Philosophy as a Science and as a Humanity

Commentary on Philip Kitcher's *What's the Use of Philosophy?*[*]

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I. As a Science

It is a dispiriting picture. A cadre of highly intelligent individuals, ambitious and enterprising, devote their days to an assortment of abstruse puzzles that appear to lack any intrinsic significance. The puzzle-solvers themselves are in large part unable to explain the value of what they do; they are driven, it appears, by some mix of delight in the challenge of the game and desire for the admiration of their fellow puzzlers. The puzzles are not easy—they require an extraordinary level of focus and intellectual energy, and even years of concerted effort may fail to produce a solution. The rewards for success, however, are sweet: a lifetime sinecure in an institution created both as a haven for present and as a hatchery for future puzzlers.

These oases of intellectual onanism, in Philip Kitcher's portrayal, are none other than the more prestigious philosophy departments of the English-speaking world, in particular those, like my own, that specialize in varieties of philosophy associated most closely with the genre-defining term "analytic"—

[*] Dedicated to Philip Kitcher, who has been an inspiration in so many ways. Although I'm not 100% sympathetic to analytic philosophy myself, to better serve the dialectic I wrote this as a straight riposte. For the most part.

analytic epistemology, analytic metaphysics, and metaethics are on a number of occasions singled out.

"Philosophy at its greatest is synthetic," declares Kitcher (p. 54). One of the great recent models of philosophical synthesis, he rightly suggests, is Thomas Kuhn's *The Structure of Scientific Revolutions*. It might, indeed, be a model for everything: "We ought to think of other areas of life as Kuhn invited us to think about the sciences" (p. 150). I gladly accept the invitation, in order to compare the ways of analytic philosophy with those of empirical science.

"Normal science", in Kuhn's portrayal (Kuhn 2012), is conducted within a framework, a field's "paradigm", that lays out among other things the field's important questions, its theoretical foundations, and a set of techniques built on those foundations that are to be used to answer the questions. The paradigm is sufficiently wide in scope and replete in prescriptive detail as to transform scientific inquiry into a set of puzzles. It specifies the rules to be followed in solving the puzzles along with the criteria for a successful solution, and—like the *New York Times* crossword editor—it guarantees that a solution exists, if only the scientist is clever and committed enough to find it.

Scientific puzzles are far more challenging than *Times* crosswords; they require an extraordinary level of focus and intellectual energy, and even years of concerted effort may fail to produce a solution. Like the crossword, however, they often appear to lack any intrinsic significance. Further, the puzzle-solvers themselves—ordinary scientists—are so deeply immersed in the paradigm that they are unable to explain the value of what they do; they are driven, it appears, by some mix of delight in the challenge of the game and desire for the admiration of their fellow puzzlers. It is a dispiriting picture.

Yet Kuhn suggests quite the opposite: not only the puzzle-solving aspect of science, he proposes, but in particular the intellectual narrowness that it fosters in its practitioners, is essential to science's success. How so? In Kuhn's words, the paradigm's puzzle-posing, by "focusing attention upon a small range of relatively esoteric problems, … forces scientists to investigate some

part of nature in a detail and depth that would otherwise be unimaginable" (p. 25). It is in the course of these investigations that researchers turn up those troublesome but persistent phenomena—what Kuhn calls "anomalies"—that alone have the power to overthrow prevailing ideas about the workings of nature, and thus the power to overthrow the paradigm itself. The puzzle-posing is, in this way, "essential to the development of science" (p. 25).

Kuhn emphasizes again and again the importance of normal scientists' limited vision. They are not motivated to do potentially ground-breaking work because they grasp the possibility of shifting the paradigm. Quite the contrary: like the crossword aficionado, they are prepared to embark on the project of solving a puzzle only if they can be confident that a solution exists, and this confidence flows from their confidence in the validity of the paradigm itself. A sense of proportion; an alertness to the wider significance of the undertaking; more generally, a healthy skepticism: any of this would fatally undermine the project of normal science.

Might what Kitcher identifies as the pathologies in contemporary analytic philosophy—the narrowness, the love of technical detail, the pursuit of cerebral sport heedless of any consequential benefit—serve the same salutary function as the intellectual insularity of Kuhn's normal scientists? Driven on by "the challenge of the puzzle"[1] and untroubled by questions about the worldly importance of their endeavor, the analytic philosopher might be freed to channel the utmost energy with the fewest reservations into the creation of arguments and thought experiments, some of which stand, in the long term, to topple the philosophical prejudices of the day and to open up ground for wholly new ideas.

