

Reply to Critics of *The Analytic Tradition in Philosophy* Vol. 1
The Founding Giants
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Reply to Beaney: The Closing of the Historical Mind

In his comments, Michael Beaney sets himself up as the arbiter of what is genuine history and what isn't. While celebrating the outpouring of specialized scholarship on Frege, he has no patience with the enterprise outlined in the *Précis*, which attempts to construct a large-scale picture of the overall richness of the analytic tradition. That enterprise is one in which great figures of our recent past are challenged by aspects of contemporary thought, and our current struggles are enriched by insights of theirs that haven't been fully utilized. Although some work of this sort can be done piecemeal, one historical figure at a time, there is much to be learned from a more encompassing perspective. While no picture constructed by a single author can be authoritative about everything, the attempt to give informative, connected assessments of the major milestones in the tradition is our best hope of understanding who we are, where we have come from, and where we are going.

Beaney couldn't agree less. A work covering the major figures in a period of a hundred-plus years must be historically informed about each, while bringing each into fruitful contact with current concerns. But it can't discuss large numbers in the armies of specialized scholars devoted to each figure. To demand otherwise is to reject the enterprise I envision, presumably on the ground that, not being focused on the output of specialized armies, it can't really be history. But without showing that I get crucial points about Frege wrong, this is just narrow-minded negativity. Most of Beaney's criticism is of another sort.

Volume 1 is criticized for not starting with a summary of the analytic tradition, its historical antecedents, and its relation to other traditions. Surely that's not the only reasonable plan. I favor letting the tradition emerge gradually in the volumes before generalizing about it, providing context for specific milestones when needed. The treatment of logical positivism in Volume 2 is an example. This movement—which announced its existence in a 1929 manifesto stressing the need for an encompassing scientific world view—requires extensive introduction. It is given in a long chapter including (i) *the Origins of the Vienna Circle*, (ii) *the Scientific Positivism of Comte and Mach*, (iii) *Developments in Science, Logic, and Mathematics: Hilbert, Poincare, Duhem, Einstein*, (iv) *Schlick's Early (1915-1918) Epistemology and Philosophy of Science*, (v) *The Kantian Legacy: Continuity and Reaction*, and (vi) *The Impact of Wittgenstein*.¹ All this precedes the first chapter devoted to a major logical positivist work (the *Aufbau*). Later a longer chapter on Gödel, Tarski, Church, and Turing supplies logico-mathematical background needed to understand philosophical contributions of logical positivists and later philosophers. This way of contextualizing philosophical developments will be my strategy throughout.

In Volume 1, some context is given in the first Moore-chapter, detailing his early reactions to Kant, Bradley, McTaggart, and other Idealists. More context is given in section 1 of the first Russell-chapter, which explains early influences of Sidgwick, McTaggart, and Bradley on him. Still more is given in section 2, which derives Russell's paradox from Cantor's Theorem, and indicates how it applies to Frege. Section 3 goes further, specifying consequences of Frege's decision to identify numbers with *extensions*

¹ The *Tractatus* is covered in the first 4 chapters.

of concepts under which other concepts fall, rather than with *concepts* under which other concepts fall, as suggested in Frege (1919). While the latter would *not* have led to Russell's paradox, it would have required infinitely many concepts which (on the usual understanding of Fregean concepts) would have required infinitely many objects. The point of providing this context is to clarify the relationship between Frege's and Russell's versions of logicism, connecting the former with developments of Fregean ideas in Hodes (1990) and Rayo (2002). Finally, section 1 of chapter 9 gives Russell's and Moore's arguments of against Idealists including Bradley and Joachim and Pragmatists including William James, John Dewey, and F.C.S. Schiller.

The most important stage setting for Frege's logicism is the discussion (on pp. 33-45) of his own critique of the philosophy of mathematics of his predecessors. No similar stage setting is needed for his logic, which we have inherited and which is presented using his semantic ideas, supplemented by passages from the *Begriffsschrift* and *The Foundations of Arithmetic* plus discussion of how his ideas relate to today's model-theoretic interpretations. I took this to be sufficient.

Beaney disagrees. Not one to let facts get in the way of the narrative, he writes, "Soames proceeds as if Frege's earlier ideas had no influence on his later ideas. *The distinction between sense and reference, for example, was motivated by the problems that Frege came to realize that his own earlier notion of 'content' faced. There is no mention of this in Soames' account of the distinction.*" Here is a mention.

