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CONTENTS

DISCUSSIONS

HACKER'S COMPLAINT

By Scott Soames

My goal in writing 'Philosophical Analysis in the Twentieth Century' was to identify and explain the most important achievements of analytic philosophy which every student of the subject should be aware of, as well as those of its failures from which we have the most to learn. I attempted to do this by constructing a history that was itself a piece of analytic philosophy in its emphasis on analysis, reconstruction and criticism of arguments. In rebutting Hacker's critique of it, I explain how my goal shaped my selection of topics, with special reference to the ordinary language period. I correct his misrepresentations of my treatment of the philosophers of this period, I demonstrate his failure to grasp, or understand the significance of, the Kripkean necessary a posteriori, and I reveal the misconceptions in his criticism of my interpretation of the 'Tractatus'.

I

I am grateful to the Editors for inviting me to reply to P.M.S. Hacker's review in this journal of my *Philosophical Analysis in the Twentieth Century*.[1] I begin with his complaint about the materials I chose to discuss:

> In its selection of materials it is unrepresentative: significant figures are omitted and pivotal works are not discussed ... the book is less a history of analytic philosophy than a series of critical essays on select figures and a few of their works, often chosen primarily to substantiate a thesis that is erroneous.

Since no erroneous theses leading to indefensible choices about what material to include are identified, I shall try to tease this out as I go.

The first thing to remember in this connection is how massive the analytic tradition is. The amount of analytic philosophy done in the twentieth century dwarfs both that done in other traditions and that done throughout all previous history. Consequently any attempt to present an introductory overview of analytic

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[1] P.M.S. Hacker, 'Soames' History of Analytic Philosophy', *The Philosophical Quarterly*, 56 (2006), pp. 121–31; S. Soames, *Philosophical Analysis in the Twentieth Century* (Princeton UP, 2003), 2 vols.

philosophy must be highly selective. My volumes concentrate on eleven major figures: Moore, Russell, Wittgenstein, Ayer, Ross, Quine, Ryle, Austin, Grice, Davidson and Kripke. This is supplemented with shorter, but still substantial, chapters on Hare, Strawson and Malcolm, plus briefer sections on Stevenson, Hempel and Church, and, regrettably, only a few mentions of Carnap.[2] As with any selection, there is room to quibble. However, it would be hard to maintain that these figures do not merit the attention they receive. Hacker does not dispute this.

Instead, he criticizes my omissions. His list of those I should have discussed includes Elizabeth Anscombe, Gustav Bergmann, Isaiah Berlin, Max Black, R.B. Braithwaite, C.D. Broad, Rudolf Carnap, Herbert Feigl, Gottlob Frege, Peter Geach, Stuart Hampshire, H.L.A. Hart, Carl Hempel, William Kneale, Charles Morris, Otto Neurath, Karl Popper, H. H. Price, H.A. Prichard, F.P. Ramsey, John Rawls, Hans Reichenbach, Moritz Schlick, Alfred Tarski, Friedrich Waismann, John Wisdom and G.H. von Wright.[3] But why stop there? If these figures deserve to be discussed, surely the same can be said for an equal number of others. The reason for not treating this bloated cast of characters is obvious: it would have made my task impossible. Consequently one can take Hacker's complaint seriously only if one rejects the goal of my work, which was to introduce the reader to analytic philosophy by identifying and explaining not only its most important achievements, but also those of its failures from which we have the most to learn. The aim was to do so by constructing a history that was itself a piece of analytic philosophy in its emphasis on analysis, reconstruction and criticism of arguments. This required intense scrutiny of a small number of works by leading figures – as well as a clear conception of what did, and what did not, constitute lasting progress.

II

It is against this background that the representativeness of my selections must be judged. The key question is *representative of what*? Not of all significant work in the period; not of the mature work of every prominent analytic philosopher; and not of the documents, major and minor, published and unpublished, that best reveal the causal influences on their thought. The burden of my selections was to represent the most important and lasting lessons, the successes and failures, that all students of the subject should understand. With this in mind, the reader may evaluate the merit of Hacker's complaint that my selection *misrepresented* Strawson, Austin and their compatriots.

About Strawson, he says

[2] As I indicate in Vol. I (pp. xvii–xviii), I hope to include much of Carnap, along with Frege, Tarski and others, in a future volume on the contributions of more formal work to analytic philosophy. As indicated in my 'What is History For?', forthcoming in *Philosophical Studies*, I also hope to include the Carnap side of the Quine–Carnap debate in a second edition of Vol. I.

