



Essay review

## Epistemological derangement

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### **A nice derangement of epistemes: Post-positivism in the study of science from Quine to La-tour**

John H. Zammito; University of Chicago Press, Chicago & London: pp. x+390, Price US\$27.00 £19.00 paperback, ISBN-0-226-97862.

So, the further I get with the description of the radical social constructionist programme and a particular version of postmodernism, coupled to the acid tools of critical discourse in the human sciences, the more nervous I get. *Like all neuroses, mine is rooted in the problem of metaphor, that is, the problem of the relation of bodies and language.*

(Donna Haraway, 1991, p. 185, my emphasis)

I have learned, for example, that many anthropological features of my work have been read by my [science studies] colleagues as sloppy sociology or history. I am not claiming that my work has no weaknesses, but I do find it strange to be read as if I were trying to do sociology, whether English, French, or American.

(Sharon Traweek, 1992, pp. 433–434)

John Zammito has written an important and insightful book that merits close reading and careful reflection from anyone seriously interested in a philosophical, historical, or social and cultural understanding of the sciences. Zammito's book is important not merely because he has reconstructed the past half-century of philosophy and sociology of science with a judicious temperament and the craft skills of an intellectual historian. The book is also especially timely. Zammito's critical assessment of many of the main lines of research within the post-positivist tradition accords with a widespread sense of malaise within science studies (including the kind of 'general' philosophy of science that concerns Zammito),

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and the disdain of many outside the field. So Zammito's diagnosis of where the field has gone wrong, and where it offers more promising alternatives, deserves serious consideration.

The book must also be read with caution, however. Zammito's understanding of science studies is circumscribed by specific philosophical commitments that would be sharply disputed within the field. Where these commitments lead to a contentious treatment of dissenting views, readers can judge for themselves whether Zammito has made his case. Yet Zammito's commitments also render interesting and illuminating developments in science studies utterly invisible (in my view, these are among the most important developments). Moreover, these commitments, and a consequent unfamiliarity with what is omitted or neglected within his account, are widely shared among philosophers. Philosophical readers will therefore readily recognize where the book is genuinely inclusive and fair-minded, while remaining largely oblivious to why other readers will rightly find it narrow and parochial. I share both responses: an appreciation for the book's careful and judicious treatment of some influential scholarly work, and a dismay that it will reinforce a widespread parochialism, blocking better understanding of the sciences and a more inclusive conversation among those who study them.

My ambivalence shapes the structure of what follows. I begin by summarizing Zammito's narrative, its strengths, and the implications he draws for science studies now. I then highlight what I regard as significant omissions and occlusions in this narrative, and the philosophical commitments that I believe lead to them. Despite my resulting reservations about Zammito's moral for science studies, my respect for the book itself significantly outweighs my reservations. Indeed, a real strength of Zammito's clarity and meticulousness is that the limitations imposed by his philosophical preconceptions consequently stand out in sharp relief.

### 1. Zammito's story of epistemic derangement

Zammito's narrative treats Quine and Kuhn as the pivotal initial figures in 'post-positivist philosophy and theory of science since 1950' (p. 1). While their importance is unquestionable, one might have expected an intellectual history of this period to situate Quine alongside Wittgenstein, Goodman, and Sellars, or at least to acknowledge and defend the treatment of Quine alone rather than as *primus inter pares*.<sup>1</sup> That Kuhn stands out so sharply is more readily defensible in retrospect, but it would have been instructive to ask why *The structure of scientific revolutions* emerged as pre-eminent among the historicist criticisms of logical empiricist philosophy of science between 1958 and 1962 (Polanyi, 1958; Hanson, 1958; Toulmin, 1961; Feyerabend, 1962). Zammito regards the prominence of Quine and Kuhn in this period as a cautionary tale, summarized in the titles of the relevant chapters: 'The perils of semantic ascent' and 'Kuhn's misadventures with incommensurability'. The former chapter seeks to disentangle Quine's proclamations of epistemological holism, theoretical underdetermination, referential inscrutability, and translational indeterminacy, through a close reading of the interpretive and critical secondary literature. The latter chapter similarly looks to the philosophical reception of Kuhn, in the well known criticisms by Shapere, Scheffler, and others, and subsequent work

<sup>1</sup> A yet more comprehensive consideration of the philosophical background to subsequent science studies would have to take note of Foucault and Derrida's contemporary criticisms of Husserlian phenomenology as well. For those not yet convinced of the relevant parallels between Carnap and Husserl, see Rouse (2002), Ch. 1.

in the philosophy of language by Davidson, and by proponents of a causal theory of reference. For Zammito, the moral of this tale is clear: the “‘linguistic turn’” in philosophy of science, the view that [its] problems could be resolved (only) by semantic ascent into philosophy of language’ (p. 66) was a serious mistake.

