

Displacing Epistemology: Being in the Midst of Technoscientific Practice

Robert C. Scharff

Published online: 6 October 2010
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Abstract Interest in the Erklären–Verstehen debate is usually interpreted as primarily epistemological. By raising the possibility that there are fundamentally different methods for fundamentally different types of science, the debate puts into play all the standard issues—that is, issues concerning scientific explanation and justification, the unity and diversity of scientific disciplines, the reality of their subject matter, the accessibility of various subject matters to research, and so on. In this paper, however, I do not focus on any of these specific issues. I start instead from the fact that the very existence of the debate itself is an issue; in fact, it poses a philosophical problem that almost everyone but the hardest line logical empiricists has come to realize cannot be resolved epistemologically. In my view, however, that it cannot be resolved ontologically, either. I think the problem is at bottom hermeneutical, and its resolution requires that we focus first, not on the objects of science or the methods of studying them, but on the character of the philosophical orientation assumed by those who would try to resolve it. In this paper, I explain why I think this is so by analyzing (1) Dilthey’s contribution to the original debate, (2) Husserl’s reaction to Dilthey, and (3) Heidegger’s critical evaluation of both. This line of philosophical development—this movement of self-understanding from critiques of objectivism to hermeneutical phenomenology—is of course already a central feature of much work in continental philosophy of science. In my conclusion, however, I argue for the less well-established—even if apparently approved—idea that it ought to be a central feature of technoscience studies as well.

Keywords Philosophy of science · Philosophy of technology · Heidegger · Husserl · Dilthey · Verstehen · Positivism

1 Two “Kinds” of Science? What is at Stake

For traditional philosophy of science, the main problem posed by the Erklären–Verstehen debate is easy to state in general terms. The idea that there might be different “types” of

R. C. Scharff (✉)
Department of Philosophy, University of New Hampshire, Durham, NH 03824-3574, USA
e-mail: robert.scharff@unh.edu

science and scientific methodology challenges the foundational principle of the methodological “unity” of the sciences. In nineteenth century philosophy of science generally and for twentieth century positivism specifically, three things are taken to be self-evident. There is the social fact of scientific success; this success is guaranteed by the use, in every science, of some variant of a single method; and the main job of the philosophy of science is to articulate and defend this method. Given these three assumptions, to suggest that there might be fundamentally different “types” of science and scientific methods seems to threaten the very idea of scientific success. Even now, loose talk about many sciences is often dismissed as postmodernist excess.

Today, we sometimes forget just how high the stakes regarding this issue have traditionally been taken to be, not just epistemologically but socio-politically.¹ For in the opinion of everyone from Comte to the Vienna Circle positivists, and for many more down to the present day, all talk about different ways of knowing and multiple types of reality can mean only one thing—the threat of a return to the bad old pre-Enlightenment days of superstition and speculation, when all manner of fools and dogmatists claimed to know all manner of alleged truths using all manner of alleged methods.

The unity-of-science argument was thus no brainchild of the logical empiricists; and it was never a strictly epistemological concern. It was, from the beginning, the philosophical expression of a broadly secular, anti-speculative, science-promoting, and socio-politically progressive worldview.² I stress the point here, in order to forestall one very common mistake about the Erklären–Verstehen debate. Twentieth century logical empiricists and their followers have always been among the most vocal critics of Verstehen, and since their

¹ Judging by the claims of at least the more conservative participants in today’s science wars, there are many thinkers who still share the perception that all scientific practice must be Scientific or the modern world is in danger—although they are no longer as reflectively honest about it as their predecessors. I refer of course to the kind of outrage still expressed in some quarters whenever anyone challenges the positivist conception of science and its political importance as the prime expression of Enlightened Rationality. See perhaps most famously, Jean Bricmont and Alan Sokal, *Intellectual Impostures*, 2nd rev. ed. [1997] (London: Profile Books, 2003); Steven Weinberg, *Facing Up: Science and Its Cultural Adversaries* (Cambridge, MA: Harvard University Press, 2001); Paul R. Gross and Norman Levitt, *Higher Superstition; The Academic Left and Its Quarrels with Science* (Baltimore, MD: Johns Hopkins University Press, 1994.); and the collections edited by Noretta Koertge, *Scientific Values and Civic Virtues* (Oxford: Oxford University Press, 2005), and *A House Built on Sand: Exposing Postmodernist Myths about Science*, new ed. [1998] (Oxford: Oxford University Press, 2000). For a recent account of how deeply political the philosophy of science initially seemed to the logical empiricists, and ironically how their increasingly strident defense of objectivism just happened to help them stay out of political hot water during the McCarthy era, see George A. Reisch, *How the Cold War Transformed Philosophy of Science: To the Icy Slopes of Logic* (Cambridge: Cambridge University Press, 2005). See also, n.2.

² In *Cold War*, Reisch argues plausibly that the McCarthyism which was rampant in the US at the time of the immigration of numerous logical empiricists encouraged their silence about the public, political, and value-expressive dimension of “the scientific view of the world” so clearly stated in the logical empiricist “manifesto” of 1929 (“The Scientific Conception of the World”). However, Reisch tends to overstate the point by identifying this dimension with the “sentiment” described in the Manifesto, so that he can say that the Unity of Science Movement “died because its methods, values, and goals were broadly sympathetic to Socialism at a time when America and its colleges and universities were being scrubbed clean of red or pink elements” (“From ‘the Life of the Present’ to the ‘Icy Slopes of Logic’: Logical Empiricism, the Unity of Science Movement, and the Cold War,” in the *Cambridge Companion to Logical Empiricism*, ed. Alan Richardson and Thomas Uebel [Cambridge: Cambridge University Press, 2005], 60). As I am arguing here, the logical empiricist worldview is not just a “set of ideas” prominent in a “movement” that one might decide to suppress in bad times and resurrect in better ones. Rather, the Unity of Science Movement is itself expressive of the philosophical understanding that constitutes logical empiricism, and its suppression in the long run has more to do with its members own self-understanding, according to which the assumption that reflection on how it is to “be” a philosopher belongs to social science not philosophy keeps them mute about the need to consider the question even in good times.

criticisms are mainly epistemological, the general textbook accounts of the debate often simply assume this focus, rehearse the epistemological arguments and counter-arguments, and depict the whole debate as a methodological one that pits positivists and mainstream empiricists on one side against the progeny of the German historical school, Dilthey, and Southwest German neo-Kantians on the other.³

Yet telling the story this way ignores the underlying *worldview* that motivates the typical positivist objections to the allegedly unscientific character of *Verstehen*. From John Stuart Mill and to well past the last logical empiricist, most English-speaking philosophers would agree with what Carnap wrote 1928. Whenever our movement faces religious, political, or metaphysical opposition, he says, we have confidence that

those opposing powers belong to the past. We feel that there is an inner kinship between the [scientific] attitude on which our philosophical work is founded and the intellectual attitude which presently manifests itself in many other walks of life....We feel all around us the same basic orientation, the same style of thinking and doing....⁴

Given his commitments, Carnap can only speak of this “inner kinship” in passing, as a “sentiment” or attitude; for he denies, in principle, that sentiments are “philosophical” topics. In retrospect, however, we see this particular sentiment for what it really is—namely, the expression of a full-blown philosophical position—what Carnap calls the “scientific view of the world”—that is loaded not only with epistemic, but also with cultural, social, and political convictions.⁵

Carnap, of course, believed he was just defending what is scientifically essential, and doing so in a scientific way, namely, by being an epistemologist of science and philosophizing from a neutral position that, precisely because it is neutral, needs no defense. In retrospect, however, the unreflective and dogmatic character of the many variations of this once widely-shared position is unmistakable.⁶ And once this is seen, it is also clear why

³ When they are not strictly historical, most of these accounts conclude the story with the explanation that in retrospect, one sees that the debate was really just an artifact of excessively zealous positivists facing opaque Continental writers—at a time when the latter could not make plain and we did not yet realize that all science is interpretive and all interpretation is contextual. With the disappearance of logical empiricism and with the introduction of allegedly clearer Anglophone restatements of the initially obscure Continental claims about context and interpretation, what could there be left to debate? See e.g., the well-known quip by Charles Taylor, which is frequently used to confirm this post-positivist story, where we are asked to picture “old-guard Diltheyans, their shoulders hunched from years-long resistance against the encroaching pressure of positivist natural science, suddenly pitch[ing] forward on their faces as all opposition ceases to the reign of universal hermeneutics” (“Understanding in Human Science,” *Review of Metaphysics* 34/1 (1980), 26). It goes without saying that this story does not satisfy very many Continental philosophers.

