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**Reply to Critics of *Philosophical Analysis in the
Twentieth Century*:**

**Christopher Pincock, Thomas Hurka, Michael Kremer, and
Paul Horwich**

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Volume 1

Reply to Christopher Pincock and Thomas Hurka

Pincock

Chris Pincock is offended that I presumed to write a historical overview of analytic philosophy without filling it with scholarly detail provided by specialists. Instead of relying on them, *I simply read the works of leading philosophers and tried to figure out for myself what they were up to*. Didn't I know that this is *impossible*? I myself point out in the Epilogue that the history of philosophy is now a *specialized* discipline. How, Pincock wonders, could I have failed to recognize the implications of this lesson for my own project? *Don't try this at home!* Read the original works, if you must, but *don't dare* say anything about the views you find – let alone *evaluate* them by *contemporary* standards -- unless you first vet your remarks with those in the archives. *History isn't easy, you know!* On the contrary, Pincock tells us, “the *overriding* lesson of work in the history of analytic philosophy is that history is hard.”

Conveying that lesson should, he tells us, be the main goal of any historical introduction to the subject. “*Above all,*” he says, “I would hope that the reader would finish reading such a book with an appreciation of the difficulties inherent in the study of the history of philosophy.” This, I submit, is self-serving nonsense. Conveying its own difficulty is *not* an overriding goal of any worthwhile intellectual enterprise. The chief difficulty that daunts Pincock is, of course, the secondary literature produced by those like himself. According to him, any proper historical introduction “would have to build on *the mountain* of books and papers” – by which he means *the mountain of secondary literature* – and, “judiciously choose from all the proposed interpretations of those details,” carefully referencing alternative interpretations. I disagree.

There are different kinds of historical work, with different goals, which make different contributions. My goal was to present analytic philosophy by identifying both its most important

achievements and those of its failures from which we have the most to learn. I tried to do that by writing a history that was itself a piece of analytic philosophy in its emphasis on the analysis, reconstruction, and criticism of arguments. This required intense focus on a limited number of texts, plus a clear conception of what did, and what did not, constitute progress. I wouldn't be doing philosophy if I didn't think that it progressed, and that we know more now than we did a century ago. For that reason, I *don't* view its history as a story of the clash of defensible but irreconcilable views, from which the most we can expect is a sympathetic understanding of how things looked to the participants. Instead, I see it as a challenge to identify what we have learned. For those who find this too judgmental, I pose two questions.

Q1. If you don't think that progress is made in philosophy, or that history should chronicle it, why should we be interested in the subject, or its history?

Q2. If you agree that an historical investigation may properly be concerned with real philosophical progress, but you think I have misidentified it, what have been the most important developments in the past century, and what shortcomings in earlier philosophy does our more advanced knowledge allow us to spot?

Pincock doesn't answer my second question, and is evidently uncomfortable with criticizing earlier philosophers by current standards. Instead, he criticizes me for doing this, purporting to find incoherence in my approach. Referring to my contention that there are important modal distinctions we can see clearly now which our predecessors could not, he says:

But doesn't this prove that we cannot assume that [to quote me] "the philosophy done in this period is still close enough to speak to us in terms we can understand without a great deal of interpretation"? Indeed [Pincock continues] the more that this assumption leads us to ascribe confusion and obviously incorrect doctrines to intelligent writers, the more we should revise our fundamental preconceptions about how the history of this period should be written.

This is simply historicism, run amok. The legitimate principle of charity cautions against attributing doctrines to intelligent authors the incorrectness of which should have been obvious *to*

them. It says *nothing* about attributing doctrines the incorrectness of which are obvious *to us*, because of advances in the subject to which they were not privy. Of course, if you don't believe that there have been important advances, this distinction will be lost on you, as it is on Pincock. As I see it, you understand earlier philosophers *better*, not worse, when you see how their excusable ignorance of later advances led them down paths we now know to have been detours.¹

Declining to judge past philosophers by present standards, Pincock *does* answer my first question, *What makes the history of philosophy valuable, if not a concern with progress?* He says:

One of the main benefits of historical studies is that it exposes us to alternative conceptions of philosophy and forces us to examine our own presuppositions. At the end of the day, of course, we will probably stay with the philosophical positions that we began with, but the range of alternatives that are open to us will be enriched, as will our appreciation of how difficult it is to convince others that our answers are the best.

So we study history, *not* to change our views, or to critique others, but to become *more appreciative* of other views. Pretty thin gruel, don't you think? No leading analytic philosopher has ever adopted such a frivolous conception of the subject, and neither should we.