Subsequent chapters primarily trace Kuhn’s legacy in science studies rather than Quine’s legacy in philosophy. The key developments discussed are the ‘failed marriage’ of philosophy and internalist history of science, the Strong Programme in the sociology of knowledge and its multiple sociological descendants, and feminist epistemology. The ‘Science Wars’ of the 1990s provide a fitting conclusion to his story, although not because Zammito finds serious intellectual merit in the critical interventions by the likes of Gross and Levitt, Sokal, or Koertge. Their specific criticisms substantially miss the mark on his account, but their critical message is nevertheless vindicated by the internal dissolution of many of the targeted research programs. Zammito’s narrative is not one of unrelieved fiasco, for in each chapter he commends exemplary, philosophically modest successes that belie the surrounding failures of more epistemologically extravagant projects. Yet even these successes offer small comfort to anyone seeking the philosophical exhilaration that once accompanied the major episodes in his story. These successes are apparently not models for *philosophical* emulation, for Zammito seems to discern no unresolved issues motivating further philosophical reflection upon science. Implicitly, the sensible future he projects for interdisciplinary science studies seems to be one of empirical inquiry largely devoid of philosophical pretensions.

The strength of the book is unquestionably Zammito’s careful tracking of the argumentative twists in internalist history and philosophy of science (HPS), and the positional permutations within epistemologically ambitious sociology of scientific knowledge (SSK). He has read both literatures in extraordinary detail and with considerable insight. Having followed the course of both developments at the time, I encountered repeated shocks of recognition in Zammito’s retelling. Long-forgotten papers, and the disputes they once engendered, came back to mind quite clearly, in substantial part because Zammito tellingly captures not only their claims and arguments, but their sense of what was at stake. Of course, good intellectual history provides a narrative coherence unavailable at the time, with the attendant satisfaction of settling what was once unsettled. Yet part of what makes Zammito’s larger story in these chapters convincing is his rendition of constituent details. Moreover, Zammito’s authorial voice is least intrusive here. *He* need not to tell us where and how various attempts went wrong in rewriting the history of science as a philosophical reconstruction, or why successive grand programs in the sociology of scientific knowledge went astray, for he takes that argumentative work to have been accomplished already by their philosophical and sociological contemporaries. Zammito convincingly assembles these developments to make intelligible how these projects could have seemed plausible and even convincing despite fundamental difficulties apparent even to contemporary colleagues.

The thoroughness with which Zammito unpacks the history of SSK may be especially illuminating to philosophers. The Edinburgh School and its successors only came to philosophical attention quite late in their development. The intellectual roots of SSK, and its subsequent internal divisions, have thus been obscure to many philosophical readers. Zammito locates the early papers of Mulkay, Barnes, and Bloor in post-1960s criticisms of the complacency of Mertonian and Parsonian functionalism, and traces their relations both to earlier projects in the sociology of knowledge, and to the anthropology of Mary Douglas and the philosophical work of Mary Hesse and Peter Winch. He then carefully tracks the rapid proliferation of competing theoretical programs, from Barnes’s

