

Philosophy of Science and Science Studies in the West: An Unrecognized Convergence

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Abstract During the 1990s the dominant research programs from the 1970s and 1980s in both philosophy of science and interdisciplinary science studies seemed to reach dead ends. During the same period, new and interestingly convergent research programs emerged in both philosophy of science and science studies, but their mutual relevance was not widely recognized. In earlier work, I called attention to important developments in anthropology, history, and feminist scholarship as “cultural studies of science” that rejected epistemological assumptions widely shared among both philosophers and sociologists of scientific knowledge. Here I highlight four subsequent developments in philosophy that constructively engage this new work in science studies: (1) a resurrection of interest in causality, (2) work on theoretical models and their relations to model laboratory systems, (3) finer-grained accounts of conceptual articulation in the sciences, and (4) broader philosophical work on the relation between conceptual understanding and perceptual and practical skill.

Keywords Philosophy of science · Science studies · Causality · Experiment · Models · Conceptual understanding

Academic disciplines convene to reassess the state of their field and its future direction when they are no longer confident they know where they are going. In recent years, multiple conferences have been called to consider where science studies are and

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should be going.¹ The embarrassing public controversies of the “science wars” in the late 1990s undoubtedly set the stage for such reassessments, as several scientists and a few colleagues in science studies publicly questioned the credentials and motives of prominent scholars and influential research programs (Ross 1996; Koertge 1998; Editors of *Lingua Franca* 2000). Yet historian John Zammito (2004), in a richly detailed intellectual history of postpositivist philosophy of science and science studies, persuasively argues that the public controversies arose at a time when the primary research programs in philosophy and sociology of science had already lost their way.² For Zammito, by the mid-1990s, the fragmentation of postfunctionalist sociology of science and the intellectual exhaustion of postpositivist philosophy had set the field adrift.

My assessment of the state of these disciplines offers a counterpoint to Zammito’s diagnosis. I use Zammito’s diagnosis as a foil for my account both because his criticisms of earlier approaches are insightful and because the assumptions that lead him astray are widely shared among philosophers of science. I believe that during the 1990s, when Zammito (and others) rightly called attention to the dissolution of the dominant research programs from the 1970s and 1980s, both philosophy of science and interdisciplinary science studies also embarked on other exciting and mutually reinforcing lines of research. These new directions have continued to flourish since then, despite their lack of the scholarly visibility that provided earlier research programs with recognizable acronyms, such as history and philosophy of science (HPS) or sociology of scientific knowledge (SSK). This lack of recognition should not surprise us. These new initiatives have taken place in various specialist enclaves. Their practitioners often do not read one another or communicate at the same conferences. They write in mutually strange idioms. In some cases, their very existence is unknown to one another.

I have been arguing for the importance of part of this new synthesis for a long time. In a widely cited article in the inaugural issue of the journal *Configurations* (Rouse 1993), I argued that feminist scholars, anthropologists, historians, and some sociologists and philosophers were pursuing new directions in science studies that rejected some widely shared epistemological assumptions among both philosophers of science

¹ Examples include a Mellon Conference titled “What Is Science Studies?” at the University of Chicago in 2005 and a meeting of the Boston Colloquium for the Philosophy of Science in 2007 titled “After the Science Wars: Whither Science Studies?” The former meeting invited mostly sociologists, historians, and anthropologists of science; the latter primarily included philosophers and sociologists of science.

² Zammito’s title, *A Nice Derangement of Epistemes*, may seem strange and indecipherable. The title involves a double play on the title of a famous article in the philosophy of language by Donald Davidson (1986), “A Nice Derangement of Epitaphs.” Davidson’s article addressed the question of how listeners can understand what speakers mean even if their words make no literal sense. His title was taken from a line in Richard Sheridan’s eighteenth-century play *The Rivals*, whose character Mrs. Malaprop frequently used the wrong word or ungrammatical constructions and yet was nevertheless understood by her listeners, to humorous effect. Such understandable errors are thus now known as “malaprops,” after this character. When Mrs. Malaprop said, “That’s a nice derangement of epitaphs,” what she actually meant was “That’s a nice arrangement of epithets.” Zammito then substituted Foucault’s term “episteme,” which denotes successive periods in the history of science and knowledge in which different governing assumptions changed how knowledge was formulated and assessed. Thus, his title was intended to suggest that philosophy and sociology of science since Quine and Kuhn have presented crazy or “deranged” accounts of scientific knowledge, accounts whose derangement resulted from their excessive epistemological holism and historicism.

