Discussion

Kuhnenstein: or, the Importance of Being Read

STEVE FULLER
University of Warwick

I respond to Rupert Read's highly critical review of my *Kuhn vs Popper: The Struggle for the Soul Science*. In contrast to my pro-Popper take on the debate, Read promotes a Wittgenstein-inflected Kuhn, whom I dub "Kuhnenstein." Kuhnenstein is largely the figment of Read's—and others'—fertile philosophical imagination as channeled through scholastic philosophical practice. *Contra* Read, I argue that Kuhnenstein provides not only a poor basis for social epistemology but Kuhnenstein's prominence itself exemplifies a poor social epistemology for philosophy. Nevertheless, like Read, I wish to speak in favor of amateurism in philosophy; for me, the exemplar is the dialectical Popper rather than the gnomic and dogmatic Kuhnenstein.

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I have always liked Rupert Read. He is one of the few people of my generation who brings an emotional intensity to the page that recalls an older existentialist attitude toward philosophy. Read is a living reminder that ideas matter, and those fortunate to possess ideas do not deserve to be treated lightly. Alas, it seems that I stand accused of precisely this, at least in the case of Thomas Kuhn. Still, I should not complain. My *Kuhn vs Popper* (Fuller 2003) is accorded two-thirds of his recent review essay (Read 2005), the final third of which is devoted to Nickles (2003), a much more favorably regarded academic treat-

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ment of Kuhn by a set of authors whose collective efforts double the length of my merely "popular" book.

The title of my response is not a feeble attempt to imitate Saul Bellow (i.e., *Ravelstein*) but a slightly less feeble attempt to imitate Mary Shelley, author of *Frankenstein*, the monstrous creation soldered together out of spare human parts by a misunderstood scientific genius. For Victor Frankenstein read Read, who has concocted an intellectual monstrosity out of bits of at least two personally alienated, culturally assimilated, technoscientifically trained, philosophically half-educated members of the early-20th-century industrial aristocracy. From a sociological perspective, what is most striking about the bits of Kuhn and Ludwig Wittgenstein that Read—but not only Read—has assembled into "Kuhnenstein," the brilliant amateur who in a few pages of simple prose can dismiss—as well as deter—technical philosophizing is the ease with which economic capital has been converted into cultural capital. This is a familiar theme from the work of the late sociologist Pierre Bourdieu, who observed this process in artistic achievement in modern France: offbeat rich guys with the right connections can be unleashed on an impressionable audience to great effect—especially when the connections remain hidden.

My response to Read proceeds in five parts. The first and largest section concerns some errors of fact and interpretation of my work, which I believe are not malicious but simply the unintended consequence of Read's parsimonious approach to reading. Here the contested nature of "social epistemology" is highlighted. The second concerns how one judges the relative significance of Kuhn and Popper, an issue that exercises Read considerably. Third, I critique the normative status of Read's Kuhnenstein, especially the style of philosophical amateurism and language therapy associated with the monstrosity. Fourth, I explicitly critique Read's own rather superstitious text fetishist approach to philosophy. Fifth, and finally, I discuss briefly the sense in which Popper's style of philosophical amateurism can be seen as a worthy alternative to Kuhnenstein's.

1. READ'S HERMENEUTICAL HOWLERS

Read writes at such length about my book that he makes his hermeneutical strategy clear. As he says, "I did not have to read far into this book in order to conclude that it is worthless" (Read 2005,

370) But suppose he had read the rest of its contents—let alone the works of mine on which he comments in passing? At the very least he would have been spared some howlers.

First, Read says I am "following Hollinger" in claiming that both Kuhn and Popper are misunderstood by both philosophers and nonphilosophers. Unfortunately, David Hollinger, a prominent historian of 20th-century U.S. intellectual life with some interesting things to say about the deformation of science and liberalism in the cold war era, is also a prime contributor to this misunderstanding. He is responsible for the ill-advised historical analogy that would have us imagine Kuhn's "encounter" with Popper (such as it was) in London in 1965 as a latter-day version of Galileo's appearance before Cardinal Bellarmine at the Inquisition (Hollinger 1995, 452). Hollinger gets the historical drift exactly backward (Fuller 2000, 303; Fuller 2004, 7-9). The force of the Galileo episode is to suggest the opening up of a previously closed intellectual space, whereas the Kuhn episode is precisely about the closure of such space, as the Popperians brandished Enlightenment ideals in a vain attempt to stave off Kuhn's heads-down, authoritarian conception of science, which happened to be empirically adequate to the condition of scientists on both sides of the Iron Curtain.