[3] Hacker fails to notice my discussions of Hempel, and my mention of Tarski's theory of truth in Vol. II, where the reader is directed to the chapters on him in my *Understanding Truth* (Oxford UP, 1999).

His most important and representative work is not the minor paper he wrote in *Analysis* 1949 on truth, in which he suggested a performative account of truth, never again repeated in his many later papers on the subject. His highly influential 'On Referring' (1950) is passed over in silence. There is no mention of his *Introduction to Logical Theory* (1949), ... of *Individuals* (1959), ... *The Bounds of Sense*, ... or of *Subject and Predicate in Logic and Grammar* (1974).

Here Hacker takes himself to be making the supposedly devastating criticism that my selection does not represent Strawson's *best work*. However, this is no criticism at all. The aim of the chapter on Strawson was not to show him at his best (or worst); nor was it to evaluate his standing among philosophers of his time. Rather, it was to illustrate three serious errors common among Oxford philosophers of his day which, in time, would contribute to the demise of their distinctive approach. The errors were (i) adopting analyses of terms that overlooked the systematicity of meaning by focusing on uses of simple sentences containing them (e.g., to call something 'true'), while ignoring or mischaracterizing complex sentences in which they occur; (ii) failing to identify factors (like Gricean conversational implicatures) distinct from meaning which have important effects on use; and (iii) exaggerating the extent to which explicitly performative sentences differ from others by encouraging the idea that the former do not express propositions that provide them with truth-conditions (II, 126–34). Similar errors identified in the chapter on Hare drive home the point that these problems were not unique to Strawson. This was why I discussed Strawson's paper on truth. Was the thesis guiding my selection erroneous? Does Hacker doubt that (i)–(iii) are errors, that Strawson (and others) really committed them, or that recognizing them is an important lesson to be learnt from the period? If so, I invite him to defend that view.

What of my decision not to include 'On Referring', *Introduction to Logical Theory* and the rest of Strawson's greatest hits? Although these works have considerable merit, they do not rise to the level of major advances the understanding of which is crucial for analytic philosophers generally. 'On Referring' makes the useful point that when indexicals are involved, it is not meanings of sentences, but (statements made by) uses of them that are bearers of truth and falsity. *Introduction* can be seen as taking a step towards the now widely accepted analysis of definite descriptions and other quantifier phrases as generalized quantifiers. However, Strawson's work on descriptions was not a serious threat to Russell's, his treatment of indexicals missed David Kaplan's crucial distinction between content and character, and his theory of presupposition conflated utterances involving singular referring uses of demonstratives with those involving quantifiers and descriptions.[4] Though shortcomings such as these do not negate Strawson's genuine accomplishments, they do explain why his work did not reach the first rank.

[4] See my 'Presupposition', in D. Gabbay and F. Guenthner (eds), *Handbook of Philosophical Logic*, Vol. IV (Dordrecht: Reidel, 1989), pp. 553–616, and 'Entailment, Presupposition, and Implicature', in *Encyclopedia of Philosophy*, 2nd edn (London: Macmillan, forthcoming).

III

As for Austin, Hacker takes me to task for concentrating on *Sense and Sensibilia*, rather than *How to Do Things with Words*. Here there is substance to argue about. After Grice's theory of conversational implicature, the latter was the second most important achievement in linguistic theory construction of the ordinary language era. Although this was noted in the introduction (II, p. xiv), I acknowledge that a fuller discussion of it would have served to qualify my criticism of the ordinary language approach as being too unsystematic. However, it would not have negated that criticism, for although Austin's discovery of speech acts was a step forward, it was not the major breakthrough it appeared to be at the time. Its central idea, that the study of linguistic meaning is to be subsumed under the study of human action generally, has not been born out by fifty years of subsequent philosophy of language and linguistic semantics. Speech acts of roughly the type he identified do, of course, play a role in contemporary semantic and pragmatic theories, but only a minor one. As for the significance of his theory for philosophy generally, outside technical issues about language, it has been something of a bust, as my discussions of performative analyses of truth and goodness illustrate.