So much for Pincock's critique of the aim of my enterprise. His three examples of my alleged errors fare no better. The first concerns my critique of Russell's epistemology in *Our Knowledge of the External World*. Russell claims that material objects are logical constructions out of sense data; therefore, since we can know about sense data, we can know about material objects. The problem I find is that "if material objects are to be logical constructions out of sense

¹ Regarding criticisms involving modality, Pincock evidently doesn't understand them. For example, he wrongly says that I criticize Moore's open question argument for a (pre-Kripkean) conflation of analyticity and necessity. I do no such thing. On pages 48-58, I criticize the argument most naturally suggested by Moore's text for requiring a notion of analyticity that can't be had -- one that grounds both (i) the inference from (a) the indefinability of *good* to (b) the claim that there are no analytic generalizations of equivalences involving it, and (ii) the inference from (b) to (c) the claim that no statements not containing *good* can be used to prove any conclusion about what is good. On pages 58-62, I try to improve on the argument by dropping the focus on definition and formulating a more expansive notion of analyticity, but find that doing so requires a principled distinction between analytic obviousness (due to meaning) and self-evidence (not due to meaning) that Moore does not, and perhaps cannot, explain (64-67, 74). None of this has anything to do with pre-Kripkean understandings of necessity, or with identifying the necessary and the analytic.

data, then they must be logical constructions [not simply out of one's own sense data but] out of everyone's sense data."² Since this makes one's knowledge of material objects dependent on dubious knowledge about the private sense data of agents in arbitrary conditions, it makes the explanation of our knowledge of material objects harder, not easier.³

Pincock responds by accusing me of historical malfeasance. He says:

From his initial rejection of idealism in 1898 onwards, Russell *insisted* that sense data did *not* depend on a subject *for their existence*. His epistemology *constantly* invokes principles about sense data ... which outstrip the experiences of all conscious agents. How could Soames have missed this *central* fact about Russell's conception of sense data? I suggest that he did not review Russell's writings where this point is unequivocally made, or engage with the scholarship in which this conception of sense data is conclusively established. (my emphasis)

A word to the wise, one shouldn't make such a charge if one doesn't know the texts. Pincock doesn't. In the original 1914 edition, Russell says it is "probable" that sense data "*depend for their existence* upon physiological conditions in ourselves, and that, for example, the coloured surfaces that we see *cease to exist* when we shut our eyes."⁴ In the 1926 revised edition, Russell characterizes sense data as mental.

According to some authors – among whom I was *formerly* included – it is necessary to distinguish between a sensation, *which is a mental event*, and its object, which is a patch of colour or a noise or what not. If this distinction is made, the object of the sensation is called a "sense datum" ... *Nothing in the problems to be discussed in this book depends upon the question whether this distinction is valid or not. . . .* For reasons explained ...[elsewhere]... I have come to regard the distinction as *not valid*, and to consider the sense-datum *identical with* the sensation.⁵

² Volume 1, page 181.

³ The chief criticism I make of Russell on this point is *not* the one Pincock cites – namely that Russell's account of our knowledge of material objects must appeal to other agents the existence of which he cannot prove. Although that is a problem, the main problem is that *even if* we take our knowledge of the existence of other agents for granted, the claim that we have the detailed knowledge of their private sense data needed by his logical construction is philosophically far more problematic than the knowledge claims about material objects the construction is supposed to vindicate. Since this amounts to replacing one philosophical problem with a more difficult one, Russell's strategy can't succeed.

⁴ OKEW, 64. My emphasis.

⁵ OKEW₂, 83 My emphasis.

The distinction Russell is talking about – between *sense datum* as a perceived appearance and *sensation* as the experience of perceiving it – does, as Pincock notes, date back to the rejection of idealism. But, by the time of *Our Knowledge of the External World*, it had become irrelevant. Russell’s system of perceptual perspectives, sketched in chapter 3, does *initially* make use of unoccupied perspectives containing unperceived, nonmental, sense data. However, Russell introduces these “ideal elements,” as he calls them, only as a heuristic to simplify his presentation.⁶ In the very next chapter he *eliminates* them as going beyond what strictly can be known.⁷ The end result is a reconstruction of all knowledge of material objects in terms of the *actual* sense experiences of agents, plus experiences they *would* have if various conditions were fulfilled. *Unperceived sense data are no part of this.* Although their existence is not categorically denied, they cannot be known, and so are excluded from Russell’s reconstruction of our knowledge. Like the material “things in themselves” that the construction is designed to avoid, the merely possible existence of unperceived sense data is, as Russell, recognizes, *irrelevant* to his project -- and to my criticism. This is the second way in which Pincock goes wrong. Not only does he misrepresent Russell’s text, he misunderstands the philosophy in it.