Read's next howler is to suggest that I got from *him* the idea that Popper offered a critique of Kuhn from the political left, not the right, as Popper is often portrayed. But this is not an idea that lends itself to proprietary claims, since it is easily obtainable by taking literally the multiple political references and allusions that characterize the arguments contained in Lakatos and Musgrave (1970), the official record of the 1965 encounter. Indeed, this seems to be what Read did in his own book, since he adds nothing new to the discussion (cf. Sharrock and Read 2002, 99-103). I began to delve more deeply into the politics underlying the Kuhn-Popper debate when pondering the misbegotten nature of Hollinger's Galileo analogy. Here I was influenced by Mark Notturno's archival work on the leftist roots of Popper's vision of the "open society" (Fuller 2000, 305, fn. 94), which at roughly the same time was being pursued by Jeremy Shearmur (1996) and Malachi Hacohen (2000). Indeed, in *Kuhn vs Popper*, I singled out Notturno (1999) as the book that Popper would have most welcomed about his work. It is worth observing that Sharrock and Read (2002) is bereft of references to Shearmur, Notturno, or Hacohen.

A third howler concerns Read's understanding of social epistemology, especially as a movement within the recent history of philosophy. Read clearly sees Kuhnenstein as a potential foundation for social epistemology, a project with which he seems to want to identify. However, he apparently fails to recognize the philosophers who are the actual target of my complaints about the current state of the project. These are the analytic philosophers—Alvin Goldman, Keith Lehrer, Philip Kitcher, and perhaps Helen Longino—who see social epistemology as an amplification or application of the individualist tradition in the theory of knowledge that extends from Descartes to Quine. For these people, "the social" is a "factor" that may impede or facilitate knowledge acquisition under various conditions but is not itself constitutive of knowledge. Social epistemology thus becomes a special case of analytic epistemology rather than a challenge to its individualist foundations.

In contrast, I hold that knowledge is constitutively social, which implies that the institutionalized pursuit of knowledge—aka science—ultimately picks out a certain range of social organizations (Fuller 1988). Individuals may be licensed to make knowledge claims, but the holders of those claims—to whom the individuals are accountable—include universities, academic disciplines, professional guilds, accrediting agencies, and the like. These bodies are, in turn, under the normative regulation of the larger society, some of which is codified as law and subject to regular political scrutiny, not least through the electoral process. In other words, as my conception of social epistemology is fleshed out, it becomes clear that philosophy of science turns into a kind of normative sociology of knowledge.

On the surface, this looks compatible with Read's Kuhnensteinian social epistemology—at least we seem to share the same individualist foes. But, I fear, in this case my enemy's enemy turns out *not* to be my friend. Kuhnensteinians reduce the sociology of knowledge to intersubjective negotiations that result in groupthink. Of course, groupthink persists only as long as the constitutive members hold common views. Yet Kuhnensteinians do not seem to recognize that individuals may distinguish in their own minds between a dominant or presumptive position and the one they themselves adopt, at least for the sake of argument. For them, entertaining a hypothesis is tantamount to holding a belief. Indeed, like much fashionable analytic social epistemology these days, Kuhnensteinians appear willing to outsource their beliefs to experts and others to whom they

defer as worthy of "trust." Yet as Popper rightly stressed, science is about hypothesis testing, *not* belief formation—let alone belief delegation. From this perspective, what passes for "trust" among analytic and Kuhnensteinian epistemologists is simply wishful thinking about what follows from the fact that we rarely have the cognitive, temporal, or material resources to test everyone's knowledge claims (Fuller 1996).

A society that does not have regular means for placing interpretations of reality at odds with the reality so interpreted does not have science. Of course, the Kuhnensteinians are empirically correct to say that reality appears to us as already interpreted, seeing is always seeing as, and so on. But from a normative standpoint, these facts should be seen as liabilities to be overcome, or at least sublimated, not badges of honor proudly worn. Thus, my own social epistemology shares with the logical positivists and the Popperians the view that "science" is not simply the name of one among many sets of practices that happen to command the allegiance of people in modern societies. Rather, it is the name of, so to speak, the metapractice dedicated to challenging the terms of all such allegiances. In this respect, science is continuous with critical philosophy, except that it is more grounded in the practices criticized. Indeed, this is what makes science appear more "empirical" and "technical" than philosophy, at least as it is normally practiced today.

