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History of Late Analytic Philosophy

edited by

Guido Bonino and Paolo Tripodi

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Introduction

Guido Bonino, Paolo Tripodi

Over the last thirty years historical attention has been directed toward analytic philosophy: some analytic philosophers have begun reflecting on the philosophical tradition they belong to, while many other scholars have been working on what has now become a well-established discipline known as "history of analytic philosophy" (for a comprehensive bibliography see Beaney 2013). Yet this historiographical perspective mainly focuses on the origins of analytic philosophy or on the central decades of the 20th century. These two periods can be labelled respectively as early analytic philosophy (Frege, Russell, Moore, the early Wittgenstein, etc.) and as middle analytic philosophy (Carnap, Ryle, the later Wittgenstein, Quine, etc.) The use of the former label is firmly established, whereas the latter is less common, yet fairly natural. By contrast, a proper historical investigation of the most recent stages of analytic philosophy is greatly needed. Some contributions towards a better understanding of this issue are available. Among them: Baldwin (2001), Priest (2003), Soames (2003: vol. II, 461-476), Williamson (2007, chapter i and "Afterword"), Beaney (2013), Williamson (2014), Tripodi (2015, chapter iv). But they are still few and far between. This special issue of *Philosophical Inquiries* is intended to be a further stimulus for such an investigation.

The issue includes a series of interviews with contemporary philosophers, based on a fixed set of questions:

> (1) What are the main philosophical and metaphilosophical similarities and differences between early analytic philosophy and late analytic philosophy?
>
> (2) Is it possible to identify a mainstream in late analytic philosophy? If so, what are its main (cultural, ideological, philosophical, methodological, metaphilosophical) features?
>
> (3) What are the main critical and controversial aspects of late analytic philosophy?

It seems to us that these interviews can be instructive not only – as is obvious – by virtue of the content expressed in each of them, and the pondered

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views and reflections of each of the interviewees, but also when considered together as a sort of sociological survey, revealing the main convergences and divergences among "experts".

The central part of the issue is composed of five articles, which investigate the topic under discussion from very different points of view. "Past Present", a customary section of *Philosophical Inquiries*, comprises the first English translation of the "Discussion générale" of the fourth Colloque philosophique de Royaumont (1958), in which some well-known analytic philosophers met with representatives of various "continental" traditions; the translation is accompanied by a revealing introduction by Mathieu Marion. The issue ends with the reviews of two recently published collections of essays that are especially relevant for the history of analytic philosophy.

Following a suggestion made by Weatherson (2014: 517), we refer to the most recent period of analytic philosophy as *late* analytic philosophy; more precisely, we propose to apply this formula to the analytic philosophy developed approximately over the last forty years, which would seem to be a long enough period of time to deserve a separate investigation. It might be argued (and it has been argued in some of the interviews) that the term "late" sounds tendentious, in that it suggests that the phenomenon at issue is in its last stages. This is certainly true when the phenomenon is irremediably past, as in "late Renaissance". While aware of these possible undertones, we do not wish to suggest that analytic philosophy is finished or is finishing. The term "late" must therefore be understood in a purely chronological way, as a stage of development that simply succeeds the early and middle stages, and that happens to be the last only in relation to the present time, which is of course our viewpoint.

It should be made explicit, however, that the present issue is based on the working assumption that late analytic philosophy is a quite distinct phenomenon. Of course, this assumption mainly rests on impressions, rather than on a preliminary study, and could be questioned itself. For example, as Timothy Williamson suggested in his interview, perhaps from a more distant perspective in the future we would say that the period 1970-2010 belongs to the last phase of the history of *early* analytic philosophy. As Eric Schliesser wrote some months ago on his blog commenting on our call for papers,[1] perhaps we should wait "until a future philosopher or philosophical movement/network gives a philosophical reason to rewrite our times in light of their understanding of the direction of philosophical telos". When reviewing the interviews and the articles published in this issue, however, it is possible to select a list of recurrent views,

[1] http://digressionsnimpressions.typepad.com/digressionsimpressions/2016/05/on-late-analytic-philosophy-or-the-age-of-david-lewis.html

on which several (though by no means all) authors seem to agree, thus providing an initial, provisional picture of the history of late analytic philosophy.

One aspect of this picture is that over the last forty years analytic philosophy has been more historically self-conscious than it used to be in previous decades. Not only is the rise of a specific sub-discipline called "history of early analytic philosophy" significant in itself, but historical awareness makes it easier to realize that – as held by some interviewees – late analytic philosophy can be regarded as more similar, under several respects, to analytic philosophy in the early period than to middle analytic philosophy. The main reason for this is that early and late analytic philosophy came respectively before and after the so-called Linguistic Turn, which took place in the middle period (Wittgenstein, the Vienna Circle, Oxford ordinary language philosophy). The flourishing of metaphysics – and, more generally, the tendency to answer genuine substantive questions, rather than to dissolve philosophical problems by means of linguistic and conceptual analysis – seems to be a distinctive feature of both early and late, as opposed to middle, analytic philosophy. Substantive philosophical theory in the later period, however, is different from that of the early period, not only because of the refinement and expansion of the analytic *methodological* toolbox, but also because of new *contents* of philosophical theories. The philosophical treatment of necessity and possibility is an instance of such innovation, both from a methodological and a substantive point of view. As suggested by Nicholas Rescher in his interview, another example is the growing importance of the study of normativity.

If the picture of late analytic philosophy that has just been outlined – the predominance of substantive philosophy, the centrality of metaphysics, the importance of modalities – is even approximately correct, then it is all too natural for all the articles and most of the interviews in the present issue to touch upon the figure of David K. Lewis. Lewis's contribution to late analytic philosophy is worth investigating under several respects: not only is Lewis's work central to many debates (such as the debate over realism), but it also seems to establish several parameters of philosophical research, both by formulating an explicit methodology and by providing an implicit model. The recognition of Lewis's centrality is confirmed by the undertaking of projects such as "The Age of Metaphysical Revolution. David Lewis and His Place in the History of Philosophy", directed by Prof. Helen Beebee and Prof. Fraser MacBride at the University of Manchester.[2] It seems to us that a study of the relationship between Lewis and Quine would be particularly relevant for understanding late

[2] https://www.socialsciences.manchester.ac.uk/philosophy/research/projects/age-of-metaphysical-revolution/

analytic philosophy. The reason for this is not only that Lewis studied under Quine, and much of his work can be seen as a development and a radicalization of his teacher's work (Divers 2018; Janssen-Lauret 2017); but also that their names are often associated with two of the main meta-philosophical options available in analytic philosophy in the last decades: airmchair metaphysics, based on (a difficult equilibrium between) intuitions and theory-building, on the one hand, and methodological naturalism, based on the elimination of any conceptual/factual divide and on the continuity between philosophy and science, on the other hand.

Although this Lewis-centered picture of late analytic philosophy seems to be widely shared (at least in our sample, that is, among the contributors to this issue), and even though only few of the interviewees explicitly maintained that there is no mainstream in late analytic philosophy, most of them were cautious with respect to the very notion of a mainstream. The main motivation for this attitude is the extreme fragmentation of late analytic philosophy, which in turn likely depends on sociological factors such as professionalization and specialization. Philosophy of language seems now to lack the privileged role it used to have in the middle period, and no other sub-discipline took a comparable position, not even metaphysics: for example, as Brian Weatherson points out in his interview, in the early period philosophy of science was a large part of philosophy, and some took it to be central to all philosophy, whereas today it has mostly turned into a cluster of more particular and less central philosophies of the different sciences.

Having declared what our objectives were in designing this special issue, we are now left to consider to what extent such objectives have been met. It seems that we face a sort of mixed picture. On the one hand, the contributed articles and the interviews provide an array of interesting analyses, reflections, suggestions, etc. As has just been shown, it is also possible to discern an inchoate consensus concerning the relative importance of some questions or themes. On the other hand, there are still significant questions that have not been addressed at all, or for which no answer is available, and there are entire areas of the history of late analytic philosophy that still await a thorough scholarly investigation. The legitimacy itself – and the usefulness – of the label "*late* analytic philosophy" is still in need of full justification. It seems to us that the present issue has two main inadequacies.

The first inadequacy is a methodological one. We would have liked to have more "external" history, both because it is interesting *per se*, and because the sheer quantity of philosophical production in the recent phase of analytic philosophy, together with its progressive specialization and fragmentation, makes it peculiarly difficult to pursue the traditional kind of (internal) history of phi-

losophy, and especially to assess the role of particular philosophical episodes within the overall picture. We are convinced that the contributions of the sociology of knowledge, the institutional history of science and of education, the social epistemology, etc. would be of considerable help in tackling the complicated tasks and questions involved in investigating the history of late analytic philosophy. We are also convinced that a quantitative approach to the history of philosophy could be profitably combined with the attention to external factors shaping late analytic philosophy. This is in fact the approach taken by Buonomo and Petrovich in their paper, which is mainly based on scientometrics, though other "quantitative" methods are probably equally feasible, such as *distant reading*, originally fashioned by Franco Moretti for the study of literature.

The second inadequacy is thematic. In this issue there is much less moral and political philosophy than we would have wished for. This is likely due, at least in part, to mere chance. Yet, maybe there are also more interesting reasons: perhaps an appropriate periodization for moral and political philosophy should be somewhat different from that which is suggested here, or perhaps – a more radical reason – the category itself of "analytic philosophy" applies to moral and political philosophy in a different way than, say, to metaphysics, epistemology, philosophy of language, etc. (see Jonathan Wolff's interview).

Talk of inadequacies should not be taken to mean we are in any way disappointed with the result of our efforts and of those of the contributors. Rather, it is a way to point to prospective lines of research, to further questions that need to be asked, to problems that still wait for a solution. The main purpose of this issue is to open a debate and certainly not to have the last word. In addition to the questions concerning the history of late analytic *practical* philosophy – Does it deserve a separate investigation? What are its peculiar features? Does it share historical framework and periodization with the history of late analytic metaphysics, epistemology, philosophy of language and mind? – there are several further questions that the present issue has brought to our attention and that are still waiting to be taken in serious consideration by the scholars. To conclude this brief introduction, we would like to point out a few of them, which we consider to be particularly interesting.

The first question for future research concerns conceptual analysis. Several interviewees seem to agree that its role and status in analytic philosophy have changed since the earlier period (for a comprehensive account of conceptual analysis in the early and middle period, see the works of Michael Beaney, for example Beaney 2007 and 2017). However, it is still controversial and not entirely clear how this difference should be described. Perhaps the point is that late analytic philosophers do *less* conceptual analysis than they used to when taking the Linguistic Turn; therefore, a better understanding

of the main methodological aspects of their work requires different categories, such as the notion of inference to the best explanation. Perhaps, on the contrary, late analytic philosophers practice traditional, armchair versions of conceptual analysis as much as they always have, but they interpret the results differently, that is, as genuine and substantive answers rather than as ways to dissolve philosophical problems.

A second question that has yet to be addressed concerns formalization and mathematization: How do they take place in late analytic philosophy? Have any similarities and differences with analogous processes occurred in disciplines such as economics and linguistics during the 20th century? Formalization and mathematization are often associated with professionalization, a process that in turn is strictly related to fragmentation and specialization. Therefore, a further question arises: are fragmentation and specialization a real trademark of late analytic philosophy? The maps provided by Buonomo and Petrovich at the end of their paper suggest that things are not as simple as one might initially believe: the diachronic application of clustering techniques seems to indicate that sub-disciplinary fragmentation strongly characterizes the last decade but is not equally significant in the eighties and the nineties. We consider this result to be very interesting. It certainly provides a stimulus for further investigation, better if sociologically well-informed, which could also shed some light on the relative weight, in the history of late analytic philosophy, of philosophical sub-disciplines such as metaphysics, logic, epistemology, philosophy of language, philosophy of mind, moral and political philosophy, so as to understand whether there is any sort of hierarchy among them. Ultimately, however, the clusters provided by Buonomo and Petrovich can even put in doubt the usefulness of the very chronological notion of late analytic philosophy, on the basis of the claim that such a difference and such a divide (the presence versus the absence of great fragmentation and specialization) are too big to be regarded as part of a single homogeneous historical-philosophical phenomenon.

All things considered, however, it seems to us that most of those who have contributed to the present issue – either as interviewees or as authors – regard the chronological notion of late analytic philosophy, analytic philosophy over the last forty years, as potentially fruitful and, more generally, that many of them seem to share the impression that late analytic philosophy is, at least to a certain extent, a sufficiently uniform phenomenon, both methodologically and sociologically. Among them, however, we find very different ways to interpret and evaluate this alleged uniformity. The third question we posed in the interview, What are the critical aspects of late analytic philosophy?, was meant as an attempt to bring out such different evaluative attitudes. One kind of attitude is exemplified by Williamson who, in his influential book from 2007,

The Philosophy of Philosophy, when speaking of the present state of analytic philosophy stated: "This is not the end of philosophy. It is not even the beginning of the end. But it is, perhaps, the end of the beginning". Williamson's Churchill-inspired remark is paradigmatic of the view according to which late analytic philosophy has achieved the suggested status of scientific maturity and can be regarded as a normal science. Another sympathetic but more critical attitude towards late analytic philosophy is that taken by Weatherson, who in his interview points out that the sociology of late analytic philosophy has an effect not only on the kind of *questions* that analytic philosophers ask but also on the *answers*. We believe this remark is worth examining in depth. On the opposite end of the evaluative spectrum there are those who interpret the methodological agreement among late analytic philosophers as a form of scholasticism (the other face of the science-like nature of their work). According to such critics, recent analytic philosophy fails to be "critical" or, as Cora Diamond puts it in her interview, "responsive", in that it is not capable of challenging its own assumptions, presuppositions and prejudices. Unless one takes an external history approach, gaining a clearer view of these issues and becoming at least somewhat able to understand which interpretation and attitude is most reasonable is likely to be a difficult task. Such an approach might perhaps make it feasible to answer some yet unexplored questions such as, What kind of philosophical work do late analytic philosophers regard as an innovation or progress in philosophy? What is the role of intellectual cooperation and peer review? What is the role of "philosophical fashions"? Are there any philosophical taboos in late analytic philosophy? Are there any periods/subjects in the history of philosophy that have been neglected by the analytic tradition? What has the role of leading departments been in the development of late analytic philosophy? How have funding policies influenced such development? More generally, one might outline an answer to what we consider to be *the* big questions concerning the history of late analytic philosophy: How has late analytic philosophy been influenced by the socio-economic and political context in which it has developed? In what sense and to what extent can we see it as "its own time comprehended in thoughts"?

Guido Bonino
guido.bonino@unito.it
University of Turin

Paolo Tripodi
paolo.tripodi@unito.it
University of Turin

References

Baldwin, Thomas, 2001, *Contemporary Philosophy. Philosophy in English since 1945*, Oxford University Press, Oxford.

Beaney, Michael, ed., 2007, *The Analytic Turn: Analysis in Early Analytic Philosophy and Phenomenology*, Routledge, London.

Beaney, Michael, 2013, "Analytic Philosophy and Its Historiography", in Michael Beaney, ed., *The Oxford Handbook of the History of Analytic Philosophy*, Oxford University Press, Oxford.

Beaney, Michael, 2017, "Analysis", in Edward N. Zalta, ed., *The Stanford Encyclopedia of Philosophy* (Winter 2017 Edition), https://plato.stanford.edu/archives/win2017/entries/analysis/.

Divers, John, 2018, *De Re Modality in the Late 20th Century: The Prescient Quine*, in Mark Sinclair, ed., *The Actual and the Possible: Modality and Metaphysics in Modern Philosophy*, Oxford University Press, Oxford.

Janssen-Lauret, Frederique, 2017, "The Quinean Roots of Lewis's Humeanism", *The Monist*, 100: 249-265.

Preston, Aaron, 2017, *Analytic Philosophy. An Interpretive History*, Routledge, London.

Priest, Graham, 2003, "Where Is Philosophy at the Start of the Twenty-First Century?", in *Proceedings of the Aristotelian Society*, 103: 85-99.

Soames, Scott, *Philosophical Analysis in the Twentieth Century*, vol. II: *The Age of Meaning*, Princeton University Press, Princeton NJ.

Tripodi, Paolo, 2015, *Storia della filosofia analitica. Dalle origini ai giorni nostri*, Carocci, Roma.

Weatherson, Brian, 2014, "Centrality and Marginalization", in *Philosophical Studies*, 171: 517-533.

Williamson, Timothy, 2007, *The Philosophy of Philosophy*, Blackwell, Oxford.

Williamson, Timothy, 2014, "How Did We Get Here from There? The Transformation of Analytic Philosophy", in *Belgrade Philosophical Annual*, 27: 7-37.

Interviews on the history of late analytic philosophy

Guido Bonino, Paolo Tripodi

As editors of this special issue, we thought it useful to ask the same three questions on the history of late analytic philosophy to some philosophers.

(1) What are the main philosophical and metaphilosophical similarities and differences between early analytic philosophy and late analytic philosophy?
(2) Is it possible to identify a mainstream in late analytic philosophy? If so, what are its main (cultural, ideological, philosophical, methodological, metaphilosophical) features?
(3) What are, in your view, the main critical and controversial aspects of late analytic philosophy?

We warmly thank all the interviewees for their collaboration and their interesting answers.

Guido Bonino
University of Turin
guido.bonino@unito.it

Paolo Tripodi
University of Turin
paolo.tripodi@unito.it

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Thomas R. Baldwin
University of York

1) In response to your first question concerning the main differences and similarities between early and late analytical philosophy, I begin by pointing to the title of this issue, namely *History of Late Analytic Philosophy*. For one of the main differences is precisely the historical self-consciousness of contemporary analytical philosophy as opposed to the largely ahistorical manifestos of the early period, when the leaders of this "revolution in philosophy" taught that their new method of logico-analytic philosophy provided philosophy with a new beginning. By contrast in this later period there is much debate concerning the history of the emergence of analytical philosophy, for example concerning the importance of Bolzano's contributions to logic and the philosophy of language. Not surprisingly, however, the most important difference concerns the conception of analysis and its role in philosophy. In the early period the emphasis on analysis was part of a critical dialectic aimed against the idealist holism of philosophers such as F.H. Bradley. Philosophical analysis was conceived as a way of getting back to the ontological and/or epistemological foundations of some area of inquiry, such as ethics or knowledge of the physical world. However analysis of this kind had been employed by many philosophers of the past and the move that was central to the development of analytical philosophy as a distinctive type of philosophy was the emphasis on the logical analysis of language based on the new logical theories of Frege, Russell and others. For this led to Wittgenstein's new analytical conception of philosophy as an activity, as the logical clarification of thoughts and critique of language.

This conception came under pressure from later analytic philosophers, most notably Quine and Davidson, who argued that the conception of philosophy as an inquiry into linguistic "conceptual schemes" separated off from the content of scientific and other inquiries was misconceived. Hence later analytical philosophers have been happy to embrace "naturalism" in its many forms so that they can connect philosophical arguments with evidence from cognitive psychology, evolutionary theory, theoretical physics and other scientific disciplines. Nowhere is this difference more apparent than in ethics. In the early period analytical ethics was primarily metaethics, the inquiry into the metaphysics of value and the "language of morals". But in this later period, ethics is a much broader family of inquiries which addresses practical questions in the context of debates in bioethics, political philosophy, feminism and so on while also addressing debates about practical rationality and the foundation of values.

2) This shift to a conception of philosophy which seeks to make connections with other disciplines of many different kinds invites your second question concerning a "mainstream" of contemporary analytical philosophy. For once philosophy is not thought of as the conceptual analysis of language separated from substantive theories of the world it is important to clarify how there can be a distinctively "analytical" style of philosophy at all. In part the answer to this is that even when one has repudiated the analytic/synthetic distinction there is no need to abandon the activity of analysis informed by logical and semantic theory (as the work of Quine and Davidson shows); and it is a mark of analytical philosophy that it continues to attach central significance to analyses of these kinds, although the analyses are usually conceived as identifying and systematising the connections between propositions rather than identifying basic foundations, epistemological or metaphysical. More generally, contemporary analytical philosophy preserves an enduring commitment to a style of philosophical writing which values the construction of explicit arguments for the positions that are being advanced and a reflective self-consciousness concerning the assumptions inherent in these arguments. This commitment to disciplined argument remains the core of analytical philosophy, but it is now applied in a much broader way, in the development of new transcendental arguments and criticism of them, in the construction of thought experiments and reflection on their significance, and equally in the wide range of formal techniques that are now used in metaphysics, epistemology, and decision theory as well as in logic and the philosophy of mathematics. This commitment has been especially prominent in logic itself in the development of non-classical logics of many kinds, including the construction and defence of paraconsistent logics which allow for true contradictions. Similarly there has been an explosion of work in modal logic, especially concerning the logic of counterfactuals and epistemic modals; and an important feature of work in these areas is that because the boundaries between semantic truth-conditions and pragmatic appraisals are not clear, philosophers of language have developed sophisticated theories which combine semantic and pragmatic considerations.

3) But to turn, finally, to your third question concerning the "main critical and controversial aspects of late analytical philosophy", one such aspect is this variety of formal and informal methods of argument which demand expertise that is not widely shared, with the result that many important new contributions to analytical philosophy command only a small readership. A different issue arises from the way in which contemporary analytical philosophy sometimes draws upon natural science, for this gives rise to the suspicion that philosophical questions can be dealt with by purely scientific inquiries. But this suspicion

is misconceived: take, for example, the question as to what causation amounts to. While contemporary physics is obviously relevant to this issue, especially to the question of backwards causation, there remain many debates about causation that are not going to be settled by physical theory alone, for example how causal claims relate to counterfactuals and to natural laws, whether causation is best conceived as the exercise of causal powers, how far pragmatic considerations determine the identification of causes and so on. Similarly in the philosophy of mind while there are many ways in which discussions of intentionality and mental content have been moved forward by investigations of animal behaviour and cognitive science, there remain long-standing puzzles about our capacity for making mistakes and for rational conduct which do not appear to admit of empirical solutions. Take the case of rational conduct: it is not easy to understand how mental content can enter into the explanation of behaviour that involves physical changes which, on the face of it, should be susceptible of a purely physical explanation. There are many different ways of attempting to show how appearances here can be preserved and this is not the place to attempt to adjudicate between them; but one thing that is clear is that this is not a question that is going to be resolved by a straightforward empirical inquiry. So the "naturalism" of contemporary analytical philosophy is not, I think, a proper cause for alarm that philosophy is being tacitly assimilated into natural science; instead, properly understood, it is a recognition that philosophical questions reach out into inquiries of all kinds, including those of natural science.

I started my comments by noting the reference to 'history' in the title *History of Late Analytic Philosophy*; I end by commenting on another term used in the title – 'late'. For to describe a stage in some temporally extended process or event as "late" is normally to imply that it comes shortly before the end; thus Wittgenstein's "late" philosophy is the philosophy that he constructed in the last stage of his life. So to write of contemporary analytical philosophy as "late analytic philosophy" is to suggest that it is the final stage of analytical philosophy. Is there any reason to accept this suggestion? Richard Rorty famously argued that analytical philosophy assumes that our thoughts represent the world in a way which does not depend on the world so that they can then be compared with it and assessed as true or false. Invoking Davidson's criticism of the scheme/content dualism, Rorty argued that because this founding assumption of analytical philosophy is an illusion, analytical philosophy is misconceived, and philosophical debates should be recast as "edifying" discourses in which later thinkers discuss the works of earlier philosophers without, however, aiming to argue for their truth or falsehood. As my earlier comments indicate, I see no good reason to accept Rorty's sceptical argument. So contemporary analytical philosophy is not "late".

Michael Beaney
Humboldt-Universität zu Berlin and King's College, London

1) As I see it, analytic philosophy has two main sources: Frege's creation of modern quantificational logic and its use in his logicist project, and Russell's and Moore's rebellion against British idealism. Filtered through the linguistic turn effected by Wittgenstein's *Tractatus*, this gave rise to two main strands in early analytic philosophy, the Moore/Wittgenstein "ordinary language" strand and the Frege/Russell "ideal language" strand; and analytic philosophy can be seen as developing through the creative tension between these two strands. The term 'analytic philosophy' was only introduced in the 1930s, at first to describe the Cambridge School of Analysis, but it soon came to include logical positivism as well, and the extension of the term has broadened ever since, both backwards, sidewards, and forwards in history. As a result, the most substantial difference between early and late analytic philosophy is that the latter now includes considerably more as analytic philosophy has ramified into all areas of philosophy, building on the work of more and more philosophers and expanding the topics and themes it addresses. For virtually every subfield of philosophy, there is now an "analytic" version – from analytic aesthetics and analytic feminism to analytic phenomenology and analytic theology.

This broadening of analytic philosophy has gone hand-in-hand with an expansion of its methodological toolbox. This includes a wide range of analytic techniques, from conceptual analysis, logical formalization, contextual definition, and the use of abstraction principles to identifying presuppositions, constructing counterexamples, elaborating thought experiments, and testing "intuitions". One might suggest that there has been a shift over time from reductive to more connective forms of analysis, to use a distinction first drawn by P.F. Strawson (see my entry on "Analysis" in the *Stanford Encyclopedia of Philosophy*), but the important development to highlight is the enlargement and refinement of these techniques and their application to more and more philosophical problems and domains of thought.

This raises the question as to what the similarities are between what is going on in analytic philosophy today and the work of its founders. Perhaps a short answer might be given in terms of their use of the methodological toolbox, but a full answer can only be provided by explaining the relevant historical developments. (See M. Beaney, ed., *The Oxford Handbook of the History of Analytic Philosophy*, Oxford University Press, 2013. I elaborate on what I have just said here in my two introductory essays: "What is analytic philosophy?" and "The historiography of analytic philosophy").

2) Some philosophers might suggest that naturalism now pervades late analytic philosophy to a much greater extent than it did in early analytic philosophy. In a weak sense of naturalism, whereby appeals to anything "supernatural" or to any kind of transcendental realm are rejected, this might be true. In a stronger sense of naturalism, according to which the natural sciences are seen as providing the methodological model for philosophy, I do not think that this is true – and I certainly hope that it is not true, as such a view is profoundly mistaken. As far as arguments, assumptions, concepts, doctrines, ideas, positions, problems, themes, theories, or topics are concerned, I do not think that there is anything to which appeal might be made in characterizing any "mainstream". And even if we see there as being a common methodological toolbox, there is such widespread variation in the tools that are selected, and the uses to which they are put, that talk of any mainstream in this respect, too, is unlikely to be helpful.

Let me offer one answer, however, to raise a rather different issue that I suspect will become increasingly controversial over the next few decades. Analytic philosophy has spread throughout the world, with Societies for Analytic Philosophy established in very many non-English-speaking countries. Many works of analytic philosophy written in English have been translated into other languages and analytic philosophy is now discussed in these other languages. But as far as this linguistic dimension is concerned, *English-language* analytic philosophy is nevertheless the mainstream. This has one enormous benefit: there is now a universal language in which we can all discuss analytic philosophy and anyone who is competent in this language can publish in "international" journals, where "international" is often a euphemism for "English-speaking". Yet there are also many downsides. If there is anything to the view that our thought is partly determined by our language (and I think there is), then the restriction to just one language, however rich and global it becomes, is of deep philosophical concern for all sorts of reasons. I mention just three here. First, discussing philosophy only in English threatens to obscure the concepts and elide the fine-grained distinctions that may be characteristic of other languages and that reveal alternative ways of experiencing and thinking. Second, the importance of the history of philosophy for philosophy (in which I also believe) means that we want philosophers properly trained in the relevant languages if historical texts are to be kept alive and revisited in the light of later developments. Third, I see the ability to translate from one language into another as important a philosophical skill as knowing how to formalize propositions and arguments in logic. Allowing philosophy to be pursued only or even primarily in English would, in my view, be an intellectual catastrophe.

3) I have already mentioned one critical and controversial aspect of late analytic philosophy – the dominance of the English language. This is related to what I regard as its main critical and controversial aspect – its continued opposition to so-called "continental philosophy", though I must immediately say that I find the latter term extremely unhelpful, encompassing as it now seems to do a whole range of rather different traditions, from German idealism, neo-Kantianism, and hermeneutics to phenomenology, existentialism, and deconstruction. This opposition takes many forms, from failure to engage based simply on ignorance to outright hostility and antagonism. It is increasingly misleading to characterize this as an opposition between English-language and other-European-language philosophy, but there are certainly German-language and French-language traditions that offer challenges to analytic philosophy. These challenges need to be taken much more seriously than most analytic philosophers seem prepared to admit, although in recent years there has been a concerted effort in some quarters to facilitate dialogue, which I greatly welcome.

In my recent book (*Analytic Philosophy: A Very Short Introduction*, Oxford University Press, 2017), I identify two criticisms of analytic philosophy that deeper engagement with "continental" traditions helps appreciate. One concerns its naturalist tendencies on which I have already commented. The other concerns its ahistoricist and even anti-historicist tendencies. As also noted above, I believe that history of philosophy is essential to philosophy. Let me mention three reasons for holding this view, too (and for fuller discussion, see the second essay cited at the end of my answer to the first question). First, even if innovations are made with no apparent reference to the past, sooner or later they will need to be clarified and defended by locating them in the historical space of previous views. Second, all philosophical debates and doctrines involve presuppositions that may only become clear with sufficient historical distance and against the wider historical background. Third, there may also be all sorts of potentially misleading or obscure allusions, analogies, metaphors, and intertextual references at play in philosophical thinking and writing that can only be identified through historical work. In appreciating these three reasons, and in responding to the criticisms of analytic philosophy that they imply, we can certainly benefit from the greater historical self-consciousness of most of the various traditions of "continental" philosophy.

Cora Diamond
University of Virginia

1) [Prof. Diamond's reformulation of the first question is: What are the main philosophical and metaphilosophical differences between early, middle and late analytic philosophy?] I take a characteristic and central feature of early analytic philosophy to be a conception of logic and of its significance for philosophy. This conception can be seen in Russell's "Logic as the Essence of Philosophy" in *Our Knowledge of the External World*. One part of logic, on this view, "investigates what propositions are and what forms they may have"; the other part contains completely general logical propositions. The profound philosophical significance of the first part was emphasized by Russell; and a form of the same idea carries over into Wittgenstein's thinking. Both parts of what Russell speaks of as logic are important also in Frege's writings. A descendant of what Russell thought of as the first part of logic also plays a significant role in the middle period of analytic philosophy, as can be seen, for example, in the use of the word 'logic' in the title, *Logic and Language*, of two collections of essays illustrative of mid-century analytic philosophy, edited by A.G.N. Flew. That title also illustrates the importance of attention to language in mid-century analytic philosophy. While the importance of attention to language can be traced back to early analytic philosophy, especially to the *Tractatus*, it becomes the characteristic feature of what Richard Rorty spoke of as "the linguistic turn" – including both logical positivism and ordinary language philosophy. These various ideas about the significance of logic and language to philosophy virtually disappear in late analytic philosophy; and this change goes with the resurgence in late analytic philosophy of metaphysics. From Wittgenstein's early philosophy through middle analytic philosophy, the conception of philosophy as concerned with logic and language went with a profoundly critical attitude to metaphysics, which began to change only in 1959, with the publication of Strawson's *Individuals*. Late analytic philosophy is in some ways closer to ideas in Russell's thinking; he wanted to improve metaphysics not to consign it to the flames.

2) Late analytic philosophy has carried much further than middle analytic philosophy the professionalization of philosophy and (along with that) its specialization. It's useful to note here three very different philosophers of the mainstream of middle analytic philosophy: Paul Grice, Bernard Williams and Elizabeth Anscombe. All three had extensive backgrounds in the history of philosophy and had philosophical interests that did not fall into any narrow category. Grice, for example, was known to have explicitly rejected

the compartmentalization of philosophy. But the culture of philosophy now makes their kind of approach extremely difficult. The mainstream of late analytic philosophy is also distinguished by the prevalent conceptions of the way philosophy is related to the empirical sciences. It may be seen as continuous with the empirical sciences or as itself able to draw on scientific work and to make use of the methods of the empirical sciences. The shift here, away from the middle-analytic understanding of the relation between science and philosophy, reflects to a considerable degree the influence of Quine. Within mainstream late analytic philosophy, the prevalent conception of philosophical methodology is that there are here two possibilities: an armchair approach drawing on supposed "intuitions", or an approach continuous with the empirical sciences. What goes missing is any idea that philosophical methodology might involve attention to experience but not in the kinds of way characteristic of empirical science. I would instance here David Wiggins (whom I take to be a middleanalytic philosopher) and his advice to the readers of his *Ethics*: that they "draw constantly upon [their] lived experience in the world, enlarging that experience by imaginative reference to some larger stretch of human history and human discourse". Here there is a drawing on experience which is not modelled on the sciences.

3) What I take to be problematic in late analytic philosophy is the general attitude to the forms of responsive philosophy that characterized much of early and middle analytic philosophy. A "responsive philosopher", as I use the term, is someone who sees thought as having gone wrong in some significant way, and who responds to that going-wrong of our thinking. Thus Berkeley's *Three Dialogues* express responsive philosophy: Hylas is someone who is being led wrong by contemporary strands of thought (especially materialism), and Philonous's philosophy is directed to leading him back from these wrong paths. Responsive philosophy was important for much of early and middle analytic philosophy. An excellent example would be Philippa Foot's account of what leads us down a misleading path towards consequentialism. Within late analytic philosophy, there is considerable hostility to the kind of responsive philosophy that characterized the earlier and middle periods. That sort of philosophy is seen as disdaining to answer serious philosophical problems, and in particular disdaining to answer metaphysical questions, and as dismissive of such problems. At the heart of much responsive philosophy is the idea that we don't fully see what we are doing in asking philosophical questions – what assumptions we are making and what misunderstandings we may unwittingly be relying on. But the responsive philosopher's insistence on querying the questions can be seen as a kind of quietism that simply ignores real questions.

Within the professionalization of analytic philosophy, problems define areas of expertise and fields of research; and problematizing the problems is not the way to make progress. Hence there is a kind of hostility towards responsive philosophy that runs through much contemporary analytic philosophy.

Hans-Johann Glock
Universität Zürich

1) There is one striking similarity between the dawn of analytic philosophy, epitomized by Moore, Russell, the early Wittgenstein and the logical positivists on the one hand, and recent analytic philosophy on the other. It is the preoccupation with methodological issues, issues that, following Witgenstein's eccentric pupil Lazerowitz, we nowadays call "metaphilosophical". Reflections on the nature, scope and limits of philosophy were key to the formation of the new analytic current, with its turn from epistemology and metaphysics to logic and the philosophy of language. After World War II this interest waned and metaphilosophical reflections were often decried as an unfruitful form of navel-gazing. Over the last 40 years they have been rehabilitated, and for very good reasons. The analytic movement has turned into a well-entrenched mainstream. At the same time it has become extremely diverse and frayed, not least because some of the earlier methodological views, notably the linguistic turn, have fallen out of favour. This special issue bears witness to one fallout from this development, namely the rise of the history of analytic philosophy as a distinct and flourishing sub-discipline. The unsurveyability of the current scene creates a need for clarity about the paths leading us there; and the feeling that analytic philosophy is dying on its feet as a distinct movement fuels a kind of "Owl of Minerva" syndrome. Another, connected, fallout is the resurgence of interest in metaphilosophy. Analytic philosophy was never united by doctrines of even the most general kind. Nevertheless there were certain widely accepted ideas about some central tasks of philosophy and how to tackle them. Among them were the need to clarify questions and terminology for propaedeutic purposes and the need for argument without appeal to authority (whether to sacred texts or the alleged discoveries of fashionable empirical disciplines). But since the 1980s even this minimalist consensus has vanished. The result has been a passionate struggle over the heart and soul of analytic philosophy, which includes the question of whether the latter should give way to a "post-analytic" philosophy that has been promoted in the wake of Rorty. Even among those who still pay allegiance to analytic philosophy, the most basic methodological convictions are hotly contested. There is a debate between

three identifiable (though not precisely demarcated) camps: diehard naturalists, keen to turn philosophy into a continuation of natural science in the spirit of Quine; defenders of a priori metaphysics, more or less strongly influenced by the idea of possible worlds forcefully developed by Kripke and Lewis; and those who try to rehabilitate diverse forms of conceptual analysis, sometimes inspired by classical conceptual analysis à la Wittgenstein and Ryle (e.g. Hacker and Horwich), sometimes seeking to integrate naturalist and essentialist ideas (e.g. Jackson and Chalmers), and sometimes belonging to a broadly pragmatist tradition (e.g. Putnam and Blackburn). More recent additions to this titanic struggle include the debate over what role, if any, appeal to intuitions should play in philosophy and whether the aspiration of rational philosophical debate is undermined by the phenomenon of "peer disagreement".

2) Speaking as a relative outsider, practical philosophy in the analytic tradition has recently been shaped by a revival of work on meta-ethical issues, widely conceived. These range from the renaissance of ethical particularism (i.e. intuitionism) through the continuing controversy over the tenability of neo-expressivism to the debate about the nature of moral disagreement. Political philosophy has finally overcome its obsession with fighting over the true mantle of liberalism, and especially of Rawls. It may also be closer to political theory and actual politics and economics, partly because of a renewed debate about the merits of democracy, partly because of the continuing spread of novel variants of decision theory. This last development is part and parcel of a neopositivist trend that is also evident in theoretical philosophy. It is the ambitious project of a "mathematical philosophy" (as practiced e.g. at the Munich Centre of Mathematical Philosophy), which rekindles hopes for a definitive solution of philosophical problems through the development and employment of ever more sophisticated formal tools promising terminological, methodological and doctrinal rigour. In both moral and political philosophy, finally, the important connections to the theory of action as pioneered by Anscombe and Davidson have been explored (e.g. in the work of Raz). A central bone of contention in this area has recently been the nature of reasons and of explanations that refer to reasons or reasoning. The topics of actions and reasons (for belief, speech and action) also form a crucial link to theoretical philosophy, where they play a role in epistemology, philosophy of language and the philosophy of mind.

In theoretical philosophy the aforementioned three metaphilosophical stances – naturalism, a priori metaphysics and conceptual analysis – also mark main currents within first-order philosophizing in contemporary analytic philosophy. This holds in particular for physicalist endeavours which try to show that non-natural "higher" phenomena are either unreal (eliminativism), or

that, correctly understood, they are really nothing over and above certain physical phenomena (reductionism). As regards their respective importance, this varies from one geographical area to another, and also depending on whether one is concerned with academic philosophy, the wider academic community, or the educated public at large. Concerning academic philosophy, conceptual analysis remains most prominent in the old strongholds of classic conceptual analysis, notably Britain. But it also has followers in continental Europe, though many of them would prefer to regard themselves as descriptive metaphysicians in the spirit of Strawson (e.g. Künne). Conceptual analysis is a minority movement, however. Its impact on other university disciplines is dwarfed by that of naturalism, except perhaps for jurisprudence, which has always had a natural affinity to conceptual clarification and engineering. As regards the majority of university departments in North America, it may be a close race between possible world semantics and metaphysics on the one hand, and diverse branches of naturalism on the other. When it comes to the impact on other academic disciplines, however, naturalism wins hands down. The most striking case in point is the cognitive sciences. Their main philosophical influences, such as they are, derive from more or less strident naturalists such as Fodor, Dennett and Searle. One remarkable recent development in this area is the rise of the "philosophy of animal minds" as a distinct subdiscipline; it is situated between cognitive ethology, mainstream philosophy of mind and a rejuvenated philosophical anthropology.

In some central fields of theoretical philosophy, notably the philosophy of mind and language, there is a Homeric struggle between naturalists and neopragmatists. In the wake of Wittgenstein and Kripke, the latter have contended, for instance, that thought and language involve rules that defy capture by natural science. In line with the more general script summarized above, naturalists have reacted to this "normativist" challenge in either the eliminativist vein, by denying that norms are essential to content and meaning after all, or through reductionist programmes purporting to capture normative force in purely naturalist terms. An especially popular and forceful variant of this second course is the teleosemantics of Millikan and Neander.

3) At the same time there remain areas in which naturalism and pragmatism in a loose sense seem to go hand in hand, such as the continuing attempts to analyse linguistic meaning and propositional content along Gricean lines (e.g. Schiffer). 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. But 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". 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. This view is echoed by countless observers who believe that the customary distinction between analytic and continental philosophy has become obsolete. Loss of identity is one general worry; loss of vigour another. Analytic philosophers have by and large abandoned 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 show that we are past the heroic age of analytic philosophy. 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.

In such a constellation, analytic philosophy's novel interest in its own history and its renewed methodological self-examination – emphasized in my answer to question (1) – are welcome and timely. The unexamined philosophical practice is not worth pursuing. Unfortunately, radical metaphilosophical disagreements have sometimes led to counterproductive acrimony and partisanship. But modesty before our great and important subject demands that all parties pay heed to the difference between philosophy well done and philosophy that chimes with our own philosophical and meta-philosophical views.

This provides a cue for picking up one last loose thread from my answer to question (2). Before considering which current of recent analytic philosophy has been most important to the cultural and political world at large, it behoves us to note and deplore the fact that the wider impact of analytic philosophy has been negligible, even compared to that of other philosophical currents such as post-modernism. There are, however, signs that this may be about to change. We are undergoing a period dominated by right-wing populism, with brazen attacks on science and rational thought, ideologically fuelled by "post-truth" inanities. This has awoken many analytic philosophers, including practitioners of theoretical philosophy, from their ivory-tower slumbers. One must hope that this very recent trend will mature into a current that future historians of analytic philosophy will be able to record with pride.

Matthew Haug
The College of William & Mary, Williamsburg

1) Within the last thirty years or so, the history of early analytic philosophy has emerged as a distinct subfield of analytic philosophy. Work in this subfield has shown that the philosophical landscape during the early analytic period was more complicated and variegated than it was often claimed to be during the middle decades of the twentieth century. This increased historical sophistication is itself an important new development in the analytic tradition. Some may argue that fully appreciating this development should make one wary of providing any other answer to this question. On this view, it is a mistake to try to identify overarching themes or commitments in early analytic philosophy that could be contrasted with those from late analytic philosophy. Attempting to do so can only distort our understanding of both the history of the analytic tradition and the philosophical issues with which it has been concerned: better to take a more fine-grained, piecemeal approach to particular historical figures and debates.

Although there is much to be said in favor of this idea, even caricatures have their uses. For instance, it is not too misleading to claim that much important work during the early analytic period was focused on language and logic and that many projects in early analytic philosophy were based on the analysis of the meaning of linguistic terms and on accounts of how that meaning is determined and immediately available to users of those terms.

Two developments that helped to usher in late analytic philosophy raised serious questions about whether such projects were feasible. First, work on reference in the 1970s by Hilary Putnam, Saul Kripke, and others called into question whether the meaning of many terms is immediately accessible to users of those terms. Second, W.V.O. Quine's critical engagement with logical empiricism offered an alternative approach to philosophy that rejected a cluster of distinctions (analytic/synthetic, practical/theoretical, external/internal) that was central to much linguistically-oriented early analytic philosophy. These two lines of thought contributed to a resurgence of interest in metaphysical issues that has been a major current in mainstream late analytic philosophy for roughly three decades (and to which I return in response to the next question).

Regarding similarities, early and late analytic philosophy tend to share a respect for the methods and results of the sciences (including those of logic and mathematics). In both periods, philosophers differ on how this respect should be expressed. Some hold that the proper application of scientific methods will deliver substantive answers to traditional philosophical questions, while others believe that applying these methods will show that such questions are misguided and call for dissolution rather than straightforward resolution. This

issue will come up again in my response to the last question.

2) I will focus on two strong methodological currents in late analytic philosophy that loosely correspond to the two key developments that I identified in my response to the first question. The first methodological current is related to Quine's rejection of the importance of the analytic/synthetic distinction and to the holistic account of theory confirmation that he adopts in its place. On this view, scientific theories should be evaluated with respect to global virtues such as simplicity, theoretical fecundity, and how well they are integrated with other well-supported theories, and, it claims, philosophical theories should be evaluated in the same way. For example, as Quine puts it, ontological questions are "on a par" with questions of natural science. In this way, metaphysical and other philosophical issues are allegedly naturalized, by being shown to be amenable to the same methods that scientists are thought to use to choose between competing theories.

According to the second current, agreement with our firmly held, ordinary or commonsense judgments is among the theoretical virtues that should be used to evaluate philosophical theories. Further, these judgments are taken to be revealed by our pre-theoretic, "intuitive" responses to possible cases. This component is related to Putnam's and Kripke's work on reference in that their main support for their claims about reference relied on what they took to be widely shared intuitions about what proper names and natural kind terms refer to in counterfactual situations.

I am not sure that these currents are so strong that they deserve to be called "the mainstream" in late analytic philosophy as a whole. However, the first current has been identified as the methodology of "mainstream analytic metaphysics", and the second current is present not only there but also in many other subfields of philosophy, where it intersects with the widely sought goal of achieving reflective equilibrium between general philosophical principles and judgments about individual cases.

Maintaining agreement with our ordinary, "intuitive" judgments may pull against the other theoretical virtues, as even a passing familiarity with contemporary physics strongly suggests. Philosophers who still want to swim in both currents may try to quell this potential turbulence by pointing out that there are tradeoffs between theoretical virtues and that there may be good reason to give less weight to agreement with intuition than to the other virtues. Others may see a deeper conflict here and suggest that we would do better to escape the second current altogether and give no evidential weight to intuitions. I touch on this important dispute in my next response.

3) The two methodological currents discussed above have been subject to criticism from several different directions. In my view, these two sets of controversies are among the most important and interesting debates in contemporary philosophy. In the broadest terms, they concern the place of philosophy in the larger intellectual landscape. Resolving them would go a long way toward determining not only the goals that philosophy can hope to achieve but also the methods that it should adopt to achieve them.

The neo-Quinean, realist approach to ontological questions has been challenged by philosophers who urge a more deflationary point of view, often by developing elements of earlier attacks on metaphysics, such as, for example, Carnap's idea that metaphysical questions admit of "internal" and "external" readings or the global anti-theoretical perspective found in Wittgenstein's later work. Other criticisms of neo-Quinean metaphysics are potentially less sweeping. For example, some have suggested that metaphysical questions should be pursued only to the extent that they contribute to solving problems of broad societal concern. Others have claimed that analytic metaphysics fails to engage with the results of our best current science and instead relies on a superficial or outmoded picture of the world, resulting in so-called solutions to problems that are poorly motivated from the start.

The fact that many of these positions are labeled by a term that begins with 'neo' – neo-Carnapian, neo-Wittgensteinian, neo-positivist, neo-pragmatist, etc. – shows that many contemporary positions are strongly influenced by work from early and middle period analytic philosophy. However, recent work on these issues has not just rehashed old debates but has significantly clarified what is at stake in them and how they might be resolved.

Critical discussion of the second current – the use of intuitions as evidence for philosophical theories – has been catalyzed by work in experimental philosophy, which seeks to apply empirical methods (especially those of social psychology) to address philosophical questions. Experimental philosophy is by no means monolithic, but one of its prominent sub-movements argues that appealing to intuitions in philosophy is illegitimate since subjects' intuitive responses have supposedly been shown to be determined by factors that are irrelevant to the philosophical issues at hand (factors such as the order in which cases are presented or the culture in which a subject grew up).

This critique has inspired a number of responses, both from those who seek to rebut it directly and from those who claim that it is off the mark since, in their view, philosophers typically do not rely on intuitions as evidence in the first place. More generally, these debates have contributed to efforts to get clearer about just what intuitions are and about what kinds of capacities are responsible for their production.

Cheryl Misak
University of Toronto

1) It's very difficult these days to get a grip on just what "analytic philosophy" is. One might think, then, that the difference between early analytic philosophy and late analytic philosophy is that we knew what we were talking about when we employed the phrase in, say, the 1920s and we don't know what we're talking about now. But even that isn't clear. Three overlapping attempts at defining analytic philosophy spring immediately to my mind, but each quickly disintegrates, even in the early days.

a. Conceptual analysis. It used to be that there was a distinct methodology that travelled under the banner "analytic philosophy": the attempt to provide analytic definitions, or necessary and sufficient conditions, for our concepts. But even in the heyday of analytic definition, we had some recognizably analytic philosophers pushing back on the idea that we could provide such definitions. Was G.E. Moore not an analytic philosopher because he thought that some of our central philosophical concepts are indefinable? Was F.P. Ramsey not an analytic philosopher because he thought that definition was of limited value?

b. Reductionism. Sometimes "analytic philosophy" is used as a label for any attempt to reduce a category A to a more fundamental category B – for instance, to reduce the meaningful to what can be stated in the terms of observation and formal logic; to reduce belief to behaviour; and so on. But again, even in the heyday of reductionism, there were recognizably analytic philosophers who pushed against this kind of project. When the logical positivists gave up on the strong programme of reduction, did they cease to be analytic philosophers?

c. Philosophy as Formal Logic. It may be thought that early analytic philosophy was driven by the new formal logic developed by Frege and Russell. But in the *Tractatus*, Wittgenstein said, and meant, that what *cannot* be expressed in the primary or logical language is more important than what *can* be expressed there. Does that mean that the *Tractatus* is not a work in analytic philosophy?

Even if one of these definitions of analytic philosophy is thought to not admit of counterexamples – that Moore, for instance, was a logical analyst who simply thought that not all concepts could be defined – surely it is still the case that the history of analytic philosophy includes all three of these ways of doing philosophy, and more. It also includes, I have argued, the brand of empiricism called pragmatism. To say that the history of analytic philosophy doesn't include great logicians C.S. Peirce or C.I. Lewis because they argued

that there was more to analyzing a concept than offering a definition, or that they argued against reductionist projects or against the fact-value dichotomy, would be very strange indeed.

2) Since I think that analytic philosophy is a broad church, I would be loathe to try to identify a mainstream. I think such an attempt would be misguided. Even if one could identify a sociological trend amongst analytic philosophers, I would expect that so-called mainstream to be a changing, evolving thing.

I suppose I could say that a similar core runs through both early and contemporary analytic philosophy: a methodology that seeks to scrutinize our deepest conceptions and convictions and offer a coherent account of them, based on careful argument. Not very exciting, but sometimes the truth is like that.

3) I suppose the very nature of this volume suggests that one controversial aspect of current analytic philosophy is to say whether it is broad or narrow. It will be clear on which side of the controversy I stand. Given that I take analytic philosophy to be a broad church, concerned with the most important issues that face us, it will be unsurprising that I think that some of the more vital issues of current analytic philosophy have to do with the nature of truth, the evaluation of belief, and whether disputed beliefs such as ethical, political, counterfactual, and general beliefs are truth apt. Philosophy, William James said, "is at once the most sublime and the most trivial of human pursuits. It works in the minutest crannies and it opens out the widest vistas". Surely analytic philosophy can and must do both.

Philip Pettit
Princeton University

1) Early analytic philosophy began from a strange theory of meaning, evident in the work of G.E.Moore and Bertrand Russell up to about 1905, when Russell published the theory of descriptions. This theory assumed that every word had a corresponding meaning, often described as a concept, and led Russell even to wonder what meaning – by assumption, what atomic meaning – the definite article had. On that picture, analysis was only appropriate with compound terms, which were taken to be analyzable into simple, unanalyzable terms. This atomistic view began to break down with the theory of descriptions but remained fundamentally in place until about 1930, appearing in a distinctive form in Wittgenstein's *Tractatus* of 1922.

The work of the later Wittgenstein and of the Oxford School of Ordinary

Philosophy recast the task of conceptual analysis, arguing for a greater sensitivity to what made sense in ordinary locutions and an awareness of the possibility that philosophizing often broke the ordinary rules and ended up making little sense. This development was severely inhibited by Grice's development of the distinction between semantic incoherence and pragmatic infelicity. It may be infelicitous in ordinary usage to say that I know I have a pain but it is hardly incoherent, as had been claimed, and hardly involves a deep misunderstanding of the notion of knowledge.

Logical positivism, which had developed independently in Austria and Germany, had little truck with ordinary language philosophy, arguing instead for a strict ruling on what should count as meaningful sentences and distinguishing those into sentences true by meaning and empirically true. While Willard Quine cast doubt on the possibility of making the latter distinction, it was reinstated in a new, clearer form by David Lewis's work on convention which effectively associated terms with conventionally established assumptions. And while Saul Kripke undermined the idea that reference is always fixed by narrow assumptions about the referent – by meaning in a sense expressed in those assumptions – Lewis and Frank Jackson made room for Kripke's insights by allowing that the assumptions may sometimes rigidly identify the referent: on this account the referent of 'water' is not whatever fits established assumptions about water but the actual stuff that does so.

Building on this work, Lewis inaugurated the program of vindicating the problematic terms of ordinary language, say in mental vocabulary, by identifying a way of defining them, or at least near relatives, in functional terms. The entities that deserved to be designated or ascribed by use of those terms would be defined, often defined in a holistic or package deal, by the causal or other roles they play, with those roles being described in less problematic vocabulary. Thus, the state of believing that p would be identified as the state that played the role of being responsive to evidence that p or not p, that led the agent to act for desire-satisfaction as if it were the case that p, and so on.

This sketchy history underlines one continuity between late and early twentieth century philosophy: beginning from language in giving an account of how the world is and what it is about various properties and other entities that makes them deservers of the terms in which we ascribe or posit them. But the differences are also salient. For in the functionalist picture of at least much of philosophy there is a concern to give an account of those deservers, at least in problematic vocabularies such as the language of mind and morality, that locates them within a naturalistically or scientifically intelligible universe.

2) I think of the functionalist paradigm of conceptual analysis as still fitted to be cast as the orthodoxy in late analytic philosophy. But it has come under attack on a number of fronts, generating alternative approaches.