Regarding the comparative value of *How to Do Things with Words* and *Sense and Sensibilia*, Hacker has it backwards. Where the former is methodical and tedious, the latter is sparkling and delightful – one of the century's finest examples of philosophical prose. Where the former is narrowly focused on technical linguistic issues, the latter deals not only with language, but also with issues of perception, knowledge and scepticism that are central to philosophy. The lessons it holds derive from (i) its largely successful critique of the sense-data theory of perception (discussed II, pp. 178–87), (ii) its insightful observations about perceptual language (II, pp. 185–7), and (iii) its fascinating, but flawed, attempt to convict the sceptic of semantic incoherence (II, pp. 187–92). The latter has the additional value of extending a central lesson (II, pp. 166–70) of my discussion, in chs 3 and 7, of related attempts by Ryle and Malcolm to disarm scepticism about perceptual knowledge by linguistic means. It is one thing to show that judged by the best non-sceptical account of meaning, the sceptic violates presuppositions which give his words the meanings they have; it is quite another thing to produce an anti-sceptical argument *all* the premises of which the sceptic himself must accept, semantic and non-semantic alike. Whereas the latter is a will-o'-the-wisp, the former may, in limited cases, be possible. Although Austin, Ryle and Malcolm did not fully succeed in this, their failures were instructive, and their goal should not be abandoned prematurely. This thesis guided my selection of works by Austin, Malcolm and Ryle. Is it too erroneous?

IV

Regarding Ryle, what worries Hacker is not my selection of works, but my 'mischaracterization' of him as a logical behaviourist. Strangely, Hacker thinks that

a quotation from Ryle that speaks of 'acts of mind' such as calculating and pondering is sufficient to make his point. It is not. Someone who holds that talk about As is to be understood as talk about Bs is under no obligation to abjure A-talk. Hence a logical behaviourist can speak freely of a person's mental life so long as this talk is understood as being ultimately about the person's behavioural dispositions. That said, there is a puzzle here. On the one hand, Ryle was clearly a logical behaviourist. Since he was neither a dualist nor an eliminativist, and he rejected the view that mental states are brain states, his views left nothing else for them to be but behavioural dispositions. On the other hand, he disliked and resisted the 'behaviourist' label, and his dispositional analyses of mental terms freely invoked other mental terms, without any systematic effort to show that the chain of analyses bottoms out in entirely behavioural language (II, pp. 106–9). The key to resolving this puzzle lies in Ryle's general opposition to reduction, and his insistence that it is wrong to expect philosophical analyses to yield necessary and sufficient conditions for the application of a term (II, pp. 79–81). Because of this, he would have rejected the claim that there are any *definitions* or *analyses* $\ulcorner\forall x(Mx \Leftrightarrow Bx)\urcorner$ of mentalistic predicates M in terms of purely behavioural and physicalistic formulas B. In this narrow sense, Ryle 'rejected' behaviourism. Nevertheless, he *was* a behaviourist in the broader sense of believing that talk of the mental is, in the end, nothing more than talk of behaviour.

V

I now turn to the importance of our growing understanding of *analytic, a priori* and *necessary truth*, their gradual disentanglement from one another, and the contribution made by Kripke's account of the necessary *a posteriori*. In my discussion of ordinary language philosophy, I show how the conflation of these categories adversely affected Malcolm's paradigm case argument (II, pp. 162–3), Ryle's discussion of the truths of physics and the truths of ordinary life (II, pp. 81–2), his account of perception (II, pp. 86–8), and his argument against beliefs and desires as causes of actions (II, pp. 95–9). Of course, not all problems of the era stemmed from the conflation of these categories of truth. For example, those identified in my discussions of Strawson, Hare and Austin do not. Strangely, this does not stop Hacker from taking me to task for not sourcing the claim that Austin assimilated the necessary to the analytic. The reason why no source was cited is that no such claim was made – because the Austinian arguments I addressed did not raise the issue. Nevertheless, the conflation of the analytic, the *a priori* and the necessary *was* widespread and deeply ingrained, as a perusal of Grice and Strawson's classic 'In Defense of a Dogma' will attest.[5]

　　Concerning Kripke, Hacker's incomprehension is astonishing. Apparently he thinks that by the time Kripke came along the necessary *a posteriori* was old hat. It was all there in *The Development of Logic* in 1962, and *Probability and Induction* in 1949.[6]

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[5] *Philosophical Review*, 65 (1956), pp. 141–58.
[6] W. Kneale, *Probability and Induction* (Oxford: Clarendon Press, 1949); W. and M. Kneale, *The Development of Logic* (Oxford: Clarendon Press, 1962).

