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The Epistemic, the Cognitive, and the Social

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My aim in this chapter is to direct attention to a matter philosophers of science barely examine let alone satisfactorily address: the relationship between the philosophy of science and the theory of knowledge. Like many things we take for granted, this relationship is not well understood. Most philosophers, whether of science or the more traditional sort, would respond that the philosophy of science is applied epistemology—that is to say, it brings the categories and tools of analytic epistemology to bear on understanding the practices called science. Sidney Morgenbesser was, I believe, voicing the conventional wisdom when he quipped in 1968 that “philosophy of science is epistemology with scientific examples” (Morgenbesser 1967, xvi).¹ (There is, of course, another aspect to the philosophy of science—traditionally called “foundations” of science—that is seen not as applied epistemology but rather as applied metaphysics, a topic I do not investigate here.)

This probably all seems harmless enough, not least because of its utter familiarity. But this way of construing the provenance of the philosophy of science is not innocuous. To the contrary, the notion that one can make sense of science by conceiving of it principally as epistemology teaching by example is not only hubris on the part of the epistemologist—hubris after all is an occupational hazard of the philosopher and thus forgivable—but also, I will argue, presupposes the correctness of one particular approach within the philosophy of sci-

ence, specifically epistemic realism, while denying legitimacy to various other philosophies of science that have at least as distinguished a track record as realism itself does. Still worse, the vision of philosophy of science as applied epistemology forces us to treat as irrational many of the most interesting and important evaluative strategies used in sound science.

I argue that the philosophy of science is not, and should not be conceived as, an exclusively or even principally epistemic activity. This is because science is neither exclusively nor principally epistemic. I will arrive at these interrelated conclusions by a slightly circuitous route. I will begin by focusing on a familiar and specific example of the thesis that philosophy of science is reducible to epistemology. I refer to the conception of the philosophy of science as rational reconstruction, especially as that notion was developed by Hans Reichenbach (and, to a lesser degree, by Rudolf Carnap) in the 1930s and 1940s. I will show that Reichenbachian reconstructions are not simply, as some might have supposed, broad-based case studies of philosophically interesting episodes in science but are instead subject to severe constraints with respect to which bits of real science are reconstructible and which are not. I will show that these constraints are imposed by the acute limitations of the tools of epistemology.

The first point to establish, and it is easy work, is that Reichenbach saw rational reconstructions as devices for identifying the epistemically salient features of any given scientific episode. This means that they are—and this is the first important thing to note about them—rational reconstructions only in a very attenuated and idiosyncratic sense. As conceived by Reichenbach in the opening chapter of his *Experience and Prediction*, rational reconstructions are not attempts to clean up the details of a scientific episode by showing how or to what extent the elements of the episode promoted the ends of the investigator. That sort of instrumental rationality is patently not what Reichenbach had in mind when he talked about rational reconstructions. Rather, for him, the freight that the term *rational* carries in that phrase was purely epistemic. He argued that the only features of any actual situation that appropriately belonged in a rational reconstruction were those bearing on the truth or the falsity of the theory or hypothesis being evaluated in the episode in question. I repeat: for Reichenbach, rational reconstructions were purely and simply epistemic reconstructions. Insofar as the actual case involved activities or principles that,

however rational in their own right, had nothing demonstrable to do with determining the truth or falsity of an hypothesis, those activities and principles found no rightful place in the so-called rational reconstruction of the case. The same point applies to Reichenbach's oft-mentioned but little understood distinction between the contexts of discovery and justification. This set of polarities marked, for him, not different temporal stages in an investigator's research but simply the difference between a descriptively rich but philosophically irrelevant account of an episode of the sort you might see in a history book and the very different but purely epistemic account that was to constitute, for him, the rational reconstruction of the episode. For Reichenbach the context of justification consisted of all and only those factors essential to the epistemic evaluation of the theory in question. Everything else — that is, everything not epistemic — was relegated to the context of discovery and consigned to the psychologist or the anthropologist for further investigation. The philosopher's interest in the episode was limited strictly to those elements that passed epistemic muster.