and sociologists of scientific knowledge.³ Twelve years later, I argued in an essay review of Zammito's book (Rouse 2005) that his otherwise richly informative history of science studies omitted or obscured this same body of work and its successors. Among the scholars whose work exemplified these new directions are feminists such as Donna Haraway (1989, 1991, 1997), Karen Barad (2007), or Evelyn Fox Keller in her later work (1995, 2000, 2002); anthropologists such as Sharon Traweek (1988, 1992, 2000), Emily Martin (1994, 2007), Paul Rabinow (1996a, 1996b, 1999), Sarah Franklin (1997, 2003, 2007), or Stefan Helmreich (1998, 2009); such historiographically reflective historians as James Bono (1990), Hans-Jörg Rheinberger (1997), Paul Edwards (1996), Peter Galison (1997), Timothy Lenoir (1997), or V. Betty Smocovitis (1996); symbolic interactionist sociologists like Joan Fujimura (1996), Adele Clarke (1998; see also Clarke and Fujimura 1992); Leigh Star (1989), or Charis Thompson (2005); and even the occasional philosopher, such as Annemarie Mol (2002), Arnold Davidson (2001), Alison Wylie (2002), or myself. A representative and influential sample of such work has been collected in Biagioli 1999. Zammito is not alone in omitting or misconstruing these developments, however; even today, most North American or European philosophers writing about science do not read or cite this work.

Even though few philosophers of science have engaged these new developments in science studies that move beyond the constitutive assumptions of SSK, some of the most important and exciting developments in the philosophy of science and in philosophy more generally constructively complement the interdisciplinary work in other science studies fields. Some but not all of these developments have been brought together and given new prominence by the newly organized Society for the Philosophy of Scientific Practice, which held its first meeting in the Netherlands in 2007, and met again in Minnesota in 2009. Yet most scholars in science studies outside philosophy are similarly unaware of these philosophical developments and how they might speak constructively to their own concerns. In noting this mutual lack of awareness of complementary work, my aim is not to criticize scholars on either side for inattention, but instead to expand our horizons and encourage mutual conversation. I shall begin by noting briefly where I think some of the important new philosophical work is being done that complements the new directions in anthropology, history, or feminist science studies. I shall then try to indicate how these disparate lines of research come together to suggest a new scientific image.

At least four interconnected themes in recent philosophy mesh constructively with the work of the anthropologists and cultural historians of science:

- (1) Recent work in philosophy of science has begun to recover causal relations from their philosophical occlusion behind neo-Humean accounts of laws. The roots of this theme can be traced back to work on causal explanation by Wesley Salmon (1999) or even Michael Scriven (1975), but more recently, the articulation of causal relations has been freed from its shackles to the traditional problem of explanation. Work by Nancy Cartwright (1989, 1999, 2004, 2007), William Bechtel (2006), James Woodward (2004), or Christopher Hitchcock

³ This essay was reprinted as the final chapter of Rouse 1996, which is soon to appear in Chinese translation from Suzhou University Press.

(2003, 2004) emphasizes the complex entanglement of multiple causal relations and the scientific challenge of disentangling causal relations to demonstrate their disparate roles in mechanisms or other complex causal arrangements. The causal idiom is irreducible in these accounts even where causal concepts are expressed in relation to probabilities—for example, where the relevant probabilities are conditioned on other causal factors. Thus, in place of philosophical accounts of scientific explanation (such as deductive-nomological or unificationist accounts) that were proposed as substitutes for understanding causal relations, or accounts that sought to reduce causation to changes in probabilities, philosophers of science now accept causal relations as an unavoidable target of scientific understanding.

