THE HISTORICAL TURN IN ANALYTICAL PHILOSOPHY

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History of Analytic Philosophy

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The Historical Turn in Analytic Philosophy

Edited by

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University of California, Riverside, USA
To Sally
Through Thick and Thin
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Series Editor’s Foreword

During the first half of the twentieth century, analytic philosophy gradually established itself as the dominant tradition in the English-speaking world, and over the last few decades it has taken firm root in many other parts of the world. There has been increasing debate over just what ‘analytic philosophy’ means, as the movement has ramified into the complex tradition that we know today, but the influence of the concerns, ideas and methods of early analytic philosophy on contemporary thought is indisputable. All this has led to greater self-consciousness among analytic philosophers about the nature and origins of their tradition, and scholarly interest in its historical development and philosophical foundations has blossomed in recent years, with the result that history of analytic philosophy is now recognized as a major field of philosophy in its own right.

The main aim of the series in which the present book appears, the first series of its kind, is to create a venue for work on the history of analytic philosophy, consolidating the area as a major field of philosophy and promoting further research and debate. The ‘history of analytic philosophy’ is understood broadly as covering the period from the last three decades of the nineteenth century to the start of the twenty-first century, beginning with the work of Frege, Russell, Moore and Wittgenstein, who are generally regarded as its main founders, and the influences upon them, and going right up to the most recent developments. In allowing the ‘history’ to extend to the present, the aim is to encourage engagement with contemporary debates in philosophy, for example, in showing how the concerns of early analytic philosophy relate to current concerns. In focusing on analytic philosophy, the aim is not to exclude comparisons with other – earlier or contemporary – traditions, or consideration of figures or themes that some might regard as marginal to the analytic tradition but which also throw light on analytic philosophy. Indeed, a further aim of the series is to deepen our understanding of the broader context in which analytic philosophy developed, by looking, for example, at the roots of analytic philosophy in neo-Kantianism or British idealism, or the connections between analytic philosophy and phenomenology, or discussing the work of philosophers who were important in the development of analytic philosophy but who are now often forgotten.

The current volume, edited by Erich Reck, was specially commissioned when the series as a whole was established. Reck has played a key role in shaping the new field of history of analytic philosophy, beginning with an influential collection published in 2002 entitled From Frege to Wittgenstein:
Perspectives on Early Analytic Philosophy. His brief for the present volume was to invite some of the leading scholars in the field (both established and from the next generation) to either provide case studies of individual analytic philosophers, address certain broader themes, or reflect on some of the methodological issues that are raised by work on the history of analytic philosophy. The case studies (focusing on Russell, Carnap, Quine and Ryle) form Part I of the volume, discussions of certain broader themes (in philosophy of language, logic, mathematics and mind) constitute Part II, and methodological reflections are offered in Part III.

As Reck explains in his introduction, analytic philosophy originally arose with an ahistorical – and even anti-historical – agenda, and even today analytic philosophy is still seen by many in this light, and either commended or criticized for it, depending on one’s point of view. The very existence of history of analytic philosophy (as a field of philosophy), and this series itself, not only bears witness to a change of attitude within analytic philosophy but also poses a challenge to analytic philosophers still working in ahistorical or anti-historical ways. Questions concerning the relationship between philosophy and history of philosophy are forced to the surface, and many of the contributors to the present volume offer accounts of this relationship and views on what a healthy perspective should be. The contributors do not speak with a single voice, but there is certainly agreement about the value of studying the history of analytic philosophy and the importance, in general, of history of philosophy for philosophy.

Michael Beaney
October 2012
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Rules, Reason, and Self-Knowledge (2013). She has also written several articles on Ryle, which she hopes to fashion into a book, including a critical introduction to The Concept of Mind and prefaces to his two volumes of collected papers, each re-published by Routledge in 2009, as well as the entry on Ryle in the Stanford Encyclopedia of Philosophy.
Introduction: Analytic Philosophy and Philosophical History

Erich H. Reck

During the last 25 years, a large number of publications on the history of analytic philosophy have appeared, significantly more than in the preceding period. As most of these works are by analytically trained authors, it is tempting to speak of a ‘historical turn’ in analytic philosophy. The present volume constitutes both a contribution to this body of work and a reflection on what is, or might be, achieved in it. In this introduction, the growing interest in the history of analytic philosophy is put into context. The introduction has two parts. In the first part, the traditionally uneasy relationship between analytic philosophy and history of philosophy is explored. This is done in several ways: by acknowledging the bias against studying the history of philosophy often associated with analytic philosophy (section 1.1); by establishing that, nevertheless, analytic philosophers have engaged with the works of historical figures in a number of ways (1.2); and by exploring, against that background, various forms in which analytic philosophy and ‘philosophical history’ may be combined fruitfully (1.3). In the introduction’s second part, a survey of work on the history of analytic philosophy from the last 25 years is provided (2.1), together with abstracts for the new essays in this volume (2.2), and both are supplemented by a representative bibliography (2.3).

1 History of philosophy in the analytic tradition

1.1 Analytic philosophy’s anti-historical self-image

Considered as a general movement, analytic philosophy has long had an uneasy relationship with historically oriented approaches to philosophy. One might say that it has had an a-historical, or even anti-historical, image of itself. While this self-image has been challenged and revised in recent years, it remains influential. In this and the next section, the uneasy relationship between analytic philosophy and history of philosophy will be reconsidered, starting with an extreme anti-historical attitude that can still
be encountered among analytic philosophers occasionally. After that, it will be illustrated that much more nuanced attitudes have played a role in the analytic tradition as well.

Expressions of an extreme anti-historical bias in analytic philosophy can be found, among others, in remarks that have been attributed to several well-known analytic philosophers. Thus, W.V.O Quine is reported to have said: ‘There are two sorts of people interested in philosophy, those interested in philosophy and those interested in the history of philosophy’. When introduced to a phenomenologist, thus to someone working in a more historically inclined tradition, John Searle supposedly declared: ‘I am an analytic philosopher. I think for myself.’ And Gilbert Harman allegedly had the following blunt note on his door: ‘Just say no to the history of philosophy!’ If taken seriously, what such remarks suggest is that doing analytic philosophy and studying the history of philosophy are separate endeavors, or stronger, that the two are in tension with, and perhaps even fundamentally opposed to, each other.

It is hard to be sure how seriously to take such remarks. One might view them more as quips, infused with a sense of humor, than anything else. They were also uttered in informal conversation. Sometimes the sources of the attributions are philosophical opponents, so that they should be taken with a grain of salt for that reason as well. Then again, occasionally one can find remarks with strong anti-historical implications also in print. To mention just one example, Jerry Fodor has boasted about his ‘ignorance of the history of philosophy’ and his ability to write a ‘book on Hume without knowing anything about him’. However much one is inclined to discount such remarks, as tongue-in-cheek humor or gentle provocations, the resonance they had, and still have, reveals something about the analytic tradition. At the same time, so far we are only dealing with slogans or epigrams, thus with little of philosophical substance.

What is usually seen as providing such substance, especially within analytic philosophy, is arguments. How might an analytic philosopher argue that studying the history of philosophy can be dismissed or largely discarded? One relevant line of thought relies on the distinction – familiar from the works of Reichenbach, Carnap, Popper, and others – between ‘context of discovery’ and ‘context of justification’. The argument is then that, while historical investigations may have a place in tracing the discovery of ideas and theses, their justification should be seen as separate and as proceeding non-historically. In this context, history and psychology are sometimes put in the same camp, as related ways of studying the discovery of ideas. Frege’s anti-psychologist writings are one source of this general perspective. As he put it:

The historical mode of investigation, which seeks to trace the development of things from which to understand their nature, is certainly legitimate; but it also has its limitations. [...] We imagine, it seems, that
concepts originate in the individual mind like leaves on a tree, and we suppose that their nature can be understood by investigating their origin and seeking to explain them psychologically through the working of the human mind. But this conception makes everything subjective, and taken to its logical conclusion, abolishes truth. (Frege, 1884, p. VII)

Frege goes on to distinguish the analysis of concepts, thoughts, and inferences from the study of ‘either the history of our knowledge of concepts or of the history of meanings of words’ (ibid.). And like many analytic philosophers after him, he takes the former to be his task while putting the latter aside.

Bringing in Frege also suggests a second, though not unrelated, argument for discounting work on the history of philosophy. What is it, first and foremost, that allows philosophers to analyze concepts and to evaluate corresponding arguments since Frege? It is the availability of a new tool: modern logic. Not only is this an extremely powerful tool for such purposes; much earlier philosophy was also systematically misled, or had its progress blocked, by not having it at its disposal – or so this second line of thought continues. The latter is especially prominent in Bertrand Russell’s writings on metaphysics and epistemology, and many of his followers have adopted it since then. Not coincidentally Frege’s and Russell’s works in logic, from the late nineteenth and early twentieth centuries, are then taken to constitute the very beginning of ‘analytic philosophy’. At times, analytic philosophers even act as if it was only then that philosophy really started, or at least the kind of philosophy worth taking seriously.

But within analytic philosophy, it is not just formal logic that has been seen as having changed philosophy fundamentally. For instance, the method of linguistic analysis, based on ordinary language and common sense, was taken to be equally revolutionary, in the 1940s and 50s, by Wittgenstein and other ‘ordinary language philosophers’ (Austin, Ryle, Strawson, and others). The early work of Russell’s colleague G.E. Moore is often taken to be a main source for this second strand in analytic philosophy, which situates the origins of the tradition again at about one hundred years ago. And jumping forward all the way to the present for another example, experimental philosophy has been hailed as having a similarly revolutionary effect in recent years, i.e., as allowing for the dismissal of large parts of earlier philosophy, including much of analytic philosophy. Experimental philosophers see the latter as relying on naïve appeals to ‘intuition’, to be replaced by different, more empirically grounded work.

Now, this basic posture – the dismissal of earlier work in the name of a radical new beginning – is far from novel in philosophy. As Charles Taylor has noted:

There is an ideal, a goal that surfaces from time to time in philosophy. The inspiration is to sweep away the past and have an understanding of
things which is entirely contemporary. The attractive idea underlying this is that of liberation from the dead weight of past errors and illusions. (Taylor, 1984, p. 17)

Descartes’ new start for philosophy in the seventeenth century, based on his method of radical doubt, comes to mind prominently. Descartes’ approach was also tied to the rise of *modern science*, and as Taylor continues, ‘one great model for this kind of thing is the Galilean break in science’ (ibid.). Thus, with Galilei, Descartes, and their followers all previous Scholastic philosophy was finally swept away (or so the story about the beginning of ‘modern philosophy’ goes). While a romantic reaction against privileging science followed in the eighteenth century, using it as the model to emulate for philosophy played again a big role in the nineteenth and early twentieth centuries. At that point, not only analytic philosophers had the goal of finally making philosophy ‘scientific’, but also Husserl, thus inaugurating phenomenology. And the same theme recurred again in later ‘continental philosophy’, e.g., when the structuralist tradition in France tried to make the study of language and the mind more ‘scientific’. In all these cases, a new method was supposed to make what came before largely dispensable (and in all of them, the break was not so clean and complete as typically assumed, as careful historical research has shown since then).

At the same time, continental philosophy is often taken to provide main examples of the opposite perspective too, i.e., of *philosophical historicism* – the view that studying the history of philosophy is crucial, or even essential, for philosophy. Kant is an interesting, somewhat ambivalent, case in this connection (which partly explains his enduring appeal to both analytic and continental philosophers). On the one hand, he remarks in his *Prolegomena* (in words reminiscent of Quine):

> There are scholars to whom the history of philosophy is itself their philosophy; the present Prolegomena are not written for them. They will have to wait until those who endeavor to draw from the fountain of reason have finished their business, and thereupon it will be their turn to apprise the world of what happened. (Kant, 1783, Preface)

Elsewhere, Kant adds (along Searle’s lines) that we cannot learn to philosophize except by engaging in it ‘autonomously’, since only this gives us ‘rational knowledge’; he also insists on a strict distinction between ‘validity’ and ‘genesis’ (a main source of the justification-discovery dichotomy). On the other hand, the text from which these quotations are taken – the *Critique of Pure Reason* – ends with a chapter called ‘The History of Pure Reason’, which assigns an important role to coming to terms with previous philosophical thinkers. Kant also introduced into philosophy the immensely influential historical distinction between ‘empiricists’ and ‘rationalists’. 
Several of Kant’s successors took the idea of a ‘history of reason’ very seriously indeed, most famously Hegel. According to Hegel, philosophy is not only always ‘its time apprehended in thought’, we also have to think through its whole history, its overall ‘dialectical’ development, to arrive at ‘absolute truth’. Thus Hegel’s well-known dictum: ‘Philosophy is the history of philosophy’. In the twentieth century, the same basic conviction occurs again in Heidegger, although with a twist. Heidegger does not think that we can arrive at the truth through a synthesis of Western philosophy’s history. Rather, we have to think through that history to be able to break away from it, i.e., find radically different ways of thinking. Still, this involves an intense engagement with history. A third form of philosophical historicism takes Hegel’s idea that philosophy is ‘its time apprehended in thought’ in a more relativist and pessimistic direction. Here the view is that philosophy is always so closely tied to its original context that all we ever get are self-contained, self-justifying worldviews. None of them represents the truth any more than the others, and the idea of a progressive ‘history of reason’ is an illusion.

All three forms of historicism just mentioned, or the works of their proponents more generally, can be seen as part of the backdrop for the rise of analytic philosophy. In the cases of Hegel and Heidegger, this is well known. Moore and Russell, while initially attracted to the Neo-Hegelianism prominent in Britain at the end of the nineteenth century (Bradley, McTaggart, and others), rebelled against it around 1900, thereby turning British philosophy in the ‘analytic’ direction. Carnap, who had read Heidegger’s works and gone to public lectures by him in the 1920s, criticized him strongly in the 1930s. And while the criticisms by Moore, Russell, and Carnap focused on logical and metaphysical aspects, Karl Popper articulated a direct attack on ‘historicism’ in its Hegelian form in the 1940s and 50s. For all these critics, the problem with strong forms of historicism is that they are methodologically suspect. Opposition to them is then another explanation for, and an integral part of, analytic philosophy’s anti-historical self-image. Such opposition extends to later ‘continental philosophers’, including many post-structuralist and post-modernist thinkers, insofar as they have inherited historicist views.

So far I have made explicit several slogans and some arguments by analytic philosophers against studying the history of philosophy. I have also contrasted anti-historical approaches in analytic philosophy with some strongly historicist ones in continental philosophy. In doing so, I have not tried to be comprehensive. I have also painted with a broad brush. On closer inspection, some of these positions would, no doubt, reveal themselves to be more nuanced, harder to classify, and partly compatible with each other. But establishing that would require careful historical investigation – the very task whose value is in question. As analytic philosophers tend to assume they have found a different, more powerful, and fundamentally a-historical
methodology (or several of them), this task was not seen as important until relatively recently.

1.2 From philosophical legends to rational reconstructions

Analytic philosophy is over one hundred years old now. Thus it has a significant history of its own, i.e., an extended temporal development that invites systematic reflection. Increased attention to that history has brought the question of how to see the relationship between doing analytic philosophy and studying the history of philosophy closer to home; since how should analytic philosophy relate to its own history? Actually, in some sub-fields of analytic philosophy historically oriented work gained momentum already earlier. In the philosophy of science in particular, the issue of how to connect, or integrate, history and philosophy became central from the 1960s on. One major event in that connection was the publication of Thomas Kuhn’s *The Structure of Scientific Revolutions* (1962), frequently seen as a challenge to the anti-historical attitude of the logical empiricists. But several of the latter, including Carnap, actually welcomed Kuhn’s work with open arms, as is well established by now. Others, like Philipp Frank and Hans Reichenbach, had paid attention to both the history of science and the history of philosophy already in the 1940s and 50s. Moreover, since the 1980s and 90s, serious attention to history by philosophers of science has extended to the history of the philosophy of science itself.

The general view of analytic philosophy as a- or anti-historical is misleading and has exceptions in other respects as well. On the one hand, a number of influential analytic philosophers, such as Peter Strawson and Wilfrid Sellars, developed their views in explicit, thorough engagement with Kant’s philosophy. Indeed, engagement with historical figures extended through Leibniz, Descartes and others, all the way back to Ancient philosophy, as Gilbert Ryle’s work on Plato illustrates. Current philosophers like Robert Brandom, Tyler Burge, John McDowell, and Charles Parsons illustrate that phenomenon further. On the other hand, work on the history of analytic philosophy has been done by several well-known participants, including J.O. Urmson, A.J. Ayer, Michael Dummett, and Scott Soames. Others, like Richard Rorty, have added methodological reflections on how to approach the history of philosophy more generally, although such reflections are still relatively rare. And if we go beyond metaphysics and epistemology, which are often seen as the core areas of analytic philosophy, to moral, social, and political philosophy, attention to the history of philosophy is even more widespread (cf. historically oriented works by H. Frankfurt, J. Schneewind, Q. Skinner, J. Rawls, B. Williams, and others).

Contrary to the stereotypical view, it is not even true that the founding fathers of analytic philosophy saw no role for historical considerations. Frege’s most philosophical book, *Die Grundlagen der Arithmetik* (1884), does not just contain the criticism of historicist approaches mentioned above; its
whole first third is devoted to a discussion of both contemporary and earlier thinkers (e.g., Kant, Leibniz, and Hume), thereby providing important motivation for Frege’s project. For Russell the case is even clearer. Among others, his early engagement with Leibniz, in *A Critical Exposition of the Philosophy of Leibniz* (1902), had a lasting impact on him. Finally, even someone like Quine uses historical, or quasi-historical, considerations at points. As an illustration, consider how he presents Carnap (or a caricature of the early Carnap) as the culmination of the empiricist/positivist tradition in “Two Dogmas of Empiricism” (1951). Doing so is arguably a core part of Quine’s classic essay, i.e., indispensible for its main argument. (It drastically limits the range of alternatives Quine has to refute while promoting his own approach.)

What happens in a text like Quine's is not that a detailed historical account of an earlier figure is developed; Quine does not engage in history of philosophy in *that* sense. Crucial is, instead, how he sets up the dialectic situation in terms of a few (quasi-)historical ‘asides’, thus setting up a certain narrative. Quine’s followers elaborated this narrative further into what one might call a *philosophical legend* – a (quasi-)historical tale that is not examined critically but shapes people’s philosophical outlook. Here it is the legend of Quine having *refuted*, not just Carnap, but *logical positivism as a whole*, thereby inaugurating, together with Kuhn, a ‘post-positivist’ era. Likewise, there is the legend of Russell, together with Moore, having *refuted British Hegelianism*, with (a caricature of) F.H. Bradley as its representative. There is also the legend of Frege’s logic being *totally unprecedented*, in the sense of having simply sprung from his brow. Note again that accepting the latter two legends is integral for analytic philosophy’s self-understanding as a *radically new* kind of philosophy.

Such legends have lasting effects, although not necessarily in bad ways as they can motivate novel, fruitful work. But if assumed uncritically, they distort analytic philosophers’ perceptions of previous figures (more or less, depending on the case). At the same time, there is again the question of how seriously to take these legends (parallel to the anti-historical slogans discussed earlier). More generally, one can ask which exact role they should be seen as playing or what kind of status they are supposed to have. Do they constitute *actual historical claims*, to be evaluated accordingly; are they better seen as *guideposts* or *philosophical heuristics*; or something else? This issue would seem to deserve serious attention, both historically and philosophically. My basic point in this context is that (quasi-)historical assumptions such as these arguably do *philosophical work*. If this is true, not only Strawson or Russell but also Quine and others are ‘doing analytic philosophy historically’ (in a minimal, relatively a-historical way).

To pursue this point further, let me now distinguish between several different forms that attention to the history of philosophy can take. Neither Strawson, Sellars, or Ryle, nor Russell or Frege, and much less
Quine, practice what one might call *contextual history* of philosophy, with careful *historical reconstruction* at its core (nor did Kant for that matter who, like Frege, based his corresponding claims on textbook accounts). The latter would involve a sustained effort to do several things: to place the historical figures in question – not only Kant and Plato, but also Carnap, Russell, and Frege – in their original context; to carefully probe the background against which they developed their views; and to acknowledge differences to today. What analytic philosophers typically offer, instead, are engagements with these figures *as contemporaries*, i.e., as interlocutors assumed to have the same projects, questions, and assumptions as we do. All that matters, then, is that we analyze their concepts in an ‘anti-psychologistic’ manner and that we evaluate their thesis and arguments in a ‘justification-centered’ way.

This is clearly *one* form of engaging with historical figures philosophically. It can lead, and has led, to philosophical insights (as many of the cases mentioned illustrate). So as to have a label for it, we can talk about the *rational reconstruction* of past philosophers and developments. One great strength of such reconstruction is that it makes salient philosophically relevant aspects (concepts, theses, and arguments). On the other hand, features of a past thinker’s views that seem jarring are discounted or simply ignored, since the focus is on what can be ‘reconstructed rationally’. The main weakness of rational reconstruction is directly related to such a selective focus. Not only is the approach misleading if mistaken for historical reconstruction; the views of earlier figures may be distorted so much that the fruitfulness of the approach is limited also for its own purposes, especially in terms of not critically examining basic assumptions.

As we have seen, analytic philosophers often embrace rational reconstruction. By doing that, they are not entirely a-historical; but on the spectrum of more or less historically attuned approaches they are far on one side. Sometimes analytic philosophers also assume that rational reconstruction is the *only* way in which one can engage fruitfully with a figure such as Plato or Kant. And when it comes to paradigmatic analytic philosophers like Frege, Russell, and Carnap – who are taken to have set the very agenda still pursued today and to have provided us with the very tools still in use – engagement with them tends to be seen as involving no reconstruction at all. We can simply read their writings ‘straight’, or so the implicit, uncritical conviction. It may even appear strange to try to approach Russell, say, in a more contextual or historically reconstructive way. What would be the *point*, especially for philosophical purposes? It makes more sense to evaluate where someone like him got things right and where wrong, to record the successes while putting aside the rest, and thus, to accumulate the achievements we have so far. It is such an attitude, together with the underlying neglect of the weaknesses and limits of the method of rational
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reconstruction, which makes analytic philosophers still frequently appear a- or even anti-historical.

1.3 The varieties of philosophical history

Rational reconstruction can be fruitful, as already acknowledged, especially in cases where there is sufficient overlap between the background assumptions in play. But in other cases, the approach is more problematic. Moreover, by insisting on it exclusively – as the only way of doing history of philosophy – we limit ourselves unnecessarily. It is convictions such as these that can motivate taking historical reconstruction seriously philosophically. But then the question arises: What would that involve? To answer that question, note first that one can provide a historical reconstruction for various purposes, most obviously as a contribution to the discipline of history. In that case, we might talk of doing historical reconstruction as history. In what follows, my focus will be on historically oriented work done as philosophy, or in other words, on what may be called ‘philosophical history’.

Earlier, I made some suggestions about the sources of analytic philosophers’ a- or anti-historical attitude, as well as about how this attitude might be seen as justified (in terms of the discovery-justification distinction, the availability of a-historical tools, and reactions against radical forms of historicism). But other factors are most likely involved as well. In particular, what might play a role are certain stereotypes about doing history of philosophy. One example would be to think that such work can, or at least often does, address only historical questions (of the ‘when, where, and who’ form), or more specifically, that no philosophical evaluation is involved (no attention to ‘why’ questions in the relevant sense). This would mean that historical reconstructions are only possible, or are typically only pursued, as history. They are then often dismissed as mere ‘history of ideas’, or as ‘antiquarian history’ (history for the sake of the historical record).

One particular version of antiquarian history is ‘doxographic’ history: the simple listing of what past thinkers thought or wrote. Doxographic history can be useful philosophically, namely when it is taken as the basis for further analysis. It is more problematic when the views of past thinkers are reported and then accepted, pretty much directly, on the basis of mere authority (because they were held by thinkers ‘too deep and subtle to be approached critically by us’). This is the kind of ‘heteronomous’ thinking, or lack of thinking, that both Kant and Searle reject. It also falls under the following critical verdict by Arthur Schopenhauer (Hegel’s contemporary and fierce opponent):

History has always been a favourite study among those who want to learn something without undergoing the effort required by the real branches of knowledge, which tax and engross the intellect. (Quoted in Glock 2008, p. 93)
If doing history of philosophy amounted to nothing more, there would indeed be reason to dismiss it. However, a large amount of more sophisticated work in the history of philosophy should long have undermined that stereotype.

There are subtler assumptions that may also be associated, implicitly, with investigating the history of philosophy, or more specifically, the history of analytic philosophy, thus again discrediting it in the eyes of some. Let me briefly mention two examples. First, there is the view that, to study its history, one must be convinced that analytic philosophy is *moribund* or already *dead* (cf. the calls for various kinds of ‘post-analytic’ philosophy). This constitutes a form of *nihilism*, or of the view that the only task left is to record analytic philosophy’s follies, its contributions to the ‘graveyard of ideas’. A second example is to assume that historical considerations are able to *replace* philosophical ones, not just to supplement them. This might be based on some general *skepticism* about philosophy. Or the claim could be that philosophy can be *reduced* to psychological, sociological, or political factors, which can then be studied historically. Determining sociological mechanisms, say, for the acceptance of philosophical claims would then take the place of their logical analysis and justification. Here we are dealing with a descendent of the psychologistic/historicist views rejected by Frege.

Many analytic philosophers will dismiss such nihilist, skeptical, or reductionist views (as unjustified or self-undermining). Alternatively, one can treat them as substantive philosophical claims that require examination and further evaluation. Perhaps one can also re-interpret the second in a less exclusive way, so that it might be combined fruitfully with more ‘analytic’ approaches. In any case, I would think that it is only a small *minority* of philosophers working on the history of analytic philosophy that holds such views. And while there may be a good number of historians of analytic philosophy who are motivated in part by the conviction that the analytic tradition has taken some *unfortunate turns* recently, this does not necessarily mean that they take analytic philosophy, or philosophy more generally, to have no future. In fact, reflecting on its history, in a self-conscious and philosophically insightful way, can constitute an attempt at putting it back on track. Or more generally, it can be an attempt to *enrich* analytic philosophy rather than to replace it.

This brings us to my main concern. Assume that we reject the stereotypes and controversial views just mentioned. Assume, in other words, that we want to engage with philosophy historically but *not* along such lines. Are there any alternatives to rational reconstruction, especially ones that are genuinely historical *and* philosophical? There are three reasons, I would suggest, which make this question especially urgent for historians of analytic philosophy today. First, it has become questionable whether Frege, Russell, or Carnap, say, share their basic concepts, questions, and projects *fully* with us. Second, it has become problematic that there really is *one* set of basic
assumptions shared by all contemporary analytic philosophers, thus giving it a unified focus and agenda. Third, the boundaries between the analytic and neighboring traditions, such as pragmatism and phenomenology, have come to appear somewhat arbitrary as well.

Once more, are there any alternatives to rational reconstruction, including ones that would allow investigating such worries and suspicions further? In the rest of this section, I will discuss – tentatively and in a preliminary way – two approaches that would seem to fit the bill. The first is to let rational reconstruction and historical reconstruction correct each other, while the second is to engage in historical reconstruction as philosophy. Before describing each of these alternatives further, let me stress one point: My goal is not to establish that philosophy has to be done historically (as is assumed in strong forms of historicism); nor do I reject rational reconstructions in general. Rather, my concern is whether there are other forms of ‘philosophical history’, in addition to rational reconstruction, that could be used and, if so, what might be gained.

My first alternative approach brings us back to rational reconstruction, but not just by itself. As indicated above, the main weakness of this approach is that, by not questioning whether a past philosopher used the same concepts, relied on the same basic assumptions, or pursued the same projects as we do, we may distort his or her views too much even for the good of the rational reconstruction. Now, being aware of that weakness can lead us to a way of avoiding it, at least to some degree. Namely, why not confront our rational reconstruction with a historical reconstruction (done as history), thereby keeping the former honest? To be really effective, such a confrontation may have to occur repeatedly, in which case the correcting – also in the other direction, by keeping the historical side philosophically honest – would take several rounds. But that is fine. One might even have in mind a continuing dialectic between rational and historical reconstructions, i.e., a sustained back-and-forth between them.

How should such an enriched procedure be assessed, i.e., what are its strengths and weaknesses? The procedure presupposes that we have the two ingredients at hand, i.e., we already know how to do rational and historical reconstruction. The idea is to combine the strengths of both, while correcting their respective distortions. But how exactly is that supposed to work? For example, it may be fairly clear in which way historical reconstructions can help with respect to the distortions rooted in stereotypes or quasi-historical legends. Yet how can it convince us to go beyond the basic philosophical assumptions typically held fixed in rational reconstructions? In other words, how are we to use historical methods to refine underlying, often only implicit philosophical convictions? The more general difficulty here is this: How can we prevent the two sides in our new procedure from remaining too external to each other? Would it not be better to combine, or synthesize, rational and historical reconstruction more thoroughly, in one procedure? That thought leads to my second suggestion.
As a second alternative to rational reconstruction, we can try to do the following: We study an earlier philosopher without simply assuming that he or she shares all relevant background assumptions with us. Instead, we carefully recover – using tools borrowed from history, philology, as well as from philosophy (archival research, close textual exegesis, and attention to context) – what the philosopher’s core concepts, basic assumptions, and main project actually were. We also refrain, at least initially, from evaluating the recovered ideas by using current standards. What we do, on the other hand, is to think them through internally, i.e., to evaluate them by using the standards and the understanding of the time. The latter is what makes the approach philosophical (not just ‘historical reconstruction as history’, but ‘historical reconstruction as philosophy’). It is also what makes it a form of ‘historicism’, albeit a relatively modest one.

One main strength of this second alternative is that it can bring to light philosophical assumptions that are different from ours, thus de-familiarizing us from the latter in fruitful ways. It may also make us realize that certain views of a past philosopher that we initially discarded make sense after all, or even, that they provide the key to the philosopher’s positions. One worry and potential weakness is this: Does this approach presuppose that we can get at past thinkers completely on their own terms, thus giving a ‘fully proper’ interpretation; and if so, is that realistic? In other words, can we simply ignore all present concerns in it? One might respond that it seems clearly possible, as examples illustrate, to put aside the latter to some degree; and if done self-consciously, that should be enough for most purposes. But now a converse worry, or tension, emerges: The more we bracket present concerns (moving away from rational reconstruction), the less relevant the results will be for current philosophy, won’t they?

That tension might suggest looking for a further alternative, one in which an even more thorough integration of logical analysis and historical understanding is achieved. But what form could that take? Should we think of it as a more sophisticated form of rational reconstruction, open to calling its own assumptions into question? Or should we consider a subtler version of historical reconstruction as philosophy (my second alternative), where we balance evaluating past philosophers on their own terms more with current concerns and use current standards? Or again, should we try to combine rational reconstruction and historical reconstruction as philosophy (a hybrid of my two alternatives)? Actually, these suggestions all point in the same general direction. The basic goal would be to use historical, context-sensitive tools as well as those of rational reconstruction and analysis to bring to light ideas of current philosophical interest.

With respect to any resulting approach, if applied successfully, it would make sense to speak of ‘philosophical history’ or of ‘doing philosophy historically’. How exactly the historical tools are used will vary. Similarly it will vary in which way, or when exactly, current philosophical concerns are
brought to bear. Further *hybrid* forms may also be possible, since the boundaries between the approaches discussed so far are not sharp. (They are more like *ideal types*, including rational reconstruction.) In any case, each such approach should be applicable to the analytic tradition and to other developments in philosophy. What, then, would make something an instance of ‘doing analytic philosophy historically’? The combination of two things: that the primary focus is on a figure or theme in the analytic tradition; and that the study is guided, to a significant degree, by concerns from analytic philosophy, past or present.

Finally, let me return to a question touched on repeatedly along the way: Why might an analytic philosopher *want* to ‘do analytic philosophy historically’? Why all the extra effort, in other words? One quick answer is: to counteract the distortions and other limiting effects of stereotypes, quasi-historical legends, and rational reconstructions. Another answer has already come up as well: to de-familiarize us from our current concepts and assumptions so as to be able to question them fruitfully. Further benefits include: to recognize fine, otherwise missed nuances in the views of past philosophers, especially ones that can still play a role today; to understand better, and partly to recover, what the agenda of analytic philosophers was, is, and should be; to motivate corresponding projects further, by becoming aware of their original contexts, their developments, and possible extensions; and to evaluate more accurately the importance, as well as the limits, of the results that have already been achieved. This list is far from complete. In fact, it is meant more as a starting point for further discussion than as anything final. Continuing methodological reflections are called for in this area.\textsuperscript{39}

2 Historical reflections on the analytic tradition

2.1 A survey of work on the history of analytic philosophy

Research on the history of analytic philosophy has produced rich results already, especially during the last twenty-five years. These results illustrate several of the points made, more generally and abstractly, in the first part of this introduction. Their presentation has also taken several different literary forms. The two primary forms, for our purposes, are: the research article and the research monograph (including single-authored collections of essays).\textsuperscript{40} A variant of the research monograph that is historically oriented by nature is the philosophical biography.\textsuperscript{41} Research articles on the history of analytic philosophy have appeared in mainstream and more specialized journals; but one main venue for publishing them is collections of essays. There are again several variants: collections of newly commissioned essays on a single philosopher or group of philosophers; retrospective collections of classic articles on a significant figure; and collections on more general themes, often
In addition, various survey articles on the history of analytic philosophy have been published, also in wider-ranging works. At this point in time, ‘history of analytic philosophy’ is an established subfield within the history of philosophy (with special sessions at conferences, job advertisements, etc.). It is also a fascinating subfield, one that requires no further justification for its practitioners.

What have been some of the main themes and objectives in this connection? One goal has been to counteract the bad effects of stereotypes and other distortions, including those introduced via rational reconstructions, as discussed above. Some methodological debates have also occurred, as prompted by particular cases. Certain rational reconstructions have been challenged more internally, others have been refined, and new ones, for individual philosophers and broader developments, have been proposed. Along all of these lines, neglected or forgotten but arguably important figures have been rediscovered. In addition, the relationship between analytic philosophy and other philosophical traditions has been explored in novel ways. Finally, there have been debates about the direction analytic philosophy should take in the future, given its trajectory so far. I now want to exemplify each of these themes via some paradigmatic cases or clusters of cases.

A first important cluster of work in history of analytic philosophy concerns the ‘founding fathers’ of the analytic tradition: Frege, Russell, Moore, and Wittgenstein. Here Gottlob Frege was studied by himself in various ways, but he also occasioned an early methodological debate. That debate started when Michael Dummett’s rich and influential, but also blatantly a-historical, rational reconstruction of Frege’s views on logic and language was challenged by Hans Sluga, who opposed it in terms of a much more historically oriented perspective. One of Sluga’s main targets was Dummett’s claim that Frege’s logic was totally unprecedented, i.e., had no roots in earlier philosophy. Other interpreters have challenged Dummett’s reading of Frege also in less historical ways. The ensuing debate has produced several different outcomes: refinements of Dummett’s position, as developed by him in response to Sluga; alternative rational reconstructions; and alternative historical reconstructions. A more recent, and again largely a-historical, reconstruction of Frege, as developed by Crispin Wright, Bob Hale, and others, concerns his philosophy of mathematics. In that case too, various responses have been offered, including approaches that would qualify as ‘philosophical history’.

While the debate about Frege is largely confined to academic scholarship, Bertrand Russell has long had a more widespread impact, even outside of philosophy, and this is reflected in the writings on him. With respect to the roots and early development of his views, several historically sensitive and philosophically subtle accounts are now available, including some philosophical biographies. They clarify not only his philosophical views and their development, but also his role as a public figure. More recently, Russell’s
‘refutation’ of Bradley and British Neo-Hegelianism has been addressed critically, including its lasting effects on analytic philosophy’s self-image. Other historical work on Russell has clarified, and partly corrected, how to view his relationship to Frege, Wittgenstein, and, e.g., to the Austrian philosopher Alexius Meinong. Compared to Frege and Russell, G.E. Moore has received less attention, although in his case too, novel light has been shed on the origins, development, and remaining significance of his views. As some of Moore’s ideas continue to play an important role in sub-fields such as epistemology and ethics, as well as in debates about philosophical methodology (in several ways), this has led both to rational reconstructions and to more historically based studies of him.

A second thinker from the analytic tradition who has captured the imagination of people far beyond philosophy is Ludwig Wittgenstein. Like in Russell’s case, this has led to some rich biographical studies. With respect to the early Wittgenstein, one lively debate over the last 25 years concerns the clash between ‘traditional’ or ‘metaphysical’ readings of his writings (prominently defended by Peter Hacker and his students) and ‘resolute’ or ‘therapeutic’ alternatives to them (as introduced by Cora Diamond and defended by others as well), with ‘elucidatory’ readings attempting to find a middle ground. Which of these interpretations is most appropriate matters not only for understanding Wittgenstein’s early views, but also his relationships to Frege, Russell, and to the whole analytic tradition. Moreover, it affects how we should approach Wittgenstein’s later writings (including whether or not to see them as part of ‘analytic philosophy’). The core of these debates has been how best to characterize Wittgenstein’s methodology. One further issue in the recent literature is whether to distinguish not just between ‘early’ and ‘later’ Wittgenstein, but perhaps, to recognize ‘very late’ changes in his views in addition (in notebooks from the last few years of his life).

I mentioned earlier that research on the history of the philosophy of science has been pursued vigorously for a while now. Sometimes this overlaps with work on other themes in the history of analytic philosophy. One good example is the sustained attention paid to Rudolf Carnap, the Vienna Circle, and the fate of logical empiricism. In fact, such work constitutes a second main cluster in history of analytic philosophy. The philosophical legend of Quine’s refutation of Carnap, or of logical positivism as a whole, has been challenged thoroughly. Similarly for other stereotypes – some based on taking A.J. Ayer’s *Language, Truth, and Method* (1936) to be representative of logical empiricism. By now it is clear that Carnap’s views have deep Neo-Kantian, partly also Husserlian, roots. They underwent subtle changes, from his early (more positivist) position to a later, significantly modified (more pragmatist) perspective. Accordingly, attention has shifted, first, from Carnap’s *Der Logische Aufbau der Welt* to *Logische Syntax der Sprache*, and then, to his later work on ‘explication’. Very recently, the
notion of ‘conceptual engineering’ has become central for interpreting Carnap.48

Carnap’s views do not just depart from crude positivist stereotypes in a number of ways; they were also more embedded in a social and political agenda than was often assumed, especially early on in his career. Wider-ranging work on the Vienna Circle, e.g., on its connections to the Bauhaus movement in architecture, has illustrated that fact. What has emerged along the way is that the Vienna Circle was far less monolithic and dogmatic than often presumed. Various later criticisms of logical positivism already came up within it and were discussed in a pluralistic manner, as the ‘protocol sentence debate’ from the 1930s illustrates. Recognition of that fact has led to the additional insight that members of the Circle other than Carnap deserve serious, sustained attention as well, so that Quine’s reduction of logical positivism to Carnap is misleading in that way too. Otto Neurath, in particular, is now seen as an important figure, partly because of his anticipations of (and, for some, superiority to) Quine’s later naturalism.49

Beyond Carnap and Neurath, Moritz Schlick has started to receive careful scholarly treatment; similarly for Hans Reichenbach, the head of the Berlin Group of logical empiricism. And besides the influence of Neo-Kantianism (especially of the Marburg School: Cohen, Natorp, Cassirer) and Husserl on them, connections to other figures and movements, also outside of analytic philosophy, are being reconsidered in depth. This includes: the Lvov-Warsaw school of ‘Polish Logic’, most prominently Alfred Tarski; Henri Poincaré and related conventionalist thinkers; phenomenologically inspired scientists like Hermann Weyl; and American pragmatism (including its German and Austrian offshoots). One recently rediscovered theme in this connection is the development of mathematical logic during the 1920s and 30s, not just in Göttingen (Hilbert and his school), but also in Vienna (Gödel’s work, including its relation to Carnap), in Warsaw (Tarski and his co-workers), and elsewhere. And of course, there has long been interest in the philosophy of physics from the early twentieth century; similarly for the philosophy of mathematics from that amazingly fertile period.50

Besides Frege, Russell, Wittgenstein, and Carnap, four analytic philosophers who have received sustained attention so far are: Karl Popper, W.V.O. Quine, Kurt Gödel, and Alfred Tarski. Like Russell and Wittgenstein, Popper was a public intellectual, influential far beyond academic philosophy. Hence, not only his role in the history of the philosophy of science has been re-examined (his relationship to the Vienna Circle, to Kuhn’s work, etc.), but also his social and political views. Less widely known outside philosophy, Quine was a hugely influential ‘philosophers’ philosopher’ during the second half of the twentieth century, both by turning many analytic philosophers in a naturalist or pragmatist direction and by opening up the possibility of ‘analytic metaphysics’ (partly against his own intentions).
All of this is reflected in the literature. The case of Gödel, including the mystique surrounding both his personality and his famous incompleteness theorems, has again led to substantive biographical work. As he was a pivotal figure in logic and the foundations of mathematics for much of the twentieth century, he has been a focal point in that context as well. Tarski, finally, exerted a significant influence not only on logic and the philosophy of language, among others through his interactions with Carnap and Gödel, he also influenced the disciplines of mathematics and linguistics strongly, as various recent studies have shown.51

More briefly now, the literature on the history of analytic philosophy has started to concern itself with the following figures as well: A.J. Ayer, Donald Davidson, Michael Dummett, Paul Grice, Carl Hempel, Thomas Kuhn, Imre Lakatos, Frank Ramsey, Richard Rorty, Wilfrid Sellars, and Peter Strawson. Curiously, neither J.L. Austin nor Gilbert Ryle has received comparable attention yet (although this is changing); similarly for, e.g., Elisabeth Anscombe, Paul Feyerabend, and Gareth Evans. With figures such as Jaakko Hintikka, Saul Kripke, Hilary Putnam, and John Searle we still lack the critical distance for doing genuine historical work (also since they are still alive); but this has not prevented rational reconstructions of their views from appearing. In terms of main themes in the history of analytic philosophy, those that have received the most attention so far include: the sense-reference distinction (Frege, Russell, Kripke, and others), the analyticity debate (Carnap versus Quine); rule following and realism (Wittgenstein, Kripke, Putnam); and pragmatic themes (Rorty, Putnam, Brandom); similarly for the relationship between analytic philosophy and neighboring traditions, e.g., Austrian philosophy and, related to it, early phenomenology (Bolzano, Brentano, and Husserl). And again, there have been some attempts at general surveys of analytic philosophy’s past, as well as discussions of possible directions for its future.52

Let me round off this survey with a few suggestions about what is still missing, and some speculations about what might come next, in research on the history of analytic philosophy. I already mentioned that Austin, Ryle, and ‘ordinary language philosophy’ have been somewhat neglected. This holds even more for a strand in the philosophy of science that was rooted in their works: that represented by R.N. Hanson, M. Hesse, M. Scriven, S. Toulmin, and others. Both of these groups are overdue for careful historical reconsideration, I would say. Similarly, more attention seems due to the relation between analytic philosophy and some schools in the philosophy of science whose approaches have a strong historical flavor: not just Neo-Kantianism (including, say, Ernst Cassirer’s work in the history and philosophy of mathematics) and phenomenology (also including Heidegger, Gadamer, and later figures), but French ‘historical epistemology’ (e.g., L. Brunschvicg, G. Bachelard, J. Cavaillès, and J. Canguilhem).

The development of several sub-fields of analytic philosophy, too, seems ripe for genuinely historical treatment. Here I would include: metaphysics
(from Frege’s, Russell’s, and Moore’s metaphysical views, through the rejection of metaphysics in logical positivism, to its rehabilitation as ‘analytic metaphysics’ by Quine, Kripke, D. Lewis, and others); epistemology (echoes of Moore and the later Wittgenstein, but also the Gettier debate); the philosophy of mind and of psychology (the ‘Cognitive Revolution’ and the ‘mentalistic turn’); and the philosophy of language (treated almost exclusively in terms of rational reconstructions so far). Finally, which half-forgotten or largely neglected analytic philosophers will be rediscovered as still, or again, important? Perhaps some of the following: C.D. Broad, Roderick Chisholm, Philipp Frank, Susanne Langer, Ruth Marcus, Ernest Nagel, Susan Stebbing, A.N. Whitehead, Cook Wilson, or John Wisdom, to mention ten figures with a range of different views and interests?53

After this brief survey, let us now turn to the contributions to the present volume.

2.2 Abstracts for the essays in this volume

Part I: Case Studies

The first part of this volume contains four case studies focused on particular analytic philosophers: Russell (and his role in the rise, as well as for the self-image, of analytic philosophy), Carnap (and the particular form of conceptual engineering practiced by him), Quine (and the relation of his work to Carnap’s, as reinterpreted in recent literature), and Ryle (his methodology and the continuing relevance of his philosophy of language). Each of these studies either continues an earlier debate or pushes research in the history of analytic philosophy in a new direction.

Stewart Candlish, “Philosophy and the Tide of History: Bertrand Russell’s Role in the Rise of Analytic Philosophy” This first essay examines the distinction between analytic philosophy and its history by looking afresh at several prominent themes in the most influential writings of Bertrand Russell, from the end of the nineteenth century through the first quarter of the twentieth century. These include: Russell’s early conception of propositions and their constituents; the theory of denoting phrases; the multiple relation theory of judgment and its associated account of truth; the theory of definite descriptions; Russell’s view of the status of mathematics; and his treatment of the controversial topic of relations. Overall, the essay exposes some myths, and their attendant dangers, about the origins of analytic philosophy.

Alan Richardson, “Taking the Measure of Carnap’s Philosophical Engineering: Metalogic as Metrology” In recent years, a number of scholars, including Richard Creath, A.W. Carus, and Sam Hillier have attributed an engineering conception of philosophy to Rudolf Carnap. This second essay attempts to specify a particular type of engineering sensibility one might attribute to him; it argues that Carnap’s attitudes in logic and metalogic were based in
the development of measurement technologies in the science of metrology. This perspective is explored in relation to Carnap’s long-term interest in scientific measurement, especially his often overlooked 1926 monograph, *Physikalische Begriffsbildung*. The resulting account of Carnap’s philosophical attitude is then used to illuminate his side of the famous debate with Quine regarding the analytic/synthetic distinction. The essay ends with a consideration of whether questions in philosophy of technology reveal more telling weaknesses in Carnap’s position than do the standard Quinean objections.

*Peter Hylton, “Quine and the Aufbau: The Possibility of Objective Knowledge”* The third essay also concerns Carnap and Quine, but now with the focus on the latter. Quine interpreted Carnap’s *Logischer Aufbau der Welt* as putting forward an empiricist epistemology, along the lines suggested by Russell’s *Our Knowledge of the External World*. Over the last twenty years or so, this interpretation has been disputed by various interpreters. They have put forward an alternative that emphasizes the neo-Kantian aspects of the *Aufbau* and, in particular, its concern with what makes objective knowledge possible. This essay investigates how we should think about the relationship between Quine’s epistemology and that of the *Aufbau*, in the light of this new interpretation. It argues that Quine is engaged in what is in some sense the same enterprise as that which the new interpretation attributes to the *Aufbau* but in very different ways; and that there is something general to be learned about the relation between Carnap and Quine from these differences.

*Julia Tanney, “Ryle’s Conceptual Cartography”* The fourth essay in this volume traces ideas in philosophical logic that ground Gilbert Ryle’s work in *The Concept of Mind*. Although he is sometimes mentioned along with Wittgenstein and Austin as an ‘ordinary language’ philosopher, Ryle’s affinities and his independent development of notions that are often associated with Wittgenstein and Austin have gone largely unnoticed, especially in discussions of circumstance-dependency or context-sensitivity in the philosophy of language and related areas. In locating Ryle’s thoughts in the context of his own elaboration of twentieth-century philosophical logic and in emphasizing his rejection of referential theories of language still assumed in much work today, the essay hopes to make evident the continuing relevance of Ryle’s work for a more satisfying understanding of the distinctively conceptual nature of philosophical investigation.

**Part II: Broader Themes**

In the second part of this volume, the following broader themes come into focus: the philosophy of logic (and Frege’s place in its history); the philosophy of mathematics (Frege’s and Dedekind’s legacies, including their relation); epistemology and the philosophy of psychology (starting with Russell’s work on our knowledge of the external world); and the philosophy of language
(the analytic/synthetic distinction, not only in terms of the Carnap-Quine debate but also C.I. Lewis’ role in it). There is no sharp dividing line between the essays in Parts I and II, as this second group consists of case studies as well, although ones with a somewhat farther reach.

Jeremy Heis, “Frege, Lotze, and Boole” Thirty years ago, Michael Dummett and Hans Sluga engaged in a prolonged controversy over the value of locating Frege's writings in the context of late nineteenth century German philosophy. This essay argues, against Dummett, that, by reading Frege in his historical context, one can judge in a more balanced way the philosophical significance of the new logical language that Frege developed. This historiographic point is illustrated by showing that Frege's criticism of the theory of concept formation implicit in Boolean symbolic logic agrees in significant ways with a criticism given by his contemporary Hermann Lotze. But the essay also argues, against Sluga, that the substantial overlap in their criticisms of Boole should not obscure for us the great philosophical advance that Frege made over Lotze, one that would not have been possible without the invention of Frege's Begriffsschrift.

Erich H. Reck, “Frege or Dedekind? Towards a Reevaluation of Their Legacies” Within the analytic tradition, the philosophy of mathematics has long played an important role, ever since the pioneering works of Frege and Russell. Both their and Richard Dedekind’s related contributions to the foundations of mathematics are widely acknowledged. The philosophical aspects of Dedekind’s contributions have been received more critically, however. In this sixth essay, Dedekind’s philosophical reception is reconsidered. At its core lies a comparison of Frege’s and Dedekind’s legacies, within analytic philosophy and outside it. While the discussion proceeds historically, it is shaped by concerns from current philosophy of mathematics, especially by debates about neo-logicist and neo-structuralist positions. Philosophical and historical considerations are thus intertwined. The underlying motivation is to rehabilitate Dedekind as a major philosopher of mathematics, in relation, but not necessarily in opposition, to Frege.

Gary Hatfield, “Psychology, Epistemology, and the Problem of the External World: Russell and Before” The seventh essay in this volume brings us back to Russell’s works. It examines the background to Russell’s invocation of psychological considerations in his work on our knowledge of the external world from 1913–14. This background includes the natural realism of William Hamilton, its criticism by J.S. Mill, and the ongoing discussion of the problem of the external world by English philosophers in the 1890s and the following decade, including James Ward, G.F. Stout, S. Hodgson, T. Case, L.T. Hobhouse, and G. Dawes Hicks. In light of this examination, as well as Russell’s own description of his ‘logical analytic’ method, the conclusion
is that, on one historically reasonable conception of psychologism (that deriving from Kant), Russell’s appeal to psychology is not psychologistic, whereas it can be so classed in accordance with the more extreme view (stemming from Frege) that any appeal to the data of experience in epistemology counts as psychologism.

**Thomas Baldwin, “C.I. Lewis and the Analyticity Debate”** In ‘Two Dogmas of Empiricism’, Quine brackets together C.I. Lewis and Carnap as two pragmatists who, by remaining committed to the analytic/synthetic distinction, have not taken their pragmatism to its proper conclusion. The ensuing debate between Carnap and Quine has been much discussed. In this essay, Lewis’s side in this debate is reconsidered, beginning with an account of his pragmatist theory of our a priori conceptual schemes and comparing this with Carnap’s empiricist logic of science. In assessing the impact of Quine’s arguments on Lewis’s position, it is proposed that Kuhn’s paradigms indicate an enduring role for a modified version of Lewis’s conceptual schemes in philosophy of science; and also, that Wittgenstein’s rule-following discussion constitutes a non-Quinean pragmatist approach to logic which connects with some themes from Lewis’s writings, though without the odd combination of Platonism and voluntarism which Lewis affirms.

**Part III: Methodological Reflections**

Finally, the volume contains four essays with sustained reflections on what, from a methodological or historiographic point of view, is involved in studying the history of analytic philosophy and what its benefits might be. This includes: an analysis of the origin and later uses of the notion of rational reconstruction; a reconsideration of the relationship between logical empiricism and the study of the history of science; a critique of some recent work in the history of analytic philosophy as well as the exploration of alternatives to it; and an answer to the question of whether analytic philosophy is moribund, since we are now so much concerned with its history.

**Michael Beaney, “Analytic Philosophy and History of Philosophy: The Development of the Idea of Rational Reconstruction”** Analytic philosophers often either scorn or simply ignore the history of philosophy. Where interpretations have been offered of past philosophical works, in what we can call ‘analytic’ history of philosophy, they have tended to be ‘rational reconstructions’. In recent years, however, philosophers trained in the analytic tradition have begun to look at the history of analytic philosophy itself more seriously, thus bringing questions about the relationship between philosophy and history of philosophy closer to home. This essay considers some of the philosophical and historiographical presuppositions and implications of this debate, focusing on the idea of rational reconstruction. The latter developed alongside analytic philosophy and holds the key to understanding one
central thread in the relationship between analytic philosophy and history of philosophy.

_A.W. Carus, “History and the Future of Logical Empiricism”_ In studies of the history of the philosophy of science, it has become almost conventional wisdom to regard the differences between Carnap and Thomas Kuhn as resting on misunderstandings and differing rhetorical emphases. This essay argues, in contrast, that the differences were quite fundamental; it was no accident that the acceptance of Kuhn's agenda brought about the demise of logical empiricism. However, room was left in Carnap's conception of 'descriptive pragmatics' for a quite different approach from Kuhn's to the history of science. This opening was exploited by Howard Stein, one of Carnap's students, who used a historical perspective to overcome some of the weaknesses in Carnap's philosophy. Though much less in the limelight than Kuhn and his progeny, Stein's work has had some influence. The resulting body of writings deserves more attention and can be regarded as the basis for a historically informed continuation of logical empiricism.

_Michael Kremer, “What Is the Good of Philosophical History?”_ The next essay in the volume uses Scott Soames's recent work in the history of analytic philosophy as a springboard to examine the value of doing philosophy historically. Soames's work presents a choice between two unsatisfactory conceptions of philosophical history, antiquarianism and presentism. The author of this essay agrees with Soames in rejecting antiquarianism, but draws on general historiography and the historiography of science to show the dangers of Soames's presentism. Following Bernard Williams, a third possibility for understanding the value of philosophical history is developed. Along its lines, work in philosophical history is distinctive in that it is a way of doing philosophy. This requires that we attempt to understand the philosophical past, a task that both presentism and antiquarianism avoid. The essay concludes with a brief discussion of some examples illustrating the value of the approach to philosophical history it recommends, drawn from the work of Cora Diamond.

_Hans-Johann Glock, “The Owl of Minerva: Is Analytic Philosophy Moribund?”_ The current state of analytic philosophy is a combination of triumph and crisis. On the one hand, it is now the dominant force within Western philosophy. On the other hand, there are continuous rumors about the 'demise' of analytic philosophy and complaints about its actual or alleged ills. In view of this situation, the last essay in the volume addresses the following related questions: Has analytic philosophy ceased to be a distinct and potentially vibrant movement? Is the historical turn a manifestation of, or perhaps even a contributing factor to, its demise? Is analytic philosophy in the course of being replaced by a 'post-analytic' philosophy? And should it be
superseded by such a movement? The author gives a tentative and qualified ‘No’ in answer to all of these questions. To substantiate these answers, he draws not just on classics of analytic philosophy, but also on recent contributions to the burgeoning field of the history and methodology of analytic philosophy.

2.3 Representative bibliography (history of analytic philosophy)


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Notes

I would like to thank Michael Beaney, as editor of the series ‘History of Analytic Philosophy’, both for supporting the present collection from the beginning and for helpful suggestions concerning this introduction. I am also indebted to Jeremy Heis, Pierre Keller, Sally Ness, and Clinton Tolley for various conversations related to the introduction.

1. Talking about ‘the history of philosophy’ is systematically ambiguous: it can refer to a historical development or to the theoretical study of it. I have not adopted a strict terminological distinction in this connection, assuming it will be clear in context what is meant (sometimes both, as in this case).

2. Cf. Glock (2008), pp. 91, 92, and 211, respectively, also for the sources of these remarks. For another illustration, cf. the quote at the beginning of Stewart Candlish’s essay in this volume.

3. For example, the remark attributed to Quine above comes from MacIntyre (1984), an article that defends a much more historically oriented approach to philosophy than Quine’s.

4. These remarks are from Fodor’s *Hume Variations* (2003), again as quoted in Glock (2008), p. 92.


6. The theme is introduced in Russell (1900); for a more sweeping application, cf. Russell (1945).

7. Not all ‘ordinary language philosophers’ were dismissive of studying the history of philosophy, though. As two important exceptions, I will come back to Ryle and Strawson below.

8. Cf. Knobe & Nichols (2008) for a manifesto for experimental philosophy. When I was in graduate school (during the 1980s and early 90s), it was cognitive science that played a similar role. Thus a cognitive scientist in my department liked to provoke us students, as well as some of his colleagues, with the declaration that ‘nothing older than five years is worth reading’.
10. Cf. Kant (1881/87), A 836/B 836; and there are similar remarks elsewhere.
11. For Hegel, cf. the Preface to Hegel (1820), also his (1807). For Heidegger, cf. his (1926), (1930), and with respect to his later work, the discussion in Piercey (2009).
12. One early exponent of such views was J.G. Hamann; for a twentieth-century version, cf. Spengler (1923). For the related rise of ‘historicism’ in the discipline of history, cf. Beiser (2012). As is not hard to see, these forms of historicism are in direct conflict with each other; cf. also Bambach (1995).
14. Neo-Kantianism, both in its Marburgian and its Southwestern version, is a different case. It focused on history (of philosophy and science) as well, but in a less speculative, less pessimistic way. It thus represents a much less radical form of historicism. Similarly for Husserl who, while trying to make philosophy scientific, engaged with history (of science and of philosophy) fruitfully in his later writings. For Neo-Kantianism, cf. Richardson (1998), Friedman (2000), Makreel & Luft (2010), and Beiser (2012); for Husserl, cf. Hyder & Rheinberger (2010).
15. In Foucault’s, Lacan’s, and Derrida’s versions of ‘neo-historicism’, the importance of historical context is again emphasized (in reaction to the anti-historical bent of French structuralism).
16. Cf. Reichenbach (1951) and, especially, Frank (1957). For Carnap’s sympathetic reaction to Kuhn’s work, see Reisch (1991); but compare the essay by A.W. Carus in the present volume.
17. This is witnessed by the founding of HOPOS – the International Society for the History of the Philosophy of Science – in the 1990s; cf. www.hopos.org.
21. The present volume focuses on metaphysics and epistemology (including the philosophy of science, logic, and language). This reflects my own predilections, no doubt, but it also corresponds to where most work on the history of analytic philosophy has been done so far (cf. section 2.1 below).
22. Concerning Quine, Carnap, and the argument in ‘Two Dogmas’, cf. Creath (2007). There are further cases like Quine’s. To mention just one, the quasi-historical way in which Saul Kripke appeals to (idealized versions of) Frege and Russell in Naming and Necessity (1972) is arguably like Quine’s use of Carnap.
23. For Russell, cf. Candlish (2007), also Stewart Candlish’s essay in this volume (in which he talks about corresponding ‘myths’); for Frege, cf. the debate between Dummett and Sluga as examined in Jeremy Heis’ essay. I am indebted to Sally Ness for suggesting the phrase ‘philosophical legend’.
24. Similarly for various quasi-historical organizing schemas as used in analytic philosophy, especially in pedagogical contexts. Consider, e.g., the assumption that a ‘linguistic turn’ took place in the 1950s or earlier (cf. Rorty 1967, also Dummett
the division between ‘ideal language’ and ‘ordinary language philosophy’ (cf. Stroll 2000); or the claim that Kripke and others brought about a ‘revolution’ in the philosophy of language in the 1970s (cf. Wettstein 2004); similarly for the occurrence of a ‘mentalistic turn’ in the 1980s (Williamson 2007). Finally, the very distinction between ‘analytic’ and ‘continental’ philosophy fits in here insofar as it has a historical dimension.

25. Cf. Rorty et al. (1984), Sorell & Rogers (2005) for reflections on ‘doing philosophy historically’ (including earlier uses of that phrase). I will come back to this issue more systematically below.

26. For earlier uses of the term ‘rational reconstruction’ in a similar context, see Rorty (1984). For further discussion, including more examples, cf. the essay by Michael Beaney in this volume.

27. The way in which Strawson is willing to discount large parts of Kant’s idealism is a good example.

28. A recent, almost classic example of such an approach is Soames (2003). When the development of analytic philosophy is analyzed along such lines, it results in ‘presentist’ or ‘Whiggish’ history; cf. Michael Beaney’s and Michael Kremer’s essays in this volume for further discussion.

29. The essay by Jeremy Heis in this collection contains further suggestions in this connection.

30. A good example is Diogenes Laertius, whose record of past views has been quite helpful upon further analysis. Note that, like rational reconstruction, ‘doxastic’ history typically assumes that past philosophers were trying to answer the same questions as we are; cf. Rorty (1984).

31. A version of such nihilism has been attributed to Burton Dreben, who famously quipped: ‘Nonsense is nonsense, but the history of nonsense is scholarship’; cf. the motto to Floyd & Shieh (2001). One may wonder, again, about how seriously to take this quip; but see also Hart (2010), p. ix. For further discussion of this issue, cf. the contribution by Glock in this volume.

32. As examples, cf. Kusch (2000) and Pulkkinen (2005), both articulated in direct response to Frege (see also Kusch 1995). The ‘strong programme’ in the sociology of science provides a third example; cf. Bloor (1991). A fourth might be Foucault’s work, at least according to some interpretations.

33. One reason to be worried about this second issue is the increasing fragmentation of analytic philosophy, including the mutual alienation of various sub-groups within it: analytic metaphysicians, experimental philosophers, formal philosophers, historically oriented philosophers of science, and others.

34. Thus, one may have come to think (like me) that, say, Frege, Peirce, the early Cassirer, and the early Husserl are in many ways closer to each other than they are to a lot of current analytic philosophy.

35. Actually, I think examples of both of my alternatives to rational reconstruction can be found in the literature already. This should become clear in Part 2 of this Introduction.

36. In his contribution to the present volume, A.W. Carus recommends this approach. Cf. also Michael Beaney’s discussion of ‘dialectical reconstruction’ in his contribution, although it also point in the direction of my second alternative approach.

37. For further discussion, cf. Hatfield (2005) and both Michael Beaney’s and Michael Kremer’s contributions to this volume. There are also clear connections to the hermeneutic tradition in continental philosophy.
38. As John McDowell puts this point: ‘One of the benefits of studying a great philosopher from an alien age is that it can help us to see that we do not have to swim with the currents of our time’ (McDowell 1998, pp. 37–38). Similarly, Richard Rorty has emphasized the importance of cultivating a ‘healthy skepticism’ concerning current philosophy so that we can go beyond it; cf. Rorty (1984).

39. For further reflections on ‘doing philosophical history’, see the essays in this volume. Cf. also Rorty et al. (1984), Sorell & Rogers (2005), and more generally, Rée et al. (1978), Hare (1988), Campbell (1992), Gracia (1992), Krüger (2005), Chimisso (2008), Piercey (2009), and Rheinberger (2010).

40. Further such forms include: scholarly editions of an author’s writings, e.g., Carnap (forthcoming); editions of philosophical correspondence, e.g., Creath (1990); selective readers, e.g., Frege (1997); and broader anthologies, e.g., Martinich & Sosa (2001a). More representative examples of each (all from the last 25 years) are included in the bibliography, although it is not meant to be complete.


42. For the first two sub-forms see, respectively: Open Court’s series, Library of Living Philosophers, or the more recent Cambridge Companion series; and Routledge’s Critical Assessments of Major Philosophers series. For the third sub-form, cf. Giere & Richardson (1996), Glock (1997), Reck (2002), and Beaney (2007). All relevant volumes of the series by Open Court, Cambridge University Press, and Routledge are listed in the bibliography. For other kinds of volumes, representative samples are provided as well.


Cf. Uebel (1992, 2000), Cartwright et al. (1996), Giere & Richardson (1996), Nemeth & Stadler (1996), Stadler (1996), and Reisch (2005); cf. also Parrini et al. (2003) and Richardson & Uebel (2007). Other publications by the Vienna Circle Institute could also be listed here.


Further reading


Part I
Case Studies
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Philosophers are reluctant to take time off to do history, even the history of their own subject. Philosophy is too entrancing.
(Jackson, 2004, p. 652)

1 Introduction

The remarks I have chosen as an epigraph to this chapter encapsulate, neatly though perhaps inadvertently, a striking view of philosophy's relation to its own history: that writing the history of philosophy involves time off from philosophy itself. Through examination of an intertwined set of examples, an examination which gives us a new perspective on those examples, I shall try to display the indefensibility of this view, and show that lack of attention to our own history leads us into bad philosophy, characterized by complacency and hubris, generating misguided projects, false assumptions, and the overlooking of serious alternatives.

Of course, Jackson's comments, at least by the breathless standards of the modern academic journal, have themselves begun to slip into the past; but this attitude to the history of philosophy has been common enough, especially amongst analytic philosophers – my own tradition, as it happens – where I have come across the view many times, across a variety of philosophers one might expect to agree on little else; were there received wisdom in analytic philosophy, this might be a component of it. I call it an attitude, rather than, say, a view, because is rarely expressed, especially so publicly and with such brutal frankness as that displayed above, appearing more often, and obliquely, in casual conversation (though sometimes in journal policies, where it eventually provokes a reaction, leading to the setting up of specialist journals such as the British Journal for the History of Philosophy, established in the early 1990s). The
attitude is well expressed by a joke attributed to Quine, that there are two sorts of people interested in philosophy, those interested in philosophy and those interested in the history of philosophy. As Alasdair MacIntyre pointed out, the counter-joke is this: ‘[T]he people interested in philosophy now are doomed to become those whom only those interested in the history of philosophy are going to be interested in in a hundred years’ time’, though he added, more sombrely, ‘So the philosophical nullifying of the past by this conception of the relationship between past and present turns out to be a way of nullifying ourselves in advance’ (MacIntyre, 1984, p. 40). All of us will stand at history’s bar, and at some stage, if we are not just ignored, most of our contributions to the professional journals will certainly look quaint, perhaps bizarre, maybe even unintelligible. We shall be dependent on the kindness of strangers to retrieve our issues, and even our meanings. On this view, the historian of philosophy is a kind stranger, one who is interested in what actual philosophers say, and not just as a justification for saying something different; rather, the historian is a model listener, patient and concerned to understand what is said, and why, before assuming the right to reply. Such a listener will not be so rude as to discount a philosophical suggestion because of its proponent’s age. Nor would they be content, for instance, to form their impression of Wittgenstein on the basis of the corrupt texts and occasional outright mistranslations with which early readers had to work, let alone on the variously tendentious secondary (and now extensive and even more tendentious tertiary) literature. Of course, Wittgenstein is a particularly exasperating example: it is hard to think of a philosopher who made a bigger mess of bringing the bulk of his own writings into publishable form. And it is all too possible for the task of understanding to become an end in itself: the sense that another and incomparably greater mind is almost within one’s reach is too entrancing, so that one may forget the point of making contact.

Perhaps the primary point of making contact is, of course, to find out whether this or that philosopher is telling us the truth about something, or is at least saying something we can use. But another – and it is not a rival – is to put our own practice into some kind of perspective, by looking at where we have come from and how we got here. Communities tend to have myths about this sort of thing, and there is no reason to suppose that philosophers are immune. In fact, what is going on in episodes of philosophical development is often below the surface, not to be found in standard histories or textbooks – especially when they are written by those involved. We can divide the subterranean influences into two kinds: the internal, that is, those which arise from philosophy itself, and the external, those which come from the wider society in which the practice of philosophy is embedded. Most of this chapter will concern an example of the internal influences; I shall say a very little about the external ones in the final section.
2 The theory of denoting phrases and Russell’s conception of propositions

If we take MacIntyre’s figure of a hundred years and look back, one of the most prominent sights in the vicinity is Russell’s ‘On Denoting’ of 1905. According to Robert Marsh, the editor of Logic and Knowledge, Russell called this ‘his finest philosophical essay’. It is certainly his most famous and most overtly influential philosophical essay, introducing a theory which Frank Ramsey famously called ‘that paradigm of philosophy’. Even Wittgenstein – notoriously hard on Russell in most other respects – just took over the theory in the Tractatus (1922, section 3.24). In fact, until Strawson attacked it in 1950, hardly anyone uttered a word of criticism. The essay was reprinted time and again even in collections intended for hapless undergraduates. No one seemed to care that its central argument proved baffling even to professionals, with the result that the history of twentieth-century philosophy resembled a map of 1970s Berlin as edited by the Stasi, with darkness at its heart. Few seemed to care that, of the three puzzles Russell announces as solved by the new theory, there is doubt either about the genuineness of the puzzle, or about whether Russell’s solution works, or about whether he even needed a solution since he had already solved some of them in a different way. None of this detracted from the paper’s iconic status as the first published appearance of the Theory of Definite Descriptions. Again, no one seemed to notice the article’s actual title, or to care that Russell clearly thought of it as providing an account of denoting phrases, thus implicitly connecting it with his 1903 (Principles of Mathematics) theory of denoting concepts, a connection which was largely ignored until the 1980s. And few, until recently, have queried the standard – and false – story of the theory’s genesis and purport. (Quine, true to his own joke, was one of the principal sources of this falsehood.) Partly as a result, after Strawson, the Theory of Descriptions provided one of the staples of discussion in the philosophy of language for decades.

And this fact is very odd, because, like that other major influence on the philosophy of language, Frege, Russell was not especially concerned with language, and for very many years, including the period of ‘On Denoting’, his conception of propositions was decidedly non-linguistic; and that conception (along with his obsessive attempts to resolve the paradoxes which threatened the foundations of mathematics) did far more than the Theory of Descriptions to motivate the major changes in Russell’s metaphysical views over the next few years.

This comes out most dramatically in Russell’s correspondence with Frege. Frege, arguing that ‘the word “true” is not a predicate like “green”’, had said in a letter of November 1904, ‘Truth is not a component part of a thought, just as Mont Blanc with its snowfields is not itself a component part of the thought that Mont Blanc is more than 4000 metres high’. Russell ignored the
point about truth and took up the example to attack Frege’s *Sinn*/*Bedeutung* distinction, saying,

I believe that in spite of all its snowfields Mont Blanc itself is a component part of what is actually asserted in the proposition [*Satz*] ‘Mont Blanc is more than 4000 metres high’. We do not assert the thought [*Gedanke*], for this is a private psychological matter: we assert the object of the thought, and this is, to my mind, a certain complex (an objective proposition, one might say) in which Mont Blanc is itself a component part. If we do not admit this, then we get the conclusion that we know nothing at all about Mont Blanc [itself]. (Russell, 1904b, p. 169)

Ignore the cross-purposes about thoughts. The crucial claim in this passage is the last one. It is one of those Russellian ideas which appear only infrequently, but are nevertheless a constant presence, shaping the direction of the reasoning. The idea helps explain the occurrence of some of the odder features in the early sections of the 1903 *Principles of Mathematics*. And – given that Russell’s primary interest was in propositions rather than mountains – it makes it unmistakably clear that the motivation behind his peculiar account of the nature of the proposition is epistemological. No doubt, he is imposing a subjectivist and idealist interpretation on Frege which Frege himself would surely have rejected. Still, justified or not, his worry seems to be that if our propositions consist of senses rather than of the actual things we are talking about, then there will be an impenetrable barrier to knowledge of those things.

We can discern the same train of thought at work several years later:

But in this view [that ‘judgments consist of ideas’] ideas become a veil between us and outside things – we never really, in knowledge, attain to the things we are supposed to be knowing about, but only to the ideas of those things. (Russell, 1911, p. 155)

This part of Russell’s reaction against idealism, evident as early as 1903, is to insist that the very objects about which we speak actually compose our thoughts. In these earlier years, moreover (that is, prior to the formulation of the multiple relation theory of judgment), years which include the appearance of ‘On Denoting’, the existence of a proposition does not depend on anyone’s formulating it – they are all out there waiting for us.

This extreme reaction is difficult enough to sustain. But Russell made it harder for himself by combining it with an extreme atomism, claiming that every such object is in a sense complete; in *The Principles of Mathematics*, he argues that Frege’s notion of a kind of incomplete thing, namely a concept, which cannot be made a logical subject, is self-contradictory [1903, section 49]. In contrast, he considered the constituents of judgments to be entities
in their own right: not aspects of a prior whole, but real parts each capable of figuring as a logical subject. For example, each of the three words expressing the judgment that Goebbels admired Hitler serves to introduce a separate element of reality with equal ontological status, namely, the man Goebbels, the universal relation of admiration, and the man Hitler. Russell’s monist (and usually idealist) predecessors had thought that a judgment is a bit like something carved from a single piece of wood: the fact that we can treat it as having parts does not show that it was assembled from such parts; these components are notional rather than real. For Russell, on the other hand, a judgment is like a model assembled from pieces existing in their own right, all of which are, in Frege’s terms, ‘saturated’. There is just one fundamental ontological category, the term:

Whatever may be an object of thought, or may occur in any true or false proposition, or can be counted as one, I call a term. ... [E]very term has being, i.e. is in some sense. A man, a moment, a number, a class, a relation, a chimaera, or anything else that can be mentioned, is sure to be a term; and to deny that such and such a thing is a term must always be false. ... A term is, in fact, possessed of all the properties commonly assigned to substances or substantives. (Russell, 1903, section 47)

He noticed one of the difficulties posed by this view immediately:

Consider, for example, the proposition “A differs from B.” The constituents of this proposition, if we analyze it, appear to be only A, difference, B. Yet these constituents, thus placed side by side, do not reconstitute the proposition. The difference which occurs in the proposition actually relates A and B, whereas the difference after analysis is a notion which has no connection with A and B.... A proposition, in fact, is essentially a unity, and when analysis has destroyed the unity, no enumeration of constituents will restore the proposition. The verb, when used as a verb, embodies the unity of the proposition, and is thus distinguishable from the verb considered as a term, though I do not know how to give a clear account of the precise nature of the distinction. (ibid., section 54)

Russell’s problem, then, is that while he cannot deny propositional unity, he can find no account of the proposition which can do justice to it. In his ontology, there is no room for a propositional unifier; to continue with our analogy, it is as though he expects one special component of the kit to hold all the rest together, while no component is endowed with the capacity to do so. To change the analogy to one more familiar: it is as if Russell expects to be able to form a chain from a collection of rings by adding one master ring to bind them. But he is in the dark about how this can be done.
No doubt keen to get on with mathematical business, which is where, as he must already have known, his significant achievements were to come, he left the matter unresolved in 1903. Opinions differ over how serious this so-called problem of the unity of the proposition is: certainly a number of philosophers in recent years have argued that it was fatal to Russell's ideas (at least, his ideas over the period 1903–1918); others have thought it just a muddle; others again have suggested that the problem is one not peculiar to Russell and have proposed solutions, while still others, unconcerned with history but whose views are close enough to Russell's for it to seem a potential threat, have apparently overlooked it altogether, so busy have they been reinventing the square wheel. Anyway, whether or not we think that propositions consist of independent bits, a related difficulty is certainly serious. Let us see how it arises. Propositional unity seemed to Russell an undeniable datum: a proposition is a unity, and hence on his views an entity. At this stage of his thinking, propositions are objects of belief (or judgment as he called it then). That is, judgment is a dyadic relation between a mind and a proposition. But Russell's conception of the proposition as a complex entity whose constituents are the things the judgment is about makes it hard to see how it can differ from what in his later vocabulary would be called a fact. The difficulty is an old one – how is false judgment possible? – but it is exacerbated by the combination of Russell's attachment both to the unity of the proposition and to the idea that propositional constituents are the represented and not mere representations. There have to be false propositions to serve as the objects of false beliefs (otherwise, as Russell said later, in 1907, false belief would be belief in nothing); this is the traditional problem. But false Russellian propositions seem to be entities as concrete as the things that they are about – a kind of shadowy ontological status for falsehoods was already bad enough, but on Russell's views they are not shadowy at all. This makes it hard for him to give a plausible account of truth, and the correspondence theory (so prominent in his work only a few years later) is noticeably absent from *The Principles of Mathematics*. Rather, following Moore, and, incidentally, ignoring Frege, he says merely that truth is an unanalyzable property: true propositions just have it, false ones just lack it (section 52). On this view, sometimes called 'primitivism', the world contains both objective falsehoods and objective truths: 'objective', here, meaning that they are entities in no sense mind-dependent.

But Russell was clearly uneasy about primitivism. While he was still defending it in his 1904 article on Meinong – 'this is', he says, 'I believe, the correct view' – he nevertheless immediately goes on to articulate the difficulties in a way which sets the agenda for many later philosophers:

The fundamental objection may be simply expressed by saying that true propositions express *fact*, while false ones do not. This at once raises the problem: What is a fact? And the difficulty of this problem lies in this,
that a fact appears to be merely a true proposition, so that what seemed a significant assertion becomes a tautology. It is very difficult to avoid recurring to the notion that a proposition is a judgment, and it might be thought that this is why the statement that true propositions express facts seems significant. But even when this error has been avoided, it seems to remain that, when a proposition is false, something does not subsist which would subsist if the proposition were true. (Russell, 1904a, p. 473)

By the end of the article, Russell has managed to reassure himself that primitivism survives the objections, though falsehood now appears to be not so much the lack of the property of truth as the possession of an incompatible one:

Thus the analogy with red and white roses seems, in the end, to express the matter as nearly as possible. What is truth, and what falsehood, we must merely apprehend, for both seem incapable of analysis. And as for the preference which most people... feel in favour of true propositions, this must be based, apparently, upon an ultimate ethical proposition: ‘It is good to believe true propositions, and bad to believe false ones’. (ibid., p. 474)

It is hard to see how anyone could be satisfied with this – indeed, the substitution of the idea that falsehood is lack of truth by the idea that it is a different, equally primitive, but mysteriously incompatible property just seems to make matters worse – and the worry about false propositions resurfaces in Russell’s 1907 Aristotelian Society paper, where he canvasses – without endorsing – some radically different views, involving the replacement of primitivism. The differences, however, are by no means confined to truth. And, as is well known, in 1910 Russell came to embrace the new views: a correspondence theory of truth, and – one of the great subterranean influences on twentieth-century philosophy – the multiple relation theory of judgment.

3 The multiple relation theory of judgment

While many have been prepared to embrace some version of the correspondence theory of truth, hardly anyone has had a good word for the multiple relation theory of judgment in any of its versions, of which there are at least three. (Possibly four, if one counts Russell’s 1907 flirtation.) The two theories do not sit together easily, as we shall see.

It is worth looking at the way he introduces the first full version, of 1910:

The theory of judgment which I am advocating is, that judgment is not a dual relation of the mind to a single objective, but a multiple relation of the mind to the various other terms with which the judgment is
concerned. Thus if I judge that $A$ loves $B$, that is not a relation of me to "$A$’s love for $B$," but a relation between me and $A$ and love and $B$. If it were a relation of me to "$A$’s love for $B$," it would be impossible unless there were such a thing as "$A$’s love for $B$," i.e. unless $A$ loved $B$, i.e. unless the judgment were true; but in fact false judgments are possible. When the judgment is taken as a relation between me and $A$ and love and $B$, the mere fact that the judgment occurs does not involve any relation between its objects $A$ and love and $B$; thus the possibility of false judgments is fully allowed for. (Russell, 1910, p. 122)

Russell adds that this is a first approximation; he needs to take into account the direction of the relation; that is, he needs to add something to distinguish the judgment that $A$ loves $B$ from the judgment that $B$ loves $A$. After some difficulty and adjusting, he eventually came up with a technically ingenious solution in 1913 while drafting his intended book *Theory of Knowledge*. But at this point he had perhaps the most famous conversation in the history of twentieth-century philosophy, the unrecorded one in which Wittgenstein objected to the multiple relation theory in a way which left Russell ‘paralysed’. Russell’s correspondence from this period is frustratingly indefinite about the content of the conversation, yet it can be heart-wrenchingly poignant about its effect on him, as we see in the following extract from a letter to Lady Ottoline Morrell:

Do you remember that... I wrote a lot of stuff about theory of knowledge, which Wittgenstein criticized with the greatest severity? His criticism, though I don’t think you realized it at the time, was an event of first-rate importance in my life, and affected everything I have done since. I saw he was right, and I saw that I could not hope ever again to do fundamental work in philosophy. My impulse was shattered, like a wave dashed against a breakwater. I became filled with utter despair, and tried to turn to you for consolation. But you were occupied...and could not give me time.... I was and am convinced that all fundamental work in philosophy is logical.... Wittgenstein persuaded me that what wanted doing in logic was too difficult for me. (Russell, 1916)

There is a certain amount of hyperbole in this, however; as the Editor of Volume 8 of Russell’s *Collected Papers* points out, in the letters of early 1914, ‘Russell is brimming with confidence in his ability to do original work’ (Slater, 1986, p. xx). (We should not forget to whom he was writing in 1916, and what reactions he may have been trying to evoke.) Nevertheless, even if the effect may not have been quite as catastrophic and permanent as he is pretending, it is undeniable that his work on *Theory of Knowledge*, which until then had proceeded apace, suddenly stopped at this point, never to be resumed; and, strikingly, that he turned away from logic to other areas
of philosophy – the work about which he is so confident in early 1914 is an example – never to return. So, even if Russell is laying it on a bit thick in this letter, it remains true that he had found Wittgenstein’s objection to the multiple relation theory devastating. But why?

The person most likely to mislead us on this question is Russell himself. As we saw, his official introduction of the multiple relation theory in 1910 offers it to us as a solution of some puzzles about truth and falsehood. In particular, it is apparently aimed at avoiding the need to posit so-called objective falsehoods (about whose existence, required by his 1903 account of judgment, he had previously been utterly nonchalant). There is no indication of any connection with wider concerns or projects. Such information only makes the question more pressing: why did an obscure objection to an apparently peripheral theory have such an effect on him?

One thing we should never forget when reading Russell’s early-twentieth-century work is the discovery of the paradox which threatened to bring down the entire logicist project, and his lengthy, obsessive, all-consuming, marriage-destroying concern for a resolution of it. His attempts at resolution, as we all know, involved various forms of the theory of types. Two years before the appearance of *Principia Mathematica*, he published the first version of the ramified theory (Russell, 1908). This postulated two hierarchies, one of types and one of orders. At one point, he remarks that ‘Functions of various orders may be obtained from propositions of various orders by the method of substitution’ (ibid., p. 77). This is an allusion to his substitutional theory of classes and relations, on which he had worked for the preceding two or three years, but which did not come to general notice until the 1970s. Now this theory requires an ontology of propositions on which the substitutions are performed. If one thinks of propositions as linguistic entities, the idea of dividing them into orders is not especially disturbing. However, as we have seen, Russell had an extreme realist account of the nature of propositions. And this was combined with something else we have remarked: that, unlike Frege’s, his ontology was fundamentally simple – at bottom, everything is a term and can figure as a logical subject in propositions without restriction. Now he is disturbing this simplicity, dividing non-linguistic entities into fundamentally different kinds which somehow constrain how they can be put together and thereby what it makes sense to say, and moreover creating actual infinities of these kinds. Driven solely by the need to avoid the paradoxes, Russell looked like committing himself to what even one of Russell’s most sympathetic commentators has called ‘the most baroque ontology ever devised by the philosophical imagination’ (Griffin, 1985, p. 217).

But it was not long before he seemed to have found a way out. The multiple relation theory of judgment offered the possibility of managing without propositions as entities, and the hierarchy of orders needed by the ramified theory of types could be provided by a hierarchy of meanings
of the words ‘true’ and ‘false’. Section 3 of Chapter II of the Introduction to the first edition of *Principia Mathematica* is headed ‘Definition and Systematic Ambiguity of Truth and Falsehood’ (Whitehead and Russell, 1910, p. 41). In it, Russell claims that ‘the words “true” and “false” have many different meanings, according to the kind of proposition to which they are applied’ (ibid., p. 42). This is ‘not difficult to see’, he says (loc. cit.), and demonstrates this by explaining the distinction between the simple cases of first truth and second truth, where second truth is truth about first truth. He goes on to apply the idea to truth-functions, effectively giving a recursive definition of truth in terms of the correspondence relation between atomic judgments and facts. The whole is then briefly, but crucially, embedded in the multiple relation theory of judgment, so that propositions-as-entities are abandoned, without the loss of the hierarchy needed to exclude the paradoxes. He retains his extreme realism about propositional constituents, but, now that propositions as unified entities are banished from his ontology, these constituents compose not propositions but propositional acts.

The castle in the air looked complete. So it must have been annoying for Russell that niggles about the multiple relation theory persisted, concerning the incorporation of the direction of non-symmetrical relations like *loves* in a theory where everything is, as it were, just another piece of kit and nothing is the arranging rather than the arranged. Some sort of epicycle, it seemed, was needed to enable the theory to distinguish the judgment that Desdemona loves Cassio from the judgment that Cassio loves Desdemona. In 1912, he quietly modified the theory for inclusion into *The Problems of Philosophy*, but by 1913, when writing *Theory of Knowledge*, he realized that he needed to modify it again, as well as extend it to cope with quantification. Still, he began writing with a speed even more dizzying than usual, and with enormous optimism. And it was at this point that Wittgenstein struck. We need not worry here about what exactly his objection was, nor whether it was conclusive. Indeed, perhaps one of the more alarming things for Russell was that he could not quite understand the objection himself, and it took Wittgenstein several weeks to formulate it even to his own satisfaction; but Russell was, in any case, already convinced that it was fatal to any version of the multiple relation theory. Wittgenstein’s deadly remark that Russell needed ‘a correct theory of propositions’ suggested that he had to start again. In despair, as we have seen, he abandoned work on *Theory of Knowledge*, referring to these events three years later as showing that he ‘could not hope ever again to do fundamental work in philosophy’.

Why should the refutation of a theory for which hardly anyone has ever had a good word have had such an impact on Russell? By now, the answer should be obvious. Although he had presented it, on more than one occasion, merely as a way of addressing an old conundrum about truth and falsehood, the theory, far from being peripheral, was central to his most
determined attempt to resolve the paradoxes, and without a resolution the great project which had taken ten years of his life was apparently in ruins.

