., “before” and “after” modernity) and anthropology (e.g., “civilization” and “savagery”), and, indeed, Latour’s active attempt to reconfigure these understandings in the mode of networks and practical ontology, suggest that his writing is “suffused with a powerful interest in getting out of ‘history.’”⁶⁶ The presumed move out of history entails that all relevant differences are effaced: “‘No difference’ involves accepting that nothing is separate, and this is part of the ‘new canon of the savant’

64. Sande Cohen, “Reading Science Studies Writing,” in *The Science Studies Reader*, ed. Mario Biagioli (London/New York: Routledge, 1999), pp. 84–94.

65. Latour has certainly spent a good deal of energy demonizing what he refers to as “deconstruction” and “postmodernism.” Such denunciation has become increasingly problematic in light of the sophisticated use to which deconstruction has been put in *science studies* in recent years. See, e.g., Casper Bruun Jensen, “An Experiment in Performative Historiography: Electronic Patient Records as a Future-Generating Device,” *Social Studies of Science* 35 (2005): 241–267; Arkady Plotnitsky, *Complementarity: Anti-Epistemology after Bohr and Derrida* (Durham, N.C.: Duke University Press, 1993); Hans-Jörg Rheinberger, “Experimental Systems: Historiality, Narration, and Deconstruction,” *Science in Context* 7 (1994): 65–81; Rheinberger, *Towards a History of Epistemic Things* (above, n. 8).

66. Cohen, “Reading Science Studies Writing” (above, n. 64), p. 85.

where ‘translation is the very soul of the process of relating.’⁶⁷ It could be argued that Latour’s work presents an attempt to *rethink* history, anthropology, and the entities that make up these endeavors. But from a critical vantage point that has already identified the relevant differences, such an alternative can only be understood as regressive. Cohen’s crucial differences have familiar and horrifying contours, and to deny them can only be seen as deliberately naïve and dangerous—as blurring what ought to remain distinct, and thereby dulling political sensitivity (elsewhere Cohen talks of “passive nihilism”). Latour’s reluctance to make use of these crucial distinctions makes his analyses seem self-evidently out of touch with political reality:

two hundred years of, say, industrial deaths should not shake us out of the belief that capital is merely a network, prosaic and non-violent in its elimination of other, contentious, semantisemes.⁶⁸

Or, again:

the language of enthusiasm turns study into a kind of revivalist salvation, a persistent downgrading of violence in the name of those “tiny networks” with their “minuscule” differences. This involves the very repression WHNB [*We Have Never Been Modern*] claims to reject. One wonders what the forced diaspora of dozens of millions round the world since WWII could mean to this grammar of a flattened world.⁶⁹

If the question is sufficiently narrowly construed, one might indeed wonder what *any* academic text could mean to millions of subalterns. However, one could also point out that scholars from such varied fields as STS, literary history, and development studies are, in fact, already constructively engaging the question of what it *could mean* to think about postcolonial issues in terms of Latour’s “grammar of a flattened world.”⁷⁰ Cohen’s rhetorical posing of the question, and his implied answer—that the grammar of a flattened world can mean *nothing* to the postcolonial subjects—suggests that it is lit-

67. *Ibid.*, p. 89.

68. *Ibid.*

69. *Ibid.*, p. 92.

70. Akrieh, “Gazogene” (above, n. 8); Srinivas Aravamudan, *Tropicopolitans: Colonialism and Agency, 1688–1804* (Durham, N.C.: Duke University Press, 1999), pp. 269–288; Verran, *Science and the Dreaming* (above, n. 12); Sarah Whatmore and Lorraine Thorne, “Nourishing Terrains: Alternative Geographies of Food,” in *Globalising Food: Agrarian Questions and Global Restructuring*, ed. David Goodman and Michael J. Watts (London: Routledge, 1997), pp. 287–304.

erally impossible for him to imagine what might be the point of such explorations; that is, which questions their inquiries might attend to, or which constructive answers they could lead to. His conclusion therefore follows straightforwardly:

In Latour's treatment of science studies, all the "theoretical enigmas" pertaining to language are directly suppressed; science studies is plunged into the discursivity of enthusiasm such that it aggressively precludes any "residue of indetermination" (de Man) which might intrinsically belong to knowledge production or cultural analysis. . . . De Man's notion that epistemic suppression requires the violence of language against epistemology, delivered by aesthetic formations of grammar and logic, seems confirmed.⁷¹

An obvious response to this argument is that far from suppressing language, Latour lets it overflow (I would say elegantly) in a stream of images, tropes, and word-plays, of which some, admittedly, are bluntly targeted at audiences they perhaps need not have aimed at. Exuberance might thus work better than suppression as a characterization of the rhetoric of Latour. This is, in a sense, acknowledged by Cohen's reference to Latour's so-called discursivity of enthusiasm. But Cohen never considers what might be the *deliberate* and *constructive* reasons for this "discursivity." Rather, he believes that Latour is "unable to think such thoughts, no doubt because they evoke a difference which science studies can only acknowledge by negation of difference."⁷² Similarly, when Latour asks "are you not fed up with language games, and with the eternal scepticism of the deconstruction of meaning," he is seen as "unaware that the question telegraphs a terroristic preanswer—'of course we are,'"⁷³ and his language use is presumed to be a (possibly unconscious) reflection of repressive "order-words." One may here note that, substantial and stylistic differences aside, the mode of critical unveiling in which all of this is formulated connects with Collins and Yearley's call for a skeptical sociology, just as Cohen's analysis links up with their criticism of ANT as ultimately prosaic and essentially conservative.

One could also give Latour the benefit of the doubt. It would require, first of all, at least attempting to imagine that there might be constructive (rather than terroristic) reasons for his rhetorical style and practice. This would already make quite a difference, because it would not posit Latour as a benighted writer of his own text. With such an outlook, one could suggest that Cohen has identified a false

71. Cohen, "Reading Science Studies Writing" (above, n. 64), p. 94.

72. *Ibid.*, p. 93.

73. *Ibid.*

problem. But this identification is instructive because it articulates an important difference between a critical and a Latourian (and, I would say, Deleuzian) understanding of writing and reading.⁷⁴ For Latour, as noted, essence is existence is action, and surely texts are effective in many ways. Texts, like other agents, are also delegates that can function in more or less effective and faithful ways for their writers. This, of course, is also a deconstructive insight, and probably one that Latour has in part gleaned from Derrida, although he has never stopped to acknowledge it.⁷⁵ But he draws from this insight a different conclusion than the deconstructionist: whereas Cohen becomes interested in taking apart discourses to show the many layers and internal destabilizations that form the basis of their functioning (notably excepting his own basic political categories), Latour becomes interested in using exactly these capacities of texts to construct new ways of thinking about and acting upon the world.

Maybe this argument is hedged, because Cohen can claim that these are also his goals—but the difference nevertheless remains, because their assumptions of the relationship between texts and readers are quite different. In critical theory and deconstruction, Latour suggests, the problem the writer faces is that the reader is too naïve; too prone to be *taken in* by the subtleties of logocentric discourse.⁷⁶ Deconstruction thus requires that one *first* takes apart established logocentric binaries and *then*, slowly and with due qualifications, indicates alternative possibilities (while usually also emphasizing their ultimate instability). This analytic movement is slow and grinding so as to ensure that the reader cannot escape its consequences and naïvely fall back on his or her preconceptions. For Latour, on the other hand, the reader is presumed to be largely indifferent to the activities of the writer. For the text to function as a successful delegate, what is needed is therefore not (or not always) a rigorous slowness, but a lightness and vividness capable of seizing the reader and holding firm his or her interest. Deconstruction sometimes presents itself as a critical analysis (even as it is reformulating the notion of criticism) simultaneously and interminably pointing to the logocentric assumptions of both the text under investigation and those of its

74. See Daniel W. Smith, "Deleuze and Derrida, Immanence and Transcendence: Two Directions in Recent French Thought," in idem, *Between Deleuze and Derrida* (New York: Continuum Press, 2003), pp. 46–67; Bruce Baugh, "Making the Difference: Deleuze's Difference and Derrida's Différance," *Social Semiotics* 7:2 (1997): 127–146.