- (2) The logical empiricist and early postpositivist philosophical emphasis upon scientific laws and other linguistic expressions of theoretical understanding is also being challenged from a different direction. Here I include both work on theoretical modeling by Ronald Giere (1988, 1999, 2006), Margaret Morrison (2000), Mary Morgan (Morgan and Morrison 1999), Nancy Cartwright (1989, 1999, 2004, 2007), Paul Teller (2001), and Eric Winsberg (2001, 2003, 2006) and work on experimental systems by Rachel Ankeny (2000, 2001), Richard Burian (2005), Hans-Jörg Rheinberger (1997), and Peter Galison (1997). This work emphasizes the autonomy of models and model systems from more systematic theoretical representations. It also highlights the role of models and laboratory practices in articulating the world conceptually, and thereby partly frees our understanding of scientific concepts from the undifferentiated theoretical holism that Zammito and others have lamented in both postpositivist HPS and sociological SSK.
- (3) A third theme involves more directly the topic of conceptual articulation in the sciences. This theme is perhaps taken up most explicitly and exhaustively in Mark Wilson's (2006) massive book on scientific concepts, *Wandering Significance*, and emerges in a different guise in Marc Lange's (2000, 2007, 2009) work on laws. Lange's revisionist treatment of laws emphasizes the role of laws in expressing the norms of inductive reasoning implicit in the use of scientific concepts. Like Wilson, or Nancy Cartwright (1999: ch. 1) in her criticism of "scientific fundamentalism," Lange seeks to understand how concepts become empirically projectible. These attempts at more general accounts of conceptual articulation are complemented by particular case studies. Examples include Hasok Chang's (2004) marvelous book, *Inventing Temperature*, and a great deal of work in the philosophy of biology, most notably the edited collection *The Concept of the Gene in Development and Evolution* (Beurton, Falk, and Rheinberger 2000). In each case, the articulation and use of concepts has turned out to be rather messier, and more empirically and practically localized in models and experimental practices, than might be expected given the received wisdom in epistemology, philosophy of language, or postpositivist philosophy or sociology of science.
- (4) The fourth theme I identify seems to go far beyond philosophy of science, but I think it contributes a crucial component to the alternative image of the sciences I find emerging in recent science studies and philosophy. Donald Davidson (1987) and Richard Rorty (1991) once presented an influential image of

conceptual and epistemic understanding as causally connected but rationally impervious to perception and action. As Davidson famously put it, “Nothing can justify a belief except another belief.” In Pittsburgh and elsewhere, some philosophers, notably John McDowell (1994), Robert Brandom (1994), John Haugeland (1998), and Brian Cantwell Smith (1996), have taken up the implicit challenge to incorporate perceptual and practical interaction with our surroundings within the Sellarsian space of reasons. My own most recent book, *How Scientific Practices Matter* (Rouse 2002), falls squarely within this line of descent from Quine, Sellars, Davidson, and, in some quarters, Heidegger or Foucault. Among these philosophers, Haugeland, Smith, and Rouse make explicit and central the implications of their work for philosophy of science and science studies, while McDowell also challenges the received conceptions of the “scientific image” of nature as impervious to norms.

These four themes encompass a wide range of philosophical work. When coupled with the very different strands of work in STS by anthropologists, feminist scholars, symbolic interactionist sociologists, and cultural historians, readers may rightly wonder how this extraordinary range of scholarly work, within and outside philosophy, might possibly be complementary. These scholars certainly start from very different assumptions and write in extraordinarily disparate styles. Anyone who tried to read in succession Donna Haraway, Mark Wilson, Hans-Jörg Rheinberger, Marc Lange, Sharon Traweek, and Margaret Morrison might well experience rhetorical vertigo rather than expanded philosophical or theoretical horizons (Fig. 1).

To understand how there might nevertheless be a genuine theoretical project that connects these disparate visions of science, I will return to John Zammito’s (2004) historical study. The very rigor and clarity with which Zammito constructed his



Fig. 1 The juxtaposition of unfamiliar literatures in science studies (photograph by Martin Rouse)

critical reading of postpositivist philosophy and sociology of science lends itself to explicating the common roots of what such readings exclude.

