

## What is the Good of Philosophical History?<sup>1</sup>

One can find at many universities 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 paper.

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.<sup>2</sup> Such questions are my main target in the body of this paper. I

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<sup>2</sup> In addition to my critical notice, Soames’ books have been reviewed by many others. 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); (Beaney 2006); (Livingston 2006); (Hacker2006); (Wilson 2006); and

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 favored by Soames. Inspired by Bernard Williams's distinction between 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 between antiquarianism, presentism, history of

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(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 papers on vol. 1 – (Stoljar 2006), (Burgess 2006), (Proops 2006), and (Sainsbury 2006) – three papers 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.

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.<sup>3</sup> 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 2005-06, 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

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<sup>3</sup> 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, Beaney, 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.

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, xi).

In response to criticisms from historians of analytic philosophy that his books get the history wrong, Soames (2006d, 645; 2006b, 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, 655). Soames (2006d, 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, 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 2005-06, 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, 654).

Soames (2006d, 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, 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, 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, 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.”<sup>4</sup> Yet even the most historicist of historians would agree with Fischer that antiquarianism is a “fallacy.” G.R. Elton (1987, 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, 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, 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, 152). Even the most historicist of historians seek *understanding* and historical *explanation*. As John Tosh explains the “mainstream academic view,” “[t]he 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, 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

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<sup>4</sup> 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?”, 45-47.

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 don't 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, 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, thumbnail summaries of books cited, and so on. 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, 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, 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.<sup>5</sup>

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<sup>5</sup> McEwen (2006, 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 (2002) wisely warns, “The disappearance of manifest

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, 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, 259), philosophical history is a *way* of philosophizing, which "can help

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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."

us to deploy ideas from the past in order to understand our own.”<sup>6</sup> 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, 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, 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, 655).

Soames’s historical method fits Fischer’s (1970, 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

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<sup>6</sup> Tadeusz Szubka (2007, 289) draws attention to Williams’s distinction as well.

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 emphasise 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, v).<sup>7</sup> Fischer (1970, 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.<sup>8</sup> Soames (2007b 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, 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

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<sup>7</sup> Cited in (Fischer 1970, 139). Soames (2006c, 12) quotes this as *Fischer’s* description of “Whig history,” missing the reference to Butterfield.

<sup>8</sup> E.g. (Weatherson 2007, 432); (Beaney 2006, 255, 267).

2007b, 467):<sup>9</sup>

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 is 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 which he draws to elucidate his "philosophically and pedagogically motivated history" gives a clue (Soames 2006b, 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 papers, 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

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<sup>9</sup> 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?"

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, 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, 8). The term "Whig history," used in Butterfield's sense, first acquired a vogue in the history of *science*, in the 1960s.<sup>10</sup> While Jardine (2003, 126)

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<sup>10</sup> Political historians already had a more precise use for this term to refer to history

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.<sup>11</sup>

In the Introduction to *Structure*, Kuhn (1962, 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, 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

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written *by Whigs*, and recognized with Fischer that historians of various political views can be guilty of presentism. See (Jardine 2003, 125-6).

<sup>11</sup> I have been helped here by (Moro-Abadía 2009).

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, 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, 110):

Insofar as it is 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, 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 paper, “In Defense of Presentism,” for example, David L. Hull (1979, 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.”<sup>12</sup> 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, 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

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<sup>12</sup> (Moro-Abadía 2009, fn 1, 72) provides a good overview of the resulting debate.

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 towards 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, 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 historiographical 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, 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.”<sup>13</sup> If one peruses the table of contents of the recent multi-volume series *Handbook of the History of Logic*, one can see that the Kneale’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*

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<sup>13</sup> Cited by (Mayr 1990, 304).

*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 contributions 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 Kneale'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. But, 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, 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 erroneous 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, 305). Yet Mayr (1990, 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, 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].'"<sup>14</sup>

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

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<sup>14</sup> Mayr misquotes Butterfield in the last quotation, writing "the glorification of the past."

dangers.<sup>15</sup> 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, 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 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, for example, Russell's (1959, 161) well-known remarks from the last chapter of *The Problems of Philosophy*:<sup>16</sup>

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,

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<sup>15</sup> 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.

<sup>16</sup> Similar remarks would apply to Wittgenstein's (1981, 4.112) view that "the object of philosophy is the logical clarification of thought."

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.<sup>17</sup> 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, 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 don’t 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,

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<sup>17</sup> I avoid the term “progress” due to the heavy intellectual baggage it carries.