Here I should add that perhaps the biggest mistake Read makes in his understanding of the history of social epistemology is to suppose that it is new. This is to take analytic philosophy's historiography of the discipline too much at face value (for a critique, see Fuller 2002). On the contrary, social epistemology is better seen as a revival movement that aims to *return* the theory of knowledge to its more robustly sociologized status from, say, 1750 to 1950—of course, taking into account our increased empirical understanding of the nature of knowledge. Here I mean to encompass inter alia Diderot, Condorcet, Kant, Hegel, Comte, Whewell, Mill, Marx, Mach, Peirce, Dewey, Neurath, and of course Popper. All of these philosophers placed what is nowadays called the "division of cognitive labor," the "economy of inquiry," and "science as a social problem" at the center of their thought. This tradition of course remains strong in continental philosophy, which never really lost touch with Kant and Hegel. Here I include Habermas and Foucault, despite, at least to my lights, their respectively earnest and jaundiced approaches to the topic.

Common to all these thinkers—and what distinguishes them from both the Kuhnensteinians and the analytic social epistemologists—is that they conducted epistemological or metascientific inquiry from the standpoint of a state-like entity. In other words, they treated the pursuit of science as centrally taken decisions on how to organize a set of people and material resources in charge of producing knowledge with purchase for an entire society. Over the past two centuries, the substance of this task came to be taken up by both "science policy" and "policy science" as branches of social science, while the most formal and general features of the enterprise remained—typically in a depoliticized form—in philosophy as "methodology" focused on the problem of "theory choice." Be it called "science policy" or "methodology," the goal was neither "truth" as a universal whose content is perpetually deferred nor a simple consensus of the current epistemic practitioners. Rather, in most general terms, the goal was to construct a sense of "humanity" that is greater than the sum of the individual members of *Homo sapiens* who constitute it at any one time. It was, in short, the project of the Enlightenment. At any given moment, of course, this project was subject to specific criteria of participation and achievement, with the understanding that over time these would become increasingly ambitious—roughly, "the greatest good for the greatest number."

Popper's distinctiveness lay in his emphasis on the fallibility of the central epistemic planner (and hence the need to institutionalize the reversibility of any of its decisions)—which is *not* the same as what I take to be the Kuhnensteinian (not to mention Hayekian) position, namely, the removal of this fallible planner in favor of the de facto infallibility of dispersed agents capable of reaching mutually agreeable epistemic settlements. A bellwether term in marking the difference between the positivist-Popperian and the Kuhnensteinian perspectives is *convention*. Both perspectives hold knowledge to be "conventional" in some sense, but the senses differ sharply. Positivists and Popperians tend toward a "shallow" and the Kuhnensteinians a "deep" view of convention. Whereas the former stress the status of conventions as decisions that one may wish to reverse in light of consequences (because they do not get you what you want), the latter stress their status as working traditions that are not given up lightly (because they have come to constitute who you are) but may be open to alternative paths of development.

Given my own version of social epistemology, a classical philosophical problem like the demarcation of science from

nonscience (or "pseudoscience") looks like a sketch for a theory of epistemic institutions set in the context of a modern liberal democracy. That the logical positivists and the Popperians did not talk as much about sociology as one might have wished merely meant that it was presupposed, not disregarded. This helps to explain why especially the Popperians were never terribly concerned with providing necessary and sufficient conditions for science: a normatively adequate sociology would capture the remaining characteristics that science shares with other institutions in a modern liberal democracy, the philosopher's job simply being to specify the conditions that are *unique* to science in that setting (Jarvie 2001).

As it turns out, the demarcation problem lies behind one of the few sentences of *Kuhn vs Popper* that Read claims to have read before passing judgment over the entire work. It includes the following passage: "It comes as no surprise that philosophers today sooner criticize Creationists for violating evolutionary strictures than evolutionists for violating more general scientific norms" (Fuller 2003, 6). Read is prompted to ask, "Could Fuller really be saying that it is more important to question whether evolution is really science than whether Creationism is?" (Read 2005, 374)—the answer to which, according to Read, is yes. For someone who repeatedly advertises himself as a careful reader of philosophical texts, this gloss is crude, if not obtuse to what should be patently obvious to the alert tracker of words on the page. In philosophical jargon, Read has collapsed the first- and second-order issues raised in my quote. I was referring to the fact that, in these Kuhnified times, philosophers prefer to defend a dominant scientific paradigm like evolutionary biology than to advance, as the logical positivists and the Popperians did, an independent standard, in terms of which even the dominant paradigm may be shown wanting. At this point, perhaps Read needs to be reminded about the point of the demarcation problem.

