How and How Not to Write on a "Legendary" Philosopher

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Steve Fuller, *Kuhn vs. Popper: The struggle for the soul of science*. Cambridge: Icon, 2003. Pp. iv + 227. $24.50 (hardcover).

Thomas Nickles, ed., *Thomas Kuhn*. Cambridge: Cambridge University Press, 2003. Pp. xiv + 298. $27.99 (paper).

The author argues that Fuller's book, with the single exception of its correct reinterpretation of Kuhn as no apostle of postmodernism—such that his "fans" and "foes" alike are boxing with (or cheering on) only a shadow Kuhn—is worse than worthless. For, in a disreputable and outright propagandistic fashion, it consists in a series of serious distortions of and outright falsehoods about Kuhn and recent philosophy of science, distortions and falsehoods which may well mislead the unwary reader. Nickles's collection by contrast is a competent, useful, and workmanlike performance, although the author argues that the editor's focus on cognitive science uses of Kuhn (and of Wittgenstein) is unhelpful, in that these uses again distort the philosophy of Kuhn (as of Wittgenstein), who was on balance no apostle of cognitive science either.

Keywords: *Kuhn; Fuller; Nickles; scientific revolution*

There has been a gradually growing—and very welcome—resurgence of interest in Kuhn's philosophy of the sciences since his death in 1996. The two books under review are (though ultimately in very different ways) symptoms of this resurgence. One is in a major Cambridge series on contemporary philosophy; the other is in a large print run with a "popular" press. I shall discuss Fuller's "popular" book first, and then discuss the contrasting more "academic" approach taken by Nickles et al.

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FULLER'S "POPPER" VERSUS FULLER'S "KUHN"

Let me start by accentuating the positive. Following Hollinger, Fuller emphasizes, quite correctly, that the widespread images of Kuhn and Popper beyond and even within their professions are fundamentally misleading. Kuhn is widely taken to advocate methodological and conceptual relativism, Popper to be a narrow-minded ideologue of scientific method. In reality, Kuhn had no truck with methodological relativism, and his "conceptual relativism," if such it be, is extremely circumscribed and circumspect; while Popper was not an enemy of scientific revolution but an advocate of *continuous* scientific revolution. As Fuller puts it, seemingly borrowing (without attribution) from my own book, *Kuhn* (Polity, 2002), Popper in effect tries to outflank Kuhn on the Left, not on the Right. If the middle ground were the Russian Revolution, then Popper would be closer to Trotsky than to the White counterrevolutionaries . . . while Kuhn, far from being an advocate of introducing "new paradigms" two-a-penny, in fact *defines* science by its *refusal* to do so.

This point is of fundamental importance, for it follows directly that Kuhn and (to only a slightly lesser extent) Popper have been *fundamentally* misunderstood. Kuhn is no apostle of postmodernism, and his "fans" and "foes" alike are boxing with (or cheering on) a shadow Kuhn, a legendary and not a real figure from the history of ideas, when they write or speak otherwise.

Unfortunately, the point that I have brought out above is the only thing of any worth in Fuller's book. As I shall explain, the book as a whole fails to follow through on this—genuine—insight. It mainly delivers extremely loosely connected insinuations, "spins," and (sometimes) plain falsehoods. It offers in most respects a deeply misleading picture of 20th-century philosophy of science.

I did not have to read far into this book in order to conclude that it is worthless. I will focus primarily on Fuller's short introduction. (There remains a problem: this being an Icon book, intended for popular consumption, some potential readers will be not even *vaguely* informed about its subject. Fuller perhaps relies on this—if so, his work here is even more reprehensible.)[1]

Let us start, then, at the start: "The easiest way to start a discussion about science with people from varied backgrounds is by mentioning Kuhn. Usually the response is positive, even enthusiastic, except from those who still want to uphold 'falsifiability' as science's gold standard" (p. 1). This is simply false. If you want to start a discussion

about (philosophy of) science with people truly from varied backgrounds, you will find that most of them have never heard Kuhn's name, so "mentioning" it will do you little good. Most people who *have* heard of Kuhn have also heard of other philosophies of science, such as Lakatos's, or at least of Positivism, or perhaps of some recent Realist philosophy of science; or alternatively of "Continental" figures in the field such as perhaps Husserl, Canguilhem, Foucault—or Duhem. And many more people have heard of Planck, Heisenberg, or Einstein as philosophers of science than have heard of Kuhn. Or, if we are talking about more or less contemporary figures, Gould, Dawkins, or Chomsky are better known as "philosophers of science" than Kuhn is. And *certainly* Popper is better known than Kuhn. When graduate students come to start work in philosophy or the social sciences at my own university, University of East Anglia, there are some who have not even heard of Kuhn, but they have all heard of Popper.

But there is a more significant distortion lurking here. By giving the impression that there is a generally favorable view of Kuhn out there, except among noble but beleaguered Popperians, Fuller hides the undeniable fact that not only do most philosophers and theorists of science (regrettably, in my view) think that the debate has moved on from the (allegedly) stale confrontation of Kuhn with Popper, but also, more crucially, *many still have a fairly unremitting hostility to Kuhn*. To read the opening—and the whole—of Fuller's book, one would have the impression that Kuhn is in the ascendancy everywhere. In fact, in the places that Kuhn cared about—philosophy and history of science circles, and in science-apologetics—his reputation is mostly poor.