Pincock’s next example of a grievous error is my bland remark that Ayer’s version of positivism was “quite representative of the *general tendencies* of the movement,” “though *differing* in some respects from other versions.” Professing to be *stunned* by this supposed inaccuracy, Pincock notes that recent scholarship has emphasized the variety of views held by different positivists. To be sure. But this is compatible with my overall focus on the positivists’ verificationism, plus their treatment of analyticity as the source of necessity and apriority. As for

⁶ See, my “Reply to Pincock,” *Russell*, 25 (2005): 167-71; and my reply to Mark Sainsbury in “What is History For?” forthcoming in a symposium on Volume 1 in *Philosophical Studies*.

⁷ Pp. 87, 111-12 of the 1914 edition (*OKEW*), pp. 94, 116-17 of the 1926 edition (*OKEW*₂).

including different time-slices of Schlick, Neurath, Carnap, *et al.*, there simply wasn't room. What led me to Ayer, Hempel, and Church was the need to focus on the clearest, most succinct presentations and criticisms of the view. About Carnap, Pincock is partly right and partly wrong. He is wrong to suggest that I *utterly* neglect Carnap, and wrong to imply that there is any question about Carnap's credentials as a verificationist – as Carnap's papers, "The Elimination of Metaphysics" and "Testability and Meaning" make clear. In the latter, Carnap implicitly lays down his own verificationist criterion, later explicated by Hempel, which I discuss at length -- though you wouldn't know it from Pincock. In addition, Carnap's evolving views of the role of meaning in explaining the modalities were broadly in line with those criticized by Quine in "Truth by Convention," to which I devote chapter 12. So Carnap's views aren't entirely neglected. Still, Pincock is right about the need for more. For that reason, a new chapter giving both sides of the Quine/Carnap debate on analyticity and ontology will be added to the second edition.

Pincock's final example of a supposed historical blunder is my interpretation of the holistic verificationism of "Two Dogmas" as continuing a strand of positivism – an interpretation he finds "completely mysterious." There is no need for puzzlement.⁸ Quine makes explicit his commitment to holistic verification, and his affinity with The Vienna Circle, in "Two Dogmas," "Epistemology Naturalized," "On the Reasons for Indeterminacy of Translation," and "On Empirically Equivalent Systems of the World."⁹ Pincock should consult them. He should also reread my text. In discussing verificationism, I formulate criteria of meaning in terms of the relationship between observation and non-observation statements, leaving what counts as an observation statement for separate discussion. This is important, since part of Pincock's complaint hinges on the idea that

⁸ See pp. 380-5 of Volume 1 for a line-by-line explication of the end of section 5 of "Two Dogmas," explaining precisely how Quine arrived at this position.

⁹ A good discussion of Quine's holistic verificationism in the secondary literature is "Meaning and Theory," by Quine's student, Gilbert Harman -- also cited and discussed in my text.

my critique depends on foisting an account of observation in terms of *sensory experiences* on Quine. Not so. As I show, Quine's heady talk of "phenomenalistic conceptual schemes" and physical objects as "convenient myths" "comparable, epistemologically, to the Gods of Homer," found in "On What There Is" and the original version of "Two Dogmas," is linked to corresponding talk of *sensory experience* as the touchstone of empirical significance.¹⁰ After demonstrating the absurdity of the resulting position, I point out that in later work, Quine *stopped* speaking of observation in these terms.¹¹ Nevertheless, his holistic verificationism continued to suffer from crippling difficulties, independent of contentious doctrines about observation.¹² Since this was clear from my text, any fair and open-minded critic – let alone a historian with a supposedly scrupulous regard for texts -- should have seen it. Pincock didn't.

I now turn to Tom Hurka's critique of my contention that G.E. Moore would have done better had he followed, in ethics and metaethics, his characteristic method of starting with pre-philosophical certainties about particular cases, and using them to test general philosophical principles, rather than going the other way around. In epistemology, this method takes ordinary perceptual knowledge to be self-evident – in the sense of not requiring special philosophical justification – and uses it to evaluate principles stating necessary and sufficient conditions for knowledge, rather than taking such principles to be independently supportable, and using them to assess particular knowledge claims. In ethics, the method would take the most obvious ordinary moral judgements about particular cases to be similarly self-evident, and would use them to

¹⁰ See my discussions on pp. 380-5, and 399-403, and the Quinean texts quoted there. Note in particular Quine's repeated talk of "confirmatory experiences" providing the "factual component" on which the truth of statements or theories depends, of "the prediction [by science] of future experience," of the "myriad scattered sense events [that] come to be associated with single so-called objects," and of the introduction of physical objects as enabling us "to get more easily from one statement about experience to another." What do you suppose he is talking about?