As in a legal proceeding, the demarcationists would have the relevant epistemic standard specified in terms neutral to the parties contesting the title of "science." Exactly what might constitute such a "neutral language" of evaluation led to endless wrangling amongst the positivists and, eventually, their abandonment of the demarcationist project. However, they managed to agree that the criterion could *not* simply consist of judging a newcomer theory in the terms of the incumbent. In the case at hand, creationists could not be judged simply in terms of evolutionists' standards. This was for

two reasons: either the trial would be clearly biased to the incumbent, thereby inhibiting any truly novel scientific breakthroughs, or the newcomer would be encouraged to recast, perhaps misleadingly, its own knowledge claims in the incumbent's terms. In other words, the philosopher's responsibility is to ensure that neither evolutionists nor creationists are allowed to play to their worst tendencies.

Unfortunately, instead of taking their legislative and adjudicative responsibilities seriously, philosophers nowadays follow the path of ideological least resistance. Read contributes to this baleful trend. He appears to believe that if philosophers do not stand squarely behind established science, U.S. school boards will be allowed to remove Darwin from the curriculum. Without denying this as an outside threat, the opposite is more likely—namely, when school boards are sued for expanding the curriculum to include scientifically updated forms of creationism (aka, intelligent design theory), the law tends to fall back on a conservative—and historically misleading—construal of the demarcation of science from nonscience that effectively reduces "science" to the empirically best supported theory or research program. The high school science textbook thus becomes a site for the promulgation of dogma, not the promotion of criticism: students are not exposed to alternative interpretations and explanations of, in this case, the same biological data that might inspire them to pursue lines of inquiry at university and beyond that challenge dogmatic evolutionism. Here philosophers of science have much to answer for—and recently some have begun to atone (e.g., Ruse 2005). After all, if alternatives to the scientific orthodoxy are not explicitly introduced to students early in their academic careers, it is hard to imagine how the next scientific revolutions are supposed to occur, and the Enlightenment spirit of inquiry promoted more generally, except as palace coups initiated by frustrated elites unable to make as much progress within their inherited paradigm as their forebears. That would effectively turn Kuhn's view of scientific revolutions into a self-fulfilling prophecy.

Here it is worth recalling that the original targets of the positivist-Popperian demarcation criteria were not organized religions but Marxism and Freudianism for attempting to overextend the epistemic authority of their disciplines into the public domain where their claims could not be properly tested. These fields, the general claims of which engaged the sympathies of the positivists (including Popper), stood out in the Weimar culture of the 1920s and 1930s as

they have not since the 1960s: they were ostracized by the academy, which practically meant they were either ignored or dismissed in the grossest terms. At the same time, the Weimar Republic was Germany's first constitutional democracy. It encouraged people to believe that all claims were equally entitled to a hearing. Thus, the lack of serious objection was widely taken to imply tacit acceptance or at least tolerance of the claims. This "spiral of silence" was largely responsible for enabling Nazism's rise to power in the same period, even though most academically trained intellectuals found it beneath contempt, let alone critique (Noelle-Neumann 1982). In a less dramatic but similar fashion, psychoanalysis and Marxism also managed to infiltrate the general culture. The quest for demarcation criteria was designed to address this liability in democratic communication. Were the demarcationists alive today, they would be concerned about the overextension of *evolution*—what Michael Ruse (2005) now rightly calls "evolutionism"—as a threat to the spirit of free inquiry, certainly more than anything emanating from creationist quarters.

2. INTERLUDE: READ AS SIGNIFICANCE TESTER

Read makes much of the relative significance of Kuhn and Popper. I cannot speak for what goes on at the University of East Anglia, where Read claims that Popper is much better known than Kuhn, but I believe most people would find this an anomaly. Certainly, as Read himself is forced to admit, Thomas Nickles, the editor of the volume Read reviewed much more favorably than mine, also seems to think that Kuhn has had a massive influence. Nevertheless, when trying to get some empirical purchase on the issue, I realized that Read may have a point. As of the day I write this (23 June 2005), "Thomas Kuhn" appears on 261,000 Web pages and "Karl Popper" on 284,000 Web pages. Roughly the same ratio is reproduced on the Google Scholar search engine, with Kuhn at 4,550 and Popper at 5,150. Another indicator is the number of readers' comments on Amazon.com. Here Kuhn surges ahead of Popper—101 to 67 comments made about the five main books of each. But whereas 60% of Popper's comments were directed at *The Open Society and Its Enemies*, a full 83% of Kuhn's were directed at *The Structure of Scientific Revolutions*. Indeed, the 84 comments on that book alone exceeded the number of comments on Popper's entire corpus. Looking behind these figures, I conclude that

Popper's impact is much more diffuse than Kuhn's and may even be stronger in discussions concerning social and political matters than those concerning science, whereas Kuhn's impact is quite clearly focused on discussions of science—however amateur or professional.