⁴ Rudolf Carnap, *The Logical Structure of the World*, trans. Rolf A. George (Berkeley: University of California Press, 1967), xvii–xviii, my emphasis, translation slightly altered.

⁵ Today there is general agreement about the presence in logical empiricism of these convictions, and typically also agreement about the generally reformist and anti-traditional political leanings of its advocates (in spite of the obvious differences between “left” leaning—e.g., Carnap, Hahn, Frank, Neurath—and other—most famously, Schlick—members of the Vienna circle). But occasionally, someone questions this consensus by emphasizing the political diversity of their convictions and suggests that this very diversity weakens the claim that extra-epistemic concerns are rightly considered integral to the logical empiricist position. See, e.g., Sarah S. Richardson, “The Left Vienna Circle, Part 1. Carnap, Neurath, and the Left Vienna Circle Thesis,” *Studies in History and Philosophy of Science*, Part A, 40/1 (2009): 14–24. This argument, however, misses the main point I am stressing here, viz., that the unacknowledged “sentiments” in logical empiricism lie primarily in their philosophical commitment to scientism (Heidegger might say, to an ontology of *Vorhandensein*), however different their specific political and pedagogical expressions of this might be.

⁶ On these variations, see Reisch, *Cold War*, xx–xxx. Cf., Michael Friedman, *Reconsidering Logical Positivism* (Cambridge: Cambridge University Press, 1999), who argues that, at least in the case of Carnap, Schlick,

the Erklären–Verstehen debate cannot be settled in strictly epistemological terms. For the underlying problem is not whether Verstehen can be made “scientific” in the predetermined positivist sense. The issue is this very sense of science as an adequate characterization of any actual human practice. Hence, positivistic arguments against Verstehen have to be met in the first instance, not with epistemic counterarguments, but with a critical evaluation of positivism itself.

Today, of course, virtually all philosophers see themselves as post-positivists of some sort.⁷ Everyone—whether analytic, pragmatic, or continental in orientation—wants a more socio-historically “contextualized” account of science. Few are eager to think of themselves as advocates of a unity-of-science “movement”; and fewer still aspire to be the formal analysts of the one true, scientific method.⁸ Yet if rejections of the positivist worldview and self-image are now common, and less overtly “ideological” ideas of science are now widely embraced, reflections on the issue of what makes a position genuinely *post*-positivist are less so. Here, then, is my topic. In historical retrospect, the Erklären–Verstehen debate provides us with an opportunity to ask, How—if not positivistically—should we philosophize about science?

This self-reflective question is in fact familiar, not only to some of today’s North American post-positivists, but to most Continental philosophers as well. It is not just scientific practice but reflection on that practice that must be understood to operate “in the midst of things.” Indeed, as Heidegger taught many of us, we always actually do this anyway, even when we are busy assuring everyone that as epistemologists, we have assumed a standpoint outside of life from which to survey it in neutrality. “Being objective” is just as much a mode of being-in-the-world as existing in some other way in the midst of things which demands a less pinched and cognitively uniform conception human agency.

But the historicity of human existence—and thus the historicity of all attempts to think about this existence—was already a central issue for Dilthey before it became a theme for post-positivists and phenomenologists. He confronted this issue at the end of the nineteenth century as a philosopher of the Geisteswissenschaften, when he realized—as his opponents and neo-Kantian contemporaries did not—that the question of legitimizing Verstehen is ultimately not about two methods, or two subject matters. It is about finding

Footnote 6 continued

and Reichenbach, one must explain their differences primarily in terms of their varying reactions to German (readers need to add, specifically Marburg) neo-Kantianism (e.g., xvi., 232–233).

⁷ As I will try to clarify in what follows, I am using “post-positivism” in a more substantive sense than it is often employed by historians of analytic philosophy who classify, say, Quine, Hempel, and Popper as already post-positivists, simply because they raise questions about the specific epistemological doctrines usually associated with logical empiricism. See e.g., John H. Zammito, *A Nice Derangement of Epistemes: Post-Positivism in the Study of Science from Quine to Latour* (Chicago: Chicago University Press, 2004), 6–14. It is characteristic of those who use the term in this narrower sense to conclude with merely “deflationary” assessments of the future of the philosophy of science—according to which all of the legitimate problems raised by critics of positivism can be assimilated into a perspective that is simply less epistemologically arrogant, more Lockean and respectful of the way “empirical research” is actually conducted in the real world (Zammito, 2–3, 14, 275). See note 24, below.

⁸ But is it really the case that the “rational reconstruction” of scientific practice is, in its essence, an apolitical act? At the very least, this requires a defense it does not receive, and at worst, it suggests the quietistic suppression of a very political complicity with the McCarthyist spirit of the times—as suggested, e.g., in John McCumber, *Time in a Ditch American Philosophy and the McCarthy Era*: (Evanston, IL: Northwestern University Press, 2001). My own view is that both the idea of philosophy of science as neutral and the idea that later logical positivism (and by implication, at least the earlier versions of analytic philosophy which followed) were politically complicit are equally pitched too low philosophically and miss the deeper commitment to modern scientism that makes both of these positions possible options in the same universe.

a philosophical perspective from which to distinguish the disciplined (*wissenschaftlich*) study of our “living through” of historical life from the disciplined study of our external encounters with material nature. Near the end of his career, Dilthey began to call this needed perspective the “standpoint of life.” In what follows, I start by revisiting Dilthey’s struggle with this issue and consider what light it can still shed today on the question of how to philosophize about the sciences. This theme will lead us inevitably in the direction of Husserl and Heidegger, and, finally, to some implications for technoscience studies.