It would be convenient for Fuller if the story—and that is all that it is—that he tells about Kuhn's ascendancy were true. For it would pit him as a righteous underdog: it would cast Fuller as, in the peculiar phrase of his chapter 9, a Tory valiantly battling the ascendant Whigs of Kuhnianism (let the point pass for now that Fuller's attempt to paint Kuhn as a kind of Whig historian is perhaps one of the most misleading suggestions of his [misleading] book).

1. Steve Fuller, "The Case of Fuller against Kuhn," *Social Epistemology* 18, no. 1 (2004): 3-49, is more professional/academic in style than the book under review. But in my opinion, it too is little more than a piece of propaganda, in which Fuller goes as far as he can within the constraints of the libel laws to denigrate his various opponents (including, of course, Kuhn and also myself) without in some key cases providing even the slightest scintilla of evidence for these denigrations.

Of course, a key reason for all this is again the one good point Fuller makes in the whole book. The fact is that most of Kuhn's "friends," "followers," or "supporters" are worthless to him. They turn him into a postmodern relativist—exactly the figure who his "enemies" in much of the intellectual world, especially the philosophers of and apologists for science, take him to be, too. Most famous scientists who write about science for non-scientists—leading examples include Steven Weinberg and Alan Sokal—are very critical of Kuhn. Fuller *knows* this; for, as I say, it structures his work. He rightly exposes the mere legendariness of the image of Kuhn that grips most people who have heard of Kuhn. This begs the following important question: why does Fuller not let his audience know that in philosophy and science there is frequently still intense hostility to or sheer lack of interest in Kuhn?[2]

To sum up, so far: it is false to claim that the response to a mention of Kuhn is usually positive or enthusiastic. It is, rather, usually either (close to) zero, or hostile. The main exception is in fields such as literary studies, fields distant from those which Kuhn wrote in and on, but fields which are, perhaps not coincidentally, closer to the fields that Fuller has made his own. (Fuller teaches in a sociology department. Even in some such "soft" fields, such as politics, however, Popper *is still far more widely taught* than Kuhn. Fuller dangerously overplays the extent to which Kuhn is actually intellectually present in any meaningful way in the training of most graduate students in most academic fields.) When the response to Kuhn is hostile, this is (by the way) more likely to be due to (what I would call) "Scientism," à la Weinberg for instance, than it is specifically due to an advocacy of Popperianism.

When the response is positive/enthusiastic, it is usually founded on ignorance. Enthusiasm for what Kuhn actually thought is, regrettably in my view, a rare phenomenon.[3]

Fuller needs to present Kuhn as a genuinely and massively influential philosopher of science whose views are dogmatically adhered to, for the point of his book to be as he says it is and for the book to be of the moment that he claims it is. But Fuller, unfortunately, knows that Kuhn is misunderstood more often than not, *and* knows that Kuhn is very, very far from universally admired, even where he is known. Fuller is familiar enough with the philosophical literature to know all

2. I shall try in effect to answer this question, below.

3. In fairness, one should again be clear here that this is, sadly, almost equally true of Popper.

about Scheffler, Haack, Shapere, and so on, who took themselves to oppose Kuhn and helped to solidify the image of him as a relativist and an irrationalist. The excuse is not available to Fuller that might be available to some writers in (e.g.) literary studies or feminist theory talking about Kuhn: lack of awareness of the Analytic philosophical literature, and of the *many* contexts in which Popper is far better known than Kuhn. Fuller has, in fact, *no* excuse for his highly misleading presentation of Kuhn's historical placing and reception.

Moving onto page 2 (!): here it is worth remarking that, in the course of beginning to inform his readers, usefully, about the misleading popular images of Kuhn and Popper, Fuller manages rapidly to insert his own misleading impressions. For instance, he writes that Popper is remembered as a "grumpy autocrat," so far as his philosophy of science is concerned. It is true that that version of Popper is misleading; but virtually nowhere in this book does Fuller come close to acknowledging the reasonable *reason* for this misleading image (principally, that Popper *was* something of a would-be autocrat so far as his policing of the boundaries of science was concerned).[4] Popper's important remarks about the "social and psychological sciences," which in large part he condemned as unscientific,[5] are just about entirely ignored by Fuller. Now, my own view, rather like Kuhn's (and, still more so, Winch's), is that there are indeed some good reasons for withholding the term "science" from the so-called human sciences, including the "social sciences." Indeed, as even Fuller occasionally allows himself to note, far from being a relativistic philosophy, Kuhn's philosophy of science is *founded* on noting the massive difference between subjects such as sociology, where every-

4. Fuller loves to regale his reader with stories from Kuhn's life that (allegedly) show Kuhn in a sad or bad or politically problematic light. In passing, then: why does he not see fit to mention Popper's real-life autocratic leanings, such as his cutting off of all his leading graduate students? Take for instance Joseph Agassi: arguably, Popper cut him off, not because Agassi criticized him but because he felt that Agassi had not been vigorous enough in his defence of him in a review! (For more details of a story which is of course far more subtle than I can do justice to in this footnote, see Joseph Agassi, *A Philosopher's Apprentice: In Karl Popper's Workshop* [Amsterdam and Atlanta: Workshop Rodopi, 1993].)