It seems that the most I should grant Read is that Popper may be slightly more influential across all philosophical topics than Kuhn. In any case, they are in rough parity, with Kuhn enjoying an advantage in strictly scientific matters—which is exactly how he would have wanted it. Of course, this leaves open the question of how Read could have so grossly underestimated Kuhn's influence. There must be more to it than his having simply taken his students too seriously as competent informants about the state of play in contemporary philosophy of science. I have frequently met Read-like responses to my claim that Kuhn's is the taken-for-granted position in philosophy of science today. Moreover, I believe that one of Kuhn's keener insights, the so-called Planck Effect, accounts for it. The basic idea is that paradigm change corresponds to generational change: the old are not persuaded to the views of the young; rather, the young succeed the old without ever having held their views. It was precisely against this brutal coincidence in the longevity of individuals and their ideas that Popper struggled so valiantly.

In short, it all depends where you stand in terms of the generational divide: are you part of the generation who fought for Kuhn's acceptance into the philosophical canon or part of the generation that was born with Kuhn already canonized? If you belong to the former, you may never realize that the war is over. It is striking that all the main authors in the Nickles volume—who go to great lengths to show how Kuhn's intellectual legacy remains radical and underutilized—are all more than fifty years old. Had this volume been published twenty years ago, it would probably have had the same contributors and editor. Back then, they were the "Young Turks," several of whom—not least Nickles himself—were helpful to my younger self, a black sheep of the positivist fold, then completing his Ph.D. at the University of Pittsburgh. But my guess is that a future historian with a cruel streak will say that the contributors to the Nickles volume constituted the branch of science studies that time forgot. They continued to fight battles that either had been already won (e.g., against an obsolete "positivism") or were unlikely to be engaged with the means at their disposal (e.g., for an unconsummated "cognitivism"). Read's concession of the point is evident to the discerning reader who observes the excessively

generous—if not condescending—remarks he makes about the authors in the Nickles volume. Unfortunately, Read seems to think that Wittgenstein, even more than Kuhn, remains a basis for launching the philosophical revolution abortively attempted by the contributors to the Nickles volume. At this point, we enter the lair of the dreaded Kuhnenstein.

3. KUHNENSTEIN AS THE HEROIC AMATEUR AND LANGUAGE THERAPIST

Although he never quite admits it, Read is a fan of philosophical *amateurism*—the idea that any clever and open-minded person can make a significant contribution to philosophy, no matter how much a field of philosophical inquiry has been already encrusted with technicality and scholasticism. This idea provides the inspiration for Kuhnenstein. Following the lead of two great philosophical amateurs of the past century, Ludwig Wittgenstein and Thomas Kuhn, the one a self-loathing engineer and the other a disillusioned physicist, this contribution may consist in a more "back to basics" approach that peels off the technicality and scholasticism, specifically by reinterpreting these nominal badges of professionalism as an obsessive-compulsive neurosis in need of therapeutic treatment. The therapeutic approach is especially arresting in philosophy because of the field's traditional Platonic strictures of "mental preparation" as necessary for becoming a fit receptacle for "the light of reason." Whereas Plato's descendants have attempted to impose discipline on our otherwise unruly passions (aka "method"), the therapeutic approach prescribes that we relieve ourselves of such discipline to return to a more "natural" and "receptive" mode of experience. Thus, Wittgenstein would have us observe "society" as people's contextualized interactions rather than, say, the reproduction of some part of a larger social system. Similarly, Kuhn would have us see "science" not as the instantiation of some positivist methodology but the contextualized practices of certain trained individuals in consort with each other.

Now, I too generally approve of philosophical amateurism, but I object to this particular expression of language therapy. I say "this particular expression" because two strands of language therapy can be found in the history of philosophy, which I call *sophistical* and *skeptical* (Fuller forthcoming). I approve of the former and disapprove

of the latter. Both the sophist and the skeptic are concerned with language's tendency to mystify our grip on reality. Whereas the sophist's language therapy imparts a form of knowledge that confers a previously unrealized sense of power, the skeptic's therapy imparts a knowledge that disabuses us of the false sense of power we thought we had over reality. Thus, the sophist's therapy aims to activate people from the word magic exerted over them by theologians, while the skeptic's aims to alleviate people's anxieties by enabling them to see the self-created nature of their problems.