4 The theory of definite descriptions

Unlike the multiple relation theory, the theory of definite descriptions survived and took on a life of its own. But the connection of the theory of descriptions to Russell’s obsessive search for a resolution of the paradoxes was similarly concealed at the time: only half a century later (Russell, 1959, p. 79), and still misleadingly, did he say ‘This [the theory of descriptions] was, apparently, not connected with the contradictions, but in time an unsuspected connection emerged’, as though the connection came as a surprise even to him. This presentation of the theory as isolated helps to explain some of the oddities about the way it was treated subsequently, including the large amount of time wasted on debating whether or not it gives an account of the way in which proper names are used in ordinary language, as though its failure to do this would show it to be false. (Of course, it does not, and not just because it is a theory of descriptions and not of proper names at all, though hardly anyone seems to notice this, but mainly because it was never meant to. It, too, was embedded in a project, one which concerns mathematics and logic, not the semantics of natural languages. Regarding this as a fault is like taking a road map of Europe and complaining that you cannot use it to find the footpaths.)

I remarked in Section 2 above that the standard story of the theory’s genesis and purport is false. That story runs as follows. According to their surface grammar, utterances of the form ‘The F is G’ and ‘a is G’ apparently pick out through the definite description ‘the F’ or the name ‘a’ a single object that the proposition is about, in such a way that, should this picking out fail, the sentence will lack meaning. This view seemingly has extravagant ontological consequences, as Russell recognized in section 427 of The Principles of Mathematics, and Quine emphasized in his otherwise determinedly unhistorical ‘On What There Is’ (Quine, 1948), consequences flowing from the recognition of the meaningfulness of empty definite descriptions and names. The discovery of the theory of definite descriptions in 1905, so the story continues, was motivated by Russell’s increasing wish to avoid such consequences; that discovery enabled the crucial distinction to be drawn between, on the one hand, the surface grammar of both definite descriptions and at least many grammatically proper names, and, on the other, their true logical form; and the discovery of this logical form reveals that the grammatical form of utterances involving these kinds of expressions is purely a surface phenomenon which disappears under analysis, so that such picking out of a single object turns out to be inessential to the sentence’s possession of meaning.

In various formulations, this story has been standard in the textbooks for decades. However, in fairness to the textbook writers, it is worth
emphasizing that sometimes (especially with hindsight and in old age), Russell himself subscribed to at least part of it, when he presented the theory of descriptions as freeing one from the assumption that words must stand for entities in order to be meaningful: ‘The central point of the theory of descriptions was that a phrase may contribute to the meaning of a sentence without having any meaning at all in isolation’ (Russell, 1959, p. 85; cf. Russell, 1937, p. x). The ‘may’ here is telling: the fact that Russell’s comments imply that having meaning is something which some words or phrases ‘may’ have in isolation, others not, indicates that he was thinking in terms of his earlier contrast between names and other words, where a name is ‘a simple symbol, directly designating an individual which is its meaning, and having this meaning in its own right, independently of the meanings of all other words’ (Russell, 1919, p. 174; my claim is verifiable at Russell, 1937, p. x). That is, despite the ‘central point of the theory of descriptions’, his conception of a name was precisely that of an expression which does have meaning in isolation. And in fact, just as such a conception of a name would lead one to expect, and despite this presentation of the theory of descriptions, the theory’s real function as applied both in ontologically non-reductive and reductive analysis is to reinforce that assumption that entities are necessary for meaning, to protect it against obvious counter-examples, by showing that, rightly understood, such words are in fact just codifications of more complex expressions, for each individual component of which the assumption holds true. Although this function, if noticed at all as a separable feature of the theory of descriptions, is likely to be associated with the famous ‘fundamental epistemological principle’ of ‘Knowledge by Acquaintance and Knowledge by Description’ (Russell, 1911, p. 154), it in fact appears much earlier, as does the principle, in ‘On Denoting’ itself:

Thus in every proposition that we can apprehend (i.e. not only in those whose truth or falsehood we can judge of, but in all that we can think about), all the constituents are really entities with which we have immediate acquaintance. (Russell, 1905, p. 427)

So much for the idea that the theory of descriptions frees us from the assumption that existence is necessary for meaning. The small kernel of truth in that idea is that the theory makes it possible to explain neatly and cogently how one can say both truly and unequivocally ‘There is no such thing as the unique solution of the equation \(y^2 = 4ax\)’; although Russell had had an explanation in The Principles of Mathematics, it had been clumsy and, unlike the new one, did not offer a way of explaining the obvious entailment of ‘Someone broke the bank at Monte Carlo’ by ‘The man who broke the bank at Monte Carlo strolled along the Bois de Boulogne’.
5 Relations

I have been urging that Russell’s most famous theories must be understood through their connection with his philosophy of mathematics. The same is true of his view of relations (such as greater than, to use one of his own favourite examples); the nature and status of relations formed the principal issue between him and his idealist opponents. As Russell conceived idealism, its adherents, such as F.H. Bradley, denied the absolute and unqualified truth of any statement, whether it be one of, for example, everyday discourse, of science, or of mathematics, no matter how simple, how carefully phrased, or how conscientiously established, these may be. Russell himself, after his abandonment of his early adherence to idealism, came to hold that mathematical statements are straightforwardly and absolutely true: not merely true to a degree; not just temporarily true as one moment of a dialectical transition; not merely true by being part of a wider whole; not just empirically true though transcendentally false; not just conditionally or relatively true. This view of the status of mathematics, Russell thought, requires a certain kind of metaphysics, one marked by an extreme realism and a lack of epistemic restrictions concerning the objects recognized in that extreme realism: the mind has direct and unmediated contact with many separate propositions and their many separate constituents; propositions are truth-bearers; they are real entities, not linguistic, ideal or mental, as much part of the world as rocks and stones and trees, and can be quantified over; they are objective and independent of our formulating them; their constituents are likewise real, and include not only physical objects but universals which, in turn, include relations. It is easy to see why the absolute idealists, with their commitment to a mind-soaked world and their metaphysical monism, would object to such a metaphysics as this. But how did the question of the status of relations, in particular, become the touchstone?

In part, this is because of disagreements about the nature of propositions flagged in Section 2 above: for Russell during this period, relations like greater than are independent, stand-alone constituents of propositions; for the idealists, they were abstractions from propositions, creatures of the intellect with no independent existence. But the question of relations had more importance for Russell than this: his account of the nature of mathematics had put relations at its heart. Developments in nineteenth-century mathematical thinking (the important figures include Cantor, Dedekind, Gauss, Hilbert, Peano, and Weierstrass) had resulted in the replacement of quantity by order as the fundamental concept in mathematics, and required a concomitant emphasis on the significance of relations. Russell’s work after his break with idealism explicitly reflected these changes, whose influence appeared repeatedly in the first statement of his logicism, *The Principles of Mathematics*: from, for example, section 1’s definition of pure mathematics through section 187’s explanation of the importance of order to section...
208’s insistence that ‘all order depends upon transitive asymmetrical relations’. Any suggestion that relations were not objective existents thus apparently threatened Russell’s greatest intellectual achievement by undermining what he himself thought were its metaphysical foundations. What really motivated Russell’s own rejection of idealism, then, so it seems, was his concern to provide an adequate account of mathematics. An examination of Russell’s writings from *An Essay on the Foundations of Geometry* (1897b) to *Principia Mathematica* (Whitehead and Russell, 1910), especially vividly displayed in Volumes 2–4 of his *Collected Papers*, reveals how intensively he occupied himself with the analysis of mathematics and the development of a logic of relations.

Certainly Russell succeeded in developing a logic of relations, which enabled reasoning involving relations to be successfully formally handled for the first time in history. This was a monumental achievement, an undeniable great leap forward. But what is at issue here is not a formal matter; it is the question of how relational expressions function: Do they function by introducing a kind of object into the proposition, or not? Russell held (at this stage) that they do; Bradley rejected this: his notorious claim that relations are unreal is the denial that they are substance-like objects. Russell, as we have just seen, thought that something important was at stake. In *The Principles of Mathematics*, he opens section 212 with a reference to ‘the philosophic dislike of relations’ and immediately goes on to describe this ‘dislike’ as the view that ‘no relations can possess absolute and metaphysical validity’. It is not obvious what this means, but we soon get an explanation: it is ‘the denial that there are any relations’ (Russell, 1907, p. 142; the passage is reprinted in his 1959, p. 57). These telling phrases reveal his thinking more clearly than his explicit arguments. That thinking takes the form of a *reductio*, followed by a corollary: ‘The idealist view that relations are abstractions from whole propositions comes to this: there are no relations. Consequently, every proposition invoking a relation is appealing to something that does not exist. Hence, every such proposition (and mathematics is full of them) is false. The sole alternative is to recognize relations as mind-independent existents which enter propositions as independent constituents, complete entities on an ontological par with the other constituents’. Later, under the influence of Wittgenstein, he changed his mind, proclaiming a thesis which in every respect other than its clarity could have come straight from one of his idealist opponents:

If we mean... that the relation is a third term which comes between the other two terms and is somehow hooked on to them, that is obviously absurd, for in that case the relation has ceased to be a relation, and all that is truly relational is the hooking of the relation to the terms. The conception of the relation as a third term between the other two [that is,
exactly his own conception in 1903] sins against the doctrine of types, and must be avoided with the utmost care. (Russell, 1924, p. 335)

Although Russell is widely believed to have won the battle with the idealists over the status of relations, it is not commonly observed that, if indeed victory was achieved at all, this was only by changing sides.

One result of this (to hark back to a theme introduced in Section 1) is that philosophers trained in the analytic tradition, by and large, no longer took monism as a live option, so that serious debate on these matters was postponed for decades. This situation has begun to change only very recently, as a result of the kind of historical work we saw disparaged by Jackson and by Quine.

6 External influences

So far, I have tried to dispel some of the myths surrounding the development of philosophy in our own lifetimes, by looking at neglected and subterranean factors in that development. One of the questions implicitly raised by what I have been saying is this: Given that so many of Russell's philosophical positions turned out – even very quickly – to be untenable (so that he himself came to adopt views which he had previously anathematized as ‘condemn[ing] the sort of things dealt with by mathematics and physics’ (1959, p. 62)); and given that these positions were often not intended to have the significance they later acquired: how can we account for his massive influence on subsequent philosophy (which has, of course, taken mathematics and physics extremely seriously)?

This question has, of course, no short answer. But we can make it manageable, or at least approachable, by breaking it up into a set of more easily answerable questions.

First, we need to remember that Russell and Moore succeeded in breaking the hold of Absolute Idealism on English-language philosophy. Part of their influence resulted from the fact that this left an intellectual vacuum to be filled. But how did they break this hold? Did they have any decisive (or at least seemingly decisive) argument against the idealists whom they sought to displace? Or, failing a decisive argument, was there any cumulative set of arguments which together made idealism a rationally less preferable metaphysic than the extreme realism which Russell and Moore proposed? Although I have not argued this here – I have done it elsewhere – it seems to me that the answer to both questions is clearly No. The idealists may have been forced into retirement, or made figures of fun, but they remained unrefuted.

This makes the displacement of idealism by the various forms of early analytic philosophy into a somewhat puzzling fact, especially given the analysts' devotion to ideals such as proof. Did people really believe that
idealism had been refuted? Did they just get bored with it? We should not underestimate the desire for novelty. And Russell certainly offered philosophers something new. Most obviously, new tools, such as multiple quantification, with the accompanying potential for disambiguation and precisification. But also new programmes, such as logicism and the idea of logical construction, with their attendant optimism. Nevertheless, given that logicism was stillborn, and that no one ever managed to produce a logical construction of anything more significant than the average plumber, these suggestions do not necessarily make the question of influence easier to answer. Here we need to begin looking at what I called earlier the ‘external factors’, those influences on philosophy that do not belong to the internal dynamic of philosophical debate.

I think it is clear that larger forces were at work. And for those of us who, like me, share the analysts’ ideals, the historical evidence is a bit alarming. It is hard to avoid the impression that, at least at this period, philosophy is, if not epiphenomenal, at any rate not the locomotive of intellectual history.

To see this, let us go back briefly to the question of the displacement of idealism. To those of us with an interest in these things, it is fairly familiar that by the turn of the century, Absolute Idealism had gained a hold on what one might call the common intellectual consciousness: idealist slogans were not confined to philosophers. It is also familiar that this was happening at about the same time that orthodox Christian belief was being eroded by the impact of Darwin and of the emerging sciences of geology and palaeontology. This could be coincidence, but a more plausible suggestion is that idealism appeared to answer a persisting spiritual need to which Christianity no longer seemed adequate.

But even at the time, it was becoming apparent that idealism’s role as a substitute for religion was to be short-lived. One reason for this is a problem that was already familiar from Thomas Reid (if not Plato). It consists in the need to reconcile the rational demands of private and of public interest. Reid had believed that there could be no clash between the two. He maintained that because ‘the world is under a wise and benevolent administration, it is impossible that in the issue any man should be a loser by doing his duty’. The problem may have filtered through to Russell via Henry Sidgwick, who was unable to avail himself of Reid’s convenient theistic solution. Russell himself faced it in one of his undergraduate essays (Russell, 1893) and attempted to deal with it by invoking McTaggart’s optimistic idealism, according to which not only is ‘reality exclusively spirit’ but also the ‘universe and ourselves are implicitly in harmony – a harmony that must one day become explicit’. Our immortality guarantees that this harmony will at some stage be experienced, and the prospect accordingly affords something normally provided by religion, namely comfort in adversity. This long-run solution was unavailable to Russell once he rejected immortality, and in his next effort, this time in a very early paper to the
Apostles (Russell, 1894), he abandoned McTaggart and entertained what he called ‘a more Bradleian view’, in which the harmony is no longer left to a future we shall not live long enough to encounter but is present in a timeless Reality of which individual selfhood is a merely partial aspect. In a later Apostles paper (Russell, 1897a), though, Russell in effect argues that such a Bradleian metaphysical harmony belongs to Reality and not Appearance, accordingly still cannot be experienced, and hence can offer no comfort or consolation. In flight from the Victorian Christianity of his childhood, Russell was finding no refuge in idealism.

Although Russell himself attributed his break with idealist metaphysics to the influence of Moore in the period immediately following the events just described, and others (not incorrectly) attribute it to his own desire to maintain the absolute and unqualified truth of mathematics, it is clear from this brief account that he had already prepared the ground himself through his growing dissatisfaction with idealist metaphysics as a basis for a philosophy of living. That is, Russell’s fundamental break with idealism took place earlier than is commonly believed, and is based not on metaphysical but on practical considerations.

But how do we account for the wider decline in allegiance to idealism? It’s not as though Russell had an army of disciples. I shall finish with a sketch of a partial answer.

We need to focus again on idealism’s dubiously satisfactory role as a replacement for conventional religion when it came to providing consolation. Russell demonstrated its dubiety to the Apostles. But to understand why it began to fall out of favour more generally, we should observe that, once the lessons of Darwin had been fully absorbed, the need for a replacement began to wane; room became available for metaphysical views less overtly consolatory. Indeed, consolatory views began to look positively distasteful in the aftermath of the Great War. After all the mud, the gas, the relentless shelling, the blood and slaughter, idealism’s central tenet of the spirituality of the universe was offensive when not merely laughable, particularly so when we remember its Germanic origins, which tainted it by association with the ideology of the Prussian state.

This is a point worth illustrating with a particular example. As Paul Fussell (1975, chapter 2) has shown us, by the time the war started, sunrise and sunset had for more than a century furnished Romantic poetry with stock ‘tokens of hope and peace and rural charm’, often described in religious vocabulary, to the point where they ‘had become fully freighted with implicit aesthetic and moral meaning’. But, during the war’s years of anxious stalemate, dawn and dusk were the time of ‘stand-to’, when those caught in the front lines of the trenches, on both sides of the Western Front, were obliged to stand, silent and alert, looking for indications of an attack. Dawn, in particular, was the traditional time for an infantry attack to begin, and from being symbolic of hope became the occasion of a ‘daily routine of quiet terror’. ‘Dawn’, says
Fussell, by now speaking of English prose as well as poetry, ‘has never recovered from what the Great War did to it’. The world had become a bleak place, and Moore’s mockery of the claim that Reality is spiritual could now find a wider audience among philosophers, for whom Absolute Idealism could eventually become an object of derision, entering undergraduate education, if at all, only in the form of a fossil, one as strange and remote as some of the contents of the Burgess Shale.

You may, like me, think that we are well rid of Absolute Idealism. But we should not pretend that the corpse is safely staked through the heart. It is, of course, far easier to discern the external influences on a philosophical paradigm when one has the advantage of hindsight. But we ourselves are sometimes closer to the past than we realize: the post-World War II generation was no less determined to shake off the past than its post-World War I predecessor, as the analytic philosophers of the 1950s and 60s illustrate. And unless we are able to discern these influences as contemporaries, and from the inside, we risk being their mere creatures. That does not mean, of course, that we all have to be looking over our shoulders; a division of labour is required. But a generation of philosophers which does not include, and train, a scholarly and healthily self-reflective cohort, against whom they can test their ideas, risks becoming flotsam on the tide of history.

References and Further reading

Stewart Candlish


Russell, B. and Whitehead, A. N. (1910) [See Whitehead and Russell].


Scholars of Rudolf Carnap's philosophy have increasingly found themselves moved to express their understanding of his philosophical project in terms such as 'linguistic engineering' or 'conceptual engineering'. This mode of expression has been used by Richard Creath for about twenty years in his discussions of Carnap. More recently, A.W. Carus's Carnap, whose signal achievement is the method of explication of concepts, seems to be importantly imbued with a spirit of philosophical engineering (Carus, 2007). Indeed, Carus's insistence on reading Carnap as engaged in an Enlightenment project is tied, in the first instance, to an understanding of the place of engineering in the French Enlightenment (Carus, 2007, pp. 14ff). Notwithstanding this sort of convergence of views on the importance of conceptual engineering for Carnap, there has been little agreement about the specific nature of Carnap's commitment to engineering. Indeed, there has been little curiosity about what engineering is, how it has been theorized, or where Carnap's understanding of engineering might have come from. Carus, for example, provides little evidence that Carnap was aware of, or inspired by, the group of French engineers that Carus himself evokes in discussing Carnap's views. Nor is there any clear sense as to how it would be best to frame this engineering perspective within an explanatory narrative of Carnap's significance in twentieth-century philosophy. Sam Hillier's work uses the engineering perspective as a sort of radical anti-philosophical point of view in Carnap's work (Hillier, 2007). This contrasts with Carus's approach, at least insofar as the Enlightenment Project might well be taken to be a long-standing philosophical project that finds one expression in Carnap's engineering perspective – and Carus ends up connecting his Enlightenment Project all the way back to Aristotle (cf. Carus, 2007, p. 272). Similarly, Creath's engineering Carnap is not less philosophical for that fact, although, of course, he is not beholden to answering all questions anyone has ever deemed philosophical. Thus, the idea of Carnap as philosophical
engineer does not, in and of itself, seem to solve some of the largest issues of Carnap interpretation: what are his ultimate philosophical commitments? To what extent is Carnap's project anti-philosophical or, at least, deflationary of philosophical ambitions? Where should we locate Carnap's commitment to empiricism?

This fact about the Carnap literature reflects a larger issue in the history of analytic philosophy. There is much excellent work in the history of analytic philosophy and our sense of that history has grown enormously in the last thirty-or-so years. Much of the historical work is highly focused, however, on specific figures, specific texts, or, indeed, on specific theoretical innovations or argumentative moves in the work of those figures. (Consider, for example, the large and ever-increasing literature on Wittgenstein's account of logical form or on Carnap's Principle of Tolerance.) Larger narratives of the trajectory of analytic philosophy through the twentieth century are exceedingly rare. There are a few germinal ideas for such a history, none with more staying power and potential for explanation than Richard Rorty's notion of a 'linguistic turn,' especially as elaborated in Michael Dummett's attempts to specify what was unique in Gottlob Frege's philosophy by contrasting it with a thought-centred phenomenological philosophy. Of course, much of the detailed literature on specific figures or schools in analytic philosophy has uncovered several different senses in which a figure or school took a linguistic turn in his or its philosophy, when, and why. The more complicated the notion of a linguistic turn becomes, however, the less clear is the larger framework within which such a turn is to be understood and explained: why is any given instance of a 'linguistic turn' either a philosophical move or a move within something specifically and properly called 'analytic philosophy'?

The chapter seeks, in an admittedly preliminary fashion, to bring some specificity into a claim that Carnap's philosophy is based in an engineering sensibility. It does so by specifying a branch of applied science that Carnap clearly intervened in and that formed the basis of an important aspect of his philosophy of science both early and late. It also offers a couple of reflections of use in our historical thinking about the significance of Carnap's philosophy in light of its relations to applied science. One of these lessons is largely historical and systematic; I will argue that Carnap's engineering sensibility underscores the sorts of responses he made to Quine in the analyticity debates. The other is critical: I will suggest that Carnap's own understanding of engineering and technology need not be our own, and it would be appropriate to raise issues for Carnap's philosophy from the perspectives of philosophy of technology and technology studies.

There is a larger framework within which my historical remarks fit. It is not the Enlightenment framework of Carus's Carnap. There is, I believe, a more apposite framework for the story of Carnap's work, one that is very much a post-Enlightenment framework. The Vienna Circle's 1929,
Wissenschaftliche Weltauffassung (Neurath et al., 1929) was, as the title indicates, concerned to present a vision of philosophy consistent with a scientific world conception – and since 1848 or so, the scientific status of philosophy was the major metaphilosophical preoccupation of German-language philosophers. This was indeed a post-Enlightenment problem – it was a problem presented to philosophy by the anti-philosophical philosophy of positivism and was emblematized by the rise of the scientist as having a social role distinct from that of the natural philosopher. How to make philosophy fully participant in the methods and techniques of positive science is the project of the Vienna Circle in 1929. Carnap is of special interest in this matter due to the clarity, specificity, and completeness of his answer to this question, which is, I would argue, the central question of his philosophical work.

1 Carnap’s interest in the science of measurement

If called upon to opine regarding the central scientific preoccupation of Carnap's work, many philosophers of science would, I suspect, offer the view that Carnap was most concerned the theory of relativity or space-time physics or something similar. After all, that is what his dissertation and many of his earliest papers were about (Carnap, 1922). He returned to it throughout his early work and then again in his final work of philosophy of science, the book Introduction to Philosophy of Physics (Carnap, 1966). Insofar as Carnap's philosophy of science takes science as a topic or seeks to illuminate science, physics seems very much the paradigm case of a science. This is one thing that ties his work so closely to his fellow founders of logical empiricism, Moritz Schlick, Hans Reichenbach, and Philipp Frank. This central concern with physics is so evident in logical empiricism that, as is well-known, one of the ways in which logical empiricism has been pilloried in recent years is exactly that it foregrounds physics far too much – to the detriment of its understanding of other sciences.

My view is that this answer is both importantly true and importantly misleading. Carnap is interested in physics. It is his paradigmatic science. His principal interest in it is, however, I would argue, not quite what his reputation as a philosopher of physics would indicate. His are not quite and not always the standard interests of his contemporary or today's philosophers of physics, even the philosophers of physics who today worry about the foundations of space and time. Carnap is interested, he tells us again and again, in the introduction of quantitative concepts in science. Physics provides not only the most robust and most important but also the most fundamental such concepts. But the introduction of any such concepts is a matter of measurement. It is the conditions under which measurement is possible that are most fundamental to his discussions of science from his 1926 Physikalische Begriffsbildung (Carnap, 1926) to his Introduction to
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Philosophy of Science, Section 2 of which is both the longest section (eight chapters) and entitled ‘Measurement and Quantitative Language’ (Carnap, 1966, pp. 51–121).

Now there is a science of measurement. It is and was when Carnap began his career called ‘Metrology.’ This science of measurement was, in fact, one of the real growth areas and key fields of methodological advancement in nineteenth-century physics, precisely because it was an area of science within which scientists themselves thought about the conditions of measurement and the empirical and technical issues involved in the use of any given instrument for measurement. Any scientist who in 1926 picked up the Physikalische Begriffsbildung, which contains a long meditation on the development of temperature scales, would recognize it as a contribution to metrology. It is a work of theoretical metrology that tries to delimit as precisely as possible the conditions under which thermal effects can be measured. It also contains remarks on why measurement is such an important thing in science. This is all stock in trade for the theoretical metrologist and had been for decades before 1926.

Now it is true that Einstein’s theory of relativity renewed the theoretical interest of measurement precisely by tying proper concept formation to measurement, especially in the formulation of the theory that makes it depend on the behaviour of transported rods and clocks. This is surely one reason Carnap is interested in the topic. In his metrological discussions, however, Carnap’s main example does not have much to do with relativity. In 1926 and in the 1960s, Carnap discusses temperature more than anything else. Carnap’s interest in temperature is four-fold. First, temperature is a familiar and interesting case of a quantitative concept in science and everyday life. We care about temperature. We also can come to see that it has features that make it different from other quantitative concepts. For example, if we put something a meter long against something two meters long, the total length of the combined object is three meters. Masses add that way also. But temperatures do not. You do not heat up but rather cool off a liquid at twenty degrees by adding more of it at ten degrees. It is a non-additive quantity, but not for that reason unmathematizable. Second, we know enough about thermometers to become convinced of a more general claim that Carnap wishes to make – namely, that all measurement ultimately depends on length and time measurement. We know that thermometers work by the thermal expansion of the length of the operative object – very often liquid mercury. We measure temperature by measuring changes in length due to temperature. Carnap follows a long tradition of philosophically-minded scientists to claiming not an ontological but a metrological priority to space and time.

Third, measurement is complicated, and our understanding of how, say, thermometers work depends on our understanding of the laws of heat transfer. Indeed, much sweat was expended in the nineteenth century looking at various different possible materials from which to make
thermometers and was complicated by the fact that the rates of thermal expansion are not constant over the temperature range. In other words, while thermometers are necessary to formulating thermal laws in the first place, those laws help us see that the thermometers do not work as we first thought. To get a sense of how difficult actually making a precision thermometer was in the nineteenth century, just meditate on the fact that both the mercury and the glass casing expand when heated, that they expand differently one from the other, and that they expand differently at different temperatures. Metrology is the science of recalibration and reiteration and correction par excellence. Carnap presents all this in a characteristically non-realist language. The corrections come not from somehow knowing that we have not gotten temperature ‘right’ but from pragmatic choices about how difficult it would be to write down laws based on some thermometer readings compared to others. In fact, on this matter, Carnap follows Schlick quite closely: We could, if we wished, take the heart rate of the Dalai Lama as our clock and then write up a very complicated physics in which all the processes in the world speed up when the Dalai Lama is relaxed, compared to when he is rushing to his next press conference. There are only pragmatic reasons not to write up and work with such a Buddhist physics, according to Carnap. Fourth, the philosophical point of all this is really the formulation of mathematically expressed laws of nature that allow objective prediction and control. For science to have the epistemic advantages is for it to be expressed in the language of mathematics, and for this to happen, measurement – the assignment of consistent and commensurate numerical values to concepts – must be possible.

2 Carnap’s account of logic: Metalogic as metrology

So, there is clearly an interest that Carnap expressed in the science of measurement. There is, however, a closer connection between metrology and Carnap’s philosophical work than is to be found in the fact that Carnap had an interest, as a philosopher of science in the science of measurement. It is a closer connection than is even to be found in his insistence that quantified concepts are necessary for the overall epistemic virtues of science. To see this, we should recall that Carnap’s philosophical work was primarily in pure and, he importantly insisted, applied logic. Moreover, Carnap’s central metaphor for thinking about the place of logic in his philosophy was explicitly in the idiom of technological development and application. Here is how he put the point in his *Formalization of Logic*:

Semantics – more exactly, pure semantics as here conceived – is not a branch of empirical science; it does not furnish knowledge concerning facts of nature. It is rather to be regarded as a tool, as one among the logical instruments needed for the task of getting and systematizing knowledge.
As a hammer helps a man do better and more efficiently what he did before with his unaided hand, so a logical tool helps a man do better and more efficiently what he did with his unaided brain. ... [The development of modern logic has made it] possible not only to increase the safety and precision of the deductive method in realms already known, but also to reach results which could not have been obtained at all without the new tools. (Carnap, 1943, pp. viii–ix)

Logic, Carnap wishes his readers to understand, is not best thought of as the laws of thought or the rules of truth or the norms of reasoning or whatever other philosophical gloss has been put upon it; logic is a set of tools. The passage, moreover, gives clear expression to the well-known and wide-spread prosthetic understanding of tools – tools make it easier to do certain things you already do and allow you to do things you would like to do but have not to this point been able to do. In Carnap's mature work, if you wish to know why we should attend to logic, it is this understanding of logic and its place in philosophy that Carnap routinely offers. In fact, this sort of remark in Carnap's work seems, in his own view, not to be a metaphor at all. Carnap's considered view was that as a philosopher he engaged in the development of conceptual technologies for science and the science of science. This is Carnap the conceptual engineer.

This passage contains, however, a clue to a more precise and more significant understanding that we can give to this engineering perspective. The clue is to be found in the phrase toward the end of the passage that says, 'to increase the safety and precision of the deductive method.' The operative notion of safety here is epistemic safety or reliability. There was in the early twentieth century, as there is today, a discipline within science that takes as a large part of its task the testing of the safety and precision of instruments. This branch of science is metrology, the theory of measurement, especially that branch of it that in German is (and was in Carnap's time) called *Instrumentenkunde*. I conjecture that a large part of Carnap's sense of the goals and methods of metalogic come from his early exposure to, and interest in, metrology.

That Carnap would have been interested in metrology and *Instrumentenkunde* makes historical sense. *Instrumentenkunde* had its own journals that were well known during the years Carnap was a university student and young professor. It was a crucially reflexive branch of natural science, concerned with how reliable the instruments of empirical research allowed to the co-ordination of physical concepts and mathematical structures. Carnap, moreover, worked in the development of instruments – not measuring instruments, but interestingly communications instruments (wireless telephones) during his service in World War I. Arguably, Carnap's most successful conceptual intervention in science was work in measure theory: He and Yeshua Bar-Hillel advanced a measure for the information content
of messages that was important in the development of information theory. Indeed, throughout his life, deeply Carnap was also concerned with the engineering of human languages. He was a speaker of, and advocate for, Esperanto, for example.¹⁰

There are multiple connections between metrology and Instrumentenkunde, on the one hand, and Carnap’s philosophy, on the other. The formal conditions of measurement and the ‘axioms of coordination’ (to use Reichenbach’s (1920) term) between the formally defined notions such as the zero point and the unit of a measurement scale and physical processes form, it is true, the leading examples of Carnap’s analytic sentences within the empirical sciences. Thus, metrology involves the key notion of Carnap’s account of empirical knowledge. But the connection goes deeper than this. Carnap’s attitude in philosophy is that of someone who understands himself engaged in work at the border of purely formal work – the conceptual conditions that any system of measurement must meet, for example – and the development of technologies – the conditions of reliable application of such formal systems, the ‘safety and precision’ of logical instruments.

Reflection on this can alter the terrain in which we think of the philosophical project of Carnap. For example, there are philosophers who confidently tell us that logical empiricist philosophy of science was a normative discipline. The logical empiricist account of normative judgments ought to persuade us that such a view is, at best, implausible. If there is no cognitive meaning to assign to value judgments, the work of philosophy of science could scarcely consist in issuing such judgments. The whole issue is, I argue, misconceived. Consider these questions: ‘Is it a normative or descriptive claim that any quantitative system of measurement needs a unit?'; ‘Was the development of the mercury thermometer or the Celsius temperature scale normative or descriptive?'; ‘Is mechanical engineering normative or descriptive?’ I find it hard to assign any clear meaning to such questions, and even if we were to succeed, I think those questions rather miss the focus of the issues involved in measurement, technological development, or the nature of engineering.

We could say, then, in a manner of expression reminiscent of work by Thomas Ricketts, that for Carnap the clear questions of engineering replace the unclear questions of value or normative methodology. Rather than have a philosophical argument about, for example, whether simpler theories ought to be preferred in science, Carnap suggests the following: We can note that the judgment that simpler theories are easier to use is a reasonably widespread judgment in science. We can further note that its remaining ambiguity or vagueness is due to unclarity regarding when one theory is simpler than another. To remedy this, in an engineering mode, we can offer formal criteria of simplicity. Once we have these, we can ask whether simpler theories on such measures are in fact easier for scientists to employ an empirically tractable question. We also have a philosophical way forward in debate in
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which the original judgment seems important: Anyone invoking a postulate of simplicity is required to specify formally what she means by it. Informal, inchoate philosophy disappears without remainder into the construction of technology, the proposal of its adoption, and the remaining empirical questions.11

3 Metrology and the analyticity debate

How, then, can my emphasis on metrology and Instrumentenkunde help us understand Carnap’s views in analytic/synthetic debate? In this section, our interest will be focused on two questions: Why did Carnap think Quine raised no objections in principle to his work? Was Carnap right in that assessment? My answers will be these: First, Carnap’s philosophy contained a fundamental discipline called, ‘the logic of science’, that was the perspective from which the analytic/synthetic distinction was offered and within which it was meant to operate; Quine did not recognize the existence of such a perspective distinct from epistemology or metaphysics. Once one restricts the philosophical burdens of Carnap’s analytic/synthetic distinction to those burdens appropriate to the logic of science, it seems to me that Quine in fact has raised no question that Carnap need have answered. It is this perspective – this wissenschaftslogische perspective – that is Carnap’s philosophical engineering perspective.

The brief version of my perspective, then, on Carnap’s perspective on the debates he had with Quine on analyticity is that for Carnap, the analytic/synthetic distinction was not a dogma of empiricism but a proposal within the logic of science, both for understanding science and scientific methodology and for regimenting heretofore unclear philosophical debates.12 It was not a way to secure the certainty of mathematics by grounding it in meaning relations and not in connections to the empirical facts. Rather, it was a way of making scientifically precise what old philosophical terms like ‘certainty’ or ‘independence from empirical facts’ meant or could mean in the first place. Thus, Carnap could not quite figure out what Quine was asking in the debate.

This is to say that Carnap did not view himself as taking sides on a fully comprehensible question of informal philosophy – what are grounds of logical and mathematical truth? – and then employ technical means to specify his answer. Rather, here as elsewhere, Carnap thought the philosophical questions were ill-formed until such time as a technical term is introduced to give fully precise and determinate meaning to the obscure terms that philosophers tend to use. Analyticity, that is to say, a term within Wissenschaftslogik that induces a true-by-virtue-of-meaning versus true-in-virtue-of-the-facts distinction or a subject-to-confirmation-or-confirmation-by-experience versus confirmed-come-what-may distinction. The analytic/synthetic distinction is not beholden to some antecedent
philosophical distinctions against which a given analytic/synthetic distinction can be checked for accuracy.

Such claims mirror ways in which various questions in the theory and practice of measurement do or do not make sense. It would be foolish to ask whether the universe really has Fahrenheit or Celsius temperatures, for example. The foolishness of that question is not simply a function of the fact that there is a one-one mapping from each scale to the other. It is due to the fact that antecedently to the determination to regiment thermal properties into quantitative concepts, the question has no meaning – neither Fahrenheit nor Celsius temperatures exist somehow in nature prior to the defining of the scales themselves. To say this is not to deny that there are thermal properties of objects prior to our enunciation of temperature scales, but it is to deny that such thermal properties can be represented in epistemically-useful ways antecedent to such scales. Once a scale is in place, we can ask all manner of questions about particular recording instruments – are they calibrated properly? For what range of temperatures do they provide accurate readings? How might we design a different thermometer to extend the range of temperatures we can accurately record?

As with both the conceptual technologies of measurement (for example, temperature scales, which are based on certain formal, but only empirically determinable, features of the thermal properties of objects) and with specific measuring instruments, so with the general technologies of objective meaning (the determination of analyticity for any given language) and the specifics of the representational capacities of any given language. This is not to say that the questions and answers are exactly the same in the two cases. The question of analyticity is a general question of languagehood. Since Carnap is committed to there being no general question of the structure of the world or of experience that languages are beholden to in order to be languages, the concern with the specifics of the formal, but only empirically determinable, qualities of objects at the basis of Carnap's account of measurement cannot go over to the question of analyticity. Languages as languages are purely conceptual technologies. Thus, from Carnap's perspective, the terrain of the argument about the analytic/synthetic distinction looks rather like this: Can an analytic/synthetic distinction be made for various logical systems? Yes, including for systems within which all the sentences turn out synthetic. Quine, moreover, seems to agree that one can induce a formally defined analytic/synthetic distinction for formal languages. Can the exact same way of drawing the distinction be given for all logical languages? That is, does the analytic/synthetic distinction for any two given languages capture 'the same distinction'? Well, informally, you can outline a general recipe – roughly the analytic sentences are those true sentences whose truth follows from the truth definition for the language – but not technically, since the analytic/synthetic distinction for any given language is drawn within a suitable metalanguage, and we
know from the limitative results of Gödel and Tarski that no metalanguage
is strong enough to contain the definition of analyticity for all languages.
Quine, however, knows that, too. Does Carnap have to give a behavioural
criterion of analyticity? No, because it is a semantic concept, and there is
no need to reduce all semantic concepts to pragmatic ones; in fact, that is
impossible, since semantics is in part a formal, and pragmatics is an entirely
empirical science. Quine knows that, too, as his own remarks on truth make
clear. Are there informal constraints on the definition of analyticity? For
example, must the definition show how analytic sentences do not refer to
empirical facts? No, since there is no language-transcendent notion of fact
against which to check the analytic sentences of a given language to see
whether they made reference to them; this is why all such talk is brack-
eted by Carnap as merely suggestive or, to use his phrase from Meaning and
Necessity, ‘merely explanatory’ (Carnap, 1948). And surely Quine knows that
analyticity is not a concept in metaphysics!

However, the analytic/synthetic distinction cannot be made as Carnap
does, and be made to do the work Carnap wants from it, from the point
of view of Quinean metaphysics, epistemology, or semantics. For example,
Quine allows – indeed demands – as intelligible certain informal ways of
talking (the confirmation of belief by experience, for example) that Carnap
has ruled out as meaningless. Or, again, Quine demands a theory of empir-
ical meaning be tied to specifiable behaviour. So, the whole debate becomes
one of whether there is the philosophical perspective called ‘the logic of
science’ that Carnap proposes and whether it does the work it needs to
do there. That is rather an unusual, odd, almost vertiginous question. It
is ‘metaphilosophical’ in the sense that it invites curiosity about whether
and when a proposed philosophical perspective is either genuinely avail-
able or genuinely philosophical. I have no great wisdom to offer on such
matters generally. I do have a suggestion regarding an answer to the ques-
tion about Carnap’s logic of science. First, the logic of science is not meant
to be an esoteric perspective that only philosophers occupy. It is continuous
in Carnap’s mind with a very common practice of scientists, who often have
occasion to remark on the precise meanings the terms have within their
science. This is why Logical Syntax (Carnap, 1934) ends with an interpreta-
tion of Einstein’s 1905 electrodynamics paper; Carnap takes it as dead clear
that we can and should sort scientific sentences into meaning claims and
empirical claims. He does not offer a rigorous behavioural criterion for this;
he takes it to be part of how we are educated into the reading of scientific
articles and books. In a Wittgensteinian idiom, we might say that Carnap
would find it both remarkably odd and outside the scientific form of life if
someone were to reject, for example, the definitions of groups, rings, fields,
and so on in abstract algebra because those meanings lack behavioral corre-
lates and are ruled out by there being a ring on my finger or a cow in a field
or five people in a music group. Such an objection misses the central place
of specialist, defined language in the practice of science, and it is not how anyone actually reads and understands scientific textbooks. Ultimately, on my view, Carnap takes his fundamental distinctions to be part of the necessary armament of anyone learning how to be a scientist.

That is to say, Carnap takes for granted the experience of those brought into the practices of technical science in the normal manner. This, too, is a somewhat practical point of view. Carnap is not trying to tell us a story in which we move from some pristine notion of experience to the details of scientific theory – he recognizes no notion of empiricism that raises a serious question as to whether we can be said to know scientific theories at all. His concern, rather, is to illuminate the problems internal to the knowledge-producing practices of science, especially to intervene on matters of scientific judgment where clear criteria of application and formal specification of content are missing.

4 Critical consequences: Carnap and the philosophy of technology

Nothing I have said so far, even if I am right about the ultimate significance of Quine’s objections to Carnap, should be taken to preclude the possibility that Carnap’s philosophy does face fundamental objections. Indeed, our remarks on Carnap suggest a place where one might press upon Carnap’s logic of science. We have argued that Carnap’s philosophy occupies an engineering perspective involving the development and application of conceptual technology. This suggests that philosophy of technology or engineering might help us formulate pertinent questions to ask of the logic of science. One thing we can note straight-away is that Carnap’s prosthetic account of technology seems, after 80 more years of thinking about technology, an importantly impoverished one. The development of new technology does not merely allow you to do better what you already did nor simply to extend what you have wanted to do into a new realm. Technologies and technological infrastructures such as the ubiquitous cell phone and its surrounding networks of technologies and businesses and government agencies do allow you to stay in touch with people more efficiently, of course, and, as we know from sad everyday experience, brings telephonic communication where once it was not possible. However, they have also, and in consequence, changed quite a bit of the contours of the public/private divide and altered our relations to one another in various ways that would have required enormous insight to predict. The DVD player in the back of the family automobile that in North America is advertised as a way to mollify your children and make traveling by car more enjoyable for everyone also clearly increases the domain of the entertainment industry, alters human relations within the automobile, and changes relations between children and their environment. Such information and entertainment technologies fundamental alter
our places in human and physical space. These are just some quite quotidian examples of the well-known phenomena of unintended consequences and the co-evolution of the social and the technological.

We can, and should, at least use what we know about such matters in thinking about Carnap’s overall philosophical attitude as well as his specific proposed philosophical technologies. One thing to note immediately, in a sociological mood, is that Carnap’s engineering logic of science is open only to the technically adept; only a small subset of philosophers could begin to understand the project or engage in the constructions and reconstructions it offered in the first place. You cannot convert philosophy into a technical discipline without restricting the pool of philosophers accordingly. Carnap was under no illusion about that – it was part of the point – but it did bring attendant problems for Carnap and other logical empiricists and, ultimately, for analytic philosophers generally. Philosophy so conceived is still not generally accepted as philosophy in our learned or, indeed, our more general culture. The philosophical engineer investigating the precision and safe application of logical systems does not look much like the philosopher offering critical wisdom or understanding or edification to society. Establishing this vision of the philosopher was a persuasive problem that the logical empiricists never solved. In consequence, today’s analytic philosophers have a very hard time appearing as philosophers to anyone who has not already bought in to the project. This has led to enormous frustration in some corners of analytic philosophy, in which scholars are reduced to a sort of angry sputtering when faced with a world in which Jacques Derrida, Slavoj Zizek, and their ilk get all the attention (and the film deals). However, frustration and anger are rarely convincing, and the problem is a really a rhetorical problem for analytic philosophy, one its practitioners seem scarcely to acknowledge, let alone solve.

I suspect, actually, that this sort of frustration is why we are now seeing, in the work of Carus and others, invocations of big, culturally-valuable words like Enlightenment in discussing logical empiricist philosophy. It is a way to attach the project of logical empiricism – and our project of understanding logical empiricism – to a larger project of cultural edification and progress in which the philosopher has an assignable, but not necessarily technical, place. This allows me to make more precise the concern that I have with Carus’s vision of Carnap that I sketched in the opening: I do not think that Carus’s Enlightenment engineering project is Carnap’s project. He is a modernizing technician, a philosophical engineer, more interested in technocracy than in Enlightenment as understood in any eighteenth-century text I have ever read. Nowhere does Carnap, to my knowledge, engage in the sort of discourse stock in trade in the Enlightenment – for example, the positing of an indefinite perfectibility of the human person, or the insistence that scientific and moral progress are two sides to the same coin. This does not prevent anyone here from finding some ways in which to marry
something like Carnap's project to an Enlightenment vision – but anyone doing that is not endorsing Carnap's views, but bending them to fit a different project from Carnap's own. Indeed, anyone motivated to tell such a story is worried about finding a social project for philosophy that connects contemporary philosophy to its eighteenth-century legacy, whereas Carnap, as his own historical framing narratives indicate, had no such interests. Far from building a bridge to the eighteenth century, Carnap sought in word and deed to overcome the history of philosophy and offer a new task for philosophy in a scientific age.

Philosophy as a form of engineering that offered philosophical technology as its fruits would seem to invite direct questions from any decent account of engineering and technology. I confess, however, to finding standard philosophies of technology in the analytic tradition or in the tradition of Heidegger and Marcuse unenlightening. The latter seem more rhapsodic and judgmental than usefully critical, while the former seem too much in the mode of applied ethics, with the technology dropping from the sky and then being investigated for 'consequences.' Moreover, both of them seem to treat technology largely in abstraction from both its originators and its savvy users. Carnap's philosophical perspective and its historical trajectory remind us, however, that technologies are both made and used, and often not used as those who made them anticipated. Technology studies have offered important perspectives on such issues regarding technologies from pharmaceuticals to early automobiles. We could do worse than take Carnap's philosophy as applied science perspective seriously enough to interrogate it with our best philosophically, historically, and culturally cognizant accounts of engineering and technology, wherever we may find them.

5 Historiographic conclusions: Rationally reconstructing Carnap

There is one final question it might be well to answer. I have offered interpretations of Carnap in the past that have stressed Carnap's continuity with certain themes in neo-Kantian philosophy. Such concerns have been submerged in this chapter. Have I repudiated those claims? The short answer is no – nothing I have read in the dozen years since my book came out has led my to think that Carnap can be properly understood without attending to the neo-Kantian literature that forms much of the background to his work in the 1920s. What then is the relation between Carnap's debt to neo-Kantianism and the metrological engineering point of view I stress here? The answer there is longer and worth attending to, for it touches on issues in the historiography of philosophy.

I see no necessary relation between neo-Kantianism and Carnap's engineering point of view in his mature philosophy. Neo-Kantianism, even the
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scientifically-savvy ‘logical idealism’ of Ernst Cassirer (1910), did not have to unfold into Carnap’s peculiar philosophical project. Nor must anyone with a generally engineering frame of mind ultimately stand in a clear relation to real, extant projects in neo-Kantianism. I abjure all essentialist and teleological history of such sorts. Rather, I take Carnap to have both learned something about how to think about the objectivity of scientific knowledge from neo-Kantianism – something deeply significant to all his subsequent theorizing about scientific knowledge – but also to have performed an applied science point of view on neo-Kantianism itself. This being the case, there are some issues at the heart of the development of post-Kantian philosophy in various German traditions that arise in a particular way in thinking about Carnap’s philosophy.

What is Carnap’s ultimate debt to neo-Kantianism? It is this sequence of ideas: that the vehicle of knowledge is the proposition, that the proposition represents by virtue of its logical form, and thus that the key questions of the objectivity of knowledge must be answered by attending to questions of the logical form of knowledge. In the elaboration of this key theme, I argue that Carnap discovered a clear difficulty in implementing it explicitly as a claim about objectivity given the resources of modern formal logic – this is the failure of the Aufbau. In response to that problem, Carnap adopted the formal mode of speech and the methods of meta-logic – and, in the process, no longer could use any remnant of neo-Kantian epistemological language to discuss his own project. But we can see that the methodological role of the analytic sentences in formalized languages for science is precisely to provide the meanings of empirical terms and, metaphilosophically, to limit rational philosophical inquiry to the formal conditions of representation.

Carnap’s characteristic engineering attitudes are rather orthogonal to all this conceptually but are implicated in it materially from the start. It is only because in the Aufbau (Carnap, 1928) he sought to turn neo-Kantian epistemological views into a technical project that the inability of his technical means precisely to match his philosophical goals became clear. Moreover, his applied science perspective provides Carnap a clear version of an irrealist attitude that need not, and does not, match in its details either the phenomenalism of some versions of positivism or the specific bracketing of substantial ontological questions in Cassirer’s neo-Kantianism. This could provide him with his early sense that he was trying to capture a philosophically-neutral core of a variety of German epistemological projects.

This is to say that I am not now, nor have I ever been, committed to all of Carnap’s characteristic philosophical ideas coming from neo-Kantianism. Similarly, I do not feel the slightest pull to answer questions such as ‘Is Carnap ultimately a (neo-)Kantian, a Humean, a Machian, or a Wittgensteinian?’ on this or that matter. I reject the alternatives. The sense to be made of Carnap’s philosophy should not be viewed as coming from without from
some historically-available or interpretatively-dreamt-up list of possibilities. The sense we make of his philosophy comes from a richer engagement with the details of his own philosophy as well as the philosophy and science (and other aspects of intellectual and cultural life) that we know he took seriously.

This is also the source of the sense we do not make of his philosophy. While we should be committed to making as much sense as possible of the work of any historical figure in philosophy, we also should note the clear tensions in that person’s work, those evident to her and those not quite so evident to her. In the case of Carnap, one tension that is there to see – and one that Carnap’s own commitments preclude him from addressing in any full sense – is the tension between the deflationary aspect of his engineering perspective and the remaining elements of transcendental philosophy. One drive of Carnap’s engineering perspective is to make any real philosophical issue into a technical issue. It is for this reason that Carnap rejects any transcendental explanation of why logical form has the status of the formal condition of the possibility of knowledge. This status can be exhibited and investigated but not really explained – certainly not in Kant’s own way as due to logical forms being the forms of the transcendental mind. Nonetheless, Carnap’s work still bears remnants of the transcendental perspective – even in the clear distinction between theoretical reason (which demands a language be in place) and practical reason (which always operates but operates alone when the question of which language to choose is being decided). Carnap’s epistemology posits a location for practical reason and will in the realm of knowledge, but Carnap cannot provide any philosophical account of that location. This is, indeed, a place where philosophical engineering, which is not be able in quite so straightforward a way simply to presuppose an ‘us’ who serve as the developers and adopters of technology as does ordinary engineering, faces a particularly interesting and deep problem.  

The history of analytic philosophy is often written as if the philosophical framework for that history may be taken for granted – it is a chapter in the history of empiricism, say, or in the history of philosophical analysis. The general trend toward reading Carnap through the lens of philosophical engineering has, since its inception, been salutary in avoiding such attempts at simplicity and transparency. To this point, however, the engineering perspective on Carnap has lacked a clear shape or, in the work of Carus, has been attached to another philosophical framework that is at best puzzling both as a framework for understanding Carnap and as a framework for understanding engineering. This chapter is an attempt to sketch – but only to sketch – a perspective on Carnap as offering a particular applied science perspective within a general project within logical empiricism of securing the scientific status of philosophy. One benefit of such a perspective is that it helps specify what engineering might amount to for Carnap.
Another benefit is that it can help relocate the question of the adequacy of Carnap's philosophy. One final benefit that I hope it will have is to further enrich the resources available for offering an historical account of Carnap's philosophy but also of logical empiricism and of scientific philosophy more generally.

Notes

1. For example, Creath, in a paper given in 1990, writes regarding Carnap's combination of conventionalism and pragmatism, 'This is an engineering perspective' (Creath, 1991, p. 409).
2. See Rorty (1967) for 'the linguistic turn' and Dummett (1996) for the specific claims about the sort of linguistic turn one can find in Frege and phenomenology.
3. Of course, the 1929 manifesto does make reference to the French Enlightenment Encyclopédistes. I do not find this terribly significant for our understanding of Carnap, for several reasons. First, the manifesto makes reference to many historical figures whose actual importance to logical empiricism at all needs to be argued, not assumed. Second, encyclopedism is a much more important project to Otto Neurath than it is to Carnap. Third, we cannot take for granted that the eighteenth-century encyclopedic ambitions of the French philosophes are substantially similar to those of Neurath or logical empiricism more generally.
4. On logical empiricism and scientific philosophy, see Richardson (1997a) and (in preparation).
5. A rekindled interest in the history of experimental practice has led to a renewed interest in the history of metrology among historians of science. Some characteristic perspectives can be found in Shaffer (1997), Wise (1996), and Buchwald (1996). In the last-mentioned volume, the essay by Olesko (1996) is especially important for understanding the German context.
6. Carnap (1926) cites Helmholtz's "Zählen und Messen" (Helmholtz, 1887) in this matter.
8. This point, on my view, is the main argumentative point of Carnap (1926).
9. Julius Springer began publishing the Zeitschrift für Instrumentenkunde in 1881. It was still being published during the years when Carnap was a student.
10. For the work with Bar-Hillel, see Bar-Hillel and Carnap (1953). For Carnap's war-time activities and his advocacy of engineered human languages such as Esperanto, see Carnap (1963).
11. Carnap deals with this very example in Carnap (1923).
12. This is a very compressed version of an account I provide in more detail in Richardson (1997b).
13. There has been some confusion about whether showing all sentences of a system synthetic shows there is no distinction to be drawn between the analytic and the synthetic. Carnap actually outlines ways of putting all of mathematics into the inference rules of a language and getting a language in which the sentences all turn out synthetic. That does not show, even for that very language, a well defined analytic/synthetic distinction is impossible; in fact, it presupposes that the distinction is in hand, since otherwise the claim that all the sentences are synthetic is without determinate meaning.
14. Most notably in Richardson (1998), but I have written other chapters in this matter, for example, Richardson (2003).

15. I discuss this issue in light of some recent remarks by Bas van Fraassen (2002) and Michael Friedman (2001) in relation to Carnap’s and Reichenbach’s work in Richardson (2005) and (forthcoming).

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Quine and the **Aufbau**: The Possibility of Objective Knowledge

*Peter Hylton*

1 Introduction

From one perspective, Carnap and Quine appear to be rather similar in their preoccupations and their doctrines; seen from another, however, they seem to be diametrically opposed, and on absolutely fundamental issues. The particular point of comparison with which I am concerned here has to do with Carnap’s *Aufbau* (1928). Quine devotes considerable space to the discussion of that work. A more or less representative passage is as follows:

Russell reflected in 1914 on realizing the dream of empiricist epistemologists: the explicit construction of the external world, or a reasonable facsimile, from sense impressions, hence from simple ideas. He adumbrated it in *Our Knowledge of the External World*, and a dozen years later Rudolf Carnap was undertaking to carry it out. Carnap’s effort found expression in *Der Logische Aufbau der Welt*. (Quine 1995, p. 10).

This passage, and others like it, seem to indicate that Quine attributes to Carnap, in the *Aufbau*, a phenomenalist version of empiricism, with all our knowledge based upon, and in the strictest sense reducible to, a fundamental kind of knowledge which is immediately given in sensation.1

The problem with Carnap’s view, so the Quinean story continues, is that the reduction fails. It fails at a crucial point: the step from claims about experience – the subject’s psychological states – to claims about the external world (sections 125–128). This step involves the introduction of a new concept, the relation ‘is at’, which enables us to state that perceptual qualities (colours, for example), are located at objective points in space and time. The concepts introduced in the earlier stages of the construction were introduced by definitions which make it possible, in principle, to eliminate them. The ‘is at’ relation, however, is introduced by a different method. Carnap does not attempt to give an eliminative definition. Instead, he merely gives guidelines for the use of the concept: He lays down *desiderata* and tells us that perceptual qualities are (or should be) assigned to points in...
such a way as to maximize the satisfaction of those desiderata. This method does not make it possible, even in principle, to eliminate the ‘is at’ relation. At this crucial point, Carnap does not give a reduction, in the sense of an eliminative definition. (Carnap later came to accept this point.)\(^2\) Once we understand the underlying reasons for the failure of Carnap’s reductionist project at this point, Quine holds, we see that it is overwhelmingly plausible that any similar project would also fail.\(^3\) The failure of Carnap’s reductionist empiricism opens the way for Quine’s naturalized version of empiricism.\(^4\)

Recent work on the *Aufbau* challenges this Quinean reading of that work.\(^5\) These interpretations argue that Carnap is not there putting forward a kind of ‘strict empiricism’.\(^6\) In particular, the reinterpretation challenges the idea that Carnap is there putting forward any kind of phenomenalistic epistemology, or anything similar to Russell’s view in *Our Knowledge of the External World*. Instead, it suggests a rather different picture of what Carnap is up to in the *Aufbau*, a picture which emphasizes Carnap’s neo-Kantian roots, and claims that Kantian concerns underlie the *Aufbau*.

It is no part of my concern here to dispute the reinterpretation of the *Aufbau*. I hope, rather, to use it to shed light on the differences between Carnap and Quine quite generally. To this end, I focus on what the comparison and contrast between Carnap and Quine looks like, in the light of the reinterpretation. I shall argue, in particular, that Quine’s epistemology provides an answer to what is, in some sense, the same question as that which is now seen as the focus of the *Aufbau*.\(^7\) Emphasizing this point of similarity will enable us to achieve deeper insight into the differences between the two philosophers.

2  The re-interpretation of the *Aufbau*

Recent work on the *Aufbau* emphasizes the neo-Kantian influences on Carnap and sees that work as Kantian rather than as empiricist in spirit. The contrast, according to this interpretation, is as follows. An empiricist approach to knowledge takes it for granted that certain and unproblematic knowledge is simply given in sensory experience. The question which the empiricist attempts to answer is then: How is that certain and unproblematic character to be transmitted to the rest of our knowledge, which is more remote from experience? According to the new interpretation, Carnap is asking a quite different question in the *Aufbau*. It focuses not on the transmission of knowledge, but rather on the constitution of knowledge. How can knowledge, which is objectively valid and intersubjectively communicable, arise on the basis of experience, which itself appears to be subjective and private? How is objective knowledge constituted from experience, which is variable and subjective? What are the constitutive principles which make objective knowledge – or, indeed, objective error – possible at all? The question is assimilated to that which Kant sought to answer by invoking synthetic *a priori* principles and
forms of intuition. Carnap’s answer, however, avoids anything as metaphysically loaded as a Kantian conception of the synthetic \textit{a priori}. He attempts, rather, to show how logic alone can play the required constitutive role. Logic here is Russellian simple type theory, taken as analytic.\textsuperscript{8}

Let us flesh out this very cursory account of the re-interpretation by looking at some relevant passages from some of its leading advocates. In his chapter, ‘From Epistemology to the Logic of Science’, Alan Richardson writes:

\begin{quote}
The epistemological project of the \textit{Aufbau} is... to show \textit{how objective knowledge is possible given the subjective origin of knowledge in individual experience}. This Kantian-sounding problem is given a Kantian-sounding answer in the stress Carnap puts on the notion of the form of experience. The form of experience is identical for us all and is sufficiently rich to allow the definition of the concepts of science as themselves purely structural entities. It is the logical form of experience that allows us to ascend from a purely subjective world of primitive experience to an objective world of science (Giere & Richardson, 1996, p. 312, emphasis added).
\end{quote}

He appends a note that starts like this: ‘This is not strict Kantianism since this form is now logical form alone’. (note 10, p. 331).

The issue of the objectivity of knowledge could be understood as simply a concern about whether we can be objectively right about the world – whether we can know that our assertions are true in virtue of the way the world is. But this is not how Richardson intends the issue of objectivity. As he means it, it is rather a question about how it is possible to make objective claims about the world, whether those claims are true or false. In particular, it concerns the question of how we can make objective claims which are answerable to sensory experience, given the subjective nature of that experience. A helpful passage on this point occurs in Michael Friedman’s work.\textsuperscript{9} He is discussing Carnap’s neo-Kantian background. ‘The primary problem’, Friedman says, ‘does not involve the justification of our beliefs, the refutation of philosophical skepticism’:

\begin{quote}
Instead, such neo-Kantian philosophers occupy themselves with what they take to be the prior problem of how ‘objective judgements’ are possible in the first place: what makes such things as judgments – which are essentially capable of either truth or falsehood, justification or disconfirmation – possible? How does it come about that our thought, which initially appears to be confined to merely subjective representations...nonetheless acquires objective meaning...so that questions of truth and falsity (and thus questions of epistemic justification) then apply? (Friedman 1999, p. 125–126)
\end{quote}

It is this \textit{constitutive} question that Carnap is primarily concerned to answer, according to Friedman. He avoids transcendental logic and synthetic \textit{a priori} forms of intuition; he attempts, rather, to answer the question by relying
on Russellian logic, assumed to be analytic. Thus, as Richardson puts it in another chapter:

The analytic/synthetic divide took over the methodological significance of the divide between the synthetic a priori and the synthetic a posteriori for Kant – it captured the division between the principles that constituted [sic] the possibility of objective judgment for a framework and the objective judgments made within that framework. (Richardson 1997, p. 161)

Again, a note appended to the passage if of particular interest. It says, in part:

‘[Carnap] is...interested in showing...that logical principles can serve as conditions of the possibility of objective, scientific knowledge’ (ibid. p. 168, note 48).

3 Quine’s epistemology

How do Quine’s epistemological endeavours relate to those of Carnap in the Aufbau, interpreted in the way I have indicated? The answer to this question is complicated and reveals much about Quine’s thought and about its differences from that of Carnap. To begin with, consider the words which I emphasized in the first passage that I quoted from Richardson. Carnap, he says, attempts to show ‘how objective knowledge is possible given the subjective origin of knowledge in individual experience’. Quine’s work more or less implicitly contains an answer to a question that could be phrased in exactly this form of words.

I put the matter in this somewhat cautious way for two reasons. First, I do not wish to claim that Quine’s work answers the same question, in any very strong sense, as that which occupies Carnap in the Aufbau. Criteria for the identity of questions are sufficiently unclear to make this a fruitless issue. But I certainly do not deny that there are great differences between the Quinean and the Carnapian answers. To the contrary; as I have indicated, it is precisely these differences which are my primary concern, and I shall return to them in the final section of this chapter.

There is also a second reason that leads me to state my claim cautiously. I do not think that Quine sets out to answer the Carnapian question; I do not think he ever formulates exactly this question. His concern is with a more general question: how we, human animals, acquire the knowledge that we take ourselves to have, and the cognitive language in which that knowledge is embodied. Still, the way the question is phrased certainly corresponds to elements in Quine’s epistemological work, as we shall see. Quine certainly thinks that our language and knowledge are public and objective. He comes to recognize, moreover, that the starting point for our knowledge is, in one sense, subjective. But that point was not clear to him until his
epistemological project was well underway, and certainly does not form part of the overt motivation for that project.

So our constitutive question – How is objective knowledge possible, given the subjective origin of knowledge in experience? – is not what motivates Quine. Nevertheless, his work does end up more or less implicitly providing an answer, or the sketch of an answer, to this question. In the rest of this section, I shall briefly rehearse some of the crucial stages in Quinean epistemology in order to show how it answers our question.

Quine, as is well known, interprets the notion of (sensory) experience in terms of the stimulation of sensory nerves. More accurately, perhaps, he thinks that ‘for scientific and philosophical purposes’ we shall do well to employ the latter [stimulations] rather than the former. Is the stimulation of a given person’s sensory nerves subjective? In one sense, it is not. It is an objective physical event, with physical causes and consequences. (It is for just this sort of reason, of course, that Quine holds that talk of stimulation of nerves is preferable to talk of subjective experience; it is accessible to the scientific method.) In another sense, however, the stimulation of sensory nerves is indeed subjective. Such an event is peculiar to a single subject. My nerves are not yours: There is no sense in which we can say that you have the same nerves stimulated on a given occasion as I do on the same or a different occasion. Stimulations of the nerve endings are in no sense shared. The project, then, is an attempt to explain how knowledge which is shared, public, and objective is possible on the basis of ‘experience’ (or Quine’s scientific substitute) that is idiosyncratic and, in this sense, subjective.

As I have indicated, however, Quine himself did not begin by thinking of the matter in this way. Early on, he did not fully appreciate the subjectivity of stimulations. This point is clear from his first attempts to define the crucial idea of an observation sentence. The version that he gives in Word and Object is, very roughly, this: For an observation sentence, the same stimulations lead to the same verdict, both for various people and for one person on various occasions. For the one person case, the individualistic criterion for being an observation sentence, the matter is complicated but not, in principle, problematic. The complication arises because what we need is not the idea of identical stimulation-patterns but rather the idea of relevantly similar stimulation-patterns, and defining this idea (which Quine comes to call perceptual similarity) turns out to be quite tricky. For the cross-person case – the intersubjective criterion – however, the matter is far worse: What Quine says in Word and Object assumes that we can equate stimulation-patterns across persons, and speak of two people as having the same stimulations. In Word and Object Quine thus builds in a certain kind of intersubjectivity to the notion of experience, or his substitute. He simply allows himself to speak of two people undergoing the same stimulations and does not raise questions about this idea. The idea is, however, clearly illegitimate.
Quine fairly quickly came to see that something was amiss. It was not until the late 1980s, however, that he began developing what he took to be a satisfactory solution, and he did not achieve full clarity about the matter until the mid-1990s. In Quine’s eventual solution, there is no longer any thought of equating stimulation-patterns across persons. Stimulation-patterns are firmly subjective. So also is the crucial relation of perceptual similarity which – to speak very roughly – holds between two of a given person’s stimulation-patterns if that person tends to respond in similar ways to each. The individualistic criterion for being an observation sentence is phrased in terms of stimulation-patterns and their perceptual similarity. Each individual is taken to be responding to their own stimulations, not to events or objects in the world. The intersubjective criterion for being an observation sentence is explained by invoking the world beyond the stimulations. It is that ‘all members of the language community are disposed to agree on the truth or falsity of such a sentence on the spot, if they have normal perception and are witnesses to the occasion’ (Quine 1995, p. 22). Roughly, we are saying that a stimulation-pattern of yours is relevantly similar to one of mine if an occasion which gives rise to the one in you would give rise to the other in me.

What underlies the possibility of language, on this account, is that various individuals’ standards of perceptual similarity line up in the right ways. Two occasions which give rise to perceptually similar stimulation-patterns in me must, often enough, give rise to stimulation-patterns in you which are perceptually similar for you. Without this sort of correspondence, we could never have developed the sort of uniformity of behaviour – the Übereinstimmung, one is tempted to say – which makes language possible. Quine speaks of ‘coordination of behaviour across the tribe’ (Quine 1995, p. 20) and of ‘a preestablished harmony of perceptual similarity standards’ (p. 21). To explain them, he invokes natural selection, which he calls ‘Darwin’s solvent of metaphysics’ (Quine 1996, p. 161).

Such, in briefest outline, is the first stage of Quine’s account of the possibility of knowledge. It addresses the question of how we are able to coordinate our responses to sensory stimulations – and thus how observation sentences are possible – although the stimulations themselves are not shared. This is, in effect, the question of how something intersubjective, namely the shared capacity to use observation sentences, is possible on the basis of something private and subjective, namely sensory stimulations.

Observation sentences, as we have thus far considered them, however, are merely responses to stimulation. This fact suggests that we have not yet reached the level of objective knowledge: a mere response to stimulation, it may be thought, is not yet a claim about the world, objectively true or false. The point emerges clearly from a consideration of the corrigibility of observation sentences. Responses to stimulation are not corrigible. If I feel a sudden sharp pain and say, ‘Ow!’ , I cannot be mistaken. Objective
judgments about the world, unless simply about the speaker’s current condition, are, by contrast, notoriously subject to sceptical doubt.

If observation sentences are to embody objective knowledge, then, they must be corrigible. Corrigible sentences, however, cannot be mere responses to stimulation. The sentence ‘There’s a horse’, for example, is corrigible because it may look for all the world as if there is a horse in front of us when in fact there is a carefully contrived illusion. So, to understand ‘There’s a horse!’ as an objective report about the world, one has not merely to come to assent to it when receiving the appropriate stimulations – the sight or sound of the beast. One has also to learn when those stimulations may be deceptive – that seeing a man with half coconut shells should make one discount what seems to be the sound of hooves on pavement, for example. The adult user of the sentence is presumably disposed to withhold or retract assent under such circumstances, i.e. she is not invariably disposed to assent when receiving appropriate stimulations. Even for a simple observation sentence, the disposition to respond appropriately to stimulations will not suffice; some background knowledge of the world is also required.15

So background knowledge is required for the full learning of even a very simple observation sentence. How is that knowledge to be acquired? Quine says little or nothing that is directly relevant. (As indicated in the previous footnote, indeed, it seems that he does not fully recognize the need for an answer to the question.) But nothing seems to rule out an answer’s being given along Quinean lines. The answer would presumably have the child first learning mere responses to stimulation – defective versions of observation sentences. Using these, he picks up some rudimentary, and partially defective, knowledge of the world, which then enables him to acquire something more closely approximating a mastery of some corrigible English sentences. This enables him to acquire some knowledge of the world which is less rudimentary and defective.

All of this is both speculative and exceedingly sketchy: I am speculating about a Quinean response to the problem which the corrigibility of observation sentences raises for his picture of how objective language, and thus also objective knowledge, is acquired. A full response would be quite complicated. The child must learn that what appeared to be so at one time can turn out later not to have really been so. For this to occur, the child must presumably acquire some idea of the distinction between the way things seemed and the way things later turn out to be. (Perhaps he hears adults say, ‘It looked like a rabbit, but it wasn’t one really’, and things of that sort.) He must also, of course, acquire some understanding of the circumstances which justify subsequent retractions of earlier assertions, and this is likely to involve at least rudimentary knowledge of ways in which objects behave and causally interact. (That they do not, for example, pop into existence, or out of existence, without provocation or trace.) There is no reason to think that such a story could not be told along Quinean lines. Certainly, the
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need for such a story is implicit in Quine’s epistemology from the outset. The child does, somehow, come to be able to speak adult language. As part of this process, he must come to be able to correct (putative) observation sentences, to join other speakers of the language in retracting some of his earlier verdicts. How the child might acquire this ability is thus something that the Quinean must explain.

We have been talking about the learning of observation sentences – not merely as responses to stimulation, but as full-fledged claims about the world, objective and corrigible. Such sentences are, in Quinean terminology occasion sentences – true on some occasions of utterance, false on others. Our serious knowledge, however, is not of this sort. It is, rather, embodied in what Quine calls standing sentences, sentences true or false once for all. So, again, there is a question of how we get from the one kind of knowledge to the other. Quine, at this point, is offering no more than a speculative reconstruction of how we might have acquired the knowledge that we have. He holds that one plausible first step into standing knowledge, so to speak, is the learning of what he calls observation categoricals. An observation categorical is a sentence of the form: ‘Whenever X’, where X and Y are themselves observation sentences. ‘Whenever smoke, fire’, for example, or, less cryptically, ‘Whenever there is smoke, there is fire’. The claim here is that it is psychologically plausible that an infant who has mastered the observation sentences could come to pick up this further idiom. Quine does not, however, offer any sort of account of how we acquire the ability to form and use such sentences, although he explicitly recognizes that such an account is needed.

Observation categoricals embody primitive inductions. They also embody, in schematic form, the evidence for our scientific theories. And an observation categorical, Quine says, is itself ‘a miniature scientific theory’ (Quine 1995, p. 26.). Beyond this point, Quine’s speculations about the development of objective knowledge chiefly concern our ability to refer to objects.

One noteworthy feature of the story we have been telling is that it contains no mention of logic. Logic, as Quine sees the matter, plays no role in the first steps of the development of the system of objective knowledge. By the time the rudiments of that system are established, there will be inferential relations among sentences of the system. (For example, between observation categoricals and the observation sentences of which they are made up.) Logic is then imposed to systematize and clarify these inferences, sentences reformulated so as to fit the pattern that logic can easily deal with. In the fully developed system of knowledge, logic plays a vital role. It imposes a clear and well understood syntax on the sentence of the system; it reveals and clarifies inferential relations among the sentences; and it provides the standard by which the ontology of the system is gauged. At the beginning of the development of the system, however, it does not figure in the story.
4 Carnap and Quine

We have seen, in exceedingly sketchy and compressed form, with gaps that would require considerable work to fill, a Quinean story that takes us from stimulations of sensory nerves to observation categoricals. The stimulations are, in one sense, subjective and do not, of course, count as knowledge. Observation categoricals are public, and clearly a form of objective knowledge. If the story could be spelled out, and the gaps filled in, then it would indeed show, in Richardson’s words, ‘how objective knowledge is possible given the subjective origin of knowledge in individual experience’. These words might not describe Quine’s explicit epistemological aim, but they do describe a result that his epistemological project, if successfully carried out, would in fact achieve. This is a striking fact. But I do not emphasize it merely for its own sake, nor because I wish to assimilate the views of Carnap and Quine, and to claim that they are really – if perhaps unwittingly – engaged in the same enterprise. To the contrary: it is, rather, my hope that seeing matters in this way will provide a way of attaining a deeper understanding of the differences between them.

How, then, in the light of the above, should we think of the differences between Quine’s epistemological endeavour and Carnap’s project in the Aufbau? One obvious starting point is that the Quinean explanation is empirical. On any plausible understanding of the notion, it is clearly correct to say that the Quinean explanation proceeds on an empirical level. It draws freely on physiology and physiological psychology, for example, and, as we have seen, Quine invokes Darwin and the idea of natural selection at a crucial point. On a Quinean view, the epistemological problem is straightforwardly one of explaining how the human animal acquires knowledge, and the language in which that knowledge is expressed or embodied. The animal lives in a world of physical objects, which are governed by laws and regularities. Somehow it acquires knowledge of (at least some of) those objects and regularities; the problem is to account for this acquisition.

This way of conceiving of epistemology leads to Quine’s idea of ‘reciprocal containment’. The epistemological problem, as Quine conceives it, is a question within natural science; in particular, within psychology and physiology. In that sense, epistemology is contained in natural science. But the epistemological question is precisely a question about the origin and possibility of natural science, including, of course, psychology and physiology. A fully realized Quinean epistemology would explain how we know those things. In that sense, natural science is contained in epistemology. Psychology is to explain how we know, among other things, that very psychology itself (see Quine 1969, p. 83). Our earlier discussion emphasized the idea of epistemology as dealing with the constitutive question: How is objective knowledge possible? Quine’s notion of reciprocal containment, seen from this perspective, is the idea that
the answer to that constitutive question is part of the very knowledge whose possibility is being explained. But for these reasons, it is perhaps no longer a genuinely constitutive question that is under discussion at all – certainly, one cannot imagine Kant accepting anything like a Quinean answer.

According to his recent interpreters, however, Carnap is the heir of Kant. His project is, presumably, to give an a priori account of the possibility of objective knowledge. His break with Kant is primarily in disavowing the idea of synthetic knowledge which is a priori. Instead, he assumes only the logic of Principia Mathematica. A notion of structure, definable in logical terms, plays the constitutive role. Friedman puts the point like this:

Scientific knowledge is objective solely in virtue of its formal or structural properties.... the notion of form or structure in question here is a purely logical one, understood in terms of formal logic.... In other words, whereas Carnap retains the Kantian connections among objectivity, the notion of form or structure, and the a priori (....) he now has no need whatever for Kant’s synthetic a priori. (Friedman 1999, p. 98)

By drawing on the resources of type theory, Carnap hopes to account for the possibility of objective knowledge in a way which is not psychological, yet which avoids the metaphysics inherent in the Kantian notion of the synthetic a priori. How can we do epistemology in a way that does not involve us either in metaphysics or in psychology? Carnap’s answer, according to Friedman, is that logic is all that we need to do epistemology.

This point, if we take it as seriously as Friedman does, leads to an important point of contrast between Carnap at the time of the Aufbau and Quine over the role that logic plays. As we saw at the end of the previous section, logic plays no role in Quine’s account of the first stages of objective knowledge. He attributes great importance to the role of logic in the developed system of knowledge, but clearly logic in his view develops along with that system. It is not presupposed; it is, rather, imposed after the beginnings of objective knowledge are already in place. Hence, it is not constitutive of the very possibility of objective knowledge. For this reason, the question of the justification of logic – and the justification for choosing one system rather than another – makes sense for Quine. He answers it primarily in terms of the clarity of first-order logic, and its efficacy in revealing the inferential structure of our knowledge. But the present point is that it is, for him, a sensible question which requires an answer.

For Carnap, on the interpretation we are considering, by contrast, no question about the justification of logic can make sense. Without some notion of logic in place, there is no notion of objective knowledge, nothing to which we might appeal to justify anything. In the Aufbau, Carnap seems to assume one particular system of logic – simple type theory – as neutral and, so to speak, given. But we can perhaps see here a foreshadowing of the view that
later found expression in the Principle of Tolerance: Since there is no possibility of justifying logic, there is no right or wrong in choice of one system of logic over another.

The importance of logic in the *Aufbau* can hardly be overstated: it is the basis of his answer to the fundamental question, how inter-subjective knowledge is possible on the basis of experience which is *not* intersubjective. He begins with what he calls the 'basic elements' of experience and with the relation of 'remembered similarity' (*Rs*) which holds among them in some cases. The elements are not shared, but the logical structure which the *Rs* relation holds among those elements may be shared. It is in terms of this logically-defined notion of structure that Carnap answers his fundamental question. Science – which here means any form of serious knowledge (*Wissenschaft*) – should be intersubjective. It is also based on experience but '[t]he series of experiences is different for each subject' (*Aufbau*, section 16). But only if a statement can be reduced to a purely structural claim does it merit a place in objective science: *'each scientific statement can in principle be so transformed that it is nothing but a structural statement'* (loc. cit., italics in the original). Hence, the problem, as he conceives it: ‘how, through the application of uniform formal construction rules, entities result which have a structure which is the same for each subject.... This is the problem of inter-subjective reality’ (loc. cit.).

Carnap’s system, however, appears to have non-logical aspects which make it impossible to achieve his goal of transforming objective statements into purely structural statements. The basic elements can be defined as the domain of *Rs*. But what of the *Rs* relation itself? Carnap says – although in sections which he says ‘may be omitted’ – that leaving it undefined is ‘not in harmony with the earlier thesis that statements of science are purely structural statements.... A purely structural statement must contain only logical symbols’ (section 153). Because Friedman and Richardson see Carnap as arguing that objective knowledge is constituted *a priori*, by logic, they see this problem as a fundamental one.

Carnap attempts to resolve the problem by defining *Rs* in purely logical terms. Given the vast edifice built upon it, one might think that it could be defined by a definite description, as the one and only relation which supports that edifice. (This can be done in purely logical terms, by using Russell’s theory of descriptions: One asserts that there is a relation which plays such-and-such a role, and also that any relation playing that role is identical to the first.) However, given the resources of type theory, there will be many such relations. (In technical terms, the uniqueness clause will be false: There will be more than one relation playing the same role.) So, Carnap introduces the idea of ‘foundedness’ as a primitive term. He explains it by saying that relations are *founded* if ‘they correspond to some experienceable, “natural” relations’ (section 154). The hope, of course, is that if we add the requirement of foundedness, then the tactic of defining *Rs* by a
definite description will work after all: it will be the one and only \textit{founded} relation playing the relevant role. But all of this manoeuvring does not solve the original problem unless we have some reason to accept foundedness as a \textit{logical} notion. Not to quibble over a word, it is at the least quite unlike the other notions that more widely pass for logical.

In retrospect, indeed, the whole project of a purely logical epistemology is perhaps doomed from the outset, as Friedman himself indicates. If the project succeeded, all knowledge would be a matter of logic; we could not do justice to the idea that some of our knowledge is answerable to the contingencies of sensory experience. As Friedman says:

We are motivated to pursue a program of complete formalization by a conception of scientific objectivity that seeks to disengage objective meaning entirely from ostension. But what can ‘experienceable, “natural”’ relations’ be except precisely those relations somehow available for ostension? Our original motivations, in other words, have been totally undermined by Carnap’s final move. The difficulty is an extremely fundamental one. If we succeed in disengaging objective meaning and knowledge from ostension and lodge them instead in logical form and structure, then we run the risk of divorcing objective meaning and knowledge from all relation to experience or the empirical world at all. We run the risk, that is, of erasing completely the distinction between empirical knowledge and logico-mathematical knowledge. (Friedman 1999, p. 103)

Carnap in the \textit{Aufbau} does not succeed in transforming epistemology into logic unless we count ‘foundedness’ as a logical concept. On the face of it, however, foundedness is clearly \textit{not} a logical concept; it is a psychological concept. As the passage from Friedman makes clear, moreover, it is essential that foundedness \textit{not} be a logical concept, if we are to retain the idea that (some) knowledge is non-logical in character. The moral here is that epistemology cannot be done by logic alone; if we are to avoid metaphysics, we cannot wholly avoid psychology.

Carnap, it seems, comes to recognize this point. In the final chapter of \textit{Logische Syntax der Sprache}, published in 1934, six years after the \textit{Aufbau}, he says that logical questions occur ‘in the so-called theory of knowledge (or epistemology), where they are, however, for the most part, entangled with psychological questions’ (Carnap, 1937, p. 278). In his proposed clarification of the situation, ‘the logic of science takes the place of the inextricable tangle of problems which is known as philosophy’ (p. 279; emphasis in the original). Along the same lines, he says in the 1936 chapter ‘Von der Erkenntnistheorie zur Wissenschaftlogik’:

As it seems to me, theory of knowledge (Erkenntnistheorie), in the form in which it has existed so far, is an unclear mixture of psychological and
logical components. This holds also for the work of our circle, my own earlier work not excluded. Much unclarity and many misunderstandings result from this (Carnap 1936a, p. 36, my translation).

Carnap's epistemological work after the 1920s does make a clearer separation of psychology from logic. His concerns continue to be with the logical aspect: He continues to make a sharp separation between philosophy and empirical subjects, including psychology. One consequence of this is that he no longer attempts to give a philosophical account of the most elementary kind of objective knowledge. His later works discuss the relation between the theoretical sentences of a language and its observational sentences; they do not, however, offer any account of how we know the latter. That he sees as a task for psychology, and so not of philosophical concern. This point is explicit in 'Testability and Meaning'. He introduces the terms 'observable' and 'realizable' and says that 'Definitions for them [these terms] would have to be given within psychology, and more precisely, within the behaviouristic theory of language'. (Carnap 1936b, p. 454, n. 4). He does not attempt to give such definitions himself.

One general lesson which Quine and others have drawn from the Aufbau is the failure of radical reductionism. Our recent discussion suggests another: that we cannot do (non-metaphysical) epistemology without psychology. We cannot explain human knowledge unless we take account of the role of the propensities of the human organism, or the human mind, to respond to stimuli. Epistemology, at least of the most basic kind of knowledge, requires psychology; if it is not to be transcendent psychology, it must be empirical psychology. In the face of this fact, Carnap sticks to the separation of philosophy from anything empirical and gives up the idea of a philosophical account of the most basic kind of knowledge. We might see Quine as making the opposite response to the same fact. He simply accepts the need for psychology and gives up on the idea that philosophy cannot be empirical. He sticks to epistemology – or at least to something that might be described by that word – and approaches that subject in a way that is psychological through and through.

Generalizing the point, the status of philosophy has, I think, been a particular problem since the rise of psychology as an independent and (supposedly) scientific discipline. The power of the logic introduced by Frege and Russell gave rise to a new philosophical tradition which, far from embracing psychology, insisted on the distinction. Within that tradition, some – perhaps most notably Russell – sometimes claimed that philosophy simply is logic. As interpreted by Friedman and Richardson, the Aufbau is perhaps the high-water mark of that idea, since on their view it claims to allow us to do epistemology by means of logic alone. Quine's revolution is to claim, by contrast, that, when it comes to epistemology, it is psychology that is fundamental.16
Notes

1. Many other passages in Quine's work express the same view of the *Aufbau*. In 'Two Dogmas of Empiricism' (Quine 1953, pp. 20–46, originally published in 1951), he says: "Radical reductionism...set itself the task of specifying a sense-datum language and showing how to translate the rest of significant discourse, statement by statement, into it. Carnap embarked upon this project in the *Aufbau* (p. 39). In 'Epistemology Naturalized' (Quine 1969, pp. 69–90), Quine speaks in terms strikingly similar to those quoted in the main text, above. Immediately after mentioning Russell *Our Knowledge of the External World* (1914), he says: 'To account for the external world as a logical construct of sense-data – such, in Russell's terms, was the program. It was Carnap, in his *Der Logische Aufbau der Welt* of 1928, who came nearest to executing it' (p. 74).


3. See, again, 'Two Dogmas of Empiricism' (Quine 1953, pp. 20–46), especially section 5. See also 'Russell's Ontological Development' (Quine 1986, pp. 73–85); the most directly relevant passage is on pp. 84–85.

4. For one telling of this story – though one that does not mention Carnap explicitly – see 'Five Milestones of Empiricism' (Quine 1986, pp. 67–72). See also again 'Epistemology Naturalized' (Quine 1969, pp. 69–90).

5. Most notable here are a group of essays by Michael Friedman, originally published between 1983 and 1997, and now collected in Friedman (1999), and a number of works by Alan Richardson, including his monograph, Richardson (1998).

6. See, for example, Richardson (1998), pp. 10f, *et passim*. It seems to me a weakness of Richardson's approach that he takes for granted the idea of empiricism ('good old-fashioned empiricism', he calls it at one point), as if it were a single more or less unified doctrine, well understood by all. This seems to me very far from the truth; substantive philosophical issues arise over the issue of exactly how we can and should understand the idea of empiricism.

7. One point that I am not going to go into, however, is the comparison that Quine draws between his own treatment of global stimuli and the *Aufbau* treatment of elementary experiences. See Quine (1995), Chapter II.

8. Here there is a clear comparison with Frege, largely unremarked by the commentators I am considering. Frege wished to show that one central branch of our knowledge which Kant had thought of as synthetic *a priori* can be understood as purely a matter of logic, where logic is again conceived of as essentially including set theory. Carnap attempted to carry out the replacement of the synthetic *a priori* with logic in a more thoroughgoing fashion, in every part of our knowledge.


10. Carnap, in the *Aufbau*, interprets the notion of sensory experience as the whole of a person's experience at a given time, 'experiences...in their totality and undivided unity' (section 67). Quine follows him in drawing the notion of experience from scientific theory; he diverges in taking the relevant science to be physiology rather than Gestalt psychology.

11. For Quine's use of the phrase 'for scientific and philosophical purposes' in a closely related context see, for example, his 'Responding to Richard Schuldenfrei' (Quine 1986, pp. 184–186), *passim*.

13. For early signs of this progress, see Quine’s ‘Three Indeterminacies’ (Barrett & Gibson 1990) (section 3 of the essay in particularly relevant). The fully worked-out and explicitly endorsed solution is to be found in Quine (1995) and Quine (1996).

14. Compare Wittgenstein: ‘If language is to be a means of communication there must be agreement [Übereinstimmung] not only in definitions but also (queer as this may sound) in judgments’ (Wittgenstein 1953, section 242).