2 Dilthey on the Second Kind of Science

Dilthey is famously known, of course, as the methodological pioneer for specifically “human” sciences. From the start, however, he himself sees his defense of their separate method as necessarily grounded in an anti-positivistic “Critique of Historical Reason,” in terms of which he must challenge the dominant philosophical position that prevents *Verstehen* from getting a fair hearing. Thinking especially of Comte and Mill, Dilthey argues that judging from both their general conception of scientific practice and their specific analyses of the objects of genuinely scientific research, their general epistemological model is overly dependent on physical science. It is true that an observational outlook, the quantification of findings, and a focus on prediction—all of these requirements and restrictions—are rightly stressed if one’s aim is to “explain” natural phenomena. But they are inappropriate for disciplines that want instead to “understand” human-historical life—that is, to consider what Dilthey calls “the total nexus of psychic/historical reality” as this is “possessed” in lived experiences (*Erlebnisse*).⁹ Those who study natural events by considering the way they confront us externally can know nothing of this other possibility, since for them it is a matter of epistemic principle that one is only permitted to say, of lived experiencing, that it is going on inside an observable body. Yet in contrast to the way reality, as “nature,” is there for me in experimentally enhanced observation, argues Dilthey,

lived experience is another, distinctive and characteristic way in which reality is there-for-me.... [Experienced reality, or “historical life”] does not confront me as perceived or represented...but is there-for-me because I...possess it *unmediated* and as in some sense belonging to me.¹⁰

Today, Dilthey’s line of reasoning seems perfectly familiar. I rehearse it here only to underscore the fact that, contrary to the way it is often reported, Dilthey’s approach is primarily ontological, not epistemological. For him, the reason why we need a second way of studying human beings is precisely because we are “human” beings. Of course, it is true that human actions can be observed and predictive theories can be generated about their behaviors. But it is also true that human beings (and their creations) can be encountered and understood. We “are,” in other words, both natural *and* psychic-historical beings. Hence, as Dilthey conceives it, *Verstehen* is not another way to look at reality; it is the right way to look at another reality.

⁹ E.g., Wilhelm Dilthey, *Gesammelte Schriften* [hereafter, GS], 23 vols. (Stuttgart: B.G. Teubner; and Göttingen: Vandenhoeck and Ruprecht, 1913), GS 1: 359–373 [*Selected Works, Vol. 1: Introduction to the Human Sciences* (hereafter SW 1), trans. Rudolf A. Makkreel, et al. (Princeton: Princeton University Press, 1989), 192–206].

¹⁰ GS 6: 313 [*Selected Works, Vol. 5: Poetry and Experience*, trans. Rudolf A. Makkreel, et al. (Princeton: Princeton University Press, 1985), 223, trans. slightly altered].

In considering the implications of Dilthey's position, then, we must keep in view not just his epistemic arguments but the viewpoint from which he makes them. As an *epistemologist of the human sciences*, he does indeed see himself as legitimating a second conception of scientific method. But in order to do this successfully, he sees that he must also *think philosophically about science in general*. In this latter respect, he regards himself as an ontological opponent of positivism, not just an epistemologist of human science. For he realizes that if it is one's default position to define "reality," "experience," and "knowledge" in deference to natural science, this is not just epistemic preference. It is fundamental ontological bias; and in a philosophical atmosphere shot through with this bias, the case for a second kind of scientific method, no matter how well-formed and forcefully argued, cannot get a fair hearing. One must attack the hegemony of the natural-scientific background imagery of "external world," "knowing subject," and "explanatory" method, and show that its ontology does not define a topic-neutral philosophical orientation, as positivists assume. Only by challenging the hegemony of this implicit ontology can one establish that a positivistic outlook—however appropriate it may be in regards to the epistemology of nature—is not competent to be a general frame of reference for philosophy.

In fact, says Dilthey, that the positivistic idealization of the standpoint of natural science gets things backwards. When we study natural science as a human practice, instead of taking positivist self-descriptions of this practice at face value, we discover that that its theories are the main example of one of the three classes of life's manifestations (*Lebensäußerungen*)—specifically, that class of cognitive expression comprised of representative "concepts, judgments, and larger thought-formations." The primary characteristic of these cognitive expressions is that, in order to insure that their content "remains the same in every context," so that their meaning "is the same for the one who formulates them and the one who understands them," they must be "detached from the lived experience in which they arose, and...adapted to logical norms."¹¹ In other words, for observation reports and predictive theories to BE observation reports and predictive theories, they must be thought solely in the direction of their use and never in the direction of their genesis. Indeed, their efficacy depends on this. Explanations in the natural sciences are like the Great Oz. They do their work so long as one ignores what is behind the curtain—just as long, that is, as one refrains from "disclos[ing] how the[ir] logical content...is related to the dark background and the fullness of psychic life" out of which such theories develop.¹²

Dilthey's account here is not a criticism of natural science or of its often forgetful and detached abstractness. Nor is it merely a reminder that philosophers of science, unlike practicing scientists, cannot ignore the context of discovery. Rather, his aim is to make explicit and underscore the necessity of the relation between its self-imposed restrictions and the effectiveness of its theories. For theirs is a very powerful set of restrictions indeed. It involves a kind of blanket refusal, or studied disinterest in any disclosure of natural science *as the human*

¹¹ GS 7:205–206 [*Selected Works, Vol. 3: The Formation of the Historical World in the Human Sciences*, trans. Rudolf A. Makkreel, et al. (Princeton: Princeton University Press, 2002), 226–227]. This volume contains all of the notes for Dilthey's unfinished *Critique of Historical Reason*. It is worth mentioning, then, that the recent post-positivist search for "normativity" in life tends to presuppose hidden "norms" somehow buried in it that are now being unearthed by analysis. For Dilthey, life's basic "sense" is of a different order.

¹² GS 7: 206. There are complications here that I must ignore. Of course, in a certain sense, we *do* want to know the "origin" of particular scientific explanations—viz., in the sense that we want to be able to retrace the process by which their logical and empirical content came to be part of the actual theoretical claims we make. Otherwise, we could not test their degree of "representativeness." What Dilthey means, however, is that this is the only sort of "origin" we are permitted to consider, not the sort that would tell us more generally how we came to be concerned so exclusively with predictability, or with obtaining knowledge that gives us power, or to prefer this sort of knowledge to human understanding, or to wisdom.

*practice it is.*¹³ Natural science thus exists as the practice it IS, precisely in and through a *suppressed* utilization of, as Dilthey calls it, “the standpoint of experienced life” itself. Once the matter is put this way, the crucial philosophical implication is not hard to see. The very same characteristic that makes the outlook of natural science possible also makes this same outlook unsuitable for philosophy. Natural science becomes the success it is by a studied forgetfulness of its lifeworld origins. When philosophy does the same thing, however, it simply becomes arrogant.¹⁴ The question then obviously arises, what the standpoint of philosophy should be instead.

There is some evidence that Dilthey himself identified this issue, but it would be an overstatement to say he considered it.¹⁵ Stated quickly, Dilthey’s efforts to discriminate between natural and human science repeatedly drive him back, in his late work, toward the idea of what he calls the standpoint of life—a standpoint to which, depending on the context, he gives either a narrower epistemic or a wider philosophical interpretation. On the one hand, he describes it more narrowly as the standpoint both of what the human sciences study—namely, directly experienced “historical human life” as we live through it and as something intelligible as such—and also of the outlook human scientists must adopt if they are to understand this life “in terms of itself.” In this narrower sense, the standpoint of life is thus for the human scientist what the standpoint of observation is for the natural scientist.

On the other hand, Dilthey sometimes also does say, in a quite general way, that the phrase, “standpoint of life,” is really just a comprehensive philosophical name for the outlook that is attuned to the everyday, lived-through sense of things in terms of which we human beings already possess and *understand in a pre-scientific way* all of our “manifold powers”—including, of course, our powers to develop both natural- and human-scientific knowledge.¹⁶ In this sense, the standpoint of life is the ultimate phenomenon, our basic

¹³ Does this account of explanation make Dilthey a social constructivist? I think it does not, given that although he defends the possibility of a human scientific understanding of natural scientific practice, he (a) does not try to substitute this socio-historical account for the positivists’ formal-logical one, and (b) does not in any case think that his analysis of the nature of the practice is fundamentally causal. As I explain below, Dilthey really does not fully address this issue—one which, as Husserl and Heidegger both saw, cannot be resolved by epistemological or meta-scientific means alone.