In the modern period, the sophistical strand became the bourgeois Protestant challenge to aristocratic Catholicism, while the skeptical strand characterized the Catholic response in recognition of its inevitable decline. The contrasting styles of "humanism" promoted by Ludwig Feuerbach and George Santayana subsequently captured this distinction for a more secular age. The former is outward looking, eventuating in politics; the latter inward looking, eventuating in psychiatry. In 20th-century terms, we might say: Popper versus Wittgenstein. But as this opposition suggests, the Vienna Circle, despite its self-promoted reputation as the beacon of clarity in a world of intellectual confusion, failed to keep the sophistical-skeptical distinction straight in its own collective thinking. However, the elaboration of this and the preceding points surrounding the distinction must await another occasion. In what follows, I will focus on the skeptical strand, with which the therapeutic approach to philosophy is normally associated and which, more to the point, leads us back to Kuhnenstein.

A genealogy of the skeptical strand of therapeutic philosophizing would also include the Epicureans and the Stoics, who flourished, along with the Skeptics, in the wake of the fall of Athens. All of these movements appealed to traditional elites desperately trying to reconcile themselves to politically diminished circumstances. Originally it was a matter of Greeks adapting to Alexandrian and then Roman colonization. But as the Romans themselves came to realize that their grasp of the world fell far short of their reach, Stoicism became de rigueur within the Roman imperial bureaucracy—including even the occasional emperor like Marcus Aurelius. We might think of the therapeutic approach as the more affluent and (therefore?) less ambitious cousin of two other general philosophies born of subjugation in ancient times, Judaism and Christianity. Whereas the Jews and Christians appealed across class differences with a promise of revenge over their joint oppressors in the long term,

the philosophical therapists more modestly and selectively appealed to those who could afford to make peace with a world over which they could exert little control except perhaps in matters concerning their own lives. In the modern period, Marxism and Freudianism have offered the respective analogous alternatives.

Fast-forward two millennia, and welcome to the world of Kuhnenstein. Wittgenstein and Kuhn were from socially and economically privileged backgrounds that entitled them to advanced scientific training. But also each lived through the pointless devastation of a "world war" to which their training—and they themselves—contributed. Wittgenstein and Kuhn were groomed for a world that never materialized, and they spent the rest of their lives trying to come to terms with that fact. Of course, their elite connections did not fail them in times of personal crisis: Wittgenstein had his Bertrand Russell and Kuhn his James Bryant Conant. Whatever suffering Wittgenstein and Kuhn underwent is not evident from their curricula vitae. This is not to deny the authenticity of that suffering. Rather, I refer here only to what might be called its *public sublimation*, whereby Wittgenstein's and Kuhn's relatively high visibility in elite circles allowed their suffering to be a source of inspiration to spectators in ways not afforded to the many others who suffered similarly. My point is to draw attention to the *superstitious* character of the continued veneration of Wittgenstein and Kuhn. They are unique *merely* in the post facto sense of having been the ones who made it into the philosophically relevant networks.

4. READ AS SUPERSTITIOUS TEXT FETISHIST

Superstition set in once many well-appointed people—Read is only a latecomer—started to fetishize the texts of Wittgenstein and Kuhn rather than treat them for what they are: symptoms of the more general problem of how scientific knowledge has been implicated in power relations over the past century. To be sure, Wittgenstein and Kuhn developed their own, somewhat similar strategies for dealing with this problem. Nevertheless, devoted Kuhnensteinians like Read need to ask, What were the range of alternatives from which you decided that, say, Wittgenstein provides the best foundation for a social science or social epistemology, or Kuhn provides the best foundation for a history or a philosophy of science? My guess is that the devout are rarely conversant in the full range of available options,

even at the time their chosen authors wrote. Rather, they already shared their chosen authors' sense of alienation, and those authors happened to be the ones who first provided them with articulate expression of that experience. That initial corroboration—"the context of discovery," if you will—was sufficient to shut down the critical faculties evermore. The rest of the devout's career then provides learned witness to this conversion experience.