15. Quine came to accept that even the best candidates for being observation sentences are corrigible, and that observers may subsequently retract their verdicts (see Quine 1996 “”). Corrigibility, however, has a consequence which he does not seem to have realized. Suppose, as above, that we are in a situation in which the illusion of a horse has been carefully contrived. Then those who are ignorant of the illusion will presumably be disposed to assent to ‘There’s a horse’, whereas those who are in on the illusion will not be. It follows that ‘There’s a horse’ does not count as an observation sentence by Quine’s definition, for part of that definition is that there must be community-wide agreement about when it is correct to assert (or assent to) the sentence: all who are ‘witnesses to the occasion’ will agree. The point can, presumably, be duplicated for any corrigible sentence; none will entirely satisfy his definition. The correct moral to draw, I think, is that we should work with the notion of a sentence being more or less observational, where this is understood in terms of the frequency of deceptive situations (such as the contrived illusion of there being a horse in the offering). Sentences which are, by this criterion, very highly observational will more or less play the role in which Quine cast his observation sentences.

16. For comments on earlier drafts of this chapter I am indebted to Gary Kemp, Andrew Lugg, Sally Sedgwick, and the editor of this volume.

References

Quine and the Aufbau

Ryle’s Conceptual Cartography

Julia Tanney

1 Introduction

Interested as a young philosopher in the ‘massive developments in logical theory’ of the early part of the 20th century (many of which he ‘read back’ into the works of the mature Plato). Ryle found himself sympathizing with Platonic, because anti-psychologistic, theories of meaning. Yet, from the start, he was convinced that this Platonic ‘tendency to populate the world with Objects’ had to be resisted.

After reading the Tractatus as a young man, and gleaning its implications for philosophical logic, Ryle anticipated in print by almost 30 years a view of the nature of philosophical investigation that would come to be associated with the writings of (the middle and later) Wittgenstein. Today, he is regrettably unacknowledged for his early and independent development of a number of key ideas: philosophy’s tendency to generate puzzles by mistakenly assimilating the logical form of an expression with its grammatical form; the existence of a radical context-sensitivity – a systematic variability of the logical powers of expressions in their indefinitely many (acceptable or correct) employments; and (thus) the parasitic role of concepts on the use of expressions in the various practices and activities from which they can be discerned as abstractible features; and (as I gloss it) the ‘amorphousness’ of the concepts so discerned. My goal in this chapter is to highlight these ideas in Ryle in order to show how very different is the conceptual cartography he favours from a priori conceptual analysis as it is conceived today.

2 The Cambridge transformation of the theory of concepts

The trend from the mid-1800s, both in Germany and in England, Ryle tells us, involved a recoil against the British School of Thought exemplified by
Locke, Hume and J. S. Mill. For Husserl, Meinong, Frege, Bradley, Peirce, Moore and Russell

were [all] alike in revolt against the idea-psychology of Hume and Mill; all alike demanded the emancipation of logic from psychology; all alike found in the notion of meaning their escape-route from subjectivist theories of thinking. (CP1, 187)

However, Ryle tells us, nearly all of them championed a Platonic theory of meanings, or of concepts and propositions. They distinguished the conceptual enquiries of philosophy from the factual enquiries of natural science, yet

nearly all of them talked as if these conceptual enquiries of philosophy terminated in some super-inspections of some super-objects, as if conceptual enquiries were, after all, super-observational enquiries. (Ibid.)

Russell talked of acquaintance with Universals, Moore talked of inspecting concepts, and Husserl talked of intuiting essences. Philosophy was construed as a sort of observational science like geography, but the objects it inspects are semi-Platonic ones, out of space and time. Conception was supposed to involve the apprehension of Concepts or word-meanings; judgment the grasp of Propositions, Thoughts or sentences-meanings. It is this traditional view that was challenged by what Ryle calls the ‘Cambridge transformation of the theory of concepts’ (CP1, 189); though Ryle seems to have been overly optimistic in calling it a ‘transformation’.

J. S. Mill, Ryle tells us, was one of the first philosophers to start talking about meanings and not merely operate with them. For Mill, the primary vehicle of meaning is a word, and a sentence is the compound of these components. This, Ryle tells us, was the first false move. The next false move was to construe words as names and then to say that what a word means is the object. But the two moves (as the later Wittgenstein recognized, in his battle against the ‘Augustinian’ view of language) are disastrous. First, a sentence is not a mere list: a sentence says something either true or false, but a list of names says nothing. This alone shows that a sentence is not decomposable into a list, even if this list functions to name a series of objects. Second, as Mill himself allows, we can use different phrases to stand for the same thing in cases in which it is quite clear that the two phrases mean something different; thus, the meaning of the phrase cannot be the thing. Third, we can use phrases that are in some sense meaningful but do not stand for anything, not only because such objects as a golden mountain do not exist but also because other objects like a squared circle cannot exist.
Mill recognized some of the problems when he introduced the distinction between denotation and connotation; Frege concurred with Mill in saying that two co-referential expressions might have different meanings; and eventually Russell was forced to admit that certain expressions previously thought to be names should instead be conceived as ‘incomplete symbols’ whose role is auxiliary to expressions which do, as a whole, denote. Russell was here, according to Ryle, on the brink of saying that the meanings or significations of many kinds of expressions are matters not of naming things but of saying things. But he was still shackled by the idea that saying is itself just another variety of naming, i.e. naming a complex, an ‘objective’, a proposition or a fact.

The transformation began, Ryle tells us, when Russell, writing *Principles of Mathematics*, came to see that it is not enough to allocate a separate Platonic universal or Essence to every meaningful word. Russell was at the time bothered by ‘syncategorematic’, ‘form’ words or logical connectives such as ‘and’, ‘not’, and ‘or’. Logical constants do not behave as names of abstract objects; the phrase ‘Socrates and Plato’, for example, cannot be just a list of Socrates, Plato and ‘and’-ness. For one thing, the conjunction of ‘and’-ness with Socrates and Plato would require again the notion of *and*. Logical constants, it would seem, are doing work of a different kind: they do not behave as ‘terms’; they do not stand for objects in the same way as ‘Fido’ stands for Fido. ‘And’ conjoins; it is not just a further notion to be conjoined’ (ibid.).

‘All men are mortal’ and ‘Some men are not mortal’ *say* different things; but they are not about different subject-matters. The former is not about *Allness*, nor is the latter about *Someness* and *Notness*. (CP1, 191)

They are nevertheless significant, so it remains to be shown how they contribute sense to whole sentences.

Russell, Ryle explains, saw that in order to see what is meant by logical words, it is necessary to examine their contribution to the entire statements in which they occur. The point holds not only for these logical words but – crucially – for any statement that is to be cashed out by these logical concepts. The same problem also arises for live verbs. The contribution to the meaning of the sentence made by the word ‘assassinated’ in ‘Brutus assassinated Caesar’ is not captured by a list which conjoins Brutus, *Assassination* and Caesar. To examine the meaning of this live verb is to examine the contribution it makes to the entire statement in which it occurs. The verb does nothing by itself; it is merely *auxiliary* to the saying of true or false things as wholes. In particular, the verb ‘assassinated’ does not get its meaning from naming an abstract object that is signified by the correlative abstract noun.

A diagnosis of what was – and continues to be – so tempting about this view can be found by considering one way the philosopher is likely to be
misled by ‘grammatical prima facies’. We are not at all likely to be misled by expressions of the form ‘$x$ means what $y$ means’. But when we use the expression quasi-descriptively, as in ‘The meaning of $x$ is the same as the meaning of $y$’ or ‘The meaning of $x$ is doubtful’, we are liable to be misled into thinking that we are referring to some ‘queer new object’ (CP2, 59). The trouble begins when we abstract from expressions using a live verb a common factor which we designate with an abstract noun. But it becomes deadly when the two disastrous Millian assumptions come into play: that every word names something and that the meaning of the word is what it names. In accepting these assumptions, we succumb to the fallacy that there must be something referred to by such expressions as ‘the meaning of the word (phrase or sentence) $x$’ which is analogous to the policeman referred to by the descriptive phrase ‘Our village policeman is fond of football’ (ibid.).

It was Wittgenstein, Ryle tells us, who, developing the arguments of Frege, showed that it is wrong (in general) to think of the meanings of the words in a sentence as independently thinkable parts. The sense of a sentence is not a molecule to which the meanings of the words in it are atoms. Rather, the meaning of the parts of a sentence are ‘abstractible differences and similarities between the unitary sense of that sentence and the unitary senses of other sentences which have something but not everything in common with that given sentence’ (CP1, 191–2). Consider, by way of analogy, a human face, which ‘is not a molecule of which its profile, its complexion and its expressions are the atoms; yet still we can discern similarities and dissimilarities between different faces in respect of these features’ (ibid.).

Concepts such as reasons for acting, perception, belief, intention, meaning, and thought are not objects; a fortiori they are not names for the ‘ideals’ at which our language-adulterated thoughts can only gesture. They are abstractions from propositions in which the correlative live verbs, adjectives and adverbs perform their roles. These concepts, like formal ones, embody the logical structures of the propositions from which they are abstracted. We discern their features, not by inspecting them ‘in isolated splendour’ as we would coins in a museum, withdrawn from their native transactions, but by comparing and contrasting the logical powers of live sentences at work; for these are the units of meaning and not the words of which they are composed.

Propositions can still be understood, correctly, as abstractions from what sentences of different languages, idioms, authors or dates say when these sentences say the same thing. So, too, can proposition-factors such as concepts (as well as particulars, qualities, relations, complexes of concepts or entire propositions) be understood in abstraction from the sentence-factors or phrases or words which ‘express’ them. But the logical priority is reversed by the ‘Cambridge transformation’: it inverts the natural assumption that
the meaning of words and phrases can be understood (learned, classified or discussed) before consideration begins of entire sayings. Instead, to consider the meaning of an expression is to consider what can be said, truly or falsely, with it, as well as what can be asked, commanded, advised or any other sort of saying. In this (normal) sense of ‘meaning’, ‘the meaning of a sub-expression like a word or phrase is a functional factor of a range of possible assertions, questions, commands and the rest. It is a tributary to sayings. It is a distinguishable common locus of a range of possible tellings, askings, advising (CP2, 372).

3 Conceptual cartography and abstractions

A local villager knows his way by wont and without reflection to the village church, to the town hall, to the shops and back home again. He knows every house, stream, road and alleyway from the personal point of view of one who lives there. But suppose he is asked to draw or consult a map of his village. This way of thinking about his village is new and strange, for it employs compass bearing and units of measurement. What was first understood in the personal terms of local snapshots now has to be considered in the completely general terms of the cartographer. Whereas the villager knows from the point of view of someone who lives in it the whereabouts of the places in the village, in the sense that he could lead a stranger from one place to another, this is a different skill from one requiring the villager to tell the stranger, in perfectly general terms, how to get to any of the places, or indeed, how to understand these places in relation to those of other villages.

Where he normally thinks of his home, his church and his railway station in personal terms, now he has to think of them in impersonal, neutral terms. For him his village is unlike every other village in being the centre of his own life; but the map is neutral as between his village and any other. It represents them all by different arrangements of the same dots, lines and colours. All their distances, compass bearings and heights above sea level are given in the same unemphatic, impartial, impersonal code. The map is not a local snapshot; or an album of local snapshots; it is a slice out of an universal diagram. (CP2, 454–5)

We are like the villager with respect to our employment of words and phrases. We understand and know how to answer (or how to find out the answer to) concrete questions about a person’s reasons for acting or intentions: we know, for example, how to ascertain whether the slamming of the door was executed in anger or merely in haste; we are able to operate with the concepts of reasons for acting and intention in order to make ourselves,
as well as our descriptions of others, more intelligible. But we are perplexed when it comes to answering questions about reasons for acting and intention. We are not sure, for example, how to answer questions about their nature, their relation to the brain, or whether animals or indeed computers can ‘have’ them.

Similarly, although Augustine knew how to specify dates, durations, time of day, epochs, seasons and moments, and though he knew how to use a calendar and understood tensed verbs, he nonetheless could not give a reply to the question why Time could never have started and could not come to a stop, or what sort of thing, process, or relation Time is.5 Ryle suggests that ‘...Augustine’s puzzlement about the concept of Time has a good deal in common with the puzzlement of [the] local villager who is asked to think about his home-village in cartographical terms’ (CP2, 454). The knowledge of the villager is one of method, not methodology.6

Knowledge by wont of the use of expressions and of the more concrete ideas is something every language user learns in the course of growing up. But just as people often know their way about a village, say, without necessarily being able to describe the distances or directions between places within it or its relation to other villages, so too do people often know how to operate with ordinary, non-technical, and even semi-technical and technical expressions as well as with ‘concreter’ ideas without being able to codify the rules, permissions, or sanctions that govern their operations. ‘This workaday knowledge is knowledge but it is knowledge without system and without checks. It is knowledge by wont and not knowledge by rules’ (CP2, 211).

To the extent that I understand what I am saying, I understand the particular differences that the expression contributes to my statements, questions, and commands. In making everyday (non-philosophical) statements, or in asking ordinary factual questions, or in giving concrete, practical advice I am, Ryle suggests, like the villager who simply walks to his destination without having to think about what he is doing or turning back in his tracks. But even in making every day, non-philosophical statements, I employ a plurality of expressions. The implication threads of some of these expressions may ‘pull against’ the implication threads of others. When the implication threads pull against one another, this generates puzzles. And an answer to the question of how to reconcile uses of expressions that seem to pull against one another belongs to philosophy.

We have now to operate upon what we ordinarily operate readily and unquestioningly with. We now need the theory of our daily practice, the geography of our daily walks. When two or twenty familiar implication threads seem to pull across and against one another, it is no longer enough to be able unperplexedly to follow along each one by itself. We need to
be able to state their directions, their limits and their interlockings; to think systematically about what normally we merely think competently with. (CP2, 457)

However, when we put on our philosophical hats and begin to operate not only upon concrete ideas, but abstract ideas or abstractions, we tend to get into trouble. Since expressions of the same grammatical patterns function to express thoughts of multifarious logical sorts, those starting to philosophize (those operating upon ideas and not merely with them) ‘tend to be blind to the fact that different ideas have different logical powers or at least they tend to treat the varieties of logical types as being few in number’ (CP2, 210).

Impressed, Ryle says, by the fact that ‘Unpunctuality is reprehensible’ looks grammatically like ‘Jones merits reproof’, a philosopher might mistakenly believe that, because the second sentence has in its subject place a (proper) name of an individual or object, the first has in its subject-place the name of a different kind of object. An abstract noun such as ‘unpunctuality’ might then be mistakenly construed as referring to an abstract object. If abstract objects are, in turn, construed as analogous to ordinary objects (e.g. apt to enter into genuine relations, themselves the subjects of attributes, as early Plato supposed in his Substantial Theory of Forms), then absurdities and regresses can be generated (of the kind Plato later recognized in Parmenides). Unpunctuality, considered as a universal, is not blameworthy nor is Virtue commendable, for universals are not the sorts of things that can be commended or blamed. Such a mistake can be avoided with the realization that the abstract noun ‘unpunctual’, in this example, is simply a way of generalizing over expressions, on the one hand, such as ‘Jones is unpunctual’, ‘Smith is unpunctual’, ‘the meeting was unpunctual’, etc., and expressions, on the other, such as ‘Insofar as they are unpunctual, Jones and Smith, and whoever was responsible for the meeting are blameworthy’. A universal, like a meeting, cannot be blamed for anything.

Ideas, for example, such as ‘spaniel, dog, ache, thunder’ in their original use are instances of concrete concepts [...] their “logical geography” is taught by one’s daily walks. Such concepts are formed by noticing similarities in the real world’ (CP2, 217). Abstract ideas, by contrast, are those to which a person cannot be introduced by being presented with the ‘corresponding realities’; for

[n]othing in the world exemplifies the economic man, the Spaniel (as this idea occurs e.g. in the Spaniel is a descendant of the Wolf), or 2 (as this occurs e.g. in 2 is a prime number). (Ibid.)

Abstract ideas are formed not out of similarities in nature but out of similarities between propositions about things in nature. At a higher level, they
are formed by noticing similarities between propositions and propositions about things in nature.

But, as we have seen, propositions are also abstractions: ‘The world’, as Ryle says, ‘does not contain propositions. It contains people believing, supposing, and arguing propositions’ (CP2, 218). Talk about propositions is talk about ‘what is expressed by any expression (of no matter what linguistic structure) having the same logical force as some given expression, as such expressions are or might be intelligently used by persons (no matter whom)’ (ibid.).

Concepts, according to the transformation, are abstractions from propositions: indeed, they are double abstractions, since propositions are themselves abstractions from live sentences.

For any given proposition there may be found a range of different propositions sharing with it and with each other some one common factor. ‘Socrates is wise’ expresses a proposition having something in common with what is expressed by ‘Plato sapiens est’. This common factor can be expressed by a skeleton sentence of the pattern ‘so and so is wise’ (where ‘so and so’ announces the gap in the skeleton sentence.) Similarly the skeleton sentence ‘if $p$ then $q$’ expresses what is common to a range of hypothetical propositions. (Ibid.)

Some of these higher-order propositions are about partial similarities between otherwise different propositions. Such higher-order propositions (or propositions about such factors of propositions) – say, ‘Wisdom is to be valued over cleverness’ – are ordinarily thought to be propositions about abstractions or abstract ideas. According to our reorientation, we should understand by this that ‘[t]hey are higher-order propositions about isolable features of ranges of lower-order propositions and describe the logical force of skeleton sentences equipollent with a given skeleton sentence’ (CP2, 219). Since a proposition about wisdom does not mention Socrates or Plato, facts about these individuals are irrelevant to the truth of the proposition. But not irrelevant to its truth is the fact that there are (possible) subjects about whom it can be said that they are wise. This, says Ryle, illustrates the sense in which abstract propositions do not describe the world (or any other); but rather apply indirectly to the world.7 It is because they do apply indirectly to the world that it is always possible to accuse a purported abstract idea of absurdity: or better, ‘to accuse an expression purporting to express an abstract idea of being an absurd expression’ (CP2, 219).

There is, of course, an unlimited variety of types and orders of abstract ideas, but all alike can generate philosophical puzzles, just because experience of the real world gives us no drill in their correct use. Mistaken
views about abstractions are not rebutted by a bruised shin or a parched throat. (Ibid.)

Since abstract propositions describe factors that are common to lower level propositions, this implies that at this lower proposition-level an idea is actually being used and not described. Thus,

[t]here must be at this lower level knowledge by wont of some powers of this idea before there can begin the higher-level research into the rules governing those powers. We must know in practice how to decide whether Socrates is wise or clever before we can debate the abstract question of the relations between wisdom and cleverness’ (Hence philosophy is sometimes said to tell us only what we knew before...).8 (CP2, 220)

This explains why, for the philosopher of Ryle’s ilk, there is no such thing as studying reasons for acting or intention without studying the many and varied correct or acceptable employments of the cognate expressions. Studying their correct employment is to study the correlative concept. Later, I shall argue that to study the concept is to study the nature of the phenomenon: or better, that there is no distinction to be made between concepts and phenomena at these high levels of abstraction.

4 Systematic ambiguity and type-errors

Not only do sentences exhibiting the same grammatical pattern function to express thoughts of multifarious logical sorts: most of our expressions have the capacity to express indefinitely many ideas.

Wittgenstein famously noted a family of structures, more or less related, that can be discerned in the use of natural language expressions, as opposed to the existence of ‘some one thing in common which makes us use the same word for all’. As a result of the kind of philosophical investigation he describes ‘we see a complicated network of similarities overlapping and criss-crossing: sometimes overall similarities, sometimes similarities of detail’ (1953, sections 65 and 66). Ryle, for his part, suggested the philosopher’s job is to trace the ‘elasticities of significance’ and ‘inflections of meaning’ in expressions, which, he tells us, affect their logical power; for example, the inferences that one is allowed to make from one occasion to another.

Late, great ‘Mollie Munchkin’, my feline companion for 17½ years, was a solid red Persian, descended from a line of Grand Champions on her father’s side. Her ‘sister’, Ivy, was a gorgeous Blue ‘Exotic’: a cross between British Blue and Persian. One can tell from looking at photographs that Mollie’s coat was the same colour as the pale oak floor of our century-old Parisian flat. Mollie’s coat was red. It was the same colour as the oak floor. But the oak was not red: it was light-to-medium brown.
The word ‘red’ undergoes a particular inflection of meaning when it is used to describe cat fur (or human hair) as distinguished from wood (or sand or clay) and many other substances and inanimate objects. This particular inflection foils a normally acceptable inference: in this case, one is not permitted to infer that the oak floor is red from the fact that the cat is red, even on the assumption that the cat and the oak are the same colour.

Note that ‘red’ is used consistently as a colour word. The inflections of meaning it undergoes do not render the word a robustly ambiguous one such as ‘crane’ or ‘pitcher’; the example reminds us that what it is for cat or human hair to be red is something other than what it is for many objects to be red. Note, too, that the ‘systematic ambiguity’ or ‘circumstance-dependence’ of the expression is not a mere consequence of the fact that the uses of ‘red’ in question here simply track kinds of objects (e.g. certain cats), in special circumstances (cat shows), for even when cats feature among the items described, and even if cat shows feature in the background of this description, we can think of particular circumstances in which it is false to say of those cats, in those circumstances, that they are red.9 Nor can it be objected that the cat is not really red; for Mollie and her forebears were, in fact, really red. They were not white Persians, for example, who had been doused with powder.

Ryle thought this sort of elasticity of signification characterizes the use not of a few but of most or all expressions. For almost every word or phrase we use contributes to what we say in a way such that had it been replaced by a word or phrase with a different inflection, it would have had different implication threads:

> It would have been a different statement, different in having different implications, in requiring different tests for truth or falsehood, in being compatible and incompatible with different affiliated statements, in being evidence for or against different corollaries, and so on. (CP2, 456)

The paraphrases and translations of these expressions will normally have ‘a precisely similar elasticity’ (CP2, 215). This means that if we were to paraphrase the expression as it is used in one context in order to make clear its implication threads in that context, we can expect the paraphrase, like the original expression, to express a different idea in a different context. The paraphrase, like the original expression, is context-sensitive. Unlike ‘pun-words’ the inflections of meaning to which most of our expressions are susceptible nonetheless have affinities: the ideas expressed by these expressions in their various uses are ‘intimately connected’ with each other; they are ‘different inflections of the same root’ (ibid.). The capacity for words, complex expressions and grammatical constructions (or patterns) to have the power to express an indefinite range of differing logical types
and therefore with different logical powers is systematic; their capacity ‘to acquire new inflections of logical forces is one of the chief factors making original thought possible’ (CP2, 216).

Since concepts and propositions do not wear their logical form on their grammatical sleeves, the possibility exists that the philosopher, in attempting to abstract and generalize about the logical form of sentences of a certain type, will be misled by the surface grammar. Misled by ‘grammatical prima facies’, the philosopher is prone to commit ‘type-trespasses’ when he operates with an idea as if it belongs to one category instead of another. Both Ryle and Wittgenstein thought failure to appreciate the capacity of expression to convey an indefinite variety of ideas is the reason for philosophical perplexity and that correspondingly, anomalies that lead to philosophical puzzles can be dissolved by pointing to the variety of conventions that underlie language. As Wittgenstein complained, we are continually gripped by various pictures that force themselves upon us as a result of focusing too narrowly on some uses of language to the exclusion of others.

Consider, as a brief example, the question: ‘How can one seem to see a dragon or hear a tune if there is not a dragon to be seen or a tune to be heard?’ Instead of inventing a psychological proxy for what is seen or heard, we should use our linguistic competence to examine carefully the way the expression, say, ‘he fancied or she imagined she saw a dragon’ works. Note that we have no trouble using or understanding this expression: we know the kind of circumstances in which it is appropriate or not; in which it would be confirmed or challenged, and none of this requires theoretical knowledge or as Ryle says, knowledge of the ‘wires and pulleys’ kind. The meaning of this expression does not require that we ask the question that a theory of mental mechanisms would answer. (Nor, incidentally and a fortiori, does it require a theory of mental mechanisms augmented by a theory about the mechanisms’ alleged physical realizers.) The problem, as far as it is one, says Ryle, is to construe descriptions of people as imagining that they see or hear or do things ‘without falling back on the idioms in which we talk of seeing horse-races, hearing concerts, or committing murders’ (CM, 228). To say someone has committed a stage-murder or a mock-murder ‘is to say, not that a certain mild or faint murder has been committed, but that no sort of murder has been committed’, similarly, to say that someone imagines seeing a dragon is not to say that she sees a dragon-image or that she has a mental representation of a dragon (which counts as non-veridical because it is not the causal effect of a real dragon) but rather to say that she ‘does not see a dragon or anything dragon-like at all’ (ibid.). When we speculate that when one fancies she hears a tune she really hears a mental tune, we are failing to recognize that ‘seemings’ concepts are at least partly designed to act as factual disclaimers and not to suggest the existence of things. Not only do they fail to suggest the existence of things, we might add, nor do they
suggest the existence of ‘mental representations’ as more palatable, because ‘natural’, proxies for the ghostly ones.

5 Conceptual analysis and linguistic philosophy

On the traditional view, if meanings are Objects that exist in ‘isolated splendour’, then it is our intuition or apprehension of these concepts that is supposed to explain our ability to wield them – first in thought and then in language. Though philosophers today may demur at the suggestion that we are contemplating Objects when we grasp the meaning of an expression, the epistemological accoutrements of the view are still in evidence. For consider the following. Although, as Mark Platts explains, most concedethat language is a practical skill that does not require of language users that they work out explicitly in advance how to say or interpret what is said:

there is a recurrent temptation to think of there being something about my inner, mental life, some further, non-behavioural component of my understanding, which explains these successful performances.... The meaning of an expression, we want to say, is what grounds a competent speaker's understanding... [and] one, intuitively persuasive, remarkably persistent, and highly abstract thought about the notion of meaning is [that] the meaning of an expression is given by a rule which determines that expression's correct usage.... A sufficient condition of understanding an expression is explicit propositional knowledge of that rule. ...[F]or any meaningful expression there is a rule governing its usage knowledge of which would suffice for understanding, for mastery, of that expression. (Platts, 217–18)

According to the picture suggested here, a language user apprehends word meanings or Concepts much as Frege held that a thinker grasps or apprehends Thoughts and Russell believed we have direct acquaintance with Universals. But on this modern twist, the ‘cognitivist’ finesses what it is to grasp a concept into having knowledge of a rulebook: being in possession of sufficient conditions (knowledge of a theory of meaning) for its application.

There are a number of problems with this idea – in particular, the explanatory regresses it generates – in spite of its continued popularity today.¹¹ The fact that we tend to use abstract nouns to talk about what is conveyed by various words should not mislead us. In spite of how Plato, Hegel, Russell and Moore described their task, they did not execute it by investigating supra-mundane entities. When we investigate, we would not get anywhere, any more than Aristotle did, by ‘gazing hard’ at an entity or essence designated by this abstract noun (CP1, 192): we consider, as a first step, what we are saying when we talk about someone (say) who
enjoys walking her cat along the river or finds smoked salmon with capers and purple onions on rye delicious. Nor should meanings be construed as rule-books, or as conditions necessary and sufficient for the relevant expressions’ application and knowledge of which explains the ability to wield concepts in thought and language. Although Ryle talks of ‘the higher-level research into the rules governing [the powers of concepts]’ (CP2, 220), these rules are like the cartographer’s symbols: they belong to a discourse of a higher order; they are rules of the referee or theorist. The transformation would thus suggest that the correct view is the reverse: the practices in which expressions are employed have the logical priority, and common features that have been abstracted from these expressions – ideas or concepts – are re-integrated into these practices at ascending levels of discourse. Furthermore, Ryle’s emphasis on the ‘elasticity of significance’ or ‘inflections of meaning’ that attend most of our expressions reveals that the logical behaviour of an expression used in one context cannot be taken for granted when the same expression is used in another, just as Wittgenstein’s discussion of family resemblances among concepts amounts to a renunciation of the idea that there is a compendium of rules, which, once understood or grasped, determines the use of the expression in all of the circumstances in which it is correct to use it. There is in general no set of rules, set out in advance, for the application of our concepts. A fortiori, there is no set of rules, propositional knowledge of which could causally explain our correct employment of expressions from which these concepts are abstracted.

If contemporary philosophy of language conceives its task as displaying the connection between language and the world, it would seem that for Ryle, as for the later Wittgenstein, this connection is not forged by representation: sentences do not say things in virtue of the composition of the meaning of the words of which they are composed, nor do their components acquire their meaning by naming, picturing or representing bits of the world. Ryle would acknowledge that if we conceive of sentences in abstraction from the statements, commands, promises and pleadings they embody then it is the latter and not the former that have semantic value. When he talks of sentences, he does not consider them as bearers of meaning, or replete with logical powers, in abstraction from their jobs on particular occasions, for such is to commit the category mistake, he urges, of confusing Speech with Language. For sentences, unlike words, are dicta or parts of speech – what is said, alleged, or promised, for example – and not dictions or language pieces that can be used, like words or ‘blocks-of-words-that-have-congealed-into-a-phrase’ to say, allege, or promise. It thus seems that Ryle’s way of looking at sentences enables the connection between language and the world, via linguistic activity or ‘speech’, whose impact is felt directly in the linguistic practices in which it occurs. Sentences, as bits of dicta or speech, are already alive and not simply put to use, as words are, in various circumstances.
Like most early analytic philosophers (with the exception of Quine), Ryle agreed that philosophy is *a priori* conceptual investigation. The philosopher is not to shy away from describing her task as conceptual analysis, as long as we are clear that this does not involve the reification of concepts. The philosopher engaging in such analysis is not using her ‘intuitions’ – conceptual or modal – as her tool. That picture belongs to the Platonic remnants of the theory of meaning and should be rejected. She is using her linguistic competence to reflect upon the integration of the relevant expressions into their ‘domains of discourse’ and tracing and untangling their logical threads – their meaning – as they are so integrated. For example, in his brilliant discussion of different uses of ‘about’, Ryle outlines the conceptual contours of *aboutness* by exploring the higher-order rules that govern propositions from which *aboutness* is an isolable feature. In taking a painstaking look at live sentences in their various employments in order to map the concept or logical threads for his particular purposes, Ryle gets his reader to agree not only on what we do count as different, and not-to-be conflated, correct or acceptable uses of the cognate expressions, but on what we should count as such. Thus the philosopher’s task is a normative one.

None of this, as some critics have maintained, is to deny that a philosopher such as Ryle is interested in the nature of (say) reasons for acting. The fact that we are to view the contribution sentences make (and from which propositions and concepts are abstracted) as parts of (live) speech ensures, or so it seems to me, that a conceptual investigation of *reasons for acting* is an investigation of the nature of reasons for acting. A conceptual investigation sheds light on the nature of the mind, beauty, time or justice, just as it does on the nature of ‘aboutness’, through an examination of the linguistic practices or activities in which the relevant expressions are integrated. To suggest that the concept is one thing, reality another, is again to fall back on a version of the picture-theory that the Cambridge transformation has repudiated.

What is the purpose of conceptual cartography? There is no doubt that for Ryle as well as for Wittgenstein this includes, primarily, showing what leads to philosophical puzzlement and bad philosophical solutions (which may indeed infect other areas of enquiry). One perennial way of being misled by grammatical constructions is to assume that language functions in the main to report or describe states of affairs and that it does so by attributing a quality, property, or relation to an object, state, or event. A more thorough philosophical investigation will reveal that the sentences under investigation do not always function to describe or report a state of affairs, can only sometimes be assessed for truth or falsity, and can only with great risk of misunderstanding be said to attribute a quality, property or relation to an object that is signified by the grammatical subject. Another way to be misled is to suppose that the same expression carries with it the same implication
threads across its various employments. The way to resist the philosophical problems that result from being thus gripped is to remind ourselves of the wider set of circumstances in which the relevant concepts are correctly and appropriately employed.

What view of meaning and of abstractions emerges? Concepts exist, as it were, only as abstractible features which can be discerned from the functioning or working of the relevant expressions. If these function to express an indefinite variety of ideas, then that which is discerned – the abstract concept/phenomenon – will have a certain amorphousness, as I see it, like a soap bubble whose contours change depending on the atmosphere, rather than a pure crystalline structure that preserves its shape circumstances. To draw attention to this amorphism is, in effect, to question the assumption that single meanings governing their logical behaviour can be attached to single expressions, whatever the contexts, purposes or interests. A fortiori, it is doubtful that the logic of these statements can adequately be represented by imagining they have a single function. As Ryle puts it, both the non-formal expressions of everyday discourse as well as those of technical discourse have their own unscheduled logical powers. The only way to uncover these is to ‘pull’ at the implication threads of the expressions as their uses are explored from one set of circumstances to another. That, for Ryle, is the nature of philosophical investigation.16

Notes

1. What were the massive developments in logical theory that he had in mind? The distinction, not between truths and falsehoods, but between sense and nonsense which began with Russell’s revisions to Mill’s logic but in early Wittgenstein’s hands became ‘the fulcrum for transforming the whole theory of meaning’ (CP1, 193).

2. According to Ryle, the Parmenides anticipates most of the logical embarrassments that befell Meinong, and it anticipates Hume’s and Kant’s accounts of assertions of existence, Kant’s account of forms of judgement and categories, Russell’s doctrine of propositional functions and theory of types, and ‘more than any other, nearly the whole of Wittgenstein’s Tractatus Logico-Philosophicus’ (Collected Papers, (henceforth, referred to in the text as CP) Vol. 1, 46).

3. Ryle rejected Wittgenstein’s conclusion in the Tractatus – and sees in Philosophical Investigations Wittgenstein’s own rejection of his earlier view – that philosophy can only show but not say what makes expressions significant or non-sensical: that philosophical talk is nonsensical:

What had brought him to this frustrating conclusion? When he wrote the Tractatus, he was, I think, over-influenced by his own analogies between saying things and making maps, diagrams and scale-models... After the Tractatus he realised that though saying things does resemble depicting things in the respect for which he originally drew the analogy, it does not resemble them in all respects. (CP1, 262–3)
4. Ironically, however, though well-known for his work in the philosophy of mind, his *chef d’œuvre*, *The Concept of Mind* (henceforth CM), is read and interpreted almost exclusively without reference to or knowledge of Ryle’s views on the nature of propositions, concepts, and the role of philosophical investigation. Thus, he is frequently misinterpreted, for example, as espousing a version of philosophical behaviourism, and anti-realism about dispositions, while his anti-rationalism is construed by many as leaving empiricism his only option.

5. ‘St Augustine said, “When you do not ask me what Time is, I know perfectly well; but when you do ask me, I cannot think what to say”’ (*CP* 2, 448).

6. For a discussion of this distinction, see (*CP* 2, 335).

7. Ryle reminds us that there are various types of such indirect application which are not best construed as descriptions. Just as arithmetic does not describe inventories, but inventories satisfy arithmetical propositions, do geometry not describe Asia, though geography is an application of geometry. For more discussion on various senses of ‘application’, see *CP*, chapter 16 (‘Why Are the Calculuses of Logic and Arithmetic Applicable to Reality?’).

8. Compare Wittgenstein: ‘The problems [of philosophy] are solved, not by giving new information, but by arranging what we have always known’ (1953, section 109), and ‘Philosophy may in no way interfere with the actual use of language; it can in the end only describe it... It leaves everything as it is.’ (ibid. section 124)

9. See ‘Conceptual Amorphousness, Reasons, and Causes’, from which I have borrowed this example, for a more extended discussion. Note that the gist of this discussion is, if not to raise questions about the notion of ‘literal’ meaning, at least to suggest that elasticities of significance can be found well within its domain.

10. For a similar suggestion, see Sellars 1956.

11. For the regresses generated, and for more discussion, see Tanney’s ‘Ryle’s Regress and the Philosophy of Cognitive Science’.

12. Which mistake lends support to the suggestion that we can talk about the meaning, and thus the logical powers of a sentence, independently of its particular use.

13. Instead of thinking of the relation between words and sentences as akin to the one between faggots and sticks, think of it as the relation between the purchasing power of coins and the purchases that are made with them. It makes sense to ask for the use of certain words, but it makes no sense to ask a parallel question about the use of sentences. Just as an execution is not erected to hang someone on; it is the hanging of someone, so my last sentence is not something with which I once learned how to say something; it is my saying of it. See *CP*, chapter 23 ‘Ordinary Language’ and chapter 31 ‘Use, Usage, and Meaning’.

14. Consider the (broadly Davidsonian) idea that sentences of a language (e.g. English) have statable conditions for truth, and meanings can be given by stating these. Charles Travis (1997) suggests that for J. L. Austin, this is wrong: ‘[...] questions of truth arise at a different level entirely from that of expressions [i.e. sentences] of a language. [...] conditions for truth depend, perversely, on the circumstances in which, or the way in which, words were produced.’ Travis’s formal statement of the pragmatist position is: ‘It is intrinsically part of what expressions of (say) English mean that any English...sentence may, on one speaking of it or another, have any of indefinitely many different truth conditions, and that any English...expression, meaning what it does, make any of many different contributions to truth conditions of wholes in which it figures as a part.’ Ryle, it seems to me, ends up (as it were) in the same pragmatist space.
Note, however, that he focuses on all the logical powers of expressions (and not just on their truth-aptness) and forges the connection between language and the world in Speech without stopping midway to consider the meanings of sentences (as language-pieces) in abstraction from their particular employments in Speech.

15. CP2, chapter 5 ‘About’.

16. This chapter uses material from my other publications on Ryle, and in particular, the first chapter of a book in progress. Special thanks to David Corfield for pressing me on questions I address in the last section.

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Part II

Broader Themes
5
Frege, Lotze, and Boole
Jeremy Heis

1 Dummett and Sluga

In the ‘analytic tradition’, Hans Sluga wrote thirty years ago in his book *Gottlob Frege*, there has been a ‘lack of interest in historical questions – even in the question of its own roots. Anti-historicism has been the baggage of the tradition since Frege’ (Sluga, 1980, p. 2). The state of the discussion of Frege among analytic philosophers, Sluga claimed, illustrated well this indifference. Despite the numbers of pages devoted to Frege, there was still, Sluga claimed, little understanding of the sources of Frege’s ideas, his intellectual debts, and the historical circumstances of his thought. Sluga singled out Michael Dummett’s *Frege: Philosophy of Language* ‘as a paradigm for the failure of analytic philosophers to come to grips with the actual, historical Frege’. In that book, Dummett famously wrote that the logical system that Frege put forward in his 1879 *Begriffsschrift* ‘is astonishing because it has no predecessors: it appears to have been born from Frege’s brain unfertilized by external influences’ (Dummett, 1981a, p. xxxv) – and Dummett devoted almost none of its 700 pages to the relationship between Frege and his contemporaries.

What followed was an academic controversy remarkable for covering so many topics and for filling so many pages. In addition to his book from 1980, Sluga criticized Dummett’s reading of Frege at length in five papers published between 1975 and 1987.1 Dummett responded with four papers published between 1976 and 1982, and published in 1981 a book, *The Interpretation of Frege’s Philosophy*, whose 600 pages are devoted largely to defending his interpretation against Sluga.2 The discussion ranged over almost every significant topic in Frege’s philosophy of logic and language. What is of particular interest more generally, however, is the explicitly historiographical and methodological character of the debate. The explicit topic of debate was not just, for example, ‘What is the correct interpretation of Frege’s context principle?’ The topic was also ‘What method should we use in giving an interpretation of that principle?’ Sluga and Dummett disagreed
on how much Frege took from Kant and from nineteenth-century logicians. But even more fundamentally, they disagreed on the value for a historian of philosophy in placing a philosopher in his historical context. For this reason, perhaps the most central issue in the debate was Sluga’s assertion that a philosopher writing on Frege should acknowledge the pervasive influence of the philosopher Hermann Lotze on Frege. Sluga argued that the proper interpretation of Frege’s ‘realism’, of his conception of objectivity, and of his context principle required seeing these doctrines as derived from Lotze’s philosophy. For Dummett, on the other hand, Lotze’s writings do not afford ‘any useful comparison with Frege’s views’: though ‘a great many of Frege’s leading ideas appear, in Sluga’s book, as having been derived from Lotze’, in fact this interpretive claim only ‘results in a far-reaching misinterpretation of Frege’s thought’ (Dummett, 1981b, pp. 396, 525).³

Sluga saw Dummett’s failure to acknowledge the indebtedness of Frege to Lotze as symptomatic of a more general failure of Dummett’s, and indeed, of analytic philosophy itself. Analytic philosophers since Frege have been uninterested in comprehending ‘concrete historical processes’ or in the ‘examination of actual historical discourse’ (Sluga, 1980, pp. 2, 186). They have preferred instead to engage in an ‘unhistorical kind of meaning analysis’ that abstracts from the ‘subjective, historical, and personal’ features in a philosopher so as to provide a ‘rational reconstruction’ of the philosopher’s thoughts (pp. 181, 3). Analytic philosophers, Sluga seems to be arguing, assume that they can solve the philosophical questions that currently vex them without first reflecting on the histories of these questions – that is, why we ever came to think that these questions were the important ones. Similarly, they presume to know what questions a past philosopher was trying to answer and what her words mean, without going through the hard work of placing that philosopher in her historical setting. Sluga thinks these assumptions are mistaken and that the blame lies with the philosophy of language that the analytic tradition derived from Frege.

There are deep reasons why the writings of the analytic tradition are unhelpful at this point. From its very beginning, the tradition has been oriented towards an abstract, formal account of language and meaning, and not towards the comprehending of concrete historical processes. Frege himself considered his task that of the analysis of timeless, objective thoughts. (p. 3)⁴

For Frege, the sense of an expression is a timeless, impersonal entity that exists independently of whether any actual individual thinker ever grasps it. Thus, Sluga alleges, a Fregean historian of philosophy will approach the thoughts of a past philosopher as themselves abstract, timeless, and impersonal: she will ignore the contingent facts about the philosopher as
a person – whom she knew, what she read, what her contemporaries were saying – and the historical facts about the time in which she lived and wrote.

One might be skeptical about Sluga’s diagnosis of the implicit causes of analytic philosophy’s alleged anti-historicism. In any case, though, Dummett himself was quite explicit in his reasons for downplaying the significance of the investigation of various influences on Frege’s thought – for preferring the method of ‘rational reconstruction’ and ‘logical analysis’ over ‘historical analysis’. The first step in Dummett’s defense identifies a criterion for the success of a philosophical interpretation of a philosopher like Frege:

A good, though partial test for the perceptiveness of any exposition of the thought of a great philosopher may be had by asking how interesting the result would be simply as a piece of philosophy, for someone who had neither read that philosopher nor felt any special curiosity about the correct way of interpreting him. (1981b, p. 528)

There is surely some reason, after all, why philosophers write more on Descartes than on Gassendi; more on Kant than Jacobi; Bolzano than Drobisch. This does not mean that there are not good reasons for reading these other philosophers. But a philosopher reading historical texts differs from an historian in trying to pick out those philosophers whose work – whether or not it is historically significant in other ways – is good as a piece of philosophy. The second step singles out a particular reason why contemporary philosophers should see Frege’s philosophy as a good piece of philosophy: ‘Frege is so interesting a writer because we have got so comparatively short a way beyond the point he reached. [...] Frege’s problems are therefore still our problems; his thoughts still answer to our concerns’ (1991b, p. 158).

These two ideas together provide an apparently compelling argument for philosophers to be indifferent to Lotze and to Frege’s other contemporaries. If Frege’s questions were not our own, then to see the philosophical merits in his writings would require us to imagine ourselves in the position of the typical German logician in 1879. However, we already know that his questions are interesting and that his answers merit philosophical reflection and close reading. This does not mean that Frege’s philosophy emerged entirely free from outside influences. It is just that tracking influences does not help us to understand Frege’s philosophy as a piece of philosophy. A historian of ideas, who is concerned more with the question of historical causation, and is thus free to abstract away from the question of the philosophical value of a work, would of course be interested in questions of influence. But a philosopher, Dummett is arguing, should not be like such historians.

In this chapter, I will be presenting a contextual reading of Frege that, like Sluga’s, explores Frege’s relationship with Lotze. If my reading succeeds,
I will have demonstrated contra Dummett that seeing the philosophical merits of Frege’s writings does indeed require locating them in the context of late-nineteenth-century German philosophy and logic – that contextual readings of Frege are not irrelevant or opposed to the interests of philosophical readers.

Where, then, does Dummett’s argument go wrong? One possible reply would be to argue that Lotze’s philosophy of logic is a real philosophical contender to Frege’s, much as, say Thomas Ryckman (2005) has argued that Cassirer’s and Weyl’s philosophies of physics are genuine and serious philosophical alternatives to Reichenbach’s. In Section 3, I will argue that there is more of philosophical interest in Lotze’s philosophy of logic than might first meet the eye; however, I do not think that Lotze’s logical writings – standing on the far side of the logical revolution brought about by Frege and Russell – are a genuine alternative to Frege’s. Another possible reply would emphasize the philosophical value of unfamiliar ideas. Perhaps reading Frege in the context of his contemporaries will uncover for us a set of questions and a vocabulary for answering these questions that, for us at least, are new. And perhaps if we try hard enough to think in those foreign terms, we will see the philosophical merit in these ways of thinking – ways of thinking that (who knows?) we might use some day. At the very least, studying thoughts not like our own can provide for us a healthy skepticism about our own questions and assumptions.

Though this second reply might be compelling in other contexts, there is a significant obstacle for a historian writing on Frege to accept it. Dummett writes:

Frege’s formal logic has no predecessors: in the writings of nineteenth-century logicians before Begriffsschrift, not one hint can be found of the ideas underlying Frege’s discovery of quantification theory. But Frege’s formal logic is the principal factor determining the subsequent development of his philosophy, and certainly of his philosophy of language; it forms the backbone of that philosophy, which collapses if it is extracted. (1981b, p. xvii)

Sluga may be correct, for instance, that both Frege and Lotze speak of the ‘priority of judgments over concepts’, but, Dummett contends, Frege meant this thesis to provide a key to understanding Frege’s new logic and the kinds of logical analyses that can be carried out with its aid. Lotze, who was ignorant of Frege’s logic, could not have understood what Frege meant by this phrase, and he could not even have understood the questions Frege was trying to answer. Frege’s new logic introduces a break in the philosophy of logic and language, Dummett contends, and those of us whose understanding of logic and language has been shaped by Frege’s discovery can gain little from reading Frege’s predecessors and contemporaries. A contemporary ethicist
may profit from reading Aristotle’s *Nicomachean Ethics*, but a philosopher of physics will be wasting her time studying Aristotle’s *De Caelo*. Why, then, should a philosopher of logic read Lotze’s *Logic*?

Dummett is surely correct that Frege’s Begriffsschrift was the principle factor driving his philosophy – both to understand this new tool and to explore its implications. However, Dummett concludes too quickly that this fact about Frege reduces the value of contextual readings. Here is why. One central feature of the analytic tradition’s self-narrative is that the invention of the new logic did bring about a sea change in philosophy and made possible progress (maybe even definitive progress!) on philosophical issues that would otherwise be intractable. But this early optimism has not been substantiated. We no longer believe that logical analysis using the tools of Frege and Russell’s logic will allow for the eventual solution of every genuine philosophical problem. So, what then is the philosophical pay off provided by Frege’s new logic? Why are we better off now than philosophers were 130 years ago?

It is precisely here – I will be arguing – where a contextualized reading of Frege can be of service. When we see what the philosophical world looked like before the introduction of the new logic, we can begin to evaluate the real value of the new logic for Frege and his contemporaries. We can identify the nineteenth-century philosophical problems that Frege thought himself to have solved using the new logic. It will, of course, take substantial historical work to think ourselves into the position of a philosophically sensitive logician who is unacquainted with Frege’s work. But when we have gotten past the excessively bullish predictions of analytic philosophers past and have learned to see Frege’s logic with fresh eyes, we can learn to appreciate in a more balanced way what makes the new logic such a powerful tool – and we can appreciate what makes Frege’s philosophy of logic (entwined as it is with his new technical tool) good as a piece of philosophy.

Though I side with Sluga in maintaining the philosophical importance of locating Frege contextually, I nevertheless do not think that there is good reason to prefer Sluga’s ‘historical analysis’ to Dummett’s ‘logical analysis’ and ‘rational reconstruction’. The contextual reading I will present spells out the particular philosophical advances that Frege was able to make with the help of his new technical tool. Since this is my goal, cataloguing the affinities and influences of other philosophers on Frege will not be a primary goal. On the contrary, I will engage in detailed readings of particular arguments put forward by Frege and Lotze, with an eye to the ways that Frege, having taken up the questions and themes of his contemporaries, was able to make substantial progress beyond them. Indeed, in the closing section of this chapter, I will argue that Sluga’s own contextual reading actually obscures the philosophical significance of Frege’s work. (And, ironically, Sluga’s contextual reading of Frege falls short precisely because he avoids
the kind of detailed logical analysis of Frege's and Lotze's arguments that he – wrongly – opposes to historical readings.)

My reading will begin in the next section by looking in particular at Frege's writings from the early 1880s that compare his Begriffsschrift with the systems put forward by George Boole and his followers. The reason for looking at these writings is threefold. First, Frege is most explicit about the philosophical significance of his new logical system when he is arguing its merits over older logical systems, like Boole's. Second, recent historical work has made it clear that these Fregean works were intended to engage an ongoing discussion within the German philosophical community about the value of Boolean logic – and, by extension, about the value of systems of logical notation. These writings are therefore ideal subjects for a contextual reading of Frege. Third, we will see that Lotze himself had criticized Boolean logic in 1880. Looking at Lotze's criticisms of Boole will be an effective way, then, to see both what Frege has in common with Lotze, and – most importantly – what kind of surplus philosophical work the Begriffsschrift allows Frege to do.

2 Frege's new theory of concept formation and his criticism of Boolean logic

Almost twenty years after the publication of Begriffsschrift, Frege described the philosophical purpose for which the Begriffsschrift was invented:

I became aware of the need for a conceptual notation [Begriffsschrift] when I was looking for the fundamental principles or axioms upon which the whole of mathematics rests. Only after this question is answered can it be hoped to trace successfully the springs of knowledge upon which this science thrives. (Frege, 1897, p. 235; cf. 1884, section 3)

In order to isolate these fundamental principles of mathematics, Frege needed a way of determining whether a candidate derivation of some theorem is free of gaps or in fact requires some unrecognized further concept or principle. Since even Euclid was led astray by the imprecision of ordinary language to assume certain principles without acknowledgment, it was clear to Frege that a logically improved language was needed for carrying out inferences (1882, p. 85; 1979, p. 253). Furthermore, any incomplete analysis of the fundamental concepts out of which mathematical judgments are composed will lead to an incomplete analysis of the conceptual content of a mathematical judgment; and without a complete analysis of the content of a judgment, a candidate proof of that judgment might contain a hidden gap or be insufficiently general. So, for Frege, (1) the precise analysis of mathematical concepts, (2) the construction of gap-free proofs within a new symbolic language, and (3) the determination of the basic laws of mathematics are all essentially connected elements in the same project.
Frege called the symbolic language that he invented for this project a ‘Begriffsschrift’ because a language that allowed for the successful execution of Frege’s project would be a characteristic language for mathematics or a ‘lingua characterica’, in Leibniz’s sense.\textsuperscript{15} The symbolic language that Leibniz described would perform three interrelated functions. First, the language would ‘compound a concept out of its constituents rather than a word out of its sounds’ (1880–1, p. 9). Since the primitive symbols of a lingua characterica would express simple concepts, and symbols for compound concepts would be composed from the symbols for their component concepts, the content of a concept could be directly read off from its symbolic expression. Second, once the complete analysis of concepts has been completed and expressed perspicuously in the symbolic language, all inferring could become calculating. The language would be, in Leibniz’s terms, a ‘calculus ratiocinator’ – a calculus for carrying out inferences. Third, Leibniz hoped that then the truth of every judgment could be determined by calculating with the symbolic expression that expresses that judgment.

In a series of papers written between 1880 and 1883, Frege argued that only a symbolic language like Begriffsschrift – and not the algebraic logical languages proposed by Boole and his followers – provides the necessary tools for a characteristic language.\textsuperscript{16} One reason is that the Boolean logicians lack an adequate representation of generality.

It is true that the syllogism can be cast in the form of a computation... Even if its form made it better suited to reproduce a content than it is, the lack of representation of generality corresponding to mine would make a true concept formation – one that didn’t use already existing boundary lines – impossible. (1880–1, p. 35)

For instance, without Frege’s quantificational notion of generality, a Boolean logician could not fully analyse the concept <F is a hereditary property in the f-series>, which in modern notation is ‘(\forall x)(Fx \rightarrow (\forall y)(f(x, y) \rightarrow Fy)’.\textsuperscript{17} This inadequate analysis would become clear when we represent proofs of theorems containing that concept. For instance, Frege needs only logical primitives and logical laws to prove ‘if z follows x in a sequence, and if y follows z, then y follows x in that sequence’ – even though logicians before Frege had thought that this fact rested on intuition or a non-logical rule of inference.\textsuperscript{18}

This difference between the expressive power of the Begriffsschrift and that of Boolean and traditional logic is a fact familiar to any undergraduate student of logic today. What is less familiar now – and is absolutely essential for understanding Frege’s relationship to his contemporaries\textsuperscript{19} – is that Frege thinks that this difference in expressive power is based in a further difference in how concepts are formed in the old and new logic. He illustrates the
new method of forming concepts made possible by the Begriffsschrift with
the following example:

The $x$ [in $2^x = 16$] indicates here the place to be occupied by the sign for
the individual falling under the concept. We may also regard the 16 in
$x^4 = 16$ as replaceable in its turn, which we may represent, say, by $x^4 = y$.
In this way we arrive at the concept of a relation, namely the relation of
a number to its 4th power. And so instead of putting a judgment together
out of an individual as subject and an already previously formed concept
as predicate, we do the opposite and arrive at a concept by splitting up
the content of a possible judgment. (1880–1, p. 17; cf. 1879, p. 16)²⁰

The concepts <4th power>, <4th root of 16>, <base 2 logarithm of 16> are all
formed in this example, not by compounding simple concepts by addition
(or inclusion and exclusion), but by starting with a relational expression, $2^4
= 16$, and replacing one or more singular terms by variables. None of these
concepts need be explicit in first framing the judgment, which can be put
together from the relational expression ‘$x^4 = z$’ and the singular terms ‘2’, ‘4’,
and ‘16’. The variables in these newly formed concepts can then be bound
by the sign for generality to form quantified relational expressions.

There are a number of fundamental ways in which Frege’s Begriffsschrift
departs from traditional and Boolean logics: it allows for the expression of
relations, it represents generality with quantifiers, and it introduces a divi-
sion between constants and variables. Of course, these three innovations
are indissolubly linked. The distinction between variables and constants is
significant only because variables can be bound by quantifiers. The quanti-
ficational notion of generality is significant only because relational expres-
sions can allow for polyadic quantified sentences like ‘Every number has a
successor’. It is extremely important for Frege, though, that each of these
three features is similarly interlinked with the new way of forming concepts
made possible by his notation. A further example will make this clear.
Suppose you are trying to determine whether 121 is uniquely decomposable
into primes, and suppose you already know that 121 is a square and that 11
is uniquely decomposable into primes. You then arrive at the judgment:

\[ S(11, 121) \rightarrow D(121) \]

with ‘$S(x, y)$’ expressing <$x$ is a square of $y$> and ‘$D(x)$’ expressing <$x$ is
uniquely decomposable into primes>. By replacing ‘121’ with a variable, we
form the concept <$x$ is decomposable if it is a square of 11>. By quantifying
that variable, we get a new quantified expression

\[ (\forall y)( S(11, y) \rightarrow D(y)). \]
After some further steps, we reach the judgment

$$(\forall x)( D(x) \rightarrow (\forall y)( S(x, y) \rightarrow D(y))),$$

which says that unique decomposability into primes is hereditary in the series of squares. Finally, replacing the predicates ‘D(x)’ and ‘S(x, y)’ with predicate variables, we can form the relational concept <F is a hereditary property in the f-series>. In this way, the uses of variables, quantifiers, and relations are all bound up with Frege’s new way of forming concepts in Begriffsschrift.

Given the central importance of Frege’s new way of forming concepts, it is then not surprising that Frege criticizes Boolean logic for adhering to an older, inadequate theory of concept formation. For Boole, Frege argues, all concepts are formed by taking the unions, intersections, or complements of the extensions of given concepts. (In Boole’s notation: from the classes \(x\) and \(y\), the Boolean logician forms the new classes \(xy\), \(x+y\), and \((1-x)\).)\(^{21}\) When compared to the extraordinary expansion of inferential and expressive power made possible by the new way of forming concepts in Begriffsschrift, the Boolean theory of concept formation appears to be an insignificant departure from the traditional view, enshrined in logic texts since Aristotle, that concepts are formed by noticing similarities or differences among particulars and abstracting the concept, as the common element, from these similarities or differences. Frege writes:

My concept-script commands a somewhat wider domain than Boole’s formula-language. This is a result of my having departed further from Aristotelian logic. For in Aristotle, as in Boole, the logically primitive activity is the formation of concepts by abstraction, and judgment and inferences enter in through an immediate or indirect comparison of concepts via their extensions. [...] As opposed to this, I start out with judgments and their contents, and not from concepts...I only allow the formation of concepts to proceed from judgments. (1880–1, pp. 14–15)\(^{22}\)

Because of this limitation, Boolean logic is simply ‘not suited for the rendering of a content’ (1882–3, p. 93). There could never be a *lingua characterica* constructed from a Boolean symbolic logic.

Frege’s primary criticism of the Boolean logicians that he knew, then, was that the theory of concept formation implicit in their work was too weak to allow for the rich expressive capacities required by a *lingua characterica* for arithmetic like Frege’s Begriffsschrift. Frege added to this a second, corollary criticism – a criticism that, as we will see below, picks up and develops a theme familiar to German logicians in the 1870s. Frege’s theory of concept formation allows for the same judgment to be decomposed in multiple ways, by replacing one or more constants with variables. (In Frege’s example, the three concepts <4th power>, <4th root of 16>, <base 2 logarithm of 16> are
all formed from one relational expression, \(2^4 = 16\)). Boolean logic, lacking relational expressions and the syntactic distinction between variables and constants, does not allow for the same judgment to be decomposed into constituent concepts in more than one way. Now, since expressions for judgments can be decomposed in multiple non-trivial ways, Frege argued that inferences in Begriffsschrift can thus exploit this multiple decompositionality: they can exhibit to us structures or patterns in our premises that were not already apparent to us in first forming these sentences (1880–1, pp. 33–5; 1884, section 88). The conclusions of these inferences can then be genuinely new and surprising extensions of our knowledge. Because forming concepts in the new Fregean way allows us to see new patterns and so to perform epistemically ampliative inferences, Frege describes concepts formed in Begriffsschrift as ‘fruitful’. Boolean logic, wedded to the old method of forming concepts, cannot express the content of these fruitful concepts and so cannot explain how deductive inference can expand our knowledge. Boolean logic leaves us wondering what the point of deductive inference is, thus reinforcing ‘the impression one easily gets in logic that for all our to-ing and fro-ing we never really leave the same spot’ (1880–1, p. 34).

3 Lotze’s theory of concepts and his criticism of Boolean logic

Frege argues that his Begriffsschrift, unlike Boolean logic, can serve as a lingua characterica for mathematics and thus allows Frege to isolate the ‘springs’ of mathematical knowledge. He argues that the Begriffsschrift, unlike Boolean logic, avoids the expressive limitations of the traditional theory of concept formation and explains how deduction can be epistemically ampliative. In this section, I provide a historical context for these arguments by looking to the writings of the German philosopher and logician Hermann Lotze.

There are good historical reasons to look to Lotze. Lotze’s Logic was perhaps the most widely read logic text in Germany during Frege’s early career, and it was a work that we know Frege read. The only philosophical course that Frege took as a graduate student was from Lotze, and Frege’s colleague in Jena, Bruno Bauch, claimed that Frege himself had told him that Lotze’s work was of ‘decisive importance’ for his own. And, as we will see, Lotze’s Logic (1st ed., 1874; 2nd ed., 1880) also presents an objection against the suitability of the traditional theory of concepts for capturing the structure and formation of specifically mathematical concepts. This objection is not only broadly similar to Frege’s, but Lotze, like Frege, turned it against Boolean logic in the second edition of his Logic.

Before looking in detail at Lotze’s theory of concepts and his objections to Boolean logic, it is important to recognize that Lotze’s conception of logic differed fundamentally from Frege’s. Frege, in his notes on Lotze’s Logic, claimed (against Lotze’s opposing view) that logic is chiefly concerned with inference (Frege, 1979, p. 175). Though this is a standard view today, it was
Frege, Lotze, and Boole
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not common among nineteenth-century German logicians. Lotze, by contrast, shared the view (dominant at that time) that philosophers of logic should stake out a middle position between a modest Kantian ‘formal’ logic and an extremely ambitious logic, like Hegel’s, that combined traditional logical topics with a full metaphysics (Lotze, 1843, pp. 1–36).

Lotze’s Logic begins with an account of how the operations of thought allow a subject to apprehend what is true and distinguish it from the mere current or stream of ideas [Vorstellungsverläufe] (section II). In this current of ideas, some ideas flow together only because of accidental features of the world or because of idiosyncratic features of a particular subject’s mind; other ideas flow together because the realities that give rise to them are in fact related in a non-accidental way. How are these two cases to be distinguished? Lotze insists that it is the task of thought to so distinguish them; and it is the task of logic to investigate what thought does when it distinguishes them.

On Lotze’s view, before thought can separate the true ideas from the false ones, it must first act on ideas so that they become capable of being true or false. Thought accomplishes this by the addition to the stream of ideas of certain ‘accessory thoughts’ or ‘accessory notions’ that ground this stream in reality (section VI). An animal may connect together the idea of a tree with the idea of its leaves and associate them through memory with the idea of a tree with no leaves. But a human will add to these connections of ideas the accessory thought of a thing and its property and will see these ideas as corresponding to a particular tree that before had leaves and now has lost them. The human subject connects these ideas of the tree and its leaves with this idea of a tree with no leaves because they are ideas of the same thing, and these ideas are separated in the human subject because they are ideas of the very same tree now with and now without its leaves.

This will no doubt remind many readers of Kant’s argument in the Critique of Pure Reason that objective validity requires the application of categories to intuitions, and that the ordering of representations in a ‘synthetic unity of apperception’ differs from a mere association of ideas. A contemporary reader might also wonder what this has to do with logic as it was traditionally understood – as the doctrine of concepts, judgments, and inferences. For Lotze, these elements of ‘transcendental philosophy’ are integral parts of a unitary investigation that also includes the subject matter of traditional ‘formal’ logic. Thinking allows a subject (among other things) to connect together representations so as to represent a thing and its attribute; to move from associating the ideas of stick and pain to representing the stick as the cause of an effect; and to frame a judgment as a conclusion that follows from a universal and particular premise. All three of these activities are essential parts of the overall project of ‘reducing coincidence to coherence’ (section XI) – even though Frege (and Kant) would recognize only the third as a topic for logic.

These ideas have significant implications for Lotze’s theory of concept formation. In the chapter titled ‘The Theory of the Concept’, Lotze emphasizes that even the stream of ideas is itself partially a product of thought,
since only thought can transform a mere series of given impressions into ideas [Vorstellungen]. Therefore, Lotze argues that the formation of concepts is in fact the third operation in a three-step process, and he gives a lengthy discussion of the first two steps – the objectification of impressions into ideas and the ‘composition, comparison, and distinction of the simple contents of ideas’ – before finally turning to a discussion of concepts themselves.33

For Lotze, the formation of concepts is an instance of the characteristic function of thought: ‘to separate the merely coincident in the manifold of ideas that are given to us, and to combine the content afresh by the accessory notion of a ground for their coherence’ (section 20). Certain streams of ideas are connected in my mind: at one moment together in my consciousness is <spherical, hard, small, blue>; at another is <spherical, hard, small, red>; at another is <spherical, hard, small, green>. I form from these streams of ideas a new universal representation – <spherical, hard, small>. By treating this new universal as a unity, I am isolating a relation within my stream of ideas as belonging together in a non-accidental manner: I say that the ground for my having the series of ideas that I've had is that there is a kind of thing to which these series of ideas correspond.

On the traditional view, a concept is a sum of marks, formed by abstraction, which differs from non-conceptual representations chiefly in virtue of its generality. Lotze protests, however, that this picture does not capture at all the essential function of thought when it forms concepts: the grounding of the connection of the ideas. A concept is not just a universal representation: it must contain a rule or a determinate law (section 121) that explains why certain marks belong together. Lotze makes his point vivid with examples of degenerate general representations, like the ‘concept’ <red, juicy, edible, body> formed by abstraction from cherries and raw meat (section 31). This universal is degenerate because it was formed, not by looking for the rule that unites the marks that together compose a concept, but by identifying common elements in a series of particulars and abstracting away from the differences. As a result, the common elements or marks are simply listed, not compounded according to a determinate rule, and the knowledge that some particular falls under the ‘concept’ tells us little about it.

Lotze therefore replaces the model of the concept as the sum of its marks with what he calls the ‘functional’ model, where the structure of the concept is expressed by some complicated interrelation of its marks:

As a rule, the marks of a concept are not coordinated as all of equal value, but they stand to each other in the most various relative positions, offer to each other different points of attachment, and so mutually determine each other;...an appropriate symbol for the structure of a concept is not the equation $S = a + b + c + d$, etc, but such an expression as $S = F (a, b, c, etc.)$ indicating merely that, in order to give the value of $S$, $a$, $b$, $c$, etc,
must be combined in a manner precisely definable in each particular case, but extremely variable when taken generally. (section 28)

In the functional model, the marks are *interdependent* and vary together according to a rule. Every particular that falls under a concept \( S = F(\ a, \ b, \ c, \ \text{etc.}) \) has its own specific way of exhibiting the marks of \( S \); still, though, *how* a particular \( S \) exhibits a mark will in general be determined by how it exhibits other marks. This is particularly clear for actual mathematical functions: at each particular point on a plane curve of second degree, 
\[
a_2x^2 + a_1x + b_2y^2 + b_1y + cxy + d = 0,
\]
its ordinate \( y \) is determined by its abissa \( x \). Every particular that falls under \( <\text{triangle}> \) (= *<figure with three sides meeting at three angles>* ) will not just have three sides and three angles, but the specific angles that it has will be determined by the magnitude of its three sides. The mark \( <\text{angle}> \) is then like a *dependent* variable in the concept \( <\text{triangle}> \): the angles are *functionally* dependent on the sides. The concept \( <\text{triangle}> \) is not \( <\text{three sides}> + <\text{three angles}> \), since the two marks ‘are not coordinated as all of equal value’. However, when concepts are formed by *interrelating* component universals like *interdependent* variables in a function, thought is in a position to capture real, nontrivial relations among the things themselves: it represents its connected ideas as *grounded* or justified by relations that hold in reality.

Frege argued against the traditional, abstractionist model of concept formation that it fails for mathematical concepts and cannot explain how deductions can extend our knowledge. Lotze, after concluding his discussion of the different kinds of syllogistic inferences (the figures of the syllogism, along with syllogisms where the major premise is disjunctive or hypothetical in form), begins his discussion of ‘mathematical inferences’ with the following argument:

The above considerations have taught us that there have to be still other logical forms of thought beyond the Aristotelian figures of the syllogism, forms that provide for the first time a fruitful application to the content of knowledge. [...] Every inference should be an acquisition of new knowledge from the premises, from which this knowledge *comes to be*, although it is not already contained in them analytically. [...] When the mind seeks a necessary law in the combination of manifold marks, it first believed it could find it in that general concept, but this concept itself came to be only through summing marks, and we can therefore not ground a conclusion through this without surreptitiously presupposing the thing we are seeking. [...] We have sought to compensate for this deficiency of the subsumptive mode of inference through the assumption of constitutive concepts; but in order to find these concepts and their logical form, we must oppose the Aristotelian figures with a series of different [inferences], which are grounded on the *content of concepts*. (1843, p. 190; cf. 1880, section 105)\textsuperscript{34}
Lotze goes on to isolate three different types of non-Aristotelian inferences that are employed in mathematics and argues that these inferences require concepts whose structures are not captured in the traditional way. These mathematical concepts exhibit most clearly the value of forming concepts, since inferences containing these concepts can extend our knowledge in highly nontrivial ways. For example:

Analytic geometry possesses in the equations by which it expresses the nature of a curve just that constitutive concept of its object that we are looking for. A very small number of related elements [abscissae and ordinates, plus constants and their arithmetical combination]...contain, implicit in themselves and derivable from them, all relations that necessarily subsist between any parts of the curve. From the law expressing the proportionality between the changes of the ordinates and the abscissae every other property of the curve can be developed. (section 117)

Both Frege and Lotze, then, want to identify forms of inference that lead to new mathematical knowledge. Both think that the traditional Aristotelian forms fail to do that: according to Frege, an Aristotelian inference only 'takes out of the box what we have already put in it' (Frege, 1884, section 88); according to Lotze, it is just 'a tautological repetition of its presuppositions' (Lotze, 1880, p. 190). Both identify the cause of this futility in the fact that the concepts deployed in syllogisms need only be sums of unordered marks (1880, section 122). Both argue that epistemically ampliative inferences – like those in mathematics – require a different kind of concept, whose content has a ‘functional’ rather than an Aristotelian structure (what Lotze here calls ‘constitutive concepts’ as opposed to merely ‘general concepts’). And both think that we logicians can discover these more complicated conceptual forms only by reflecting on inferences that do not fit the Aristotelian patterns.

Furthermore, Lotze, like Frege, turned this criticism of the traditional model against Boole’s logic itself in 1880 (in an appendix entitled ‘A note on the logical calculus’, added to the second edition of his Logic) – the same year in which Schröder’s review of Begriffsschrift appeared and within a year of the period in which Frege wrote ‘Boole’s Logical Calculus and the Begriffsschrift.’ The bulk of Lotze’s discussion is given over to collecting together objections to Boole’s procedure that were also given by others. What is interesting and novel in Lotze’s criticism, though, follows directly from Lotze’s non-Aristotelian, functional model of the structure and formation of concepts. Boolean operations like logical addition and multiplication on symbols for concepts can express ‘merely the simultaneous presence of their elements [viz., their marks]’ (1880, p. 278). Thus, the only ‘concepts’ that can be adequately represented by Boolean logic are the degenerate concepts like <red, juicy, edible, body>, which do not allow us to infer anything new
about the objects that fall under them. In particular, the expressive resources of Boolean logic would never be enough to represent the structure of mathematical concepts ‘for the form which the result of the calculation is finally to take, is here completely and solely determined by the definitely assignable nature of the connection which this science requires to be introduced between its elements [namely, the marks in a concept]’. The algebraic combinations of simple symbols simply cannot express ‘the reciprocal determination of the component parts’, that is, the functional interdependence of the marks that make up a concept (p. 279).

4 Frege’s advance over Lotze

Frege and Lotze identify the same set of problems in the traditional logic, and they think that the failure of traditional logic to represent inferences in mathematics rests on a commitment to a faulty understanding of how concepts are formed and so also how they are structured. Both think that Boolean logic, however much it might improve on traditional syllogistic as a tool for problem-solving, simply repeats the fundamental flaws in Aristotelian logic. And they share a general conception of how these flaws would be rectified in an improved account of concepts and inferences. These historical facts, I contend, allow us to understand Frege better. They explain, for instance, why Frege chose to argue for the superiority of the Begriffsschrift over Boolean logic by highlighting the theories of concept formation implicit in each. And they identify some of the constraints that Frege thought a symbolic logic like the Begriffsschrift had to satisfy. In this way, Sluga was correct to argue for the value of a reading of Frege that places him in his historical relationship with Lotze.

Dummett, in criticizing Sluga’s concern with discovering the influences on Frege’s thought, writes:

In expounding a philosopher, one has of course to report the theses which he held in common with his contemporaries or borrowed from his predecessors; but what makes him interesting will usually be the ideas that were original with him. Emphasis on the historical background will often be useful in making the original ideas stand out prominently from the derivative ones; but the historical method, as Sluga employs it, makes it hard to see what was original about Frege at all. (Dummett, 1981b, p. 529)

The contextualized reading of Frege that I have given here avoids the mistake that Dummett attributes to Sluga. In fact, the value of such a reading for a philosopher is the opposite of what earlier we saw Dummett assert. Locating Frege historically puts us in a unique position to see the original philosophical insights that Frege – armed with his Begriffsschrift – could bring. In this
closing section, then, I will show that—although Frege shared with Lotze some very significant common theses—the invention of his Begriffsschrift put Frege in a position to answer questions that Lotze left open, cash out ideas that Lotze left metaphorical, and correct philosophical errors that Lotze fell into.

Lotze argued that Boolean logic does not capture the interdependence of component concepts in mathematical concepts, and he models the structure of concepts with the dependence of one variable on another in mathematical functions. However, there is no detailed positive story forthcoming with which we can flesh out the model: Lotze does not pretend to have isolated all of the types of inferences that are used in mathematics, and he does not think that there are a few basic operations by means of which all compound concepts can be composed. Though Frege also traffics in metaphors (in his Begriffsschrift, the elements in definitions are ‘organically’ connected to one another (Frege, 1884, section 88)), the quantifiers, relations, and variables in his logical symbolism allow for a precise characterization of this ‘functional’ interdependence. Similarly, Lotze recognized that deduction can be epistemically ampliative, and, like Frege, he thought that traditional Aristotelian inferences cannot allow us to acquire new knowledge. But he had no spelled-out story of how this was really possible. Frege, on the other hand, can show in his Begriffsschrift how the same judgment can be decomposed differently from the way in which it was first formed; he can therefore provide a positive explanation of how we can learn new things from deductive inferences. According to Lotze, concepts fruitfully employed in inference cannot be formed by abstraction, and he helpfully suggests (as Frege did) that we can identify these new kinds of concepts by reflecting on forms of inference that do not fit the traditional patterns. Again, however, Frege has a concrete positive proposal: the Begriffsschrift, with its distinction between constants and variables, functional expressions and singular terms, shows us how to form new concepts from judgments by replacing constants with variables.

All of these innovations depend on the invention of the Begriffsschrift, Frege’s execution of Leibniz’s project of a characteristic language. It is extremely significant, then, that Lotze, though he did not declare Leibniz’s project impossible, had no optimism about its successful execution, and doubted its central importance for logic. His skepticism was well motivated at the time. Leibniz was convinced that the truth of a judgment consisted in the containment of the predicate in the subject, that the axioms of a science were really definitions, and that therefore all inferring was unpacking the definitions of the subject and predicate concepts. For this reason, Leibniz thought the project of a universal characteristic would consist primarily in identifying simple concepts. Lotze argued, however, that formulating a characteristic language for a science would require two other highly non-trivial tasks besides identifying simples. For one thing, in order to determine the truth of a judgment by calculating, we need to identify the
'general laws' of the various special sciences, and discovering these axioms necessitates ‘dissecting our judgments and tracing them back to simple principles’ (section 198). Second, Leibniz assumed that all concepts could be formed from simple concepts by algebraic operations, and he overlooked the fact that the components in a compound concept mutually determine one another; in fact, Lotze argued, Leibniz would have needed to know all of the special rules by means of which the marks in a concept could interrelate to form new structured concepts. On this second point, all of the criticisms of the concept as a sum of marks come into play, and it is no surprise that Leibniz's proposals for a *lingua characterica* look a lot like Boolean logics. 39

These criticisms of Leibniz are sound, and they are damning against any attempt to carry out Leibniz’s program for mathematics using a symbolic language like Boole’s. Nevertheless, Frege's Begriffsschrift – which isolates the basic laws of logic (and therefore also arithmetic) and isolates all of the ways in which concepts can be formed by interrelating component concepts – shows that a symbolic language can be devised that avoids these criticisms. Lotze, dissatisfied with the Boolean and Leibnizian proposals, thought that symbolic logic was a waste of time and destined simply to fall prey to the philosophical and expressive shortcomings in the traditional logic. 40 Again, however, Frege's Begriffsschrift shows Lotze to be mistaken: the flaws that Lotze identified in traditional logic in fact could be solved *only with* a new symbolic language.

Sluga obscures the decisive advance that Frege made over Lotze by over-emphasizing the affinity between Lotze's conception of concepts as functions and Frege's idea that concepts are functions. 41 For Lotze, a compound concept is (not itself a function, but) the value of applying a function to a collection of marks. For Frege a concept is a function, and the value of the function when applied to an argument is a sentence. This difference cannot be ignored, since it allowed Lotze to hold onto the subject/predicate analysis of sentences, while Frege rejected it. For example, Frege argued that the same sentence could be viewed as the result of applying different functions to different arguments – ‘Cato killed Cato’ is the result of applying the function ‘killing Cato’ to ‘Cato’ and also the result of applying ‘being killed by Cato’ to ‘Cato’ (Frege, 1879, section 9). It was thus because Frege hit on the idea of the function/argument analysis of sentences (as opposed to the traditional subject/predicate analysis) that he was able to discover the possibility of multiple decompositionality. This, in turn, made it possible for Frege (unlike Lotze) to give concrete and philosophically satisfying accounts of how concepts can be formed in new ways and how deductions can expand our knowledge. When Sluga, however, views Frege’s idea that concepts are functions as derived from Lotze’s different idea, he papers over what was new and important in Frege's logic and philosophy.

The fundamental thesis of Frege's philosophy of mathematics is that ‘arithmetic is a branch of logic and need take no ground of proof from either
experience or intuition’ (1893, p. 1). Earlier, Lotze called mathematics ‘an independently progressive branch of universal logic’ (1880, section 18). It is therefore tempting to conclude, as Sluga does, that ‘among the many things that Frege owes to Lotze, the most important is perhaps the idea of logicism’ (1980, p. 57). Sluga elaborates:

Whatever the details of Lotze’s position, it is clear that in some sense he subscribed to the claim that arithmetical propositions are grounded in general logical laws alone... Though Lotze claimed that arithmetic was really part of logic he never tried to show that conclusion could be established in detail nor did he list the additional logical principles which he considered necessary for that task. It was Frege who set out the necessary details. (1984, pp. 343–4)\(^42\)

However, as Dummett correctly argued (1981b, pp. 525–6), there are very fundamental differences between Frege’s logicism and Lotze’s philosophy of mathematics. Lotze claims to be ‘in entire agreement with Kant’ that the truths of arithmetic and geometry are synthetic (section 353). Mathematical judgments and inferences rest on a pure form of intuition: geometry on a pure intuition of space (section 354ff; section 152); arithmetic on a pure intuition of quantity and an intuition of our own mental ‘operations’ (section 353, section 361). ‘No mere logical analysis’, he writes, could inform us of the truth of arithmetical equations (section 361), whose self-evident truth is analogous to that enjoyed by the ‘simplest principles of mechanics’ (section 364).

In fact, the conflict between Frege and Lotze here is even more fundamental than Dummett or Sluga realized. Frege described his project in Foundations of Arithmetic as showing that arithmetical truths are ‘analytic’ – they depend only on ‘general logical laws and definitions’ – and he says that his thesis would be refuted if these laws and definitions were ‘not of a general logical nature, but belong to the sphere of some special science’ (Frege, 1884, section 3). But this is precisely what Lotze asserts. Above, I noted that Lotze isolates three kinds of non-Aristotelian inferences used in mathematics. These forms of inference, Lotze acknowledges, are ‘confined to the region of mathematics, and primarily to the relations of pure quantities’ (section 111).\(^43\) Because of the unique subject matter of mathematics – the nature of space for geometry, the nature of pure quantity for arithmetic – these forms of inference are applicable in these sciences but not elsewhere.\(^44\)

Lotze recognizes that the limited applicability of the three mathematical forms of inference presents a prima facie objection to including a discussion of them in a treatise on logic. But he replies:

The fact that the use of [these forms of inference] is confined to mathematics, cannot hinder us from giving [them] a place in the systematic series of forms of thought. For in the first place we must not forget that
calculation in any case belongs to the logical activities, and that it is only their practical separation in education which has concealed the full claim of mathematics to a home in the universal realm of logic. (section 112)\textsuperscript{45}

These two sentences, however, seem to be in open tension with one another. The second quoted sentence might motivate some readers to side with Sluga and Gottfried Gabriel to argue that ‘Lotze and Frege both subscribed to the reducibility of arithmetic to logic’ (Sluga, 1980, p. 73), while the first sentence might lead others to side with Dummett and deny this. But the tension here is only apparent, and Lotze seems confused only if we fail to appreciate just how different our post-Fregean conception of logic is from Lotze’s. As we saw above, logic for Lotze is the study of the operations of thought that allow for the reduction of coexistence to coherence. On this view, logic begins earlier than it did for Frege, with a discussion of how thinking transforms merely subjective states into objective ideas that can be true or false. And it also extends more broadly than it did for Frege, covering all of those ways in which thinking introduces an order into our ideas that can then model the reciprocal interactions of the things that exist independently of our ideas. In following out these later developments of thinking, Lotze investigated mathematical concepts and inferences, arguing that mathematics, with its functional concepts and non-Aristotelian inferences, uniquely fulfilled the task of thought. In this sense, the mathematical inferences belong necessarily in logic. But it simply does not follow that these inferences are of universal scope, and it does not follow that Lotze agreed with Frege that arithmetical truths are non-intuitive.

As Sluga and Gabriel point out, Lotze makes a similarly Fregean-sounding claim about mathematics earlier in the book:

All ideas which are to be connected by thought must necessarily be accessible to [...] quantitative determinations...I exclude [from our present investigation] the investigation of the consequences which may be drawn from these quantitative determinations as such: they have long ago developed into the vast structure of mathematics, the complexity of which forbids any attempt to re-insert it into universal logic. It is necessary, however, to point out expressly that all calculation is a kind of thought, that the fundamental concepts and principles of mathematics have their systematic place in logic. (section 18)

These sentences appear in the long discussion of the formation of concepts with which the book begins. As I briefly mentioned above, the formation of concepts for Lotze is the culmination of a three-step process that begins with merely subjective impressions. The first operation of thought is the ‘objectification’ (section 3) of the subjective impression, whereby I distinguish my act of sensing from its content, which is that which is sensed (section 2).
With these objective contents in hand (‘the red’, ‘the toothache’), a thinker can now interpret the relations that impressions have to one another as in fact ‘aspects of the content of the impressions’ themselves (section 9). This is the second operation of thought, ‘the composition, comparison, and distinction of the simple contents of ideas’, wherein thought distinguishes one content from the content of other ideas, and ‘estimates by quantitative comparison its differences and resemblances’ (section 19). Lotze’s point in the quoted passage, then, is that the contents of sensation, for instance the brightness or saturation of this or that red, can always be compared to one another quantitatively. And so both the concept <quantity> and the various principles of quantity ‘have their place’ in logic.

Readers familiar with the history of German philosophy will recognize this argument at once. Kant had argued (in the chapter entitled ‘The Anticipations of Perception’ in the first Critique, 1781/1787) that all sensible qualities come in a degree, and so the matter of any empirical intuition can be compared with that of another intuition quantitatively. This argument is part of Kant’s explanation for the applicability of the concept <quantity> and for the necessary applicability of mathematics in experience – rest, he thinks, on the conditions of the possibility of (objective) experience. Though it seems odd to us to find this in a logic text, Lotze is not confused when he gives this argument from ‘transcendental logic’ in his book, since his conception of logic is so much broader than Frege’s. But we only misunderstand him when we try to see this passage as an attempt to answer the question – Is arithmetic analytic? – for which Frege’s logicism is an answer. It was only because Frege had his Begriffsschrift that he could set about determining whether arithmetic is analytic. On the other hand, because Lotze did not pose this question, he failed to see the philosophical payoff that a new logical language could provide.

Notes

3. Sluga makes these kinds of historical claims at, for instance, pp. 55, 60, and 181 of Sluga (1980).
4. Sluga contrasts this faulty Fregean philosophy of language (and historiography) with a less blinkered ‘Wittgensteinian’ one on p. 186.
5. Richard Rorty (1984, p. 57) advocated a stronger historiographical position, according to which the standards we use in determining how interesting a piece of philosophy is as a piece of philosophy are always our standards: the ones we use in evaluating the philosophy written by other philosophers writing today.
6. ‘Sluga is so keen to discover sources for Frege’s ideas...that he fails to convey what was great about Frege’ (Dummett, 1981b, p. 529).
7. Jarmo Pulkinnen (2005) argues against Dummett that historians of philosophy should give only causal explanations for why certain ideas emerged – explanations that abstract from the philosophical merits of those ideas. This seems to me too radical.
8. ‘Historical reconstructions remind us of all those quaint little controversies the big-name philosophers worried about, the ones which distracted them from the ‘real’ and ‘enduring’ problems which we moderns have managed to get in clearer focus. By so reminding us, they induce a healthy skepticism about whether we are at all clear and whether our problems are all that real.’ (Rorty, 1984, p. 71). See also Hylton (1990, p. 6).

9. One need not assent to the exaggeration in Dummett’s first sentence to see his point.

10. I use the following convention: *Begriffsschrift* is the book written by Frege in 1879; *Begriffsschrift* is the logical system propounded in that book.


12. I would be gratified if the following discussion of Frege and Lotze were read in the spirit of Peter Hylton’s book on Russell. Noting the failure of modern logic to translate philosophy into a progressive science (p. 391), he argues that it is necessary for historians of analytic philosophy to identify the philosophical issues that occupied Russell’s British contemporaries and to trace out how Russell uses the new logic to make progress on these issues. He is not interested in ‘influences’ or historical causation *per se*, but on the way that Russell’s philosophy (along with his new logic) could or could not make up for the *philosophical deficiencies* of his contemporaries.

13. Pulkkinen (2005, chapter 4) has pointed out that Frege’s writings on Boole were part of a larger discussion of the philosophical significance of Boolean logic that was carried on in Germany between 1877 and 1882. Although Boole’s *Laws of Thought* was published in 1854, it received virtually no attention in Germany until Alois Riehl, Ernst Schröder, Wilhelm Wundt, Hermann Lotze, Hermann Ulrici, Friedrich Lange, Louis Liard, Leonard Rabus, and, of course, Frege all wrote works within that five-year period defending or praising Boolean logic. See also Peckhaus (1988).

14. Frege, of course, thinks that the converse point holds as well: Without a complete analysis of inferences into their simplest components, we would be unable to have complete analyses of the concepts employed in those inferences. A good example of this point is Frege’s analysis of theorems about sequences – if we did not see that these inferences do not require intuition but rest only on logical laws, we would not be in a position to see that the concept <x is a hereditary property> is in fact a compound concept analysable into logical primitives.

