DISCUSSION

What is history for? Reply to critics of *The Dawn of Analysis*

S. Soames

I will begin by replying to the constructive, substantive criticism of Stoljar and Burgess. After that, I will take up the harsher criticism focused on matters of historical scholarship presented by Proops and Sainsbury. In addition to setting the historical record straight, I will locate the roots of the dispute in a conflict between the history-for-history's-sake conception implicitly advocated by Proops and Sainsbury and my own philosophically and pedagogically motivated approach to the history of the subject. In doing so, I will articulate and defend the goals of philosophically oriented history, and use the resulting conception both to illuminate my discussions of individual philosophers, and to clarify how they contribute to what I take to be the leading achievements of the analytic tradition.

Reply to Stoljar

In chapters 3 and 4, I claim that there is a tension in the moral philosophy of *Principia Ethica* that could have been alleviated, though not fully resolved, had Moore followed what was to become his trademark philosophical method of starting with pre-philosophical certainties about particular cases, and using them to confirm or disconfirm general philosophical principles, rather than going the other way around. Stoljar agrees that there is a philosophical tension in Moore of the sort I point out, but disagrees that it could have been alleviated by following my methodological suggestion. The reason for the disagreement is that he has not correctly characterized the tension in Moore I was talking about.

According to Stoljar, the tension arises from the claim that no moral statement is empirical or analytic, and the claim that since some moral claims are knowable,

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there must be self-evident moral truths that don't require justification, but can be used in justifying other moral truths. He notes that there is a tension here because it is hard to come up with any positive characterization of what the self-evidence of fundamental moral truths amounts to when it is insisted that moral claims are neither empirical nor analytic. Because he characterizes the tension in Moore in this way, he is rightly mystified by the suggestion that it could be alleviated by using self-evident *particular* truths to justify general ethical principles, rather than the other way around. But that isn't the tension I had in mind.

Here is how the tension is described in the section of chapter 3 to which Stoljar refers.

> As I see it, there is no resolution of these issues in Moore himself. We simply have a tension in his philosophy. On the one hand, he is sensitive to the fact that *good* is difficult if not impossible to define, and to the fact that interesting claims about goodness seem, for some reason, to be resistant to proof. On the other hand, he tries to do justice to the further fact that we standardly do take claims about goodness to be the sorts of things that may not only be true or false, but also, in some cases, to be known to be so—a fact that is hard to explain unless some of these claims are self-evident. (67)

Note the two halves of the dilemma: (i) *interesting claims* about goodness (and other moral notions)—of the sort we hope to receive guidance about from ethical theorists—are resistant to proof, and *definitions* of 'good' are difficult to come by, and (ii) some claims about goodness are self-evident, and must be appealed to in justifying other beliefs about goodness (and other moral notions). If the interesting claims about goodness include sweeping claims of the sort that philosophers have traditionally argued about—e.g. 'All (and only) instances of pleasure are good'—and *these* are to be either self-evident or self-evidently obvious consequences of other self-evident moral truths, then it is hard to credit the idea that interesting claims about goodness are resistant to proof. After all if p is self-evident, or self-evidently follows from other things that are, then proofs will be easy to come by. A similar point holds for definitions. One can—as Moore did—impose such narrow requirements on what counts as a definition that no instances of

1. All and only D's are good.

will satisfy them. However, this cuts little philosophical ice, if it is admitted that such instances may nevertheless be necessary, apriori, and self-evident.

As shown in the book, the main problem with Moore's overall meta-ethical argument lies in his uncharacteristically insecure grasp of, and uncritical reliance on, the central methodological concepts he employs—definition, analyticity, and entailment. Briefly put, he tends toward very narrow construals of these notions, on which each of the claims

(i)   that 'good' is indefinable,
(ii) that no instance of (1) is analytic, and
(iii) that no conclusion to the effect that something x is, or is not, good is (analytically) entailed by any descriptive statement ⌜x is D⌝ (for any philosophically interesting replacement of 'D')

is highly plausible. However, this result lacks philosophical punch because it doesn't rule out compelling proofs in which sweeping and highly interesting conclusions about goodness, badness, and the like are derived from purely descriptive premises by steps that are self-evidently truth-preserving.

As indicated on pages 58–62 of my volume, Moore's argument can be improved by putting indefinability aside, and appealing to the notion of an *analytically obvious* sentence as one that expresses a necessary apriori truth that is so obvious that to understand the sentence is to be disposed to accept it. Using this notion, one can construct a plausible Moorean argument that (a) no instance of (1) is either analytically obvious, or provable from analytically obvious premises by analytically obvious steps, and (b) that no broadly interesting moral claim is provable by such steps from premises each of which is either a descriptive truth, establishable by normal empirical means, or a nonempirical but analytically obvious moral truth. This result is potentially interesting—provided that what it is for a philosophical argument to be compelling is for it to be a proof of this sort.

However, this is precisely what is threatened by Moore's free and easy appeal to self-evidence. As indicated on pages 64–66, he takes the most general and fundamental truths of ethics to be not just necessary and knowable apriori, but also to be as self-evidently obvious as those of elementary arithmetic. In so doing, he leaves open the possibility that all interesting truths of ethics can be rigorously proven—as decisively as the theorems of arithmetic—by self-evidently obvious steps from self-evidently obvious premises. This undercuts the very thing that made the Preface and first chapter and *Principia Ethica* so interesting and influential. The idea that Moore attempted to get to the bottom of—that there is something irredeemably contentious and philosophically open-ended about ethics—is powerful. Whatever difficulties may plague his diagnosis of the reality behind this idea, it was the sense that he was onto something that made his meta-ethical analysis so exciting.

That sense can be preserved by adopting my methodological suggestion. According to it, no general ethical truths are self-evident, and no interesting ethical truths—of the sort we hope to learn by studying ethics—can be derived by self-evident steps from self-evident premises (with or without the help of purely descriptive premises established by normal empirical means). Nevertheless, at least some such truths—the justification of which rest, in part, on other, self-evident, moral truths—are capable of being known. We depart from Moore in holding that the self-evident truths are highly restricted claims about particular cases. On this conception, these pretheoretic moral certainties play the role of data points for general moral principles that are supported by something like inference to the best explanation (73–74). The conception is an improvement on Moore in that it would allow him to retain both his strict epistemological distinction between fact and value, and his conviction that general moral principles and interesting moral claims are knowable, even though their truth cannot be established by anything resembling a proof.

The reason this improvement wouldn't *entirely* eliminate the tension in Moore's moral philosophy is that he would still have to either give up his view that no evaluative claims—particular or general—are analytic, or face the task of explaining their self-evidence independent of analyticity. Stoljar is right in thinking that this point of tension—which I also emphasize—would remain. Although this problem is serious, it doesn't conflict with the historically most significant aspect of Moore's meta-ethics in the way that his promiscuous characterization of central tenets of a

correct moral theory as self-evidently establishable does. The avoidance of this conflict is what I had in mind when I suggested that Moore's moral philosophy would have been improved had he incorporated in his moral epistemology and meta-ethics the consequences of adopting in ethics proper what was to become his standard method of using pre-theoretic certainties about particular cases to confirm or disconfirm general philosophical principles, rather than going the other way around.