and Bloor's aspiration to a causal explanation of belief, through historical case studies (Pickering, 1984; MacKenzie, 1981; or Shapin, 1982), Latour's (Latour and Woolgar, 1979) and Knorr-Cetina's (1981) European ethnographies of California laboratory life, Gilbert and Mulkay's (1984) discourse analysis, Collins's (1985) controversy studies, the actor-network theory of Latour, Callon, and Law (Latour, 1987; Callon, 1986; Law, 1987), Lynch's (1985) ethnomethodology, the radical reflexivity of Woolgar (1988) and Ashmore (1989), and Pickering's (1995) later treatment of scientific practice as a 'mangle'.<sup>2</sup> Central to Zammito's account, however, is Shapin and Schaffer (1985), a book conspicuous both for avoiding intramural theoretical argument, and being acknowledged as an important achievement on all sides (even by strident philosophical critics). His treatment of Shapin and Schaffer best illustrates the constructive side of Zammito's contentiousness. Readers may disagree with his unsparing theoretical criticism, but his treatment is fair, well-informed, and appreciative of its historical erudition and central role in renewing historical attention to experimentation. Zammito locates the denouement of this entire tradition in some bitter disputes arrayed in Pickering (1992). Zammito's conclusion extends to the entire field Collins and Yearley's (1992) objection to the projects of Callon, Latour, and Woolgar. Collins and Yearley urged return to an epistemological orthodoxy drawn from Quine, Kuhn, and Bloor's Wittgenstein, abandoning other sociologists' supposedly ever-more-radical, extravagant, and unwarranted extensions of early SSK's core sociological and epistemological commitments. Zammito disagrees only in thinking that the field had already crossed over into epistemic 'derangement' at its very outset, not just in moving beyond its initial programmatic commitments.

Zammito offers a more sympathetic, but briefer and less comprehensive treatment of feminist science studies. Even in briefer compass, he is admirably attentive to the larger intellectual context of feminist work, such as debates about essentialism and identity politics. He rightly connects Haraway's historical studies of field primatology and epistemological reflections upon 'situated knowledges' with the politics of her 'Manifesto for cyborgs', for example. In the end, however, Zammito follows Harding's early tripartite taxonomy of feminist philosophical conceptions of science as empiricist, standpoint-theoretic, and postmodernist conceptions of scientific *knowledge*. His own sympathies clearly lie with feminist empiricism, with Helen Longino's work presented as exemplary (although he takes Haraway's 'Situated knowledges' to move constructively 'toward a more fruitful dialogue with "feminist empiricism"'); p. 216). Indeed, Zammito concludes with a pointed rejoinder to a flurry of recent attacks on the very idea of feminist epistemology:

There are no uniquely feminist methods of explanation. There is no distinctly feminist science. But there is trenchant feminist critique of practices and conceptions in science. And doing science as a feminist has begun to impact a variety of fields, even in the natural sciences. (p. 224)<sup>3</sup>

<sup>2</sup> Notably, Zammito only mentions Michael Lynch's work, but does not discuss it. It is not obvious that his criticisms of other sociological descendants of SSK are applicable to Lynch.

<sup>3</sup> Those unfamiliar with the feminist literature should not infer from this quotation that any prominent participants in feminist science studies advocated uniquely feminist methods or distinctively feminist science, nor even that Zammito attributes such views to them. He is instead, rightly, rejecting various critics' insistence that that is what it *must* mean to reflect upon science *as* a feminist.

Sokal's *Social text* hoax provides a symbolic climax to Zammito's story:

If Woolgar's radical reflexivity is, for me, the internal *reductio ad absurdum* of post-modernism in science studies, I see the Sokal affair as the external—public and political—*reductio* of that same misguided impulse. The Sokal affair exposed the hyperbolic character of postmodernist 'theory' in science studies that is my overarching concern. (p. 251)

Zammito's stance toward the 'Science Wars' is nevertheless ambivalent. He sharply criticizes the most prominent salvoes against science studies: Gross and Leavitt (1994), for example, comes off as a self-proclaimed defense of rationality whose own critical practice is contemptuous of even minimal standards of conceptual clarity and evidence. Yet he also finds many defensive responses to Sokal's hoax fatuous and disingenuous. More centrally, he endorses the blanket rejection of epistemological and theoretical 'postmodernism', and indeed, says that vindicating the rejection of its supposed hostility to empirical inquiry was his principal motivation in writing the book. On its face, this claim seems odd, since the book primarily discusses naturalistically-inclined philosophy and sociology of science and never engages prominent post-structuralist or postmodernist theoretical programs in the social sciences and humanities. Yet Zammito's target is not cultural theory directly, but instead rhetorical uses of post-positivist science studies he attributes to some of its proponents. He seeks to demolish any rationalization of a disdain for empirical inquiry and evidential norms defended by ironic invocation of science studies as having 'established' observational theory-ladenness, theoretical underdetermination, cultural relativism, and radical historicism (pp. 269–270).

