REVIEWS ARTICLE

HISTORY/HISTORY/PHILOSOPHY/SCIENCE:
SOME LESSONS FOR PHILOSOPHY OF HISTORY


ABSTRACT

Rheinberger’s brief history brings into sharp profile the importance of history of science for a philosophical understanding of historical practice. Rheinberger presents thought about the nature of science by leading scientists and their interpreters over the course of the twentieth century as emphasizing increasingly the local and developmental character of their learning practices, thus making the conception of knowledge dependent upon historical experience, “historicizing epistemology.” Linking his account of thought about science to his own work on “experimental systems,” I draw extensive parallels with other work in the local history of science (the ideas of Latour, Pickering, Rouse, and others) and consider the epistemological implications both for the relation between history and philosophy of science and between history and theory more broadly. In doing so, I suggest that the long-standing gap between the natural sciences and history as a “human science” has been significantly bridged by the insistence upon the local, mediated, indeed “historicized epistemology” of actual science.

Keywords: epistemology, historicity, experimental systems, science as practice, history of science, the practice of history

For all too long, though with good reason, historians have been fearful of any “thinking-together” of the humanities with the natural sciences. In the epoch of positivism this always meant subordination, if not trivialization. But things have changed dramatically in the understanding of the sciences, and the news—if we

1. Wilhelm Dilthey, Heinrich Rickert, and their cohort launched a massive effort at the turn to the twentieth century to defend the cultural sciences (Geisteswissenschaften) from the positivistic encroachment of the natural sciences. See W. Dilthey, Introduction to the Human Sciences: Selected Works, vol. I, ed. R. Makkreel and F. Rodi (Princeton: Princeton University Press, 1989); Heinrich Rickert, Kulturwissenschaft und Naturwissenschaft (Tübingen: J. C. B. Mohr, 1926). As the twenty-first century has turned, Frank Ankersmit, among others, carries on a similar effort. This is most explicit in Ankersmit, Sublime Historical Experience (Stanford: Stanford University Press, 2005). My admiration for all these figures is heartfelt, but my message is that the battle lines have shifted.
can get past the suspicions induced by two hundred years of bullying—is salutary for historians. The work of the last fifty years has demolished the unified theory of “science” that logical positivism/empiricism projected upon actual natural-scientific practices and presumed in order to discredit the very idea of the human sciences. No unified theory of science—and a fortiori not the positivist one—stands. In its place we have a far more disunified, situated, and contingent theory of empirical inquiry that suits both the natural and the human sciences. The grand-scale “normative” prepossessions of the “Received View” in philosophy of science (think of Hempel’s “covering-law” claim) have proven ultimately incongruous with any effective descriptive or explanatory investigations of concrete areas or problems. Foremost among the changes is the collapse of the view that philosophy is authorized to impose a priori rules upon empirical inquiry, a notion that Joseph Rouse has called “epistemic sovereignty.” As Philip Kitcher has noted, virtually nothing survives of such a priori conceptions in current philosophy of science, epistemology, or philosophy of language. There is now a real prospect of fundamental rapprochement between the “two cultures” that does not begin by consigning historical inquiry to essential inferiority, as was the case with positivism. This is not to say there are no differences between the natural sciences and the humanities, but the nature of those differences is very different from, and the interfaces are vastly more fruitful than, what we have long been able to conceive.

In part we have missed this because we have not really incorporated history of science into our theoretical self-reflections about history more generally. History of science is not only a fairly recent sub-specialty, but it has operated largely at the border—more honestly, just over the border—of disciplinary history. Most historians find late-modern science beyond their competence, something they do not know how to integrate into their standard account of the modern world, for all that they recognize its salience. Conversely, historians of science for a long

2. I have tried to reconstruct that historical development in A Nice Derangement of Epistemes: Post-Positivism in the Study of Science from Quine to Latour (Chicago and London: University of Chicago Press, 2004).


7. Thomas Kuhn wrote very perceptively about this in the 1970s. See Kuhn, “The Relations between History and the History of Science” [1971]; reprinted in The Essential Tension (Chicago: University of Chicago Press, 1977), 127-161. He observed, correctly: “Despite the universal lip service paid by historians to the special role of science in the development of Western culture during the past four centuries, the history of science is for most of them still foreign territory” (128). Kuhn offered a shrewd hunch about why: “What historians generally view as historical in the development of individual creative disciplines are those aspects which reflect its immersion in a larger society. What they all too often reject, as not quite history, are those internal features which give the discipline a history in its own right” (152). In many ways things have not changed that much in disciplinary history, though history of science has been through tumultuous times.
time longed for other disciplinary consorts. The desired partner has changed dramatically over the last several decades, especially with the rise of “science and technology studies.” In any event, all this has not yet, in my estimation, led to a theoretical consideration of the implications of historiography of science—and of modern science as an unsettling cascade of disparate events in the career of knowledge—for the understanding of historical practice (or of philosophical practice, for that matter). Hans-Jörg Rheinberger’s work presents a major contribution along this line, and I wish to accentuate just these implications and impulses for historical practice.

1. A DIGEST OF ON HISTORICIZING EPistemology

Rheinberger’s slender new book, published first in German and now translated into English, is the fruit, as he notes, of two seminars held at the Technical University of Berlin from 2005 to 2007 (Preface, unnumbered). His thesis is: “the historicization of epistemology represents a decisive moment in the transformation of twentieth-century philosophy of science” (1). This would seem to suggest that the topic is a development in disciplinary philosophy, but Rheinberger does not mean this. His conception of epistemology is not synonymous with “theory of knowledge” or with “what makes knowledge scientific,” as has been the traditional (disciplinary) sense of epistemology and of philosophy of science, respectively. Rather, he claims to follow French practice, “reflecting on the historical conditions under which, and the means with which, things are made into objects of knowledge” (2). Referring to a pioneer of this French view, Gaston Bachelard, Rheinberger says, “it is not the task of the philosophers of science to dictate to scientists the conditions of possibility and the norms of their knowledge.” Instead, “it is better first of all to examine precisely what went on in actual laboratories” (21). More generally, “epistemology is the quest for tools that help us to understand a little better the historical evolution of the sciences.” But the only way that can succeed is to drill down into the detail, the “local wisdom” of concrete cases, of their “internal historicity” (29). “In order to understand particular historical developmental processes, there is no alternative but to pursue detailed investigations. Empirical inquiry is inescapable . . .” (60).

Rheinberger discerns over the course of the twentieth century a “transition from attempts at historicizing epistemology to attempts at epistemologizing history of science” (51). That is, “in the course of time, historical reflection on epistemology began to merge with epistemological reflection on the history of science” (89).

8. One short-lived expression of this was the (rocky) “marriage” known as “History and Philosophy of Science”—HPS for short—that struggled through the later decades of the previous century, leaving as its most admirable offspring the journal Studies in History and Philosophy of Science. At a recent conference on the state of science studies I offered my sense of this episode and of its legacy: “Soundings in Once and Future HPS,” Conference on “Science Studies in Context,” UCSD Science Studies Program, San Diego, California, March 19-20, 2010. An ulterior motive of this review article is to revive—by redefining—this locus of practice.