75. For example, he could have referred to Jacques Derrida, *Of Grammatology* (Baltimore: Johns Hopkins University Press, 1976).

76. Bruno Latour, "The Politics of Explanation," in *Knowledge and Reflexivity*, ed. Steve Wodgar (London: Sage, 1988), pp. 55–177.

own making. Latourian texts, on the other hand, are constructive efforts using all available means to effectively make a case.⁷⁷

Robert Koch has, I think correctly, identified the position of Latour on this matter—that is, that critique shares with science the problematic gesture, that it “is founded upon the need to establish the metalanguage of Critique in the place that would otherwise be occupied by the object or work under examination.”⁷⁸ Critique and epistemology thus have as a common feature the goal of *policing* the world-building activities they investigate, and of substituting for the specificity of these activities their own generalities. As Koch puts it, “What becomes more pronounced in Latour’s later work is the idea that science and critique are in fact ‘doubles,’ locked together in a fierce struggle for supremacy.”⁷⁹

Interventions and Incommensurabilities

Can one be unhappy with aspects of the world and seriously want to change it, while at the same time denying the (received) grounds for critique? This, of course, has been strenuously denied by the vast majority of philosophers and other scholars over the last two thousand years, and is currently signaled by the impotence and vague (or blatant) heresy ascribed to researchers that can be termed “relativist.” As Barbara Herrnstein Smith documents, even fairly radical such scholars regularly feel the need to defend themselves against these accusations, usually by claiming to “steer between” the dangers of some “naïve” orthodoxy and an “extreme” relativism, often characterized as “Scylla and Charybdis.”⁸⁰

Even sophisticated and friendly minded scholars such as Malcolm Ashmore find the Latourian strategy difficult to understand:

For infrareflexive [one of Latour’s terms for his noncritical style of conducting science studies] writing, on the other hand, you “just offer the lived world and write” (1988:70). The necessary reflexivity is achieved by applying “principles of analysis which are self-exemplifying” (1988:171), by multiplying genres, by

77. This is well put by Barbara Herrnstein Smith, who first suggests that according to Latour’s own model he is *presently* unable to argue for its superiority (as this decision is in the hands of *later users*); as she then goes on to say, however: “On the other hand, the rhetorical energy and power that his account secures at the expense of historical modesty may turn out, in the long run, to be what makes it more effective (and credible) than the more scrupulously modest accounts of his rivals, sociological as well as philosophical” (Smith, *Belief and Resistance* [above, n. 5], pp. 137–138). That, of course, is exactly the point, and the self-exemplification, of the Latourian writing strategy.

78. Robert Koch, “The Case of Latour,” *Configurations* 3 (1995): 339.

79. *Ibid.*, p. 338.

80. On this point see Smith, “Cutting-Edge Equivocation” (above, n. 19).

getting on the side of the known (1988:173), by gaining explanatory equality with those we study, and by refusing to build a metalanguage (1988:174). It would really be very simple if it wasn't quite impossible. As "a story, just another story" (1988:171) Latour's tale of infrareflexivity is a romance; and I've seldom read a better one.⁸¹

Of course, many other criticisms have been considerably less benign. Critically minded scholars have repeatedly, and repetitiously, pointed to the presumed complicity of nonhumanist thinking with its Machiavellian bent, and to its seeming lack of concern for properly emancipatory projects, such as feminism or updated versions of Marxism.⁸²

The move is double. On the one hand Latour and his nonhumanist colleagues are criticized for being too critical of established categories, in their insistence on using a "flattened ontology" where things and people, social and natural entities, institutions and microbes are treated as analytically symmetrical; when Latour's agnosticism infuriates critics it is therefore in part because he works to deprive them of the conceptual tools with which they achieve their critical effects. But if Latour is viewed as too critical in his denunciation of modern categories, this is seen as making him too *uncritical* in not drawing the usual foregone consequences—for his focus on the specificity of situations ("throw-away explanations") and on the transformative capacities of all involved actors prevents him from pronouncing at the end of his story on who were really the bad guys. Why and how is this a problem?