Now, if you support the idea that philosophy of science is applied epistemology, you may find nothing unseemly in Reichenbach's delineation of the task of philosophy of science as that of working with rational (understood now as epistemic) reconstructions of episodes rather than the episodes themselves. Besides, you might add, any philosophically interesting account of any human practice will have to simplify and idealize the blooming, buzzing confusion of the real world in order to have a manageable unit of analysis. I have no problems with simplifications, not even with oversimplifications, when they serve a useful purpose. But what is fishy here is that much of what drives scientific activity, even scientific activity at its rational best, are concerns that have no epistemic justification in a strict sense and that must be excluded from rational reconstructions of science as understood by Reichenbach and most of the others who have construed the philosophy of science as applied epistemology. The rest of this chapter will attempt to deliver on this claim. I aim to show that many, and arguably most, of the historically important principles of theory appraisal used by scientists have been, though reasonable and appropriate in their own terms, utterly without epistemic rationale or foundation.

I focus on one family of examples, among many that I might have chosen. My central argument will depend on noting the frequency and persistence with which scientists insist on evaluating theories by asking

about their scope and their generality. Several familiar and important rules of thumb in theory appraisal speak to such concerns. For example, acceptable theories are generally expected to explain the known facts in the domain (“saving the phenomena”), explain different kinds of facts (consilience of inductions), explain why their rivals were successful (the Sellars-Putnam rule), and capture their rivals as limiting cases (the Boyd-Putnam rule). I trust no reader will dispute the ubiquity of rules of this sort in evaluating scientific theories. The question is whether such rules have, or can be conceived to have, any grounding in epistemology per se.

Consider the first rule on this list, to explain the known facts in the domain. Steady-state cosmology was rejected in the 1960s not because it had been refuted but because it offered no account of the cosmic background radiation discovered at Bell Labs. The uniformitarian theories of Hutton, Playfair, and Lyell were rejected by most nineteenth-century geologists, not because they faced massive refutations, but because they steadfastly refused to say anything about how the earth might have evolved from its primitive initial condition to the condition of habitability. Plate tectonic theory triumphed in the 1960s over stable continent geologies principally because the former, but not the latter, could explain long-familiar patterns of continental fit and similarities of fauna and flora between the Old World and the new. Galileo famously argued for the rejection of Ptolemaic astronomy because it could not explain why Jupiter should have moons or why the sun should have spots. He plumped for the Copernican alternative because it could explain such facts about the solar system. The Jovian moons and the sun’s spots did not refute any claim in Ptolemy’s *Almagest*. Their potency derived from Ptolemy’s system evidently lacking any mechanism for making sense of such phenomena.

More generally, it should be uncontroversial that scientists frequently argue for one theory over another if the former can explain or predict something about the world not accounted for by its rivals. I daresay no one regards this as a specious form of argument against a theory. Few would quarrel either with the notion that a theory is, all else being equal, better if it can explain or predict facts from different domains or if it can show its rivals to be limiting cases. This is, nonetheless, a form of argument that has, and can have, no epistemic foundation. Our other three rules about the scope of a theory exhibit the same disconnection from epistemology.

None of these rules can have an epistemic rationale since it is neither necessary nor sufficient for the truth of a statement that it exhibit any of these attributes. That a statement fails to explain a fact with which it is strictly compatible is no argument against its being true. Indeed, most true statements do not exhibit this virtue. Similarly, the fact that a statement explains only one type of fact, rather than several, is no reason to believe that it is false. Indeed, most true statements do not explain different kinds of facts. Likewise, the fact that one statement cannot explain why one of its contraries worked so well is no argument against its truth since most true statements cannot explain why their contraries, if successful, are successful. Finally, we do not generally expect true statements to be such that some of their contraries can be shown to be limiting cases of them.

If these attributes of scope and generality are virtues, and I believe they are, they are not epistemic virtues. They address questions about the breadth and range of our theories rather than questions about their truth or probability. (It is true that philosophers have sometimes tried to describe these virtues of scope as epistemic virtues. Recall, for instance, William Whewell's labored but unsuccessful efforts to show that consilience-achieving inductions are bound to be true. Boyd and Putnam tried to argue that capturing a predecessor as a limiting case was an argument for the truth of a theory.) As I have argued elsewhere (Laudan 1981), no one has shown that any of these rules is more likely to pick out true theories than false ones. It follows that none of these rules is epistemic in character.