15. Frege most likely learned of Leibniz’s idea, and the terms ‘lingua characterica’ and ‘Begriffsschrift’ from Friedrich Trendelenburg’s 1867 essay ‘On Leibniz’s Project of a Universal Characteristic’, which Frege cites at 1879, pp. v–vi. Frege describes what he takes a Leibnizian *lingua characterica* to be and argues that his Begriffsschrift is an instance of such a language at Frege (1880–1, pp. 9–10). (1882–3, pp. 90–1), (1897, p. 235). Leibniz had conceived of a *lingua characterica* as a *universal* characteristic. Though Frege thought that the Begriffsschrift could be employed outside of arithmetic (1882, p. 89) and he speculated that it could be extended to other areas (1879, p. vi), he only claimed that he had succeeded in formulating a characteristic language for arithmetic. (In this paper, I will not be considering the suitability of the Begriffsschrift for acting as a *lingua characterica* for other sciences.)

16. These writings were prompted by Schröder (1880), which compared *Begriffsschrift* unfavorably with the works of Boolean logicians. On the Frege-Schröder controversy, see Peckhaus (1997, pp. 287–96; 2005) and Sluga (1987). It is important
to remember that Frege’s criticisms of Boolean logic were aimed against the Boolean writings that he knew: Schröder’s *Operationskreis* (1877), as well as the work of Boole and Jevons. In particular, Boole’s system was modified and greatly expanded by Peirce, who (exploiting ideas from De Morgan) arrived independently in 1883 at a system expressively equivalent to the first order fragment of *Begriffsschrift*. But in the Boolean works that Frege knew, there were no equivalents to Frege’s use of variables, quantifiers, and relations.

17. Throughout the paper, I refer to concepts in brackets, and linguistic expressions in single quotes.

18. See 1879, section 28, theorem 98. Frege emphasizes that this theorem (and ones like it) do not require intuition at 1879, section 23 and 1880–1, p. 32. The non-logical rule of inference is, of course, mathematical induction, which Frege actually derives from the laws of *Begriffsschrift* (1879, section 27; 1880–1, p. 31).

19. The theory of concept formation, as we will see below in the case of Lotze, was one of the most active areas of debate among nineteenth-century German logicians. See Heis (2012) (section 4).

20. Frege made the point that his logic, unlike that of Leibniz, Aristotle, or the Booleans, forms new concepts from completed judgments, and not vice-versa, from early in his career till very late: see 1882, p. 94; ‘Notes for Ludwig Darmstaedter’ (1919) in Frege (1979, p. 253).

21. Frege does not cite any passages when he attributes this theory of concept formation to Boole. An apt citation would have been Boole (1854, pp. 42–7), where Boole describes the ‘acts of conception’ whereby any simple or compound conception is formed. There, he gives two primitive operations of the mind: selecting from a given class \( x \) those individuals that also belong to a class \( y \); and ‘form[ing] the conception of that collection of things which two classes taken together compose.’ (On p. 48, Boole adds the operation of taking the complement of a class.) Boole adds some brief comments about the faculties of the mind at work in these acts: attention, imagination, comparison, and abstraction (1854, p. 43; 1847, p. 16).

22. Frege will sometimes characterize the traditional view as one according to which concepts are formed by abstraction, and sometimes as one according to which concepts are formed by Boolean combinations of simpler concepts. Frege is on good ground in moving back and forth between these descriptions, since the two ideas were indissolubly linked in the tradition. See Heis (2012) (section 4).

23. Readers interested in a detailed explanation of how Frege’s argument works here may consult Dummett (1991a, pp. 36–42).

24. I think that when Frege calls a concept ‘fruitful’ he means that inferences involving that concept can extend our knowledge. Jamie Tappenden (1995) thinks that Frege uses the word ‘fruitful’ to pick out those concepts that are *mathematically* significant in other, more interesting ways. As I explain below, however, other German logicians in the 1870s were using the word ‘fruitful’ to pick out those ways of forming concepts that allow for inferences that extend our knowledge. Moreover, Frege’s account of fruitful concept formation by decomposition does successfully explain how inferences can extend our knowledge. It seems better, then, to read Frege’s use of the word ‘fruitful’ to describe concepts in the way that other logicians were using that word.

25. On Lotze’s place among late-nineteenth-century German logicians, see Gabriel (1989a).

27. See (Schottler 2006, p. 45). The most extensive discussion of the relationship between Frege and Lotze is Gabriel (1989a) and (1989b). See also Carl (1994, pp. 47ff; 2005); Dummett (1981b, 1991b, pp. 65–125); Gabriel (2002; Milkov (2007); Peckhaus (2000); Schmit (1990); Sluga (1976), (1977), (1980), and (1984). Most of the discussion of the relationship between Frege and Lotze has understandably focused on Frege’s so-called ‘platonism’ and Lotze’s theory of objectivity and validity. Less attention has been given to the issues I discuss in this paper.

28. Gabriel has pointed out that Frege could take Lotze as an ally or even as a source for his attack on Boolean logic (1989b, pp. xxv–vi). In this paper, I greatly expand on these brief remarks from Gabriel – and I argue in the closing section of the paper that Gabriel takes these affinities too far.

29. On the rival conceptions of logic in nineteenth-century Germany after Hegel, see Heis (2012), section 3; Peckhaus (1997, pp. 130–63); Vilkko (2002, chapters 3–4). Frege does not mention it, but his view that logic is primarily concerned with inference was a common view among British logicians, like Whately, Mill, and Boole.

30. References to Lotze’s Logic will be to section numbers, which are common to the German original and the English translation. The translations will be from the 1888 English translation, edited by Bosanquet, though with some modifications of my own here and there.

31. These are the three examples Lotze gives of ‘accessory notions’ in Section 6.

32. Lotze’s contemporary, Friedrich Ueberweg (1857, section 28), also noted that Lotze’s notion of logic was close to Kant’s transcendental logic.

33. I will return to the first two steps below, p. 33.

34. Christoph Sigwart (1878, section 75.2) argued that forming concepts by summing marks is an ‘unfruitful’ method of concept formation, and he therefore rejects the attempts (like Leibniz’s) to represent compound concepts as algebraic combinations of simples. Similarly, Schröder (1890, pp. 101, 566–8) defends Boolean logic against the view – which he attributes to Lotze but suggests is extremely widespread – that a symbolic logic that treats of algebraic relations among concept extensions is ‘unfruitful.’

35. The three forms of inference are inference by substitution, inference by proportion, and inference from constitutive equations. See also Peckhaus (1997, pp. 159–163).

36. Lotze explains in more detail why reasoning in the Aristotelian way cannot produce new knowledge also at 1880, section 98.

37. In this note, Lotze discusses the writings of Boole, Jevons, and Schröder. (Although Frege’s Begriffsschrift appeared in 1879, Lotze does not mention it, and there is no evidence that Lotze ever knew Frege’s work.) As Pulkinnen (2005, p. 123) points out, Lotze’s criticism of Boole was the most comprehensive criticism of Boolean logic written in Germany at the time. (Pulkinnen does not mention, though, that Lotze’s criticism draws on his critique of the traditional theory of concepts, and he does not point out the affinities between Frege’s and Lotze’s discussions.) On Lotze’s note, see also Peckhaus (1997, pp. 159–163).

38. Frege does not illustrate what he means by the ‘organic interconnection’ of elements in his definitions. One apt illustration might be his notion of quantifier dependence – where ‘(∀x)(∃y)R(x, y)’ expresses a different relation among the variables than ‘(∃y)(∀x)R(x, y)’. The Begriffsschrift captures these ‘interconnections’ because it includes relations and polyadic quantifiers – elements that Frege thinks depend on his new way of forming concepts.
39. Trendelenburg also criticizes Leibniz's project for apparently requiring the faulty view that concepts are algebraic combinations of marks (1867, p. 24). Gabriel nicely points out that Frege’s criticism of Boole can also be seen as a defense against Trendelenburg’s criticisms (1989a, pp. xxiv–v).

40. Lotze’s hope was that German philosophers would seek ‘not merely to calculate the course of the world, but to understand it’ (section 365).

41. Sluga (1980, pp. 53, 56–7). The connection between Lotze’s and Frege’s theories of concepts and functions was made earlier by Thiel (1968, p. 155), and ultimately by Bauch (1918, pp. 47–8). Other writers have questioned the connection: Gabriel (1989a, pp.xxv–vi); Kreiser (2001, p. 150).

42. Gottfried Gabriel has argued for a similar conclusion (1989a, p.xxii).

43. Lotze makes this point in detail for each of the three ‘mathematical’ forms of inference in sections 111, 115, and 118.

44. Similarly, the primary technical result of Frege’s Begriffsschrift is the purely logical analysis of mathematical induction. Lotze, on the other hand, though he discusses mathematical induction in section 210, never feels the need to reduce it to forms of deduction that are universally applicable; indeed, he never even suggests that it is reducible.

45. Both Gabriel and Sluga quote this sentence (but not the first!) in support of their interpretation.

46. Clinton Tolley has pointed out to me that Frege does sometimes use the word ‘logic’ more broadly – in his unpublished works called ‘Logic’ and in his late ‘Logical Investigations’. But I have in mind the narrower notion of logic that Frege uses (say, in 1884, Section 3) when he is arguing for logicism.

47. Many thanks to Penelope Maddy, Richard Mendelsohn, Thomas Ricketts, Gila Sher, Clinton Tolley, Mark Wilson, and the participants of the Southern California History and Philosophy of Logic and Mathematics Group. I owe special thanks to Erich H. Reck, as both an editor and philosophical interlocutor, for helping me to improve this paper significantly.

References


Frege or Dedekind? Towards a Reevaluation of Their Legacies

Erich H. Reck

The philosophy of mathematics has long been an important part of philosophy in the analytic tradition, ever since the pioneering works of Frege and Russell. Richard Dedekind was roughly Frege’s contemporary, and his contributions to the foundations of mathematics are widely acknowledged as well. The philosophical aspects of those contributions have been received more critically, however. In the present chapter, Dedekind’s philosophical reception is reconsidered. At the chapter’s core lies a comparison of Frege’s and Dedekind’s legacies, within and outside of analytic philosophy. While the comparison proceeds historically, it is shaped by current philosophical concerns, especially by debates about neo-logicist and neo-structuralist views. In fact, philosophical and historical considerations are intertwined thoroughly, to the benefit of both. The underlying motivation is to rehabilitate Dedekind as a major philosopher of mathematics, in relation, but not necessarily in opposition, to Frege.

The chapter is structured as follows: In Section 1, a brief reminder about Frege’s and Dedekind’s contributions will be provided, together with a look at how they saw the relationship between their works themselves. In Section 2, we will turn to the early reception each received in analytic philosophy, from Russell on, with the focus on critical responses to Dedekind. Then, in Section 3, the revival of Frege’s ideas since the 1950s, the rise of neo-logicism since the 1980s, and further criticisms of Dedekind within those contexts will be discussed. In Section 4, after noting the more positive response Dedekind received in mathematics, I will bring to bear the rise of neo-structuralism since the 1980s, thereby starting to turn the tables. This will be followed, in Section 5, by more direct defenses of Dedekind, to be found in Ernst Cassirer’s discussions of his works and in current philosophy of mathematics. The chapter will end with some reflections on where this leaves us, with respect to Frege, Dedekind, and their philosophical legacies.
1 Frege, Dedekind, and their Relationship

Most of Dedekind’s philosophical remarks can be found in two small booklets, *Stetigkeit und irrationale Zahlen* (1872) and *Was sind und was sollen die Zahlen?* (1888). They were published during the same period as Frege’s main works, *Begriffsschrift* (1879), *Die Grundlagen der Arithmetik* (1884), and *Grundgesetze der Arithmetik* (1893/1903). There is quite a bit of overlap between these texts. Both authors present new foundations for the theories of the natural and real numbers; they both proceed without relying on geometry or, more generally, any ‘intuitive’ assumptions; and they present ‘logicist’ alternatives instead, based on new theories of relations, functions, and classes. There are further similarities with respect to details. For instance, the ways in which they analyze mathematical induction logically – Frege in terms of the ‘ancestral’ relation, Dedekind in terms of the notion of ‘chain’ – are not only equally innovative but equivalent.

Besides such similarities there are also differences. A commonly mentioned one is that Dedekind’s foundational contributions lie more on the model-theoretic side (studying, e.g., models of theories and isomorphism results), while Frege’s are primarily proof-theoretic (based on his new proof system). A more general difference is that, while Frege produced some mainstream mathematical works besides his trailblazing contributions to mathematical logic, they remained minor. Dedekind, in contrast, was a major, highly influential contributor to mathematics, especially to algebra and number theory. With respect to philosophy, the situation is reversed. Frege wrote extensively on philosophical topics, in ways that had a strong impact over time; but only a few philosophical remarks are sprinkled though Dedekind’s writings. Still, an important difference between them, for present purposes, concerns a philosophical matter. Namely, Dedekind articulated a structuralist view about the nature of mathematical objects, based on certain kinds of ‘abstraction’ and ‘free creation’; Frege constructed his logical objects in a non-structuralist way, as explicitly defined equivalence classes.

I will explore both the similarities and the differences further as we go along. But let me address another question first: How did Frege and Dedekind perceive their relationship themselves? The two thinkers never met in person; nor did they have a correspondence, as far as I know. It is also evident that they developed their basic ideas independently. Thus, in the Preface to the second edition of *Was sind und was sollen die Zahlen?* (published in 1893) Dedekind remarks that it was only ‘about a year after the publication of my memoir that I became acquainted with G. Frege’s *Grundlagen der Arithmetik*’ (Dedekind, 1963, p. 42). Dedekind does not say anything about Frege’s *Begriffsschrift* here; but since he had settled on his core ideas already before its publication, he clearly developed them independently. Frege mentions Dedekind’s works that predate his own, such as *Stetigkeit und*
irrationale Zahlen, neither in Begriffsschrift nor in Grundlagen; and in the later Grundgesetze his disagreements with Dedekind predominate. 6

After having become aware of each other’s writings, both Frege and Dedekind commented on the relation between their projects. Above, I quoted from Dedekind’s only explicit reference to Frege in print, in the second edition of Was sind und was sollen die Zahlen? (1893). He continues:

However different the view of the essence of number adopted in [Frege’s Grundlagen] is from my own, it contains, particularly from section 79 on, points of very close contact with my paper, especially with my definition (44) [of the notion of chain]. The agreement, to be sure, is not easy to discover on account of the different form of expression; but the positiveness with which the author speaks of the logical inference from \( n \) to \( n + 1 \) [...] shows plainly that here he stands upon the same ground with me. (Dedekind 1963, pp. 42–43)

Dedekind does hint at some differences to Frege in this passage (more on those below). But his emphasis on positive connections between their approaches is typical for him. (His response to, say, Cantor’s rival theory of real numbers is similar.)

Equally typical for Frege is that his published reactions to Dedekind’s works, in both volumes of Grundgesetze, are strongly critical. Yet they are not entirely negative. In the Preface to Volume I of Grundgesetze, Frege calls Dedekind’s essay on the natural numbers ‘the most thorough work on the foundations of arithmetic that has come to my attention in the last few years’ (Frege, 1893, p. 196). He also sees an agreement with respect to their basic convictions, since ‘Dedekind too is of the opinion that the theory of numbers is a part of logic’ (ibid.). Indeed, in the original Preface of Was sind und was sollen die Zahlen? Dedekind had talked about developing ‘that part of logic which deals with the theory of numbers’ as his goal, then adding:

In speaking of arithmetic (algebra, analysis) as a part of logic I mean to imply that I consider the number-concept entirely independent of the notions of intuition of space and time, that I consider it an immediate result from the laws of thought. (Dedekind, 1963, p. 31)

It is based on such programmatic statements, together with corresponding technical details, that Frege could acknowledge Dedekind to be a fellow ‘logicist’.

While Frege does see connections between his and Dedekind’s projects, he couples their acknowledgement with a battery of criticisms. His main criticisms in Volume I of Grundgesetze concern Was sind und was sollen die Zahlen? Frege’s first such criticism (still in the Preface) is the following:
While Dedekind is also pursuing a logicist project, the conciseness of his proofs – the fact that they are ‘merely indicated’, not ‘carried out in full’ (Frege, 1893, p. 196) – does not allow one to be sure that all presuppositions have been identified. This problem is aggravated by the fact, also pointed out by Frege, that Dedekind does not formulate his basic laws explicitly; much less does he provide a complete list of them. Consequently, it is not clear why Dedekind’s theory of ‘systems’ should be seen as logical. Altogether, it is thus questionable whether a logicist reduction of arithmetic has actually been achieved.

Further criticisms raised by Frege in Volume I of Grundgesetze (now its Introduction) concern details of what Dedekind does say about ‘systems’. Let me mention two of them. First, Frege sees Dedekind’s treatment of systems with one element (his identification of singletons with their elements) as problematic because, among others, it encourages confusing the element and subset relations. Together with his exclusion of the empty system, it also makes one wonder whether Dedekind thinks of systems as ‘consisting’ of their elements (like mereological sums), a view Frege rejects strongly. Second, while Dedekind conceives of systems extensionally – as Frege notes approvingly – some of his remarks about them are problematic. In particular, when Dedekind writes about ‘regarding [various objects] from a common point of view’, this makes it appear as if what underlies the existence of systems is some mental operation of ‘putting together in the mind’. Their nature and existence thus become too subjective, while Frege insists on their objectivity. In short, Dedekind’s position on systems seems ‘psychologistic’.

In later parts of this chapter, I will defend Dedekind against a number of criticisms, including the charge of psychologism. But let me formulate initial evaluations of Frege’s other charges, as just mentioned, right away. Arguably, several of them are based on an uncharitable reading. Thus, Dedekind explicitly acknowledges the possibility of introducing an empty system; he works with a clear distinction between elements and subsets elsewhere; and he treats systems in an abstract (non-mereological) way in general. Yet, with his first main charge Frege did put his finger on a sore spot. While one can defend Dedekind’s way of ‘sketching’ proofs as acceptable from a usual mathematical point of view, he does in fact smuggle in unnoticed presuppositions and use unstated laws at times. For example, in his treatment of infinity, the Axiom of Choice is used implicitly (as Zermelo pointed out later). More basically, it is hard to be sure what exactly Dedekind’s conception of ‘systems’ is; even more so for ‘logic’.

While Frege formulates these criticisms of Was sind und was sollen die Zahlen? in Grundgesetze, Volume I, it is also noteworthy which ones he does not raise. Let me again mention two: First, he does not object to Dedekind’s notions of ‘abstraction’ and ‘free creation’ here (although he will do so later). Second, Frege does not bring up the part of Dedekind’s essay that would soon became most infamous: his ‘proof’ of the existence of an infinite
system (Proposition 66), and more specifically, the appeal to ‘the totality of all things which can be objects of my thinking’ in it (Dedekind, 1963, p. 64). While not mentioned in Grundgesetze at all, there is another place where Frege addresses that appeal, however: his posthumously published ‘Logic’ (drafted in 1897). In that piece, Frege defends his usual distinction between objective ‘thoughts’ and subjective ‘acts of thinking’. After acknowledging that this involves a non-standard use of the word ‘though’, he points out that there are others who use it similarly – including Dedekind. More specifically, he argues: As we may assume that Dedekind ‘has not thought infinitely many thoughts’, he, too, must use ‘thinking’ in a non-psychologistic way. Note two aspects here: Not only does Frege not criticize Dedekind for holding psychologistic views in this context; he also rejects neither Proposition 66 nor its proof.

As we saw, Frege’s main criticisms of Dedekind in Volume I of Grundgesetze are directed against the theory of ‘systems’ from Was sind und was sollen die Zahlen?, which he had clearly studied carefully by that time. Those in Volume II of Grundgesetze concern the earlier essay, Stetigkeit und irrationale Zahlen, and more particularly, Dedekind’s introduction and famous characterization of the real numbers in it. Frege starts again with a positive remark in this context. He commends Dedekind for implicitly rejecting formalist view – by making an explicit distinction between signs and what they stand for, by treating the real numbers as objects referred to by means of signs, and by conceiving of equality for numbers in a corresponding ‘objectual’ way, all details Frege agrees with. But then his critical assault resumes.

Frege’s first criticism at this point concerns the following: He observes that Dedekind, after introducing his system of cuts on the rational numbers, does not identify the real numbers with the cuts; rather, he talks about the ‘creation’ of new objects, one for each cut. Frege’s objection is not that the notion of ‘creation’ at play here is psychologistic. Nor is it that there cannot be such objects, with only structural properties. Rather, he points out that Dedekind has not inquired generally into when such ‘creation’ is feasible, including whether there are any limits to it. One obvious limit is when one is led to an inconsistency, a case he accuses Dedekind of ignoring. Frege then groups him with other thinkers, such as Hermann Hankel and Otto Stolz, who use ‘creative definitions’ without any justification, concluding sarcastically: ‘[T]he inestimable advantage of a creative definition is that it saves us a proof’. But this charge against Dedekind can again be deflected, since it ignores the role Dedekind’s construction of the system of cuts plays for him (similarly for Proposition 66 in Dedekind’s treatment of the natural numbers).8

Frege’s second main criticism of Dedekind in Volume II of Grundgesetze is the most subtle but also the most slippery. Just before admitting, rather surprisingly, that his own introduction of extensions via Basic Law V might
perhaps be seen as a kind of ‘creation’ as well (although expressly not as a ‘definition’), Frege declares:

If there are logical objects at all – and the objects of arithmetic are such objects – then there must be a means of apprehending, or recognizing, them. This service is performed for us by the fundamental law of logic that permits the transformation of an equality holding generally into an equation [i.e., Basic Law V]. Without such a means a scientific foundation for arithmetic would be impossible. (Frege, 1903, pp. 278–279)

The criticism of Dedekind's procedure is, thus, that he does not provide us with a ‘means of apprehending or recognizing’ for the novel objects he introduces. One intriguing aspect here is the connection to the well-known ‘Julius Caesar problem’, as brought up in Frege’s *Grundlagen*. Another is that Dedekind, if read charitably, does actually provide the required ‘means’, albeit implicitly. Namely, his structurally conceived numbers have only ‘arithmetic’ properties, which differentiates them from objects like Julius Caesar. Perhaps this Fregean charge can therefore be deflected as well. 9

2 Russell’s Criticisms of Dedekind and their Immediate Impact

In *Grundgesetze*, Frege expressed frustration about the lack of attention his works had received so far. Dedekind’s two foundational essays were also not widely appreciated initially, especially by philosophers.10 One of the first to pay careful attention to both was Bertrand Russell. Most famous in this connection is, of course, Russell’s discovery of the antinomy named after him, which applies to Frege’s and Dedekind’s theory of classes. The fact that the Russell class (of all classes that do not contain themselves) can be formed according to these theories, thus leading immediately to a contradiction, confirmed Frege’s concerns about consistency in the worst possible way. After being told about it by Russell in 1902, his response – in an Appendix to Volume II of *Grundgesetze* – showed consternation. Obviously, there was a problem with his Basic Law V. But without it, how could arithmetic be ‘scientifically established’? When Dedekind found out, already in 1899, about antinomies like Russell’s from Georg Cantor (who had discovered them independently), he was equally dismayed. According to one report, he wasn’t sure any more whether ‘human thinking was really rational’.11

Besides the devastating impact of his antinomy, Russell’s more general reception of both Frege’s and Dedekind’s writings is crucial, especially for us, in two other respects as well. First, it was with Russell’s writings that a now entrenched view of ‘logicism’ emerged, one that gives pride of place to Frege and Russell while tending to exclude Dedekind. Second, it was through Russell’s works, together with those of his students and successors,
that Fregean ideas became a central part of the analytic tradition, while Russell’s criticisms of Dedekind led to his relative neglect by philosophers. In the rest of this section and the next, I will elaborate on both of these points. I will also provide a brief summary of Russell’s further criticisms of Dedekind.

I already noted that, despite his own criticisms, Frege saw Dedekind as a fellow logicist. Actually, he was widely recognized as such in the late nineteenth century – a number of writers, from C.S. Peirce through Ernst Schröder to the early Hilbert, saw in Dedekind a main, and perhaps the original, ‘logicist’. This changed in the twentieth century. Why? Several factors played a role, perhaps most importantly the following: After the discovery of his and related antinomies, Russell’s response to them, as worked out in Principia Mathematica (1910–1913), became the primary logicist option. Indeed, it came to be seen as its paradigm case, thus as almost definitional of ‘logicism’. Moreover, Principia was clearly more a successor to Frege’s theory than to Dedekind’s (with its explicit logical laws and its deductive emphasis). This is also how Russell viewed the matter, including in some retrospective accounts. Dedekind’s approach, in contrast, came to be seen as a predecessor to axiomatic set theory, to model theory, and to Hilbertian formalism (in striking contrast to Frege’s praise of Dedekind as an anti-formalist).

What were Russell’s criticisms of Dedekind, besides his antinomy? Like in Frege’s case, let me go over several main ones. In Principles of Mathematics (1903), his first relevant book, Russell too starts out positively, by acknowledging several ‘brilliant contributions’ by Dedekind (as well as by Cantor, Frege, and Peano). These include: Dedekind’s general treatment of relations, including the notion of ‘progression’ (Dedekind’s ‘simple infinity’); his corresponding notion of ‘chain’ and analysis of mathematical induction (which Russell took over mainly from Dedekind, not from Frege, as Quine pointed out later); his definition of infinity; and his use of cuts for introducing the real numbers. Again like Frege, Russell then added various negative points, in Principles and later texts. These concern Dedekind’s treatment of both the natural and the real numbers.

It appears that Russell struggled from the beginning with getting a good, or even any, handle on Dedekind’s structuralist position. He remarks that Dedekind prefers to view the natural numbers as ‘ordinals’, not as ‘cardinals’. One initial, vague complaint is, then, that ordinals are more ‘complicated’ than cardinals. Russell continues:

Now it is impossible that this account should be quite correct. For it implies that the terms of all progressions other than the ordinals are complex, and that the ordinals are elements in all such terms, obtained by abstraction. But this is plainly not the case. A progression can be formed of points or instants, or of transfinite ordinals, or of cardinals,
in which, as we shall shortly see, the ordinals are not elements. (Russell, 1903, pp. 248–249)

What Russell seems to claim in this passage is that the entities (‘terms’) in any simple infinity (‘progression’) must contain Dedekind’s ordinal numbers ‘as elements’; and he rejects the latter as false. But how is that related to Dedekind’s position? The fact that Russell struggles in this regard comes through further when he writes: ‘What Dedekind intended to indicate was probably a definition by means of the principle of abstraction, such as we attempted to give in the preceding chapter’ (p. 249). It seems that the only way for Russell to make sense of Dedekind’s ‘abstraction’ was to assimilate it to his own ‘principle of abstraction’. Yet, Dedekindian abstraction works quite differently.  

Russell’s second main objection, which follows immediately after the first, concerns Dedekind’s corresponding structuralist conception of mathematical objects:

Moreover it is impossible that the ordinals should be, as Dedekind suggests, nothing but the terms of such relations as constitute a progression. If they are to be anything at all, they must be intrinsically something; they must differ from other entities as points from instants, or colours from sounds. (ibid.)

Here the charge is that there cannot be entities as conceived of by Dedekind. According to Russell, every ‘term’, ‘entity’, or ‘object’ simply has to have non-structural properties. This seems to be a fundamental ontological conviction, or prejudice, for him – it is not justified further. Next, Russell is led to the following suggestion:

What Dedekind presents to us is not the numbers, but any progression alike, and his demonstration nowhere – not even where he comes to cardinals – involve any property distinguishing numbers from other progressions. (ibid.)

What Russell suggests in this passage is that, along Dedekind’s lines, any statement about numbers is really a statement about all ‘progressions’, i.e., it should be understood in terms of a universally quantified proposition. Russell’s attribution of this ‘universalist’ position to Dedekind – seemingly in an attempt to be charitable – again misses its mark. However, it turned out to be quite influential later on (as we will see below).  

Let us move on to Russell’s criticisms of Dedekind concerning the real numbers, in *Principles* and later. In this context, too, Russell makes some claims that are puzzling. For example, it is difficult to see how one can find a clearer analysis of the notion of ‘continuity’ in Cantor’s writings.
compared to Dedekind’s; but that is what Russell maintains. He also raises the following objection: For Dedekind, the existence of the real numbers remains a ‘sheer assumption’, i.e., it is not backed up by argument. Like Frege, Russell lumps Dedekind together with other writers in this connection, namely ones that simply ‘postulate’ the existence of mathematical entities. And again like Frege, he has only scorn and ridicule for such views. As he famously puts it later:

The method of ‘postulating’ what we want has many advantages; they are the same as the advantages of theft over honest toil. Let us leave them to others and proceed with our honest toil. (Russell, 1919, p. 71)

I already gave a response to this kind of charge above. Namely, it ignores Dedekind’s construction of the system of cuts before introducing the real numbers; similarly for his explicit attempt to establish the existence of a simple infinity (Proposition 66). In other words, Dedekind does provide some ‘honest toil’ in this connection.

But perhaps Russell’s most interesting comment on Dedekind concerns exactly the ‘proof’ of Proposition 66. It helps to be a bit more explicit about it now. Dedekind does not just appeal to ‘the totality \( S \) of all things which can be objects of my thinking’ in it; he also brings in his own ‘ego’, or ‘self’, as a distinguished element, and the function that maps a thought \( s \) to ‘the thought \( s' \), that \( s \) can be object of my thought’ (Dedekind, 1963, p. 64). The argument is, then, that the collection of all the successors of the distinguished element under that function (the corresponding ‘chain’) forms an infinite system. Now, in *Principles* Russell first notes the similarity of this argument to one provided in Bernard Bolzano’s *Paradoxien des Unendlichen* (as does Dedekind in the second edition of his essay). He then reconstructs the Bolzano-Dedekind argument as follows:

For every term or concept there is an idea, different from that of which it is the idea, but again a term or concept. On the other hand, not every term or concept is an idea. There are tables, and ideas of tables; numbers and ideas of numbers; and so on. Thus there is a one-one relation between terms and ideas, but ideas are only some among terms. Hence there is an infinite number of terms and of ideas. (Russell, 1903, p. 307)

What is the problem, then? Is Russell’s criticism that, if understood in a mental or psychological sense, there may not exist enough ‘ideas’ for Dedekind’s purposes? Not exactly, since he adds the following in a footnote: ‘It is not necessary to suppose that the ideas of all terms exist, or form part of some mind; it is enough that they are entities’ (ibid.). So far, this is not a strong objection, if any, to Dedekind’s proof.\(^{16}\)
In Russell’s article, ‘The Axiom of Infinity’ (published in 1904, a year after *Principles*), a new twist is added to this line of thought. Russell asks us to consider the following sequence: 0 = the number of the empty class; 1 = the number of \{0\}; 2 = the number of \{0, 1\}, etc. He notes that the entities introduced along such lines – the finite cardinal numbers – are all different; and there are entities different from all of them, such as ‘the number of all finite cardinal numbers’, i.e., the first infinite cardinal number. What we get, then, is a proof of the existence of an infinite class that is parallel to Dedekind’s and Bolzano’s but avoids using the notion of ‘idea’. Why might such a modified proof be preferable for Russell? Because it provides ‘a strict proof appropriate to pure mathematics, since the entities with which it deals are exclusively those belonging to the domain of pure mathematics’ (pp. 257–258). This leads to the following criticism of Dedekind:

Other proofs, such as the one from the fact that the idea of a thing is different from the thing, are not appropriate to pure mathematics, since they [...] assume premises not mathematically demonstrable. (Russell, 1904, p. 258)

In other words, for Russell (the Russell of this early period) the problem is not that we cannot get a proof of the existence of an infinite class by appealing to ‘ideas’. In fact, he adds: ‘Such proofs are not on that account circular or otherwise fallacious’ (ibid.). It is, rather, that they involve a dimension ‘not appropriate to pure mathematic’.

Russell’s variant of the Bolzano–Dedekind proof works only if we have the operator ‘the number of...’ at our disposal. When writing his 1904 article, Russell seems to still think that his initial conception of the natural numbers, as equivalence classes of classes (essentially Frege’s from *Grundgesetze*), provides what is needed here. Moreover, in *Principles of Mathematics* the following related remark occurs: ‘There seems, in fact, to be nothing to choose, as regards logical priority, between ordinals and cardinals, except that the existence of the ordinals is inferred from the series of the cardinals’ (Russell, 1903, p. 241). But with the collapse of Russell’s early theory of classes this option vanishes. His response is to replace that theory by a ‘no-classes theory of classes’, within a ramified theory of types. The existence of an infinite class (at one type level) is then no longer provable. At that point, Russell adopts an axiom of infinity (for individuals), most prominently in *Principia Mathematica*, since no other option seems available. But what is the status of that axiom? In particular, can it be seen as a logical axiom?

By the time of *Introduction to Mathematical Philosophy* (1919), Russell has come to acknowledge that he has a basic problem in this connection: his axiom of infinity, while not contradictory, is ‘not demonstrably logical’
Frege or Dedekind?

This leads him back to (his version of) Dedekind's original ‘proof’, which he now criticizes as follows:

If the argument is to be upheld, the ‘ideas’ intended must be Platonic ideas laid up in heaven, for certainly they are not on earth. But then it at once becomes doubtful whether there are such ideas. If we are to know that there are, it must be on the basis of some logical theory, proving that it is necessary to a thing that there should be an idea of it. We certainly cannot obtain this result empirically, or apply it, as Dedekind does, to ‘meine Gedankenwelt’ – the world of my thoughts. (Russell, 1919, p. 139)

As the subsequent discussion makes clear, Russell now doubts whether we can assume the existence of an ‘idea’ corresponding to every object. In fact, he has become skeptical about the very notion of ‘idea’. As he puts it: ‘It is, of course, exceedingly difficult to decide what is meant by “idea”’ (ibid.). The basic problem with Dedekind’s procedure remains, however, that no ‘logical theory’ can assure us of what is needed in it.

Clearly, Russell was quite critical of Dedekind’s philosophical views, as opposed to his technical achievements. On the other hand, he had high praise for Frege as a philosopher, from *Principles* on. Both reactions proved hugely influential. Let me illustrate that fact by considering three of Russell’s main heirs briefly: Ludwig Wittgenstein, Rudolf Carnap, and W.V.O. Quine. In Wittgenstein’s *Tractatus Logico-Philosophicus* (1921/1922), the non-logical nature of Russell’s axiom of infinity is pointed out; Russellian logicism is thus rejected. Nevertheless, the *Tractatus* is deeply influenced, not only by Russell, but also by Frege. In contrast, Dedekind is not mentioned at all in the text. In Wittgenstein’s later writings, Russell- and Frege-inspired topics remain central. Dedekind now comes up occasionally as well, for example, in the *Remarks on the Foundations of Mathematics* (1956), but in highly critical, even dismissive terms. Among others, Wittgenstein criticizes Dedekind’s theory of the real numbers along finitist and constructivist lines.

Carnap was another of Russell’s main heirs. He was also strongly influenced by the *Tractatus*, at least for a while. Carnap does not challenge the significance of Dedekind’s technical achievements, as Wittgenstein seems to do. But like Wittgenstein, he engages much more with Frege’s philosophical views than with Dedekind’s, as works such as *Meaning and Necessity* (1949) illustrate. In Carnap’s very influential article on logicism, ‘The Logical Foundations of Mathematics’ (1931), he also further entrenches the view that Frege and Russell were the two main founders of logicism, while Dedekind hardly matters. Similar remarks apply to Quine, Russell’s third main heir. In Quine’s works on logic and the foundations of mathematics, there are numerous references to Dedekind’s mathematical results, which
are taken for granted. Yet, Frege is mentioned much more frequently; and Dedekind is usually not engaged as a philosopher.

3 Frege Revivals, Neo-Logicism, and Further Criticisms of Dedekind

Frege was valued highly, as a philosopher, by several of the most influential figures in the analytic tradition, as we just saw. Nevertheless, his writings were not read widely until the 1950s, especially in the English-speaking world. This changed with the publication of several new translations of his works, including J.L. Austin's English rendering of Grundlagen der Arithmetik (1950), and Peter Geach and Max Black's collection, Translations from the Philosophical Writings of Gottlob Frege (1952). Characteristically, work on the latter was strongly supported by both Russell and Wittgenstein. The parallel impact of Carnap and Quine in the U.S. is reflected, among others, in Paul Benacerraf and Hilary Putnam's influential collection, Philosophy of Mathematics: Selected Readings (first published in 1964). It contains substantive excerpts from texts by Frege and Russell, Carnap's article on logicism mentioned above, and several pieces by Quine – but nothing by Dedekind.17

From the 1960s on, the philosopher who contributed most to the revival of Fregean ideas was Michael Dummett. His highly influential book, Frege: Philosophy of Language, was published in 1973, after having circulated in manuscript form earlier. Its author had set himself the task of providing not only an exegesis of Frege's views on logic and language, but also a thorough, more general exploration of Fregean topics. Dummett's book appeared during a period when the philosophy of language was quickly becoming the central sub-discipline of analytic philosophy (partly due to Wittgenstein's, Carnap's, and Quine's influence). Consequently, Dummett's discussion of Frege led to widespread debates about his corresponding views, especially the sense-reference distinction. And even reactions against Frege in that connection, as provided by, for example, Saul Kripke and John Perry, consolidated his status as one of the 'founders' of the analytic tradition.

From early on, Dummett had meant to supplement his first book by another on Frege's philosophy of mathematics; but its publication was long delayed. In Frege: Philosophy of Language, some relevant topics were covered, including questions about abstract objects and identity. Dummett even claimed that it was Frege's work 'which inaugurated the modern period in the philosophy of mathematics' (ibid., p. 656). But it was the writings of one of Dummett's students, Crispin Wright, which led to a revival of Fregean views about mathematics in the 1980s. Crucial here was the publication of Wright's book, Frege's Conception of Numbers as Objects (1983). Its Preface starts as follows:

In the middle and later years of this century Frege's ideas on a wide class of issues in the philosophy of language have assumed a deserved
centrality in the thinking of philosophers interested in that area. Of his philosophy of mathematics, in contrast, it is fair to say that its felt importance to contemporary work remains largely historical (p. ix).

Like Dummett, Wright was not really interested in historical aspects in his book. Instead, he wanted to provide a rational reconstruction of Frege’s approach to mathematics, one that established its continuing relevance (parallel to Frege’s by then classical approach to language). In Wright’s own words, the goal was ‘to revitalize discussion of the questions [in the philosophy of mathematics] to which Frege’s constructive effort was aimed, and of his specific answers’ (ibid., p. x). Crucial for this purpose was to find a way around the problem that seemed to still doom a Fregean approach: its inconsistency.

Building on Dummett’s remarks about abstract objects, identity conditions, and the use of singular terms, Wright went further than him in defending Fregean ‘platonism’ about mathematical objects. He soon found an ally in Bob Hale, whose book, Abstract Objects (1987), added to the defense on the epistemological side. Together they launched an influential ‘neo-logicist’ research program. As that program is well known today, I will not recapitulate its details here. But let me provide reminders about a few core ideas that will be relevant for us. The central technical result – ‘Frege’s Theorem’ – establishes that all of arithmetic can be derived (in second order logic) from ‘Hume’s Principle’:

\[
\#F = \#G \iff F \text{ and } G \text{ can be mapped 1–1 onto each other}
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Frege had formulated this principle but had not treated it as a basic law. Instead, he tried to derive it from his theory of classes (and corresponding definitions). Wright’s new suggestion is to drop that problematic theory and start with Hume’s Principle itself.

This ‘neo-Fregean’ suggestion is attractive because the resulting theory – ‘Frege Arithmetic’ – can be shown to be (relatively) consistent, i.e., not subject to Russell’s antinomy. It can also be generalized by adding other ‘abstraction principles’, e.g., to ground the theory of real numbers. Beyond that, Wright and Hale argued that what results should count a form of logicism. Their argument in the simplest case, that of the natural numbers, is this: Frege Arithmetic relies solely on a principle of numerical identity, encapsulated in Hume’s Principle, that is ‘quasi-definitional’, or in some sense ‘constitutive’, of the concept of cardinal number. The latter view remains controversial, however. The most interesting, but again controversial, aspect for present purposes is that such a neo-logicist approach appears to allow for a proof of the existence of many abstract objects, such as the infinite sequence of natural numbers.

Various aspects of the neo-logicist program have been called into question by now; thus, its philosophical significance remains in doubt. Nevertheless,
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Wight, Hale, and their co-workers clearly succeeded in reviving Fregean questions and answers, or broadly Fregean approaches, in the philosophy of mathematics, which now form an established part of the philosophy of mathematics in the analytic tradition. Besides their aim to rehabilitate Frege, what unites many neo-Fregeans is a critical attitude toward Dedekind. Georg Boolos makes that attitude explicit when he declares:

One of the strangest pieces of argumentation in the history of logic is found in Richard Dedekind’s *Was sind und was sollen die Zahlen?*, where, in the proof of that monograph’s Theorem 66, Dedekind attempts to demonstrate the existence of an infinite system. (Boolos, 1998, p. 202)

Why is Dedekind’s argument so exceedingly ‘strange’? The reason is that it starts with ‘as wildly non-mathematical an idea as his own ego’ (ibid.). Boolos’ remark clearly echoes one of Russell’s criticisms. But it is striking how much less charitable, and more rhetorically charged, his formulation is than Russell’s, even if their final conclusion is similar.

Within the neo-Fregean literature, the most detailed criticism of Dedekind can be found in Michael Dummett’s later book, *Frege: Philosophy of Mathematics*, which finally appeared in 1991. It contains a whole chapter in which Frege’s and Dedekind’s approaches are compared explicitly; and further relevant remarks are sprinkled throughout the text. Dummett is as polemical as Boolos, as we will see. Also like Boolos, he repeats points raised by Russell against Dedekind; but he also adds new criticisms, presented in a ‘Fregean’ spirit. Dummett starts by acknowledging that Dedekind provided valuable contributions to issues Frege barely touched on, with his recursive treatments of addition, multiplication, and exponentiation for the natural numbers. His real sympathies start coming through, however, when he states:

There is indeed a significant contrast between the contemporary but independent work of Frege and Dedekind on the foundations of number theory; the difference could certainly be characterized by saying that Dedekind’s approach was more mathematical in nature, Frege’s more philosophical. (Dummett, 1991, p. 11)

Compared to Dedekind’s works, Dummett characterizes Frege’s *Grundlagen der Arithmetik* – which he praises as his masterpiece – as ‘by far the more philosophically pregnant and perspicacious’. Once again, Frege is valued much higher. But to his credit, Dummett does engage Dedekind as a philosopher in what follows.

What are Dummett’s main criticisms of Dedekind? The first one is familiar by now, from both Russell and Boolos, namely: Dedekind’s alleged proof that infinite systems exist is based on ‘a piece of non-mathematical reasoning’ (p. 48). Dummett’s second major criticism concerns Dedekind’s
view that ‘abstraction’ and ‘free creation’ are crucial for explaining what the natural and real numbers are. However, the objection here is not, along Russellian lines, that this involves a case of ‘theft’. As Dummett admits:

The case [...] is quite different from one in which a mathematician postulates a system of numbers satisfying certain general conditions. Dedekind provided a totality, composed of classes of rationals with which the real numbers could be correlated one to one; he had done all the honest toil required (p. 250).

Instead, Dummett argues that Dedekind’s procedure ‘leads to solipsism’ (ibid.); or at the very least it tempts us, misleadingly, ‘to scrutinize the internal operations of our minds’ (p. 311). Connected with the latter point, Dedekind is again placed in ‘bad company’:

It was virtually an orthodoxy, subscribed to by many philosophers and mathematicians, including Husserl and Cantor, that the mind could, by this means, create an object or system of objects lacking the features abstracted from, but not possessing any others in their place (ibid., p. 50).

To be more precise, Dummett acknowledges that Dedekind’s position is different from Husserl’s and Cantor’s insofar as he speaks not of ‘creating’ individual numbers but whole systems of numbers by ‘abstraction’. Yet, that difference is brushed aside when he concludes: ‘Frege devoted a lengthy section of *Grundlagen*, sections 29–44, to a detailed and conclusive critique of this misbegotten theory’ (ibid.).

As Dummett appeals to Frege as his authority in this context, it is worth pausing for a moment. As pointed out above, Frege does actually *not* voice this objection to Dedekind. It is true that he criticizes his psychologistic-sounding language concerning the notion of ‘system’. But with respect to ‘abstraction’ and ‘creation’, Frege argues instead that Dedekind does not investigate the conditions and limits of his procedure enough, including formulating basic principles for it. This Fregean objection deserves a careful response (more on it below), while the one expressed by Dummett looks more like a ‘criticisms by association’, coupled with dismissive rhetoric. (As we saw, both Frege and Russell also used such strategies at points.) Moreover, Dummett seems far less charitable to Dedekind than Frege, just like Boolos was less charitable than Russell.

A third Dummettian objection, directed at the results of Dedekind’s use of ‘abstraction’ and ‘free creation’, leads us back to Russell as well. It concerns the view that these operations result in objects with only relational or structural properties. After lauding *Principles of Mathematics* as Russell’s ‘great book of 1903’, Dummett points to Russell’s claim that ‘it is impossible that the ordinals should be, as Dedekind suggests, nothing but the terms of such relations as constitute a progression’. He comments sympathetically: ‘Russell
is here obstinately refusing to recognize the role assigned by Dedekind to the process of abstraction’ (ibid., p. 50). Then he adds:

[Dedekind] believed that the magical operation of abstraction can provide us with specific objects having only structural properties: Russell did not understand that belief because, very rightly he had no faith in abstraction thus understood (p. 52).

Why exactly was Russell right in opposing Dedekindian ‘abstraction’; or why can’t there be such objects? Russell’s opposition seemed to be based simply on an ontological prejudice, as noted above. All Dummett has added, so far, is more rhetoric. But to be fair, he then provides a relevant argument (again rooted in Russell’s writings).21

The argument goes like this: Compare the natural number series starting with 0 (as Frege did) and that starting with 1 (as Dedekind did). Clearly, they are different. But conceived of structurally, we seem to loose the difference. Dummett comments:

The number 0 is not differentiated from the number 1 by its position in a progression, otherwise there would be no difference between starting with 0 and starting with 1. That is enough to show that we do not regard the natural numbers as identifiable solely by their positions within the structure comprising them (p. 52).

If this is correct, Dedekindian ‘abstraction’, or corresponding structuralist positions more generally, are simply incoherent. At the same time, Dummett acknowledges:

Mathematicians frequently speak as if they did believe in such an operation. One may speak, for example of ‘the’ five-element non-modular lattice. There are, of course, many non-modular lattices with five elements, all isomorphic to one another; if you ask him which of these he means, he will reply, ‘I was speaking of the abstract five-element non-modular lattice’ (p. 52)

It appears, then, that Dedekind’s and similar approaches accord with mathematical practice. How are we to deal with this recalcitrant fact?

Dummett’s solution takes us back to another Russellian suggestion: ‘[E]ven if [the mathematician] retains a lingering belief in the operation of abstraction, his way of speaking is harmless: he is merely saying what holds good of any five element non-modular lattice’ (ibid.). Later in his book, Dummett returns to this issue. He contrasts the position he sees as implicit in mathematical practice (that a mathematical theory ‘always concerns all systems with a given structure’) with Dedekind’s position (that mathematics ‘relates to abstract structures, distinguished by the fact that their elements have
Frege or Dedekind?

no non-structural properties’). He also notes that the former, labeled ‘hard-headed structuralism’ by him, was ‘misattributed by Russell to Dedekind’. And with another rhetorical flourish, he dismisses the latter as ‘mystical structuralism’ (ibid.).

I have reserved Dummett’s most original argument against Dedekind for last. It leads us back to his initial differentiation, and his corresponding evaluation, of our two thinkers:

Dedekind approach to the question posed in his title [Was sind und was sollen die Zahlen?] differs utterly from Frege’s. [...] Dedekind’s treatment was that of a pure mathematician, whereas Frege was concerned with applications. Dedekind’s central concern was to characterize the abstract structure of the system of natural numbers; what those numbers are used for was for him a secondary matter (p. 47).

Crucial in this passage is seeing Frege as ‘concerned with applications’. For Dummett, this is ‘a leading component of his general philosophy of mathematics’ (p. 61). What is meant by ‘application’ in this context? For the natural numbers, it is their use as ‘cardinal numbers’; for the reals, it is their use as ‘measurement numbers’. Dummett is aware that these are not the only applications of the two number systems; but they are the ‘salient’ ones, those we should take as ‘central to their definitions’ (ibid.). In contrast, for Dedekind the question of application is ‘external, an appendage which could have been omitted without damaging the theory as a whole’ (p. 51).

Dummett’s core point is this: ‘[T]he general principles [of their application] belong to the essence of number, and hence should be made central to the way the numbers are defined or introduced’ (p. 262). That is why Frege’s approach is seen as superior to Dedekind’s.

Actually, for Dummett there is a second point at issue here as well, one that goes back to Frege’s concern about how numbers are ‘given to us’, or about how we can ‘recognize’ and ‘identify’ them. The relevant Fregean question is as follows: Can this be done in purely structural terms? With respect to the real numbers, Dummett answers:

Any system of objects having the mathematical structure of the continuum is capable of the same applications as the real numbers; but, for Frege, only those objects directly defined as being so applicable could be recognized as being the real numbers (p. 61).

And in connection with the natural numbers, he puts the same basic issue thus:

[C]onstitutive of the number 3 is not its position in any progression whatever, or even in some particular progression, nor yet the result of adding 3 to another number, or of multiplying it by 3, but something
more fundamental than any of these: the fact that, if certain objects are counted ‘one, two, three’, or equally, ‘Nought, one, two’, then there are 3 of them (p. 53).

Dummett is convinced that Frege is on the right track in this connection, and also that the issue really matters. In a final swipe at Dedekind, he adds (somewhat condescendingly): ‘The point is so simple that it needs a sophisticated intellect to overlook it’. (ibid.)

4 Dedekind’s Broader Reception and Defenses of Neo-Structuralism

At this point in our discussion, it may appear that Frege’s superiority to Dedekind has been firmly established. His critics are sometimes too polemical, to be sure. Some of their arguments can also be disarmed fairly easily, at least if one reads Dedekind charitably. Still, a whole slew of other arguments remains. Surely, they are decisive, as is natural to assume. In this and the next section, I want to start turning the tables. This will involve considering several very different, much more positive responses to Dedekind’s works. It will also lead to defenses of him against most, even if not all, of the criticisms mentioned so far.

A first point to observe here is that, while Frege has had numerous admirers within analytic philosophy, Dedekind’s reputation has always been high among mathematicians and historians of mathematics – higher than Frege’s, in fact. Dedekind made several lasting contributions to non-foundational parts of mathematics, and his foundational contributions have become firmly entrenched as well. The latter started with the impact his characterization of the natural numbers had on Giuseppe Peano and with the positive reception of his theory of chains by Ernst Schröder; it continued with David Hilbert’s axiomatic approach to geometry, clearly inspired by Dedekind; and it reached a high point in Ernst Zermelo’s and John von Neumann’s generalization of his treatment of mathematical induction in transfinite set theory. In connection with set theory, another detail is noteworthy for present purposes: the way in which Dedekind’s often maligned ‘proof’ of Proposition 66 influenced the form of the axiom of infinity in ZF set theory directly. And one can go on: to model theory (Dedekind’s categoricity result, the idea of non-standard models), basic recursion theory (the focus on recursive functions), and other parts of logic.23

What about philosophy, however, especially in the analytic tradition? A development that is relevant, although somewhat indirectly, is the re-emergence of structuralist positions in the philosophy of mathematics during the last few decades. Two early instances were Paul Benacerraf’s ‘What Numbers Could Not Be’ (1965) and Hilary Putnam’s ‘Mathematics without Foundations’ (1967). But the defense of ‘neo-structuralist’ views really took
on steam in the 1980s, with publications by Michael Resnik, Stuart Shapiro, Geoffrey Hellman, Charles Parsons, and others. One outcome of their joint efforts was the differentiation between two versions of structuralism: ‘eliminative structuralism’, as represented by Hellman (following Putnam), and ‘non-eliminative structuralism’, as represented by Shapiro and others (partly following Benacerraf). People on both sides, while disagreeing in terms of their metaphysical convictions (nominalist or realist), also had a common motivation: dissatisfaction with the set-theoretic approach long dominant in the philosophy of mathematics.

Crucially for present purposes, eliminative and non-eliminative structuralists alike claimed Dedekind as their distinguished forefather. Thus, Hellman writes:

The idea that mathematics is concerned principally with the investigation of structures of various types in complete abstraction from the nature of individual objects making up those structures is not a novel one, and can be traced at least as far back as Dedekind’s classic essay, ‘Was sind und was sollen die Zahlen?’ (Hellman, 1989, p. vii)

In Shapiro’s main presentation of his structuralist position, we can read:

A direct forerunner [of my position] is Dedekind. His development of the notion of continuity and the real numbers in [Stetigkeit und irrationale Zahlen], his presentation of the natural numbers via the notion of Dedekind infinity, in [Was sind und was sollen die Zahlen?], and some of his correspondence constitute a structuralist manifesto’ (Shapiro, 1997, p. 14).

However, neither Hellman nor Shapiro is all that concerned about interpreting Dedekind accurately. The goal is, instead, to develop their respective versions of structuralism systematically. Besides a general a-historical attitude, part of the reason seems to be a deep-seated hesitancy to say more about Dedekind. Why? Because his specific philosophical views, as opposed to his general structuralist approach, are seen as flawed. It appears that the sustained attacks by Frege, Russell, and their followers have left their mark – especially the psychologism charge. This is also why, in debates with neo-Fregeans, it is sometimes Hilbert who is presented as the main alternative to Frege; neo-structuralist positions are then presented more as ‘Hilbertian’ than ‘Dedekindian’.

The degree to which some of Russell’s criticisms influence later perceptions of Dedekind even among neo-structuralists can be illustrated further by two details. First, writers on both side of the structuralism divide, when trying to be charitable to Dedekind, follow Russell in associating a universalist, thus eliminativist, version of structuralism with him (the view that
arithmetic statements are about all progressions). This underlies Hellman’s reference to Dedekind quoted above. Similarly, in Charles Parsons’ recent defense of non-eliminative structuralism (Parsons 2007), Dedekind is used to introduce the eliminative alternative (even though Parsons is careful not to attribute that position to him in the end). Second, already in Benacerraf’s influential article, “What Numbers Could Not Be,” the author echoes Russell in doubting that there can be ‘objects’ with only structural properties; and once more, no further argument is provided. Even a committed ‘realist’ structuralist like Stuart Shapiro tends to call the relevant entities ‘points’ or ‘positions’ in a structure, rather than full-fledged ‘objects’, thus again siding with Russell.26

Having said that, the neo-structuralist literature contains arguments for why one should accept structurally conceived entities as ‘objects’ as well. Thus, Parsons (in the first chapter of his 2007 book) contrast a narrower conception of objecthood, rooted in the paradigm of medium-sized physical objects, with a broader ‘logical conception’ which he traces back to Quine’s and Frege’s works.27 In terms of the latter, anything that is the referent of a singular term in truth-evaluable statements counts, basically, as an object. (A closely related ‘logical’ notion of object is defended by neo-Fregeans like Dummett, Wright, and Hale.) Parsons then connects the second, broader notion of object with his own considerations in favour of non-eliminative structuralism. At least implicitly, he thus provides a defense of Dedekind against this particular Russellian charge. He also brings Fregean and Dedekindian ideas together in a more positive way.

Parsons’ book contains further defenses of a kind of structuralism similar to Dedekind’s against some of the criticisms mentioned above.28 Take Dummett’s argument that, for a structuralist, the number series starting with 0 and that starting with 1 are indistinguishable, and if so, one is unable to differentiate between 0 and 1. Parsons responds as follows: It is true that the structuralist starts with a ‘bare progression’ (i.e., with ‘a’, ‘succ’, and ‘N’ as the only basic symbols); and whether the base element a ‘is’ 0 or 1 is thereby not decided. But things get more determinate once we add one of two ingredients: we introduce addition recursively (thus determining whether a is neutral with respect to it or not); we use initial segments of the progression to measure the cardinality of finite sets (assigning the number n to a set either if it is equinumerous to {a, ..., n–1} or to {a, ..., n}). Nevertheless, is Dummett not right that the initial setup does not determine whether the base element ‘is’ 0 or 1? Parsons’ further response is this: In the kind of structuralism defended by him, the base element just is the base element; what gets added later is whether it plays the role of 0 or 1. As such, the position is entirely coherent.

This line of defense against Dummett’s neo-Fregean criticism of Dedekind can be spelled out further, as follows: The two sides really have different aims. Frege and the neo-Fregeans, with their emphasis on application, try to
analyze the concept of (finite) cardinal number (what Frege called ‘Anzahl’). Structuralists, on the other hand, aim at characterizing one of the most basic mathematical structures, in terms of the closely related notions of ‘simple infinity’, ‘progression’, or ‘model of the Dedekind-Peano axioms’. Considered as such, the two sides are compatible. A structuralist can even admit that the neo-logicist has, perhaps, finally succeeded in analyzing the notion of cardinal number. Nevertheless, for inner-mathematical purposes we can put that notion aside and use a different, more minimal one, namely that of a progression.

Additional defenses of Dedekind against the same Dummettian criticism are possible. Consider Frege and Dedekind on the natural numbers (the case of the reals is parallel). The thrust of Dummett’s argument is that, while for Frege the core application of these numbers is ‘built into’ their very definitions, as it should be, for Dedekind it remains ‘external’. But do the two approaches really differ so sharply in that respect? Dummett relies on the observation that Dedekind, after introducing his natural number structure, adds an explanation for how to apply it to measure the cardinality of finite sets – by using initial segments as tallies, via 1–1 mappability. However, doesn’t Frege’s approach contain a corresponding step? Namely, a class has cardinality \( n \) if it is contained in \( n \) as an element. Why is Dedekind’s account of application ‘external’, while Frege’s isn’t? The only reason would seem to be that the element relation is privileged over the 1–1 mappability relation. But what is the justification for that?29

Perhaps the most basic defense of Dedekind against the same charge is the following: Dummett, like his neo-logicist followers, focuses on certain ‘salient’ applications of the natural and real numbers. They are seen as essential, as what needs to be built into their very definitions. However, both the natural and the real numbers have a variety of different applications. Why single out some of them? Once again, it is hard to see what a (principled and non-question-begging) reason would be. Indeed, if pushed just one step further, this line of thought can be turned into an argument in favour of Dedekind over Frege. Dummett himself remarks in the case of the reals (see above): ‘Any system of objects having the mathematical structure of the continuum is capable of the same applications as the real numbers’. If so, it is arguably an advantage of Dedekind’s approach that it focuses squarely on the ‘structure’. All applications can then be studied based on it; no detour through some supposedly privileged one is necessary.30

5 More Defenses of Dedekind, while Broadening the Horizon Further

As we saw, neo-structuralists tend to be reluctant to defend Dedekind himself, while offering rebuttals to attacks on related versions of
structuralism. Are there any more direct defenses of Dedekind in the current literature? Yes, there are. But before turning to them, let me mention some ‘pro-Dedekind’ considerations that have been around longer, although we have to go beyond the analytic tradition to find them. The treatment of Dedekind by the neo-Kantian philosopher Ernst Cassirer provides a rich source for such considerations.

Already in one of Cassirer’s earliest publications, the long article ‘Kant und die moderne Mathematik’ (1907), he displays a good appreciation, not only of Dedekind’s technical results, but of his philosophical views as well. In Cassirer’s book, Substanzbegriff und Funktionsbegriff (1910), he goes further, by arguing explicitly for the superiority of Dedekind’s approach over Frege’s and Russell’s. And in Cassirer’s later writings, related themes and further refinements appear. Three aspects of Cassirer’s Dedekind reception are particularly noteworthy for us: his characterization of Dedekind’s structuralist conception of mathematical objects; his defense of that conception against Russellian and Fregean criticisms; and his historically grounded argument that Dedekind’s approach represents the culmination of a long development within mathematical science.31

With respect to Dedekind’s basic approach to the natural numbers, Cassirer writes:

[Dedekind showed that] in order to provide a foundation for the whole of arithmetic, it is sufficient to define the number series simply as the succession of elements related to each other by means of a certain order – thereby thinking of the individual numbers, not as ‘pluralities of units’, but as characterized merely by the ‘position’ they occupy within the whole series (Cassirer, 1907, p. 46, my translation).

Similarly concerning the reals, or more specifically, the irrational numbers:

We thus see that, to get to the concept of irrational number, we do not need to consider the intuitive geometric relationships of magnitude, but can reach this goal entirely within the arithmetic realm. A number, considered purely as part of an ordered system, consists of nothing more than a ‘position’ (p. 49, my translation).

In these passages, Cassirer characterizes Dedekind’s structuralist position very aptly (as a form of non-eliminative structuralism, to use current terminology). He also brings it into relief against earlier positions (the ‘pluralities of units’ view for the natural numbers, the appeal to ‘intuitive geometric relationships of magnitude’ for the real numbers). He thus places Dedekind’s views into a certain historical context: the arithmetization of analysis and, more generally, the rise of ‘pure mathematics’ in the nineteenth century.32
Not only has Cassirer, in contrast to Russell, no problem in grasping Dedekind’s structuralism, he also responds directly to one of Russell’s corresponding objections:

If the ordinal numbers are to be anything, then they must – so it seems – have an ‘inner’ nature and character; they must be distinguished from other entities by some absolute ‘mark’, the same way in which points are different from instants, or tones from colors. But this objection mistakes the real aim and tendency of Dedekind’s formation of concepts. What is at issue is just this: that there is a system of ideal objects whose content is exhausted in their mutual relations. The ‘essence’ of the numbers consists in nothing more than their position. (Cassirer, 1910, p. 39)

What is it that underlies Russell’s and other critics’ resistance to a structuralist conception of mathematical objects? Cassirer also has an answer to that question, one that partly anticipates Parsons. The answer is that the critics have not let go of an old (Aristotelian) conception of object based on the notion of ‘substance’, while what is really needed in modern mathematics is a broader and ‘function-based’ conception. For Cassirer, the ‘real aim and tendency’ of Dedekind’s work is precisely to provide the latter. Moreover, this is the main respect in which his approach is superior to Frege’s and Russell’s.

Concerning Dedekind’s notion of ‘abstraction’ – and as a defense against the charge that it involves a form of psychologism – Cassirer observes this:

[Dedekind’s form of abstraction] means logical concentration on the relational system, while rejecting all psychological accompaniments that may force themselves into the subjective stream of consciousness, which form no constitutive moment of this system (Cassirer, 1910, p. 39).

The phrase ‘logical concentration’ in this passage indicates that Cassirer interprets Dedekindian abstraction as a logical procedure, not a psychological process. As such, it is quite different from the kind of ‘abstraction’ dominant from Aristotle through the British Empiricists to Mill and others in the nineteenth century, as Cassirer also notes. Moreover, he connects this point to the ‘givenness’ and the ‘existence’ of numbers: ‘Givenness can here [...] mean nothing other than complete logical determinateness’ (Cassirer, 1907, p. 49, my translation); and ‘the “existence” of [a number] in Dedekind’s sense is not intended to mean more than such determinateness: its “being” consists simply in its function of marking a [...] position’ (ibid., fn. 26, my translation).

One especially valuable feature of Cassirer’s reception of Dedekind is, once more, that he puts his structuralist position in the context of broader changes in nineteenth-century mathematics. With views like Dedekind’s,
‘mathematics is no longer – as it was thought of for centuries – the science of quantity and number, but henceforth encompasses all contents for which complete law-like determinateness and continuous deductive inner-connection is achievable’ (Cassirer, 1907, p. 40, my translation). Even more broadly:

Here we encounter for the first time a general procedure that is of decisive significance for the whole formation of mathematical concepts. Wherever a system of conditions is given that can be realized in different contents, we can hold on to the form of the system as an invariant, putting aside the difference of contents, and develop its laws deductively. In this way we produce a new ‘objective’ formation whose structure is independent of all arbitrariness [...]. (Cassirer, 1910, pp. 40)

According to Cassirer, both Frege and Russell made lasting contributions to the development at issue, by spelling out the logical frameworks in which the laws for various systems can be ‘developed deductively’. But it was Dedekind who pushed our understanding of ‘mathematical concepts’ in a structuralist direction.

Cassirer also weights in on whether a Fregean and Russellian conception of numbers, as cardinal numbers, is superior to a Dedekindian conception, as ordinal numbers. His main argument in this connection is contained in the following remark:

[I]t becomes evident that the system of the numbers as pure ordinal numbers can be derived immediately and without circuitous route through the concept of class [...]. The theory of the ordinal numbers thus represents the essential minimum that no logical deduction of the concept of number can avoid (Cassirer, 1910, p. 53, translation modified).

For Cassirer the question of superiority has, at bottom, to do with which approach – the Frege-Russell approach or Dedekind’s – is more in line with the development and the desiderata of modern mathematics. And insofar as Dedekind aims at distilling out the ‘essential minimum’ required for doing arithmetic, his procedure is preferable.

One core part of Cassirer’s reading of Dedekind is, as we just saw, to shift the focus of discussion to ‘logical determinateness’. By doing that, Dedekind’s position becomes defensible against the charge of psychologism. This also clarifies the sense in which it might be seen as a form of ‘logicism’ (pace Russell, Carnap, and others, but in line with Schroeder, Peirce, and Hilbert).33 Having said that, Cassirer does not help much with respect to spelling Dedekind’s logicism out more precisely, i.e., what we should take its basic laws to be. In other words, Frege’s corresponding criticism is not addressed. Nor does Cassirer provide any grounds for rebutting the standard criticisms
of Dedekind’s Proposition 66 (from Russell to Boolos and Dummett). While his take on Dedekind is subtle and rich in a number of respects, it is limited and incomplete in others.

Cassirer’s reception of Dedekind is interesting in itself. It also anticipated, by several decades, two important developments in current history and philosophy of mathematics. First, Dedekind’s role in the rise of modern mathematics has been elaborated further along historical lines. Second, Dedekindian ‘abstraction’ and the resulting structuralist conception have been defended more philosophically. Concerning the former, several recent investigations of the transformation of mathematics in the nineteenth century mention Dedekind very prominently. This recognition is also reflected in new anthologies on the history of the philosophy of mathematics, e.g., William Ewald’s collection, From Kant to Hilbert: A Sourcebook in the Foundations of Mathematics, Volumes I and II (1996). And it is in line with the turn to ‘mathematical practice’ in the philosophy of mathematics during the last decade or so. But these are all aspects too big to be pursued further here; I can only point to them in the present essay.

For a recent, direct defense of Dedekind’s notion of abstraction, developed in reaction to Dummett’s work, we can turn to W.W. Tait’s article, ‘Frege versus Cantor and Dedekind: On the Concept of Number’ (1997). Independently of Cassirer, Tait argues that ‘Dedekind abstraction’, as he calls it, has a logical core; and consequently, it is misinterpreted along psychologistic lines. The specifics of the structuralist conception of mathematical objects that results from it is further developed in my own article, ‘Dedekind’s Structuralism: An Interpretation and Partial Defense’ (2003). The case for Dedekind’s continuing relevance has thus been carried into current analytic philosophy.

My considerations in this and the previous section are not meant to show that Dedekind is immune to every criticism raised against him. The dialectical situation is surely more complex than that, thus requiring further attention. But what such considerations do establish, I would submit, is that some of the critics’ polemics against Dedekind – as relying on ‘one of the strangest pieces of argumentation in the history of logic’ (Boolos), or as holding ‘mystical’ views (Dummett) – are uncharitable, superficial, and inadequate in themselves. They also indicate that the case for thinking of a Dedekindian approach as superior to a Fregean, at least in some respects, is not as hopeless as one might have thought. But let me close my discussion with a more conciliatory gesture. It brings us back to the criticism of Dedekind’s approach perhaps voiced most often.

Dedekind’s ‘proof’ of the existence of an infinite set, in Proposition 66 of his 1888 essay, has been challenged in a variety of ways (as relying on an inconsistent theory of classes, as bringing in nonlogical, non-mathematical, or even psychologistic assumptions). Now, his position does, indeed, depend on securing such existence. If Dedekind’s original approach doesn’t
work, how else could we proceed? The second half of a remark by Russell in *Principles*, which we already encountered, provides a hint:

There seems, in fact, to be nothing to choose, as regards logical priority, between ordinals and cardinals, except that the existence of the ordinals is inferred from the series of the cardinals. (Russell, 1903, p. 241)

As noted before, Russell still assumes at this point that his original construction of ‘cardinals’, in terms of equivalence classes, can be made to work. Russell’s antinomy undermined that hope, as he then realized. His own remedy – the construction of cardinals in his ramified theory of types – leaves us with a different problem: the reliance on a non-logical assumption, i.e., (his version of) the axiom of infinity. Yet, with the advent of neo-logicism, an alternative has become available.

Recall that the core of the neo-logicist approach to the natural numbers is Frege’s Theorem: the fact that we can prove the existence of an actual infinity based on Hume’s Principle alone (in second-order logic). This result is sometimes presented as a decisive advantage of neo-logicism over other approaches, including Dedekind’s. But could we not see neo-logicism and Dedekindian structuralism as compatible, even complementary? What I have in mind is this: Why not adopt Frege’s Theorem for establishing the existence of a simple infinity, and then add a structuralist notion of ‘abstraction’ to it? In other words, why not combine ‘Frege abstraction’ and ‘Dedekind abstraction’, also more generally? One objection might be that, once the neo-Fregean approach is available, the structuralist side becomes superfluous. But we have encountered a response to that already: Dedekind abstraction distills out the ‘essential minimum’ needed for arithmetic; similarly for analysis. It thus provides an additional, distinctive benefit.

6 Concluding Assessments

This essay started with the observation that Frege and Dedekind pursued closely related projects: to provide logicist foundations for the natural and real numbers. Frege acknowledged that fact but also formulated various criticisms of Dedekind. The chapter then traced the later reception of both Frege’s and Dedekind’s works. Frege’s philosophical views were valued much more highly by several influential early analytic philosophers; they also experienced a remarkable revival since the 1950s. Even Frege’s approach to the philosophy of mathematics has been resurrected since the 1980s. The reception of Dedekind was more negative. Outside of analytic philosophy, there were exceptions; and currently a general revival of interest in him is underway. One of my overall goals was to expose these trends and to provide clarification about what is at issue in them.

Several of Frege’s main works contain sustained philosophical discussions, e.g., *Die Grundlagen der Arithmetik*. Frege also contributed to areas
beyond the philosophy of mathematics, especially to the philosophy of language and mind. In contrast, Dedekind’s publications were all in mathematics, including the foundations of mathematics, and the philosophical remarks in them are sparse. There is no mystery, then, why Frege received more attention within philosophy. Still, the relative neglect or continued dismissal of Dedekindian ideas in analytic philosophy is striking. I argued that it is rooted in the strong influence of Frege and Russell, whose criticisms of Dedekind keep getting recycled by their followers. Another of my goals was to reveal the relations between such criticisms, thus reassessing their appropriateness and relative weight.

While a barrage of attacks has been directed at Dedekind’s philosophical views over the years, some defenses have been forthcoming as well. This started in Cassirer’s writings from early in the twentieth century, which themselves are beginning to be rediscovered. But philosophers in the analytic tradition are now providing defenses of Dedekind as well, more or less directly. A third goal of mine was to collect these defenses, thus making them available more widely and juxtaposing them with the attacks. In addition, I put a suggestion for how to reconcile ‘Frege abstraction’ and ‘Dedekind abstraction’ on the table, thus calling into question the frequently assumed opposition between neo-logicism and neo-structuralism. While the proposals and arguments involved have not been analyzed in conclusive detail in this chapter, it should be somewhat more plausible now that not only Frege’s but also Dedekind’s approach can be partly rehabilitated. Many subtle questions and open problems remain, of course, in both cases.

A final point: Compared to the tendency of favouring Frege over Dedekind in the analytic tradition, Cassirer’s assessment exemplifies a striking reversal. His opposite evaluation – his arguments that Dedekind’s position is superior to Frege’s and Russell’s – is based on a combination of historical and philosophical considerations. In current philosophy of mathematics, the historical dimension is increasingly taken seriously as well, also with respect to its own history. This development is contrary to the a-historical, or even anti-historical, self-image that has long shaped analytic philosophy. The result should, over time, be a more balanced view. The present essay is offered as a step toward such balance. Insofar as it is successful, this means: Not only can Frege’s and Dedekind’s achievements be acknowledged more accurately now, also in relation to each other; the ways in which they played a role in the development of analytic philosophy becomes clearer as well, thus their philosophical legacies more transparent.

Notes

1. Often, I will refer to Frege’s and Dedekind’s writings via their original titles and publication dates. In quotations, I will use references to their standard translations; cf. the bibliography.

3. For a survey of Dedekind's contributions to mathematics, concerning both foundational and non-foundational areas (as well as their relationship), cf. Reck (2008).


6. There is one possible exception to this lack of influence. Namely, Frege's adoption of a theory of classes as central to his project, in *Grundgesetze*, may reflect reading Dedekind (1888); cf. Sundholm (2001), p. 61.


8. In a letter to the teacher Keferstein (Dedekind 1890), Dedekind makes clear that Proposition 66 is precisely supposed to play the role of insuring consistency (via satisfiability); cf. Reck (2003).

9. For more on this point, including some necessary refinements, cf. Reck (2003). Frege might respond: This is not an explicit part of Dedekind's theory; it is not clear how to formulate it generally as a law; and could it be regarded as a logical law then? Thus more is required for a full defense of Dedekind here.

10. For Dedekind's side—less often discussed than Frege's—cf. Sieg & Schlimm (2005), p. 121.

11. For Frege's response to the antinomy, cf. Reck (2005); for Dedekind's, see Reck (2003), fn. 9.


13. One has to be careful, however, as the term 'formalism' is ambiguous. For more on the Dedekind–Hilbert connection, cf. Reck (2003), Sieg & Schlimm (2005), and Ferreirós (2009).


15. In Reck & Price (2000), the view Russell attributes to Dedekind is called 'universalist structuralism'. For more on why Russell's misses his mark in this connection, cf. again Reck (2003).

16. What seems to be going on here is that Russell, still early in his career, accepts something close to Meinong's distinction between 'being' (of entities) and 'existence' (of objects).

17. One partial exception to this trend is Van Heijenoort (1967), which contains one letter by Dedekind (and one to him by Cantor. However, even here the Frege part is much longer and more substantive.


19. $F$ and $G$ are supposed to be (sortal) concepts, and '#Fs' means 'the number of Fs'.

20. This aspect is highlighted in Demopoulos & Clarke (2007); but compare Antonelli (2010).
22. In Wright’s and Hale’s later work, this is called ‘Frege’s Principle’; cf. Wright (1997).
27. Discussions of such a logical notion of object are already central to Parsons (1983).
28. Parsons’ version of structuralism is not identical with Dedekind’s version but close to it. I intend to explore the similarities and differences further in a future publication.
29. This is a central part of the response to Dummett’s criticisms of Dedekind in Tait (1997). Note also that the notion of 1–1 mappability is built centrally into Frege’s approach as well.
30. For the argument that, from a mathematical point of view, no application of the natural and the real number system should be seen as privileged, cf. Stein (1987).
31. My discussion of Cassirer in this essay will have to be very brief and sketchy. I plan to address his relation to Dedekind more fully in future publications; cf. also Heis (2011).
32. For more on the ‘pluralities of units’ and ‘magnitudes’ views, including Frege’s and Dedekind’s reactions to them, cf. Reck (2005); concerning the rise of ‘pure mathematics’, cf. Ferreirós (2007).
33. In some recent surveys, such as Demopoulos & Clarke (2007), Dedekind is acknowledged again as a main logicist; but reservations often remain; cf Ferreirós (forthcoming).
36. Cf. again Demopoulos & Clarke (2007), as well as the references in it.
37. This suggestion can also be found in Simons (1998).
38. Some neo-logicists have expressed sympathy for this kind of approach in the case of the real numbers; cf. ‘Wright (1997); but see Hale (2001) for a less conciliatory, more austere attitude.
39. I am grateful to Jeremy Heis, Ansten Klev, and Clinton Tolley for comments on an earlier version of this essay. All the remaining mistakes and other problems should, as usual, be attributed to me.

References


Psychology, Epistemology, and the Problem of the External World: Russell and Before

Gary Hatfield

The epistemology and metaphysics of perception were central topics in early analytic philosophy. These topics are best known through the manifold discussions of sense data, sensibilia, and sensory qualities during the early decades of the twentieth century, which continued into the second half of the century (as in Swartz, 1965). In connection with the status of sense data, epistemological questions arose concerning the fallibility and directness of perception. Taking the case of vision, which was primary, these questions concerned whether we are directly acquainted with the objects of vision – whatever they may be – or instead apprehend an extra-mental world through the mediation of sensations or sense data, as in a representative theory of perception. They also concerned the nature of the mental act through which we know perceptual objects. A run through the pages of Mind, the Proceedings of the Aristotelian Society, and Journal of Philosophy from 1900 to 1920 reveals that perceptual acts and their objects were central concerns of anglophone philosophy, engaging Moore and Russell, as well as other noted figures, including Samuel Alexander (1909–10), George Dawes Hicks (1912), and A. O. Lovejoy (1913).

In a recent study, Omar Nasim (2008) has enriched the context for understanding these discussions by examining an interchange that he calls ‘The Controversy’, which was initiated by the Cambridge psychologist–philosopher G.F. Stout. In 1904, Stout published ‘Primary and Secondary Qualities’ in Proceedings of the Aristotelian Society. Subsequent discussions by Dawes Hicks (1912), Alexander (1909–10), and T.P. Nunn (1909–10) were not limited to epistemological concerns regarding knowledge of external objects (although these were present). They examined the immediate object of perception and asked whether secondary qualities are mere subjective experiences or are, in one sense or another, qualities of physical objects. As to the mental acts by which objects and qualities are perceived, all agreed in distinguishing the act of sensing from the thing sensed, although they disagreed on how and when this distinction comes to self-consciousness (Dawes Hicks, 1907–08) and over its ontological implications. Indeed, the distinction between act and object (or
content) allowed for a variety of metaphysical claims about the nature of the immediate object. Stout agreed with Alexius Meinong in classifying that object as a mental existent, distinct from the act by which it is apprehended. Others took the immediate object to be a mind-distinct sense datum (Moore and Russell at various times), and others (such as Alexander) held that the mental act of sensing is directed upon ordinary physical things without mediation.

Nasim seeks to widen the context for understanding Russell’s ‘constructive’ approach to bodies in the external world. In particular, he would reduce the importance of British Empiricism (from Locke through Mill) for Russell’s project of constructing the external world from sense data and give greater attention to the Austrian philosophers Franz Brentano and Meinong, in part mediated by Stout. The latter authors discussed the relation between the act of sensing and its object or content, a relation that was central to the Controversy and was taken up by Russell himself. Nasim plausibly relates discussions of this relation to the reception of Brentano and Meinong in British thought at the turn of the century, although he understates the role of James Ward in this reception and bypasses discussions of the relation in earlier British figures, such as Hamilton and Mill.

Nasim’s book appears in the same series as the present work. The series encourages history of analytic philosophy that looks deeply and broadly into the philosophical context of well-known figures such as Moore and Russell while also revealing the importance of lesser-known figures and of larger frameworks of thought. In this spirit, I aim to broaden again, beyond the Controversy, the context for understanding Russell’s middle period (after 1911) and particularly his appreciation of the relevance of psychology for the theory of knowledge. Russell wrote in 1914 that ‘the epistemological order of deduction includes both logical and psychological considerations’ (1914b [1992, p. 50]). Indeed, the notion of what is ‘psychologically derivative’ played a crucial role in his epistemological analysis (1914c, pp. 69–70). His epistemological discussions engage psychological factors in the perception of external objects that had been closely examined in the nineteenth century, among other places in J.S. Mill’s response to William Hamilton’s conception of knowledge of the external world. These considerations came to Russell in various ways, including his contact with British Empiricism (Mill, with Berkeley and Hume as background), his engagement with Austrian philosophy, his close acquaintance with the philosophy and psychology of William James, and his exposure to the Controversy and to contemporary British writings concerning the problem of the external world. The latter writings, which precede and subsume the Controversy, provide a crucial context within which natural scientific psychology came to be seen as distinct from epistemology and yet as relevant to its concerns.

In widening the context for understanding Russell, I survey aspects of the history of philosophy in relation to psychology in the later nineteenth
century, especially in connection with the problem of the external world as found in British philosophy.

**Historical Accounts of Philosophy and Psychology in Opposition**

From the middle decades of the twentieth century, two complementary stories have described the history of psychology in relation to philosophy. Both tell of a separation of psychology from philosophy that was initiated in the latter decades of the nineteenth century and completed in the first decades of the twentieth. In each story, philosophy and psychology took on their modern identities by establishing distinct methods and problems and by enforcing strict disciplinary boundaries that entailed the mutual irrelevance of these fields to one another.

The story for psychology was set down by E.G. Boring (1929). Although recent scholarship variously challenges Boring’s work, the overall shape of his picture has exhibited remarkable staying power (e.g., as in Kusch, 1995). As Boring tells it, scientific, experimental psychology emerges from philosophical discussion of mental faculties and phenomena when methods from physics and sensory physiology are brought to bear. He locates the ‘founding of experimental psychology’ in the psychophysics and sensory physiology of Fechner, Helmholtz, and Wundt. Accordingly, Brentano and Meinong, as well as Ward and Stout, are continuers of a philosophical tradition that influenced but was not a part of ‘real’ psychology.

In Boring’s story, ‘real psychology’ is experimental psychology. In fact, historical study (O’Donnell, 1979) suggests that Boring originally composed his book in order to consolidate the discipline of psychology as experimental psychology, in opposition to encroachments from applied branches, such as clinical or testing psychology, and with full separation from philosophy. In his view, no one really had been doing psychology until experimental techniques from physics and sensory physiology were applied to questions about sense perception and cognition that had been raised by philosophers, who could not address them adequately from their armchairs. The experimentalists rose from the armchair and entered the laboratory, thereby creating psychology as it must be: an experimental science. As it happens, academic psychology today is largely defined as experimental psychology (with clinical and applied branches). Boring’s 1929 history was victor’s history before the fact, or to facilitate the fact.

In the history of philosophy, Passmore (1957) has characterized general trends in late nineteenth- and early twentieth-century philosophy. As he sees it, in the second half of the nineteenth century, philosophers focused on the ‘theory of inference’ (Passmore, 1957, p. 174). They were skeptical of formal logic and rather viewed logic as a discipline that should examine ‘the human activity of inferring’, that is, the mental processes by which thinkers
come to new cognitive results. Passmore (1957, Chs. 1, 7) associates this attitude with Mill, Bradley, Bosanquet, and Dewey, among others, and finds it expressed earlier in Descartes, Locke, and Hume. Accordingly, philosophy pursues the psychogenesis of beliefs and attitudes, including the belief in the external world. This approach was reinforced by the tremendous growth of ‘genetic’ sciences – viz., psychology, biology, and anthropology – in the latter part of the nineteenth century. These sciences are ‘genetic’ through their interest in the causal origins and development of, respectively, thought and feeling, life, and human culture.

In the first decades of the twentieth century, Passmore finds a ‘movement towards objectivity’ (1957, Chapter 8). Those moving in this direction hold that logic is concerned with implication rather than inferences, which is to say that they focus on ‘the formal relationship of implying’ rather than the activity of inferring. From this point of view, ‘we shall reject as “psychologism” all reference to the processes of inquiry’ (1957, p. 174). Meinong and Husserl (after his early period) belong to one stream that takes this direction. The other stream involves some main figures in early analytic philosophy: Frege, Russell, the early Wittgenstein, and the logical positivists. In either case, philosophers should eschew any concern with actual psychological or mental processes and states. To engage such concerns is to commit the fallacy of psychologism: the attempt to settle epistemological or logical questions through the empirical study of the mind and its states and processes.

According to Boring’s story, real psychology separated from philosophy in the latter decades of the nineteenth century, and good riddance. According to Passmore, analytic philosophy (just as Husserl’s pure phenomenology) turns away from psychology, and good riddance again. Many historians of analytic philosophy have come to a similar conclusion. Studies of Russell’s philosophy, for instance, often give little attention to his connections with James, Ward, and Stout, and may offer only brief notice of his conception, in 1913–14 and after, of the role of psychology in philosophy.¹

Focusing on Russell, I want to contextualize his relation to psychology. It is well known that Ward and Stout were among his tutors, and they were psychologists as well as philosophers. Within a wider context, going back to Hamilton and Mill, the relations between philosophy and psychology were in flux, as were the disciplinary identities of the two fields (Hatfield, 2010). Hamilton enlarged the domain of ‘psychology’ (so-called) to include epistemology, while also indicating that questions of justification could not be addressed through mere empirical description of psychological processes. Subsequent to Hamilton, logic and epistemology were increasingly seen as distinct from psychology. Even so, and without denying the strong anti-psychologistic currents in early and middle analytic philosophy, the relevance of psychology to epistemology was by no means settled by Russell’s middle period. Indeed, many of Russell’s predecessors and contemporaries believed
both that epistemology is distinct from natural scientific psychology and that the findings of the latter are relevant to epistemology. Accordingly, we should not simply read current conceptions of ‘psychology’ and ‘epistemology’ (or indeed of ‘psychologism’) into these earlier periods, or suppose that these fields were teleologically tending toward one’s favourite current conception of them. We must attend to the actors’ own conceptions of these disciplines.

Philosophy and Psychology in Flux

When the term ‘psychology’ was introduced in the sixteenth century, it described the science of the soul stemming from Aristotle. In the Aristotelian discipline, study of the soul (psyche, anima) was a part of physics or the study of nature in general. The Aristotelian soul included not only rational, sensitive, and motor powers, but also vegetative powers (growth, nutrition, reproduction). Viewed retrospectively, ancient and medieval psychology comprised both psychological and biological phenomena. Nonetheless, the cognitive functions dominated ‘psychological’ discussion, so-called, into the eighteenth century, when Christian Wolff explicitly narrowed the term to include only the sensitive, appetitive, and rational functions (Hatfield, 1995).

In Britain, the term ‘psychology’ was slow on the uptake, although it occurs in Hartley’s well-known work of 1749 as denoting the theory ‘of the human mind’ and ‘of the intellectual principles of brute animals’ (1749, p. 354). Hartley classified psychology as a division of natural philosophy, along with mechanics. In eighteenth-century Britain, mental phenomena were studied under various headings, including the ‘theory’ or ‘science’ of mind in natural philosophy and, more commonly in Scotland, as a division of ‘moral philosophy’ or the ‘science of man’ (human beings).