My response to Zammito's discussion of the Science Wars is ambivalent in turn. He is typically judicious and insightful in discussing specific contributions, from Gross and Levitt's posturings to the more substantive exchanges between Norton Wise and Steven Weinberg. Yet his discussion also replicates a troubling feature of the public contretemps. These debates conflate quite distinct fields and research programs, often drawing inferences about one field from arguments against another. Zammito is not immune to these temptations, especially when invoking the term 'postmodernism'. In a striking one-sentence dismissal, for example, he defends those who regard Sokal's hoax as an indictment of science studies: 'the contributions of figures like Sandra Harding, Steve Fuller, Dorothy Nelkin, Sharon Traweek, and others are hardly without a strong post-modernist intellectual agenda' (p. 260). As I argue below, Traweek (whom Zammito otherwise ignores) belongs to a different tradition in science studies that rejects the *epistemological* theses he associates with 'postmodernism' more fundamentally than he does. We can thus only assess Zammito's treatment of post-positivist science studies if we look more broadly to the unstated philosophical commitments that shape his vision of the field.

## 2. Limitations of Zammito's narrative

Zammito's unstated presupposition, shaping the entire book, is that the central philosophical issues confronting science studies are epistemological questions about the legitimation of empirical inquiry. Moreover, he takes empirical inquiry itself to depend upon overcoming skeptical or relativist answers to these questions:

Some ‘theorists’ have drawn upon post-positivism to initiate an attack upon the *practice of empirical inquiry* itself. . . . Certain extravagant gestures in philosophy of language, when taken seriously, threaten to undermine indispensable canons of empirical inquiry. (p. 1)

Zammito thereby commits two fundamental mistakes from the very outset: taking empirical inquiry to depend upon its wholesale epistemological legitimation, and misinterpreting important work in the philosophy of language and science studies as providing ‘deranged’ answers to epistemological questions, rather than setting those questions aside to get at more interesting and important issues. These mistakes are not simply oversights by Zammito, however, but are accepted by many philosophers also, and shape the public debates over the significance of science studies. Moreover, Zammito rightly criticizes some post-positivist developments in these terms: internalist HPS, SSK, and some feminist scholars share his commitments, along with those philosophical critics whom Zammito chastises for their untenably stringent epistemological standards. And he is surely right that some scholars in the humanities mistakenly celebrate the supposedly radical consequences of such epistemic derangement.

Zammito almost completely overlooks a different body of work, however, that challenges the terms in which his narrative is written, and yet is now more central to science studies. A prominent, field-defining anthology (Biagioli, 1999) emphasizes this alternative tradition, with few residual traces of the epistemological positions that concern Zammito.<sup>4</sup> A fundamental oddity in Zammito’s account highlights the significance of omitting this alternative tradition from his narrative. Supposedly, epistemic derangement results primarily from the deleterious effects of the philosophy of language within philosophy of science and science studies:

Probably the most extravagant theses in philosophy of language have been the ‘theoretical’ uptakes of Thomas Kuhn’s notion of ‘incommensurability’, of Willard van Orman Quine’s dogma of ‘indeterminacy of translation’, of Donald Davidson’s modulation of that into ‘indeterminacy of interpretation’, and—in a different tradition—of Jacques Derrida’s disquisitions on *différance*. (p. 1)

Yet Zammito nowhere discusses Derrida, and incommensurability and especially indeterminacy virtually disappear from the book after Chapter 3, along with philosophy of language more generally. The SSK tradition never made significant appeal to Quinean or Davidsonian indeterminacy, for example, and was much more influenced by Kuhn’s account of normal science than by his conception of incommensurability.<sup>5</sup>

Reflection upon language *is* central to more recent work in science studies, however. Most of that work has not drawn upon the tradition stemming from Quine, Goodman, Davidson and Sellars, but many of their philosophical concerns are continuous. If,

<sup>4</sup> The authors who do appear in both Biagioli’s anthology and Zammito’s narrative (notably Haraway, Latour, Callon, Schaffer, and Shapin) include some whose work moves in new directions from the work Zammito criticizes, and others where I find Zammito’s treatment problematic (see below).