9. Rheinberger, Toward a History of Epistemic Things: Synthesizing Proteins in the Test Tube (Stanford: Stanford University Press, 1997), 228. This review article will find itself continually reverting to Rheinberger’s earlier text to explicate the later one.
This “new, genuinely historical-epistemological discourse,” he asserts, “came out of the sciences themselves” (90). He accords this “decisive significance for the relationship between both the history and the philosophy of science in the second half of the twentieth century” (51). I take this somewhat strange use of the word “both” to gesture to the internal mutations within history of science and philosophy of science individually as well as to their mutual relation. The point is, after World War II, history of science challenged the purview of philosophy of science by claiming to offer a clearer access to what scientific practice actually had been and proclaiming that this access should be the decisive basis for conceiving what science could and should be.

Rheinberger’s point is that the “epistemology and history of experimentation crystallized conjointly,” and this conjoint project was “conducted within the sciences and by scientists themselves” rather than as part of disciplinary philosophy (3). It was concrete historical experience in the sciences that not only drove their highly disparate projects but also elicited the meta-reflection upon the generic character of actual science that philosophy of science has always taken itself to be. The crucial upshot was that “theory of knowledge” became “subject[ed] . . . to an empirical-historical regime, grasping its object as itself historically variable, not based in some transcendental presupposition or a priori norm” (3). In light of this formulation, I would like to bring Rheinberger’s post-structuralist discourse into correlation with a pragmatic-naturalist one that has arisen in recent American philosophy (and social reconstruction) of science.10 There are gratifying convergences.11

Rheinberger’s narrative concatenates in a roughly chronological series (with no intent whatsoever to draw causal links) the most radical rethinking of modern science by its own practitioners and by its most astute observers (in history and philosophy). For Rheinberger, over the course of the twentieth century unanticipated events induced a recursive cascade reconfiguring what science—and modernity—could any longer have meant. The first was the overthrow of “classical” physics. The second was the proliferation of scientific pursuits beyond any corralling within some “unitary” science. These two developments in the twentieth century form the widest contextual frame for his concrete narrative (4). In light of them, it makes no sense to theorize a unified science. Nor are established disciplines the real drivers of the modern sciences. Instead, “islands of scientific rationality . . . suddenly arise on the basis of an instrument or an experiment . . .” (25). Each is a “highly mobile ensemble” that cannot but evolve and that may well dissolve or merge with other islands in this “rhizomic” archipelago.12

Rheinberger’s narrative begins in the late nineteenth century, with Emil Du Bois-Reymond, Ernst Mach, Henri Poincaré, and Émile Boutroux—all of whom, he contends, abandoned the mechanistic-determinist model for natural science that had been privileged in the epoch of positivism (chap. 1, 5-17). They stressed,

10. See, for example, Kitcher, “The Naturalists Return.”
11. I will discuss these in part II of this article.
12. In Toward a History of Epistemic Things, Rheinberger writes of “a rhizomatic network of recurrent epistemic practices” (16), and he proposes to “envisage an ensemble of experimental systems and their intricate interactions” by conceiving “coherence, just as in the case of individual experimental systems, [as] a tinkered and patched-up coherence . . .” (137).
instead, the contingency of scientific results and the intervention of the investigator in all science. This demanded a shift of focus onto “the conditions in which [a] scientific system could take shape” (16). Such conditions would be disparate and they would be local. This imperative drove the decisive theoretical endeavors of Gaston Bachelard and Ludwig Fleck in the years between the wars (chap. 2, 19-33). As Rheinberger understands Bachelard, “experimental contexts are recursive arrangements in which new knowledge arises that constantly challenges to rethink the presuppositions of the method in use and calls for adjustment” (22). In this process scientific action is “permanently transforming the truths of yesterday into the errors of today” (26). In a fruitful circle, “knowledge is not simply iterated, but is differentially driven forward” by the experimental system. “Knowledge does not proceed asymptotically toward something like an absolute reality; rather it moves away from something: it is fundamentally ‘development-conditioned,’ that is, determined by the steps it takes” (28-29). “The direction that such a flow of experiments takes is not mapped out in advance. It depends on the ‘signal[s] of resistance’ that emerge in the research process” (31). Thus, “objectivity is not something given but rather is produced in a process of objectification . . .” (24).

It is clear that Rheinberger’s own fruitful notion of “experimental systems” finds great resonance in the ideas of these earlier thinkers (31). In the Epilogue to his first book, he wrote: “What we need, in history of science, is an economy of scientific change,” where the word “economy” signals processes that cannot be explicated at the level of individual consciousness.13 In this new work Rheinberger (re)constructs a tradition of discourse evoking and enabling his own.14 “I frequently refer to a series of French-speaking philosophers, scientists, and historians of science, from Gaston Bachelard, Georges Canguilhem, Michel Foucault, Louis Althusser, Jacques Lacan, and Jacques Derrida to Michel Serres and Bruno Latour; from Claude Bernard to François Jacob, Isabelle Stengers, and Ilya Prigogine.”15 Rheinberger insists he is presenting here a series, not a genealogy; these figures form a network defined by their difference, and yet they share the common pursuit of making sense of science in our time.

Rheinberger harvests a discourse of processual complexity, whose centers are local and emergent. Thus, with Bachelard, he affirms, “the achievements of science are and remain emergent phenomena. They have the character of events, and although they may form a chain, the individual links remain historically contingent.” Historical contingency accentuates path-dependence: each iteration reacts out of and against the backdrop of prior iterations. “[E]xperimental systems are necessarily localized and situated generators of knowledge. Their reproductive situatedness, not their logicality, mark their cohesion over time, and thus their historicity. . . . Epistemic things, therefore, are recursively constituted and thus intrinsically historical things. They derive their significance from their future, which is unpredictable at the real time of their emergence.”16 Openness plus

13. Ibid., 224.
14. I submit this is a fundamental feature of intellectual self-constitution, and a key to historical reconstruction in intellectual history.
16. Ibid., 76.
path-dependency creates historical structure by inducing historical novelty within constraints. “It is the hallmark of productive experimental systems that their differential reproduction leads to events that may induce major shifts in perspective within or even beyond their confines. In a way, they proceed by continually deconstructing their own perspective.”\textsuperscript{17}

There are some interesting revisions in Rheinberger’s historical narrative. Thus, he depicts Karl Popper’s abstract and logical orientation to the problem of scientific development as ultimately less fruitful than the (anxious) historicizing of the problem of modern science in Edmund Husserl and Martin Heidegger (chap. 3, 35-49). Husserl, in his famous \textit{Crisis}, maintained that the “‘positivist concept of science in our time’ . . . did justice neither to the constitution of the modern sciences nor to their history” (39). In Husserl, Rheinberger stresses the conviction that “epistemological grounding and historical explanation must coincide in the last analysis,” even if he is not persuaded that Husserl could cash out his demand for a “universal historical \textit{a priori}” (42). In Heidegger, Rheinberger finds fruitful the conceptualization of the local and situated character of modern sciences as particular, technologically orchestrated research projects. Each such research domain creates an “open sphere” in which rigorous pursuit is “dependent on the particularities of the objects that can be disclosed in the sphere in question” (44). Scientific experimentation works within a technologically framed procedure, and modern natural science achieves a “recursive cohesion . . . by drawing on technological practice and returning its results back into that practice” (45). While Rheinberger recognizes the centrality of this looping action, he warns against such terms as “technoscience” and “black-boxing” for their penchant to reduce to a technological determinism.\textsuperscript{18} He insists that there is a crucial difference between science and technology, even if all (modern) science definitely takes place in a technological matrix with technological payoffs.\textsuperscript{19}