During the seventeenth and eighteenth centuries, the mind and its capacities were invoked in writings that do not properly belong to the psychological traditions mentioned thus far. Descartes, Spinoza, Leibniz, and Kant used mentalistic language in their analyses of human knowledge, but they did not consider their primary accounts of knowledge to be ‘psychological’ – whether that term is taken with its early meaning as the natural philosophy of the powers of the soul, or with a later meaning, as the empirical investigation of mental phenomena. Descartes’ appeal to intellectual perception as a basis for knowledge was no appeal to empirical psychology. When Descartes invoked his clear and distinct intellectual perception that mind and matter (thought and extension) are distinct, he was not merely introspecting. He was appealing to the (allegedly) truth-certifying character of some of his mental acts – those involving clear and distinct intellectual perception of the essences of things. To categorize Descartes’ philosophy as ‘psychologistic’, as Passmore implicitly suggests, is simply to suppose that because
his notion of intellectual perception no longer is deemed plausible, he must have been doing psychology by default (Hatfield, 1997). In Descartes’ metaphysical works, we find a concern with the mental that we should retrospectively place under the normative discipline of epistemology rather than under psychology conceived as a natural science.

Kant explicitly distinguished his transcendental investigation of the knowing capacities from what he called ‘empirical psychology’. His analysis of space as a form of intuition was not, by his lights, an exercise in empirical psychology, but a regression to the elements of knowledge, or to the conditions that make knowledge possible. Notoriously, these investigations discovered those conditions to consist in cognitive structures and activities, including forms of intuition, categories of the understanding, and synthoses in thought. He distinguished these transcendental conditions from empirical laws, such as those of association. Kant further asserted that empirical psychology is irrelevant to logic, whether the ‘transcendental logic’ of his critical philosophy or the ‘pure general logic’ which concerns the ‘form of thought’ (1781, p. 54; 1787, p. 78) or the ‘formal rules of all thought’ (1787, p. ix). In effect, Kant created the charge of a ‘psychologistic’ fallacy before the invention of the term. His transcendental philosophy concerns the basis for the cognitive validity of thought. As a description of the conditions for thought itself, transcendental philosophy constrains any possible empirical psychology of the natural laws of mind (Hatfield, 1990, p. 101; 1992).

Kant’s distinction between empirical investigation of the mind and discernment of the conditions for knowledge occurs in only some of his eighteenth-century contemporaries and nineteenth-century followers. In Germany, many authors of logics, including the prominent metaphysician Hermann Lotze, adopted this distinction, as did Hamilton in Scotland. But others took another path, according to which the normative powers of the mind are subject to empirical investigation modeled on the observational basis of natural philosophy. Two such were Hume and Reid.

Hume (1739–40, Intro.) and Reid (1785b, Chapter 1, Section 1) each described their investigations into human knowledge as the application of observational techniques like those in physical science to a new domain: the mental. Their resulting theories of mind differed. Hume resolved mental phenomena into least elements, perceptions and feelings as characterized by a quality with an intensity. He posited minimal perceptual and appetitive capacities, such as perception of sameness or difference and appetite for pleasure and away from pain, and sought to construct mental life by applying the laws of association to presumed or observed regularities among elemental perceptions. By contrast, Reidian psychology posited innate mental powers, innate perceptions, and innate concepts, including a conception of and a natural belief in the existence of an external world of extended matter.
Hume and Reid each addressed the difficulties in observing mental states and processes. Both affirmed that the adult mind is filled with habits that may mask its elemental contents. Reid maintained that adult mental experience is shot through with accretions of belief and memory associations from past experience, so that we cannot tell what is basic and what acquired (1785b, pp. 7–11; 1785a, pp. 59–64). It requires the reflective attitude of a conscientious investigator such as himself properly to describe sensations and perceptions and to uncover the ‘natural’ beliefs inhering in the mind, such as the belief in an external world. The common sense of the ‘vulgar’ can help, although it is not completely authoritative (e.g., the vulgar do not naturally hit upon the distinction between primary and secondary qualities, which Reid held to be fundamental: 1785b, pp. 179, 195; 1785a, p. 241). Hume was more optimistic that the elementary contents of the mind can be isolated introspectively. And he held that once we are attending to simple perceptions, their properties are infallibly known. Nonetheless, the operation of custom and habit on the imagination may generate a belief in the distinct and continued existence of bodies (independent of our perception of them), even though strict attention to our impressions and ideas would reveal this belief as a delusion (1739–40, 1.2.6, 1.4.2).

In the Scottish universities, the science of mind became a fixed part of the curriculum. Scottish authors who wrote treatises on the mind tended to hold university appointments. These included Reid at Glasgow and Dugald Stewart at Edinburgh, both appointed as professors of Moral Philosophy, as was Stewart’s successor at Edinburgh, Thomas Brown, who lectured on the theory of mind (1820).

In 1836, William Hamilton was appointed to the chair in Logic and Metaphysics at Edinburgh. As befit his title, he delivered two series of lectures, the first on Metaphysics and the second on Logic. The first was the general basis of philosophy, which he equated with what he called ‘psychology’. Whereas Brown viewed psychology as a branch of natural science, Hamilton deemed psychology to be the core of philosophy itself. He refused to call physics ‘natural philosophy’, since in his view the term ‘philosophy’ should be limited to the sciences of the mind (1861, 1: 63).

Although Hamilton aligned ‘philosophy proper’ with the ‘science of mind’ (1861, 1: 1), this science cannot simply be identified with a precursor to experimental psychology or even with empirical psychology as understood in Hamilton’s day. It had various special branches, including logic, ethics, politics, and fine arts, and some connection to theology. In Hamilton’s terms, the philosophical core of these disciplines is psychology, which itself divides into three branches: ‘empirical psychology’, or the ‘phænom-enology of mind’, which observes and classifies the phenomena of mind; ‘nomological psychology’, or the study of the laws of our mental faculties, both descriptive and normative; and ‘inferential psychology’, also known as ‘ontology’ or ‘metaphysics proper’. The first branch divides the phenomena
of mind into cognition, feeling, and the conative powers (will and desire); the second finds the laws of each division; the third considers the being of God and the existence and immortality of the soul. Hamilton's *Lectures on Metaphysics* considered cognition and its empirical laws, with a briefer look at feeling. He left to logic the elaboration of the formal laws of thought, considered (in Kantian mode) apart from any subject matter and as independent from empirical psychology (1866, 1: 17–25).³

Consciousness and introspection were the fonts of Hamilton's philosophy. He equated consciousness with 'the general faculty of thought' and considered it to be 'the only instrument and only source of philosophy' (1861, 1: 375). By contrast with Scottish predecessors such as Reid and Stewart, he maintained that consciousness is not a special faculty but is the root function of the mind, of which all other faculties (cognition, feeling, conation) are modifications. A significant portion of his *Metaphysics* concerns the conditions and deliverances of consciousness. Hamilton believed that it would be generally accepted that consciousness is 'an actual and living, and not a potential or dormant knowledge'; that it is immediate knowledge; that it involves a discrimination or contrast that makes a conscious state be consciousness of one thing rather than another; that it involves a judgment, in which something is affirmed or denied in an act of discrimination; and that it involves memory, in which mental states are recognized as a succession that all belong to the same self. He further claimed for consciousness that its results are 'clear' and 'unerring' (1861, 1: 266), that is, infallible. Finally, he purported to derive substantive philosophical conclusions from the immediate deliverances of consciousness, including the identity of the self over time, the fact of human freedom, and the existence of the external world. His attitude combines the notion of infallible awareness of the contents of the mind as found in Hume, with the distinction between empirical psychology and logic found in Kant, with the claim that we immediately perceive an external world, which he modified from Reid. By contrast with Reid, Hamilton considered the non-ego itself to directly confront the mind, as opposed to external objects being directly perceived by means of an innate conception.

Mill advocated a different conception of psychology and its place among the sciences. In Hamilton's scheme, psychology comprised logic and metaphysics; Mill realigned these sciences. In his *System of Logic*, he described logic as 'the Science of Proof, or Evidence': 'In so far as belief professes to be founded on proof, the office of logic is to supply a test for ascertaining whether or not the belief is well grounded' (1872b, Intro, Section 4).⁴ Metaphysics, by contrast, concerns 'the original data, or ultimate premises of our knowledge'. To it fall questions concerning mind, matter, the reality of space and time, and 'the nature of Conception, Perception, Memory, and Belief' (ibid.), inasmuch as these pertain to the basis for knowledge. These latter topics fell within psychology for Hamilton, but Mill limits psychology to the science
of the empirically determined laws of mind. These laws include the principle that impressions (in the Humean sense) produce ideas as well as the laws of association (similarity, contiguity, and intensity) (1872b, VI.4.3).

For our purposes, the most important difference between Mill and Hamilton concerns the methods of psychology. Mill did not deny Hamilton's premise that whatever is immediately and intuitively known in consciousness is known with certainty (1872a [1979, p. 126]). But he disputed Hamilton's claims as to what is so known. He contended that some states of consciousness which seem intuitive are actually the result of previous associative processes. What Hamilton took to be simple and evident perceptions of the self, its freedom, and the external world, Mill contended may result from psychological laws operating on previously experienced sequences of impressions and so not be instances of intuitive certainty after all – a position Russell later echoed. Mill called Hamilton's method 'introspective', because it claimed to detect by introspection which beliefs are simple and intuitive. Mill termed his own method 'psychological', because it relied on psychological explanations (based on observation and the laws of association) to show how apparently intuitive beliefs might be the product of previous experience. As an illustration, Mill took up the problem of the external world.

Hamilton and Mill on the External World

Hamilton's doctrine concerning our perception of an external (material) world exemplifies the role of consciousness in his philosophical (and therefore psychological) method. Mill disputed both the method and its results.

Hamilton appealed to the unerring deliverances of consciousness to establish that we are immediately aware of both ego and non-ego (1861, 1: 288, 292). By ego and non-ego he did not mean mental and material substance as they are in themselves. These are unknown. The mental and the material are known only through their phenomena or qualities, as experienced. Nonetheless, we are correct in dividing mental from material qualities and in seeing them as manifestations of distinct, but otherwise unknown, underlying substances (1861, 1: 138). Consciousness displays the subject's mental acts and the various objects of those acts, including feelings and sensations as states of the subject and material objects as distinct from the subject.

In external perception, or perception of material objects, we are in every instance immediately aware both of our act of perceiving and of an object perceived. Accordingly, at least with the primary qualities, we do not perceive representations or subjective mental states such as sensations but are directly aware of a material object. In support of this claim, Hamilton appealed to the (allegedly) manifest fact that perceptual consciousness divides into act and non-mental object. This appeal illustrates his precept 'that we must look to consciousness and to consciousness alone for the materials and rules of
philosophy’ (1861, 1: 288). The ‘Duality of Consciousness’ as act and material object is ‘clear and manifest’:

When I concentrate my attention in the simplest act of perception, I return from my observation with the most irresistible conviction of two facts, or rather two branches of the same fact; – that I am, – and that something different from me exists. In this act, I am conscious of myself as the perceiving subject, and of an external reality as the object perceived; and I am conscious of both existences in the same individual moment of intuition. The knowledge of the subject does not precede, nor follow, the knowledge of the object, – neither determines, neither is determined by, the other. (1861, 1: 288)

Hamilton contends that other philosophers, such as Berkeley and Hume, experienced this duality in consciousness but then denied their own experience. His disagreement with them, he believes, does not concern the basic facts but arises from their unwillingness to stick with those facts; instead, they end up ‘distorting or mutilating’ them (ibid.).

On this basis, Hamilton swiftly resolves the problem of the external world, and he just as quickly dispatches a representative or mediate theory of perception:

Such is the fact of perception revealed in consciousness, and as it determines mankind in general in their almost equal assurance of the reality of an external world, as of the existence of their own minds. Consciousness declares our knowledge of material qualities to be intuitive or immediate, – not representative or mediate. (1861, 1: 288–9)

In external perception, consciousness not only presents a division between act and object, but it reveals the object to be material and therefore the external world to exist.

The object in question, as it turns out, is not the distant object, the table and chair or the sun or the moon. Hamilton rejects awareness at a distance. The senses all function as modifications of touch: we directly perceive only what is in immediate contact with our sense organs. In vision, the light on the retina is the immediate object:

Through the eye we perceive nothing but the rays of light in relation to, and in contact with, the retina; what we add to this perception must not be taken into account. The same is true of the other senses. Now, what is there monstrous or inconceivable in this doctrine of immediate perception? The objects are neither carried into the mind, nor the mind made to sally out to them; nor do we require a miracle to justify its possibility. (1861, 2: 130)

Consciousness comes into contact with the external world in the organs of sense. In this way, the relativity of perception is easily explained. The
table appears to diminish as we recede from it because of how the reflected rays entering the eye change. (The retinal image of the table diminishes.) Hamilton disarms the relativity of perception as an objection to the immediacy of perception; in his account, what we directly perceive changes as we change our relation to the distal object. Through the retinal presentation, we are able to perceive, mediatelly, the distance, size, and shape of non-retinal objects. Whether this occurs through our learning to interpret cues for distance or by innate processes and mechanisms, Hamilton does not decide (1861, 2: 179–84).

Mill framed his attack on Hamilton’s introspective method as a dispute over the scientific basis for knowing the external world. Although allowing that the definition of ‘science’ remains provisional as the sciences progress, he regarded all sciences as inductive. Intuitive knowledge, which precedes science, is limited to immediately experienced sensations and feelings (Mill 1872b, Intro, Section 4). The inductive sciences, which extend beyond immediate consciousness, include: logic; metaphysics; mathematics; physical, chemical, and life sciences; psychology; moral sciences; and history. Metaphysics contains ‘that portion of mental philosophy which attempts to determine what part of the furniture of the mind belongs to it originally, and what part is constructed out of materials furnished to it from without’ (ibid.). Mill thus classified his dispute with Hamilton over the basis for a belief in an external world as ‘metaphysical’, and he contrasted the introspective and psychological methods ‘of metaphysical inquiry’ (1872a [1979, p. 148]). Hamilton would have agreed with the classification but disagreed with Mill’s psychological theory and his use of it, since, in Mill’s scheme, Hamilton’s allegedly intuitive knowledge is not intuitive at all.

Mill accused Hamilton of accepting the duality of consciousness as a simple, intuitive fact on insufficient grounds. Hamilton purported to find the experience of material existence as a simple and ineliminable (or ‘necessary’) fact of consciousness. In this appeal to what is ‘necessary’ in consciousness, Mill groups Hamilton together with Reid, Stewart, Victor Cousin, William Whewell, and Kant, and he indicts the lot:

The test by which they all decide a belief to be a part of our primitive consciousness – an original intuition of the mind – is the necessity of thinking it. Their proof that we must always, from the beginning, have had the belief, is the impossibility of getting rid of it now. This argument, applied to any of the disputed questions of philosophy, is doubly illegitimate: neither the major nor the minor premise is admissible. For, in the first place, the very fact that the questions are disputed, disproves the alleged impossibility. Those against whose dissent it is needful to defend the belief which is affirmed to be necessary, are unmistakable examples that it is not necessary. It may be a necessary belief to those who think it so; they may personally be quite incapable of not holding it. But even if this incapability extended to all mankind, it might merely be the effect
of a strong association; like the impossibility of believing Antipodes; and it cannot be shown that even where the impossibility is, for the time, real, it might not, as in that case, be overcome. (1872a [1979, pp. 143–5])

According to Mill, the origin of a belief is of direct metaphysical relevance because it can reveal the quality of the support for the belief (intuitive certainty vs. habitual affirmation). The origin is decided by psychological investigation. Those beliefs that are explicable as arising through association from psychologically basic elements should be classified as acquired:

These philosophers, therefore, and among them Sir W. Hamilton, mistake altogether the true conditions of psychological investigation, when, instead of proving a belief to be an original fact of consciousness by showing that it cannot, by any known means, have been acquired, they conclude that it was not acquired, for the reason, often false, and never sufficiently substantiated, that our consciousness cannot get rid of it now. (ibid., pp. 145–6)

This, of course, raises the question of how psychological investigation is to sort out those beliefs that are properly based on simple, intuitive apprehensions, and those that are acquired but are now so firmly fixed as to seem originally ‘necessary’.

Mill’s argument against Hamilton unfolds in three steps. First, he invokes widespread disagreement among previous theorists over whether a ‘non-ego’ (an external world) is directly intuited in consciousness (1872a, Chapter 10). In assessing Hamilton’s history of the question, he maintains that not even Reid, to whom Hamilton assigns his own position of natural dualism or natural realism, subscribed to that position. Further, he observes that, in the case of memory, Hamilton himself acknowledges illusions in immediate consciousness. We seem, in memory, to be immediately aware of past events, and yet Hamilton allows that we are aware only of a present representation of a past event. These considerations aim to show that the introspective method is not reliable for solving the problem of how and whether we perceive an external world.

Second, Mill argues that the belief in an external world can be explained as the product of known psychological processes, namely, the laws of association along with the capacity of the human mind to form expectations about future or ‘possible’ sensations.

Setting out from these premises, the Psychological Theory maintains, that there are associations naturally and even necessarily generated by the order of our sensations and of our reminiscences of sensation, which, supposing no intuition of an external world to have existed in consciousness, would inevitably generate the belief, and would cause it to be regarded as an intuition. (1872a [1979, p. 178])
He explains that a belief in the permanent possibility of sensation is equivalent to what we mean when we say that ‘the objects we perceive are external to us, and not a part of our own thoughts’ (ibid.). Having seen an object, if we take it to be an external object and not a mere ‘phantom’, then we believe that if we returned to its location we should see it again, that is, we should have sensations similar to those that we experienced earlier (assuming the object has not been moved, for example). We take these possibilities for sensation to be independent of our own minds and to be available to other observers. That is enough, Mill contends, to account for what can be properly defended in our conception of an external object (although he goes on to explain how we form the conception of a transcendent object, which conception he believes is not justified).

Third, Mill argues that his account is preferable to Hamilton’s on the latter’s own principle of parsimony. Mill’s account explains the belief in an external world by appealing only to known causes: the ability to form expectations, and the operation of the laws of association on series of sensations. Mill further contends that Hamilton’s reliance on intuition is, in effect, an appeal to an ‘original principle of our nature’ that is applied ad hoc to account for our belief in an external world (1872a [1979, p. 182]). Hamilton might rejoin that Mill must assume sequences of sensations to have occurred in childhood that he cannot now remember. The argument perhaps does not come out even, because Mill effectively shows that there is no agreement on what Hamilton says are immediate deliverances of consciousness, and he provides a plausible explanation for how the belief in an external world could seem intuitive when in fact it is acquired.

Passmore (1957, p. 28) suggests that Mill’s *Examination of Sir William Hamilton’s Philosophy* (1872a) buried that philosophy, but he allows that Mill’s work, despite a challenge from British Idealism, continued to be read (1957, Chapter 7). In fact, although Hamilton’s philosophy fell from the dominant position it had held into the 1860s, it also continued to be read. Hamilton’s *Metaphysics* was cited both positively and negatively by Brentano (1874), as well as by Ward (1886) and his student Stout (1896, 1: 39, 113), who together helped bring Brentano’s work, and that of his associate Meinong, to the attention of British philosophers, including the young Russell. The continuing controversy concerning psychological vs. intuitive (or introspective) accounts of belief in an external world provides background for Russell’s subsequent discussion of the topic.

**Ongoing Controversy over Psychological Theories of the External World**

Mill’s response to Hamilton on consciousness and the external world ranged over many topics and themes that continued to be discussed into the first decades of the twentieth century. These included representative, as opposed
to directly intuitive, theories of perception; the history of such doctrines since Berkeley and Hume; the distinction between primary and secondary qualities, which was often invoked in connection with the first topic; the distinction in consciousness between act (or ego) and object; and the proper analysis of the notion of an external world. These ongoing discussions frequently referred to Mill’s ‘psychological’ theory of the belief in an external world and sometimes invoked Hamilton, or at least an ‘intuitive’ theory of the awareness of an external world.

Ward’s frequently cited article on ‘Psychology’ in the ninth edition of the Britannica offered an analysis of the perception of material things that framed subsequent discussions. Ward (1886, pp. 60–2) proposed five factors that must be included in a psychological account of the perception of things: (a) reality, in the sense of being material rather than merely mental, being existent, and being actual as opposed to merely possible; (b) solidity or impenetrability, as discovered especially through resistance to the subject’s motor efforts as accompanied by a tactual sensation of the resisting object; (c) unity, as the object to which the subject refers multiple sensory impressions; (d) temporal continuity, discovered first with respect to the subject’s own body and then attributed to other bodies that act upon it; and (e) substantiality and the distinction between substance and attribute, including the distinction between primary qualities (persisting physical solidity in space) and secondary qualities (varying qualities such as temperature, colour, tastes, and smells). Ward treated these topics as those that a psychological theory must account for; the list is one that a philosophical theory of external objects might also address. More generally, the article ranges across topics concerning knowledge and the self that possess both psychological and philosophical aspects. Ward (1890) himself held that both logic and epistemology or theory of knowledge are distinct from psychology, while also suggesting that logic and epistemology (like psychology) had become disciplines of their own, independent of philosophy itself (which, in his view, primarily examines metaphysical questions concerning being) but still retaining intellectual ties.

Ward’s discussion provided a reference point on the external world from 1890 forward. In that year, Julius Pikler, of Budapest, published a slim volume on The Psychology of the Belief in Objective Existence. He divided the problem of the external world into three (Pikler, 1890, pp. 2–4): (1) the meaning of asserting the existence of an objectively existing external world; (2) the genesis of the belief involved in this assertion; and (3) the truth of that belief. He took the first two questions to be primarily psychological and the third to belong to ‘philosophy proper, or metaphysics’ (1890, pp. 4–5). At the same time, he held that the first two questions are relevant to the third, since the interpretation of the belief is relevant to assessing its truth, and its genesis may also be relevant. He advanced an account that was similar to Mill’s permanent possibilities of sensations, with some important
differences. First, existence in space became the key factor in belief in an external world. Second, he emphasized the role of will (interpreted as desire) in the development of the belief. Third, he held that only a subset of the possibilities of sensation imply an external world, namely, those that can be obtained merely through volitional motions of our bodies or sense organs, which unleash one stream of possible sensations rather than another. He set his theory in opposition to Ward’s (1886) view – as reaffirmed by Stout (1890) – that the notions of self and non-self arise correlatively.

Stout published a series of articles and chapters on the problem of the external world from 1890 to 1913 (and beyond), largely following Ward. He treated the problem primarily from a psychological point of view, as concerning the genesis of the belief in an external world. He emphasized the role of the active self, whose movements are impeded by an external agency; who thereby distinguishes self from non-self; who gains the notion of a persisting thing first from his own body and then transfers it to external objects; and who, by a process of ideal construction that interpolates objects into spatial, temporal, and causal sequences, moves beyond objects as experienced to posit the same object as existing when not perceived and other objects existing that are never perceived (1890; 1899, III.ii.2, IV.6; 1903, Chapter 14). These points primarily address Pikler’s second question, although they touch on his first question and might rule out some answers to his third. Another article by Stout speaks more exclusively to the first question (Stout 1900–01), another recounts how sensations represent an external world (1904), and another (1911) responds to Joseph’s philosophical criticisms (1910).

Shortly after Pikler and Stout published their works in 1890, the Aristotelian Society held a symposium on the ‘Origin of the Perception of an External World’. The Society’s President, Shadworth Hodgson, analyzed the problem into philosophical and psychological issues. Here is his division:

1. Analysis of the perception of an external world, i.e., combination of its sufficient and necessary constituents (as just explained).
2. Epoch and conditions of its arising as an event or existent in a percipient’s development (as just explained).
3. Analysis of the perception that an external world exists as the real object of the perception of it.
4. Epoch and conditions of this latter perception of reality (No. 3) arising as an event or existent in a percipient’s development. (Hodgson et al., 1891–92, pp. 26–7)

As regards (1) and (2), Hodgson has ‘just explained’ that (1) is the primary philosophical problem of the external world, and that (2) is a psychological question which is irrelevant to it. He also takes (3) to depend on (1). Hodgson further contends that the concept of the self does not play a role
in the concept of the external world, a point that the first respondent, Vice President Bernard Bosanquet (ibid., p. 34), connected to Stout’s (1890) claims. Bosanquet observed that in pursuing (1), Hodgson’s analysis drew on psychological facts concerning the spatiality of touch or vision (Hodgson et al., 1891–92, p. 33). In reply, Hodgson remarked that the words used to describe consciousness may be ambiguous as to whether they describe ‘some function of a Subject’ (psychological sense) or ‘some content of sensibility’ (an object of philosophical analysis). He concedes that, in addressing philosophical questions concerning the external world, he makes use of ‘the psychological knowledge, be it more or be it less, which is at my command’ (ibid., p. 41). The second respondent, David G. Ritchie, maintains that the problem of the origin of the perception of the external world is ‘primarily a psychological one, as it concerns the origin, and not the philosophical implications of our perception of an external world’ (ibid., p. 36), while making clear that he considered philosophy and psychology each to be relevant to the other. One upshot of the symposium was to distinguish philosophical and psychological aspects of the problem of the external world, and to acknowledge their mutual relevance.

The problem of the external world was regularly taken up by philosophers, with great attention to psychogenetic theories of how the concept of the external world arises. Dawes Hicks (1900–01) examined the works of Pikler and Stout, generally favouring Stout. However, he did not stay with Stout’s view that resistance to motor volitions is the initial spur to distinguish self from non-self and eventually to perceive an external world; he assigned the initiating role to feelings and desires arising from external objects. The Oxford philosopher L.T. Hobhouse (1896) wrote an extensive treatise on Theory of Knowledge, including a chapter on the ‘Conception of External Reality’ that showed his awareness of the psychogenetic approach. Hobhouse adopted a direct realism that was similar to Hamilton’s in affirming that, in perception, we are directly acquainted with an external reality. However, he rejected Hamilton’s view that we know this to be so immediately via the deliverances of consciousness (Hobhouse, 1896, p. 537). To distinguish illusion from veridical perception, we must fit our current perception into a coherent scheme. Hodgson (1903–04) pursued the psychogenesis of the concept of reality, and specifically of material reality, as distinct from the consciousness that knows it. In distinguishing sensation from thing sensed, he found that the development of spatial representation and the impact of other solid bodies on our own are the crucial elements (as opposed to Stout’s self-oriented theory). Joseph (1910) criticized Stout’s psychogenetic theory on its internal coherence; while invoking elements of philosophical analysis, his criticism remained in Stout’s psychological domain. For better or worse, philosophers found that the content and so the origin of the concept of an external world were relevant to arguments concerning our knowledge of its existence. Russell was no exception.
Russell’s Epistemological Turn

Russell’s early and middle career as a philosopher is divided into his idealist phase, up to 1898; his *Principia* period, through 1910 (divided into subperiods by the appearance of ‘On Denoting’ in 1905); followed by a middle period (from 1911), which may be thought to end with his adoption of James’s neutral monism ca. 1919. Early in his middle period, he undertook a new project on the theory of knowledge, in the course of which he announced a new ‘logical analytic’ method of philosophizing, which would make philosophy ‘scientific’ (a modest, piecemeal, cooperative venture). During this early part of his middle period, he discussed the relation of philosophy to psychology.

By way of further background, we may note that during the mid-1890s, Russell was trained in philosophy at Cambridge by McTaggart, Ward, and Stout. As his commitment to idealism ended, Russell conceived an ambitious philosophical project concerning the analysis of the fundamental concepts of the sciences, starting with physics and the notion of matter. A need to examine mathematics first led him into *Principia Mathematica* (via Russell, 1903). During these years he reviewed several of Meinong’s works and responded to them in a three-part article. After the *Principia* years, in 1912 he came back to the problem of matter but decided that he needed to achieve a wider examination of the theory of knowledge. He began by rereading (unspecified) past philosophers on knowledge, and then, from 7 May to 7 June 1913, he composed part of a book on the *Theory of Knowledge* (TK). At that time, Wittgenstein visited and criticized his theory of propositions, which ultimately caused him to abandon the book and to question the value of some of the later parts of his extant manuscript. He nonetheless continued to value the first part of the work, ‘On the Nature of Acquaintance’, and from January 1914 to April 1915 he published six chapters in the *Monist* (in somewhat revised versions). These included Chapter 4, ‘Definitions and Methodological Principles in Theory of Knowledge’, in which Russell discussed the place of psychology in theory of knowledge.

During 1912, Russell agreed to give the Lowell lectures at Harvard. In March, 1913, he proposed to lecture on good and evil in the universe, but this topic was rejected, and in July he proposed the topic of the external world. During September, 1913, he completed a draft of *Our Knowledge of the External World* (OKEW, 1914c), the text of the Lowell lectures (delivered in Boston in March and April, 1914). Especially in Chapter 3, ‘Our Knowledge of the External World’, he invokes psychology in his analysis. He also gave a seminar at Harvard during this time, in which he used some of the *Monist* articles. In January 1914, he completed ‘The Relation of Sense Data to Physics’ (1914e), which he used in the seminar and in revising Chapter 3 of the Lowell lectures.
Epistemology, Logic, and the Psychologically Primitive in Russell

In the preface to OKEW, Russell describes the work as ‘an attempt to show, by means of examples, the nature, capacity, and limitations of the logical-analytic method of philosophy’ (Russell, 1914c, p. v). In Chapter 2, he announces that logic is ‘the essence of philosophy’ and that every philosophical problem is ‘logical’ (p. 33). The chapter explains that the relevant forms of logic concern probability as a relation of evidence to assertion and, more emphatically, the new mathematical logic or logistic, which catalogues the forms of propositions and makes some general assertions about the truth of all propositions of a certain form (p. 57). Only the beginnings of mathematical logic are directly relevant to philosophy (p. 41). These beginnings include the logic of relations, which Russell considered to be a philosophically important advance over traditional syllogistic logic.

In saying that logic is the essence of philosophy and that all philosophical problems are logical, Russell manifestly did not mean that every philosophical problem is a problem in mathematical logic. Rather, he meant that analysis using logical forms is an essential tool of philosophy; it serves as an ‘instrument of discovery’ in epistemology but is not the only one (OKEW, pp. 65, 68). In Chapter 1 of OKEW, Russell asserts that ‘philosophy is not a short cut to the same kind of results as those of the other sciences: if it is to be a genuine study, it must have a province of its own, and aim at results which the other sciences can neither prove nor disprove’ (p. 17). The other sciences have charge of many questions of intrinsic human interest, such as the future of humankind. What remains to philosophy? It can ‘help us to understand the general aspects of the world and the logical analysis of familiar but complex things’ (p. 17). As he further explained (Chapter 3), philosophy takes as its ‘data’ (in a general sense) the beliefs found in ‘common knowledge’, including everyday beliefs as well as those of the sciences (pp. 65–6).

Russell denies that philosophy has its own ‘superfine brand of knowledge’, by which it can overturn the ‘facts of experience’ and ‘laws of science’ (pp. 66–7). The most the philosopher can do ‘is to examine and purify our common knowledge by an internal scrutiny, assuming the canons by which it has been obtained, and applying them with more care and with more precision’ (pp. 66–7). Philosophy must seek the firmer portions of its data and use them to criticize and reformulate the other portions. That is the task of the logical-analytical method as applied to the problem of knowledge. The method is stated in OKEW, and it had been applied in TK. (The method was adumbrated in Russell, 1912a; Russell, 1900 used it, on which see Beaney’s chapter, this volume.)

The firmest sorts of knowledge are ‘hard data’, consisting of ‘the particular facts of sense and the general truths of logic’ (OKEW, pp. 70–1). However,
what counts as the ‘particular facts of sense’ is not immediately clear. In our common knowledge, we may take ordinary objects such as table and chairs, and their specific properties, such as size and shape, to be the immediate objects of sense and thus as prime examples of ‘particular facts’ of sense. In that we are mistaken. How does Russell know? Here he enlists the aid of another part of common knowledge, the findings of natural scientific psychology.

Russell was clear in TK and OKEW that psychology is of great importance for epistemology. In Chapter 4 of TK, he ascribes the difficulty in defining ‘epistemology’ or ‘theory of knowledge’, in part, to its overlap with psychology and logic.

It is obvious that much of epistemology is included in psychology. The analysis of experience, the distinction between sensation, imagination, memory, attention, etc., the nature of belief or judgment, in short all the analytic portion of the subject, in so far as it does not introduce the distinction between truth or falsehood, must, I think, be regarded as strictly part of psychology. (1914b [1992, p. 46])

The notions of truth and falsity, in his view, belong to logic, and logic has much to contribute in analyzing the forms that judgments may take (e.g., subject–predicate, relational). But the ‘analytic’ part of epistemology draws upon what is properly psychology. This does not mean that epistemology becomes psychology; rather, it means that analyses that are fundamental for epistemology are aided by, or initiated within, psychology. These analyses aim at finding epistemologically basic premises: those premises that can be ‘known without inference’ (ibid., p. 50).

Russell developed the notions of ‘logically primitive’ and ‘psychologically primitive’ beliefs and knowledge in OKEW, Chapter 3. Logically primitive beliefs are those not reached by logical inference, whereas logically derivative beliefs are reached by inference or by logical construction. Psychologically primitive sensory beliefs are those not caused by other beliefs or by any other fact of sense except what the belief asserts. Psychologically derivative beliefs may result from these other causes, including logical derivation. But they also may arise without logical derivation, ‘merely by association of ideas or some equally extra-logical process’ (p. 69). As with Mill’s apparently but not actually intuitive beliefs, before epistemological analysis many beliefs about sensory things are logically primitive (because not reached through logical derivation) but psychologically derivative (because caused by extra-logical psychological processes, such as association – or by ‘inferences’ undertaken without conscious logical scrutiny). We discover, upon reflection, that we have less confidence in beliefs that are logically primitive but psychologically derivative (pp. 69–70). Among facts of sense, those that are both logically and psychologically primitive provide the ‘hard data’ for our knowledge (as
in Mill’s actually intuitive knowledge of basic elements). One aim of epistemology is to replace beliefs that are logically primitive but psychologically derivative with corresponding beliefs that have been logically derived from data that are both psychologically and logically primitive.

Russell praised psychology for discovering that many of our beliefs about the spatial properties of external objects are psychologically derivative:

Psychologists, however, have made us aware that what is actually given in sense is much less than most people would naturally suppose, and that much of what at first sight seems to be given is really inferred. This applies especially in regard to our space-perceptions. For instance, we instinctively infer the ‘real’ size and shape of a visible object from its apparent size and shape, according to its distance and our point of view. (OKEW, p. 68)

Russell presents a psychologically derivative belief that is, prior to analysis, logically primitive: our belief that we directly perceive an object, say, a table, as three feet high with a rectangular top, when what is really ‘given in sense’ is a trapezoidal top and an image that varies in size inversely with our distance from the table.

This conception of size and distance perception was widely repeated in the psychological literature on perception. Textbook accounts described a process of inference or association from a two-dimensional sensory core to the real size of the object (Le Conte, 1881, p. 157; Stout, 1913, pp. 502–03). James explained that the same retinal image produces an experience of a larger area when the area is seen as being farther away (James, 1890, 2: 231), and he recounted how a circle may appear elliptical. He attributed to ‘experience and custom’ the recollection of the ‘true’ shape from the elliptical appearance (ibid., p. 239). Russell operated at this level. Some perceptual theorists had rejected an original sensation of a two-dimensional form. Thus, although Helmholtz famously held that we infer sizes and shapes from sensations via unconscious inference (Helmholtz, 1867, Section 26), he argued that this process moves from non-spatial sensations to three-dimensional structures (Hatfield, 1990, Chapter 5). Hering (1868) rejected Helmholtz’s sensations and inferences, contending that three-dimensional visual structures are directly produced by physiological processes. Nonetheless, Russell’s starting point of phenomenally given perspective images9 reflected the commonly received science of his day. Beyond size perception, we have seen that Mill, Stout, and others examined the psychological development of belief in external objects as permanently existing and distinct from the subject. For Russell, that was another logically primitive, but psychologically derivative, belief needing analysis and reconstruction.

In the quoted passage, Russell speaks of the real size as being ‘inferred’ from apparent size. At first blush, this seems to undermine the example,
which should show that beliefs about real size and shape are psychologically derivative but logically primitive, that is, not produced by logical inference. There are two ways to understand Russell’s use of the word ‘infer’ in the quotation. He might be loosely describing psychological theories in their own terms, which sometimes (as in the case of Helmholtz, 1867, Section 33) equated ‘inference’ with association (an equation Russell did not accept). Or he might consistently hold that the ‘inference’ here is not a purified, epistemically certified logical inference, and so must count as an ‘extra-logical’ psychological process. Indeed, Russell subsequently speaks of ‘unconscious inference’ as filling in gaps in our sensations during a conversation. Because we are less adept at this process when listening to a foreign language, we have trouble hearing the language and so must sit nearer the stage when attending a play (OKEW, p. 68). Our belief that the problem is with the intensity of the sound (rather than with our comprehension) might well be logically primitive, because we are unaware of the extra-logical inferential processes that must take place and so do not take account of them epistemically.

Russell relies on scientific psychology to reveal the psychologically primitive ‘appearances’ that serve as the basis for psychologically derivative perceptions such as that of the ‘real’ size and shape of the table. These appearances are the hard data of sense, or sense data. The sense data for a table vary depending on our point of view, while our psychologically derivative belief about the size and shape of the table remains constant. Russell considered his analysis of the table into a sequence of sense data, taking into account multiple points of view, to be a substantial breakthrough (1914e; OKEW, Chapter 3). He then spoke of a process of construction, in which any notion of the table as a material object – as a ‘thing in itself’ beyond the sense data and inferred from them – is replaced by an explicit logical construction. In this construction, beliefs about the table are now logically derivative, but they are derived by logical construction using only data that are both psychologically and logically primitive. This construction replaces our previously logically primitive conception of the table as a material object with logically certified constructions. These constructions have the further benefit of avoiding epistemically shaky inferences to material things behind the sense data.10

Russell’s ‘analysis of experience’ (TK, Chapter 3) concerns the structure of experience itself. Russell contends that we find in experience two factors: an act of experiencing (by a subject), and an object experienced. The immediate objects of sense (sense data) are not mental, not part of the mind. Russell (1912b) previously had written as if non-mental sense data are intermediaries (third things) by which we know physical objects as ordinarily conceived: a representative realism. His conception that physical objects are constructions out of sense data alleviated this representative realism (about which he expressed grave doubts in 1912a). In the new conception,
in external perception there are only two things: selves and non-mental sense data (OKEW, p. 85; 1914e). The physical object as ordinarily conceived has been excised.

Early in TK, Russell described a ‘dualism’ of act and non-mental object. He reports that ‘dualists’ who believe ‘that we know by introspection things having the character we call “mental” have urged that we also know other things not having this character’ (1914d [1992, p. 7]). The position he describes is the ‘natural dualism’ of Hamilton (1861, 1: 293), which claims introspective awareness of the act of consciousness and of the fact that its object is non-mental. (Russell does not name Hamilton.) This ‘natural dualism’ does not purport to establish by introspection that mind and body are distinct substances, but it does claim to reveal a dualism of mental act and object (1861, 1: 288). (Neither Hamilton nor any other author considered herein held that introspective awareness extends to the ego as bare subject.) Hamilton supported his dualism directly by intuition or introspection. Russell reluctantly denies that the distinction between act and object is immediately evident. He found it nearly ‘obvious’ that we directly experience our experiencing of objects, but he knew that James and the American Realists, in their position of neutral monism, rejected any awareness of the mental act of perceiving or of ‘consciousness’ as its agent. Because they could deny intuitive knowledge of a really distinct actor who enters into the act–object distinction, Russell (echoing an argument that we have seen in Mill) conceded that the distinction is not patent (1914a [1992, p. 33]).

But Russell endorsed an epistemological dualism nonetheless. He did not say how he knew that perceptual objects are non-mental and so different in kind from the act of perceiving them. In OKEW, he simply announces this position: ‘A patch of color, even if it exists only when it is seen, is still something quite different from the seeing of it: the seeing of it is mental, the patch of colour is not’ (pp. 84–5). The patch of color is a sense datum, an item among the ‘hard data’ that underlie his epistemological reconstructions.

In fact, the view that act and object are distinct was widely, but not universally, held by philosophers whose work Russell knew or may have known. Hodgson (1896), as we have seen, held that act and object are distinct and that the immediately known object is non-mental, but he denied Hamilton’s view that this is known by introspection. Rather, this conclusion is reached by theoretical reflection. Moore, who espoused a naïve realism regarding the perception of physical objects in his ‘Refutation of Idealism’ (1903 [1922, pp. 29–30]), subsequently distinguished sensible qualities both from the perceiving of them and from physical objects as described by science. In accepting that some of the ‘sensible qualities which we perceive as being in certain places, really do exist in the places in which we perceive them to be’ (1905–06 [1922, p. 95]), he accepted that some sensible qualities (or
the particulars that possess them) are non-mental. These positions are to be distinguished from the views of Brentano, Ward, Stout, and Meinong, who agreed in distinguishing mental act from apprehended object, but then rendered the object as mental (as a ‘content’ or ‘presentation’), and either (as in Brentano, 1874) left aside systematic examination of the question of a mind-independent object corresponding to this content, or posited such an object at least provisionally (Ward, Stout, Meinong).

Russell held other aspects of scientific psychology to be relevant to epistemology, including the relation between sensation and imagination, the phenomena of memory, and the role of attention, to name only three. I have considered his use of psychology in an epistemological analysis of the problem of the external world. This use bears strong similarity to Mill’s ‘psychological method’. Further, he appears to have followed Mill in rejecting the Hamilton-like view that introspection establishes epistemological dualism.

Psychology, Philosophy, and Psychologism

As Russell began his epistemological project in 1912, the notion that psychology was relevant to epistemology was commonplace. This outlook, which was shared by Hamilton and Mill, despite their other differences, had been challenged by British Idealism. Be that as it may, during the 1880s and 1890s, the relevance to epistemology of psychological considerations concerning the structure of experience, the processes of perception, and the belief in an external world was widely endorsed. More generally, the newly founded *Mind: A Quarterly Review of Psychology and Philosophy* (Robertson, 1876, 1883) declared the mutual relevance of philosophy and scientific psychology.

We have seen this outlook espoused not only by Stout and Ward, who bore witness to Brentano in their broad conceptions of psychology’s relation to philosophy, but also by thinkers as diverse as Hobhouse and Hodgson. Indeed, Hodgson, in *Mind*, summed up the prevailing attitude as follows:

> The title Psychological Philosophies may, not improbably, suggest the objection – But are not all philosophies of necessity psychological? And it is true, that any philosophy must include some psychological theory or other, inasmuch as, being or aiming at being a Knowing of the most comprehensive kind, it would be incomplete without taking account of the Subject, process, and function of Knowing, as well as of its content or object known. (Hodgson, 1899, p. 433)

As the twentieth century turned, this outlook came to be questioned, in an off-hand way by Moore (1899, p. 193), and then by H. A. Prichard (1907), who argued that psychology was of no use to philosophy and questioned
its viability as a science. Nonetheless, Prichard acknowledged the prevailing view of psychology’s relation to philosophy:

- in the case of knowledge, psychology seeks to show how it is that a life which begins with sensation and feeling comes to acquire the articulated knowledge of the world which we now possess. Its results are considered not only to be of intrinsic importance but also to bear in an important way on the problems of other subjects, and especially on those of metaphysics. We even find it said that the future of philosophy is obviously with psychology. (Prichard, 1907, p. 27)

As it happens, this last prediction turned out not to be true for the short-term, especially in Britain and especially at Oxford, so that retrospectively some may see Prichard’s article as a clarion call that foreshadowed the proper separation of philosophy from all psychological concerns. Of course, the attitude of complete separation, so dominant in the middle decades of the twentieth century, was not itself eternal (Hatfield, 2006, 2009a, b). In his own time, Prichard’s criticisms of the prevailing view were immediately met by Stout (1907) and Schiller (1907). Even Joseph’s (1910) criticism of psychological theories of the external world remained on psychological ground.

The historiography of this issue is muddied by a too-common assumption that Prichard and others were engaged in a righteous battle to save philosophy from the fallacy of psychologism. Leaving aside the false teleology of this view, to make historical sense of it we need a definition of the term ‘psychologism’. The term itself apparently was introduced by J.E. Erdmann (1870, 2: 636), who used it to characterize F.E. Beneke’s attempt to ground philosophy, and especially the theory of knowledge, on psychology. The Dictionary of Philosophy and Psychology drew on this usage in defining ‘psychologism’: ‘The doctrine of Fries and Beneke (see the histories of Falckenberg and Windelband), which translates the critical examination of reason (of Kant) into terms of empirical psychology’ (Dewey, 1901). The definition is vague, or it relies on prior knowledge of the relation between Fries and Kant. Historically, Fries held that Kant had intended, and should have intended, his critical philosophy to rest entirely on empirical psychology. The ‘translation’ that Dewey speaks of should thus be understood as the full assimilation of philosophical problems to psychology; the belief that empirical psychology can solve (or define) the problems of philosophy.

On this definition, Russell’s appeal to psychology in his theory of knowledge does not count as psychologism. We have found that Hodgson, Hobhouse, Russell, and others recognized the relevance of psychology to epistemology while also contending that philosophy has its own concerns that transcend those of psychology. If admitting the relevance of psychology to epistemology amounts to psychologism, then many philosophers at the turn of the twentieth century were guilty of that alleged fallacy, even those
who explicitly distinguished – as did Hodgson, Hobhouse, and Russell – the aims of psychology from those of philosophy and theory of knowledge. But that seems too wide a criterion for psychologism, on both philosophical and historiographical grounds. As a specific conception of psychologism, it stems from Frege’s and Husserl’s belief that any appeal whatsoever to subjective or mental processes in logic is illegitimate. On this conception, psychologism does not consist in confusing psychological processes with justification; it consists in treating thought as mental in any way.12

In the end, there was nothing novel in Russell’s appeals to psychology in his epistemological work. In developing his theory of knowledge, he would have found the relevance of psychology to be widely acknowledged. So, too, with his finding separate aims for psychology and epistemology. If his appeal to psychology seems somehow philosophically surprising, I take that to be a sign that Prichard and others were, for a time, successful in their efforts. But to follow the history of their efforts would take us beyond Russell’s middle period, during which he continued to follow developments in psychology, resulting in his *Analysis of Mind* (1921), which marked a change in his philosophical outlook and an expansion of his psychological interests, responding to, but not fully adopting, the outlook of American behaviourism and working out his own version of the neutral monism of James.

**Notes**


2. Hamilton’s ‘science of mind’ is comparable in extent to Mill’s (1872b) ‘moral sciences’ and to the German category of the *Geisteswissenschaften*. 

3. In distinguishing logic from empirical psychology, Hamilton quoted liberally from Krug (1819) and Esser (1830), who generally adopted the Kantian view of logic as formal.

4. The question of logic’s relation to psychology in Mill’s thought is vexed, partly by his seemingly contradictory pronouncements (Godden, 2005). Mill distinguished the prescriptive function of logic from the descriptive task of psychology, and he (typically) viewed the object of logic ‘objectively’, as concerning the most general laws of the world (as opposed to the most general laws of the mind). Accordingly, his claim that logic is empirically based is not in itself psychologistic. Skorupski (1998) argues that Mill’s epistemology of logic was not psychologistic in the sense of equating logical laws with psychological laws or limiting judgments to relations between mental contents, but that his ultimate justification of induction was psychologistic in stemming from reflection on the mind’s actual practices in reasoning. By contrast, Godden (2005) finds that Mill’s phenomenalism, in limiting the objects of thought to phenomena, renders his epistemology psychologistic. For our purposes, it is enough to note that Mill clearly distinguished the prescriptive function of logic from the descriptive task of psychology.
5. R. Hodgson (1885) related his position to Hamilton (1861) and, more extensively, to Herbert Spencer’s (1870–72) ‘transfigured realism’. Case (1888, pp. 21–8) and Hobhouse (1896, p. 537) each contrasted his position with Hamilton’s. Abraham Wolf (1908–09) defended a version of Hamilton’s natural realism. Russell (1983, p. 358) read Hobhouse’s book upon publication and cited Wolf (Russell, 1914e, Section 12).

6. In the articles listed thus far, Stout writes as a realist who posits spatially extended matter as distinct from minds; his position in the cited materials underwent small modifications that we can safely ignore. His deeper metaphysical commitments changed during this period, as he testifies (1911, 9 n. 1), but we are focusing on his writings on the origin of the conception of a mind-independent material world.

7. Metz (1938, p. 46) observes that ‘the little which is still alive of the Scottish school’, of which Hamilton was the leading exponent, appears in the ‘new realism’ of ‘J.C. Wilson, G.F. Stout, G.E. Moore, J. Laird, and C.E.M. Joad’, Metz subsequently classes Wilson as an ‘old’ realist and treats Stout separately, as he does Hobhouse, classifying his position as ‘critical realism’ (1938, p. 157). Passmore (1957, p. 310) classifies Stout as an idealist based on Stout’s later works and overlooks Hobhouse, who was an Oxford realist in the 1890s, indebted to Case’s teaching on the external world (Hobhouse, 1896, p. 517).

8. The events and dates in the next two paragraphs are drawn largely from Eames (1992).

9. Perhaps in response to Whitehead’s comments (Lowe, 1974) on sense data in Problems (Russell, 1912b), Russell developed his six-dimensional account, which relates multiple perspective images to one another through viewing positions (1914e; OKEW, Chapter 3). This account distinguishes between physical and psychological space, and it treats psychological space as phenomenally three-dimensional in that the data are perceived as being at some distance from the subject. But the penny is still treated as appearing elliptical under certain perspectives (1914e, Section 7; OKEW, p. 90), rather than as a circle-at-a-slit in three dimensions (or, regarding size, as possessing a constant size-at-a-distance). We cannot enter here into the interesting intricacies of Russell’s construction. The notion that our visual experience is fundamentally of perspective images was subsequently challenged by Gestalt psychology (e.g., Koffka, 1935) and by Gibson (1950).

10. Many interesting questions arise about this logical construction and Russell’s appeal to unsensed sense-data (or sensibilia) in it, but they are not directly pertinent to the place of psychology in Russell’s epistemology. See Nasim (2008) for discussion and additional references.

11. Hylton (1990, p. 330) acknowledges Russell’s ‘turn towards the psychological’ after his Principia period, characterizing it thusly: ‘Philosophical theories therefore appear to be answerable to the data of experience, to facts about what is or can be plausibly supposed to be present to our minds. This is clearly a considerable concession to psychologism’. He does not say what he means by ‘psychologism’; if it amounts simply to the claim that philosophical theories should be answerable to the data of experience, even Moore and Prichard are guilty of psychologism. Hylton further suggests that Russell’s theories were non-psychological because ‘the objects of acquaintance themselves are not in general mental’, including the objects of perception (sense data). Here, Hylton appears to adopt the Frege–Husserl conception that psychologism consists simply in viewing the objects
of thought as mental (subjective), on which, see the text below and the subsequent note. Russell separates philosophical from psychological subject matter by assigning the analysis of experience in general to psychology and reserving the notions of truth and falsehood and of the logical forms of judgment for logic, with philosophy using the latter to construct a theory of knowledge that yields beliefs that are both justified and true (or that suggests how to attain such beliefs).

12. Passmore’s reference to psychologism (1957, p. 174), quoted in Section 1, also assumes that the charge applies for merely considering psychology as relevant to, in his case, logic. His conception (1957, p. 150) may rest on the tendency, during the middle decades of the twentieth century, to adopt a Frege–Husserl notion of ‘psychologism’. Anderson (2005) usefully examines two types of psychologism: the neo-Kantian charge of confusing normative rules with descriptive claims about how the mind in fact works, and the Frege–Husserl charge of making thought subjective. Further investigation is needed of the historical interplay between the original Kantian notion of psychologism and subsequent notions. Kusch’s (1995) sociological study makes the ‘subjectivity’ (Frege–Husserl) case primary and does not systematically consider the Kantian notion.

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Gary Hatfield


8

C.I. Lewis and the Analyticity Debate

Thomas Baldwin

In the final paragraph of ‘Two Dogmas of Empiricism’ Quine brackets together C.I. Lewis and Carnap as two pragmatists who, by remaining committed to the analytic/synthetic distinction, have not taken their pragmatism to its proper conclusion:

Carnap, Lewis and others take a pragmatic stand on the question of choosing between language forms, scientific frameworks; but their pragmatism leaves off at the imagined boundary between the analytic and the synthetic. In repudiating such a boundary I espouse a more thorough pragmatism. Each man is given a scientific heritage plus a continuing barrage of sensory stimulation; and the considerations which guide him in warping his scientific heritage to fit his continuing sensory promptings are, where rational, pragmatic. (Quine, 1953, p. 46)

The ensuing debate between Carnap and Quine concerning the validity of the analytic/synthetic distinction and its significance has been much discussed and debated.¹ But relatively little has been written about Lewis's position on this issue and about the significance of Quine's arguments for this position; and this is what I shall attempt to address here. I shall also discuss Carnap's position because it is useful as a familiar point of reference in this debate; but I shall start (and end) with Lewis.

1 Lewis, the a priori and the analytic

Lewis develops his position from a Kantian starting point in which it is in the first instance the existence and importance of a priori truth which is emphasised. Lewis takes it that ‘mind’ has a central role in interpreting sense experience as the ground for knowledge of an objective world and he takes it that this role brings with it a commitment to a priori truths which represent this role:

The a priori represents the activity of mind itself; it represents an attitude in some sense freely taken. (Lewis, 1929, pp. 196–7)
What lies behind this are the claims, first, that sense experience itself is a purely qualitative ‘given’ which as such lacks both the intentional content and the rational connections to constitute knowledge of anything; but, second, that this sense experience nonetheless lends itself to conceptual interpretations which are deliberately constructed (‘freely taken’) in such a way that as thus interpreted it warrants judgments concerning what is ‘real’, what is really there:

Truth which is a priori anticipates the character of the real; otherwise, it would possess no significance whatever. The real, however, is not the given as such, but the given categorially interpreted. (Lewis, 1929, p. 197)

Thus far, we have a recognisably Kantian position – sensations are given but lend themselves to conceptual interpretation as intuitions of real objects (though it is debatable how far Kant is committed Lewis’s purely qualitative account of the given ‘as such’). Furthermore, Lewis’s way of speaking of the mind’s ‘activity’ and its ‘freedom’ with respect to the a priori is reminiscent of Kant’s thesis that the understanding represents the ‘spontaneity of cognition’ (A51/B75). But for Kant, the a priori truths which represent this aspect of the mind’s activity are typically synthetic, whereas Lewis departs from Kant precisely by maintaining that these truths are analytic:

*The a priori is not a material truth, delimiting or delineating the content of experience as such, but is definitive or analytic in its nature.* (Lewis, 1929, p. 231 – Lewis’s italics)

Lewis has several reasons for making this change. One is that he sets aside Kant’s story about the synthesising role of the transcendental unity of apperception in both necessitating and validating the application of the categories to sense experience. So, although Lewis emphasises the mind’s contribution to objective knowledge, the mind in question is our own reflective capacity for rational judgment, not that of some non-empirical agent somehow at work within us:

The a priori does not need to be conceived as the inscrutable legislation of a transcendent mind, the objects of which, being limited by its forms of intuition, are phenomenal only.... The a priori is knowable simply through the reflective and critical formulation of our own principles of classification and interpretation. (Lewis, 1929, pp. 231–2)

One can agree with Lewis about this point but still wonder whether it is appropriate to regard the a priori as analytic. But here another, positive, reason for Lewis’s change comes into the picture. Kant famously regarded
logical truth as analytic, and Lewis’s view was that the implications of this thesis are more extensive than Kant recognised. In particular, whereas Kant regarded mathematics as a paradigm of the synthetic a priori, Lewis takes it that the logicist thesis developed by Whitehead and Russell in *Principia Mathematica* shows that mathematics is analytic (Lewis, 1929, pp. 244–5).

Lewis, however, does not hold that all a priori truths are truths of logic. In additions to truths of this kind, he holds that there is a second type of priori truth which expresses ‘criteria of reality’:

Second, the concept in its application to the given exhibits the predetermined principles of interpretation, the criteria of our distinguishing and relating, of classification, and hence the criteria of reality of any sort. This is most clearly evident in the case of those basic concepts, determining major classes of the real, which may be called the categories, though in less important ways it holds true of concepts in general. (Lewis, 1929, pp. 230–1)

It is here that the issue of analyticity, on the face of it, looks most pressing: for given that the a priori principles involved here are neither logical nor explicit definitions of the ‘Bachelors are unmarried males’ type, it needs to be explained why they are ‘definitive or analytic’ in nature. For Lewis, there are several further considerations to be brought into play here. First, building out from the Kantian thesis that logical truth is analytic, Lewis holds that logical analysis should not be thought of as primarily the decomposition, or the ‘dissection’, of a complex whole into simple parts; ‘*Logical analysis is not dissection but relation*’ (Lewis, 1929, p. 82 – Lewis’s italics). That is, logical analysis is primarily a matter of mapping logical relationships between propositions, not showing how logically complex propositions can be constructed from some arbitrary set of simple ones. Further, because ‘the nature of a concept as such is its internal (essential or definitive) relationships with other concepts’ (Lewis, 1929, p. 83), the same point applies to the analysis of concepts we employ in classifying things, even though the analysis is not logical. Thus analysis of, say, the concepts we employ in classifying thoughts and feelings is primarily a matter of elucidating the internal relationships between them, not fixing on some basic types (belief and desire, perhaps) and then attempting to construct the others on this basis.

It still must be explained how conceptual analysis of this kind contributes to formulating the ‘criteria of reality’ of which Lewis writes in the passage quoted above. But Lewis’s view seems to be that insofar as conceptual analysis provides an interpretation which organises the empirical data into objective knowledge, it provides a classification of things such as thoughts and feelings which enables one to distinguish real items from spurious ones. As Lewis notes in the passage quoted above, however, this role of analysis is
more clearly evident ‘in the case of those basic concepts, determining major classes of the real, which may be called the categories’. Lewis is not very explicit in *Mind and the World Order* (Lewis, 1929) about his own account of this matter, but it must include the classification of the types of experience which confirm the presence of a real thing, and he presents a very detailed account of the ‘terminating judgments’ which provide an analysis of this kind in chapter VIII of *An Analysis of Knowledge and Valuation* (Lewis, 1946). The main features of his account can be grasped from the example he provides (Lewis, 1946, pp. 248–9): it is an ‘analytic consequence’ of the ‘sense-meaning’ of the statement ‘a sheet of real paper lies before me’ that if a visual sheet-of-paper presentation were to be given and I were to move my eyes, then a seen displacement of this presentation would probably follow. This analytic consequence of the statement does not, by itself, constitute its ‘sense meaning’, but it exemplifies the way in which this meaning is constituted by an indefinite series of analytic terminating judgments of this kind which indicate the kinds of hypothetical experience likely to be associated with the presence of a sheet of real paper.

Many aspects of this account are debatable, but the classic objection to it was advanced by Chisholm, who argued that the supposed confirming connection between sense experience and the properties of real objects obtains only subject to certain objective conditions such as the actual spatio-temporal location of the objects in relation to the perceiver. This challenged Lewis’s belief that the sense-meaning of objective judgments could be specified in terms of potential sense experiences, the given ‘as such’, without reference to any such objective conditions. Lewis thought that he could avoid this objection by his appeal to probability, but Chisholm argued that this does not avoid the objection (see Chisholm, 1948, 1968, pp. 236–8, Lewis, 1948; I return to this issue in Section 4 below). Despite this issue, however, it is clear enough what basic categories are employed here, namely action, spatio-temporal location (or at least apparent location), probability and the kind of ‘real connection’, as Lewis puts it (Lewis, 1946, p. 250), which sustains counterfactual conditionals of this kind. (One might well think of causation in this connection.) The resulting position is, therefore, recognisably Kantian, though with the interesting pragmatist addition of action to the list of basic categories, and it is clear both how it exemplifies Lewis’s approach to the a priori and his thesis that in the end, a priori criteria of reality of this kind are analytic.

Another context which Lewis takes to identify important a priori concepts which function as criteria of reality is that of the natural sciences:

In every science there are fundamental laws which are a priori because they formulate just such definitive concepts or categorical tests by which alone investigation becomes possible. (Lewis, 1929, p. 254)
What Lewis says here fits well with the emphasis on natural science which one associates with earlier pragmatists such as C. S. Peirce; but it remains, on the face of it, surprising that Lewis should describe ‘fundamental laws’ as a priori rather than empirical, since he also maintains that ‘principles of the order of natural law are reached by some generalisation from experience’ (Lewis, 1929, p. 262). However, he goes on to qualify this by adding ‘– that is from veridical experience’, and then adds: ‘what experience is veridical, is determined by the criterion of law’. As he acknowledges, this seems to take us round in a circle: from law to veridical experience and thence back to law. However, he holds, this circle reveals the fundamental contribution of natural science to selecting a priori criteria of reality in the light of experience:

\[\text{The determination of reality, the classification of phenomena, and the discovery of law, all grow up together.} \ldots \text{If the criteria of the real are a priori, that is not to say that no conceivable character of experience would lead to alteration of them. (Lewis, 1929, p. 263 – Lewis's italics)}\]

Lewis does not say why fundamental laws of this kind are to be regarded as analytic, but again he seems to be relying on his conception of analysis as the revelation of the internal connections between concepts which gives them their identity. This is similar to regarding the fundamental laws as implicit definitions of the terms employed, and this is what he suggests when he says (in the italicised passage quoted above from Lewis, 1929, p. 231) that fundamental laws formulate ‘definitive concepts’.

Lewis’s claim here, concerning the possibility of experience leading us to alter the scientific a priori principles which provide fundamental classifications of phenomena, introduces a theme which is fundamental to his pragmatist appropriation and modification of Kant’s account of the a priori. For whereas Kant conceived of his account of pure theoretical reason as capturing, once and for all, the a priori principles inherent in the use of theoretical reason to justify claims to knowledge of an objective world in the light of sense-experience, Lewis holds that no such claim is warranted. This disagreement with Kant applies both to the a priori principles which capture the abstract exercise of reason, that is logic, and to those which specify the substantive ‘criteria of reality’. In the first case, Lewis draws on his own experience as a logician when he notes that ‘there are several logics, markedly different, each self-consistent in its own terms…’ (Lewis, 1929, p. 248); in his paper ‘Alternative Systems of Logic’ (Lewis, 1932) he mentions the differences between standard two-valued logic and many-valued systems of logic which reject excluded middle, and also alludes to Brouwer’s different challenge to this principle and thus implicitly to intuitionist logic. One can also think here of the way in which he develops a plurality of systems for strict implication (or, equivalently, modal logic) S1-S5, without
seeking to show that any one such system, such as S5, is uniquely rational (Lewis and Langford, 1932, chapter VI). In the second case, as we have just seen, he recognises that profound changes in scientific theory are likely to bring with them different fundamental classifications of phenomena which count as a priori insofar as they define the concepts employed, and in a striking passage in his paper ‘The Pragmatic Element in Knowledge’ (Lewis, 1926), Lewis expresses this point in a way which anticipates Kuhn’s later description of this phenomenon:

There is hardly a category or principle of explanation which survives from Aristotle or the science of the Middle ages. Quite literally, men of those days lived in a different world because their instruments of intellectual interpretation were so different. (Lewis, 1970, p. 253)²

It is not, however, easy to see how this line of thought applies to the ‘criteria of reality’ inherent in Lewis’s account of terminating judgments, for he does not suggest that there are alternative basic categories which provide radically different general ways of connecting objective judgments with sense experience. His account implies that in any particular case the connections can be made in indefinitely many different ways in the light of all the different ways in which sense-experience might confirm an objective judgment, but this variety does not provide a challenge to our common-sense categories of observation. One might argue that challenges do arise from the use of new instruments such as microscopes and telescopes whose results draw on scientific theories and potentially lead to significant qualification of common-sense judgments; and in principle this is right. However, it is unclear how this line of thought can be applied to Lewis’s account of terminating judgments, since (as Chisholm’s objection to it brings out) in that account Lewis explicitly abstracts from all the objective conditions on sense experience which would be needed to take account of the results of the use of scientific instruments. Hence, it is not clear how, if at all, Lewis’s pragmatic conception of the a priori applies in this context.

Having argued that there are alternative systems of logic, each ‘capable of being of being interpreted in such a way that all of its laws are true, and represent logical principles in an easily definable and quite proper sense’ (Lewis, 1970, p. 401), Lewis infers that we cannot affirm them all, since this would produce ‘not a canon, but a chaos’ (Lewis, 1970, p. 420). Hence, he concludes:

Some choice is necessary: and in the nature of the case, the grounds of choice can only be pragmatic. (Lewis, 1970, p. 420; cf. Lewis, 1929, p. 247).

Lewis generalises this point to all a priori principles, drawing mainly, as before, on the history of science. His line of thought starts from his doctrine
of the given, and the contrast between this and our conceptual scheme with its a priori principles:

when we recognise that without interpretation it is not a world at all that is presented to us, but only, so to speak, the raw material of a world; then may it not plausibly be urged that, throughout the realm of fact, what is flatly given in experience does not completely determine truth – does not unambiguously fix the conceptual interpretation which shall portray it. (Lewis, 1970, p. 253)

We move from this to the need for choice on pragmatic grounds when it is added that the history of science shows how more than one ‘conceptual interpretation’ can be found to make reasonable sense of experience (Lewis, 1970, p. 253). For in such cases, the choice between fundamental theories with their different a priori principles can be made neither on a priori grounds, for these are in dispute, nor on uninterpreted sense experience, the given as such, for this is indeterminate. ‘Pragmatic’ grounds for choice then come in as grounds which are neither a priori nor empirical. But what are they?

The key consideration which is novel is an appeal to our ‘needs and interests’. At first, Lewis puts this point in a way which seems just behaviourist:

In brief, while the a priori is dictated neither by what is presented in experience nor by any transcendent and eternal factor of human nature, it still answers to criteria of the general type which may be termed pragmatic. The human animal with his needs and interests confronts an experience in which these must be satisfied, if at all. Both the general character of the experience and the nature of the animal will be reflected in the mode of behaviour which marks this attempt to realise his ends. This will be true of the categories of his thinking as in other things. (Lewis, 1929, p. 239)

But later, he makes it clear that in this context our ‘intellectual’ interests should have priority:

But certain important ends, such as intellectual consistency and economy, completeness of comprehension, and simplicity of interpretation, occupy a place so much higher, for the long-run satisfaction of our needs in general, that they rightfully take precedence over any purpose which is merely personal or transitory. (Lewis, 1929, p. 267)

So, although we may commonly describe choices made on the basis of short-term personal interests as pragmatic, that kind of pragmatism is misguided. Instead, the clear-minded pragmatist will recognise the priority of intellectual values when making their choice of a priori principles.
Several points arise here. One is just the connection here between the a priori and the exercise of choice. This confirms Lewis's thesis, cited at the start of this section, that ‘the a priori represents the activity of mind itself; it represents an attitude in some sense freely taken’. In turn, however, this raises the question of the truth of the a priori; for it would be a case of wishful thinking to suppose that choice alone can make something true. Lewis’s answer to this challenge is that the relationships between the concepts employed in these principles obtain altogether independent of our adoption of them for the purpose of classification, so that a priori truth is not dependent on our choice, but only its relevance for us. This claim clearly raises important further questions, but I shall postpone discussion of them, since the answer to them is best addressed in the context of Quine’s challenge to analyticity. (see Section 6 below)

A different range of questions is posed by Lewis’s remark above that our intellectual ends ‘rightfully take precedence over any purpose which is merely personal or transitory’. For, we must ask, what account of values, including these intellectual values, does Lewis have? Does his account include a conception of practical reason which supports the ‘rightful precedence’ of intellectual over personal ends? Furthermore, since there appear to be several potentially conflicting values among those favoured by Lewis (e.g., simplicity and completeness), does a Lewisian conception of practical reason permit a plurality of reasonable choices? I shall also return to these questions later (see Section 5); but before addressing them, it is useful to turn briefly to Carnap’s account of analytic truth.

2 Carnap and analytic truth

In ‘Testability and Meaning’ Carnap remarked:

It seems to me there is agreement on the main points between the present views of the Vienna Circle... and those of Pragmatism, as interpreted e.g., by Lewis. (Carnap, 1936, p. 427)

Is this right?

Carnap and Lewis do indeed agree about the existence and importance of analytic truths; but how far does this agreement go? In presenting Lewis's position, I emphasised his Kantian starting point so that although he holds that a priori truths are analytic, what matters most to him is the role of these truths as the a priori structure of the conceptual schemes which enable us to obtain empirical knowledge of a real world. When one turns to Carnap, things seem very different. In works such as The Logical Syntax of Language (Carnap, 1937) Carnap does not use the expression ‘a priori’ and is dismissive of traditional epistemology – it is said to be ‘permeated by pseudo-concepts and pseudo-questions, and frequently in such a way
that their disentanglement is impossible’ (Carnap, 1937, p. 280). Instead, Carnap holds, once these pseudo-questions are swept away, ‘all that remains of philosophy is the logic of science’ (Carnap, 1937, p. 280), and the importance of analytic truths belongs within his account of this ‘logic of science’. As will emerge later, it turns out that epistemological concerns do, in fact, enter into this account, but they are below the surface.

Carnap takes it that developing an account of the logic of science requires one to show how a scientific theory can be formulated as a formalised theory in which the deductive apparatus of logic and mathematics is made explicit alongside the basic laws and the observational basis of the scientific theory in question. Insofar as this deductive apparatus is completely formal, with the requirement in particular that proofs be formalised, Carnap maintains that it constitutes a kind of ‘syntax’, the ‘logical syntax’ of the language to which it belongs. This logical syntax includes axioms and rules of inference (‘L-rules’), and thus also the theorems which can be derived from these primitive sentences by the rules (Carnap, 1937, pp. 30–6). In *The Logical Syntax of Language* Carnap develops several ‘languages’ with different syntactic and logical resources to indicate how a scientific theory might be formalised; but the guiding principle throughout is that ‘The logic of science (logical methodology) is nothing other than the syntax of the language of science’ (Carnap, 1937, p. 7 – Carnap’s italics).

Because the syntactic specification of each language includes a logical theory, the theorems of that logic are internal to the language; and it is in the first instance these theorems that are said to constitute the ‘analytic’ sentences of the language. In calling them ‘analytic’, Carnap alludes to Kant and Frege (Carnap, 1937, p. 44), and he suggests that by means of his ‘syntactic’ conception of logic he can provide a precise account of the way in which analytic sentences are those which are ‘true on logical grounds’ (Carnap, 1947, p. 41). However, writing as he is in the aftermath of Godel’s incompleteness theorem (Carnap, 1937, p. 100), but with an enduring commitment to the logicist presumption that mathematics is logic, he accepts that where a language is rich enough to accommodate arithmetic, there will be arithmetical, and thus logical, truths which are not straightforward theorems of the logical syntax of the language. Carnap nonetheless takes it that it is legitimate for him to classify these sentences as analytic, since, he holds, they can be established by ‘a method of deduction which depends upon indefinite individual steps, and in which the number of the premises need not be finite’ (Carnap, 1937, p. 100).³

So far, it is only the analyticity of logic that is in question, and in *The Logical Syntax of Language* that is how the matter rests. Nonetheless Carnap recognises that a language suitable for the expression of a scientific theory such as physics as a formalised theory will need to include an appropriate vocabulary (‘P-terms’), the basic laws (‘P-primitive sentences’), and also some transformation rules (‘P-rules’) which permit the derivation from P-sentences
of protocol sentences ‘by means of which the results of observation are expressed’ (Carnap, 1937, p. 317). By means of these extensions to the syntax of the language, including the extra-logical premises and rules, it will be possible to derive new, non-logical, theorems which Carnap calls ‘P-valid’, and Carnap recognises that these P-valid sentences of the extended language have much the same status as the analytic sentences of the purely logical language. For –

It is a matter of convention whether we formulate only L-rules, or include P-rules as well; and the P-rules can be formulated in just as strictly a formal way as the L-rules. (Carnap, 1937, p. 186)

In later writings such as *Meaning and Necessity* (Carnap, 1947) Carnap, having assimilated Tarski’s semantical definition of truth, switched from the syntactical approach of *The Logical Syntax of Language* to one in which analyticity is defined in semantical terms. On this approach, semantical rules are introduced to define truth-conditions for the atomic sentences of a language, and Carnap takes it that maximal sets of these atomic sentences or their negation (but not both) constitute state-descriptions; he then adds further semantical rules to define the truth-conditions of complex sentences in terms of simpler ones and thus determine whether they hold in a given state-description. Finally, he asserts that a sentence is ‘L-true’ where it holds in every state-description (Carnap, 1947, p. 10), and it is L-truth that is now held to provide an explication of analyticity (Carnap, 1947, p. 8). This conception of analyticity is dependent upon the semantical rules for the language, and where these deal only with atomic sentences and logical constants, it is only logical truths which will qualify as analytic. But Carnap now allows that there can be other semantical rules, for example, a rule giving the same extension to ‘human’ and ‘rational animal’, which implies that the sentence ‘All humans are rational animals’ is analytic (Carnap, 1947, p. 15). Thus, analyticity is here extended to apply beyond logical truth. The further extension to count the basic laws and transformation rules of a formalised scientific theory (‘P-sentences’ and ‘P-rules’) as described in *The Logical Syntax of Language* as semantical rules is not explicitly envisaged in *Meaning and Necessity*. But there seems no reason, in principle, why this should not be permitted, and in ‘Meaning Postulates’ (Carnap, 1952) Carnap allows that ‘All ravens are black’ can indeed be employed as a ‘meaning postulate’, which is the term that here replaces ‘semantical rule’ as an explication of analyticity. Thus result of all this is that ‘analytic truth in a language’, or rather its explications, ends up with much the same extension for Carnap as it has for Lewis, except that for Lewis the relativity is to a conceptual scheme. Even Lewis’s analytic terminating judgments have an analogue among the P-rules which connect P-sentences with protocol sentences, especially once these transformation rules are formulated in the complex way envisaged.
by Carnap in ‘Testability and Meaning’ (Carnap, 1936, 1937), though there remain important differences between Lewis and Carnap in this area, to which I return below. But the route to this common extension seems, on the face of it, different in the two cases: for Lewis, it is a post-Kantian conception of the a priori informed by an enriched conception of logical and conceptual analysis; whereas for Carnap, it is bound up with the project of providing a ‘logic of science’ that is fit for the scientific world-conception advanced by the Vienna Circle.

Yet, one of the main purposes of Carnap’s logic of science is to identify the observational basis of a scientific theory by specifying protocol sentences which are derivable from the theory’s laws, hypotheses and transformation rules and which express the content of possible observations; these protocol sentences show how the theory can be tested (Carnap, 1937, p. 318). Hence, it turns out that Carnap’s logic of science can be regarded as a way of fulfilling Lewis’s project of identifying the structure of the conceptual schemes which enable us to obtain objective knowledge of a real world on the basis of sense experience; for the primary instances of objective knowledge of this kind are precisely the natural sciences. As a result, just as Lewis’s enriched conception of analysis leads him to maintain that all a priori truths are analytic, the overall purpose of Carnap’s logic of science makes it tempting to attribute to him the view that the analytic truths of a language in which a science is expressed constitute the a priori element of the science. However, Carnap never says this, and given the traditional associations of the term ‘a priori’, it is best not to characterise his position in this way.

Nonetheless, the similarity between their positions is enhanced in other ways. Like Lewis, Carnap acknowledges that there are alternatives to the standard logic of Frege and Russell, such as the intuitionist logic of Brouwer and Heyting (Carnap, 1937, pp. 46–8). Carnap’s initial response to this situation is his ‘Principle of Tolerance’ that ‘In logic, there are no morals’ (Carnap, 1937, p. 52). By itself, this treats different logics as if they were different religious faiths and does not suggest any way of choosing between them. But once a logic is put to use as a logic of science within the formalisation of a scientific theory, Carnap allows that pragmatic considerations have a legitimate place. Their place arises from the role of logical syntax in connecting the basic laws of the science and the rules which specify the observational content of these laws with protocol sentences. Carnap spells this out in connection with physics:

*The construction of the physical is not effected in accordance with fixed rules, but by means of conventions.* These conventions, namely, the rules of formation, the L-rules, and the P-rules (hypotheses), are, however, not arbitrary. The choice of them is influenced, in the first place, by certain practical methodological considerations (for instance, whether they make for simplicity, expedience, and fruitfulness in certain tasks). This
is the case for all conventions, including, for example, definitions. But in addition the hypotheses can and must be tested by experience, that is to say by the protocol-sentences – both those that are already stated and the new ones that are constantly being added. (Carnap, 1937, p. 320)

As this passage indicates it is not just the choice of P-rules which is grounded in pragmatic considerations (‘simplicity, expediency, and fruitfulness in certain tasks’); the point applies also to the L-rules of logic. But whereas the chosen P-rules are then to be tested by experience, the implication seems to be that no such test applies to the L-rules, so that logic retains a special status. However, in the following passage Carnap qualifies this implication:

No rule of the physical language is definitive; all rules are laid down with the reservation that they may be altered as soon as it seems expedient to do so. This applies not only to the P-rules but also to the L-rules, including those of mathematics. In this respect, there are only differences in degree; certain rules are more difficult to renounce than others. [If, however, we assume that every new protocol-sentence which appears within a language is synthetic, then there is this difference between an L-valid, and therefore analytic, sentence $S_1$ and a P-valid sentence $S_2$, namely, that such a new protocol-sentence – independently of whether it is acknowledged as valid or not – can be, at most, incompatible with $S_2$ but never with $S_1$. In spite of this, it may come about that, under the inducement of new protocol-sentences, we alter the language to such an extent that $S_1$ is no longer analytic.] (Carnap, 1937, pp. 318–9)

As the passage within square brackets indicates, while there is no question of logic directly failing the test of experience, the application of pragmatic criteria to the choice of logic implies that even logic is not ‘immune to revision’ (as Quine would put it: Quine, 1953, p. 43) under the ‘inducement’ of experience. For Lewis, the situation is, I think, similar. On the one hand, he maintains that the laws of logic are ‘independent of the given because they impose no limitations whatever upon it’ (Lewis, 1929, p. 247). On the other hand, because he maintains that our adherence to principles such as excluded middle ‘represents only our penchant for simplicity and similar considerations’ (Lewis, 1929, p. 247) he ought to allow that if we came to see that our experience was such that considerations of simplicity and completeness favoured a different choice, then we should make the change. Thus, when considering the application of pragmatic criteria to the choice of arithmetical principles in a situation of this kind, he writes that:

Our present categories would not – could not – be prohibited but other modes might more simply reduce the phenomenal to order and facilitate control. (Lewis, 1929, p. 252)
3 Disagreements

So far, I have been describing the main points concerning which Carnap and Lewis are in substantial agreement, despite their different starting-points, vocabularies and intellectual styles. In 1941, Lewis wrote a paper ‘Logical Positivism and Pragmatism’ (Lewis, 1970, pp. 92–112) comparing his position with that of Carnap, whom Lewis takes to be exemplary. After briefly noting the general similarity between their positions, Lewis chooses to dwell on the differences between them. Lewis begins by questioning Carnap’s anti-metaphysical rhetoric; he suggests that Carnap’s thesis that philosophy is just the logic of science is metaphysical, as is his physicalist thesis that all natural laws are reducible to the laws of physics. Lewis also criticises Carnap’s expressivist, non-cognitive, treatment of ethics and sketches briefly his own very different empiricist theory of value and rationalist theory of norms. This disagreement is potentially of some relevance here, since it is important for both Lewis and Carnap that they should have an account of the rational grounds for pragmatic choices, and I shall come back to it later. But the details of Lewis’s naturalistic theory of valuation (as set out in Lewis 1946, Book III), interesting though they are, are not central to his pragmatist conception of the a priori.

There is, however, one further point of greater import. Lewis complains that Carnap’s account of empirical confirmation is inadequate because it ends with the affirmation of certain sentences, protocol-sentences, rather than with the occurrence of sense experiences whose content confirm that which has been affirmed. Lewis first expresses his own position:

What determines the observation-statement to be true or credible, and thus confirms (partially) the statement the statement to be confirmed, can be nothing but the content of an empirical observation. The observation-statement is found ‘acceptable’ if the empirical presentation accords with what the ‘observation-statement’ asserts. It is no logical relation to any other statement which is here in question; (Lewis, 1970, p. 98)

Hence, he continues, with Carnap now in mind:

An analysis of confirmation cannot be given in statements in the formal mode alone, because confirmation does not end in what observation-statements mean but in the determination of them as true or credible, by experience. To leave that obvious fact out of a supposedly empiricist theory of verification or confirmation is to give us Hamlet without the Prince. (Lewis, 1970, p. 98)

This complaint is partly a misunderstanding, but with a substantial disagreement behind it. The misunderstanding concerns the role of
protocol-statements; these are for Carnap statements in which ‘the results of observation are expressed’. So, they are not uninterpreted sentences awaiting a further account of their experiential assertibility-conditions (‘Hamlet without the Prince’). Instead, their meaning is precisely that they are assertible when they express the contents of observation, or experience. But it is here that the substantial disagreement arises: whereas Lewis takes the content of experience to be, fundamentally, a matter a subjective sensory qualia (‘the given as such’), Carnap takes it that sense experience is fundamentally the presentation of aspects of the world as it is objectively (even though mistakes are inherently possible). In *Aufbau* (Carnap, 1928) Carnap had in fact held a view similar to that of Lewis, but under the influence of Neurath (Neurath, 1932) he rejected it in favour of an objectivist conception of the contents of experience. Lewis held (Lewis, 1970, pp. 98–9) that no such observations provide the kind of certainty that is needed when the issue of empirical confirmation is at stake: for ‘if anything is to be probable, then something must be certain’ (Lewis, 1946, p. 186). However, following Popper (Popper, 1935), Carnap argued that complete certainty is not necessary for empirical confirmation.\(^6\)

Since one of the main reasons Lewis gives for maintaining his conception of the given is that it alone offers the promise of certainty (Lewis, 1946, p. 28), this disagreement marks a major difference between his position and Carnap’s. Indeed, since Lewis’s account of the a priori element in our conceptual schemes is that it provides a conceptual interpretation of experience as experience of what is real which experience does not of itself provide, this disagreement leads one to wonder how far Lewis’s conception of the a priori would be undermined if his conception of the given were to be rejected. Does Carnap’s position show what Lewis-without-the-given would look like? I shall consider this question in the light of Quine’s criticisms of both Lewis and Carnap in ‘Two Dogmas of Empiricism’.

4 Lewis, Carnap, and Quine’s two dogmas

Quine’s ‘two dogmas’ are, first, the analytic/synthetic distinction and, second, ‘reductionism’, the thesis that statements about the physical world are reducible to statements about immediate experience. In the present context, it is convenient to take this second dogma first. Quine remarks that once Carnap had moved on from the position adopted in *Aufbau* his work does not straightforwardly exemplify this dogma (Quine, 1953, p. 40);\(^7\) but he then adds the following paragraph:

But the dogma of reductionism has, in a subtler and more tenuous form, continued to influence the thought of empiricists. The notion lingers that to each statement, or each synthetic statement, there is associated a unique range of possible sensory events such that the occurrence of any
of them would add to the likelihood of truth of the statement, and there is associated also another unique range of possible events whose occurrence would detract from that likelihood. (Quine, 1953, pp. 40–1)

Although Quine does not say so, Lewis's theory of the terminating judgments which give the ‘sense-meaning’ of objective judgments fits this description. Quine’s objection to this position is that the confirmation and ‘infirmation’ (probable refutation) of a statement are always conditional upon other assumptions which can themselves be confirmed or infirmed in further ways – so that there is no well-defined range of possible sensory events to provide that statement’s sense-meaning. This objection resembles Chisholm’s objection, mentioned earlier, that Lewis’s account of the sense meaning of objective judgments is undermined by the role of further objective conditions concerning the perceiver’s location (and sensory systems) in determining the way in which sense experiences confirm or infirm these objective judgments. For the role of these objective conditions obviously obstructs the identification of ‘a unique range of possible sensory events’ which exhaust the statement’s confirmation and infirmation conditions; and however much one were to attempt to spell out the sense meaning of these objective conditions, Chisholm’s argument implies that further objective conditions would be introduced.

Assuming that this objection is correct (as seems to me to be the case), one should ask how much damage it does to Lewis’s position overall. By undermining his account of the terminating judgments which specify the empirical content of objective judgments, this objection poses a serious challenge to Lewis’s conception of the given. For the hypothesis that the sense experiences on which we rely for empirical knowledge are fundamentally constituted by the given must be wrong. Instead, it will have to be accepted that such sense experience is in some respects inescapably conceptual, or at least intentional. It does not follow from this that all aspects of Lewis’s conception of the given element in sense experience must be abandoned; further arguments are needed to confront the very idea of sensory qualia. Nor does it follow that there is no a priori element in the content of sense experience. On the contrary, the way lies open for Kantians to demonstrate that space, time, causation and identity are inescapable a priori ingredients of the content of the kind of sense experience which enables beings such as us to gain objective empirical knowledge of a real world. But what is rejected here is the suggestion that these a priori concepts are aspects of a conceptual scheme that we have freely chosen to employ in order to make sense of a pure qualitative stream of sensory consciousness, and in this respect, therefore, the pragmatist side of this aspect of Lewis’s theory of the a priori has to be abandoned.

On the face of it, however, this change does not undermine all aspects of Lewis’s pragmatism concerning the a priori. Carnap’s position suggests that
abandoning a purely qualitative conception of sense experience is compatible with maintaining pragmatism concerning a priori elements of logic and natural science. According to Quine, however, there are deeper implications here. The objection to reductionism was that one cannot assign empirical content to particular statements one by one because of the inter-relationships between the objective claims made by these statements and other objective conditions as well as potentially confirming or infirming sense experiences. Quine now argues that once the existence of these interrelationships is acknowledged, the whole business of drawing distinctions within a body of beliefs between the a priori and the empirical, or within a language of science between the analytic and the synthetic, becomes unsustainable. So, the untenability of the first dogma, the analytic/synthetic distinction, is implicated in the untenability of the second one: ‘the two dogmas are, indeed, at root identical’ (Quine, 1953, p. 41).