What is the change in view from the *Begriffsschrift* to "On Sense and Reference?" A case can be made (i) that the metalinguistic view of identity remained roughly intact, (ii) that Frege came to see that the information contained in *judgments* we use identity sentences to express should be included in their *meanings*, (iii) that the post-*Begriffsschrift* development of the view that sentences refer to truth values helped him to see that they must have another semantic value as well, *and to*

*realize that his earlier talk of “the content of a sentence” had been ambiguous between talk of their truth values and talk of the information they carry,[See “Letter to Husserl,” 1891] (iv) that recognizing the senses of sentences brought with it recognition of the senses of all “proper names,” and (v) that it occurred to him that such senses would also solve “the identity problem,” and that this problem would be a good vehicle for introducing the general notion of sense.*²

What Beaney resents is not my failure to chart Frege’s evolution on this point, but that my chart criticizes Frege for failing to recognize that there is no alternative to taking identity to be a relation between objects and that the sense-reference distinction has nothing essential to do with identity.³

Beaney’s inaccuracies don’t stop there. He says:

Other than Dummett 1973 and writings by either him or Nathan Salmon, Soames only refers in the two chapters on Frege to the following works on Frege...Beaney 1996, Burgess 2005, Currie 1982, Dejnozka 1981, 1996, Kripke 2008, Perry 1977, 1979, Thau and Caplan 2000, Yourgrau 1982, 1986–87... to imply that there is little else worth reading is inexcusable.

First, several other pieces are discussed and cited, including Kneale and Kneale (1962), Kaplan (1968-69, 1978, 1989), Stalnaker (2006, 2008), Hodes (1990) and Rayo (2002).⁴

Second, the idea that I “imply” that there is little else worth reading about Frege beyond those I cite is preposterous.

Although not all of Beaney’s quibbles are off point, some are – e.g., his remark that my use of ‘satisfies’ below in explaining Frege is “anachronistic.”

[$\forall x\Phi x$] is true iff every object o is such that the concept designated by Φx assigns o the value the True iff o “satisfies” the formula Φx iff replacing occurrences of ‘ x ’ in Φx with a name n for o would result in a true sentence.

² ATP pp. 93-94.

³ Pp. 86-96. Beaney asserts that he disagrees with my presentation here, but he doesn’t say why.

⁴Kneale and Kneale (1962) are cited, with Beaney, as providing “informative discussions of the relationship between Frege’s system to syllogistic logic, as well as to the logical contributions of Leibniz, Boole, and De Morgan.” p.20. Hodes (1990) and Rayo (2002) are cited in chapter 7.

The thought, I guess, is that I imported something foreign – Tarski – into Frege. But I didn't. For o to satisfy Φx is just for Φx to be true of o , which, for Frege, was for the concept Φx to map o onto truth. There is no important, non-terminological difference between Frege and Tarski here. Of course, Frege didn't define truth for a formal language. Even then, there would be no formal difference between *satisfaction*, *being true of*, and *mapping onto truth*. The account of quantification as higher-order predication (not explicit in Tarski) was Frege's breakthrough insight, no matter how it was ultimately formulated. That Frege didn't formalize concepts as functions to truth values until 1891 doesn't negate this.

Another off-point quibble chides me for not using the posthumous pre-*Foundations* "Dialogue with Punjer on Existence." Since section 53 of *Foundations*—which I quote and criticize—is often seen as a more rigorous treatment of the issue in "Dialogue," the later didn't need to be cited. Contrary to Beaney, it contains nothing that affects my critique of Frege (i) that *existence* isn't the 2nd-level concept *assigning truth to some object* (if it were only certain concepts would exist, p. 61), (ii) that quantification can't, in general, be restricted to things that exist (pp. 62-64), and (iii) existence can and should be treated as a 1st-level concept.⁵ Although "Dialogue" does briefly allude to presupposition, it has no effect on these issues.

Beaney's criticism about how to understand Frege's logicist definitions is off-point in another way. The problem posed in chapter 2 section 8 is unavoidable. Either our justification and knowledge of arithmetic was already secure, prior to the logicist reduction, or it wasn't. If the former, then the reduction wasn't needed for that reason,

⁵ On page 63 I take ' $\exists y x=y$ ' to be what should, from Frege's point of view, be an acceptable existence predicate. Unfortunately, given (ii), it isn't.

though one might imagine it made explicit what was already known. One paragraph in “Logic in Mathematics,” likening understanding to “seeing in the mist,” gestures at this possibility. Though I don’t cite the paragraph, I argue against the idea. The latter alternative—that logicized arithmetic *replaces* ordinary arithmetic—is also sketched in “Logic.” Though I don’t cite that paragraph either, I develop the idea, showing how logicized arithmetic could be presented as a new system knowledge of which could then be used to provide knowledge of, and justification for, ordinary arithmetic. (See p. 120, where a worry about that view is also mentioned.) In short, no substance gestured at in “Logic” was missed.