Philosophers of course pride themselves on being methodologically self-conscious, and Kitcher urges them to be more so. But perhaps the most productive philosophers owe their success precisely to their lack of self-reflection? To their taking the rules and conventions of the field at face value and running

1. Kuhn (2012), p. 36

with them, rather than brooding about the deeper meaning of the game?

The omphaloskeptic character of institutionalized philosophy, then, may not be as decadent as at first it appeared. There is plainly reason to worry, however, that it does not play the same beneficial role as it plays (according to Kuhn) in science: philosophy has made nothing like the same kind of progress as science, either in degree or in character. The anomalies turned up by Kuhnian puzzle-solvers have precipitated intellectual revolutions; those turned up by philosophical analysts seem only to have precipitated disagreement and confusion—albeit numerous tenure-earning papers—concerning the biggest questions, at least. Scientists have figured out the nature of light; why can't philosophers figure out knowledge?

Kitcher would account for the impasse, on the evidence of this book, by maintaining that philosophical anomalies—clever counterexamples, in particular—for the most part do no more than adjudicate minor issues of merely provincial importance; they consequently lack the giant-slaying capacity of their scientific counterparts.

Consider, for example, the attempt to answer the question "What is water?". A part of the solution to the problem has been provided by empirical science, beginning with Lavoisier's discovery that water is made of hydrogen and oxygen, and then by subsequent discoveries that allow us to understand how these two elements are put together to create the H_2O molecule. But there is more work to do. As a number of philosophers have observed, to say that water is H_2O is inadequate in a number of ways. Water containing impurities, such as sea water, may still count as water—but not always, as coffee, which contains about the same amount of non-H_2O content as sea water, seems not to qualify as a kind of water. Perhaps more alarmingly, it seems correct to say that water is electrically conductive, yet pure H_2O is not conductive (it is solutes that make it so). Does that mean that pure H_2O is not water?

We might generate a barrage of ingenious counterexamples to beat the empirical raw material provided by science into a more perfect formulation

capturing necessary and sufficient conditions for waterhood.[2] Yet this is surely a prime example of the "intellectual busy work carried on by socially absent-minded men" denounced by John Dewey with Kitcher's staunch support.[3] Or rather, although such an exercise might be of considerable use to cognitive psychologists or semanticists attempting to understand natural kind terms and concepts, to researchers who study water and other substances for their own sake, the metaphysical interest of the "What is water?" question seems largely to have been exhausted once the science is done; what the philosophy adds is by comparison of rather slender interest.

The predicament might be explained as follows. Analytic philosophers' demand for "complete clarity" (Kitcher's leading pathology of philosophy), that is, for necessary and sufficient conditions, assumes that there is a sophisticated and exhaustive rule for determining of any given specimen whether or not it counts as water.[4] The "What is X?" question is interesting when the contents of that rule reflect some significant fact about our world—that the clear, refreshing liquid in our rivers and seas is largely composed of H_2O, for example. But the rule, if it exists, may reflect more than that; it may reflect somewhat arbitrary decisions (no doubt largely tacit) about exactly where to draw a kind's boundary lines that are of limited interest even to the deciders themselves.

You might even suppose that insofar as necessary and sufficient conditions do exist for any intellectually tantalizing item of our vocabulary, they are bound to reflect some of this finicky border-drawing, "busywork" whose social role is more to give our terms determinate meaning than to reflect

2. As in Strevens (2019, §10.2), drawing on many previous thinkers.

3. Dewey, *Reconstruction in Philosophy*, quoted in *What's the Use?* on several occasions (pp. 27, 103, 111).

4. The demand does not rule out the possibility that "water" is vague, as the necessary and sufficient conditions may themselves contain vague terms or terms put together in ways that engender vagueness. Many philosophical analyses (rightly, I think) take advantage of this affordance. But still it is assumed that the criterion for waterhood will give a definite answer to the "Is it water?" question, with the allowance that "borderline" is an acceptable answer.

anything of real importance out there in the universe. (Or at least you might suppose that this is the case outside of logic and mathematics.) Then the crafty counterexampling of analytic philosophers would turn out to be nothing more than the equal and opposite busywork that painstakingly recovers the content of these original conventions—a kind of ethnography that examines facts about the subjects' conceptual framework that even they themselves consider to be of nugatory importance.