It is central to the paradigm that all the terms in our folk psychology of the mind can be defined after a functional pattern in other simpler terms and this is often used to vindicate a physicalism about the mind of the kind originally put forward by Jack Smart and David Armstrong. But one line of attack on this approach has been to argue that various mental concepts, in particular those associated with consciousness, defy functional analysis. This has led to theories of the mind in which physicalism is rejected or at least understood in a very different manner from that associated with the functionalist variety. Those theories have pressed people into arguing for different models of how problematic terms gain their meaning, yielding a different picture of what conceptual analysis involves.

Lewis recognized that the functionalist analysis of a term may sometimes leave it open whether the term designates one or another entity or whether it predicates one or another property. Although this is now disputed, he appears to have suggested that among the candidates for the semantic value of such a term, some may be more natural than others, where naturalness comes in degrees and tends to be understood on an intuitive basis. And, so this reading of his intentions goes, he proposed that the term should be taken to refer to the most natural candidate. This has led many thinkers to recast the role of basic philosophy altogether, presenting it as an attempt to limn the boundaries of the natural in this sense, looking for what purportedly are the joints at which reality in itself is carved, not at the presumptively conventional distinctions that we introduce there in service of our own interests.

This last development has been buttressed by a novel twist introduced under the influence, mainly, of Kit Fine. Lewis and others had argued that a functional analysis of "belief" or "desire" or any such problematic, quasi-theoretical term showed how a physical state might play the role ascribed and deserve to be named by the term. And on this picture, that meant that the facts described in mental terms were "supervenient" on the physical facts, in the sense that they could not vary without a variation at the physical level. In such a case the physical and mental facts not only satisfy the formal condition defined as supervenience; plausibly, the physical facts ground the mental facts, as it is not often put, where grounding is a substantive relation *in rebus*, not something that lends itself to full analysis. Since grounding involves a real relationship in the world, that has supported the idea that basic philosophy has the substantive aim of identifying the grounding relationships in things, a task that is distinct from any form of conceptual analysis.

3) I am one of those who hankers still for the methodological clarity of the functionalist program, whether pursued in thinking about the law and the state, about moral value, free will and responsibility, or about causation and personhood. I do not see how philosophy can claim to be positioned to explore the world except via an exploration and critique of the assumptions about the world that are built into our ways of speaking and thinking, whether in commonsense or in science. And I believe that the main challenge in metaphysics and related areas is still that of looking for how far commonsense assumptions can be squared with those that science supports: how far the manifest image, as Wilfrid Sellars put it, can be squared with the scientific image. But I am interested, particularly, in exploring new ways of approaching the goal held up in such analysis.

One insight that is important by my lights, and has been pushed recently by David Plunkett and others, is that ordinary language is often so context-sensitive that it allows us to construct a number of candidate concepts or representations that answer equally well to its connotations. The task this multiplicity of concepts then gives us is that of choosing the best candidate for whatever are our purposes in philosophy; these would certainly include the aim of reconciling the manifest and scientific images. Thus, while I distinguish between different concepts of freedom that are each more or less faithful to ordinary usage, I hold that one of these (freedom as non-domination rather than freedom as non-interference) can help to build a more satisfactory normative theory of government; it can better satisfy John Rawls's test of reflective equilibrium.

Another insight that has influenced my own work recently is that many of the more exciting ventures in analytic philosophy have involved, not looking case by case for the assumptions that appear to be linked with the use of one or another problematic term or concept, but constructing a narrative in which the protagonists come in plausible, unproblematic stages to give currency to a term or concept that, on reflection, looks to be equivalent to ours. Arguably, H.L.A. Hart tries to do that for the concept of law, for example, David Lewis for the concept of convention, Edward Craig for the concept of knowledge, Bernard Williams for the concept of truth. In a forthcoming book, *The Birth of Ethics*, I try out this methodology for a raft of ethical concepts; the book looks at a way in which a pre-moral community might come in plausible, naturalistic steps to give currency to concepts that answer intuitively to our various moral concepts.

Nicholas Rescher
University of Pittsburgh

(1) How does the early analytic philosophy – say that of the 1920-1940 era of Russell, Moore, Ayer, Charles Stevenson, and C.I. Lewis – differ from that of its more recent version in the 1970-2020 era of Putnam, Quine, Kripke, David Lewis and their progeny in the next generation?

The following points are prominent among the contrasts to be noted here:

- The earlier analysts contemplated a program of philosophical reductionism. They deployed the tools of analysis to undermine or even eliminate various traditional philosophical projects. For example, they were opposed to mere (Meinongian) possibilia, to nonexistent objects, to the bare speculation of thought experiments. In all these regards the later analysts became increasingly open-minded and accepting, prepared not only to retail but even broaden the philosophical agenda in both thematic and methodological regards.
- Moreover, the earlier analysts were dedicated to the reality and facticity of what is and had little or no patience for the normativity of what ought to be. They deemed matters of value, worth priority, and the like as beyond the reach of rigorous philosophical inquiry. Here too their successors took a very different line.
- The earlier analysts were given to broad and sweeping generalizations, inclined to think that what holds for one item of a certain salient sort is typical and apt to hold for all. For example, Russell's theory of descriptions that what holds good for one fictional item "the present king of France" will hold for nonexistents at large. The later analysts, by contrast, were inclined to descend into detailed particularities. They exhibited a vastly greater concern for analytical microdetail and manifested a corresponding reluctance to embrace far-reaching generalizations.
- Such reluctance to see the detail of cases as generally typical enmeshed the later analysts with a greater concern for detail and distinctions. Their proceeding became a venture in conceptual microscopy. And this in turn led them into the increasing complexity of division of labor and its consequent specialization.
- The earlier analysts wanted to surpass philosophical history: they generally regarded earlier philosophizing as based on outdated and untenable commitments. The later analysts sought to turn analytical methods upon historical materials in a search for instructive lessons. Analytically inspired historiography has become a gold-mine for latter-day philosophizing.

In all these regards, the later analysts radically transformed the philosophical landscape envisioned by their earlier compeers.

(2) Issues of philosophical history apart, two thematic features mainly separate early from later analytic philosophy, namely possibility and normativity.

Unlike the early analysts who focused devotedly on actuality, factuality, and reality, the later analysts increasingly emphasized matters of possibility, hypothesis, and nonexistent but possible worlds. Here these figures in logic, in metaphysics, and in theoretical and practical philosophy now became focal issues.

And this brought another realm of concern into the forefront, namely value theory and practical philosophy. Reality is simply what it is, but probability carries comparative assessment and evaluation in its wake, both cognitively (in point of comparative probability) and evaluatively (in point of comparative merit).

For the earlier analysts science – the investigation of the actual world and the investigation of the domain of fact – was paradigmatic. But the later analysts turned increasingly to speculation and thought experimentation – the investigation of "what would we say if" – became an increasingly paramount concern of philosophical inquiry.

With norms, evaluations, and criteria in the forefront, the rational basis of discourse and inquiry came to be seen as an increasingly pressing topic. While with practical matters factual observation provides the ultimate ground of validation, with speculation matters we will need to launch in other directions. The topic of consistency and the epistemology of plausibility will become the pivotal issues.

In the later phase of analytic philosophizing the doors were thus thrown wide open to the prospect of new areas of concern. Not just the cognitive sphere of knowledge and its limits but the issues of ethical judgment and social policy (justice, fairness) came to figure on the agenda, with matters of justice, fairness, equality, and the like now at the forefront. Even the rationality of religious belief came on the scene and analytical philosophy of religion was a thriving concern rather than a contradiction in terms.

(3) The prime difference between early and late analytic philosophy is the product of increasing specialization and fragmentation that has resulted from the growth of the philosophical profession. The resultant technicalization of investigations has transformed philosophy into an aggregation of specialties and of specialists given to investigating minute issues in highly technical ways. The increasing remoteness from philosophy's formulative starting-point concerns was doubtless something that its pioneers would not have appreciated.

The use of far-fetched thought experiments and wild hypothesis is a particular questionable aspect of latter day analytic philosophy. For the philosopher's "clarifications" by the use of extreme cases and fanciful science-fiction examples engender pressures that burst the bonds holding our concepts together, seeing that the concepts we standardly employ are geared to an implicit view of the nature of the real and that to press our philosophical concepts beyond the limits of the realities that make them viable does not conduce to clarification but leads ad absurdum. A striking consequence of this situation is that on its basis the entire bizarre demonology of much contemporary philosophy can be averted. We no longer have to worry about cross-wired brains that share the same thoughts (or don't they?) or shrewd aliens from outer space that can inspect our visual fields (will they "see the same things" even though their concepts are different?).

Then too, the technicalization of the discipline has been accompanied by a striking lack of self-criticism and self-awareness at large. While the philosophical landscape is nowadays replete with philosophy-of-this and philosophical-of-that (of science, of law, of feminism, of spirit, etc.), the philosophy of philosophy is a decidedly under-exposed terrain. Self-criticism is something present day analytical philosophers lack. Unlike these earlier confreres who thought that philosophy had the mission of unmasking error and mistaken demonstration, present day analytic philosophers are remarkably reluctant to explain why what they are doing is significantly useful and important.

On the other hand, the shift from early to late analytic philosophy has had some very substantial benefits. For broadening of the agenda not only led to the recovery of many of the interesting traditional preoccupations of philosophers, but has also witnessed an opening of horizons of concern and a remarkably broadening of perspective. For example, the analytic philosophy of religion represents a remarkable recovery of prior abandonments, and the philosophy of science has seen a flourishing of expertise that has transmuted its status as a critic of science into an appreciated collaborator of its practitioners

In conclusion, it deserves comment that the very flourishing of analytic philosophizing has itself created a substantial structural gap with which the field had been unwilling or unable to deal. The proliferation of increasingly small scale studies by means of increasingly technical thought instrumentalities has created the need for works of syntheses and integration to give an account of the bigger picture providing some idea of what useful lessons emerge from the mass of inconsistencies. The near-total absence of those much-needed works of synthesis and integration is a deficit that is a disgrace to the discipline. And this situation is unlikely to be remedied as long as graduate schools train philosophical fledglings to see it as their mission to plant more trees without bothering about the forest.

John Skorupski
University of St. Andrews

1) "Early" analytic philosophy (in the present sense) can be thought of in three phases. There is the work of such figures as Moore, Russell, and Wittgenstein up to the end of the first world war. There is Wittgenstein's "middle period" and the work of the Vienna Circle to around the second world war. The final and longest phase then stretches through to the 1970s or so: it includes Wittgenstein's "late period" and the various forms of ordinary language philosophy. I would also include here work in Harvard by Putnam and Quine, and the work on meaning of such writers as Davidson and Dummett, where the assumption is still that the theory of meaning is the basic philosophical discipline.

The development of analytic philosophy throughout these years, though undoubtedly a very varied story, is a thematically continuous one. Internal debates, often intense, actually contributed to the thematic continuity. Taking the three phases together, one can speak of an analytic *tradition*. The idea that philosophical problems can and should be dissolved by analysis of language was central, though not uncontested. When historians of philosophy look back on the twentieth century the analytic tradition, in all its variety, will probably be their main focus of interest, or at any rate at least on a par with phenomenology and existentialism.

Now, in "late analytic philosophy" there is a widely shared rejection of this tradition. Indeed, rejection of its fundamental theses about philosophy and language are about the only thing the diverse strands of late analytic philosophy can agree on. Philosophical questions, specifically, metaphysical questions, are, it is now generally held, genuine questions. How they relate to science is debated. But (it is thought) they certainly cannot be dissolved by linguistic analysis.

How did this striking turn happen? Clearly a pivotal element was the work of Quine. His debate with Carnap on truth and convention, his assaults on analyticity, synonymy, and modality, his thesis of the indeterminacy of translation, his holistic verificationism, his minimalism about truth – all of these belong with the analytic tradition. And yet they were important in ending it, because they undermined the tradition's central claims about meaning, from within. However – perhaps for this reason – since Quine's critique has done its work it has taken a back seat. Instead, new impulses to metaphysics have come from Saul Kripke, David Lewis and "possible-world semantics". To some extent independently, a traditionally realist and intuitionist understanding of necessity and aprioricity has been revived. However another element in Quine's philosophical approach has strongly lasted: his naturalism. This is a "thesis" which in the period of the analytic tradition could only have been seen as an

old-fashioned, probably vacuous, bit of nineteenth century pseudo-science. Its status is very different now. The combination of metaphysical realism and naturalism is one of the most influential standpoints in "late analytic philosophy".

2) Given that the central, even defining, tenets of the analytic tradition have lapsed I question whether it is useful to call this period "analytic philosophy" at all – at least in a thematic as against a primarily institutional sense. If the term 'analytic philosophy' refers to anything now, it is to a style of writing, a professional familiarity with and liking for some formal techniques, and a set of university philosophy departments in which the use of such techniques is well accepted. 'Analytical philosophy' in this institutional sense refers to a distinctive social praxis in academe. It is not defined thematically by any main stream, and increasingly, it does not have one. It is in this sense that we can understand such labels as "analytic" Marxism, or "analytic" Thomism. (I should mention at this point that I cannot here discuss developments in moral and political philosophy, important as they have been. With the exception of meta-ethics, they do not fit into the historical framework we are considering, and must be discussed independently.)

The academic aspects of philosophy are of course affected by the institutional development of universities, where philosophy is nowadays almost exclusively pursued. In terms of sheer numbers of researchers, philosophical activity is much bigger now, and that in itself makes a difference. Ever more philosophy academics are writing ever more papers, and chasing ever larger grants of money. The effect, if any, on the content of philosophy is not yet clear. On the one hand, it is reasonable to worry that interactions between intellectual fashion and grant-giving may discourage genuine as against artificial philosophical innovation, and discourage large ambitions. On the other hand, bureaucratic funding systems usually have attempts at diversity and neutrality built into them. In any case genuine philosophical innovation has always been the preserve of a strong-minded few, and such people can probably flourish irrespective of this or that system of academic research incentives.

3) While philosophy has greatly diversified, I noted that rejection of the analytical tradition has been accompanied by a widespread return to the combination of naturalism and realism.

This important shift stands out clearly if we view it in a longer historical perspective. Important strands in nineteenth century philosophy adopted the same standpoint. In logic, epistemology, ethics, they sought philosophical illumination from empirical psychology and human evolutionary biology. Important trends today (e.g. "experimental philosophy") once again take this line.

In due course Frege reacted against psychologism – by extension against all forms of naturalistic reduction – in logic, while Sidgwick and Moore reacted against naturalistic reduction in ethics. Sidgwick and Moore were committed to the synthetic a priori status of ethics; as I have discussed elsewhere, Frege accepted a broad notion of analyticity which effectively raises the same epistemological questions. However none of them was very forthcoming about answers.

The analytic tradition went further; it sought to answer them. I agree with historians of the analytic tradition who see the analytic tradition, notably in its second and third phase, as a version of "Critical" philosophy, in the sense in which that term was used by Kant. Like Kant, it rejected a conception of philosophy as the broadest science of reality. Kant called this conception "transcendental realism", and famously thought it had to be rejected if "empirical realism" was to be preserved. In the analytic tradition we see a similar dialectic. But where Kant's doctrine of the a priori turned on forms of sensibility and categories of the understanding, in the analytic tradition something else did the work. In broad terms it was an epistemic, or "use", conception of meaning. Its final stages in the analytic tradition appeared in Michael Dummett's notion of anti-realism, and in Hilary Punam's distinction between metaphysical and empirical realism.

It is virtually definitive of Critical philosophy to reject the combination of naturalism and metaphysical realism – the very combination which again dominates (and which is one of the things people have in mind when they criticise "scientism".) Yet the criticisms by Quine, and others, of the analytic tradition's basic idea, that of grounding the a priori on a theory of meaning, were sound. It seems then that neither transcendental idealism nor the analytic tradition's appeal to meaning provided a stable platform for the Critical standpoint.

Does that mean rejecting the Critical approach itself? Or is that throwing away the baby with the bath water? It is one thing to reject metaphysical realism (in effect, that is, a correspondence conception of truth), quite another to endorse some epistemic conception of meaning. We should leave meaning to the semanticists, empirical and formal. The right approach to the a priori is not through transcendental idealism, nor through semantic anti-realism, but through taking seriously the idea that aprioricity is normativity. So I and others have argued (in my case, in *The Domain of Reason*, Osford University Press, 2010).

The Critical approach is not dead! Reverting, however, to the historical view, I have to say that it is certainly a minority standpoint in current "analytic" philosophy. The distinctive combination of naturalism and metaphysical realism – which the Critical outlook dismisses as dogmatism, "transcendental realism", etc. – *is* the default Anglo-Saxon view. If we see things in this way then it is actually the analytic tradition's temporary dominance in the Anglo-Saxon philosophical world that looks like the exception.

Brian Weatherson
University of Michigan, Ann Arbor

1) There are a lot of strong cultural similarities. Both traditions are extremely Anglophone, late analytic even more so given the importance of philosophy in German to early analytic philosophy. Both are very white and very male. Both take roughly the same set of historical figures to be central to the canon, though this matters not a great deal because so many in both traditions downplay the importance of history of philosophy. Both see philosophy as continuous more with the sciences than the humanities. Both are rather fond of theories that can be expressed in formal languages, especially if the formalism is not especially challenging.

But, from my perspective at least, the differences are more striking. I'll focus on the areas I know best – those around language and epistemology. The story in moral and political is I suspect different, but I don't know it well enough to comment.

Philosophy of science is a huge part of early analytic philosophy; to some practitioners it is central to all philosophy. This is not true for late analytic philosophy. Philosophy of science has splintered into philosophy of the different sciences; there is little written at the level of generality that was common in early analytic philosophy of science. I think this has been a great thing philosophically, since we learn more from detailed study. But none of the resulting sub-disciplines are as central as philosophy of science once was.

Early analytic philosophy was much more concerned with ideal languages and ideal minds. It didn't matter (to many people) whether Russell's theory of definite descriptions was an adequate account of certain English noun phrases, just as long as it could work as an account of descriptions in an ideal language. Late analytic philosophy is much more concerned with actual languages and actual minds. This was no doubt helped along by arguments (due to, among others, Turing, Grice, Montague and Fodor) that we could investigate actual minds and languages without giving up our beloved formal tools.

The most important change has been a movement towards metaphysical realism, and towards anti-scepticism, and away from transcendental arguments. (I'll call this package realism in what follows.) These are related; if scepticism is false and moreover knowledge is easy, then we can't do metaphysics by asking what things must be like for knowledge to be possible. That won't constrain the possibilities sufficiently.

The trend towards realism is by no means universal. There are plenty of anti-realists, and sceptics, around. But one can nowadays simply presuppose a much stronger kind of realism than could be presupposed in early analytic phi-

losophy. (Or, for that matter, in English language modern philosophy.) And it is a trend that is continuing. David Lewis was more realist than the prevailing trends when he wrote *Counterfactuals* and "New Work", but less realist than the prevailing trends when he wrote "Ramseyan Humility". And this wasn't because Lewis's worldview had changed, but because the discipline had become more realist-leaning.

2) The cultural and ideological features are easiest to identify. The people who make up late analytic philosophy are, in general, white, Anglophone, male, relatively wealthy, fairly urban, and politically left-wing. This has had a dramatic effect on the questions that mainstream late analytic philosophy takes seriously, and I suspect a large effect on the answers it has taken seriously too. There are signs that these cultural features are changing. The mainstream includes more philosophers from non-Anglophone countries than it did a few decades back, though most of the exchanges are still in English. PhD programs in philosophy are less white, and less male, than the discipline as a whole. And, at least anecdotally, it seems that this is making a difference in what questions and answers are taken seriously. But there is a long way to go on these fronts.

I've already mentioned one big philosophical feature of the mainstream: a widespread acceptance of realism. This has consequences across the discipline. It means that error theories have a hard time gaining widespread acceptance. Such theories exist, and have prominent defenders, but they rarely become orthodoxy. On the other hand, there has been an upsurge in interest in projects like naturalized epistemology. Philosophers have become more interested in starting with practices as we find them, and critiquing those practices by our own standards, not a possibly mythical external standard. As Elizabeth Anderson and Louise Antony (among others) have noted, feminist epistemology has long had such a naturalist approach, and a big part of the story of the last few decades is the mainstream becoming more appreciative of feminist insights (though unfortunately not always under that description).

Another striking feature of the mainstream is its use of vignettes and thought experiments. From Philippa Foot's runaway trolleys, to Peter Singer's drowning child, to Judith Jarvis Thomson's dying violinist, to Frank Jackson's imprisoned scientist, to David Chalmers's zombies, late analytic philosophy has been full of stories and characters. It is controversial how much and what kind of argumentative work these stories and characters are doing in late analytic philosophy. But there is a strong norm that, no matter how abstract one's subject matter, one ought to include such stories in one's philosophical work. I think this is continuous with the previous point; one point of these stories is to show how the theoretical issues being debated are grounded in our current

views and practices. And this in turn leads to a familiar critique; the stories can't play their intended role if they are so fantastic.

3) This focus on stories is clearly controversial; indeed, it is a thriving controversy. I think Tamar Szabó Gendler's work on thought experiments did a lot to make philosophers appreciate the philosophical issues at stake here. There are at least two big questions: What role do these stories play, and are they fit to play that role? And a common critique is that they play an evidential role – intuitions about cases are givens in inquiry – that they aren't fit to play. They aren't fit because intuitions are too variable, and too unreliable, especially when the stories are so removed from everyday life. For what it's worth, I'm sympathetic both to Herman Cappelen's argument that these stories are used more frequently to illustrate and clarify than to argue, and to Timothy Williamson's argument that it's perfectly reasonable to use what we know about these stories in philosophical reasoning. But it's hard to talk about controversies and not talk about this issue.

The move to realism has gone along with a move to more applied questions. In epistemology, for example, there are fewer papers nowadays on the nature and possibility of knowledge, and more on perception, on testimony, and on moral epistemology. The same pattern recurs in a bunch of different sub-disciplines. (I noted above a similar trend in philosophy of science.) I think this has been a very welcome change. But it has had some difficult side-effects.

One is that there has been a sequence of fads sweeping the field. From nonconceptual content to fictionalism to zombies to Sleeping Beauty to vagueness to de se content to contextualism to grounding to peer disagreement we have seen a pattern where for a few years it seems everyone is talking about one hot problem and then for some reason (perhaps resolving the questions, perhaps boredom) we move on. I think most of these debates have been interesting, and I've enjoyed playing a part in several of them, but I suspect we'd be better off slowing the cycle down.

The more applied our research topics are, the more important it is to listen to researchers outside of philosophy. And the more important it is to listen to philosophers who listen to researchers outside of philosophy. It seems to me that we're doing better on the first of these – philosophy papers at least seem to include more citations of non-philosophers than was true some years back. It's not so clear that we're doing well on the second. It's common to see philosophers whose work draws heavily on research from elsewhere being told their work "isn't really philosophy", getting chilly receptions at talks and struggling to publish in generalist philosophy journals. In principle, most late analytic philosophers would sign on to a version of confirmation holism that

says evidence could come from all sorts of sources. Whether we all live up to that in practice is an ongoing question.

Timothy Williamson
University of Oxford

1) Early analytic philosophy was not a homogeneous movement, even when pursued by just a handful of people. Frege, Russell, Moore, Wittgenstein, and Carnap had very different philosophical styles from each other, very different ideas of what they were doing. Late analytic philosophy is even more diverse. It's active in most countries in the world, dominant in many, and still growing. It covers most branches of philosophy. The terminology suggests that the early version had a more revolutionary or pioneering feel to it than later, which is probably true, though classification in hindsight can be misleading – it is easy to forget how much the early analytic philosophers were in dialogue with immediate predecessors and contemporaries now assigned to different periods or traditions.

The phrase "late analytic philosophy", like "late capitalism", hints at wishful thinking of imminent collapse, though I understand that the editors intended it more neutrally. Perhaps one day the present will be classified as still belonging to an early stage of analytic philosophy. More likely, though, it will not be marked off as so different from a long development through figures such as Plato, Aristotle, Leibniz, and Hume – prototypical philosophers whose ambitious theorizing nevertheless engages with, or helps create, other sciences. A recent example is the way in which intensional semantics, developed by philosophers such as Carnap, Richard Montague, David Kaplan, and David Lewis has led to formal semantics as a branch of linguistics.

In some ways, late analytic philosophy has more in common with early analytic philosophy than with the intervening period of "middle" analytic philosophy. The "Linguistic Turn", whatever its exact nature, belonged mainly to that middle period – Wittgenstein, the Vienna Circle, ordinary language philosophy. Frege, Russell, and Moore were not linguistic philosophers in any distinctive sense. Nor are most contemporary analytic philosophers. Non-linguistic analytic philosophers can still be seriously concerned with language in various ways, as were Plato, Aristotle, and the Stoics. One wants to understand the semantics of natural languages for its own sake, or in order to use the instrument of most of our philosophizing more accurately. One may devise artificial formal languages as better instruments for systematic theorizing and arguing. Those activities don't make one a linguistic philosopher, because they

imply no special relationship of philosophy to language. Linguists, historians, literary theorists, psychologists, and sociologists all study natural languages too. Computer scientists study artificial languages. The Linguistic Turn was supposed to be more than that. Late analytic philosophy has woken up to the failure of linguistic philosophy to deliver on its methodological promises, its failure even to study language systematically enough. Nevertheless, contemporary philosophy of language comes from a synthesis of the best of two rival traditions in middle analytic philosophy.

2) It's tricky to identify a mainstream in late analytic philosophy because philosophy, almost as much as most other disciplines, has become so specialized. Epistemology, metaphysics, philosophy of language, moral philosophy, and so on are pursued as separate branches of inquiry, though with some interesting interactions between them. Philosophy of language has no privileged position; it isn't the driving force for the rest of philosophy as it perhaps was in the mid-twentieth century. Nevertheless, at a higher level of abstraction, there are common features – not universal, of course, but at least typical. As already mentioned, late analytic philosophy is not linguistic philosophy. Nor, despite its name, does it tend to regard itself as doing linguistic or conceptual analysis in any distinctive sense. For instance, if you ask contemporary analytic metaphysicians whether they are interested in temporal words or concepts, they will tend to say, no, what really interests them is the nature of time itself. This often goes with a more or less realist attitude to the subject matter of philosophy.

A related feature of contemporary analytic philosophy is that it generally takes as obvious that the findings of the natural and social sciences can be philosophically significant. For instance, it would sound extremely old-fashioned for a philosopher of perception to say "I don't need to know what experimental psychologists of perception have discovered, because their questions are empirical; mine are conceptual". Attitudes now are very different from Wittgenstein's or Ryle's. There is also much less resistance than there was to the use of formal methods in philosophy, where relevant – another similarity with early analytic philosophy.

A welcome trend is the increase in applied philosophy. Applied ethics is an obvious example, but now analytic philosophy of language and analytic metaphysics are being applied to political philosophy, the philosophy of gender and race, and so on. There are lots of new questions to ask. Late analytic philosophy is not at all "purist" about what counts as philosophy.

Of course, change in philosophy is never uniform. You can still find young philosophers who look on contemporary analytic philosophy as a sad decline

from the golden age of Wittgenstein or Austin. The point is that they feel alienated from contemporary analytic philosophy in a way that, for example, most other young analytic philosophers don't.

Culturally, late analytic philosophy is slightly ill at ease in the humanities (especially those most influenced by post-modernism), because its methodology is more scientific in spirit, though of course one finds scientific attitudes in linguistics, scholarly history and so on. Geographically, the biggest change is that the mainstream is less concentrated in English-speaking countries. For example, there are world centres of formal philosophy in Amsterdam and Munich.

3) Most critics of analytic philosophy are deeply ignorant of it. Their criticisms tend to be obsolete when made, based on a stereotype of it as logical positivism. Admittedly, one does find logical positivists, some "hard", some "soft", amongst contemporary analytic philosophers, who criticize analytic metaphysics on the basis of what I regard as the most sterile aspects of Carnap's work. Incidentally, Carnap is an example of analytic philosophy's capacity to undersell itself. He was highly creative, but felt compelled by the scientific spirit to write in the most boring, monotonous style possible.

Some late analytic philosophers take deference to natural science too far, making philosophy little more than pop science. Extreme naturalists read their metaphysics off fundamental physics (or their dream of it), without recognizing how much their reductionism depends on philosophical dogma rather than the physics itself. Philosophy has its own distinctive, valuable skill-set, just as mathematics, biology, and history have. We offer most to other disciplines when we don't try to ape them.

Recently, many practitioners of analytic philosophy have been criticizing it for being insufficiently diverse – too many white males. Of course, many other disciplines face similar issues, especially the natural sciences. Philosophers like to present their discipline as more exceptional than it really is, in bad ways as well as good. Historically, language has been a major problem too: once Nazism drove most analytic philosophy out of German-speaking universities, being a native English speaker was a huge unfair advantage in analytic philosophy. Fortunately, things are changing, though more quickly in some respects than others. Predictably, some people try to exploit this movement to advance their own extraneous agenda. For instance, they suggest that there should be less emphasis on formal methods and rigorous criticism in order to make philosophy more welcoming to women. That attitude patronizes women, and isn't based on evidence. Philosophers like Ruth Barcan Marcus, Elizabeth Anscombe, Judith Jarvis Thomson, and Delia Graff Fara are at least as tough-minded as their male colleagues. A good model is semantics as a branch of lin-

guistics. Many of the most important semanticists are women – Barbara Hall Partee, Irene Heim, Angelika Kratzer – and have been leaders in introducing and applying rigorous formal methods like those of analytic philosophy. Women don't need a dumbed-down, "gentler" sort of philosophy. Similarly, it's insulting to suggest that non-whites only need some sort of philosophy-lite, 10% philosophy and 90% political polemic. It is turning out that many premodern Indian and Tibetan philosophers used methods reminiscent of analytic philosophy. Their texts can be studied and engaged with philosophically just like those of the ancient Greeks, as can those of great Islamic philosophers such as Averroes and Avicenna.

Jonathan Wolff
University of Oxford

[Prof. Wolff answered a single unified question concerning late analytic philosophy and political philosophy]. I am going to respond with reference primarily to political philosophy, and to the piece I wrote for the *Oxford Handbook of the History of Analytic Philosophy*, on "Analytic Political Philosophy". I had not thought hard about the connections between analytic philosophy and political philosophy until I was asked by Mike Beaney to write that paper. Of course one way of writing such a paper would be simply to describe the developments in political philosophy during the heyday of analytical philosophy, but I was more interested to see whether the claimed innovations of analytical philosophy had influenced that development.

My general conclusion was that it is not especially helpful to use the general category of "analytic philosophy" to describe developments in political philosophy in the twentieth century. Hence, it is not likely that a division into "early" and "late" will be much help either. My general feeling is that political philosophy, as it has emerged in the post-Rawls era, is, at least in the English-speaking world, a continuation of political philosophy as conducted by Hobbes, Locke, Hume, Kant, Bentham and Mill. This is broadly an Enlightenment project, confident in the power of abstract reasoning to defend progress, individual liberty, and some forms of equality, although it comes under pressure from the critical romanticism of Rousseau, which also leads to Marx, Hegel and British Hegelianism. In brief, my view is that, at least in the UK, at the end of the nineteenth century a form of Hegelianism had become dominant, and the rise of analytic philosophy, though presenting itself as something new, was a return to the older traditions. Rather than a radical new view, analytic political philosophy is continuous with pre-Hegelian political philosophy.

To explain in a bit more detail, in my judgement the three main features of early analytic philosophy were the emphasis on precision in definition, the use of new formal methods of argumentation, and the rejection of the doctrine of internal relations. This is the denial of the metaphysical thesis that distinct objects can stand in logically necessary relations to each other. It is a way of rejecting the forms of idealism and holism that had been promoted by British Hegelianism. Russell, and later, Ayer, clearly rejected these forms of thought, which could be associated with highly illiberal political regimes in which the individual could legitimately be sacrificed to the whole, understood as the state. Later on, and rather oddly, one site in which the struggle between holism and analytical political philosophy was played out, in my view, was in Isaiah Berlin's paper "Two Concepts of Liberty", in which the ideas of positive freedom of the British Hegelians were rejected in favour of a much more individualist account of negative liberty.

It is, therefore, possible to identify a movement in twentieth century English-speaking political philosophy as taking up one of the general themes of analytical philosophy, although it was a reaction against the previous few decades, rather than the broader history of the subject. But I mentioned three themes of analytic philosophy, of which the rejection of internal relations was only one. The other two, however, are much more problematic. Formal methods were not really used very much in political philosophy until R.B. Braithwaite's 1954 inagural lecture "The Theory of Games as A Tool for the Moral Philosopher". This, it has been said, was a result of appointing a philosopher of science to the Knightbridge Chair, then a chair in moral philosophy; an interesting experiment that could perhaps be tried more often. As I understand this lecture was Braithwaite's sole attempt to contribute to moral philosophy.

The introduction of game and decision theory went hand in hand with the revival of social contract theory, through the work of Rawls, and more notably, Gauthier. But Gauthier is interesting in this respect, in that he also initiated a tradition, followed later in a more detailed way by both Gregory Kavka and Jean Hampton, of interpreting Hobbes by use of game theory. That contemporary tools can shed considerable light on earlier texts helps demonstrate the continuity of modern and contemporary political philosophy. Amartya Sen and Jon Elster have also insightfully used formal methods to illuminate failures of rationality and problems of collective action, but my sense is that it is their conceptual insights rather than formal methods that have proven to be of lasting value.

The other main area that comes to mind where formal methods have been widely used is in relation to democratic theory, but there the first move was made by Condorcet with the introduction of the Jury Theorem in 1785, and

much work in democratic theory builds upon this result. Again we see connections much more than new beginnings.

The final element was an insistence on precise definitions. Of course given the pompous and meaningless prose of the worst of the British Hegelians, and other metaphysical philosophers of the early twentieth century, impatience is understandable. But at the same time it is the same complaint that Hobbes, in *De cive*, made of his predecessors. Therefore it is hard to see the insistence on definition as something altogether new. And my own view is that it is probably a mistake. I side with Aristotle, who argued against assuming that all areas of thought can be subjected to the same disciplines of precision, and also with Nietzsche, who claimed that no concept that has a history has a (single) definition. Hence I think too much energy has been diverted to the fruitless task of undertaking conceptual analysis and hoping for a single, compelling, answer in opposition to alternative accounts. I would not deny that conceptual analysis is important and needed, but in political philosophy at least it should be one stage in a project that should lead to substantive results, rather than an end in itself. Hence I would say that its encounter with analytic methodologies has brought mixed results for political philosophy.

David Lewis's place in the history of late analytic philosophy: his conservative and liberal methodology

Frederique Janssen-Lauret, Fraser MacBride

Abstract: In 1901 Russell had envisaged the new analytic philosophy as uniquely systematic, borrowing the methods of science and mathematics. A century later, have Russell's hopes become reality? David Lewis is often celebrated as a great systematic metaphysician, his influence proof that we live in a heyday of systematic philosophy. But, we argue, this common belief is misguided: Lewis was not a systematic philosopher, and he didn't want to be. Although some aspects of his philosophy are systematic, mainly his pluriverse of possible worlds and its many applications, that systematicity was due to the influence of his teacher Quine, who really was an heir to Russell. Drawing upon Lewis's posthumous papers and his correspondence as well as the published record, we show that Lewis's non-Quinean influences, including G.E. Moore and D.M. Armstrong, led Lewis to an anti-systematic methodology which leaves each philosopher's views and starting points to his or her own personal conscience.

Keywords: David Lewis; W.V. Quine; philosophical method; history of analytic philosophy; commonsense.

1. *Introduction: systematicity in the history of analytic philosophy*

Inspired by the revolutionary developments of mathematics and science during the nineteenth century, Bertrand Russell had "great hopes" for philosophy in the twentieth (1901: 95). The progress of philosophy, Russell diagnosed, had been stymied in the past by two methodological tendencies, bad in themselves but often in tension with one another too. On the one hand, there is the conservative tendency that philosophers of the past have often relied upon intuition and common sense. Now intuitive and commonsense judgments are subjectively certain and so difficult to doubt. But, Russell argued, this is a demerit rather than a merit of them when it comes to scrutinizing their objective credentials – even if they're mistaken, intuitive and commonsense judgments are liable to remain "irresistibly deceptive" because of their subjective certainty (1914b: 33). Russell conceived of commonsense as a theoretical relic left behind by prehistoric metaphysicians, a theory whose original justifica-

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tion is lost to us, whose longevity may be the very reason for its continuing, so very likely a dangerous combination of fallibility and subjective certainty. On the other hand, there is the individualistic tendency that philosophers of the past have often wanted to break away from their predecessors, to understand everything anew in light of an original system of their own invention. Russell held that philosophers taking this path had typically been victims of haste and ambition. But the consequence of each of them seeking to lock everything up into his or her favoured system was far worse for the discipline. Because each system had been constructed all in one block, Russell wrote, "if they were not wholly correct, they were wholly incorrect"; this stymied progress, Russell explained, because "each original philosopher has had to begin the work again from the beginning, without being able to accept anything definite from the work of his predecessors" (1914a: 110).

To achieve progress, Russell recommended applying to philosophy a broad principle of scientific method, he called it "Divide and conquer", the method of distinguishing different questions and answering them piecemeal, so that a failure in one part needn't result in the collapse of the whole and enough might thereby be saved for one generation to carry on the work of its predecessor. The scientific philosophy Russell recommended was therefore collectivist in spirit, by contrast to the individualism of traditional philosophy. Russell's high hopes for the twentieth century were for a scientific philosophy which was not only instructively informed by recent developments in mathematics and science but which was propagated by a scientific community in which co-workers would be able to draw upon the modest and patient work of each other, appealing to principles to "which, independently of temperament, all competent students must agree" (1914a: 120).

By 1917 Russell's hopes for such a scientific community had been dashed because of the tragic losses in the trenches of the First World War. Russell compared the effects upon Europe of the First World War to the effects upon Greece of the Peloponnesian War which ended its greatest age (1918: 95). But as the light of scientific philosophy dimmed in Cambridge, it grew stronger in Vienna and Carnap's *Aufbau* kept the flame alive. In the preface to the *Aufbau* Carnap credited the orientation and line of argument of his book to "a certain scientific atmosphere which is neither created nor maintained by any individual" (1928: xvi). He likened traditional philosophers to poets because of their individualistic tendency to invent entire systems in bold strokes. But the unhappy result is an end state of a multiplicity of incompatible systems. So instead of our each penning a personal philosophical system, Carnap recommended we each find our special place within the community of scientific enquiry: "If we allot to the individual in philosophical work as in the special sciences only a partial task, then we can look

with more confidence into the future: in slow careful construction insight after insight will be won. Each collaborator contributes only what he can endorse and justify before the whole body of his co-workers" (1928: xvii).

The scientific philosophy Russell and Carnap recommended was systematic in the following sense. It relied upon common starting points and shared standards of justification to make progress possible. So agreement in philosophy, by contrast to poetry, isn't a matter of temperament. Carnap began the *Aufbau* by asking the question "What is the purpose of a scientific book?", i.e. a book of philosophy of the kind he'd written (1928: xv). Carnap's answer was, "It is meant to convince the reader of the validity of the thoughts which it presents". This might sound obvious but it isn't. If a philosopher is only engaged in gilding his or her own style of conceptual poetry then agreement isn't to be sought. It's only if there are common starting points and shared standards of justification that a philosopher can reasonably expect to convince his or her readers.

Is analytic philosophy practiced today systematic in this sense? At first glance, systematic approaches to philosophy might appear to be thriving. Many philosophers in the late twentieth and early twenty first centuries were inspired by the works of David Lewis and David Lewis has been described by Mark Johnston as the "the greatest systematic philosopher since Leibniz" (Boxer 2001). But appearances are deceptive. Lewis did not share Russell's and Carnap's vision of scientific philosophy. He did not expect to convince his readers of the validity of his opinions, as he often reflected in his letters but also in his published writings. For example, in "Reduction of Mind" Lewis wrote, "Philosophical arguments are never incontrovertible – well, hardly ever. Their purpose is to help expound a position, not to coerce agreement" (1994: 304). In a similar vein, Lewis wrote to Jack Smart concerning his dispute with Putnam over reference, "You'd like a win; so would I, but I don't think there's any hope of that. (I think there almost never is in philosophy – it's too easy to force a draw.)" (Lewis to Smart, 22/8/91).

Lewis even denied that his approach to philosophy was systematic in the sense of striving for a synoptic world-view, as, for example, Sellars had done. Graham Priest, in a review of Lewis's latter three volumes of philosophical papers, surmised that "it would be wrong to think of Lewis as a systematic philosopher … Lewis works like this: he gets interested in puzzles and problems; he likes to solve them; he does so by applying his technical expertise, his great ingenuity, his prowess in the thrust, parry, and counter-thrust of philosophical debate" (2002: 352). Lewis wrote to Priest, "I applaud your paragraph 'it would be wrong to think of Lewis as a systematic philosopher' […] I also find it more than a little off-putting when industrious Germans write systematic expositions of the Lewisian system" (Lewis to Priest, 9/1/2001).

It wasn't that Lewis didn't feel the force of at least some of the considerations that Russell and Carnap had raised in favour of a systematic, scientific philosophy, at least during the earlier part of his career. Lewis appreciated that progress in science would be stymied by scientists being too open-minded about which theories have a chance of being true, because then there would be an uncontrollable torrent of scientific theories and scientists would never have the time to appreciate the merits of even one of them. And Lewis believed that progress in philosophy was in fact stymied for the reason science would be, "Philosophers tend to be too open-minded to persevere on programmes that seem to have the promise of success, and philosophy *is* a chaos of new beginnings" (Lewis to Kissling, 5/2/73; see also Lewis to Ziolkowski, 24/5/83).

It's plausible that Lewis would also have appreciated Russell's point that progress in philosophy will be stymied if a philosophical system cannot be wholly correct without being wholly incorrect. When pressed in later years to take advantage of what might appear to be supportive connections between his various views, Lewis began more and more to refuse. When Schaffer suggested in correspondence that Lewis wed his semantics to his epistemology, Lewis wrote back, "I don't want each of my views to depend on all the rest, so that readers will think they have a choice between accepting the lot and rejecting the lot" (Lewis to Schaffer, 15/12/2000). This wasn't a throw-away reflection and it recurs in Lewis's letter to Priest just mentioned, "I really don't want people thinking they have to agree with everything I say in order to agree with anything I say! […] I'm willing to present views premised on my other views if I have to, though I (increasingly) try to avoid this" (Lewis to Priest 9/1/2001).

It is tempting to speculate, on the basis of these remarks, that Lewis appreciated well enough the attractions of a scientific philosophy, no less than Russell or Carnap, only that Lewis thought scientific philosophy was not for us – that Lewis was ultimately a pessimist about progress where Russell and Carnap had been optimists. We can imagine Lewis, when pressed, reasoning as follows. Because philosophy is a chaos of new beginnings, common starting points and shared standards are lacking, and without consensus there's no remedying the situation. In the fallen state we find ourselves, the best we can do is to present our views separately rather than together, because then our readers are more likely to find something with which they can agree in what we say – remember affirming a disjunction is weaker than affirming a conjunction. Of course, Lewis, because of his avowed preference for particular puzzles and problems to philosophical panorama, might have found this reconstruction of the reasoning behind his anti-systematic outlook as off-putting as a systematic exposition of his system. But this wouldn't mean the reconstruction wasn't a good one.

Nonetheless these remarks in his letters to Priest and Schaffer are open to a subtly different reading. In his explicit reflections on philosophical method in *The Plurality of Worlds*, Lewis advocated both methodological conservatism – refusing to let philosophy question established knowledge, whether drawn from science or common sense – and methodological liberalism – leaving each philosopher to decide on the basis of his or her own personal conscience whether a philosophical theory squares with common sense and constitutes a starting point for further philosophy (1986: 134-135). So when in his later correspondence Lewis talked about wanting to keep his views independent of each other where possible, his motivation for doing so was to leave others the maximum degree of freedom to pick and choose from those views according to his or her own starting points and the exercise of their personal conscience ("I don't want to make each of my views depend on all the rest, so that readers will think they have a choice between accepting the lot and rejecting the lot!"). So, on this reading, Lewis's preference for presenting his views as disjunctions rather than conjunctions reflects his methodological liberalism. His explicit commitment to methodological liberalism shows, moreover, that Lewis wasn't just a pessimist about the possibility of progress in philosophy. If Lewis wasn't always out of sympathy with Russell and Carnap's vision of what philosophy could and ought to be, he became so. His methodological liberalism, espoused in *The Plurality of Worlds*, combined with his methodological conservatism gives rise to the chaos of new beginnings that Russell and Carnap had sought to circumvent with their scientific approach to philosophy – because each philosopher will have his or her own starting points depending upon the exercise of his or her personal conscience.

In this paper we will explore some of the different respects in which Lewis's philosophy may be assessed as systematic or not, drawing upon his *Nachlass* as well as his published writings. In section 2 we investigate certain elements of Lewis's philosophy that really do suggest a scientific philosophy in Russell and Carnap's sense, principally his possible worlds ontology, most of his meta-ontological views, and his Humeanism. Not coincidentally they are the ones related to the methods and questions he inherited from his teacher W.V. Quine, heir to Russell and Carnap. But, in section 3, we consider Lewis's various remarks on philosophical method, inspired by D.M. Armstrong and his Moorean approach to philosophy, which lead Lewis away from scientific philosophy. We argue that Lewis's methodological conservatism and liberalism are severally and jointly problematic. The result is an interpretation of Lewis's methodology that combines elements of the scientist and the poet, as Russell and Carnap described them, but not, we think, in a good way.

2. *Systematic aspects of Lewis's system: the influence of Quine*

It was Lewis's doctrine of the plurality of worlds, his extreme modal realism according to which other possible worlds are as real and concrete as our own, which made a name for him as a systematic philosopher on a par with Leibniz. And Lewis himself was partly responsible for creating this impression. In the introduction to the first volume of his *Philosophical Papers*, Lewis wrote, he would have liked to be an "unsystematic philosopher, offering independent proposals to a variety of topics. It was not to be. I succumbed too often to the temptation to presuppose my views on one topic when writing on another" (1983: ix). At the top of the list of recurring themes that had frustrated his efforts to remain unsystematic, Lewis put "Extreme Modal Realism".

Lewis's adherence to extreme modal realism also made a name for him as a revolutionary metaphysician. This marked, in popular conception at least, an advance Lewis had made upon his teacher Quine, because according to popular conception, Quine's philosophy had been inimical to metaphysics. But this popular conception was and is a misconception, unfortunately even more wide-spread now. Lewis took Armstrong to task for being a victim of it, writing to Armstrong, "I don't see Quine as part of a climate altogether hostile to systematic metaphysics. In fact, I see Quine as himself a systematic metaphysician … When I took and failed my metaphysics exam as a Harvard graduate student in 1963, it was mostly Quine I'd studied in preparation. Certainly that was too narrow a plan of study. But I don't think I was studying the wrong subject altogether!" (Lewis to Armstrong 28/10/94). Armstrong also held the now popular view that D.C. Williams had performed an important role keeping metaphysics alive whilst the star of Quine was in the ascendency. But Lewis poured cold water on this too, remarking that Quine was a metaphysician with "a system in some respects allied, in some respects opposed to Williams".

These misconceptions about Quine (and D.C. Williams) have resulted in misconceptions about the place of Lewis in the history of analytic philosophy. So we devote this section to explaining how Lewis's extreme modal realism, the doctrine with the most systematic significance for Lewis's philosophy, emerged from Quine's more austere metaphysics.

Lewis began his journey towards extreme modal realism when he engaged with Quine's system as a PhD student. Quine had taken Russell's conception of scientific philosophy to heart. Yet Quine's approach could not be Russell's approach because of Quine's epistemological holism. In putting forward his conception of scientific philosophy, Russell had distinguished between the results of science and the piecemeal method of science. Russell counseled against importing the latest a posteriori results of science into philosophy, because

invariably the latest results are subsequently revised. But even though the latest results aren't to be trusted, Russell still argued that philosophy needed to take over the piecemeal method of science to achieve progress. The piecemeal method when carried over to philosophy would, Russell envisaged, involve the a priori analysis and enumeration of logical forms (1914a: 109-110). Quine's adherence to epistemological holism meant that he could not endorse Russell's distinction between the a posteriori activity of science and the a priori activity of philosophy because ultimately science and philosophy face the tribunal of experience *en bloc* (1951: 39). But this didn't mean the demise of scientific philosophy for Quine: science and philosophy now belonged to a seamless fabric of total theory whilst it remained the business of philosophers to analyse and enumerate the logical forms of scientific theories.

From Quine's earliest publications on ontological commitment, he stressed that one of the key functions of philosophy is to investigate the underlying logical forms of scientific theories. He noted that "factual questions of zoology and medicine" (Quine 1939: 704) may lead us to believe in certain entities (like diseases) or refuse to believe in alleged entities (like winged horses), but that in the absence of a rigorous treatment of the language of sciences like zoology and medicine, our grounds for doing so are difficult to state or assess properly. We cannot infer much from the use of individual words, even in factual statements, since not all words are names of objects or pronouns which refer to objects. Rather than look to the alleged referents of individual words within factual statements, we should take a broader view and ask questions about the proposed language forms, taken together. Are they up to the explanatory tasks science sets for us? What array of entities do they invoke, and can that collection of entities plausibly do the explanatory job required of them? Is there any hope, for instance, for the "nominalist [who] claims that a language adequate to all scientific purposes can be framed in such a way that its variables admit only concrete objects, individuals, as values" (Quine 1939: 708)?

In subsequent work on ontological commitment, Quine argued that concentrating on logical forms helps us sidestep key fallacies which had hitherto foiled efforts at systematic, scientific metaphysics. Such fallacies include ascribing existence as an idea, different manners of existing, existence in different metaphorical realms or worlds, or existence in space-time vs. subsistence or mere being outside it – all in order to avoid ascribing non-existence. Quine diagnosed the flaw in the argument as a confusion between terms which are used to designate and terms used otherwise. We easily succumb to fallacious reasoning about existence if we assume words in non-existence claims, e.g. 'Pegasus', are used to stand for something, or that they must stand for something in order to be meaningful. In that case "does not exist" would have to

be a predicate ascribed to a being doing something other than existing in the ordinary way. Such fallacies are dispelled once we realise the true logical form of non-existence claims: they say that absolutely nothing there is is the referent of 'Pegasus', or, better still, that nothing meets the condition of being Pegasus. Logical form is also key in ascribing existence claims to others which we do not want to endorse. "It is important to be able to say 'The Greeks affirmed that Pegasus exists', or 'The Greek myths imply that there is such a thing as Pegasus'", Quine asserted (1944: 160), but "use of the word 'Pegasus' does not imply acceptance of Pegasus … It is not the mere use of a substantive, but its designative use, that commits us to the acceptance of an object designated by the substantive" (1944: 165). These substantives, in their turn, are explained away in terms of definite descriptions.

Key to the systematic, scientific metaphysics Quine envisaged was an appreciation of the significance of ontological idioms. He looked to logic to provide a semantics of these idioms, i.e. the vocabulary of existence and identity – such as 'exists', 'there is', 'is identical to'. He also looked to logic to tell us under what circumstances some collection of statements implies a statement with an existentially quantified logical form. According to Quine we need to look to the science to which the existential statement belongs in order to settle whether the statement is true. "The question whether 'Pegasus' designates, for example, is a question of natural science" (Quine 194: 167). Logical vocabulary and logical form help pinpoint where an ontological assumption, an assumption about what there is, is made: via the use of a variable in an existentially quantified context. Predicates occurring in descriptions, the ideology of a theory, express what the theory in question claims to be true of the ontology. These claims are assessed for truth according to the standards of the science to which the theory belongs. Alternative scientific, mathematical, and philosophical theories can be meaningfully compared with respect to "the explanatory value of … entities" they posit (Quine 1948: 31). We have reason to prefer theories which are both explanatorily fruitful and ontologically parsimonious: theories which explain more with less. Comparing theories does not imply accepting or referring to everything in their ontologies ourselves, because, Quine argued, we can ascend to the meta-language. To do so, Quine recommended first rendering theories in the logical form of first-order logic and closing them under first-order entailment. Then we can excerpt all of the existentially quantified claims made by the theory, and ascribe ontologies to others by putting their existential claims in quotation marks. Parties to an ontological dispute can then coherently differ over what there is by speaking of linguistic expressions – which they all believe in – and expressing contrary views about which existentially quantified statements are true.

Quine deemed physics to have an especial place amongst the sciences because he thought there was no change without a physical change. Because of his physicalism and his approach to ontological commitment, Quine recommended an austere metaphysics which disavowed modal ontology. Quine had been strongly anti-modal in his early career, regarding all modal discourse as mired in use-mention confusions and committed to an implausibly rigid division of an object's attributes into the accidental and the essential. "Meaning is what essence becomes when it is divorced from the object of reference and wedded to the word" (Quine 1951b: 22). He also objected that little sense could be made of a quantified logic of modality. According to Quine its characteristic posits, whether individual concepts or possibilia, were incompatible with physicalism (Quine 1947: 47) and in any case did not have well-delineated criteria of identity (Quine 1948: 23). After Ruth Barcan Marcus had proposed an exemplarily clear quantified modal logic without essentialist or non-physical commitments (Barcan 1947), Quine found himself constantly on the back foot when debating modal logic with her, and could no longer maintain his blanket opposition to modal language (Janssen-Lauret 2015: 161). By the time Lewis came to Harvard as a postgraduate student in 1962-63, Quine had begun to take a more permissive line on modal discourse. Quine advocated a Hume-inspired analysis of necessity, taking all forms of necessity, including logical and mathematical necessity, to be analysable in terms of regularity (Quine 1976 [1963]: 70). Quine was also Humean in another respect, namely abjuring necessary connections between matters of fact. Though a little more tolerant of modal language understood in a Humean spirit, Quine had not softened his stance on modal ontology. Ontological questions were still to be settled in non-modal first-order logic. And Quine continued to maintain that modal ontology was both incompatible with physicalism and resisted being given clear criteria of identity in a first-order non-modal language.