¹⁴ It is interesting to note that the original positivist, Auguste Comte, treats this reductive/formalistic tendency in natural science much more astutely. He argues that this is a fully defensible tendency, as long as (i) one never “forgets” that, like all conceptualization to some extent, it is reductive and formalistic, and (ii) one refrains from being so impressed by the ease with which conceptualized phenomena can be arranged in theoretical systems that one comes to believe that the scientific method *as such* should be formalized. See my “Comte and the Possibility of a Hermeneutics of Science,” in *Hermeneutic Philosophy of Science, Van Gogh’s Eyes, and God: Essays in Honor of Patrick A. Heelan, S.J.*, ed. Babette Babich (Dordrecht: Kluwer, 2002), 117–126.

¹⁵ See e.g., GS 1: 384 [217], but esp. GS 7: 217–218 [238], where Dilthey refers to a “new important task” to be assigned to the human sciences through hermeneutics. Hermeneutics, he says, must now “define its task relative to the epistemological task of demonstrating that it is possible to know the nexus of the historical world and to find the means of bringing it about.” The comment is of course open to a narrower construal (in which case it is just neo-Kantian epistemology, applied to the human sciences) or a broader one (in which case, as Heidegger will insist, it points away from strictly epistemological accounts of *Verstehen* and toward the question of what is the nature of a science and what sort of philosopher is fit to ask this question.) See numerous exchanges on the epistemological significance of hermeneutics between Dilthey and Count Yorck in *Briefwechsel zwischen Wilhelm Dilthey u. dem Grafen Paul Yorck von Wartenburg 1877–1897*, ed. Erich Rothacker (Halle: Niemeyer, 1923), e.g., 185–187. One related issue, viz., the implications of the fact that no interpretation of experience will ever exhaust its content, has recently been treated in detail (though without specific reference to the issues raised here) by Jos de Mul, *The Tragedy of Finitude: Dilthey’s Hermeneutics of Life* (New Haven, CT: Yale University Press, 2004).

¹⁶ Cf., Paul Ricoeur’s observation that in Dilthey’s work, “historical understanding is not exactly the counterpart of the theory of nature; the relationship between life and its expressions was rather the common root of the double relationship of man to nature and of man to history. If we follow this suggestion, the problem

orientation toward a world of meaningful encounters and actions of all sorts. It is the living-thought of life itself, as Dilthey says, “back behind which thought cannot go.” Yet if thought cannot go behind experienced life, a hermeneutical philosophy might try to start with this truth about it. Hence, Dilthey occasionally talks about cultivating a reflectively enhanced philosophical self-awareness—a “*Selbstbesinnung*”—precisely of this lived-through sense of things, so that we might come to recognize how both “understanding life” and “explaining nature” can equally be regarded as manifestations of “lived-through” possibilities.¹⁷

The upshot of Dilthey’s scattered remarks on this subject, then, is that there appears to be the possibility of establishing a “*selbstbesinnlich*” perspective from which all life-possibilities could be considered without playing ontological favorites. As both Husserl and Heidegger realized, such a perspective would really deserve the elevation to general philosophical status that positivists wrongly claim for the objectivistic outlook of the natural sciences.¹⁸ Both of them take up this issue and make it central to their own concerns. I will treat their efforts serially, to frame a bit more concretely the question I am raising: How *do* we become philosophically *post*-positivist—about science, or about anything else—once we realize that this issue must be considered and resolved from the standpoint of life, that is, while remaining in the midst of things?

3 Dilthey’s Standpoint, “Phenomenologically” Construed (Husserl)

For Husserl, Dilthey needs more to be corrected than appropriated (the latter being what Heidegger will call “destructively retrieved”). He argues that because Dilthey’s conception of the “standpoint of life” only emerges in conjunction with his efforts to discredit positivism, it comes too late and is too much conceived as a mere negation to be able to stand philosophically on its own. Granted, Dilthey *claims* that the “experiential possession of life” is the root source for the ontological and epistemological constructs in both kinds of science. But how, asks Husserl, does he know this? Dilthey *claims* the standpoint of life is not merely

Footnote 16 continued

is not to strengthen historical knowledge in the face of physical knowledge but to burrow under scientific knowledge, taken in all its generality, in order to reach a relation between historical being and the whole of being that is more primordial than the subject-object relation in epistemology” (*The Conflict of Interpretations: Essays in Hermeneutics* [Evanston, IL: Northwestern University Press, 1973], 8–9). Nevertheless, as Ricoeur’s focus on Dilthey’s *work* suggests, my phrasing here is probably somewhat too generous to Dilthey himself—formulated as it deliberately is to frame it as an anticipation, as Heidegger acknowledges, of his own notion of being-in-the-world. See Martin Heidegger, “Wilhelm Dilthey’s Research and the Struggle for a Historical Worldview,” trans. Charles Bambach, in *Supplements*, ed. John van Buren (Albany: SUNY Press, 2002), esp. 162–165; but cf. *Ontology: The Hermeneutics of Facticity*, trans. John van Buren (Bloomington: Indiana University Press, 2002), where Dilthey is said to have had “little understanding” of the philosophical issues involved in furthering this aspect of his thought (11). See also notes 18 and 20, below.

¹⁷ Dilthey’s idea of a *Selbstbesinnung* that seeks “the foundation for action as well as for thought” and that is wide enough to be philosophically concerned with “differentiating among the facts of consciousness as well as the articulations based on this differentiation” appears as early as the “Breslau Draft” (circa 1880) of what was to be Book Four, Section One, of the *Einleitung*’s second volume [GS 19:79–80 (SW1:268)].

¹⁸ Heidegger is already discussing this matter in his early Freiburg lectures, as I explain in “Heidegger’s ‘Appropriation’ of Dilthey before *Being and Time*,” *Journal of the History of Philosophy* 35/1 (1997): 99–121. See also, István Fehér, “The Early Heidegger. Phenomenology, Hermeneutics, *Lebensphilosophie* on His Way to *Being and Time*: The Confrontation with Husserl, Dilthey, and Jaspers,” *Existential: An International Journal of Philosophy* (Hungary) 2 (1992): 69–96. Just when Husserl began to think through this issue, and how much his analysis owes to Heidegger, is still hotly contested. See e.g., Donn Welton, *The Other Husserl: The Horizons of Transcendental Philosophy* (Bloomington: Indiana University Press, 2001), 325–326, and references cited there.

the epistemological perspective of one kind of science. But Husserl objects that even in his late works, Dilthey still thinks of this standpoint as “psychological” and (most troubling) “historical.”

Husserl, as we know, at least initially insists upon hearing these adjectives with an empirical ear—that is, as betraying an essentially relativistic-historicist position, and so as a threat to the “rigorous” philosophical ideal that Husserl himself wishes to defend.¹⁹ Hence for Husserl, phenomenology must necessarily *replace* Dilthey’s “reflections from the standpoint of life,” not just exploit it. Dilthey may have avoided the naturalism which results from universalizing the outlook of mathematical physics, but his position is still tainted by psychologistic and historicist versions of the same disease.

Here as elsewhere, Husserl is famously and resolutely anti-naturalist—which he understands to involve the rejection of any philosophy that seems to make foundationalist use of an empirical perspective. Just as famously, Husserl has been widely criticized for the vestiges of traditional objectivism that his anti-naturalism appears to bring with it. The two sided reaction to Husserl’s phenomenology is well-known.²⁰ On the one hand, there has been wide acceptance of his view that practicing scientists are too focused on their little corners of the world, and traditional epistemologists of science have been too focused on analyzing the procedures of science, to ask the right philosophical questions about science itself. On the other hand, Husserl’s idea of the sort of phenomenology that would avoid these pitfalls has proven much less acceptable.