For Zammito, what has been at stake in science studies since Thomas Kuhn (1962) has been the legitimacy of “the practice of empirical inquiry itself” and its “indispensable canons” (2004: 1). Internalist HPS went wrong in trying to defend the rationality or truth conduciveness of inquiry in overly idealized terms too far abstracted from its messy historical details. The Strong Programme in the sociology of scientific knowledge and its successors supposedly erred in an opposing direction by explicating scientific beliefs in ways that utterly disconnect them from accountability to their objects of study. In response, Zammito offered a moral to his story: science studies should occupy a moderate, historicist middle ground between these extremes of what he calls “epistemic derangement.”

To borrow a geometrical metaphor from Bruno Latour (1992), I am calling attention to work that lies off the axis on which Zammito sought a midpoint between the supposed extravagances of postmodernist skepticism and hyperrationalist HPS. Zammito’s treatment of Donna Haraway’s work is instructive. He used words like *acknowledges* or *revisionism* to read “Situated Knowledges” (Haraway 1991), one of her early reflections on objectivity and social construction, as a retreat to a more moderate epistemological position, rather than as an attempt to redirect science studies away from epistemology. In sympathetically reading Haraway as moving in the direction of the more modest feminist empiricism that he endorses, Zammito quotes this Haraway passage among several others: “the further I get with the description of the radical social constructivist programme and a particular version of postmodernism, coupled to the acid tools of critical discourse in the human sciences, the more nervous I get” (Haraway 1991: 185, quoted in Zammito 2004: 209). Haraway’s next sentence, which Zammito omitted, shows that her concern was to change the question rather than moderate the answer: “Like all neuroses, mine is rooted in the problem of metaphor, that is, the problem of bodies and language” (Haraway 1991: 185). Most philosophers would find this claim more familiar if expressed in terms of “causes and reasons” rather than “bodies and language,” but the key point is to recognize that the issue that matters to Haraway is not epistemological. I think she would rightly construe it as simultaneously metaphysical, semantic, and political, concerning how aspects of the world become meaningful in ways that affect how we can intelligibly act within it.

Adapting a famous phrase from Quine, we might say that Zammito has a commitment to “epistemic ascent” that he shares with the philosophical and sociological views that he criticizes. For Quine (1960), “semantic ascent” was a shift from talk about things to talk about talk about things. Epistemic ascent similarly shifts attention from the practical and discursive activities of the sciences to the justification of propositions or timeless sentences that can be extracted from these activities. Perhaps the most dramatic example of epistemic ascent in philosophy of science was the neo-Humean retreat from causality to nomological explanation. Laws and explanations have long served as philosophical stand-ins for the articulation of causal relations in the world; only recently has causality reemerged as a topic for direct philosophical consideration.

Epistemic ascent has had two important and unfortunate consequences in science studies. Most prominent has been the focus on the wholesale legitimation of scientific

belief, in defenses or refutations of relativism or skepticism. Zammito himself endorsed this attentiveness to questions of wholesale legitimation, while advocating only sensibly moderate, historicist responses to such issues. Once these questions are accepted as legitimate, however, their corrosive effects are hard to contain, as Zammito himself nicely illustrates. Having celebrated Helen Longino's (1990) moderately historicist epistemological views, for example, he then acknowledged that her work leaves him still "troubled by the question, 'what is left to adjudicate scientific disputes?'" (2004: 222). Such questions could be troubling, however, only if one thinks there could and should be a general answer to what adjudicates scientific disputes. Such skeptical questions are not so easily banished once one accepts the assumptions that get them off the ground. By contrast, I am linking together work in philosophy and science studies that more or less explicitly shares a response once memorably expressed by Donald Davidson (1987: 134): they do not try to answer the skeptic (or the relativist), but instead tell him to get lost. From their perspective, those who still find it urgent to refute skepticism or relativism about scientific knowledge share the relativist's most problematic assumptions.