The largest rupture in Rheinberger’s serial narrative came with a shift of primacy from philosophical epistemology reflecting on its own historicity to historical reconstruction reflecting on its epistemological implications. It set in around mid-century, gaining traction especially after World War II (chap. 4, 51-64). This was the moment history of science came into its own. Rheinberger sees Alexandre Koyré’s “history-of-ideas approach” already as a “theoretically demanding historicization [that] contested the field of traditional philosophizing about science” (53). Above all, Koyré introduced the idea of a rupture of modern from traditional science, a “scientific revolution.”\textsuperscript{20} The idea of such a “break” would be taken up and radicalized by Thomas Kuhn. His promise, enunciated on the opening page of \textit{Structure of Scientific Revolutions}, was that a “quite different concept of science .

\textsuperscript{17} Ibid., 36.
\textsuperscript{18} Ibid., 30.
\textsuperscript{19} Ibid., 31-33.
\textsuperscript{20} Of course, that notion of “scientific revolution” and of a rupture with medieval science has become quite contested more recently (see, for example, \textit{Reappraisals of the Scientific Revolution}, ed. David Lindberg and Robert Westman [Cambridge, UK, and New York: Cambridge University Press, 1990]), but that does not diminish the importance of Koyré or of his ideas for the constitution of the discourse of history of science.
can emerge from the historical record of the research activity itself. . . .”21 At the extreme, Paul Feyerabend argued that methodological prescription was counterproductive for the advancement of science. “Contingency intervenes in the actual course of history in a way that lies beyond any critical-rational schematism” (63). Success attached retrospective validity to scientific results.

Rheinberger turns next to the post-structuralist moment during the 1960s in France, set within the larger frame of the development of French science studies from Bachelard and Cavailléès through Canguilhem (chap. 5, 65-77). Against this backdrop, Rheinberger traces the development of Michel Foucault’s “archaeology of knowledge.” Foucault put the accent emphatically on discontinuity. More, he sought “deep structures, historically located rules of production which give an episteme its form, . . . a historical a priori . . .” (72). With Louis Althusser, Foucault discerned “process without a subject,” a displacement into what Rheinberger calls “a structuralism of contingency” (73). Not surprisingly, given his formulations in *Toward a History of Epistemic Things*, Rheinberger highlights Jacques Derrida as the key contributor to this line of thought (74-77). He reminds us that “from 1960 to 1964, Derrida was assistant to Canguilhem at the Sorbonne” (75).22 Rheinberger ultimately prizes Derrida for his “historical logic of supplementarity” or “historiality”—the “iterative-recursive production of meaning” in the “trace.”

Rheinberger’s narrative concludes with a discussion of Ian Hacking and Bruno Latour as “anthropologists” of science, though in radically different senses (chap. 6, 79-87). For Hacking, Rheinberger argues, the practice of science is a consequence of the ontological nature of humans, their inveterate need to “represent” (81). For Latour, anthropology is a “field-work” approach to the hybrid interactions of nature, science, and society (82-83). At the end, but certainly with no claim to telic significance, Rheinberger situates his own work and Andrew Pickering’s conception of a “mangle of practice” (86). What they share with Hacking, with Latour and with all the earlier protagonists of Rheinberger’s chronicle, is a commitment “to reconstruct the concrete course of particular scientific paths of discovery” (86).

II. THE HISTORY OF SCIENCE AND THE PRACTICE OF HISTORY

The conceptual achievement of history (and sociology) of science has been to retrieve rich instances of local generativity (“science in the making”) and thus to model that and how such a process is accessible to reconstruction. Scientific problems emerge from “a structure of constraints (on the problem solution) plus a general demand, goal, or explanatory ideal of the research program in question that certain types of gaps in those structures be filled.”23 These constraints “con-
stitute a rich supply of premises and context-specific rules for reasoning toward a problem solution.”

Thus every problem is emergent, in a situation, and the constraints that situate the emergent problem also equip the inquiry with (some of) the terms for its solution. Moreover, this whole syndrome must be taken as a dynamic, not static, process, resulting in “successively sharper reformulations of a problem.”

The point of departure was articulated well by Michael Lynch, not against the universalism of philosophy of science but against the universalism of conventional sociology:

Sociology’s general concepts and methodological strategies are simply overwhelmed by the heterogeneity and technical density of the languages, equipment, and skills through which mathematicians, scientists, and practitioners in many other fields of activity make their affairs accountable. It is not that their practices are asocial, but that they are more thoroughly and locally social than sociology is prepared to handle.

By the end of the 1980s, Andrew Pickering came to reject the “science as knowledge” agenda as “thin, idealized, and reductive.” More concretely, it “simply does not offer us the conceptual apparatus needed to catch up the richness of the doing of science, the dense work of building instruments, planning, running, and interpreting experiments, elaborating theory, negotiating with laboratory manage-
ments, journals, grant-giving agencies, and so on.” Thus the distinctive move in recent science studies has been the shift from conceiving of science as knowledge to conceiving of science as practice. To get the requisite concreteness, Karin Knorr-Cetina has argued, science studies needed to “adopt a genetic and micro-
level approach.”

Two of the most impressive theorists of this shift are Pickering and Bruno Latour, mentioned but not really engaged in Rheinberger’s narrative. I will try to draw out their powerful convergences with Rheinberger.

Pickering’s “practical realism” or interpretation of “science as practice” offers a robust appreciation for the complexity of science, its “rich plurality of elements of knowledge and practice,” which he has come to call “the mangle of practice.” As against the “statics of knowledge,” Pickering situates the essence of scientific life in the “dynamics of practice,” that is “a complex process of reciprocal and interdependent tunings and refiligings of material procedures, interpretations and theories.”

24. Ibid.
25. Ibid., 38.
28. Ibid.
29. Ibid., 2.
tance and accommodation.” It is an extraordinarily complex “topology” of relations: the disunity, patchiness, and fragmentation of actual science turns out to be a far cry from its mythic “unity.” One must “see scientific culture as endemically patchy, composed of all sorts of heterogeneous elements . . .” The inspiration for scientific practice is “the possibility of making connections between disparate cultural elements” by envisioning “associations” or “intersections,” points where there would be a “closure” of the theoretical and the experimental elements floating in some measure of disaggregation in the field. The aspiration of scientific practice is to achieve coherence, that is, to overcome that patchiness of the research environment. Here Pickering’s conceptualization of experimental practice is at its most dense and complex. He discerns three elements: a “material procedure,” which involves setting up apparatus, running, and monitoring it; an “instrumental model,” which conceives how the apparatus should function; and a “phenomenal model,” which “endows experimental findings with meaning and significance . . . a conceptual understanding of whatever aspect of the phenomenal world is under investigation.” The “hard work” of science comes in trying to make all these work together. It generally involves modeling after exemplars given in the research tradition. Thus, the modeling is not entirely random but arises out of a trajectory of prior endeavors. Hence path-dependency provides structure. Yet the element of radical historical contingency cannot be evaded, and the experimenter must admit, at last, that novelty “just happened.”