<sup>5</sup> Latour might seem to be an exception. Zammito notes, for example, that *Laboratory life* describes scientists as attaching ‘modalities’ to statements. These modalities do not, however, determine semantic content or reference, but instead mark *epistemic* attitudes. When Latour does develop a more discursive conception of scientific practice, in his conception of ‘translation’, Zammito takes only passing notice (p. 194). See my further discussion of this issue below.

counterfactually, one were to reconstruct its guiding assumptions in Anglo-American terms, one might begin with Goodman's (1983) new riddle of induction. Goodman proposed a telling shift in philosophical concern, from whether evidence was sufficient to justify a hypothesis, to the questions of which hypothesis the evidence confirmed and what concepts are projectible. Davidson's (1986) later work amplifies this concern with conceptual interpretation: if 'there is no such thing as a language', then there is some indeterminacy to the context of other statements and actions within which an interpreter should make sense of it.<sup>6</sup>

Haraway's work tellingly exemplifies this shift in philosophical concern, for despite Zammito's scrupulous attention to her writings, he significantly misses the point. Haraway indeed rejects the 'epistemic derangements' of skepticism and relativism, but not because she hopes to vindicate a more modest epistemological position. She rejects the questions that a skeptic or constructivist-relativist would ask (as attempts at 'the god-trick'), rather than arguing against their answers. She calls for:

a different kind of strong program in science studies, one that really does not flinch from an ambitious project of symmetry that is committed as much to knowing about the people and positions from which knowledge can come and to which it is targeted as to dissecting the status of knowledge made. . . . No layer of the onion of practice that is technoscience is outside the reach of technologies of critical interpretation and critical inquiry about positioning and location; that is the condition of articulation, embodiment, and mortality. (Haraway, 1997, pp. 36–37)

Haraway's aim is 180 degrees from a general epistemic debunking of the sciences: she thinks scientific work has been integral to how we make sense of ourselves and our political situation, and critically engages the meaning-making practices of the sciences in order to contest some interpretations of their significance. Her aim is not a retrospective assessment of whether scientific statements are justified, but a reinterpretation of how they inform our scientific and political prospects, in part by re-interpreting their significance in a broader cultural context. Indeed, a central theme throughout Haraway's career has been to make scientific work more integral to contemporary cultural and political imagination. Zammito's insistence upon reading feminist science studies in epistemological terms distorts not only Haraway's work, however. The philosophical continuity in Evelyn Fox Keller's work, in which her early work on gender in science prompted sustained critical reflection upon language and modeling in the life sciences (Keller, 1995, 2002, 2003) becomes opaque if one frames the issues epistemologically. Karen Barad (1996, 1998, 1999) whose work on ontological issues shifts feminist science studies away from 'gender' altogether, is likewise implicitly read out of the field.

In bypassing Zammito's questions, which have been central to *both* SSK and the dominant philosophical tradition, Haraway, Keller, and Barad are joined by scholars from other science studies disciplines (for some exemplary theoretical articulations, see Bono, 1994; Rheinberger, 1994; Edwards, 1996, Ch. 5, in addition to Haraway, 1991, Ch. 9, and Barad, 1996). History and especially anthropology of science have been primary contributors to these new directions in science studies. In the case of history of science, Zammito himself notes (p. 113) that the divorce ending the 'failed marriage' of HPS was initiated by the historians, who found philosophers' questions increasingly alien to their

<sup>6</sup> Although Zammito adapts his title from this important essay, surprisingly he never discusses its substantive relevance to science studies.

concerns. The only issue he mentions, however, concerns the epistemological standing of history itself, and not the divergence of historians' and philosophers' concerns in writing about *science*. The more striking omission from Zammito's story, however is his complete neglect of anthropologically-conceived alternatives to SSK (e.g., Downey & Dumit, 1997; Layne, 1998; Traweek, 1988; Martin, 1994; Rabinow, 1996; Helmreich, 1998; Franklin & Lock, 2003; Goodman, Heath & Lindee, 2003—sociologists influenced by symbolic interactionism also belong here, e.g. Fujimura, 1996; Clarke, 1998; Star, 1989). The polemical exchange between Latour and Traweek over what it means to study science anthropologically takes place, after all, within papers and volumes (Latour, 1990; Pickering, 1992) that are central to Zammito's story. Moreover, Traweek's (1992) paper is also a telling rejoinder to Woolgar's conception of reflexivity, which Zammito reads as the extreme of epistemic derangement.<sup>7</sup> The reflexive considerations that prompted Woolgar toward a more 'radical' epistemological questioning guide Traweek instead toward a very different consideration of political positioning and strategies for making sense within science and science studies.<sup>8</sup>