From the start, Lewis agreed with Quine's approach to ontological commitment, to his physicalism and his Humean suspicion of necessary connections (Janssen-Lauret 2017: 258-259). But whilst metaphysics was a significant side interest for Lewis as a PhD student, it had been no more than a side interest (Lewis to Quine, 21/5/65). Lewis's main focus in his PhD, revised and later published as Lewis 1969, had been on natural-language semantics. He had aimed to justify linguistic conventions while avoiding the strong objections Quine had brought against the conventional truth of analytic sentences. To pull this off, Lewis drew upon the latest developments in game theory and linguistics as well as upon philosophy. As he took up his first job at UCLA in 1966, he continued to work on semantics and had fruitful discussions on the topic with Richard Montague, Barbara Hall Partee, Hans Kamp, and David

Kaplan. As a consequence, Lewis became more and more intrigued by the idea of explaining natural-language modals by appealing to possible worlds. But his Quinean conscience pulled him in two directions. On the one hand, Lewis was drawn to posit possible worlds for the reasons Quine had recommended for positing things in general, i.e. because of their explanatory value. On the other hand, he found them difficult to justify, struggling to reconcile modal posits with his other commitments. By the standards of his teacher, possible worlds seemed ontologically profligate and physicalistically suspect, with murky criteria of identity.

Lewis worried especially over what he called "inter-world identity" (Lewis to Quine, 1 October 1968). More sophisticated than the old Quinean quip about fat and thin possible men in the doorway, inter-world identity was the problem of the alleged identity between actual and possible individuals. Is actual Queen Elizabeth, the English monarch, for instance, identical to possible Princess Elizabeth, who might have lived a life of relative quiet as a minor royal if her uncle Edward VIII had never abdicated? On the usual possible-world interpretation of modal logic, "Elizabeth might not have been Queen" would be formalised as '$\Diamond\neg Qe$', with the diamond symbolising the possible world, represented in the model by an alternative domain of discourse, different from that of the actual world. No criteria of identity are provided for checking whether the domains overlap, whether the individual assigned the name '*e*' in one 'world'-domain is identical to the individual assigned the name '*e*' in another. Quine's ontological commitment required a single domain of discourse, in order to be able to formulate questions of identity. To formulate an identity statement, it must be grammatical to put names or variables referring to the entities in question on both sides of the identity predicate. But modal operators, the box and the diamond, are always prefixed to a well-formed formula which is already true or false in some domain ('world') or other. So identity statements between entities drawn from different domains are not well-formed, even though this is easily overlooked because the inhabitants of different domains are sometimes presented under name-tokens of the same type, like '*e*'. We cannot formulate criteria for identity between actual and possible objects in a modal logic with primitive box and diamond so interpreted.

Over time Lewis developed answers to all these Quinean objections. First he solved the problem of inter-world identity (Lewis 1968). He dispensed with the box and diamond as primitive world-quantifiers altogether, and collapsed all domains of discourse into one big first-order domain of discourse. Lewis allowed ordinary first-order quantifiers to range over possible worlds in exactly the same way as they range over individuals. He construed worlds as very large individuals, and the individuals inhabiting worlds as mereological parts

of those worlds. Actual Queen Elizabeth and possible Princess Elizabeth are parts of distinct, non-overlapping worlds. As a result we have similar criteria of identity for actual and possible individuals and a clear answer to the question of inter-world identity: individuals existing in distinct possible worlds are always distinct. Discussion of what Princess Elizabeth might have done had she not ascended to the throne sounds like counterfactual discourse about the actual queen, but it is not. It is about counterpart-Elizabeth, the person living in some possible world where a counterpart of Edward VIII remained king who has most in common with the woman who became queen in the actual world.

Second, Lewis began to build his case that our best theories of language and linguistics imply the existence of possible worlds, and that despite appearances such worlds do not offend against ontological parsimony. Ordinary-language modals appear to state or imply quantification over possible worlds, and, he claimed, theories that don't take ordinary language modals at face value are less explanatory than theories that do. So, for example, Lewis dismissed theories which purport to reduce possible worlds to maximally consistent sets of sentences. He argued these theories are circular because consistency is itself a modal notion (Lewis 1973: 85). Later on, he would defend positing possible worlds in even more Quinean terms. He claimed that they avoid obscurantism in ontology, such as ascribing to possibilia a different "manner of existing" (1986a: 2-3), and that, like set theory, his extreme modal realism "offers an improvement in what Quine calls ideology, paid for in the coin of ontology [...] the benefits in theoretical unity and economy are well worth the entities" (1986a: 4).

Third, Lewis argued that possible worlds are acceptable from an ideological point of view. After Lewis had proposed first-order non-modal quantification over worlds in 1968, Quine stopped calling possible worlds incomprehensible. In that same year Quine tentatively admitted that it was coherent to believe in possible worlds in the sense of alternative distributions of matter over spacetime (Quine 1968), although, unlike Lewis, he did not believe that there was any straightforward path from ordinary-language sentences to those alternative cosmic distributions of particles. But, said Lewis, those alternative spatiotemporal universes and their material parts were just what he thought possible worlds were. His worlds were not mathematical models, sets, concepts, or abstracta, but simply more of the sorts of things we already believe in. Numerous as they are, they are nevertheless parsimonious from a qualitative point of view: they introduce no unfamiliar new kinds of things. What's more, Lewis put his possible worlds to work doing something Quine generally approved of: providing reductive explanations of abstract or mentalistic posits which are difficult to reconcile with physicalism. Lewis proposed that propositions could be interpreted as sets of possible worlds (1986a: 53-55), properties as sets of ac-

tual and possible instances (1986a: 50-52), and that set theory can be accounted for in terms of mereology and plural quantification (1993).

Finally, Lewis eschewed necessary connections, in keeping with the Humeanism he had inherited from Quine. For Lewis, any *possibile* is supposed to be able to co-exist with any other. "We can take apart the distinct elements of a possibility and rearrange them. We can remove some of them altogether. We can reduplicate some or all of them. We can replace an element of one possibility with an element of another. When we do, since there is no necessary connection between distinct existences, the result will itself be a possibility" (Lewis 2009: 208-209).

All in all, Lewis's doctrine of the plurality of worlds, his extreme modal realism, can be clearly seen to have arisen within a tradition of scientific philosophy learned from Quine leading back to Russell, reflecting systematic features of Quine's metaphysics – his approach to ontological commitment, choosing posits for their explanatory value and so forth. Nonetheless, we will argue in the next section, Lewis was a far less systematic philosopher than Quine, further from Quine than Quine was from Russell and Carnap in this regard. This was in part because of another important influence upon him, D.M. Armstrong.

3. *Unsystematic aspects of Lewis's system: the influence of Armstrong*

In Lewis's first published paper, "An Argument for the Identity Theory", he had set out to refute what he believed to be the "dualism of the common man" in favour of a version of the mind-brain identity theory (1966: 25). But seven years later, in *Counterfactuals*, so far from dismissing common persons' opinions in philosophy, he began to assign them positive weight. "One comes to philosophy already endowed with a stock of opinions. It is not the business of philosophy either to undermine or to justify these pre-existing opinions, to any great extent, but only to try to discover ways of expanding them into an orderly system" (Lewis 1973: 88). Lewis held that before we undertake philosophical training we have a pre-existing commitment to alternative ways the world might have been; he found evidence for this claim in what we ordinarily say. Beyond this Lewis did not specify what other pre-existing opinions he had in mind. He did not answer the question whether what he had previously described as the "common" person's dualism might be such a pre-existing opinion which another philosopher could legitimately build into his or her own orderly system.

In "Radical Interpretation", which appeared the following year, Lewis argued that folk psychological platitudes count amongst the pre-existing

opinions of which philosophy must take account. "The concepts of belief, desire, and meaning are common property. The theory that implicitly defines them had better be common property too. It must amount to nothing more than a mass of platitudes of common sense" (1974: 335), specifically, "our common-sense theory of persons" (1974: 337). Thus far there was no overt inconsistency between Lewis's commitments and Quine's philosophy. This was because Lewis thought that the relevant pre-existing opinions about persons could be worked up into the science of decision theory: "Decision theory [...] is the very core of our common-sense theory of persons, dissected out and elegantly systematized" (1974: 338). The influence which made Lewis invoke common sense to contradict Quine came later, and it came from Australia via the influence of Armstrong.

Lewis and Armstrong first met in 1968 when Armstrong visited Stanford, while Lewis worked at UCLA (S. Lewis 2015: 12). But it was only when Lewis visited Australia in the summer of 1976 that they struck up a close intellectual friendship. Afterwards, they corresponded regularly. Armstrong had for some time advocated, based on his reading of G.E. Moore, the overthrow of philosophical theories if they conflict with entrenched common-sense existential claims: "it would be rational to accept the existence of the physical world and of time, rather than the philosophical arguments, *even if we cannot see what is wrong with the arguments*" (Armstrong 1968: 51, his italics). A few months after his 1976 trip to Australia we see Lewis, too, making arguments in the same vein. By this point Lewis explicitly invoked Moore's methodology and explicitly contradicted Quine's. Peter Unger had argued against ontological commitment to ordinary objects – like the swizzlestick in his cocktail – on the grounds that a sorites-style argument shows that they do not begin or end anywhere. Lewis wrote to Unger, "you say [...] that it's possible for common sense to mislead (I agree) and that a Moorean response is 'extremely dogmatic' (why should I mind?). I think the crude stuff from Moore is better than the fancy stuff from Quine; it's more certain that there are swizzlesticks than that there are no false steps in the sorites, but it's *not* more certain that the fundamental principles of Quine's epistemology are right than that there are no false steps in the sorites" (Lewis to Unger, 1/11/1976, his italics).

Ten years later, appeals to common sense had become an integral component of Lewis's reflections on philosophical method. In *The Plurality of Worlds*, he asseverated, "theoretical conservatism is the only sensible policy [...] [p]art of this conservatism is reluctance to accept theories that fly in the face of common sense" (Lewis 1986a: 135). He admonished the reader to "never put forward a philosophical theory that you yourself cannot believe in your least philosophical and most commonsensical moments" (Lewis 1986a: 135). By

this point he had strayed very far from the standards of Quine, who viewed common sense as deserving of some respect, but only insofar as it is a kind of proto-science (Quine 1957: 2). In the end it is not common sense by itself, but fully-fledged, grown-up science which, according to Quine, ought to constrain philosophical theorizing.

Lewis must have liked the sound of the doctrine of methodological conservatism. He continued to describe himself as a conservative in correspondence: "I am philosophically conservative: I think philosophy cannot credibly challenge either the positive convictions of common sense or the established theses of the natural sciences and mathematics" (Lewis to Pyke, 27/7/90). He repeated that claim almost verbatim in an unpublished paper the following year (Lewis 1991b: 2). This conservatism, this wanting to hold on to our hard-won established knowledge, scientific and mathematical knowledge as well as common sense, has the ring of a sensible, systematic philosophical methodology. But is it?

No, we argue; methodological conservatism can only serve as a fruitful prescription for us philosophers if science, mathematics and common sense cohere together and we understand them. But we cannot take either their coherence or our interpretation of them or indeed their standing for granted. So there is no guarantee that an orderly system will result from hanging onto what we take to be established knowledge.

One case where Lewis appears to be mistaken about the interpretation of established knowledge is the following. According to Lewis, mathematics is up to its ears in set theory. Applying his methodological conservatism in *Parts of Classes*, Lewis concluded that it would always be more rational to accept mathematics than any philosophical argument against the existence of classes. "Renouncing classes means rejecting mathematics. That will not do. Mathematics is an established, going concern. Philosophy is as shaky as can be" (Lewis 1991a: 58). To make his point vivid, Lewis imagined how absurd it would be for a philosopher to go down the hallway to the Department of Mathematics and try "telling the mathematicians that they must change their ways" (59). Using the second person he asks us to exercise our personal conscience; "How would you like the job […] Can you tell them, with a straight face, to follow philosophical argument wherever it may lead?" (*ibid.*).

As Lewis appeared to be making an empirical claim about what would happen if philosophers went down the hallway, we tested it by going down the hallway ourselves – to the Department of Mathematics at the University of Manchester, where we work. We found the distinguished mathematician and Fellow of the British Academy, Jeffrey Paris and told him classes don't exist. He replied he was a formalist, so questions about ontology didn't really make

sense to him because the real business of mathematics concerns what follows from what on such-and-such assumptions. So, contra Lewis, Paris didn't think anything would change for him as a working mathematician if classes didn't exist. It's our experience, and Paris agreed, that there is a great variety of opinions about mathematical ontology amongst working mathematicians – some take ontology seriously but many are formalists, some are fictionalists and so on. It can hardly then be said to be part of the established understanding of mathematical practice that classes exist. So Lewis misconstrued the character of the established knowledge about which we should be conservative. He should have gone down the hallway and seen what happened when he tried to tell working mathematicians that classes really exist and their existing was essential to the practice of mathematics.

There is a further worry about how established a branch of science needs to be before philosophers are methodologically compelled to be conservative and go along with it. Lewis appreciated that quantum physics looks to be committed to unlocalised, physical entities, because of Bell's Theorem, a commitment which conflicts with his philosophical adherence to Humean Supervenience, the doctrine that all there is to the world is a vast mosaic of local matters of particular fact. But Lewis did not defer to quantum physics on the grounds that philosophy is "as shaky as can be" – as we might expect Lewis to do because he did defer to mathematics on such grounds. Instead Lewis said, "I am not ready to take lessons in ontology from quantum physics as it now is", maintaining we should wait until "it is purified of supernatural tales about the power of the observant mind to make things jump" (1986b: xi). Lewis did not merely say that philosophers should wait until the interpretation of quantum physics is settled before they take it as established knowledge. He made the stronger claim that we shouldn't take quantum physics as established knowledge. Lewis's justification for this claim was that quantum physics currently relies upon "the power of the observant mind" – although no practicing physicist would think that the observant mind has the power to make things jump. But, irrespective of details, Lewis's justification looks like putting philosophy ahead of science when even a less established or profoundly successful branch of science as quantum physics is a long way off being "as shaky as can be"; it is still more established than philosophy. Perhaps Lewis should have gone down the hallway to the Physics Department as well. We tried this one too and Stephen Barnett, a distinguished physicist and Fellow of the Royal Society, at the University of Glasgow, replied by denying that the observant mind has any power in the matter of quantum phenomena and reflected that physicists have learnt to bend when the observed facts fit a theory that is not in accord with their own preconceptions.

Common sense poses even more of a challenge to Lewis's methodological conservatism. It is often unclear what counts as a platitude of common sense. And even if in some cases a platitude can be identified, it is often unclear what the platitude means or implies. It's then a further matter of unclarity how that platitude should be weighed against other common sense platitudes as well as other mathematical, scientific and philosophical commitments. Remember Lewis's methodological prescription for philosophers: "never put forward a philosophical theory that you yourself cannot believe in your least philosophical and most commonsensical moments" (1986: 135). That's fine if common sense constitutes a stable body of opinion that's internally coherent and doesn't conflict with science or mathematics and we understand it. But we can't take any of these things for granted (MacBride and Janssen-Lauret 2015). Just think of the all too familiar difficulties we get if we try to unite the claims of science and common sense when they appear to conflict. Eddington ended up with two tables: one, recognized by science, a swarm of particles, another, recognised from the point of view of common sense, a solid thing (1928: xi). Methodological conservatism doesn't make for systematic philosophy unless what we think we know fits together – but we know that science and common sense don't always fit together. It's also questionable whether our opinions can be quantitatively classified as more or less philosophical or commonsensical, such that they can be arranged along a single continuum with a least philosophical and most commonsensical opinion at one end.

Thinking in such one-dimensional terms also conflicts with G.E. Moore's common sense approach to philosophy, the approach that had originally inspired Armstrong. In "Defence of Common Sense" Moore had said that the truth of certain common sense judgements is certain, namely judgements all ordinary English speakers agree upon – with the possible exception of the odd philosopher. But Moore emphasised we cannot infer from the certainty of such a judgement what its correct analysis is (1925: 9). So, for Moore, common sense judgments and philosophical analyses belong to different levels, rather than lying side to side upon a continuum. The judgement is common sense but its analysis isn't – the analysis is philosophical. We absolutely have to hold onto the common sense judgement but we can legitimately differ over the philosophical analysis. Susan Stebbing, who developed her own but more authentically Moorean approach to philosophical analysis, went further (Janssen-Lauret 2017a). Stebbing wrote, "Nothing but confusion can result if, in one and the same sentence, we mix up language used appropriately for the furniture of the earth and our daily dealings with it with language used for the purpose of philosophical and scientific discussion" (Stebbing 1937: 42). So both Moore and Stebbing would have been sceptical of Lewis's claims that we

can hold intelligible opinions that mix common sense and philosophy, whether to a greater or lesser degree, or that we can arrange opinions along a spectrum from philosophical to commonsensical because really they operate at different levels (Stebbing 1929: 152).

Lewis conceived folk psychology to be a paradigm example of established common sense knowledge and so something which philosophers are methodologically compelled to take seriously. But does folk psychology tell us anything? If it does, what does folk psychology tell us? Lewis was deeply impressed by the fact that we are very often able to predict the behaviour of one another in folk psychological terms, i.e. in terms of beliefs and desires, and that homo sapiens has been successfully doing so for millennia. Lewis's favoured explanation of this remarkable fact was that folk psychology is a more or less accurate description of the inner causal mechanisms in human brains that give rise to their outward behaviour: the theory tells us how mental states are apt to cause behaviour and how mental states are apt to change under the impact of perceptual stimuli and other mental states, so associating with each state a causal role, albeit usually one that can only be understood in terms of a network of such roles (1994: 298-299). Lewis went further and made the even stronger claim that folk psychology is a theory whose extraordinary success depends upon its having accurately described the causal roles of mental states in purely non-mental terms, i.e. physical terms.

But how compelling is this account *qua* description of what is supposed to be common knowledge among us? If our capacity to successfully predict behaviour is essentially a practical skill, a case of know-how rather than know-that, then folk psychology shouldn't be classified as a theory held by us at all – anymore than the (extraordinary) skill of balancing on two legs should be. And how psychologically realistic is it anyway that we have knowledge of the causal roles of mental states conceived in physical terms and that we all rely upon this knowledge when we make predictions about one another?

In "Psychophysical and Theoretical Identification" Lewis obviously didn't feel the force of these worries. He simply wrote "Collect all the platitudes you can think of regarding the causal relationships between mental states, sensory stimuli, and motor responses… Perhaps there are platitudes of other forms as well. Include only platitudes which are common knowledge among us – everyone knows them, everyone knows that everyone else knows them, and so on" (1972: 256). According to Lewis, the resulting assemblage of platitudes implicitly defines the meanings of the names we use for mental states. But Lewis didn't actually assemble enough platitudes about mental states to make it credible that by putting them together the result would be a causal theory fit for the purpose of predicting human behaviour. In fact he only mentioned one platitude,

toothache is a kind of pain. This doesn't seem to have been just an oversight because there appear to be very few platitudes about the mind that everyone knows and that everyone knows that everyone else knows etc. By the time he wrote "Reduction of Mind" Lewis had taken this particular worry to heart. He no longer required the platitudes of folk psychology to be common knowledge in the exacting sense of being known and being known to be known, because, he wrote, "we cannot expound these principles systematically" (1994: 298). Instead, Lewis maintained, our knowledge of folk psychology "is tacit, as our grammatical knowledge is". But if what is supposed to be common knowledge is tacit and we cannot expound our knowledge of folk psychology in a systematic and explicit fashion, then we cannot know that folk psychology is a causal theory which describes the causal roles of mental states in purely physical terms. For all we know and are able to expound, it may be the case that folk psychology often describes the causal roles of mental states in mental terms or doesn't describe them as occupants of causal roles at all. It may be that folk psychology is itself irreducibly dualist, a suspicion that perhaps Lewis ought to have entertained when he wrote in his first paper, "The dualism of the common man holds that experiences are nonphysical phenomena which are the causes of a familiar syndrome of physical as well as non-physical effects" (1966: 25).

Conservatism makes sense as a methodology for us if we know what to be conservative about. But if the principles behind folk psychology are hidden from us then we can hardly be conservative about them. This reflects the more general point that common sense is ill-suited to serve as the unmovable point Archimedes sought – because common sense is difficult to pin down or interpret and because it comes into conflict with science and mathematics and philosophy. Lewis combined methodological conservatism with methodological liberalism, inviting "you", the reader, to decide whether a philosophical theory squares with common sense. Lewis left the decision to the personal conscience of his readers because he realised it isn't a matter to be determined mechanically whether a philosophical theory squares with common sense. To decide whether a theory does square with common sense requires an exercise of judgement. Lewis granted both that "Sometimes common sense may properly be corrected" if the theoretical benefits outweigh the costs but also that "a theory cannot earn credence just by its unity and economy" (1986a: 134). The "inherited credence" of pre-established opinion is pitted against unity and economy of theory and the "proper test" for determining whether we have balanced the costs and benefits of a theory is for each of us to use "a simple maxim of honesty": never to put forward a theory unless you can believe it in the very moment when you've done your best to put philosophy aside and embrace common sense (1986: 135).

One problem with Lewis's maxim is that it is difficult to ever apply it: when we inhabit our least philosophical moments, our philosophical ideas are no longer clearly and distinctly before us, so we're no longer in a position to evaluate whether our philosophical theory fits with common sense. Famously Lewis applied the maxim to himself, deciding even in his least philosophical moment that the benefits of extreme modal realism outweighed the costs of offending the common sense opinion that donkeys don't talk. But applying the maxim of honesty to themselves nearly all of his readers felt differently. Without more guidance we are left adrift because aside from temperament we don't know how to balance prior commitments with new ideas. The only common standard in play for Lewis is to be true to yourself – at least when deciding whether a theory squares with common sense. But this is just the kind of methodological individualism Russell and Carnap had bemoaned. It is an individualism which leads to a continual chaos of new beginnings because different philosophers get different results when they apply Lewis's maxim of honesty. We are left with the very chaos of new beginnings Lewis once lamented as the reason philosophy fails to progress.

The upshot is that Lewis's methodology became a heady but unstable mix of conservatism and liberalism. Lewis felt the pull of established opinion but was a rugged individualist too. But what's needed for progress is a more patient and piecemeal approach which can only be undertaken collectively because of the extraordinary epistemological challenges that now face us as a species – the challenges of simultaneously comprehending contemporary science, mathematics and common sense, settling what they really mean and figuring out whether they fit together and if they do how they do. This isn't a task to be undertaken by one philosopher working as an individual, even keeping honest. In his early career, Lewis had performed the kind of collaborative role of which Russell and Carnap would have thoroughly approved, bringing together philosophers and linguistics in the late 60s and early 70s (Lewis to Partee, 12/11/69; Partee 2015). Russell and Carnap would not have been surprised either that some of Lewis's most influential contributions (including "General semantics" and "Adverbs of Quantification") come from this period, drawing upon both formal semantics and generative grammar. But the sciences and mathematics have become more and more specialized and more and more difficult for philosophers to understand and integrate. If philosophy is not to degenerate into conceptual poetry, each of us needs to find a place in a community of enquirers so that enough will abide that the next generation can continue the work.

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Frederique Janssen-Lauret
frederique.janssen-lauret@manchester.ac.uk
University of Manchester

Fraser MacBride
fraser.macbride@manchester.ac.uk
University of Manchester

References

Armstrong, David, 1968, *A Materialist Theory of Mind*, Routledge and Kegan Paul, London.

Barcan, Ruth C., 1947, "The Identity of Individuals in a Strict Functional Calculus of Second Order", in *Journal of Symbolic Logic*, 12 (1): 12-15.

Carnap, Rudolf, 1928, *Der Logische Aufbau der Welt*, Weltkreis, Berlin; Eng. tr. *The Logical Structure of the World*, Carus Publishing Company, Peru IL, 2003.

Carnielli, Walter, Frederique Janssen-Lauret, William Pickering, eds. and tr., 2018, *The Significance of the New Logic: A Translation of Quine's O Sentido da Nova Logica*, with an accompanying article by F. Janssen-Lauret, "W.V. Quine's Philosophical Development in the 1930s and '40s", Cambridge University Press, Cambridge.

Eddington, Arthur, 1928, *The Nature of the Physical World*, Cambridge University Press, Cambridge.

Janssen-Lauret, Frederique, 2017a, "Susan Stebbing, Incomplete Symbols, and Foundherentist Meta-Ontology", in *Journal for the History of Analytical Philosophy*, 5(2): 6-17.

Janssen-Lauret, Frederique, 2017b, "The Quinean Roots of Lewis's Humeanism", in *The Monist*, 100 (2): 249-65.

Lewis, David, 1966, "An Argument for the Identity Theory", in *Journal of Philosophy*, 63: 17-25.

Lewis, David, 1968, "Counterpart Theory and Quantified Modal Logic", in *Journal of Philosophy*, 65: 113-126.

Lewis, David, 1969, *Convention: A Philosophical Study*, Harvard University Press, Cambridge MA.

Lewis, David, 1970, "General Semantics", in *Synthese*, 22: 18-67.

Lewis, David, 1972, "Psychophysical and theoretical identifications", in *Australasian Journal of Philosophy*, 50: 249-258.

Lewis, David, 1973, *Counterfactuals*, Blackwell, Oxford.

Lewis, David, 1974, "Radical Interpretation", in *Synthese*, 27: 331-344.

Lewis, David, 1975, "Adverbs of Quantification", in E.L. Keenan, ed., *Formal Semantics in Natural Language*, Cambridge University Press, Cambridge: 3-15.

Lewis, David, 1983, *Philosophical Papers*, vol. I, Oxford University Press, Oxford.

Lewis, David, 1986a, *On The Plurality of Worlds*, Oxford University Press, Oxford.

Lewis, David, 1986b, *Philosophical Papers*, vol. II, Oxford University Press, Oxford.

Lewis, David, 1991a, *Parts of Classes*, Blackwell, Oxford.

Lewis, David, 1991b, "Acceptance Speech for the Behrman Award", in *David Lewis Papers*, Firestone Library, Princeton University.

Lewis, David, 1994, "Reduction of Mind", in Samuel Guttenplan, ed., *A Companion to Philosophy of Mind*, Blackwell, Oxford; reprinted in his *Papers in Metaphysics and Epistemology*, Cambridge University Press, Cambridge: 291-324.

Lewis, David, 2009, "Ramseyan Humility", in David Braddon-Mitchell and Robert Nola, eds., *Conceptual Analysis and Philosophical Naturalism*, MIT Press, Cambridge MA: 203-222.

Lewis, Stephanie, 2015, "Intellectual Biography of David Lewis (1941-2001): Early Influences", in Loewer and Schaffer, 2015: 3-14.

Loewer, Barry, Jonathan Schaffer, eds., 2015, *A Companion to David Lewis*, Wiley Blackwell, Chichester.

MacBride, Fraser, Frederique Janssen-Lauret, 2015, "Meta-Ontology, Epistemology and Essence: On the Empirical Deduction of the Categories", in *The Monist*, 98 (3): 290-302.

Moore, George Edward, 1925, "A Defence of Common Sense", in J.H. Moorhead, ed., *Contemporary British Philosophy* (2nd Series), George Allen and Unwin, London: 193-223.

Partee, Barbara, 2015, "Asking What a Meaning Does: David Lewis's Contributions to Semantics", in Loewer and Schaffer, 2015: 328-344.

Quine, Willard Van Orman, 1939, "Designation and Existence", in *Journal of Philosophy*, 36 (26): 701-709.

Quine, Willard Van Orman, 1944, *O Sentido da Nova Logica*, Martins, Sao Paulo, translated in Carnielli, Janssen-Lauret, and Pickering 2018.

Quine, Willard Van Orman, 1947, "The Problem of Interpreting Modal Logic", in *Journal of Symbolic Logic*, 12(2): 43-48.

Quine, Willard Van Orman, 1948, "On What There Is", in *Review of Metaphysics*, 2(5): 21-38.

Quine, Willard Van Orman, 1951, "Two Dogmas of Empiricism", in *Philosophical Review*, 60: 20-43.

Quine, Willard Van Orman, 1957, "The scope and Language of Science", in *British Journal for the Philosophy of Science*, 8 (29): 1-17.

Quine, Willard Van Orman, 1960, *Word and Object,* MIT Press, Cambridge (MA).

Quine, Willard Van Orman, 1968, "Propositional Objects", in *Critica*, 2(5): 3-29; reprinted in Quine, 1969: 147-55.

Quine, Willard Van Orman, 1969, *Ontological Relativity and Other Essays*, Columbia University Press, New York.

Quine, Willard Van Orman, 1976 [1963], "Necessary Truth", in his *Ways of Paradox*, Harvard University Press, Cambridge MA.

Quine, Willard Van Orman, 1995, *From Stimulus to Science*, Harvard University Press, Cambridge MA.

Russell, Bertrand, 1901, "Recent Work in the Philosophy of Mathematics", in *The International Monthly*, reprinted in Russell, 2004: 75-95.

Russell, Bertrand, 1914a, "On Scientific Method in Philosophy", pamphlet, Clarendon Press, Oxford; reprinted in Russell, 2004: 75-95.

Russell, Bertrand, 1914b, "Mysticism and Logic", in *The Hibbert Journal*, 12: 780-803; reprinted in slightly revised form in Russell, 2004: 20-48.

Russell, Bertrand, 1918, *Mysticism and Logic*, George Allen & Unwin, London.

Russell, Bertrand, 2004, *Mysticism and Logic*, Routledge, London.

Stebbing, L. Susan, 1929, "Realism and Modern Physics", in *Proceedings of the Aristotelian Society*, Supplementary volumes, 9: 146-161.

Stebbing, L. Susan, 1937, *Philosophy and the Physicists*, Penguin, London.

Lewis and Cavell on ordinary language and academic philosophy

Abraham D. Stone

Abstract: I first show that Lewis and Cavell, regarded as ordinary language philosophers, both part from Austin on the same point, and that in both cases this leads to a similar account of the way ordinary language rules allow successful philosophical (ethical and/or ontological) arguments to reveal truth, as well as a similar account of the way such arguments can fail. I then explain how, beginning with this common basis, they nevertheless end up drawing opposite conclusions about the value of academic philosophy.

Keywords: David Lewis; Stanley Cavell; John L. Austin; Thomas Kuhn; Rudolf Carnap; ordinary language philosophy; academic politics.

1. *Introduction*

David Lewis is in many ways a successor to Carnap. Lewis's modal realism amounts, in the terminology of the *Aufbau*, to a choice of basis: one which gets universal reduction past the seemingly insuperable obstacle of counterfactuals. Similarly, Lewis's mereological interpretation of sets and of the universal-particular relation are the reflexes of Carnap's attempt to carry out such reduction using the fewest ascension forms possible (subject to Goodman's suggestion that we try the part-whole relation).

Lewis also follows Carnap on the possibility of an institution aimed at seeking truth. According to Carnap, "the goal of science consists in this: to find and to order the true statements about the objects of knowledge" (1974: 252, §179). Lewis agrees, and adds that (almost) every department of a "lucky" university would be in that sense scientific. He also adds, however, that the members of a department will typically not agree on what is true. Hence, in most departments, the truth will be sought via dispute:

> Not perhaps [in] the department of frenchified literary theory, where skepticism runs rampant and the pursuit of truth is reckoned passé. Not perhaps [in] the mathematics department, where they are in confident agreement about what's true and how to tell, and they disagree only about what's fruitful and interesting. But in most depart-

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ments, as in philosophy, (1) the advancement of knowledge is the agreed aim; but (2) there are prolonged disputes over what's true. (Lewis 2000a: 5)

Note that the department of philosophy is explicitly included.

It is somewhat controversial whether actual philosophy departments make progress toward truth. Indeed, there is controversy even about physical science. When Lewis was hired by the Princeton philosophy department, in 1970, one of its (decidedly unfrenchified) members was the man who had written this:

These last paragraphs […] show that a sort of progress will inevitably characterize the scientific enterprise so long as such an enterprise survives. In science there need not be progress of another sort. We may, to be more precise, have to relinquish the notion, explicit or implicit, that changes of paradigm carry scientists and those who learn from them closer and closer to the truth. (Kuhn 1996: 170)

Kuhn reaches this conclusion, moreover, by examining the way scientific disputes are forced to end. A science has "matured", according to him, only when its practitioners end their prolonged interschool debates and settle into the consensus brought about by a common paradigm.

Lewis never, to my knowledge, refers to Kuhn in his writings. In the case of philosophy, however, he faces an attack from closer quarters. For the disputes which Lewis everywhere describes as philosophical concern, in Bargle's phrase, "debates over ontic parsimony" (Lewis and Lewis 1983: 9). But these are questions of the type Carnap would later call "external", and about which, early and late, he always maintained two things. First, that they are not questions at all in the "strict logical sense", in which "the posing of a question consists in this, that a statement is given and the task is posed, to establish either this statement itself or its negation as true" (Carnap 1974: 254, §180). Second, that, therefore, although answers to such questions may legitimately be used to express an "emotional and practical attitude" (*gefühls- und willensmäßige Einstellung*), we fall into delusion as soon as we treat them as true or false, and hence as proper subjects for *dispute*:

The metaphysician believes himself to move within the realm in which true and false are in question [*in dem es um wahr und falsch geht*]. In actuality, however, he has said nothing, but only brought something to expression, like an artist. We cannot conclude that the metaphysician finds himself in this delusion simply from the fact that he adopts speech as medium of expression and declarative sentences [*Aussagesätze*] as form of expression; for the lyric poet does the same, without thereby lying under this delusion. But the metaphysician produces arguments for his propositions [*Sätze*]; he demands agreement with their content; he polemicizes against the metaphysicians of other movements, in that he seeks to refute their propositions in his treatise. (Carnap 1931: 240)

Carnap thus repeats, in a more radical register, Kant's diagnosis of metaphysics: it contains antinomies, disputes in which the recognized procedures of argument do not produce agreement, because its practitioners are subject to an illusion of meaning something when they do not.

Lewis disagrees. But his most emphatic statement of that comes in the midst of *agreeing* with Carnap about the premise that there are, in general, no procedures for reaching agreement in ontological disputes:

> If you say flatly that there is no god, and I say that there are countless gods but none of them are our worldmates, then it may be that neither of us is making any mistake of method. We may each be bringing our opinions to equilibrium in the most careful possible way, taking account of all the arguments, distinctions, and counterexamples. But one of us, at least, is making a mistake of fact. Which one is wrong depends on what there is. (1983a: xi)

So for Lewis to maintain his position he must claim that it is rational to hold beliefs, and rational to dispute, about matters in which dispute is not a path to agreement.

Hence the connection to Cavell. Part III of *The Claim of Reason* begins with, and largely centers around, Cavell's criticism of "two assumptions, one about the nature of rationality and one about the nature of moral argument":

> The first is the assumption that the rationality of an argument depends upon its leading from premises all parties accept, in steps all can follow, to an agreement upon a conclusion which all must accept. The second assumption is that the goal of a moral argument is agreement upon some conclusion, in particular, a conclusion concerning what ought to be done. (1979: 254)

The topic is moral, rather than ontological, argument. But the two cases are closely related. At the time of the *Aufbau*, Carnap still regarded moral questions as in a sense empirical and therefore scientific, but he soon adopted the view that they, too, are pseudoquestions.[1] This is the view, later adopted by Ayer and, following Ayer, by Stevenson, that Cavell confronts first of all: the view that disagreements in science are "disagreements in belief", whereas disagreements in ethics are "disagreements in attitude" (Cavell 1979: 259, citing Stevenson 1944: 7). The position is supported, Cavell says, by the fact that moral arguments are "always, and dishearteningly" liable to end in a mere "stalemate", in which "the questions which prompted the argument [are] either left without answer or with incompatible answers which any further argument would seem helpless to resolve" (Cavell 1979: 247).

[1] See (1974: 203-204, §152) and cf. (1931: 237).

If it is natural to regard Lewis as a successor to Carnap, however, it is not so natural to regard him as an alternative to Cavell. Neither Lewis's single explicit engagement with Cavell (1969: 62) nor his more general remarks about ordinary language philosophy (1983a: x) are much help, and we may guess that he didn't intend them to be. He is in general very selective in the views he chooses to criticize or to compare with his own. The same can be said about Cavell, of course, who never, as far as I know, takes any notice of Lewis's work.[2] Being thus left in the lurch by the principals, we will need to reconstruct a relationship between them from what they each say about topics of common interest.

2. *Context*

The principle of ordinary language philosophy, which distinguishes it from a vaguer philosophical appeal to common sense, is the principle of ordinary *context*: that "the profoundest as well as the most superficial questions can be understood only when they have been placed in their natural environments" (Cavell 1976b: 41). The same principle is central to Lewis's thought. I have said above that modal realism provides a system-form which will allow the reduction of counterfactuals to indicatives. Equally important, however, is the way this reduction allows the truth of counterfactuals to depend on the context in which they are asserted.

Because a counterfactual conditional is normally assertible only in contexts in which the antecedent and the consequent are false, the context of assertion must, if the counterfactual utterance can be used to assert anything at all, serve to determine certain *other* contexts at which the antecedent and the consequent are to be evaluated: namely, contexts in which (a) the antecedent is true, but (b) actual background conditions and general principles more or less continue to hold. To assert the counterfactual is to claim that (c) the consequent is true in all those contexts. The combination of (a) and (c) means, in particular, that a counterfactual is normally threatened on two sides by adjustment of the parameter implicit in (b): too far towards "more", and there may be no contexts left in which the antecedent is true (so the conditional becomes vacuous); too far towards "less", and new contexts become available in which the consequent is false (so the conditional becomes false). If the counterfactual is assertible in some contexts and not in others, then the context of assertion must, generally speaking, resolve the vagueness of the "more or less", or, as Lewis puts it, must

[2] I once asked Cavell in person what he thought of Lewis, and in reply he said something about time constraints which prevent us from reading everything.

determine how "strictly" the conditional is to be taken. "That is not altogether wrong", he continues,

> but it is defeatist. It consigns to the wastebasket of contextually resolved vagueness something much more amenable to systematic analysis than most of the rest of the mess in the wastebasket. (1973: 13)

He then proceeds to introduce his own view, according to which a counterfactual is a "variable strict conditional", one which "is as strict, within limits, as it must be to escape vacuity, and no stricter" (*ibid.*).

Later, however, the systematic analysis expands over the whole wastebasket. The rule that strictness changes to prevent vacuity is of the type Lewis later calls a "rule of accommodation": a rule according to which "conversational score [tends] to evolve in such a way as is required in order to make whatever occurs count as correct play" (1983d: 240). The strictness of counterfactual conditionals is only one of many components that make up the "conversational score", and non-vacuity of counterfactuals is only one among many criteria which determine the correctness of conversational "play". Among other things, the analysis now covers all vagueness in which the strictness of some semantic standard must vary with circumstances. In what sense of "must"? Lewis is explicit:

> If Fred is a borderline case of baldness, the sentence "Fred is bald" may have no determinate truth value. Whether it is true or not depends on where you draw the line. […] We cannot pick a delineation once and for all (not if we are interested in ordinary language). (244)

We all have an interest in ordinary language, are parties to the convention that constitutes it. In saying that we cannot avoid vagueness on pain of breaking that convention, that is, failing to speak at all, Lewis declares himself an ordinary language philosopher.

True, at this point he also invokes Austin, in a way which emphasizes their differences. "Austin's 'France is hexagonal,'" he writes, "is a good example of a sentence which is true enough for many contexts, but not true enough for many others" (245), which means: "true under a large enough part of the range of delineations of its vagueness" (244). Whereas what Austin says is this:

> Suppose that we confront "France is hexagonal" with the facts […], is it true or false? Well, […] I can see what you mean by saying that it is true for certain intents and purposes. […] But then someone says: "But is it true or is it false?" […] How can one answer this question, whether it is true or false that France is hexagonal? It is just rough, and that is the right and final answer to the question of the relation of "France is hexagonal" to France. It is a rough description; it is not a true or a false one. (1975: 143)

So Austin and Lewis agree that, first, if someone asks "Is France hexagonal?", the right answer, in some contexts, is "yes", and, in others, "no"; second, that if they ask "Is 'France is hexagonal' true?", the answer is: true enough for certain purposes but not enough for others; third, that if they continue: "But is it true or false?", the answer is: neither. The issue is not whether this last answer is right, but whether it is right and *final*. If our interlocutor continues: "But must not every meaningful declarative sentence express a proposition which is either true or false?", Austin replies that, no, that is not what we say, whereas Lewis shifts to a new context – call it a semanticist's context – in which he can say: "France is hexagonal" expresses different propositions depending on an index which varies with the context of assertion. "'France is hexagonal' is either true or false", was false in the old context, but is true in this new one.

When we turn to Cavell and Cavell's Wittgenstein, however, matters are less clear. The major difference that Cavell identifies between Austin and Wittgenstein is that Wittgenstein "has as fully worked out a theory of how language becomes metaphysical as he does of how language becomes ordinary, that is, of what is acquired in acquiring language" (1994: 6-7). Austin has no explanation beyond the wile of the metaphysician as to how an ordinary question like "But how do you know?" sprouts into a threat to our knowledge of the external world. But Wittgenstein, according to Cavell, has both a theory as to what is acquired in learning to use such an ordinary question, and a theory as to why just that acquisition leaves us inclined to make the question "metaphysical" – where, for Cavell, "metaphysics" is always traced back to Descartes and no further, and Descartes is understood primarily as a proposer of skepticism.

Cavell must explain, then, how the constraints at work in ordinary conversation dictate a skeptical conclusion, once a special context has been established:

> The philosopher's conclusion seems […] to be right, and indeed to be deeper than our everyday, average ideas. […] The convincingness of the conclusion [depends] upon its proceeding, or seeming to proceed, in just the ordinary way any ordinary person must (grammatically) proceed to establish a claim to know of something's existence. But the methods any competent speaker and actor would use to establish ordinary claims seem, in the hands of the philosopher, to establish the inferiority or weakness of those very ordinary claims themselves. (1979: 165)

And this explanation is Lewis's, as well. In the semanticist's context, he will say: an ordinary "might" sentence, "It might be that φ", is true if φ evaluates to true at some world within a certain range, the boundary of which moves in response to a rule of accommodation. The skeptic depends on that ordinary rule:

> The commonsensical epistemologist says: "I *know* the cat is in the carton – there he is before my eyes – I just *can't* be wrong about that!" The skeptic replies: "You might

be the victim of a deceiving demon". Thereby he brings into consideration possibilities hitherto ignored, else what he says would be false. The boundary shifts outward so that what he says is true. Once the boundary is shifted, the commonsensical epistemologist must concede defeat. (Lewis 1983d: 247)

If, moreover, the skeptic's conclusion seems deeper than our everyday ideas, that, too, is thanks to ordinary language constraints. There is, Lewis explains, an asymmetry to certain rules of accommodation. "Because of this asymmetry", for example, "a player of language games who is so inclined may get away with it if he tries to raise the standards of precision as high as possible – so high, perhaps, that no material object whatever is hexagonal" (245). The same thing holds in the case of the skeptic's claim, "You might be wrong":

> We get the impression that the sceptic […] has the last word. Again this is because the rule of accommodation is not fully reversible. […] Because of this asymmetry, we may think that what is true with respect to the outward-shifted boundary must be somehow more true than what is true with respect to the original boundary. (247)

The skeptical argument works by shifting us to a special context – Cavell calls it "the philosopher's context" – in which the rules of ordinary language themselves dictate the skeptical conclusion, and dictate that it will seem more true, deeper, than our everyday knowledge claims.

The semanticist's context bears at a least a passing resemblance to the philosopher's context. It is a context in which "But it must be either true or false" seems deeper or more accurate than Austin's everyday "It is just rough". The semanticist, like the skeptic, must have achieved this by relying on rules of accommodation – that is, by using ordinary language rules themselves to force a context in which ordinary conversational purpose is thwarted. Lewis makes such moves over and over, but one key example has just been quoted: "If Fred is a borderline case of baldness, the sentence 'Fred is bald' may have no determinate truth value. Whether it is true depends on where you draw the line" (244). The first sentence says, with Austin, that "Fred is bald" is neither true nor false. The second sentence, however, presupposes the law of excluded middle, and straightaway a context is established in which it holds: a context in which it may be truly asserted that: "'Fred is bald' is either true or false, depending on the context in which it is asserted".

Aside from the *similarity* between the philosopher's context and the semanticist's context, however, there is also the following relationship between the two: it is only in the semanticist's context that Lewis can explain how the skeptic's inclination motivates her to establish the philosopher's context, and how the resulting argument is neither fully correct nor fully incorrect – not

incorrect play, but nevertheless, so to speak, unsporting. But then if Cavell also differs from Austin in making just such a diagnosis, we should expect to find him, too, establishing something like a semanticist's context. Which he does, for example, here:

> It will help to ask: Can a child attach a label to a thing? […]
> Mightn't we wish to say *either* Yes or No? Is it a matter of *deciding* which to say? What is it a decision about? Should we say, "Yes and No"? But what makes us want to say this? (1979: 174)

We might answer this question "yes" for certain purposes and "no" for others. But is the answer "yes" or "no"? The right and final answer, from Austin's point of view, is: "Yes, sort of". Cavell, who has more to say, must have entered the same type of context that Lewis does. Once in that context, however, do they not say entirely different things? Different in some ways, perhaps, but we should not forget the similarity that led us here in the first place, namely that both use that context to give essentially the same diagnosis of the skeptic's argument.

3. *The ends of language games*

Asymmetrical rules that cause irreversibility are familiar from competitive game such as baseball, chess, and tic-tac-toe. The absence of correct moves leading back to a previous state is built in to such games for a good reason: it helps ensure that games will typically be finite. Correct play always, or for the most part, leads forwards towards the point where the game will be over. But then, why do we want to ensure *that*? Games in general do not need such a guarantee: in non-competitive games such as Dungeons and Dragons or Minecraft, there is a typical (not irreversible) progression towards a more advanced state of play, but there are no rules that will ever cut it off. The point of a competitive game, however, is for someone to win. Irreversibility in such games is therefore by definition an irreversibility of *progress*: progress toward the end of the game, in both a chronological and a practical sense.

Competitive games conform to this requirement of finitude in different ways. When, as in tic-tac-toe and in tournament chess, the rules as such supply an *absolute* guarantee that the game will end, they do so usually by including the possibility of a draw. A draw is a kind of failure of the game and hence of its rules: competition is designed to determine a winner. But the players do at least part without any disagreement as to who has won. Indeed, one might distinguish between the spectator's interest in competition (to see who wins)

and the player's interest (to see whether she can win), and say that the players' end, though not the spectators', is as well accomplished in a draw as in a win for either side. This would help explain why the rules of Major League Baseball, in which the spectators' interest so completely dominates, aim to avoid ties at all cost. Be that as it may: given that a game has got to end sometime, the failure that consists in ending regularly without a winner is preferable to the more serious failure that would consist in ending irregularly, with each player perhaps claiming to have won.

Irreversibility thus serves the practical end of a competitive game. What, however, is the practical end of ordinary argument? In this case agreement as to who is the "winner" will not normally be, in itself, desirable – not in the "lucky" case that *es um wahr und falsch geht*. We hope rather for agreement about the topic under discussion. "Without the hope of agreement", Cavell says, "argument would be pointless" (1979: 254). Or, as Lewis puts it: "each of two debaters tries to get his opponent to grant him – to join with him in presupposing – parts of his case" (1983d: 239). I don't want my opponent to agree that I am winning, but rather to agree with *me*. And yet, both Lewis and Cavell deny that such agreement is the end – either in a practical or, typically, in a chronological sense – of moral or ontological argument. Towards what end, then, and *to* what end, are the rules irreversibly moving us?

It may help to note how such arguments can fail when, and in fact because, they *do* end with a winner: that is, with an outcome in which one party must rationally grant the whole case to the other. Consider, for example, an argument Lewis imagines in (1983d), which can easily be put in the form of one of Cavell's sample moral arguments from *The Claim of Reason*:

> A (an elected official): You see, I must either destroy the evidence or else claim that I did it to stop Communism. What else can I do?
>
> B (rudely): There is one other possibility – you can put the public interest first for once!
>
> A (mistakenly): I can't do that.[3]

Lewis analyzes this conversation as analogous to a skeptical argument. A's initial statement is true, just as Descartes's Meditator is initially correct to say: "I am seated by the fire in my nightgown, etc. How could I be wrong about that?" The initial contexts are such that "An evil demon might be deceiving me" and "I can put the public interest first (at the expense of my political

[3] In Lewis's own presentation: "Suppose I am talking with some elected official. […] He says: 'You see, I must either destroy the evidence or else claim that I did it to stop Communism. What else can I do?' I rudely reply: 'There is one other possibility – you can put the public interest first for once!' […] If he protests 'I can't do that,' he is mistaken" (247).

career)", are, respectively, false. But when the skeptic and the "rude" critic, respectively, assert these possibilities, a rule of accommodation ensures that the context changes so as to make their assertions true. An asymmetry in the rules of accommodation then prevents the context from shifting back, so that if the Meditator now says (without further justification) "But that couldn't be!", or the official now says (without further justification) "I can't do that", they are mistaken. The truth conditions for "It couldn't be that φ" or "I can't do φ" have changed. Therefore, the rude critic, like the skeptic, wins.

The parallel with the skeptic, however, suggests that this is not an example of a *good* moral argument, one that achieves its end. Lewis explains:

> We get the impression that the sceptic, or the rude critic of the elected official, has the last word. Again this is because the rule of accommodation is not fully reversible. […] I see no reason to respect this impression. Let us hope, by all means, that the advance toward truth is irreversible. That is no reason to think that just any change that resists reversal is an advance toward truth. (1983d: 247)

The problem is not that the conclusion is false: Lewis agrees that the official ought to put the public interest first,[4] and also that the skeptic gives a valid reason for doubt.[5] The critic is *not* mistaken in the final context, any more than the official was mistaken in the original one. The problem is that the rule by which the conversation proceeds irreversibly from first word to last is not a rule of advance toward truth. There is a final answer, to which both parties ought to agree, and it is a right answer (is a true statement, given the new context), but its rightness is in no way a consequence of its finality. The initial answer was just as right as the final one.

But then, if getting one's opponent to give away the whole case is not the goal, what rational point could there be in getting *parts* of it? With respect to ontological arguments, at least, Lewis gives an answer, in the Introduction to his *Philosophical Papers*. "The reader in search of knock-down arguments in favor of my theories", he begins "will go away disappointed" (1983a: x). So much we have already come to expect: after all is said and done, Lewis and his opponents may still differ. But then he adds: "Whether or not it would be nice to knock disagreeing philosophers down by sheer force of argument" – and, in a footnote: "It would not be nice, of course" – "it cannot be done" (*ibid.*). The effect of this sly footnote, with its sudden reminder that what is "nice" for me may not be "nice", per se, is to put philosophical argument into a moral and

[4] See his remarks on the additional reasons "decent men" have for respecting the "convention of truthfulness" in a language (1983b: 31).

[5] See (1986: 116).

political context, and to remind us that the *argumentum ad baculum* lies always at hand. We will return to that point. Meanwhile, Lewis continues:

> Philosophical theories are never refuted conclusively. (Or hardly ever. Gödel and Gettier may have done it.) The theory survives its refutation – at a price. Argle has said what we accomplish in philosophical argument: we measure the price. Perhaps that is something we can settle more or less conclusively. (*ibid.*)

The reference is to concluding part of "Holes":

> Bargle: I, for one, have more trust in common opinions than I do in any philosophical reasoning whatever. In so far as you disagree with them, you must pay a great price in the plausibility of your theories.
>
> Argle: Agreed. We have been measuring that price. I have shown that it is not so great as you thought. […]
>
> Bargle: The price is still too high.
>
> Argle: We agree in principle; we're only haggling.
>
> Bargle: We do. And the same is true of our other debates over ontic parsimony. Indeed, this argument has served us as an illustration – novel, simple, and self-contained – of the nature of our customary disputes. (Lewis and Lewis 1983: 8-9)

So this is an example of a successful conversation. The parties do not come to agree with one another about the point at issue. But they do advance towards the truth about something, namely, about the true prices of their respective positions. This advance in accuracy of measurement depends, moreover, on the procedure of *argument*: the apparent price of Argle's view will increase insofar as she grants Bargle parts of his case, and vice versa. Moreover, the parties do end by acknowledging an agreement: not a new agreement forced by the argument, but rather an agreement pre-existing from the first ("in principle"), which made the argument possible, and which the argument brings to light because actuality implies possibility. It is, quite literally, an agreement about *values*: an agreement, that is, about what kind of features in a thing are responsible for its price. The purpose of argument in this case, in other words, is self-revelation. What Argle and Bargle each learn is what they were already prepared to demand from and grant to each other. Irreversibility, then, is here a consequence of what Lewis elsewhere calls the Rule of Attention: what is not ignored at all is never properly ignored (1999a: 434). I try to get the other to agree with me in order to learn, about myself, what it is I am prepared to request agreement with, and what I learn should not, and normally cannot, be unlearned. Irreversibility is in the service, not of progress towards the end, but of progress away from the beginning: away from the initial state of forgetfulness.