This property of radical emergence is crucial for Pickering, hence his insistence upon attending to temporality, to “real time struggles to make things work.” As Pickering sees it, “resistance ‘just happens.’” It is “truly emergent in time.”

33. Ibid., 697.
36. Ibid.
37. “[C]oherence, I am inclined to believe, is a constant telos of scientific practice” (Pickering, “Knowledge, Practice, and Mere Construction,” 694); “the search for coherence is constitutive of practice in its full temporality” (ibid., 697).
40. William C. Wimsatt and James R. Griesemer write of “scaffolding”—temporary frames for construction—as contrasted with “foundations” (“Reproducing Entrenchments to Scaffold Culture: The Central Role of Development in Cultural Evolution,” in Integrating Evolution and Development: From Theory to Practice, ed. Roger Sansom and Robert N. Brandon [Cambridge, MA: MIT Press, 2007]). Similarly, Deweyans like Nickles talk of “platforms.” Both recognize the contingent, historical, and fragile nature of these structures, but that does not detract from their actuality and indispensability.
41. For this phrase, “it just happened,” see Pickering, “Knowledge, Practice, and Mere Construction,” 702, etc.
sistances have to be understood as situated with respect to goals; they are defined and exist only reciprocally with respect to the associations that they block.” This leads Pickering to argue for a distinctive historicity of scientific practice. Emergence in real time, this utter unpredictability, this radical contingency, this “relativity-to-chance,” marks what Pickering calls “a culturally situated historicism.” He wants to reckon with historicity as emergence, but that means he has to acknowledge structure as well: “I do indeed want to defend a historicist understanding of scientific knowledge. . . . contingency is not all that there is. . . . There is structure . . . and it is by virtue of this structure that one can make sense of the obvious fact that different cultures do sustain different practices and produce different knowledges.”

Catch scientists in the act, at the frontier of their ignorance, and they will show you a different order from what they tell. That is what Bruno Latour insists all science studies is trying to get at: not to debunk science but to reconstruct it “in the making.” In trying to understand science in the making “we do not try to undermine the solidity of the accepted parts of science.” Science in Action (1987) is Latour’s manual for the conduct of science studies. He offers a general theory of knowledge-production. The key to the whole work is: “what is called ‘knowledge’ cannot be defined without understanding what gaining knowledge means.” Emergence is the key: “an original event. . . . creates what it translates as well as the entities between which it plays the mediating role.”

The argument of Science in Action is a movement from local to global claims and structures of knowledge. We are led from “subjective” to “objective,” from a private (and therefore weak) assertion through its collective mediation to its theoretical entrenchment as knowledge. One could conceive this as a “dialectic.” Latour, a good son of the French sixties, loathes Aufhebung. Yet his Janus metaphor embodies temporality, contradiction, advance—elements that strongly approximate Aufhebung! Above all, the Janus figure epitomizes historicity: prospect and retrospect simultaneously on the cusp of an open present. Latour constantly seeks to rescue openness from the always already of the a priori. I take him to be reinventing a Hegelian processual-learning approach to knowledge in rebellion against a Kantian a priori one. In Hegel’s Phenomenology, the “phi-

44. Ibid.
47. Ibid.
49. “Bruno Latour has been following scientists around for years. Now he wants us to follow them around” (Steven Shapin, “Following Scientists Around,” Social Studies of Science 18 [1988], 533).
51. Ibid., 78.
53. Of course, Latour repudiates Hegel explicitly: “by believing that he was abolishing Kant’s separation between things-in-themselves and the subject, Hegel brought the separation even more
losopher” knowingly accompanies the groping “subject” on the path to (self- and world-) discovery that the “philosopher” has already traced. That fits: for Latour science or knowledge is always only recognition. The risk in recapitulation is: we misremember how we got here. Latour urges we resist the overweening temptation to tell the story as if we knew all along what we only came to know. He sees philosophy (not just Hegel’s)—and science, too—always trying to cover its own tracks, tidying all-too-human process into “pure” products. Latour (but also Hegel in what is most memorable in his Phenomenology) wants to uncover the making. “The first time we encounter some event, we do not know it; we start knowing something when it is at least the second time we encounter it, that is, when it is familiar to us.”

Knowledge really is recognition. Simply, “the new object emerges from a complex set-up of sedimented elements each of which has been a new object at some point in time and space.”

Latour has his Janus make the indicative utterances: the old face proclaims, “Nature is the cause that allowed controversies to be settled,” while the young face rejoins, “Nature will be the consequence of the settlement.” Laboratories manufacture facts and machines: “Laboratories generate so many new objects because they are able to create extreme conditions and because each of these actions is obsessively inscribed.”

Intervention combined with documentation: “Laboratories are now powerful enough to define reality.”

Three key figures in feminist science studies have advanced this new conception of knowledge, evidence, justification, and objectivity—Karen Barad, Donna Haraway, and N. Katherine Hayles. Invoking Niels Bohr’s philosophical-physical critique of Newtonian physics, Barad makes clear what representationalism has meant for conventional thought: “that observation-independent objects have well-defined intrinsic properties that are representable as abstract universal concepts.” Bohr insisted that the object could not be conceived apart from the “agencies of observation,” and that these formed an irreducible “non-dualistic whole,” a phenomenon or event behind which it was impossible to reach. That is, “what

fully to life. He raised it to the level of a contradiction, pushed it to the limit and beyond, then made it the driving force of history” (Latour, We Have Never Been Modern, 57). Dewey found another, more congenial Hegel; see John Dewey’s Philosophy of Spirit, with the 1897 Lecture on Hegel, ed. John Shook and James Good (New York: Fordham University Press, 2010), and James Good, A Search for Unity in Diversity: The “Permanent Hegelian Deposit” in the Philosophy of John Dewey (Lanham, MD: Lexington Books, 2005).

55. Ibid., 92.
56. Ibid., 94.
57. Ibid., 99.
58. Ibid., 90.
59. Ibid., 93.
is being described by our theories is not nature itself, but our participation within nature.” Reality is “not an observation-independent object, but a phenomenon.” Barad argues, “the world in which we live . . . is sedimented out of particular practices that we have a role in shaping,” and this is not only an epistemological but an ontological point, in her view. Making the world intelligible through certain practices and not others . . . we are not only responsible for the knowledge that we seek, but, in part, for what exists.