That would provide a clear answer to Kitcher's question. What is the use of philosophy? No use at all. Indeed, even synthetic philosophers might worry that their labors amount to little more than the recovery of the minutiae of a formerly opaque filing system, that they then fuss around filling with the truly significant work of the scientists, legislators, or artists whose collaborators they fancy themselves to be.

But the water analogy is a bad one. The famous counterexamples of analytic epistemology—the Gettier cases, Truetemp, or the excursion to fake barn country—were not developed to settle mere border disputes, but to adjudicate hypotheses about the fundamental stuff of knowledge and justification, to do for epistemology what Lavoisier did for chemistry. You might contest their efficacy, but not their ambition.

That, any fair assessment of recent work in analytic philosophy would surely have to agree, is in general true. Counterexamples in the philosophy of causation are intended to decide whether causality is at bottom counterfactual, statistical, or something else. Those in normative ethics aim to determine whether it is pleasure or duty that animates moral obligation. And so on: philosophy's most influential counterexamples are intended, and widely understood, to play the same role as anomalies in Kuhnian science: to serve as the brave little facts that bring down great theories and make progress possible. If philosophical thought-experimenters conduct themselves in the same way as empirical scientists, then, even with all the ugliness that implies, it might be no bad thing for the enterprise of philosophy as a whole.

It doesn't follow that there's nothing to worry about—either in science or philosophy. Kuhnian normal science is in effect a "gamification" of inquiry, substituting for the discipline's true goals more proximal goals that are closely connected but not identical (Nguyen 2020). When the pressure to win the game is intense—as when jobs and attention are scarce—players search ever more intently for the cheapest effective strategies, which tend to encourage behavior that realizes the true goals rather poorly. Hence the advent of the least publishable unit, *p*-hacking and data dredging, *h*-index bravado, the replicability crisis, and all that.[5]

Some of what Kitcher rues in contemporary professional philosophy has the same character. I've talked to philosophers struggling to find a niche in the profession who know quite well that their published work is of limited value—but they need a job all the same. As in the case of science—and indeed across the academy—these problems ought to be taken very seriously. But the core of the problem lies in the social and institutional implementation of the methodology, not in the methodology per se.

That said, the contrast between philosophy and science remains embarrassing to those on the armchair side of the divide. Perhaps the analytic method, even when executed with skill and determination, is not sufficiently powerful to answer the corresponding epistemological, metaphysical, or meta-ethical questions? Perhaps the questions do not have determinate answers? These possibilities are explored by writers such as Williamson (2007), Machery (2017), Stoljar (2017), and Strevens (2019), but they are not discussed at any length in *What's the Use of Philosophy?*, which is more a moral than a methodological treatise. What most certainly is discussed, and what I will address in the second part of this commentary, is a worry about the value of the questions posed by analytic philosophy—about whether, even if philosophical analysts

5. These morbidities are surely not so much exploits of any particular paradigm as of certain more general rules governing scientific research across paradigms, such as those described by Strevens (2020).

could bring their inquiries to a successful conclusion, it would be worth their while to do so.

II. As a Humanity

"What is DNA?" is a question worth answering, writes Kitcher, and so is "What is justice?". Other philosophical inquiries fare less well:

> It's hard to see what any new answer to "What is knowledge?" would do for us. (p. 102)
>
> Suppose that, tomorrow, the philosophical community arrived at an unusual consensus: Moral realism is correct. What would be the payoff? Extraordinarily little. (p. 104)

If you respond that the knowledge might be worth having for its own sake, then you are verging on "narcissistic elitism", or putting aside the psychopathological epithets, you are failing to respond to "interests that are widely shared" (p. 103), pursuing "clever solutions to puzzles of interest only to a privileged coterie" (p. 152).

It is true that you seldom hear the nature of knowledge or the metaphysics of value discussed on the F train. But nor are the structure of DNA or numerous other, far more obscure scientific questions subjected to searching examination between West 4th St and Broadway-Lafayette. Why is an interest in knowledge so much more shameful than an interest in entropy?