Switching back to the case of moral argument, then, we may expect Lewis to agree that "questioning a claim to moral rightness", in the course of moral

argument, "takes the form of asking […] 'Have you really considered what you're saying?', 'Do you know what this means?'" (Cavell 1979: 268), and that

> assessing the claim is […] to determine *what* your position is, and to challenge the position itself, to question whether the position you *take* is adequate to the claim you have entered. (*ibid.*)

and that

> The point of the assessment is not to determine *whether* it is adequate, […] the point is to determine *what* position you are taking, that is to say, *what position you are taking responsibility for* – and whether it is one I can respect. (*ibid.*; Cavell's emphasis throughout)

If a moral argument results in a more accurate measurement of prices, in other words, it also reveals a prior agreement on values: it shows the opponents can not only take responsibility for (pay for) their own positions but also respect (accept payment for) one another's. This practical end is achieved, not in particular at the chronological end of the argument, but rather *at every point*, at every irreversible step, as long as each party continues to find the other's moves respectable. The continued commerce enriches both, insofar as, by means of it, they come to own (possess justifiably) parts of their positions which they otherwise would not.

If an argument ends without agreement on the moral issue, then, that is not actually analogous to a stalemate (a draw), because progress towards a hoped for agreement on that issue was all along in the service of revealing a pre-existent agreement on values. Something like a draw – a regular failure – will occur only if it turns out there is no such prior agreement. "The outcome of the argument will affect whether the parties concerned are to continue to live in the same moral world" (Cavell 1979: 295-296). For example, consider the following:

> A: I've decided against offering him the job.
> B: But he's counting on it. You most explicitly promised it to him. […]
> A: I know, but it has suddenly become *very* inconvenient to have him around, and there is someone else really better qualified anyway.
> B: If you do this to him, I'll never speak to you again.
> A: Don't make such an issue of it. I'll see that he gets a job, and I'll give him some money to see him through.
> B: Goodbye. (266)

The parties, it turns out, cannot respect one another, cannot agree on the price of what A intends to do. Commerce, on this point, has come to a halt. Still, the failure is regular. Neither party is mistaken; each has competently tak-

en responsibility for a position. The type of irregular failure thereby avoided is the one evident in Lewis's case, where the rudeness of the critic, combined with the official's mistake, prevent the parties from determining *whether* they are in the same world, i.e. whether or not they can agree on a price.

The rudeness, moreover, is responsible for the mistake. The official knows that the critic's move is valid, and that "I can't do that" is not a valid reply. But what alternative reply is there? The old context, in which "I can't do that" expressed something true, was not arbitrary: it was suited to the cares and commitments of the elected official, as the critic is well aware. The language game which the critic exploits, with its underlying conventions of trust and truthfulness, was only one of those commitments, and the critic's move has done nothing to change the others. Under pressure, the juncture between linguistic and non-linguistic commitments breaks, and the official is left with a choice between saying something true but insincere ("Yes, of course, that is my duty") or something sincerely felt but false ("I can't do that"). Lewis is correct, then, to draw an analogy between this moral case and the epistemic case of the skeptic: the skeptic's interlocutor faces just this choice between a reply that is true but not sincerely believable ("Yes, of course, I don't really know") and one that is believable but false ("There can be no doubt"). The type of rudeness involved here, in other words, is *inconsiderateness*. The critic fails to take the official's cares and commitment into account in deciding which move to make. If the conversation chronologically ends here, its practical end has been frustrated. The official's "I can't do that", because it is *mistaken*, does nothing to take responsibility for any position; nor, therefore, does it reveal whether the official's position is one that the critic can respect.

The possibility of rudeness, and of the mistakes it can force, is itself a price we have payed. The rules of moral and ontological argument might, as in competitive games, guarantee a regular chronological end, and in that case rudeness of this kind would be impossible. Austin can seem rude, and this is perhaps the effect, or perhaps the cause, of taking philosophical argument to have the nature of such a game. Whole philosophical cultures can seem infused with rudeness, with like causes or effects. Lewis and Cavell, however, have in common an understanding of the ends of philosophical argument according to which it is an activity in which interlocutors can make progress only by respecting one another's cares and commitments. The rules governing the activity cannot force this respect, and cannot make this respect deserved. In its absence, a forced regular chronological end would serve no purpose; in its presence, there is no need to force one. Hence the rules are such that a rude interlocutor can bring about an irregular conclusion.

4. *Truth in the Academy*

Among the things Lewis inherits from Carnap is also this: that he is willing to speak on behalf of a school of philosophy, in response to those outside the school. Carnap's famous "Principle of Tolerance" concerns just such a situation:

> *In logic, there are no morals.* Everyone may construct [*aufbauen*] his logic, i.e. his language-form, as he wishes. Only, if he wishes to debate with us, he must clearly state how he intends to do it, give syntactical determinations instead of philosophical arguments. (1934: 45, §17)[6]

Tolerance, notice, runs only so far: use whatever language you want, but, if you want to talk to *us* – i.e., to the members of "our 'Vienna Circle'" (IV) – you must learn to speak as we do. "Logical syntax is to supply a structure of concepts [*Begriffsgebäude*], a language, with the help of which the results of logical analysis can be exactly formulated" (*ibid.*). Here, in other words, Carnap adopts the suggestion which he elsewhere attributes to Neurath: recondition others to replace their speech dispositions with ours. If some cannot be reconditioned in this way (reconditioning, after all, "succeeds in some cases, with some animals and humans, and then in other cases does not"), we simply rule them out of "the circle of those by whom intersubjective science is constructed and applied" (Carnap 1932: 222). Lewis, taking on Richard Routley/Sylvan's "noneism", finds himself actually in this situation: "Routley sees himself as defying an established orthodoxy; and I am prepared to appoint myself spokesman for the orthodoxy he defies. Or at least for those among the orthodox, if any, who will accept me as their spokesman" (Lewis 1999b: 154). Again the problem is that Routley has not expressed himself as we would: "His own words do not answer the question what *we* ought to say in reporting his position" (156). But Lewis's response is subtly different.

> [Routley] does not have the final word either on how his position should be expressed in our language, or on how ours should be expressed in his. Nor do we. There is no authoritative final word; we can only seek the translation that makes him make sense to us, and us to him. (156-157)

Our school lacks authority over him. He can be expected, therefore, to continue speaking as he does. If we want a debate, the burden is on us to become translators.

[6] The translation (1959: 52) reads "if he wishes to discuss it", but the original is *wenn er mit uns diskutieren will.*

That Lewis should give up hope on getting our opponent to speak our language is surprising, given that he groups philosophy with science as institutions whose aim is the advance of truth. For the situation he describes and accepts with respect to Routley is the one Kuhn finds characteristic of pre-paradigm, "immature" science: a period in which "evidence of progress, except within schools, is very hard to find" (Kuhn 1996: 163). Kuhn blames this lack of general progress on a feature famous from his account of scientific revolution, namely on *incommensurability*: the various schools are differentiated by "their incommensurable ways of seeing the world and of practicing science in it" (4). Kuhn also, especially in the "Postscript" to (1996), describes incommensurability as breakdown in communication due to linguistic difference: proponents of incommensurable views "cannot […] resort to a neutral language which both use in the same way and which is adequate to the statement of both their theories" (201). The only hope, then, is for the two sides to "recognize each other as members of different language communities and then become translators" (202). But we can expect to find *progress* in science, according to Kuhn, only once these disputes between schools come to an end, namely with the acceptance of a common paradigm. If Lewis expects progress in philosophy, should he not, as Carnap advises, try to establish for it a common paradigm – that is, a common language?

But how do paradigms avoid dispute, according to Kuhn? One might imagine a set of rules for science which were established for a definite practical end, namely, to settle scientific arguments in a rational way and in finite time. Such rules, if well designed, would be applicable to any mature science in any period. To determine correct rules of this kind is the task Popper assigned to the discipline of scientific methodology or *Logik der Forschung*, and the particular rules he determines indeed resemble the rules for competitive games – Popper even says that "one might call them the rules [*Spielregeln*] of the game, 'empirical science'" (2002: 25, §11). But Kuhn claims that the actual rules by which science normally proceeds are entirely unlike this. The rules do not aim to settle arguments, because there is no tolerance for argument: scientists do not "normally aim to invent new theories, and they are often intolerant of those invented by others" (24). In a mature science, he says, there is rather, normally, an "apparent consensus" (11) as to what theory is true. In reality this theory is always underspecified (remains to be "articulated"), but what is real, not merely apparent, is that those who share a paradigm "are committed to the same rules and standards for scientific practice" (*ibid.*). These rules are not constant, as if designed in advance for some purpose; they contain "an apparently arbitrary element, compounded of personal and historical accident" (4). And the apparent consensus at any given (normal) time is the *effect* of these arbitrary

conventions, which are or amount to the arbitrary conventions characteristic of different linguistic communities. The arbitrary rules serve to specify, to within some small range of ambiguity, a particular theory – the "paradigm theory" – which is to account for all results. "The range of anticipated, and thus of assimilable, results is always small compared with the range that imagination can conceive. And the project whose outcome does not fall in that narrower range is usually just a research failure, one which reflects not on nature but on the scientist" (35). The rules produce agreement directly, not via a rational exchange between two parties. They are there not to decide between outcomes (a win for A versus a win for B), but rather to ensure that, with sufficient skill, anyone can achieve the one acceptable outcome. In other words: they are unlike the rules of a competitive game and like, rather, the rules of a puzzle.

The solution of a puzzle does reveal a certain truth, namely about the skill of the solver. This is part of the interest of the puzzle, at least to the one who solves it – and, in general, puzzle-solving does not excite the interest of spectators. But if we expected science was to advance our knowledge *of nature*, then the rules Kuhn depicts as characteristic of normal science are unsuitable for producing that result. Science can't *normally* be expected to reveal new truths. As for "extraordinary" periods when a paradigm breaks down and is eventually replaced, those are characterized by the return of incommensurability, and hence by the impossibility of any kind of general progress at all. In such a situation, scientists addicted to puzzle-solving will look for knock-down arguments, as Kuhn explains in his key comparison between scientific "revolutions" and the real, political kind. As a political revolution proceeds,

> society is divided into competing camps or parties. […] And, once that polarization has occurred, *political recourse fails*. Because they differ about the institutional matrix within which political change is to be achieved and evaluated, because they acknowledge no supra-institutional framework for the adjudication of revolutionary difference, the parties to a revolutionary conflict must finally resort to the techniques of mass persuasion, often including force. (93)

But that would not be nice, of course.

Lewis may or may not agree with Kuhn's account of the "mature" sciences. His story about the "prolonged disputes over what's true" in every department is relevant only to the extent that he believes those science departments to be "lucky". When he offers a defense of inductive methods, it is rather weak and Kuhnian, if not Humean: we call it "inductive reason", "as we are right to do, because that is indeed the name we have given it" (1986: 117): that is, it is reasonable because *this* is what we call "reasonable". Sticking to philosophy, in any case: Lewis thinks that it remains, in Kuhn's terms, "immature", and, un-

like Carnap, he proposes no attempt to mature it. May we hope, then, that it is sufficiently lucky as to advance towards truth?

We have seen that Lewis, like Cavell, thinks ordinary arguments aim at such an advance: an advance towards a more accurate measurement of prices. But, precisely because they are *ordinary*, their rules are not the rules of any special institution. In a related case, indeed, Cavell argues that *promising* cannot correctly be called an "institution" at all, not at least as Rawls defines the term: "a public system of rules which defines offices and positions with their rights and duties, powers and immunities, and the like" (1971: 55). Cavell points out that the "office" of promisor is unlike offices properly so called: "there is no special procedure for entering it (e.g., no oaths!), no established routes for being selected or training yourself, etc." (1979: 297). The same points apply equally well to the "office" of participant in an ordinary argument. The same applies, moreover, to the office occupied by Socrates: his claim that the god has appointed him official gadfly is of the same kind as Thoreau's list of public offices, beginning with "for many years I was self-appointed inspector of snow-storms and rain-storms, and did my duty faithfully" (Thoreau 1992: IB.7), and concluding

> In short, I went on thus for a long time […] till it became more and more evident that my townsmen would not after all admit me into the list of town officers, nor make my place a sinecure with a moderate allowance. (IB.9)

It is a joke, in other words, and one whose punch lies precisely in calling attention to the incongruity between our true situation and the purposes for which we have established public offices. "If you are chosen town clerk, forsooth, you cannot go to Tierra del Fuego this summer: but you may go to the land of infernal fire nevertheless. The universe is wider than our views of it" (18.2). To justify the Academy, then, in both the original and the extended sense of the term, would be to somehow bridge that laughable incongruity. And the task is indeed laughable: as if we expect every hiring committee to reverse the verdict of Athens and grant Socrates a stipend.

It is difficult to determine Cavell's opinion about this, in part because, when he uses the term "academic", as he does with some frequency, he typically has in mind more the French *Académie des Beaux-Arts* than the Academy of Plato. When he says, for example, that "academic art is (with notable exceptions) bad art, whereas academic science is – just science" (1976a: xxvii), he is drawing the same contrast between science and art that Kuhn implies when he contrasts the solution of a jigsaw puzzle with the way "a child or a *contemporary* artist" (my emphasis) might use its pieces to "make a picture", and adds: "The picture thus produced might be far better, and would certainly be more original, than the one from which the puzzle had been made" (1996: 38). The word "con-

temporary" is there because, as is clear from his brief discussion of the history of painting (161), he believes that painting was, like science, a form of puzzle solving before the Impressionists rebelled against the Academy. Nevertheless, both Kuhn and Cavell connect the academic nature of (mature) science with a feature characteristic of Plato's Academy, namely its withdrawal from the agora. Kuhn's normal science, like puzzle-solving more generally, is of interest only to the participants. Outside of these mature sciences, Kuhn points out, "there are no other professional communities in which individual creative work is so exclusively addressed to and evaluated by other members of the profession" (164) – that is, by those who "by virtue of their shared training and experience, must be seen as the sole possessors of the rules of the game or of some equivalent basis for unequivocal judgments" (168). In the context cited above, Cavell makes the exact same point about the distinction between art and science:

> It is tautological that art has, is made to have, an audience, however small or special. […] It could be said of science, on the other hand, that it has no audience at all. No one can share its significance who does not produce work of the same kind. The standards of performance are institutionalized; it is not up to the individual listener to decide whether, when the work meets the canons of the institution, he will accept it – unless he undertakes to alter those canons themselves. (1976a: XXVII)

Cavell, then, regards the Academy generally speaking as both a good site for science and a bad site for art.

As to the divergence between Socrates and Plato in its original form, however, Cavell reaches no definite decision: "Now, what is academic philosophy? It seems significant that this question has no obvious answer" (*ibid.*) This could be taken to mean that, after all this time, it remains unclear what features are particular to academic philosophy, and hence whether it is like academic art (bad) or like academic science (simply philosophy). But the statement could also be taken to mean, more pessimistically, that, after all this time, it is unclear whether or why we should expect anything worthy of the name "philosophy", whether good *or* bad, to take place in the academy. The latter interpretation gains support from what Cavell says elsewhere: for example, about the activity of "looking for an explanation in a region in which you have no inclination to suppose it may lie" that "we might call such an activity 'academic'" (1979: 21). If you are addicted to some type of puzzle, you are "inclined to suppose" that the *solution* lies in a certain region. But you are neither inclined to suppose that the solution will produce anything useful, for example an explanation, nor inclined to examine your motives for nevertheless undertaking to solve it. You must be distracted from any question about what value the solution will have. If insulation from the demands of spectators is a prerequisite to such distrac-

tion, then the continuation of puzzle-solving as a field of purported study depends on the closed political structure of a mature scientific discipline, which forbids appeal to any paradigm-external standard. The quote suggests that Cavell takes this closed political structure and its consequences to be both characteristic of the academy in general and generally inimical to the kind of self-revelation which is philosophy's only path towards truth.

Since Lewis agrees with Cavell about the latter, he must, if he has a more benign view of academic philosophy, disagree both with Kuhn and with Cavell about how it might be organized. And so he does. For Kuhn's mature science to take hold in a field, recall, there must be an end to competing schools. This "is usually caused by the triumph of one of the pre-paradigm schools" (Kuhn 1996: 17). But Lewis's main answer to the title question of (2000a), "Why ignore the advantage of being right?", is that rival "schools of thought", within a lucky field, are parties to a "tacit treaty", the purpose of which is precisely to prevent any such triumph.

To clarify the reasons for this, he imagines first a case both simplified and ironically described. The simplifications are, first, that there are exactly two schools – materialists and dualists – and, second, "that all concerned think the errors of their opponents matter more than the errors of their misguided allies" (2000a: 198). Lewis, himself a materialist (about the actual world), describes the situation as follows:

> In my own opinion as a materialist, the best thing for the advancement of knowledge would be the universal acceptance of the true philosophy: materialism. Or near-universal, anyway. […] Worst would be the universal, or near-universal, acceptance of dualist error. Second best would be a mixture, as at present. A treaty requiring us all to ignore the advantage of being right when we make appointments will raise the probability of that second-best outcome and lower the probability both of the best and of the worst. (*ibid.*)

Dualists will rate the outcomes in the opposite order, but they may well find common interest with the materialists in a treaty that establishes what both agree to be the second best outcome, thus averting either's worst fears. The simplifications, moreover, are easily seen to be inessential: if anything, the case for the treaty is stronger if we are all involved in multiple disagreements and also cannot always count on our supposed allies.

The irony in the description, however, is this. Lewis describes dualists and materialists, himself included, as hoping that the institution of academic philosophy will reveal truth by choosing the correct side of the argument, much as we may hope academic physics will do in the case of string theory (and much as an inquisitor might hope that society will protect true religion by suppress-

ing heresy: see (Lewis 2000b)). The dispute between materialists and dualists, however, is a dispute about ontic parsimony, and we know that Lewis's real hope for such arguments is quite different: he hopes they will reveal the truth about *prices*, and hence about our antecedent agreement on values. Why would Argle, asked to vote on hiring Bargle, ignore the "disadvantage" that, in her opinion, he is wrong? Isn't it because there is no such disadvantage, because, rather, Argle could not purchase her view without Bargle as a counterparty? Or, to put it differently: whether or not it would be nice to use the *argumentum ad baculum* and win our arguments by literally knocking our opponents down – it would not be nice, of course, but, in any case, Lewis assures us, *it can't be done* (almost ever: perhaps Gödel or Gettier could have done it). What use a treaty to prevent it?

We cannot win our arguments this way, but we have an inclination to try, and the rules of ordinary argument will not prevent such rudeness. In short: on the surface, the treaty is necessary to prevent the bad outcome that my opponents win; but, in truth, it is necessary to prevent the worse outcome that I and my supposed allies do. The inclination to *win*, which has no place in philosophical argument, is characteristic of schools of thought, with their offices and positions, their presupposed limits on what can be said and done (consistent with continuing one's career), their formal and informal rules of successorship. Only the humiliating need to treat with other schools can restrain this *libido sectarum*.

If Lewis and his followers have been more comfortable in academic philosophy than have Cavell and his – and have they not? – then I suggest that this is why. Cavell sees Kuhnian maturity as a threat to the existence of philosophy; Lewis sees Kuhnian immaturity as the only hope of its survival.

Abraham D. Stone
abestone@ucsc.edu
University of California Santa Cruz

References

Austin, John L., 1975, ed. by J.O. Urmson and Marina Sbisà, *How to Do Things with Words*, 2nd ed., Harvard University Press, Cambridge MA.

Carnap, Rudolf, 1931, "Überwindung der Metaphysik durch logische Analyse der Sprache", in *Erkenntnis*, 2: 219-241.

Carnap, Rudolf, 1932, "Über Protokollsätze", in *Erkenntnis*, 3: 215-228.

Carnap, Rudolf, 1934, *Logische Syntax der Sprache*, Springer, Vienna.

Carnap, Rudolf, 1959, *The Logical Syntax of Language*, tr. Amethe Smeaton, Littlefield, Adams, & co., Princeton.

Carnap, Rudolf, 1974, *Der logische Aufbau der Welt*, 4th ed., Meiner, Hamburg.

Cavell, Stanley, 1976a, "Foreword: An Audience for Philosophy", in Cavell 1976c: XVII-XXIX.

Cavell, Stanley, 1976b, "Must We Mean What We Say?", in Cavell 1976c: 1-43.

Cavell, Stanley, 1976c, *Must We Mean What We Say?: A Book of Essays*, Cambridge University Press, Cambridge.

Cavell, Stanley, 1979, *The Claim of Reason: Wittgenstein, Skepticism, Morality and Tragedy*, Oxford University Press, Oxford.

Cavell, Stanley, 1994, *A Pitch of Philosophy: Autobiographical Exercises*, Harvard University Press, Cambridge MA.

Kuhn, Thomas S., 1996, *The Structure of Scientific Revolutions*, 3rd ed., University of Chicago Press, Chicago.

Lewis, David K., 1969, *Convention: A Philosophical Study*, Blackwell, Oxford.

Lewis, David K., 1973, *Counterfactuals*, Harvard University Press, Cambridge MA.

Lewis, David K., 1983a, "Introduction", in Lewis 1983c: IX-XII.

Lewis, David K., 1983b, "Languages and Language", in Lewis 1983c: 163-198.

Lewis, David K., 1983c, *Philosophical Papers*, vol. 1, Oxford University Press, Oxford.

Lewis, David K., 1983d, "Scorekeeping in a Language Game", in Lewis 1983c: 233-249.

Lewis, David K., 1986, *On the Plurality of Worlds*, Blackwell, Oxford.

Lewis, David K., 1999a, "Elusive Knowledge", in Lewis 1999c: 418-445.

Lewis, David K., 1999b, "Noneism or Allism?", in Lewis 1999c: 152-163.

Lewis, David K., 1999c, *Papers in Metaphysics and Epistemology*, Cambridge University Press, Cambridge.

Lewis, David K., 2000a, "Academic Appointments: Why Ignore the Advantage of Being Right?", in Lewis 2000c: 187-200.

Lewis, David K., 2000b, "Mill and Milquetoast", in Lewis 2000c: 159-186.

Lewis, David K., 2000c, *Papers in Ethics and Social Philosophy*, Cambridge University Press, Cambridge.

Lewis, David K., Stephanie Lewis, 1983, "Holes", in Lewis 1983c: 3-9.

Popper, Karl, 2002, *Logik der Forschung*, 10th ed., Mohr Siebeck, Tübingen.

Rawls, John, 1971, *A Theory of Justice*, 1st ed., Belknap Press, Cambridge MA.

Stevenson, Charles L., 1944, *Ethics and Language*, Yale University Press, New Haven.

Thoreau, Henry David, 1992, *Walden*, in William J. Rossi, ed., *Walden and Resistance to Civil Government*, Norton, New York: 1-223. (Cited by chapter and paragraph number).

Naturalistic metaphysics at sea[1]

Matthew C. Haug

Abstract: In this paper I return to the mid-20th-century debate between Quine and Carnap on the status of metaphysical questions with an eye toward advancing contemporary debates about whether naturalists can coherently undertake substantive metaphysical inquiry. Following Huw Price, I take the debate between Quine and Carnap to hinge, in part, on whether human inquiry is functionally unified. However, unlike Price, I suggest that this question is not best understood as a question about the function(s) of descriptive discourse. This goes along with rejecting a "linguistic conception" of the starting point of metaphysical inquiry, which, although shared by Quine and Carnap, Price gives us no good reason to think is mandatory for naturalists. I sketch two reasons naturalists have to reject a particular manifestation of this linguistic conception in Quine's work – his criterion of ontological commitment. Finally, I show how these reasons can help us identify the grains of truth in some recent critiques of "mainstream metaphysics of mind".

Keywords: naturalism; meta-ontology; ontological commitment; W.V.O. Quine; Huw Price.

1. *Introduction*

Two of the most significant trends in late analytic philosophy have been the widespread adoption of methodological naturalism and the resurgence of metaphysics as a substantive area of inquiry. Arguably, no single philosopher is more responsible for these trends than W.V.O. Quine. Both naturalism and the legitimacy of metaphysics are expressed by one of Quine's favorite images: Otto Neurath's boat, which Quine takes (in the original German) as one of the

[1] In this paper, I try to bring historical scholarship on late analytic philosophy into even closer conversation with contemporary work in meta-metaphysics. In this way, this paper is a companion to my (2014b), in which I attempted to achieve the same kind of goal, although in that paper the focus was more on Carnap. I presented portions of this paper at a symposium on "The Future of Philosophy of Mind" at the 2016 Eastern APA meeting in Washington, DC. Sections 3 and 4 also incorporate some passages from my (2014a). I thank the audience at the APA, Guido Bonino, and Paolo Tripodi for helpful comments.

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epigraphs for his 1960 book *Word and Object*:

> We [philosophers and scientists] are like sailors who have to rebuild their ship on the open sea, without ever being able to dismantle it in dry-dock and reconstruct it from the best components. (Neurath 1932/3: 92)

For Quine, this expresses naturalism by denying the possibility of an "external vantage point" or a "first philosophy" (the solid ground of a "dry-dock") that would provide a foundation for scientific inquiry as a whole (Quine 1969: 127). At the core of Quine's naturalism is the rejection of any second order or transcendental philosophical activity that is independent of, or prior to, scientific inquiry (see Maddy 2007; Verhaegh 2017c). As he puts it, naturalism is "the recognition that it is within science itself, and not in some prior philosophy, that reality is to be identified and described" (1981: 21).

Although this statement of naturalism rejects philosophical inquiry that is supposedly prior to scientific inquiry, it does not express the idea that, according to the naturalist, philosophy should be part of, or continuous with, science. Naturalistic philosophers take their place along with scientists as busy sailors on Neurath's boat, and, as a result: "All scientific findings, all scientific conjectures that are at present plausible, are therefore … as welcome for use in philosophy as elsewhere" (Quine 1969: 127). Philosophers use the same methods as scientists, which, in the naturalist's view, are the only legitimate methods of investigating the world that there are. So, developing a metaphysical theory of the general structure of reality is not a different kind of enterprise than developing a theory in a particular science. As Quine puts it in two, oft-quoted passages:

> Our acceptance of an ontology is, I think, similar in principle to our acceptance of a scientific theory, say a system of physics: we adopt, at least insofar as we are reasonable, the simplest conceptual scheme into which the disordered fragments of raw experience can be fitted and arranged. Our ontology is determined once we have fixed the over-all conceptual scheme which is to accommodate science in the broadest sense. (Quine 1948: 16-17)

> Ontological questions, under this view, are on a par with questions of natural science.[2] (Quine 1951b: 45)

[2] In a footnote appended to this sentence Quine quotes the following passage (in French) from Émile Meyerson: "Ontology is a part of science itself and cannot be separated from it". Although Quine claims not to have been significantly influenced by Neurath's writings (see Uebel (1991: 629n15, 639n33); Verhaegh (2017b: 337n71)), it seems that Meyerson may have had a bigger impact on Quine's thought. Sandra Laugier claims that Quine took the idea of "positing" from Meyerson (2009: 100) and

This approach to ontology, in which questions about the existence of (say) numbers, composite material objects, possible worlds, fictional characters, or moral values are substantive, difficult, but tractable questions that are no different in kind than scientific questions, was elaborated by Quine's student, David Lewis, and has been called "mainstream metaphysics" (Manley 2009: 4).[3] On this approach, ontological methodology is "quasi-scientific" with competing positions about the existence of the entities in question assessed "with a loose battery of criteria for theory choice", including theoretical insight, simplicity, integration with other domains, and consilience with our pre-theoretical beliefs and "intuitive" verdicts about thought experiments (Sider 2009: 385; see also Sider 2011: 166ff.).

Many practitioners of mainstream metaphysics think that a proper subset of the entities that exist in the actual world are fundamental and thus seem to face the task of finding a place for other purportedly existing things within a description of the world that uses only terms for the fundamental entities. If descriptions of the non-basic things cannot be found implicitly within the fundamental description, then we must admit that those non-basic things do not in fact exist. We must locate the putative non-basic features of the world on pain of eliminating them. This project of "serious metaphysics" thus inevitably involves "location problems" (Jackson 1998). According to one of the progenitors of serious metaphysics, Frank Jackson, the only way to solve a location problem is to show that (a description involving) the non-basic feature is entailed by a description of the world in basic terms. This approach to metaphysics, the so-called Canberra Plan, thus claims that conceptual analysis will play an essential role in metaphysical inquiry (*ibid.*: Ch. 2). Such conceptual analysis will involve identifying common sense platitudes about the alleged non-basic feature and consulting intuitions about the application of a term for that feature in different possible cases. The Canberra Plan has been called "the most influential self-proclaimed naturalistic approach in the contemporary philosophical literature in metaphysics" (Ismael 2014: 86).

However, since Quine's debates with Carnap in the 1950s on analyticity, internal and external existence questions, and the theoretical/practical contrast (Carnap 1950; Quine 1951a; 1951b), there have been philosophers in the

that he "owes to Meyerson even his conception of naturalism" (*ibid.*: 104). M. Anthony Mills, however, argues that the latter claim is an overstatement and that, for Meyerson, some areas of ontology are irreducible to science (2015: 324n20, 343).

[3] Manley introduces this label "with the caveat that [the view] has only come to ascendency lately, and is still widely challenged" (*ibid.*). He also notes that mainstream metaphysics "repudiates the more pragmatist [in the sense at issue at the end of Quine (1951b)] elements of Quine's approach to ontology" (*ibid.*: 5).

analytic tradition who are (if we accept Manley's characterization) outside the mainstream. These philosophers claim that mainstream metaphysicians' approach to ontological questions is mistaken and that these questions are somehow merely verbal, or trivial, or entirely practical (instead of theoretical). In doing so, they advocate what I'll call *meta-ontological deflationism*. Debates between meta-ontological deflationists and their opponents have attracted increasing attention in recent years (see, e.g., Chalmers, Manley, and Wasserman 2009; Hirsch 2011; Thomasson 2014; Blatti and Lapointe 2016; Hofweber 2016). These debates often lie at the intersection of contemporary metaphysics and the history of late analytic philosophy, since what is as issue is not only how we should approach apparent metaphysical questions but which approach should be seen as emerging victorious from the debate between Quine and Carnap in the middle of the last century.

This paper falls squarely in this intersection. I aim to show that contemporary meta-metaphysics and the history of late analytic philosophy can be mutually illuminating. On one hand, I claim that contemporary naturalists who want to defend substantive metaphysical inquiry can get clearer about exactly what that inquiry is like, and how best to defend it, by reflecting on the elements of Quine's philosophy that made it difficult for him to avoid meta-ontological deflationism. On the other hand, I hope that outlining a naturalistic approach to substantive metaphysical inquiry will help us better understand some aspects of the history of late analytic philosophy, such as the core issues at stake in the debate between Quine and Carnap.

My entry point into these issues is Huw Price's claim that naturalism itself is in tension with substantive metaphysical inquiry and that contemporary mainstream metaphysicians are mistaken to think that Quine provides support for their approach. On this view, meta-ontological deflationism simply follows from the right kind of naturalistic methodology. Indeed, the idea that metaphysics should not be part of naturalistic inquiry can be traced to the rhetorical origins of Quine's naturalism: the sentence from Neurath that immediately follows those that appear as Quine's epigraph to *Word and Object* is: "Only metaphysics can disappear without a trace" (Neurath 1932/3: 92).

If this view is correct, then proponents of substantive metaphysical inquiry should be cast off the boat and left truly "at sea", with grave doubts about the legitimacy of their project and at risk of slipping beneath the waves "without a trace". This paper is offered as a lifeline to those metaphysicians who want to find a place on Neurath's naturalistic boat. I claim, contra Price, that naturalistic methodology is consistent with substantive metaphysical inquiry and that naturalistic metaphysicians still have important work to do that is not simply investigation of our linguistic practices. However, I think that this work will differ in

significant ways from the projects pursued by (much of) mainstream metaphysics. In particular, I argue that naturalistic metaphysicians should (i) not adopt a linguistic conception of metaphysical issues (as both Quine and followers of the Canberra Plan do), (ii) reject Quine's criterion of ontological commitment, (iii) not take agreement with our pre-theoretic beliefs to be a constraint on metaphysical theorizing, and (iv) adopt a more piecemeal approach to existence questions, instead of trying to develop universal, science-wide arguments for, say, realism.

Thus, in this paper I advocate an intermediate position about how best to keep the boat of inquiry afloat. Unlike mainstream metaphysicians, I think that the boat bequeathed to us by Quine, Lewis, and Jackson needs a significant overhaul. Some of the planks that keep mainstream metaphysics afloat are rotten and need to be jettisoned. However, unlike meta-ontological deflationists, I do not think that this calls for abandoning metaphysics completely. Rather, I think that naturalistic metaphysicians deserve to be at sea with every other inquirer, venturing into unknown waters on a common voyage of discovery.

In Section 2, I outline Price's argument that Quine is best interpreted as a meta-ontological deflationist and how, according to Price, whether Quine has an argument against deflationism turns largely on whether he can consistently defend the idea that descriptive discourse serves a single function. In Price's view, whether this kind of "functional monism" is true will ultimately be settled by studying human language use and is intimately connected to the question of whether truth and other semantic notions are substantive, causal-explanatory properties. In Sections 3 and 4, I argue that Price is mistaken on both counts. In Section 3, I suggest that Quine's "functional monism" is best interpreted as a strong version of the *seamlessness* of inquiry, which implies a kind of *disciplinary holism* that is not best thought of in linguistic terms, arguably itself poses a challenge to Price's naturalism, and can be used to show how a "unified, all-purpose ontology" is consistent with naturalism. In Section 4, I suggest that naturalists are well advised to reject (contra both Quine and Price) a "linguistic conception" of metaphysical inquiry (and of location problems, in particular), according to which metaphysical issues arise via reflection on human language and thought and how they relate to the world (see Price 2004: 188).[4] By rejecting a linguistic conception, naturalists can argue that me-

[4] Adopting a linguistic conception of metaphysical inquiry need not involve thinking that there is a distinction between choosing a linguistic framework and choosing a theory within a framework. Thus, one can adopt a linguistic conception without weighing in on debates about analyticity, the relation between questions that are internal to a linguistic framework and those that are external to a framework, and related issues. As I discuss below, I think that endorsing Quine's doctrine of ontological commitment reflects a linguistic conception of ontological issues since, on this view, (a) ontological commitment is revealed by making "verbal reference" to objects, and (b) figuring out

ta-ontological claims are independent of semantic issues (such as whether or not semantic minimalism is true). Further, I sketch two reasons that naturalists have for rejecting a particular manifestation of Quine's linguistic conception: his criterion of ontological commitment. Finally, in Section 5, I bring this discussion to bear on some other, largely independent, critiques of metaphysics of mind, identifying what is right about these critiques and outlining some differences between mainstream metaphysics and my proposed approach.

2. *Quine, Carnap, and meta-ontological deflationism*

In a series of papers, Huw Price (1992; 2007; 2009) has argued that it is a mistake to think, as mainstream metaphysicians do, that Quine successfully defends robust metaphysical inquiry from Carnap's meta-ontological deflationism. He claims that the ontology that Quine has revived is a "pale zombie" of traditional metaphysics and that Quine's attack on Carnap's argument in "Empiricism, Semantics, and Ontology" misses its mark and leaves Carnap's argument "if anything, stronger than before" (2009: 282).[5] Thus, according to Price, the attempted union of methodological naturalism and substantive metaphysical inquiry was broken-backed from the start.[6]

Carnap's argument for meta-ontological deflationism hinges on the distinction between questions that are internal to a linguistic framework and questions that are external to any framework.[7] He claims that external ontological

"what there really is in the world" hinges on regimenting one's total theory into first-order logic (see note 9 and Section 4).

[5] As far as I know, Marc Alspector-Kelly (2001) was the first to offer a detailed argument that Quine's views on ontology are closer to Carnap's than the received view has taken them to be and that "contemporary ontologists … underestimate the deflationary impact of Quine's (and Carnap's) ontological naturalism" (*ibid.*: 94-95). I focus on Price's discussion since he offers a more fully fleshed out positive view of meta-ontological deflationism and the arguments for it.

[6] How best to interpret Quine on metaphysical issues has received some attention in very recent work in the history of late analytic philosophy. For instance, Sander Verhaegh has argued that "both the received view that Quine saved metaphysics *and* the opposite view that Carnap and Quine are on the same anti-metaphysical team" are too one-sided (2017a: 873, italics in original). In his view, Quine, like Carnap, does reject the idea of trying to "ask what reality is really like in a distinctively philosophical way", but he rejects this project for different reasons than Carnap does (*ibid.*). Similarly, Frederique Janssen-Lauret claims that "both heavy-duty metaphysicians and neopragmatist anti-metaphysicians are wrong" about the role Quine played in the development of current metaphysics (2017: 249). However, she seems to think that Quine provides more comfort to "heavy-duty metaphysics" than Verhaegh's interpretation does. For instance, she calls the antimetaphysical reading of Quine "ahistorical" and "historically ill-informed" (*ibid.*: 251). And she claims that Quine "was not an antimetaphysician or flat-footed deflationist, but an interesting, empiricist metaphysician, striving to fit metaphysics around scientific discovery" (*ibid.*: 250).

[7] Verhaegh argues that there are actually two different kinds of external questions (2017a: 879).

questions are merely pseudo-questions if they are intended as theoretical questions with answers that are true or false. Rather, external ontological questions about a class of entities are only intelligible as practical questions about whether it is advisable to adopt the linguistic framework in which discourse about these entities is carried out. Only ontological questions internal to a framework are intelligible as theoretical questions with true or false answers.

Quine rejects any philosophically important distinction between choosing a linguistic framework versus choosing a theory within that framework. So, he rejects a hard-and-fast distinction between the theoretical and the practical. In his view, settling the question of whether numbers exist is subject to the same kinds of consideration as settling the question of whether there is a general "reward system" in the human brain (as opposed to particular reward systems devoted to sex, food, social contact, etc.). In this way, ontological questions are "on a par" with scientific questions. But, as Price notes, this, by itself, should provide cold comfort to traditional metaphysicians: "Quine himself has sunk the metaphysicians' traditional boat, and left all of us, scientists and ontologists, clinging to Neurath's Raft" (2009: 286). "[T]he force of Quine's remarks is not that metaphysics is like science as traditionally (i.e., non-pragmatically) conceived, but that science (at least potentially, and at least in extremis) is like metaphysics as pragmatically conceived" (*ibid*.: 287). Up to this point, it looks like there is little difference between Quine and Carnap. They seem to agree about which kinds of questions are legitimate and to disagree merely about whether those questions should be labeled with traditional terms like 'ontology' (see Quine 1951a: 203-204; Hylton 2007: 236; Verhaegh 2017a: 883).[8]

However, as Price notes, room for a more substantive difference between Quine and Carnap opens up when we consider the possibility of there being a "single grand framework" in which to treat all ontological questions (2009: 287).[9] Sure, Quine would still reject the traditional metaphysical ques-

I am eliding this distinction, but I think what I say about Quine's view of metaphysics is in the same general vicinity as Verhaegh's interpretation.

[8] "Quine's philosophy allows for a revival of what may well look like metaphysics: it makes sense of the question whether there really are numbers, for example, or modal facts. But there is nothing transcendent, or even transcendental, in Quinean metaphysics. … It is, we might say, metaphysics naturalized; in some contexts, indeed, it may seem odd to call it 'metaphysics' at all" (Hylton 2007: 367). This passage occurs in a context in which Hylton draws a contrast between Quine's views and those of Carnap, but this contrast hinges on the idea that, for Carnap, but not for Quine, the concepts of truth and justification are language-relative (*ibid*.: 69ff., 234-236). Contemporary meta-ontological deflationists tend to deny at least *this* kind of language-relativity.

[9] According to Quine: "Various languages are suitable for various purposes; but one language, his fully regimented canonical notation, is appropriate when we are concerned with 'the true and ultimate structure of reality'. That notation is the one to use when our concern is to maximize objectivity, to get at the world as it really is. […] [I]f we have succeeded in choosing the best canonical notation, then our

tion of whether this framework *really* matches up with reality, at least if answering such a question requires us to adopt some supposed perspective outside of science (broadly conceived), the single "grand" framework itself (see Quine 1950: 79; 1992: 405; 1981: 22; Verhaegh 2017a: 884).[10] But this is simply to make the general naturalistic move; it does not entail meta-ontological deflationism. If there is a "single grand framework" and a unique best theory of the world within that framework, then there is nothing to stop us from reinterpreting traditional ontological questions as questions about whether our best theory of the world is committed to a certain kind of entities. If it is so committed, then we could say that those entities *really* exist in the only sense of "really" that makes any sense (see Quine 1954: 229; 1996: 348; Verhaegh 2017a: 884).

So now it looks like Carnap's meta-ontological deflationism depends on whether there is a "principled plurality in language" – on whether the different linguistic frameworks we use to talk about (at least what seem to be) different kinds of entities (especially, mathematical, moral, meaning and mental entities – what Price (1997) calls the "M-worlds") serve importantly different kinds of function while, at the same time, all sharing the core features of descriptive discourse (2009: 289; see also Price 1997: 136-140). That is, it now looks like Carnap's deflationism depends on whether or not what Price calls "functional pluralism" is true, and whether Quine has an argument against deflationism depends on whether he has an argument against functionalism pluralism.

Price suggests that Quine has no such argument. As a naturalist, he "seems poorly placed to reject the suggestion that there might be important functional differences of this kind in language. The issue is one for science" (2009: 294). Anthropologists or biologists will investigate the function of different kinds of human discourse, and "Quine can hardly argue that the results of such investigations may be known a priori" (*ibid.*). Quine does often seem to assume that there is a single, core function of descriptive discourse: playing some role in predicting observations sentences, i.e., sentences that are directly correlated with sensory stimulations. And he assumes that this function can be used to demarcate the realm of the genuinely cognitive or theoretical from the non-cognitive (merely expressive or instrumental) (see Hylton 2007: 22-23). However, Price suggests (drawing here on a passage from Hookway 1988: 68-69), that it is doubtful whether this assumption is

theory as phrased in that notation tells us what there really is in the world" (Hylton 2007: 242).

[10] It is interesting to note that what is, according to Verhaegh (2017b: 337), the first published instance of Quine using Neurath's boat metaphor occurs in (Quine 1950), and he uses it there to support the idea that "we cannot detach ourselves from [our conceptual scheme] and compare it objectively with an unconceptualized reality. Hence it is meaningless, I suggest, to inquire into the absolute correctness of a conceptual scheme as a mirror of reality" (*ibid.*: 79).

consistent with Quine's minimalism or deflationism about truth (if in fact he is a minimalist or deflationist about truth) (Price 2009: 294).[11]

According to Price, then, the viability of meta-ontological deflationism depends, in large part, on whether functional pluralism is true, and this, in turn, is a question that (a) hinges on the nature and function of human *language* and (b) is closely connected to the question of whether truth (and related semantic notions like reference) is a substantive, causal-explanatory property. In the next two sections, I'll argue that Price is mistaken about both (a) and (b).

3. *The seamlessness of knowledge, being "in the same boat", and disciplinary holism*

Quine's main philosophical goal is to reconceive the traditional fundamental problems in epistemology and metaphysics and to solve those problems once they have been so reconceived (Hylton 2007). He adopts a naturalistic "doctrine" that fundamentally informs his pursuit of that goal: the *seamlessness* of knowledge, "the idea that there are no fundamental differences of kind within our body of knowledge" (*ibid.*: 8, 11). This doctrine has a number of important corollaries, including the continuity of common sense and science, the continuity of science and philosophy, and the idea that there is no philosophically important distinction between a priori and a posteriori knowledge.

Importantly, for our purposes, the seamlessness of knowledge implies that all areas of inquiry – all domains that employ descriptive discourse – employ fundamentally the same methods to pursue a common goal.[12] How the functional pluralist responds to this alleged seamlessness depends on what goal the Quinean ascribes to inquiry. If the goal is fairly narrow – like Quine's proposal of predicting sensory stimulations – then the pluralist will argue directly against seamlessness, claiming that some kinds of descriptive discourse are used to achieve other goals. However, if the proposed goal is more general, such as figuring out what the world is like (see note 12), then the pluralist can claim that this superficial unity disguises a deeper kind of diversity.

[11] Janssen-Lauret (2015: 153) claims that Price is wrong to interpret Quine as a minimalist about truth. See also Hylton (2007: 274-278). If the discussion in Sections 3 and 4 below is on the right track, then major questions in meta-ontology are independent of whether semantic minimalism is true.

[12] As Price puts it, "for Quine, *the* significant task of the statement-making part of language is that of recording the conclusions of an activity that is ultimately continuous with natural science" (Price 2011a: 13, italics in original). Quine takes the goal of natural science to be, roughly, predicting sensory stimulations. I take it to be broader than this: roughly, discovering important truths about the world. (See Maddy (2007: 89-91) on the arguably other-than-naturalistic origins of Quine's taking the "sensory stimulations" as "data" or "evidence" for the construction of theories).

That is, the pluralist can claim that this general goal is tied to the core properties of descriptive discourse (which can be accounted for by a minimalist semantic theory). Further, the pluralist can claim that these core properties are "multifunctional", useful not only for asserting what the world is like but also for expressing psychological states with importantly different functional profiles (differences that, nevertheless, according to the functional pluralist, do not entail that such states are not truth-apt or descriptive) (Price 1997: 138-141). Relatedly, it seems that functional pluralism need not entail that there are clear-cut joints between domains of descriptive discourse with different functions (although Price sometimes writes in a way that suggests that it does; see 2004: 199 and 2009: 293). Pluralism can allow that the boundary between functional domains is a "vague matter of degree" (Quine 1995: 257), as (this version of) seamlessness claims, while still insisting that there are cases in which two domains of descriptive discourse serve clearly different functions.

So, if the Quinean concedes that predicting experience is too narrow a goal for all of descriptive discourse, then she needs a stronger version of seamlessness if it is to serve as a kind of functional monism that contrasts with functional pluralism. I think that Quine does endorse such a strong version when embraces a "drive" for "the unity of science" or "a unified all-purpose ontology" (*ibid.*: 260). As Hylton points out, this idea – that "[t]he various parts of knowledge … [form] a single integrated whole" – "functions for Quine as a regulative ideal; is not an established fact, and not a requirement of our having knowledge at all, but it is something towards which we should strive" (2007: 24).

One might think that, as a regulative ideal or methodological directive, this strong version of seamlessness is not something that could be true or false – that it is a non-cognitive stance that is distinct from, and independent of, cognitive, theoretical statements. However, Quinean naturalists will reject this, due to (something like) the "reciprocal containment" of epistemology (and methodology) and ontology (*ibid.*: 21-22; Quine 1969: 83). The "description of the theory-building process" (and the theory-building process itself, with its methodological norms) is not independent of the "theory that is being built". On this view, there is no coherent second-order, purely philosophical or independent, project of investigating, systematizing, and regimenting the first-order, scientific project of finding out about the world; such investigation, systematization, and regimentation *is* just part of finding out about the world (Hylton 2007: 6; Maddy 2007).[13] So, the Quinean naturalist will agree with Price that the question of

[13] See van Fraassen (2007) for an opposing view that imposes such a distinction. He draws a contrast between "objectifying epistemology", "an attempt to come up with a theory of cognition,

whether this strong version of seamlessness should be accepted is "one for science". As Quine puts it, it is a question "within science itself" (1995: 260).

However, the Quinean should balk at Price's suggestion that whether monism or pluralism is true depends primarily on facts about the function(s) of human *language*. (*Quine* arguably in the end cannot reject this suggestion, given his language-based accounts of scientific theories and of how to go about answering metaphysical questions. However, as I discuss in Section 4 below, I think that the Quine-inspired naturalist has good reasons to reject these accounts). That is, although seamlessness, if it is true, will be *reflected* in a kind of unity at the level of human language, this linguistic unity is derivative from either a broader, non-linguistic unity in the world or a (not-exclusively-linguistic) methodological unity among the disciplines that investigate that world. Quine himself suggests that the monism/pluralism question cannot be settled simply by turning to anthropological or biological investigations of the function(s) of human language when he writes that the "unity of science" question is "more remote from observational checkpoints than the most speculative questions of the hard and soft sciences ordinarily so called" (1995: 260).

Now, formulating monism as the claim that there is a single, all-purpose *ontology* begs the larger question that is at issue, i.e., whether meta-ontological deflationism is true.[14] However, I think that focusing on the not-exclusively-linguistic methods of inquiry offers monists a more promising avenue for defending their view. It does so in at least three ways. First, it gives the monist a way of avoiding the charge that monism is in tension with minimalism about truth. Second, it supports a kind of disciplinary holism, which, as an arguable implication of naturalism, may pose a challenge to any version of meta-ontological deflationism that is based on functional pluralism. Third, since disciplinary holism applies to philosophy itself, it allows the monist to respond to the worry that seeking a unified, all-purpose ontology is inconsistent with naturalism. I will elaborate on these three points in the remainder of this section.

First, recall that if Quine defends monism solely based on the idea that sciences are unified by the fact that they all seek the truth, then the pluralist has a ready reply: once one adopts minimalism about truth, seeking the truth is a

whether naturalistic or metaphysical" and "inquiry into the explication and evaluation of various forms of the 'enterprise of knowledge', concentrating on norms and values that guide rational management of opinion", which issues in a fundamentally non-cognitive, typically tacit, *stance* regarding how inquiry should be conducted (*ibid.*: 364).

[14] Hylton (on behalf of Quine) supports the unity of knowledge as a plausible regulative ideal for inquiry by claiming that "[i]t is, after all, a single world that we attempt to know" (2007: 24). But whether this "world", taken broadly to include numbers and beliefs along with trees and bricks, is a *single* unified domain is precisely what the functional pluralist calls into question (although she would prefer to raise this issue with respect to language).

"thin" goal that is common to the sciences *and* other domains that arguably have radically different functions. Now, however, the monist is proposing that methodological unity will be revealed by a close study of the myriad methods that humans use to produce descriptive accounts of the world. (This proposal, I think, would be of a piece with arguing that armchair reflection is not importantly different from observational or experimental inquiry, for example). Establishing that this kind of unity exists is a large and difficult project in the philosophy of science and philosophy of philosophy (naturalistically construed). Here I am making the fairly small point that the Quinean can avoid potential conflict between methodological monism and semantic minimalism by looking for common threads (precision, replicability, intersubjectivity, robustness or stability across methods, etc.) in all of the not-exclusively-linguistic ways – fieldwork, brain imaging, microscopy, and on and on – in which humans interact with the world.

Second, focusing on the methodological unity in disciplines that produce descriptive discourse opens up pluralists to empirical challenges that they seem to want to avoid. I think that the drive for a seamless field of knowledge implies (or is expressed by) what Jeffrey Roland calls "disciplinary holism" (2007: 430). According to disciplinary holism, no individual discipline is evidentially insulated from any another. There can be both conflicting and converging evidence – both legitimate critique and mutual support – between any pair of disciplines, and the presence of this evidence "significantly enhances the chances of getting things right" (*ibid.*). Any given discipline can potentially contribute to any relatively large-scale question about the nature of the world. Given the way the world actually is, some of these potential contributions are unlikely. For instance, it is very unlikely that human epidemiology will make a significant contribution to resolving the inconsistencies between quantum theory and general relativity. However, any view that endorses disciplinary holism cannot put in principle restrictions on the contributions of any discipline.

However, it seems that functional pluralists attempt to impose these restrictions when they posit deep functional divides within descriptive discourse as a whole. For instance, the pluralist will say that explaining what humans do with moral language should answer any supposedly "metaphysical" questions that we have about moral properties. Due to the fact that moral language performs different functions than scientific language, there is no need to try to locate moral properties in the scientific world. (To think otherwise is to rely on an inaccurate "matching game" or "mirroring" account of language, more on which in Section 4). However, this seems like an attempt to isolate or insulate moral

discourse from scientific discourse.[15] It seems to close off the real possibility that advances in our scientific knowledge will lead to radical changes in how we think about morality.[16]

I do not want to put too much weight on this second point, as it primarily targets the "quietist" elements in some of Price's discussion of pluralism, which other proponents of meta-ontological deflationism have suggested that Price is better off abandoning (e.g., Ismael 2014: 100, 103n28). So, I will now turn to the third point from above: using disciplinary holism to rebut a worry that the search for "a unified all-purpose ontology" (such as physicalism) is inconsistent with naturalism. Rather than being inconsistent, I think that Quine is right that such a search is consonant with naturalism, in that it is "typical of the scientific temper" and "of a piece with the drive for simplicity that shapes scientific hypotheses generally" (1995: 260).

The strong version of seamlessness – the unity of inquiry – applies to naturalistic philosophy itself; philosophy and science are "in the same boat". The idea of being "in the same boat" conveys not only the idea that both philosophy and science are in the same difficult, but thrilling, situation (at sea with no prospect of finding solid, dry ground on which to overhaul the boat of inquiry) but also, at least with respect to some important issues, epistemically on a par. This latter idea is not emphasized as often as the former. When Quine claims that "all scientific findings […] are […] as welcome for use in philosophy as elsewhere" (1969: 127), he is noting the importance of scientific *input* into philosophy, and, taken by itself, this may suggest that philosophy's role is solely to interpret and synthesize this input. However, if both scientists and naturalistic philosophers are "busy sailors" on the boat, then they each can play an active role in rebuilding it (see Quine 1975: 72). As busy sailors, neither has a higher rank than the other, and thus philosophy can also provide (first-order) *output* that influences the (other) sciences.[17] Less metaphorically, the strong version of seamlessness implies that philosophy *itself* is a discipline to which disciplinary holism applies.