Donna Haraway vehemently repudiates “representationalism” as a ventriloquist and “depoliticizing expert discourse” of “passionless distance,” in which the natural scientist serves as the “perfect representative of nature, that is, of the permanently and constitutively speechless objective world.” Rejecting such “sun-worshipping stories about the history of science and technology as paradigms of rationalism,” Haraway insists upon “artifactualism”: not new representations, but new practices. What Haraway seeks, she writes in “Modest Witness,” is “questions of pattern, . . . shifting sedimentations of the one fundamental thing about the world—relationality.”

Like Barad and Haraway, N. Katherine Hayles is critical of representationalism because she fears “representation may be too passive a concept to account for the complexities involved” in what she claims is “an active process of self-organization that depends on prior learning and specific contexts.” Hayles calls this conception “constrained constructivism.” It is not that constraints “tell us what reality is. This they cannot do. But they can tell us which representations are consistent with reality, and which are not.” That is why consistency is the best that can be hoped for: it is local, it is limited, but it can be “good enough.” The concrete “interactions with the flux are always richer and more ambiguous than language can represent,” and yet “a synergy between physical and semiotic constraints . . . brings language in touch with the world.”

Drawing on these theorists and others, Joseph Rouse identifies as “most central to empirical work in recent science studies . . . the embodiment of scientific understanding in laboratories and material practice, in non-verbal images and models, and in the textual materiality of language.” Rouse embraces a “post-epistemological conception of science and scientific knowledge,” as part of a general “de-

69. Ibid., 35.
70. Ibid., 38.
flationary approach to knowledge.”

What is the post-epistemological conception, in Rouse’s view? My sense is that it entails two key tenets: that ontology takes precedence over epistemology and that knowing is always situated. Rouse sees traditional epistemology as “the aspiration to a detached assessment of the totality of scientific knowledge and its relation to an objective world understood as something apart.” Crucial here is the contention that epistemology “presumed that ‘knowledge’ demarcates a coherent, surveyable domain of inquiry.” Rouse proposes to “disentangle the notion of knowledge from the twin telos of completeness and certainty that has haunted the epistemological tradition.” Equally suspect is the notion of detachment: the idea of “disembodied minds representing the world.” He identifies all these errors with the notion of “representationalism.” We must “conceive of ‘knowing’ as concretely situated and as more interactive than representational.” “Knowledge (in the form of epistemic alignment) is best understood not as a system of propositions or a cognitive state but as a situation in the world.” This “deflationary” view of knowledge is a “shift from thinking about a putative object that a concept could describe to thinking about a practice in which the concept is used.” An “ontology of knowing” would pursue “significance and intelligibility . . . without postulating any representations in which knowledge is located.” We must “understand knowledge as embedded within specific ways of engaging the world.”

The essential point for Rouse is: “science discloses not objects or laws independent of us and our concerns, but phenomena that we are a part of.” The only nature we can encounter “manifests itself through scientific practices.” Thus “objects only exist within phenomena.” That is, “an object shows up as a definitely bounded system with determinate capacities within such phenomena, through the modification of some other aspect of the phenomenon.” What gets disclosed is “not some definite object . . . but a field of not-fully-determinate possibilities.” This is so because “the material systems that focus scientific research always outrun projections and conceptualizations of them.” Rouse invokes Rheinberger’s notion of “epistemic thing” as precisely this sort of not-yet-determined phenomenon “at issue” that mobilizes further research. For Rouse, the new approach entails a radical

73. Rouse, Engaging Science, 159.
74. Ibid., 140.
77. Ibid., 146.
78. Ibid., 187.
79. Ibid., 199.
81. Ibid., 149.
82. Ibid., 331.
83. Ibid., 22.
84. Ibid., 274.
85. Ibid., 343.
86. Ibid., 338.
87. Ibid., 75. Here, Rouse draws on Rheinberger, Toward a History of Epistemic Things.
new temporality in scientific practices: “if one takes seriously the material, institutional, and discursive embodiment of scientific understanding, scientific research runs ahead of what it can already clearly articulate.” 88 In such a naturalistic frame, Rouse writes, “questions of justification, consistency, clarity, and so forth . . . now arise more locally . . . drawing upon more wide-ranging considerations . . . not scientific knowledge as a totality, but particular scientific practices, projects, and claims, understood as ongoing interactions between knowers and the world known.” 89 That is, “normativity arises from practical involvement in a situation whose subsequent development is not yet determined. . . . [There are] real possibilities for making a (significant) difference in how things subsequently turn out.” 90

Rouse discerns six “significant common themes” in the new processual reconstructions of scientific practice. 91 First, all emphatically acknowledge the disunity of science. One must renounce the “attempt to survey scientific practice as a whole and pronounce on its aim and legitimation.” 92 Second, they abjure offering explanations of science, whether “internal” or “external.” Instead, they concern themselves with “the emergence of meaning within human practices” in which “meaning is best understood as an open and dynamic engagement with the world.” 93 Third, they accentuate the localism and materiality of scientific practices as against “conceptions of the effortless and immaterial universality of scientific reasoning and knowledge.” 94 Fourth, they recognize “constant traffic across boundaries” between scientific and other practices in the world. 95 Boundaries are always tentative and fluid. There is “traffic in all directions across whatever boundaries . . . too heavy for any significant autonomy of a domain of scientific practices” from others. 96 In particular, this emphasis on openness of boundaries highlights “the importance of difference, power, ideology, and the proliferation of incommensurabilities, distortions, silencings, and other failures of understanding and communication.” 97 Fifth, according to Rouse, they adopt “a subversive rather than an antagonistic stance” to scientific practices; they “challenge the formulation of the question rather than proposing an alternative to its traditional answers.” 98 Over against the “legitimation project” common to all earlier efforts, the new project emphasizes that “indeterminacy, instability, opacity, and difference must play a more prominent role.” Finally, a critical engagement with the practices of the sciences must replace the “normative anemia” shared ironically by both “value-free” science and “relativist” social constructionism. 99

88. Rouse, Engaging Science, 75.
89. Ibid., 159.
90. Ibid., 26.
91. Ibid., 242.
92. Ibid., 236.
93. Ibid., 33, 221.
94. Ibid., 249.
95. Ibid., 250.
98. Ibid., 254.
“interpretive readings are thus part of the culture of science and not an explanation or interpretation of it from ‘outside.’”

There is much that is persuasive about Rouse’s synthesis, but it occasions some reservations as well. I do not believe that in dismissing “epistemic sovereignty” one forecloses “epistemology” altogether, though, of course, it will require re-specification. Rouse’s view seems less plausible as a disputation with actual empirical practice of science studies than as one with a universalism that certainly characterized the “received view” of philosophy of science and the early, Strong Programme view of a sociology of scientific knowledge. In stressing meaning and understanding as against explanation, my first impression is that Rouse is recurring to the long-standing quarrel in the human sciences between interpretive or hermeneutic approaches and those that pursue nomothetic universalism along more positivistic lines. However, dismissing any sort of explanation, including the kind of specific accounting that is a key part of contextual-historical practice, seems to me to extend a legitimate philosophical quarrel with one species of explanatory thinking to a general rule that does not sit well with a lot of concrete practice. To be sure, such concrete practices of interpretation and explanation aim at a localism that takes the materiality of situations very seriously—whether this be a laboratory apparatus or a channel of distribution for claims, that is, whether physical or “textual.” But questions of cumulation and stabilization as positive features of scientific practices get too short shrift in Rouse’s account. Looking from within a situation, I believe that context, consistency, and above all the constraints of cumulation and stabilization need to be taken more richly into view, alongside both the radical novelty of emergence and the problematics of power/knowledge that Rouse rightly stresses. The emphasis on path-dependency in Rheinberger seems, accordingly, a more apt conceptualization.