Indeed, one can piece together a perfectly general proof that these attributes cannot be epistemic indicators. I do not set much store by such arguments myself, but for those who do, they look like this:

Let T be some theory exhibiting one of the virtues of scope, v . Now, T will have many consequences; and infinitely many of those consequences will lack v , since many of the logical consequences of a statement of broad scope will fail to exhibit such scope. Focus on any one of those consequences, which we will call c_1 . Now, if T is true, c_1 must likewise be true. If T is highly probable or verisimilar, c_1 must be even more probable or have more verisimilitude. In short, thanks to the truth-preserving character of entailment, c_1 will necessarily possess all the epistemic virtues of T , while failing to have v . It follows that v cannot be an epistemic virtue since statements failing to exhibit v (such as c_1) are at least as solid epistemically as statements like T , that exemplify v .

This fact should discomfort no one save the epistemologist. It does not show that subjective values drive science or that "merely aesthetic"

yardsticks predominate. What it does show is that scientists have expectations about good theories that go well beyond worries about their veracity. If you have any residual doubts on this score, simply ask yourself whether any serious scientist would countenance every statement that he or she believed to be true to be an acceptable theory. Scientists may regard truth as an important virtue (we can argue about that another time). But what cannot be gainsaid is that there are other virtues of theories that loom at least as large in theory evaluation as truth does. By definition, these cannot be epistemic virtues since many false statements exhibit them and many true statements do not. By definition, they can find no place in a so-called rational reconstruction of science driven by an epistemic agenda.

Bas van Fraassen famously argued that a theory does not have to be true to be good. We can add to that dictum a new twist: a theory does not have to be false to be bad. A theory may be bad because it fails the test of possessing the relevant nonepistemic virtues. In other words, we expect our theories to do much work for us, work of a sort that most merely true statements fail to do. However, we may cash out precisely what that additional work is, and when we do so, we will move beyond the epistemic realm into one I call cognitive but nonepistemic values.

Such values are constitutive of science in the sense that we cannot conceive of a functioning science without them, even though they fail to be intelligible in the terms of the classical theory of knowledge. These values have nothing to do with philosophical semantics or with justification conditions, as usually understood. For that reason, I call them cognitive virtues or values, of which the epistemic virtues form a proper (I almost said uninteresting) subset. I have focused thus far on one family of cognitive virtues having to do with the range or scope of theories. Another family of such concerns is whether, in Phillip Kitcher's language, the theories in question achieve "explanatory unification."² Like the virtues of scope and generality, the virtue of explanatory unification cannot — counter to claims that Kitcher has sometimes made about it — be a truth-related virtue since it is obvious that every unifying theory, T , will entail non-unifying counterparts, T_1^* , T_2^* , ..., T_n^* , which must be true if T is true. If scientists regard T as a better theory than any of those weaker counterparts — and they invariably will — this must be because T possesses, and T_i lacks, virtues that are nonepistemic.

If all of this is even half correct, we see that the Reichenbachian formula for putting together a rational reconstruction is fatally re-

stricted and that this restriction speaks to the limits of application and relevancy of epistemology itself. What goes on in science at its best eludes the resources of the theory of knowledge to explain or to justify. Moreover, the Reichenbachian formulation declares to be philosophically irrelevant — mere fodder for the psychologist, the anthropologist, and the sociologist — many of the most important factors that go into theory evaluation in the sciences. Using Reichenbach's own language, my criticism is that he shunts far too much into the context of discovery and leaves little more than bare bones in the context of justification.

If, however, we were to understand rational reconstruction as a technique for analyzing science using the cognitive values that constitute it, not just the epistemic values, the line of demarcation between these two contexts would shift drastically. The context of justification would now recognize concerns about scope, generality, and range of application — and possibly explanatory scope as well — as a part of the rational reconstruction of any episode. The fact that such factors are nonepistemic would be neither here nor there, since rational reconstructions in terms of cognitive values would not, on my proposal, be limited to strictly epistemic factors. Imre Lakatos once argued that the appropriate criterion for evaluating the adequacy of a rational reconstruction of an episode in science involves asking how much of the activity of the scientists involved is captured by the rational reconstruction. By that yardstick, cognitively based reconstructions are clearly preferable to epistemically based ones.

But much more is at stake here than what sort of rational reconstructions we countenance. As I said at the outset, the notion of a rational reconstruction is merely a stalking horse for a larger target of my critique. I refer, of course, to analytic epistemology itself. I submit that once we consider the role that issues of scope, generality, coherence, consilience, and explanatory power play in the evaluation of scientific theories, it becomes clear that science is an activity only marginally or partially epistemic in character. Because that is so, the instinct to reduce the philosophical analysis of science to epistemic terms alone — and there are entire philosophies of science (Bayesianism, for example) committed to doing just that — must be resisted.