Reply to Burgess

As usual when I read John Burgess I am instructed and entertained. Who else could weave together Nabokov, Kant, Gauss, Frege, Dummett, Mach, Duhem, Poincare, plus the philosophers covered in my two volumes into such an enjoyable and insightful commentary? Not surprisingly, I agree with much of what he says, and have only a few things with which to take issue.

The omissions Burgess mentions are of two types. The first concerns the lack of references to important precursors to logical positivism. Although these could not have been discussed in any detail without sacrificing pedagogically more central material, they could have been referenced, and will be in future editions. The second concerns the Quine-Carnap debate of the early fifties, in which two different lessons are drawn from the failure of the hardcore versions of positivism discussed in chapter 13. Burgess proposes what would amount to an expansion of Part V to correct this omission. I am convinced, and will try to add a discussion of the debate to future editions. Though it will make the volume too long to be covered in its entirety in many courses, different instructors can make different selections—and the autodidact may benefit from a fuller picture of an important chapter in mid-century analytic philosophy.

As for my discussion of Quine's views of meaning, synonymy, and analyticity, Burgess and I differ regarding our ''sympathy'' for Quine. His first point concerns an example of synonymy-by-stipulation, which I touch on in discussing a strategy for avoiding Quine's circle argument by defining synonymy in terms of substitution in belief, rather than modal, contexts. After pointing out that this strategy for securing synonymy without presupposing necessity would, at best, yield a notion of analyticity too narrow for the philosophical purposes of Quine's dialectical opponents, I conclude:

> If anything like ... [this] position on analyticity is the position to which defenders of analyticity are forced to retreat in the face of Quine's argument, then one must judge his argument to have been substantially successful—even though he may have been wrong in thinking that there is no distinction at all to be drawn between the analytic and the synthetic. (364)

So far, then, Burgess and I *are* on the same page. We diverge when I point out that Quine's reaction to this argumentative strategy does *not* grant even this limited conception of analyticity, but instead implicitly *rejects* the notion of belief used to define it, putting both notions in the same problematic category as necessity and synonymy (Quine, 1970). To reject a notion as incoherent, unscientific, or irredeemably obscure—which Quine does—is quite different from granting its

legitimacy while noting that it can't be used for expansive philosophical purposes. To point this out isn't to lack sympathy; it is simply to refuse to avert one's eyes when Quine goes too far.

Burgess cites other works in which Quine seems to grant stipulated synonymy, while insisting that such synonymies are transitory. Point taken. But once the legitimacy and intelligibility of synonymy, analyticity, and necessity are granted—and only the temporal duration of their extensions is at issue—Quine's criticism becomes very different from the one he standardly gives, and is understood as giving. In addition, the idea that synonymies are invariably short-lived because meanings are in constant flux is tied to contentious versions of holistic verificationism (and in some cases to rampant descriptivism about theoretical and natural kind terms) that are themselves subject to objections at least as powerful as any that Quine brings against his targets. (See chapter 17.) Consequently, one does Quine no favor by promoting interpretations of his positions that lean heavily on such contentious doctrines.

One factor contributing to the desire to construe Quine sympathetically is that although he frequently gives sweeping arguments for radically eliminativist conclusions, he also, when pressed, insists on the right to speak with the vulgar in using the eliminated notions. However, this doesn't take us very far. Since he never explains how his original arguments allow for this, the result often reads like an illegitimate attempt to have it both ways. My interpretive strategy is to take the arguments for which he is famous to be controlling, and to credit subsequent disclaimers only when one can see how the arguments can be made to accommodate them.

Burgess's next point concerns the sense in which Quine *rejects* meaning, synonymy, belief, and the like. He quotes a Quinean remark to the effect that the "rejected" notions are worthy *objects* of philosophical and scientific clarification and analysis, while being illegitimate *instruments* of such clarification and analysis. In my opinion, there is less here than meets Burgess's eye. For Quine, the sense in which these notions are worthy objects of analysis is the sense in which any notions to be eliminated from true descriptions of reality are proper objects of study. We study them to learn how to replace them, and to formulate acceptable replacements that are capable of doing well the theoretical tasks they do badly. For Quine, there is no such thing as meaning, synonymy, reference, belief, or any other propositional attitudes as we ordinarily understand them. Instead, there is stimulus meaning, stimulus synonymy, disquotational reference, and dispositions to verbal and non-verbal behavior. The vast gap between the ordinary notions eliminated and their Quinean replacements does *not* license Quine to speak with the vulgar, no matter how much he may sometimes want to do so. (See chapter 11 of Volume 2.)

What are the "instruments" of philosophical and scientific analysis and clarification that Quine talks about? Roughly put, they are any concepts that can properly be used in stating genuine truths of correct philosophical and scientific theories. Among them, according to Quine, are his behaviorist replacements of ordinary semantic and psychological notions. By contrast, the notions they replace are not (Quine, 1960)[1]. This is something the falsity of which I would expect Burgess to recognize. Just as Burgess insists that mathematicians themselves, rather than philosophical critics with their own agendas, are the proper judges of which are the mathematically legitimate notions, and what are the commitments of mathematical theories, so he should

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[1] See section 45, especially pp. 220–21 (Discussed in my Volume 2, pp. 277–279)

recognize that the same is true of contemporary practitioners of linguistics, psychology, and cognitive science, and their co-workers in philosophical logic, philosophical semantics, and the philosophy of mind. It is meaning, synonymy, reference, linguistic content, mental content, belief, and other propositional attitudes—and not their Quinean substitutes—that are embraced by these practitioners. Although it is conceivable that truly compelling eliminativist arguments might persuade them to give up these notions, Quine hasn't produced any such arguments.

One reason Burgess doesn't see things this way involves a parallel he draws between Quine and the positivists. Just as the positivists rejected certain Heideggarian texts as *cognitively meaningless* while recognizing that not every translation of them is as good as any other, so Quine rejects the meaning not only of Heideggarian but also Fregean texts without committing himself to the absurdity that all translations are equally good. The implicit lesson drawn by Burgess is that since linguistic meaning is what must be preserved by good translations, the positivists must be understood as granting that Heideggarian texts have ordinary linguistic meanings over and above the cognitive meaning they lack, while Quine must be understood as granting that all texts have ordinary linguistic meanings over and above whatever special philosophical type of meaning he rejects. However, this ''lesson'' won't stand. Although the positivists did recognize kinds of linguistic meaning—including ''poetic,'' ''emotive,'' and ''dynamic'' meaning—in addition to cognitive meaning, they never said enough about them, or about translation, to justify the claim that what distinguishes good translations of Heidegger from bad is the preservation of that portion of linguistic meaning that excludes cognitive meaning. For Quine, the point is even clearer. According to his theory of translation, the facts about stimulus meaning that must be preserved in order for a translation to be acceptable fall far short of what anyone would recognize as constituting the linguistic meanings of the sentences being translated. (See chapter 10 of Volume 2.) Beyond this minimal acceptability, it is a practical matter for Quine—independent of the preservation of semantic *facts*—which translations are best. The best are simply the ones we find most useful. In this way, Quine thought he could reject ordinary linguistic meaning—in something approximating the way in which we now reject witches, while continuing to recognize certain witch-like behavior—without throwing out the useful practice of translation. The multiple problems with this view are detailed in Volume 2.