¹¹ Pp. 403-4.

¹² Pp. 389-98, 404-5.

evaluate principles stating necessary and sufficient conditions for goodness and rightness, rather than treating such principles as self-evident themselves.

Had Moore followed this method in ethics, he might not have arrived at the Bloomsburian view that the appreciation of beautiful objects and the pleasures of human companionship are the only, or at any rate, *by far* the most important, intrinsic goods; nor, I hope, would he have combined that view with the preposterous claim that an act is one's duty iff it would produce more good – i.e. enjoyment of beauty and human companionship -- than any alternative.¹³ More importantly, he might have done justice to the two ideas that made his metaethical position so exciting: (i) that *interesting moral truths* – of the sort we hope to learn from ethical theory -- are resistant to proof, or demonstration, and (ii) that some moral truths are, nevertheless, self-evident, and needed to justify other moral truths. Instead, he unwittingly threatened these ideas by claiming that *interesting* truths specifying necessary and sufficient conditions for goodness and rightness are *themselves* self-evident (or obvious consequences of such truths) -- thereby undermining his insight that they are resistant to proof. This problem could have been avoided by extending the method he employed in epistemology to ethics. On the view I suggest, *no* fully general ethical truths are self-evident, and *no* interesting ethical truths can be derived by self-evident steps from self-evident premises. However, some can be known, or justifiably believed, by appeal to self-evident moral truths about particular cases – which serve as data points justifying general principles by something like inference to the best explanation. Had Moore adopted this picture, his metaethical position would have been more plausible and coherent.

Hurka's verdict on this critique is mixed – mostly incorrect, he says, about *good*, superficially correct about *right*, but at a deeper level incorrect about *right* as well. I think he

¹³ See section 113 of *Principia Ethica*.

misunderstands the critique. It has nothing to do with how Moore would respond to the form of moral skepticism that Hurka imagines – one which says that morality is a sham because we have no reason to be moral. Although Moore never, to my knowledge, seriously addressed this form of skepticism, I agree that, had he done so, he probably would *not* have attempted a Kantian or Aristotelian refutation. Instead, he would have dismissed it as incompatible with the pre-theoretic certainties that some acts are duties, and that an act's being a duty *is* a reason to perform it. In short, his response to this form of moral skepticism would parallel his response to skepticism about the external world, just as Hurka maintains. However, this point doesn't affect my critique.

The question at issue is how we determine precisely which things are good and which actions right – given the presumption that there are genuine instances of both. We are looking for sweeping generalizations, and, if we can get them, universally quantified biconditionals. The way to get them is *not* to look, as Moore did, for those that are self-evident, but to identify restricted truths the justification of which is strong enough for them to be provisionally accepted without argument, and to generalize from there. This is something we do in epistemology -- not only when we reject overly *restrictive* analyses of knowledge that rule out ordinary knowledge of hands, but also when we reject overly *expansive* ones refuted by Gettier cases. The point of extending this method to ethics is *not* to refute moral skepticism, but to make similar progress in normative theory. This lesson applies equally to my critique of Moore's theory of goodness, and to his theory of rightness. Both remain intact despite Hurka's objections.¹⁴

¹⁴ Three final observations. (i) Ross isn't quite the hero for me that Hurka makes him out to be. Although his recognition of many good and right-making features was a breath of fresh air that constituted a good methodological starting point, his resistance to generalizing beyond it is something I criticize. (ii) In claiming that emotivism restricted attention to too narrow a range of evaluative terms, I didn't mean that emotivists were the only offenders. However, if you take the content of a claim that an act is morally right to consist entirely in one's expression of approval, then it is hard to properly distinguish that claim from the claim that the act is merely permissible but morally good. (iii) I want to counter Hurka's suggestion that I have a low opinion of Austin. I don't. *Sense and Sensibilia* is a classic. The fact that one criticizes an author doesn't mean that one holds him in low esteem – something I try to bear in mind as I confront my own critics.