To state the punch line of a nearly 100-year debate, the real problem his readers have found with Husserl’s phenomenology does not lie in what he attempts to *do*. It lies with his still very traditional understanding of *who does it*. For as he sees it, phenomenology has a founder; it must be a movement; it must become the ultimate positivism and true guardian of the Western scientific ideal—and most problematically, it must think of itself as the phenomenology of a transcendental-rational *consciousness*, whose “pure” and methodologically secured standpoint guarantees the establishment of a properly rigorous philosophical approach that finally gets us “to the things themselves” as they really are.

Heidegger, especially, is put off by the traditionalism of Husserl’s self-descriptions; hence, even while he is still Husserl’s assistant, he is urging others to distinguish what phenomenology promises to become from what Husserl says it must be. On the one hand, many of Husserl’s substantive accounts—for example, of perception, of the lived as opposed to the

¹⁹ See most famously, “Philosophy as Rigorous Science,” in *Phenomenology and the Crisis of Philosophy*, trans. Quentin Lauer (New York: Harper and Row, 1965), 77–79, 123–127. The relation between Husserl and Dilthey is much more complex than this brief summary suggests. In their correspondence, Dilthey succeeded in disabusing Husserl of his initially harsh judgment of Dilthey’s work (Husserl later confessed to being “overly influenced” by Ebbinghaus’ review of Dilthey’s *Ideen*). Nevertheless, it is important to note that all his later generosity remains the reconsideration of a Husserlian phenomenologist, who now sees new possibilities for enriching that phenomenology by turning to Dilthey’s descriptions of psychic and historical life—but only after establishing that a phenomenological consciousness can never be a historical consciousness. For Husserl’s earlier reactions to Dilthey, see the introduction to the reprinting of “Rigorous Science” (Peter McCormick) and the translator’s preface to the English selections from the Husserl–Dilthey correspondence (Jeffner Allen) in *Husserl: Shorter Works*, ed. Peter McCormick and Frederick A. Elliston (Sussex: Harvester Press, 1981), 161–165, 198–202; and for his later reactions, Ronald Bruzina, *Edmund Husserl and Eugen Fink: Beginnings and Ends in Phenomenology, 1928–1938* (New Haven, CT: Yale University Press, 2004), 319–323.

²⁰ Perhaps in the next few decades, as commentators work through the late manuscripts, a picture will emerge of a less Eurocentric, science-minded, and essentialist Husserl. In the meantime, it is the phenomenology that originates with his *Logos* article and the familiar critiques of the problems with this model of phenomenology on which I will focus here, for this is the “phenomenology” that has figured in post-phenomenological developments. For a recent review of these problems, paired provocatively with remarkably similar problems in Carnap, see Joseph Rouse, *How Scientific Practice Matters: Reclaiming Philosophical Naturalism* (Chicago: University of Chicago Press, 2002), 28–76.

anatomical body, of the lifeworld origins of and differences between the mathematical and empirical sciences—all of these accounts are clearly superior to the accounts of the various positivists, neo-Kantians, and traditional metaphysicians of his day. Yet when Heidegger listens to the arguments among the members of these schools, *including Husserl's*, what is most striking to him is how similarly—and unsatisfactorily—they all understand themselves as philosophers. Each of them claims to speak for scientists who are too preoccupied with what they are doing to reflect on how it works; each defends a model of essential epistemic structures not relative to place and time; and all of them tend to think that first we must settle what *science* knows and then we can figure out what else there is.

In short, Heidegger sees himself surrounded by philosophers who—whether neo-Kantian, positivist, traditionally metaphysical, or Husserlian—still talk as if they were Cartesian subjects—that is, idealized meta-scientific knowers, who have resolutely removed their minds from the circumstances not only of scientific practice, but of culture, society, history...and, we ourselves should now surely add, race, ethnicity, and gender. However, Dilthey's descriptions of historical life taught the young Heidegger to see the irony here. Observe all these supposedly objective thinkers, quarreling among themselves in disregard of the deeply different and anything but "neutral" outlooks that prompt them to disagree in the first place!²¹ The conclusion one must draw is obvious. Before we plunge into any more explorations of what there is and how we know it, we must have some "preparatory" reflection on the question of what it means to "be" a thoroughly contextualized, historically determinate philosophical questioner...of *anything*. And this, famously, is Heidegger's lead question in *Being and Time* [SZ].

4 Dilthey's Standpoint, "Hermeneutically" Construed (Heidegger)

As he tells us, Heidegger develops his own approach to problem of the factual-historical determinateness of philosophical thinking, not primarily through a critique of Husserl, but in terms of what he calls a "positive appropriation" of the researches of Dilthey.²² In his view, what is essential to Dilthey's project is not the narrower, human-scientifically defined aim

²¹ In his comments on Husserl and Dilthey during this period, in the case of the former, Heidegger directs his criticism at both Husserl's self-conception and in his phenomenological descriptions (all of which are alleged to be tainted by Husserl's insistence that the whole of factual life is to be seen through the lens of consciousness, its acts, and its intentional comportment), whereas in the case of the latter, Heidegger pointedly distinguishes Dilthey's still somewhat traditional-bound epistemological self-presentations from his properly phenomenological intuitions, and he praises Dilthey's willingness to repeatedly affirm the results of these intuitions, even when they conflict with what his epistemology dictates "must" be assumed. Hence, in my view, the works of the early 1920s, show Heidegger coming to the view that Dilthey needs to be *appropriated*, but Husserl, *surpassed*. See my "Heidegger's 'Appropriation' of Dilthey....," esp., 117–128. The Being-question, says Heidegger, is "alive" in Dilthey, but not in Husserl (120).

²² He thinks of SZ's hermeneutic phenomenology as taking up Dilthey's "ownmost philosophical tendency," by means of an interpretation of the "elemental restiveness" that is evident in all of his writings. "Studies in the history and theory of science, and hermeneutic-psychological studies are forever overlapping and interpenetrating. Wherever one aim predominates, the others are already the motive and the means. What looks like disunity and an unsure, haphazard 'trying things out,' is really an elemental restiveness [*Unruhe*] in the direction of the one goal: To understand 'life' philosophically and secure for this understanding a hermeneutical grounding in terms of life itself" (*Sein und Zeit* [hereafter SZ], 10th ed. [Tübingen: Max Niemeyer, 1963], 398 [*Being and Time*, trans. John Macquarrie and Edward Robinson (New York: Harper and Row, 1962), slightly altered translation; references to German pagination in the margins]. I chose "restiveness" rather than "restlessness" for "*Unruhe*" because the former carries more forcefully the suggestion of "resistance to something that constrains"—specifically here, as I argue below, the constraint of residual loyalty to an epistemological ("spectator"-like) orientation.

of redeploying the old ideas of “persons” and their “psychic-historical structure,” as Husserl thinks. Rather, Dilthey is moving toward the more “substantial” task of “disclosing a new horizon for the question...in the broadest sense” of our very being.²³ It is this “new horizon” that Heidegger thinks is intimated in Dilthey’s late talk about the wider hermeneutical implications of starting from the standpoint of life. Hence for Heidegger, “appropriating Dilthey’s labors” means actually *starting* with the task that is already the implicit goal towards which all of Dilthey’s work is “restively” on the way.