Since writing the book, I have come to think that the problem logicians struggled with was much like the problem *What makes Hamiltonian mechanics and Lagrangian mechanics different versions of the same physical theory?* Although the propositions involved in accepting one are different from those involved in accepting the other, each is reconstructable from the other using the right logical techniques. Though daunting, the question *What are the right logical techniques (for this purpose)?* is now being investigated in general philosophy of science.⁶ In my opinion, this investigation holds more promise for advancing our understanding of attempts to ground mathematics in logic than discussions of the “paradox of analysis” do.

Again, Beaney seems to think otherwise. In a rare constructive interlude, he notes that Frege offers different, non-equivalent identity criteria for thoughts in different places. Acknowledging that I do take up the issue, Beaney thinks my criticisms of Frege could have been unified around it. Though I understand his thought, I differ with him

⁶ Hans Halvorson (2012), Thomas William Barrett and Hans Halvorson (forthcoming a,b,c).

on two points. As just intimated, I reject attempts to ground theory-identity in sameness of propositions expressed by different theory formulations. Propositions are objects of the attitudes; since accepting different formulations of the same theory often involves adopting markedly different attitudes, no criterion of propositional identity will solve the theory-identity problem. Second, the problems I raise for Frege in chapter 2 are, in the main, not variations on the theme of propositional identity. Although some do centrally involve that theme, my critiques of Frege's accounts of existence, truth, propositional unity, the Frege hierarchy, and the significance of identity for the sense-reference distinction touch do not crucially depend on it.

As for the criticisms themselves, I am dismayed that Beaney takes them in the wrong spirit. I don't think of Frege's philosophy as "something to be dismembered" and certainly not as "as just a collection of errors and dead ends." On the contrary, as I suggest in the section "Frege's Legacy," I take him to be the founding giant who, more than anyone else, introduced and made possible the science of language and information, which is a crowning jewel of the last century and a half. As one devoted to furthering that work by improving our conception of propositions as pieces of information, I continue to learn from his insights.

Having disagreed sharply with Beaney on several points, I close by agreeing with his remark that my failure to discuss a line of recent work attempting to revive Fregean logicism using Hume's Principle was a "lost opportunity". Although I mention that line at the end of chapter 1, and refer the reader to John Burgess's wonderful book, *Fixing Frege*, explaining and evaluating it, I don't explore it myself. My excuse is that I couldn't improve on Burgess and wasn't confident a short summary of mine would do it justice.

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Reply to Kelly and McGrath: Moral Data

Kelly and McGrath raise an important question about my critique of the relationship between Moore's normative methodology and his anti-skeptical epistemology. I will respond by clarifying the respect in which I took the latter to contain important but unheeded lessons for the former and asking whether those lessons can be extended.

Moore's normative aim in *Principia Ethica* is to lay the foundation of a *scientific system of ethics* independent of empirical theories and metaphysical doctrines. Its chief tasks are to identify all and only the (kinds of) things that are intrinsically good and to characterize right actions in terms of the intrinsic value of their consequences. The account of the intrinsic value of states of affairs and the right-making features of actions were to be necessary and apriori. All foundational statements about what makes good things good were held to be synthetic apriori, while some fundamental statements about what makes right acts right are held to be analytic. But this contrast was short-lived. In his favorable 1904 "Review of *Principia Ethica*" Russell criticized Moore for wrongly trying to *define* rightness in terms of *goodness* in the same manner in which Moore criticized others for trying to *define* goodness. Moore recanted in *Ethics*, acknowledging that his consequentialist theory of rightness is, like his normative theory of goodness, synthetic apriori.⁷ Thus, Moore's mature normative *science* was to be thoroughly synthetic, necessary, and apriori.

In addition to speaking of this "science" in sections 3, 4, 5, and 14 of chapter 1 of *Principia Ethica*, Moore describes his "scientific" aim in the preface.

⁷ATP pp. 252-257.

I have endeavored to write ‘Prolegomena to any future ethics that can possibly pretend to be scientific.’ In other words, I have endeavored to discover what are the fundamental principles of ethical reasoning; and the establishment of these principles rather than any conclusion which may be attained by their use, may be regarded as my main object.⁸

In contrasting “ethical reasoning” with ethical conclusions, Moore distinguishes his aim of specifying what makes right acts right and wrong acts wrong from showing particular acts, or types of acts, to be right or wrong. Though both jobs are important, he thinks only the former can reliably be done, as his discussion of ethics vs. casuistry indicates.