Finally a word about necessity and apriority. Burgess is right in thinking that Kripke's contribution to distinguishing the two, and elucidating the former, exceeds his contribution to elucidating the latter. Burgess is also right to distinguish the roles of necessity and apriority in pre-Kripkean analytic philosophy. As for his different senses of necessity, we may put *gnecessity* aside, and focus on *pnecessity* (analyticity) and *knecessity* (Kripkean counterfactual necessity). There was, in my opinion, a great deal of equivocation and confusion of these two prior to Kripke. Burgess correctly suggests that there is something to be said for Quine's critique of modal logic, understood as a formalization of pnecessity, and for his general skepticism about the philosophical significance of this notion. I agree, that is certainly part of the story. However, it is also part of the story, and ultimately a more important part, that Quine was blind to another, philosophically much more interesting, notion of necessity—knecessity—that was struggling to emerge from pre-Kripkean work in modal logic. I hope to tell both parts of this story in some detail in the third, more technical volume that I have planned.

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Reply to Proops

We now turn to less philosophically, and more historically, focused criticism—which, as I shall argue, raises fundamental questions about the proper aims of work done by philosophers, for philosophers, in the history of their subject. Ian Proops's critique of my treatment of the metaphysics and epistemology of Moore and Russell is of this sort. He beings with what he labels a conundrum—*Did I take myself to be doing history of philosophy, and so to be making claims about what the philosophers said and meant, or was I merely using their writings pedagogically?* Of course, I took myself to be doing both. The fact that Proops sees a chasm between these two aspects of the same enterprise is, in my opinion, indicative of a larger failure of perspective that informs his remarks. My task was to extract from the voluminous writings of the philosophers covered, done throughout their entire careers, those lessons that every analytic philosopher today, and every student, should be aware of. Doing this required making their major achievements and failures as clear and comprehensible as possible, but it did not require—or allow—following every twist and turn in the development of their thoughts. Nor did it mean, in every case, reproducing the exact route to a philosopher's most important results, if simpler and more comprehensible routes were available that preserved the philosopher's central insights.

This is the strategy routinely used to introduce students to historical achievements in logic, like Gödel's incompleteness and Tarski's indefinability theorems. Our confidence in what constitutes progress underwrites our judgments of what was essential and what inessential in the texts of great logicians. The same is true in philosophy. It is because philosophy has progressed, and we know more now, that we can separate the essential from the inessential in presenting the contributions of a philosopher like Russell. That is the spirit in which I approach the task. The opposing spirit denies philosophical progress, or at best adopts a value-neutral stance, and as a result takes the historical enterprise to consist simply in elucidations of all the different strands of past philosophical thought, and identification of lines of influence. There are, of course, other less tendentious routes to this antiquarian conception of the history of philosophy, but they are beside the point. My aim was to present an historically accurate picture of the main lines of progress in the analytic tradition.

Proops's complaint that the formal system I use to explain Russell's logicist reduction differs in certain respects from the system of *Principia Mathematica* should be viewed in this light. My system is a version of naive first-order set theory with identity. Proops objects that the system of *Principia* is not a set theory at all—which is right if one takes the contested view that higher-order logics, though they have set-theoretic semantics, are not to be understood as merely set theories in another form. Although I am sympathetic to this view, it's significance for present purposes should not be overstated. We now know that second-order sentences can be given either a set-theoretic or a plural quantification interpretation. Only the former is relevant to elucidating Russell. Consequently, if asked to state the content of the second-order sentence

2. $\forall x \forall y (x = y \leftrightarrow \forall Z(Zx \leftrightarrow Zy))$

occurring in *Principia*, the right answer would be

3. For all individuals x and y (in the set D of individuals constituting the domain), x is identical with y iff any subset of D in which one is a member is a set in which the other is too.

Given that this is what (2) is used to say, there is considerable justification for rewriting it in a way that makes the statement explicit. To be sure, the decision to do so is connected to certain logical niceties—including reasons for requiring model-theoretic interpretations of second, but not first, order systems to be restricted to those in which the totality of things in the domain constitute a set (proper class) (Boolos, 1975). Although these niceties have some bearing on the question, *Does the system to which arithmetic was reduced properly qualify as logic?*, the relevant considerations are mostly post-Russellian, and the question remains vexed. As for the nuts and bolts of the reduction, and the leading philosophical ideas behind it, the decision to express them in a first-order, rather than a second-order, language makes no significant difference.

I had two reasons for my choice. First, the level of technical complexity in the chapter is already so daunting for many undergraduates that the extra complexity of a second-order system would, I suspect, cause many to fall by the wayside. Second, the main primary source on logicism which I ask students to read, and from which I quote in the chapter, is obviously not *Principia*, but Russell's *Introduction to Mathematical Logic* (Russell, 1993). There, Russell is very sparing with symbolism, and uses the English set-membership predicate again and again in formulating and explaining the logical and arithmetical axioms and definitions. My choice of formalism was intended to mirror what the students would actually be reading. Consequently, Proops's comment that ''a student who turns from Soames's text to Russell's logical writings will simply be lost,'' has it backward. Given the choice of *Introduction*, the first-order notation is appropriate

These considerations answer another Proopian concern—namely that I present 'ϵ' and '=' as primitives, whereas Russell defines them in *Principia*. Since the Russellian definitions are higher-order, *of course*, I took them as a primitive in my first-order reconstruction. Proops also mentions that Russell gives class terms contextual definitions—but so do I (142–143). In short, the substance of Russell's reduction is preserved in my discussion. To be sure, I could—and now think I should—have included a short explanation of how the formal system I use differs from that of *Principia* in several ways that are inessential to our philosophical purposes. But that is all that is required.

Next Proops turns his attention to my chapter on Russell's Theory of Descriptions. Although he has nothing to say about the 90% of the chapter in which I discuss the theory, he does object to an introductory section in which I use an argument involving negative existentials to contrast the ontological extravagance of *Principles of Mathematics* with the parsimony of ''On Denoting'' (Russell, 1903, 1905). Proops objects (i) that my reconstruction of the argument doesn't fit the passages from *Principles*, (ii) that the universe of non-existent entities which, according to *Principles*, ''are'', or ''have being,'' is smaller than I maintain, and (iii) that I wrongly argue that ''the Theory of Descriptions was the means Russell *first* employed to dispense with [these] unreal objects.'' These claims are false.