5. To be precise: Popper attacked some social science as metaphysics pretending not to be metaphysics, for example he attacked Marxists and Freudians because they made propositions untestable. He commended Marx for being scientific for advancing a refutable hypothesis about the occurrence of a revolution. He also made some interesting and in many ways reasonable/apposite remarks on the proper methodology of the "social sciences" in terms of rational reconstruction of courses of action.

thing is constantly being torn up to start all over again, and subjects such as chemistry, which exhibit progress.[6] But I for one have significant reservations about the manner and nature of Popper's criticisms of Marx, Freud, and so on (and about his seeming lack of understanding of the "hermeneutic" aspect of "social science"). And, more significantly still perhaps for this review essay, should not Fuller have some such reservations? And will not many of his readers? Indeed, perhaps they would—if Fuller had risked mentioning this stance of Popper's to them.

Obviously there is insufficient room in this review essay for an evaluation of Popper's own philosophy of science. Suffice to say that Fuller's "understanding" of Popper is no less shallow than his "reading" of Kuhn. A careful examination of Popper's philosophy—and in particular of his policing the borders of science, his criticisms of psychoanalysis, and so on—would, I believe, provide at least some good grounds for a judgment of "autocracy." Even if Fuller is right in saying that this judgment could not finally survive scrutiny, he certainly does not undertake the analysis which would support this view. His manner of presenting the claim that Popper was not autocratic—as an obvious, glaring fact that only a malevolent fool would miss—ironically does Popper a disservice.

A somewhat similar moment of misinformation occurs on pages 5-6, where we encounter the following remarkable sentence:

> For those who have inherited Kuhn's Cold War belief that normal science is a bulwark in a volatile world, it comes as no surprise that philosophers today sooner criticise Creationists for violating evolutionary strictures than evolutionists for violating more general scientific norms—an activity for which Popper had been notorious.

I did a double-take when I first read this remark. Could Fuller really be saying that it is more important to question whether evolution is really science than it is to question whether Creationism is? Astonishingly, it appears that the answer to this question is "Yes." At a time when evolutionary theory is the only game in town, and yet is disbelieved by more than 100 million Americans, who in states like Kansas are again (as I write, in early 2005) on the march to remove Darwin from the school curriculum, and whose scientific ignorance is now known even to be a drag on their economy, this is a highly bizarre

6. See Thomas Kuhn, "Preface," in *The Structure of Scientific Revolutions* (Chicago: University of Chicago Press, 1962, 1970); henceforth *SSR*.

stance for a public intellectual to take. (One does not have to be a Dawkins-esque polemicist about evolution to make any of these points against Fuller; one has only to be someone with some basic understanding of and respect for the discipline of biology.) We are responsible for the consequences of what we write—I am drawing attention to an implication and thus a possible consequence of Fuller's remark here that is in my view genuinely shocking.

Furthermore, Fuller fails to mention that Popper in later years thankfully *gave up* his challenge to evolutionary theory as (allegedly) metaphysical and therefore unscientific.[7] Again, in a book intended for a lay public, Fuller's failure to report this is reprehensible. Either he is deliberately misleading his audience, or else he is simply too ill-informed/incompetent to be writing a book on the topic of *Popper* and Kuhn.[8]

Lower on the same page, as the introduction approaches its end, Fuller returns to his opening theme, of Kuhn's alleged ascendancy:

> [T]he outcome of [Popper's] debate with Kuhn really mattered. With the defeat of Popper (and his followers), the normative structure of science drastically changed. Whereas actual scientific communities existed for Popper only as more or less corrupt versions of the scientific ideal, for Kuhn the scientific ideal is whatever has historically emerged as the dominant scientific communities. *In the wake of* Kuhn's victory,

7. To be precise, it is important to be clear that using a term such as "unscientific" (according to the/Popper's criterion of demarcation) did not in any case necessarily involve an underestimation of Darwin's theory. According to Popper, it was to be viewed as a metaphysical theory—a theory that, along with other unfalsifiable hypotheses (such as purely existential ones, or indeed fundamental scientific principles), may add to the empirical overall content of our views, providing scientists with new ways of conceiving the world and new methods to explore it with. "Metaphysical" or "metaphysics," according to Popper and his followers, do not necessarily bear any negative value. Indeed, in his later years, Popper developed his views of the progress of knowledge within an evolutionary framework ("evolutionary epistemology")—as, intriguingly, did Kuhn. (Has the difference between Kuhn and Popper been exaggerated? While accentuating the differences between them [clearly!] is important, it is also important to understand the often underestimated ways in which they were not in reality versus each other at all. On this point, Fuller's book is of zero [or, rather, negative] utility; see Wes Sharrock and Rupert Read, "Kuhn and the Methodologists of Science," in Wes Sharrock and Rupert Read, *Kuhn* [Oxford: Polity, 2002], ch. 4, for an account which is I think of use.)