In 1962, we are told, the Kneales 'emphasized that the three distinctions [analytic/ synthetic, *a priori/a posteriori*, necessary/contingent] are not synonymous, and not even co-extensive'. Yet when we turn to the designated section (*Development of Logic*, pp. 637ff.), the main points we find relating to non-coextensiveness are (i) that *if* by *analytic* we mean what Frege did, namely, that which follows from logical truths plus definitions, then not all *a priori* truths are analytic; and (ii) that although *every truth that can be shown to be necessary will thereby have been established as known a priori*, there *might* be necessary truths (like Goldbach's conjecture) that cannot be known at all. What can Hacker have been thinking? No one who knows the subject could imagine these weak and hedged observations capturing Kripke's distinctions. Indeed, the italicized part of (ii) is flatly inconsistent with one of Kripke's main lessons.

The 1949 text is more complicated. Kneale's subject (in part II) is natural laws. He insists that these are not statements of fact but 'principles of necessitation', like *It is impossible for a thing which is red to be green at the same time*, which make claims about which facts are possible and which are not (*Probability and Induction*, p. 32). Natural laws are principles of this type that cannot be established by intuitive induction, or known *a priori* (pp. 71, 80, 89). Two of his examples are (a) *Salt dissolves in water* and (b) *Dodos have a white tail feather*. We are told that to take (a) to be a natural law would be to take it as stating *It is impossible that salt should not dissolve in water* (p. 71), while to take (b) to be a natural law would be to take it as stating not only that all recorded dodos have white tail feathers but also that 'if there had been any dodos other than those mentioned in our records, they too would have had a white feather in their tails' (p. 75). The point to notice here is the vacillation, characteristic of Kneale's discussion, between taking natural laws to be counterfactuals and taking them to be unrestricted claims about possibility and impossibility. Though these two types of statements are related, it is now clear to us, as it was not to him, that they are significantly different. Since counterfactuals are not, in general, necessary, if true, it does *not* follow from an account of natural laws as counterfactuals that they are necessary truths – in which case the question of the necessary *a posteriori* does not arise.

Although this question may arise if natural laws are taken to be unrestricted statements of possibility, the answer to it is unclear, because such an analysis is arguably too strong, because the idea that statements of possibility and impossibility might, in some cases, themselves be contingent is not out of the question, and because Kneale offers no clear account of how *a posteriori* knowledge of such statements is possible, while expressing doubt that natural laws in his sense are, strictly, knowable at all (pp. 88–9). As a result, his position in 1949 appears to be compatible with his claim in 1962 that *every truth that can be shown to be necessary will thereby have been established as known a priori* – which, as I have said, is the antithesis of Kripke.[7] For all these reasons, no one who understands the subject would dream of locating the Kripkean necessary *a posteriori* in Kneale. The fact that Hacker purports to find it there is an important point to remember when assessing his competence to

[7] *Contra* Kripke, Kneale also holds that 'no meaning can be attached to the suggestion that there are truths of possibility and impossibility about particulars as such' (*Probability and Induction*, p. 36).

understand, let alone evaluate, my thesis about the philosophical import of our post-Kripkean understanding of the difference between analytic, *a priori* and necessary truth.

VI

The level of incomprehension in Hacker's criticism of my discussion of Wittgenstein rivals that of his discussion of Kripke. Since space limitations prevent me from correcting all his errors, I shall confine myself to a few comments about the logic and ontology of the *Tractatus*. Regarding the ontology, he claims that I misrepresent Wittgenstein's claim that (simple) objects are colourless, which I take as indicating that such objects

> do not themselves exhibit or possess specific properties like shape or colour, nor do they determine which things actually instantiate such properties. Rather ... such properties are to be analysed in terms of the relations among the simples.... Since being a certain colour – say red – is simply a matter of being made up of simples that stand in a certain configuration, the simples themselves can't be coloured (I, p. 208).

Hacker objects:

> Soames misrepresents the *Tractatus* ontology. He supposes that ... simple objects do not exhibit specific properties such as colour, not realizing that Wittgenstein assumed that minimally discriminable shades of colour *are* simple objects. So Soames claims that colours are material properties, whereas, according to Wittgenstein, it is *having a certain colour* that can be a material property of a complex – the shade of colour had is one among the many objects that form the coloured complex.