A second, related consequence of epistemic ascent is an attitude that I shall call "semantic complacency." Semantic complacency is the presumption that we already know what our concepts and judgments mean. It generalizes Mark Wilson's (2006: 55) notion of "tropospheric complacency," in which "we readily fancy that we already 'know what it is like' to be red or solid or icy everywhere, even in alien circumstances." Semantic complacency is under attack in Nancy Cartwright's (1999) criticism of the "fundamentalist" assumption that laws are universal in scope, in Marc Lange's (2000, 2007) effort to understand how scientific concepts and laws become inductively salient against the background of disciplinary aims and assumptions, and throughout Wilson's (2006) rich analysis of the gritty and messy details of conceptual articulation in various sciences. When philosophers or sociologists focus on the justification and acceptance of scientific knowledge, they tend to overlook the complex challenges of articulating the world materially and conceptually. By contrast, the work I am linking together is more concerned with how the sciences articulate our understanding of the world, in models, experimental systems, and discursive practices, than with questions about the justification or acceptance of knowledge claims.

The difference between two competing philosophical images of science helps clarify the resulting shift in concern. One image is manifest in the traditional philosophical conception of knowledge of the "external" world (whether it refers to the world outside the mind or to nature apart from a community's shared language and worldview). The resulting concern is how, or whether, knowers can transcend their own experience, thoughts, paradigms, or webs of belief, to assess whether they have accurately characterized the world or its underlying nomological structure. This image directs attention toward the all-too-familiar philosophical issues of theoretical under-determination, theory-ladenness, social or cultural pluralism, and historical theory change, whether as obstacles to be overcome or as insurmountable barriers to acknowledge in arriving at an adequate conception of scientific understanding.

A different image emerges, however, if we look at some recent work in philosophy and science studies. In this work, knowers are agents already entangled in the world's causal complexity rather than spectators on "the passing show" (Quine 1960: 5). The question of how science can take us beyond our beliefs to the world itself does

not arise, because we are always already in the world. The issue, instead, is how the sciences articulate the world in significant and revealing ways to allow it to be intelligible. Philosophical focus shifts from what we can know to what we can meaningfully say, do, and understand and what is at stake in alternative ways of articulating and engaging the world.

Scientific articulation of the world is accomplished in multiple, interconnected ways. Consider first the importance of understanding science as material practice. Ian Hacking long ago remarked that “in nature there is just complexity, which we are remarkably able to analyze. We do so by distinguishing, in the mind, numerous different laws. We also do so by presenting, in the laboratory, pure, isolated phenomena” (1983: 226).

At first Hacking’s remark may seem perplexing. Traditional conceptions of laws and experimental phenomena assign them very different roles in science. Laws make the phenomena intelligible by explaining them. A noteworthy phenomenon may suggest possible laws or theories, and may even be an indispensable means of discerning a law. Yet the phenomena supposedly become intelligible only when they are brought explicitly within the space of reasons through the statement of a well-confirmed law or theory. How could the presentation of natural or experimental phenomena amount to a distinct mode of scientific “analysis” complementary to theoretical or nomological representation?

To understand Hacking’s claim, we must first recall that phenomena in his sense are publicly accessible events in the world rather than linguistic or perceptual representations of those events. His discussion effects a subtle shift in emphasis, from what is observed or recognized to what is salient and noteworthy. Phenomena show us something important about the world, rather than our merely finding something in it. The term has a normative significance. Most events in nature or the laboratory are not phenomena, for they show us little or nothing. Creating a phenomenon is thus an achievement, whose focus is the salience and clarity of a pattern against a background. Phenomena, and the experimental systems and practices to which they contribute, “analyze” complexity by articulating the world itself into meaningful patterns.

The recognition that meaning and understanding can be embedded in artifacts and patterns of interaction with our surroundings has long been important in anthropology under the heading of “material culture.” Nevertheless, insistence on attending to the material culture and material practice of science is one of the important and constructive legacies of SSK for science studies. That legacy will outlive the theoretical and epistemological controversies in which Zammito and others have seen the threatened dissolution or derangement of science studies. Even philosophy of science has gone beyond merely taking experiment seriously, to recognizing the material and practical articulation of scientific understanding. In 1984, after the very first conference session jointly sponsored by the Philosophy of Science Association and the Society for Social Studies of Science, one philosopher said to me in exasperation that no one would ever convince him that philosophers of science need to attend to where the pipes come into the laboratory, and other such mundane, material details of scientific work (Fig. 2). Now we have important books by philosophers of science with titles like Davis Baird’s (2004) *Thing Knowledge* and Alison Wylie’s (2002) *Thinking Through Things* that regard such mundane details as central.