All these endeavors in the study of science provide resources for robust hermeneutic practice. Indeed, one could redescribe the new histories of science in quasi-Hegelian language (of a pragmatist-revisionist, Deweyan variety) as models of dialectical objectification, making a substantial contribution to the understanding of historical change generally. The challenge for historical theory is to find a

100. Ibid., 255.

101. Rouse’s last two claims occasion the most doubt in me. First, he argues for a “subversive” rather than “antagonistic” stance toward scientific practices. How does this “subversion” by science studies reconstruct the practice of sciences from the inside? I am not so confident as Rouse about all this. Just this makes problematic Rouse’s argument that science studies play a participatory rather than external role in the practices of science.


language to articulate this extraordinarily rich dialectic in its concreteness without losing purchase on its situatedness in larger nests of causality and meaning. We need to develop a historical language with the dialectical richness to articulate what Pickering has called the topology, the temporality, and the materiality of practices.104

III. CONCLUDING IRENIC HOPE

Rheinberger sees twentieth-century science and philosophy becoming inveterately embroiled in “historicization.” But, conversely, he adds, history (as practice) gets embroiled with science and philosophy: it becomes experimental, epistemological. “Historians of science . . . have to conceive of themselves as being engaged in an experimental exploration of experimental reasoning.”105 “[T]he historian’s practice . . . continuously reorients the historical understanding of the process of reorientation we see at work in the sciences.”106 “Epistemological grounding and historical explanation must coincide in the last analysis . . . in a ‘regressive inquiry’ (Rückfrage),” Rheinberger infers from Husserl (42). It was Canguilhem who proposed that “the history of the sciences” could itself be taken as “an ‘epistemological laboratory’” (66). That is, “the object of the history of science is . . . the particular ‘historicity of scientific discourse, insofar as it expresses a procedure that is normalized from within, but is punctuated by accidents, impeded or thwarted by obstructions, and interrupted by crises . . .’” (67). That framework models more than just the history of the sciences.

The scientists’ representation of their own histories provoked a radical new departure in the history of science as a field of study. “The ‘living practice’ of the natural sciences must be distinguished from the ‘official paper form,’” as Rheinberger cites from Ludwig Fleck (29). As Nickles put it, “Good Science [i]s Bad History.”107 Scientists tend to tell “just-so stories” about their successes, tacitly rewriting the past to conform to their present.108 That seeming historical naiveté betokens something essential about historicity, however.109 Each scientific novelty did reconfigure everything that had gone before, just as each work of art displaces the meaning of all the artworks that preceded it.110 But the converse is also the
case: “What can be discovered is laid down by the successive series of preceding discoveries” (29). This “internal historicity” is “the product of a history as process” (29). Retrospect and recursiveness are the essence of historicity. In Hegel’s old and beautiful formulation, “the owl of Minerva takes flight only at dusk.”111 In the words of Bachelard, “empirical thought is clear in retrospect, when the apparatus of reason has been developed.”112 Rheinberger elaborates: “The new, in this conception, does not come into the world as a clear and distinct idea, but rather brings itself into relief ex post facto, as it were. It results from a recursive process” (63). Recursive displacement is both the goal and the engine of the experimental system of science.

In the Epilogue of Toward a History of Epistemic Things, Rheinberger invokes two seminal voices. The first is the legendary philosopher of biology, Marjorie Grene, who wrote: “Why can’t we check our beliefs against reality? Not, as sceptics believe, because we can’t reach out to reality, but because we’re part of it.”113 (We can read this as the primacy of ontology over epistemology.) The second is a philosophically minded biologist, Stuart Kauffman, who concluded his At Home in the Universe with the penetrating admonition: “All we can do is be locally wise.”114 (We can read this as the situatedness of knowing.) Rheinberger cited these authors to sum up his long argument about “science and writing,” an argument that was in fact about the practice of history as much as about the practice of science. In his new book, Rheinberger accentuates the interface of science, philosophy, and history in/as “historicizing epistemology.” “Not only the history, but also the historiography of the sciences is a process ‘driven from behind’. . . . The concepts of historical narration are shaped and reshaped from immersion in an ‘epistemological laboratory.’”115 Such a historicizing comes to grips with Grene’s point about our embroilment in the “real” by embracing Kauffman’s ad-

monition: not only is being locally wise all we can do; there is no place else to be (real)! Humans are always already situated. This is why we have to reject a “view from nowhere.”

Rheinberger shows us the historicity of scientific practice as concrete empirical inquiry; what I wish to do is to read that conversely, to see historical practice as yet another empirical inquiry whose historicity is recuperable precisely as localized process. If “all we can do is be locally wise,” then what we need to be doing is reinterpreting our practices as the situated retrieval of situated process. This means we have to renounce grand systems as premises, but the “metanarrative” that has been disparaged relentlessly for the last thirty years of historical theorizing was never a necessary foundation for historical practice. We have to understand that history proceeds by widening frames from an always local and situated point of departure. It is the iterative path of actual experience—always local, partial, contingent, and fallible—that history retraces in a manner that is equally local, partial, contingent, and fallible. But neither path is entirely blind groping and neither need be fruitless.

What I propose to harvest first is that science, philosophy, and history are so inextricably interwoven that none can make sense outside their nexus. Philosophy of science traditionally saw its project as the appraisal of scientific products (theories, laws) in terms of validity, with no regard to the process of their emergence. This distinction had not only conceptual but also disciplinary implications. Having distinguished conceptually the normative enterprise of appraisal from the descriptive enterprise of accounts, it allocated these enterprises to separate disciplinary contingents in the study of science. Appraisal was the exclusive purview of philosophy, whereas accounts could be left to psychology or sociology or history. “The task of evaluation of the products of scientific inquiry, of establishing the epistemological

117. Rheinberger celebrates the idea that scientific practice entails a significant element of “groping”—in French, tâtonnement—as he cites from Michel Serres (Rheinberger, Toward a History of Epistemic Things, 45, 74, 107, 126). This is a significant vindication of empirical inquiry along Baconian lines against a very long line of rationalist-positivist insistence on the precedence of theory that we can trace back at least to a famous exchange between Immanuel Kant and Georg Forster in the late eighteenth century.
118. This is why in his first study Rheinberger explicitly avowed: “This book is written for both scientists and historians as well as philosophers of science” (Rheinberger, Toward a History of Epistemic Things, 2).
120. “[T]he old epistemology claimed to be prior to, and conceptually more fundamental than, the diverse empirical sciences...” (Larry Briskman, “Historicist Relativism and Bootstrap Rationality,” The Monist 60 [1977], 509).
cal import of such products... falls squarely on the shoulders of the philosopher of science,” wrote Harvey Siegel. Not even the scientists themselves were privy to this highest sanctum. “Scientific decisions are one thing, the justification of scientific decisions another. ... For what we are after, in the context of justification, is an account of the justificatory force of the scientist’s reasons for adopting [a given theory]; an account that enlightens us as to why those reasons are good reasons.”