Whereas Hacker identifies the property red with a particular simple object, I make no claim about its ultimate analysis. If this is misrepresentation, there is a lot of it going around. Standard interpretations of the *Tractatus* are split between those, like Hacker's, which include properties and relations among the simples, and those, like Anscombe's, which do not.[8] I view the *Tractatus* as inexplicit on this issue, and pulling in both directions. Since many of its main tenets can be appreciated without taking a definite stand, my intention was to leave the issue unresolved.

Crucially, Hacker's view has no effect on whether simple objects do or do not 'exhibit specific properties such as colour'. My claim, that for anything x to be red, x must be a complex the simples of which are composed in a certain way, is unaffected, since on Hacker's view, the configuration of objects that make up x will be one containing the simple object red. On this view, neither the colour, i.e., property, red itself, nor any other simple object, is red. If, as it appears, Hacker thinks otherwise, he may be confusing the 'is' of predication with the 'is' of identity. That

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[8] G.E.M. Anscombe, *An Introduction to Wittgenstein's Tractatus* (London: Hutchinson, 1959). Others agreeing with Anscombe include J. Griffin, *Wittgenstein's Logical Atomism* (Oxford UP, 1964); R. Fogelin, *Wittgenstein*, 2nd edn (London: Routledge, 1987); P. Carruthers, *Tractarian Semantics* (Oxford: Blackwell, 1989).

some such confusion is involved is suggested by the way he attempts to distinguish *being red* from *having the colour red*. The latter, he suggests, is the property of being a complex that contains red among the simples making it up. However, since to be red ('is' of predication) is just to have the property/colour red, which, on Hacker's view, is just to have the property of being a certain sort of complex containing the property/colour, the property red, which we predicate when we say *x* is red, *is* the property of being a complex of that sort. Though admittedly somewhat strange, this view is not obviously incoherent – until, that is, Hacker suggests that the properties are different, and non-coextensive.

The larger point at issue is our ability to describe Tractarian simples. Since Hacker takes this ability to depend on including properties and relations among them, he sees no obstacle to our being able to describe simples. He is wrong. As I have shown, one cannot truly say of any simple that it is red, or any other colour. The same goes for all material properties. It is true that the view that properties are themselves simples does not preclude simples from having other non-relational properties – though it is strange that Wittgenstein does not mention them at 2.0233 (where he denies that simples with the same combinatorial possibilities have intrinsic properties that distinguish them). However, even if there are such properties, the same Tractarian doctrines, about the necessary independence of atomic sentences and facts, which make it impossible to give examples of *relations* holding among the simples, also make it impossible to give examples of their *properties*. It is these doctrines (I, pp. 209–12), not any view about whether properties and relations are simples, which lead to the dark and implausible Tractarian view that all meaningful talk about the world reduces to talk about simple objects that have no properties and bear no relations that we can imagine (I, pp. 212–13). Since Hacker misconceives the grounds for this conclusion, his criticism has no force.

Turning to questions of logic, he criticizes my – quite unremarkable – claim that in the *Tractatus*, logical truths are true in virtue of meaning. To say that they are such truths is to say that their truth is not due to correspondence with any necessary facts, and that they can be known to be true simply by understanding them. So understood, the doctrine is clearly Tractarian. For Wittgenstein, logically complex sentences are truth-functional operations on sets of atomic sentences. Since the role of the vocabulary of such a sentence is to indicate what the operations are, and what sentences are operated upon, understanding such a sentence involves knowing the operations and their arguments. According to the *Tractatus*, when a sentence is a logical truth, this understanding provides one with everything one needs to determine its truth. Hence it is true in virtue of meaning. Though, in point of fact, it is unlikely that all logical truths are knowable in this way – especially if one allows second-order logical truths and certain 'inadvertent' Tractarian logical truths (I, pp. 29–32) – the thesis that all logical truths have this status is central to the *Tractatus*.

So is the thesis that all and only necessary truths are logical truths. Hacker's claim to the contrary – that 'there *are* metaphysical necessities (e.g., that the world consists of facts, that space and time are forms of objects) but they are not *logical* [or analytic] necessities' – misconstrues the status of Wittgenstein's metaphysical propositions. According to the *Tractatus*, they are nonsense, which, after serving their purpose,

must be dispensed with. As such, they are in a different category from propositions which are strictly, and lastingly, true.