Kuhn, but not Popper, would be pleased by this trenchant example of what cognitive psychologists call "confirmation bias" (Fuller 1993, 108-10, 176-77). It serves as a vivid reminder of the need to distinguish clearly the context of discovery from the context of justification. In this case, you cannot vouch for the intellectual *uniqueness*—let alone superiority—of Kuhn and/or Wittgenstein unless you have controlled for the fact that you have had much more exposure to these impressive amateurs than to other potential competitors. But ultimately confirmation bias is the scholastic's sin, since it is easy to see how text fetishism might develop in pedagogical contexts. Students are routinely presented, not with an array of problems that may be tackled in different ways, only some of which are suggested by the instructor, but a set of texts whose selection is presumed to be ideal to the lesson. Even if students go on to challenge the arguments in an assigned text, they are compelled to cast their objections in its terms, thereby implicitly reproducing its uncontested claims and assumptions. This results in what economists call "path dependency," whereby the founding text both channels and arrests intellectual mobility (Fuller 2005, 84).

When the scholastics first flourished in the High Middle Ages, Aristotle's newly recovered and translated texts served in this capacity as a secular complement to the Bible. Moreover, scholasticism was not quite as irrational back then as it is today. In particular, text fetishism was a by-product of the dominant practice of knowledge transmission, namely, the reproduction of texts for personal use, either in the solitude of a monastic scriptorium or the company of fellow students transcribing an academic lecture. This very labor-intensive activity literally required the copyist to recapitulate prior reasoning on a topic, typically leaving a strong memory trace that would enable the copyist to quote the copied text at will that would, in turn, provide a collectively recognized pretext for making new points.

However, as Eisenstein (1979) famously showed, the proliferation of the moveable type printing press revolutionized the shape and

tempo of intellectual change, perhaps more deeply than any of the texts published in Gutenberg's wake. As it became possible to reproduce texts both quickly and cheaply, it was no longer necessary to expend time and effort copying them by hand. Would-be innovators could simply presume that their readers would have access to the relevant texts under challenge; hence, an abbreviated form of citation would suffice. Indeed, writing at the end of the 17th century, Isaac Newton had no need to reproduce all the views he was displacing in *Principia Mathematica*, the labor of which would have deprived him of the time needed to set forward his views in a clear and coherent fashion from first principles. But equally, the memory work corresponding to copying no longer occurred. From a cognitive psychology standpoint, it implied, in Donald Norman's sense, a "smartening" of the ambient environment as texts that copyists in earlier generations would have internalized were now "outsourced" to books on the shelves of the personal libraries of discriminating readers. By the 18th century, the academically trained scholastic whose authority derived from his capacity to recall and extend the form of words of the ancients was being supplanted by the more diversely trained bourgeois connoisseur of books whose authority rested on a discerning sense of what was worth and not worth reading.

It is all too easy to underestimate how much the Enlightenment depended on this retooling of the brain. The printing press demystified the texts that the scholastics committed to memory and used as a pretext to exert epistemic authority. The authority of such texts was now revealed to have been a product of scarcity: these were the only texts that happened to be reproduced—and only with great labor. The Enlightenment made great play of the element of choice that what Marshall McLuhan two centuries later called the "Gutenberg Galaxy" injected into the appeal to textual authority. For example, *L'Encyclopédie* radicalized its readers by drawing attention to contradictions between entries that forced them to decide between competing authorities. When the popular success of Jean-Jacques Rousseau's 1761 novel, *La Nouvelle Héloïse*, inadvertently reinvented the idea that a text should reconfigure one's sense of self, the *voluntariness* of the reconfiguration was stressed, which in turn inspired the Romantic movement's call to have life imitate art. The basic idea—one that never seems to have crossed Read's mind—is that the increased range and availability of books places a greater onus on the reader to decide what is to be ignored, sampled, and

mastered. One can no longer responsibly rely on received authorities, yet regrettably that continues to be the source of Kuhnenstein's appeal.

Much of the modern history of science and technology has generalized the struggle highlighted here: on one hand, a tendency to outsource, or alienate, previously distinctive human capacities to artifacts that are then regarded as mere inanimate means to the achievement of the remaining human ends that escape the capacities of those artifacts; on the other hand, a tendency to reappropriate, or reenchant, those very artifacts as models for human conduct and loci of human value that might otherwise be lost. On this matter, "the sciences" and "the arts" parted ways as distinct bodies of knowledge as they never had before. The dynamo and the computer were pivotal in, respectively, the 19th- and 20th-century versions of the struggle, whereby some saw these inventions as removing drudgery and others as ennobling humanity. This crossroads could even be encountered in the disposal of waste products: are they targets of public hygiene and sanitation systems or are they necessary to psychosexual development (a la Freud's anal phase) and aesthetic transfiguration (a la Duchamp's urinal)?