We may be told Casuistry differs from Ethics in that it is much more detailed and particular...But it is most important to notice that Casuistry does not deal with anything that is absolutely particular...Casuistry may indeed be *more* particular and Ethics *more* general, but that means that they differ only in degree and not in kind...Just as chemistry deals with discovering the properties of oxygen *wherever it occurs*...so Casuistry aims at discovering what actions are good [Moore here means *right* in the sense *productive of good results*] *whenever they occur*... Casuistry forms, therefore, part of the ideal of ethical science: Ethics cannot be complete without it...It has failed only because it is far too difficult a subject to be treated adequately in our present state of knowledge.⁹

It is not so much that Moorean ethical theory – which aims to discover what makes good things good and right acts right -- is more general than casuistry; both aim at providing true, universally quantified biconditionals. But whereas the knowledge required for ethical theory is abstract and apriori, the knowledge that particular acts, or acts types, are right or wrong requires empirical information that Moore thought we neither have nor are in a good position to acquire.¹⁰

Unfortunately, his aprioristic normative theory is unsuccessful. His normative theses

1. The contemplation of beautiful objects and the pleasures of human companionship are the only two things that are intrinsically good.

⁸Page 35, Preface to the first edition, reproduced in the second edition Moore (1903).

⁹Ibid., pp. 56-57.

¹⁰Ibid., sections 16 and 17, pp. 73-78.

and

2. Our single moral duty is to maximize the amount of those two things.

are breathtaking oversimplifications at best and outright falsifications at worst. The contention that (1) and (2) are *self-evident* apriori truths is a *reductio ad absurdum* of his moral epistemology.

In section 6.1 I criticize Moore's attempt to reconcile his theses M1-M3.

M1 No *interesting* ethical thesis – about which we may look to philosophy for guidance to resolve controversy or overcome uncertainty -- is either itself analytically obvious or provable from analytically obvious premises by analytically obvious steps.

M2 Nevertheless, some such theses – stating necessary and sufficient conditions for a state of affairs to be good (or to have greater intrinsic value than another such state), and similarly for conditions for an action to be right – are true and knowable apriori.

M3 Since the foundational normative theses mentioned in M2 are apriori, they must either be self-evident or justified by appeal to other self-evident normative claims.

I criticize Moore for attempting to retain M1-M3 by claiming that the interesting foundational theses of his ethical theory are self-evident. They aren't. He would have done better to look for interesting theses justified as *explanations* of self-evident but restricted moral claims (of the sort illustrated b 1-7 in the Kelly-McGrath paper). To have done so, I suggested, would have brought his normative methodology closer into line with his general epistemology. Just as purported necessary and sufficient conditions for empirical knowledge are justified by obvious epistemic data points, e.g. *that I know I have a hand*, (rather than thinking that the latter must be justified by the former), so necessary and sufficient conditions for goodness and rightness should be justified by obvious evaluative data points (rather than seeking justification in the opposite direction).

Kelly and McGrath object that the proper evaluative data points for my parallel are not the ones I identify – which evaluate hypothetical (types of) individuals, actions, and

states of affairs. Instead, the data points should identify particular “act tokens” as right, wrong, or not wrong. For example, they take us to know, pre-theoretically, *that the 9/11 hijackers acted wrongly in flying planes into the World Trade Center*, and they take themselves to know *that they did nothing wrong in writing their paper*. It is, they contend, propositions like these that would have constituted Moore’s normative starting points had his normative methodology matched his epistemic methodology.

They have a point, but their suggestion also faces a problem. Following it would have required Moore to give up ethics as autonomous, apriori “science.” One’s knowledge that one has hands is aposteriori, as is any statement of necessary and sufficient conditions for knowledge that rests on the claims of this sort it explains. By contrast, the normative data points in my reconstruction of Moore’s ethics might plausibly be taken to be self-evident apriori truths -- e.g. *if one promises y that one will do x, one has an obligation to do x unless y releases one from the obligation*. If they are, then moral principles justified by the explanations they provide of these data points may themselves be apriori. This would not be so if the data points were contingent aposteriori truths, like the proposition that *Bill has an obligation to call his mother this weekend, since he promised to do so, and she didn’t release him from that obligation*. Thus, while the data points suggested by Kelly and McGrath require Moore to reject M2 and M3, the data points I suggest allow him to retain M1-M3, while rejecting the non-essential, and obviously false, claim that interesting normative theses provided by ethical theories are self-evident truths.

The normative methodology I urged on Moore was designed to preserve central metaethical commitments for which there are no comparable counterparts arising from

his general theory of knowledge. Because of this, the parallel between my reconstructed version of his normative theory and his general theory of knowledge is only partial. Although starting points for the latter include real “token instances” of empirical knowledge, starting points for the former don’t include claims about the rightness or wrongness of “token acts.” Kelly and McGrath note this by pointing out that whereas my suggested Moorean normative starting points are mere good- or right-making features, Moore’s starting points for constructing a theory of empirical knowledge are *not* mere knowledge-making features.