Fig. 2 Does it matter philosophically where the pipes come into the laboratory? (photograph by Martin Rouse)

Hacking's comparison of the acts of creating experimental phenomena and discerning laws also suggests different ways of thinking about how laws confer intelligibility. We traditionally think of laws as representations of regularity or necessity. Such representations invite the usual questions about their representational adequacy. Yet Marc Lange (2000, 2007) has argued that laws have a different role in scientific practice. The content of a law expresses what it would be for unexamined cases to behave in the same way as the cases already considered. Laws thus express the appropriate use of empirical concepts, as Sellars (1948) argued long ago. In taking a hypothesis to be a law, scientists thereby claim that the best set of inductive strategies to pursue in this research context will vindicate the reliability of the inference rule corresponding to the hypothesis and its concepts.

The familiar difficulty, of course, is that many inference rules are consistent with a given body of data. Lange responds by asking which inference rule is salient within this context. "Salience" in this sense indicates a normative status rather than a psychological propensity. A salient inference rule imposes neither artificial limitations in the scope of induction nor unmotivated bends in its subsequent extension. Of course, the matter of which limitations are artificial and which bends are unmotivated is contextual, dependent in part on background assumptions and disciplinary interests. Yet this dependence is not explanatory. Beliefs and interests are themselves at issue in scientific contexts; they do not explain inductive salience, but express its significance. The context in which inferential relations become salient incorporates prior beliefs and interests within a larger pattern of practical and discursive interaction with the world. In this respect, the salience of inductive strategies expressed in laws, and the salient patterns that constitute natural or experimental phenomena, indeed play

complementary roles in scientific understanding, as Hacking suggested. In each case, a phenomenon's or a law's normative status as a salient pattern is a meaningful articulation of a causally complex world, which renders aspects of its causal complexity intelligible, albeit defeasibly so.

Hacking only mentioned laws and phenomena, but subsequent work in philosophy and science studies highlights a third mode of articulating complexity: building and working with models. I am not talking about the formal concept of a model that is central to the semantic conception of theories. Scientific models are instead a motley crew of mathematical structures, visual displays, computer simulations, verbal descriptions, and three-dimensional physical arrangements. As Paul Teller recently noted about this odd assortment, “[The demand for a general account of models] can be heard as a demand for those intrinsic features of an object which make it a model. But there are no such features. WE make something into a model by determining to use it to represent” (2001: 397).

Moreover, “representation” in this sense should not be confused with traditional philosophical accounts of linguistic representation. Think instead about how heating a pan of water on a rotating turntable can represent the circulation of the earth’s atmosphere, how a Monte Carlo simulation represents neutron trajectories in a critical mass of uranium-235, or how a schematic diagram represents the biological mechanisms of DNA repair. Models are structured systems whose internal relations or dynamics can be instructively compared to aspects of the world in ways that disclose and highlight their putative features or patterns. Often what matters is what we can do with the model, as much as or more than what the model itself contains. Bechtel (2006) points out that even visual models, which seem to be static, permit both the mental animation of a functioning mechanism and the sequential decomposition of simultaneous processes. Modeling is not merely representation; rather, it involves ongoing interaction with the model and what it represents.

In scientific practice, experimental systems, inductively salient concepts, and models do not function in isolation, of course, but play off one another. Wilson (2006) and Cartwright (1999) emphasize how concepts like force, and the laws that express their appropriate use, acquire definite content only through detailed models, which often link together in discontinuous “facades” rather than seamless, univocal theories.⁴ Yet conceptual articulation also takes place experimentally, despite Quine’s (1953) famous contrary image of scientific theory as a fabric rewoven only internally to accommodate how the world tugs solely at its experiential edges. Chang’s (2004) study of the experimental articulation of a unified scale of temperature or Bechtel’s (2006) and Rheinberger’s (1997) work on the conceptual integration of structure and function in cell biology emphasize the interplay between concepts, experiments, and visual displays. Experimental systems themselves then come to seem like one more kind of model. We can evaluate the aptness of the standard model organisms for expressing the relevant features of biological development (Bolker 1995) in much the same way that we ask whether Global Circulation Models capture the most important determinants of human impact on climate.