8. If Fuller wanted an illustrious ally for such a stance vis-à-vis evolution and creationism, he should of course have called upon (one strand in) Feyerabend, not Popper.

> science *has come to be* justified more by its paradigmatic pedigree than by its progressive aspirations. (Italics mine)

It is hard to understand what Fuller is actually saying here, because of his employment of the extremely vague (and undefined in his text) term "the normative structure of science." But, as my italics bring out, Fuller here seems to be trying to imply (without quite stating it, for then its falsity would I think be quite plain) that there is a causal relation between Kuhn's "victory" [*sic*] in his struggle against Popper on one hand and recent public understanding of science's legitimacy on the other. But, as Fuller admits elsewhere (e.g., p. 21), this is surely nonsense: Kuhn's influence on how science is actually done is virtually nil, and his influence even on science-apologetics has been extremely limited.[9]

Fuller *claims* to be righting the historical record. Chapter 1 of his book is ostensibly an attempt to tell the truth about the debate between Kuhn and Popper, which Fuller claims started in 1965 on the occasion of their famous public encounter in London.

But Fuller shows no sign of understanding the subtle and coruscating critique of Popper implicitly present already *in* Kuhn's *Structure of Scientific Revolutions* (hereafter, *SSR*).[10] Indeed, he does not even register its presence. Again, this is evidence of bad scholarship. It is a terrible—fatal—failing/absence in a book supposedly about the Kuhn versus Popper debate. That debate was opened *in* the pages of *SSR*.

One of the points made by Kuhn against Popper there is that it is not clear that Popper's vision of science actually succeeds in being a vision of *science* at all. Of science, that is, as opposed to philosophy.[11] To the kind of activity engaged in by the pre-Socratics, for instance, who conjured bold hypotheses which they then subjected to criticism. The pre-Socratics are closer to Popper's image of science than, say, Galtonian chemists are. I guess Popperians like to make bold hypotheses; it is certainly "bold" to count as good scientists a group of thinkers who were not evidently doing science—systematic study of how

9. The seemingly deliberately deceitful rhetoric here in Fuller is in my view shameful. Likewise, in the especially risible claims that Fuller makes for Kuhn's influence on page 86, where we find for instance that Kuhn is to blame for the (alleged) fact that scientists and philosophers accept "the counter-intuitive implication that reality consists of many distinct worlds, each roughly corresponding to a scientific discipline"!

10. For justification of these claims, see Sharrock and Read, *Kuhn*, 42, 43, 101-16, 121-26, 156, 219n22.

11. Are Popperians subject to a deformation professionelle, here? It is so tempting, as a philosopher, to make science look rather like . . . philosophy.

the world actually works, as opposed to open-ended unevidenced speculation—at all, and to count as less good scientists a group of thinkers/practitioners who are actually among scientists' usual paradigms for what science is!

Of this important point, as with all the other points Kuhn makes in *SSR* against Popper, there is in the book of Fuller's under review no trace.[12]

All this is of course a great shame, for Fuller is a clever man, full of bibliographies and erudition and ideas. Why has he come to this? Why has this wit and this wide knowledge produced a veritable mine of misinformation for the lay reader?

What may be quite a large part of the answer is (involuntarily?) confessed, again, in his book's introduction, and is worth quoting at length:

> Epistemology . . . is now more than ever preoccupied with face-saving exercises to shore up expertise, the elusive quest for what philosophers call 'credible testimony' and sociologists call, more brutally, 'boundary maintenance'. This is a project that Kuhn could understand. In contrast, when founding a field called 'social epistemology' fifteen years ago, I defined the social character of knowledge in terms of the need to bring order to an inherently divisive situation consisting of many self-interested and fallible agents. This is a project Popper could understand. However, most of those who nowadays call themselves social epistemologists are concerned with determining the spontaneous patterns of deference in a socially distributed knowledge system: Who should I believe? This pressing question is more likely to be answered by delegating than assuming responsibility for whatever informs one's actions. As students of political thought will appreciate, it is as if Kuhn's triumph over Popper has enabled social epistemologists to take the great leap backward. (pp. 4-5)

Poor Fuller. How green was his valley. What bliss it was, to be alive in those days of the founding of a discipline, his very own discipline; and how cruelly it has been taken from him. By those dreadful "Kuhnians"!

What the unfolding of this paragraph of Fuller's (presumably involuntarily) discloses is that the clause early on, "In contrast," is in

12. Just recall the famous opening sentence of *SSR*, going at the heart of the methodology used by previous philosophers of science, most obviously Popper and Carnap. However, it is true that Kuhn's book is not primarily a polemic against Popper, or any other philosopher. This is perhaps its greatest merit: instead of nitpicking within the established framework, it proposes a different "framework" altogether.

fact no contrast. Fuller, although his own dicta speak against it, wants badly to maintain the boundary of "his" discipline. *He is terribly annoyed that an image of Kuhn* (and, as I made clear above, this image is very largely false; that is the one decent point that Fuller makes!) *has "messed up" his "own" field*, that is, has taken "his" field in a direction quite different to that in which he, Fuller, hoped it would go. "Backwards," allegedly.