What is at stake in belaboring these historical issues is the direction of science studies and its relation to contemporary philosophy. Zammito takes the history of post-positivist philosophy of science and science studies to have reached an impasse in the early 1990s. He seeks a way out of that situation through a modestly historicist, 'naturalist' epistemology that circumvents questions about language or reality, proposing the work of Larry Laudan, Helen Longino, and Dudley Shapere as exemplary. I offer a different narrative frame for the same events, and a different moral. Questions about whether evidence justifies a scientific hypothesis are often of great scientific and practical import, but such questions are not amenable to general philosophical solution. Critical reflection upon the relation of hypothesis and evidence is always local and contextual.<sup>9</sup> The travails of HPS and SSK illustrate yet again that the epistemological turn has been a philosophical dead end, even when it takes the more modest, sensible forms Zammito defends.<sup>10</sup>

In the very period around 1990 when Zammito characterized epistemologically ambitious science studies as having stalled, however, anthropologists, and some historians, sociologists and philosophers were already taking the field in some quite different, and to my mind far more promising directions. The familiar epistemological questions

<sup>7</sup> Zammito also does not discuss Haraway's (1997, pp. 33–39) criticism of Woolgar on reflexivity.

<sup>8</sup> In fairness to Woolgar, his criticisms of 'positionism' are probably better read not as a more radical version of 'Cartesian' epistemological skepticism, but as analogous to the 'classical' skepticism of Sextus Empiricus or Montaigne, in which skeptical arguments are employed as a practical guide to a less dogmatic way of life rather than to justify a 'theoretical' claim about knowledge or representation. In fairness to Zammito, Woolgar nowhere explicitly attends to this difference.

<sup>9</sup> Lloyd (2005) exemplifies how such local epistemic criticism remains a vital part of science studies.

<sup>10</sup> I cannot argue the point here, but once one allows general epistemological questions to get off the ground, their corrosive effects are hard to contain. Zammito himself nicely illustrates the dilemma. Having celebrated Longino's modestly historicist epistemological views, he concludes with a cautionary remark: 'this reader remains troubled by the question, "what is left to adjudicate scientific disputes?"' (p. 222). He does not consider whether this question admits of an adequate answer, for Longino or anyone else who accepts assumptions about knowledge that make such questions seem inescapable. Contrast Robert Brandom's principled refusal to answer such questions: 'Sorting out who should be counted as correct . . . is a messy retail business of assessing the comparative authority of competing evidential and inferential claims. . . . There is no bird's-eye view above the fray of competing claims from which those that deserve to prevail can be identified, nor from which even necessary and sufficient conditions for such deserts can be formulated' (Brandom, 1994, p. 601).



obscure a different theoretical issue, which has also recently returned to the center of philosophical attention in metaphysics and philosophy of language. Haraway (1991, p. 185) highlights this concern when she located the source of her own anxiety about social constructivism in ‘the problem of metaphor, that is, the problem of the relation between bodies and language’ (most philosophers will be more familiar with Sellars’ or Davidson’s formulations in terms of how causes and reasons bear upon one another).<sup>11</sup> This issue re-emerged in philosophy precisely at the point at which Zammito’s discussion of philosophy of language breaks off after Chapter 3, with the rejection of *both* semantic empiricism (the claim that a non-discursive ‘given’ can directly adjudicate the application of concepts) and intra-linguistic holism (the claim that conceptual norms can only be settled intra-linguistically). The work of McDowell (1994), Brandom (1994), and Haugeland (1998), among others, stems from their criticisms of Davidson and Rorty, with whose views Zammito ends his discussion of philosophy of language. A virtue of much recent work in anthropology, history or sociology of science is that it contributes to such central philosophical discussions by attending to the details of experimental work, theoretical modeling, cultural interpretation, and the simultaneous effects of scientific work upon how we think and the world we live in. Whether such work by anthropologists, historians, philosophers, and sociologists yields important insight into or ways around these perennial philosophical concerns remains to be seen.<sup>12</sup> We cannot even ask that question, however, until we get beyond *epistemological* derangement, the obsessive insistence that intellectual engagement with science must be read in epistemological terms.