Regarding (i) and (ii) Proops maintains that *Principles* is committed only to the claim that when A is a *proper name*, ⌜A is⌝ and ⌜A has being⌝ are true if meaningful, and ⌜A is not⌝ and ⌜A lacks being⌝ are false if meaningful. This ontological commitment includes the god Apollo, but not round squares, golden mountains and the

like. For Proops, all Russell had to do to rid himself entirely of unreal objects was to treat names as disguised descriptions—a step which, as Richard Cartwright notes in an article Proops relies on, Russell took in unpublished manuscripts written after *Principles* but before "On Denoting." (Cartwright, 1987). According to Proops, these manuscripts, not "On Denoting," contain Russell's *first* means of disposing of "unreal objects."

The manuscripts do contain attempts to diminish the ontological commitments of *Principles*. However, Proops's claims about *Principles* itself are not born out by the text. First, in *Principles*, Russell never explicitly limits his commitment to "unreal objects" to those that are referents of meaningful *proper names*. Second, as Cartwright points out, the crucial passages do *not* state an explicit argument and conclusion, but leave it unclear whether Russell is claiming (a) that anything that can be *mentioned* must have being (coupled with a liberal but ill-defined conception of what can be mentioned), or (b) that ⌈There is no such thing as A⌉ is false for, as Cartwright puts it, *any singular term* A.[2] Third, despite Proops's unsupported denial, there is evidence that Russell *did* intend A to include descriptive phrases.

Here is how Russell states his conclusion in the two key passages in *Principles* (quoted on page 96 of my volume).

> A man, a moment, a number, a class, a relation, a chimera, or anything else that can be mentioned is sure to be a term [i.e. an entity that "is" or "has being"]; and to deny that *such and such a thing* is a term must always be false. (*Principles* 43, my emphasis)
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> Numbers, the Homeric gods, relations, chimeras, and four-dimensional spaces all have being, for if they were not entities of a kind, we could make no propositions *about* them. Thus being is a general attribute of everything, and to *mention* anything is to show that it is. (*Principles* 449, my emphasis)

The first passage begins by using indefinite descriptions to formulate positive statements of being that must be accepted, and ends by confirming that A in Russell's ontological thesis can be of the form *such and such a thing*. Since this phrase can trivially be paraphrased *a thing which is such and such*, it is strongly suggested that A can be an indefinite description (and, in all likelihood, a definite description as well). The second passage uses plural noun phrases to give additional examples of things that must be admitted to have being—only one of which, the Homeric gods, are things that all have names. As for chimeras and four-dimensional spaces, I am not aware of Russell naming any. Nevertheless, he takes it to be evident that his argument applies to them. The reason for this, I suggest, is that it was not just P1, but also P2, that was driving, or at least tempting, him at the time.

P1. In order for ⌈A is not⌉ to be meaningful, the meaning of the proper name A—which is also A's referent—must have being.

P2. In order for ⌈A is not⌉ to be meaningful, and hence to express a proposition p, p must *be about* something—the thing *mentioned* in asserting p.[3]

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[2] Cartwright, p. 98.

[3] In section 46, p. 43, of *Principles* Russell says, "In every proposition ... we may make an analysis into something asserted and something about which the assertion is made."

Since *Principles* provided denotations for the denoting concepts expressed by plural nouns and descriptive noun phrases—which propositions expressed by sentences containing these expressions were taken to be about—the range of 'A' in P2 includes such phrases.

For these reasons, Proops's minimalist construal of the ontological commitments of *Principles* and his criticism of my more expansive interpretation are unfounded. It is true, as he points out, that the expansive interpretation renders *Principles* inconsistent, since in section 73 Russell denies that there is such a thing as the null class. However, no viable interpretation of *Principles* saves Russell from such inconsistencies, for he also denies in section 73 (pp. 73–74) that there are chimeras, and implies that 'x is a chimera' is false for all arguments. Yet we know that Russell *explicitly* characterizes chimeras as entities the being of which *cannot* be denied in the key passages in which he gives the argument for "unreal objects."

In point of fact, *Principles*, for all its interest and energy, is a vast, unwieldy, and immature work in which Russell's reach exceeded his grasp, and he struggled with a ideas not yet under control. Consequently, it was only natural that in the next few years, he should continue to modify and improve the system with an eye to eliminating difficulties, and reducing its ontological commitments. As Cartwright shows, he was making headway, finally reaching a point in his unpublished work in which he was in a position to renounce "unreal entities" altogether. However, when he discovered the Theory of Descriptions in June of 1905, he abandoned these patchwork attempts as misguided. From then on, he came to view his new theory as releasing him both from his earlier need to distinguish being from existence, and from his system of denoting concepts and their elaborate, but obscure and tangled web of logically complex denotations.[4]

This, in broad outline, is how the Theory of Descriptions is introduced in my chapter. Neither the elaborate system of denoting concepts of *Principles* nor Russell's unpublished modifications of it are discussed because they did not constitute lasting achievements. On page 95, I credit the Theory of Descriptions with allowing Russell to take the first step—eliminating commitment to (unreal) objects of thought—in a journey of ontological reduction which would later include several more steps. The main steps discussed in the book are (i), the elimination of "unreal objects" (done by the Theory of Descriptions, discussed in chapter 5), (ii), the elimination of numbers over and above sets (accomplished by the logicist reduction, discussed in chapter 6), and (iii), the elimination of material objects (attempted in *Our Knowledge of the External World*, discussed in chapter 7). In characterizing the role of the Theory of Descriptions in Russell's ontological development in this way, I was *not* saying what Proops uncharitably takes me to be saying—namely, that Russell had no intermediate, or unpublished, thoughts about other ways of taking step (i). That would be absurd. Rather, I was claiming that the theory of descriptions is what, when all was said and done, he considered to be the *best* way of taking that step.

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[4] The defects of that system in comparison with "On Denoting" involved (i) the lack of the latter's useful scope distinctions, (ii) the mysterious logically complex "objects" (corresponding to different "combinations" of individual F's) that (a) were supposed to be the denotations of the concepts expressed by denoting phrases *every/any/some/a/the F* and (b) that were originally thought to be what the propositions expressed by sentences containing those phrases were about, and (iii), in Russell's mind, the problem posed by his Gray's Elegy argument.

Proops's failure to see this is, I believe, connected to his larger failure to appreciate what I was trying to do. To reiterate, analytic philosophers—including historians—need to do more than investigate highly specialized topics in finer and finer detail. In addition, we should try to construct larger, synthetic pictures which, while informed by precise and detailed understandings of particular issues, transcend that. In the history of philosophy, this means going beyond the antiquarian study of minor works, unpublished manuscripts, and private correspondence—as valuable as they may be for specialized purposes—and attempting to develop a broad and useable picture of where we are now and how we got there. In philosophy, the history of the subject may well be of greater relevance to its contemporary practice than it is in many other fields. Still, as in any discipline, if progress is to be made, there must at some point emerge a clear demarcation between genuine accomplishments that need to be assimilated by later practitioners, and other work that can be forgotten, disregarded, or left to those whose interest is not in the subject itself, but in history for its own sake. The aim of my volumes was to contribute to making that demarcation.

Finally, a word about Proops's objections to my reconstruction of the argument from negative existentials. My reconstruction gives three examples.