When one looks to Carnap’s way of deploying the analytic/synthetic distinction, this claim at first seems odd. Post-Aufbau, Carnap was certainly innocent of the second dogma, and in The Logical Syntax of Language he explicitly acknowledges Duhem’s insight that auxiliary hypotheses are liable to become entangled in the testing of single statements, so that, as he puts it: ‘the test applies, at bottom, not to a single hypothesis but to the whole system of hypotheses (Duhem, Poincare)’ (Carnap, 1937, p. 318 – Carnap’s italics). Nonetheless, Carnap remained strongly committed to the task of clarifying the inferential connections between scientific hypotheses and the protocol sentences which might confirm or infirm them; and he took it that this was best achieved by giving a special status to logic (L-rules) and the fundamental principles of the science in question (P-rules). Why, then, does Quine think that this position is untenable? His reason is that he thinks that there is no basis for a distinction in kind between the ordinary business of revising one’s beliefs in the light of infirming experience and the kind of radical revision of logic or the fundamental principles of scientific theory in the light of experience, which as pragmatists, both Carnap and Lewis explicitly allow for. In the first case, where the beliefs are empirical or synthetic, no change of conceptual scheme or language is involved; in the second case, where the principles are analytic or a priori, such a change is said to be called for. And yet, in both cases, it is ultimately a conflict with experience that prompts the change, and pragmatic considerations which determine what changes to make. Quine, of course, concedes that there are differences of degree here; but he argues that this is all that is involved. It is already an implication of Duhem’s observation concerning the role of auxiliary hypotheses in testing scientific hypotheses that when a series of tests produce a negative result, there is always in principle a range of revisions in belief to be considered, from simply discounting the type of measurement involved to changes in these auxiliary hypotheses. Adding the possibility of changes to rules or principles previously set apart as analytic or a priori is
just climbing one step higher up the ladder of revision, not throwing away
the ladder to start something altogether different.

In thinking about this challenge, it is, I think, sensible to separate the
question of the status of the fundamental scientific principles from that of
the status of logic.

5 Pragmatism, the a priori and empirical science

As we have seen, Lewis holds that the fundamental classifications intro-
duced by the sciences provide an a priori framework for the interpretation
of experience; but he also holds that this framework is revisable when ‘the
classification of experience’ makes it appropriate to do so because the pragmatic
criteria of consistency, completeness, and simplicity are better satisfied by
an alternative conceptual scheme (Lewis, 1929, p. 263). Quine’s challenge
is that in allowing for revisions of this kind Lewis ought to have recog-
nised that all he was dealing with all along were empirical beliefs, more
or less deeply entrenched, instead of taking it that the change amounted
to the adoption of a new a priori conceptual scheme. In considering this
challenge, it is worth observing at the start that Lewis’s conception of the
given provided him with a reason for his position. For, if experience as such
is non-conceptual and requires interpretation in the light of a conceptual
scheme provided by rational agents, the resulting radical dualism of ‘scheme
and content’ (Davidson’s ‘third’ dogma – Davidson, 1974) does motivate the
assignment of a non-empirical status to those fundamental aspects of the
conceptual scheme which give structure to the conceptual interpretation of
the given as experience of a real world; and among these aspects, the funda-
mental classifications of natural kinds provided by the natural sciences are
indeed good candidates for this status. But for the reasons discussed earlier,
Lewis’s conception of the given is vulnerable to Quine’s criticisms of the
second dogma; so, in the context of considering whether Lewis has an effec-
tive response to Quine’s criticisms of the first dogma, invoking the given is
not much use. And once it is abandoned, and with it the radical dualism of
scheme and content, the distinction between fundamental principles and
ordinary hypotheses does indeed look to be just a difference of degree, not
kind, at least as far as epistemological status is concerned. One may still
want to argue that the classification of natural kinds implied by funda-
mental scientific principles has a privileged metaphysical status insofar as
it reveals the essential properties of items of these kinds. But this is a quite
different issue from that raised by Lewis’s treatment of fundamental scien-
tific principles as a priori: the point of the example ‘Water = H₂O’ is that
despite being empirical and synthetic it is (arguably) necessary because it
reveals the essence of water.

A different way of approaching the thesis that there is a distinction in
kind here is suggested by Carnap’s argument in ‘Empiricism, Semantics,
and Ontology’ (Carnap, 1950). Carnap here sketches a variety of ‘linguistic frameworks’ each of which includes a suitable vocabulary for entities of some kind, some basic principles concerning them and some rules for testing statements concerning them. This is basically a generalised version of the conception of languages and P-rules introduced in The Logical Syntax of Language, which Carnap here applies to what he calls ‘the problem of abstract entities’. But what is novel about the approach taken in this paper is that Carnap uses it to introduce a distinction between ‘internal’ and ‘external’ questions. Internal questions use the principles and rules internal to the linguistic framework, whereas external questions concern the merits and defects of a linguistic framework. Carnap treats the distinction between internal and external questions as a distinction in kind, such that external questions are ‘non-cognitive’ (Carnap, 1956, p. 214) but to be answered by pragmatic criteria which assess whether a linguistic framework is ‘more or less expedient, fruitful, and conducive to the aim for which the language is intended’ (Carnap, 1956, p. 214). The answers to internal questions which are posed from within a given linguistic framework, by contrast, may be found by ‘purely logical methods or by empirical methods’ (Carnap, 1956, p. 206); and these questions are, therefore, cognitive.

Since Carnap’s distinction is based on his conception of linguistic frameworks, there needs to be a distinction within each linguistic framework between the framework principles which fix the identity of the framework and further statements made within the vocabulary of the linguistic framework whose acceptance is not required by the linguistic framework but can be assessed by the logical and empirical methods specified by the framework. This distinction between framework principles and the rest is clearly comparable to the analytic/synthetic distinction, so if the internal/external question distinction is robust, it will provide a reason for holding on to an analytic/synthetic distinction and, by implication, an a priori/empirical distinction, too.

Carnap's distinction between internal and external questions is certainly one which many philosophers have found useful. For example Barry Stroud's discussion of scepticism in The Significance of Philosophical Scepticism (Stroud, 1984) is constructed around the distinction between internal and external doubts. For Stroud, what is distinctive about G. E. Moore is that he was deaf to external doubts; but the value of his stance is that he thereby raised the issue as to whether external doubt really makes sense at all. Yet, when one thinks more about this case, the thesis that there is a distinction here of kind, as opposed to degree, is hard to sustain. The structure of potent sceptical arguments is always to move from more or less familiar ‘internal’ doubts such as the possibility that one might be dreaming to more hyperbolical ‘external’ doubts concerning one’s subservience to an evil genius or being a brain in a vat; and the merits of the arguments precisely arise from the fact that there does seem to be no more than a difference of degree
between these cases. So, Carnap’s distinction, useful though it is, does not need to be regarded as a distinction of kind; and, indeed, once it comes to be connected with the cognitive/non-cognitive distinction, as it is by Carnap, its value is radically diminished. For the implication that all external questions exclude cognitive considerations appears to undermine the possibility of rational debate in philosophy; and, equally, the implication that all cognitive judgments are internal to a linguistic framework implies a potentially problematic form of relativism concerning the possibility of knowledge.

The pragmatic value of Carnap’s internal/external distinction can be seen, therefore, as a reason for rejecting Carnap’s own account of it. One option here would be to offer an alternative account. In his reply to his critics Lewis remarks, concerning criticism of the analytic/synthetic distinction (without mentioning Quine by name but with him clearly in mind):

I wish to acknowledge that the whole body of my philosophic conceptions, in logic, epistemology, theory of value, and even ethics, depends on the validity of this distinction; and if that plank is pulled from under me, the whole structure will come tumbling down. (Schilpp, 1968, p. 659)

In effect, Lewis is complaining that without the analytic/synthetic distinction, his external ‘philosophic conceptions’ will come tumbling down, and just be replaced by ‘internal’ scientific theories – ‘physics and psychology’, as he puts it (Schilpp, 1968, p. 659). But the problem Lewis faces here, if he wants to defend his conception of philosophy and the possibility of rational debate in philosophy, is not just that thrown up by the difficulties inherent in his account of the given; it is his whole pragmatist approach to the a priori. For what Quine’s arguments strongly suggest is that pragmatic considerations sustain only a difference in degree, not one of kind. Indeed, if external philosophical questions are not to be treated as non-cognitive challenges, in the way in which Carnap treats them, then they need to be recognised as questions inviting radical revisions of an established conceptual scheme which are not in principle different from questions concerning the modest revisions in belief which arise from ‘internal’ challenges. A better response to Quine, therefore, would be to accept this conclusion, but then to explore the way in which a more modest, pragmatically grounded and contextual a priori/empirical distinction remains of value in articulating priorities within the web of belief.

Lewis, of course, does not attempt this, and this is not the place for me to make a detailed attempt on his behalf. But a useful place to start is provided by Kuhn’s famous distinction between normal science and scientific revolutions (Kuhn, 1962). I noted earlier that there is a striking similarity between Lewis’s account of radical changes in scientific belief and that presented by Kuhn, and Kuhn’s distinction is obviously a variant on the internal/
external distinction. But Kuhn’s distinction is not Carnap’s. For Kuhn was very insistent that ‘revolutionary’ changes are not irrational, non-cognitive, developments; instead there is an established rationality to them which draws on the judgment of the scientific community (see Kuhn, 1970a, pp. 259ff). Furthermore, as Kuhn acknowledged in the ‘Postscript’ to the 1970 enlarged edition of *The Structure of Scientific Revolutions* (Kuhn, 1970b), once one looks in any detail at the history of a science, it becomes apparent that there is no sharp distinction in kind between revisions in established scientific theory and practice on the one hand and radical revolutionary changes on the other. So, although the paradigm shifts that are characteristic of certain periods in the history of science should not be counted as changes in a priori conceptual scheme as conceived by Lewis, Kuhn’s conception of a paradigm (or ‘disciplinary matrix’ – Kuhn, 1970a, p. 271) offers an attractive model for a modest, but defensible, version of Lewis’s a priori conceptual schemes, at least insofar these are based on the fundamental principles of scientific theory.

6 Logic

Lewis, Carnap, and Quine agree that there are alternative systems of logic and that the choice between them is to be made on pragmatic grounds. Quine is clear that these grounds include considerations taken from the empirical sciences:

Revision even of the logical law of the excluded middle has been proposed as a means of simplifying quantum mechanics (Quine, 1953, p. 43)

Similarly, for Carnap, ‘under the inducement of new protocol-sentences’ we may choose to revise our logic (Carnap, 1937, p. 319). And although Lewis is less clear on this point, he allows that it could be rational for us to revise our arithmetic, and thus our logic, if thereby we were able ‘more simply reduce the phenomenal to order and facilitate control’ (Lewis, 1929, p. 252).

Fifty years later, this line of thought no longer seems convincing. The only significant proposal for a revision of logic on empirical grounds has been that for the revision, not of excluded middle as mentioned by Quine, but of de Morgan’s rule for distributing disjunction within conjunction (which states that ‘A & (B v C)’ is equivalent to ‘((A & B) v. (A & C))’ in an attempt to address puzzles in quantum theory (Putnam, 1980). But this proposal has not stood the test of time: addressing the problems of interpreting quantum theory requires sophisticated mathematical theories, not a simple modification of logic (Maudlin, 2005). At the same time, however, the pace of research in logical theory has not slackened, and there are plenty of new alternative systems to consider – most notably, dialethic logic which tolerates explicit contradiction in order to accommodate the semantic paradoxes...
(Priest, 2002). So, the issue of the grounds for choice recurs: if empirical considerations are largely irrelevant, what other grounds are there?

In some cases, the introduction of semantical considerations has helped to advance the debate by clarifying the logical significance of different interpretations of key logical operators. Thus, in the case of the choice among Lewis’s systems for modal logic, S1–S5, the issue has been considerably advanced by the introduction of semantic techniques which reveal that the differences between Lewis’s systems match different assumptions about the relative accessibility of possible worlds to each other (given the role of accessibility in determining the truth-conditions of modal claims). S5 emerges, for example, as a strong candidate for the system most appropriate for ‘metaphysical’ necessity, truth in absolutely all possible worlds; whereas once one allows that natural laws vary from one possible world to another in such a way that accessibility is not symmetric, it is plausible to hold that S4 is the system of modal logic appropriate for natural necessity. So, although much here remains disputed (including, most notably, the significance of reference to possible worlds), it looks as though in cases of this kind a choice between different systems of logic can often be made on broadly pragmatic-cum-conceptual grounds: given an interpretation of the key logical concepts, it is determinate which system of logic best satisfies the criteria of soundness, completeness, and simplicity.

But not all differences in logical theory can be handled in this way. Both Lewis and Carnap mention the intuitionist critique of classical logic which focuses on the validity of excluded middle. This is an issue which divides Lewis’s pragmatic criteria: on the one hand, considerations of simplicity clearly favour classical logic; but, on the other hand, the pragmatist emphasis on the connections between meaning, verification and the consequences of judgment favours the intuitionist emphasis on the importance of proof. Similarly, semantical considerations tend to favour classical logic, whereas proof-theoretic considerations tend to favour intuitionistic logic, especially since the intuitionist rules of inference for negation have a kind of harmony which the classical rules appear to lack (though this may just be a matter of the way in which they have been formulated – see Rumfit, 2000). The result of debate on this issue has certainly been a much deeper understanding of the issues at stake, but without a decisive argument in favour of either side. Furthermore, this is not an issue amenable to the kind of disambiguating solution that helps in the case of different systems of modal logic; for although one can distinguish classical from intuitionist negation, the issue remains as to which concept should be used in logical inference. Carnap’s account of external questions suggests that we might just accept that there is a variety of competing intellectual ideals in play here, with no rational grounds for choice between them, so that choice is just the exercise of a non-cognitive preference for one system rather than another. But non-cognitive pluralism in this area is unsatisfactory, since it
provides no answer to the question of whether mathematical proofs and techniques which rely on classical assumptions are acceptable or not. So, the task remains to find arguments that enable one to line up the pragmatic criteria on just one side of the dispute, rather than leaving them divided, as at present.\(^9\)

Although the classical/intuitionistic dispute in logic remains unresolved, one conclusion can, nonetheless, be drawn from it – namely, that Quine was wrong to think that disputes in logic differ only in degree from empirical disputes in the natural sciences. This dispute, at any rate, has been conceptual through and through, fought out over questions about the relationships between meaning, truth, proof, and inference. Hence, it suggests that even if pragmatic considerations are inescapable when one confronts choices between alternative logical systems, these considerations need not be thought of as dependent upon broader empirical considerations. Instead, the issue is better thought of as a priori and analytic (in Lewis’s sense, where logical analysis is a matter of displaying connections, not decomposition). However, if one does return in this way to a pre-Quinean attitude to logic, there is a further Quinean objection that needs to be considered, namely that this attitude commits one to the untenable doctrine of ‘truth by convention’ (see Quine, 1936, 1960).

Quine writes mainly, of course, of Carnap, who maintained that the adoption of the L-rules, the logical system, definitive of a language is a matter of ‘convention’ (cf. Carnap, 1937, p. 320). Lewis tends to write instead of ‘stipulation’: here is a characteristic passage from an early paper (Lewis, 1923):

\[
\text{What is } \text{a priori} \text{ is necessary truth not because it compels the mind’s acceptance, but precisely because it does not. ... The } \text{a priori} \text{ represents an attitude in some sense freely taken, a stipulation of the mind itself, and a stipulation which might be made in some other way if it suited our bent or need. Such truth is necessary as opposed to contingent.}
\]

The traditional example of the \text{a priori} par excellence is the laws of logic. (Lewis, 1970, pp. 231–2)

This passage from Lewis makes it clear, however, that his position was not that logic involves ‘truth by stipulation’; his suggestion appears to be rather that logic is ‘necessary by stipulation’. The distinction here is important. Suppose that excluded middle (‘A v not-A’) is at issue: it is unproblematic to hold that, like any disjunction, the truth of ‘A v not-A’ depends on the truth of ‘A’ or of ‘not-A’, and nothing special needs to be stipulated to secure this. The point of stipulation, or convention, is rather that these cases – A/not-A – are to be treated as exhaustive, and thus that the truth of ‘A v not-A’ is necessary. This clarification deflects Quine’s objection to the Lewis–Carnap account of logic as analytic, which was that it is a mistake to regard the truth of an analytic statement as different in kind from that
of synthetic statements insofar as the former is supposed to depend merely upon linguistic convention/stipulation (Quine, 1953, p. 41). For the positions of Lewis and Carnap require no such distinction with respect to truth. However, the claim that the necessity of analytic truths of logic depends in some way on linguistic convention/stipulation now needs discussion instead. In addressing it, I shall just concentrate on Lewis’s discussion of this matter.

In the abstract, there are three main ways of accounting for necessity. (i) One can take it, as Lewis put it in the passage quoted above, that necessity expresses an ‘an attitude in some sense freely taken, a stipulation of the mind itself’. On this view, to affirm ‘Necessarily A’ is to express one’s determination to hold ‘A’ true. (ii) One can take it that necessity is not an expression of a distinctive attitude by the subject but just depends on the concepts employed. So, a statement ‘A’ is necessary where the concepts which occur in ‘A’ are such as to guarantee the truth of ‘A’ in all contexts. (iii) One can take it that necessity is not a matter of attitudes or concepts at all; instead it is inherent in the ‘essential’ properties of objects. So, a statement ‘A’ is necessary where the properties and objects referred to in ‘A’ are such as to guarantee the truth of ‘A’ in all contexts.

The third position is rejected by Lewis without argument: he writes

> It is, of course, meaningless to speak of the essence of a thing except relative to its being named by a particular term. (Lewis, 1946, p. 41)

So, as far as the interpretation of Lewis is concerned, therefore, the choice lies between positions of the first two kinds. The passage quoted earlier concerning stipulation suggests that Lewis held a position of the first kind. However, that passage appears to be an incautious early exaggeration of his real view. For only three years later, in ‘The Pragmatic Element in Knowledge’ (Lewis, 1926), Lewis clearly affirms a position of the second kind in which the role of the will is restricted to the selection of concepts and does not underpin the relationships between them:

> We select, or call down from Plato’s heaven, those concepts which meet our needs. Plato said we are ‘reminded’ of them by experience; we are more likely to say that we invent or formulate them ourselves. In either case, two points are to be remarked: first that the logical relations of – and hence the truth about – any determinate concept is fixed and eternal and independent of experience; second, that what concepts we shall use of apply we are left to determine ourselves in the light of our needs and interests. (Lewis, 1970, p. 255)

Despite passages in *Mind and the World-Order* in which Lewis appears to revert to the first position (‘These [consequences of definition] are
necessarily true, true under all possible circumstances, because definition is legislative’ – Lewis, 1929, p. 240) his settled view appears to be of the second kind (‘Categories and precise concepts are logical structures, Platonic ideas; the implications of them are eternal...’ – Lewis, 1929, p. 269). This impression is reinforced by the position unequivocally affirmed in An Analysis of Knowledge and Valuation:

That the verbal expression ‘All squares are rectangles’ conveys a logically necessary fact, could not be determined in entire independence of what the constituent expressions ‘square’ and ‘rectangle’ convey, but the fact of the relation expressed by ‘All squares are rectangles’ has no dependence upon our conventions of expression or even on the existence of language. No manner of devising a system of language could affect it, and no decision or ours could make it otherwise than it is. (Lewis, 1946, p. 153)

Lewis’s adoption of this Platonist position is disappointing and appears somewhat at odds with his pragmatist approach to philosophy – which is why in Mind and the World-Order there is a tension between the pragmatist rhetoric in which he emphasises the ‘legislative’ role of the mind and his Platonist account of necessity in terms of mind-independent relations between concepts. As the passage quoted here from An Analysis of Knowledge and Valuation indicates, his position involves a sharp distinction between questions of meaning (‘Which concepts are expressed?’) and questions of fact (‘What are the relations between them?’) and thus assumes a form of the analytic/synthetic distinction. We have seen that Quine’s objection to the use of this distinction here, namely that it fails to take account of the way in which apparently analytic logical truths are revisable in the light of experience, is off-target. But Wittgenstein’s rule-following discussion in Philosophical Investigations (Wittgenstein, 1953) suggests a different critical response to Lewis’s position.

Lewis holds that the ‘conventions of expression’ suffice to determine which Platonic concepts are being expressed, and thus how the concepts expressed are to apply to previously unencountered cases. Wittgenstein’s argument implies, however, that no explicit conventions (or stipulations) can determine this; they always leave it indeterminate which Platonic concept is being expressed, e.g., whether ‘+’ is to be interpreted in such a way that ‘1000 + 2 = 1004’ is correct or not (Wittgenstein, 1953, 185–7). This appears to invite the sceptical conclusion that the way in which we express concepts in language fixes nothing since any answer can be made to accord with the ways in which the terms have been used. Wittgenstein, however, offers a way out, the thesis that obeying the rules of a language is not, as Lewis supposes, just a matter of following stated conventions and stipulations, but also, and more deeply, a matter of participating in a ‘practice’
in which language is used in a variety of contexts by a variety of speakers whose background agreements and disagreements alone make it possible to determine which concepts are being expressed in the language. Once that is determined, one can treat these concepts as Platonic ‘rails invisibly laid to infinity’ (¶218); but that is just a ‘mythological description of the use of a rule’ ((¶221), where all the real work has been done by the social practices inherent in the language-game.

Wittgenstein’s approach provides a way of thinking about logic which moves beyond Quine’s emphasis on the role of logic in scientific theory while remaining true to the pragmatist emphasis on practice. As such, while sharing Quine’s view that Lewis erred in sharply separating linguistic conventions from Platonic conceptual facts, it suggests that a ‘more thorough pragmatism’ would have led him to espouse a conception of language which locates concepts, not in Plato’s heaven, but in the social practices which underpin the use of language. There is, in fact, a significant suggestion to this effect in Mind and the World-Order itself:

Thus even our common categories may be, in part, a social achievement of like-mindedness. The sharing of a common ‘reality’ is, in some part, the aim and result of social cooperation, not an initial social datum, prerequisite to common knowledge. (Lewis, 1929, p. 115)

Unfortunately, Lewis does not carry this suggestion forward into his discussions of logic and a priori necessity. Had he done so, he would have anticipated a great deal of future discussion of these issues. Nonetheless, I hope I have shown that despite the legitimate criticisms of Lewis, both explicit and implicit, in Quine’s ‘Two Dogmas’, there is still much to be learnt from his discussions of the a priori.

Notes

1. See Quine and Carnap (1990) and several of the essays in Friedman and Creath (2007).
2. Kuhn (1962), p. 121: ‘though the world does not change with a change of paradigm, the scientist afterwards works in a different world’.
3. Carnap’s treatment of this issue is questionable, but it is not necessary to pursue it here.
4. There are other points in their later writings where, again, there is significant overlap. For example, the account of ‘linguistic meaning’ which Lewis advances in Lewis (1946) is an early form of possible world semantics combined with a theory of the semantic significance of linguistic structure; one year later, Carnap presented a similar position in Carnap (1947). Though it is striking, this similarity is not directly relevant to the issues under discussion here.
5. Although written in 1941, the paper was first published in Lewis (1970).
7. It is disputed whether Carnap's position in *Aufbau* was reductionist in a way which exemplifies Quine's first dogma (see Richardson, 1998). But it is not necessary to take a stand on that issue here.

8. Quine had, in fact, alluded to Lewis at an earlier stage in his discussion of the reductionist dogma: see Quine (1953), p. 37, fn. 15.

9. For a significant new argument in favour of classical logic, see Smiley (1996)

References

This is an extended translation by Amethe Smeaton of Carnap (1934).
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Part III
Methodological Reflections
Analytic philosophy has had an uneasy relationship with the discipline of history of philosophy throughout its life. Analytic philosophers often either scorn or simply ignore history of philosophy. Where interpretations have been offered of past philosophical works, in what we can call ‘analytic’ history of philosophy, they have tended to be ‘rational reconstructions’. In recent years, however, philosophers trained in the analytic tradition have begun to look at the history of analytic philosophy itself more seriously, and the debate about the relationship between philosophy and history of philosophy has been brought closer to home. In this chapter, I consider some of the philosophical and historiographical presuppositions and implications of this debate, focusing on the idea of rational reconstruction. This idea developed alongside analytic philosophy itself and holds the key to understanding one central thread in the history of the relationship between analytic philosophy and history of philosophy.

1.1 Two caricatures

As a subdiscipline of philosophy, history of philosophy has often been derided by analytic philosophers, and at a crude level, it is easy to see why. For if analytic philosophers (‘real philosophers’) are concerned with substantive philosophical questions, such as ‘What is meaning?’, ‘What is truth?’ or ‘What is virtue?’, then historians of philosophy seem only to be concerned with what particular (usually long dead) philosophers have thought about them in the past. If historians of philosophy are interested in these questions, then their interest seems curiously indirect and vicarious, as if they were unable to work things out for themselves, and they seem surprisingly reluctant to bring themselves up to date on the issues. It may be useful to find out what previous philosophers thought as a preliminary to ‘real work’;
but the aim is always to think through something for oneself and present it as clearly as possible. History of philosophy, on this view, is subordinate to analytic philosophy. It also seems methodologically flawed, for we must first have a reasonably clear conception of what, say, ‘meaning’, ‘truth’ or ‘virtue’ means, before we can make sense of what past philosophers have thought. History of philosophy appears to presuppose grasp of fundamental concepts, the analysis of which is the task of analytic philosophy, a task that can be undertaken independently of history of philosophy. Here the analogy with history of science or history of art is frequently drawn. Just as the scientist does not need to study the history of science in order to carry out research, or the artist can produce original work without knowledge of the history of art, so too the ‘real philosopher’ needs no knowledge of the history of the subject to clarify philosophical concepts.

From the point of view of the historian of philosophy, on the other hand, analytic philosophy might be seen as subordinate to history of philosophy. Analytic philosophy is itself an historical tradition, which just happens to be dominant now. To assume that it embodies the state of the art simply because it is now dominant begs precisely the questions that need to be investigated by studying previous traditions. In fact, its unity and coherence are by no means as clear as some have assumed. The apparent fragmentation of the analytic tradition, and the growing self-consciousness amongst its practitioners as to its nature and foundations, only reinforce the need for history of philosophy. On the methodological level, too, there is a powerful objection raised by the historian of philosophy to the analytic philosopher. Since the concepts we use, whose analysis is supposedly the concern of the analytic philosopher, have been shaped by our predecessors, and our thought only has its significance in the context of the work of others, we must understand our past to properly understand our concepts. It is widely accepted that the concept of virtue has changed over time, but so too have the concepts of meaning and truth, which the analytic philosopher frequently treats as timeless. So analytic philosophy depends on history of philosophy.

1.2 Dissolving the dilemma

Both these accounts are caricatures, but aspects of them can be found in contemporary views. The dilemma that they pose is spurious, but the relationship between analytic philosophy and history of philosophy is all too rarely considered. There are many things to say in repudiating the caricatures and dissolving the dilemma. What I want to focus on in this chapter is the role played by the idea of rational reconstruction. This idea emerged explicitly in analytic philosophy in the 1920s and 1930s, though its roots go back earlier: to neo-Kantianism and logicism. I shall outline these roots in Section 2 and explain its explicit emergence in Section 3. The idea influenced a whole generation of subsequent analytic philosophers. The story here is complex, and I can do no more, in Section 4, than select
three highlights: a paper each by Quine, Lakatos, and Rorty. In Section 5, I consider some actual examples of rational reconstructions in analytic history of philosophy, before offering an assessment, in Section 6, of how we should see rational reconstruction in attaining a healthy view of the relationship between analytic philosophy and history of philosophy.

2 The Roots of Rational Reconstruction

The idea of rational reconstruction has two main roots: in neo-Kantianism and in the logicism of early analytic philosophy. I shall say something briefly about each in turn.

2.1 Neo-Kantianism

Central to neo-Kantianism was the distinction between discovery and justification – or between genesis (Genese) and validity (Geltung or Gültigkeit), to adopt the terms used by Hermann Lotze. This distinction goes back (at least) to Kant’s distinction between quid facti and quid juris questions – questions of fact and questions about legal entitlement – which he drew in motivating his Transcendental Deduction in the first Critique (cf. A84/B116). This was connected with his anti-psychologism, or at least those anti-psychologistic features of his philosophy that came to be emphasized and elaborated in neo-Kantianism. In his account of pure logic in the Critique, for example, Kant writes that ‘it has no empirical principles, thus it draws nothing from psychology’ (A54/B78); and in the Jäsche Logic, he states that ‘the question is not about... how we do think, but how we ought to think’.5

Both anti-psychologism and the distinction between discovery and justification can be found asserted by many German philosophers writing after Kant, not just those who are generally classified now as ‘neo-Kantian’. J.F. Herbart, for example, wrote: ‘In logic it is necessary to ignore everything psychological, because here proof is required only of those forms of possible connections of thought which the nature of thought itself allows’ (Herbart, 1813, section 34). Lotze is also a pivotal figure in the transition from Kant to neo-Kantianism. Anti-psychologism is as key a feature of his thought as the distinction between genesis and validity just mentioned.6 It is in the work of the neo-Kantians who came after him, however, that the two ideas come together in an especially powerful form. Building on Lotze’s conception of validity, Wilhelm Windelband and Heinrich Rickert developed a whole theory of value, which became a distinguishing feature of the Southwest (or Baden) School of neo-Kantianism. Underlying this is the distinction Windelband drew between the ‘genetic method’, concerned to explain the psychological origin of our beliefs, and the ‘critical method’, concerned to validate the normative structure of our knowledge.7

Windelband himself was influenced by Hermann Cohen, the founder of the Marburg School, the other main branch of neo-Kantianism. It was
Cohen, in particular, who attempted to sift out what he saw as the legitimate anti-psychologistic from the illegitimate psychologistic elements of Kant's philosophy. Kant's talk of the *a priori* structures of experience easily lent itself to psychological construal, mental processes being seen as conditioning experience, and apriority being understood as innateness. Cohen criticized this naturalizing tendency and sought to purify Kant's philosophy accordingly. Kant’s theory of cognition – *Erkenntnistheorie* – was interpreted as an epistemological theory concerned with the validity of knowledge rather than as a psychological theory concerned with its genesis.\(^8\)

The distinction between the genetic method and the critical method – between discovery and justification – became a fundamental tenet not only of neo-Kantians of the Southwest and Marburg Schools but also of many later philosophers. There were variations, of course, and different attitudes to the two methods. The positivist Theodor Ziehen, for example, writing on the state of epistemology in 1914, distinguished the genetic method, which he saw as characteristic of positivism, from what he called the ‘reconstructive method’, the method of neo-Kantianism.\(^9\) In neo-Kantianism, mathematics and (mathematized) natural science were seen as ‘reconstructive’ in their projects of ‘rationalizing experience’. All reconstruction, Jonas Cohn wrote in 1908, is ‘partial rationalization’.\(^10\) The term ‘rational reconstruction’ does not yet seem to have been used, but the stage was set.

### 2.2 Logicism

Analytic philosophy has its origins, to single out the two most important events, in the creation and use of quantificational logic by Gottlob Frege and the rebellion of Bertrand Russell and G.E. Moore against British idealism. Central to the work of both Frege and Russell was their concern to demonstrate logicism – the thesis that mathematics (just arithmetic, in the case of Frege) can be reduced to logic; and it is in this project that we find a further source of the idea of rational reconstruction as it developed in the analytic tradition.

Fundamental to Frege's and Russell’s logicism is the definition of the natural numbers as equivalence classes of classes (extensions of concepts). In Frege's *Grundlagen* of 1884, for example, the number 0 is defined as follows:

\[(E0) \, \text{The number 0 is the extension of the concept ‘equinumerous to the }\]
\[\text{concept not identical with itself’}.\(^{11}\)

Frege shows how the concept of equinumerosity (*Gleichzahligkeit*) can be defined logically (in terms of one-one correlation), and on the assumption that the concept of an extension of a concept is also a logical concept, Frege's logicist project at least looks feasible. As Frege recognized at the time, however, objections might well be raised to such a definition. ‘For is an extension of a concept not thought to be something different [from
a number? ’ (Frege, 1884, section 69). Frege admits that we do not say, for example, that one number is more inclusive than another in the way that we do of extensions of concepts; but he suggests, in response, that there is nothing to stop us doing so if we want (ibid.).

What underpins this response is his view that there are equivalences between statements about numbers and corresponding statements about extensions of concepts. In particular, he notes that the following two propositions are equivalent (in the sense that one is true if and only if the other is true):

(Nb) The number of Fs is equal to the number of Gs.

(Nd) The extension of the concept ‘equinumerous to the concept F’ is equal to the extension of the concept ‘equinumerous to the concept G’.

(1884, section 69)

The equivalence here can be seen more clearly if we note also the equivalence between (Nb) and the following:

(Na) The concept F is equinumerous to the concept G (i.e., the objects falling under concept F can be correlated one–one with the objects falling under concept G).

That (Na) and (Nb) are equivalent is the content of the Cantor–Hume Principle,12 which plays a fundamental role in Frege’s logicism. (Na) and (Nd) are also equivalent,13 from which it follows, by the Cantor–Hume Principle, that (Nb) and (Nd) are equivalent. If this is right, then it would seem that every identity statement concerning numbers, if given in the form of (Nb), can be transformed into a corresponding identity statement concerning extensions of concepts.

Now the details of Frege’s logicism need not concern us here.14 What is important is the governing idea that number statements can be transformed into corresponding statements about extensions of concepts. With the benefit of terminological (or conceptual) hindsight, it seems most natural to describe what Frege is doing as ‘rational reconstruction’. The logicist project involves systematically rebuilding arithmetic on logical foundations. Numbers, we might say, are reconceived as extensions of concepts. Frege himself, however, uses the term ‘reduction’ rather than ‘reconstruction’, which arguably suggests more of a realist predilection.15 Extensions of concepts are already ‘out there’ (in Frege’s ‘third realm’, as he later comes to refer to the realm of abstract objects; 1918); all that needs to be done is to specify which of these are the natural numbers.

Alongside this realism, however, is recognition of the philosophical work that needs to be done prior to logical construction. It is not just a matter of specifying the relevant objects and concepts, in introducing the primitive
terms, and then getting on with the constructive task. The ground has to be prepared properly, by clearing it of mistaken views and somehow ensuring that the basic concepts are grasped. Frege called this preliminary work ‘elucidation’, and in recent years there has been growing appreciation of its significance. This makes ‘reconstruction’ a more appropriate term than just ‘construction’ in describing Frege’s logicist project.

Anti-psychologism was also fundamental to Frege’s philosophy. The principle that ‘There must be a sharp separation of the psychological from the logical, the subjective from the objective’ is the first of the three principles he lays down in the introduction to the *Grundlagen* (1884, p. X), and he never repudiates this principle. It underlies his remarks in the passage that sums up his view of the place of historical investigations in mathematical and philosophical work:

The historical mode of investigation, which seeks to trace the development of things from which to understand their nature, is certainly legitimate; but it also has its limitations. If everything were in continual flux and nothing remained fixed and eternal, then knowledge of the world would cease to be possible and everything would be thrown into confusion. We imagine, it seems, that concepts originate in the individual mind like leaves on a tree, and we suppose that their nature can be understood by investigating their origin and seeking to explain them psychologically through the working of the human mind. But this conception makes everything subjective, and taken to its logical conclusion, abolishes truth. What is called the history of concepts is really either a history of our knowledge of concepts or of the meanings of words. Often it is only through enormous intellectual work, which can last for hundreds of years, that knowledge of a concept in its purity is achieved, by peeling off the alien clothing that conceals it from the mind’s eye. (1884, p. VII/1997, p. 88)

Frege allows that historical investigations may play a preliminary role, however, and in fact, in the first three parts of the *Grundlagen*, he presents the results of his own historical investigations. He discusses the views, for example, of Euclid, Descartes, Hobbes, Newton, Locke, Leibniz, Berkeley, Hume, Kant, and Mill. I have called what Frege does here ‘historical elucidation’ (Beaney, 2006a), since it plays an essential role in motivating his own conception, by clearing the ground of mistaken views, and identifying aspects of our concept of number – such as its applicability to the whole domain of what is conceptual (and not merely what is spatio-temporal) – that need to be captured in an adequate account. Frege may be right that we do not typically discover concepts in all their purity in the earliest stages of human development, but that does not mean that historical investigation is not required in achieving a clear grasp of the requisite concepts.
After his rebellion against British idealism at the end of the nineteenth century, Russell was also concerned to demonstrate that arithmetic was reducible to logic. He followed Frege in defining numbers as classes. But in response to the paradox he discovered in Frege’s system in 1902, Russell’s own approach was more complex, both technically and philosophically. He developed his theory of types to avoid the paradox, and introduced his theory of descriptions to ‘analyse away’ talk of classes, allowing such talk to be meaningful without having to suppose that classes exist. His approach here was generalized into the method of logical construction, in accord with what Russell called ‘the supreme maxim in scientific philosophizing’: ‘Wherever possible, logical constructions are to be substituted for inferred entities’ (Russell, 1914, p. 115). The method was applied elsewhere, such as in his work on the analysis of matter (1927), and influenced subsequent philosophers such as John Wisdom and Rudolf Carnap. Russell, too, did not talk of ‘rational reconstruction’, although this might well seem to us now to be an appropriate term to describe his theoretical projects.

3 The Explicit Emergence of the Idea of Rational Reconstruction

As far as I know, the first use of the term ‘rational reconstruction’ to denote a philosophical method occurs in Rudolf Carnap’s first major work, Der logische Aufbau der Welt, published in 1928. Carnap’s own term was ‘rationale Nachkonstruktion’, but this was rendered as ‘rational reconstruction’ when an English translation of the book eventually appeared in 1967. By then, the term, in both German and English, was well established. Karl Popper and Hans Reichenbach, in books written in the 1930s, for example, use the term, and Reichenbach explicitly credits its first use to Carnap. I explain these uses in the present section.

3.1 Carnap’s idea of rational reconstruction in Der logische Aufbau der Welt

The aim of Carnap’s Aufbau was to develop what he called a ‘constructional system’ (‘Konstitutionssystem’), showing how all our concepts can be organized into a structured system based on a few fundamental concepts (Carnap, 1928, Section 1). Central to the project was the idea of reducibility: ‘An object (or concept) is said to be reducible to one or more other objects if all statements about it can be transformed into statements about these other objects’ (Section 2). The paradigm of reduction that Carnap had in mind was the work on the foundations of mathematics that had culminated in the logicist project of Frege and Russell. A constructional system was then characterized as a hierarchical ordering of objects, with the basic objects at the lowest level and all other objects constructed from them (ibid.). Despite this
initial talk of ‘construction’ and ‘constructional system’, however, the term ‘rational reconstruction’ is not itself used until section 81, when Carnap is explaining his method in more detail. The term appears five more times in sections 98–102, and two cognate phrases are also used. These uses are sufficient, though, to determine what Carnap meant.

Influenced by both neo-Kantianism and Gestalt psychology, Carnap held that the fundamental units of experience were not the qualities (e.g., colours or shapes) as perceived in individual experiences (e.g., seeing a physical object), but those experiences themselves, taken as indivisible wholes. Since these elementary experiences (‘Elementarerlebnisse’) are indivisible, however, they cannot be ‘analysed’ to get at the qualities involved. Instead, according to Carnap, these qualities must be ‘constructed’ by what he called ‘quasi-analysis’, which is analogous to analysis but which yields ‘quasi-constituents’ rather than constituents.

Carnap’s method of quasi-analysis is essentially the method of contextual definition or logical abstraction that Frege had introduced in the Grundlagen. Consider the Cantor–Hume Principle, asserting the equivalence between (Na) and (Nb), as formulated in Section 2.2 above. Here we have an equivalence relation holding between things of one kind (concepts) being used to define – or ‘construct’, as Carnap would put it – things of another kind (numbers). Numbers are not constituents of the concepts to which they are ascribed, but are constructed from the appropriate equivalence relation. Taking the case of colour, then, consider the following (seemingly trivial) analogous contextual definition:

(Fa) Object X is equicoloured to (has the same colour as) object Y.
(Fb) The colour of X is the same as the colour of Y.

Accepting such a definition as unproblematic, and given that being ‘equicoloured’ is an equivalence relation, we can then proceed to form the equivalence classes (the set of objects related to one another by the relevant relation), by means of which the individual colours can be (structurally) defined.

Carnap applies this method to constructing the quasi-constituents of (i.e., abstracting them from) elementary experiences, taking as basic not an equivalence relation but a similarity relation – the relation of similarity of recollection – to form what he calls ‘similarity circles’ and ‘quality classes’. The details need not concern us. What is relevant here is that it is in this context that we find Carnap’s first use of the term ‘rational reconstruction’. He writes: ‘In constructing similarity circles and quality classes, we must pay especial attention to the fact that the construction does not have to reflect the actual process of cognition, but that it is only a rational reconstruction which must lead to the same result’ (1928, section 81).
The idea here receives its fullest account in section 100, entitled ‘Construction as Rational Reconstruction’. Carnap begins by noting that ‘The “given” is never found in consciousness as mere raw material’, but is always synthesized unconsciously. He gives the examples of perceiving a house, which we do ‘immediately and intuitively’, and of a botanist’s recognizing a plant of a particular species. The synthesis of cognition, he writes, generally takes place intuitively, which has the advantage of ‘ease, speed, and obviousness’. But to understand such cognitive processes, and for scientific purposes, such ‘intuition’ needs to be ‘rationally justified’. It is not enough that the botanist can simply recognize the species of plants, for example; the essential characteristics of each species need to be identified. It is here that ‘rational reconstruction’ comes in, to abstract out the qualities that are merely intuitively apprehended. ‘The constructional system is a rational reconstruction of the entire formation of reality, which, in cognition, is carried out for the most part intuitively’.

Returning to the example of colour, the basic idea is this. In everyday life I experience colours: I recognize colours, I describe things as coloured, I compare and make judgements about colours, and so on, much of it ‘intuitively’. Perhaps there is a ‘phenomenal feel’ to an individual experience, and in looking at a coloured object, there may be all sorts of other aspects to my experience than just its coloured aspect. But for scientific purposes, we need to abstract from all these other aspects. This is where quasi-analysis comes in. Assume that someone calls something ‘red’. We do not know what they mean by ‘red’ until we see the kinds of judgements they make in calling this object similar to that object. From a list of such judgements of similarity we can form similarity classes, and then define something as ‘red’ if it is a member of the relevant similarity class. Generalizing (and ignoring the various difficulties that such a strategy actually faces), our experiences can thus be ordered by means of their structural relations. We abstract from the subjective content of experiences and rationally reconstruct in accord with their logical relations.

Carnap may not have introduced the term ‘rational reconstruction’ until section 81 of the Aufbau, then, but it offered a succinct way of encapsulating his method. From the associated talk of ‘synthesis of cognition’, we can detect the neo-Kantian influence, but the idea is clearly rooted in his conception of a constructional system, which also shows the influence of Frege’s and Russell’s logicism. So both influences come together in this key idea of rational reconstruction. Certainly, by 1961, when Carnap wrote the preface to the second edition of the Aufbau, the idea is at the centre of his thinking. After noting that he would no longer put things in the same way, he nevertheless endorses the essential method:

The main problem concerns the possibility of the rational reconstruction of the concepts of all fields of knowledge on the basis of concepts
that refer to the immediately given. By rational reconstruction is here meant the searching out of new definitions for old concepts. The old concepts did not ordinarily originate by way of deliberate formulation, but in more or less unreflected and spontaneous development. The new definitions should be superior to the old in clarity and exactness, and, above all, should fit into a systematic structure of concepts. Such a clarification of concepts, nowadays frequently called ‘explication’, still seems to me one of the most important tasks of philosophy, especially if it is concerned with the main categories of human thought. (Carnap, 1967, p. v)

There is room for dispute about the extent to which ‘explication’ means the same as ‘rational reconstruction’. The project of explication arguably owes more to logicism than to neo-Kantianism, albeit refined in the aftermath of Carnap’s endorsement of the principle of toleration in the Logical Syntax of 1934. But there is a common methodological core, which Carnap highlights here: redefining our old concepts and systematizing them to make clear their logical relations. At the root of both, too, was rejection of psychologism. This comes out clearly in his ‘Intellectual Autobiography’ of 1963. In describing his Aufbau project, he writes:

Although I was guided in my procedure by the psychological facts concerning the formation of concepts of material things out of perceptions, my real aim was not the description of this genetic process, but rather its rational reconstruction – i.e., a schematized description of an imaginary procedure, consisting of rationally prescribed steps, which would lead to essentially the same results as the actual psychological process. (Carnap, 1963, p. 16)

As we will see, this rejection of psychologism was shared by both Popper and Reichenbach, writing in the decade that followed the publication of the Aufbau.

3.2 Popper’s anti-psychologism in Logik der Forschung

Logik der Forschung was Karl Popper’s first book, published in 1934 (and in English translation as The Logic of Scientific Discovery in 1959). His concern was to give a logical analysis of the growth of scientific knowledge, which he saw as the key problem of epistemology. He rejected inductivism, according to which universal statements such as hypotheses or theories are verified in proportion to the number of singular statements that confirm them. Instead, he argued, we should be deductivists: science proceeds by formulating bold hypotheses or theories, the consequences of which are then drawn out with a view to falsifying them.
The main features of Popper’s falsificationism are well known. All I want to highlight here is his endorsement of anti-psychologism, on which he agreed with the logical empiricists, and his mention of ‘rational reconstruction’ in doing so. In Section 2, entitled ‘Elimination of Psychologism’, he writes:

Some might object that it would be more to the purpose to regard it as the business of epistemology to produce what has been called a ‘rational reconstruction’ of the steps that have led the scientist to a discovery – to the finding of some new truth. But the question is: what, precisely, do we want to reconstruct? If it is the processes involved in the stimulation and release of an inspiration which are to be reconstructed, then I should refuse to take it as the task of the logic of knowledge. Such processes are the concern of empirical psychology but hardly of logic. It is another matter if we want to reconstruct rationally the subsequent tests whereby the inspiration may be discovered to be a discovery, or become known to be knowledge. In so far as the scientist critically judges, alters, or rejects his own inspiration we may, if we like, regard the methodological analysis undertaken here as a kind of ‘rational reconstruction’ of the corresponding thought-processes. But this reconstruction would not describe these processes as they actually happen: it can give only a logical skeleton of the procedure of testing. Still, this is perhaps all that is meant by those who speak of a ‘rational reconstruction’ of the ways in which we gain knowledge. (Popper, 1959, p. 31)

Popper presumably has in mind here Carnap’s Aufbau, where there is indeed talk of rational reconstruction of a process. The idea of rational reconstruction clearly lends itself to application to the history of thought, where a distinction can also be drawn between the actual process by which an idea or theory was developed and a ‘schematized description of an imaginary procedure, consisting of rationally prescribed steps, which would lead to essentially the same results’, to use Carnap’s words. Since Popper thinks that processes are of no concern at all to logic, however, he is reluctant to endorse the idea, suggesting that he holds an even more robust anti-psychologism. As he goes on to say, his own view ‘is that there is no such thing as a logical method of having new ideas, or a logical reconstruction of this process’; every discovery, he writes, contains ‘an irrational element’ or ‘a creative intuition’ (p. 32). This is the crucial point for Popper. Despite the English title of his book, there is no logic of discovery. There may be a logic of research (Forschung), but this is a logic of justification, operating through the deductive and falsificationist methods that drive scientific progress, according to Popper. We will return to this in Section 4.2, where we will see how the idea of rational reconstruction did indeed come to be applied in history of science, quite explicitly, in the work of Lakatos.
3.3 Reichenbach’s distinction of contexts in *Experience and Prediction*

Hans Reichenbach reviewed Popper’s *Logik der Forschung* in 1935, and defended his own inductivist views more fully in *Experience and Prediction*, which was published in 1938. I shall simply note here what he says about rational reconstruction in the Section 1, in distinguishing between epistemology and psychology, and distinction he draws between the context of discovery and the context of justification. He writes:

> Epistemology does not regard the processes of thinking in their actual occurrence; this task is entirely left to psychology. What epistemology intends is to construct thinking processes in a way in which they ought to occur if they are to be ranged in a consistent system; or to construct justifiable sets of operations which can be intercalated between the starting-point and the issue of thought-processes, replacing the real intermediate links. Epistemology thus considers a logical substitute rather than real processes. For this logical substitute the term *rational reconstruction* has been introduced; it seems an appropriate phrase to indicate the task of epistemology in its specific difference from the task of psychology. (Reichenbach, 1938, pp. 5–6)

Reichenbach adds a footnote here, stating that the term ‘rationale Nachkonstruktion’ was used by Carnap in the *Aufbau*. His endorsement of Carnap’s idea is thus clear, rooted in a shared rejection of psychologism. He continues:

> If a more convenient determination of this concept of rational reconstruction is wanted, we might say that it corresponds to the form in which thinking processes are communicated to other persons instead of the form in which they are subjectively performed. ... I shall introduce the terms *context of discovery* and *context of justification* to mark this distinction. Then we have to say that epistemology is only occupied in constructing the context of justification. (pp. 6–7)

Reichenbach’s distinction between the context of discovery and the context of justification has been a familiar theme in history and philosophy of science ever since. It clearly echoes the neo-Kantian distinction between discovery and justification, however, so it is far from new. It has been suggested that Reichenbach did not always maintain the distinction, and that what he meant by ‘discovery’ was not so much the search for hypotheses and theories as the search for the rational (deductive and inductive) relations between theories and facts.27 Be that as it may, just like Popper, Reichenbach said very little about issues of discovery, and these were only taken up properly in the 1960s and 1970s.
4 Criticisms and Further Developments of the Idea of Rational Reconstruction

Logical empiricism and the scientific philosophy it inspired originated in Germany and Austria in the 1920s, although there are roots in earlier thought, as we have seen. With the growth of Nazism in the 1930s, however, most of its proponents emigrated, especially to the United States. Reichenbach went to Los Angeles via Istanbul, and Carnap to Chicago and from there to Los Angeles, for example, while Popper went to London after spending the war years in New Zealand. As we have seen, all three rejected psychologism and distinguished between discovery and justification, the main aim being to relegate questions of discovery to empirical psychology so that they could focus on questions of justification, pursued through rational reconstructions. Their ideas influenced a whole generation of philosophers, working in the analytic tradition that they helped promote. As those ideas were discussed, however, more and more problems became apparent and further developments occurred. In this section I shall highlight three key moments in this later history, focusing on a paper each by W.V. Quine, Imre Lakatos, and Richard Rorty.

4.1 Quine’s critique of Carnap’s idea of rational reconstruction

Quine’s critique, in ‘Two Dogmas of Empiricism’ (1953), of Carnap’s distinction between analytic and synthetic truths is well known. I shall briefly look here at a later paper, ‘Epistemology Naturalized’ (1968), where Quine criticizes Carnap’s idea of rational reconstruction in the context of a psychologistic reconception of epistemology. Quine identifies Carnap’s *Aufbau* project as the one that came closest to deriving knowledge from sense experience, but he raises two objections to it. The first may be stated as follows. If the aim is to derive psychology – together with the rest of science – from sense experience, then appealing to psychology itself in doing so would be circular; so empirical psychology must be set aside in such foundationalist projects. If the foundationalist aim is abandoned, on the other hand, then the way is open to make use of psychology – along with any other science – in explaining how we acquire scientific knowledge (cf. Quine, 1968, pp. 75–6).

Now we might agree that if we abandon foundationalism while remaining committed to empiricism, then we should make use of whatever empirical science helps us in explaining the knowledge we have. But the antecedent here makes major assumptions, and we might well feel that Quine has missed the point of rational reconstruction. He goes on, however, to acknowledge a different reason for pursuing rational reconstruction: ‘We should like to be able to translate science into logic and observation terms and set theory. This would be a great epistemological achievement, for it would show all the rest of the concepts of science to be theoretically superfluous’ (p. 76). He then
points out, however, that Carnap did not succeed in giving translational reductions in the *Aufbau*, and was later led to weaken his reductionism, demanding merely implications rather than equivalences (pp. 76–7). And once we do this, Quine argues, we have lost the advantage of translational reduction, namely, its eliminativism:

> If all we hope for is a reconstruction that links science to experience in explicit ways short of translation, then it would seem more sensible to settle for psychology. Better to discover how science is in fact developed and learned than to fabricate a fictitious structure to a similar effect. (p. 78)

As I see it, Quine raises a dilemma here for any project of rational reconstruction. Either rational reconstruction aims to provide translational equivalents, or it does not. If it does, then all well and good, but no attempts have yet been successful. If it does not, then there will always be something to explain, in which case appeal will need to be made to actual history (or psychological genesis). But if such appeal is needed, then why not seek to explain the actual history in the first place? This argument clearly motivates Quine’s backlash against the anti-psychologism of his predecessors; but it is interesting when set in the wider context of Quine’s work. For Quine was in some ways even more Russelian than Carnap – in the emphasis he placed on regimenting theories to display their ontological commitments.\(^{29}\) This, too, counts as a form of rational reconstruction; but it is clear that Quine understands Carnap’s idea epistemologically, not ontologically. Certainly, if we think of Carnap as answering the (neo-)Kantian question ‘How is knowledge possible?’, then one can see how an empiricist like Quine regards an explanation of how it actually occurs as better than a mere ‘rational reconstruction’.

Despite Quine’s critique of Carnap, then, it would be wrong to see Quine as repudiating rational reconstruction altogether. He continues to endorse its use in arguing, for example, that regimentation into first-order logic shows that we do not need to posit the existence of attributes.\(^{30}\) To this extent, he is as much the heir of Frege and Russell as Carnap, and indeed, is the key transmitter of an idea that continues to be important in analytic philosophy today. Pursuing this further here, however, would lead us away from the central aim of this chapter, which is to trace the development of the idea of rational reconstruction into the disciplines of history of science and history of philosophy. With this in mind, it is to Lakatos that we must now turn.

### 4.2 Lakatos’ idea of rational reconstruction in history of science

In 1971 Lakatos published a paper entitled ‘History of science and its rational reconstructions’, which shows the extent to which the term ‘rational
‘reconstruction’ had caught on in history of science. Despite Popper’s own hesitance in using the term, Lakatos has no qualms in seeing Popper’s falsificationism as offering a rational reconstruction of scientific progress, and criticizing him for the form it took. Although Lakatos nowhere explicitly defines ‘rational reconstruction’, it is clear that he means the construction of what he calls an ‘internal history’ in accord with a methodology or logic of discovery. The idea is that the reconstructor has a certain normative methodology or theory of scientific rationality which determines what is selected from the history of science, which is then organized into a narrative obeying the normative rules. As Lakatos writes, ‘History of science is a history of events which are selected and interpreted in a normative way’ (Lakatos, 1971, p. 121).

What is omitted by internal history is left for ‘external history’, which explains ‘the residual non-rational factors’ (p. 118) such as social context and psychological motivation. Lakatos talks of the ‘vital demarcation between normative−internal and empirical−external’ (p. 102), which echoes the neo-Kantian distinction between justification and genesis (p. 102). He also follows the neo-Kantians in privileging the former: ‘rational reconstruction or internal history is primary, external history only secondary, since the most important problems of external history are defined by internal history’ (p. 118). Anti-psychologism is thus also a feature of his thought: ‘subjective factors’, he writes, ‘are of no interest for any internal history’ (ibid.).

In his endorsement of the neo-Kantian distinction and of anti-psychologism Lakatos clearly follows Popper. Yet, unlike Popper, who seems to think that his own logic of discovery gives us the logic of discovery, Lakatos is far happier to talk of ‘logics of discovery’ in the plural. For Lakatos, there can be different rational reconstructions, each of which draws its own internal/external distinction in accord with its logic of discovery. This raises the question of how we are to compare and assess them, and Lakatos answers this in the second part of his paper. His objection, in essence, to all earlier rational reconstructions is that they leave too much to external history, in other words, that they render too much of what happens in the history of science as non-rational. On Lakatos’ view, a rational reconstruction of science is better the more it reconstructs ‘actual great science as rational’ (p. 132).

According to Popper, the process of generating new theories is not itself rational. Once a theory has been conjectured, it can be rationally tested by attempting to refute it, but the conjecturing is not explicable by a logic of discovery. Popper’s concern with falsifying rather than conjecturing leads him to interpret the history of science in a particular way. At any given time, a theory may be faced with all sorts of anomalies, which may or may not turn out to be fatal. Popper tries to turn them into ‘crucial experiments’, Lakatos argues, or else ignores them altogether, thereby distorting the actual history of science. Applied to the history of science,
then, Popper’s falsificationism falsifies itself, according to Lakatos (pp. 127–8).

At the heart of Lakatos’ critique of Popper is the idea that ‘all methodologies function as historiographical (or meta-historical) theories (or research programmes) and can be criticized by criticizing the rational historical reconstructions to which they lead’ (p. 122). This idea is applied in making similar criticisms of ‘inductivist’ and ‘conventionalist’ methodologies. His own methodology of scientific research programmes, he then goes on to argue (pp. 131–6), offers the prospect of better rational reconstructions of science. The details need not concern us here. What is important is the greater space he opens up for internal history.31 Although Lakatos denies that there are any ‘hard’ or ‘neutral’ historical facts to which independent appeal can be made (cf. p. 120), he still assumes that actual history acts as the constraint on rational reconstructions and as the arbiter in assessing rival rational reconstructions. I will return to this in the final section.

### 4.3 Rorty’s four genres of historiography

The final paper I will consider here is one that Rorty published in 1984, entitled ‘The historiography of philosophy: four genres’. This will set the scene for the examples of rational reconstructions discussed in the next section. The four genres Rorty distinguishes are rational reconstruction, historical reconstruction, *Geistesgeschichte* and doxography. As Rorty describes it, rational reconstruction treats great dead philosophers ‘as contemporaries, as colleagues with whom [one] can exchange views’ in the search for philosophical truth (p. 49), giving an account of their views ‘in our terms’ in finding out whether what they said was true (Rorty, 1984, p. 54). Historical reconstruction, on the other hand, respects a maxim laid down by Quentin Skinner: ‘No agent can eventually be said to have meant or done something which he could never be brought to accept as a correct description of what he had meant or done’.32 *Geistesgeschichte* are big sweeping stories that aim at ‘self-justification in the same way as does rational reconstruction, but on a different scale’, working ‘at the level of problematics rather than of solutions to problems’ and giving ‘plausibility to a certain image of philosophy’ (pp. 56–7). *Geistesgeschichte* play a role in canon-formation, while doxography, by contrast, takes a canon for granted and tries to make a given question fit it. Doxography assumes that philosophical positions are eternally available and that different philosophers simply opt for different positions (pp. 62–3).

Rorty criticizes doxography for lacking the courage to alter the canon when new discoveries suggest it (p. 63). But he regards the other three genres as indispensable and as complementing one another (p. 67). There is a certain amount of tension in his account of them, however. In his initial description of rational and historical reconstruction, in rejecting the dilemma that they might be thought to pose, he remarks that ‘We
should do both of these things, but do them separately’ (p. 49). Later on, in commenting on the idea that the historical reconstructor is concerned with *meaning*, while the rational reconstructor is concerned with *truth*, he writes:

The two genres can never be *that* independent, because you will not know much about what the dead meant prior to figuring out how much truth they knew. These two topics should be seen as moments in a continuing movement around the hermeneutic circle, a circle one has to have gone round a good many times before one can begin to do *either* sort of reconstruction. (p. 53, fn. 1)

The interdependence of questions of meaning and truth is now familiar to us from the work of Donald Davidson, as Rorty notes (p. 55, fn. 3); but if this is right, then rational and historical reconstruction cannot be done separately. Rorty’s talk of a hermeneutic circle is a far better way to characterize their relationship.

Later still, Rorty suggests that the genre of *Geistesgeschichte* provides a synthesis, in the Hegelian sense, of rational and historical reconstruction.

It is precisely the tension between the brisk Whiggery of the rational reconstructors and the mediated and ironic empathy of the contextualists – between the need to get on with the task at hand and the need to see everything, including that task, as one more contingent arrangement – that produces the need for *Geistesgeschichte*, for the self-justification which this third genre provides. (p. 68)

But this third genre in turn, according to Rorty, requires a further genre to keep it honest, in the same way that historical reconstruction keeps rational reconstruction honest (p. 71). This further genre is not doxography but intellectual history, which provides ‘the ground out of which histories of philosophy grow’ (p. 70).

Rorty offers an attractive conception of the relationship between the various genres he distinguishes. Although he does not mention his own *Geistesgeschichte*, the sweeping – and controversial – story he told in *Philosophy and the Mirror of Nature*, published just four years earlier, it is clear that his sympathies lie with this genre. The more competing *Geistesgeschichte* we have, he writes, offering different canon-formations, ‘the more likely we are to reconstruct, first rationally and then historically, interesting thinkers’ (p. 74). His historiography – or perhaps better, meta-historiography – may itself be self-justificatory, but it offers a way of seeing how rational and historical reconstructions can indeed be synthesized in a grander project, one in which matters of fact still provide a constraint on issues of justification.
5 Rational Reconstructions in Analytic History of Philosophy

In his discussion of rational reconstruction, Rorty gives three examples: the accounts of British Empiricism offered by A.J. Ayer (1936) and Jonathan Bennett (1971) and the interpretation of Kant given by P.F. Strawson (1966). Bennett’s and Strawson’s works are often cited as paradigmatic rational reconstructions, Strawson’s being particularly notorious in attempting to interpret Kant without his transcendental idealism. There have also been rational reconstructions of the views of analytic philosophers themselves. Examples are J.O. Urmson’s monograph on philosophical analysis (1956), Michael Dummett’s first volume on Frege (1973), Saul Kripke’s interpretation of Wittgenstein’s remarks on rule-following and private language (1982), and Scott Soames’ history of analytic philosophy (2003). Others have offered rational reconstructions that bring together the work of analytic and non-analytic philosophers: Robert Brandom, one of Rorty’s own students, has done so in constructing an inferentialist tradition linking Leibniz, Kant, Hegel, Frege and Sellars, among others. These have generated a great deal of debate, which has played a major role in encouraging analytic philosophers to take history of philosophy more seriously – inspiring the historical turn that has occurred over the last two decades within at least some parts of the analytic tradition. I shall say something about Dummett and Soames in Sections 5.2 and 5.3, respectively. Before that, however, I consider the first example of rational reconstruction in analytic history of philosophy, Russell’s book on Leibniz.

5.1 Russell’s book on Leibniz

Russell published *A Critical Exposition of the Philosophy of Leibniz* in 1900, at the very time of his rebellion against British idealism. Indeed, it played a crucial role in that rebellion, in making him realize the importance of the question of relations. Russell’s main concern in his book is to identify what he sees as the five principal premises of Leibniz’s philosophy, and to show how they give rise not only to (most of) Leibniz’s doctrines but also to inconsistencies. At the beginning of Chapter 1, Russell criticizes Leibniz for never finding the time to present his philosophy as a systematic whole. What this then requires in a commentator, he goes on to remark, ‘is to attempt a reconstruction of the system which Leibniz should have written – to discover what is the beginning, and what the end, of his chains of reasoning, to exhibit the interconnections of his various opinions’ (Russell, 1900, p. 2). Such exposition is then to be followed by criticism – concerned, in particular, to identify inconsistencies. Indeed, Russell argues, exposition and criticism are never really separable, since if inconsistencies are not identified, an expository claim can easily be rejected by citing textual evidence that points to the opposite view (p. 3).

Russell talks merely of ‘reconstruction’ here, but what he advocates is a paradigm example of rational reconstruction: Leibniz’s views, the
expression of which is scattered over numerous pieces of writing, are re- described and reorganized into a system that reveals their logical relations. Russell’s approach is further explained in his preface, where he distinguishes two conceptions of history of philosophy, one ‘mainly historical’ and the other ‘mainly philosophical’. The first is concerned with influences, causes, context and the relations between philosophies, while the second is concerned with discovering ‘the great types of possible philosophies’, the examination of which enables us to ‘acquire knowledge of important philosophic truths’ (pp. xv–xvi). In the latter case, Russell writes,

the philosopher is no longer explained psychologically: he is examined as the advocate of what he holds to be a body of philosophic truth. By what process of development he came to this opinion, though in itself an important and interesting question, is logically irrelevant to the inquiry how far the opinion itself is correct. (p. xvi)

Like the neo-Kantians and Frege before him, then, Russell draws a sharp distinction between psychological genesis and logical justification, and nails his philosophical colours firmly to the latter.

Russell’s book on Leibniz received a number of reviews, one of which was by the neo-Kantian Ernst Cassirer. Cassirer commends Russell for the ‘decisiveness of his systematic interest’, which enables him to ask important questions often overlooked in traditional accounts (Cassirer, 1902, p. 533). But he criticizes Russell for his focus on identifying contradictions. This assumes that any conflicting views are of equal significance in Leibniz’s philosophy, yet they may have been expressed at different points in his intellectual development or in different contexts, such as in response to different pressures or concerns. For Cassirer, there may be ‘tensions’ in philosophical systems, but this requires historical explanation, not logical condemnation. I will return to this important point in the final section of this chapter.

5.2 Dummett’s work on Frege

Dummett’s first book, *Frege: Philosophy of Language* (1973), was a ground-breaking work: it did much to place Frege’s ideas at the forefront of debates in analytic philosophy and to help people see Frege as more than just an influence on Russell and Wittgenstein. But as the title itself indicates, it interpreted Frege as having particular concerns. According to Dummett, Frege was the founder of modern philosophy of language, and his entire interpretation is shaped by his conviction that Frege had sought to develop a theory of meaning. Frege’s logicist project is mentioned in the chapters that frame his discussion of topics such as names, sense and reference, truth, and quantification, but little sense is given of the significance it had for Frege. In his preface, Dummett says that the book was only intended as the first of two volumes about Frege, the second dealing with his philosophy
of mathematics (Dummett, 1973, p. ix). Since this second volume took a further eighteen years to appear, however, a one-sided view of Frege was dominant for quite a while.

Frege: Philosophy of Language is also characterized by a lack of concern with the context in which Frege was writing. Dummett remarks, for example, that the logical theory expounded in Frege's Begriffsschrift of 1879 ‘is astonishing because it has no predecessors: it appears to have been born from Frege's brain unfertilized by external influences’ (p. xxxv). It is difficult to imagine a more absurd claim to make about an historical event; it is like a biologist reporting a case of spontaneous generation. Frege's notation for quantification may have been new, but his logic clearly depended on the mathematical theory of functions which had been developed earlier in the nineteenth century and which Frege himself had worked on in his Habilitationsschrift. Dummett also writes as if Frege single-handedly slew the dragon of psychologism, regarded as characteristic of idealism (cf. e.g., pp. 683–4). But as we saw in Section 2.1, anti-psychologism was a central feature of neo-Kantianism, which certainly had an influence on Frege.

The historical omissions and distortions in Dummett's reconstruction came under increasing attack. Hans Sluga's work, especially his book of 1980, was notable in this respect. Sluga sought to show just how Frege had been influenced by earlier thinkers, such as Kant and Lotze. The criticisms from Sluga and others had a positive effect on Dummett. He modified his interpretation to some extent, conceding certain points and taking more account of other philosophers (1981, 1991b), and finally, in 1991, published his volume on Frege's philosophy of mathematics (1991a), which is far more sensitive to both the evolution of Frege's thought and the influences upon it.

Dissatisfaction with Dummett's reconstruction was also a motivation for others' concern with Frege, such as Baker and Hacker (1984), Burge (2005), and Weiner (1990). It also motivated my own book on Frege (1996), in which I aimed to show how Frege's philosophy of language developed out of his philosophy of mathematics, doing justice to the evolution of Frege's thinking. Interest in Frege has blossomed over the last forty years, and today we have a much clearer understanding not just of Frege's ideas themselves but also of their place in the history of philosophy. What we have witnessed provides an excellent illustration of Rorty's claim about how historical reconstruction works – and should work – to keep rational reconstruction honest.

5.3 Soames' history of analytic philosophy

In 2003, Scott Soames published Philosophical Analysis in the Twentieth Century, a two-volume work billed as a history of analytic philosophy. It has received mixed reviews. The clarity and argumentation of the work have been widely praised, but established scholars have made serious
criticisms. The reason is not hard to identify. In Rorty’s terms, what we are actually offered is a series of rational reconstructions of certain canonical texts, which steers an uneasy course between doxography and Geistesgeschichte, with a lack of historical reconstruction to keep it honest. Soames has a definite story to tell: the Whiggish story of how confusions about modal notions gradually get clarified until light bathes the whole in the work of Kripke. The Geistesgeschichte that Soames offers is valuable and instructive, but it is too partial to count as a genuine history of analytic philosophy.

I shall confine myself here to noting two general criticisms, which illustrates the way in which historical reconstruction is needed to keep Geistesgeschichte honest. The first concerns the omissions in his account, both local and global. An example of the former is his treatment of Russell’s theory of descriptions. No account of the history of analytic philosophy would be adequate without discussion of this paradigm of analysis. Soames devotes a chapter to it, but ignores its main motivation: the problems that Russell’s own earlier theory of denoting faced. There has been a great deal of honest toil on all this over the last two decades, but Soames mentions none of it. The most serious global omission is the lack of any discussion of Frege and Carnap. As I see it, there are two main subtraditions of analytic philosophy, the Fregean strand and the Moorean strand. Soames does no justice to the Fregean strand. Frege is written out entirely: the impression is given, for example, that Russell invented logicism (Soames, 2003, I, p. 193). Carnap is occasionally mentioned in discussing Quine, but given the influence that Carnap had on Quine alone, a proper account is required. A history of analytic philosophy which excludes Frege and Carnap is like a history of neo-Kantianism that ignores the Marburg School or a history of classical Greek philosophy that omits Plato and Plotinus.

This global omission is partly responsible for the second failing, concerning methodology. What is characteristic of the Fregean strand in analytic philosophy is the role played by ‘explication’, to use Carnap’s term. This seeks to replace our ordinary vague concepts by rigorously defined concepts. But this explicatory conception of analysis is in some tension with the Moorean conception of analysis, according to which philosophy aims to make clear what we already know, our ordinary concepts taken as essentially reliable and coherent. Soames officially endorses the Moorean conception, emphasizing from the beginning that ‘philosophical speculation must be grounded in pre-philosophical thought’ (p. xi). But he says nothing about how this relates to his own methodology. Soames is clearly engaged in rational reconstruction, but this is to have Fregean, rather than Moorean, methodological sympathies. Soames shows no sensitivity to the tension here. Recognizing the Fregean strand in analytic philosophy might have helped him appreciate this tension, which not only runs through the history of analytic philosophy but is also (unconsciously) reflected in
the story that he himself tells. His work is entitled ‘Philosophical Analysis in the Twentieth Century’, but the concept of analysis, ironically, is not subjected to anything like the critical examination that the concepts of analyticity, apriority and necessity receive. Greater historical and methodological honesty, then, might have allowed Soames to live up to what was promised in the title of his work. 41

6 Rational Reconstruction and Dialectical Reconstruction

We have now considered some key moments in the development of the idea of rational reconstruction, a development that proceeded alongside the development of analytic philosophy itself, both feeding and in turn being fed by that broader development. In discussing these moments, we have also raised various philosophical, methodological and historiographical issues. In this concluding section, I want to draw the threads together, first, in summarizing the story told about the development of the idea of rational reconstruction, and second, in proposing a healthy methodology for analytic history of philosophy, which I call ‘dialectical reconstruction’, which incorporates, but is not exhausted by, rational reconstruction.

6.1 The story of rational reconstruction

The idea of rational reconstruction, as it developed in twentieth-century analytic philosophy, has two main roots, neo-Kantianism and logicism. From the first came the distinction between discovery and justification (genesis and validation), with the emphasis on justification; from the second came a model for rational reconstruction. The two influences came together in Carnap’s Aufbau, where the term ‘rational reconstruction’ was first used. For Carnap, this meant abstracting from the genesis and subjective content of our experiences and ordering the resultant concepts by means of their logical relations. Both Popper and Reichenbach recognized the idea as crystallizing anti-psychologistic methodology, wherever the context of justification is paramount, although Popper was hesitant about using the term himself, given his rejection of all concern with processes of discovery.

After the Second World War, the idea of rational reconstruction received both criticism and further development. Quine objected to Carnap’s epistemological conception (as he understood it), as he sought to re-psychologize epistemology, but at the same time he continued the Russellian project of using logical reconstruction for ontological purposes. Lakatos’ work showed how the idea had become established in history of science, and offered a more sophisticated conception – via his idea of ‘internal history’ normatively governed – that improved on those of his predecessors. Rorty’s discussion of historiography showed how the idea had informed work in history of philosophy, especially of the analytic variety.
This raises the question of whether we can really speak of one notion of rational reconstruction here. If we can, then the following might be offered as the definition that best captures all of its uses:

A rational reconstruction of a (purported) body of knowledge or conceptual scheme or set of events is a redescriptions and reorganization of that body or scheme or set that exhibits the logical (or rational) relations between its elements.

Of course, it is open to us to distinguish different notions here, but I would prefer to think of the object of rational reconstruction as varying – whether it be a (purported) body of knowledge, conceptual scheme or set of events. We can then make sense of the idea as coming to apply to a greater range of things as it developed. This, I think, is what happened as analytic philosophy itself developed.

6.2 Dialectical reconstruction

Rational reconstructions have been offered throughout the history of analytic philosophy, from Russell’s book on Leibniz to Soames’ recent two-volume work. These have an important role to play in making us aware of the logical relations between the views a philosopher holds and facilitating assessment of the validity and soundness of their arguments. As part of a Geistesgeschichte, they can also provide exciting (even if controversial) new readings of the history of philosophy. In so far as the views reconstructed are ascribed to the relevant philosopher (and are not merely ‘prompted’ by a reading of their work), however, the ascription must be constrained by the actual history. In Rorty’s terms, historical reconstruction is always required to keep rational reconstruction honest. The history of the reception of rational reconstructions, and the historical turn that has recently taken place within the analytic tradition, provides evidence that this is just what happens.

According to Rorty, rational and historical reconstruction ‘should be seen as moments in a continuing movement around the hermeneutic circle, a circle one has to have gone round a good many times before one can begin to do either sort of reconstruction’ (quoted in Section 4.3 above). I think this is right, although I see more possibility of a genuine synthesis of the two than does Rorty. In Frege: Making Sense, I called the requisite synthesis dialectical reconstruction (Beaney, 1996, pp. 3ff). This is an appropriate term to use not just because it suggests the interplay between rational and historical reconstruction that must continually go on in doing good history of philosophy. It also alludes to a further feature that I regard as important, namely, sensitivity to the dynamic development of a philosopher’s thought. Cassirer made this point in his review of Russell’s book on Leibniz: it is often tensions between concepts or positions that drive
philosophical thinking, and these need to be elucidated and explained in any satisfactory history.

The interplay between rational and historical reconstruction is manifested in various ways. We have noted how historical reconstruction has actually worked to keep rational reconstruction honest – as in the case of responses to Dummett’s first book on Frege. Lakatos’ account of how the distinction between internal and external history varies according to the logic of discovery advocated is also a sign of the negotiated nature of the relationship here. We have seen, too, how historical considerations come in even where one might least expect it: in the work of paradigm rational reconstructors who themselves officially repudiate such considerations. Frege is the obvious example. As noted in Section 2.2, what I call ‘historical elucidation’ plays a crucial role in motivating Frege’s concepts and doctrines.

As far as the second feature of dialectical reconstruction is concerned, drawing attention to apparent inconsistencies in a philosopher’s views is only the starting-point for proper history of philosophy: we must embed an explanation of those inconsistencies, whether real or apparent, in a contextualized account that is sensitive to all the concerns and pressures that shaped their views. As chapters in a history of analytic philosophy, a good account of Russell’s theory of descriptions, for example, must show how that theory developed out of Russell’s concern to solve the problems faced by his earlier theory of denoting, just as a good account of Frege’s distinction between \textit{Sinn} and \textit{Bedeutung} must see it as motivated by the problems he recognized in his earlier notion of content, in the context of his logicist project. To write good history of philosophy in offering a dialectical reconstruction, then, we must think through the problems as they were faced by the philosophers studied. If we do this, it seems to me, then the dilemma posed at the beginning of this chapter is automatically dissolved.

Does this mean that the distinction between discovery and justification is also dissolved? Does dialectical reconstruction commit the genetic fallacy? In response, we must recognize that genetic processes, too, can be reconstructed: some motivations are inevitably going to be judged as more significant than others, in accord with the relevant normative theory. Furthermore, the actual history of philosophy – as of anything else – is itself a genetic process, so must be understood as such. Of course, how someone comes to write a history of philosophy, or makes the ‘discoveries’ about it that they narrate, may be irrelevant to that history itself: it depends on how closely they tracked the ideas they were attempting to think through. But imparting some sense of the development of philosophical thinking will be essential in any dialectical reconstruction. A more controversial question is whether philosophy itself, and not just history of philosophy, has an essentially historical dimension. I think it does, and I hope I have made this plausible by providing an illustration of dialectical reconstruction in offering an
account of the important methodological idea of rational reconstruction in the present chapter. Whatever the adequacy or usefulness of that account may be, however, I hope I have at least shown the possibility of combining both good analytic philosophy and good history of philosophy.43

Notes

1. In what follows, I use ‘history of philosophy’ to denote the discipline of history of philosophy and ‘the history of philosophy’ to denote the actual history of philosophy.


3. For the classic account of the history of the virtues, see MacIntyre, 1981. I focused on the way that Frege developed the notion of sense in Beaney, 1996. The historicity of conceptions of truth is well explored in Campbell, 1992.

4. One recent exception is Sorell and Rogers, 2005. Despite its title, however, all the contributors are scholars of early modern philosophy, and there is no discussion of, let alone a chapter on, the history of analytic philosophy. To be sure, many of the issues concerning the relationship between analytic philosophy and history of philosophy can be explored with reference to early modern philosophy; but it was a lost opportunity not to have considered analytic philosophy’s own history. Most of the chapters make useful contributions to the debate, however, especially the excellent chapter by Gary Hatfield.


7. See, esp., Windelband, 1884. On Windelband’s influence on Frege, see Gabriel forthcoming.

8. For a fuller account of Cohen’s neo-Kantianism, to which I am indebted here, see Anderson, 2005, section 3.


11. For details of Frege’s argument for this, see my editorial material in Frege, 1997, pp. 116–20.

12. This is now often called ‘Hume’s Principle’, but for reasons of historical justice it is best referred to as the ‘Cantor-Hume Principle’. Cf. Beaney, 2005b, p. 307, n.4.

13. Cf. Frege, 1884, section 68. For discussion of the equivalences here, see Beaney, 1996, section 5.3.


15. See, e.g., 1893, ‘Introduction’, pp. 1, 3 (the German verbs used are ableiten and zurückführen).


18. For an account of the relationship between Frege’s and Russell’s logicism, see Beaney, 2005a.

19. For further discussion of all this, see, for example, Hylton, 1990, the relevant chapters in Griffin, 2003, and on the method of logical construction, Linsky, 2007, forthcoming.
20. Russell’s attitude to psychologism is more complicated, and changed over time. For discussion, see Hatfield’s chapter in this volume. In focusing on psychologism, Hatfield’s chapter can be regarded as complementing the present chapter.

21. As his ‘Intellectual Autobiography’ (1963) makes clear, Carnap was profoundly influenced by both Frege and Russell. He had attended three of Frege’s lecture courses at Jena, two on Frege’s new logic and one on ‘Logic in Mathematics’; see Frege, 2004. For discussion of this influence, see Beaney, 2004. I draw on the latter in the account of Carnap’s views offered in what follows.

22. For the record, Carnap uses the term ‘rationalizing reconstruction’ (rationalisierende Nachkonstruktion) in section 94 and the verb ‘rational reconstruct’ (rational nachkonstruieren) in section 143.

23. The issue is complicated by the fact that Carnap takes the case of colour, which he actually thinks of as a property rather than ‘quasi-property’ of an object, to illustrate quasi-analysis, although analysis and quasi-analysis are seen as analogous. I discuss this complication in Beaney, 2004, section 5, and ignore it here.

24. The difficulties have been extensively discussed in the literature. For a recent account, see Richardson, 1998, chapters 2–3.

25. Cf. the summary Carnap provides of section 100 of the Aufbau; 1967, p. 171.


28. For a recent account, see Hylton, 2007, chapter 2.

29. For detailed discussion of this, see Hylton, 2007, esp. chapter 9. I am grateful to Erich H. Reck for pressing me to make this point, and to Andrew Arana for helping me to see how to respond.


31. Cf. Lakatos, 1971, p. 116. For more on his positive account, see Lakatos, 1970; and for discussion, see esp. Cohen et al., 1976.


33. See Brandom, 2002. This collection of Brandom’s chapters is notable in including a substantial essay on methodology (chapter 3).

34. Cf., e.g., Russell, 1959, p. 48. For an account of his views on relations, see Candlish, 2007, chapter 6.


36. For more on Frege’s philosophy in context, see the chapters in Beaney and Reck, 2005, Vol. I.


39. For more on this, see, e.g., Beaney, 2007b, 2007c.

40. For Carnap’s classic account, see Carnap, 1950, chapter 1. I discuss explication and its roots in Frege’s work in Beaney, 2004.

41. For fuller discussion of my objections to Soames’ work, see Beaney, 2006b.

42. One classic example of this is Kripke’s reading of Wittgenstein’s remarks on rules and private language. Kripke was quite open in presenting the argument ‘as it struck me’ (Kripke, 1982, p. viii), an attitude that led to the ‘philosopher’ interpreted being called ‘Kripkenstein’ rather than Wittgenstein.

43. This chapter is a revised and shortened version of a paper written between October 2009 and February 2010. Talks based on this paper were given in Bologna in
October 2009, and in Paris and Reading in February 2010. I am grateful to participants in the discussion for helpful questions and comments, and in particular, to Annalisa Coliva, Sebastiano Moruzzi, Peter Pagin, Eva Picardi, Andrew Arana, Olivia Chevalier, Silvio Pinto, Dirk Schlimm, Mark van Atten, Pierre Wagner, John Preston, and Severin Schroeder. I would also like to thank Erich H. Reck for sound advice on revising the paper.

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10

History and the Future of Logical Empiricism

A.W. Carus

When Kuhn published his *Structure of Scientific Revolutions* in 1962, he and many of his readers thought that introducing a historical dimension into the study of scientific theories and their languages was a decisive break with logical empiricism. But it has now been shown that Carnap himself – the editor of the series in which Kuhn's book was published – welcomed it unreservedly, and that he had good reason to.¹ Kuhn's position, it is now widely agreed, was to some degree compatible with Carnap's later view, which had developed considerably since the Vienna Circle doctrines of the 1920s.² But why, then, have history and philosophy of science since Kuhn largely rejected logical empiricism? Evidently, Kuhn added more than just a historical dimension; his conception of knowledge was also quite different from Carnap's (Section 1 below). Could Carnap have accommodated a historical dimension that fit better? This chapter argues that Carnap's framework (Section 2) allows a role for the history of science that is distinct from 'history proper,' or history as it is ordinarily conceived by historians (Sections 3 and 4). Moreover, history of science in just this Carnapian spirit began to appear soon after Kuhn's first writings (Section 5). And although it attracted less attention than Kuhn at the time, it has grown into a flourishing alternative tradition, which, I conclude (Section 6), deserves more attention, as it can interact fruitfully with the post-Kuhnian mainstream to open new perspectives for a historically-informed logical empiricism.

1. Situating the Different Conceptions of Knowledge

The complex relation between Carnap's and Kuhn's conceptions of knowledge – their overlaps, incompatibilities, and differing priorities – is hard to characterize precisely, and this is not the place to attempt it. However, there is by now a substantial literature, which this section will briefly review. George Reisch's (1991) publication of Carnap's letters to Kuhn first discredited the received view (and Kuhn's own in 1962) that Kuhn's book was in direct conflict with logical empiricism. Reisch also pointed out that much in Carnap's
later philosophical position made his endorsement of Kuhn plausible. Since arriving at the ‘principle of tolerance’ in the *Logical Syntax* (Carnap, 1934, p. 45), Carnap had maintained a radical pragmatic pluralism, in which language choice (and concept choice) became an entirely practical matter, to be decided not by some framework-transcending truth, any more than for Kuhn, but by what we find most useful for our purposes, whatever those may be.3

Wesley Salmon (1990) approached the same problem from a different angle when he tried to accommodate at least some dimensions of Kuhn's critique within a Bayesian framework. A more comprehensive and penetrating comparison of Carnap and Kuhn along those same lines, by John Earman (1993), was concerned with giving due credit to the insights of both of 'these two giants,' but discourages a 'shotgun wedding' between them. Earman tries to fit certain Kuhnian insights into a framework like Carnap's late quasi-Bayesianism. He concludes that the fit is poor; paradigm shifts are 'non-Bayesian' changes in probabilities that cannot be attributed to conditionalization (of any kind) alone, but involve a process that while 'far from being irrational, since it is informed by reasons,' is nonetheless not amenable to conclusive (deductive or inductive) logical argument either:

...the reasons, as Kuhn has emphasized, come in the form of persuasions rather than proof. In Bayesian terms, the reasons are marshaled in the guise of plausibility arguments. The deployment of plausibility arguments is an art form for which there currently exists no taxonomy. And considering the limitless variety of such arguments, it is unlikely that anything more than a superficial taxonomy can be developed. (Earman, 1992, p. 26)

And from this, in turn, he concludes that 'Kuhn's purple passages' – the notorious pronouncements interpreted by many as reducing scientific controversies to nothing better than tribal warfare4 – 'do not seem overblown when applied to revolutions in the strong sense distinguished above.'

The persuasions that lead to the adoption of the new shape for the possibility space cannot amount to proofs, since the very assignment of degrees of belief presupposes the adoption of such a space. After a revolution has taken place, the new and old theories can often be fitted into a common frame that belies any vicious form of incommensurability... But this retrospective view tends to disguise the shake-up in our system of beliefs occasioned by the adoption of the new shape for the possibility space. Bayesianism brings the shake-up to light, albeit in a way that undercuts the standard form of the doctrine. (ibid., p. 27)5

Earman confines this endorsement to ‘the first stage of the revolution, when the initial probabilities are established for the expanded possibility
Bayesian learning by conditionalization on the accumulating evidence of observation and experiment could take it from there, you might think, and the ‘long-run result is to force a merger of posterior opinion for those Bayesian agents who initially assign zeros to the same hypotheses.’ Alas, no; the ‘mathematically impressive merger-of-opinion theorems...are of dubious applicability not only to the sorts of cases [discussed by Kuhn] but also to examples from normal, as opposed to revolutionary, science.’ Among other problems, these results are long-run or limit results that say nothing about how long the long run is (p. 28). Earman considers various options and ends by, on the one hand, casting doubt on the degree to which consensus actually characterizes even ‘normal’ science, and on the other, conceding that while the Bayesian formulation clarifies the problem, no version of it adequately models ‘the merger of opinion that, for the Bayesian epistemologist, constitutes the heart of scientific objectivity’ (ibid.). As we will see, however, there are conceptions of scientific objectivity that while perhaps not convertible into Bayesian currency without remainder (any more than Earman’s Kuhn), are nonetheless consistent with Carnap’s ideal of knowledge and reason.