Volume 2

Reply to Michael Kremer and Paul Horwich

I begin by responding to Michael Kremer, who attacks my methodology as antithetical to the good in history. In this Kremer joins Pincock, and a small group of specialists who don't just object, but take offence, to my work. Like Pincock, whose published comments express resentment, and call for what he terms a "partisan" response to the fact that I "show no interest" in the work of specialists like himself, Kremer has written of his anger at my text.¹⁵ The standard trope is to attribute this reaction to a scrupulous regard for historical accuracy – an interpretation undermined by the inaccuracy of a number of the charges. A more likely interpretation is that they view an approach to history that attempts to explain its most important successes and failures, while ignoring much of the rest, as a threat to their own more biographical approach, that chronicles the twists and turns of a philosopher's thought, and traces lines of influence. Although there is room for both approaches, the fear, I take it, is that the more philosophical will drive out the more biographical, and undermine the authority of those who practice it. My advice is to get over it. Philosophy needs specialists, but it also needs a broad picture of its history.

Kremer claims that my work fits what D. H. Fischer calls *the fallacy of presentism*, one version of which is Whig history, which Fisher describes as a "tendency ... to write on the side of Protestants and Whigs, to praise revolutions provided they have been successful, to emphasize certain principles of progress in the past and to produce a story which is the ratification if not the glorification of the present." Substitute *Kripke* and *Grice* for *Protestants* and *Whigs*, and change the topic from political history to the history of philosophy, and you get Kremer's caricature of my volume. The comparison is foolish.

¹⁵ Pincock, *Russell*, 25 (2005): 167-71; Kremer, *Notre Dame Philosophical Reviews*, ndpr.nd.edu/review, 2005.09.19

The real fallacy is in equating the history of a discipline with political and social history. No one would accuse a history of a subject that makes clear and recognizable progress of committing a fallacy because it assesses contributions of past practitioners in light of what can now be recognized as real advances. If those who make the facile comparison between philosophical and political history were more confident that philosophy does advance, they wouldn't stigmatize attempts to identify the advances as unhistorical. Nor would they respond to the challenge either to identify what *they* take real progress to be, or to explain why we should be interested in the history of a subject that never makes it, with a version of the remark, *Progress is in the eye of the beholder*. As Kremer puts it, "there are many reasons to philosophize...[e]ach motive for philosophizing brings with it a conception of philosophical progress; and the history of philosophy can expose us to these distinct ways of philosophizing, and conceptions of philosophical progress."

I think we can do better. Kremer's paean to nonjudgmentalism, with its admonition to understand past philosophers on their own terms, not ours, is a recipe for *failing* to understand what separated their misconceptions from their lasting insights. It's a hallmark of great philosophers that they are often the first to glimpse large and elusive truths. Inevitably, however, their grasp of these truths is insecure and incomplete. The more successful *we* are in distinguishing those truths from the falsehoods that partially obscured them, the better we understand both our predecessors and the subject itself. Since that is how the great analytic figures who actually *made* history viewed the matter, why should we be expected to submit to Kremer's defeatist view of their history?

Constructing a plausible picture of recent progress is, of course, itself a philosophical task, and my volumes are only a start. But whatever their defects, it is simply not true, as Kremer says, that in them "[o]ur sense of what has been achieved, where things have gone

wrong, and where they have gone right, is taken for granted.” *Taken for granted?* There is no such thing as *our* (collective) *sense* to be taken for granted. There was no pre-existing consensus about where Quine was right, and where wrong, on analyticity, indeterminacy and inscrutability, about how to separate success from failure in ordinary-language philosophy, about where Davidson’s theory of meaning fits in, or how missteps obscured some of Kripke’s central insights. That was my task. A discipline as fragmented as ours *needs* a unifying sense of its present and its past. My task was not to impose a narrow, but already well-understood, philosophical present on our past, but to construct an illuminating picture of both. The task was too important *not* to use every tool that advances in the subject have put at our disposal.

There is, of course, plenty of room to debate my discussion of particular philosophers. Yet Kremer’s only example of how I supposedly subvert the value of history is my critique of the methodological lessons Ryle draws from his discussion of fatalism . They are:

- (i) that philosophical problems, or dilemmas, *always* involve many concepts, which are unproblematically employed in their own familiar domains, but which come into conflict when generalized and brought into contact with one another.
- (ii) that philosophical problems are *never* the result of one central confusion, or a series of separately resolvable confusions; instead they are *always* nests of inter-related confusions requiring the untangling of many conceptual knots.

Ryle’s holistic version of conceptual analysis contrasts with older versions emphasizing the identification of necessary and sufficient conditions, and the baring of hidden logical forms. My criticism is that Ryle overstates his case; *some* problems – including his own example of fatalism – are more amenable to the old paradigms than the new.