3. Santa Claus doesn't exist.
4. The creature from the black lagoon doesn't exist.
5. Carnivorous cows don't exist.

Proops objects (i) that these speak of existence rather than being, (ii) that (5) contains a plural noun phrase, and (iii) that the argument's intermediate conclusion,

6. There are no true, meaningful negative existentials.
   is immediately followed by the self-refuting conclusion
7. True, meaningful negative existentials don't exist.

Proops objects that (i–iii) are not faithful to the explicit passages in *Principles* in which Russell gestures at an argument. That is true, as far as it goes. However, the variations are obvious to the reader, since the key passages are printed right next to the reconstructed argument. Moreover, there were sound textual, pedagogical and philosophical reasons for formulating the argument as I did.

First, I wanted an argument for an unacceptably expansionist ontological conclusion that could be rejected either by distinguishing being from existence (as in *Principles*) or by distinguishing logical from grammatical form (as in ''On Denoting''). Second, I wanted the ontological conclusion to have the expansiveness of Russell's remarks in the passages quoted, while arising from negative existentials of various grammatical types—all of which would later be analyzed as incomplete symbols by his Theory of Descriptions. Example (5), involving 'carnivorous cows', fits these desiderata. Since the ''On Denoting'' analysis of (5) is simpler than the analysis of the example (4) containing the definite description, and the example (3) which is assimilated by Russell to an example containing a description, I use (5) as an introduction, before tackling those more complicated analyses. As for *Principles*, the passages quoted suggest that Russell would have denied the existence of carnivorous cows, while affirming their being, on a par with chimeras. Moreover, in section 56, he gives an analysis of the grammatically singular (8a) in which it is understood in terms of the plural (8b)—which provides a clear indication of how he would have analyzed (9a) and (9b).

8a. Man is moral.
8b. Men are mortal.
9a. Carnivorous cows exist.
9b. Carnivorous cows are—i.e. have being.

In (8), the grammatical subject, whether singular or plural, is said to express a denoting concept, the denotation of which is supposed to be a logically complex object which is a kind of conjunctive combination containing each individual man. This object, though admittedly mysterious, is taken to be what the proposition is about. The proposition is true iff each individual man combined in the conjunctive denotation has the property of being mortal. Applying this analysis to (9), we get the result that (9b) is true and (9a) is false—if, as Russell seems to suggest in the quoted passages, carnivorous cows, like chimeras, have being (but not existence). Thus, contrary to Proops, the use of plural noun phrase subjects in negative existentials does *not* take us beyond what we are given in *Principles*.

Last, I wanted to make it clear that the argument from negative existentials, understood in this naturally expansive way, is self-refuting (and would remain so even if denials of existence were replaced with denials of being). This was the reason for including the paradoxical conclusion (7), despite the fact that Russell did not. I suspect that at the time he hadn't formulated his argument precisely enough to see the danger of self-refutation—otherwise he would have taken care to avoid the expansive language he actually used.[5] In any case, I wanted the students to see that the ontological position suggested in *Principles* is untenable. Although different modifications of it are certainly capable of improving it—including those explored by Russell in his unpublished manuscripts—the replacement that every student needs to know is the Theory of Descriptions.

Since my reply to Proops is already too long, I will limit my response to his discussion of Moore to one point of agreement and one point of defense. I agree with Proops that two of Moore's important purposes in ''Proof of an External World'' were (i) to demystify the notions of ''the external world'' and ''things outside of us'' and (ii) to extend the common sense conception of the world presented in ''Defense of Common Sense'' to include the claim that we can *prove* the external world to exist (by acceptable common-sense standards). (Moore, 1925, 1939) However, I continue to maintain that an important part of the *philosophical point* of the work, in addition to (i) and (ii), is that *seeing Moore's argument as a proof worth giving* involves seeing why the sorts of proofs normally attempted by philosophers—those that even skeptics would be forced to accept—are illegitimate. For Moore, this involves implicit appeal to his ''argument from differential certainty.'' In making this claim about the *philosophical point* of the work, I am making no psychological claim about where Moore would have ranked this point in the inventory of goals he was consciously trying to achieve in writing the lecture. Rather, I am indicating the centrality of the point to our evaluation of the significance of the work as a contribution to the analytic tradition. Again, the focus is not just on what thoughts were in the philosopher's mind, and where they came from, but on what was achieved, and why it was significant.

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[5] This conclusion is in line with Cartwright's discussion on pages 98–99, where he points out the self-refuting nature of an expansive understanding of the argument in *Principles*.

Reply to Sainsbury

Mark Sainsbury's criticisms, even more than those of Proops, raise issues that go to the heart of the historical enterprise. His specific complaints can be grouped under four headings. First, he deplores the lack of scholarly references to standard histories, alternative interpretations, and other secondary sources. Second, he takes issue with the two threads I identify as main themes running through the analytic tradition that encompass its most important overall achievements. Third, he disagrees with what he takes to be my attitude toward the history of philosophy. Fourth, he takes issue with several points of my interpretation of Russell. I will take these up in turn.

I begin with a point of almost complete agreement.[6] The ''Additional Recommended Readings'' sections at the ends of the five Parts of Volume 1 are indeed too sparse, and will be substantially enlarged along the lines Sainsbury suggests in later editions. Already in Volume 2 the lists are more substantial, in part because I changed my policy for including items on them as the project wore on. Throughout Volume 1, I included only those works that had been explicitly mentioned or footnoted in the text, whereas in Volume 2 I included other material that I thought was relevant and important. Even there, however, my selections were unsystematic, and other histories, alternative interpretations and some standard secondary works were often left out. In future editions they will be added—including Sainsbury's own *Russell*, which he was too discreet to mention (Sainsbury, 1979).

Sainsbury's second criticism is directed at the second paragraph of my introduction.

> To my mind the two most important achievements that have emerged from the analytic tradition in this period are (i) the recognition that philosophical speculation must be grounded in pre-philosophical thought, and (ii) the success achieved in understanding, and separating one from another, the fundamental methodological notions of logical consequence, logical truth, necessary truth, and apriori truth. Regarding the former, one of the recurring themes ... has been the realization that no matter how attractive a philosophical theory might be in the abstract, it can never be more securely supported than the great mass of ordinary, pre-philosophical convictions arising from common sense, science and other areas of inquiry ... All philosophical theories are, to some extent, tested and constrained by such convictions, and no viable theory can overturn them wholesale....Regarding (ii), no philosophical advance of the 20th century is more significant, more far-reaching, and destined to be more long-lasting than the success achieved in distinguishing logical consequence, logical truth, necessary truth, and apriori truth from one another, and in understanding the special character of each. (xi–xii)

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[6] The agreement is only ''almost'' complete because Sainsbury's discussion may give the misleading impression that the ''additional recommended reading'' at the end of each of the five major Parts of the volume is the only extra reading I list. In fact each Part ends with a section entitled ''Suggested Further Reading,'' which is divided into three sections: ''Main Primary Sources Discussed,'' ''Additional Primary Sources,'' and ''Additional Recommended Reading.'' For example, the readings at the end of Part 2, on Russell, include not just the two articles by Salmon listed under ''Additional Recommended Reading,'' but also an article by Moore, as well as Hempel's later, and very well-known, interpretive article on logicism, ''On the Nature of Mathematical Truth,'' listed under ''Additional Primary Sources.''