Likewise infected by the reduction-to-epistemology bug is the whole of the statistical theory of error. As everyone knows, statisticians recognize only two types of error: accepting a false hypothesis and rejecting a true one. But, of course, once we see that science has aims other

than the truth, we recognize that there are whole families of error types associated with each of the relevant cognitive values. Thus, a scientist may accept as explanatory a theory that is not explanatory and reject as nonexplanatory a theory that is. Likewise, a scientist may wrongly believe a theory to be capable of explaining different types of phenomena. These errors—which find no place in the contemporary theory of error—can be just as fatal to a theory as the more familiar errors of accepting the false and rejecting the true. Statisticians do not recognize these errors as errors because they are not epistemic errors. The error statistician, like the Reichenbachian rational reconstructor, takes his cues entirely from the epistemologist. That would be intelligible if and only if we had reason to suppose that the only demand scientists appropriately made of their theories was that they be true. We have no reason for such a premise. Consider, for example, that most scientists would reject, or at least consider badly flawed, a theory that failed to explain prominent facts in its intended domain of application, however good that theory was at capturing the facts it chose to explain. Error statisticians can no more make sense of such an appraisal than epistemologists can. Since scientists do make these additional demands on a theory, and have good reasons for doing so, it is time that the statisticians, like the epistemologists, recognized how severely limited the tools are that they currently bring to the task of explicating scientific rationality. I was sorely tempted to make the same observation about the Bayesians who, like the error statisticians, are fixated on adjusting probability assignments, and indifferent to the role of values in theory assessment other than that of avoiding a Dutch book and falling into losing betting strategies about the truth. I said I was tempted, but I know better, since ingenious Bayesians, like Paleozoic omnivores, can find some way of digesting almost anything in their path.

By way of summary of the argument thus far: We would like to have theories that are true, elusive as that ideal may be. But we would also like theories that are of great generality, that focus on the things we are particularly interested in understanding, that explain as well as predict, and that consolidate existing successes while moving us beyond them. Of all these matters save the first, the epistemologist knows little or nothing. Because, like the early Wittgenstein, epistemologists cannot speak of things they know not; they must maintain a studied silence with respect to most of the values that drive scientific research.

To this point, I have said nothing about the third element in my title,

the social. This is perhaps just as well since my thoughts on that topic are more fluid than on the other two. But if forced to fulfill the contract implicit in my title, here is what I would say, at least on Thursdays: There is a century-long philosophical tradition, dating back at least to Marx and Mannheim, of supposing that theories whose acceptance seems to involve exclusively epistemic values do not require the same sort of social-psychological explanation as theories lacking the epistemic virtues. Recall that the whole theory of ideology was developed specifically to explain why people came to believe notions for which there were no compelling epistemic arguments. Strictly speaking, there is something wrong with this way of proceeding since epistemic factors themselves function in, and evolve out of, social interactions among inquirers. In that very broad sense, every human artifact, including human beliefs and conventions about belief authentication, is grounded in social processes of communication, negotiation, and consensus formation. But this sense of the term *social* is so broad as to be vacuous. What I think Reichenbach had in mind, when he identified the context of discovery as the appropriate domain of the social, is the idea that social processes of belief fixation that lack an epistemic rationale are of no philosophic interest (except perhaps as sociopathologies) and their study should be left wholly to the social scientists. By contrast, thought Reichenbach, where there is an epistemic justification for a belief, the philosopher has a legitimate interest in exploring that justification and in arguing the relevance of that justification to the belief itself. If we are minded to draw a line between the social and the rational along these lines, my own suggestion, of course, would be that the philosopher should lay claim to interest in all beliefs for which there is a cognitive rationale, as opposed to only those beliefs for which there is an epistemic rationale. Unlike Mannheim, who defined the scope of the sociology of knowledge in terms of beliefs for which no epistemically compelling rationale exists, I would prefer to see that scope defined in terms of beliefs for which there is no cognitive rationale. Still, as I said earlier, my views on the relationship of the cognitive to the social are too complex, and too tentative, to be reduced to a simple formula.

What I have no hesitation about is my insistence on the explanatory poverty of purely epistemic values and the resultant need to talk philosophically about science in categories that go well beyond the merely epistemic.

NOTES

1. During the late 1960s, Lakatos frequently made similar observations during his seminars at the London School of Economics.

2. Kitcher has formulated this argument in many places; for its most detailed elaboration see Kirchner (1993).

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