Even history of science acknowledged this hierarchy in its distinction of internal from external history—of the immanent, rational evolution of scientific knowledge from the intervention of psychological, political, economic, or other “extraneous” influences. Richard Burian characterized such philosophy of science as acting the sovereign judge of scientific achievement: “it is the philosopher who first reveals theoretical structures perspicuously, who clarifies theories and cognitive standards by which they are judged, and who assesses the logical consequences of theoretical claims” according to “universally valid methodological and epistemological standards.”

Burian raised three objections to this “Received View.” First, it made real scientific practice totally irrelevant, triggering the question of why such “logicism” should be considered a philosophy of science. Second, it could not prove that these standards held constant for all scientific practice over the course of history. Third, it could not grasp the distinct “career of a theory” (what Rheinberger would characterize as the local, developing experimental system) and its relevance to scientific rationality. Provoked by all this hubris, history of science and science studies set about debunking this “Received View.”

Thus, history of science came to be at loggerheads with philosophy of science. Nicholas Jardine posed the plaintive query: “Why do historians and philosophers of science expect to profit from each others’ inquiries: and why are their expectations so apt to end in disillusionment...? The expectations were perhaps most elegantly formulated in a squib from Imre Lakatos: “Philosophy of


122. Ibid., 310. Karl Popper exemplified this disciplinary sense of philosophical hegemony: “to me the idea of turning for enlightenment concerning the aims of science, and its possible progress, to sociology or to psychology (or... to the history of science) is surprising and disappointing.” Popper minced no words in 1965: “while the Logic of Discovery has little to learn from the Psychology of Research, the latter has much to learn from the former” (Karl Popper, “Normal Science and Its Dangers,” in Criticism and the Growth of Knowledge, ed. Imre Lakatos and Alan Musgrove [Cambridge, UK: Cambridge University Press, 1970], 58). He lamented, “all I have said before against sociologist and psychologistic tendencies and ways, especially in history, was in vain” (ibid., 57-58).

123. The Rise of Modern Science: External or Internal Factors, ed. George Basalla (Lexington, MA: D.C. Heath, 1968); For a more recent and quite different view of internal vs. external history, see Steven Shapin, “Discipline and Bounding: The History and Sociology of Science as Seen through the Externalism/Internalism Debate,” History of Science 30 (1992), 333-369.


125. For this increasingly confrontational relation, see William Rehg’s characterization of “Kuhn’s gap” in his Cogent Science in Context: The Science Wars, Argumentation Theory, and Habermas (Cambridge, MA: MIT Press, 2009).

science without history of science is empty; history of science without philosophy of science is blind.” 127 Working through these expectations we can elucidate the mutual disillusionment. One element in the squib Lakatos lifted from Kant seems fairly straightforward: “history of science without philosophy of science is blind.” 128 The reasons why historians of science need philosophy of science, Kuhn observed in the late 1960s, “are, at once, apparent and well known.” 129 Not only did historians borrow concrete terms from the philosophy of science, but it supplied them with methodological and epistemological standards, and perhaps even with the problems that needed investigating. 130 However, no single philosophy of science—not the “historicism” versions of Imre Lakatos, Steven Toulmin, or Larry Laudan, not Kuhn himself, and certainly not the “Received View”—proved adequate to historical purposes. In the words of Mary Hesse, “What has in fact happened is that, far from philosophy providing criteria for history, all forms of historical investigation, internal as well as external, have led to radical questioning of all received philosophical views of science.” 131 As Nickles put it, “providing intelligible descriptions and explanations of conceptual discoveries” is extremely difficult both for philosophers and for historians. 132 He elaborated, “reasoning is so complex and content-specific that no simple logic of discovery could possibly do justice to it.” 133 For the empirical disciplines the abstract-universal norms thematized in philosophy of science began to appear increasingly irrelevant. 134 “Philosophical models of proper evaluation are irrelevant to the historian’s task. Indeed, with their typical stress upon the formal, abstract properties of verbal argument, they can even impede an adequate naturalistic understanding of actual judgments. . . .” 135 Historians have made the best of eclectic choices among plausible terms, programs of investigation, and problems to generate a robust body of work. “The younger generation of historians of science . . . define themselves explicitly, if sometimes oppositionally, to the theo-

128. Ibid.
133. Ibid., 16.
134. Even in philosophy of science a clear trend has emerged aimed at disciplinary or problem-specific methodological and meta-methodological study. Philosophy of science has increasingly opted to work within the local domain of a scientific specialty. Indeed, Steven Fuller proclaimed in 1993, “the future of the philosophy of science lies either in some other branch of science studies (especially history and sociology) or in the conceptual foundations of the special sciences.” (Fuller, Philosophy of Science and Its Discontents, 2nd ed. [New York: Guilford Press, 1993], xii).
retical agendas of the sociology—not the philosophy of science.”136 Specifically, “no historian after Kuhn has tried her hand at divining philosophical lessons from the history of science—though some philosophers have: Lakatos, Laudan, and [Dudley] Shapere. . .”137 Instead, the estrangement became quite deep.138

What remains to be clarified is the other element in the Lakatos squib: “philosophy of science without history of science is empty.”139 The challenge is: “Just how does history serve as evidence for normative methodology; how do the facts of the past connect with what ought to be done in the future?”140 Richard Bernstein, for example, claims: “[A]n appeal to the past, to the history of philosophy, or to a more general cultural and social history is never sufficient. . . . I . . . reject the idea that history—in any of its forms—is or can be a foundational discipline, that it can answer the questions that we ask in philosophy.”141 That is to say, the sacrosanct “fact–value dichotomy,” ostensibly upheld from Hume forward, remained entrenched as the “last dogma” of positivism.142 By the 1980s, in fact, epistemological “historicism” appealed to only a distinct minority within mainstream American philosophy of science, whose preponderant concerns turned to the reassertion of scientific realism.143 As Steve Fuller put it, “with the latest round of the scientific realism debates, philosophers of science have returned to a quasi-transcendental mode of arguing. . . .”144 By the beginning of the 1990s, the link between history of science and mainstream philosophy of science became significantly attenuated. Thus, Fuller observed in 1991, “the past fifteen years have witnessed a steady retreat behind disciplinary boundaries.”145 “Sad but true. . . . HPS has gradually lost its momentum.”146 In several publications, the co-founder of the journal Studies in History and Philosophy of Science bemoaned this eventuality, and the Philosophy of Science Association organized a panel to discuss it.147 The trend did not abate, and many programs in HPS closed or mutated into other configurations.