Although Sainsbury raises no objection to (ii), he evinces puzzlement and exasperation over (i), while seeming to indulge in a dose of dismissive skepticism about providing ''a story line for the philosophical developments under review''—which is how he characterizes my selection of the two themes. The characterization is quite wrong. I wasn't looking for a story line. Rather, I was expressing my view of the two most significant dimensions along which important achievements have been made in the tradition as a whole. Since Sainsbury doesn't cite any competitors to (i) and (ii), I suspect that he thinks that the whole idea of trying to identify long-lasting trends in the tradition, and to indicate which have been responsible for the greatest progress, is mistaken. But why should that be? If, as I believe, there has been philosophical progress, then unless the important achievements have been isolated singularities, it is natural to look for patterns or trends connecting them that can be identified and assessed. The idea that there have been no general trends in analytic philosophy, or that they have all been of equal value is not, to my mind, very plausible.

Perhaps Sainsbury's skepticism is not that radical. At one point he asks, ''Can these threads [(i) and (ii)] adequately indicate the overall shape of philosophical development in the period?'' Of course not. To say that (i) and (ii) represent the most important lines of achievement is *not* to say that they represent the *only* lines of achievement, or that together they ''indicate the overall shape of philosophical development in the period.'' In the introduction, I mention additional lines of specific achievement such as those in the philosophy of logic of language, which have led to advances in logic and linguistics proper. I also discuss analytic philosophers' characteristic commitment to clarity, rigor and argumentation, their characteristic goal of providing truth and knowledge as opposed to moral or spiritual improvement, and their characteristic openness not simply to constructing philosophical systems—which they sometimes do—but also to more piecemeal investigations of individual philosophical problems outside the confines of any system. All of these—and much more—contribute to ''the overall shape of philosophical development in the period.''

My point in identifying (i) and (ii) as lines of achievement was *not* to define the tradition in terms of them, or to use them as a template for understanding and evaluating every piece of work in the period. On the contrary. The point of the introductory overview was to resist *any* reductive characterization of the analytic tradition—in terms of its methods, its doctrines, or its accomplishments. That is why I end up characterizing it as primarily ''a trail of influence.'' (xiii) However, even a tradition with no fixed doctrines or methodology, in which philosophers read and interact with each other, and influence those who follow them, sometimes yields valuable results that form patterns and fit certain themes. In my opinion, none of the patterns and themes one finds running throughout the analytic tradition are more important than (i) and (ii).

As for Sainsbury's complaints about (i), I believe they stem from serious misunderstandings. Most importantly, the contrast in (i) is between philosophical speculation and our deepest *pre-philosophical* convictions—not just of common sense, but also of science, and other areas of inquiry (like history, or even geography in the days of Magellan). Science routinely *extends* common sense, and in certain cases, when the evidence is strong enough, science *modifies* and *reforms* common sense. Developments in philosophy, particularly when they are continuous with science, can do the same. Two of the examples on which Sainsbury places the most emphasis—Russell and the logical positivists—provide cases in point. For example,

although the Theory of Descriptions was motivated in part by ontological concerns, its ability to solve the "logical puzzles" in "On Denoting" provided solid empirical evidence that it captured something important about the way we understand sentences containing descriptions, and the theory itself served as one of the models for the scientific study of language and mind that eventually blossomed into modern linguistics and cognitive science. Similar remarks apply to Russell's work in logic, the philosophy of logic, and the foundations of mathematics. To take one case, even though the philosophical motivations behind the logicist reduction fell well short of being completely fulfilled, the logical and mathematical achievement remains. For reasons like this, most of Russell's genuine philosophical accomplishments fall within the scope of (i).

Although the accomplishments of the positivists were—how should I put it—not of the same magnitude, what they attempted, and what is to be learned from their attempt, make sense from the perspective of (i). The positivists' respect for a certain kind of pre-philosophical thought, namely science, led to an interesting question. Could it be that the stunning progress in natural science, in contrast with the lack of progress in metaphysics, is due to the fact that the sentences in scientific theories possess the kind of meaning needed in descriptions of the world, whereas those in metaphysical theories do not? The idea that the answer to this question might be "Yes," that we might be able to precisely characterize the kind of meaning required, and that this might lead to substantial reform of some of our common-sense views of language and the world falls within the scope of (i)—as does the lesson learned from the failure of verificationism due to its massive conflict with our pre-theoretic convictions, including its failure to encompass the meaningfulness of science itself, without sanctioning what we pre-theoretically recognize to be patent nonsense.

Once these examples are understood, the import of (i) and the way in which it is relevant to thinking about the philosophers discussed in my volumes, should be clearer. The achievements—such as they are—to be found in Moore, Ross, the later Wittgenstein, the ordinary language philosophers, Grice, and Kripke are clearly encompassed by (i). Most of Russell's accomplishments, some of the positivists', some of Quine's (e.g. his insistence on philosophy as continuous of with science and his rejection of an expansive and philosophically contentious notion of analyticity), and central aspects of Davidson (including whatever there is to be learned from his proposals about empirical theories of meaning) are too. Wittgenstein's *Tractatus* is an outlier, and Russell's attempt to reduce material objects to the sensory contents of what he called "perspectives," Ayer's doctrines that other people are logical constructions out of their bodies and that bodies are logical constructions out of sense data, plus Quine's holistic verificationism, indeterminacy of translation, and inscrutability of reference are, in my opinion, failures from the perspective of (i), because they are instances of apriori philosophical speculation that, in the end, are unsupported by, and indeed in conflict with, science and common sense.

Sainsbury's third criticism concerns what he takes to be my "attitude" to the history of philosophy. Here are two noteworthy examples:

> Soames's attitude to history is revealed in a remark which closes the discussion of the criterion of significance:
> "A few attempts were made to reformulate Ayer's criterion to save it from objections like the ones just considered. However, none proved successful."