Even the developments that Zammito discusses do not always fit seamlessly within the narrowly epistemological terms of his narrative. Zammito expresses (p. 37) and displays perplexity about just what Quinean or Davidsonian indeterminacy means. Part of the difficulty is that he is inclined to read it as an epistemological thesis, akin to the underdetermination of theories, rather than as setting the stage for questions about the relation of bodies and language or causes and reasons.<sup>13</sup> Zammito’s discussion of Kuhn is perplexing in a different way. For all his care in exposition, Zammito’s account would lead readers unfamiliar with the field to expect Kuhn’s book to have faded into obscurity after the criticisms of Shapere, Scheffler, Davidson, and others. A very different and more promising view of Kuhn arises, however, if one reads his attack on ‘the textbook view’ of science (1970, Ch. 1) as also rejecting the epistemological orientation with which Zammito and Kuhn’s philosophical critics interpret the book itself.<sup>14</sup>

Latour’s work after *Laboratory life* poses yet a different problem for Zammito’s narrative. Latour rejects the kinds of ‘epistemic derangement’ that most concern Zammito, from the social constructivism of Shapin and Schaffer to the radical reflexivity of Woolgar, and Zammito acknowledges that stance. He also attempts a sympathetic exposition of Latour,

<sup>11</sup> Zammito (p. 215) truncates his quotation of Haraway’s objection to social constructivism and postmodernism, omitting her insistence that ‘the relation between bodies and language’ is the central concern.

<sup>12</sup> My own views on the contribution of recent science studies to the philosophical issues central to the neo-pragmatists are developed in Rouse (2002).

<sup>13</sup> Zammito gives considerable attention to Rorty’s (1972) influential claim that translational indeterminacy should be regarded as just a special case of theoretical underdetermination, but does not indicate that Rorty (2000) now explicitly repudiates that interpretation of Quine.

<sup>14</sup> Rouse (2003) explicitly contrasts the familiar philosophical (and sociological) interpretations of Kuhn to one that makes his work more consonant with the tradition in science studies that Zammito neglects.

seeking to encompass simultaneously Latour's whimsical sense of humor, the extraordinary scope of his philosophical ambitions, and his temperament as a fox rather than a hedgehog. Yet in the end, Latour evokes in Zammito an impatience strikingly absent from his similarly sympathetic exposition of Longino or Pickering. The locus of his exasperation is instructive, however. First, he is unhappy with Latour's unwillingness even to ask the epistemological questions that remain central to his own account:

With personification run amok, Latour can play the devil with the line between ontology and epistemology. 'It is not possible to distinguish for long between those actants that are going to play the role of "words" and those that will play the role of "things" [Latour, *Irreductions* 2.4.4]. ... Is there no difference? Ontology and epistemology seem to be perilously close to blending. (p. 187)

Here, I think, Zammito fails to recognize (or at least accept) Latour's engagement instead with the issue that also preoccupies figures as diverse as Haraway ('the problem of bodies and language') and McDowell ('Davidson manages to be comfortable with his [epistemological] coherentism ... only because he does not see that emptiness is the threat'; McDowell, 1994, p. 68).<sup>15</sup> Zammito then attributes to Latour a different sort of 'derangement' in attempting an impossibly ambitious first philosophy:

Latour goes hyperbolic in his lust for origins, for the primordially seamless beginning of knowledge, of history, of language. He forgets what he surely knows: that we only have *situated knowledge*. ... Latour missteps in the hypermodernity of his impatience with available language, with ongoing disciplinary frames, with the extraordinary accretion of indispensable black boxes, without which we just do not travel well, even in networks. (p. 202)