I would suggest that the Latourian stance remains impossible to understand precisely to the extent that one situates oneself as a critical (anti)epistemologist, trying to adequately *represent* the situation under investigation. Under a *representational idiom* it seems obvious that representation is accomplished in a selective fashion, and to the extent that Latour's writing does not match the preferences of the reader, criticism is easily enabled; as, indeed, criticism is *always* easily enabled.

In a *performative* understanding, however, the situation is quite different, for the writer is, first and foremost, a participant, with all the problems this involves. For the critical stance, the first of these is that it becomes impossible to claim for oneself a moral high ground. Or, rather, it is possible to *exactly the same extent* as it is for everyone else in the field. To the *same extent*, however, is not good enough if one wants one's critical perspective to *get an edge over* other positions.⁸³

81. Ashmore, *Reflexive Thesis* (above, n. 7), p. 60.

82. See above, n. 27, for examples.

83. This is precisely the problem analysed by Latour, in "Politics of Explanation" (above, n. 76).

At this point the idea of strategic essentialism is regularly invoked;⁸⁴ that is, the idea that at *certain points* one should cling to ideas otherwise acknowledged as theoretically crude, or even fictive, because it is imagined that doing so offers better opportunities for practically engaging in “the real world” than sticking to one’s more subtle actual beliefs.⁸⁵

It can be questioned, however, whether the adoption of various essentialized concepts from realism or positivism provides for adequate intellectual, political, or practical responses and solutions to the multitude of challenges facing the world today. It might even be argued that this strategy is politically irresponsible in its own way. Jean-François Lyotard, for example, has suggested in *The Differend* that “a universal rule of judgment between heterogeneous genres is lacking in general.”⁸⁶ Since discursive genres often function in resolutely divergent ways, and there is no position from which to formulate an

84. The term “strategic essentialism” is from Gayatri Chakravorty Spivak, who defines it in the following way: “A strategic use of positivist essentialism in a scrupulously visible political interest” (Donna Landry and Gerald Maclean, eds., *The Spivak Reader* [London: Routledge, 1996], p. 214. An important recent example is found in Michael Hardt and Antonio Negri’s *Empire*. Here, the authors withdraw from their otherwise defended post-structuralist inspiration and turn realists when normative pronouncements are called for. They suggest, for instance, that “in the context of state terror and mystification, clinging to the primacy of the concept of truth can be a powerful and necessary form of resistance. Establishing and making public the truth of the recent past—attributing responsibility to state officials for specific acts and in some cases exacting retributions—appears here as the ineluctable precondition for any democratic future. The master narratives of the enlightenment do not seem particularly repressive here, and the concept of truth is not fluid or unstable—on the contrary! The truth is that this general ordered the torture and assassination of that union leader and this colonel led the massacre of that village. Making public such truths is an exemplary Enlightenment project of modernist politics, and the critique of it in these contexts could serve only to aid the mystificatory and repressive powers of the regime under attack” (Michael Hardt and Antonio Negri, *Empire* [Cambridge, Mass.: Harvard University Press, 2000], pp. 155–156). Easy points are scored on this account, as it is impossible to argue with the idea that those responsible for torture and murder should be identified. The willful naïveté of the statement becomes more visible when one considers the complexities and uncertainties that pertain to evincing and displaying evidence, even for what would seem to *Hardt and Negri* quite obvious cases of wrong-doing. See, e.g., Michael Lynch and David Bogen, *The Spectacle of History: Speech, Text, and Memory at the Iran-Contra Hearings* (Durham, N.C.: Duke University Press, 1996); Jean-François Lyotard, *The Differend: Phrases in Dispute* (Minneapolis: University of Minnesota Press, 1988); Smith, *Belief and Resistance* (above, n. 5), chap. 2.

85. This is obviously a “majority concept” in Stengers’s sense: “Furthermore, I do not know which kind of power would define what it is to be anti-racist, anti-classist, anti-sexist, and anti-culturally-coercive. All I know is that if it is in a position to select and direct it will be a majority power, and thus it will demand submission and refuse putting at risk the majority formulation of their values” (Stengers, “From Describing Falling Bodies” [above, n. 45], p. 45).