Heidegger stresses, however, that this task must be conceived from the outset as a distinctively ontological one, and that as an ontological task, it faces a two-part problem. What makes Dilthey instructive is that he struggles with both parts but really only sees the first one. (1) What he sees is the need for an ontological reinterpretation of human be-ing, for he recognizes that historical life as it is “possessed” in experience reveals human beings to “be” in a way that is irreducibly different from and more basic than any observation of their material presence—or those observations, plus a lot of talk about interior mental states—could possibly capture. In his actual descriptions of how human-historical life appears when it is understood rather than explained, says Heidegger, Dilthey is clearly proto-phenomenological, and this leads directly to SZ’s analysis of temporal-historical Dasein in Division 2, Section 5 (esp. 397–405).

(2) But what Dilthey does not see is how his own initial philosophical self-conception blocks the development of this needed reinterpretation of our be-ing. He does not realize that his descriptions of historical life can never lead to its ontologically more appropriate characterization, so long as he accommodates these descriptions to a traditionally conceived epistemology of science. Dilthey, argues Heidegger, fails to take to heart the radical implications of his own phenomenological intuitions, to elevate these intuitions into acts of reflection, and to ask what it is to “be” a phenomenologist at all.

But this characterization of Dilthey’s position, cautions Heidegger, tells us only about how he fails. What makes Dilthey’s work so valuable is that his tradition-bound self-conception only *threatens* to subvert his findings. In his actual writings, he often displays a marked willingness to affirm the results of his “phenomenological intuitions” about historical life, even when these intuitions conflict with his initial research plan. In this willingness to be open to direct experience, even in the face of the “formal demands” of being a traditional epistemologist of science, Dilthey unknowingly tells us of the possibility of a new philosophical orientation towards which his own work is “on the way.”²⁴

Taking his cue from what Dilthey silently “tells” us, then, Heidegger’s develops a critique of traditional philosophy of science that is more “radical” than many later Anglo-American critiques. Take, for example, this recent account of the main revisionist line among analytic philosophers.²⁵ As post-positivists, we all agree to avoid the excesses of logical

²³ “From, respectively, SZ, 46 and 398; and *Prolegomena zur Geschichte des Zeitbegriffs (Gesamtausgabe 20)* Frankfurt: Vittorio Klostermann, 1979), 164 [*History and the Concept of Time: Prolegomena*, trans. Theodore Kisiel (Bloomington: Indiana University Press, 1985), 118–119].

²⁴ For more on Heidegger’s critique itself, see especially, the ten Kassel lectures of 1925, “Wilhelm Diltheys Forschungsarbeit und der gegenwärtige Kampf um eine historische Weltanschauung,” Walter Bröcker’s transcript of which is in *Dilthey-Jahrbuch 8* (1992–1993), 143–180 [trans. Charles Bambach, in Martin Heidegger, *Supplements*, ed. John van Buren (Albany: SUNY Press, 2002), 147–176, 195–197].

²⁵ My account here follows Zammito, *A Nice Derangement*, 272–275. I have attempted to bring out explicitly the author’s own position, which lies largely between the lines of his critique of science studies and “radical” post-modern philosophy of science, especially in his final two chapters. This position is, I think, representative of what is probably the majority reaction to other, more radical trends in philosophy of science after logical empiricism. As his citations show, Zammito’s own sympathies lie with what one might call “reformed” mainstream philosophers of science like Philip Kitcher, Ronald Giere, and Susan Haack.

empiricism; but this does not mean the basic issues have changed. No matter how pluralistic and anti-formalist we choose to be about method, there is nothing wrong with promoting an objective, third-person viewpoint; indeed, “epistemological realism”—with its stress on establishing criteria for justifying scientific claims about an independently existing world—is the prerequisite for the very possibility of any science, whatever its specific procedures. All the “contextualizing” that has occurred since the demise of positivism—all the talk about the social circumstances of scientific practice and the historically changing content of scientific theories—should of course make us less sanguine about the finality of present scientific claims and more mindful of how we make use of them. Yet none of this can raise any serious doubts that science is indeed the objective, empirical study of our surroundings or that its procedures and the language in which it expresses its results are, at least ideally, universal and non-contingent.

Heidegger cannot accept this line of reasoning, whether revisionist (as in the account just summarized) or radical (as with the social constructivists who embrace precisely the anti-realism the revisionists oppose). For as long as critics of traditional philosophy of science continue to argue about procedures, knowledge claims, their warrant and use, they continue to face away from the question of what kind of philosophizing about science produces this set of criticisms and arguments, and only these.

Zammito’s analysis thus shows (though perhaps not entirely on purpose) that today it is possible, even likely, that one can be deeply committed to a thoroughly contextualized conception of scientific knowledge and to the inclusion in this conception of political and ethical issues related to the acquisition and use of this knowledge...and still see all of this as nothing more than a revision and updating of the traditional epistemology of science.²⁶ But to remain silent about the status of any “revising,” conservative or radical, of traditional philosophy of science is not really silence—and it is certainly not innocent.

We should recall here what everyone from Comte to Carnap believed, even when it became unprofessional to say so: BEING an epistemologist of science means more than just analyzing and defending its methods. It involves embracing the cultural superiority of the broadly

Footnote 25 continued

[Incidentally, he misrepresents Ian Hacking—by reading his conclusions about the three epistemological “sticking points” concerning the status of scientific theories (i.e., issues concerning their apparent contingency, their alleged representativeness vs. nominalism, and their stability as favoring internalism) and by ignoring everything Hacking says about the grounds for sympathizing with externalist/social constructivist accounts (252–53).] According to Zammito’s own account, both positivists and radical post-positivists suffer from an inflated view of their role as philosophers of science who, according to long-standing assumption, are capable of producing a better idea than practicing scientists about the essence of the process. In the old days, it was logical empiricists who displayed this kind of arrogance; today, it is the radical post-positivists. The former dictated a priori rules to science; the latter denies there are any rules—or rather, that there are any rules apart from ones that are socially constructed and historically accepted. Thus neither positivists nor post-positivists, Zammito concludes, are in really in touch with how science works. The lesson to be learned from all this is that we still need to more “rigorously deflate” the old conception of a reconstructive philosophy of science, so that in the future, our analyses will be more respectful of the enterprise of science itself. In fact, he says, most scientific procedures are largely self-correcting; hence he urges a revival of the old image of the Lockean underlaborer as sufficient for philosophy of science. I omit this last claim from my text because it seems to me one way in which his view probably does not reflect the current majority.

²⁶ For example, in his recent analysis of the controversy over social constructivist accounts of scientific knowledge, Hacking shows how it is perfectly possible to remain committed to the fundamental distinction between epistemology proper and questions of the origin and use of science, and simply give it a social constructivist interpretation. See Ian Hacking, *The Social Construction of What?* (Cambridge, MA: Harvard University Press, 1999). Hacking’s own reliance on this distinction comes out most clearly in his chapter on the effect of the controversy on the epistemology of natural science, where he identifies those who reject and those whose sympathize with social constructivism as defending internalist/rationalist and externalist/empiricist accounts, respectively (esp., 90–96).

secular, instrumentally cognitive, device-filled, allegedly progressive world that science promises to create. This worldview did not lose its power simply because logical empiricism labeled it a “sentiment”; nor has it disappeared because everyone now says they are post-positivists.

Heidegger’s main objection, then, is not to the pinched, decontextualized, formalistic, and unreflective character of logical empiricism specifically. He objects to the very idea that a philosophical understanding scientific practice can be at bottom fundamentally epistemological; and it makes no difference to him how liberalized and self-conscious the epistemology might be. We must, however, understand this objection in the right way. It has often been assumed that Heidegger wants us to substitute an ontology of science for the traditional epistemologies; but this assumption is mistaken in two ways.