Fuller should have considered alternatives. For starters, that he had, by his own strictures, no right to police his "own" field. The idea of a field's founder having parental rights over it is deeply naive.[13]

And indeed, yet more challenging alternatives. For example, that "his" field might not have been well defined or well founded in the first place. And/or that it may be of very little use, or even meaning, in any case. And/or that it is a flawed attempt to substitute what has been done already, with deep care and subtlety, by others: I would argue that Wittgenstein, Winch, Louch, Harold Garfinkel, and some of their followers *have already undertaken* the main tasks of what could actually deserve to be called "social epistemology," the understanding of how knowledge is socially produced. (A drop of Wittgensteinian ethnomethodology, in this reviewer's opinion, is worth more than a cloud of Fullerian philosophy.)

What Fuller is claiming, in the paragraph I have quoted above, is that he has been misunderstood and traduced, that his "followers" are not his followers, and so on. He has suffered the same fate as Kuhn. But because he blames "Kuhnianism" for this, he decides to get his revenge—but not by patiently explaining how Kuhn's "followers" have got Kuhn wrong. Rather by interleaving tiny fragments of such an explanation with a series of attempts quite literally to tar Kuhn by association with the brushes of McCarthyism, Nazism, Sta-

13. It is important to note that the very idea of social epistemology—like many ideas in late 20th-century philosophy—developed in a favorable intellectual climate in which the focus of attention was shifting from an individual knower to a community of knowers, from a theoretical structure of well-ordered truths to the rather messier practice of discovering, debating, changing, updating, and applying knowledge. The influence of Wittgenstein, probably the most important philosopher of the 20th century—and who in passing Fuller also tries to savage in the title under review—is quite visible in this matter. Of course, Wittgenstein was not alone in forcing the reconceptualization of epistemology which Fuller now claims for his own: Kuhn, more than any other philosopher of science, made the point explicit. (Of course, there are views and ideas that are genuinely Fuller's, but their existence is inconceivable without Wittgenstein and Kuhn in the background—even if he largely [and perhaps willfully] misunderstood them.)

linism, and so on. He hopes that his assault on Kuhn's reputation may enable him to regain possession of social epistemology, so that when (and surely it will be soon!!) our universities are clamoring to endow chairs in social epistemology, it will be Fuller and not Kuhn who will be revered as the (grand-)father of the discipline, have chairs named after him, "and so on." Fuller does not see the obvious: that the reputation that will more probably be tarred, irreparably, in this process, is the (humbler) reputation of the assaulter. Of Steve Fuller.

Perhaps Fuller might have done better, then, to have taken a slightly humbler and less comfortable route. Perhaps he should have considered the possibility that the biggest single reason for Kuhn's high reputation (in *some* quarters) is that he was a fairly brave, innovative, and deep thinker, something of a Wittgenstein of the sciences, one might even say. Someone likely to be read with passionate interest and learnt from, long after Fuller's works and mine are dead and gone, and, dare I say it, perhaps Popper's too.

But whether Kuhn and/or Popper will survive for some time into the canon of philosophy, it is in the end just ridiculous for Fuller to cite Popper as an intellectual ally. Popper would, I suspect, be utterly appalled by the "methodology" of this book of Fuller's. He would consider it an unscientific rant, and a polemical piece of writing that does not satisfy the basic criteria Popper believed should characterize philosophical prose: namely, proceeding by careful statement, analysis, and criticism of arguments; anticipating objections and answering them honestly; and so on. *He would see right through Fuller's pseudo-historicist desire to be founding a field of (supposedly!) immense political significance*. My own attitude is not dissimilar to that that I have imputed to Popper here. I care about the things that Fuller is talking about; and I even have a similar attitude to his to these things, at a number of points. Like him, for instance, I am a passionate advocate of democratic politics, and I try to be a radical questioner of the direction of some contemporary science. Like him, I am critical of much "big science" and of the corporatization of science and of the academy. (Then again, Kuhn too was critical of "big science"; Fuller has to pirouette more madly than usual, to avoid admitting this transparently!) In the end, I think that Fuller's book will be positively harmful because it is liable to tarnish causes such as those that I share with him with a kind of . . . guilt by association (and guilt by association is something Fuller understands well: it is, sadly, close to being his standard trope). If Green or Left criticisms of the contemporary science establishment are brought into disrepute by Fuller, that will be bad news, a stupid

"own goal." The moral is: we must not let voices like Fuller's be left to sound like they *actually* have anything genuinely Left or radical about them. In terms of intellectual substance, they do not.

Even so—even in light of how this issue may then be of some real importance beyond the ivory tower, beyond the question of rescuing the reputation of a major philosopher (Kuhn)—I nevertheless apologize if any readers have been offended by the intemperate tone of parts of this review essay thus far. In mitigation, it may be worth mentioning that another cause of my frustration is this: a number of us have for several years now been engaged, against perhaps our better judgment, in an attempt rationally and sensibly to debate these matters with Steve Fuller, in person in various public and private fora, and in print.[14] We hoped that he might come to understand rather better the nature of Kuhn's project, or at least to stop publishing dressed-up gossipy speculations, innuendos, and plain falsehoods about it. The publication of *Kuhn vs. Popper* shows that our hopes were quite in vain.