The reason is apparent from two crucial sentences of his statement of fatalism:

But if it was true beforehand – forever beforehand – that I was to cough and go to bed at those two moments ... then it was *impossible* for me not to do so.

There would be a *contradiction* in the joint assertion that it was true that I would do something at a certain time and that I did not do it.¹⁶

Ryle's first sentence, which states the crucial step the fatalist needs, may be put:

If it was forever true before t that p (where p is a sentence describing actions taken at t), then it was impossible for me (prior to t) to bring it about that ~ p.

His second sentence, which is meant to provide grounds for this, may be expressed:

It is contradictory, and hence impossible (for anyone to bring it about) that both it was true before t that p and ~p.

Since the impossibility of a conjunction does *not* entail that the second conjunct is impossible, if the first is true, this argument suffers from a simple logical error. This error doesn't depend on any special sense of possibility, or equivocation between different senses. Whatever sense is selected, the error can be reduced to a simple scope confusion by taking the impossibility of a proposition to be equivalent to the necessity of its negation.

Necessarily, if it was forever true before t that p, then p

does not entail

If it was forever true before t that p, then necessarily p.

This highlights the parallel between the argument that no one could have done other than what he actually did and the argument that no entity could have had any property other than those it actually has. Both turn on the same confusion.

In the grip of his methodological holism, Ryle neglects this central point, and tries to deflate the argument by tracing connections between *knowledge, prediction, conjecture, proposition, truth, falsity, reliability, correctness, incorrectness, logical necessity, causal necessity, and inevitability*. Much of this is, of course, tangential. However, one point strikes

¹⁶ *Dilemmas*, 15, my emphasis

home – the ineffectiveness of the future-tense version of the argument, which holds that if I *will*, in fact, bring it about that p, then I must, *necessarily*, bring it about that p. The ineffectiveness of this argument shows that we tend to understand the fatalist as appealing to type of causal necessity. Since we recognize that the future can't cause the past, the future-tense argument falls flat. As I say in the book, Ryle is right about this.¹⁷

However, the lesson he draws from it about the *standard* version of the fatalist argument isn't. Contrary to Ryle, the standard argument need *not* run together a notion of causal necessity relating only events, with a notion of logical necessity relating only statements, thereby producing nonsense. Let *Necessarily S* mean *It is not possible for me, now or later, to do anything to bring it about that ~S*. Analytic truths plus truths wholly about the past are necessary in this sense. When *p* is a sentence about some present or future action, the resulting argument is coherent, though fallacious. Ryle's discussion misses this.

The point of this criticism was simply that Ryle's analytic holism had dangers of its own, and so was not everything it was cracked up to be. Given the modesty of the point, I'm astounded that it is Kremer's *prime* example of how my supposed unfairness to past philosophers subverts the value of history. *Is this a joke?* His own reconstruction of the fatalist's argument, involving the notions *necessity* and *inevitability* is more baroque than mine, and less faithful to the text in which Ryle presents it.¹⁸ Like Ryle's discussion, Kremer's nicely points out different relevant senses of necessity. However, also like Ryle's, it *wrongly* condemns the argument as nonsensical for unavoidably running together a sense that applies only to events with one that applies only to propositions. Thus, Kremer's single concrete example, which could never have supported his

¹⁷ Page 79.

¹⁸ *Dilemmas*, 15

exaggerated criticism in any case, fails in its own right – as do his subsidiary criticisms, which I must pass over.¹⁹

I now turn to Paul Horwich's objections. I agree with the first. There *is*, in Wittgenstein, a sense of following a rule *implicitly* that is continuous with what I say about his account of our knowledge of the reference of numerals, and the truth conditions of sentences containing them. Although this semantic knowledge is constitutive of understanding these sentences, it is not something *conceptually prior* to that understanding that *explains* it. Instead, the explanation goes the other way. The same is true of following a rule. When I say that for Wittgenstein using words meaningfully is *not* a matter of rule-following, I only mean that there is no notion of rule-following that is *conceptually prior* to our meaningful use of words that *explains* it. There is, however, a different notion -- of implicit rule-following -- according to which the particular way we use a word *is* our following a certain rule. Paul is right about this.