⁴ Wilson uses the analogy of an atlas, in which one maps the world not in a single, all-encompassing map, but in successive pages or “facades” that instead provide a series of partially overlapping maps and that may also have gaps.

The significance and robustness of the model rather than the truth or justification of the resulting knowledge claims are the most central issues raised by scientific practices of experimental and theoretical modeling and conceptual articulation. Questions of justification arise, of course, but they are typically more local and contextual, integrated within the larger issue of whether a significant, robust, and clearly articulable pattern has been discerned. Asserting that scientific significance has been more central to philosophy of science than issues of justification is not a novel claim. Explanation was the most central issue within logical empiricist and postempiricist philosophy of science, and explanation was to some extent a stand-in for the larger issue of scientific significance: explanatory power (usually determined by the generality of a law or theory) then served as the principal philosophical marker for significance, which partly accounts for the prominence of laws and general theories within the philosophical tradition.⁵

Philip Kitcher (2006: ch. 6) argues persuasively that explanation is not an adequate stand-in for scientific significance. Grasping how and why scientific work matters requires situating it within a much larger matrix of questions, proposals, and practices. The effort to think more comprehensively about scientific significance is the principal meeting point between recent philosophical work on causes, models, experimental systems, and conceptual articulation and the work in anthropology, cultural history, and feminist science studies that I have long tried to bring to philosophical attention. Having emphasized so far how new work in philosophy has moved away from the epistemological concerns that guided Zammito's narrative, I want to conclude by briefly indicating some of the ways in which work in other disciplines expands how we think about scientific practice and scientific significance.

A central concern in much of the anthropological literature has been to widen our frames of reference in order to grasp the significance of detailed methods, instruments, practices, and concerns from the sciences by connecting them to images, tropes, and salient events in a broader political and intellectual culture. Donna Haraway's (1989) *Primate Visions* was one of the earliest deployments of this strategy, but she has had many successors, such as Stefan Helmreich in his studies of artificial life (1998) or deep-sea oceanography (2009), Paul Edwards (1996) on the Cold War history of computation and cognitive science, or Joseph Dumit (2004) on PET scanning. Anthropologists often work back and forth between the cultural resources that scientists appeal to in making sense of why they do what they do, and the diverse uses that others make of the often potent scientific images, tools, objects, and concepts that circulate beyond the immediate settings of their production and use. Not surprisingly, such strategies have been especially prominent in work on genetics and reproductive technologies, where the stakes in scientific work encroach upon the anthropologically rich topic of kinship.

⁵ I argue that explanation was indeed the central issue for logical empiricist philosophy of science partly because the substitution of explanation by logical deduction from laws for causal understanding was exemplary of logical empiricism's hostility to "metaphysics." It is also instructive, however, that the recognized absence of any remotely adequate theory of empirical confirmation long predated the effective demise of logical empiricism. Logical empiricism instead collapsed when historicist accounts of theoretical understanding replaced the deductive-nomological theory of explanation.

Yet there have also been studies of the dynamics of scientific and cultural significance that hold a relatively constant frame. Hans-Jörg Rheinberger (1997) stays focused on the details of laboratory practice, and nevertheless finds a shifting play of “epistemic things” at stake in the dynamics of scientists’ experimental systems in cell biology. Evelyn Fox Keller (1995, 2000, 2002) maintains a comparable focus in her historical reflections on the gene concept and the practice of theoretical biology. At the other end of the scale are efforts to capture larger patterns of discursive exchange among scientific discourses and other cultural practices, from James Bono’s (1990) historiography of metaphor to Lily Kay (2000) on interpretations of genes as coded texts. In a few cases, the broader cultural appropriations of scientific themes have been so striking that it seemed fruitful to leave the science behind to explore how its concepts fare elsewhere, as in Emily Martin’s (1994) study of shifting conceptions of the immune system, or Paul Rabinow’s (1999) account of a controversy over the fate of “French DNA.” In some cases, as illustrated in Jenny Reardon’s (2005) narrative of the collapse of the Human Genome Diversity Project, scientific work becomes ensnared in the proliferation of conflicting interpretations of what is at issue and at stake in the research.