[15] Price claims that the meta-ontological deflationist "offers an olive branch to non-naturalists", explaining "in the naturalists' own terms how topics such as morality and meaning might remain high and dry, untouched and unthreatened by the rise of the scientific tide" (1997: 133).

[16] For instance, it might be that some of our moral thought has empirical presuppositions that our best science reveals to be false. For such an argument against deontological moral theory, see Greene (2008). I am not endorsing this particular argument but merely giving it as an example of the kind of naturalistic argument that seems to be in tension with functional pluralism.

[17] This is to reject, at least as a blanket recommendation, Hilary Kornblith's suggestion that philosophers attempt to "construct … theories which are scientifically well informed, rather than attempt to inform the sciences" (1994: 50). I briefly discuss an example, concerning the alleged psychological trait of *self-control*, where it might be helpful for naturalistic metaphysics to "attempt to inform the sciences" below in Section 5.

So seamlessness, via disciplinary holism, implies that, at least with respect to some topics, there is a kind of epistemic *symmetry* between naturalistic philosophy and the other sciences. That is, it allows for cases in which both naturalistic philosophy and the other sciences provide genuine insight into a shared topic of investigation, and neither philosophy nor science is in an epistemically better position than the other, with respect to that topic. In these cases, philosophy and other scientific disciplines should be mutually constraining, and the results from any discipline (including philosophy) should be relevant to the findings of any other.

These points can help dispel the worry that "naturalistic metaphysics" is an oxymoron. The claim that naturalistic philosophy inevitably leads to meta-ontological deflationism can be expressed by claiming that naturalists must "follow the course of science wherever it may lead" and that, according to naturalism, "science tells us what there is" (e.g., Montero 2001: 78; Keil 2003: 255; Gibson 2004: 181). These slogans seem to be in tension with any substantive role for naturalistic metaphysics.[18] For example, given that current physics is false and incomplete, endorsing a global, ontological doctrine like physicalism seems to close off avenues of future scientific inquiry. (For this worry, see Montero 2001 and Maddy 2007: 143).

However, if we understand naturalistic metaphysics as *itself* part of the scientific enterprise, then "following science wherever it may lead" does not require naturalistic metaphysics to follow the *other* sciences wherever *they* may lead. If naturalistic metaphysics of mind is another science of the mind alongside other such sciences, and not logically posterior or secondary to those sciences, then physicalism (or any other empirically well-supported account of the mind-body relation, for that matter) will not represent an arbitrary or unjustified attempt to impose a priori constraints on the future course of science.

This means that "science tells us what there is" is true only if it means something different (and is less informative) than its most straightforward reading. That is, if we accept this slogan we are not claiming that the physics, psychology, chemistry, biology, etc., individually or collectively, tell us what there is. Rather, the *science of naturalistic metaphysics* typically tells us what there is. It is not that naturalistic metaphysicians have some exclusive ownership of ontological questions, nor do they have some special methods of determining what we do, or should, believe in. (More on this below). Rather, they synthesize and

[18] Price claims that Quine's views on ontological commitment should be interpreted as a kind of "ontological quietism – the principle that there is no separate second-order science of ontology, but simply the mundane business of existential quantification carried out by first-order specialists in the course of their working lives" (1992: 50). I return to this issue below.

reflect upon (and in some cases, correct[19]) the methods and results from a wide variety of scientific disciplines, using the skills that training in (naturalistic) philosophy is particularly well suited to provide (Maddy 2007: 115-117). Thus, it is perfectly open to the naturalist to argue that there is a family of inductive arguments for the "unified, all-purpose ontology" of physicalism that draws on ordinary, empirical evidence, and thus that physicalism is not an attempt to impose a priori constraints on the future course of science.

4. *The linguistic conception and Quine's criterion of ontological commitment*

One might worry that the discussion in Section 3 is off base. After all, Quine explicitly takes our general theory of the world to be embodied in language (Hylton 2007: 23-24), and he adopts explicitly linguistic/logical methods in his metaphysical project of attempting to discover "the true and ultimate structure of reality", i.e., regimentation and reformulation of our best theory of the world into the language of first-order logic (*ibid.*: 4, 6, 26). In this way, Quine adopts what Price calls a "linguistic conception" of how to approach metaphysical matters. According to this conception, "the starting point [of metaphysical inquiry] lies in human linguistic practices, broadly construed. Roughly, we note that humans (ourselves or others) employ the term "X" in language, or the concept X, in thought" (Price 2004: 188).

Because of this, Quine is vulnerable to Price's argument that the viability of Quine's drive for "a unified all-purpose ontology" – i.e., what Price calls "object naturalism", the claim that "all there *is* is the world studied by science" (*ibid.*: 185, italics in original) – depends on a controversial account of the way language relates to the world. That is, if we begin with the linguistic conception of metaphysical issues, then substantive naturalistic metaphysics requires what Price calls "Representationalism", "roughly, the assumption that substantial 'word-world' semantic relations are a part of the best scientific account of our use of the relevant terms" (*ibid.*: 190). For, if we see metaphysical issues initially as questions about linguistic usage "then it takes a genuine shift of theoretical focus to get us to an issue about the nature of non-linguistic objects", a shift that can be mediated only by *substantial* semantic properties, if it is mediated by semantic properties at all (*ibid.*). Further, Price thinks that there is good reason to doubt that Representationalism will be validated by our best scientific account of human language use (*ibid.*: 187). If all of this is right,

[19] Maddy mentions feminist critiques of primatology in this regard (2007: 407).

then substantive naturalistic metaphysics rests on faulty semantic foundations.

I have argued that Price provides no good reason to think that naturalists cannot adopt an alternative "material conception" of metaphysical issues as a starting point, as least when those issues concern minds and their properties (Haug 2014a: 352-355). According to the material conception, we are confronted with some alleged phenomenon or entity – such as intentionality, consciousness, or moral goodness – and in light of a commitment to object naturalism, come to wonder how this phenomenon or entity could fit into the natural world. One of the main ideas motivating this approach is that "we know much more about the way the world is than we do about how we know about, or refer to, that world" (Devitt 2010: 2). That is, we are on a firmer epistemic footing concerning even some fairly recondite aspects of the way we humans (non-epistemically and non-referentially) interact with the world than we are concerning our epistemic and semantic relations to the world. And we should start from that solid epistemic footing when we go on to inquire how our minds, in general, fit into the natural world.

If naturalists can coherently adopt the material conception of metaphysical issues, then Price's worries about Representationalism are rendered otiose. Starting from the material conception, substantive naturalistic metaphysics does not presuppose Representationalism (or minimalism or any other semantic theory), so it is not undermined if Representationalism turns out to be false.[20]

Not only should we naturalists avoid the linguistic conception in general, but we also have good reasons to reject a particular form that that linguistic conception takes in Quine's own philosophy: his criterion of ontological commitment (i.e., his procedure for how to go about engaging in ontological disputes, which is often summarized by the slogan "to be is to be value of a variable"; see van Inwagen 2009). I'll briefly discuss two such reasons here. (For a little more detail, see Haug 2014a).

First, when Quine introduces the problem of determining one's ontological commitments, he does so by talking about which entities we "assume" (Quine 1981: 2) or "believe in" (Quine 1951b: 44; 1981: 21).[21] However, Quine argues

[20] I think that Penelope Maddy's Second Philosopher is an example of someone who endorses semantic minimalism (2007: 164-165) (and thus would reject Representationalism) but rejects meta-ontological deflationism. "[W]hen the question is ontology, her focus is on what there is, not on what various people are inclined to think or say there is" (*ibid.*: 399). Further, she does not rest content with the quantification of other "first-order specialists". "Second Metaphysics is emphatically not a purely descriptive enterprise: the Second Philosopher holds [definite views about when the existence of atoms was established], not merely that scientists thought this or that at various times" (*ibid.*: 403).

[21] He also writes of which entities we "accept", "acknowledge", "admit", "countenance", "hypostatize", "posit", "presuppose", "reify", or "reckon" (For this list and references see Szabó 2003: 585).

for a reductive account of this psychological attitude, in which one's ontological commitments are ultimately determined by the existentially quantified sentences that are logically entailed by the theory that one believes. As Quine puts it, "what had been a question of assuming objects becomes a question of verbal reference to objects" (Quine 1981: 2). Given that this reductive account is largely motivated by Quine's behaviorism and his accompanying suspicion of intentional mental states (both of which are now widely, and rightly in my view, rejected in the sciences of the mind), I think that contemporary naturalists should be suspicious of it.

Further, Szabó (2003; 2010) has given strong arguments for the idea that we cannot safely ignore the difference between believing in (or being ontologically committed to) things and referring to (or quantifying over) them – that it is not enough to quantify over a purported entity in order to evince an ontological commitment to it. Roughly, on Szabó's most recent published view, ontological commitment to Xs requires believing that Xs exist *and* being able to explain why Xs exist, which requires knowing what Xs *are*—knowing what their nature is (Szabó 2010, esp. 37-38).[22] Importantly, as I discuss below, knowing what Xs are may require significant empirical investigation.

The second reason that naturalists have to reject Quine's criterion is that it does not accurately capture the way that rational debates about ontological commitment have actually been carried out in the sciences (Maddy 1997: 135-143; 2007: 95-97, 398-407). For instance, Maddy argues that work on atomic theory at the turn of the twentieth century shows that "Pace Quine, determining what our successful theories tell us about what there is cannot be a simple matter of reading off their existential claims" (2007: 107).

Maddy frames her critique of Quine as aimed primarily at his confirmational holism (e.g. 2007: 95). However, she apparently does not see much, if any, substantive difference between her way of putting things and one that takes aim directly at Quine's criterion of ontological commitment (1997: 143; 2007: 95n20).[23] I suggest that Maddy's argument is best interpreted as motivat-

[22] I agree with Szabó that being ontologically committed to Xs requires knowing what Xs are, but I am unsure if it also requires being able to explain why the claim that Xs exist is true. For, if Xs are fundamental, it is not clear to me that the claim that Xs exist has any explanation at all. (Perhaps one could say that adverting to Xs' fundamentality is itself an explanation for why Xs exist, but I am not sure whether this is an adequate explanation or not). Szabó also claims that he has "grown dissatisfied that [the conclusions in his 2003 paper] are so tightly connected to semantic considerations" (2010: 39n13). He does not give a reason for this dissatisfaction, but the above suggestion that we should reject the linguistic conception of metaphysical issues would provide one.

[23] Maddy (1996: 333; 1997: 143) also suggests that her critique might be taken to be aimed at the univocality of either "there is" or the existential quantifier (both of which are fundamental tenets of Quinean meta-ontology; see van Inwagen 2009).

ing a position similar to Szabó's, according to which scientists in 1900 were right to believe that there are atoms but also correct to withhold ontological commitment to atoms (in this case because detection of atoms, access to their behavior as individuals, awaited the work of Jean Perrin). But whether we deny that entire theories are confirmed as a unit or deny that we are committed to everything that our theories quantify over, the important point for my purposes is that either critique undermines the idea that we can simply read ontological commitments off of what a theory says there is.[24]

One important upshot of rejecting Quine's criterion is that we can rest content with "the mundane business of existential quantification carried out by first-order specialists in the course of their working lives" (Price 1992: 50), but this does now *not* amount to meta-ontological deflationism. That is, we can accept that certain "cheap" arguments for the existence of various entities are sound without thereby taking the relevant ontological questions to be settled (cf. Szabó 2003: 591). This makes room for a non-deflationary conception of naturalistic metaphysics that avoids Price's (2007) false dilemma between a "thin" metaphysics that merely acquiesces in the ontological verdicts of the other sciences and a "thick" metaphysics that uses only logical methods of regimentation and supposedly stands outside of science altogether. In the next section, I will outline how these reasons to reject Quine's criterion of ontological commitment lead to other differences with the Canberra Plan and mainstream metaphysics of mind.

5. *The metaphysics of mind is dead. Long live the metaphysics of mind!*

Largely independently of meta-ontological deflationism, in the last couple of decades a number of philosophers have criticized the practices and debates that had become dominant in philosophy of mind beginning in the 1960s. These attacks are not on metaphysics *per se* but on the focus on certain kinds of metaphysical questions and the attempt to answer these questions without any significant input from the sciences.

For instance, in an amusing and provocative critical notice of Jaegwon Kim's 1998 book *Mind in a Physical World*, Clark Glymour criticizes what he calls "mainstream-metaphysical-philosophy-of-mind", of which he takes Kim's book to be an exemplary instance (1999: 458, 471). In Glymour's view, this

[24] Verhaegh (2017b) claims that evidential holism is one of the three main commitments that provide support for Quine's naturalism. If this is right, it may provide some support for framing the critique in the latter way, since framing it in the former way (as a critique of holism) would undermine some of Quine's support for naturalism.

kind of philosophy of mind is misguided because it is "walled off from any real use of (or for) mathematics or the sciences. Aside from a bit of formal logic, to be informally used, and the philosophical tradition itself, the philosopher faces the dragons in the labyrinth of metaphysics armed only with words and a vivid imagination" (*ibid*.: 458). He disparages Kim's project as merely "interior redecoration" of a house whose foundations are "adrift". To shore up these foundations, Glymour suggests, philosophers of mind need to take a more naturalistic approach and "build on science" (*ibid*.: 471).

Similar attacks on mainstream-metaphysical-philosophy-of-mind have appeared in the intervening years. For instance, in a paper published in 2008, Antony Chemero and Michael Silberstein declare that "[t]he philosophy of mind is over", by which they mean that metaphysical debates about the mind-body problem and the nature of mental content have reached a standstill (1). They applaud the replacement of these "relatively armchair discussions" with "empirically oriented debates in philosophy of the cognitive and neural sciences" (*ibid*.). Indeed, there is some empirical evidence that there has been a "dramatic shift" in the methods philosophers of mind employ and the topics they investigate: away from supposedly "a priori reasoning" on "distinctively philosophical questions regarding the metaphysics of mind" toward engagement with "the results of empirical studies" on "questions about the workings of specific cognitive processes" (Knobe 2015: 36).

By drawing on my discussion above, I think that we can identify what is right in these critiques as well as a way in which they go wrong. Doing so will distinguish my approach to naturalistic metaphysics from the Canberra Plan and other prominent versions of mainstream metaphysics. In short, on my approach we still need "armchair" work in naturalistic metaphysics to make progress, even on some questions about the "workings of specific cognitive processes". However, this work is not a priori in any interesting sense: to do it well we need to pay close attention to the results of (usually a wide variety of) empirical studies.

First, I will illustrate how my approach to naturalistic metaphysics does not employ a priori conceptual analysis (and thus is armed with more than "words and a vivid imagination") by contrasting it with the Canberra Plan. When the Canberra Planner attempts to locate some apparently non-natural phenomenon within the natural world, she proceeds in two cleanly separable stages. She begins with the "platitudes" or the "folk theory" about that phenomenon. These platitudes are supposed to be in principle knowable to any competent user of a term for the phenomenon. Collecting these platitudes together delivers the functional or semantic role for that term. After this a priori conceptual analysis is complete, she turns to the empirical sciences in the second stage

to tell us what natural phenomenon, if any, satisfies the platitudinous or folk-theoretic description. Importantly, the sciences are not in the business of substantively revising the deliverances of common sense (or, if they do provide such revisions, they have, in effect, merely changed the subject).

This view about the evidential role of common sense in metaphysical inquiry stems, at least in part, from some of David Lewis's views:[25]

> One comes to philosophy already endowed with a stock of opinions. It is not the business of philosophy either to undermine or to justify these preexisting opinions, to any great extent, but only to try to discover ways of expanding them into an orderly system. A metaphysician's analysis of mind is an attempt at systematizing our opinions about mind. It succeeds to the extent that (1) it is systematic, and (2) it respects those of our pre-philosophical opinions to which we are firmly attached. (1973: 88)

Given Quine's claim that philosophy is continuous with common sense, it may seem that he also endorses this role for common sense. However, I think that this is a mistake. As Hylton puts the point: "Fundamental to Quine's view is the idea that our ordinary ways of thinking, just as we find them, should not be taken as telling us the way the world is" (2007: 367).[26]

Quine claims that folk psychology does not carry ontological commitments—that it is too vague and imprecise and must await revision and systematization "in light of the ideal of a systematic, overarching, and empirically based theory of the world" (Hylton 2007: 367). Usually this is portrayed as a broadly logical task for philosophy of science, as merely bringing formal rigor to the fixed content of a theory by regimenting the folk theory into first-order logic. However, if the discussion above in Section 4 is correct, naturalistic metaphysics has another way, aside from logical regimentation, to undermine an apparent ontological commitment of common sense or "folk theory" to a

[25] The historical origins of the idea that contemporary analytic philosophers (since sometime around the mid-1960s) rely on pre-philosophical "intuitions" as a source of evidence for philosophical theories have not been fully identified. Hintikka (1999) claims that Noam Chomsky's linguistic theory and its methodology are important sources for this idea, but I think that Herman Cappelen is right that this is merely an "interesting suggestion" that awaits support from "a more detailed historical investigation" than Hintikka provides (2012: 22-23). I suspect that Chomsky's influence on philosophy may have been at least partially mediated by John Rawls, especially his account of the role of "considered judgments" in reflective equilibrium (see Rawls 1999: 41). See Hylton (2007: 380n4) for an interesting suggestion about why the use of the word 'intuition' may have caught on.

[26] Elaborating on this point, Hylton writes that "By relying on unreconstructed common sense, or on 'intuition', [some versions of mainstream metaphysics reinstate] metaphysics with no reliable [empirical] constraints… There is considerable historical irony here. … [By undermining] the idea that there was a basis on which attempts at metaphysics could be definitely ruled out as meaningless … Quine's work may well have had the effect of encouraging a revival of just the sort of metaphysics which he would most strongly oppose" (*ibid.*).

kind of entity. Namely, it can do so by showing that common sense is mistaken about the nature of that entity, that it is mistaken about what those entities are (or would be, if they were to exist).

Many philosophers have argued that the Canberra Plan requires *too much* of solutions to location problems by claiming that they require the "base" or "preferred" facts to *a priori entail* the facts that are to be located. But the current point is that the Canberra Plan in another way requires *too little*. For, it holds that ontological commitments are easier to accrue than they in fact are. On the Canberra Plan, the ontological commitments of a theory, say, folk psychology, are already determined in the first stage, once we have formed the "Ramsey sentence" that states the functional/semantic role of the term to be located. If the above discussion of ontological commitment is on the right truck, this need not be true. For, one could think that folk psychology is by-and-large true but that it still does not accurately capture the nature of some of the mental entities it concerns. This is enough to warrant believing that those mental entities exist while withholding full-blown ontological commitment to them.

Thus, I think that empirical inquiry often plays a more extensive role in determining one's ontological commitments than even many critics of the Canberra Plan envisage. It is not just that empirical inquiry is needed to determine what, if anything, satisfies the functional role associated with a mental state. Empirical inquiry – drawn from the full range of the sciences of the mind and integrated by naturalistic metaphysicians – is also needed to determine if this functional role provides an accurate account of what the mental state is, and thus whether we are ontologically committed to it, in the first place.[27]

Importantly, none of this implies that we should rely less on work done "from the armchair". It is just that we need to pay more attention to the results of the empirical sciences (and not rely on our pre-theoretic intuitions or on far-fetched thought experiments) in order to do this work well. For example, one may wonder if it is possible to locate the psychological trait of *being self-controlled* in the natural world. Plausibly, reflection on the common sense lore concerning this trait would reveal that self-controlled individuals are "strong willed" and good at resisting or inhibiting impulses (see Levy 2017: 203-204). However, recent empirical studies suggest that individuals who score high on scales that are supposed to measure trait self-control are actually relatively bad at resisting temptation (Imhoff, Schmidt, and Gerstenberg 2014). Instead of being good at employing "willpower" in their ordinary lives to achieve their long-term goals it seems that such individuals employ other, often implicit,

[27] This may seem to be merely arguing for psychofunctionalism over analytic functionalism, but it is not. For both of these views are consistent with Quine's criterion of ontological commitment.

strategies, like avoiding situations that involve temptation in the first place. Further, recent work on the structure of executive function and its relation to trait "self-control" (and related traits like conscientiousness) suggests that, perhaps contrary to common sense, highly self-controlled individuals are not particularly proficient at the *inhibition* component of executive function but rather show greater cognitive *flexibility* (are more proficient at set shifting) (Fleming, Heintzelman, and Bartholow 2016). (This paper draws on an influential "unity and diversity" model of executive function that comes from cognitive neuroscience (see, e.g., Friedman and Miyake 2017).

Integrating all of this work from social/cognitive psychology and neuroscience is required to figure out what trait self-control *is*, if in fact it is a distinct character trait at all, and it will involve a lot of reflection from the "armchair", but it is no less empirical for that. Naturalistic metaphysicians contribute to the "first-order" study of the mind not by employing some distinctively philosophical methods (much less a priori "intuition") but by synthesizing results from disparate fields and, often, connecting up those results with relevant discussions from the history of philosophy.

So, with Carnap and against Quine, we should admit that ontological commitments cannot be read off the existential commitments of the total theory one accepts. But, with Quine and against Carnap, we should acknowledge that ontological inquiry is not conceptually prior to scientific inquiry. One's ontological commitments are still determined by the total theory one accepts. However, that determination proceeds differently than the procedure dictated by Quine's criterion of ontological commitment. It is not enough to simply look at the existentially quantified sentences that are implied by that total theory. We must also look at the *content* of the theory, at what the theory tells us the relevant entities *are.* When engaging in ontological disputes, we should approach our total theory primarily in the "material mode" – as telling us what the world is like – and not in the "formal mode" – as a linguistic object to investigate. Instead of beginning with how we *talk* about minds, as the Canberra Plan does (much less *ending* with such talk, as Price's approach does), my approach sees location problems from the start as questions about the relation between (putative) entities in the world.

Turning to the second reason to reject Quine's criterion of ontological commitment provides further support for the recent trend in the philosophy of mind of focusing on the "workings of specific cognitive processes". For, this reason goes along with the idea that my naturalistic metaphysician is "born native" to the contemporary scientific worldview rather than later electing "to enlist [in the naturalistic project], perhaps in reaction to some deep disappointment [or despair about traditional philosophy]" (Maddy 2007: 14, 85). As

Maddy continues:

> This may seem a fine point, but it's important to maintain the distinction between 'I believe in atoms because I believe in science and it supports their existence' (as the enlistee might say) and 'I believe in atoms because Einstein argued so-and-so, and Perrin did experiments such-and-such, with these results' (as the [naturalistic metaphysician] says). (Maddy 2007: 85-86)

Because of this, my naturalistic metaphysician will be suspicious of global or "explanationist" defenses of realism that are supposed to apply to science across the board. It is not that the methods (including, most notably, inference to the best explanation) that such defenses use are unacceptable or unreliable. Rather, it is that the question that such defenses seek to address is not one that a consistent naturalist should even deign to answer.[28] Note, however, that refusing to rise to the bait of a "second order" kind of existence question does not compel naturalistic metaphysicians to stop thinking about general metaphysical issues like the mind/body problem. Rather, it just reconfirms the idea that thinking about the mind/body problem should be informed by significant engagement with all of the relevant sciences.

6. *Conclusion*

Michael Friedman claims that Quine, "the great opponent of the analytic/synthetic distinction[,] unwittingly made room for essentially a priori philosophizing through the back door" (2010: 544n17). That is, according to Friedman, Quine's confirmational holism allows mainstream metaphysicians to claim that "[their] armchair philosophizing merely occupies an especially central and abstract level in our total empirical theory of the world – and, as such, it operates within the very same constraints, of overall 'simplicity' and 'explanatory power,' governing ordinary empirical theorizing" (*ibid*.).

If Price's interpretation of Quine were right, this would not represent the vindication of metaphysical inquiry that it seems to be. I have suggested a way in which Quine himself, by defending a strong kind of unity of science, may

[28] As Maddy puts the point (in the context of debates about scientific realism between van Fraassen and Boyd), the problem is that global explanationist realism "grants van Fraassen too much at the outset, in particular, it buys into his 'stepping back' to the 'epistemic stance', and as a result, it implicitly grants that the Einstein/Perrin evidence isn't enough by itself, that it stands in need of supplementation. Once this move is made, the game is lost … Even if the Realist's effort to answer van Fraassen is couched in purely naturalistic terms, he has betrayed his naturalism the moment he allows that evidence like Einstein and Perrin's is inadequate" (2007: 310-311). For some recent discussion of "explanationism" in science and metaphysics, see Reutsche (2016) and Saatsi (2017).

be able to avoid the slide into meta-ontological deflationism. Whether or not this defense is successful on Quine's own terms, I think that contemporary naturalists are in a much better position to defend substantive metaphysical inquiry. However, if I am right, this defense will result in significant changes to (at least some versions of) "mainstream" metaphysical inquiry. My naturalistic metaphysician's attitude toward the practices of mainstream metaphysics that she finds suspect will be Quinean in spirit. She will not try to provide a clear-cut criterion by which to rule them out as meaningless or misguided but rather will investigate them on a case-by-case basis to see if they are likely to achieve the goals that they intend to. This is messier than many proponents of a broadly "scientific philosophy" would like, but it is, I think, the best that we can hope for.

Matthew C. Haug
mchaug@wm.edu
The College of William & Mary

References

Alspector-Kelly, Marc, 2001, "On Quine on Carnap on Ontology", in *Philosophical Studies*, 102: 93-122.

Blatti, Stephan, Sandra Lapointe, eds., 2016, *Ontology after Carnap*, Oxford University Press, Oxford.

Cappelen, Herman, 2012, *Philosophy without Intuitions*, Oxford University Press, Oxford.

Carnap, Rudolf, 1950, "Empiricism, Semantics, and Ontology"; Repr. in *Meaning and Necessity*, 2nd ed., 1956, Chicago University Press, Chicago: 205-221.

Chalmers, David J., David Manley, Ryan Wasserman, eds., 2009, *Metametaphysics: New Essays on the Foundations of Ontology*, Clarendon Press, Oxford.

Chemero, Anthony, Michael Silberstein, 2008, "After Philosophy of Mind: Replacing Scholasticism with Science", in *Philosophy of Science*, 75: 1-27.

Devitt, Michael, 2010, *Putting Metaphysics First*, Oxford University Press, Oxford.

Fleming, Kimberley A., Samantha J. Heintzelman, Bruce D. Bartholow, 2016, "Specifying Associations Between Conscientiousness and Executive Functioning: Mental Set Shifting, Not Prepotent Response Inhibition or Working Memory Updating", in *Journal of Personality*, 84: 348-360.

Friedman, Michael, 2010, "Logic, Mathematical Science, and Twentieth Century Philosophy: Mark Wilson and the Analytic Tradition", in *Noûs*, 44: 530-544.

Friedman, Naomi P., Akira Miyake, 2017, "Unity and Diversity of Executive Functions: Individual Differences as a Window on Cognitive Structure", in *Cortex*, 86: 186-204.

Gibson, Roger F, Jr., 2004, "Quine's Behaviorism cum Empiricism", in Gibson, Roger

F. Jr., ed. *The Cambridge Companion to Quine*, Cambridge University Press, Cambridge: 181-199.

Glymour, Clark, 1999, "A Mind Is a Terrible Thing to Waste", in *Philosophy of Science*, 66: 455-471.

Greene, Joshua, 2008, "The Secret Joke of Kant's Soul", in Sinnott-Armstrong, Walter, ed., *Moral Psychology*, vol. III, MIT Press, Cambridge MA: 35-80.

Haug, Matthew, 2014a, "Must Naturalism Lead to a Deflationary Meta-Ontology?", in *Metaphysica*, 15: 347-367.

Haug, Matthew, 2014b, "On the Prospects for Ontology: Deflationism, Naturalism, and Carnap's Principle of Tolerance", in *European Journal of Philosophy*, 22: 593-616.

Hintikka, Jaako, 1999, "The Emperor's New Intuitions", in *Journal of Philosophy*, 96: 127-147.

Hirsch, Eli, 2011, *Quantifier Variance and Realism*, Oxford University Press, Oxford.

Hofweber, Thomas, 2016, *Ontology and the Ambitions of Metaphysics*, Oxford University Press, Oxford.

Hookway, Christopher, 1988, *Quine*, Stanford University Press, Stanford CA.

Hylton, Peter, 2007, *Quine*, Routledge, London.

Imhoff, Roland, Alexander F. Schmidt, Friederike Gerstenberg, 2014, "Exploring the Interplay of Trait Self-Control and Ego Depletion: Empirical Evidence for Ironic Effects", in *European Journal of Personality*, 28: 413-424.

Ismael, Jenann, 2014, "Naturalism on the Sydney Plan", in Haug, Matthew, ed., *Philosophical Methodology: The Armchair or the Laboratory?*, Routledge, London: 86-104.

Jackson, Frank, 1998, *From Metaphysics to Ethics*, Clarendon Press, Oxford.

Janssen-Lauret, Frederique, 2015, "Meta-Ontology, Naturalism, and the Quine-Barcan Marcus Debate", in Kemp, Gary, Frederique Janssen-Lauret, eds., *Quine and His Place in History*, Palgrave Macmillan, Basingstoke, 146-167.

Janssen-Lauret, Frederique, 2017, "The Quinean Roots of Lewis's Humeanism", in *The Monist*, 100: 249-265.

Keil, Geert, 2003, "'Science Itself Teaches'. A Fresh Look at Quine's Naturalistic Metaphilosophy", in *Grazer Philosophische Studien*, 66: 253-280.

Kornblith, Hilary, 1994, "Naturalism: Both Metaphysical and Epistemological", in *Midwest Studies in Philosophy*, 19: 39-52.

Knobe, Joshua, 2015, "Philosophers are Doing Something Different Now: Quantitative Data", in *Cognition*, 135: 36-38.

Laugier, Sandra, 2009, "Science and Realism: The Legacy of Duhem and Meyerson in Contemporary American Philosophy of Science", in Brenner, Anastasios, Jean Gayon, eds., *French Studies in the Philosophy of Science*, Springer, Dordrecht: 91-112.

Levy, Neil, 2017, "Of Marshmallows and Moderation", in Sinnott-Armstron, Walter, Christian B. Miller, eds., *Moral Psychology*, vol. 5: *Virtue and Character*, MIT Press, Cambridge MA: 197-213.

Lewis, David, 1973, *Counterfactuals*, Harvard University Press, Cambridge MA.

Maddy, Penelope, 1996, "Ontological Commitment: Between Quine and Duhem", in *Philosophical Perspectives*, 10: 317-341.

Maddy, Penelope, 1997, *Naturalism in Mathematics*, Clarendon Press, Oxford.

Maddy, Penelope, 2007, *Second Philosophy*, Oxford: Oxford University Press.

Manley, David, 2009, "Introduction: A Guided Tour of Metametaphysics", in Chalmers, David J., David Manley, Ryan Wasserman, eds., 2009: 1-37.

Mills, M. Anthony, 2015, "Explicating Meyerson: The Critique of Positivism and Historical *Épistémologie*", *HOPOS*, 5: 318-347.

Montero, Barbara, 2001, "Post-Physicalism", in *Journal of Consciousness Studies*, 8: 61-80.

Neurath, Otto, 1932/3, "Protocol Statements" ("Protokollsätze"), in *Erkenntnis*, 3: 204-214; Repr. in *Philosophical Papers 1913-1946*, Robert S. Cohen, Marie Neurath, eds. and trans., 1982, Reidel, Dordrecht: 91-99.

Price, Huw, 1992, "Metaphysical Pluralism", in *Journal of Philosophy*, 89: 387-409. Repr. in Price, 2011b: 34-53.

Price, Huw, 1997, "Naturalism and the Fate of M-Worlds", in *Proceedings of the Aristotelian Society*, *Supplementary Volume*, 71: 247-67; Repr. in Price, 2011b: 132-47.

Price, Huw, 2004, "Naturalism without Representationalism", in De Caro, Mario, David Macarthur, eds., *Naturalism in Question*, Harvard University Press, Cambridge MA: 71-88. Repr. in Price 2011b: 184-199.

Price, Huw, 2007, "Quining Naturalism", in *Journal of Philosophy*, 104: 374-402.

Price, Huw, 2009, "Metaphysics After Carnap: The Ghost Who Walks?", in Chalmers, David J., David Manley, Ryan Wasserman, eds., 2009: 320-46; Repr. in Price, 2011b: 280-303.

Price, Huw, 2011a, "Moving the Mirror Aside", in Price, 2011b: 3-33.

Price, Huw, 2011b, *Naturalism without Mirrors*, Oxford University Press, Oxford.

Quine, W.V.O., 1948, "On What There Is"; Repr. in *From a Logical Point of View*, 1980, Harvard University Press, Cambridge MA: 1-19.

Quine, W.V.O., 1950, "Identity, Ostension, and Hypostasis"; Repr. in *From a Logical Point of View*, 1980, Harvard University Press, Cambridge MA: 65-79.

Quine, W.V.O., 1951a, "On Carnap's Views on Ontology"; Repr. in *The Ways of Paradox and Other Essays*, 1976, Harvard University Press, Cambridge MA: 203-211.

Quine, W.V.O., 1951b, "Two Dogmas of Empiricism"; Repr. in *From a Logical Point of View*, 1980, Harvard University Press, Cambridge MA: 20-46.

Quine, W.V.O., 1954, "The Scope and Language of Science"; Repr. in *The Ways of Paradox and Other Essays,* 1976, Harvard University Press, Cambridge MA: 228-245.

Quine, W.V.O., 1960, *Word and Object*, MIT Press, Cambridge MA.

Quine, W.V.O., 1969, "Natural Kinds", in *Ontological Relativity and Other Essays*, Columbia University Press, New York: 114-138.

Quine, W.V.O., 1975, "The Nature of Natural Knowledge", in Guttenplan, Samuel, ed., *Mind and Language*, Clarendon Press, Oxford: 67-81.

Quine, W.V.O., 1981, *Theories and Things*, Belknap/Harvard University Press, Cam-

bridge MA.

Quine, W.V.O., 1992, "Structure and Nature", in Føllesdal, Dagfinn, Douglas B. Quine, eds., *Confessions of a Confirmed Extensionalist and Other Essays*, 2008, Harvard University Press, Cambridge MA: 401-406.

Quine, W.V.O., 1995, "Naturalism, or Living within One's Means", in *Dialectica*, 49: 251-261.

Quine, W.V.O., 1996, "Quine /'kwaɪn/, Willard Van Orman (b. 1908)"; Repr. in Føllesdal, Dagfinn, Douglas B. Quine, eds., *Quine in Dialogue*, 2008, Harvard University Press, Cambridge MA: 461-472.

Rawls, John, 1999, *A Theory of Justice*, rev. ed. Belknap/Harvard University Press, Cambridge MA.

Reutsche, Laura, 2016, "Physics and Method", in Cappelen, Herman, Tamar Szabó Gendler, John Hawthorne, eds., *The Oxford Handbook of Philosophical Methodology*, Oxford University Press, Oxford: 465-485.

Roland, Jeffrey W., 2007, "Maddy and Mathematics: Naturalism or Not", in *British Journal for the Philosophy of Science*, 58: 423-450.

Saatsi, Juha, 2017, "Explanation and Explanationism in Science and Metaphysics", in Slater, Matthew H., Zanja Yudell, eds., *Metaphysics and the Philosophy of Science*, Oxford University Press, Oxford: 163-191.

Sider, Theodore, 2009, "Ontological Realism", in Chalmers, David J., David Manley, Ryan Wasserman, eds., 2009: 384-423.

Sider, Theodore, 2011, *Writing the Book of the World*, Clarendon Press, Oxford.

Szabó, Zoltan Gendler, 2003, "Believing in Things", in *Philosophy and Phenomenological Research*, 66: 584-611.

Szabó, Zoltan Gendler, 2010, "The Ontological Attitude", in Czarnecki, Tadeusz, Katarzyna Kijania-Placek, Olga Poller, Jan Woleński, eds. *The Analytic Way: Proceedings of the 6th European Congress of Analytic Philosophy*, King's College Publications, London: 21-42.

Thomasson, Amie L., 2014, *Ontology Made Easy*, Oxford University Press, Oxford.

Uebel, Thomas, E., 1991, "Neurath's Programme for Naturalistic Epistemology", in *Studies in the History and Philosophy of Science*, 22: 623-646.

van Fraassen, Bas C., 2007, "From a View of Science to a New Empiricism", in Monton, Badley, ed., *Images of Empiricism*, Oxford University Press, Oxford: 337-384.

van Inwagen, Peter, 2009, "Being, Existence, and Ontological Commitment", in Chalmers, David J., David Manley, Ryan Wasserman, eds., 2009: 472-506.

Verhaegh, Sander, 2017a, "Blurring Boundaries: Carnap, Quine, and the Internal-External Distinction", in *Erkenntnis*, 82: 873-890.

Verhaegh, Sander, 2017b, "Boarding Neurath's Boat: The Early Development of Quine's Naturalism", in *Journal of the History of Philosophy*, 55: 317-342.

Verhaegh, Sander, 2017c, "Quine on the Nature of Naturalism", in *The Southern Journal of Philosophy*, 55: 96-115.

The resurgence of metaphysics in late analytic philosophy: A constructive critique

Dionysis Christias

Abstract: The purpose of the first part of this paper is to examine the major turning point events that transformed the attitude of analytic philosophers towards metaphysical discourse. We will focus on one such turning point, the modal revolution, based on the resources of possible world semantics, developed by Kripke (who devised suitable models for modal logic) and by philosophers such as Lewis and Plantinga (who offered influential metaphysical interpretations of those models). We shall see how the modal revolution, by bringing an unprecedented change in the way in which modal notions were understood by analytic philosophers, was central to the revival of metaphysics in contemporary philosophy. Yet, analytic philosophers encountered serious obstacles in their attempt to understand the ontological and epistemological foundations and implications of one of the most basic notions of the modal revolution, that of a possible world. In the second part of the paper, it will be argued that, surprisingly enough, the work of the pre-Kripkean "middle" analytic philosopher Wilfrid Sellars, especially as interpreted and reconstructed by Robert Brandom, can perhaps throw light on the semantic, epistemic and ontological dimension of possible world talk. Sellars does this mainly through 1) (what Brandom calls) the "Kant-Sellars thesis about modality", 2) his understanding of modal discourse as non-descriptive, expressive, categorial and "metaliguistic", and 3) his nominalism about abstract entities. Thus, it will be suggested that the implications of this Sellars-inspired position are such that make it an unexpectedly relevant and novel contribution to contemporary debates in analytic metaphysics.

Keywords: modal revolution; possible worlds metaphysics; Kant-Sellars thesis about modality; nominalism.

1. *Introduction*

In this paper, I shall first describe what is commonly called the "modal revolution" in analytic philosophy, inaugurated mainly by figures such as Saul Kripke and David Lewis in the 1960s. The modal revolution, based on the resources of possible world semantics (first developed by Kripke 1959; 1963, who devised suitable models for modal logic, and Lewis 1968, who offered a

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metaphysical interpretation of those models), brought a sea change in the way in which modal notions (possibility, necessity, contingency) were understood by analytic philosophers, and was central to the revival of analytic metaphysics, a philosophical research area still thriving in contemporary analytic philosophy. I shall then proceed by offering what I take to be a constructive critique of this whole line of thought about alethic modality (which stands behind the resurgence of analytic metaphysics), based on the works of a relatively neglected "middle" analytic philosopher, Wilfrid Sellars, and of a "late" analytic philosopher whose work is deeply influenced from Sellars (which probably explains why he is a minority figure in contemporary analytic philosophy), namely Robert Brandom. I will suggest that Sellars' work, especially as interpreted and reconstructed by Brandom for his own philosophical use, contains valuable insights about alethic modality, which can significantly contribute to discussions on the philosophical foundations of contemporary analytic metaphysics. Finally, based on a version of Sellarsian nominalism about abstract entities, I will explore the possibility of providing an account of possible world metaphysical talk, which though ultimately nominalistic, acknowledges the reality of modal phenomena and attempts to legitimate rather than eliminate them (i.e. it attempts to show what modal phenomena "really are" rather than that there are no such things as modal phenomena).

2. *The modal revolution in analytic philosophy*

It is interesting to be reminded of the fact that, as a result of the modal revolution in analytic philosophy, virtually all analytic philosophers nowadays not only do not have any reservations about the intelligibility of modal notions but, even more radically, they make free use of them – i.e. they consider them as unproblematically available – to explain philosophically puzzling phenomena, such as the semantic status of normative or intentional vocabulary. It is surely worthwhile to remind ourselves how surprised early or "middle" analytic philosophers would be to find out that by the end of the 20th century modal notions such as dispositions, counterfactual dependencies and nomological relations would be considered as unproblematically available to explain allegedly more puzzling phenomena such as the semantic status of normative, intentional or even semantic vocabulary itself. For up until the late 1960s, most analytic philosophers were highly suspicious of modal notions. A whole tradition of 20th-century analytic philosophy, from Russell, through Carnap and the other logical positivists to Quine (himself an ardent critic of logical positivism), expressed serious reservations about the very intelligibility of modal concepts.

The latter were not exactly considered to be the paradigm of clarity, and the only way for them to be philosophically unproblematic would be by being reduced to decidedly non-modal notions.

This extreme suspicion of the legitimacy of modal talk was a consequence of the fact that most of early and middle analytic philosophers were part of a broadly *empiricist* philosophical tradition.[1] Early analytic philosophers – from Russell, through Carnap and the other logical positivists, to Quine –, were all heirs of this Hume-inspired broadly empiricist tradition. Hence, their viewing modal notions with suspicion was only to be expected. Moreover, these reservations were reinforced for 20th-century versions of empiricism because the latter were strengthened and made more precise by the invention of extensional, first-order quantificational languages, which could express regularities and generalizations in a new, far more powerful and precise manner. And the fact that lawlikeness or counterfactually supporting necessity distinctive of some such generalizations (those that amount to natural laws) extended beyond what can be captured by the expressive resources of extensional, first-order quantificational logic, made modal vocabulary look even more problematic and led empiricist-minded analytic philosophers (including Quine) to the view that modal notions could be legitimized only if they could be explained in resolutely non-modal terms. If this could not be done, modal notions should be eliminated, explained away; we should just learn to live without them.

However, developments in formal logic, and especially in the field of modal logic (the logic of necessity and possibility), in the 1960s, primarily induced by Saul Kripke, led to a most remarkable development in the recent history of analytic philosophy: the resurgence of metaphysics – of a traditional speculative form – as a legitimate area of research for analytic philosophers. The transformation in analytic philosophy could not be greater: far from being inhibited by the logical positivists' exclusion of metaphysics as cognitively meaningless (as a result of their austere verificationist principle of significance) or by ordinary language scruples about the ways in which metaphysicians strained the use of ordinary words, the new analytic metaphysicians shamelessly began to engage in boldly metaphysical speculations, which, as Williamson notes, "might be

[1] As is well known, modality was treated with suspicion ever since Hume forcefully formulated his epistemological objections to the concepts of law and necessary connection. Specifically, Hume argued that even one's best understanding of actual observable empirical facts did not automatically yield a corresponding understanding of the rules (causal laws) relating them. That is, those facts did not by themselves settle which of the things that *actually* happened were *necessary* (i.e. had to happen, given other such facts) and which of the things that did *not* actually happen, nonetheless were *possible* (i.e. not ruled out by the laws concerning what really happened). Possibilities and necessities are not observable states of affairs nor can they be deduced from the latter (Brandom 2015: 149).

described by those unsympathetic to [them] as *pre-critical*, ranging far outside the domain of our experience, closer in spirit to Leibniz than to Kant" (Williamson 2017, 1).

What explains this radical transformation of recent analytic philosophy? The quick answer is 1) the formal-semantic revolution in modal logic, inaugurated by Kripke, who, based on the notion of a "possible world" (as well as that of "accessibility relations" among possible worlds), legitimized intensional modal logical vocabulary[2] by analyzing it in terms of a complete extensional semantic metalanguage,[3] and 2) the fact that the analytic tradition gradually gave up empiricism in favor of naturalism (which was much less suspicious of modal talk).[4]

The Kripkean new semantics for modal logic, together with his new "causal" or "direct" theory of reference (applied to proper names and natural kind terms) (Kripke 1972), provided an unexpected legitimation to – hitherto discredited – metaphysics since it offered a new and more precise way of thinking about the traditionally metaphysical notions of essence and accident. Both

[2] A classic problem of the pre-Kripkean modal logic was that, unlike first-order quantificational languages such as the *Principia Mathematica*, it could not be formalized, one of the reasons for this being that it was intensional, i.e. not truth functional: 'Necessarily *p*' ($\Box p$) could be false even though *p* is true. This is the case whenever *p* (e.g. 'snow is white') is contingently true but not necessarily true. Similarly, it may be true that 'possibly *p*' ($\Diamond p$) even though *p* is false. It may be true that it is possible that snow is blue even though the claim that snow is blue is false. Now, for the logical positivists, Quine and Kripke alike, the paradigm of a philosophically unproblematic body of discourse is one that is extensional, because only in extensional contexts do we have an absolutely firm grasp of what we are committed to in making particular claims. Yet, until the Kripkean modal revolution nobody had shown how modal discourse could be understood extensionally.

[3] In the beginning of the 1960s Kripke (1959; 1963) showed that we could give an "extensionalistically respectable" sense to modal operators (which, of course, remain non-truth functional) by utilizing the essentially Leibnizian idea that our world, the actual world, is just one of infinitely many different *possible worlds*. The basic idea was that just as propositions can be true or false in the actual world, they can have truth values in *other* possible worlds. Thus, on this view, to say that a proposition is (actually) true is to say that it is true in that possible world that is the *actual* world; to say that a proposition is necessary (necessarily true) is to say that it is true in *every* possible world and to say that a proposition is possible (possibly true) is to say that it is true in *some* possible world or other. In this way, the notions of necessity and possibility are understood in terms of *quantification over possible worlds*. Furthermore, the framework of possible worlds proved to be illuminating in the case not only of ascriptions of *de dicto* modality but also of *de re* modality. Just as propositions are true or false in possible worlds, objects exist or fail to exist in possible worlds. Thus, in the possible world framework, to say that an object has a property necessarily or essentially is to say that it has this property in every possible world in which this object exists (including the actual world). And, to say that an object has a property contingently or accidentally is to say that while it has this property in the actual world, there is at least one possible world where it exist and fails to exemplify that property.

[4] Naturalism as a philosophical thesis is far more congenial to modal notions since modal language is essential to natural science. Fundamental physics makes essential use of the language of natural *laws*, and virtually all special sciences distinguish between true and false *counterfactual* claims.

these notions, having been revitalized by Kripke's theory of direct reference, could also now be understood in terms of quantification over possible worlds. Moreover, the application of Kripke's new theory of direct reference to proper names and natural kind terms[5] aided by his revolutionary distinction between metaphysical and epistemic modalities[6] (and backed by possible world semantics), resulted in the rebirth of something akin to Aristotelian (yet science-friendly and semantically updated) essentialism within analytic philosophy. It would not be an exaggeration to say that contemporary analytic metaphysics owes its very existence to the above cluster of (Kripkean) views.

3. *The metaphysical interpretation of possible worlds: modal realism, modal actualism and their problems*

Now, while the abovementioned novel conceptual framework of possible worlds delivered understanding and insight in a wide range of philosophical topics (besides those of *de re* and *de dicto* possibilities, it illuminated topics such as the function of proper names (Kripke 1972), the nature of counterfactuals (Lewis 1973), time and temporal relations, causal determinism, etc.), it certainly had its problematic features. The major problematic features of the possible world framework (besides technical ones, which will not concern us) are of the following kind: 1) *Ontological* problems: Are possible worlds abstract objects (Plantinga 1974; 1979; Adams 1974; Fine 1977) or concrete particulars

[5] Famously, Kripke (1972) argued that proper names and natural kind terms are "rigid designators", i.e. refer to the same individual in every possible world in which that individual or natural kind in question exists, and hence, refer independently of identifying descriptions (see also Putnam 1973). None of the identifying descriptions of a proper name or a natural kind term (i.e. its identification on the basis of superficial phenomenal properties) are essential to them. But if proper names and natural kind terms are rigid designators, then identities in which both terms are proper names or natural kind terms are necessarily true (and not contingent, as many philosophers before Kripke believed). If in the identity 'Hesperus is Phosphorus', 'Hesperus' and 'Phosphorus' are rigid designators, then since rigid designators refer to the same object in all possible worlds, there will be no world in which 'Hesperus' refers to an object other than 'Phosphorus'. The same goes with natural kind terms like 'water' and identities like 'water is H_2O'.

[6] Kripke famously distinguishes between metaphysical and epistemic modalities. Necessity and possibility are metaphysical notions, while a prioricity and conceivability are epistemological. 'Necessarily true' means 'true in all possible worlds' while '*a priori*' means 'knowable independently of experience'. 'Possibly true' means 'true in some possible world' while 'conceivable' means 'coherently imagined to obtain independent of experience'. Hence it turns out that metaphysical modal notions and epistemic modal notions are different, and not necessarily coextensional. Thus, in this Kripkean framework, it becomes possible to have *a posteriori* knowledge about 'essences' or necessary connections ('water is H2O') and to disentangle conceivability from (metaphysical) possibility, since the former does not entail the latter (something can be conceivable yet impossible and *vice versa*).

spatiotemporally unconnected to our universe? (Lewis 1986). 2) *Epistemological* problems: How are we to understand the possibility of our knowing anything about possible worlds and their accessibility relations?

Now, providing answers to those basic questions, besides being of intrinsic interest, seems necessary for getting clearer about the precise meaning and significance of the other two pillars of the modal revolution: the theory of direct reference and the distinction between metaphysical and epistemic modalities.[7] Here we will not attempt to provide an account of the complex interrelations between those three major pillars of contemporary analytic metaphysics.[8] We shall limit ourselves to providing a brief description of the ontological perplexities that occur if one adopts the two most famous and most rigorously worked out ontological interpretations of possible world semantics, Lewis' modal realism (1986) and Plantinga's modal actualism (1979).

Possible worlds are, roughly, "total ways that things could have been". But what exactly is that? Are possible worlds real things out there or just artifacts of language (such as Carnap's "state descriptions" (1947))? Do individuals in other possible worlds really exist or is this just a manner of speaking?

David Lewis' "modal realism" (Lewis 1986) was meant to provide answers to these kinds of questions. Modal realism is the view that other possible worlds are just as real and concrete as the actual world. Individuals in those worlds are just as real and exist just as fully and concretely as actual individuals. Our world is but one world among many. Yet, other possible worlds are spatiotemporally and causally isolated from the actual world and from each other. Our world – the actual world – does not have an ontologically privileged status. The term 'actual' is merely an indexical term, whose reference is determined by the context in which it is uttered. It is like 'I' or 'here'.[9]

This 'realist' Lewisian account of possible worlds has another somewhat counterintuitive consequence. If the expression 'actual world' refers to just

[7] For example, an examination of the ontology of possible worlds could throw light on the kind of ontological commitments that we make when we talk about metaphysically necessary identities involving terms that are rigid designators.

[8] There is some indication that Kripke, for example, believed that we need not answer detailed questions about the nature of possible worlds to provide an account of modal semantics of notions to be used in discussing theories of meaning such as the notion of rigid designator (see e.g. Kripke 1972: 17-19; Fitch 2004: 16). That is, he seems to believe that our ordinary intuitions about counterfactual situations suffice to semantically fix the notion of possible world and rigid designator for philosophical purposes. But philosophers disagree as to whether this is correct.

[9] To refer to a place as 'here' is not to ascribe to it a special ontological status denied to other places. The same, Lewis claims, is true of 'actual world'. The term 'actual' is a device for referring to a possible world; and the possible world it takes as its referent on any given occasion of utterance is just the possible world in which it is uttered.

one possible world, and all other possible worlds and all their inhabitants are fully existent, then it is difficult to understand how any ordinary concrete object could be a transworld individual, an individual that exists in more than one possible world. Lewis agrees, and actually embraces this counterintuitive consequence. Individuals from different possible worlds cannot be related by strict numerical identity (there is no transworld identity). However, there is a weaker relation that ties individuals from one world to individuals from another, and he considers this relation strong enough to support our prephilosophical intuitions about modality. This is the *counterpart* relation, a relation of similarity or resemblance between individuals from different worlds.[10]

Unfortunately most philosophers were not convinced by Lewis' modal realism. Their typical response to Lewis' account was what himself describes as "incredulous stares". That is, most critics take Lewis' view to be closer to a bizarre piece of science fiction fantasy than to a sober philosophical view which could be accepted as literally true. Very few philosophers are willing to believe that there exist concrete but non-actual objects. The natural view here is modal actualism, i.e. the view that only the actual world exists, or in other words, that the only things that exist are the entities that make up the actual world.