That’s right; principles like (8) and (9) in their review aren’t Moorean starting points for a theory of knowledge. But the reasons they aren’t don’t eliminate the limited normative principles I suggest as starting points for an improved Moorean ethical theory. The chief goal of Moorean epistemology is to vindicate knowledge of the external world. To that end, he chooses undeniable starting points that are themselves instances of such knowledge – e.g., *that one has a hand*. Principles like Kelly’s and McGrath’s (8) and (9) are neither instances of such knowledge nor things one must know in order to acquire it. By contrast, to know the limited normative principles I suggested is to have partial knowledge of what it is to be a good or bad person, or state of affairs, or to be a morally right or wrong action. This knowledge may even be plausibly held to be necessary for knowledge of the fully-fledged *goodness, badness, rightness, or wrongness* of individuals, states of affairs, or “act tokens.” Hence, my limited moral principles may serve as starting points for systematic ethical theorizing. On this conception, it is not the job of ethical theory to morally evaluate any existing individual, state of affairs, or “act

token;" it is its job to ground such evaluations in non-moral properties that are necessary and sufficient for determining their moral properties.

So much for my improvement on Moore, which retains his view of ethical theory as aiming at necessary, apriori, truths -- $[\forall x (x \text{ is a good person/state of affairs/etc. iff } x \text{ is } D)]$ and $[\forall x (x \text{ is a right act iff } x \text{ is } D^*)]$ (when D and D^* express naturalistic conditions). Should we continue to think of ethical theory in that way? If not, then, as Kelly and McGrath implicitly suggest, our moral starting points may not need to be apriori. Suppose that what is morally valuable depends on what, as an empirical matter, we most fundamentally value. Suppose also that we learn what these values empirically – sometimes from our own mistakes, sometimes from others, sometimes from literature and philosophy, and sometimes from human biology, psychology, and sociology. It might then turn out that both fundamental ethical truths – the interesting moral principles we seek from ethics -- and some data points supporting them, can be known only aposteriori.

What about the modal profile of such principles? We are accustomed to thinking they ought to be necessary. On the envisioned empirical conception of ethics, they will be necessary only if our values are metaphysically essential to us. Although some may be, it is doubtful that all values with moral significance for us are. Hence, a conception of ethics as aposteriori might easily lead to a conception of ethics as contingent too. Although I don't know what Kelly and McGrath think about such a conception, it wouldn't have appealed to Moore. However, I don't rule it out.¹¹

¹¹ See my "Is there a science of morality?" forthcoming.

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Reply to Pigden: Missing the Forest for the Trees

Pigden objects to the omission of serious discussion of Russell's *ramified theory of types*, which, though deployed in *Principia Mathematica*, was abandoned by Russell in 1927, after Ramsey's 1925 reconstruction using the *simple theory of types*. In chapter 10 of *ATP*, I emphasize the simple theory, supplemented by a few explicit remarks about, plus some references to, the ramified theory. Pigden thinks this renders Russell's 1910 reduction opaque to my readers. I agree that more could have been said about the ramified theory, while disagreeing about what it takes to understand Russell. The chapter presents the reduction in its most compelling and historically influential form, while identifying and criticizing the key philosophical considerations behind the ill-fated ramified theory.

It begins by summarizing Frege's logicist reduction and outlining a type-theoretic reformulation of it in Russell's conceptual framework that would avoid the paradox that defeated Frege. Section 2 develops the idea in a 1st-order type-theoretic set theory. Section 3 discusses philosophical issues, including those centering on the type theory and the need for the Axiom of Infinity positing infinitely many non-sets. Section 4 redoes the reduction in higher-order type-theoretic logic, transforming the underlying system from something that looks like a *mathematical theory* of a special kind of abstract objects – *sets* – into something that looks more like a genuine logic.

My point is illustrated by the textbook model theory of *2nd-order quantification into predicate position*. Let the range of 1st-order quantifiers be members of a set D (the domain) that contains all non-sets plus perhaps some sets too. The range of the *2nd-order (predicate) quantifiers* consists of all and only subsets of D . Next, consider $\exists X \forall y (X(y) \leftrightarrow$

$\sim y \varepsilon y$), which is true in a model in which ‘ ε ’ expresses set membership, iff there is a subset of D containing all and only members of D that aren’t members of themselves. The sentence is true, not only in this model, but in all models, and so is a 2nd-order logical truth – as are all instances of $\exists X \forall y (X(y) \leftrightarrow Fy)$.

These instances are, in effect, what the 1st-order set-theoretic axiom schema of comprehension becomes when expressed in a system of higher-order quantification. Let quantification over individuals (non-sets) be unindexed, predicates of them be indexed ‘1’, and predicates (open formulas) that take those predicates as arguments be indexed ‘2’, and so on. This gives us:

$$\begin{aligned} \exists Y_1 \forall X (F_{1X} \leftrightarrow Y_{1X}) \\ \exists Y_2 \forall X_1 (F_2(X_1) \leftrightarrow Y_2(X_1)) \\ \exists Y_3 \forall X_2 (F_3(X_2) \leftrightarrow Y_3(X_2)) \\ \vdots \\ \vdots \\ \vdots \end{aligned}$$

Interpret these sentences in a model in which the domain of individual quantifiers is a set of non-sets and the domain of quantification involving higher-order variables V_n is the set of all subsets of the domain of quantification involving variables V_{n-1} of the preceding level. These are *comprehension axioms* for each level in the simple type-theoretic hierarchy. Being true in every model, they are logical truths, which is how they are treated in simple type-theoretic reduction in section 4 of chapter 10.