The student is not told where to look for these few attempts, and the judgment that none proved successful seems unduly dismissive of relatively recent discussion. [The passage goes on to list several recent discussions of confirmation and verification that Sainsbury believes should have been discussed.]
Russell's logicism is presented as involving a simple theory of types, despite the fact that Russell explicitly rejected any such theory; no mention is made of the ramified theory of types, or of Russell's argument that a single hierarchy was needed to deal with both class-theoretic and liar-type paradoxes. Soames does indeed say that he is providing a "simplified sketch" (152) and that we can "ignore most of the complications" (154). *We can do this only if we do not mind remaining ignorant of the history of the period, and Soames makes it plain that he does not mind. For example, at one point he says that he wants to address "a composite, hypothetical philosopher—the classical logicist—rather than the views of any one person" (135). Quite so; and quite proper, in a book that does not aspire to tell us the true story, and is happy to ignore the often quirky, inconstant and awkward details of the views real philosophers have held.* (my emphasis)

There are two issues here, one minor and one major. The minor issue is that my discussions of verificationism and logicism would have been improved by the addition of a few more passages explicitly indicating (without explaining in any detail) the simplifications being made, a few more references to other philosophers, and a few more recommended readings. I will try to make such additions to future editions. However, the major issue cannot be accommodated. Sainsbury makes it clear that the only discussion of logicism that would satisfy him is one that deals with the subject in its full complexity—the ramified rather than the simple theory of types, the discussion of Russell's views about the relationship between his paradox about classes and the liar paradox plus the role of the theory of types in providing solutions, Russell's "no-class theory of classes" (Sainsbury's item (3)), and the examination of all the "quirky, inconstant and awkward details" of the views of different logicists. This strikes me as unrealistic. One who tried to follow this advice would end up with a work on logicism the size of my entire first volume which, in all likelihood, would be both inaccessible to non-specialists and subject to criticisms similar to those that Sainsbury levels against me—made by those who think that bringing in the liar paradox, for example, requires a thorough discussion of the theories not only of Russell, but also of Tarski, Kripke, Gupta, Parsons, Burge, and others.

Similar remarks apply to Sainsbury's criticism of my treatment of verificationism. It is worth noting that the passage from my discussion he quotes—regarding attempts to reformulate Ayer's final criterion—comes after I have explicitly stated and refuted *five* formulations of the verifiability criterion, and before I examine and criticize both a sixth, more expansive criterion due to Carnap, and Quine's later holistic verificationism. That is already an extensive discussion. Could there have been more about verification and confirmation? Sure. But to follow the logic of Sainsbury's prescriptions, not just for logicism and verificationism, but for all the topics in my volumes would be to end up with a twenty-volume reference work beyond the capacity of any one person to write, or of most professional philosophers to read. Students? Forget it.

The lesson of this reductio is that the real message of Sainsbury's criticism of my "attitude" toward history is: How can you call your work "history"? The only work worthy of that name is history for history's sake—which examines all the quirky, inconsistent, and awkward details of the thoughts of past philosophers in their full complexity, and traces how those thoughts developed. I disagree. There are many kinds of historical writing, and I don't denigrate the kind that Sainsbury obviously prizes. However, in my view the most important kind of history for philosophers is the kind that makes a statement about what constitutes philosophical progress, that views past philosophers in light of what they have contributed to that progress, that distinguishes the essential contributions of their work from the rest, and that tries to weave those contributions into a coherent and useable picture of the development of the discipline that is, with a little work, accessible to the uninitiated. I have no illusions that I have fully lived up to that goal, and would be satisfied if I had made a good start. However, I do not apologize for the attempt.

Sainsbury's final criticism has to do with the details of my chapters on Russell. Of the seven numbered points he raises, (1) is covered by my reply to Proops, while (2) and (3) are covered by the remarks just made about logicism. His point (4) concerns my account of structured propositions in the chapter on the Theory of Descriptions, in which '&' is treated as contributing a truth-functional operator to propositions expressed by sentences containing it. Unaccountably, Sainsbury has me indicating the time frame for this treatment of '&' as "approximately the time of *Principia Mathematica*." He then complains: *The truth emerges later: according to Russell " '&' doesn't stand for anything in the world" (187).* I find this criticism bizarre. The quote from page 187 of my chapter 8—in which I describe Russell as holding that " '&' doesn't stand for anything in the world"—is an explication of his position in *The Philosophy of Logical Atomism*, published in 1918 (Russell, 1985). The earlier treatment of '&' as contributing an entity to structured proposition, given in chapter 5, is a report of Russell's view at the time of "On Denoting," 1905. The view of structured propositions he held then was a holdover from his 1903 view in *Principles of Mathematics*—a view which requires propositions to contain constituents corresponding to the meanings of at least some truth-functionally complete set of logical connectives.[7] The reason for the disparity noted by Sainsbury is that Russell changed his mind between 1905 and 1918. Note also that the early view was that '&' contributes an entity to *propositions*—thought of as objects of belief and other attitudes—whereas the later denial that '&' stands for anything in the world was the view that '&' doesn't contribute anything to *facts* in the world. Between the two dates Russell abandoned propositions altogether, and in *The Philosophy of Logical Atomism* he offered his "multiple relation" theory of attitude ascriptions. Contrary to Sainsbury, there is no 'distortion'' of Russell's views here.

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[7] Russell is not terribly explicit about this, even in 1903. See, however, page 42 in which he writes "it must be admitted, I think, that every word in a sentence must have some meaning...The correctness of our philosophical analysis of a proposition may therefore usefully be checked by the exercise of assigning the meaning of each word in the sentence expressing the proposition." For Russell at this time, the meaning of a word was a non-linguistic entity associated with it. Since connectives like '&' and '~' have definite meanings that contribute to the propositions expressed by sentences containing them, the *Principles'* picture requires them to express propositional constituents. For a revealing discussion of Russell's later (1931) perspective on this and related aspects of his account in *Principles*, see the Introduction to the Second Edition of that work, especially pages x–xii.

Sainsbury's point (5) complains that I include "minds among the atomic elements of reality in Russell's atomism with no mention of Russell's many hesitant discussions of neutral monism." Sainsbury here fails to point out that the texts being commented on are *Our Knowledge of The External World*, and *The Philosophy of Logical Atomism*, (Russell, 1914, 1918). In the former, Russell explicitly argues for minds.[8] In the latter, he assumes the existence of minds, while entertaining at two spots the possibility of eliminating this assumption by adopting neutral monism.[9] While expressing sympathy with such a view, Russell indicates that there are serious difficulties with it that he sees no way around, and that he does not feel that the view is tenable.[10] Although evidently not wanting to foreclose the possibility of later changing his mind, he develops his (multiple-relation) view of belief and desire on the assumption that there are minds, and that neutral monism is false.[11]

So, in the texts in question, Russell *does*, as I indicate, countenance minds as really existing atomic elements of reality. Again, there is no "distortion" of his view. As before, the demand that his hesitations be discussed, that neutral monism be explained, and that the chronology of his evolving views about mind and matter be registered is a demand which—if followed regarding every hesitation on matters about which Russell was prone to changing his mind—would turn my four chapters on him into twelve, and make any historical volume of the type I intended impossible. In light of the unsatisfiability of this demand, I believe that Sainsbury's criticism on this and related points should be seen for what it really is: an attack—masquerading as a defense of standards of historical scholarship—on my departure from the sacrosanct history-for-history's-sake conception in favor of a more philosophically and pedagogically driven approach to the history of philosophy.

Sainsbury's final points—his (6) and (7)—both center on Russell's view of material objects as logical constructions, defended in *Our Knowledge of the External World*. Sainsbury claims

 (i)   that for Russell material objects were *not* constructions out of sense data,
 (ii) that they were logical constructions out of perspectives, which "unlike sense data may exist unperceived," and
 (iii) that it is clear that Russell's logical constructions did *not* require counterfactuals.