Horwich's second point raises the question "What standards of correctness determine whether a word truly applies to a given object?" The rule-following argument implies that although nothing internal to a speaker provides these standards, community agreement does. But agreement in what sense? I argue that it can't always be *simple* agreement, in the sense that a word

¹⁹ One of these involves Kremer's claim that "*the crucial point*" in Ryle's discussion of philosophical dilemmas is that they are caused by equivocation. This is false. The point of the single sentence Kremer quotes is that equivocation between different uses of the same word is *one source* of dilemmas. That sentence is the *only* one in the six-page section (*Dilemmas* 29-35) on the subject where Ryle mentions equivocation. His *crucial point* is the conceptual holism I describe, which Kremer bizarrely treats as secondary. Though taking equivocation to be central suits Kremer's polemical purpose, it is a textually inaccurate reading of the passage from which the quoted sentence is taken. Second, Kremer accuses me of calling Ryle a verificationist about *meaning*. Wrong! As he knows, my criticism (97-8) is that one of Ryle's arguments is verificationist about *knowledge* (though the label was probably misleading). Finally, Kremer says I neglect a 1931 paper on propositions, plus two brief pieces on method – chapter 8 of *Dilemmas*, "Formal and Informal Logic," and the two-page Introduction to *The Concept of Mind*. Discussing the 1931 paper would have been superfluous, since Ryle's 1953 remarks on propositions in his analysis of fatalism were tangential in any case. Discussing the two pieces on method would have been redundant, since they simply confirm his conceptual holism. The only new point, added in chapter 8, is that the meanings of *and*, *or*, and *not* in formal logic are not precisely their ordinary meanings -- which makes one wonder, "Was Ryle a target of Grice's successful attack on such views fourteen years later in *Logic and Conversation*?" No doubt, had I raised the question, Kremer would have seen it as more *Whig history*.

applies to an object iff most community members agree that it does. Recognizing that this is *not* Wittgenstein's view, I indicate that it is unclear what more sophisticated kind of agreement he might appeal to.²⁰ *Simple agreement* about application works pretty well for arithmetical primitives like successor -- thus removing the need to appeal to it for defined terms like 'plus'. For other terms I suggest a more indirect sort of agreement -- like agreement in the way claims are *justified*, rather than agreement in application itself.²¹ But I fault Wittgenstein for not making this clearer, and I conclude "the best that might be said for the thesis [that community agreement grounds meaning] is that it is too vague either to be persuasively defended or conclusively refuted."²² That still seems right. Paul may think I should have filled in the detail that Wittgenstein didn't. But like Wittgenstein, I didn't know how.

Paul next denies that for Wittgenstein language must be communal. I think the issue is more complex. PI 54 *doesn't*, I think, imply that language can be noncommunal, while 202 *suggests* that its communal aspect is fundamental, as does the primitive -- block, pillar, slab -- proto-language of the opening sections, which introduces the larger theme of language as a type of purposeful social activity, rather than simply a system of descriptive representation. This theme is reinforced by the critique (in sections 26-36) of ostensive definition as requiring complex thoughts that could not be had without *already* possessing language.²³ In 27, the block/pillar/slab language-game illustrates how the required antecedent linguistic system might be acquired communally. Since it is hard to see how such a system could be acquired on one's own, it is hard to see how, for Wittgenstein, a solitary language could get started.

²⁰ Pp. 42-3.

²¹ Pp. 43-4.

²² Page 44.

²³ See my discussion pp. 7-8.

The rule-following argument points in the same direction. Just as the determination of linguistic content by contentful rules is defeated by regress, so is its determination by beliefs and intentions – unless the latter are grounded, in the way I indicate, by contentful perceptual states.²⁴ Such grounding -- which would, in principle, make a solitary language possible -- is not mentioned by Wittgenstein. Though the *Investigations* should not, I think, be seen as categorically *denying* the possibility of a solitary language, it does not encourage the thought. What is clear is (i) that for Wittgenstein, language must be at least *potentially* communal, in that the determinates of meaning must be publicly available, and (ii) that since *our* language *is* communal, understanding it requires understanding the social practices that constitute it.

Paul's fourth point accepts my view that Wittgenstein's observations about the use of names can be accommodated by a semantic version of Millianism that recognizes pragmatic enrichments. But while I take this position to be an *extension* with which most, but not all, of the *Investigations* can be reconciled, Paul takes it to be what the text actually says. I think this reading conflicts with the uncompromising rejection, in PI 40, of ever applying the term 'meaning' to the bearer of a name. More generally, I think Paul is too willing to credit Wittgenstein with a nuanced appreciation of types of meaning that required the hard effort of later philosophers to explicate.

Paul's fifth point, is that Wittgenstein was too smart to have adopted an anti-theoretical metaphilosophy inconsistent with his actual philosophy. No, he wasn't. He did just that in the *Tractatus*, and he never outgrew his tendency to *do* philosophy as if it were unspeakably deep, while disparaging its positive fruits as barely worth mentioning. The identification of analyticity, apriority, and necessity was another aspect of the *Tractatus* he never repudiated. He did, of course, repudiate his earlier conception of the role of logic and logical form in philosophy. The

²⁴ Pp. 35-8.