Although this is attractive, (i-iii) prevent the system from qualifying as the fully general system of *logic* to which Russell wished to reduce arithmetic in 1910.

- (i) It locates natural numbers at a particular level of the hierarchy – most naturally the level at which we get sets of sets of individuals. This makes it difficult to combine Frege’s insight that *any entities* (including classes if there are such) can be numbered with Russell’s insistence that a proper definition of, e.g., the number 3 should capture “something which all trios have in common, which distinguishes

them from other collections.”¹² Although the above approach does capture the fact that all trios of *individuals* are members of the number 3, it does not (or at any rate doesn’t obviously) capture what all trios have in common, including trios of classes if there be such. (See pp. 492-93 of *ATP*.)

- (ii) The simple type-theoretic system of section 4 of chapter 10 is a formal system *interpreted in a particular model*—which, for Russell’s purposes, requires (a) infinitely many non-sets over which individual variables range (488-91) and (b) infinitely many totalities (classes/sets) over which the hierarchy of quantifiers range. Although (a) was (unfortunately) unavoidable for Russell, (b) is suspect. Even if one accepts classes, it is *not* obvious that 1st-order set theory -- which routinely quantifies over all sets, even though there is no *totality* of all sets--is illegitimate. Hence what is presented as a higher-order system of *logic* might seem to be a particular theory with its own subject matter, rather than *a general logical framework governing reasoning about any subject*.
- (iii) Although the segmentation of the formulas of the logical language into discrete types avoids Russell’s paradox, it seems to do so by artificially limiting expressive power. Indeed it appears impossible to both describe the principles governing the entire hierarchy without saying things that the system itself does not allow one to express.

Worries like these don’t undermine the technical correctness of an ontologically committal reconstruction of Russell’s reduction in a simple theory of types; nor do they rob the reduction of all philosophical significance. Indeed, this was how the reduction was most commonly viewed by those -- including Tarski, Carnap, Hempel, and Quine -- most influenced by, and admiring of, Russell’s work. However, these worries did lead him initially to present and construe his reduction in another way.

The key issues--Russell’s eliminativist “no-class theory,” his radically deflationary reconstrual of propositional functions, and his implicitly revisionary account of quantification in *Principia Mathematica* -- are discussed at length in section 5 of chapter 10. As explained in Klement (2004), classes and propositional functions had been linked in Russell’s mind from the time of the *Principles of Mathematics*. Granting the existence of classes, he provisionally adopted a type-hierarchy there as a way of avoiding paradox.

¹² Russell, *Introduction to Mathematical Philosophy*, pp. 11-12.

But drawbacks like (i)-(iii) led him to be unsatisfied with it. (p. 514) How, Russell wondered, could classes and propositional functions be *non-arbitrarily* restricted not to be members of themselves, and not to take themselves as arguments, respectively?

His question didn't extend to genuine properties and relations, which he thought could occur both as *terms* and as *concepts* in propositions (see chapter 7 of *ATP*). 'Humanity is human' was seen not as meaningless but as expressing a false proposition, while 'Concepthood is a concept' expressed a true proposition. No paradox follows provided that *being a concept that doesn't apply to itself* isn't taken to be a concept. This required complex concepts to be *logical constructions* -- where to say that *so-and-so's* are logical constructions is to say that sentences that appear to be about *so-and-so's* are analyzed as being about other things. (p. 515) Since Russell thought of classes and/or propositional functions as crucial in such analyses, they were the focus of his worries about paradox.

Since classes and propositional functions had to be virtually unbounded, admitting them as entities required restricting the range of propositions in which they could be subjects of predication. Although such restrictions could be imposed by fiat, justifying them was difficult. Russell wanted genuine restrictions inherent in all intelligible thought. These were not easy to come by -- in part because he seemed to think there must be a level at which all genuine entities could be spoken of. Since this threatened contradiction, he was struggling, at least by 1906, to find a way of treating classes and nonlinguistic propositional functions as "logical fictions."