In sum: this book offers only a cartoon opposition of a fake "Popper" to a fake "Kuhn." It fails to take the opportunity of rewriting for a wider public the legend of Thomas Kuhn such that more people will be able to understand who Kuhn really was and what he really said. Instead, it offers simply a fictive figure produced by an (inaccurate) recasting of and (propagandistic) rewriting of the history of the history and philosophy of science.

NICKLES ET AL. IN FOCUS

Putting down Fuller's book and picking up Nickles's is like moving from a squalid rubbish dump to a green meadow. One's spirit is almost overwhelmed, temporarily, by the sudden difference.

Nickles's book, published simultaneously in paperback and hardback in the new Contemporary Philosophy in Focus series from Cambridge, is a useful collection on Kuhn. Almost every essay is of fairly high quality. Together, they add up to a diverse set of influential perspectives on Kuhn's philosophy of science. There is no one focus,

14. A particularly fine example is Thomas Uebel, "The Poverty of 'Constructivist' History (and Policy Advice)," in *History of Philosophy and Science*, edited by M. Heidelberger and F. Stadler (Dordrecht, the Netherlands: Kluwer, 2002), 379-89.

though there is an orientation, deriving from Nickles's own interests, towards relating Kuhn's ideas to recent cognitive science.

The introduction begins, slightly unfortunately, by echoing Fuller's implication that Kuhn has been immensely influential. Nickles cites (p. 1) the way in which terms that Kuhn coined have become "household phrases," but omits to make clear that this does not in itself imply that Kuhn's thought (as opposed to a jargon very loosely derived from that thought) succeeded in having any influence at all, for these phrases mostly mean just what Kuhn did not want them to. Nickles's main contribution to the book is however his own essay, which demands to be read alongside the other essays in the collection that are focused on cognitive science.

Nickles's "Normal Science: From Logic to Case-Based and Model-Based Reasoning" is an interesting effort to try to cognitive-scientize Kuhn. Nickles argues (more on this in the discussion of Barnes's essay, below) that normal science is Kuhn's real topic. He goes so far as to suggest that Kuhn's "own epistemology of science failed" to handle adequately "revolutionary breakouts from the old framework" (p. 155). (This cannot be quite right: Kuhn, for the first time, gives an account of *all* of science, "normal" and "revolutionary" alike. Kuhn's real topic was *conceptual change* [and the crucialness of the normal *lack* of conceptual change] in science.)[15] Nickles hopes to understand normal science as routinized activity that can be understood as a complexification of how artificial intelligence engineers/scientists understand such activity (p. 143). But he precisely follows Kuhn in challenging

> the traditional Leibnizean-Enlightenment conception of perfect fully justified knowledge as verbally explicit, including the reduction of procedures and skills to rules. Rule-based performance is often methodical in the pejorative sense—mechanical, plodding, inefficient, and nonintuitive, by contrast with the fluidity of expert performance. (p. 151)

15. Nickles closes his essay by claiming that Kuhn "largely missed the opportunity to extend his evolutionary account [of scientific development] to normal science" (p. 169). Maybe Kuhn should have spoken more about the presence of conceptual change in normal science. But the point—contra Nickles (and Fuller)—is that Kuhn is not best read as having offered us a model of science at all. He was, as he increasingly insisted, at heart (even if not by training) a philosopher. "Normal science" and "revolutionary science" are not models but pictures. Their therapeutic point is to return us to science—to an understanding of science, as for the first time, which understands the whole phenomenon of science and not (as with the Positivists and Falsificationists) mere fragments of it.

The difficulty, once one has correctly understood Kuhn thus as breaking through the rule fetish that has fettered many philosophers (including many "Wittgensteinians") and social and cognitive scientists alike, is in seeing what cognitive science can actually add to Kuhn's own words. Understanding verbally inexplicit knowledge in such a way that you do not smuggle back in a logico-linguistic character to it (as the concept of "tacit knowledge" nearly always does, for instance) leaves very little room for cognitive science to work in. Fully scientizable knowledge—fully routinized "verbally inexplicit" knowledge—*would not* actually be *knowledge* at all, no more than a chess-playing machine or a computer (or a book!) *knows* or *understands* anything. This is the lesson Kuhn taught, following Wittgenstein: any interesting sense in which an activity can be algorithmically understood *or* otherwise rendered in some way such that *scientists* can tell something useful about its nature—for example, such that (cognitive) *scientists* can tell us (and scientifically!) about how exemplars can be systematically/repeatedly followed—is not a sense compatible with the activity being a human activity: flexible, accountable, reflexive.[16] As the ethnomethodologists might put it: a "cultural dope" who can be rendered by a scientific account cannot even demonstrate the level of "expert performance" necessary to understand the most basic novel conversational implicature, let alone to be even the most basic of normal scientists.