Unfortunately, all of this history seems to have eluded Read, who believes that by reiterating and expanding on the words of the Kuhnensteinian corpus—and ignoring the words of other worthy parties—we are somehow getting back to basics, as opposed to making a decision that then needs to be publicly defended in terms of its consequences. One obvious antidote to the confirmation bias and its sociological correlate, the path-dependent scholasticism, that has ensconced the Kuhnenstein cult is to rediscover the relevant competitors to Kuhn and Wittgenstein at the time they happened to catch the collective attention space and consider what the competitors said and why it failed to have impact—or at least to receive credit—of Kuhnensteinian proportions (Fuller 2003, 189-91).

5. POPPER AS AN ALTERNATIVE MODEL OF PHILOSOPHICAL AMATEURISM

Another strategy, one I associate with that other great 20th-century amateur—the educational psychologist Karl Popper—and his followers, is to attempt to demonstrate your superiority over more professionally qualified philosophers by explicitly arguing with

them. This may give your text a noisy, scrappy quality that appears more concerned with distancing yourself from bad positions than developing good ones. There is also none of the "back to basics" rhetoric—what Adorno, with Heidegger in mind, called "the jargon of authenticity." Instead, you recognize your initial liability as an amateur and start punching your way out of the scholastic straitjacket that constrains the field prior to your arrival. Of course, as an amateur, you can take advantage of the directness afforded to plain-speaking rhetoric because the reader does not expect you to have complete mastery over the relevant technical niceties. But in the end, your modus operandi must be dialectical not dogmatic: you must acknowledge the presence of those you oppose rather than ignore them in favor of trying to embody in your person the verities of which you speak.

Here I note that Read chastises me for not seeing "the subtle and coruscating critique of Popper implicitly present already in Kuhn's *Structure of Scientific Revolutions*" (Read 2005, 376). "Implicitly present" is of course a euphemism for "not explicitly present." Indeed, the accompanying note to Read's remark cites several pages of Sharrock and Read (2002), which may suggest to the uncharitable reader—and I admit to that status—that this "critique" is a figment of Read's fertile philosophical imagination. If nothing else, *Structure* is written as a seamless and authoritative account of the nature of scientific change, as befits the encyclopedia entry as which it originally existed. It is neither polemical nor even especially argumentative. To be sure, this has made the piece inviting to readers unfamiliar or uncomfortable with the interminable name-checking and point-scoring that normally marks academic writing.

Moreover, *Structure*'s style is more than a clever rhetorical strategy. It also characterized Kuhn's personal social epistemology. Kuhn did not see himself as engaged in a collective enterprise focused on big questions about the nature of knowledge. Rather, he saw himself pursuing a personal project about such questions, which every now and then happened to coincide with related projects pursued by other inquirers. Put bluntly, Kuhn did not see himself as having to "defeat" anyone's arguments in order to succeed, whereas Popper clearly did. Of course, Imre Lakatos, Dudley Shapere, and now Rupert Read have tried to make some philosophically interesting and career-enhancing moves by *inventing* arguments in which to involve Kuhn. While often ingenious, such arguments should not obscure the general point that Kuhn, again like Wittgenstein, exuded an elitist

indifference toward other people's specific judgments of his project. This allows me to cast a generous light on the despair Read expresses at his own (not to mention my own and Popper's!) ultimate philosophical insignificance: he might not have such a gloomy view if he took more credit for his own arguments instead of performing feats of ventriloquism for Kuhnenstein.

The general tenor of Read's reading of my work is one of outrage at my having betrayed a common philosophical legacy he and I share. Yet his response is not dissimilar to that of many specialist philosophical readers upon the original publication of Popper's *The Open Society and Its Enemies* and *The Poverty of Historicism*, works in terms of which I would not mind having my *Thomas Kuhn* and *Kuhn vs Popper* be judged. I offer this frame of reference not out of an arrogance of which Read all too easily accuses me. Rather, I mean to draw attention to the fact that now, as then, we are always discriminating the wheat from the chaff in our collective past to provide guidance for an improved future. Indeed, the clarity of our intellectual identity depends on the making of such judgments, which I (and I think Read would agree) are exercises in applied social epistemology. Popper made some very clear decisions about what should be retained and rejected of the Western philosophical heritage. Read and I have done so for, say, the past century of that heritage. And now it is the reader's turn.

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Steve Fuller is a professor of sociology at the University of Warwick, United Kingdom. He is most closely associated with the research program of social epistemology. His most recent books are The Intellectual *(Icon, 2005),* The Philosophy of Science and Technology Studies *(Routledge, forthcoming in 2006) and* The New Sociological Imagination *(Sage, forthcoming in 2006). E-mail: s.w.fuller@warwick.ac.uk.*