This led in *Principia Mathematica* to analyzing classes in terms of propositional functions, which were taken to be open formulas. Since quantification was analyzed in

terms of propositional functions, this led to far-reaching new thoughts about quantification. (pp. 515-520, 526) I show that Russell's remarks in section 3 of chapter 3 of the introduction to *Principia Mathematica* include passages that suggest ordinary objectual quantification, passages that suggest metalinguistic quantification, and passages that suggest a substitutional quantification along the lines attributed to him in Landini (1998) and Klement (2004). Although Russell wasn't entirely consistent, I concluded that his "*radically deflationist thoughts on ontology [of classes], his need for a philosophical justification for his theory of types, and his contention that 'the vicious circle principle' underlying his type theory provided a unified treatment of a broad range of logical and semantic paradoxes, may well have contained inchoate substitutional elements of the sort identified by Klement, Landini, and others.*" (p. 519)

This comes out pretty clearly in *Principia Mathematica*, where Russell characterizes his system as starting out with quantifier-free sentences. These are said to have *first truth* (or *first falsity*) on the basis of the referents of the names, predicates, and function symbols they contain. Quantifications over individuals are defined in terms of the *first truth* of their substitution instances, where the instances arise from associated propositional functions, which are *formulas with free occurrences of variables*. On the substitutional reading, the instances arise by replacing those occurrences with occurrences of individual constants. $\forall x Fx$ is said to have *second truth* iff all its quantifier-free instances have *first truth*; similarly for higher-order quantifiers. (pp. 516-519) When the system is read substitutionally, this means that what looks like higher-order quantification over classes is not objectual, and so carries no ontological commitment not carried by quantifier-free sentences.

The light this throws on Russell's *no-class theory* and his *ramified theory of types* is expressed in a passage from Klement (2004) quoted on pp. 519-20.

The core idea is that a formula involving a *free higher-order variable*, e.g., " $\Phi!a$ ", has as its substitution instances first-order formulae containing the constant " a " occurring in logical subject position...one or more times...Just as a proposition involving an individual quantifier is true just in case all its substitution instances are, similarly a proposition involving a higher-order quantifier is as well. So " $\forall\Phi(\Phi!a)$ " has "third truth" if and only if all closed substitution instances of " $\Phi!a$ " have "second truth." Russell's different notions of truth...are derived from the recursive truth definition for his language; ramification [the system of type restrictions in *Principia Mathematica*] is *required* in order for the truth conditions not to be circular. Russell uses the notation " $\Phi!a$ " for a function whose arguments are predicative functions of a , i.e. those not involving quantification over functions; if we allowed impredicative expressions to be substitution instances of " $\Phi!a$ ", then the conditions under which " $\forall\Phi(\Phi!a)$ " has third truth could not be defined in terms of second truth alone. This, and not any ontological doctrine, is the heart of the "vicious-circle principle" [justifying the type restrictions]. Russell seems to have anticipated Kripke's suggestion...that higher-order quantification can be given a substitutional semantics only if the allowable instances of the variables cannot contain expressions involving bound variables of the same order.¹³

This avoids the problems (i)-(iii) that plague ontological versions of type theory, including my reconstruction in section 4 of chapter 10 and the one Russell offered at the end of the *Principles of Mathematics* in 1903. First, locating the reduction of arithmetic to logic at one particular level of the hierarchy no longer makes it difficult to combine Russell's insistence that a proper definition of the number 3 should capture what all trios have in common with Frege's insight that *any entities* (including classes) can be numbered. If one thinks there are no classes, and indeed no entities beyond those mentioned in *first true* quantifier-free sentences, one need not worry that one has captured "only" what all trios of individuals have in common. Second, if quantifiers never range over *totalities of objects*, then there is no need for special restrictions on what counts as a legitimate totality. Since the needed restrictions fall out of the very meaning

¹³ Klement (2004), 28-29. See also Klement (2010).

of the substitutional quantifiers, one who takes all quantification to be substitutional will view the type restrictions (imposed by the ramified theory) to be inherent in *all intelligible thought*. Third, this means that the need for hierarchy in blocking paradox doesn't require any loss of expressive power, or any appeal to unstateable restrictions.

These considerations address one of Pigden's chief charges – namely, that for readers of my book it will be “utterly mysterious why Russell should have wrestled with the paradoxes for five years when he had already written up and published a version of the simple theory of types in the *Principles of Mathematics* in 1903.” Not so. Careful readers will know (from chapter 10, sections 3.3, 4, 5, and 6) not only that Russell sought to overcome (i-iii), but also why the substitutional quantification underlying the no-class theory and the ramified type theory appeared to do precisely that.

They will also know why, despite the philosophical mileage Russell hoped to gain, the real consequences of adherence to these excrescences would have been deleterious to both his logicist reduction and his general philosophical logic. (pp. 520-531) The key problem for the reduction is the diminishment in expressive power that comes from thinking of higher-order quantification substitutionally rather than objectually.