I conclude by calling attention to two very different contexts in which scholars have sought a more general characterization of the shift in science studies and philosophy away from conceptions of scientific knowledge as representation, with its corresponding focus on justification of representational adequacy. In recent science studies, the principal focus has instead been to understand discursive articulation as integral to material interaction within the world, rather than as independently meaningful representation. Thus, Haraway (1991) and Barad (2007) talk about material-semiotic apparatuses or material-discursive practices and substitute the ineliminably physical optics of diffraction for the geometrical optics of reflection as a metaphor for scientific understanding. Bono (1990) and Rheinberger (1997) talk about the materiality and textuality of discourse in processes of metaphoric exchange and the “historial” temporality of the differential reproduction of experimental systems. These idioms are unfamiliar and initially perplexing to most philosophers. I cannot now undertake the task of detailed translation and interpretation, but here is one clue. The pervasive attention to metaphor in these writings may make more sense to philosophers if we recall Donald Davidson’s (1984) and Rorty’s (1991) accounts of metaphors as “unfamiliar noises.” They argue that metaphors are not part of a system of meaning, but causal interventions whose semantic significance must be understood pragmatically. That is precisely what these science studies scholars want to say about discursive articulation more generally.

The philosophical counterpart to this work has been the post-Davidsonian semantic tradition developed in different ways by Brandom (1994), Haugeland (1998), McDowell (1994), and Smith (1996), as well as my own work. Like the science studies scholars, our central concern has been the metaphysics of meaning, seeking a rapprochement between the Sellarsian spaces of causes and reasons to avoid an untenable dualism of nature and normativity. Since the heyday of logical empiricism and the early causal theories of reference, philosophy of science and the philosophies of language and mind have drifted apart. The recent developments in philosophy of science and science studies should make it less surprising that at least Haugeland,

Smith, and I have made scientific practice central to our efforts to rethink the metaphysics of mind and language.

So how should more widespread recognition of these common concerns and themes in recent philosophy and science studies affect how scholars in the various science studies disciplines do our work? I do not pretend that science studies would merge into a happy interdisciplinary utopia if only anthropologists, philosophers, historians, and sociologists of science would read more widely in other disciplines and discover the extensive common ground that I have been describing. There are daunting substantive and rhetorical barriers to full appreciation of one another's projects and approaches across these disciplinary boundaries.

My more modest aims for expanding our "trading zones" (Galison 1997) in science studies were nicely captured some years ago in Sharon Traweek's response to Bruno Latour's (1990) quick dismissal of anthropologists' contributions to science studies:

Most of us now doing [science studies] are mired in our own disciplinary histories: our questions, our methods, our rhetorical strategies, and our boundary disputes with each other. I am not suggesting that we abandon our niches, only that we be more conscious of the limits of our little terrains so that we occasionally can, with the proper visas, passports and adapter plugs, learn to cross these borders to discuss our different versions of ... our shared concerns. (1992: 443–44) (Fig. 3)

I do believe that if and when we travel abroad to other disciplines, we will find science studies (including philosophy of science) to be alive and well. Recent North American and European work in philosophy of science and science studies takes our understanding of science and culture in new and mutually instructive directions. Moreover, such work also undercuts the epistemological debates of the 1980s and



Fig. 3 An overview of the laboratory roof from the science studies building (Wesleyan University; photograph by Martin Rouse)

1990s, in which philosophers and sociologists sharply opposed one another over questions about rationality and realism. These epistemological concerns were the principal locus of the new round of “science wars” that were briefly prominent in the United States and Great Britain in the 1990s (Ross 1996; Koertge 1998; Editors of *Lingua Franca* 2000). Recent work in philosophy of science and science studies has made these epistemological topics increasingly irrelevant to how scholars think about the practices and achievements of the sciences.

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