Nancy Nersessian, in "Kuhn, Conceptual Change, and Cognitive Science," presents an explicitly "Wittgensteinian" "theory" of concepts, which she wants to undergird, understand, and extend Kuhn:

> What one acquires in learning a conceptual structure are not sets of defining characteristics and specifiable rules for the concepts that participate in the problem exemplars comprised by the paradigm. Rather, one acquires sets of "family resemblances" that include both similarities and differences among instances. In presenting this view, Kuhn explicitly drew from the philosopher Wittgenstein, who in his *Philosophical Investigations* had argued against the "classical" view of concepts, originating with Plato and Aristotle and carried into twentieth-century philosophical analysis by Frege and Russell. (p. 180)

The problem with this is that it has Wittgenstein offering us a rival—nominalist, or Anti-Realist—theory of concepts. This was just what

16. See John Preston and Mark Bishop, eds., *Views into the Chinese Room: New Essays on Searle and Artificial Intelligence* (Oxford: Oxford University Press, 2002), for discussion.

Wittgenstein did not want to do. See section 65 of *Philosophical Investigations*:

> Instead of producing something common to all that we call language, I am saying that these phenomena have no one thing in common which makes us use the same word for all, —but that they are *related* to one another in many different ways. And *it is because of this relationship, or these relationships, that we call them all "language."* (Italics mine)

As is made even clearer in Wittgenstein's subsequent discussion and in the "metaphilosophical" sections that follow, this is no theory at all, and certainly not the endorsement of an Anti-Realist rival account (e.g., a sociological or cognitive scientific account) to the "classical" account.

Nersessian argues, allegedly following Wittgenstein, that "concepts show *graded structures*. That is, some instances of a given concept are better examples of the concept than other instances" (p. 181). But when *in a real context* we say, "That is a good example," what we mean to index is usually that it clearly/certainly *is* an example, it *is* an x, as opposed to possibly not being an x at all. The problem with accounts such as Nersessian's, and the same problem recurs in other essays in the collection (see below), is that they draw on crude psychology, crude experimentation, in "undergirding" their weird claims with alleged empirical data. The data—about what people say is a good example of what—come almost entirely from entirely artificial, irreal "contexts," where people are simply asked decontextualized questions such as "Which of these is a good example of a bird?" The results of such bizarre/arbitrary/random/artificial/abstract questionings, which mostly involve asking questions that just do not mean anything (for often the selection offered is among things that are, to continue with this example, all birds), are, I submit, of no evidential value whatsoever.

Now, Nersessian rightly points out that Kuhn was himself quite attracted at times to approaches that at least bear some resemblances to those that I have here criticized. Famously, he relied quite heavily in his most famous work on Gestalt psychology. And, as Nersessian sets out, his late work evinced a return to psychology, this time with a specifically cognitive scientific bent. I think that the primary reason why Kuhn's last book was left unfinished at his death was that there was no way to marry his deep philosophical insight to the biological and cognitive scientific material that he came to hope might explain it, as he had (evidently) never satisfactorily explained his philosophy to his

audience.[17] I believe that evolutionary biology, child psychology, and cognitive science could never give Kuhn what he wanted—a *philosophy* of and for science, a way of enabling people (including himself) better to understand what science is, to avoid misunderstanding its different aspects. I think that Kuhn may have more or less realized this too, as he started to die, and saw that his work would never be completed.

Nersessian's essay is very abstract in parts, and the parts which are not and which are reasonably compelling are basically either common sense or *historically* established: through good historiography (and biography and autobiography) of experimentation. Kuhn need not, I think, be sorry not to have taken up more thoroughgoingly Nersessian's intriguing but (in the end) ineffective and misfiring invitation to cognitive science.

The classiest essay in the collection that focuses on cognitive science is that by Barker, Chen, and Andersen, on "Kuhn on Concepts and Categorization." Its subtle nature is quite beyond any easy summary. Its task, like Nersessian's, is to follow through "the Roschian revolution" in psychology, in relation to Kuhn's writings. I will therefore restrict myself to saying that I think this task again founders on the conceptual incoherence of "the Roschian revolution." As suggested above, the alleged "graded structure" of concepts (p. 219) plays no part in Wittgenstein's account, for good reason: it makes no sense. Take this passage from the Barker-Chen-Andersen discussion:

> The prototype of the concept "chair" . . . includes such features as the number of legs, the type of back, and the construction materials, yielding (for U.S. or European informants) a representation very similar to the four-legged straight-backed kind often seen in a dining room. Other kinds *of chairs*, such as modernistic single-pedestal armchairs, are less typical, and barstools are atypical. These different degrees in typicality constitute the graded structure of the concept. (pp. 221-22, italics mine)

The authors self-refute in the very utterance of the phrase "Other kinds *of chairs*": modernistic armchairs *are still chairs*! Not "75% chairs," or something like that. "Less typical" could mean many things: less common, less good, less bad—compare the expression "typical day at the office"! The theory of concepts here is, I am afraid,

17. I am indebted here to remarks of James Conant, Kuhn's literary executor, in a class on Kuhn at the University of Chicago in 2001. However, we will have to wait for much fuller publication of Kuhn's *nachlass*, including of his uncompleted last book, in order to confirm or deny my thought here.

founded on a misreading of Wittgenstein, on bad philosophy, and (again) on *crude* scientistic psychology.