Now, more specifically, according to one of the best known and most vigorously defended versions of modal actualism (Alvin Plantinga's), other possible worlds and everything in their domain must consist of things found in the actual world. Hence, modal actualists, on their part, have the burden of explaining how other possible worlds are to be constructed out of things found in the actual world. Here is how Plantinga attempts to pull this trick off. For him, possible worlds are maximal states of affairs, i.e. ones that for every state

[10] Lewis' major philosophical motivation for developing the above initially counterintuitive views about the ontological status of possible worlds and the transworld identity of individuals, is that by treating possible worlds as (spatiotemporally isolated) concrete particulars we are in a position to reduce a host of other related notions, such as propositional necessity, possibility, contingency, essential and accidental properties to the nominalistically respectable notion of a (Lewisian) possible world, thereby avoiding an extravagant metaphysically realist account of all the above modal notions. Note, for example, that the above analysis of transworld identity in terms of counterpart relations turns facts about essences into facts about similarity relations between concrete particulars. Facts about an object's essence are thus not mysterious new facts over and above the features the object in fact has. More generally, from this point of view, all the above kinds of propositions and properties turn out to be, each in its own distinctive way, nothing more than set theoretical constructions out of concrete particulars – the inhabitants of possible worlds (see also Nolan 2004: 67-74). Thus, when we speak of a proposition as necessarily, possibly or contingently true or false, or when we say that an object exemplifies a property actually, essentially or contingently/accidentally, we are not ascribing mysterious properties or relations; we are simply engaging in a complicated form of set theoretical discourse which ultimately commits us ontologically only to the inhabitants of possible worlds, i.e. concrete particulars (and their qualitative similarities and differences).

of affairs S, they either include S or preclude S. A state of affairs is defined as an individual having a property (e.g. David Lewis' being a philosopher) (Plantinga 1979: 258). Now, some possible states of affairs obtain, and thus are actual, while others do not obtain (such as Lewis' being a track and field athlete). Yet, those non-obtaining states of affairs exist and are part of the actual world. They are abstract entities that need not be exemplified in the actual world, but nonetheless exist in the actual world as serenely as the most solidly actual states of affairs. How is this so much as possible? The key to understand this lies in the notion of *individual essence*. An essence is a property or conjunction of properties that is necessary and sufficient for being a particular individual. Essences exist necessarily but need not be exemplified.[11] Hence, according to this line of thought, individual essences necessarily exist as abstract objects in the actual world and in every other possible world (which, in turn, exist in the actual world).[12] In some of these worlds, some essences are exemplified and others not (see also Loux 1998: 190-194).

Yet here it seems that a serious problem occurs for Plantinga's modal actualism. How does modal actualism understand the commonsensical belief that I might have had a younger brother (but do not)? Modal actualists are committed to interpreting examples such as the above as follows: In some alternative possible world an individual essence of a younger brother of mine is exemplified, but it is not exemplified in the actual world. Some alternative possible world contains the state of affairs of that essence being exemplified. My younger brother exists in that possible world but his essence is not exemplified in the actual world. However, this way of understanding ordinary commonsensical modal statements such as the above populates the world with countless individual essences, one for every possible person, for every possible object, and, perhaps, even for impossible objects. For example, we want to say that the round square does not exist in any possible world. According to modal actualism, this means that the essence of round square is not exemplified in

[11] As Plantinga puts it: "Socrates is a contingent being; his essence, however, is not. Properties like propositions and possible worlds are necessary beings. If Socrates had not existed, his essence would have been unexemplified, but not nonexistent. In worlds where Socrates exists, Socrateity is his essence; *exemplifying Socrateity* is essential to him. Socrateity, however, does not have essentially the property of being exemplified by Socrates; it is not exemplified by him in worlds where he does not exist" (Plantinga 1979: 268).

[12] Note that, on this view, even our world, the actual world, is an abstract entity. As Plantinga explains, the actual world "has no center or mass; it is neither a concrete object nor … a sum of concrete objects; [it] … has no spatial parts at all" (Plantinga 1979: 258). Hence, the actual world is something different from the physical universe (including myself and all my surroundings). The latter is, for Plantinga, a contingent being, while the actual world, being a state of affairs, is a necessary being. It could have failed to obtain, but it could not fail to exist (see also Loux 1998: 192-193).

any possible world. Yet, as all essences, it exists (as an abstract entity) in the actual world, since everything that exists is actual. But this view seems to be committed to an extreme version of essentialism (combining familiar Aristotelian and Platonic themes), which, as Quine predicted, leads directly to a completely unconstrained "metaphysical jungle" of essences (Quine 1966: 174; see also Quine 1948: 1961). How is this less counterintuitive than Lewis' views discussed above? Instead of concretely existing alternative possible worlds and possible individuals (Lewis), we have all kinds of existing states of affairs and essences (such as Socrateity) exemplified and unexemplified, supposedly "serenely" existing – as abstract objects – in the actual world. Does this not seem just as outlandish as Lewisian concretely existing but non-actual worlds?[13] (see also Schwartz 2012: 219-223).

I think that by now enough has been said to show the controversial – i.e. metaphysically inflated – status of both modal realism and modal actualism as interpretations of the ontological status of possible world talk.

4. *Providing a semantic legitimation for modal talk: the "Kant-Sellars thesis about modality"*

What does "middle" analytic philosophy have to offer in the above possible world metaphysical battles? Has not early and middle analytic philosophy, with its suspicion of modal and metaphysical discourse, become obsolete since the Kripke-Lewis modal revolution? I will suggest that, at least in the case of Wilfrid Sellars, this is not the case. What is more, I shall argue that some relatively neglected parts of Sellars' work on alethic modality, as the latter is interpreted and reconstructed by Robert Brandom (2015), contain valuable insights about alethic modality, which can throw light on the semantic and epistemological import of modal talk and, by implication, to the semantic foundations of contemporary analytic metaphysical discourse.

In section 2 we mentioned that early and middle analytic philosophers – from Russell, through Carnap and the other logical positivists, to Quine – were all heirs of a Humean broadly empiricist tradition which treated modal notions with extreme suspicion. Possibilities and necessities were not observable states of affairs nor could they be deduced from the latter. Moreover, we saw how these reservations were strengthened by the invention of extensional, first-order quantificational logic. The fact that lawlikeness or counterfactually

[13] Of course, the modal actualist could claim that his divergence from common sense at this point costs less than the modal realists' claim that e.g. my younger brother (and countless many other such "younger brothers") exist concretely in other possible worlds but are not actual.

supporting necessity distinctive of some empirical generalizations (those that amount to natural laws) extended beyond what can be captured by the expressive resources of extensional, first-order quantificational logic, made modal vocabulary look even more problematic and led empiricist-minded analytic philosophers (including Quine) to the view that modal notions could be legitimized only if they could be explained in resolutely non-modal terms. Finally, we saw how the radical change of attitude toward modal notions in analytic philosophy can be explained by the formal-semantic developments in modal logic, and by the fact that the analytic tradition gradually gave up empiricism in favor of naturalism (which was much less suspicious of modal talk).

Yet, at this point it is important to understand exactly *which* questions those developments did offer answers to, and to which they did not. As was mentioned in section 2, Kripke showed that we could give an "extensionalistically respectable" sense to the notions of necessity and possibility by developing a novel possible world semantics. Obviously, this *is* an adequate response to empiricist worries stemming from the extensional character of the first-order logical vocabulary in which semantics had been conducted. That is, it provides the missing expressive resources needed in order for this first-order extensional logical vocabulary to capture the formal-logical "multiplicity" of modal notions. But these developments in formal logic do not provide an adequate response to residual empiricist worries about the overall *intelligibility* of modal concepts. This is because the extensionality of the semantic metalanguage for modality is bought at the price of relying on a notion of possible world and of accessibility relations among such possible worlds which not only is problematic with respect to its proper ontological interpretation (see section 3), but, more importantly, remains epistemologically and semantically unfounded. Does the appeal to our prephilosophical intuitions about what possible world talk might mean and what accessibility relations among *possibilia* really amount to suffice to silence empiricist qualms about the semantic and epistemic status of possible world talk? It seems that, even abstracting from problems concerning the proper ontological interpretation of possible world talk (which, as we saw in section 3, are serious enough to cast doubt on our ability to use this notion in an ontologically transparent way), both the epistemological question of how we are to understand the possibility of our *knowing* anything about possible worlds (and their accessibility relations) and the semantic question how, if the possibility of such cognitive contact is mysterious, the idea of our having the semantic contact necessary so much as to talk or *think* about them, can be made intelligible, is left untouched by the Kripkean formal-logical apparatus.

It is precisely at this point that Sellars' ideas about modal notions and the function of modal discourse become relevant. For they can provide a (semantic

and epistemic) *justification* for the new comfort of late analytic philosophers with modal idioms, while at the same time *criticizing* metaphysically inflationary conceptions of modality.

4.1. Quine's and Sellars' attack on semantic atomism

The semantic and epistemic justification of modal vocabulary is to be found in Sellars' principled rejection of some crucial presuppositions of the empiricist critique of the credentials of modal concepts. One such crucial unquestioned presupposition of this empiricist critique regarding the legitimacy of modal vocabulary is that there is an independently and antecedently intelligible stratum of empirical discourse that is purely descriptive and involves no modal commitments; this "purely descriptive" level provides a semantically autonomous background and model with which the credentials of modal discourse can then be (unfavorably) compared.

Interestingly, the above unquestioned presupposition can be discerned even in Quine, an otherwise ardent critic of related empiricist doctrines. This is ironic, since Quine was one of the first philosophers to challenge the underlying semantic atomist picture of traditional and 20th-century logical empiricism. In his classic "Two Dogmas of Empiricism" (1951), Quine, among other things, connected the meaning of an expression with its inferential role noticing that what follows from or is evidence for or against a claim depends on what other claims are available as auxiliary hypotheses or collateral premises. From this he derived his *semantic holism*: he famously concluded that the smallest unit of meaning is not a sentence (even in the case of observational sentences) but a whole "theory", i.e. the whole constellation of all sentences held true in our conceptual scheme.

Sellars, on his part, in his "Empiricism and the Philosophy of Mind" (1956) presented his own version of semantic and epistemic holism, arguing that even observational beliefs acquired non-inferentially through perception can be cognitively significant only if they are inferentially related with other conceptually contentful items. To cut a long story short, the bottom line of his critique is that for something to function in semantically and epistemically significant (i.e. efficacious) ways is for it to be a "node" (play a functional inferential role) within a wider network of contents and practices, i.e. within what Sellars calls the "logical space of reasons" or the "language game" of "giving and asking for reasons" – the rationality of which does not depend of its having any foundations, but in its self-correcting character[14] (Sellars 1956, § 38).

[14] This self-correcting character, in turn, goes hand in hand with the fact that within the space of reasons any claim can be put into jeopardy, though not all at once.

Both Quine's and Sellars' arguments were leveled against a certain problematic empiricist foundationalist layer-cake picture: that of a semantically and epistemically autonomous "base" of perceptual experiences or reports, on which, at a second separate step, is erected a semantically and epistemically "dependent" ("second-class") superstructure of unobservable entities and "theories" inferentially dependent on the observational "base" (see also Brandom 2015: 148). If successful, these arguments undermine the logical empiricist worries about the status of laws, necessary connections, dispositions and counterfactual possibilities since the latter stem from the alleged inherent difficulty of justifying the inferences that would add them to the supposedly semantically and epistemically autonomous base of nonmodal "purely descriptive" reports of actual perceptual experiences. However, the above Quinean and Sellarsian criticism of the traditional empiricist's semantic and epistemic atomism (foundationalism) is of *general* application. It does not concern only modal notions but it can equally be applied to a variety of other vocabularies traditionally treated with suspicion (or as having "second-class" status) by the empiricists, such as theoretical (non-observational) discourse, normative discourse, probabilistic discourse, talk about primary qualities, and so on.

4.2. The Kant-Sellars thesis about modality

Now, as Brandom forcefully argues in his *From Empiricism to Expressivism* (2015), there is another more *direct* and positive connection between arguments against semantic atomism and our understanding of specifically *modal* vocabulary. As we saw above (4.1), the ultimate reason why traditional and logical empiricists viewed modal talk with suspicion and thought that the only way for it to be considered legitimate is to be explained in resolutely non-modal terms was that they believed that a) there exists an independently and antecedently intelligible stratum of empirical discourse that is purely descriptive and involves no modal commitments, and that b) this purely descriptive level can function as a semantic and epistemic foundation or criterion for assessing the semantic and epistemic credentials of modal discourse. Against this, Sellars argues that the ability to use ordinary descriptive terms such as 'green', 'rigid' and 'mass' *already presupposes* grasp of the kind of properties and relations made explicit by modal vocabulary. As Sellars himself puts this point:

> Although describing and explaining (predicting, retrodicting, understanding) are distinguishable, they are also, in an important sense, inseparable. It is only because the expressions in terms of which we describe objects, even such basic expressions as words for perceptible characteristics of molar objects, locate these objects in a space of implications, that they describe at all, rather than merely label. The descriptive and explanatory resources of language advance hand in hand. (Sellars 1957, § 108)

Describing something as being of a certain kind, as opposed to labeling something in the sense of grouping it one way rather than another, is to place it in a space of implications, which articulates the *inferential consequences* of it falling in one group rather than another (Sellars 1957: § 108; Brandom 2015: 41-42, 180-181). For Sellars, these (material) "implications" must be *counterfactually robust* ones – that is, they must *remain* good under various merely hypothetical circumstances, otherwise the putatively "descriptive" term could not be consistently applied to new cases (Sellars 1953b; 1963a). An important consequence of this line of thought is that the inferences in this "space of implications" always include inferences that involve collateral premises or auxiliary hypotheses not drawn exclusively from one's actual commitments.[15]

On this view, which Brandom terms the "Kant-Sellars thesis about modality", *every* empirical descriptive concept has *modal* consequences. That is, its correct application has necessary conditions that would be expressed explicitly using subjunctive conditionals, and hence depends on what is true in *other* possible worlds besides the one in which it is being applied. For example, the ordinary descriptive sentence 'That lion is sleeping lightly' has as necessary conditions that some moderate stimulus (e.g. a sufficiently loud noise, bright light) would wake the lion, while the ordinary descriptive sentence 'This patch is red', among other things, entails that e.g. 'The patch would look red under standard conditions, and would look brown to a standard observer under green light.' Thus, describing something in the actual situation always involves substantial commitments as to how it *would* behave, or what else would be true of it, in other *possible* situations (Brandom 2015: 67-68). And an important consequence of the Kant-Sellars thesis about modality is that one who understood none of the subjunctive implications one was committing oneself to by applying the descriptive term 'lion' or 'red' could not count as grasping the concepts in question.

If this is right, then one cannot be in the position the atomist empiricist critic of modality professes to find himself in: having fully understood and mastered the use of "purely descriptive" non-modal vocabulary, but having thereby afforded himself no grip on the use of modal vocabulary and no access to what it expresses. Thus, the Humean or Quinean predicament with respect to modal notions can be diagnosed as resulting from a failure properly to understand that, as Brandom puts it "in using ordinary empirical vocabulary, one already knows how to do everything one needs to know how to do in order

[15] As Brandom puts this point: "Part of taking an inference to be materially good is having a view about which possible additional collateral premises or auxiliary hypotheses would, and which would not, defeat it. Chestnut trees produce chestnuts – unless they are immature of blighted. Dry, well made matches strike – unless there is no oxygen" (Brandom 2015; 141-142).

to introduce and deploy modal vocabulary" (Brandom 2015: 152). I think that this suffices to show how the Kant-Sellars thesis about modality can be used to legitimize possible world talk at least at the semantic level.

4.3. Modal discourse as non-descriptive, expressive, categorial and "metalinguistic"

However, in the beginning of this section we said that Sellars' views about modality can be understood not only as justifying possible world talk but also as simultaneously *criticizing* metaphysically inflationary conceptions of modality. To see how this is so we must first note that, beyond the Kant-Sellars thesis about modality, Sellars also attempts to sketch a "big-picture" view about the place and function of modal discourse as a whole in our practices. Specifically, Sellars believes that the function of modal vocabulary (along with that of semantic, intentional, and even categorial vocabulary) is not *descriptive*. That is, its function is not fact-stating in the narrow sense that that assimilates fact-stating to describing how the world is. To the extent that we can speak of modal or conditional facts (or, for that matter, normative facts, semantic facts, facts about abstract universals) as "true" – which, *pace* empiricism, we can surely do –, we should not be carried away into thinking that we are thereby committed to the existence of *sui generis* metaphysical facts, irreducible to ordinary empirical or scientific ones, that "make" our modal beliefs and sentences true (see also section 5). According to Brandom's persuasive interpretive line, for Sellars the job of modal discourse and facts is *expressive*: it makes explicit necessary structural features of the framework within which alone empirical description and explanation are possible.[16] In other words, the expressive role of modal concepts is to make explicit what is implicit in the use of ground-level (empirical) concepts: the conditions under which it is possible to apply them, use them to make judgments and revise them (for this latter function of the "causal modalities" see e.g. Sellars 1957, § 103). Specifically, the job of alethic modal concepts is to make explicit the *subjunctively robust* consequential relations between ground-level descriptive concepts. It is precisely those relations that make possible explanations of why one empirical description applies because another does. For example, that force *necessarily* equals the product of mass and acceleration means that one can *explain* the specific acceleration of a given mass by describing the force that was applied to it. Furthermore, for Sellars, modal concepts thereby

[16] Note the characteristic Kantian ring of this view. Modal concepts function as "categories of the understanding". Note, however, that this does not commit one to the further Kantian view that such concepts thereby articulate the structure of the "phenomenal" world, or to a spurious absolute distinction between phenomena and "noumena".

function "metalinguistically", as *material inferential rules* for the proper use of ground-level empirical concepts. And they do this, i.e. they determine the descriptive meaning of empirical terms, precisely by making explicit the counterfactually robust, explanation-supporting, inferential connections between them (Sellars 1948; 1957; see also Brandom 2015: 35-48).[17]

With these theoretical moves, Sellars, in one stroke, objects both to the traditional "dogmatic metaphysician" (a figure often revived in contemporary analytic metaphysics) who reifies the semantic irreducibility of modal to non-modal discourse into a *sui generis* ontological irreducibility of the former to the latter, *and* to the "skeptical empiricist", who, for fear of this ontological reification, denies the very *legitimacy* or truth of modal talk. Sellars traces the error of both the dogmatic rationalist metaphysician and the skeptical empiricist to their common acceptance of what he calls "the *descriptive fallacy*": the idea that the business of all non-logical concepts is to describe. And, as he characteristically puts it:

> Once the tautology 'The world is described by descriptive concepts' is freed from the idea that the business of all non-logical concepts is to describe, the way is clear to an *ungrudging* recognition that many expressions which empiricists have relegated to second-class citizenship in discourse are not *inferior*, just *different*[18] (Sellars 1957, § 79).

4.4. A novel kind of semantic externalism

Now, a very interesting characteristic of Sellars' views about modality, and in particular, of the Kant-Sellars thesis about modality, is that it entails a pecu-

[17] Sellars is careful not to commit himself to the implausible view that modal statements literally *say* that some (e.g. counterfactually robust) entailment holds. He distinguishes between what is said by using a specific vocabulary (i.e. its content) and what is *contextually implied* or *conveyed* by doing so (Sellars 1953c). This distinction can be understood, roughly, as one between semantic and pragmatic inferences (Sellars 1957, § 101). And what Sellars says about modal vocabulary must be understood as referring to this latter dimension of pragmatic inferences, or in Brandom's words to "what one is *doing* in making a modal claim" (Brandom 2015: 140). By doing that, one is endorsing a pattern of inference (which is not to say that modal statements are semantically *about* patterns of inference).

[18] Interestingly, Sellars' move here is reminiscent of (and, I would argue, directly descended from) the later Wittgenstein's attack to this kind of Procrustean descriptivism in the beginning of *Philosophical Investigations* (1958). There Wittgenstein warns us again and again of the dangers of being "bewitched" by the descriptivist picture. We must not simply assume that the job of all declarative sentences is to state facts or that the job of all singular terms is to pick out objects. This does not, of course, mean that there are no differences between Sellars' and Wittgenstein's anti-descriptivism. For example, Sellars characterizes a broad class of non-descriptive vocabularies (modal, intentional, categorial, semantic) as playing generically the same expressive role: they are broadly "metalinguistic" tools expressing necessary features of the framework of discursive practices that make description and explanation possible. By contrast, there is no such binary distinction of expressive roles in Wittgenstein.

liar version of *semantic externalism*. However, this semantic externalism is very different from the one that many "late" analytic philosophers espouse (which comes from Kripke and Putnam), and, as we shall see, it is a novel position worthy of serious consideration by contemporary analytic philosophers.

As we shall see below, this novel kind of semantic externalism emerges as a way of responding to a Kripke-inspired objection to the viability of the Kant-Sellars thesis about modality.

We said above that, for Sellars, the expressive role characteristic of alethic modal vocabulary is to make explicit conceptual connections and commitments that are already implicit in the use of non-modal empirical vocabulary. But, on the face of it, this view, from a Kripkean, "late" analytic point of view, faces at least one serious objection. For, as was mentioned in section 2, semantic investigations of modally rigid designators reveal the sort of necessity they articulate as metaphysical, not conceptual, and as knowable only *a posteriori*. But, as Brandom observes (2015: 152-153), the conclusion that such necessity should not be understood as conceptual necessity follows only if one either identifies conceptual content with *descriptive* content (by contrast to the causally-historically acquired content of proper names and demonstratives) or takes it that conceptual connections must be knowable *a priori* by those who have mastered those concepts. However, both of these views can be rejected without flying in the face of reason.

For example, regarding the first point, as McDowell has shown, the content expressed by demonstrative vocabulary can and should be understood as conceptual (McDowell 1987). And Brandom, in *Making It Explicit*, has proposed that the same can be done in the case of the phenomenon of modal rigidity (Brandom 1994: 367-376, 547-583).

As regards the second point, Sellars' responds in a way that shows extremely interesting points of contact with Kripke's notion of *a posteriori* necessity, and yet equally interesting divergences from the latter. As mentioned earlier, Sellars holds that the inferential relations that determine the conceptual content of descriptive terms are those that are counterfactually robust. But a consequence of this view is that to discover what is contained in an ordinary empirical or scientific *concept* one needs to *empirically* investigate the laws of nature. More specifically, Sellars accepts *both* that 1) physical or causal necessity and possibility are a kind of *conceptual* necessity, *and* that 2) physical or causal necessities and possibilities must ultimately be established *empirically*. (This is the Sellarsian peculiar version of *a posteriori* necessity.) But he is in a position to do so only because he rejects a deeply ingrained assumption operative *both* in middle and late analytic philosophy (e.g. in Quine, Carnap, Wittgenstein, but also in Kripke and Putnam). This is the assumption that *conceptual* necessi-

ties and possibilities can be established *a priori*.[19] Sellars emphatically rejects this (seemingly platitudinous) assumption and this reveals the innovative and radical nature of his semantic externalism. In effect, Sellars believes that we cannot discover the contents of our concepts just by introspecting. Concepts are understood by Sellars (and by Brandom) as rules (norms) we bind ourselves by *without* knowing everything about what we are committing ourselves to by applying those concepts. In other words – and this shows the radical nature of Sellarsian semantic externalism –, to find out what the contents of the concepts we apply in describing the world really are, we have to find out what the laws of nature are.[20] And this is not a purely *a priori* matter (see e.g. Sellars 1957, § 86).[21]

It would be, of course, interesting to continue exploring this theme of the peculiar Sellarsian semantic externalism and compare his notion of *a posteriori* necessity (developed as early as 1948 and 1953a) with Kripke's related (but strictly speaking very different) views which revived metaphysics within analytic philosophy. Unfortunately, this is beyond the scope of this paper. Suffice it to say that the Sellarsian-Brandomian notion of *a posteriori* necessity agrees with the Kripkean in that necessity and possibility are different notions from a prioricity and conceivability. The latter are epistemological while the former are "metaphysical". However, the Sellarsian-Brandomian and the Kripkean would not mean the same thing by using the word 'metaphysical' since, according to the Sellarsian-Brandomian view, *pace* Kripke, this notion is inseparable from the notion of *conceptual*. This does not mean that modal claims (e.g. laws of nature) depend for their *truth* on the *existence* of modal expressions or language users. Yet, it does mean that to grasp the *sense* or meaning of modal claims one needs to know how to *use* ordinary empirical vocabulary, and this, in turn, presupposes that one needs to know one's way around within the "realm of the conceptual" – the language game of giving and asking for reasons. Thus, the distinction between "metaphysical" and epistemological notions of modality

[19] Note that both Sellars and post-Kripkean analytic philosophers agree in that physical necessities and possibilities must be established empirically. But precisely because most late analytic philosophers tacitly or explicitly hold that conceptual necessities and possibilities can be established *a priori* (a view that Sellars rejects) they arrive naturally to the view that physical or causal necessity and possibility *cannot* be understood as conceptual in kind.

[20] Note that, in contrast to contemporary versions of semantic externalism, Sellars' version of this thesis does not need to take on the task of making sense of a notion of the "internal" (as opposed to the "external").

[21] Thus, in Sellars' words: "While one does not inductively establish that A P[hysically]-entails B by armchair reflection on the antecedent 'meanings' of 'A' and 'B', to establish by induction that A P[hysically]-entails B is to enrich (and perhaps otherwise modify) the use of these terms in such wise that to 'understand' what one now 'means' by 'A' and 'B' is to know that A P[hysically]-entails B" (Sellars 1957, § 86).

(or that between *de re* and *de dicto* modality) is not erased, but it is reconceptualized, to the extent that the distinction between metaphysical and conceptual is reconceptualized, i.e. to the extent that it is now recognized that however distinguishable, those latter notions are also essentially *inseparable*.

I think that what was said above justifies our contention that, although Sellars' Kant-Sellars thesis about modality and its corollary, semantic externalism, were products of a pre-Kripkean middle analytic philosopher, they retain their relevance, novelty and interest even in the context of late analytic philosophy. In this sense, they ought to be taken seriously not only from a historical and interpretive point of view, but also from the standpoint of contemporary debates in analytic metaphysics.

5. *Sellars' Nominalism and the Possible-World Metaphysical Battles*

In the previous section we saw how the Kant-Sellars thesis about modality and the peculiar semantic externalism entailed by it can provide a (semantic and epistemic) *justification* for the pervasive use of modal idioms by late analytic philosophers, while at the same time *criticizing* metaphysically inflationary conceptions of modality. However, the Kant-Sellars thesis about modality does not, all by itself, entail a *specific* view about possible world ontology, i.e. about what possible worlds "really are". And while the "big-picture" view about the function of modal discourse that Brandom extracts from Sellars (sketched in 4.3) does imply the rejection of metaphysically inflated conceptions of modal discourse, it does that at such a high level of generality that precludes us from drawing any direct ontological implications about possible world talk in particular. In this section we shall attempt to show how another, again neglected, aspect of Sellars' philosophy, i.e. his nominalism about abstract entities, can be used to address issues about the specifically *ontological* dimension of possible world talk. More specifically, following Kraut (2016), we will suggest that although a Sellarsian account of possible world metaphysical talk cannot but be ultimately nominalistic,[22] it acknowledges the reality of modal phenomena and attempts to *legitimate* rather than eliminate them. That is, it attempts to show what modal phenomena "really are" rather than that there are no such things as modal phenomena.

[22] As will become evident in what follows, Sellarsian nominalism about possible world talk is very different from e.g. Lewis' nominalistic position presented in section 3. For example, unlike Lewis, Sellars does not attempt to reduce the abstract to the concrete or to ground normativity in ontology. However, an interesting point of similarity between them is that Sellars, like Lewis, but for very different reasons, ultimately endorses a radical materialist *Humean* as opposed to Aristotelian or Kantian categorial ontology.

Sellars' theory of universals or abstract entities (propositions, properties, kinds, sets) provides a way of understanding the role played by such entities, i.e. to legitimize them as *essential "skeletal", "formal" features* necessary for the existence and functioning of our descriptive and explanatory practices, yet without treating them as legitimating normative grounds of the latter, supposedly provided by the "metaphysical" structure of reality (Sellars 1963b; Brandom 2015; Kraut 2016). Unfortunately, for reasons of space, we cannot provide here a detailed description of Sellars' nominalism (but see Brandom 2015: 236-272). It suffices for our purposes to observe that an essential part of Sellars' nominalism about abstract entities is the denial of the view (popular in late analytic philosophy) to the effect that the truth e.g. of the sentence 'Peter is mortal' can be explained (i.e. grounded for its correctness) by the relevant abstract entity, the "property of mortality" and its "exemplification" in a particular existing being, Peter. Reference to universals such as the property of mortality, and its exemplification, does no explanatory work. If we want to explain why Peter is mortal, which of course we can (at least in principle), we have to appeal to empirical investigations, presumably to physics, evolutionary theory, genetics and biochemistry, not to metaphysics.

The same, I suggest, goes for possible world talk in metaphysics. Possible worlds are real in the sense that they are essential "skeletal" features necessary in order for our descriptive and explanatory practices to be able to *represent themselves as such*, codify their (material inferential) commitments, and revise them in the face of "anomalies" (materially incompatible commitments) (Sellars 1948); they are *not* metaphysical entities (Plantingian individual essences or Lewisian concrete particulars) which provide *external grounds of correctness* from which our descriptive and explanatory practices derive their normative guidance.[23]

Although Sellars never explicitly addressed issues about the ontological interpretation of post-Kripkean possible world semantics, his nominalism, I think, provides the conceptual tools needed for taking a stand on the issue, and an original one at that. In section 3, where we briefly examined Lewis' modal realism and Plantinga's modal actualism, we saw that both those views (and Kripke's more ontologically neutral view), notwithstanding their differences, agree in viewing facts about possible worlds as being *truthmakers* for modal or counterfactual claims made in the actual world. (Recall that the basic

[23] According to this line of thought, normativity is constituted by patterns of human interaction (commitments, authorizations, permissions, entitlements), not from objects, concrete or abstract. Objects, considered independently of their meaning and function within a human practice, cannot tell us what we should do. At most, they provide causal-evolutionary constraints to be factored into decisions about what we should do.

idea of possible world semantics is that propositions can have truth values not only in the actual world but also in other possible worlds. Propositions are sets of possible worlds and truth conditions are functions from possible worlds to truth-values.) Yet this seemingly ontologically neutral possible world semantics embodies a certain metaphysical picture, at least to the extent to which the truthmakers in question are understood as providing either a legitimizing foundation or a causal explanation for our ordinary practices of making modal claims. This would be unacceptable from a Sellarsian point of view. As Kraut eloquently puts it, according to the latter

> possible worlds can be regarded as a helpful mechanism for codifying aspects of modal discourse: clarifying modal intuitions, regimenting modal inferences, and recursively characterizing truth for modal assertions. Possible worlds are no "metaphysical foundation" of our modal practice, nor are they part of the best explanation of that practice. The worlds do not *legitimize* or *explain* our modal practices; the worlds *represent* those practices (Kraut 2016: 74).

From this point of view, possible worlds talk is essentially a depiction of the material inferential norms (commitments and entitlements) constitutive of modal discursive practices, which enable us to see the consequences of various commitments and entitlements of ours when we engage in modal talk. But – and this is the most interesting part of what I take to be the Sellarsian position here – by functioning as such, that is, by raising us to "semantic self-consciousness" as to what we are doing when we engage in modal talk, possible world talk thereby enables the user of a conceptual framework to represent to himself (make explicit) the range of available "worldly alternatives" that are open to him and that demarcate his choices for improving his epistemic position. In this way, possible world talk enables the user of a conceptual framework to be *consciously self-critical* towards his own past, present and future tokenings of propositions licensed by the rules (counterfactually robust inferential commitments and entitlements) of a given conceptual framework in the face of explanatory anomalies (incompatible commitments)[24] (see Sellars 1948; 1957; Brandom 2015).

Again, it must be emphasized here that the purpose of this account is not to eliminate modal talk, but to acknowledge and legitimate it. Possible worlds are *real* and have real effects in the world, albeit not as "inert" abstract entities

[24] Brassier, speaking on Sellars' behalf, puts this point as follows: "Modal vocabulary allows us to regulate the explanatory frameworks within which our empirical descriptions are deployed, and in doing so it endows our theories with a rational responsiveness to the world's unresponsiveness (i.e. to anomaly), enabling us to change our theories so as to maintain our cognitive (which also means practical) grip on the world" (Brassier 2018: 75).

("real essences") that supposedly provide an external justificatory ground or causal explanation of modal talk, but as representations of norms sustaining modal talk which have the vitally important function of making our descriptive practices explicit and, through the representation of norms of explanation (counterfactually robust inferences), of sustaining, improving and reforming our descriptive practices so as for us to be able to achieve an ever-better "comportment" to the world. Possible worlds have what we might call "representative reality", but representative reality is reality enough. Not only is it not an illusion; it has real effects in the world and ourselves, and, most importantly, it is essential for our descriptive practices to get off the ground. Possible world talk is necessary for our ordinary descriptive terms to so much as *mean* something determinate (remember the Kant-Sellars thesis about modality). In this sense, possible world talk is perfectly justified and possible worlds of course exist. It is just that they are different kinds of "things" than some philosophers thought them to be.

I take it that this represents a genuinely new position on the issue of the ontology of possible worlds. Of course, this is just a rough and inadequate sketch. The position in question should be developed further and offer plausible responses in the face of criticism. But I think that it has earned its right to be considered a position worth taking seriously in contemporary debates in analytic metaphysics.[25]

[25] An issue of some interest which we do not have the space to develop in detail here is that Sellars insists that somehow all the above points about the indispensable expressive or "representational" function of modal discourse are compatible with a radically materialist picture in ontology (which has affinities with Lewis' Humean metaphysics), according to which what really exists at the fundamental level are "absolute processes" devoid of modal (or, for that matter, logical, causal and deontic) structure. How can we reconcile this "Humean" vision in ultimate ontology with the Kant-Sellars thesis according to which even empirical descriptions are modally involved? Moreover, it can be shown that the Kant-Sellars thesis is intimately bound up with an Aristotelian metaphysical framework of objects and properties (Brandom 2015: 199-204). It can also be shown that such a Kantian-Aristotelian metaphysical framework, precisely because it is derived from the Kant-Sellars thesis which inseparably relates the conceptual with the metaphysical, goes hand in hand with the view that since the world itself is modally articulated (laws of nature exist independently of language users), it is thereby *conceptually* articulated (again independently of language or concept-users). I take it that one reason, among many others, that Sellars at the end of the day accepts a Humean rather than an Aristotelian *cum* Kantian categorial framework for ontology is precisely in order to avoid this kind of modally motivated conceptual realism. But how can he be a Humean in ontology and hold the Kant-Sellars thesis at the same time, which seems to commit him to a very different categorial ontology? As Brandom suggests, one way in which one might try to reconcile the Kant-Sellars thesis with Sellars' radically "amodal" "absolute process" naturalism is to hold that, for Sellars, amodal descriptive discourse could be intelligible only as a totally unreflective and unselfconscious kind of discourse, which belongs to the stage of human language "when linguistic changes had *causes*, but not *reasons*, [before] man acquired the ability to reason about reasons" (Sellars 1957: 307). On my reading, although Brandom's proposal here is on to something important (namely, the fact that modal discourse has an essential *pragmatic*

6. *Concluding Remarks*

The purpose of this paper was to examine the major turning point events that transformed the attitude of analytic philosophers towards metaphysical discourse. We focused on one such turning point, the modal revolution, based on the resources of possible world semantics, developed by Kripke (who devised suitable models for modal logic), and other philosophers such as Lewis and Plantinga (who offered influential metaphysical interpretations of those models). We saw how the modal revolution, by bringing an unprecedented change in the way in which modal notions were understood by analytic philosophers, was central to the revival of metaphysics in contemporary philosophy. Yet, we also encountered serious obstacles in our attempt to understand the ontological and epistemological foundations and implications of one of the most basic notions of the modal revolution, that of a possible world. In the second part of the paper, we suggested that, surprisingly enough, the work of the pre-Kripkean "middle" analytic philosopher Wilfrid Sellars, especially as interpreted and reconstructed by Brandom, can perhaps throw light on the semantic, epistemic and ontological dimension of possible world talk. Sellars does this mainly through 1) the Kant-Sellars thesis about modality, 2) his understanding of modal discourse as non-descriptive, expressive, categorial and "metaliguistic", and 3) his nominalism about abstract entities. Further, we sug-

function, and reflects the framework in which a representer can be properly *critical* towards its past, present and future representings), it is not ultimately satisfactory as an interpretation of Sellars. *Pace* Brandom, I take it that Sellars' "purely descriptive" language of "pure processes" does not represent a regression to a stage where human language did not have a metalanguage at all (and hence, were completely unreflective and uncritical), but should be instead understood as having the status of a *regulative ideal* – i.e. as the culmination of a self-critical, self-correcting process of conceptual development. The regulative ideal in question points towards a kind of cognitive (and practical) "utopia" in which the critical/reflective resources of the "metalanguage" (including modal, normative and explanatory discourse) would be rendered dispensable or optional. And this would be so just in case the regularities in behavior which are implied by those critical/reflective resources were *fully materially realized* in the physical world and its relevant material mediums (in our case, the behavior – skills, habits – of embodied human beings). Hence, far from implying an impoverishment of the critical/reflective resources of discourse and a regression to more primitive stages of human language, the "purely descriptive" naturalistic "pure-process" language is actually the expression of what Sellars calls "the picture of *language triumphant* drawn in the heart of *language militant*" (Sellars 1957: 307). And, in this context, the Kant-Sellars thesis about modality can be understood as an *indispensable semantic-epistemic means* ("in the heart of *language militant*") for improving the descriptive and explanatory resources of language so as to approximate the above ideal. Of course, even if the above interpretation of Sellars is in the right direction, it does not, by itself, constitute an argument in favor of the overall position described here. Yet, I think it is fair to say that this unique combination of a radical materialist Humean ontology with the Kant-Sellars thesis about modality is a novel position not to be found in late analytic philosophy, and thus certainly worthy of being taken seriously in contemporary analytic metaphysics.

gested that the implications of this Sellars-inspired position, notwithstanding the fact that it originates in a pre-Kripkean philosophical climate before the modal revolution, are such that make it an unexpectedly relevant and novel contribution to contemporary debates in analytic metaphysics.

As a final note, it might be also interesting to highlight another dimension in which the above Sellars-Brandom alternative conception of modality is important for contemporary analytic metaphysics. We are obviously in a post-Kripkean and post-Lewisian philosophical era: their views eventually won against empiricists and other skeptical critics of modality. However, as we saw, today there are also some divergent conceptions of modality, critical of contemporary modal metaphysics, represented in the field of analytic philosophy by minority figures such as Brandom. And Brandom himself explicitly recognizes Sellars' influence in the development of his views. We saw (in sections 4 and 5) how this significant divergent path in understanding modality in analytic philosophy took shape, originating in the work of Wilfrid Sellars in the 50s and 60s. An equally important aspect of the Sellars-Brandom alternative sketched in this paper, besides the fact that it constitutes a novel contribution in the contemporary discussion about the metaphysical and epistemological status of modality, is that it does so while fully respecting the *anti-empiricist* lessons about modality drawn by contemporary post-Kripkean and post-Lewisian analytic philosophers. What is more, the Sellars-Brandom view about modality is not only resolutely anti-empiricist, but, unlike contemporary analytic metaphysics which just takes this anti-empiricism for granted, it also provides a philosophical *justification* for this view. Hence, it can be argued that the alternative conception about modality presented in this paper, despite its many points of divergence from contemporary mainstream views on the issue, is, in an important sense, a view developed *within* the (decidedly anti-empiricist) framework of contemporary analytic metaphysics, and not just an alien appendage to it, a relic from a bygone philosophical era.

Dionysis Christias
dxristias@gmail.com
University of Patras

References

Adams, Robert M., 1974, "Theories of Actuality", in *Noûs*, 8: 211-231.

Brandom, Robert, 1994, *Making It Explicit: Reasoning, Representing and Discursive Commitment*, Harvard University Press, Cambridge MA.

Brandom, Robert, 2015, *From Empiricism to Expressivism: Brandom Reads Sellars*, Harvard University Press, Cambridge MA.

Brassier, Ray, 2018, "Correlation, Speculation and the Modal Kant-Sellars Thesis", in Fabio Gironi, ed., *The Legacy of Kant in Sellars and Meillassoux: Analytic and Continental Kantianism*, Routledge, London: 67-84.

Carnap, Rudolf, 1947, *Meaning and Necessity*, University of Chicago Press, Chicago.

Fine, Kit, 1977, "Prior on the Construction of Possible Worlds and Instants", in Arthur Prior and Kit Fine, *Worlds, Times and Selves*, Duckworth, London: 116-168.

Fitch, Gregory W., 2004, *Saul Kripke*, Acumen, Chesham.

Kraut, Robert, 2016, "Norm and Object: How Sellars Saves Metaphysics from the Pragmatist Onslaught", in James O' Shea, ed., *Sellars and His Legacy*, Oxford University Press, Oxford: 60-80.

Kripke, Saul, 1959, "A Completeness Theorem in Modal Logic", in *Journal of Symbolic Logic*, 24: 1-14.

Kripke, Saul, 1963, "Semantical Considerations on Modal Logic", *Acta Philosophica Fennica*, 16: 83-94.

Kripke, Saul, 1972, "Naming and Necessity", in *Semantics of Natural Language*, 2nd ed., ed. by Donald Davidson, Reidel, Dordrecht: 253-355.

Lewis, Clarence Irving, Cooper H. Langford, 1932, *Symbolic Logic*, Dover, New York.

Lewis, David, 1968, "Counterpart Theory and Quantified Modal Logic", in *Journal of Philosophy*, 65: 113-126.

Lewis, David, 1973, *Counterfactuals*, Blackwell, Oxford.

Lewis, David, 1986, *On the Plurality of Worlds*, Blackwell, Oxford.

Loux, Michael J., 1998, *Metaphysics: A Contemporary Introduction*, Routledge, London.

McDowell, John, 1987, "*De Re* Senses", in *Philosophical Quarterly*, 34: 325-363.

Nolan, Daniel, 2005, *David Lewis*, Acumen, Chesham.

Plantinga, Alvin, 1974, *The Nature of Necessity*, Oxford University Press, Oxford.

Plantinga, Alvin, 1979, "Actualism and Possible Worlds", in Michael J. Loux, ed., *The Possible and the Actual: Readings in the Metaphysics of Modality*, Cornell University Press, Ithaca NY: 253-273.

Putnam, Hilary, 1973, "Meaning and Reference", in *Journal of Philosophy*, 70: 699-711.

Quine, W.V.O., 1948, "On What There Is", in *Review of Metaphysics*, 2: 21-38.

Quine, W.V.O., 1951, "Two Dogmas of Empiricism", in *Philosophical Review*, 60: 20-43.

Quine, W.V.O., 1961, "Reference and Modality", reprinted in *From a Logical Point of View: Logico-Philosophical Essays*, Harper & Row, New York: 139-159.

Quine, W.V.O., 1966, "Three Grades of Modal Involvement", reprinted in *The Ways of Paradox and Other Essays*, Random House, New York: 156-174.

Sellars, Wilfrid, 1948, "Concepts as Involving Laws and Inconceivable without Them", in *Philosophy of Science*, 15: 287-315.

Sellars, Wilfrid, 1953a, "Is There a Synthetic A Priori?", in *Philosophical Studies* 20: 121-138.

Sellars, Wilfrid, 1953b, "Inference and Meaning", in *Mind*, 62: 313-38.

Sellars, Wilfrid, 1953c, "A Semantical Solution to the Mind-Body Problem", in *Methodos*, 17: 45-82.

Sellars, Wilfrid, [1956] 1997, *Empiricism and the Philosophy of Mind*, Harvard University Press, Cambridge MA.

Sellars, Wilfrid, 1957, "Counterfactuals, Dispositions and the Causal Modalities", in *Minnesota Studies in the Philosophy of Science*, 2: 225-308.

Sellars, Wilfrid, 1963a, "Phenomenalism", in *Science, Perception and Reality*, Routledge, London: 60-105.

Sellars, Wilfrid, 1963b, "Abstract Entities", in *Review of Metaphysics*, 16: 621-71.

Schwartz, Stephen P., 2012, *A Brief History of Analytic Philosophy: From Russell to Rawls*, Blackwell, Oxford.

Williamson, Timothy, 2017, "How Did We Get Here from There? The Transformation of Analytic Philosophy", forthcoming in *Belgrade Philosophical Annual*.

Wittgenstein, Ludwig, 1958, *Philosophical Investigations*, Blackwell, Oxford.

Reconstructing late analytic philosophy. A quantitative approach[1]

Valerio Buonomo, Eugenio Petrovich

Abstract: Our aim in this paper is to present a quantitative approach to history of late analytic philosophy. In the first section, we focus on methodological issues. We discuss the relation between history of philosophy and metaphilosophy, distinguish between qualitative and quantitative history of philosophy, and present the theoretical framework we choose for a quantitative study of late analytic philosophy, namely scientometrics and citation analysis. In the second section, we discuss the results of our method. We present a list of high-impact authors in late analytic philosophy, and we analyze the evolution of the field in the light of citational networks (science maps) generated by VOSviewer. Finally, we propose several lines for further research.

Keywords: quantitative history of philosophy; late analytic philosophy; scientometrics.

1. *Introduction*

In this paper, we present the first results of a quantitative approach to history of philosophy, focusing on the case of late analytic philosophy.

The first section of the paper is devoted to methodological issues. We start by distinguishing history of philosophy from metaphilosophy (§ 2.1), before going on to contextualize quantitative history of philosophy, comparing it to more traditional, qualitative approaches (§ 2.2). Paragraphs § 2.3-2.4 are devoted to present the backbone of our quantitative approach, namely citation analysis. We discuss the theoretical framework of citation analysis (i.e. scientometrics) and consider the extent to which it is applicable to the history of analytic philosophy. We argue for a sharp distinction between the brute citation score of an item (author or paper), and its philosophical quality, and

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offer three theoretical arguments in its support (§ 2.5). We then operationalize the notion of late analytic philosophy, reducing it to a corpus of philosophical journals, from which articles and then citations are extracted (§ 2.6-2.7). We take the resulting corpus of more than 4,000 papers as a good representation of late analytic philosophy. Paragraph § 2.8 of this section briefly describes VOSviewer, the tool we used to analyse the data.

On the basis of these data and in the light of the methodological cautions previously mentioned, in the second part of the paper we attempt to answer two research questions, namely 1) Who are the most cited authors in late analytic philosophy? 2) What is the relation between philosophical sub-disciplines (e.g. metaphysics, epistemology, philosophy of language, etc.) in late analytic philosophy and how have such relations changed over time? In addressing the first question (§ 3.1), we present and discuss a list of the most cited authors in our corpus, offering two "canons" of analytic philosophers, namely the canon of classics and the new "canon". Moreover, we compare our results with qualitative accounts of history of late analytic philosophy. In dealing with the second question (§3.2), we use so-called "science-maps" to visualize both the overall structure of the discipline in the last thirty years and the evolution it has undergone. We interpret the pattern shown by maps as mirroring the increasing specialization of analytic philosophy, and we discuss whether specialization is an essential feature of late analytic philosophy.

We conclude by suggesting several lines of further research in quantitative history of analytic philosophy.

2. *Methodological issues*

2.1. Metaphilosophy and history of philosophy

According to Tripodi (2015), considerations carried out by analytic philosophers upon analytic philosophy have taken, in the last decades, mainly two forms: *metaphilosophy* and *history of (analytic) philosophy*.

Metaphilosophy can be defined as "the project of examining philosophy itself from a philosophical point of view – it is the philosophy of philosophy" (Rescher 2014: XI). Its mission is to facilitate an understanding of how philosophy works. Metaphilosophy is not at all a creation of analytic philosophy, since its origins can be traced back at least to Aristotle's writings. Indeed, as noted by Robert Nozick, metaphilosophy is, implicitly or explicitly, a proper part of every philosophical inquiry (Nozick 1981) given the self-reflective nature of philosophy in general. Following Nicholas Rescher's account,

metaphilosophy has two dimensions: one historical (or descriptive) and one normative (or prescriptive). Roughly, historical or descriptive metaphilosophy is concerned with how philosophical inquiry should be conducted, whereas prescriptive or normative metaphilosophy deals with what can and should be done in cultivating the subject (Rescher 2014). In the contemporary analytic landscape, Williamson (2007) can be considered the most influential in providing a normative metaphilosophy of contemporary analytic philosophy, a real manifesto of the key metaphilosophical concerns of present analytic philosophy (Tripodi 2015).

The other strand of reflection is history of (analytic) philosophy, a flourishing field that in the last thirty years has produced a vast literature, such as companions (Beaney 2013, Moran 2008), dedicated journals (*Journal for the History of Analytical Philosophy*, *HOPOS*), and a scholarly society (Society for the Study of the History of Analytical Philosophy). Before considering it in detail, however, we want to address briefly the complex relation between history of philosophy and metaphilosophy. On the one hand, history of philosophy can be considered a possible approach, among others, to descriptive metaphilosophy.[2] On the other, however, metaphilosophy can be assumed as underlying every historical reconstruction of philosophical past. In particular, it can be argued that a (normative) metaphilosophical standpoint is always present, albeit implicitly, in the work of the historian of philosophy, in the same manner that a certain philosophy of science is always presupposed by the historian of science (Lakatos 1970). Indeed, the historian of philosophy needs a normative ideal of philosophy at least for two aims. First, a metaphilosophical criterion is needed to determine what counts as philosophy and what does not, and second, to define what contribution a particular author makes to the development of philosophy. Without such a metaphilosophical framework, even the constitution of a philosophical canon is impossible, since there would be no way to distinguish the "key" authors from the "minor" ones.

History of philosophy and metaphilosophy are therefore doubly bound. History of philosophy is part of the descriptive side of metaphilosophy, whereas normative metaphilosophy is part of the history of philosophy, or, more precisely, is part of the *philosophy of* the history of philosophy.

2.2. History of (analytic) philosophy: a classification

In this section, we shall attempt to provide a possible classification of the state of the art in history of (analytic) philosophy, in order to determine the

[2] Another approach is, for instance, sociology of philosophy. See Heidegren & Lundberg (2010) for an introduction.

area in which research presented in this paper is meant to contribute. Before starting, we want to highlight the following disclaimer. Although a good starting point, the classificatory matrix we are going to propose is not intended to be exhaustive. Our main aim in this part is to situate our research in the present landscape, rather than providing a definitive account of the state of the art.[3]

Given this premise, we draw from the social sciences (Bryman 2012) a first distinction concerning the methodology used in writing history of philosophy.[4] From this point of view, we can distinguish between *qualitative* and *quantitative* approaches to the history of philosophy. The next two paragraphs are devoted, respectively, to the former and the latter.

2.2.1. Qualitative history of philosophy

Qualitative history of philosophy currently represents the majority of the work conducted in the field. It relies on the traditional tool of historiography of philosophy: mainly, close reading of texts. However, given this minimal methodological *trait d'union*, the qualitative production still differs widely under a number of aspects, such as the tools used, the scope of the reconstruction, the critical import attached to history of philosophy and the role attributed to social factors in shaping philosophical change.

As for the tools, we find a continuum of studies spanning between two extremes. On one side, there is research conducted by professional historians (such as Bruce Kuklick: see Kuklick 2001) using strictly historical methods (archival research, study of unpublished materials, previous versions of texts and private letters) (e.g. Reisch 2005). On the other side, we find philosophers approaching history of philosophy with strictly philosophical tools, such as conceptual analysis and rational reconstruction, often considering historical works as part of a wider philosophical project (e.g. Dummett 1993).

As for the scope, qualitative history of philosophy spans from the micro to the macro scale, from the careful study of one single author (Sluga 2011, Monk & Palmer 1996, Monk 1996, Haller 1991) to general histories (Tripodi 2015, Soames 2003, Stroll 2000, Biletzki & Matar 1998) and companions (Beaney 2013), passing by careful study of one school (Richardson & Uebel 2007, Stadler 2003; Giere & Richardson 1996) or period, with a particular focus on early analytic philosophy (Glock 1997, Simons 1992, Coffa 1991, Hylton 1990).

One interesting feature of contemporary qualitative history of philosophy

[3] For a comprehensive review of the literature see Tripodi (2015) and Beaney (2013).

[4] In the following, history of philosophy is meant always as history of analytic philosophy, even if we believe that the classification we propose is relevant (possibly with some modifications) for the history of philosophy in general.

is the amount of critical import attached to the historical enterprise. Next to "Weberian" value-free historians, indeed, we find *engagés* scholars, who see the history of analytic philosophy alternatively as a heroic enterprise (Biletzki & Matar 1998) or as the "history of an illusion" (Preston 2007). Tangential to the appraisal of analytic philosophy and its history is the literature dealing with the so-called analytic-continental divide (Donahue & Espejo 2016, Levy 2003, D'Agostini 1997).

2.2.2. Quantitative history of philosophy

The research presented in this paper, however, belongs to the second, less common approach: *quantitative* history of (analytic) philosophy.

Quantitative history of philosophy is a very recent field,[5] characterized by the use of a range of quantitative methods in studying and reconstructing history of philosophy. The production in the field can be divided in two strands. The first focuses on philosophers, the second on philosophical production.

The philosopher-oriented approach deals with the *profession* of philosophy, addressing the growth of professional philosophers in the twentieth century and its consequences in terms of intellectual development (Marconi 2014). The reports of the APA are an interesting source for understanding how philosophers (not only analytic) deal with the quantitative side of their profession (see Quinn 1987 and Schwartz 1995).

The second strand concerns philosophical production and can be divided in two types of research. The first focuses on the *intellectual content* of philosophical production, adopting a distant reading approach to texts, in order to process their content in a quantitative fashion. This type of research is very recent and still fragmented (see Alghren, Pagin, Persson, & Svedberg 2015 on the text analysis of *sorite* and free will debates; see also the cited bibliography). The second focuses on the *relations between philosophical products*, drawing theories and methods from scientometrics. Scientometrics is defined as the study of the "quantitative aspects of science and technology seen as a process of communication" (Mingers & Leydesdorff 2015, 1), developing "the quantitative methods of the research on the development of science as an informational process" (Nalimov & Mulcjenko 1971, 2). The research we present in this paper falls into this latter research programme.