First, in Heidegger’s view, traditional epistemologies of science have never been primarily epistemologies, let alone “objective” in the detached and neutral sense that they think themselves to be. Nor are they mainly expressions of admiration for “genuine” (which usually means natural) science. Traditional epistemology of science, with or without the recent liberalizing revisions, is in fact the articulation of a general global outlook involving a deep and longstanding sense of what is really real and what we should do about it. This “onto-theological” or “metaphysical” outlook is not even modern in origin. To be sure, it now reflects and articulates explicitly the dominant mood of “developed,” technoscientific life in the West. But it does so by understanding modern science and its technology as a kind of totalizing fulfillment of the quest for cosmic knowledge that inspired Western philosophy from the start. This is why it has remained so easy—even after positivism’s official demise—to continue to think of philosophy as necessarily being at least *some* sort of analysis of *scientifically* informed knowing and acting, even if “analysis” no longer means the “rational reconstruction” of a single, idealized cognitive procedure. Even a thoroughly post-positivist mind, like its forbears, still tends to understand the world as most “real” when it is “there” in a science-like way. For us to “be there” today, together with and related to all these real things thus still seems to call upon us above all to be, or to be “like” a scientific knower or a philosophical meta-knower “in” this sort of world. As SZ explains, “Da-sein” is not a new metaphysical label for “the human entity.” It is a verbal noun, a formal indication of how it is to be in the midst of things—equally descriptive of how we are there for everything and everything is there for us. And “for the most part,” we understand ourselves and our activities in an at least proto-scientific way.

Once this vertical depth of any epistemology is recognized, we can see the second reason why Heidegger does not think of “replacing” traditional epistemology of science with something else. For without further reflection on “who” we are in approaching the question of scientific practice, “critique and replacement” is unlikely to be anything more than traditional-philosophy-as-usual, only with a different preferred choice. This time, we will choose a New Ontology rather than some variant of Epistemology.²⁷ In either case, “one” remains a dutiful Cartesian, carrying out the advice of the 4th Mediation: Use the autonomous Will to assent to whatever becomes clear to the pure, ahistorical, and depersonalized Mind. As Nietzsche noted, the nihilistic consequences of this line of thinking—*when it is not recognized as such*—would be more amusing if they were not so tragic and dangerous.

In short, the young Heidegger is increasingly drawn toward Dilthey at the expense of Husserl, not just because of the former’s superior descriptions of the historicity of human life, but because in trying to clarify the epistemic status of these descriptions, Dilthey’s works

²⁷ The capitalization is a nod in the direction of Charles Taylor, “Overcoming Epistemology”, in *Philosophical Arguments* (Cambridge, MA: Harvard University Press, 1995), 1–19.

seem clearly to imply something that his own philosophical self-conception keeps him from recognizing—namely, that establishing the rights of a second kind of science in an atmosphere where it already seems “obvious” that there can only be one kind involves much more than just a debate over methods and subject matters. Ultimately, it involves (to use Dilthey’s own word) the “hermeneutic” question of how *be* philosophically oriented when asking about the nature of any human practice, scientific or otherwise.

Hence, when Heidegger asserts at the beginning of SZ that knowing (*Erkennen*) is a “founded mode” of being-in-the-world, he is not showing anti-scientific bias, or expressing a preference for pragmatism or naturalism, or taking sides in the realism/idealism dispute. He is identifying that “manifestation of life” through which traditional philosophical self-understanding keeps even Dilthey from seeing what he has really found—just as later, this same understanding encourages logical empiricists, more arrogantly, to suppose that people like Dilthey have found nothing. Here, I think, is the real point of Dilthey’s far from fully exploited legacy: *Under the present circumstances*, if we trace all the debates about scientific methods and subject matters back to their sources, we see that the basic question—the one to put first in an age where “one” already tends to understand everything (including its “knowers”) technoscientifically—must be, “How do we learn to think *any* issue non-positivistically?”²⁸

5 Conclusion: Post-Positivism After (Heidegger’s) Dilthey

I conclude with a restatement of this main point, and an indication of why I think it matters, not just to philosophy of science, but for technoscience studies as well. In general, I have tried explain how the issue of what it is to BE a philosopher of science can—and should—arise if we are to do deliberately what Dilthey begins to do in spite of himself—namely, shift attention away from the usual epistemic and ontological topics and ask, What kind of philosophical orientation is really appropriate for treating these topics?

To put the matter in a slightly different way, what would it take to make this question part of philosophy, when the still widely influential “sentiment” that informs the scientific view of the world” tends to commit us to the assumption that the question is not part of philosophy? Joseph Rouse, in a defense of recent cultural and gender studies of science as constituting a direct challenge to any philosophy of science still infected with some variant of this assumption, notes that

the most significant barrier to [the] philosophical recognition of...[science] studies may be the predilection within much philosophy of science for grand historical

²⁸ We have here—to borrow shamelessly from Irigaray—a question of existential atmospherics. In our era’s general ontological atmosphere—an atmosphere in which *everything including ourselves and our reflections upon all this* automatically tends to be understood as enframed and at our disposal—there is only one way to establish the real significance of any currently marginalized experience of difference. Whether this difference involves two methods or two sciences or two sexes, we must begin by determining why and how this difference is handed down to us precisely *as* excluded and forgotten, and we must retrieve it precisely *as* something excluded and forgotten. See Luce Irigaray, *The Forgetting of Air in Martin Heidegger*, trans. Mary Beth Mader (Austin: University of Texas Press, 1999). My shamelessness is of course in part due to my giving Heidegger credit for something Irigaray denies to him. She argues that Heidegger, especially in his later ruminations on and “exclusive love of” earth, “forgets” to treat air with equal dignity. However, Irigaray then goes on to assume throughout that air should be understood as what is cleared at the site of the clearing, what in numerous other images she identifies as what “is at the groundless foundation of metaphysics,” which, when “recalled,” is the “ruination of metaphysics” (5). It therefore seems to me impossible to read her first chapter without seeing (hearing? sensing?) Heidegger’s late discussion of Ereignis between every line. For her own account of the retrieval of the excluded and forgotten, with special reference to science, see “In Science, Is the Subject Sexed?” in *To Speak is Never Neutral*, trans. Gail Schwab (New York: Routledge, 2002), 247–258.

narratives of the legitimation project, despite the infrequency with which terms such as ‘narrative,’ ‘metanarrative,’ or ‘legitimation’ actually appear within the philosophical literature.²⁹

Rouse’s point, of course, is that positivism IS such a grand narrative—or rather would be, if it were allowed to see the light of day. But because this is not allowed, it operates instead, in his colorful phrase, like a philosophical vampire, something “undead that still haunts our concepts and interpretations of nature, culture, and science.”³⁰ It remains a barrier, not because so many philosophers still embrace it, but because they continue to think in accordance with it, precisely while explicitly denying that they embrace it.

So, for example, today no one fights formalist battles over the unity of science and the singularity of its method; few say flatly that science presents us with the Truth about the World,³¹ and gushing about “the spirit of science” is certainly rare. But there are plenty of seemingly quite radical revisionists who insist on retaining the idea (a) that scientific theories *converge* on the World’s Truth, and (b) that it is ultimately internalist accounts that will tell us how science really works, no matter how interesting are the contingent, externalist stories of how it got that way. These two examples are drawn from Hacking, who calls them “sticking points”—epistemological beliefs going back at least to, say, Locke (who, as an empiricist, is willing to accept at least some externalism in accounts of scientific explanation) and Leibniz (who is not).³² Even if these beliefs are revised or watered-down, he argues, there will always be some philosophical legitimacy to taking either side with regards to them, and no post-positivism can make the appeal of these points go away. Hacking suggests that we follow him in scoring ourselves on a pro-or-con scale of 1–5 as to “where we stand” on each issue (99). For myself, he says, I am a nominalist about scientific theories, “because I was born that way” (233, n23).