86. Lyotard, *Differend* (above, n. 84), p. xi.

external yardstick that would decide on their relative merits, the *possibility* of incommensurability remains the background for intellectual and political work. As Barbara Herrnstein Smith phrases it:

The resounding reaffirmation of an absolute distinction between truth and rhetoric, fact and fiction, science and superstition, will not in itself do the crucial substantive, technical and often arduous *work* of effectively differentiating among specific competing, conflicting claims of truth or between mutual charges of falsehood. Nor will a general affirmation of the inestimable value, irrefutable possibility, and transcendent ideality of genuine objectivity identify where, in any particular instance, objectivity lies—or (the pun is apt enough) “lies.”⁸⁷

Interestingly, this suggestion resonates with a formulation from the (noncritical) ethnomethodologist Michael Lynch, who relates the critical urge to “long-standing sociological pre-occupation with exposing and unmasking hidden agendas, back-stage conspiracies and *sub rosa* economies;” why, asks Lynch, do we not ask the constructive question of what makes “practices rational and effective”?⁸⁸

In a sense this is what nonhumanist STS-scholars ask. At the same time, the innocuous ethnomethodological positing of this question also replicates the idea (troubling in its positivist inspiration) that a researcher can become able to “just report what he sees” in those practices (with a suitably developed ethnomethodological method and vocabulary). While pertinent, this response does not quite address (nor does it try to) the nonhumanist question of how we can *stop being (classically) critical* while still *taking seriously our engagement with, and participation in, the field*.

Participations

In view of everything said above, it should be clear that I do not believe that any general method for solving this problem exists. Nor, I think, would my affirmation of such a method solve the issue—since, indeed, many other people have already proposed many other rules and guidelines without much luck. But, of course, this does not *prevent* me from taking a position on the matter and offering what I think are adequate reasons for maintaining it. Contra Ashmore, I think this is one way of achieving what Latour referred to as “getting explanatory equality,”⁸⁹ both with other researchers *and* with those under study.

87. Smith, *Belief and Resistance* (above, n. 5), pp. 29–30.

88. Michael Lynch, “Against Reflexivity as an Academic Virtue and Source of Privileged Knowledge,” *Theory, Culture and Society* 17:3 (2000): 40.

89. Latour, “Politics of Explanation” (above, n. 76); see especially p. 175.

In fact, I think the dictum of explanatory equality is liberating: it liberates the researcher from the belief that there is a specific set of ways by which one can properly engage in research, and a determinable set of proper outcomes of such engagements.⁹⁰ Both of these assumptions are, I think, continually falsified in practice. The suspension of explanatory categories *along with* the urge to critique, at least in principle, enables different research strategies and modes of interaction to be tested with our research subjects, those about whom we talk as scholars. In particular, it forces us to learn to respond more keenly to what *is going on in the field*, because we know that our explanations and interventions *have to be renewed* in each instance. Our research agendas are therefore forced to become increasingly shaped in ongoing engagements with the field.

I think a similar idea is implied by Annemarie Mol's phrase *empirical philosophy*, which she describes as follows:

It is possible to refrain from understanding objects as the central points of focus of different people's perspectives. It is possible to understand them instead as things manipulated in practices. If we do this—if instead of bracketing the practices in which objects are handled we foreground them—this has far-reaching effects. Reality multiplies. . . . Attending to the multiplicity of reality opens up the possibility of studying this remarkable achievement.⁹¹

Being able to follow this "remarkable achievement" does not give the researcher any critical edge in the classical sense of being able to tell *what is wrong*, and what consequently *ought to be done* about it; but, arguably, this becomes less important, because nonhumanist analyses bring with them an *automatic unsettling effect*.⁹² Under such re-description, practices and materialities, their inhabitants and their relationships, start to look different. Good examples include the work of both Marilyn Strathern and Michel Callon and Bruno Latour to refigure the classical micro/macro distinction in social theory: the former accomplishes such a shift by understanding social relationships as fractal;⁹³ the latter, by viewing the presumed macro-actors as micro-actors situated "on top of many (leaky) black-boxes," containing more-or-less stabilized associations between human and nonhuman actors.⁹⁴

90. Casper Bruun Jensen and Peter Lauritsen, "Qualitative Research as Partial Connection: By-Passing the Power-Knowledge Nexus," *Qualitative Research* 5:1 (2005): 59–77.