I see three possible answers in *What's the Use of Philosophy?*. The first, which clearly plays a role in Kitcher's low estimate of the value of analytic epistemology and its kindred areas, is his perception that a good amount of contemporary debate concerns the sort of niggling questions about exact boundaries discussed above. I don't deny that some philosophers, intent on narrow analytic game-playing alone, have gotten involved in border disputes concerning territory of no intrinsic interest. I dare say that we've all reviewed

papers like that, and we know that it is hard to issue a rejection for a technically correct submission to a middling journal. The explanation is not narcissistic elitism, I think, but something more common and more human. I have already argued, however, that much work in analytic epistemology addresses questions that are quite central, questing after the philosophical equivalent of the double helix.

The second reason that Kitcher may reject analytic epistemology wholesale is suggested by his eye-rolling attitude to the seemingly endless project of attempting to give necessary and sufficient conditions for "*S* knows that *p*". Perhaps analytic philosophers, seeking out those puzzles that are most perfectly suited to solution by their characteristic methods, have settled on questions that are singularly useless for addressing the larger concerns of the field. Perhaps "*S* knows that *p*" is a poor choice of research topic, an error that has not been corrected because epistemologists, ploughing their Kuhnian furrow in ever more "unimaginable detail and depth", have lost sight of their greater goals.

Yet again, this characterization does not ring true. The analysis of propositional knowledge has led, among other things, to a close examination of a standard for epistemic security that appears to be deeply meaningful to ordinary human beings—that, arguably, uniquely entitles us to assert something or to count something as evidence. Even if there turns out to be no such standard, the search for one is hardly trivial or elitist.

This may be far from obvious, I concede, when philosophy is caught *in media res*. A traveler finding themselves stranded in fake barn country without a guide might well infer that this prodigious prank is a "bizarre fantasy of no practical significance" (p. 62). But then the same traveler wandering the arid fields of Hanford, Washington, and coming across the cyclopean structure that is one part of the LIGO facility for detecting gravitational waves—a great "L" in the desert, with scrupulously straight arms each two-and-a-half miles long—would surely be quite as mystified as we are by the lines at Nazca.

The journals are full of tedious and inconsequential work. Everyone knows that. But mediocrity is not a distinctive feature of philosophy; it is in copious supply in the sciences, my colleagues assure me. And perhaps in all human endeavor, or so says Sturgeon's Law—"90% of everything is crap"—a precept that could be improved, perhaps, only by prepending "At least".[6]

A third and final reason to depreciate the analytic mood in philosophy lies closest to Kitcher's Dewey-inspired zeal for social, moral, and political progress:

> When philosophers no longer ask after the point of a concept … they … lose sight of possibilities of conceptual reform. (62)

The ameliorative project in epistemology and elsewhere—the project of finding standards, techniques, and practices that are superior to those we now have—is a noble one, heartily to be encouraged (Haslanger 2000; Bishop and Trout 2005; Burgess and Plunkett 2013). Why, however, should it preclude other projects principally concerned with our current practices and ways of thinking?[7]

The quotations that opened this section suggest a possible answer: of a new theory of knowledge, Kitcher asks "What would it do for us?". Of a vindication of moral realism, "What would be the payoff?". The currency is not specified, but all signs point to personal and social improvement. What's wrong with analytic epistemology, then, is that it shows our standards for knowledge as they are, rather than as they should be.

That might indeed seem objectionable to a person in a great hurry to get to the future. But Philip Kitcher, author of studies of James Joyce and Thomas Mann, is certainly not such a person. Among Joyce's and Mann's supreme

6. https://en.wikipedia.org/wiki/Sturgeon%27s_law

7. In my own work on the topic, I argue that this is a false dichotomy, as concepts such as that of knowledge operate like natural kind concepts, homing in on objectively significant aspects of the subject matter in spite of the erroneous or parochial nature of our present beliefs (Strevens 2019). I do not have the space to make that case here, however, and so I thought I would try a different line of argument.

goals is the portrayal of human beings in all their present glory and variety, in all their disrepair, disgrace, and despair. Even if *Death in Venice* is not entirely lacking in useful advice ("Don't eat the strawberries!"), its value surely lies in large part in its depiction of a human spirit who is far from exemplary.

Edmund Gettier's masterwork "Is Justified True Belief Knowledge?" does not, it is true, have quite that level of literary panache, but in its own modest manner it contributes to our understanding of ourselves and of our image of ourselves, of the norms we rely on to get through the world—and of the ways, when we fall short of our ideal, that we nevertheless scrape through.

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