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, 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, 259). Borrowing a phrase from Nietzsche, Williams (2006, 263) speaks of philosophical history as "untimely:"<sup>18</sup>

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

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<sup>18</sup> 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."

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, 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, xi). However, as Tadseuz Szubka (2007, 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, 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, xi). Bruce Kuklick (2006, 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.”<sup>19</sup>

The resulting attitude towards philosophers of the past is illustrated by Soames’s treatment of P. F. Strawson. Soames critiques at length an early paper 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, 124-125):

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, 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 paper on truth.

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<sup>19</sup> 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, 432), and (Livingston 2006, 292), among others. (The phrase “royal road to me,” due to Ivor Grattan-Guinness, is discussed below.)

Note what Soames is saying here: he *didn't* 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." (2006a, 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 claiming that notable shortcomings such as missing Kaplan's distinction between content and character "explain why [Strawson's] work did not reach the first rank." (Soames 2006a, 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.<sup>20</sup> Early on in his discussion of Bertrand Russell, Soames (2003a, 101-6) introduces "Russell's Formal Language" and

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<sup>20</sup> I draw in part on my review (Kremer 2005) of Soames's books here.

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* (1910-1913). There are a number of ways in which Soames’s presentation of Russell’s logic oversimplifies and misrepresents Russell’s work.<sup>21</sup> But the historical error which 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, 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 (1906-07, 47-9) in 1906; by 1910 he had become convinced that it was unanswerable. (Whitehead and Russell 1927, 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,

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<sup>21</sup> These are detailed in my review (Kremer 2005), among other places.

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 multigrade 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, 191). The reader may wonder, however, why Russell didn’t 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, 317-8) 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.” But, 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 must be grounded in pre-philosophical thought," so that common-sense trumps philosophical skepticism. (Soames 2003a, 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, 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 that 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, 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 70s. 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.<sup>22</sup> 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

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<sup>22</sup> See for example (Broman, 1990).

to embrace the history of ideas and discard philosophical history.

The issue posed here arises pointedly in Bruce Kuklick's review essay, "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, 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 order[ing] the books chronologically, and then summariz[ing] 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."<sup>23</sup> According to Kuklick (2006, 553), Reisch,

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<sup>23</sup> 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.

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 papers 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, 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 postpositivist 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, 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. But, 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 essays containing some of the finest exemplars of the genre of philosophical history that I know. In the Preface, Diamond (1995, vii) tells us that the essays 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 essays 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 essays “about ethics ... done in a way which reflects what I have learned from him [Wittgenstein].” Yet in the first paragraph of the introductory essay, “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, 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, behaviorism, mental causation, functionalism, cognitive science, consciousness, zombie arguments, qualia, and so on? For the most part, she doesn't. 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, 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, 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 mythological 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 essays 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 essay on “Truth before Tarski,” Diamond (2002, 253) 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.”<sup>24</sup> Whereas McDowell was

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<sup>24</sup> Quoting (McDowell 1998, 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 characteristic of many of these thinkers that when they are writing in a more historical 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

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, 253), in contrast, writes of “the alien age of early analytic philosophy.” Here too, Diamond’s aim is Williams’s (2006, 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 essay 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, 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, 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.

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whipping.”

Diamond's essay is complex, rich, challenging and rewarding, and I won't 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, 153) – and derives from this intuition the constraint that any acceptable definition of truth must yield as consequences all instances of the schema:

*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, 155-6). Tarski (1983, 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, 187-8):

CONVENTION **T**. *A formally correct definition of the symbol ‘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 ‘ $x \in Tr$  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  $x \in Tr$  then  $x \in S$ ’ (in other words ‘ $Tr \subseteq S$ ’).<sup>25</sup>*

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.<sup>26</sup>

According to Diamond (2002, 257, 259), the early analytic philosophers begin from an “inchoate understanding of truth” as involving some sort of agreement with

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<sup>25</sup> The second clause of Convention **T** requires that only sentences are true.

<sup>26</sup> 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.

reality, a dimension in which our thinking and talking is “responsible to the world.”<sup>27</sup>

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, 257).

Tarski (1983, 168-9, 172) defines truth for the sentences of a formalized language, which he defines these 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, 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, 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, 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

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<sup>27</sup> She takes the phrase “responsible to the world” from Sluga.

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, 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, 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, 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, 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, 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, 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.

Michael Kremer  
Dept of Philosophy  
University of Chicago  
1115 E. 58<sup>th</sup> St.  
Chicago, IL 60637  
kremer@uchicago.edu

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