Investigations has nothing but scorn for the idea that these play any part. Thus, Paul's observation that a logical or mathematical discovery can be surprising, even though apriori, has no bearing on how, for Wittgenstein, a *philosophical* discovery could ever give us new information.

His conviction that it couldn't was not based on any argument, let alone the one Paul foists on me. Wittgenstein simply didn't *see* how a philosophical thesis could be informative.²⁵ He took it to be obvious that philosophical problems were not empirical, and so *had* to be linguistic (thereby implicitly reducing the apriori to the analytic).²⁶ Having rejected as irrelevant both analytic truths requiring logical calculation and hidden logical forms, he saw the misuse of words the meanings of which we know perfectly well as the only source of philosophical perplexity. Since this leaves no clear alternative to his radically deflationary metaphilosophy -- which, fortunately for us, was inconsistent with his actual philosophical practice -- we should, I think, take Wittgenstein at his word, rather than burying his contradictions in false homage to his genius.

Paul's last point combines his earlier error about the role of the community in Wittgenstein's theory of meaning with a corresponding error about sensation language. The new error is that, for Wittgenstein, words like "pain" both *designate* private inner states and carry *defeasible* public assertability conditions, in accord with what I call "The Even-Weaker Thesis". Although Paul agrees with me that this thesis is both plausible and Wittgensteinian in spirit, he wrongly takes it to be Wittgenstein's real view, citing *Investigations* 304-7. This is a misreading. The point of 304 is *not* that 'pain' really *does* refer to a private sensation, but rather that we

²⁵ My discussion of this on page 27 puts it in terms of Wittgenstein's unquestioned philosophical presuppositions, which include not just the implicit assimilation of the apriori to the analytic, and the identification of philosophical problems with linguistic ones, but also his new conception of meaning, "with its rejection of abstract logical forms, its deflationary conception of rule-following and algorithmic calculation, and its emphasis on social conditioning as generating agreement in our instinctive applications of words."

²⁶ PI 109, quoted on my page 27.

shouldn't think of it as *failing* to refer, because referring is not its job.²⁷ Ironically, 304 harks back to PI 293 – which makes it crystal clear that sensation words can't be designators.²⁸ In addition, Paul can't, I think, accommodate 258, which talks about the need for public criteria of *correct application*. (PI 265, 268 are also relevant.) Since there is, for Wittgenstein, no meaning without such criteria – which are themselves not determined by defeasible assertability conditions – *they* are what his theory of meaning requires.

Finally, although Paul and I differ substantially about certain central theses of the *Investigations*, we are largely in harmony about the interesting and valuable ideas it provokes -- not a surprising result when one considers how Wittgenstein presented his ideas.

²⁷ PI 305 makes the same point about 'remember'; its job is not to refer to an inner process either. 306 explains why Wittgenstein resists the charge that he denies that there is an inner process of remembering (or being in pain). He does so because, as he tells us, *There has just taken place in me the mental process of remembering ...* "means nothing more than" *I have just remembered ...*, and he clearly doesn't want to deny that anyone ever remembers anything. In effect, he assigns a deflationary, non-ontologically-committing, reading to a sentence (the seemingly existential one) that he thinks philosophers are inclined to take the wrong way. On his view, the way to see through the illusion is to realize that words like 'remember' and 'pain' don't function as designators at all. His point in 307 is that if you think of behaviourism as something issuing in (ordinary, material-mode) denials of the sort that he has just deflated, then of course he is not a behaviourist. However, his position is something like behaviourism in holding that mental words don't have the role of designating that mentalists ascribe to them.

²⁸ "Now someone tells me that *he* knows what pain is only from his own case! – Suppose everyone had a box with something in it: we call it a "beetle". No one can look into anyone else's box, and everyone says he knows what a beetle is only by looking at *his* beetle. – Here it would be quite possible for everyone to have something different in his box. One might even imagine such a thing constantly changing. – But suppose the word "beetle" had a use in these people's language? – If so it would not be used as a name of a thing. The thing in the box has no place in the language-game at all not even as a *something*: for the box might even be empty. – No, one can 'divide through' by the thing in the box; it cancels out, whatever it is. That is to say: if we construe the grammar of the expression of sensation on the model of 'object and designation' the object drops out of consideration as irrelevant." My underlined emphasis.