Within the classificatory matrix we provided, then, our paper is meant to be a contribution to *history of analytic philosophy* (not to metaphilosophy), taking

[5] However, a first germ of this approach can be traced back to Wundt, who in 1877 wrote a state of the art of German philosophy using a table representing the number of philosophers belonging to each school (Wundt 1877, 495).

the *quantitative* (instead of qualitative) *methodology* side and, within it, choosing the *philosophical production* (instead of philosopher) oriented approach, with *scientometrics* (instead of distant reading) as theoretical framework.

2.3. Quantities concerning analytic philosophy as historical phenomenon

Before we present our results, however, let us spend some words on the use we are going to make of scientometrics in the context of history of analytic philosophy.

If a quantitative approach to history of analytic philosophy is adopted, the first question one should answer is: what is *quantitative* in history of analytic philosophy? In other words, what can be *measured* in analytic philosophy? So far, quantitative approaches to history of philosophy have provided two answers to this question (see above): the number of philosophy *producers* and the number of philosophy *products*.

The first are the people professionally engaged in philosophy that actively produce philosophical content. They form a population whose evolution in time can be traced in a historical perspective.

The second quantity concerns instead the *products* of philosophical research, that is to say the outcomes of research, such as monographs, articles, books, collections, etc. Again, they can be considered under two perspectives: either as bearers of what sociologists of knowledge name intellectual content (Mannheim 1936) or as nodes in a system of communication.

If we consider the first option, the research outcome is secondary with respect to the intellectual content it bears, which is, in turn, the object to be quantified. Intellectual content is what qualitative historians of philosophy commonly take as philosophy *an sich*, namely a set of abstract "philosophical objects" (such as theories, arguments, theses, problems and the like) for which the material container plays only the role of support for communicative purposes. A quantitative approach to intellectual content is still a very recent research programme. Currently, software for text analysis providing maps of keywords can be regarded as a first step in this direction. However, as such products are in an early stage of development, obtaining results sufficiently reliable for historical reconstruction seems, for the moment, impossible.

A more feasible way to quantifying research outcome comes instead from the second option: conceiving research output as interconnected items in a complex web of mutual links (i.e. nodes in a network). Scientometrics approaches the research output precisely in this way: as a point in the communication system of science (Wouters 1999). Following scientometrics, we can consider then the philosophical outcome in the same terms, representing it as a node in a network, provided with some mathematical properties. We argue

that this kind of approach might offer a viable way of pursuing a quantitative approach. However, we have to determine what the links among the items (the so called *edges*) are.

2.4. Scientometrics and theories of citation

According to scientometrical theory (Mingers & Leydesdorff 2015, De Bellis 2014) the links between nodes should be identified with the *citations* among items. Each document contains some references, each of them pointing to another document, so that for every document in a set it is possible to assign a number of *references* (the sources that it cites) and a number of *citations* (the documents by which it is cited). The second quantity is equivalent to the scientometrical notion of "impact" or citation score: the higher the number of citations that an item receives, the higher its impact in the relevant community. Is impact an indicator of the scientific quality of the item? Does a high citation score imply a high quality? Scientometricians and sociologists of science are divided on this (Bonaccorsi 2015). In order to introduce our perspective on the relation between impact and quality in analytic philosophy, a key point of our argument, we will now consider briefly the two main positions about this topic.

Advocates of the first argue that the relation between citations and quality is linear: the higher the citation score, the higher the scientific quality of an article. This position is grounded in the "normative theory of citation", i.e. the theory of citation entailed by Robert Merton's normative theory of science. According to Merton and his school, the citation plays a precise role in the scientific community: it is the way in which scientists pay their intellectual debts towards authors whose work they use. When an author makes use of another document, the theory says, she pays her intellectual debt by citing the source in the references. In this, scientists' citing behavior would follow the so-called Mertonian norm of communalism, in which scientists recognize the work of their peers by citing the sources.[6] Under this assumption, the citation would correspond, in the famous phrase of Merton, to a "pellet of peer recognition" (Merton 1973). If this is the case, then, a widely-cited document can be assumed to have raised wide scientific consensus (Cole 1992, Wouters 1999), since lots of scientists recognize it as useful for their work. Because scientific consensus is meant to be, in a way, bound to the recognition of scientific quality, it follows that high impact works (i.e. a works with a high number of citations) tend to be also high quality works.

[6] See Kaplan (1965) for a detailed picture of the relation between communalism and the citation behavior of scientists.

However, the normative theory of citation's equation between impact and quality has been widely criticized by the advocates of the socio-constructivist approach, the second position in the debate. The socio-constructivist theory of citation, grounded in the constructivist sociology of science (Knorr-Cetina 1981, Latour & Woolgar 1986), casts doubt on the assumption that scientists recognize intellectual debts when citing. Its advocates argue on the contrary that scientists have complex citing motives. In particular, they cite more for persuasion and social networking purposes than for recognizing intellectual debts (Gilbert 1977). From this it follows that the number of citations does not straightforwardly correspond to the scientific quality of an item.

Now, the dispute between normative theory and socio-constructivism is focused on citation and citing behavior in *science*. The literature about this topic has not considered (analytic) philosophy so far: we do not know the citing motives of analytic philosophers, nor if they follow the Mertonian norms or not. No comprehensive study of the "citation culture" (Wouters 1999) of analytic philosophy is, to date, available. Therefore, it is not possible to decide on empirical grounds in favor of a normative or socio-constructivist theory of citation for analytic philosophy.

Nevertheless, we shall present three theoretical reasons that invite to keep separated the two notions.

2.5. Impact and quality in analytic philosophy

We believe that it is reasonable to assume that each work in (analytic) philosophy has (at least) two attributes: impact and quality. We define explicitly impact as *the number of citations a work receives in the philosophical community,* i.e. as its citation score. There are three reasons that led us to keep separated the two notions of *impact* and *quality* for the case of analytic philosophy.

First, we are not interested in the reasons why members of the community cite the works they do cite. In particular, we are not interested in determining whether philosophical works are cited because of their (perceived) quality (as normative theory assumes) or because of other, non-intellectual reasons (as socio-constructivism claims). It follows that, in this paper, the notion of "impact" is not meant as a proxy of quality, but instead as a measure of the "attention" that a contribution obtains in the community.

The second reason rests upon the idea that philosophical "quality" is an elusive notion. In whatever manner we define it, however, it is for sure a *normative metaphilosophical* concept. It is *normative* since quality implies a set of standards, on the grounds of which a work can be judged. It is *metaphilosophical* because the standards concern how *philosophical* research should be

conducted. Since in this paper we do not want to address metaphilosophical issues, we will leave aside the notion of quality because of its metaphilosophical status, focusing on the notion of impact. Impact is indeed neutral in respect to metaphilosophical values and desiderata, because it does not entail any judgment about the rightness or wrongness of the citing motivations. In our picture, impact is considered as no more (and no less) than the result of the aggregated behavior of the community members, without any normative implications about the correctness of the behavior in itself. Quality, as a normative notion, should be therefore kept separated from impact, which is an empirical notion regarding the aggregated behavior of citing individuals.

Finally, the third reason depends on the difficulty of measuring quality in a strictly quantitative manner. Even if a metaphilosophical consensus is reached about metaphilosophical desiderata, it is not straightforward that these desiderata could be easily translated into a metrics. Moreover, even if such a metrics could be attained, it would probably take the form of a rating, rather than a ranking.[7] This is likely to imply a huge loss in the information carried by the metrics and its usefulness for informative quantitative analysis. On the contrary, impact is intrinsically a quantitative notion. Being already a number (a citation score), it does not need any translation from a qualitative context. Furthermore, it can easily be used to generate a ranking (e.g. arranging documents from the most to the less cited) suitable for statistical analysis.

In the light of these three arguments, in this paper we will focus on *impact* as the key concept for developing a quantitative approach to the history of analytic philosophy.

[7] The difference between a rating and ranking consists in the fact that, in the former, works are judged against a set of standards, whereas in the latter each work is compared to each other. Classically, a rating produces quality categories labelled with symbols (take for instance the ratings produced by credit rating agencies like Standard & Poor's) whereas a ranking produces a chart (see e.g. the university rankings such as the QS World University Ranking). That a ranking is more informative than a rating in the case of philosophy seems to be clear as soon as we consider cases in which agencies of evaluation of university and research provided a quality evaluation of scholarly journals based on ratings, in fields where bibliotemetrical metrics were not available (see Galimberti 2012). Take for instance the Italian National Agency of the Evaluation of University and Research (ANVUR), which opted for a minimum rating process instead of a ranking procedure. ANVUR divided journal in only two quality slots: A-journals ("riviste scientifiche di fascia A") and non-A-journals ("riviste scientifiche"). In the case of philosophy, the output of this rating process was that more than 300 journals were rated A. Being this number so big, and having the rating only two values, it is evident that the amount of information that can be extracted from this data is outstandingly low. If almost everything is rated A, it is arguable that A does not mean anything anymore even in terms of quality standards. Moreover, even this almost meaningless rating was harshly criticized by Italian scholars (Galimberti 2012).

2.6. The Citation Index

In order to measure impact, namely a citation score, a citation index is necessary. A citation index is defined as "a bibliographic tool [...] that lists all referenced or cited source items published in a given time span".[8] The citation index lists, in alphabetic order, all the references given in bibliographies or footnotes of source articles arranged by first author. Each reference is followed by brief descriptions (the citations) of the source articles which cite it. The citation index represents scientific literature "in the same way as a telephone book creates an image of the inhabitants of a city" (Wouters 1999: 5).

Eugene Garfield created the first citation index in 1964 at the Institute for Scientific Information (ISI) in Philadelphia (USA). It was called the Science Citation Index and indexed mainly scientific journals. Today it covers 3741 journals. In 1956 and 1975, Garfield launched respectively the Social Science Citation Index (SSCI) and the Arts & Humanities Citation Index (A&HCI), covering social sciences, arts and humanities. Today their coverage amounts to, respectively, 1700 and 1130 journals. The indexes can be accessed online mainly by the Web of Science (WoS) portal. At the beginning of the XXI century, two other citation indexes were launched: Scopus, by Elsevier[9] and Google Scholar, by Google.[10] In our research, we decided to use Web of Science's indexes.

Now we know *what* to measure (citations), what these measurements *mean* (impact, not quality) and *where* to find citation counts, namely in the citation index (in particular, in the database WoS). The next step is to mine the database, i.e. finding a query able to capture our object of interest, namely late analytic philosophy.

2.7. Operationalizing "Late Analytic Philosophy"

Given that the notion of "late analytic philosophy" is rather elusive, we need first of all to reduce it to a query for the database. We call the ensemble of steps necessary to achieve this aim the "Operationalization of Late Analytic Philosophy".

The first step in operationalizing late analytic philosophy is to shift from the intellectual process of producing analytic philosophy to the products of this process. In general, the difference between the process of knowledge production and the process outcomes has been diffusely pointed out by Bruno Latour and other Science and Technology Studies scholars (such as Karin

[8] Glossary of Thomson Scientific terminology – Clarivate Analytics. http://ip-science.thomsonreuters.com/support/patents/patinf/terms/

[9] https://www.scopus.com/home.uri

[10] https://scholar.google.it/

Knorr-Cetina). One of the fundamental epistemological move of STS is indeed the shift from the public product of scientific research (classically, the paper published in the journal) to the science in the making, "la science en action", to use Latour's famous phrase (Latour 1987). This kind of operation reveals features of science, such as the continuous social negotiation taking place in the laboratory, that are structurally invisible at the level of the paper, since the publication tends to remove every trace of the social construction of knowledge (an epistemological operation called "black boxing", see Latour 1987 and Knorr-Cetina 1981).

Even if we believe that undertaking a research on the process of *production* of analytic philosophy would provide extremely interesting results, in this paper we focus on the *product* side of analytic philosophy. We do not take then in consideration the various practices of analytic philosophy "in the making" (teaching, drafts, syllabi for classes, unpublished talks, informal exchanges in the department, etc.), but only the public products of these activities, namely publications. In particular, we focus on papers published in academic journals.

We choose to focus on papers instead of monographs on the grounds of two reasons.

The first has to do with intrinsic limitations in the available database: Web of Science still does not index monographs. It is important however not to misunderstand this point. The absence of monographs in the Citation Index means only that references cited in monographs are not counted in the index, not that monographs do not appear at all in the index. Indeed, monographs do appear insofar as the citing articles contain citations pointing to them. Therefore, monographs are part of the set of *cited* items but not part of the set of *citing* items of WoS.

The second reason concerns dissemination habits of analytic philosophers. As Marconi (2014) notes, in the last decades analytic philosophers tended to favor the paper instead of the book as the key medium for disseminating research, in a para-scientific fashion (see also Alghren, Pagin, Persson, & Svedberg 2015). Levy (2003) reiterates the point, adding examples of analytic philosophers favoring paper instead of monograph:

> AP [analytic philosophy] and CP [continental philosophy] present their research in differing forms. … It is easy to think of important philosophers in the analytic tradition whose reputation rests on journal articles alone, or whose books tend to consist of collections of previously published articles – Frank Ramsey, Bernard Williams, and Donald Davidson spring to mind. Gettier would be an extreme example (294).

In light of these two reasons we focused on papers in professional journals as target research outcome, leaving aside monographs.

The next step was to select a number of analytic philosophy journals which are representative of the field.[11]

There were three possible ways to deal with this issue: 1) rely on bibliometrical metrics, 2) rely on some authoritative source (such as companions of analytic philosophy) or 3) conduct a survey on the target population (generalist analytic philosophers). All three options have strengths and weaknesses.

Bibliometrical metrics, such as Impact Factor® and related metrics, are the standard option for determining the importance of journals in the sciences. Being based on the number of citations articles in a journal receive, i.e. on the aggregated behavior of the entire scientific community, they have the advantage of avoiding subjective biases. However, in the case of analytic philosophy, this option was not feasible within Web of Science, since WoS simply does not provide Impact Factor® for most humanities journals, philosophy included.

We turned then to Scopus, that, in contrast to Web of Science, provides metrics for Humanities too, via the tool SCImago Journal Ranking.[12] For the category "Philosophy", SCImago provides, for the year 2015, the following list of representative journals:

Rank	Title	SJR	SJR Quartile	H index	Country
1	The Philosophical Review	3,062	Q1	40	USA
2	Nous	2,405	Q1	38	USA
3	The Journal of Philosophy	1,992	Q1	31	USA
4	Ethics	1,938	Q1	51	USA
5	Australasian Journal of Philosophy	1,747	Q1	27	UK
6	Mind	1,671	Q1	30	UK
7	Political Psychology	1,623	Q1	60	UK
8	Business Ethics Quarterly	1,534	Q1	46	UK
9	Philosophers Imprint	1,481	Q1	5	USA
10	Bulletin of Symbolic Logic	1,405	Q1	26	UK

Tab. 1: SCImago Journal Ranking for category "Philosophy".

[11] Two things are important to notice. First, we avoid speaking of "top" journals, because "top" implies a metaphilosophical normative judgement that we do not want to endorse, because of our (already stated) neutrality in metaphilosophical matters. Second, we decided to focus on *generalist* analytic philosophy journals, excluding specialized journals (e.g. journals specifically devoted to logic or philosophy of science, such as the *Bulletin of Symbolic Logic* or the *British Journal of Philosophy of Science*), in order to gain a picture of the whole field.

[12] http://www.scimagojr.com/

The problem with this list is that the subject category is too general, including Journals that can hardly be considered representative of *general analytic philosophy*, such as *Ethics* or *Political Psychology*. We considered then SCImago list as helpful but insufficient to settle the issue of selecting target journals.

The second option was to extract a list of journals from some authoritative source, such as companions to analytic philosophy. This is the strategy pursued in (Brad Wray 2010) to determine the key journals in the field of philosophy of science. However, one may think that this method suffers from two possible selection biases, at least in the case of analytic philosophy. The first one derives from the choice of the companions considered for the research. The second one is the consequence of the subjective bias intrinsic to the choices made by the authors of the companions. Even if the former bias could be overcome by taking all companions as equally valuable, the latter cannot structurally be avoided. We did not consider therefore this option.

Conducting a survey on analytic philosophers to discover their opinion about key journals seemed to us to be the best solution. Furthermore, such a survey has already been conducted by the blog *Leiter Reports: A Philosophy Blog*.[13] The site conducted two pools among its visitors in 2015, both of which got over 500 votes each, asking precisely to rank the "top 20 general analytic philosophy journals".[14] Even if some methodological doubts can be cast upon the way in which the sample was chosen, the final list obtained a good consensus among the site visitors. We chose then to integrate it with the SCImago list, retaining the first five journals as the most representative ones: *The Philosophical Review*, *Noûs*, *The Journal of Philosophy*, *Mind* and *Philosophy and Phenomenological Research*.[15] Setting aside book reviews and editorials we considered only articles.

The final step was to impose a time limitation over the corpus formed by the set of these five journals' articles, in order to consider the case of late analytic philosophy. We opted for a timespan which surely comprehends late analytic philosophy production, namely the period 1985-2014.

Having gathered all the elements, the final query we used for retrieving data was the following:

[13] leiterreports.typepad.com/

[14] leiterreports.typepad.com/blog/2015/09/the-top-20-general-philosophy-journals-2015.html

[15] In discussing previous versions of this paper with analytic philosophers, this list was in general accepted as well representative of contemporary generalist analytic philosophy. Nonetheless, we noticed that it raised more consensus among analytic philosophers with Anglo-American affiliations, than among analytic philosophers of other countries.

(SO=(PHILOSOPHICAL REVIEW OR NOUS OR JOURNAL OF PHILOSOPHY OR MIND OR PHILOSOPHY "AND" PHENOMENOLOGICAL RESEARCH) *AND* DOCUMENT TYPES: (Article)
Timespan=1985-2014

We retrieved therefore 4 966 articles, containing 58 281 references to 17 926 authors. We take this corpus as the output of the operationalizing process of "late analytic philosophy".

2.8. The tool: VOWviewer

We used the software VOSviewer to analyze these data. VOSviewer is a tool developed by Ludo Waltman and Nees Jan Van Eck at the Center for Science and Technology Studies of Leiden (CWTS), Netherlands (Van Eck & Waltman 2010, http://www.vosviewer.com/). It allows for several types of citation analysis on data retrieved from Web of Science. The basic function of VOSviewer is a counting function: it allows for the ranking of the authors and the documents of the dataset from the most to the least cited.

However, its main feature is the visualization of the citational structure of the data, via the production of citational networks called "science maps". These networks allow to "grasp the structure of a field" and to track its evolution in history (Morris & Van Der Veer Martens 2008, Small 1999). To generate science maps, different techniques of citation analysis are available. The most useful for our purpose was *co-citation analysis* (Small 1973). In a co-citation analysis, the similarity between two items is calculated on the basis of the number of times they are cited together by other documents in the corpus. The larger the number of publications by which two publications are co-cited, the stronger the co-citation relation between the two publications is (Van Eck & Waltman 2014). A similarity matrix is then calculated including all the similarity index of the items. VOSviewer can calculate this matrix and translate it in a spatial representation, in which the higher the similarity between two items, the nearer their visualizations on the map.

Having established our dataset and the type of analysis we run on the data, we are now ready to present and discuss the results of the analysis. The third section of the paper is devoted to this.

3. *Results and discussion*

Through the results we present in this section, we shall attempt to answer two research questions, namely: 1) what are the most influential (i.e. most cited) authors in late analytic philosophy? 2) what is the relation between sub-

disciplines (e.g. metaphysics, philosophy of language, epistemology, etc.) in late analytic philosophy?

In order to answer the first question, we will provide the reader with a list of the high impact authors in late analytic philosophy, that is the authors which are most cited in our corpus. Then, we will outline the two "canons" emerging from these data: i) the canon of early and middle analytic philosophers[16] (hereafter canon of classics) and ii) the "canon" of late analytic philosophers.[17]

Then, we will first discuss whether in late analytic philosophy the list of the most influential classics is consistent with the standard account recognized by qualitative historiographical research. Secondly, we will try to offer a list of the most influential authors of the last period of analytic philosophy, offering a historical investigation concerning a subject that has not been widely considered yet.

The second question concerns then the structure of analytic philosophy. Considering the relation between sub-disciplines in late analytic philosophy, we shall attempt to account for the way they are connected, wondering whether any hierarchy among them seems to emerge from the data.

3.1. From the canon of classics to a new "canon"

The following table displays the most cited authors in the period 1985-2014.

Rank	Author	Citations
1	lewis, david	2119
2	quine, willard van orman	921

[16] Roughly, it is usual to consider authors like Frege, Russell, Moore and the early Wittgenstein as *early* analytic philosophers, whereas authors like Carnap, Ryle, the later Wittgenstein, and Quine are taken as paradigms of the so-called *middle* analytic philosophy (see Tripodi 2015).

[17] There are two things that ought to be clear, concerning the notion of "late" analytic philosophy and the so called new "canon". First, in this paper we are not aiming at introducing any specific criteria for classifying an author as a *late analytic philosopher*. In fact, since every criteria appear to incur in problematic counterexamples and borderline cases, we prefer to set this issue aside, referring to a commonsense periodization (according to which late analytic philosophers are those succeeding middle analytic philosophers such as Carnap, Quine, but also Davidson and Dummett). Second, we are aware of the fact that referring to a *canon* of late analytic philosophers can be disputable, at least for two reasons. Firstly, and more generally, the idea of a canon for late analytic philosophy is problematic since it is not possible to say which authors will constitute a canon for future generations of analytic philosophers. Secondly, and more specifically, it is surely not granted that the most cited authors in late analytic philosophy (and in particular in the period 1985-2014) will be the most influential ones in the future. For this reason, we will refer to the "canon" of late analytic philosophers (with inverted commas) just to sketch a contrast with the established canon of classics, committing us to a charitable application of this notion concerning late analytic philosophy.

3	davidson, donald	899
4	putnam, hilary	685
5	burge, tyler	668
6	fodor, jerry alan	649
7	frege, gottlob	574
8	williamson, timothy	544
9	russell, bertrand	540
10	kripke, saul	489
11	wright, crispin	477
12	dummett, michael	475
13	jackson, frank	464
14	mcdowell, john	459
15	dretske, frederick irwin	449
16	harman, gilbert	439
17	goldman, alvin ira	436
18	peacocke, christopher	426
19	williams, bernard	407
20	stalnaker, robert	389

Tab. 2: Most cited authors (1985-2014).

Notice first that David Lewis is, with a significant advantage, the most cited author: his works in fact are cited 2119 times, more than twice as much as the second author of the list, Willard Van Orman Quine (921 cit.), more than three times as much as the fourth author of the list, Hilary Putnam (685 cit.), and four times as much as philosophers like Bertrand Russell (number 9, 540 cit.) and Saul Kripke (number 10, 489 cit.).

Unsurprisingly, the geography of analytic philosophy refers to English-speaking countries, with sporadic exceptions for Germans – in particular among classics. In particular, the first 10 positions include 7 Americans (6 of which in the first 6 positions), 2 British and 1 German (Frege).

Only 2 women appear within the first 100 most cited authors, occupying just minor positions in the list, namely Ruth Garrett Millikan (number 54, with 217 cit.) and Elizabeth Anscombe (number 77, with 159 cit.).

There are only 6 philosophers born before 1900 within the first 100 authors, namely Kant (1724-1804), Frege (1848-1925), Russell (1872-1970), Moore

(1873-1958), Wittgenstein (1889-1951), and Carnap (1891-1970). Among them, the only non-contemporary philosopher (i.e. born before the XIX century) appearing in the first 100 position is Immanuel Kant, occupying a respectable 39th position (261 cit.) Hence, in contrast to other philosophical traditions (like hermeneutics) where philosophers plentifully cite the "classics" of philosophy, ranging from Plato to Heidegger, analytic philosophers seem to prefer citing their contemporary analytic philosophy fellows, the majority of whom is still alive.[18] The frequent citing of contemporary authors reinforces the standard idea that history of philosophy is rather marginal within analytic field.[19]

3.1.1. The Canon of Classics

Let us focus now on the most cited early and middle analytic philosophers within the last 30 years.

Rank	Author	Citations
2	quine, wvo	921
3	davidson donald	899
4	putnam, hilary	685
…		
7	frege, gottlob	574
9	russell, bertrand	540
12	dummett, michael	475
…		
34	rawls, john	293
38	moore, ge	267
39	kant, immanuel	261
44	goodman, nelson	231
46	strawson, pf	230
47	carnap, r	229
…		

[18] More specifically, although the majority of the 10 most cited authors are dead (60%), the percentage of living authors is higher in the 20 most cited authors (55%) and increases in the 50 most cited ones (62%). [Spring 2017]

[19] Besides that, the frequent citing of contemporary authors might be thought as an evidence for the vitality of analytic philosophy. However, in order to support this idea, one should define first the meaning of "vitality" of a field, entering in complex metaphilosophical issues. We believe these issues are worth studying but we prefer to set them aside in this paper.

62	sellars, wilfrid	200
63	wittgenstein, l	199
77	anscombe, gem	159

Tab. 3: most cited "classic" authors 1985-2014.

According to our data, Quine, Davidson and Putnam are the most influential classical analytic philosophers in the last 30 years. Quine and Davidson have a comparable citation weight (921 the former, 899 the latter), whereas Putnam follows (685 citations). Interestingly, they belong to the so called "middle" analytic philosophy, preceding two early analytics, namely Frege (574 cit.) and Russell (540 cit.), who have more or less ⅔ of the citations of Quine or Davidson.

Notice also that considering the canon of classic analytic philosophers, five (namely Quine, Davidson, Putnam, Frege and Russell) are among the 10 most cited philosophers. This fact may be considered a good evidence of the stabilization of a definite philosophical tradition, namely the tradition of analytic philosophy. However, the fact that excluding the first 10 positions, few classic analytic philosophers appear in the first 100 positions – precisely 14 (around the 20%) – supports the idea that the analytic philosophy research front is going forward. In particular, after Quine, Davidson, Putnam, Frege and Russell, Michael Dummett (475 cit.) precedes John Rawls (293 cit.) and with almost twice as much citations precedes fundamental philosophers of the early and middle tradition of analytic philosophy such as G.E. Moore (267 cit.), Nelson Goodman (231 cit.), Peter Strawson (230 cit.) and Rudolf Carnap (229 cit.). Out of the 10 most cited authors in classic analytic philosophy, we can find Sellars and Wittgenstein (respectively occupying positions 62 and 63 of the general table of the most cited authors, with around 200 citations). Other authors considered central to classical analytic philosophy – such as Austin, Grice and Ryle – occupy marginal positions in the list of citations, i.e.: Austin (number 88, 131 cit.), Grice (number 96, 122 cit.), whereas Ryle does not even appear within the first 100 positions.[20]

At the end of the day, we think that the canon of classic analytic philosophers derived from our quantitative analysis is in general consistent with standard qualitative historical reconstructions of analytic philosophy (Tripodi 2015, Beaney 2013).

[20] A possible explanation to the secondary positions of these authors is based on the fact that Austin, Grice, and Ryle substantially contributed to a research which is rather marginal nowadays, namely the philosophy of ordinary language.

3.1.2. The "canon" of late analytic philosophers

Having considered the canon of classics, the aim of this section is to outline the new "canon" of late analytic philosophers.

In the following table (tab. 4) are listed the 10 most cited authors in late analytic philosophy.

Rank	Author	Citations
1	lewis, david	2119
2	quine, wvo	921
3	davidson, donald	899
4	putnam, hilary	685
5	burge, tyler	668
6	fodor, ja	649
7	frege, gottlob	574
8	williamson, timothy	544
9	russell, bertrand	540
10	kripke, s	489
11	wright, crispin	477
12	dummett, michael	475
13	jackson, f	464
14	mcdowell, john	459
15	dreske, fi	449
16	harman, gilbert	439

Tab. 4: most cited "late" authors 1985-2014.

Again, we see Lewis' prevalence over other philosophers, a prevalence that is even greater when compared with the second author of this new "canon", Tyler Burge (number 5 in the general list, with 668 cit.); Lewis has in fact more than three times the citations of Burge. Very close to Burge, who is the first living philosopher of the general list, we find Fodor (649 cit.), and then Williamson (544 cit.).

It holds the attention that among the "canon" of late analytic philosophers, all authors among the 10 highly cited are English-native speakers (6 American [Lewis, Burge, Fodor, Kripke, Dretske, Harman], 2 British [Williamson and Wright], 1 Australian [Jackson] and 1 South African [McDowell]). The prevalence of this extended linguistic is impressive, although not surprising,

as it confirms the general idea about late (and middle) analytic philosophy as something not based principally in Continental Europe.

As for the areas of specialization of the most cited authors, we can observe the predominance of philosophy of language and metaphysics (e.g. Lewis, Kripke), followed by the philosophy of mind (e.g. Burge, Fodor) and epistemology (e.g. Williamson). In contrast, among the 10 most cited authors, only one is recognized for his contribution in moral philosophy (i.e. Harman); none in political philosophy.[21] However, since authors such as Lewis contributed to different areas of research, it is not possible to restrict them to just one subfield of philosophy. If so, the analysis of the most cited authors does not seem to provide us with a good picture of the different areas of research, their relations, and their hierarchy (if any). In order to deal with these issues, we will change the unit of analysis, leaving the author and focusing on the most cited *documents* in late analytic philosophy and their co-citational relations. This will provide a better grasp on the sub-disciplinary partitions of late analytic philosophy.

3.2. Maps, clusters, and the increase of specialization

In this last section, we shall attempt to explain the relations between the different sub-disciplines in late analytic philosophy. To achieve this aim we will focus on the most cited documents in the last 30 years in the five journals presented above, and the way they are related.

Specifically, we will present two science maps. The first aggregates all the documents in the period 1985-2014, drawing the structure of the field in the last 30 years. The second, consisting of three different maps, each one representing a 10 year timespan, allows us to observe the recent evolution of analytic philosophy. We argue that these maps support the idea of strong specialization within analytic philosophy, and they provide us with a quantitative evidence of the fact that such a specialization is increasing over the years.

3.2.1. The sub-disciplines of late analytic philosophy and their relations

The following science map shows the relations between the most cited documents in the period 1985-2014. As explained above (see § 2.8), the following maps are generated by VOSviewer on the basis of a co-citation analysis, so that the closeness of the documents is proportional to their co-citational score (i.e. the times they are cited together in the dataset). For instance, given the documents A, B, and C, and supposed that A & B are cited together 10 times (i.e.

[21] We notice, in passage, that the lack of moral and political philosophers in the list may be explained by the existence of several journals devoted to moral and political issues (such as *Ethics* and *Philosophy & Public Affairs*). On a related issue, see footnote 13 above.

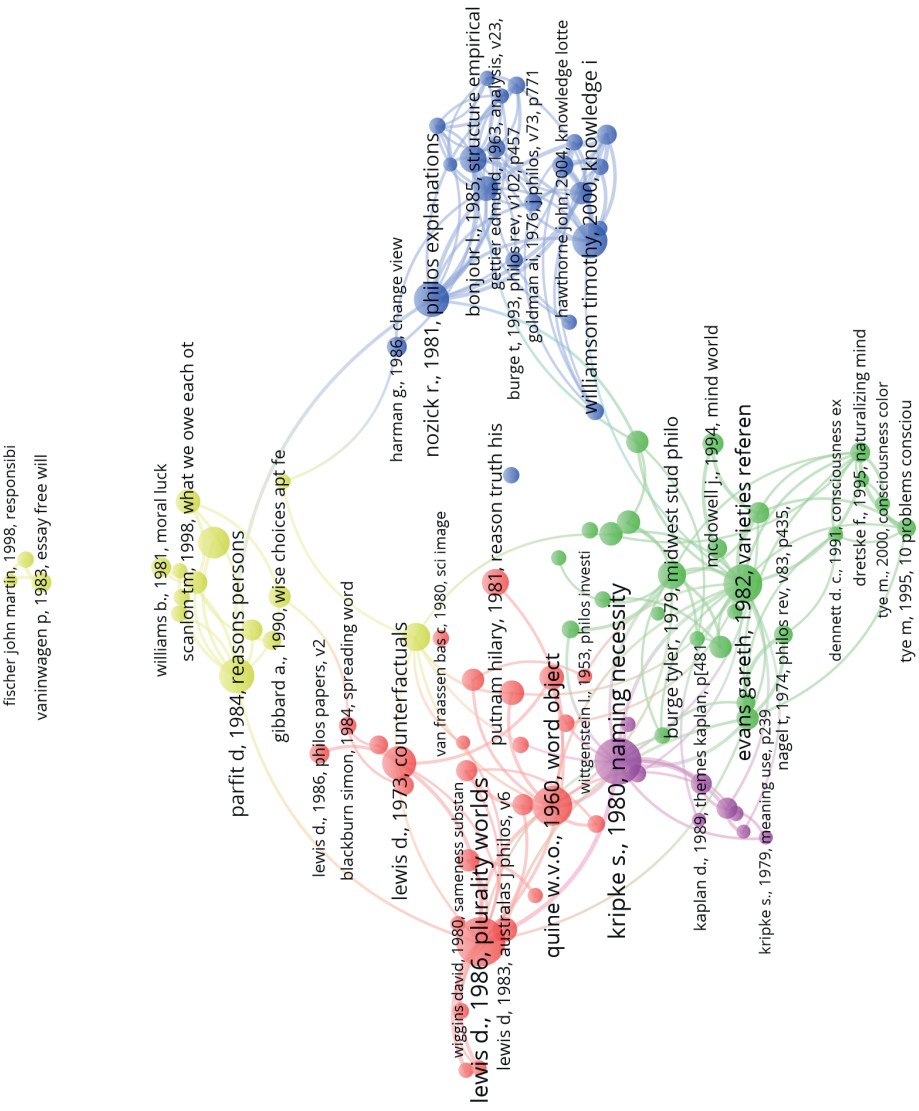

Fig. 1: Overall map of documents (1985-2014).

in 10 different documents), A and C 2 times, and B and C 7 times, A would be represented closer to B than to C, this latter in turn being represented closer to B. The dimension of the nodes, on the other hand, rests upon the number of citations received by specific documents in our dataset. Therefore, VOSviewer algorithm is designed to render meaningful not only the *topological* relations among the items but also their *relative* spatial positions. Then, even if the science maps can be rotated and flipped, the reciprocal distance among the items is conserved. Consequently, in VOSviewer visualizations, the overall *morphology* of the map is meaningful, as well as notions such as "periphery" and "center" of the map.[22]

Considering the map in Fig. 1, the first thing we notice is a clear division into clusters produced by VOSviewer algorithm. The spatial disposition of the documents, in fact, is not uniform, but rather "polarized" in some areas, in which the documents are more compact and their interconnections are significantly thicker. These clusters seem to represent rather approximately the different fields of research in late analytic philosophy (such as metaphysics, philosophy of language, philosophy of mind, epistemology and practical philosophy). Nonetheless any attempt to label the clusters would result arbitrary and disputable, given the presence of several counterexamples as well as borderline cases. For this reason, we shall avoid to assign labels to each clusters, referring instead to their colors.

Focusing on the map, one can notice, first of all, a big red aggregate of documents: the biggest nodes are Lewis' *Plurality of Worlds* (1986), Quine's *Word and Object* (1960), and Lewis' *Counterfactuals* (1973). Then we can see a small purple cluster, with Kripke's *Name and Necessity* (1980) as principal node, as well as Perry's "The Problem of Essential Indexicality" (1979) and Kaplan's "Demonstratives" (1989). Third, there is a green cluster with Evan's *Varieties of Reference* (1982) in the middle, alongside Burge's "Individualism and the Mental" (1979), Dretske's *Naturalizing the Mind* (1995) and Fodor's *Psychosemantics* (1987). Separated from the others, there is the blue cluster, with Nozick's *Philosophical Explanations* (1981), and Williamson's *Knowledge and its Limits* (2000) as main nodes. We can see thick interconnections among items of that cluster as well as their isolation from external items, which suggest a stronger specialization of that field of research over the others. Another group of documents strongly interconnected and separated from the others is the yellow cluster, which brings together works such as Parfit's *Reasons and Persons* (1984), Rawls'

[22] In technical terms, VOSviewer produces a *distance-based* visualization of networks. Other visualizations, not distance-based, are *graph-based* approaches, where edges are displayed to indicate the relatedness of nodes and the distance between two nodes need not directly reflect their relatedness, and *timeline-based* approaches (See Van Eck and Waltman 2014 for more details).

A Theory of Justice (1971), and Davidson's *Essays on Actions and Events* (1980).

Looking now at the general features of the map, we find very significant that there is no cluster occupying the center of the map, connecting all fields of research. In fact, although the red cluster seems to unify the purple and the green ones, the general structure of the research in analytic philosophy within the last 30 years takes a sort of circular structure – or better a "donut structure" – which is characterized by a hole in the middle of the map, and masses of nodes and links along the edges. The absence of a center in this representation may reinforce the idea of a fragmentation of the field of research in late analytic philosophy, as well as the absence of any defined *philosophia prima* on which all sub-disciplines rest upon, and more generally the lack of any hierarchy among them.[23]

As things stand, one may wonder whether or not such a fragmentation in closed sub-disciplines is an essential and intrinsic feature of analytic philosophy, namely the product of a determinate meta-philosophical tenet (e.g aiming at considering specific problems instead of more general issues). In order to answer this question, in the next section we will focus on something different and more fine-grained, that is the evolution of the field of research in the last 30 years.

3.2.2. Is specialization an essential feature of analytic philosophy?

Consider the following three maps generated by VOSviewer. As for the map generated in the previous section, they are based on a co-citational analysis, and they represent the relations among the most cited documents in three distinguished time-spans, namely [1985-1994], [1995-2004], and finally [2005-2014].

This sequence displays an evolution in the general structure of analytic philosophy in the last 30 years. In fact, it seems that the clusterization of late analytic philosophy into defined sub-disciplines is a recent phenomenon.

Considering the first map, representing the most cited documents and their relations in the interval 1985-1994, we observe indeed a unique aggregation of nodes, with a variety of links among documents of different clusters. There is no significant fragmentation in this period. Then, in the second map, representing the interval 1995-2004, we notice the strong development of separated clusters, as well as an increase in links between nodes of the same cluster. Finally, in the map representing the interval 2005-2014, we see a sharp division in clusters.

This seems a good evidence of the fact that the specialization into different sub-disciplines and the fragmentation of analytic philosophy in the last years

[23] Tripodi (2015: 238) offers a similar account based on qualitative grounds.

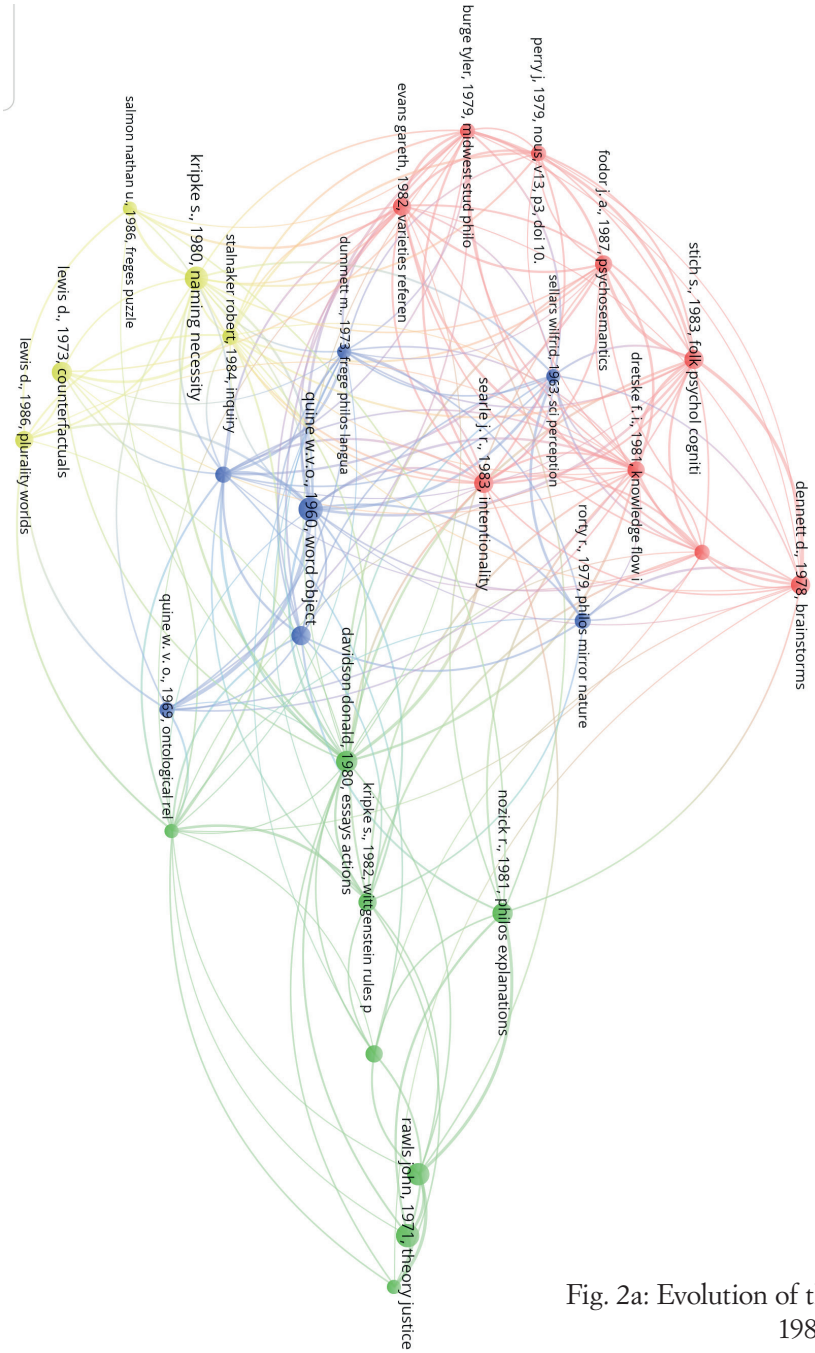

Fig. 2a: Evolution of the field 1985-1994.

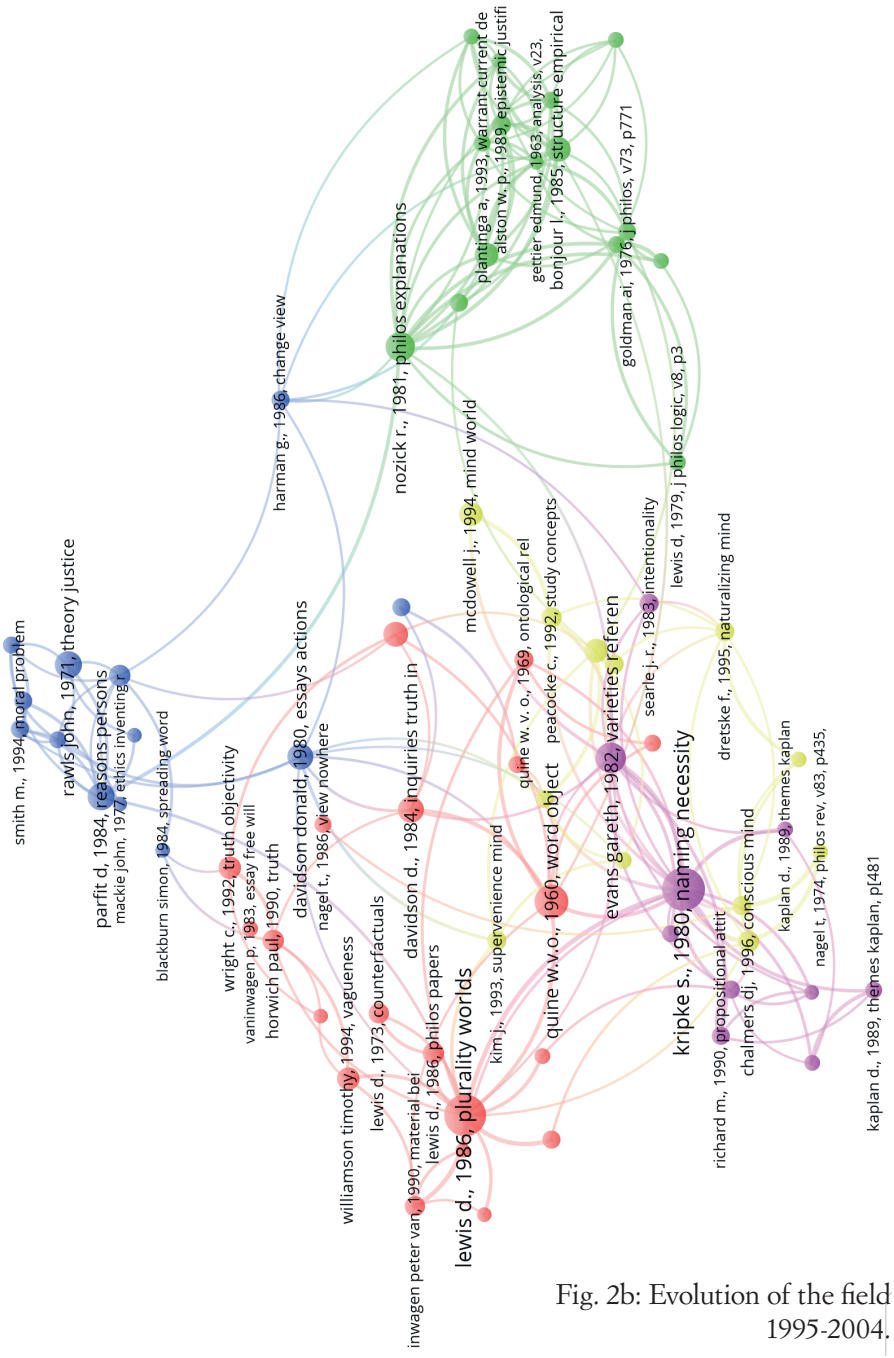

Fig. 2b: Evolution of the field 1995-2004.

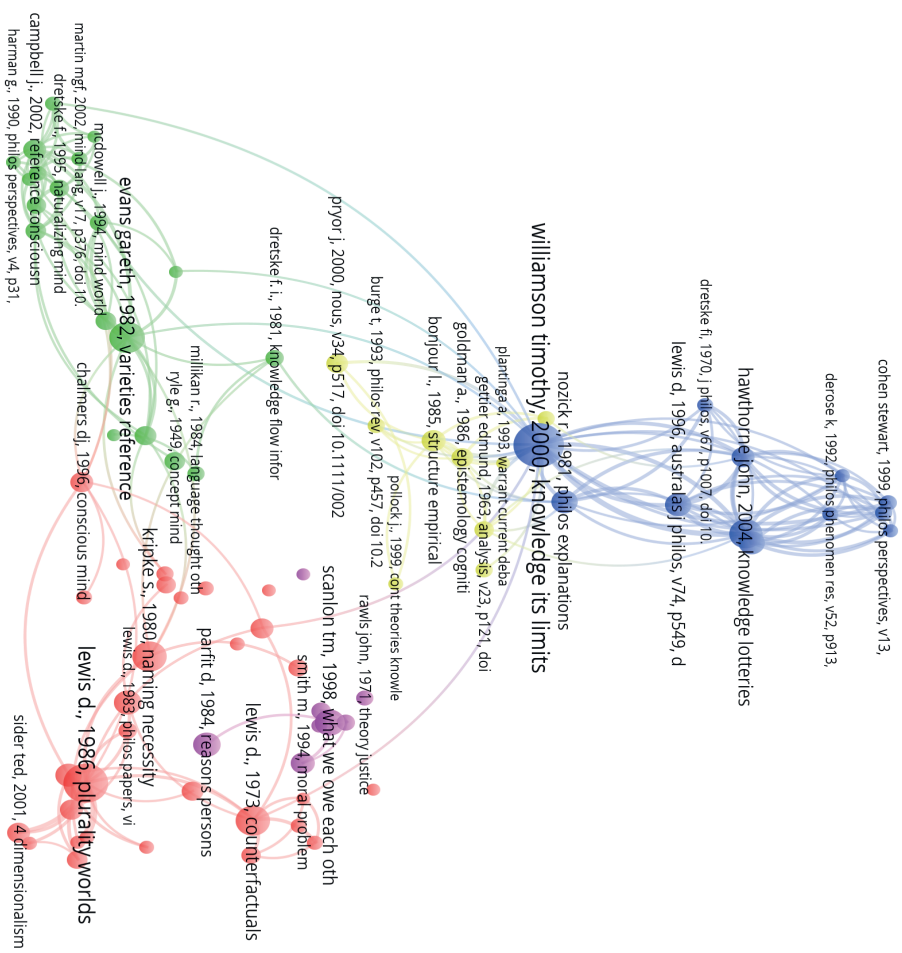

Fig. 2c: Evolution of the field 2005-2014.

is no intrinsic feature of this latter. Had it been an intrinsic feature of analytic philosophy (i.e. a metaphilosophical assumption characterizing this field of research), one should have expected to notice fragmentation into the former decades as well. Specialization seems instead the product of general and complex causes, that call for further historical, and maybe sociological, research.[24]

4. *Conclusions*

In this paper we introduced a new methodological framework for a quantitative history of late analytic philosophy, focusing on the highest-impact authors, and on the relations between the different sub-disciplines in this area. The key methodological issues concerned the relation between history of philosophy and metaphilosophy, the theoretical framework of citation analysis in history of philosophy (namely scientometrics and theory of citation), the distinction between philosophical quality and citation score (i.e. impact), and the operationalization of the notion of "late analytic philosophy".

We created the list of high-impact authors in late analytic philosophy, distinguishing within it two canons: the canon of classics and the "canon" of late analytic philosophers. We argued that the frequent citing of contemporary authors may be an evidence for the vitality of the field, which does not seem to suffer stagnation.

If we consider the overall citational network (science map) of the field in the period 1985-2014, we can see a fragmented structure, with distinguishable sub-disciplines as clusters. No clear center appears, suggesting that no discipline is dominant over the others. Moreover, the evolution of the field over the last thirty years, as represented via science mapping, shows a pattern of increasing specialization. However, specialization is shown to be a prominent feature of the very last period (2005-2014), not an intrinsic property of late analytic philosophy.

As we have seen above (3.3.1), our findings are, in general, in line with the standard picture resulting from qualitative study of the history of analytic philosophy. However, we think that this convergence does not undermine the value of a quantitative approach. On the contrary, it strengthens the results of both methodologies. We believe that substantial developments in the history of analytic philosophy could come, in the future, from an increasing integration between qualitative and quantitative methods.

As a very conclusion, let us sketch some possible further lines of research in the context of quantitative history of analytic philosophy.

[24] See Marconi (2014) for a first attempt in this direction.

First of all, it would be interesting to study the *aging of the literature* in analytic philosophy, i.e. the rate of obsolescence of papers in the field. How long is a paper in analytic philosophy usually cited by the community? Which is the average age of the literature cited by contemporary papers? Has it changed over the years? In our opinion, asking these kinds of questions might shed some light on the "state of health" of the research in analytic philosophy, and perhaps also predict the destiny of the papers published more recently.

Second, we suggest that the topic of *interdisciplinarity* is worthy of further study. We have seen how, in the last decade, analytic philosophy has undergone a process of increasing specialization. How do the different sub-disciplines interact? Is it possible, by means of advanced network analysis methods, to individuate some key documents or authors as playing the role of "bridges" among sub-disciplines? What is the relation between the sub-disciplines of general analytic philosophy and other philosophical disciplines which have undergone the process of specialization some decades before, such as the philosophy of science? And is it possible to map, via citation analysis, the relations between particular sub-fields, such as philosophy of mind, and related scientific disciplines, such as psychology, cognitive neuroscience, etc.?

A final possible line of research concerns *the implications of quantitative history of philosophy for historiography of philosophy in general*. How does a quantitative approach modify our historiographical categories? Are traditional notions of history of philosophy suitable for describing big corpora such as the one we used in this research? Should we change our fundamental assumptions about how and why philosophy changes over time, when we leave the individual level and we adopt a "big data" point of view?

For these reasons, this paper can be the first step within a larger project, fostering further research in quantitative history of analytic philosophy.

Valerio Buonomo
valerio.buonomo@unimi.it
University of Milan

Eugenio Petrovich
eugenio.petrovich@unimi.it
University of Milan

References

Alghren, Per, Peter Pagin, Olle Persson, Maria Svedberg, 2015, "Bibliometric analysis of two subdomains in philosophy: free will and sorite", in *Scientometrics*, 103: 47-73.

Beaney, Michael, 2013, *The Oxford Handbook of the History of Analytic Philosophy*, Oxford University Press, Oxford.

Biletzki, Anat, Anat Matar, 1998, *The Story of Analytic Philosophy: Plot and Heroes*, Routledge, London.

Bonaccorsi, Andrea, 2015, *La valutazione possibile. Teoria e pratica nel mondo della ricerca*, Il Mulino, Bologna.

Bryman, Alan, 2012, *Social Research Methods*, Oxford University Press, Oxford.

Coffa, J. Alberto, 1991, *The Semantic Tradition from Kant to Carnap: to the Vienna Station*, Cambridge University Press, Cambridge.

Cole, Stephen, 1992, *Making Science. Between Nature and Society*, Harvard University Press, Cambridge MA.

D'Agostini, Franca, 1997, *Analitici e continentali. Guida alla filosofia degli ultimi trent'anni*, Cortina, Milano.

De Bellis, Nicola, 2014, "History and Evolution of (Biblio)metrics", in Cronin, Blaise, Cassidy Sugimoto, eds., *Beyond Bibliometrics. Harnessing multidimensional indicators of scholarly impact*, MIT Press, London: 23-44

Donahue, Thomas, Paulina, O. Espejo, 2016, "The Analytical–Continental Divide: Styles of Dealing with Problems", in *European Journal of Political Theory*, 15: 138-154.

Dummett, Michael, 1993, *Origins of Analytic Philosophy*, Duckworth, London.