A different kind of appraisal is offered by Michael Friedman (2003), who situates the two conceptions of knowledge in intellectual history. Carnap and Kuhn, Friedman shows, emerge from two different branches of neo-Kantianism – logical empiricism from the Marburg School, especially Ernst Cassirer, Kuhn from the neo-Kantianism of Léon Brunschwig and Emile Meyerson, later developed in a more Hegelian direction by Alexandre Koyré. At issue between these schools, Friedman says, is the nature of the relativized a priori; in the Marburg School and logical empiricism, the a priori is specified as a formal system; in Meyerson and Koyré, it is something more like a (social-) psychological universal; indeed, in Koyré himself, it becomes something more akin to a Hegelian Zeitgeist or philosophical mind-set or ‘practice’ in the sense of German historicism. So, for Friedman, it is unsurprising that Kuhn should have stressed Koyré’s influence on his own work, or that (in contrast to both Marburg School and logical empiricists) Kuhn insisted on an ontological interpretation of the different ‘worlds’ perceived by the partisans of rival paradigms. Friedman points out that, late in his career, Kuhn recognized these common roots (Friedman, 2003, p. 28). His failure to notice them sooner Friedman attributes to a significant departure by logical empiricism from the common heritage – its elimination of any historical perspective and the analysis or specification of object languages solely in formal terms (ibid., pp. 26–7). The logical empiricists, ‘in their ambition to formulate philosophy, too, as a branch of exact mathematical science...removed the history of science from the purview of philosophy. And it was Kuhn’s great merit, against this common background, to have reinstated the history of science as perhaps the most important object considered in the philosophy of science’ (ibid., p. 35). Friedman, who in his
own work seeks to reconcile Kuhnian and Carnapian elements (Friedman 2001), emphasizes the common elements in the ‘two giants’ – who ‘agreed, on broadly Kantian grounds, in rejecting naive empiricist epistemology in favor of an emphasis on demands set by the mind itself’ and ‘departed from Kant in recognizing that the resulting mind sets, conceptual frameworks, or mentalities significantly evolve throughout the development of the sciences and are thus relative to or dependent on a given stage of theoretical progress’ (ibid., p. 34).

It is hardly to diminish ‘Kuhn’s great merit,’ though, to suggest that his was not the only way of reinstating, ‘against this common background’, the history of science as an integral part of philosophy. Friedman’s suggestion regards Carnap and Kuhn as largely complementary and downplays the significant differences between them⁶ – just as Earman’s Bayesian formulation of their respective viewpoints tends perhaps to exaggerate the differences. In fact, however, even for Friedman’s eirenic purposes, there are resources within the later Carnap’s perspective to license the reinstatement of history at the heart of philosophy, as we shall see, without adopting Kuhn’s rather different agenda.

For despite the mutual endorsements, and Friedman’s eirenic efforts, the differences between Carnap and Kuhn were not just rhetorical, and it seems unlikely that Kuhn, at least, would have been amenable to a meaningful rapprochement.⁷ In his response to the Earman paper discussed above, Kuhn insisted that ‘the passages which John quotes to show the deep parallels between Carnap’s position and mine also show...a correspondingly deep difference.’ The difference Kuhn has in mind is indeed quite deep, though his statement of it reflects a serious misunderstanding of Carnap’s position:

...if I understand Carnap correctly, the cognitive importance of language change was for him merely pragmatic. One language might permit statements that could not be translated into another, but anything properly classified as scientific knowledge could be both stated and scrutinized in either language, using the same method and gaining the same result. The factors responsible for the use of one language rather than another were irrelevant to the results achieved and, more especially, to their cognitive status. (Kuhn, 1993, pp. 313–14)

The misunderstanding resides partly in the word ‘merely’ of the first sentence; as we will see in the following section, Kuhn misconstrues the role of pragmatics in Carnap’s late writings. A further misunderstanding is the final sentence. Even in the 1920s, Carnap would have rejected this – when he argued, for instance, that Goethe’s hyper-empirical approach to (i.e., proposed language for) optics was not (‘cognitively’) wrong or mistaken but (‘merely’) hindered the progress of knowledge. The entire development
of optics since Thomas Young (indeed since Newton and Huygens) would have been precluded by Goethe's approach (Carnap, 1926, p. 52). So yes – Goethe's optics is not 'meaningless' in Carnap's view (its 'cognitive status' is no different from that of quantum electrodynamics), but Kuhn is quite wrong to suggest that Carnap could ever have thought the pragmatic difference 'irrelevant to the results achieved,' or that quantum optics could therefore 'be both stated and scrutinized' in Goethe's purely descriptive language, 'using the same method and gaining the same result'.

When Kuhn goes on to explain why 'this aspect of Carnap's position has never been available to me,' however, it emerges that there is more to his response than misunderstanding. The desire for language change to be 'cognitively significant,' rather than 'merely pragmatic,' is actually an insistence that there be a change in the reference of certain words – that there be a change of ontology.8 Not that Kuhn was a realist, but, like Quine,9 he had missed the point of the deflation of philosophy that the later Carnap had hoped to bring about. We turn, therefore, to a brief exposition of that project.

2. Carnap's Ideal of Explication

At the basis of Carnap's mature ideal is a quasi-utopian conception, rooted in the Enlightenment, of the role of human conceptual systems in human life.10 He believed that those who are fortunate enough to be able to devote themselves to thought and reflection have a responsibility to devise conceptual frameworks for the whole of knowledge that will maximize the usefulness of that knowledge for the species – not for some particular use, but for all uses to which humans put knowledge, especially liberation from unreflective tradition and conformity. In devising such frameworks, we are constrained by inherent human limitations, but Carnap insisted that we should not allow ourselves to be constrained by the past – the languages handed down to us by our ancestors. Those give us a starting point, and we could get nowhere without them, but we should not treat the puzzles and contradictions woven into natural languages, and thus into the historical languages of philosophy, with undue reverence. In fact, we should strive to liberate ourselves from them when planning new and better frameworks of thought. Certainly, our habitual ways of thinking and talking are deeply entrenched, but in Carnap's view, this is no reason to let them constrain us beyond the unavoidable minimum when we envision new ones.

There are three levels of language engineering or language study, in Carnap's mature conception: Syntax considers languages in isolation from anything extra-linguistic they might represent; semantics considers languages as representing, but still in isolation from their actual uses by humans; and pragmatics considers languages in relation to their use contexts and their users. Each of these three (syntax, semantics, and pragmatics) can
be considered as engineering activities (the creation or discussion of new or improved languages) or as investigative studies (of existing languages). The engineering activity Carnap called ‘pure’ syntax, semantics, or pragmatics, the investigative study he called ‘descriptive’ syntax, semantics, or pragmatics. (Linguists and ethnologists generally engage in the descriptive study of already existing natural languages, while logicians engage in the pure syntax and semantics of constructed languages.) Epistemology and methodology belong to pragmatics, then, while whatever remains of metaphysics and ontology belongs to semantics – though this now becomes a matter of deciding which entities to make fundamental to a language framework, rather than finding out what they ‘are’ (in some ultimate sense).

This voluntarist orientation was fundamental to Carnap’s outlook (Jeffrey, 1994; Carus, 2007a). The notion that something beyond the scope of science might actually be the case seemed to Carnap a back door to traditional prejudices and conformities of all kinds. Certainly, we need to make assumptions, he acknowledged, but we can decide on these and spell them out. They are not ‘out there’ for us to find. So he deprecated Quine’s talk of ontology – and would no less have deprecated Kuhn’s ascription of ontologies to successive paradigms. It makes no sense to consider ‘what there is,’ Carnap said, without specifying the language framework in which this is said. Any such claim can only be understood or judged relative to a framework. Within a framework that includes, say, the Zermelo-Frankel axioms for set theory, it makes perfectly good sense to ask whether there are infinite numbers. Such ‘internal’ questions have determinate answers. But it makes no sense, outside such a framework, to ask whether ‘there are’ such things as infinite numbers; there is no way to give such an ‘external’ question a clear meaning. In its place, Carnap suggested we put the practical question of whether it is better (e.g., for use in science) to choose a linguistic framework with infinite numbers or one without. But this is a question of which language we want, not a question of what is. This is why pragmatics, in the above sense, becomes central to philosophy in Carnap’s mature view, and accedes to the role traditionally played by the ‘philosophy of philosophy’ or the effort to define what sort of enterprise philosophy (or conceptual engineering, as he liked to call it) should become.

The process by which the human species upgrades its messy and imprecise inherited languages to newly built and more precise ones Carnap called explication. He acknowledged that this is a piecemeal, not a revolutionary, process. One example that Carnap often cited was the replacement of our vague, intuitive sense of ‘hot’ and ‘cold’ by the precise, quantitative concept of temperature, definable intersubjectively by reference to measurement devices. This concept not only replaces the vague concepts in many contexts, it gives us new capabilities. It can, for instance, provide an objective standard against which to judge subjective feelings. One is no longer limited to ‘I feel hot’ or ‘I feel feverish’ but can take one’s temperature and
locate it in a known distribution. But this is just a beginning; the replace-
ment of the vague, informal world-view by a framework of more objective
concepts is piecemeal and iterative; the concept of temperature is, in turn,
explicated within a more general framework of concepts.

Meanwhile, we live in a vaguely and fragmentarily understood world.
Even the people working at the frontier of knowledge have to use a vernac-
ular, derivative of ordinary language, to discuss the application of the more
precise calculi in which they frame their theories. Their vernacular will, of
course, be cleaner and more precise than the vernacular of the society at
large. In this scientific vernacular, the concepts used default to their scien-
tifically rigorous meanings, where available. (Behind a biologist’s use of the
word ‘light,’ for instance, lurks the entire theory of quantum electrody-
namics.) But many concepts even in this tidied-up vernacular have no such
precise meanings. They may go on being used for generations before they
are made precise. The concept of the derivative of a function, for instance,
was put to good use for nearly two centuries before Cauchy and Weierstrass
gave it a precise meaning.

Explication, the main task of conceptual engineering for the later Carnap,
consists in replacing a vague concept – the explicandum – with a more
precise one, the explicatum (Carnap, 1950a, sections 1–6, pp. 1–18). The
first step is the clarification of the explicandum, the establishment of some
basic agreement, among those using it, what they use it for. The next step
is a proposal for its replacement by a proposed explicatum, which should
have the most important uses agreed on in the clarification stage, but need
not have all of them. If possible, it should also be expressed in a language
framework that makes precise and transparent its relation to other concepts.
Many disciplines – especially those usually considered ‘scientific’ – use provi-
sionally canonical languages in which they expect, or sometimes require,
explications to be framed. Despite the continuing recourse to a vernacular
for everyday communication (as a sort of user interface), such provision-
ally canonical languages become the gold standard for the genuine content
of theoretical concepts in that discipline. There can be explicata in that
language framework, then, that correspond to no explicanda in ordinary
language. So, explications do not just replace ordinary-language explicanda
one by one; the entire system of interrelations holding them together is
also replaced gradually by the provisionally-canonical languages of science,
giving rise to entirely new concepts (such as ‘zygote’ or ‘quasar’) that have
no ordinary-language predecessors at all. From their acceptance within a
discipline’s canonical languages, such concepts diffuse into that discipline’s
ordinary language and ultimately into wider use by the community of those
who use the tidied-up scientific vernacular.

There is an obvious continuity, then, between explication and the
1920s Vienna Circle program of ‘rational reconstruction,’11 or eliminative
replacement of vague terms by more precise ones. But explication differs
in one critical respect. Rational reconstruction had been a one-way street; vernacular concepts were to be replaced with more precise ones within a single, definitive logical language. But under the new regime of tolerance after 1932, there is no longer a single correct language. There is an infinity of possible languages, and the community must decide which to use. Explication is therefore dialectical in a way that rational reconstruction was not. Knowledge has obvious and far-reaching effects on our practical life (more and more so, it seems, as history advances). It can, among other things, tell us about the likely consequences of various value systems and courses of action, far more than we could have known a few centuries ago.

On the other hand, we use our values to decide on the language(s) in which we represent and understand our knowledge. This choice among languages is not one we can make within a given language framework. It is a practical choice, a choice involving values – whether it is a global adoption of one language system over another, or a local, piecemeal replacement of a single explicandum by an explicatum. And this weighing of options becomes the main task of pragmatics, which takes center stage within philosophy as a whole. Knowledge and values are thus in a continuous feedback relation to each other; knowledge shapes values, and values shape knowledge.

Not only do values and the realm of practice make a striking reappearance in Carnap’s later thought, then, but they are tightly integrated into his view of knowledge. The conception of reason this suggests is evidently broader than Carnap’s endorsement of ‘probability as a guide in life’ (Carnap, 1947) might indicate. His account of normative statements and values in the Schilpp volume (Carnap, 1963, pp. 999–1013) is quite similar to (though articulated in a different idiom from) that proposed at about the same time by R.M. Hare, (1952). Carnap’s overall conception of reason, while permitting a kind of Bayesianism (or quasi-Bayesian quasi-utilitarianism) for a subset of choices, is not restricted to Bayesian procedure for the entire range of possible individual and social decisions.

3. Two Ways of Seeing the History of Science

Since Kuhn, the history of science has made a significant effort to emancipate itself from the shackles of subservience to the priorities, prejudices, and mind-set of science itself, past or present. Historians of science increasingly regard themselves as historians, i.e., they regard the people and institutions they study with the same skepticism historians have long adopted toward bankers or politicians, whom no one would think of taking at their word. But how does, or should, our conception of the history of science change when it is assimilated to history more generally? An authoritative answer is hard to find, since history as a discipline lacks an agreed conceptual or even methodological framework. But what some historians of science, at least, might intend by the proposed assimilation can be gathered from a
programmatic book, Shapin and Schaffer’s *Leviathan and the Air Pump* (1985). Its purpose, the authors say, is to ‘understand the nature and status of experimental practices.’ They seek a certain kind of answer and have a particular meaning of ‘understanding’ in mind:

We want our answers to be historical in character. To that end, we will deal with the historical circumstances in which experiment as a systematic means of generating natural knowledge arose, in which experimental practices became institutionalized, and in which experimentally produced matters of fact were made into the foundations of what counted as proper scientific knowledge. (Shapin and Schaffer, 1985, p. 3)

While this leaves open what they mean by ‘historical,’ it is evident – e.g. (ibid., pp. 15, 22) their stated aspiration of describing a Wittgensteinian ‘form of life’ – that their program is at least consistent with a traditional historicism going back to the origins of history as an academic discipline in nineteenth-century Germany, echoed by Peter Winch in *The Idea of a Social Science* (which relies heavily on the later Wittgenstein as well as the explicit historicism of Collingwood) and later endorsed in a somewhat different form by David Bloor, (1997).

This is of course not the only possible way to read the proposed assimilation of the history of science to history more generally (‘history’ in its present condition is hardly a well-defined field of study). But it is consistent with the few generalizations one can make. Historians usually regard their discipline as a strictly empirical one. There is disagreement whether it may eventually be capable of theoretical articulation in some form; most historians think not. At present, its default language is agreed to be an at best only very slightly regimented version of current ordinary language. There are those who say that the language of history should be *more* regimented (more ‘scientific’), but there are also those who say that our current ordinary language already contaminates our view of the past *too much* and that (in the spirit of ethnomethodology) the language in which a historical inquiry is conducted should be restricted to the concepts and categories of the past society it studies (e.g., Lynch, 1993). In any case, the criterion by which history ordinarily measures its success is adequacy to documentary evidence. The standard of acceptable evidence is essentially the same in history as it is before the law (in a society where the rule of law holds to some degree). So, the history of science would have the role of addressing the empirical question why and how certain forms of literate culture arose at certain times in certain societies. The growth of such knowledge-cultures would be regarded as an empirical phenomenon, like population growth or the growth of state bureaucracies.

Let us take such a (broadly speaking) historicist program, then, as a schematic stand-in for the program guiding the assimilation of the history of
science to history. In this conception, it is impossible to abstract or isolate an ‘internal’ strand of purely cognitive content from its multifold and complex embeddedness in its contexts. The scientific enterprise, like other social phenomena, can only be described holistically using the ordinary rhetorical and narrative resources of ordinary language – ‘thick description,’ as Geertz (1973) put it. The language in which history is conducted must therefore be restricted to those resources. It cannot invoke an Archimedean standpoint outside its own engagement with the processes it describes (e.g., an idealized standpoint from within the present-day science whose history is under study).

Surprising as it may sound, the Carnapian view described above has no grounds – given its tolerant pluralism – for excluding such a conception of history, or an elaboration of it such as Winch’s or Bloor’s. If the practitioners of a field find a language useful, Carnap thought, they should use it. Less fruitful and productive languages would be eliminated by a Darwinian selection process over time (Carnap, 1950b, p. 221). So, in fact, the program of assimilating the history of science to history, under the aegis of a historicism along the lines of Winch or Bloor, is actually compatible with the Carnapian conception of discourse and reason as described above.

However, this is not quite the end of the story, for in the Carnapian conception, unlike the historicist one, there is also room for a second, quite different way of seeing (and doing) the history of science: it can be a part of what Carnap called descriptive pragmatics. In this role the history of science also supports, or is part of, a kind of ‘understanding’ of science – not in its empirical aspect as a social phenomenon, but as a body of knowledge and as a long-term enterprise for cultivating and building such knowledge. The ‘enterprise’ is understood not as a collection of interlocking social institutions, but as a body of (tentatively) achieved knowledge resulting from the persistent application of a more or less coherent set of ideal goals, principles, and policies, anchored to historical examples of work that exemplifies those ideals. The role of the history of science in this task is essentially to characterize that enterprise, so understood. This kind of history does not, of course, deny that science is in fact a collection of social institutions, but for its purposes it brackets that contextual, empirical perspective and focuses narrowly on the knowledge produced by those institutions, along with its intellectual penumbra.

In the traditional terminology, this second kind of history focuses mainly on the ‘internal’ to the partial exclusion of ‘external’ factors. But the Carnapian distinction just suggested between two kinds of history does not map straightforwardly onto this traditional distinction. It will be better, therefore, to use a different terminology, and call this second, pragmatic or philosophical kind of history content history, while reserving the name ‘history’ – or, somewhat redundantly, context history – for the first. Context and content history have different purposes. Context history treats
the development of knowledge as a blind, impersonal process, without regard to the normative question, faced by a participant, of how to shape or guide that process,\textsuperscript{19} while content history is concerned precisely with that normative question. It asks not ‘what is that process, left to itself?’ but ‘what should we, in our present situation, make of that process, given that we are engaged in it, and have choices?’ Conceptual engineering, like any other form of engineering (or indeed any form of creative endeavour), can have a role only where there are choices and scope for active intervention. And conceptual engineering (pure pragmatics) depends crucially on descriptive pragmatics for knowledge of the landscape in which such creative generation of explicata, and the guidance of choices among them, takes place – just as the civil engineer needs to know the physical features of a valley she builds a bridge over.

Context history is ‘pure’ knowledge, available for \textit{all} the uses to which any knowledge whatever can be put; such knowledge is use-neutral.\textsuperscript{20} Its task is to ‘understand’ the cognitive dimension of scientific content by situating it within the multi-dimensional whole of intellectual, social, cultural, biological, and political processes it is entangled in. Content history, on the other hand, has one very specific practical purpose in view. This purpose is to achieve a much narrower and more specific kind of ‘understanding’ of science from that offered by context history. Content history singles out and artificially isolates the cognitive \textit{content} of past science from the impersonal process and seeks to ‘understand’ the implications of \textit{just} that content by itself – what past theories and ideas meant to their creators and audiences, what they mean in our terms (in the terms of our own best theories), how well they accounted (in their terms and ours) for the evidence available to them, how our terms differ from past terms, how subsequent evidence bears on past theories, how competing explications in the past differed at the time and in our terms, on what grounds we can now see (or argue) that some were better than others, and so on. Such questions take for granted that the cognitive dimension of the knowledge enterprise \textit{can} in fact be artificially isolated, at least for some purposes, from the stream of life as a whole. And to address such questions, content history – unlike context history – is not restricted to ordinary language alone (plus or minus), but can also draw on the full conceptual resources available to our present knowledge.

It is a binding constraint on content history that it not violate any empirical findings of context history. Content history is necessarily selective; it must simplify and idealize. But it may not contradict empirical facts. Traditional history of science (which was largely content history, in the present sense, by default) often did not live up to this ideal. The twentieth century is littered with philosophically and scientifically motivated intellectual history that distorts the facts (no less than other kinds of history employed for practical uses, such as nationalistic school or propaganda history, authorized biographies, or bland, justificatory histories of
corporations or interest groups)\textsuperscript{21} and has given (context) history more than enough cause for complaint, amply justifying its aspiration to assimilate the history of science to history.

In fact, this largely justified distrust of philosophically- or scientifically-motivated, traditional history of science may well predispose the context historian, steeped in the kind of historicism just described, to reject out of hand our Carnapian suggestion that there might also be room for another kind of history driven by what are usually considered ‘philosophical’ questions that take the availability or separability of cognitive content for granted. In the historicist view as described above, it is not just difficult but simply \textit{impossible} to abstract or isolate such a strand of conceptual content from the undifferentiated whole of the historical process. There is no alternative to ‘thick description’; no purely cognitive component can be distilled out from the murky colloidal suspension of real life. Any coincidence or overlap in the words or symbols used by past scientists and present ones cannot be regarded as constraints on their meanings, which depend on their respective contexts and uses in the different periods.

This is an old debate and will hardly be settled here. But it is perhaps worth remarking that most versions of historicism, from Droysen to Geertz, assume that it \textit{is} possible for the historian, though situated in a completely different society, to enter into the hermeneutic circle of a past conceptual vocabulary. So, when enough texts have survived for iterative constraint of all the uses of a symbol, it \textit{is} thereby possible to find out what it meant to its users, at least within sufficiently precise tolerances to make comparisons possible with later (up to and including present) concepts. The question then is not whether it is \textit{possible} to extract a thread of cognitive content from the densely interwoven cloth of history as a whole, but whether it is desirable. And that depends on the intended use. In (context) history, it may often \textit{not} be desirable. But in content history, the focus of interest is precisely the sequence or genealogy of explications over time, framed within the conceptual resources of the most recent member of the sequence. One may call this ‘\textit{strictly} internal history of science’ as it is internal not only to ‘science’ understood in some broad sense (leaving the diachronical characterization of that entity open), but internal specifically to the languages of our present knowledge in the discipline or the special question under consideration.\textsuperscript{22}

\textbf{4. Context History in Practice}

Turning from Shapin and Schaffer’s statement of their aims to the history they actually write, we find that they focus almost entirely on Boyle’s controversy with Hobbes about the legitimacy of the air-pump experiments – an intellectual controversy. The focus on Boyle seems an egregious example of what Shapin and Schaffer themselves (1985, pp. 16–17) derisively call ‘General Staff History’ – where the generals and great men at headquarters
get all the attention at the expense of the peons in the trenches. There is next to no treatment of the ‘historical circumstances in which experiment as a systematic means of generating natural knowledge arose,’ let alone of those ‘in which experimental practices became institutionalized,’ until Chapter VII – an extensive and original interpretation of Restoration censorship and repression of dissenters as the binding constraints for the rhetoric in which advocacy of an experimental philosophy could be clothed during this period. Intellectuals are still in the foreground, but at least their behaviour is placed in a larger explanatory context – not (as one might expect from an essay in ‘social constructivism’) social or economic processes or institutions, but certainly political events.

As an effort to integrate an account of the behaviour of intellectuals with that of a larger sociopolitical context, this Chapter VII is recognizably the offspring of similar efforts at contextualization in the history of political thought over the preceding years, notably Peter Laslett’s pioneering (and still classic) introduction to Locke’s *Two Treatises of Government*, and (under Laslett’s influence) Quentin Skinner’s early papers on the sociopolitical background of Hobbes’s political thought. Laslett’s work subsequently took a more radical turn, seeking to understand the wider social context of Robert Filmer’s patriarchalism by initiating the gigantic task, still in progress, of uncovering the ordinary life of the vast majority of English people who had until then been largely left out of history altogether. (A similar effort had already begun in France, and it has since spread to many other places.) Such programs had the potential of effecting a much broader and more fundamental reconciliation between intellectual history and history overall. Nor did this aspiration remain merely programmatic; it actually resulted in important empirical work on the frontier between social and intellectual history.

Compared with such work, Shapin and Schaffer’s history (in contrast to their aims) looks decidedly narrow, focused on a tiny literate elite and leaving aside the wider social context of ‘historical circumstances in which experiment as a systematic means of generating natural knowledge arose’, and those ‘in which experimental practices became institutionalized.’ Though their Chapter VII gestures helpfully toward a pursuit of this goal, most of the book is purely intellectual history, cut loose from any wider context. The questions they address are in fact, despite their stated empirical and contextual aims, almost entirely dictated by debates within content history. As they concede (Shapin and Schaffer, 1985, p. 4), their questions are largely a response to the fact that Boyle’s experiments are the subject of the first of the *Harvard Case Studies in Experimental Science*.

To escape this dependence on content history, context history of science needs an independent source of agenda items distinct from those motivating content history. It needs a program, a common language, for integrating the history of science, and intellectual history more broadly, with
history more generally. The only such program that has ever carried much conviction with historians was the historical materialism of Karl Marx, whose theory of ideology (the notorious ‘material basis’ for every ‘ideological superstructure’) was perhaps the first concrete proposal for the genuine, systematic integration of intellectual history with history as a whole. It provided a powerful unifying idea for academic history, and is probably the closest that discipline ever came to anything like a ‘paradigm.’ Even for those who did not work in the Marxist (or some rival materialist) tradition, historical materialism had a unifying function as a focus of attention and criticism for much of the twentieth century. At the very least, it provided a default and preserved the ideal that intellectual history should somehow be related to the fundamental constraints that shaped human society into the various forms it has exhibited over time.

But historical materialism no longer has the unifying function it had as recently as the early 1980s. Nor does it have a successor; the discipline of history has yielded to a thoroughgoing fragmentation, and cannot provide the history of science with an agenda distinct from that of content history. The only suggestion on the horizon is not, oddly enough, to look for explanations in the social and economic environment, but – as in Meyerson! – to try to draw a direct line from human psychological universals to the development of knowledge. Thus Scott Atran (1990), following the cognitive-science trend of the 1980s, draws on Chomsky’s nativism to argue for a universal, hard-wired ‘common sense’ as the basis for scientific knowledge (up to a certain point in time), while Kuhn himself took an interest in harnessing Piaget’s theory of mental development to the development of scientific thought. Others have taken up Kuhn’s suggestions about normal science, paradigms, exemplars, and concepts as theories of cognition (Nickles, 2003; Barker, Chen, and Andersen, 2003). Nancy Nersessian has developed a more self-consciously ‘cognitive-historical method’ along these lines which ‘assumes the need to factor into the analysis [of historical episodes in science] how human cognitive capacities and limitations could produce and constrain the practices of scientists’ (Nersessian, 2002, p. 133).

A program superficially similar to these has been suggested by Reviel Netz, who casts his work as contributing to the ‘cognitive histories’ of the practices we retrospectively classify as scientific (Netz, 1999, pp. 1–8; 2002). While not attempting to make this notion precise, he evidently does not regard these ‘cognitive’ histories as describing individual cognitive development – and does not interpret the cognitive histories of the species by analogy to individual development (or as constrained by it in the way Nersessian suggests) – but rather sees them as primarily cultural (or even institutional) processes. This might still, at a stretch, be seen as drawing on a kind of (perhaps Vygotskian) cognitive science, but one with a strong social and perhaps institutional dimension. So, it would appear to offer a more promising path toward integrating the history of science into history more
generally than the other cognitive-science-motivated programs mentioned (though Nersessian’s leaves room for a social dimension). In any case, while these programs hold varying degrees of promise, none has yet taken hold within the field as a whole as a framework for integrating the history of science into history at large. Until that occurs, the study of the development of scientific institutions or scientific thought as social phenomena, within the overall natural history of the species, will lack a common language, and context historians will continue to choose their subject matter largely on content grounds – i.e., study the figures and developments that are of most interest from the viewpoint of the sciences, and (yes) of philosophers.34

5. Content History as an Implementation of Carnapian Pragmatics

A few years after Kuhn’s first publications, a little-noticed effort got underway to use the history of science as a form of Carnapian pragmatics (as described in Section 2 above) more self-consciously and explicitly. This development brings our story back to logical empiricism, of which this effort was a direct continuation (though, as we will see, with an important change of emphasis). The leading pioneer at the beginning of this story, Howard Stein, had been Carnap’s student in Chicago in the late 1940s and early 1950s35 and realized that pragmatics was the next philosophical frontier, the sector of the overall logical empiricist conception of philosophy (or its replacement) to which a lot of the philosophical weight had shifted. The question of the hour was how to go about developing or creating this central component of what philosophy would henceforth become (Carus, 2010).

Carnap’s own preferences tended, of course, to the formal. And yet he was not entirely happy with students who followed him down this path without undertaking the hard propaedeutic labour of clarification – of pinning down the vague explicandum, exploring its range of uses, and provisionally suggesting which of them are worth preserving in an explication. Without such essential ground-clearing, he thought, we are likely to get lost in the weeds. When his student R.M. Martin,36 for instance, took some first steps toward a technical apparatus for pragmatics (Martin, 1959), Carnap wrote to Evert Beth, the editor of the series to which the book had been submitted, recommending it for publication but making plain that if he himself had been doing such a thing, he would have started by clearing the ground with more preliminary clarifications, as agreement had not yet been reached even there:

Since probably at the present time various authors would choose different concepts and different methods, it might at first be advisable to write a less technical treatise explaining the concepts, defining them informally in such a way as to indicate clearly how they would be
defined in a formalized metalanguage without necessarily giving actually their formalised definition, studying alternative explications of the various concepts, etc. (What I have in mind here is roughly analogous to Russell’s preparatory discussions in his *Principles of Mathematics* before writing *Principia* or my preliminary discussions in *Meaning and Necessity* in preparation for a not yet written treatise of a semantics of extensions and intensions in either an extensional or an intensional formalised metalanguage.) (ASP RC 088-05-05)

From our vantage point of half a century later, such cautious skepticism looks prescient. The subsequent investigations of concepts such as ‘belief’ – which Carnap himself (1955, p. 250) held to be a basic concept of pragmatics – has revealed that they contain fundamental ambiguities that would need to be decided one way or another before a formalization of any sort could even be contemplated.37

Stein’s approach, from the outset, was quite different from Martin’s, or even from Carnap’s own more cautious and clarificatory one. Rather than seeking to identify (and then clarify, let alone explicate) the fundamental concepts of pragmatics in advance, from some *a priori* or ‘philosophical’ – epistemological or common-sense – point of view, he sought out exemplary cases of real-life explication, turning points in the history of knowledge, where alternatives had been available and choices had been made, as springboards for a consideration of what might be said about explication more generally (that is, of what pragmatics might focus on). In his doctoral dissertation of 1958, he proposed ‘a reflective examination of particular problems instead of a frontal approach to first principles.’ The goal was, above all, to focus attention on these particular episodes, these exemplary instances of explication, as a way of achieving some clarity about what concepts are actually at work, and what work they are doing. Proper understanding of the complexity of these instances was to be achieved by an extremely close reading of the historical texts bearing on it, in which historical authors can be interpreted as considering not only the possibilities available within their existing vocabulary, but also as groping for ways of articulating possibilities *not* fully available to them, but available to us in our (or in a neutral) language. So, the close reading is not exactly the same as the close reading of the historian; it is not straightforwardly contextual and empirical. It is (as Abner Shimony once said of Stein’s work) more like the close reading of the literary critic. It is reconstructive, but with somewhat different and looser constraints than those of the context historian.

Stein’s first published paper (which remains one of his best-known) on ‘Newtonian Space-Time’ (Stein, 1967) illustrates the explicitly pragmatic content-orientation of this new genre. It announces its intentions in the title; the full resources of recent physics will be employed in the discussion of Newton’s conceptions of space and time. And the opening passages confirm
this orientation; two different mathematical formulations of a Newtonian space-time structure are given, stating ‘not what Newton says about this, but what is in actual fact presupposed by the science of dynamics that we associate with his name’ (ibid., p. 174). Then there is an abrupt shift to a detailed and careful reconstruction of Newton’s actual reasoning in arriving at his principal conclusions on space and time, based on a close reading of the *Principia* and other texts. Stein concludes, by reference to the mathematical exposition at the outset, that for a consistent dynamics Newton needed absolute time but not absolute space. The mathematical characterization of ‘Newtonian dynamics,’ then, plays a central and indispensable role in the precise characterization of the structure of Newton’s own conceptions of space and time within a framework that makes it possible to compare them (in precise terms) not only with the conceptions of space and time in subsequent formulations of Newtonian dynamics through the nineteenth century, but with those of special and general relativity as well. This comparison is present in the background of much of Stein’s work, as an aid in understanding both past and present theories, and especially their relations, and precisely what conceptual resources were employed at each stage.

But whether such studies, combining mathematical sophistication with close reading of historical texts, can lead to iterative clarification and eventual explication of any central pragmatic concepts (of the kind that have populated the philosophy of science journals – e.g., confirmation, theory-ladenness, explanation, causation, semantics of theories) is left open. Certainly this has not been achieved so far, and Stein sympathized in this respect, as he later made more explicit, with a view regarding what Plato calls ‘dialectic’ which Stein associates with Plato’s (or pseudo-Plato’s) Seventh Letter – the view that this kind of understanding is not reducible to *logoi* or formulas. Stein sees in the notion of ‘inquiry’ discussed by Galileo in the opening passages of the *Two New Sciences* a kind of dialectic in this sense, and points out that unlike Descartes, Galileo shared with Plato ‘the conviction that inquiry as such is not a profitable subject for positive doctrinal exposition (‘Discourse on Method’, ‘Rules for the Conduct of the Understanding’), but has to be learned and taught through its practice...’ (Stein, 1974, p. 397).

This may appear, on the surface, to be irreconcilable with logical empiricism in any form, but a closer look shows that Stein’s approach is entirely consistent with Carnap’s view, as sketched above, if we omit from it just one relatively peripheral assumption – that the central concepts of pragmatics can be identified ‘a priori,’ without reference to descriptive pragmatics. Stein takes this step. Instead of identifying any such central concepts in advance, he allows them to arise immanently from the examination of actual episodes of explication, in which we see what problems were actually being addressed by those who laid the foundations for the conceptual
frameworks we use today. The complexity and context-sensitivity of these problem-solving episodes leads him, like Plato or Galileo, to doubt whether it is possible to codify the process in explicit rules. The understanding of our explicitly articulated knowledge is not only distinct from that knowledge, but is itself not amenable (so far, at least) to the same kind of explicit articulation. In the present state of our development, understanding remains, to some significant degree, a kind of knowledge-how rather than (as Carnap had hoped) a fully explicit kind of knowledge-that. 43

The departure from Carnap’s logical empiricism here can be seen, from a certain angle, as just a difference of emphasis. Carnap’s distinction of semantics from pragmatics, and especially his distinction of pure from descriptive pragmatics, certainly leaves room for a distinction between knowledge and understanding. But where he does address this question directly, he is reluctant to give knowledge-how a serious role in relation to knowledge-that. In Foundations of Logic and Mathematics (1939, pp. 67–9), for instance, Carnap admits that understanding has a component of knowledge-how, but he tends more to trivialize it and to downplay understanding more generally than to explore its wider role. Stein can be seen as simply upgrading that role, and paying more attention to it, without actually changing much about the cognitive appraisal of the situation. This difference in emphasis expresses itself indirectly, in stylistic terms, in Stein’s exploratory, tentative, and dialogical approach to his subjects, rather than the flat and systematic expository style favoured by Carnap. Here again, Stein follows Plato and Galileo in seeing genuine insight as arising from the interplay of contrary views and juxtaposed perspectives – from a three-dimensional view of the subject – rather than from explicit doctrinal exposition.

These departures from logical empiricism are minor, in any case, compared to those of Kuhn – which are, admittedly, difficult to describe in either Carnapian terms or in the more thorough-going historicist terms favored by Kuhn’s successors. Kuhn certainly tended in certain ways to historicism, following Koyré, and yet in his own historical work (also like Koyré) he remained an old-fashioned ‘internalist’ (as he admitted in a late interview),44 openly motivated by philosophical (i.e., in Carnapian terms ‘pragmatic’) questions. But however one ultimately chooses to describe Kuhn’s position, it clearly diverges more sharply from logical empiricism than Stein’s. If (according to Friedman, as quoted in Section 1 above), ‘it was Kuhn’s great merit ... to have reinstated the history of science as perhaps the most important object considered in the philosophy of science’ after logical empiricism had so unwisely ‘removed the history of science from the purview of philosophy,’ then it was the even greater merit of Stein to have reinstated history in a way that is largely consistent with logical empiricism and opens new vistas for that tradition.

Stein’s name is hardly known outside the narrow confines of the philosophy of physics, yet the explicitly and self-consciously pragmatic approach
to content history he initiated has found widespread emulation. Beyond such immediate influence, content history has also been used for explicitly pragmatic/philosophical purposes very different from those of Stein himself or anyone he influenced more directly. The work of Mark Wilson, for instance, uses the history of physics, of mathematics, and of applied sciences (even of engineering and industrial applications) to argue for positions that appear, on the surface at least, to be in direct contradiction to logical empiricism and indeed the mainstream of analytic philosophy from Russell and Frege to the present. Nonetheless, Wilson’s work cannot be mistaken for context history, nor can it be even remotely be counted as falling within the Kuhnian tradition (which it vociferously denounces). It is a self-consciously pragmatic/philosophical use of content history to characterize the scientific enterprise, especially the use and evolution of concepts through their engineering applications.

This brief sketch of the new content history as a self-conscious implementation of Carnapian pragmatics – history of science undertaken for the specific purpose of characterizing and understanding the content produced by the scientific enterprise, rather than describing its contextual embeddedness empirically – has not only distinguished it from context history, but treated it as a separate discipline. And indeed, figures such as Stein and Wilson have worked in relative isolation from the history of science community as professionally established, which has, in turn, largely ignored them. Is this as it should be, or are there ways in which these obviously related fields can be mutually supportive and complementary?

6. Relations between Context and Content History

A recurring pattern in the interaction of context and content history (or of historians and philosophers of science) goes like this: (a) a content historian (or a philosopher of science) makes a claim characterizing science or the ideas of a particular scientist; (b) a context historian gives an alternative account of the episode in question, adducting facts left aside or glossed over in (a), and retorts that science (or the scientist in question) is mischaracterized by (a); (c) the content historian retorts that the new facts adduced in (b) have no relevance to the larger issue, or structural feature, or matter of principle, under discussion in (a); (d) and so on. Context and content historians talk past each other, that is, because they have different aims, and their criteria of ‘relevance’ (even of ‘characterization’) are accordingly different.

In the ideal division of labor between context and content historians, context history certainly subjects content history to empirical critique, but for the duration of that exercise accepts the terms of the content historian’s task – which is to understand what the past scientist was doing from our point of view, with our present state of knowledge. From the content historian’s viewpoint, this is to understand him better than he understood
himself, while from the context historian’s viewpoint (as long as she does not step into the context historian’s shoes), it is to fail to understand the past scientist altogether. But if this deep gulf of misunderstanding can be bridged, the context historian – apart from her own core task of describing and accounting for the occurrence of ‘knowledge’ in human societies – can serve the essential function of providing critical scrutiny of the content historian’s work. But the benefits can also go in the other direction, as illustrated by a long-term project begun about a decade ago (programmatically set out in Smith, 2002) to study the history of evidence for some of the best-known theories in physics.

George Smith, the instigator of this project, is a content historian concerned with the classical question of what it is about science – if anything – that gives it (or ought to give it) a higher cognitive status than pre-scientific or non-scientific knowledge. He grants Shapin and Schaffer their implicit claim that the way theories are initially accepted provides no grounds for this status. Shapin and Schaffer concentrate on the brief period between the initial formulation of Boyle’s Law and its acceptance by the scientific community, such as it was, in around 1680 – on astonishingly slender grounds, as they present it. But for Smith, that is precisely the wrong place to stop, if one is concerned not with theory acceptance, but the actual empirical testing and confirmation of theories (Smith, 2010, p. 560). Essentially, he concedes that theory acceptance is about as rational a process as theory generation. While both abduction and theory acceptance can be entirely rational, they need not be, and often are not. Both have an incalculable and creative dimension. Many factors can, after all, enter into the acceptance of a theory other than empirical robustness. Acceptance means the adoption of a common language for getting on with research, a lingua franca – and which expressions gain currency in a community notoriously depends more on convenience, ease of learning, and chance (as well as path dependence) than on accuracy. In any case, whatever factors lead to acceptance of a theory, Smith argues that systematic testing and confirmation often only begin at that point. It is only when a theory becomes an unspoken assumption (something like what Kuhn had meant by a paradigm) that it is really put to the test, as it is then assumed in all applied and experimental work. It is tested en passant when the theory is taken for granted in the ongoing research in the field.

Smith also found, in his own research on the history of the evidence for Newtonian gravitational theory, that there was a progressive stringency to the tests, over the centuries, due to the fact that every discrepancy noted had to be accounted for by a ‘robust physical source’ consistent with the theory, which was then incorporated in all subsequent calculations, ‘and the process is repeated, typically with still smaller discrepancies emerging that were theretofore masked in the calculations.’ It was this virtuous feedback-loop that eventually made the tiny, not even directly observable,
discrepancy in the perihelion of Mercury (only 43 seconds of arc) such a
critical test of Newtonian theory itself:

So what was being tested [at each step of this feedback-loop] was not only
Newtonian theory, but also all the previously identified details that make
a difference and the differences they were said to make without which
the systematic discrepancy would not have emerged. Notice that as the
loop is repeated, with more and more details incorporated into the calcu-
lations, and smaller and smaller discrepancies emerging, that tighter and
tighter constraints are being placed on finding robust physical sources
for new discrepancies. They had become tight enough in the case of
Mercury’s perihelion that no source for the anomaly in it could be found
that was compatible with Newtonian theory. (ibid., pp. 552–3)

But whether such a progressive stringency of testing actually occurs, in any
given case, is an empirical question. And Smith was surprised to find, in
the case of Newtonian gravitational theory, that in fact, the answer is some-
times no. Despite the increasing accuracy with which Newtonian theory
had been tested in the course of orbital research over the centuries, there
was a ‘surprising qualification’:

A fundamental part of Newtonian theory is that the strength of the grav-
itational attraction toward any body varies as the mass of that body. The
first two centuries of Newtonian orbital research never tested this claim
in any way at all. Throughout its history the masses of celestial bodies
have been inferred from the strengths of the centripetal acceleration
fields surrounding them. But then the only way in which the masses have
entered into any of the orbital calculations was as the product $GM$, the
strengths of the fields. In other words, those masses have all along been
mere placeholders for the field strengths from which they were inferred
and which they were then used to calculate. The thesis that gravitational
attraction varies as the mass of attracting bodies thus got a free ride
through the history of orbital research because it was not entering consti-
tutively into that research. There is a crucial lesson to learn from [this].
The only way to tell just which aspects of a theory are being tested during
ongoing research that presupposes it, and how stringent those tests are, is
to examine the history of that research in detail... (ibid., pp. 557–9)

And this lesson, Smith suggests, can also be applied to many other theories –
including the one Shapin and Schaffer looked at. ‘If we are going to reach
conclusions about the nature and scope of the knowledge achieved in pneu-
matic research, we cannot restrict our attention to its first 25 years. We need
to adopt a long vista to see how claims that had become accepted by 1680
have been revisited during the subsequent 330 years’ (ibid., p. 562). Not
that simply referring to that subsequent work answers Shapin and Schaffer’s challenge; someone actually has to do the work (the work Smith himself did on gravitational research) and find out what exactly was tested, and how stringently. ‘Perhaps some fundamental tenets of pneumatics have enjoyed the same sort of free ride in those experiments that Newton’s claim that attraction varies as the mass of the attracting body got over two hundred years of orbital research’ (ibid.). Smith provides a number of other examples of research fields where it simply is not known, yet, whether, and if so, how stringently, theories accepted long ago have undergone en passant empirical tests since then.

Here is a good example, then, of collaboration between context and content history. Here, in contrast to the syndrome we began this section with, (a) content historians make a claim characterizing science (in this case, about the role of evidence in the acceptance of scientific theories, as the Harvard Case Histories did about Boyle); (b) context historians (Shapin and Schaffer) give an alternative account, adding facts left aside or glossed over in step (a), and retort that science (in Boyle’s case) is not in fact as characterized in (a); (c) a content historian (Smith) acknowledges that the characterization in (a) was defective, and proposes a new one; (d) he throws the challenge of empirically examining this new characterization back to the context historians. This is surely closer to an ideal division of labor than the mutual incomprehension deplored at the beginning of this section.

Smith’s work also opens new perspectives for the historically-informed logical empiricism initiated by Stein. The question we began with concerned the fit between Carnap’s and Kuhn’s conceptions of knowledge and discourse. Carnap’s endorsement of Kuhn suggested that these conceptions were to some degree complementary, as in Friedman’s portrayal (where it was ‘Kuhn’s great merit’ to have ‘restored the historical dimension’ to the philosophy of science). But in Earman’s Bayesian characterization, the ‘two giants’ are separated by a considerable gulf; there was essentially no way to fit Kuhn’s conception of knowledge into anything like a Bayesian framework. We then saw, however, that Carnap’s conception of reason is broader than his quasi-Bayesian work on probability might lead one to suspect. We saw, in fact, that – despite appearances – it accommodated a role for history and that Stein actually developed a form of self-consciously pragmatic, content-focused history of science that dovetailed with Carnap’s overall conception of knowledge and reason. While we made little attempt to characterize Kuhn’s dissenting approach, it evidently departed further from logical empiricism than Stein’s. So, ‘restoring the historical dimension’ to philosophy could be – and was, by Stein and others – achieved within logical empiricism.

Smith’s work adds a rich new dimension as an exemplar of productive dialogue between context and content history. It also suggests a conception of science that, in its quiet methodical way, answers, squarely and frontally – instead of just deflecting – the arguments adduced by a long line
of skeptics about the legitimacy of scientific claims to knowledge. Whatever
the results of the detailed research on the histories of evidence Smith
proposes (e.g., on the history of pneumatics), he can adduce one theory –
the granddaddy of them all, the very paradigm of a physical theory, i.e.,
Newtonian gravitational theory (and successors) – in which his virtuous
feedback-loop of more and more precise and stringent testing has empiri-
cally been shown (by Smith himself) to occur. So, there is at least one field
(and it seems likely from his examples that there will be more) where the
history of evidence for a theory fully justifies its claim to a cognitively (and
empirically) higher status than folk knowledge. The possibility, at least, of
progressive explication – the replacement of traditional knowledge by better
knowledge – is thus vindicated, and with it the fundamental assumption of
the Enlightenment.

Nineteenth-century positivism, the Vienna Circle, and even many recent
philosophers such as Quine, shared the classical Enlightenment ideal of
providing authoritative knowledge to replace traditional folk knowledge. The
classical Enlightenment, while politically pluralistic, was not intellectually
pluralistic. It really did think that natural science (in which it included an
envisaged future social science) would progressively find the true answers.
Science would, once for all, replace the lore of church, state, and popular preju-
dice. This ideal has now been widely discredited. Carnap abandoned the last
vestiges of it in the early 1930s. Unlike Kuhn and (especially) followers, though,
he retained the essential Enlightenment idea of replacing traditional knowl-
edge by better knowledge – and the assumption that some knowledge could
be clearly identified (even if he had not quite figured out how) as ‘better’ (in
some reasonably precise sense) than other knowledge – within a pluralistic and
constructivist meta-framework. Whether one still wants to call this ‘logical
empiricism’ is, of course, a matter of taste and definition; this chapter does
so on the grounds that Carnap is usually identified as the leading philosop-
ical representative of the movement, and while this later perspective was not
fully worked out, it grew organically from earlier forms of logical empiricism
and was undoubtedly Carnap’s own.48 In any case, whatever one calls it, this
remodeled, pluralistic Enlightenment ideal did not die with Carnap in 1970
but has continued to develop in the hands of Stein, Smith, and others. While
changing the emphasis in some aspects of Carnap’s later position, they have
preserved the essentials. It is their great merit not only to have restored a histor-
ical dimension at the core of philosophy of science but also thereby to have
greatly advanced and developed the essential ideas of logical empiricism.

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Notes


2. It is still insufficiently appreciated, though, how open even the early Carnap was to such ideas. In 1928, for instance, he wrote to a friend that he regarded it as ‘highly commendable and important to unmask the atheoretical (i.e., affective) origins of theories’ and thought Freud’s *Future of an Illusion* (‘terrific’) had achieved that ‘with some degree of success’ (Archive of Scientific Philosophy, Hillman Library, University of Pittsburgh (henceforth ASP), RC 025–29–08).

3. Reisch’s paper was thus one of the earliest expressions of a new view of Carnap that, although not universally accepted, has since become almost commonplace. It is perhaps no coincidence that Reisch had studied with Howard Stein at the University of Chicago (who had himself studied with Carnap there in the 1940s); Stein’s (1992) paper on Carnap first spelled out this new view of Carnap, which is further developed in Carus (2007a).

4. Earman lists five, (ibid., p. 19), e.g., ‘In these matters neither proof nor error is at issue. The transfer of allegiance from paradigm to paradigm is a conversion experience that cannot be forced’ (Kuhn, 1962, p. 151). He also quotes the response of Lakatos, who took Kuhn to be saying that theory choice reduces to ‘mob psychology’ (Earman, 1992, p. 19).

5. He adds that while he has no way of knowing whether Carnap would have approved his ‘Bayesian reading of Kuhn,’ he does claim ‘that this is a reading that fits naturally with Carnap’s mature views on probability and induction.’ Fair enough. But Carnap is not quite so unable to address this tension as Earman supposes (pp. 28–9) on the basis of a quotation from a letter to de Finetti in 1963. He had been criticized on something like the grounds Earman here suggests by John Lenz (1956), and wrote several drafts of a substantial reply, which remained unpublished. The final typescript is ASP RC 82–07–01. A central issue arising here – also addressed in Carnap’s (1963, p. 982) reply to Arthur Burks, discussed by Irzik (2003, pp. 335–7) – is how Carnap’s quasi-Bayesianism relates to the ‘larger conception of reason’ attributed to Carnap in section 2 below.

6. As well as, perhaps, Carnap’s very significant divergences, as his career progressed, from the neo-Kantian school to which Friedman rightly connects him (Carus, 2007a, esp. Chapters 4 and 5).

7. Irzik and Grünberg (1995) and Irzik (2003) argue on systematic (rather than Friedman’s largely historical) grounds for a much closer compatibility between Carnap and Kuhn than is usually regarded as credible, or than the present chapter allows (though this is not the place to argue in detail against their well-articulated case). Irzik (2003), in particular, locates the ultimate difference between Carnap and Kuhn not in the historical dimension, as Friedman does, but in Carnap’s failure to relate his account of knowledge to the practical or ethical realm; Carus (2007a) argues that, contrary to Irzik, the cognitive and practical are indeed integrated at a fundamental level in the late Carnap.
8. ‘...the process of transition from the old state to new becomes an integral part of science, a process that must be understood by the methodologist concerned to analyze the cognitive basis for scientific beliefs. Language change is cognitively significant for me as it was not for Carnap’ (Kuhn, 1993, p. 314). This can perhaps be read as just a continuation of the misunderstanding already discussed. But more charitably, it can be read as an insistence that there is more to language change than just a difference of practical advantage; there must be a ‘cognitive basis,’ for Kuhn, of the ‘scientific beliefs’ that are at issue between the language of one paradigm and its successor.

9. Awodey and Carus (2009) show that the ‘two dogmas of empiricism’ attributed to Carnap by Quine had, in fact, been left behind by Carnap in 1931 and 1932, respectively, nearly two decades before Quine’s celebrated paper.

10. The account in this section relies heavily on Carus (2007a).


12. This is essentially the view of ‘explication’ later held by Quine; the enormous difference between what Quine and the later Carnap meant by ‘explication’ is addressed in Carus (2007a, pp. 265–6).

13. As was first pointed out by Stein (1992). Carnap himself appears to have been, at best, half-aware of these ramifications.

14. For an attempt to extract a larger conception of ‘reason’ from Carnap’s hints, see Carus (2004), where Carnap’s conception is contrasted with that of Wilfrid Sellars; Carus (2007a, Chapter 11) sketches a more general account.

15. Among many other recent discussions, see, for instance, Porter (2009), Galison (2008), Friedman (2008).

16. This loose description of the ‘enterprise’ leaves open a wide range of options, from traditional logical empiricist attempts at formal characterization of scientific progress (recapitulated to varying degrees in current Bayesian programs), and related but less formal characterizations such as those of Popper and his followers, to the opportunistic, but productive, chaos described by Mark Wilson, and even to the neo-Kantianism of the Marburg School and current efforts to revive Cassirer’s refined historicism, such as Friedman (2001). In particular, this wide range of conceptions of the “enterprise” certainly includes the characterization given by Isaac Levi in his book *The Enterprise of Knowledge* (1980), which proposes to bring within the ambit of rational discourse many dimensions of inquiry omitted from the more formal and deductive (or inductive) conceptions of knowledge, i.e., many dimensions traditionally considered ‘external.’

17. This distinction is so vague, though, that there is a wide range of possible boundaries between them (see previous footnote).

18. The distinction between context and content history is nearly identical with Hasok Chang’s (2004, p. 247–8) distinction between ‘complementary HPS’ (content history) and ‘social history of science’ (context history). (The only difference is that my ‘content history’ does not embrace the whole of descriptive pragmatics – which is approximately what Chang means by ‘complementary HPS’ – but only its historical components.) ‘My goal has been to articulate the complementary mode of HPS, not to deny the importance of other modes...the complementary mode must not be rejected simply because its aims are different from those adopted in other modes...To many historians of science, what I am proposing here will seem terribly retrograde. In recent decades many exciting works in the fields of history and sociology of science have given us valuable accounts of the sciences as social, economic,
political, and cultural phenomena. HPS as I am proposing here may seem too internalistic, to the exclusion of the...contexts in which science has developed and functioned...HPS in its complementary mode is not meant to be an incomplete sort of history that ignores the social dimension; it is ultimately a different kind of enterprise altogether from the social history of science. One might even say it is not history at all, because history does not in the first instance seek to further our understanding of nature, while complementary science does. I cannot emphasize too strongly that I do not intend to deny the essential importance of understanding science as a social phenomenon, but I also believe that the complementary function of HPS is a distinct and meaningful one’ (ibid.)

19. Such treatment of knowledge development as an ‘impersonal process’ may or may not imply a belief that the development of knowledge ‘really is’ nothing but an impersonal process, i.e., that conscious intervention is merely an epiphenomenon with no causal efficacy, as in some forms of determinism. There is also a wide spectrum of views about the kind of process(es) involved, ranging from ‘constructivist’ views (of which there are in turn many varieties), in which knowledge is seen as imposed by us on nature, to ‘naturalist’ views in which knowledge is imposed by nature on us; cf. Carus (2010, section 3).

20. This is what so mystifies policymakers about research and higher-education policy. They need to show social and economic payoff for dollars put in, while most researchers do their work ‘for its own sake,’ i.e., because they want to know (their motivation is ‘Platonic’ as in the cave story; they want to get it right). Indeed, for academic researchers, this is something of a sacred principle, taken completely for granted, and surprisingly little-discussed.

21. Two very different examples from the history of science: Reichenbach’s (1924) comparison of Newton, Leibniz, and Huygens on absolute and relative motion, revealed to be highly misleading by Stein (1967) and Earman (1992); and Foucault’s (1961) history of madness, revealed by Andrew Scull (2007) to be riddled with factual and interpretive errors.

22. Niccolò Guicciardini points out to me that the tension between content and context history is particularly acute (for perhaps obvious reasons) in the history of mathematics, which has therefore been somewhat isolated from the history of science more broadly. The present chapter was already too long to address this inherently special character of mathematics and its history, which is doubly unfortunate since the kind of rapprochement between content and context history proposed in section 6 below does not apply to it. Guicciardini (2003) proceeds from essentially the same distinction, between what Guicciardini calls ‘conceptual’ (content) and ‘contextual’ (context) history, and, like the present chapter, argues for the legitimacy and importance of both, as well as for more interaction between them.

23. Indeed, ‘the best example of the social constructivist genre in the literature on the Scientific Revolution’ as Schuster (1997, p. 119) calls it. Shapin and Schaffer themselves, programmatically, claim to be investigating experimentation and scientific method as ‘crystallizing forms of social organization and as a means of regulating social interaction within the scientific community’ (p. 14). They also regard their work, as we saw above, as studying a Wittgensteinian ‘form of life.’

24. As Skinner has himself acknowledged (Pallares-Burke, 2002).


27. E.g., Vovelle (1973); Wrightson and Levine (1995).


29. Peter Galison (2008, pp 122–3) has discussed this problem, from a somewhat different angle, under the heading of ‘relentless historicism.’

30. Avineri (1968); Cohen (1978); Thompson (1978); Elster (1985, Part II).


32. Kuhn (1971); Nersessian (2003, pp. 185–7) reports on late interviews with Kuhn in which he showed a great interest in cognitive science research about the development of concepts in children (e.g., that of Susan Carey), to support his ‘no-overlap principle’ and other preoccupations of his later work.

33. She continues in a very Meyersonian vein (though without explicit reference to Meyerson): ‘Underlying the method is a “continuum hypothesis”: the cognitive practices of scientists are extensions of the kinds of practices humans employ in coping with their physical and social environments and in problem solving of the more ordinary kind. Scientists extend and refine basic cognitive strategies.... From this perspective, scientific cognition is shaped by the evolutionary history of the human species, by the developmental processes of the human child, and by the cultural development of human societies’ (Nersessian, 2002, p. 133). This conception is further developed in Nersessian (2008).

34. This should not be taken to imply that context history inspired by content motivations is bad history; many historians of science, including many who incline more to the context than the content side, are perfectly aware that the questions they are addressing stem from content history, and this does not bother them in the least, or prevent them from producing high-quality research. It becomes a problem only within the terms of the historicist agenda to assimilate content history to context history.

35. Stein remained a loyal (if low-key) admirer of logical empiricism throughout his career. ‘I have to confess,’ he wrote in 1972, putting his cards on the table in advance of some critical remarks, ‘that with respect to the Quinean gospel I am something of a heathen; or, to be more precise, in the face of the new dispensation I am a Jew, and maintain allegiance to the Torah – though acknowledging the need for deuteronomic reforms....’ (Stein, 1972, p. 621). He occasionally reiterated this allegiance to suitably modified forms of ‘the mitzvoth of logicism and empiricism,’ e.g., in a chapter specifically taking Carnap’s side in the debate with Quine (Stein, 1992).

36. Martin had been part of a small reading group at Carnap’s Hyde Park home, in which Stein had also participated, along with Ruth Barcan Marcus and Raymond Smullyan, among others.

37. Carnap would not, of course, have been surprised about the contradictions arising from the ‘folk’ concept of belief, as famously diagnosed by Kripke (1979). In the relevant discussions since then, little of what Carnap called explication has been in evidence; attention has focused more on the natural-language folk concepts themselves rather than on any systematic replacement of them (e.g., in the social and cognitive sciences) by better or more precise concepts.

38. ‘...although he is clear that dynamics does not provide any way to distinguish motion from rest, Newton does not seem to have conceived the philosophical possibility that that distinction cannot be made at all; that is to say, that the
spatio-temporal framework of events does not intrinsically possess the structure of the Cartesian product SxT, but a weaker structure. One easily understands why Newton should not have conceived this possibility; even Poincaré, at the end of the nineteenth century, could express the view that if rotation is real then motion must be real, and if acceleration is real then velocity must be real. But the more abstract point of view that mathematics has now made available allows us to see, today, that these considerations are specious, and that the true structure of the space-time of Newtonian dynamics, with its Galilean invariance, is the one I have already described, in which there is an absolute time but no absolute space – that is to say, a natural mapping upon T but none upon S. The point that is really crucial for kinematics is that within this structure there is no absolute or intrinsic notion of velocity, but there is an absolute or intrinsic notion of velocity-difference – and therefore of rotation and of acceleration.’ (Stein, 1967, pp. 182–3)

39. In his celebrated critique, for instance, of Hilary Putnam’s (1967) deterministic interpretation of the physical geometry of special relativity (Stein 1968).

40. Stein summarizes the doctrine of the passage (342a-344d) as follows: ‘... the whole apparatus of what we might call “object-semantics,” involving both linguistic signs and ordinary things (Plato’s “images”), cannot suffice to determine meaning and truth, without some essential involvement of the language users and their conceptions and beliefs; and the writer goes on to assert that this determination can occur reliably only in discussion, with questioning and answering “free from envy” – and that, indeed, over a long time: a process which, in favorable conditions, can lead to a shining forth of the light of understanding and intelligence (phronesis and nous).’ He adds in parentheses the further point that ‘...the writer remarks that names have no fixed connection to objects, and therefore by their use alone obscurity cannot be avoided; nor can it be so by logoi, since these are made up of nouns and verbs. I believe that Plato would have been unimpressed by the causal theory of reference and the postulate of rigid designators; I wish we had the Socratic dialogue on this subject’ (Stein, 1998 p. 3).

41. While Stein himself shares this conviction, at least as a starting point, he does not reject the possibility of progressive formalization, as a long-term strategic program. Carnap’s inductive logic, for instance, though defective in certain basic respects (see Zabell, 2005, 2007), is regarded by Stein as a very interesting and worthwhile attempt.

42. ‘Relatively peripheral’ in the sense that this assumption is probably not among those that would readily spring to mind if one were asked to state the fundamental tenets of Carnap’s logical empiricism, and may even be inconsistent with those that do.

43. There is a point of contact here with what Earman says about the role of plausibility arguments (above, section 1), and the impossibility of reducing them to Bayesian terms – as well as with Kuhn’s own emphasis on exemplars rather than rules as the defining features of paradigms or ‘disciplinary matrices.’

44. ‘It constantly surprises people...that I’m an internalist. They cannot get their heads around it’ (Kuhn, 2000, p. 287). In the same interview, he mentions his gratification that, ‘as historians of science turn further and further from scientific substance, a number of important philosophers of science have gotten more and more involved with doing some history’ (p. 311), and says that ‘you are not talking about anything worth calling science if you leave out the role of [nature]’ (p. 317).

46. *Wandering Significance* (2006); also Carus (forthcoming). Wilson affirms (personal communication) that the direction of his work was also strongly influenced by Stein’s example.

47. By a ‘robust source,’ he means one ‘that has other observable and hence confirmable consequences besides accounting for the discrepancy’ (Smith, 2010, p. 551).

48. As Carus (2007a) documents in detail.

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At many universities, one can find scholars who style themselves historians of some sort, who are nevertheless not employed in history departments – the historians of philosophy that no self-respecting philosophy department can be without. Such creatures are especially remarkable when they reside in primarily ‘analytic’ philosophy departments – analytic philosophy, after all, is the most ahistorical of philosophical schools. Most curious of all, perhaps, is a still-young breed – we historians of analytic philosophy. Why do beings like us exist? What purpose do we serve? Such questions form the theme which I would like to address, in at least a preliminary way, in this chapter.

My reflections on this theme first took shape in response to the publication in 2003 of Scott Soames’s (2003a, 2003b) *Philosophical Analysis in the Twentieth Century*. I wrote a largely negative review of Soames’s books (Kremer, 2005) and again took a largely negative stance in an author-meets-critics session at a meeting of the American Philosophical Association (Kremer, 2006). One might think I should be done with Soames. Yet, his work, and subsequent lively discussion of it, in more traditional academic venues such as journals and conferences, and on blogs and comment boards, has raised fundamental issues about the nature and purpose of the philosophical study of the history of philosophy.¹ Such questions are my main target in the body of this chapter. I begin with Soames, incorporating material from my earlier responses to his work. But I hope to build on that mainly negative discussion in a more positive direction, incited in part by Soames’s incisive replies to his critics.

I use Soames’s work to draw out two unsatisfactory conceptions of the point of what I will call philosophical history: antiquarianism and presentism. I show that Soames’s approach is shaped by the choice between these two alternatives. While agreeing with Soames in rejecting antiquarianism, I draw on common lines of discussion in general historiography and the historiography of science to show the dangers of the presentist approach favoured by Soames. Inspired by Bernard Williams’s distinction between

¹ That is to say I am not engaged here with the history of philosophy for its own sake, but rather with the philosophical study of the history of philosophy.
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history of ideas and history of philosophy, I argue for a third possibility for understanding the value of philosophical history, without reducing this enterprise to a branch of history proper. On this view, work in philosophical history is distinctive in that it is a way of doing philosophy, and its form is shaped by the value of doing philosophy in this way. At the same time, doing philosophy historically requires that we attempt to understand the philosophical past, a task that both presentism and antiquarianism avoid. So, I end up with a fourfold distinction among antiquarianism, presentism, history of ideas, and philosophical history, reject the first two, and argue for the legitimacy of the fourth as an intellectual enterprise distinct from the third. I conclude with a brief discussion of some examples illustrating the value of the approach to philosophical history that I recommend, drawn from the work of Cora Diamond.

I will begin, however, with Soames. The reception of Soames’s history of analytic philosophy has been curiously divided. Reviewers whose own work is not primarily in the history of analytic philosophy have generally praised the books, even if raising specific critical points. Respondents whose academic careers have been devoted in some measure to the study of this history have been harsher critics, even if they grudgingly admit to virtues of clarity and argument in Soames’s books. In reply, Soames speaks of ‘misplaced resentment on behalf of professional historians’ and adds that they ‘should not be so quick to take offence. They don’t own the subject....’ (Soames, 2005–6, p. 177).

Soames’s responses to his critics reveal an outlook on the value of the history of philosophy for philosophers, which is already implicit in his books. In his ‘Introduction to the Two Volumes,’ Soames explains that as the philosophical works of the analytic tradition have ‘begun to recede far enough into the past to become history ... we are now in a position to separate success from failure, to discern substantial insights, and to identify what turned out to be confusions or dead ends.’ The goal is to allow the history of philosophy ‘to help us to extend the hard-won gains of our predecessors.’ To achieve this aim, Soames insists, we must not only explain the thoughts of our philosophical forebears, but also argue with them and evaluate them; ‘we must be as prepared to learn from their mistakes as to learn from their achievements’ (Soames, 2003a, p. xi).

In response to criticisms from historians of analytic philosophy that his books get the history wrong, Soames (2006d, p. 645; 2006b, p. 605) distinguishes his ‘philosophically and pedagogically motivated approach’ to history of philosophy from what he calls ‘history-for-history’s-sake.’ The latter ‘investigat[es] highly specialized topics in finer and finer detail’ and can degenerate into an ‘antiquarian study of minor works, unpublished manuscripts, and private correspondence’ (Soames, 2006d, p. 655). Soames (2006d, p. 661) sees his critics as arguing, ‘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.’ Soames (2006d, p. 661) ‘do[es]n’t denigrate’ this kind of work. But historians of philosophy should leave room for ‘historical projects with goals different from theirs which they should learn to approach with an open mind’ (Soames, 2005–6, p. 177). Such is his own work, which aims to ‘develop a broad and useable picture of where we are now and how we got here’ (Soames, 2006d, p. 654).

Soames (2006d, p. 655) sees his critics as pursuing ‘history for its own sake,’ which he equates with an ‘antiquarian’ approach. It is worth asking how historians would view this identification. David Hackett Fischer (1970, p. 140), in a well-known study of the ‘logic of historical thought,’ Historians’ Fallacies, characterizes the ‘antiquarian fallacy’ as a ‘fallacy of narration’ and explains that ‘an antiquarian is a collector of dead facts, which he stuffs full of sawdust and separately encloses in small glass cases.’ So described, antiquarianism is not a fallacy in the logician’s sense of an error in reasoning leading to conclusions that do not follow from one’s premises. What Fischer has in mind is, rather, that the antiquarian blocks the routes of inquiry proper to historical investigation. This thought presupposes some idea of the purpose of the study of history.

A cursory glance at the vast historiographical literature shows that most historians agree with Fischer and reject antiquarianism as much as Soames. In a classic polemic, What Is History?, E.H. Carr (1961, p. 14) decries the ‘...nineteenth-century heresy that history consists in the compilation of a maximum number of irrefutable and objective facts’:

Anyone who succumbs to this heresy will either have to give up history as a bad job, and take to stamp-collecting or some other form of antiquarianism, or end in a madhouse.... It is this heresy, which...has had such devastating effects on the modern historian, producing...a vast and growing mass of dry-as-dust factual histories, of minutely specialized monographs, of would-be historians knowing more and more about less and less, sunk without a trace in an ocean of facts’.

Carr (1961, p. 29) rejects this ‘heresy’ because it does not respect the ‘function of the historian’, which is ‘neither to love the past nor to emancipate himself from the past, but to master and understand it as the key to the understanding of the present.’

As this last quotation reveals, however, Carr is not a historian who studies the past simply for the sake of the past. It is commonplace to distinguish historians whose study of history subserves some other agenda, from ‘historicists’, who seek to study history ‘for its own sake.’ Yet, even the most historicist of historians would agree with Fischer that antiquarianism is a ‘fallacy.’ G.R. Elton (1987, p. 65), in another classic of historiography, The Practice
of History, labels Carr a ‘Whig’ for his belief in progress, and a ‘purpose in history’ in the sense of a telos guiding the sequence of historical events. Yet, while insisting on ‘the autonomy of history,’ that ‘the study of history is legitimate in itself, and any use of it for any other purpose is secondary,’ even Elton (1987, p. 66) sees the study of history as having an intrinsic telos: ‘The task of history is to understand the past.’ True, he adds that ‘if the past is to be understood it must be given full respect in its own right,’ and warns that ‘unless it is properly understood, any use of it in the present must be suspect and can be dangerous.’ But this does not reduce history to an antiquarian pursuit. Elton (1987, p. 151) identifies antiquarianism as the ‘lower form’ of historical description, in contrast with the ‘higher form,’ ‘the meaningful description of the past.’ The antiquarian mistakes a mass of historical detail for history and ‘wants to know, not to understand.’ Consequently ‘when antiquarianism pretends to be history ... doubts must arise’ (Elton 1987, p. 152). Even the most historicist of historians seek understanding and historical explanation. As John Tosh explains the ‘mainstream academic view,’ ‘The study of history ‘for its own sake’ is not mere antiquarianism,’ because historical explanation is essential to historical inquiry, and ‘... explanation too can be sought “for its own sake”’(Tosh and Lang, 2006, pp. 47, 52).

So, historians who study history ‘for its own sake’ reject the assimilation of their work to antiquarianism. But does this point bear on the nature of that peculiar study, the history of philosophy? Most historians would view the work of those among Soames’s critics who practice the ‘history of analytic philosophy’ as bad history. We tend to write about our authors without concern for the broader historical context. We do not spend much time on the social class of the authors we study, their cultural background, or the economic and political climate and events of the times in which they wrote. We may occasionally make reference to such matters, but our focus is clearly elsewhere. Are we, then, guilty of antiquarianism, as Soames suggests? Do we fail to fulfill the function of the historian, to understand the past?

Antiquarianism is certainly a danger in the study of the history of philosophy. Consider, for example, the vision of ‘humanities scholarship’ and ‘Wittgenstein research’ presented by Cameron McEwen, a partner in the firm InteLex, which has been responsible for making available Wittgenstein’s published and unpublished writings in a highly usable digital format. McEwen (2006, pp. 419, 420) argues that, because ‘Wittgenstein research has advanced further in the direction of digital research than has the research on any other figure in the philosophical tradition,’ it is ‘able to function as a model for research elsewhere in the humanities.’

After describing an envisioned ‘digital research platform’ for ‘Wittgenstein scholarship,’ McEwen lays out his model for ‘electronic humanities scholarship.’ Databases will be established in which ‘original language primary sources are annotated in layers of commentary,’ including biographical sketches of
individuals mentioned in the text, descriptions of places referred to, and thumbnail summaries of books cited. Electronic editing by ‘a networked group of researchers’ will result in a change in the ‘form of scholarly contributions’ which ‘may be made in a much more concise and focused way linked...to a specific passage (or passages).’ This form of scholarship will ‘de-emphasize the sort of literary exposition which is required in lectures and articles and...emphasize instead the formulation of discrete points in specific relationships to a particular passage or passages in the primary texts.’ McEwen thinks this will ‘build expert knowledge into the presentation of the texts’ allowing ‘specialized knowledge needed for interpretation’ to be accessed ‘with a single mouse-click.’ In this way, ‘all of the information and resources relating to the field are already present for everyone’ and the ‘role of the expert’ becomes that of ‘participating in the on-going indexing of the knowledge base to facilitate research, teaching and practical application.’ McEwen concludes that ‘The consequence is to shift the activity of humanities research in the direction of current scientific research,’ with ‘research in (say) Wittgenstein...defined by a knowledge of its present state (including open questions in the field) as represented in a complex digital desktop’ (McEwen, 2006, pp. 426–8).

Wittgenstein, who advised promising students not to take up professional careers in philosophy because of the corrupting influence of academic life (Monk, 1990, pp. 264, 323, 334), would have recoiled in horror at the thought that his writing could inspire the kind of scholarly industry described by McEwen. His vision of ‘humanities scholarship’ leaves out altogether the kinds of activities that generated the texts his scholars will busily annotate in layers of commentary. Equally lacking is any sense of the synthetic effort required to understand such texts, which are reduced to text-bits of information. We historians of analytic philosophy would, like Soames, reject McEwen’s antiquarian conception, for we share with our colleagues in History the ambition to, in some sense, understand the past. Yet we are not historians, not really – the understanding of the past which we seek is philosophical, and we are relatively uninterested in distinctively historical explanations of the philosophical past.

Bernard Williams’s well-known distinction between the history of ideas and the history of philosophy is apposite here. Williams (2006, p. 257) draws the distinction along two dimensions:

The two are distinguished...by their product. The history of ideas yields something that is history before it is philosophy, while with the history of philosophy it is the other way round....The two activities can be distinguished also by having rather different directions of attention. The history of ideas...naturally looks sideways to the context of a philosopher’s ideas, in order to realize what their author might be doing in making those assertions in that situation. The history of philosophy, on the other hand, is more concerned to relate a philosopher’s conception
to present problems, and is likely to look at his influence on the course of philosophy from this time to the present.

Where Williams speaks of ‘the history of philosophy,’ I will speak of ‘philosophical history,’ to respect the sense in which it is philosophy first, and history second. For Williams (2006, p. 259), philosophical history is a way of philosophizing, which ‘can help us to deploy ideas from the past in order to understand our own.’ This can sound like Soames’s description of the goal of his own ‘philosophically and pedagogically motivated’ history of philosophy, to ‘develop a broad and useable picture of where we are now and how we got here.’ But Soames’s conception of philosophical history is not Williams’s. Soames wishes to display the march of progress. ‘The most important kind of history for philosophers,’ he writes,

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. (Soames, 2006d, p. 661).

This requires that we ‘extract from the voluminous writings of the philosophers covered...those lessons that every analytic philosopher today, and every student, should be aware of...making their major achievements and failures as clear and comprehensible as possible’ (Soames, 2006d, p. 654). 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.’ Soames’s aim ‘was to contribute to making that demarcation’ (Soames, 2006d, p. 655).

Soames’s historical method fits Fischer’s (1970, p. 135) statement of the converse ‘historical fallacy’ to antiquarianism, ‘presentism’ – ‘a complex anachronism...the mistaken idea that the proper way to do history is to prune away the dead branches of the past, and to preserve the green buds and twigs which have grown into the dark forest of our contemporary world.’ Fischer notes that the identification of this fallacy is often credited to Herbert Butterfield’s The Whig Interpretation of History (1931), citing his definition of the Whig interpretation as ‘the tendency in many historians to write on the side of Protestants and Whigs, to praise revolutions provided they have been successful, to emphasize certain principles of progress in the past and to produce a story which is the ratification if not the glorification of the present’ (Butterfield, 1931, p. v). Fischer (1970, p. 139), however, sees the identification of presentism with ‘Whig history’ as ‘unfair to Whiggery,’ since ‘the same sort of error appears in works by scholars of all political persuasions.’
In spite of Fischer’s qualms, terms like ‘Whig history’ and ‘Whiggishness’ have made their way into the rhetoric of historiography, and critics have brought charges against Soames under these and similar headings. Soames (2007b, p. 467), however, sees the label ‘Whig history’ as a mere ‘pejorative,’ based on a ‘foolish comparison’ of ‘political history to the history of philosophy’ (Soames, 2006c, pp. 12–13):

The real fallacy is in equating the history of a discipline with political and social history. No one would accuse a history of a subject that makes clear and recognizable progress of committing a fallacy because it assesses contributions of past practitioners in light of what can now be recognized as real advances.

Soames takes himself to have a clear idea of philosophical progress, and sees belief in this progress as a prerequisite for the ongoing interest and value of the discipline (Soames, 2007b, p. 467):

I wouldn’t be doing philosophy if I didn’t think that it progressed, and that as a result we know more now than we did a century ago. ... For those who think this is ‘Whig history’... if you don’t think that progress is made in philosophy, or that history should chronicle it, why should we be interested in the subject, or its history, at all?

Soames thus distinguishes the ‘history of a subject that makes clear and recognizable progress’ from ‘political history’ and argues that in the case of such a discipline, presentism is a perfectly reasonable historical approach. He adds that unless one believes philosophy to be such a discipline, one must reject it as a pointless waste of time, and he states his own assurance that philosophy does make such progress.

What, though, does Soames mean by ‘progress’? An analogy he draws to elucidate his ‘philosophically and pedagogically motivated history’ gives a clue (Soames, 2006b, p. 606):

The model here is the history of logic. Just as we standardly introduce students to the theorems of great logicians like Gödel, Tarski, and Church by giving streamlined proofs of strengthened versions of the material in their original chapters, so we do the same for philosophers like Russell, Wittgenstein, and Quine. Of course, philosophy is not logic, and progress in the former, though genuine, is often less clear and more likely to be entangled with the questionable, or downright incorrect, than it is in the latter. For this reason, the interpretive task is more delicate in philosophy, and the susceptibility to criticism of even the important milestones of the subject is something from which there is much to learn.
Progress in philosophy, then, is a matter of accumulating results, hard-won truths analogous to the great theorems of mathematical logic. Let us accept this conception of the value of philosophy and the nature of its results for the moment. Does this justify Soames’s presentist methodology? Is presentism an acceptable approach even in the history of logic?

To say the least, this claim is more controversial than Soames realizes. In fact, even within the history of mathematics and logic, ‘presentism’ and ‘Whig history’ are now often seen as cardinal sins. It is worth pausing to reflect on the history of this development. As Nick Jardine (2003, p. 125) has pointed out, although the term ‘Whig history’ derives from political history, its originator, Butterfield, was Chairman of the Cambridge History of Science Committee and the author of *The Origins of Modern Science*, a book which is often characterized as containing a good deal of Whiggish history in spite of methodological warnings in its Introduction against ‘seiz[ing] upon this particular man in the fifteenth century who had an idea that strikes us as modern, now upon another man of the sixteenth century who had a hunch or anticipation of some later theory – all as if one were making a catalogue of inventions or of maritime discoveries’ (Butterfield, 1951, p. 8). The term ‘Whig history,’ used in Butterfield’s sense, first acquired a vogue in the history of *science*, in the 1960s. While Jardine (2003, p. 126) traces a first use of ‘Whig history’ as a term of criticism in the history of science to 1961, a signal event leading to the widespread rejection of presentism in the history of science by the 1970s was the publication of Thomas Kuhn’s *The Structure of Scientific Revolutions* in 1962.

In the Introduction to *Structure*, Kuhn (1962, p. 1) describes the dominant ‘image of science’ as ‘drawn, even by the scientists themselves, mainly from the study of finished scientific achievements as these are recorded in the classics, and more recently, in the textbooks...’ Since ‘the aim of such books is persuasive and pedagogic’ they do not yield ‘a concept of science...likely to fit the enterprise.’ Yet ‘even from history’ such a concept ‘will not be forthcoming if historical data continue to be sought and scrutinized mainly to answer questions posed by the unhistorical stereotype drawn from science texts.’

This presentist approach to history leads to definite conclusions about the history of science (Kuhn, 1962, pp. 1–2):

If science is the constellation of facts, theories, and methods collected in current texts, then scientists are the men who... have striven to contribute one or another element to that particular constellation. Scientific development becomes the piecemeal process by which these items have been added, singly and in combination, to the ever growing stockpile that constitutes scientific technique and knowledge. And history of science becomes the discipline that chronicles both these successive increments and the obstacles that have inhibited their accumulation.
Such a pedagogically and scientifically oriented history of science leaves the historian with two tasks: to determine who discovered or invented each ‘contemporary scientific fact, law, and theory,’ and when; and to ‘describe and explain the congeries of error, myth and superstition that have inhibited the more rapid accumulation’ of such facts, laws and theories. The resemblance to Soames’s ‘pedagogically and philosophically oriented’ history of philosophy is striking.

Yet, Kuhn (1962, pp. 2–3) argued, the experience of actual research in the history of science had begun to loosen the grip of this approach on historians of science. The sorts of questions to which this approach directed them became harder, not easier, to answer as they pushed their historical researches deeper and further. They began to ‘suspect that these are simply the wrong sorts of questions to ask,’ became doubtful of the image of science as developing by accumulation, and were brought to question any presentist distinction between ‘science’ of today and ‘superstition’ of the past. Kuhn described an ongoing ‘historiographical revolution in the study of science’ leading to new questions, and new emphases. The practitioners of this new history of science ‘attempt to display the historical integrity of that science in its own time....they insist upon studying the opinions of that group and other similar ones from the viewpoint – usually very different from that of modern science – that gives those opinions the maximum internal coherence and the closest possible fit to nature.’ This new history of science yields ‘a new image of science,’ more true to the actual practice of science as revealed by historical inquiry.

In a 1968 encyclopedia entry on ‘The History of Science,’ Kuhn described the ‘maxims’ of this new historiography of science (Kuhn, 1977, p. 110):

Insofar as possible (it is never entirely so, nor could history be written if it were), the historian should set aside the science that he knows. His science should be learned from the textbooks and journals of the period he studies...Dealing with innovators, the historian should try to think as they did...he should ask what problems his subject worked at and how they became problems for him...what his subject thought he had discovered and what he took the basis of that discovery to be.

Such maxims resemble Butterfield’s (1931, p. 16) description of ‘an alternative line of assumption upon which the historian can base himself when he comes to his study of the past’:

...he comes to his labours conscious of the fact that he is trying to understand the past for the sake of the past, and though it is true that he can never entirely abstract himself from his own age, it is none the less certain that this consciousness of his purpose is a very different one from that of the whig historian, who tells himself that he is studying the past
for the sake of the present. Real historical understanding is not achieved by the subordination of the past to the present, but rather by our making the past our present and attempting to see life with the eyes of another century than our own.

As with Butterfield, so with Kuhn: the goal of the history of science is the understanding of the scientific past.

The subsequent development of the historiography of science saw the rejection of ‘Whiggishness’ sometimes taken to extremes, as if the historian of science could return to the state of mind of past scientists, forgetting everything learned in the interim. This generated a presentist response, pointing out that even if history aims at understanding the past, we must achieve this understanding, and we can only do so from the standpoint of the present. In a 1979 chapter, ‘In Defense of Presentism’, for example, David L. Hull (1979, p. 3) argued that ‘certain forms of presentism in history of science [are] necessary evils and still others [are] perfectly legitimate.’ Yet, at the same time, he insisted that ‘certain forms of presentism are both undesirable and eliminable.’ As Hull points out, Kuhn anticipated the inevitable intrusion of the present into history, writing that the historian of science should ‘set aside the science that he knows’ only ‘insofar as it is possible,’ since ‘it is never entirely so, nor could history be written if it were.’ Similarly, Butterfield hedges his call to ‘understand the past for the sake of the past’ and to make ‘the past our present,’ noting that the historian ‘can never entirely abstract himself from his own age.’

The result of the historiographical debate over presentism in the history of science was to reject fully ‘presentist’ history while recognizing that it is impossible to erase the present from the works of the historian. The function of the historian is indeed to understand the past, but any understanding must itself be situated in the present. This general viewpoint has by now made its way into the textbooks: Helge Kragh (1987, pp. 104–6), in An Introduction to the Historiography of Science, expounds on the ‘misery of Whig historiography’ but adds that ‘the historian cannot liberate himself from his own age and cannot completely avoid the use of contemporary standards.’ Still, in spite of calls for ‘anti-anti-Whig history,’ it remains important that the historian is not ‘forced to look at the past with modern science as his starting point,’ and ‘as a methodological guide and an antidote to pitfalls of Whig history, the diachronical ideal is indispensable.’

Moving toward Soames’s example of the history of logic, we can pause to note that while presentist approaches have held sway for longer in the history of mathematics, the emerging consensus there mirrors the position sketched above in history of science. According to Leo Corry (2004, p. 4), while ‘in the not-too-distant past, the history of mathematics was mostly written by mathematicians, mostly for mathematicians, and mostly in the purest tradition of Whig history,’ this tradition came under
sustained criticism beginning in the 1970s, so that by the first decade of
the twenty-first century, ‘Whig-like history of mathematics is less and less
frequent, and certainly less and less argued for.’