91. Mol, *Body Multiple* (above, n. 8), pp. 4–5.

92. The term is from Barbara Herrnstein Smith, personal communication.

93. Marilyn Strathern, *Partial Connections* (Lanham, Md.: Rowman and Littlefield, 1991).

94. Michel Callon and Bruno Latour, "Unscrewing the Big Leviathan: How Actors Macro-Structure Reality and How Sociologists Help Them Do So," in *Advances in Social*

Providing such alternative descriptions to people both in and outside cultural and social studies of science can be seen in itself as an intervention, possibly enabling these people to respond differently to their various environments. Although the specifics of this response cannot be controlled by (and in) theory, I think this is already quite an achievement.

The effect of nonhumanist noncritical STS-studies, like everything else, would then be in the hands of later users. These might be scholars or researchers, or they might be “informants,” the old term for the coproducers of the research object. Both of these groups are audiences whose responses to our texts—as they were conceived, produced, and, finally, presented—will have also shaped the way these texts ended up looking. Assuredly, they are also audiences whose engagements with our texts are highly variable.

The continual and dynamic relationship between our own interests and those we encounter, as exemplified by the above-mentioned readers/coproducers of our texts, is very nicely captured, I think, in Gilles Deleuze’s term, *the powers of the false*. Deleuze says:

A new status of narration follows from this: narration ceases to be truthful, that is, to claim to be true, and becomes fundamentally falsifying. This is not at all a case of “each has its own truth,” a variability of content. It is a power of the false which replaces and supersedes the form of the true, because it poses the problem of impossible presents, or the co-existence of not-necessarily true pasts.⁹⁵

The falsity here mentioned is not opposed to truth, as in the classical opposition—for, as in other theories I have discussed at length above, to Deleuze this notion of truth (as universal, decontextual, etc.) is exceedingly dubious. His playful invocation of falsity therefore at most suggests that *if* the standard of evaluation were classical truth, then *we would all be falsifiers*. However, since a standard theory of truth is, in fact, *not invoked*, “the powers of the false” signals rather an opening up toward *collective explorations* of situations that are both important and complex, since no one can claim immediate access to their solution (situations, therefore, of “explanatory equality”).

I think the image of our work and texts as participating in the *powers of the false* is a good one, but not a simple or gratuitous one.

Theory and Methodology: Toward an Integration of Micro- and Macro-Sociologies, ed. Karen Knorr-Cetina and Aaron V. Cicourel (Boston/London: Routledge and Kegan Paul, 1991), pp. 277–304, quotation on p. 286.

95. Gilles Deleuze, *Cinema II: The Time Image* (Minneapolis: University of Minnesota Press, 1989), p. 131.

It is not simple and gratuitous because *no one has promised us anything*.⁹⁶ First, no one has promised us that anyone will indeed become interested in our ideas, plans, or articulations, that we will be able to make them resonate with those with whom we would like to interact. Second, no one has promised us a successful outcome of our endeavors, even if we manage to interest others and collectively produce a new response to the problems with which we are dealing. But perhaps the biggest cognitive and practical challenge relates to the fact that even when disappointments occur—as they inevitably do, due to the noncooperativeness of those with whom we would like to cooperate, or to the noneffectiveness of the interventions we have struggled for—the grounds for making classical criticism are *still gone*; having no means of scapegoating, all there is to do is to start a constructive effort *over again*.

Surely, this way of thinking about the concretizations, as both failures and successes, of one's hopes, ideas, and aspirations, is a galaxy away from many contemporary intellectual engagements.

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96. Stengers suggests that “in most cases, scientists and epistemologists have been in a great hurry to explain this history, to show that the access was deserved and legitimate, the consequence of an ultimate rational method or interrogation. They have made the method, which ensued from the event, responsible for it, and have, as a result, obscured what is essential: *no one has promised anything*, and in particular, no one has promised us that, in all the fields of knowledge, the same type of event will be reproduced” (Stengers, *Power and Invention* [above, n. 31], p. 88; emphasis in original).