It is difficult to imagine a better expression of the continuing power of an “undead” philosophical understanding, differing from its original form primarily in lower degree of stridency. Today, one may not find as many Carnaps, congratulating themselves for doing battle with the “opposing powers” of the scientific worldview. But how different, in fact, are those who were “born” like Hacking? In the end, one can forgive this one-time metaphorical lapse. Yet what does it tell us about our allegedly “post-positivist” times, when someone so deservedly well-respected for interpretive fair-mindedness would conceive today’s dispute among epistemologists of science as philosophically irresolvable, and would furthermore depict the dispute in a way that pits “social constructivists”—exemplified by those who have, say, feminist and/or political misgivings about internalist accounts of scientific objectivity—against defenders of the “modern tradition” of Locke and Leibniz—whom Hacking likens to Freedom Fighters who see Truth and Lie as the ultimate weapons against tyranny (94–96)? I refrain from pushing Hacking himself too far in the wrong direction here, but it does bother me that he fails to say anything that would help address the following question: Doesn’t his chosen model of intellectual disputes—where those with a personal or ethnic agenda are on

²⁹ *Engaging Science: How to Understand Its Practices Philosophically* (Ithaca, NY: Cornell University Press, 1996), 118.

³⁰ “Vampires: Social Constructivism, Realism, and Other Philosophical Undead,” *History and Theory* 41 (2002): 60–78, at 63.

³¹ The late Bernard Williams was an exception. See Hilary Putnam’s critique of Williams’ idea of science’s search for the “Absolute Conception of the World,” in *Renewing Philosophy* (Cambridge, MA: Harvard University Press, 1992), 80–107, with Williams’ replies in *Philosophy as a Humanistic Discipline* (Princeton: Princeton University Press, 2006), 180–199.

³² Ian Hacking, *The Social Construction of What?* (Cambridge, MA: Harvard University Press, 1999), 68–98. For a critical review of *Social Construction*, see Rouse, “Vampires,” 71–78.

one side, and those who are “objective” and focused in a time-honored way on the world’s Big Picture are on the other—sound uncomfortably familiar?

My main concern in this paper has been the way this undead, “general view of the world”—in which truth, objectivity, and knowledge of the real stand over against sentiment, subjectivity, and special agendas—still seems to animate even otherwise deeply “revisionist” trends in the philosophy of science. Of course, some revisionists themselves are aware of this problem, and at least some of them have tried to ground a “science studies” program to redirect philosophy of science toward new forms of pragmatism, naturalism, or phenomenology that transform the scientific worldview itself, not just reinterpret our concepts of scientific objects and scientific methods in light of it.³³ But what seems missing in many of these efforts is an awareness that this worldview is not only about what is real and what we should do with it; it is also about who one must “be”—namely, a knower of knowers, a meta-knower—to enact it. The failure to notice this dimension to the problem—a failure I discussed above in terms of the naïveté involved in thinking that revisions in the epistemology or ontology of science can be made simply by “choosing” them—explains why so much contemporary philosophy of science still feels like business as usual. Check any recent English-language anthology on the topic. The table of contents will typically be organized around all of the traditional epistemic issues—to which, perhaps, have been added “for balance” a couple of samplings from “continentalists,” “feminist critics,” or “historians of science.”

Consider finally, then, whether this naïveté is also in evidence in current philosophies of technology. On the one hand, analytic philosophers find technology only marginally interesting—except, of course, when the technological “applications” of science give rise to ethical or political claims for them to analyze and test. Predictably, “analytic philosophy of technology” is therefore typically (a) seen as an adjunct to (a moderately revisionist version of) the philosophy of science, (b) linked in the standard way with “ethical” issues that arise from the metaphysical and epistemological analyses that are already underway, and (c) based mostly on a literature that will be familiar to any analytic philosopher, with or without an interest in technology.³⁴

On the other hand, it would appear that continental philosophy of technology has been much more successful in making the transition from a traditionally conceived philosophy “of” technology to “technology studies.”³⁵ Most practitioners see and take for granted (a) that science is just as dependent on and materially/historically related to our technologies as our technologies are dependent on and related to science,³⁶ (b) that technoscientific practices

³³ Rouse, *How Scientific Practice Matters*; Joseph Margolis, *Science without Unity: Reconciling the Human and Natural Sciences* (Oxford: Blackwell, 1987); Patrick A. Heelan, *Space-Perception and the Philosophy of Science* (Berkeley: University of California Press, 1988); Martin Eger, *Science, Understanding and Justice* (Chicago: Open Court, 2006); Sandra Harding, *Is Science Multicultural?* (Bloomington: Indiana University Press, 1998), and the two special issues on French and German *Continental Philosophy and the Sciences in Angelaki: Journal of the Theoretical Humanities* 10/1–2 (2005). An old, but still very useful survey of the options, remarkably ahead of its time (1st edition, 1968), is Gerard Radnitzky, *Contemporary Schools of Metascience*, 3rd ed. (Chicago: Henry Regnery, 1973).

³⁴ See e.g., the *Stanford Encyclopedia of Philosophy* entry, “Philosophy of Technology,” at <http://plato.stanford.edu/entries/technology>, and the shorter summary by one of its authors, Maarten Franssen, “Analytic Philosophy of Technology, in *A Companion to the Philosophy of Technology*, ed. Jan Kyrrre Berg Olsen, et al. (Oxford: Wiley-Blackwell, 2009), 184–188.

³⁵ I’m assuming here the usual distinction now being made among philosophers of science, that has been carried over to the philosophical study of technology as well. For a good summary of the distinction and what is at stake in making it, see e.g., Joseph Rouse, *Engaging Science: How to Understand Its Practices Philosophically* (Ithaca, NY: Cornell University Press, 1996), 237–259.

³⁶ For a summary, see my “Technology as ‘Applied Science,’” in Olsen’s *Companion to the Philosophy of Technology*, 160–164.

cannot be conceived primarily through the prism of epistemological analysis, and (c) that technoscience studies itself begins and ends in the midst of the life it investigates. Thus continental technoscience studies tends to focus, in Ihde's phrase, on "...the in between..." of human beings and their world—the implication being that this leaves behind the main commitments of the whole modern tradition of the philosophy of science, with its epistemic bias, its patronizing attitude toward the history and social studies of both science itself and what it conceives as its technological appendage of (mere) applications. But has technoscience studies really done what these points say must be done? With all the recent talk of movements that have "chosen" to come "after" and be "post-" in relation to other movements, of "new waves" of thinking replacing older waves every decade, and of objectionable pasts (simply) being left behind, I am not so sure....

Author Biography

Robert C. Scharff teaches philosophy at the University of New Hampshire (Durham, USA), and served as editor of *Continental Philosophy Review* from 1994 to 2005. He is the author of *Comte after Positivism* (Cambridge University Press, 1995; 2002) and coeditor, with Val Dusek, of *The Philosophy of Technology: The Technological Condition—An Anthology* (Blackwell, 2003; revision in process). He publishes on nineteenth- and twentieth-century Continental philosophy (especially Dilthey, Heidegger, and the hermeneutics of science), the philosophy of technology, and the history of positivism (especially Comte and Mill, and the connection between classical positivism and recent analytic philosophy). He is currently finishing a book manuscript, "How History Matters to Philosophy," working on several articles concerning Heidegger's role in recent technoscience studies, and editing a Blackwell Guidebook Series volume on Heidegger's *Being and Time*.