Given all this, I am gratified that Hacker deems my discussion of these logical doctrines my 'deepest misrepresentation' of the *Tractatus*. If that is as bad as it gets, I am pretty well off, since there is no misrepresentation here at all. However, since he raises the issue of the metaphysical propositions of the *Tractatus*, I shall close with something that really is a misrepresentation – not of Wittgenstein, but of me. As everyone knows, the *Tractatus* ends with an acknowledgement that since its propositions violate its constraints on sense, they are therefore nonsense. In my discussion, I distinguish two views about this. According to the first, the system as a whole is self-undermining, despite its numerous insights. According to the second, the system is not self-undermining, because its propositions succeed in *showing* what they cannot state. I take the first view to be correct – since the idea that one's words might succeed in showing the very thing that they purport, but fail, to state strikes me as a cheat. However, I emphasize that Wittgenstein himself adopted the second view, according to which the *Tractatus* is *not* self-undermining. Despite this, Hacker criticizes me for foisting on Wittgenstein the intention of writing a self-undermining work:

> Soames' final major misinterpretation of the *Tractatus* is his claim that Wittgenstein's ultimate position is that 'the Tractarian system must be rejected', and its patent inadequacies should be avoided (I, p. 252) – *so the whole book was written in a spirit of Kierkegaardian irony. This is the orthodox view of the New American Wittgensteinians.* It is inconsistent with everything that Wittgenstein ever said about the *Tractatus*, and there exists no evidence whatsoever to support it [my italics].

Since it may not be clear what 'the orthodox view of the New American Wittgensteinians' is, it is fortunate that one of their leading members, Michael Kremer, also reviewed my history, commenting on this very point. However, his criticism is the opposite of Hacker's. Instead of criticizing me for espousing the new view, he criticizes me for ignoring it, and giving instead a 'fairly standard account' of the work.

> *He* [Soames] *presents a fairly standard account of Wittgenstein's thought and development but neglects to discuss the considerable secondary literature running counter to this interpretation....* Soames claims that Wittgenstein's closing description of the book as nonsense is based on the application of a 'test of intelligibility' derived from the book's theory of language, to that theory itself. Soames thinks that
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> > there are two ways of viewing Wittgenstein's final position. On one view [alternative 1], the *Tractatus* as a whole is self-defeating and/or self-contradictory, despite its illuminating insights on many points. Thus, the Tractarian system must be rejected, and we should strive to find ways of preserving its insights while avoiding its clear inadequacies. On another view [alternative 2], the *Tractatus* is acceptable as it stands. In it, Wittgenstein has deliberately violated the rules of language in an attempt to show us what those rules really are; to get us to see what the rules of intelligible thought and language really are, he had to go beyond them. At the time

he wrote the *Tractatus* Wittgenstein adopted the second view. Later, he had the good sense to change his mind. In my opinion, the first view of the *Tractatus* is clearly correct (I, pp. 252–3).

This neglects a recent, influential interpretation, according to which there is a third way to read the Tractatus. On this reading ... Wittgenstein traffics in nonsense, but not by violating some criterion of intelligibility in order to show the unsayable limits of the sayable [alternative 2]; yet the book is not simply a broken and self-refuting text, to be mined for insights that survive its self-destruction [alternative 1]. *Rather, the Tractatus undermines philosophical theorizing by showing how philosophical theories degenerate into nonsense – by allowing the reader to see, in trying to understand them, that in the end they fail to make sense.*[9]

This final emphasized bit is the orthodoxy of the New Wittgensteinians to which Hacker refers. In my opinion, he is right in holding, against Kremer, that this interpretation of the *Tractatus* is incredible. Why, after all, should the production of a nonsensical or self-undermining system by one philosopher support the conclusion that all philosophical systems are, by their very nature, nonsensical or self-undermining? When it comes to understanding me, however, Kremer is right. I am no New Wittgensteinian. According to me, the Wittgenstein of the *Tractatus* wrongly did *not* see his system as self-undermining.

What can Hacker have been thinking? It is not as if my text is unclear on this point, or difficult to decipher. On the contrary, it is obviously incompatible with the interpretation he gives it. Perhaps he simply did not attend to the text. Perhaps, as with his criticism of my principles of selection, and his dismissal of my discussion of the significance of Kripke, he refused to engage with the work, or to try to understand its basic premises. Noting that much of it threatened his own view of material to which he had come to feel a proprietary right, he may simply have decided to reject it, and do what he could to minimize its impact. If so, then his critique scarcely qualifies as scholarship.

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[9] M. Kremer, Review of Soames, *Philosophical Analysis in the Twentieth Century*, Notre Dame Philosophical Reviews, http://ndpr.nd.edu; my italics.