Whereas the objectual quantifiers range over *all sets at a given level* – both those that are extensions of predicates at that level...and those that are not -- the substitutional quantifier mimics only quantification over the former. If, as is standardly assumed, every sentence...is a finite sequence of the logical and nonlogical vocabulary, the domain of all sets at a given level will far outstrip the domain of all sets that are the extensions of predicates at that level. As a result, the expressive power of the underlying “logical” theory to which arithmetic is to be reduced is diminished by treating its quantifiers substitutionally.¹⁴

¹⁴ *ATP* p. 521. A footnote to the passage notes (i) that the problem can be mitigated by letting formulas to be infinitely long and (ii) that Hodes (2012) develops this strategy, showing it to be needed by a substitutional interpretation of higher-order quantifiers in *Principia Mathematica*, leading to a ramified theory of types. As Hodes notes, this approach renders Russell's logical language incapable of being understood by agents with finite cognitive powers.

I show that this diminishment blocks the most natural proof of the axiom of mathematical induction, while making it impossible to imagine successful reductions of mathematical theories that of uncountably many objects.

I also indicate that this diminished expressive power is connected to Russell's adoption of the ramified theory of types.

The idea behind Russell's definition of number and his reduction of arithmetic to logic was fundamentally the same as Frege's. But to block the set-theoretic paradox, he needed a theory of types. In point of fact, the theory he adopted – called *the ramified theory of types* – was far more complex than the simple type theory I have informally employed...In the ramified theory, each type restricting the arguments a propositional function can take is subdivided into a hierarchy of orders which impose restrictions on variables bound by quantifiers (so that whatever is defined using a quantifier cannot be among what the quantifier ranges over). The effect has often been seen as imposing something like constructability requirements on elements in the hierarchy, so that nothing can be specified at a higher order except in terms of expressions the extensions of which are already fixed at lower orders. Imposing a substitutional constraint on the quantifiers will have this effect. Thus, thinking of quantification substitutionally and seeing in it a way to avoid classes plus non-linguistic...propositional functions could, in principle, have been taken by Russell as providing a reason for adopting the ramified theory that he in fact employed. However, one who goes this route weakens the underlying logic-cum-set-theory, affecting, among a great many other things, the natural way of thinking about the definition of natural number, and of proving the induction axiom. Russell's remedy was to add another axiom, called *the Axiom of Reducibility*...Suffice it to say that [it] was not well received. (p. 523)

Although I don't present the Axiom of Reducibility in any detail, I do point the reader to historically important discussions of it.

On pages 525-530 I complete my critique of the substitutional interpretation by showing (i) that it is inconsistent with the Frege-Russell account of quantification inherent in "On Denoting" (which Russell didn't think of himself as giving up), (ii) that it imposes impossible cognitive demands on reasoners when combined with his doctrines of acquaintance and logically proper names, (iii) that it undermines his higher-order definition of identity in *Principia Mathematica*, and (iv) that adding his Axiom of Infinity

extends these problems. In other words, I argue that Russell didn't know what he was getting into. He hadn't thought through, and didn't understand, the consequences of the substitutional idea underlying his *no-class theory*, his *ramified theory of types*, and his conception of a unified *vicious circle* solution to both the set-theoretic and the semantic paradoxes.

If I am right, the *no-class theory* and the *ramified theory of types* were gigantic errors that shouldn't be overemphasized when interpreting Russell.

The best interpretation is, I think, the one that best coheres with Russell's most important philosophical views, best advances his understanding of the relationship between logic and mathematics, and best explains the impact of his work on those who followed. Such an interpretation should, I believe, dismiss his radical eliminativism about classes and his flirtation with substitutional quantification as regrettable but understandable errors, while treating the quantification in his hierarchy as objectual, ranging over individuals and classes (or non-linguistic propositional functions). The complex ramified theory of types and the Axiom of Reducibility can then be dropped in favor of the simple theory of types in Ramsey (1925), through which most of the historical influence of Russell's reduction has flowed. I believe that it is this (relatively standard) interpretation that has the best chance of illuminating the strengths and weaknesses of his logicist program, while making intelligible its impact on later philosophers and logicians. (531)

Although I think this justifies my presentation of the reduction in *ATP*, I do recognize one serious shortcoming. I *say* that the ramified theory was largely based on a disastrous and incompletely understood substitutional view of quantification, but I don't state the ramified theory, or give the reader a detailed understanding of it. So, the connection between the two isn't made transparent. Because of the complexity involved, this isn't easy to do; nor do Pigden's secondary sources do it. The best treatment I know is an unpublished manuscript, Hodes (2012), which concludes that although Russell's inclination toward substitutional quantification provided him with an intelligible reason to ramify, it conflicts with his commitment to the finitude of human cognition, with the

result that “it seems that there is no coherent philosophy that can stand behind Russell’s approach to logic.”¹⁵

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¹⁵ Abstract presented on October 6, 2012 at the USC Conference on Logic and Philosophy of Science.