These three essays in Nickles's book centered on cognitive science all aim to show that Kuhn's work would have profited from a closer engagement with/adoption of cognitive science. They make an interesting (if sometimes repetitive) case. The case is presented, cumulatively, as well I think as it can be or has ever been. Thus its failure is of no little significance.

And one bottom line, then, is this: it is a matter of regret that Nickles has no one in his book to *question* the "cognitive science" reading of Kuhn, and more specifically to argue, as I (like other Wittgensteinians or ethnomethodologists) would, that the preponderance of Kuhn's philosophy of science, properly understood, seriously *undermines* the suggestion that there is any such thing as "cognitive science," and undermines (though of course less definitively) the idea that there ever will/could be. Again, let us recall the founding contrast, in the preface to *SSR*, between the social and the natural sciences. It is arguably salient to claim that the points Kuhn makes about the social sciences can by and large be extended to the cognitive sciences: they are largely disciplines without a paradigm, and we should *not* assume that it would make sense for them to try self-consciously to acquire one.

Let me close by indicating very briefly something of the quality of three more of the essays on diverse subject matters in this collection, and the work they help do in moving us beyond the legend of Kuhn closer to a rational and hermeneutically sound assessment of his virtues and problems.

Joseph Rouse's essay will, to those familiar with his helpful work on Kuhn, offer little new in the way of conclusions, but does offer a sharper and *better exemplified* account than he has succeeded in offering in the past; and this is really quite rhetorically crucial, given his thesis: that it is Kuhn's essentially *exemplified* account of science *as practice* that is crucial. In order to avoid the potential charge of pragmatic self-refutation, Rouse needed to practice what he preached, as he has here. Rouse's conclusion (which he then goes on to example), that "Kuhn's philosophical perspective [can] accommodate far-reaching critical attitudes toward the sciences and their pervasive role in our world" (p. 118), stands as a healthy antidote to Fuller's criticisms.

Barry Barnes, in his intriguing "Thomas Kuhn and the Problem of Social Order in Science," questions (p. 124), like Rouse, the "'large' view" of Kuhn taken by most who engage with his work (including, in quite different ways, Friedman's fine essay, and also Longino's and Worrall's contributions to this volume). The "large" view focuses on Kuhn's alleged grand historical vision or model of science and scientific change; the alternative view, that Barnes (along perhaps with Rouse, Nickles, and certainly myself) explicitly favors, holds that the foremost "value and significance of Kuhn's work lies in its revelatory treatment of scientific activity and the mundane details of everyday scientific practice" (p. 124). The trouble with Barnes's essay is that, against his intentions, his too is in the end a "large" view, unlike those thoroughgoingly exemplified takes on Kuhn put forward (principally) by Wittgensteinians and their intellectual allies, such as the ethnomethodologists. Barnes powerfully criticizes the know-nothing "anti-elitism" of Fuller's view of Kuhn and science (p. 140), but ultimately Barnes himself has an imperial vision of science, and himself falls thus into having a "large" view—in his case, again, an Anti-Realist theory of science. (See p. 129 for Barnes's endorsement of the theory of Finitism, which, problematically, he finds in Kuhn and Wittgenstein; see p. 134 for Barnes's continued adherence to sociology of scientific knowledge (SSK) epistemology, which ultimately is merely a mirror of and a rival to traditional Realist and other epistemologies, not [as in Wittgenstein and in most of Kuhn] a break with the obsessive search for *any* "large" epistemology.)

And finally, there is John Worrall's "Normal Science and Dogmatism, Paradigms and Progress: Kuhn 'versus' Popper and Lakatos." As one might hope from the title, Worrall offers a *proper* pro-Popper perspective on the Kuhn versus Popper debate. He attempts *fairness*, even while taking up a stance within the debate that is critical of Kuhn. Worrall's essay shows what Fuller's book might have been, if Fuller had been concerned to be fair and reasonable. As a card-carrying Kuhnian, I would take issue with quite a number of Worrall's contentions, practices, and conclusions, including his problematic tendency to continue the Popperian vice of wanting to picture the scientist as a magisterial figure awfully like a metaphysical philosopher (see especially p. 76), and his apparent failure to read Kuhn's historical case studies as a crucial part of his corpus.

In other words: from my perspective, Worrall is a little like Fuller, except for a key difference. *Whether or not* one is a Kuhnian, Worrall is (like most of Nickles's authors) quite clearly challenging, worth read-

ing, and someone who at least tries to move the debate forward, whereas Fuller simply *is none of these*.[18]

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18. Thanks for very helpful comments go to Bojana Mladenovic (especially), Wes Sharrock, Phil Hutchinson, Stefano Gattei, and John Preston. It is extremely important, given the regrettably necessary tone and content of part of this review article, that none of these philosophers be held in the slightest responsible for the views expressed above, which as formulated are, as ever, mine alone.