Although these claims are false, there are genuine questions about what Russell really did mean, which is why I qualified my discussion in chapter 7 as follows:

> Before going further, let me make a comment about my exposition of Russell regarding the analysis of material object statements ... In giving the analysis I have made use of what are called *counterfactual conditionals*—statements of the sort *if such and such were to occur, then so and so would occur*.... [the relevant examples] are counterfactual statements about sense data. Sometimes, in giving his analyses, Russell uses counterfactuals of essentially this sort. [p. 88] However, at other times he speaks of a system of private perspectives, or

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[8] Pp. 90, 102–104 of *Knowledge*, discussed on pages 176–180 of my volume.

[9] *The Philosophy of Logical Atomism* pp. 85–87, 150–153.

[10] *The Philosophy of Logical Atomism* p. 85.

[11] *The Philosophy of Logical Atomism* p. 87.

points of view. Each such perspective consists of a set of appearances, or sense data—essentially the appearances an observer **would** experience if he occupied that perspective. (Note the counterfactual locution.) Material objects are then said to be *logical constructions* out of certain similar, or related appearances (sense data) given in different perspectives. [94–100] For our purposes, the details of this construction are not important. What is important is that the analysis of material object statements I have given in terms of categorical statements about sense data ... plus counterfactual statements about sense data ... is in all essential respects equivalent to the analysis of material object statements in terms of statements about the sense data found in perspectives occupied by actual observers plus statements about the sense data found in perspectives that no one actually occupies (at least at the moment), but which **could** be occupied if certain conditions were fulfilled. Since the two styles of analysis are equivalent, I will stick with analyses ... that are formulated using explicitly counterfactual language. (170–171)

The chief question of Russell-interpretation that I used this paragraph to avoid is the question whether for the Russell of *Our Knowledge*, (a) there exist unperceived appearances—i.e. sense data—in unoccupied but really existing "ideal" perspectives that *would* be perceived if an actual observer *were* to satisfy certain conditions, thereby occupying that perspective, or (b) there are no unperceived appearances—i.e. no unperceived sense data—in merely ideal perspectives, but there *would* be appearances—perceived sense data—in a really existing, occupied perspectives, if agents were to satisfy certain conditions. Since I didn't regard a detailed discussion of the textual status of this question to be essential to the main philosophical issues in the chapter, I resolved the issue in favor of (b)—which requires counterfactual statements about sense data to occur in the analysis of material-object sentences. In my opinion, (b) is both the most plausible position, and the one that Russell is most in sympathy with—despite the fact that when first setting out his system of perspectives at the end of chapter 3, he speaks as if he favored (a).

Here is how things are set out there.[12] The things seen, touched, or otherwise sensed are *appearances*—which are also called "sense data" and "sensible objects". These appearances are *not* assumed to be appearances of any *things-in-themselves* that causally interact with observers to cause the appearances. On the contrary, there are no such things. Sensible objects are private in that no sensible object is ever seen or sensed by two minds simultaneously. Private perspectives are scenarios or systems of sense data capable of being sensed by a single mind at a given time. Whatever totality is actually sensed by a mind at a given time is a "perceived perspective" the sensible objects in which have all and only the sensible properties they appear to have. In addition to "perceived perspectives" made up of perceived sense data, there are also "unperceived perspectives"—unperceived scenarios made up of unsensed sense data. Material objects are collections of correlated *perceived and unperceived sense data or sensible objects* in neighboring perspectives. From this we can see that Sainsbury's (i) and (ii) are incorrect. For Russell, material objects are logical constructions out of sense data in different perspectives, not the perspectives themselves. However, at this point in Russell's discussion, it would appear that Sainsbury's (iii) is born out, since if material objects are collections of perceived and

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[12] Pp. 90–97 of *Our Knowledge of the External World*.

unperceived sense data, then the statements providing the analysis of material-object sentences can dispense with counterfactuals by referring to unperceived sense data in merely ideal perspectives.

However, things are not so simple. Russell sets up his system of perspectives after illustrating his idea that—"without assuming anything beyond the existence of sensible objects at the times when they are sensible" (p. 88)—what we really know when we use a material object statement is what perceptions we would have if certain conditions were fulfilled. When he sets out making this idea more precise and systematic, he adopts a Leibnizian hypothesis which he introduces as follows:

> We will now make a new start, adopting a different method. Instead of inquiring what is the minimum of assumption by which we can explain the world of sense, we will, in order to have a model hypothesis as a help for the imagination, construct one possible (not necessary) explanation of the facts. *It may perhaps then be possible to pare away what is superfluous in our hypothesis, leaving a residue which may be regarded as the abstract answer to our question.* (my emphasis, p. 94)

What follows is his system of actual and ideal perspectives, modeled on Leibniz's monadology.

The point to notice is that in setting up the hypothesis, Russell leaves himself an out. After the systematization has been sketched, he may pare back some of the assumptions about perspectives, should they prove to be unnecessary. This is precisely what happens in the next chapter, on pp. 116–117.

> If physics is to consist wholly of propositions known to be true, or at least capable of being proved or disproved, the three kinds of hypothetical entities we have just enumerated [namely, "(α) how things *would* appear to a spectator in a place where, as it happens there is no spectator; (β) how things *would* appear at time when, in fact, they are not appearing to anyone; and (χ) things which never appear at all"] *must all be capable of being exhibited as logical functions of sense-data.* In order to show how this might possibly be done, let us recall the hypothetical Leibnizian universe of Lecture III. In that universe, we have a number of perspectives, two of which never had any entity in common, but often contained entities which could be sufficiently correlated to be regarded as belonging to the same thing. *We will call one of these an "actual" private world, when there is an actual spectator to which it appears, and "ideal" when it is merely constructed on principles of continuity.* A physical thing consists, at each instant, of the whole set of its aspects at that instant, in all the different worlds; thus a momentary state of a thing is a whole set of aspects. *An "ideal" appearance will be an aspect merely calculated, but not actually perceived by any spectator.* An "ideal" state of a thing will be a state at a moment when all its appearances are ideal. An ideal thing will be one whose states at all times are ideal. *Ideal appearances, states, and things, since they are calculated, must be functions of actual appearances, states, and things; in fact, ultimately, they must be functions of actual appearances* [i.e. perceived sense data]. *Thus it is unnecessary, for the enunciation of the laws of physics, to assign any reality to ideal elements: it is enough to accept them as logical constructions, provided we have means of knowing how to determine when they become actual.* (my emphasis)

Here, Russell withdraws his commitment to the reality of unperceived appearances (sense data) and unperceived perspectives, further undermining Sainsbury's (ii). This means that the analysis of material-object statements cannot make reference to such things, thereby eliminating the ground for Sainsbury's (iii). Although Russell is not precise about how such analyses are to be given, the most promising approach is to employ counterfactual statements about what *would* be observed if certain conditions *were* fulfilled. Since, Russell himself uses such statements in explaining his view, the best interpretation of his somewhat wobbly position on this point is the one I attributed to him in chapter 7. That is the scholarship behind my attribution. Sainsbury not withstanding, it didn't belong in the text.

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