138. See Rehg, *Cogent Science in Context*.
143. *Scientific Realism*, ed. J. Leplin (Berkeley: University of California Press, 1984). There are not a few philosophers even now who would claim that philosophy has no need of history, for example, André Kukla: “I think that the importance of history of science to philosophy of science has been greatly exaggerated” (“Scientific Realism, Scientific Practice, and the Natural Ontological Attitude,” *British Journal for the Philosophy of Science* 45 [1994], 969).
What are the prospects for a revival of HPS? There is—and there should be—no going back to the old relationship. But might there be a new one? I want to hold strongly for the prospect of renewed HPS, but I believe that “history of philosophy and science” is the ultimate warrant for “history and philosophy of science.” That is, as a historian, I am interested in adequate description and explanation of what philosophy and science have been in the modern period, and I contend that without their mutual constitution, no adequate account is possible; indeed, it is fundamentally misguided to believe that we can understand philosophy without reference to natural science or natural science without reference to philosophy. To advance the history of philosophy and science, and thus reinstate HPS on this empirical and modest foundation, it will be necessary to put behind us the master narrative that privileges physics as the paradigm of all science (indeed, all knowledge) and to consider the variety of “sciences” in the emphatic plural. It will also be necessary to recognize that knowledge is always a collective or communal acquisition, so that social epistemology and the sociology of knowledge will be at the core of any historical reconstruction. And, finally, it will be necessary to get past the hyperbole of fin de siècle “constructivism,” which could not find a way past “language” to take seriously the dogged concern with an actual material world that has been at the heart of science throughout the modern period, at the very least.\(^{148}\)

Naturalism’s return in philosophy of science is timely for those of us who seek to affirm the soundness of historicism against not only its old-fashioned positivist, but equally against its postmodern, critics. Nickles, Briskman, and others have termed this “a broadly ‘bootstrap’ account of the growth of knowledge.”\(^{149}\) In the words of Dudley Shapere, “we learn what ‘knowledge’ is as we attain knowledge, . . . we learn how to learn in the process of learning.”\(^{150}\) As Nickles puts it, “a defensible historicism does not rule out a bootstrap account of the development of knowledge; on the contrary, it requires it!”\(^{151}\) “Human knowledge has grown by...
means of a self-transforming, dialectical or ‘bootstrap’ process, rooted in variation, selective retention, and triangulation of historically available resources.”

The model here should be learning as a process, not truth as a whole: we must stress one aspect of Hegel against another. Nickles affirms a Deweyan Hegelianism: embracing “Hegel’s ‘methodological’ insights, his anti-dualism, and his historicist and sociological tendencies.” This pragmatist-naturalist reception is, he aptly affirms, only “weakly Hegelian because . . . it presupposes no transcendent Reason that shapes the overall developmental process.”

Under these new auspices, I seek a reconciliation between the human and the natural sciences—both to open out the “hermeneutic” dimensions of actual natural-scientific inquiry and to retrieve a sense of disciplinary legitimacy for the human sciences, and specifically history. Postmodern “theory” has lumped all empirical inquiry with “positivism,” a reductive simplification to discredit connection between history and evidence or warrant and thus arrogate history to fiction. If we first dispense with the positivist delusion of what science must be, as well as with the postmodernist delusion that language can never refer in any cognitively worthwhile manner, we can turn to the question of what historical inquiry can be, and put the methodological and epistemological questions of historical practice back into a sane context. History of philosophy and science, as an empirical inquiry with contingent and fallible standards of adjudication for adequacy and evidence, need not despair of intersubjectively confirmable results. History is no more a matter of fiction than natural science, though in both the scope for imaginative construction has been systematically downplayed in the epoch of positivist ascendancy.

Working out the epistemological and methodological character of historical accounting, then, is a necessary feature of this reconstitution of HPS. When philosophy of science abandoned the “context of discovery” to the empirical human sciences, it left a major theoretical matter to be clarified, namely how it was that history (and the other empirical human sciences) were to proceed in such empirical inquiry and to appraise their own efficacy.

As Nickles put it, “historians and philosophers have succeeded in making intelligible the routes to several important discoveries.” How did they do that? Historical practice presumes that “reasoning [in science and elsewhere] usually is so complex and context-specific” that it is pointless to seek a universal algorithm. That does not make it pointless, however, to take methodological bearings, to theorize the endeavor, and to use, in our theorizing, the exempla of “intelligibility” that we appear to possess. The want of totality does not betoken want of concreteness. Hilary Putnam evokes the key caution from John Austin: enough is not everything, but enough can be enough.

152. Ibid., 117.
153. That is, Hegel as theorist of dialectical process as against Hegel as philosopher of totality.
156. For a brave effort to articulate these matters across a broad canvas, see Roger Smith, Being Human: Historical Knowledge and the Creation of Human Nature (New York: Columbia University Press, 2007).
158. Ibid., 16.
That is what is so promising in Rheinberger’s “historicized epistemology,” with its “local wisdsoms.”

The whole idea of dialectic is that one never starts from nowhere. A problem is inconceivable without a context; it is always already mediated. If there is no ultimate foundation, there is always some platform. The process model of history of science offers us a rich vein through which to explore our own “path-dependency” and recover our situated point of departure. Two points can be established. First, retrospectively we can trace the routes of (some of) our insights. Second, making intelligible those successes can empower us, both psychologically and methodologically, to undertake new inquiries. Not only can we be confident that “problems and constraints do not fall out of the sky,” but we can be hopeful that we can reiterate at least some of the moves that led us through prior solutions and learn how to attempt new ones. Rationality is the concept we can articulate to affirm that science has indeed found a way of “bootstrapping stabilized past results and practices into the future.” Therefore, with Nickles I am “forced to reject [hyperbolic postmodernism], historicist though I am, as an untenably strong form of historicism.” With Nickles, too, I believe the project now is to “temper our historicism with a dose of (pragmatic) naturalism,” and achieve thereby a “more Deweyan sort of balance.” I think we need a theory that registers the entrenchment of practices, both as apparatus and as concepts, generating (path-dependent) structures that are heterogeneous and patchy, but nonetheless real and binding—hence a theory of constraints. And I think we need a theory that registers emergence: the radical novelty that erupts at the concrete level of event and agency in history. That is, I see the project to reconstitute HPS in parallel with the project of working through a robust historicist philosophy of history. I find Rheinberger’s work a vital resource in that project.

**Rice University**

160. “The dialectic does not simply set to work in a blind and immediate fashion, but proceeds rather from the indispensable insight that a beginning must always be made at a specific and determinate point, if indeed a beginning is to be made at all” (Rüdiger Bubner, “Closure and the Understanding of History,” in Bubner, *The Innovations of Idealism* [Cambridge, UK: Cambridge University Press, 2003], 179).

161. “In the long run, the conception of a sequence may serve as a scaffolding, which it may be convenient to discard later on, after it has given access to previously invisible portions of the historical edifice,” Kubler observed in *The Shape of Time*, 32.


164. Ibid., 88.

165. Ibid., 116, 89.