This criticism has some plausibility if one tries to read *Irreductions* or *Science in action* as self-contained treatises, making claims whose truth values as philosophical theses are intended to be assessed independently. That seems to me to misread the genres of Latour's various writings, however. It matters that the oracular invocations of *Irreductions* were not published separately, but only 'following' his detailed study of Pasteur.<sup>16</sup> *Science in action* is also not a self-contained treatise, but is instead presented as a response to the entire field of science studies, an attempt to 'translate' its achievements into a different frame, while *We have never been modern* similarly engages the post-Kantian philosophical tradition in Europe. Zammito seems to worry that Latour is trying, impossibly, to make claims that would adequately express the seamless relationality of everything to everything else. I think Latour is better read as attempting a variety of interventions into the conversations and practices in which we are already taking part, experimentally disrupting our taken-for-granted categorizations in order to point out what has been omitted or distorted, without having to claim that his own attempts to articulate that relationality are seamless:

Our intellectual life is out of kilter. Epistemology, the social sciences, the sciences of texts—all have their privileged vantage point, provided they remain separate. If the creatures we are pursuing cross all three spaces, we are no longer understood. ... In the eyes of our critics the ozone hole above our heads, the moral law in our hearts,

<sup>15</sup> See also note 6 above.

<sup>16</sup> The original title of Latour (1988) was *Les microbes: Guerre et paix suivi de irréductions* (Paris: Editions A. M. Métailié, 1984).

the autonomous text, may each be of interest, but only separately. That a delicate shuttle should have woven together the heavens, industry, texts, souls and moral law—this remains uncanny, unthinkable, unseemly. (Latour, 1993, p. 5)

I think Latour here accurately expresses the tone of Zammito's otherwise careful and accurate exposition of his work: 'uncanny, unthinkable, unseemly'. I do not always read Latour without misgivings, but Zammito's response to Latour called to mind David Lewis's reported remark that 'an incredulous stare is not an argument'.<sup>17</sup>

Perhaps the most interesting point of dissonance, however, arises in Zammito's discussion of Andrew Pickering and Peter Galison. Zammito (pp. 225–231) is especially insightful in discussing Galison and Pickering's prominent disagreements over whether 'constraint-talk' has a constructive role for understanding real-time scientific practice. Yet in rightly finding common ground between Galison and Pickering's epistemological views, he takes no notice whatsoever of a deep divide between them on a different score. Pickering (1995) ironically exemplifies Zammito's vision of science studies with no role for language. Pickering's conception of scientific practice has no place for *discursive* practice: concepts are just one more element to be 'mangled' in practice, along with instruments, experimental methods, and political effects. Galison's (1997) account of 'trading zones', by contrast, treats them as sites of discursive articulation, even when they involve substantial causal interventions. In replacing a conception of understanding modeled upon translation with 'the coordination of action and belief' (1996, pp. 827–840), modeled on linguistic pidgins and creoles, Galison's historiography aligns with Haraway and the anthropologists of science, whereas Pickering's model makes it hard to understand how scientific or political change makes sense and matters to anyone.

One final consideration is important for philosophers to bear in mind. The omissions and narrowly epistemological interpretations that shape Zammito's story make less comprehensible the deep and abiding anger over the Science Wars in some other areas of science studies. Zammito refers to one of Elisabeth Lloyd's (1995, 1996) powerful polemics in a footnote, but does not actually mention her thesis that the real enemies of objectivity and rationality are those who seek to silence important scholarly work, by intellectually disreputable means, with the tacit collusion of many philosophers and scientists who ought to know better. With the notable exception of Haraway, most of the anthropologists, historians, feminist theorists, and the occasional philosophers and sociologists whose work I take now to be most central to interdisciplinary science studies were not explicit targets of polemics, hoaxes, or pleas against 'epistemic derangement'. Yet they know full well that these public pronouncements sought to define the boundaries of legitimate scholarly work on science, and did so in ways that leave much of their work on the outside. We will not have adequately overcome the intellectual pathologies of the Science Wars until, along with rejecting the epistemic derangement that Zammito rightly criticizes, scholars throughout the field recognize and respond to other options for philosophy of science and science studies besides those conceived (or misinterpreted) in familiar epistemological terms.

<sup>17</sup> Haraway (1997), pp. 33–39 offers a more telling criticism of Latour's rhetorical strategies and authorial positioning.

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