In the minor subfield which is the history of logic, explicit historiogra-
phical discussion of presentist approaches to history is hard to find,
but the story seems nonetheless to parallel that in the history of science
and of mathematics. Earlier histories of logic, such as William and Martha
Kneale’s The Development of Logic, took a decidedly presentist tack. The
Kneales (1962, p. v) state in their Preface that their history ‘is an account
of the growth of logic’ in which their ‘primary purpose has been to record
the first appearance of those ideas which seem to us most important in
the logic of our own day.’ If one peruses the table of contents of the recent
multi-volume series Handbook of the History of Logic, one can see that the
Kneales’s presentist approach has at least partially been displaced by a more
historically informed sensibility. An entire volume of the Handbook (Gabbay
and Woods, 2008) is devoted to British Logic in the Nineteenth Century, with
chapter-length treatments not only of figures like Boole and DeMorgan, who
play their part in the Kneale’s narrative, but also of authors whose contribu-
tions to logic have long since been forgotten by logicians. Several of these
figures, such as George Bentham, Richard Whately, William Whewell, and
F.H. Bradley, do not even appear in the Kneales’s index, while others, such
as Stanley Jevons and Lewis Carroll, receive only brief passing mentions.
(In one case, Samuel Taylor Coleridge, the Kneales make a single reference
to his poetry which has nothing to do with his logical work.) The work of
each of these figures would be dismissed by the Kneales as ‘work which
does not deserve to be remembered for its own sake.’ Yet, each is historically
significant and so merits serious discussion in a twenty-first century history
of nineteenth-century logic.

So, within the history of science, including the history of mathematics
and logic, the presentist approach favored by Soames has fallen into disfavor.
In one sense, this undercuts Soames’s claim that the model for his approach
can be found in the history of logic. However, Soames might reply that this
only shows that the history of logic has taken a wrong turn. He might cite
the arguments of the biologist and historian of biology Ernst Mayr. Mayr
presents a spirited defense of what he calls a ‘developmental’ approach in
the history of science. In language reminiscent of Soames’s, Mayr (1990,
p. 302) writes:

...Butterfield was ill advised in his literal transfer of the whig label
from political history to history of science. It was based on the erro-
neous assumption that a sequence of theory changes in science is of the
same nature as a sequence of political changes.... In political changes
succeeding governments often have diametrically opposed objectives
and ideologies, while in a succession of theories dealing with the same
scientific problem each step benefits from the new insights acquired by the preceding step and builds on it. ... For this reason the historiography of science proceeds by necessity in many respects very differently from political historiography. This is most clearly recognized by those who write developmental history of science.

Mayr’s ‘developmental history’ serves the scientist’s main interest, ‘to illuminate or reconstruct the pathway of the currently prevailing ideas of science’ (Mayr, 1990, p. 305). Yet Mayr (1990, p. 303) admits that ‘Butterfield’s Whig label is perhaps justified ... where modern hindsight is used to make unfair value judgments about earlier authors. Any author must be evaluated in terms of the intellectual milieu of his time. ... The worst cases of bias are those where a historian completely falsifies the past. ... To omit correct components of an otherwise erroneous theory is a falsification of history.’ Mayr (1990, p. 305) further agrees with anti-Whig historians that history should not be ‘finalistic;’ ‘Butterfield quite rightly criticizes the tendency ... to describe the “present as the inevitable outcome of a triumphant historical process” or “the tendency ... to emphasize certain principles of progress in the past and to produce a story which is the ratification if not the glorification of the [present].”’

Soames, too, thinks of himself as writing a ‘developmental history’ of a ‘subject that makes clear and recognizable progress.’ Mayr’s is the best case I know of for a presentist approach to the history of such subjects, yet it has to be hedged with warnings against finalism and its attendant falsification of history. Soames’s critics have argued, to my mind convincingly, that his presentist history of philosophy succumbs to both dangers. My point here, however, is not merely to criticize Soames but to consider the alternatives. As we saw, Soames thinks of philosophy as making progress, and models this progress on advances in logic – philosophy advances through establishing results analogous to the theorems of the latter discipline. Up to now, we have accepted this vision of philosophical progress. I would now like to open this conception itself to question.

Soames’s picture of philosophical progress implies a particular conception of the point and value of philosophical inquiry. As Soames (2007b, p. 467) puts it in a rhetorical question, ‘if you don’t think that progress is made in philosophy, or that history should chronicle it, why should we be interested in the subject, or its history, at all?’ The implication is clear: the history of philosophy, and indeed philosophy itself, would not be worthy of pursuit if philosophy were not a discipline that advances through the incremental accumulation of positive knowledge.

However, to Soames’s question of why we should be interested in philosophy, many answers have been given. There is not one telos that all philosophers have pursued. Philosophers, even in the analytic tradition, have conceived of the purpose of their inquiry in quite different terms. Consider,
Philosophy is to be studied, not for the sake of any definite answers to its questions, since no definite answers can, as a rule, be known to be true, but rather for the sake of the questions themselves; because these questions enlarge our conception of what is possible, enrich our intellectual imagination and diminish the dogmatic assurance which closes the mind against speculation; but above all because, through the greatness of the universe which philosophy contemplates, the mind also is rendered great, and becomes capable of that union with the universe which constitutes its highest good.

Soames thinks of philosophy as making progress through establishing answers and solving problems. On the view expressed by Russell, there is not much hope for serious progress in this sense. Yet, one who takes this view need not deny that one can make headway in philosophy. It is just that we move forward in philosophy to the extent that, through philosophic contemplation, its questions enable us to grow in certain ways. More generally, before we can know what it means to move forward in philosophy, we must settle the issue of what we take the goal of philosophical activity to be.

Soames (2006c, p. 12), in a reply to an earlier, not very well-formulated attempt by me to make this point, takes me to espouse a kind of easy relativism. He characterizes me as holding that ‘Progress is in the eye of the beholder’ as a way of explaining ‘why we should be interested in the history of a subject that never makes [progress].’ He rejects my ‘paean to nonjudgmentalism, with its admonition to understand past philosophers on their own terms, not ours’ as a ‘defeatist view of... history.’ Needless to say, I do not accept this account of my position. My claim is not that philosophy never advances, or that whether a genuine advance has been achieved is a mere subjective matter. Rather, my point is simply that before we can even address such questions, we must consider the logically prior question of the telos of philosophical activity. One of the things that the history of philosophy can do for us is to expose us to these distinct conceptions of the value of philosophy, and corresponding ways of philosophizing. In doing so, it will, in Russell’s words, ‘enlarge our conception of what is possible’ – in philosophy – ‘enrich our intellectual imagination and diminish...dogmatic assurance.’

This brings us back to Williams’s distinction between history of ideas and history of philosophy, or, as I prefer to call it, philosophical history. Philosophical history has philosophy as its product, yet its method is historical. Why philosophize in this way? For Williams (2006, p. 260), meaningful history of philosophy must ‘yield...philosophy that can help us in reviving a sense of strangeness or questionability about our own philosophical
assumptions.’ Therefore, it ‘must maintain a historical distance from the present, and it must do this in terms that can sustain its identity as philosophy’ (Williams, 2006, p. 259). Borrowing a phrase from Nietzsche, Williams (2006, p. 263) speaks of philosophical history as ‘untimely’.\(^{17}\)

What was called in the original distinction ‘the history of philosophy’ is essential to any activity that is going to give a philosophical point to writing historically about philosophy. That point is going principally to be found in the possibility of the past philosophy’s being untimely, and helping to make strange what is familiar in our own assumptions.

Williams (2006, pp. 260–1) contrasts such ‘untimely’ history with the history of ideas, and with presentist approaches to the history of philosophy, neither of which can fulfill this point:

... the history of ideas ... does not yield much philosophy that can help us in reviving a sense of the strangeness or questionability about our own philosophical assumptions. It may be, simply and quietly, what it seeks to be, about the past. The history of philosophy, very often, does no more to release us from our preconceptions, for the different reason that it is merely constructed out of our preconceptions. ... The important thing ... is that neither of them, as things are, helps us ... to gain what Nietzsche called an ‘untimely’ perspective on our philosophical concerns. The first fails to do so because it does not, in itself, yield philosophy; the second yields philosophy, but only too much of the time it yields our philosophy.

Soames’s history falls into the second of these types. He denies that it is necessary to establish a historical distance from his subjects: ‘Fortunately, the philosophy done in this period is still close enough to speak to us in terms we can understand without a great deal of interpretation’ (Soames, 2003a, p. xi). However, as Tadseuz Szubka (2007, pp. 290–1) remarks:

It is simply not true that one can understand and give an accurate account of various twists and turns of the analytic tradition ‘without a great deal of interpretation’. To begin with, it seems illusory that early analytic philosophy is still so close to us that it does not cause any major interpretative problems. The context in which Moore and Russell developed their revolutionary views was different from the context in which we do philosophy today. Soames does not pay much attention to that context...

By reading Moore, Russell, and the others without attention to the wide differences in philosophical context, interests, and concerns that separate
them from the contemporary philosophical scene, Soames’s history erases from the past anything that might bring into question his own basic philosophical assumptions. While he works hard to determine ‘where Quine was right, and where wrong, on analyticity, indeterminacy and inscrutability,... how to separate success from failure in ordinary-language philosophy,...where Davidson’s theory of meaning fits in,...how missteps obscured some of Kripke’s central insights’ (Soames, 2006c, p. 13), Soames knows in advance that these are the important questions to answer, and knows in advance as well the two great ‘achievements’ that guide his inquiry: ‘(i) the recognition that philosophical speculation must be grounded in pre-philosophical thought, and (ii) the success achieved in understanding, and separating from one another, the fundamental methodological notions of logical consequence, logical truth, necessary truth, and apriori truth’ (Soames, 2003a, p. xi). Bruce Kuklick (2006, p. 549) pointedly remarks that a comparison of the footnotes and suggestions for further reading in Soames’s history with Soames’s own philosophical works reveals that ‘the figures included in this history, or the problems examined, are usually those that Soames or his friends have written about....the two volumes are a summary of past work that is important to Soames.’

The resulting attitude toward philosophers of the past is illustrated by Soames’s treatment of P.F. Strawson. Soames critiques at length an early chapter of Strawson’s on truth, but does not take up any of his major works. In his review of Soames’s books, Peter Hacker complains that this treatment is not fair to Strawson, since Soames has left out of account ‘his most important and representative work’ (Hacker, 2006, pp. 124–5):

His highly influential ‘On Referring’ (1950) is passed over in silence. There is no mention of his Introduction to Logical Theory (1949),...of Individuals (1959), which introduced the notion of descriptive metaphysics,...of The Bounds of Sense, which revolutionized Kant studies, or of Subject and Predicate in Logic and Grammar (1974)...

Soames’s response illustrates his approach to past philosophers with crystal clarity (2006a, p. 428):

Hacker takes himself to be making the supposedly devastating criticism that my selection does not represent Strawson’s best work. However, this is no criticism at all. The aim of the chapter on Strawson was not to show him at his best (or worst); nor was it to evaluate his standing among philosophers of his time. Rather, it was to illustrate three serious errors common among Oxford philosophers of his day which, in time, would contribute to the demise of their distinctive approach....This was why I discussed Strawson’s chapter on truth.
Note what Soames is saying here: He did not discuss Strawson’s work because something of value might be gained from its study – apparently, he had nothing to learn from Strawson. His readers, however, will benefit from the cautionary tale of an Oxford philosopher making Oxford mistakes. They will thereby avoid becoming Oxford philosophers themselves.

Soames does respond to Hacker’s criticism that he failed ‘to include ‘On Referring’, Introduction to Logical Theory, and the rest of Strawson’s greatest hits’ (Soames, 2006a, p. 428). He claims in general that ‘although these works have considerable merit, they do not rise to the level of major advances.’ He discusses in particular only ‘On Referring’ and Introduction to Logical Theory. He notes that the first makes a ‘useful point’ about indexicals and truth-bearers, and the second anticipates a ‘now widely accepted analysis of definite descriptions and other quantifier phrases.’ But he claims that notable shortcomings such as missing Kaplan’s distinction between content and character ‘explain why [Strawson’s] work did not reach the first rank’ (2006a, p. 428). Soames has nothing to say about Individuals and The Bounds of Sense, both truly revolutionary works and profoundly influential on such philosophers as Gareth Evans and John McDowell, choosing instead to mention, and dismiss as second-rate, only those of Strawson’s works closest to his own interests, the first another early journal article, and the second essentially a textbook.

That this sort of approach to philosophical history can do both historical and philosophical damage can be illustrated by another example. Early on in his discussion of Bertrand Russell, Soames (2003a, pp. 101–6) introduces ‘Russell’s Formal Language’ and provides a ‘Russellian Interpretation’ for this language, built on a structure of non-linguistic propositions. This serves as the basis for Soames’s presentation of Russell’s theory of descriptions, first presented in ‘On Denoting’ (1905), and of his logicist program, carried out in Principia Mathematica (1910–13). There are a number of ways in which Soames’s presentation of Russell’s logic oversimplifies and misrepresents Russell’s work. But the historical error I want to focus on here concerns the interpretation of that logic in terms of non-linguistic propositions. Here, there is an important shift in Russell’s thinking, which Soames papers over in his book. Although in 1905 and earlier, Russell believed that sentences express non-linguistic propositions, throughout most of the period that Soames discusses, he rejected such theories. Russell’s difficulty, never mentioned by Soames, was in understanding how a non-linguistic proposition could be false. According to Soames (2003a, p. 105), a relational proposition $aRb$ is a complex consisting of the relation $R$ and the objects $a$ and $b$. Russell’s worry was that if this complex existed, then, in it, $a$ and $b$ would be related by $R$, so that $aRb$ would be true. This worry was first raised tentatively by Russell (1906–7, pp. 47–9) in 1906; by 1910, he had become convinced that it was unanswerable. (Whitehead and Russell, 1927, p. 44).
Russell, consequently, replaced the theory of non-linguistic propositions with his ‘multiple-relation theory of judgment,’ sketched in *Principia*, and discussed in detail in *The Problems of Philosophy* (1912) as well as the *Theory of Knowledge* manuscript (1913) and *The Philosophy of Logical Atomism* (1918). He tried to account for apparent references to propositions by giving an account of the contexts in which they appear, such as attributions of judgment and other propositional attitudes. He treated propositional attitudes not as two-place relations between a subject and a proposition, but as multi-grade relations between a subject and the entities judged about. Although Soames devotes a chapter to ‘Russell’s Logical Atomism,’ based on a reading of sections of *The Philosophy of Logical Atomism*, he does not mention the multiple-relation theory of judgment. He skirts the issue by referring only obscurely to Russell’s attempts to deal with propositional attitudes: ‘Russell needed a new category of fact corresponding to... true propositional-attitude sentences... I will not present Russell’s analysis of these sentences, and the facts corresponding to them. Although it is quite interesting, it is also complicated. Russell himself had doubts about it, and never really completed his line of thought’ (Soames, 2003a, p. 191). The reader may wonder, however, why Russell did not simply take propositional attitudes to be relations between subjects and propositions. Soames has simply written out of his history the argument that leads Russell to reject such a solution.

It is not that Soames is unaware of this argument. In a recent discussion piece, Soames (2008b, pp. 317–18) admits that ‘famously,...Middle-Russell, and many others’ rejected substantial propositions because a true proposition and the corresponding fact ‘can’t be identical,’ yet ‘are so closely related that it is hard to tell them apart.’ However, he replies:

To me this position has always seemed backwards. It is facts that are mysterious, and propositions that are obviously real. Isn’t it obvious that there are things I have believed, asserted, and conjectured? That among these things some have been true, and some false? That I have used sentences to express them? That others have done the same, sometimes using the same sentences, and sometimes using different sentences of my language, or even of languages I don’t understand? If, as I maintain, these things are obvious, then it is obvious that there are propositions, in my sense. So, if there is no room in reality for countless pairs of (numerically) distinct but (qualitatively) identical twins – true propositions and the facts that make them so – then it is facts, rather than propositions, that must go.

Perhaps, then, in choosing to remain silent about Russell’s arguments against propositions in writing the history of philosophy, Soames thinks of himself as applying the first of the two ‘achievements’ around which he organizes his volumes: G.E. Moore’s lesson ‘that philosophical speculation
What Is the Good of Philosophical History?

must be grounded in pre-philosophical thought,’ so that common-sense trumps philosophical skepticism. (Soames, 2003a, p. xi). Since, obviously, there are ‘propositions,’ the things we believe, assert, and doubt, and obviously these exist independently of our believing, asserting, and doubting, we can safely discount arguments to the contrary. Yet, what is obvious to Soames has seemed to many philosophers to be a product of philosophical theorizing rather than a piece of common-sense on the order of ‘There are other people.’ Thus, Moore’s achievement in recognizing that ‘philosophical speculation must be grounded in pre-philosophical thought’ has become a means to disguise philosophical speculation as pre-philosophical thought.

To put matters bluntly, Soames’s presentism undercuts the good of philosophical history. Soames asks us to choose between presentism and antiquarianism. But both options are flawed: neither seeks to understand the past, and so both are unable to learn from it. Yet, what is the alternative? If we seek to understand the philosophical past for its own sake, though this need not lead to antiquarianism, it seems it would certainly lead to what Williams calls ‘the history of ideas.’ My brief, however, is to defend as equally legitimate the other side of Williams’s dichotomy, philosophical history. As I noted above, philosophical history is to be understood as a mode of philosophizing. In consequence, philosophical history is necessarily in some sense present-centred – its goal in the first place is not the understanding of the past, but the present philosophical understanding of its practitioners. Yet, philosophical history is also meant to be historical – it aims to achieve present philosophical understanding through understanding the philosophical past, and so it need not involve the form of presentism I have criticized in Soames.

Ivor Grattan-Guinness (1990, p. 157) has coined the phrase ‘a royal road to me’ for that kind of presentist history (of mathematics, in his case) which aims to provide an ‘account of how a particular modern theory arose out of older theories instead of an account of those older theories in their own right,’ thus confounding the questions ‘How did we get here?’ and ‘What happened in the past?’ I would like to suggest that philosophical history is always in some sense a ‘road to me,’ but only becomes objectionable if it is treated as a ‘royal road to me.’ Properly carried out, philosophical history will shape its practitioner philosophically. So, traveling on the road of philosophical history will carry us to a destination which we will recognize as our own, because it will reflect the philosophers we have become through our hard work along the road. But carried out improperly, philosophical history will be a ‘royal road’ – a road we have the kingly prerogative to travel upon because of the philosophers who we already are. Travel along such a road entails no change in us as philosophers; consequently, it does no philosophical work. The danger of confounding the road to me with a royal road to me is ever-present in work in philosophical history.
Nonetheless, one might wonder whether philosophical history is simply bad history – whether it commits another of Fischer’s (1970, p. 142) historical fallacies, ‘tunnel history,’ defined as ‘the tendency...to split the past into a series of tunnels, each continuous from the remote past to the present, but practically self-contained at every point and sealed off from contact with or contamination by anything that was going on in any of the other tunnels.’ Unlike the history of ideas, which ‘looks sideways’ to the broader historical context, philosophical history looks up and down the ‘tunnel’ of philosophical inquiry. Why is this not just bad history? What is the value of philosophical history, over and above the history of ideas?

This question of the legitimacy of philosophical history can be raised through a parallel with a further significant debate in the historiography of science, which raged in the 1960s and 1970s. The ‘internal-external’ debate concerned whether it is necessary in the history of science to pay serious attention to factors ‘external’ to science itself, such as cultural context, political developments, or economic factors. A parallel debate in the historiography of philosophy would turn on the clearly internal character of philosophical history and would concern whether there is a legitimate place for such an enterprise, as opposed to a more externalist and contextual ‘history of ideas.’ I will argue that there is indeed a place for such an enterprise, but that the justification for it must ultimately be found within philosophy. From the purely historical point of view, there is every reason to embrace the history of ideas and discard philosophical history.

The issue posed here arises pointedly in Bruce Kuklick’s review chapter, ‘Modern Anglophone Philosophy: Between the Seminar Room and the Cold War’. Kuklick, a professor of American history and an intellectual historian with a BA and MA in Philosophy, reviews and compares two books: Soames’s history, and George Reisch’s How the Cold War Transformed Philosophy: To the Icy Slopes of Logic. Kuklick makes some interesting critical points concerning Soames’s treatment of various philosophical topics, such as analytic ethics. But Kuklick (2006, pp. 551–2) reserves his sharpest invective for Soames’s presentism: ‘One cannot help but note here the enormous condescension of the present. One imagines Bertie, Ludwig, and Van in some heavenly school of higher learning, notebooks in hand, taking instruction from Professor Soames.’ He goes on to compare Soames to the main character of The Truman Show, living a ‘hermetically sealed existence,’ and to a philosopher trained ‘by reading a hefty collection of books of Greek philosophy in modern English translation’ who decides to write a history of Greek philosophy by ‘roughly ordering the books chronologically, and then summarizing what each thinker says in turn, with due regard to who you think was right; but without any knowledge of ancient Greek or of its culture.’

Kuklick contrasts Soames’s approach with that of historian of science George Reisch: ‘To read How the Cold War Transformed Philosophy of Science: To the Icy Slopes of Logic is to enter another world.’ According to Kuklick
(2006, p. 553), Reisch, an independent scholar educated at the University of Chicago’s Committee on Conceptual Foundations of Science, ‘is not at all concerned with the philosophical ideas of his protagonists. He has done an extraordinary amount of archival work in the chapters of many philosophers — the sort of research that would baffle Soames and which a historian must admire. But while How the Cold War Transformed Philosophy of Science reports on the personal views and political ideals of many men, it says very little about their philosophical positions.’ Faced with the question of explaining the downfall of logical positivism during the 1950s, Reisch rejects any account turning on the quality of the philosophical arguments of the positivists and their critics, and appeals to the politically repressive climate of the McCarthy error to explain the marginalization of the generally left-leaning positivists. Kuklick (2006, p. 556) is not persuaded:

In one sense the argument exemplifies the post hoc, ergo propter hoc fallacy: philosophy is modified after the Cold War; therefore the Cold War is the cause of the modification. But for me there is a larger issue. Scholars like Reisch do not have much time for analytic philosophy....Reisch’s response as a historian is disturbing. Rather than grappling with the post-positivist ideas that analytic philosophy propounds, Reisch has written a history that delegitimizes these ideas. They are guilty of association with McCarthyism, conceived in the sin of the Cold War. We can ignore them because of their origins in a suspect political order, or because they reflect these origins.

Kuklick (2006, pp. 556–7) concludes with a plea for a history of ideas which both recognizes that ‘some people are better than others in articulating difficult conceptual questions’ and is therefore not merely a form of social or cultural history, yet recognizes that ‘thought exists in a social setting.’ Kuklick finds it ‘imperative’ to ‘steer empirically between Platonism and the social determination of ideas’ — ‘because of the barren alternatives.’ Kuklick’s third alternative corresponds to the best form of Williams’s ‘history of ideas’ — and I, for one, have nothing against this project, which can be of great use to the philosophical historian. However, as Williams puts it, ‘it does not, in itself, yield philosophy’ — which is the ultimate goal of philosophical history.

Perhaps the best way to see the nature, point, and value of the kind of philosophical history Williams has in mind is to consider some examples. As I have emphasized more than once, to engage in philosophical history is to philosophize, albeit in a historically informed way. This point is nicely illustrated by the opening remarks of Cora Diamond’s The Realistic Spirit, a collection of chapters containing some of the finest exemplars of the genre of philosophical history that I know. In the Preface, Diamond (1995, p. vii) tells us that the chapters that make up the book have come out of her reading
of Wittgenstein, and notes that the ‘obvious way to divide them’ is into a group of ten chapters on the history of early analytic philosophy (‘seven are explicitly about Wittgenstein and three about the philosopher who most strongly influenced him’ – namely Frege – although Russell is also discussed at some length) and a group of five chapters ‘about ethics...done in a way which reflects what I have learned from him [Wittgenstein].’ Yet, in the first paragraph of the introductory chapter, ‘Philosophy and the Mind’, she claims that ‘there is a sense in which the entirety of this book lies within philosophy of mind’ (Diamond, 1995, p. 1). This can seem an extremely surprising claim, especially if one compares the table of contents to any standard anthology or textbook in philosophy of mind. Where does she discuss mind-body dualism, type and token identity, supervenience, behaviourism, mental causation, functionalism, cognitive science, consciousness, zombie arguments, or qualia? For the most part, she does not. Yet, she claims to be working in the philosophy of mind.

She explains this claim by reverting to a remark of Frege’s that logic and mathematics have as their task not the investigation of minds, ‘the contents of consciousness of individual men,’ but rather of ‘the mind.’ She continues: ‘The theme of this book, viewed as a book within the philosophy of mind, is that we misunderstand our relation to that fundamental idea, that distinction, of Frege’s. We may think that the only choices we have are to take it seriously or to debunk it, to reject it for a thoroughly empirical view of thought and the logical relations between thoughts.’ She does not, however, directly explain how there could be any third possibility between taking the distinction seriously and rejecting it. Rather, she enters into a historical discussion of a line of thought beginning with Frege, passing through Wittgenstein's *Tractatus*, and ending with Wittgenstein’s later philosophy, with the remark: ‘To understand the distinction, the first thing that is necessary is clarity about what it was for Frege’ (Diamond, 1995, p. 1). The point of this story is not only to show us the development of Frege’s and Wittgenstein’s thought, but to suggest that ‘Wittgenstein's attention to what we do is compatible with respect for Frege’s distinction between what empirical psychology might show us of people's minds and what belongs to the mind,’ without falling into ‘a mythology of that distinction’ so that we can look ‘with a realistic spirit’ at the mind, ‘at thought, at the coherence, the commitments, and connections that belong to it as thought’ (Diamond, 1995, pp. 4–5). In thus ‘respecting’ the distinction with a ‘realistic spirit,’ we do not ‘take it seriously’ in the sense of simply accepting it as correct – that would involve falling into the mythical conception of the distinction – but we also do not simply ‘debunk’ it as false.

Of course, I have not shown that Diamond’s philosophical history actually does the philosophical work she claims for it. I cannot work through the details of her argument in ‘Philosophy and the Mind’, or in the chapters which make up *The Realistic Spirit*. But I do want to draw attention to the
way in which she recovers from her reading of Frege and Wittgenstein a problem which is relatively neglected in contemporary philosophy of mind, and an approach to that problem that might at first sight seem unavailable. This illustrates the function which Williams identifies for philosophical history – in making the familiar strange, it can allow us to see a whole area of philosophy in a new light. Thus, philosophical history can fulfill what Russell saw as a function of philosophy itself, opening us to new ways of thinking and so enlarging our minds.

In a later chapter on ‘Truth before Tarski’, Diamond (2002, p. 252) explicitly acknowledges this aim of philosophical history, quoting John McDowell: ‘One of the benefits of studying a great philosopher from an alien age is that it can help us to see that we do not have to swim with the currents of our own time.’ Whereas McDowell was speaking of Aristotle, Diamond is once again concerned with Frege, Russell, and Wittgenstein, thinkers whom Soames thinks of as cozy and familiar, requiring little interpretive effort to understand. Diamond (2002, p. 253), in contrast, writes of ‘the alien age of early analytic philosophy.’ Here, too, Diamond’s aim is Williams’s (2006, p. 260), to ‘make the familiar strange again’ by not allowing us to ‘benefit from this hindsight, or the presuppositions peculiarly associated with our inherited history of philosophy.’ Her chapter also illustrates how much philosophy can be profitably done by reading, thinking about, and critically reacting to, work in philosophical history – as her subtitle ‘After Sluga, after Ricketts, after Geach, after Goldfarb, Hylton, Floyd, and Van Heijenoort’ makes clear.

Diamond (2002, p. 257), following Sluga, is concerned to argue that, with the dominance of approaches to truth inspired by Tarski’s definition of a truth-predicate for a precisely defined formal language in a precisely defined meta-language, ‘we have lost the pre-Tarski understanding of the problem of truth.’ Note well: her point is not that we have lost the pre-Tarski understanding of truth, but that we have lost the pre-Tarski understanding of the problem of truth. Tarski has not simply given us new answers to our old questions – he has given us a new question, and an answer to it – and has thereby taken away some of our old questions. Diamond (2002, p. 272) thinks that it is important to recover some of these questions and problems, and thereby ‘to provide a route into one mode of pre-Tarski thought about truth, a mode of thought that is implicitly highly critical of later developments.’ She provides this route through work in philosophical history.

Diamond’s chapter is complex, rich, challenging, and rewarding, and I will not attempt a summary here. I will add something, however, about the ways in which the pre-Tarski mode of thought about truth which she recovers is ‘implicitly highly critical of later developments’ – a point the details of which she herself leaves largely unstated. Consider the project undertaken in Tarski’s ‘The Concept of Truth in Formalized Languages’.
Tarski’s target is the concept of truth as applied to sentences of a language. He begins with the intuition that truth is correspondence to reality – ‘the so-called classical conception of truth (‘true – corresponding with reality’)' (Tarski, 1983, p. 153) – and derives from this intuition the constraint that any acceptable definition of truth must yield as consequences all instances of the schema:

\[
\textit{x is a true sentence if and only if } p
\]

where ‘\(p\)’ is replaced by a sentence and ‘\(x\)’ is replaced by an ‘individual name of that sentence’ (Tarski, 1983, pp. 155–6). Tarski (1983, pp. 157–65) then argues that the Liar paradox prevents the construction of any consistent definition meeting this requirement for natural languages, due to their ‘universality’ – the fact that in natural languages every expression of the language can be named. Tarski therefore restricts his attention to precisely defined formalized object-languages which do not contain the capacity to refer to their own expressions. In a tour de force of mathematical logic, he then proves that for such object-languages, if we employ a meta-language into which we can translate the object-language, and in which in addition we can refer to the expressions of the object-language and state the syntactical rules of the object-language, we will be able to construct a definition of truth meeting the following precisely formulated version of the above informal constraint (Tarski, 1983, pp. 187–8):

CONVENTION T. A formally correct definition of the symbol \(\textit{Tr}\), formulated in the metalanguage, will be called an adequate definition of truth if it has the following consequences:

\((\alpha)\) all sentences which are obtained from the expression ‘\(x \in \textit{Tr} \text{ if and only if } p\)’ by substituting for the symbol ‘\(x\)’ a structural-descriptive name of any sentence of the language in question and for the symbol ‘\(p\)’ the expression which forms the translation of this sentence into the metalanguage;

\((\beta)\) the sentence ‘for any \(x\), if \(x \in \textit{Tr}\) then \(x \in S\)’ (in other words ‘\(\textit{Tr} \subseteq S\)’). 24

Tarski intends his definition of a predicate satisfying Convention T to serve as a kind of ‘rational reconstruction’ or ‘explication’ in Carnap’s sense of the pre-theoretic notion of truth with which he began.

How should we evaluate the success of Tarski’s project, from the point of view of the ‘alien age of early analytic philosophy,’ as Diamond describes it to us? While Tarski’s impressive formal achievement surely cannot be denied, just as surely Diamond thinks that something significant about the philosophical problem of truth has gone missing from his explication of our pre-theoretic understanding of truth. Here I will just give a few brief indications which might help us begin to see what has been lost.25

According to Diamond (2002, pp. 257, 259), the early analytic philosophers begin from an ‘inchoate understanding of truth’ as involving some
sort of agreement with reality, a dimension in which our thinking and talking is ‘responsible to the world.’ This appears to be similar to the sort of pre-theoretic understanding of truth that Tarski is getting at when he speaks of the ‘classical conception of truth.’ But the work of Frege, Russell, and Wittgenstein, brings to light important features of this inchoate understanding of truth, which pose difficulties for Tarski’s project to rationally reconstruct our pre-theoretic understanding of truth, yet do not impinge on it as it is actually carried out. These difficulties come out when we situate the ‘inchoate understanding of truth’ within a broader ‘inchoate understanding of informative discourse’ (Diamond, 2002, p. 257).

Tarski (1983, pp. 168–9, 172) defines truth for the sentences of a formalized language, which he defines to be concatenations of expressions of the language, strings of signs. Tarski can do this because he is dealing with formalized languages of his own creation. But this conception of truth-bearers as mere linguistic strings cannot be sustained for languages in actual use. Diamond (2002, pp. 253–4) refers to Frege’s distinction between ‘what grammar counts as a Satz and what logic counts as a Satz,’ citing Geach’s example: “‘Socrates was bald’ does not occur in “A philosopher whose teacher was Socrates was bald” but does occur in “Socrates, who taught Plato, was bald’”. Frege held, of course, that truth is primarily ascribed to non-linguistic thoughts which are expressed by sentences, and, Diamond (2002, pp. 253–4) notes, the ‘two principal alternatives’ often seem to be ‘the non-linguistic proposition and the linguistic string.’ But she wants to make room for a third possibility, Wittgenstein’s notion of a Satz: ‘A Satz is a bit of language, a kind of sign, in a certain kind of use’ (Diamond, 2002, p. 254). The use of a sign that makes it into a Satz, and into the Satz that it is, is a matter of logic, for Wittgenstein and Frege – for example, a matter of it being the sort of thing that can be the premise and conclusion of inferences, and of which inferences it is caught up in. This, if accepted, puts into question any project of defining truth for sentences first, and then using that definition to generate a definition of logical consequence, as in Tarski. Moreover, it becomes clear that where Tarski can take as given the identity of the sentences of the language for which he will define truth, in any application to an actual language in use, the very identity of the truth-bearing elements will be inextricably intertwined with the logical relations in which they stand.

This provides Diamond (2002, p. 257) with a way of putting part of Sluga’s point that ‘we have lost the pre-Tarski understanding of the problem of truth’: 

On the pre-Tarski view, work in logic is essential in articulating what is involved in informative discourse, and such articulation is answerable to the inchoate understanding we have of such discourse. ...later analytic philosophy lost its understanding of the problem of truth in losing that earlier conception of answerability to the inchoate understanding of informative discourse.
In replacing natural language, in which the truth-bearing units can only be isolated by a logical analysis in which we identify ‘certain bits of language as having the logically recognizable employment’ (Diamond, 2002, p. 254) of a Satz, with a formal language in which we can identify the truth-bearing units as strings of signs meeting certain syntactic tests, we sever our account of truth from ‘answerability to the inchoate understanding of informative discourse’ (Diamond, 2002, p. 257).

The Tarskian view of the bearers of truth as concatenations of expressions faces a further set of issues, since sentences and names are treated as entities of the same basic type. A complex name such as ‘2+3’ and a sentence such as ‘2<3’ are both concatenations of signs, and it is only the syntactical rules of the language that determine which is a sentence and which a name. Diamond traces a line of thought running most explicitly in Wittgenstein, but anticipated by Frege and to some extent implicit in Russell’s struggles with the multiple-relation theory of judgment, according to which the classification of a sign as of this or that syntactic type is determined by reflection on the logical powers of the sign. One form of this logical reflection turns on the duality which is characteristic of sentences – the possibility noted by Wittgenstein of ‘communicating with sentences looking and sounding just like the sentences we now use, but having reversed sense’ (Diamond, 2002, p. 263). In a language dual to our own, each sentence will express what in our language is expressed by its negation, and vice versa – each sentence will be the dual of its negation. But names and verbs will be treated differently in dual languages: ‘the dual of a name... is the name itself,’ whereas ‘a verb is a verb, logically speaking, only if it is not self-dual.’ All of this derives from ‘the inchoate understanding of informative discourse, which includes the idea that, if you say the contradictory of what I have said, you speak of the same things and say the opposite about them’ (Diamond, 2002, p. 263).

Hence, the fundamental logical distinction between names and verbs rests on differences in the logical behavior of the two categories of signs. But since it is sentences that are logically interrelated, ultimately the whole argument rests on a more basic distinction between the logical behavior of sentences, and that of subsentential expressions. This raises a further difficulty, connected to Tarski’s treatment of truth as a predicate. Wittgenstein expresses the fundamental logical difference between sentences and names by saying that sentences are facts, not things. One point of this is to undercut talk of sentences as standing in some sort of relation, of correspondence or agreement, with reality. For Wittgenstein, ‘our inchoate understanding of the relation between what we say and the world’ gives way to consideration of inference patterns such as

\[ p \]

So ‘p’ is true and ‘not-p’ is false. (Diamond, 2002, p. 259)
Such patterns bear a resemblance to the biconditionals that play a role in Tarski’s Convention T, which are often written in the form

‘p’ is true if and only if p.

But, as the more general form of Tarski’s T-schema

x is a true sentence if and only if p

reveals, for Tarski, “p” (or ‘x’, an object-variable) fills in for an ‘individual name’ of a sentence – sentences are treated as things, nameables. Consequently, Tarski adds as the second clause of Convention T: A correct definition of truth must entail that only sentences are true. The sentences of the object-language are a part of the universe of discourse of the meta-language, the same logically homogeneous universe of discourse that contains all of the objects referred to by names of the object language. Although Tarski does not treat truth as depending on a relation of correspondence, he does treat ‘true’ as a predicate, and so treats sentences as if they were things, not facts. From Wittgenstein’s point of view, however, the second clause of Convention T is an empty gesture; no sense has been given to appending ‘is true’ to a name like ‘Julius Caesar’ – no sense, that is, which has anything to do with patterns of inference such as those above (for after all, ‘Julius Caesar is true’ may have the sense of ‘Julius Caesar is faithful’). Such patterns of inference require that what occurs as ‘mentioned’ in the conclusion should also be capable of being ‘used’ in the premise – used as a sentence.

These, then, are some of the problems and insights which Tarski’s technical achievement runs the danger of making us forget, and which Diamond seeks to recover through her use of philosophical history. Here, we see how a distinctively philosophical enterprise, aiming to produce philosophically valuable insights, can be based on a historically careful and sensitive analysis of the works of our predecessors – an analysis that respects the historical distance between us and them, allowing them to speak to us on their own terms – but without abandoning a critical stance to what we can then hear them say. Reading our forebears in this way allows our minds to be stretched, and our preconceptions to be put to the test. This, rather than a tale of progress in which we separate the philosophical sheep from the philosophical goats, is the kind of work that exhibits the good of philosophical history.

Notes

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chapter by the Franke Fellows and from comments by Erich H. Reck and Jim Conant. I also benefited from discussion at the University of Chicago Wittgenstein Workshop, where an earlier version of this chapter was presented. I dedicate this chapter to the memory of my beloved wife, Angela Gugliotta, who died on 1 June 2010. In the last months of her struggle with the cancer which took her life, she insisted that I continue to work on this chapter. Without her encouragement, I would not have completed it.

1. In addition to my critical notice, many others have reviewed Soames's books. Shorter reviews included Byrne and Hall (2004); Griffin (2005), (2006); Martinich (2005); Rorty (2005); and Pincock (2005–6). There were also several longer critical notices: Smart (2005); Beane (2006); Livingston (2006); Hacker (2006); Wilson (2006); and Szubka (2007). Soames published replies to Hacker (Soames, 2006a) and Pincock (Soames, 2005–06); the latter was met by Pincock (2006). Soames's books have also featured prominently in broader survey articles: Preston (2005); Kuklick (2006); and Floyd (2009). A Philosophical Studies symposium on his work featured Soames's summaries of his books (Soames 2006b, 2007a), four chapters on vol. 1 – Stoljar (2006), Burgess (2006), Proops (2006), and Sainsbury (2006) – three chapters on vol. 2 – Weatherson (2007), Byrne (2007), and Yablo (2007), and responses by Soames (2006d, 2007b). Soames and I also engaged in a debate on a fine point of Russell interpretation in the pages of Philosophical Studies (Kremer, 2008; Soames, 2008a). The APA symposium to which I contributed saw critical commentary from myself, C. Pincock, T. Hurka, and P. Horwich. Soames's replies from this symposium (2006c) can be found on his website. Valuable discussion occurred in the comments after each of the blog posts (Kvanvig, 2005 and Weatherson, 2005a, 2005b, 2005c). I am indebted to some of this conversation, especially to the remarks of Martin Lin in the last of these comment threads.

2. Among the early reviews, only Rorty’s was negative, with glowing responses from Martinich, Byrne and Hall, and Smart, none of them specialists in the history of analytic philosophy. Many later reviews, including more extended critical notices, were more negative – for example, those by Pincock, Hacker, Beane, Livingston, and myself, all historians of analytic philosophy. The most critical Philosophical Studies symposium pieces were again by historians, Proops and Sainsbury. The harshest and most dismissive criticism of Soames by far, however, came from the intellectual historian Bruce Kuklick, whose response is discussed below.

3. See, for example, Chapter 2 on ‘The Uses of History’ of the standard historiographical textbook (Tosh and Lang, 2006), and especially Section V, ‘History for its own sake?’, pp. 45–7.

4. McEwen (2006, p. 427) thinks that his digital research platform eliminates the need for ‘time and energy … spent getting to know the places where Wittgenstein discussed such and such a topic,’ since ‘anyone can compile such contexts in seconds through electronic searches.’ This ignores the value of the understanding gained through ‘getting to know’ a text. As Herbert Hrachovec (2000) wisely warns, ‘The disappearance of manifest meaning is, in fact, the price to pay for enhanced electronic facilities. One can easily pick any combination of terms and search constraints – but there is no guarantee whatsoever that this will lead to an interesting result…. One has to have a hunch about the possible significance of a term to profitably employ the electronic search function.’

5. Tadeusz Szubka (2007, p. 289) draws attention to Williams’s distinction as well.

8. Soames poses a second question, which I will not take up here: ‘if you agree that history is centrally concerned with real philosophical progress, but you think I have misidentified it, what have been the most important developments in the past century, and what shortcomings in the philosophy that preceded them does our more advanced knowledge allow us to spot?’
9. Political historians already had a more precise use for this term to refer to history written by Whigs, and recognized with Fischer that historians of various political views can be guilty of presentism. See Jardine (2003, pp. 125–6).
10. I have been helped here by Moro-Abadía (2009).
11. Moro-Abadía (2009, fn 1, p. 72) provides a good overview of the resulting debate.
13. Mayr misquotes Butterfield in the last quotation, writing, ‘the glorification of the past.’
14. Soames’s finalism will be discussed below in connection with Kuklick’s review essay. Specific criticisms of Soames’s representation of the history are leveled by Hacker, Proops, Kremer, Sainsbury, Griffin, Szubka, Pincock, Beaney and Livingston. I develop one example, Soames’s neglect of Russell’s rejection of non-linguistic propositions, later in this essay.
15. Similar remarks would apply to Wittgenstein’s (1981, 4.112) view that ‘the object of philosophy is the logical clarification of thought.’
16. I avoid the term ‘progress’ due to the heavy intellectual baggage it carries.
17. Williams claims, in effect, that if philosophical history is not ‘untimely,’ then there is no point to doing it – no point to philosophizing historically. There are, however, different ways of being untimely; for example, philosophical history might undercut something we have come to take for granted by revealing alternate ways of answering the questions we now have, or by showing that it might be better to ask other questions. Even granting this ‘big tent’ conception of the untimely, however, I would not go so far as to say that philosophical history must be untimely. We can do ‘timely’ philosophical history if we approach a historical figure with the desire to learn something from them and discover that what we learn is something we can integrate into our current philosophical framework. In that case, however, the historical character of philosophical history threatens to become inessential. And even in that case, we have to be prepared for history to surprise us – for the history itself to turn out to be ‘untimely.’
18. The claim that Soames is constructing a history of philosophy along the lines of a ‘royal road to me’ – or at least a ‘royal road to Kripke’ – is also made explicitly by Weatherson (2007, p. 432), and Livingston (2006, p. 292), among others. (The phrase ‘royal road to me’, due to Ivor Grattan-Guinness, is discussed below.)
19. I draw in part on my review (Kremer, 2005) of Soames’s books here.
20. These are detailed in my review (Kremer, 2005), among other places.
21. See, for example, Broman (1990).
22. I do not fully endorse Kuklick’s assessment of Reisch’s work, which I have not myself carefully studied. Discussion at the University of Chicago Wittgenstein Workshop has convinced me that Reisch’s work deserves a more nuanced assessment than Kuklick offers. Kuklick’s discussion is nonetheless useful in setting out what is, in effect, a kind of ideal type of a certain sort of intellectual history.
23. Quoting McDowell (1998, pp. 37–38). McDowell is one of a whole string of important analytic philosophers who have made crucial contributions both to
systematic philosophy and to philosophical history. Others who come readily to
my mind include Ryle, Austin, Geach, Anscombe, Strawson, Sellars, Dummett,
Brandom, Burge, and MacIntyre. (This list is far from exhaustive.) It is charac-
teristic of many of these thinkers that when they are writing in a more histor-
ical vein, their work always has a clear philosophical point, while when they
are writing on a topic in systematic philosophy, the history of philosophy is
never far from their minds. While each of these philosophers is an exemplar of
philosophical history, arguably at least some of them at some times fall for the
allure of ‘the royal road to me’ rather than taking the more difficult but more
rewarding ‘road to me.’ But, in Hamlet’s words, ‘use every man after his desert,
and who shall ‘scape whipping.’

24. The second clause of Convention T requires that only sentences are true.
25. While I draw on Diamond’s work here, none of what I say about Tarski is due
to her. The attempt to apply the lessons of her philosophical history of early
analytic discussions of truth to Tarski’s approach is entirely my responsibility.
26. She takes the phrase ‘responsible to the world’ from Sluga.

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The Owl of Minerva: Is Analytic Philosophy Moribund?

*Hans-Johann Glock*

When philosophy paints its grey in grey, then a formation of life has aged, and with grey in grey it cannot be rejuvenated but only known for what it is; the owl of Minerva only begins to take flight with the falling of dusk.

G.W.F. Hegel (Vorrede zu Grundlinien der Philosophie des Rechts, 1821)

**Triumph and Crisis**

By common though not universal consent, analytic philosophy is a little over one hundred years old. Its current state seems to be characterized by a combination of triumph and crisis. On the one hand, it is now the dominant force within Western philosophy (Searle, 1996, pp. 1–2; Preston, 2007, pp. 3, 7–8). It has prevailed for several decades in the English-speaking world. And due to brute institutional facts, especially the numerical strength of American philosophers and philosophical publications (Rescher, 1993) compared to philosophers and publications elsewhere, this amounts to a sociological predominance within Western philosophy. But analytic philosophy is also in the ascendancy in Germanophone countries; and it has made significant inroads even in places once regarded as hostile, such as France.

Analytic philosophy is now thriving in Germany, Austria, and Switzerland, having been reimported after its politically induced exodus after 1933. Although some practitioners still like to think of themselves as a persecuted minority, analytic philosophy is, without the shadow of a doubt, a growth industry and the most powerful single movement.
The development of analytic philosophy in France has been a slower and more painful process. But even in France, analytic philosophy is at present the fastest-growing movement. It is no coincidence, moreover, that some of the most vigorous proponents of analytic philosophy and opponents of the continental alternative, such as Jacques Bouweresse (1983) and Pascal Engel (1997), are French. The situation in Italy, Spain, and many Eastern European countries is similar.¹

Analytic philosophy is not just the most important contemporary philosophical movement in institutional and numerical terms. It also furnishes a point of orientation for the others. Analytic positions on any given issue are the ones that others cannot afford to ignore. Most phenomenologists, hermeneuticians, critical theorists, and deconstructivists have a line not just on analytic philosophy in general, but also on analytic theories in their field, even if that line may sometimes strike analytic colleagues as uninformed, prejudiced, or slightly out of date. By contrast, few analytic philosophers of the younger generation feel any need to have a line on non-analytic doctrines.

At the same time, there are continuous rumours about the ‘demise’ of analytic philosophy, about it being ‘defunct’ or at least in ‘crisis’, and complaints about its ‘widely perceived ills’ (Leiter, 2004, pp. 1, 12; Biletzki/Matar, 1998, p. xi; Preston, 2004, pp. 445–7, 463–4; 2007, Pt. I). A sense of crisis is palpable, not just among commentators but also among some leading protagonists. von Wright noted that in the course of graduating from a revolutionary movement into the philosophical establishment, analytic philosophy has also become so diverse as to lose its distinctive profile (von Wright, 1993, p. 25). This view is echoed by countless observers who believe that the customary distinction between analytic and continental philosophy has become obsolete (for example, Glendinning, 2002; May, 2002; Bieri, 2007).

Loss of identity is one general worry; loss of vigour another. Putnam has repeatedly called for ‘a revitalization, a renewal’ of analytic philosophy (for example, Putnam,1992, p. ix). And Hintikka has maintained that ‘the survival of analytic philosophy’ depends on making a fresh start in virtually all branches of philosophical studies (Hintikka, 1998). Searle is one of analytic philosophy’s most stalwart and uncompromising advocates. Yet, even he concedes that in changing from ‘a revolutionary minority point of view’ into ‘the conventional, establishment point of view’ analytic philosophy ‘has lost some of its vitality’ (Searle, 1996, p. 23). Small wonder that those more sceptical about analytic philosophy have been anticipating, for some time now, its replacement by a ‘post-analytic philosophy’ (Rajchman/West, 1985; Baggini and Stangroom, 2002, p. 6; Mulhall, 2002).

Finally, analytic philosophy has undergone a historical or retrospective turn. Among its practitioners, there is now a wide interest in the history of
philosophy in general and the history of analytic philosophy in particular. And to some pundits, this turn to the past is itself part and parcel of the decline or demise of analytic philosophy.

But perhaps the funeral orations are precipitate. After all, numerous burials at midnight notwithstanding, there is no doubt that something called ‘analytic philosophy’ is triumphing on the global academic market, at the expense of other trends within Western philosophy. The remainder of this chapter will therefore be devoted to the following questions:

Has analytic philosophy ceased to be a distinct and potentially vibrant movement? Is the historical turn a manifestation of, or perhaps even a contributing factor to, its demise? Is analytic philosophy in the course of being replaced by a ‘post-analytic’ philosophy? Should analytic philosophy be superseded by such a movement? I shall give a tentative and qualified ‘No’ in answer to all these questions.

**Death by Historiography?**

While analytic philosophy in general is widely perceived to be in crisis, some branches are indisputably flourishing. One of them is the history of philosophy. This ought to be surprising, at least *prima facie*. The accusation that analytic philosophy lacks historical awareness unites its two main rivals within contemporary Western philosophy: continental and traditionalist philosophy (see Glock, 2008a, p. 89). But analytic philosophy’s disregard for historical issues is not just a bud of criticism (*pace* Monk, 2011). It is also mentioned as one of the *distinctive features* of analytic philosophy (Agostini, 1997, pp. 73–4; Engel, 1997, pp. 184–96). Indeed, some leading proponents of analytic philosophy regarded it as a characteristic of the kind of philosophy they deemed proper, irrespective of whether they were given to calling the latter analytic or ‘scientific’ (a term dear to the logical positivists and to Quine). Thus, Quine is widely credited with the quip: ‘There are two kinds of people interested in philosophy, those interested in philosophy and those interested in the history of philosophy’ (MacIntyre, 1984, pp. 39–40). And Williams reports: ‘in one prestigious American department a senior figure had a notice on his door that read JUST SAY NO TO THE HISTORY OF PHILOSOPHY’ (1996, p. 18). The culprit turns out to be Gilbert Harman (Sorell, 2005, p. 43). It could equally have been Fodor, who boasts about his ‘ignorance of the history of philosophy’ and his ability to write a ‘book about Hume without actually knowing anything about him’ (Fodor, 2003, p. 1). Furthermore, a superior attitude toward the past of our subject is widely reported to be part of the self-image of contemporary Anglo-American graduate students trying to project an air of ‘professionalism’, especially in technical areas such as metaphysics, philosophy of mind, and philosophy of language (see Wilshire, 2002, p. 4; Wheeler, 2010; Hacker, 2011).
On this issue, there is even convergence between the contemporary mainstream (whether naturalists or modal metaphysicians) and Wittgenstein. Wittgenstein confessed:

As little philosophy as I have read, I have certainly not read too little, rather too much. I see that whenever I read a philosophical book: it doesn't improve my thoughts at all, it makes them worse. (MS 135: 27.7.47; quoted Monk, 1990, p. 495)

According to Ryle, moreover, Wittgenstein ‘not only properly distinguished philosophical from exegetic problems but also, less properly, gave the impression, first, that he himself was proud not to have studied other philosophers – which he had done, though not much – and second, that he thought that people who did study them were academic and therefore unauthentic philosophers, which was often but not always true’ (Ryle, 1970, pp. 10–11).

At the same time, such ‘historiophobia’ is not a universal feature of analytic philosophy and hence cannot serve either as a defining feature or a ground for sweeping accusations (see Glock, 2008b). Many analytic philosophers have laid claim to the philosophical mantle of thinkers from the past. Leibniz provided Russell with the inspirational idea that ‘all sound philosophy begins with logical analysis’ (Russell, 1900, p. 8). Ayer described logical positivism as ‘the logical outcome of the empiricism of Berkeley and David Hume’ (Ayer, 1936, p. 41). And Reichenbach (1951) purported to lay bare the historical roots of both the analytic movement and the speculative philosophy it aspired to replace. Even analyticians who are officially hostile to the history of philosophy have occasionally succumbed to what, revealingly, has come to be known as ‘precursorism’. For instance, Quine dabbled in historical questions by discussing Russell’s ontological development, as well as the origins of the linguistic turn and of contextualism (Quine, 1981, chapters 7–8).

Indeed, roughly since the 1960s, there has been an upsurge in analytic work on the history of philosophy, prompting von Wright to speak of a ‘retrospective turn’ (Wright, 1993, p. 47; see also Wilson, 1991, p. 454; Critchley, 2001, p. 61). Analytic interest in the past has always included ancient Greek philosophy, to which analytic philosophers have felt a strong affinity, though their approach has been condemned as anachronistic (Annas, 2004). But it has by no means been confined to it, and now extends to all periods. Thus, Anthony Kenny has just completed a four volume history of the whole of western philosophy. It is just as lucid in style and analytic in its approach as Russell’s famous History of Western Philosophy, while surpassing the latter in terms of textual and historical scholarship, not to mention even-handedness of judgment (Kenny, 2004–7).

Getting closer to the present, and to the topic of this collection, analytic philosophers have recently started to take a keen interest in the history of
analytic philosophy itself. There were noteworthy forerunners, for instance, J.O. Urmson's *Philosophical Analysis: Its Development between the Wars* of 1956, as well as the pertinent parts in *The Development of Logic* by William and Martha Kneale of 1962 and *A Hundred Years of Philosophy* by John Passmore of 1966. But the immediate sources of this burgeoning academic field lie elsewhere. In the 1970s, Michael Dummett opened a debate about the historical origins of analytic philosophy with his claim that it is ‘post-Fregean philosophy’ and that it is based on the conviction that the philosophy of language is the foundation of philosophy in general. Dummett’s influential *Origins of Analytical Philosophy* recounts a ‘history of thought’ – of propositions and arguments standing in abstract relations of support or conflict – rather than a ‘history of thinkers’ (Dummett, 1993, chapter1). His claims and methods were subsequently scrutinized by more historically-minded commentators, many of them based in the U.S. Provoked both by the aforementioned historiophobia and by what they regarded as doxographic and anachronistic approaches to the past, these analytic philosophers began to deplore analytic philosophy’s lack of historical self-consciousness (see, e.g., Sluga, 1980, p. 2; Baker, 1988, p. ix; Hylton, 1990, p. vii).

The last twenty years have substantially weakened the force of such complaints. There has been an incessant stream of increasingly historical works on individual giants of analytic philosophy (prominent among them Griffin, 1991; Hylton, 1990; Weiner, 1990). What is more, there have been several historical surveys of analytic philosophy (Coffa, 1991; Skorupski, 1993; Hacker, 1996; Stroll, 2000; Baldwin, 2001; Soames, 2003), detailed treatises on more specific aspects of its development (e.g., Stadler, 1997; Hanna, 2001), and at least eight collections of essays on the history and historiography of analytic philosophy (Bell/Cooper, 1990; Monk/Palmer, 1996; Glock, 1997; Tait, 1997; Biletzki/Matar, 1998; Reck, 2002; Beaney, 2007, 2010; see also Sorell/Rogers, 2005). As a result of this flurry of activity, the history of analytic philosophy is now a recognized field of study and of specialization (see Beaney, 1998; Preston, 2007, pp. 28–30). From Dummett onward, moreover, the historical questions have been intimately linked to the question of what analytic philosophy is, and to passionate fights for the soul and the future of analytic philosophy.

If Hegel is right, and (historical) self-awareness is a sign of terminal decline, analytic philosophy must be moribund. And some commentators have indeed interpreted the historical or retrospective turn in this way (e.g., Preston, 2007, pp. 28–30). Now, death by historical self-consciousness might not be a bad way to go. Yet, what precisely is the supposed link between such self-consciousness and the decline of a philosophical movement? The historical turn could be a symptom of the crisis of analytic philosophy, or it could be a cause of that crisis, or it could be both.

Turning to the first possibility, analytic philosophers have, by and large, given up on the earlier promises of providing definitive solutions
or dissolutions of all philosophical problems, or of furnishing canonical methods that would guarantee philosophical progress. Worse still, the scholastic, factionalist, dogmatic and exclusionary tendencies of contemporary analytic philosophy provide grounds for suspecting that we are past the heroic age of analytic philosophy (see e.g., Glock, 2008a, pp. 242–55; Hacker, 2011). To borrow a distinction from the history of architecture, there is a real danger that analytic philosophy has exhausted its capacity for structural progress, and is capable of progressing only with respect to the embellishments. This might help to explain why an increasing number of aspiring analytic philosophers turn from substantive to historical issues. For, ironically, the latter may actually provide better prospects for a fresh start. In this respect, the retrospective turn may appear as a symptomatic manifestation of stagnation or decline as regards philosophical substance.

At the same time, this redirection of analytic endeavours may itself constitute progress of a different kind. Peter Hacker reports:

It is striking that a pessimist about the future of analytic philosophy, Georg-Henrik von Wright predicted that perhaps all that will result from analytic philosophy in the end will be a much greater adequacy in dealing with the great philosophies of the past. I hope that he was over-pessimistic. But he was surely right that the tools developed in the analytic tradition have lent themselves to fruitful application in the studying the history of philosophy. (Hacker, 2011, p. 7)

I share the view that an analytic approach to the history of our subject makes both for better philosophy and for better history. That idea underlies numerous current efforts, including the journal Logical Analysis and History of Philosophy. Furthermore, insofar as an insight into the history of philosophy is either indispensable or, at any rate, useful to the resolution of substantive issues, the historical turn holds at least some promise of renewed substantive progress. Note, however, that many of the eminent historians to emerge from the analytic tradition – including Kenny, von Wright, and Hacker – had intellectual inputs from outside of mainstream analytic philosophy, and that this may help to account for their superior scholarship and greater sense of history.

Turning to the second possibility, historical self-consciousness may be regarded as a contributing factor to the crisis of analytic philosophy in that it may undermine its sense of identity. From the perspective of a history of thought à la Dummett, the idea that all and only analytic philosophers share a certain doctrine, topic, method, or style may appear plausible. A different picture emerges, however, once we look at the small print (or not-so-small print) and abandon the anachronistic preoccupation with those features that strike us as important from our present perspective. We shall not just find that contemporary analytic philosophy is a very broad
church indeed, but that from the start there has always been substantive diversity even on metaphilosophically crucial issues of doctrine, methodology, and style. For instance, there is no consensus on the philosophical role of language among Frege, Moore, early and middle Russell, and the TLP (see Glock, 2008a, chapter 5.1).

At the same time, while knowledge of the history of analytic philosophy may have social disadvantages, e.g., for the esprit de corps of analytic philosophers, it cannot have intellectual disadvantages. The unexamined philosophical movement is not worth participating in. Thus, Beaney writes:

The emergence of history of analytic philosophy as a recognized field within the broader discipline of philosophy is a sign of maturer metaphilosophical views. (Beaney, 2011, p. 8)

It is also, Beaney would presumably be happy to add, a major contributing factor to such maturity. If a simple self-image of analytic philosophy is historically unsustainable, then one should look for a more sophisticated one. For instance, one may regard ‘analytic philosophy’ as a family-resemblance concept, or as a historical one singling out a genetic category. Or one can maintain that analytic philosophy is a tradition held together both by historical relations of influence and by overlapping similarities (see Glock, 2008a, chapter 8, pp. 2–3).

Still a distinct movement?

Yet, could it not turn out, on closer examination, that analytic philosophy is simply no longer a distinct philosophical movement? In my view, there remains a strong prima facie case for the idea that analytic philosophy constitutes a distinct philosophical phenomenon.

At a time when the analytic/continental contrast was emerging, R.M. Hare maintained that there are ‘two different ways’ in which philosophy is now studied, ways concerning which ‘one might be forgiven for thinking... are really two quite different subjects’ (Hare, 1960, p. 107). More recently, Dummett observed that ‘an absurd gulf has formerly opened up between “Anglo-American” and “Continental” philosophy’; indeed, ‘we have reached a point at which it’s as if we’re working in different subjects’ (Dummett, 1993, pp. xi, 193).

At the same time, over the past ten years, countless observers from otherwise diverse philosophical and geographic quarters have urged that the customary distinction between analytic and continental philosophy has become obsolete.

Thus, Peter Bieri has proposed the following gruelling experiment. For a whole month, read the Journal of Philosophy in the morning, and then Seneca, Montaigne, Nietzsche, Cesare Pavese, and Fernando Pessoa in the
afternoon (Bieri, 2007, p. 334). Slightly altering Bieri’s set-up, and making it even more sadistic, devote the afternoon sessions to Plotinus, Vico, Hamann, Schelling, and Hegel, or to Heidegger, Derrida, Irigaray, Deleuze, and Kristeva. Bieri’s thought-experiment is illuminating. Yet, it points in the very opposite direction of the conclusion he favours. According to Bieri, the distinction between analytic and continental philosophy is ‘simply a nuisance’ that cannot be tolerated (p. 343). By contrast, I think that three things emerge from the proposed juxtapositions: first, there is at least some overlap concerning the problems addressed in all these writings; second, at least some of these problems are philosophical by commonly accepted standards; third, what goes on in the pages of the Journal of Philosophy is a distinctive intellectual activity, one that differs from the activities (themselves diverse) that the other figures engage in.

Small wonder, then, that the labels ‘analytic’ and ‘continental philosophy’ continue to be widely used. This holds even when it is suggested that the distinction is not a hard-and-fast one. In reviews, for instance, it is commonplace to read not just that a book or author is typical of either the analytic or continental movement, but also that X is unusually sensitive or open-minded ‘for an analytic philosopher’ or that Y is uncharacteristically clear or cogent ‘for a continental thinker’. The analytic/continental distinction colours philosophical perception even among those who do not regard it as absolute; indeed, it is often invoked willy-nilly even by those who officially oppose it. More generally, there is no gainsaying the fact that the idea of a distinct analytic philosophy continues to shape the institutional practice of philosophy, whether it be through distinct journals, textbooks, societies, job advertisements, or institutes (see Preston, 2007, chapter 1).

The famous ‘pluralist revolt’ in the American Philosophical Association (APA) of 1978 pitted the dominant analytic mainstream against non-analytic philosophers of various persuasions. The revolt may have resulted in a modus vivendi between these factions within the APA, yet it has not overcome the intellectual and academic divisions between them. On a more personal note, following my move to Switzerland in 2006, I was painfully reminded of certain academic frontlines that I had hoped to have left behind after leaving Germany for Britain in 1986. In the eyes of the cultural establishment, many colleagues from the humanities, and many students, analytic philosophy continues to constitute a deplorable ‘Anglo-Saxon’ deviation from genuine philosophy, whether it be of the traditionalist or the continental type. In many quarters, alas, what counts is neither the quality of a candidate nor her field, for instance, but what style of philosophy she engages in.

On a more cheerful note, even at present, it remains common and perfectly helpful to explain to students that a particular department, course, or individual is analytic in orientation. The label ‘analytic philosophy’ does not have an everyday use. But it does have a standard use, not just in academic
circles but in the wider educated public. In fact, what Grice and Strawson (1956) pointed out about the terms ‘analytic’ and ‘synthetic’ holds equally of the term ‘analytic philosophy’. Although we may lack a clear and compelling explanation, we by-and-large agree in our application of these terms.

To be sure, even the most established and clearly circumscribed philosophical taxonomies are liable to misuse. But in this respect, ‘analytic philosophy’ is no worse off than more venerable labels. Though there are occasional misapplications, they are generally recognized. Consider the following, presumably rhetorical, question from a circular of Continuum International Publishing Group (21 October 2003):

Are you interested in the continental philosophy of Gilles Deleuze or Theodor Adorno, or philosophy of the analytic tradition such as Friedrich Nietzsche or Mary Warnock?

No prizes for spotting the mistake. By this token, it would obviously count against a definition of analytic philosophy if it implied that Heidegger and Lacan are analytic philosophers while Carnap and Austin are not. It would also count against a definition if it implied that Russell and Quine are analytic philosophers, while Frege and Hempel are not. Furthermore, we agree not just on what the clear cases are. We also tend to agree on what count as borderline cases for various reasons, e.g., Bolzano, Whitehead, the later Wittgenstein, Popper, Feyerabend, and neuro-philosophers. Finally, the agreement is not to a list but can be extended to an open class of new cases. For instance, perusal of CVs will put most professionals in a position to identify clear-cut analytic and continental philosophers from a list of job applicants.

A merely sociological phenomenon?

Contemporary analytic philosophy is undoubtedly a very diverse and pluralistic phenomenon. Indeed, in What is Analytic Philosophy? I argued at length that there is no adequate analytic definition of ‘analytic philosophy’ as standardly used. There are no topics, doctrines, methods, or even styles shared by all and only those commonly recognized as analytic philosophers (Glock, 2008a; see also Glock, 2004, pp. 425–36).

At least as regards doctrines, a similar view has been advanced by Aaron Preston (2004, 2007, 2011). But we disagree over one of Preston’s central theses. If I understand him correctly, he holds that analytic philosophy as a sociological movement owes its existence to an illusory idea among its proponents, namely that they all subscribed to the ‘linguistic thesis’ that the proper work of philosophy consists in the analysis of language (Preston, 2007, pp. 2–3, 80–2). Instead, I distinguish the rise of analytic philosophy through the figures of Frege, Russell, and Moore from its linguistic turn at the
hands of Wittgenstein and the logical positivists. And it is indisputable that while Moore and early Russell and promoted conceptual and logical analysis, they explicitly rejected the suggestion that they were analysing words or sentences. So, they definitely did not labour under the illusion Preston diagnoses, nor did Neurath, not to mention countless analytic metaphysicians and ethicists at least since the 1960s (see Glock, 2008a, pp. 128–34).

More to the current point, both Preston and I have tried to clarify the question of whether analytic philosophy (still) constitutes a distinct phenomenon by distinguishing different possible taxonomies that one might apply to it. Thus, I distinguish between a closely knit philosophical school and looser groupings such as movements or traditions. Analytic philosophy comprised schools similar to the ancient schools of philosophy, e.g., the Vienna Circle, the Canberra planers, groups that are or were officially united by shared articles of faith. But analytic philosophy is itself a much looser phenomenon, a philosophical movement. In fact, because of its longevity across several generations, analytic philosophy amounts to a tradition (Glock, 2008a, pp. 151–2, 220–1).

Preston accepts that this is a legitimate sense of ‘school’. At the same time, he points out that there is also another sense of ‘school’ used in philosophical discourse. A ‘school of thought’ is defined by ‘ideational structures (concepts, propositions, arguments, theories)’ rather than historical ties. And even in the case of a ‘socio-historically embodied school of thought’, personal transmission of ideas is less fundamental than a shared ‘way of thinking’ according to Preston (Preston, 2011, pp. 5–6). To me, it would seem that both shared ideas and historical ties must be necessary conditions for membership in such an embodied school. Otherwise, why distinguish the notion from that of a school of thought simpliciter? The main bone of contention, however, lies elsewhere. For Preston, ‘analytic philosophy’ is not a legitimate taxon unless it constitutes a socio-historically embodied school of thought. And he further insists that the latter requires more than sharing a set of methods, namely sharing a set of doctrines or ‘views’. In the absence of doctrinal consensus, analytic philosophy can only be a ‘social phenomenon’ rather than a genuinely philosophical one (p. 12; cp. Glock, 2008a, pp. 151–3).

This verdict presupposes that only doctrines or views but not methods can count as genuinely intellectual – logical, philosophical – rather than ‘merely social’ phenomena. This assumption is mistaken. From Socrates through Kant to the present, many philosophers have felt that it is precisely the problems and methods that mark out philosophy as a rational enterprise, rather than shared articles of faith. In the case of analytic philosophy, the emphasis on methods, rather than doctrines, is particularly pronounced. Many practitioners past and present have proclaimed that the analytic movement is superior to other ways of philosophizing precisely on account of the way it tackles philosophical problems rather than on account of specific philosophical views.
Russell promoted scientific method in philosophy rather than particular philosophical views. (How could he, given his notoriously frequent changes of mind?) And he extolled the method of analysis. Thus, he maintained apodictically that ‘all sound philosophy begins with logical analysis’, and that this realization represents ‘the same kind of advance as was introduced into physics by Galileo’ (Russell, 1900, p. 8; 1914, p. 14, see also 68–9). With hindsight, he wrote:

Ever since I abandoned the philosophy of Kant and Hegel, I have sought solutions of philosophical problems by means of analysis; and I remain firmly persuaded, in spite of some modern tendencies to the contrary, that only by analysing is progress possible (1959, p. 11).

According to Moore, the ‘difficulties and disagreements’ that have dogged philosophy are due mainly

to the attempt to answer questions without first discovering precisely what question it is which you desire to answer. ... [philosophers] are constantly endeavouring to prove that ‘Yes’ or ‘No’ will answer questions, to which neither answer is correct.... (1903, p. vi)

This influential – though currently underappreciated – tactic of questioning the question attempts to clarify, rather than answer, questions which lead to misguided philosophical alternatives. It accords a more fundamental role to a method – that of conceptual clarification – than to specific doctrinal answers to questions.

Wittgenstein went one step further. According to him, there simply are no legitimate philosophical doctrines or views. Philosophical problems are based on misunderstanding and hence call out for dissolution rather than answers. What counts is solely a method of analysis and clarification. Wittgenstein pronounced his ‘new method’ of philosophizing to constitute a ‘kink’ in the ‘development of human thought’ comparable to the Galilean revolution in science. And he insisted that what mattered about his work was not its specific results but its new way of philosophizing, a method or skill which would enable us to fend for ourselves (1922, 4.003, 4.1272, 6.53f.; 1953, p. 206 and sections 126–8; 1993, pp. 113–4; see Glock, 1996, pp. 258–64, 292–9).

The manifesto of the Vienna Circle is another founding document of analytic philosophy. And, to be sure, it was entitled ‘The Scientific Conception of the World’ (Wissenschaftliche Weltanschauung). But it states in no uncertain terms that ‘the scientific world conception is characterized not so much by theses of its own, but rather by a basic attitude, its points of view and the direction of research’. ‘It is the method of logical analysis that essentially distinguishes recent empiricism and positivism from the earlier
version’ (Carnap/Hahn Neurath, 1929, p. 305). The image projected by the Vienna Circle was that of a shared canon of rational methods through which doctrinal differences – differences of philosophical opinion – might be settled.

Herbert Feigl once summed up this methodological ethos by saying that the philosophical tradition he presented revolved around two humble questions: ‘What do you mean?’ and ‘How do you know?’ (see Feigl, 1981, p. 409; Aune, 2). Finally, many contemporaries who have given up on analytic philosophy’s early promises of providing lasting and definitive solutions or dissolutions of philosophical problems nonetheless cherish a more modest ideal: essential to analytic philosophy is the value of the process rather than the durability of the result (e.g., Føllesdal, 1997; Beckermann, 2004).

Of course, philosophical methods are standardly supposed to yield philosophical views – though not by Wittgenstein and his followers. And I would agree that philosophical methods presuppose certain views. But philosophical views also require methods for their generation. Indeed, views which are simply haphazard, rather than resulting from the application of some method or other, cannot constitute a doctrine. So, the dependency of views on methods is at least as pronounced as that of methods on views. Consequently this interdependence in no way favours defining a philosophical movement through certain views over defining it through certain methods.

The fact that a bona fide philosophical movement can revolve around a method or style does not suffice to rehabilitate the notion of analytic philosophy against Preston’s animadversions, however. For not even the method of analysis or the pursuit of clarity and argument can be used to define analytic philosophy, or so I have argued. In my view, the commonly acknowledged extension of ‘analytic philosophy’ is determined both by a family of overlapping ideational features (doctrinal, methodological, and stylistic) and by historical ties of influence. On account of the latter, analytic philosophy is indeed not a purely intellectual or philosophical category; it does have a socio-historical dimension. But, of course, it does not follow that the notion is illegitimate or spurious. Indeed, it does not even follow that it is useless for the purposes of philosophy. After all, the history of philosophy is interested not just in schools of thought like rationalism, but also in embodied traditions like the ancient schools, scholasticism, continental rationalism, British empiricism, or Neo-Kantianism. Needless to say, the precise identity and contours of such embodied schools is notoriously contested among scholars. That only goes to show, however, that the case of analytic philosophy does not differ in principle from those of other recognized philosophical schools.

One point does follow, however, and it may be the underlying source of Preston’s scepticism about both the existence and the legitimacy of analytic philosophy (e.g., 2007, pp. 9–17). The label cannot carry any direct
argumentative, evaluative, or normative weight. One cannot legitimately defend or refute, praise or condemn a method, view, work, or philosopher simply by ascertaining that it/he is or is not analytic. Admittedly, some proponents of analytic philosophy have used it as an honorific title, but such an employment of philosophical taxonomies in general, and of ‘analytic philosophy’ in particular, can be and has been resisted, not least by employing analytic tools (see Glock, 2008a, pp. 4–9, 205–12).

**Going Post-Analytic?**

Let us assume, if only for the sake of argument, that at present analytic philosophy is still a distinct phenomenon and that it is fruitful to distinguish analytic from non-analytic philosophy. Even in that case, two questions arise: Will analytic philosophy retain a separate identity in the foreseeable future? And should analytic philosophy retain such an identity?

As regards the first question, one thing is indisputable. With the passing of time, the historical tradition of analytic philosophy will continue to become ever more fractured. The genetic tree has sprouted so many branches that the intellectual distance between some of them may be so great as to render their genetic connection less and less relevant for determining what kind of philosophy one is dealing with (Marconi, 2011; see also Hacker, 2011). On the other hand, from a sufficient temporal distance, those increasing differences may also come to seem less relevant. Contemporary philosophers tend to be much more alive to the differences separating the various representatives of continental rationalism on the one hand and British empiricism on the other, let us say. Yet, this has not diminished their willingness to group them together.

Of necessity, the future development of the analytic tradition remains a matter of speculation. What is more, that future will depend not just on philosophical or intellectual developments, but also on wider cultural, political, sociological factors.

What about the normative question? In the current debate, that normative question is not sharply separated from the question of how best to describe the status quo. In one corner, we find the commentators mentioned at the beginning of the chapter. They include a pioneer of analytic philosophy like von Wright, who worries that it is dying on its feet because it has lost its distinctive identity.

Against this are aligned those on both sides who regard such bridge-building as pointless or perhaps even invidious. In this corner, we find many continental philosophers, notably Rorty, who feel that analytic/continental divide has become a possibly permanent, but equally harmless, fixture of Western philosophy (Rorty, 1982, chapter 12; similarly Rockmore, 2004, p. 474). We also find, perhaps surprisingly, Bernard Williams. He maintains that analytic philosophy has overcome some of its ‘earlier limitations’ as
regards political philosophy, yet he denies that ‘reform has changed or will change it out of all recognition’. What is more, while analytic philosophy does not have ‘definite bounds’, even on a generous view of its scope it does not coincide with philosophy period (Williams, 2006, p. 167).

Unlike von Wright, most of those who question the continuing viability of a separate category of analytic philosophy and of the analytic/continental divide feel that this development is a positive one, philosophically speaking. Many of them invoke, and some of them have contributed to, attempts to build bridges between the two. There are important thinkers who have genuinely tried to synthesize the two, or at least to mediate between them, such as Føllesdal, Tugendhat, Dreyfus, Charles Taylor, Cavell, and Mulhall.

Does this show that the barriers are finally coming down? I think not. While these various syntheses are distinguished and interesting, it is fair to say that they have not set the agenda on either the analytic or the continental side. They have not produced any seismic shift toward the proverbial middle ground. It is not even clear what this middle ground could amount to. One possibility is post-analytic philosophy, i.e., continental philosophy presented by Anglophone commentators who refer to analytic thinkers like Wittgenstein, Quine and Davidson (e.g., Taylor, Cavell and Mulhall). Another possibility is ‘post-continental’ philosophy, the philosophy of apostates from thinkers like Hegel, Husserl, or Heidegger, who incorporate continental themes or ideas into a purely analytic mode of philosophizing (e.g., Føllesdal and Tugendhat). For better or worse, neither option looks like carrying the day.\(^8\) Even if, in combination, they were to narrow some doctrinal differences, they seem unsuitable for overcoming the abiding methodological, stylistic, and institutional divides.

Such a feat might be achieved by communication between more emblematic representatives of both sides. But consider the famous direct debates between analytic and continental philosophers. A complete list would have to include the following:

- Ryle’s review of Heidegger’s *Sein und Zeit*;
- Carnap’s attack on Heidegger’s ‘The Nothing noths’;
- the 1958 Royaumont encounter between British and French philosophers;
- Bar-Hillel’s attack on Habermas’ appropriation of speech act theory;
- the spat between Searle and Derrida over Austin’s speech act theory and the alleged ubiquity of writing;
- the protests against Derrida’s honorary degree in Cambridge;
- the aftermath of the Sokal hoax.

Two points are noteworthy. Considering the high profile of the analytic/continental distinction, these confrontations have been far and few between. Furthermore, I see little ground for hope that even now these clashes could
be resolved in a more amicable way, or that we are, at any rate, closer to conducting them in a more controlled and fruitful manner. For instance, in spite of their much vaunted ‘analytic training’, recent Anglophone defenders of Heidegger remain unwilling to acknowledge some important points: Carnap’s paper does not simply rely on verificationism, it considers various ways of making sense of Heidegger’s dictum, and it rejects them for noteworthy reasons. They also seem to think that a sentence occurring in a philosophical treatise cannot be condemned as meaningless simply because one can assign a meaning to this combination of words (indeed, any meaning one pleases), even if on that understanding the sentence can in no way shoulder the argumentative weight it needs to in its original context.

Or take the most extended rally between analytic and continental philosophers. One might argue over whether Searle’s (1977) response to Derrida’s (1972) critique of Austin is harsh yet fair (as I feel), or unnecessarily abrasive. But there is no arguing over the fact that Derrida’s reaction (1988) amounts to a complete refusal to engage with the issues at a rational level. Instead, it consists of obscure evasions, wails, and linguistic puns. Derrida suggests that Searle’s acknowledgement of help with his response indicates lack of intellectual responsibility and integrity. And he stoops to attributing the response to a fictitious ‘SARL’ (Société à Responsabilité Limitée), a Limited Inc. or Society with Diminished Responsibility.

In short, exchanges between the two camps have made matters worse rather than better. If past experience is anything to go by, serious engagement between analytic and continental philosophy will not lead to conciliation, but to more pronounced estrangement. Perhaps it is the terms of engagement that are to blame. Dummett has recently recommended that the analytic/continental divide should be overcome by founding a journal in which an analytic philosopher writes an chapter followed by a ‘continental’ response, or vice versa. I am far from confident that this procedure would overcome the failure of communication Dummett rightly deplores. More probably, the outcome would be as described by Marconi in a discussion following Dummett’s paper. Responding to a continental chapter, the analytic commentator would engage in a flurry of ‘What do you mean by this?’, ‘What is the justification for that?’, and ‘How are we to understand the next thing?’ The continental respondent to an analytic piece, by contrast, would ignore the general gist, pick out some tiny detail, and engage in comments about etymological or historical aspects surrounding that detail.

If the analytic/continental contrast will become obsolete, it is not because we have moved on to a new and thriving synthesis. But perhaps it will simply been superseded by other divides. Thus, it has been suggested that the analytic philosophy/continental philosophy distinction is no longer relevant, and that the real fault-line within current philosophy runs between naturalists and non-naturalists. It is striking, furthermore, that this suggestion has found favour across the divide.
Commenting on Kantian Oxford philosophers in the wake of Strawson an analytic naturalist like Papineau writes:

...a new and potentially more fruitful division is emerging within English-speaking philosophy. In place of the old analytic-Continental split we now have the opposition between the naturalists and the neo-Kantians. The naturalists look to science to provide the starting point for philosophy. The neo-Kantians start with consciousness instead. But at least the two sides can understand what the other is up to. (Papineau, 2003, p. 12)

Similarly, a post-analytic non-naturalist like Glendinning writes:

Perhaps the dominant kind of analytic philosophy today, at least in America, although it’s growing here too, is philosophical naturalism. This is the tradition opposed to the one I see myself located in, the post-Kantian tradition which doesn’t see that kind of continuity between philosophy and science. For me, and I think for many others, this is where the most fundamental issues and disputes lie today. And this is why concern with the demarcation of analytic philosophy from continental philosophy is becoming less and less significant and is being abandoned by more and more people within the profession (Glendinning, 2002, pp. 214–5).

What emerges has points of contact with Preston’s idea that unless analytic philosophy were defined by certain doctrines, it would not count as a proper philosophical movement. The proposal is this: because analytic philosophy can no longer be defined by reference to any doctrines, the analytic/continental divide should be replaced by a doctrinal distinction that marks the real fault-line.

While appreciating the reasons behind this proposal, I do not regard them as overwhelming. One of my qualms emerges by looking at the end of Papineau’s quote. He places weight on the fact that the opposing sides in a philosophical dispute nonetheless understand each other. As regards naturalists and non-naturalists within analytic philosophy he worries: ‘Whether they will stay in touch remains to be seen. Philosophical engagement depends on a shared context of basic assumptions, or at least a willingness to debate points of underlying conflict. If John Campbell’s Reference and Consciousness is anything to go by, prospects for continuing dialogue are not rosy’. Why? Because ‘the basic ideals that structure Campbell’s overall argument will seem alien to naturalistic readers, and he displays a worrying disinclination to explain them to this wider audience’. Leaving aside this particular case, I can empathize with this type of frustration, since much contemporary analytic philosophy is not as clear as it could
and should be, and fails in its duty of accounting for its assumptions. Parting company with Papineau, however, I feel that unclarity also afflicts naturalists, though perhaps to a lesser degree, and that the tendency to take their assumptions simply for granted actually afflicts them to a greater degree.

More importantly, Papineau is right to value the importance of mutual communication. Yet, surely such communication is considerably easier between naturalists and non-naturalists arguing in an analytic vein than between analytic philosophers of any coulour and most of their continental colleagues. If Papineau has genuine difficulties understanding Campbell, he should be at a complete loss reading continental thinkers like Lacan, Deleuze, or Guattari.

Philosophy is not about sharing doctrines, but about having a rational and civilized debate even about one’s own most cherished assumptions. Such a debate remains easier among analytic philosophers than between analytic and continental philosophers.

Moving on, finally, to prescriptive rather than descriptive metaphilosophy. Should we deliberately try to overcome the remaining barriers dividing analytic philosophy from continental and traditionalist philosophy, or from pragmatism, in so far as it constitutes a movement at the same level of generality?

In his capacity as the new editor of Mind, Baldwin (2006) has urged that one should not expend energy on ‘fortifying and patrolling’ the border between analytic and continental philosophy, yet remain true to the virtues of ‘open-minded clarity and rigour’. One should remain open to interesting ideas from any quarter, while insisting that they be presented in a manner that makes them amenable to fruitful debate. This advice is certainly well-taken, though it ought to be uncontroversial. But one might be tempted to go a step further. Could it not transpire that there is a premium on philosophy constituting a unified endeavour, as Western philosophy did until at least the beginning of the twentieth century (see Quinton, 1995, p. 161)? If philosophy works best as a cohesive discipline or at least a single area of discourse, bar of factions and communicative barriers, then heads should be banged together, irrespective of whether analytic philosophy or one of its rivals has a monopoly on philosophical wisdom.

At the same time, there is no overriding intellectual imperative for analytic philosophy to alter course solely to achieve rapprochement with other philosophical currents, assimilation to other intellectual styles, or recognition in other academic disciplines. While there may be a premium on reconstituting philosophy as a unified sphere of discourse, this must not go at the expense of rigour, clarity, scholarship, and intellectual honesty. Accordingly, neither division nor synthesis should be sought for its own sake, but simply
philosophical quality. What the analytic scene needs is not a deliberate switch to continental, traditionalist, or pragmatist modes of thought, but analytic philosophy in a different vein: engaging and engaged instead of scholastic and isolationist, collegial, undogmatic, and open-minded instead of factionalist and exclusionary.

In one respect, at least, there may be grounds for optimism. Many commentators, myself included, have complained that analytic philosophy is isolated and fails to engage with a wider public (Glock, 2008a, pp. 247–8). As Hales (2008) has pointed out, however, at least in the U.S. there have in fact been significant attempts to remedy this weakness, some of them successful. Even if sustained, this trend will not by itself rekindle the flame of analytic philosophy as a locus of original philosophical innovation. But analytic philosophy could do worse than to take seriously its vocation as critical thinking writ large: a means of improving debate in other areas, but one which, from case to case, engages with the details of these debates rather than legislating from above on the basis of preconceived generalities. Finally, analytic philosophy could also do worse – and has actually done worse – than to engage in the kind of critical historical self-reflection that has been promoted by its historical turn.

Notes

1. An umbrella organization, the European Society for Analytic Philosophy, or ESAP, was founded in 1991. Its website http://www.dif.unige.it/esap features links to Central European, Croatian, French, German, Italian, Portuguese, and Spanish societies of analytic philosophy. ESAP also regularly organizes major conferences.
2. I introduce the distinction between traditional, traditionalist and continental philosophy in Glock, 2008a, pp. 85–9; see also Marconi, 2011.
3. In some respects, the history of logic was a trailblazer for the history of analytic philosophy.
4. The metaphilosophical question of what analytic philosophy is or should be has also been revived independently of historical and historiographical issues, notably in a three-corner clash among Quinean naturalism, Kripkean metaphysics, and recently revived conceptual analysis. See Williamson, 2007 and Glock, 2010.
5. See Glock, 2008b, pp. 884–93. I there favour the more moderate of these two options. For an argument to the effect that history is, after all, indispensable to philosophy, see Alvarez, 2011.
6. For the difference between everyday and standard use, see Ryle, 1953, pp. 301–4.
7. To this list Marconi, 2011, adds Charles Morris, C.I. Lewis, Stanley Cavell, and Axel Honneth. I am happy with all these additions except for the last one.
8. It is perhaps symptomatic of this development that the ‘Centre for Post-Analytic Philosophy’ at Southampton founded in 1997 was renamed the ‘Centre for Philosophy and Value’ in 2005. See Preston, 2001, p. 166n.
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