Galimberti, Paola, 2012, "Quality and quantity: HSS research evaluation in Italy. A state of the art", in *JLIS.it*, 3, 5617-1–5617-25.

Giere, Roland, Alan Richardson, 1996, *Origins of Logical Empiricism*, University of Minnesota Press, Minneapolis.

Gilbert, Nigel, G., 1977, "Referencing as Persuasion", in *Social Studies of Science*, 7: 113-122.

Glock, Hans-Johann, 1997, *The rise of analytic philosophy*, Blackwell, Oxford.

Haller, Rudolf, 1991, "On Otto Neurath", in Uebel, Thomas, ed., *Rediscovering the Forgotten Vienna Circle: Austrian Studies on Otto Neurath and the Vienna Circle*, Kluwer, Dordrecht: 25-32.

Heidegren, Carl-Göran, Henrik Lundberg, 2010, "Towards a Sociology of Philosophy", in *Acta Sociologica*, 53: 3-18.

Hylton, Peter, 1990, *Russell, Idealism, and the Emergence of Analytic Philosophy*, Clarendon Press, Oxford.

Kaplan, Norman, 1965, "Prolegomena to the Footnote", in *American Documentation*, 16: 179-187.

Knorr-Cetina, Karin D., 1981, *The Manufacture of Knowledge: An Essay on the Constructivist and Contextual Nature of Science*, Pergamon Press, Oxford.

Kucklik, Bruce, 2001, *A History of Philosophy in America: 1720-2000*, Clarendon Press, Oxford.

Lakatos, Imre, 1970, "History of Science and Its Rational Reconstructions", in *Philosophical Papers*, Cambridge University Press, Cambridge: 102-139.

Latour, Bruno, 1987, *Science in Action: How to Follow Scientists and Engineers through Society*, Harvard University Press, Cambridge MA.

Latour, Bruno, Steve Woolgar, 1986, *Laboratory Life: The Construction of Scientific Facts*, Princeton University Press, Princeton NJ.

Levy, Neil, 2003, "Analytic and Continental Philosophy: Explaining the Differences", in *Metaphilosophy*, 34: 284-304.

Mannheim, Karl, 1936, *Ideology and Utopia*, Routledge, London.

Marconi, Diego, 2014, *Il mestiere di pensare. La filosofia nell'epoca del professionismo*, Einaudi, Torino.

Merton, Robert K., 1973, *The Sociology of Science: Theoretical and Empirical Investigations*, University of Chicago Press, Chicago.

Mingers, John, Loet Leydesdorff, 2015, "A Review of Theory and Practice in Scientometrics", in *European Journal of Operational Research*, 246: 1-19.

Monk, Ray, 1996, *Bertrand Russell: The Spirit of Solitude*, Jonathan Cape, London.

Monk, Ray, Anthony Palmer, eds., 1996, *Bertrand Russell and the Origins of Analytic Philosophy*, Thoemmes, Bristol.

Moran, Dermot, ed., 2008, *The Routledge Companion to Twentieth Century Philosophy*, Routledge, London.

Morris, Steven A., Betsy Van Der Veer Martens, 2008, "Mapping Research Specialties", in *Annual Review of Information Science and Technology*, 42: 213-295.

Nalimov, Vassily, B. Mulcjenko, 1971, *Measurement of Science: Study of the Development of Science as an Information Process*, Foreign Technology Division, Washington DC.

Nozick, Robert, 1981, *Philosophical Explanations*, Clarendon Press, Oxford.

Preston, Aaron, 2007, *Analytic Philosophy: The History of an Illusion*, Continuum, London.

Quinn, Philip L., 1987, "Remarks on the Sociology of Philosophy", in *Proceedings and Addresses of the American Philosophical Association*, 61: 109-113.

Reisch, George A., 2005, *How the Cold War Transformed Philosophy of Science. To the Icy Slopes of Logic*, Cambridge University Press, Cambridge.

Rescher, Nicholas, 2014, *Metaphilosophy. Philosophy in Philosophical Perspective*, Lexington Books, Lanham MD.

Richardson, Alan, Thomas Uebel, 2007, *The Cambridge Companion to Logical Empiricism*, Cambridge University Press, Cambridge.

Schwartz, Charles A., 1995, "Research Specialization and the Refereeing Process", in *Proceedings and Addresses of the American Philosophical Association*, 69: 147-153.

Simons, Peter, 1992, *Philosophy and Logic in Central Europe from Bolzano to Tarski: Selected Essays*, Kluwer, Dordrecht.

Sluga, Hans, 2011, *Wittgenstein*, Wiley-Blackwell, Oxford.

Small, Henry, 1973, "Co-citation in the Scientific Literature: A New Measure of the Relationship Between Two Documents", in *Journal of the American Society for Information Science*, 24: 265-268.

Small, Henry, 1999, "Visualizing Science by Citation Mapping", in *Journal of the American Society of Information Science*, 50: 799-813.

Soames, Scott, 2003, *Philosophical Analysis in the Twentieth Century*, Princeton University Press, Princeton (NJ).

Stadler, Friedrich, 2003, *The Vienna Circle and Logical Empiricism. Re-evaluation and Future Perspectives*, Kluwer, Doldrecht.

Stroll, Avrum, 2000, *Twentieth-Century Analytic Philosophy*, Columbia University Press, New York.

Tripodi, Paolo, 2015, *Storia della filosofia analitica*, Carocci, Roma.

Van Eck, Nees J., Ludo Waltman, 2010, "Software Survey: VOSviewer, a Computer Program for Bibliometric Mapping", in *Scientometrics*, 84: 523-538.

Van Eck, Nees J., Ludo Waltman, 2014, "Visualizing Bibliometric Networks", in Ding, Ying, Ronald Rousseau and Dietmar Wolfram, eds., *Measuring Scholarly Impact: Methods and Practice*, Springer, Berlin: 285-320.

Williamson, Timothy, 2007, *The Philosophy of Philosophy*, Blackwell, Oxford.

Wouters, Paul, 1999, *The Citation Culture*, PhD thesis.

Wray, Brad K., 2010, "Philosophy of Science: What Are the Key Journals of the Field?", in *Erkenntnis*, 72: 423-430.

Wundt, Wilhem, 1877, "Philosophy in Germany", in *Mind*, 2: 493-518.

Focus reviews

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Analytic Philosophy: An Interpretive History
Routledge, London 2017, pp. 168.

Richard Davies

This volume comprises eighteen specially-composed essays on aspects of the self-understanding and self-narration of analytic philosophy, written by acknowledged scholars on the topics under examination. Most of the contributions are relatively brief, coming in at a little more than fifteen pages per pelt, including endnotes and bibliographies (which, given the overlappings, could well have been gathered in a single uniform listing at the end of the volume). Given the track record of many of the authors, it is no surprise that there is something of value in every one of the essays. But, given the controversial nature of the overall topic, nor is it a surprise that they cannot all be taken as read. For, some of the divergences among the views expressed point to questions that it is not the role of a review to try to adjudicate but, at best, to adumbrate.

A very first approach to the book might make a brief pause at the cover of the paperback edition, which presents on the front a version of a line drawing that appears in the *Philosophical Investigations* (II, xi, p. 194) and on the back the explanation that this is "the duck-rabbit made famous by Ludwig Wittgenstein". The version reproduced on the Preston's cover matches pretty closely what Joseph Jastrow (whom Wittgenstein cites by name) reproduced in *Popular Science Monthly* (1899) on the basis of an unattributed picture featured in a humorous German magazine of 1892. To keep our liminal pause as brief as possible, we may make just a few remarks. For all the reader can tell, the choice of image was not the editor's, but the duck-rabbit has strong brand-recognisability: if there is one visual trope in common to analytic philosophers, it will be this. Second, like many of the terms and labels that crop up in the discussions that make up the body of the book, the precise origin of the bistable picture is slightly murky. Third, as a matter of empirical fact, around two thirds of those who meet the picture for the first time see just a duck, while only around 15% see a duck and a rabbit,[1] which might indicate that there is some sense in

[1] McManus, I.C., et al., 2010, "Science in the Making: Right Hand, Left Hand II. The duck-rabbit figure", *Laterality*, 15, 1/2: 166-185; tables at p. 177.

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which we must learn to recognize images (Necker cubes, Rubin vases, etc.) that have the characteristic feature of this figure. Fourth, the duck-rabbit seems to reflect the pluralism implicit in Preston's subtitle "An Interpretive History". But, fifth, if we attend to some of the things that Wittgenstein says about such phenomena, for instance in the *Remarks on the Philosophy of Psychology*, seeing it as a duck or as a rabbit is *not* a matter of interpreting, but of seeing because interpreting is an action (*Handlung*), while seeing is a state (*Zustand*).[2]

This last remark, for all its brevity, nudges us to return to the fourth and to reinterrogate Preston's subtitle. After the editor's lucid and thought-provoking introduction, the contributions are organised in an order in which the movement of the centre of gravity is basically chronological, with five chapters on the nexus Frege-Moore-Russell, two on Wittgenstein early and late, three on the various relations among pragmatism, scientific philosophy and naturalism, and then a transatlantic back-and-forth of Ryle-and-Ayer, Quine, Strawson, Austin, Davidson and Dummett with a chapter each, and the volume is rounded off by further consideration of the extent to which analytic philosophy can be regarded as forming a tradition. In this movement, the volume does indeed present a history of analytic philosophy.

But there are at least two caveats to be entered.

One is that the level of sophistication of many of the contributions is such that this is not a manual or an introduction that could be usefully, or even safely, given to an undergraduate. Not least because many points of reference and their reverberations are taken to be self-explanatory and thus presuppose previous acquaintance with the literature under discussion. Moreover, though a host of thinkers and commentators are mentioned to help contextualise and render dialectical the positions of the leading actors, the index contemplates only seventeen names (all, bar Susan Stebbing, of male philosophers) and seventeen labels for philosophical disciplines or "schools", when interesting recurrences, such as the word "piecemeal" (which I kept on stumbling on), could easily have been logged with the technology available today.[3]

The other is that we need to understand a little better what is meant by describing a history as "interpretive". For, this part of the subtitle raises the expectation that what are in store are *re*-interpretations of some key moments in analytic philosophy's history. This expectation is fully justified and at least in the main satisfied by the papers presented: most of them offer novel readings

[2] On this point, Bozzi, Paolo, 1998, *Vedere come. Commenti ai §§1-29 delle Osservazioni sulla filosofia della psicologia di Wittgenstein*, Guerini e Associati, Milano: ch. 1.

[3] Even the fullest index short of a full-scale concordance will not pick up interesting near-omissions, such as the word "quantification" (which seems to appear only once, on p. 132).

of important figures and texts that are at least corrective of some exaggerations and caricatures that have had a certain currency in what we might call analytic philosophers' "folklore" or "foundation myths" about their disciplinary predecessors. Were these mythemes not already current, they would not have to be addressed in the ways that our authors suggest.

Though it is not fully embraced by all the contributors to the volume, the editor's underlying hunch for the project of an interpretive history is that the academic formation known today as "analytic philosophy" can be viewed as the outcome of what he calls, starting on p. 1 (we are making progress), "tradition-shaping interpretations". By this phrase he means readings of a text or a thinker's thought that in one way or another create what he elsewhere calls the "illusion" that analytic philosophy forms or has at some time formed a unity of some sort.[4] It is of course salutary to be reminded of the extent to which the formation of a canon of reference-points is a process of selection that comes after the fact, and, hence, to which the formation of a tradition is, despite its etymological relation with "passing on" (*trado*), a matter of recuperation or preservation. But, as Sandra Lapointe also reminds us in her contribution on "The Traditionalist Conjecture" (pp. 269-287), the peculiarities of analytic philosophy (unlike, say, medieval philosophy, mentioned by way of contrast, pp. 273-274) are such that it may be more fruitful to allow for much underdetermination, vagueness and adjustability in our historical enquiries to account for the very various phenomena that can be grouped under this label: perhaps the notion of a tradition imposes a misleading model.

Conversely, prefacing his account of Strawson on ordinary language and descriptive metaphysics (pp. 214-228), Hans-Johann Glock takes it that an "orthodox" narrative of analytic philosophy as having some intimate concern with language and logic has at least a "*fundamentum in rebus*" (p. 215): it may not be the whole story, but it cannot be left out as one of the motors that propelled some characteristically analytic enterprises. In similar vein, Scott Soames' account of the changing role of language in this field (pp. 34-51) roundly asserts that it all "began with interest in new topics – logic language and mathematics" (p. 34) and proceeds to list a number of "currently intractable problems" in the theory of meaning as "foundational issue[s]" (p. 44) that stand in need of "urgent attention" (p. 45).

To some extent, Soames' approach takes over viewpoints that are explored in the chapters on the move from talk of "scientific" philosophy to the adoption of the label "analytic" by Alan Richardson (pp. 146-159), on Ernest Nagel's naturalism by Christopher Pincock (pp. 160-174), and on Quine by Sean Mor-

[4] Preston, Aaron, 2010, *Analytic Philosophy: The History of an Illusion*, Bloomsbury, London.

ris (pp. 193-213). In describing primarily the American scene from the 1930s to the 1950s, these essays bring out some of the ways in which the perceived continuity between philosophical activity and the procedures of the natural sciences discouraged just the sort of historiographical reflection that the book under review promotes. In particular, in his excellent account of Quine's interactions with what he learnt from Russell, C.I. Lewis and Carnap, Morris illustrates how his subject sees that the overcoming of metaphysics is the realisation that metaphysics cannot be "overcome" but amounts to "a general limning of the general traits of reality [Quine's well-known phrase] from within the confines of our current best scientific theories" (p. 208). And as Lee Braver concludes from his review (pp. 240-253) of the ways that Davidson "tries to out-Quine Quine" (p. 243), the ultimate upshot of truly charitable interpretation is that "there is no such thing as philosophy" (p. 250).

While several authors note more or less in passing the impatience that, until recently, most thinkers of an analytic bent have expressed for the history of their own undertaking, the volume contains some noteworthy revisionist readings of easily misconstrued passages. Perhaps it is invidious to mention just a few of these. But Cheryl Misak's account (pp. 131-45) of the role of Frank Ramsey in reorienting Wittgenstein's thought after the *Tractatus* is a tremendous contribution to understanding one of the standing puzzles about that puzzling man, as well as a just tribute to the brilliance of the often-neglected figure of Ramsey. Another microhistory of fruitful interactions between two philosophers who exercised enormous influence in their day (but less today) is Michael Kremer's narrative (pp. 174-192) of, first, the back-and-forth between Ryle and Ayer in the years immediately preceding the publication of *The Concept of Mind* on how to formulate a sustainable version of what might, after all, be called "behaviourism", and, later, of Ryle's own dissatisfaction with how he had expressed himself and how the book was read when it appeared. Likewise, Kelly Dean Jolley's re-reading of Austin's *Sense and Sensibilia* (pp. 229-239) is an all-too-brief illustration of how, for Austin, the sense-datum theory was not false but a muddle, so that "refuting" it would be like "adding a tilde to a string of symbols that is not well formed" (p. 232); for this reason, the attack on that theory is not to be read as a defence of its negation, which would be the direct realism that is often attributed to Austin.

Though there is not space to give credit to all the contributions to Preston's volume, it is worth reiterating that they are of uniformly high standard both in argumentation and in documentation. But it may also have transpired from the synopses already furnished that there is a marked trend – not to use Lapointe's favoured word "bias" (pp. 278-279) – towards privileging questions of the philosophy of mind-and-language to pinpoint the decisive moments in

the development of analytic philosophy. As Anat Matar notes in her chapter on "Dummett's Dialectics" (pp. 254-268), these are indeed topics that, following Frege, mark out a "first philosophy" that has taken the place of the epistemological concerns that dominated philosophical discussion for two and a half centuries after Descartes: the theory of meaning, especially in light of Frege's context principle, provides the materials for making philosophy "systematic" (p. 259). But it does leave interesting analytic work on ethics (starting with Moore's epoch-making *Principia Ethica*) rather out of the picture.

Richard Davies
richard.davies@unibg.it
Università di Bergamo

Frederique Janssen-Lauret and Gary Kemp (Eds.)
Quine and His Place in History
Palgrave, London 2016, pp. 224

Paolo Leonardi

W.V.O. Quine was the greatest philosopher of the second half of the twentieth century. His work has had a profound impact on most areas of philosophy, while his influence can be seen in the thought of Grice, Strawson, Davidson, Putnam, and Kaplan, as well as Dummett, Kripke, Burge, and Evans, among others. Leading features of Quine's work include a radical empiricism, with no skeptical inclination, and a view of philosophy as being continuous with science. Among his most influential theses is the denial of the analytic/synthetic distinction, that is, the claim that the whole complex of theories is what undergoes a confirmation test, meaning that if the test were not passed, the failure can be patched by changing any part of the complex. Given all this, Quine certainly deserves a place in history. Yet it is still too early to tell precisely what that place should be, and the title of this book – most likely the choice of the publisher – does not really reflect its content, which comprises a series of interesting papers that often examine and discuss Quine's claims from a non-historical perspective, sometimes noting its roots in American pragmatism or in the work of Bertrand Russell.

The book is enriched by the presence of a previously unpublished paper entitled "Levels of Abstraction", which Quine presented at the *First International Conference on Unified Science*, held in New York in November 1972 and organized by Ed Haskell. In addition to this are Quine's letters in response to Gary Ebbs' review of *Pursuit of Truth*. In "Observations on the Contribution of W.V. Quine to Unified Science Theory", Ann Lodge, Rolfe A. Leary and Douglas B. Quine provide valuable and little known information concerning Quine's interaction with Ed Haskell on the latter's project of a unified science. The other essays in the collection are dedicated to the task of interpreting Quine's work and discussing some of his key theses, continuing for the most part what could have been done 20 or 30 years ago, when he was still alive – a fact which highlights how relevant and lively certain Quinean issues remain to this day.

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Three papers come closer to what this reader at least expected from the book's title: "The Web and the Tree: Quine and James on the Growth of Knowledge" by Yemima Ben-Menahem; "On Quine's Debt to Pragmatism: C.I. Lewis and the Pragmatic A Priori" by Robert Sinclair; and "Quine, Wittgenstein and 'The Abyss of the Transcendental'" by Andrew Lugg. Ben-Menahem compares Quine with a relevant American philosopher from more than two generations prior to Quine, and whom the latter never cites. She succeeds in pointing out some similarities and shared interests, which perhaps were silently taken on by Quine via C.I. Lewis. Quine took Lewis' classes while a student at Harvard, and Lewis also served as a referee for Quine's dissertation. The idea of Quine being pragmatistically inclined – which also recurs in some companions to his work – encounters a major limit in the form of his denial of being so inclined. Quine is American philosophy becoming World philosophy, as the next ring in the thread of Central European and British scientific philosophy of the first half of the last century – he owes a lot to Carnap, Tarski, and Russell. Studying Quine from a pragmatist background puts him in context and emphasizes his wider relevance.

Lugg's contribution marks a more dramatic departure from Quine's style of philosophy. The title of Lugg's piece quotes a Quinean dictum in its title, "The Abyss of the Transcendental". The comparison with Wittgenstein is not a novel one; indeed, many such comparisons have already been offered. Moreover, the comparison is unbalanced, creating an excessively Quinean Wittgenstein: giving "wide berth" to "philosophical claims that do not survive scientific and logical scrutiny" (189) is true of Quine, but it does not properly fit Wittgenstein. In no sense was Wittgenstein antiscientific or alogical, though he did not see philosophy as dependent on science. "I am not aiming at the same target as the scientist and my way of thinking is different from theirs", writes Wittgenstein ([1977] 1980: 7e). Indeed, later in the essay, Lugg acknowledges where Wittgenstein stands (196), but the author goes on to claim that Wittgenstein's and Quine's respective projects are compatible. In this light, the essay reviews the similarities and differences between the two thinkers. For instance, both view meaning as use, but while Quine explains away names, Wittgenstein is said to have held that "the meaning of a name is sometimes explained by pointing to its bearer" ([1953] 2009: I, 43). This presentation of ostensive training in later Wittgenstein is misleading here. Furthermore, it is quite possible to identify and then play down certain similarities and differences between almost any pair of philosophers and their work. Looking at dates and timelines, one could have inquired into what Quine might have picked up from Wittgenstein and how he developed this. This is precisely what Sinclair does in his essay on Quine's relation to C.I. Lewis, which shows step by step how Quine further weakened Lewis' view on the a priori.

C.I. Lewis introduces the a priori as that "which is true no matter what", immediately adding that "what is anticipated is not the given, but our attitude toward it" (1929: 197). Lewis' idea is that if the conceptual principles and criteria that express our attitude fail in ordering and simplifying our experience, we reject them and seek out another set of principles (79). According to Lewis, Sinclair notes, they can be "not useful but not false" (82-83). Quine came to think that no theoretical core in principle avoids coming to be judged as false. A wrong classification is a false one. If a principle is true, it is proved so only by virtue of it being fruitful and passing any test to which it is applied – there is no foundation, only survival.

Peter Hylton's crystal clear presentation of Quine's philosophy of language starts from Russell's logically perfect language to show how Quine had a more limited project of a regimented language as the language of science. That is to say, for Quine, the language of knowledge is "first-order logic with identity" (109), enriched, using Russell's paraphrase of definite descriptions, by predicates. Such a language does not allow one to express everything that languages do; rather, it is meant to express all that is relevant to science and hence to knowledge, and to demarcate what there is – therein lies its ontological relevance. "In regimenting theory the aim is to maximize the simplicity and clarity of our knowledge as a whole" (111). This framework allows Quine to argue that what exists are only physical objects, and that the serious parts of our knowledge can be formulated by quantifying over those objects. The final part of Hilton's essay attempts to explain how we can learn and understand Quine's regimented language.

In "Reading Quine's Claim that no Statement is Immune to Revision", Gary Ebbs tries to understand the claim made in "Two Dogmas of Empiricism", through a discussion of Grice and Strawson's criticism, Putnam's defense, as well as some of Quine's texts, such as *Methods of Logic*. The conclusion is given at the outset of the essay: namely to understand Quine's claim as follows: "For every sentence S that a subject A accepts at a time t_1, there is a possible rational revision of the beliefs A holds at t_1 that (i) leads A, or another subject B, rationally to judge, at some later time t_2, that S is false, and (ii) allows for a homophonic translation of S, as A uses it at t_1, by S, as A or B uses it at t_2" (123). In this passage, the key point is that the changes are diachronic and yielded in a homophonic translation, that is, the changes are invisible on the surface of language.

In "Meta-Ontology, Naturalism, and the Quine-Barcan Marcus Debate", Frederique Janssen-Lauret compares Quine's objectual-quantification, no-names view, with Barcan Marcus' substitutional quantification and names-grounded view. The confluence between the two views comes in the form of direct reference to objects, while the substantial difference is that Quine holds

that "A theory cannot be committed to an individual qua individual, independently of how it is described. But this is precisely the kind of ontological commitment" that "Ruth Barcan Marcus advocates". It is the same commitment that Keith Donnellan and Saul Kripke picked up and significantly revised, before backing up with a rich series of arguments.

Gary Kemp, in "Underdetermination, Realism, and Transcendental Metaphysics in Quine", plays down the idea that there are possible fully alternative theories of nature. The failure to reduce theories of the external world to sensory experience plus logic convinced Quine that philosophy begins, and continues, *in medias res*, assuming the truth of the natural sciences (177). In such a frame, all objects are (theoretical) posits. Having said this, Quine does not "concede that his position is one of realism but only in his limited sense" (178). Yet he acknowledges that we have limited senses – something that Kemp explores by imagining a creature with a *sensus optimus* and by hinting at "how the language-learning child", according to Quine, "comes gradually to acquire a mastery of referential language" (183).

As should be clear by now, the last series of papers, as I have summarized, contain some further resources for evaluating Quine in relation to what came before – particularly with regard to Russell, Carnap or Barcan Marcus and Quine. However, none speaks of Quine's impact on philosophy, which can only be evaluated by looking at what has happened in the meantime. The book's theoretical papers are all about indeterminacy, *lato sensu*: indeterminacy *stricto sensu*, no direct access to objects and hence no names, and underdetermination. No one wonders, with the possible exception of Janssen-Lauret, whether indeterminacy is tenable, as, for instance, do Loux and Solomon 1974 (see also Leonardi 2003). In his hyper-empiricism, Quine, as Wittgenstein before him, never accepted that we start taking for granted some matters of fact, about which we can investigate and change our ideas, but only taking for granted some other matters of fact. One aspect missing from this collection, but one which remains crucial in measuring Quine's as yet unrealized impact on the philosophy that has come after him, concerns his attitude towards modality. This is clearly set out in terms of its philosophical value in Ballarin 2004.

Quine endeavored to be a critical hyper-empiricist, never adjusting to there being a set of a posteriori truths which, on reflection, we have to accept. Such a path was instead pursued by George E. Moore, culminating in his 1925 and 1939 papers.

Paolo Leonardi
paolo.leonardi@unibo.it
Università di Bologna

References

Ballarin, Roberta, 2004, "The Interpretation of Necessity and the Necessity of Interpretation", in *The Journal of Philosophy*, 101: 609-638.

Leonardi, Paolo, 2003, "Oggetti di riferimento", in Bianchi, Claudia, Andrea Bottani (eds.), *Significato e ontologia*, Franco Angeli, Milano: 129-144.

Loux, Michael J., David Solomon, 1974, "Quine on the Inscrutability and Relativity of Reference", in *Notre Dame Journal of Formal Logic*, 15: 16-24.

Moore, G.E., 1925, "A Defence of Common Sense", in J.H. Muirhead (ed.), *Contemporary British Philosophy*, George Allen & Unwin, London, reprinted in G.E. Moore 1959, *Philosophical Papers*, George Allen & Unwin, London, 32-59.

Moore, G.E., 1939, "Proof of an External World", in *Proceedings of the British Academy*, 25, repr. in G.E. Moore, 1959, *Philosophical Papers*, George Allen & Unwin, London: 127-150.

Past Present

Was Royaumont merely a *dialogue de sourds*? An Introduction to the *discussion générale*

Mathieu Marion

> J'ai oublié la discussion qui s'ensuivit, sauf ce trait amusant de Jean Wahl lui-même, me disant que, si j'avais fait ma conférence en Angleterre, je me serais fait tuer.[1]

The following is a translation of the transcript of the *discussion générale* (hereafter: *discussion*),[2] which took place on the last morning of the colloquium on "analytic philosophy" at the Abbey of Royaumont, north of Paris, on 8-13 April 1958. The colloquium was presided by Jean Wahl, professor at the Sorbonne since 1936,[3] and organized by him with help from Marc-André Béra.[4] The speakers were obviously meant to represent "analytic philosophy" but, with the exception of W.V. Quine, they were all from Oxford: J.L. Austin, P.F. Strawson, R.M. Hare, Gilbert Ryle, J.O. Urmson, and Bernard Williams. A.J. Ayer, who was at the time moving back to Oxford from London, was also at

[1] Pierre Hadot (2004: 10) is recollecting here the discussion that followed his paper on the limits of language in Wittgenstein's *Tractatus Logico-Philosophicus*, at Jean Wahl's Collège philosophique, in April 1959, one year after Royaumont. Hadot published his paper later on that year (Hadot 1959).

[2] Throughout this paper, references to page numbers without further indications are to the proceedings of the colloquium, *La philosophie analytique* (Anon. 1962). The *discussion générale* is on pp. 330-380.

[3] Jean Wahl (1888-1974), albeit lesser known than figures such as Jean-Paul Sartre and Maurice Merleau-Ponty, was nevertheless a central figure in the development of mid-century French philosophy. He played a crucial role in the rise of "existentialism" (at least within the university), through his teaching at the Sorbonne and his writings, which included *inter alia*, prior to the war the first study in French on Kierkegaard (1938) and after the war: *Petite histoire de l'existentialisme* (1947), *Esquisse pour une histoire de l'existentialisme* (1949), *La pensée de l'existence* (1951) and *Les philosophies de l'existence* (1954).

[4] See here Austin's and Perelman's very last comments in the *discussion*, pp. 379-380, indicating Béra's role. Marc-André Béra (1914-1990) graduated from the École Normale Supérieure before the war. He was director of the Centre culturel de Royaumont prior to the conference, from 1953 to 1957. A minor figure, he published a short superficial monograph on Whitehead (Béra 1948), and he was mainly known for his French translations of English literature.

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Royaumont, but not among speakers. There were two additional speakers from the Continent, the Belgian philosopher Leo Apostel and the Dutch mathematician E.W. Beth, presumably chosen for their affinities with analytic philosophy. Proceedings of the colloquium were published four years later in 1962, with French translations of the papers,[5] and an edited transcript of all question periods along with the *discussion*.[6] Alas, translations were of poor quality, and so was, apparently, the interpretation,[7] a fact that could not have helped mutual understanding. The purpose of this introduction is merely to provide some background information and to dispel a few misunderstandings about the colloquium, so that readers approach the *discussion* with a fresh mind.

1. *Dispelling Confusions*

Royaumont had indeed reached at some point quasi-mythical proportions, as even its precise date became controversial.[8] It has been billed as a landmark in twentieth century as important as the notorious exchange at Davos in 1929 between Ernst Cassirer and Martin Heidegger, and frequently – grandiloquently – portrayed as a first encounter between "analytic" and "continental" philosophy.[9] If its aim had been truly to bring about a *rapprochement* between these main currents, then Royaumont must certainly count as a failure. Charles Taylor, who had attended the meeting, described it, *en français dans le texte*, as a "*dialogue de sourds*" (1964: 132), while Wolfe Mays commented later on in a more understated way that it "seems to have ended in mutual incomprehension" (Mays and Brown 1972: 2).

It is indeed striking to read, for example, representatives of both sides calling each other "Platonists", when Ryle described in his paper Husserl as "bewitched by his Platonic idea that conceptual enquiries were scrutinies of the super-objects that he called 'Essences'" (p. 67) (1971: vol. 1: 180-181), eliciting

[5] Some of these papers have appeared elsewhere since in their original language, e.g., Austin (1963) and Ryle 1971, vol. 1: 179-196). Merleau-Ponty's exchange with Ryle was also translated (Merleau-Ponty 1992).

[6] Internal evidence shows that the transcripts of the discussions were edited, e.g., Austin refers in the *discussion* (*infra*, p. 231) to a pair of edited out questions by a "M. Meteroc", whose identity remains, incidentally, unknown.

[7] W.V. Quine complained in his autobiography of the poor quality of translations and interpretation, to the point that, out of despair, he "burst[ed] extempore in French" (1985: 272). For his "outburst", see the *discussion*, *infra*, p. 233.

[8] See Overgaard (2010: 900n1).

[9] There was a further conference of the same nature in Southampton in 1969, see Mays and Brown (1972).

from Father van Breda – the man responsible for saving Husserl's library before the war – the reply that Oxford philosophers "hypostatize language … concepts and words", so that they are "excellent Platonists, while Husserl isn't one" (p. 87). Ryle could only reply in turn, correctly, that he did not "hypostatize" anything. This exchange between Ryle and van Breda illustrates what *mutual* incomprehension means.[10] But there is no reason to read the whole volume as one big illustration of it. Moreover, it is quite striking to note that within a year, Charles Taylor had argued at the Joint Session, without so much as an objection from Ayer, that:

> The method of phenomenology and that of linguistic analysis are, therefore, properly understood, quite compatible. (Taylor and Ayer 1959: 109)[11]

Royaumont was the only source of information about analytic philosophy for French-speaking students until 1980,[12] when a new set of papers by analytic philosophers was published in *Critique* (Anon. 1980). If I am allowed a personal comment, the animosity between Ryle and van Breda largely colored our perception, as students, of the relations between "analytic" and "continental" philosophy, as if one or the other had to be wrong, and not really doing philosophy or at least being unbearably bad at it. We were struck by Leslie Beck's notorious claim in the *Avant-propos* to the volume that, when asked by Merleau-Ponty "Aren't our programs the same?", Ryle's answered: "I hope not!" (p. 7). (As we shall see below, Beck's report is incorrect.) Michael Dummett was to compare in *Origins of Analytic Philosophy* (1993) analytic philosophy and phenomenology to the Rhine and the Danube,

[10] Worse, when van Breda also chastised Ryle for what amounts to a caricature of Husserl, Ryle uncouthly replied that he does not care if there had been any resemblance or not (p. 87).

[11] Furthermore, his own objections to Husserl's doctrine of "essences" and talk of an "eidetic science" largely vindicate Ryle's stern assessment. Pointing out that the later Husserl and Merleau-Ponty had moved away from Husserl's early views, Taylor nevertheless admitted that talk of "pure description" of "essences" remained (Taylor and Ayer 1959: 103-104). At all events, Ryle's critique is best understood in terms of the context of the reception of Husserl in Britain – here one should recall that Husserl had lectured in London already in 1922, years before his much-vaunted trip to Paris and, for that matter, the advent of analytic philosophy – and Ryle's critique is very much a critical comment on the first edition of Husserl's *Logical Investigations*. See Marion (2003).

[12] And, indeed, for a majority of French philosophers and linguists in the 1960s, as it seems that, e.g., from Benveniste (1963) to Derrida (1973), Royaumont sparked interest in Austin's work, and the old fear of the elimination of metaphysics gave way to another set of concerns. Of course, there were other sources, especially translations arranged by Paul Ricoeur at the Éditions du Seuil, of Frege, Strawson, Austin, etc. Derrida used the translation of *How to do Things with Words* (Austin 1970). Oddly enough, Jean Wahl was also behind a French translation of Wittgenstein's *Blue and Brown Books*, for which he wrote a preface (Wahl 1965b).

[…] which rise quite close to one another and for a time pursue roughly parallel courses, only to diverge in utterly different directions and flow into different seas. (1993: 26)

With hindsight, it felt as if by the late 1950s analytic philosophy had gone with the flow beyond the Rhine gorge, well on its way to the North Sea, while phenomenology – often confused in the context of Royaumont with "continental" philosophy – had already reached beyond the Iron Gates.[13] Thus we felt we had to take sides, with the other side being *de facto* guilty of quackery.

With hindsight, we need not take so narrow and so dramatic view of the matter. To begin with, not only the labels "analytic" and "continental" are notoriously problematic in themselves, but these labels were not established back then: participants clearly used the word 'continental' in accordance with its *geographical* meaning,[14] and none of the invited speakers had self-consciously developed an "analytic" philosophy; it is not even clear at first blush under which definition philosophers so far apart as Austin and Quine should fit.[15]

Moreover, the audience was predominantly French and Royaumont can hardly have been planned as a gathering of an opposing force of "continental" philosophers. Among the French were *mandarins* such as Ferdinand Alquié, Lucien Goldmann, Maurice Merleau-Ponty, René Poirier and Éric Weil. Still, the audience was not exclusively French: it notably included *inter alia* two Poles, the historian of logic Józef Maria Bochénski and the philosopher of law and seminal figure in argumentation theory, Chaïm Perelman, along with a further pair of Belgians: Philippe Devaux, who had translated Russell and written on Alexander and Whitehead, and Herman Leo van Breda, already mentioned. There were also a number of English-speaking philosophers that hardly could qualify as "analytic", such as H.B. Acton, Alan Gewirth – who describes himself in the *discussion* as a "neutral observer" (*infra*, p. 246) –, Alan Montefiore and Charles

[13] The proximity (or lack of) between analytic philosophers and phenomenologists at Royaumont has been discussed in some amount of detail in Ovegaard (2010), Vrahimis (2013a, chap. 4) and Vrahimis (2013b), with the implication that Dummett's metaphor was inappropriate. It seems to me rather obvious that, any perceived proximity notwithstanding, Royaumont's actual failure is but a clear indication that by then the two traditions had clearly moved apart; otherwise, there would have been some measure of mutual understanding. But Overgaard seems more intent to argue against Dummett's suggestion that one can "re-establish communication only by going back to the point of divergence" (1993: 193). This is an issue that cannot be tackled here.

[14] It is possible, however, that the confusion between the geographical meaning of 'continental' and a cooked-up philosophical meaning comes from Royaumont, when some of the participants unwittingly described themselves as *continentaux*. Royaumont could thus be seen, if that were the case, as a milestone in the history of this artificial divide.

[15] It takes Hanjo Glock a whole book to argue the point (Glock 2008). Incidentally, it is hardly ever pointed out that the expression 'analytic philosophy' was coined by R.G. Collingwood in *An Essay on Philosophical Method* (1935: 141f.).

Taylor. There were, however, no German philosophers[16] and only one Italian, Andrea Galimberti, who intervenes briefly in the *discussion* (*infra*, pp. 243-244).[17] Clearly, such a motley crew could not be properly described as representative of "continental philosophy", not even according to a geographical meaning.

The presence of Merleau-Ponty, van Breda and a few others such as Gaston Berger[18] also gives the clearly false impression that analytic philosophers squared off against a team of phenomenologists, as if 'phenomenology' would be quasi-synonymous with 'continental' philosophy, an obviously bogus claim. As it happens, none of the other figures listed above, not even Wahl who had lectured on Husserl and Heidegger, could be described, *even by a stretch*, as phenomenologists. Thus, the debate following Ryle's provocative paper, "Phenomenology versus 'The Concept of Mind'" – including the striking display of mutual incomprehension cited above – should *not* be confused with Royaumont as a whole, of which it formed in the end only a part. What has to be questioned is the wish to see it as characteristic of the whole.

It looks, therefore, as if Royaumont, rather than being conceived as a confrontation between "traditions" or "schools", was simply planned, as Ferdinand Alquié pointed out in the *discussion*, as an opportunity for some French philosophers to learn about that newfangled affair, "analytic philosophy":

> One of the main goals of this colloquium is for us "continentals" to become acquainted with analytic philosophy. (*infra*, p. 235)

The war had created a generational gap and there were hardly any cross-Channel exchanges since, precisely at a time of momentous developments within "analytic philosophy". Of the older pre-war generation, only Louis Rougier had published his *Traité de la connaissance* (1955), but, as Arthur Pap (1956) quickly pointed out it, the book was merely a reflection of the logical positivism of the early 1930s, and written in ignorance of more recent developments. He was moreover an outcast because of his political views.[19] Stanislas Breton

[16] With the possible exception of Erich Weil, born in Germany, who had written a thesis on Renaissance philosophy under Ernst Cassirer in Berlin in 1928. But he had emigrated to France already prior to the war, and can hardly be seen as pertaining to post-war German philosophy. He submitted a further thesis in 1951 at the Sorbonne, with Jean Wahl on the jury. At the time of Royaumont, he had a chair at Lille.

[17] The transcript does not provide a first name for Galimberti and Andreas Vrahimis (2013b: 183n28) mistakenly took him to be Umberto Galimberti, who would have been 16 years-old at the time of Royaumont.

[18] Gaston Berger had written one of the first French books on Husserl (Berger 1941). In the 1950s, he was responsible for philosophical appointments across France, and played in that capacity an important role in the post-war institutionalisation of phenomenology in that country.

[19] If we exclude the decidedly marginal figure of Marcel Boll, Louis Rougier was the only rep-

had not yet published his *Situation de la philosophie contemporaine*, which was at any rate ill-informed about what he called "scientific philosophy", given that the dozen or so pages devoted to it (1959: 32-45) are little more than a rambling discussion of Wittgenstein's *Tractatus*. As a matter of fact, French-speaking philosophers had access to only one paper containing information on recent developments within analytic philosophy, by Albert Shalom (1956-7).[20] Jean Wahl had always shown open-mindedness towards philosophy in English-language countries,[21] and it is rather obvious that he had to be the one who organized the meeting, although he clearly disliked what he heard on that occasion.

2. *Towards a Proper Diagnosis*

Even if not exactly an encounter between "analytic" and "continental" philosophers, but roughly between English and French philosophers, was Royaumont really a *dialogue de sourds*? There are reasons to think that it was at least not *merely* one. Before coming to that, it is worth trying carefully to diagnose what went wrong.

Taylor had already commented quite rightly that "both sides were insufficiently prepared, and knew too little about the other to engage in a really fruitful dialogue" (1964: 133). On the one hand the guests, with the obvious exception of Ryle, simply felt that their job was to explain what they are doing and answer questions, not engage in any criticism of any philosophy on the continent, but at time they seem caught off guard, as if they did not expect

resentative of the Vienna Circle in France, and his reputation after the war as a *pétainiste* certainly did not help to endear French philosophers to logical positivism. One could reply here that they also notoriously brushed aside Heidegger's politics, because it suited them to do so, and Wahl, a Jew who was interned at (and escaped from) Drancy during the war, exemplifies this double standard. Rougier and Wahl also did not see eye to eye for personal reasons since Wahl's first appointment at Lyon in 1930. See Berndt and Marion (2006: 27-28).

[20] A paper by Ayer (1958) translated by Philippe Devaux also appeared in *Dialectica* at the same time as Royaumont. The first footnote of Shalom (1956) lists a number of British philosophers whom Shalom met while writing his paper. Shalom, who is mentioned by Austin in the *discussion* (*infra*, p. 251), was born in Egypt and educated at Cape Town and Paris, and he was in the late 1950s *chercheur* at the CNRS, where he remained until 1965. He then moved to McMaster University in Canada for the remainder of his career. He should be noted for having published the very first paper on Wittgenstein in France (1958), before the better-known (Hadot 1959).

[21] Wahl was indeed widely read in English-language philosophy: his doctoral thesis had been on *Les philosophies pluralistes d'Angleterre et d'Amérique* (1920), dealing with British and American currents against Bradley's monism, including William James' pragmatism, and he published in 1932 an influential book *Vers le concret. Études d'histoire de la philosophie contemporaine. William James, Whitehead, Gabriel Marcel* (1932). It is worth noting, therefore, that Wahl was interested in English-speaking philosophers that were for the most part simply by-passed by analytic philosophy.

some of the questions, relating to their alleged "method" and presumed wholesale rejection of philosophy of the continent. On the other hand, their hosts were evidently for the most part not up to date, and often voiced what are but barely veiled prejudices. To take only one example, René Poirier's comment on the insufficiencies of "linguistic analysis" in ethics – his contribution to the *discussion* comprises a compendium of such prejudices:

> I should say, however, that one could probably not get rid of moral nihilism by purely grammatical means. (*infra*, p. 242)

Of course, no definitive "refutation" is to be had in philosophy, so all that one can hope for is to provide strong enough an argument undermining the other position, and there is no reason to think that analytic philosophers could not provide such arguments in ethics.

Ignorance breeds prejudice and if some participants actually refrained from speaking their mind,[22] there were hostile remarks on both sides. However, Ryle's emblematic reply to Merleau-Ponty, mentioned above, was *not* one of them. As it has been noted many times by now,[23] Merleau-Ponty's question to Ryle (p. 95) was not about any commonality between their respective programs, but between Ryle's and Russell's pre-war philosophy. And Ryle's negative answer, a few pages later in the transcript (p. 98), was of course the correct one. It would have clarified this point to Merleau-Ponty, and was not a direct put down of his repeated entreaties to recognize that their respective programs are not so far apart (pp. 93-96).

Still, Ryle's railings against Husserl can hardly be excused. He was after all one of the very few British philosophers with any command of the phenomenological tradition prior to the war, and his negative attitude towards it goes a long way towards explaining the ignorance in which it was kept in Britain for a long time. Although commenting on Beck's misleading anecdote, Hanjo Glock was not off the mark about Ryle's attitude at Royaumont:

> Ryle seemed interested less in establishing whether there was a wide gulf between analytic and "Continental" philosophy than ensuring that there would be (Glock 2008: 63).

[22] For example, Jean Wahl himself, who harbored deep prejudices against analytic philosophy, that surfaced on occasion, e.g., in the epigraph to this paper or when he described "Anglo-Saxon philosophy" in his *Tableau de la philosophie française*, as "tangled up in and weighed down by idle positivist chatter (chatter against chatter) and naturalism" (1972: 177).

[23] See Glendinning (2006: 73), Overgaard (2010: 901-902), Vrahimis (2013a: 150), Vrahimis (2013b: 178). Culprits who swallowed Beck's account hook, line and sinker include Simon Critchley (2001: 35) who went as far as to compare Ryle's rejection to Thatcher's refusal of Delors's plan for European Union.

Even Austin also appears curtly to dismiss phenomenology in the *discussion*, if it is what he meant when speaking of a method "which is rather fashionable at the moment on the continent" (*infra*, p. 252). One should notice, however, that Austin was equally uncharitable, since his claim was that *no* actual method, including *his own*, would, *for the moment*, help us tackling metaphysical problems.

Ryle's attitude notwithstanding, it would be wrong to follow Taylor (1965: 133) and put the blame squarely at the foot of analytic philosophers. Jonathan Rée provides us with one extreme case of this:

> It was hardly a meeting of minds: the French hosts manifested a respectful curiosity about "Anglo-saxon philosophy" and "the Oxford School", but the "Chorus of Oxford analysts" huddled together in self-defense, as if they feared some kind of intellectual infection form the over-friendly continentals. (Rée 1983: 15)

Wishing to mock analytic philosophers, Rée refers here to the beginning of the question period following Leo Apostel's paper, but the transcript actually contradicts him. Reading it, we find that Austin elicited a round of applause by what was indeed derisively described as "le choeur des analystes d'Oxford" (p. 230), because he *praised* Apostel's paper for being, albeit critical, very much in the spirit of analytic philosophy. It is thus nonsense to describe this choir shouting "hear, hear", in terms of Oxonian "self-defense" in fear of "some kind of intellectual infection from the over-friendly continentals". It was obviously a mark of *respect* for the Belgian philosopher, which was, expectedly, followed by a series of searching questions by Austin. As a matter of fact, both Apostel and Beth engaged the debate with analytic philosophers in their papers, and "continentals" were largely absent from the following discussions, as if they too were "insufficiently prepared, and knew too little".

It should also be pointed out that it is also quite wrong to describe "continentals" as having manifested a "respectful curiosity". We already saw Father van Breda's *fin de non recevoir* addressed to Ryle, but he does it again in the *discussion*, when Ayer draws the distinction between scientific and conceptual/philosophical enquiries:

> First of all, I would like to express my satisfaction at having heard my friend Ayer accentuating the negative attitude of analytic philosophy towards all undertakings of continental philosophy … It is the truth, pure and simple, I believe, that there are many continentals who do not have any real interest in your philosophy. I would dare say that it is the same thing for you regarding the continentals. (*infra*, p. 226)

The first sentence of this comment is, again, incorrectly reported by Rée as if Ayer's (1983: 15), but the reader can verify that there is no basis for this attribution. Ayer merely points out (*infra*, p. 221) Russell's intention to rid philosophy

of bogus notions, but to construe this as entailing a rejection of metaphysics as a whole – or the whole of continental philosophy – is to commit a *non sequitur*: after all, Russell never rejected metaphysics wholesale and he is known to have argued *inter alia* for rather peculiar metaphysical theses such as "space has six dimensions" or to have postulated the existence of "sensibilia" as unsensed sense-data.[24] Analytic philosophy has some of its roots on the continent and it has, *pace* van Breda, numerous, by now well-investigated, points of contact with his own phenomenological tradition.

There are further instances of open hostility both in the *discussion*,[25] and elsewhere during the meeting,[26] that cannot be blamed on the invited speakers. There is a simple explanation for this hostility, which is *the elephant in the room*: *metaphysics*. Indeed, analytic philosophy was by then largely associated in France with the anti-metaphysical agenda of Wittgenstein and the Vienna Circle. A French translation of Ayer's *Language, Truth and Logic* (1956) appeared a year after Rougier's *Traité de la connaissance* (1955), and it might very well be that their combined effect was to keep that fear alive. By the late 1950s, however, the principle of verifiability at the basis of the logical positivists' "elimination of metaphysics" had been abandoned, as Gewirth pointed out in the *discussion* (*infra*, pp. 247-248). He was also perceptive enough to notice that the Oxonians such as Ryle or Austin could not avail themselves of this principle and had nothing to replace it with. But no one else picked up on this important point. This is what one means when one says that hosts too were "insufficiently prepared, and knew too little".

It is telling, therefore – and the *discussion* makes it quite clear – that analytic philosophers felt under pressure to provide reasons for their rejection of metaphysics. Only Poirier, it seems, had noticed the existence of Strawson's "descriptive metaphysics" (*infra*, p. 241), and no one discussed Quine's critique of the analytic-synthetic distinction, which re-opened the door to metaphysics. This critique loomed large in the background, given that the debate often revolves, throughout the meeting, around the "two kinds of knowledge" thesis. Lack of preparation is the culprit here again.

The Oxonians' answers turned out to be rather conciliatory, but unsatisfactory in the eyes of their hosts. While Austin's long reply on this point (*infra*, pp. 229-231) says in essence that ordinary language philosophers are, in his es-

[24] Both in Russell (1914b).

[25] For example, when Éric Weil describes the "analytic" approach to philosophical problems – to withdraw from the conceptual to the semantic level, as Quine put it (*infra*, pp. 225-226) – as "the easiest" but not "necessarily the most fruitful" (*infra*, p. 243).

[26] For example, Father Bochénski's quip on Quine's views as "simplistic, absurd and mythological" (p. 185).

timate, nowhere near a position from which they could discuss metaphysical problems, Ryle clearly saw that they were asked to provide some "general formula, which would make the audience shudder" (*infra*, p. 228), admitted that he had none to offer, and that he could only talk of philosophers having to examine

> […] the structures and the interconnections of the conceptual schemes, more particularly in the case in which we sense a certain difficulty in connecting a given part of our conceptual schemes with another one. (*infra*, p. 228)

Here, it is rather the guests – if one excludes Ayer, who refrained at any rate from stating the obvious concerning his own position – that look accommodating and respectful. As a matter of fact, these replies by Ryle and Austin form one of the interesting aspects of the *discussion*, documenting as they are their own view on metaphysics.

A corollary to this is the threat posed by analytic philosophers to the notion of "intuition". Russell had criticized Bergson on intuition in *Our Knowledge of the External World* (1914a: 32-37), translated into French by Philippe Devaux in 1929,[27] but as it turns out both Alquié and Wahl were strongly influenced by Bergson. Phenomenologists would have cause to worry too, given Husserl's own use of '*anschauung*' and his doctrine of the "*Wesensschau*",[28] although Husserl's notion of intuition wasn't meant to refer to a special capacity of the mind or a special sort of "experience". Wahl's last book was on *L'expérience métaphysique* (1965a), where, typically, analytic philosophy is summarily dismissed in less than two pages with a captious argument on the meaning of 'analytic' (1965a: 10-11). His notion is akin Bergson's "metaphysical intuition", and Alquié, who was mainly an historian of philosophy, made central use of the latter in, e.g., his studies of Descartes (1969) and Spinoza (1981). In the *discussion*, this issue was predictably raised by Alquié:

> It seems to us [Frenchmen] that we have to choose between a method that takes language as a guide and analyses it, and a method, which, on the contrary criticizes language in the name of intuition. (*infra*, p. 236)

Alas, Ryle did not address this point when answering Alquié (*infra*, pp. 244-245).[29]

Hosts appear, therefore, to have focused on the thesis that analytic philoso-

[27] This translation was re-edited in 1971, and the corresponding passage is in Russell (1971: 45-49).

[28] This was, not surprisingly, the topic of Lévinas' first book (1930), the first book in France on Husserl's phenomenology.

[29] Alquié and Ryle agreed on one point, however, namely that what goes by the name of philosophy "on the continent" more often than not reduces to history of ideas – a common complaint even today. On the other hand, analytic philosophy is commonly perceived as "ahistorical".

phy is synonymous with 'elimination of metaphysics', and the associated idea that an analysis of language would provide grounds for the impossibility of any metaphysics. As Jules Vuillemin was to put it, derisively, analysis was perceived as "destructive of Life and Truth".[30] Chaïm Perelman, acting as chair, focused the *discussion* on what seems to be the mistaken view that there is one common method shared by all analytic philosophers (*infra*, pp. 215-216),[31] and the rejection of metaphysics merely played, as pointed out, the role of the elephant in the room. Guests did little to dispel this confusion. In a sense, they "huddled together" as Rée suggested (but not for the reasons he alleges), being mindful of fratricidal strife and emphasizing common ground. This prevented them from being fully explicit about some of their divergences about method. For example, the divide over the distinction between ordinary and formal language, a topic raised in Beth's paper, was carefully avoided. As Ayer puts it here:

> […] we are convinced that philosophy is not capable of competing directly with science, that it is a second-order activity, so to speak, meaning by this that it does not bear directly on facts, but on the way one talks about facts. And this is why, although we are, as you have been able to see, deeply divided on other issues, we find ourselves in complete agreement on this point. There is no reason to consider what the French call "la réflexion philosophique" is thinking that bears directly on facts, and not on the way one describes facts. (*infra*, pp. 222-223)

Quine also emphasized a common interest in language, in the most superficial way:

> In any case, I believe that a common trait unites us: that our activities are focused on language. (*infra*, pp. 225)

With Austin and Ryle almost caught off guard, having very little to say about metaphysics, superficial statements of unity such as these merely *confirmed* prejudices, precisely at a stage when more refined comments about differences were called for to undermine them.

One can thus see the *discussion* as documenting some of the reasons why a *rapprochement* was not possible at that particular stage. If anything, Royaumont had the effect of prolonging this period of mutual incomprehension. Metaphysics was never, from Russell to Strawson, entirely absent from analytic

[30] Vuillemin (2015: 19). Vuillemin's paper, written *circa* 1966-68, is in part a belated reaction to Royaumont. It is of interest inasmuch as Vuillemin is quite explicitly siding against his own camp.

[31] Perelman did ask an interesting question, at a later stage, namely: "Would one need to carry on a similar 'analysis' for languages other than English?" (*infra*, pp. 227), to which Austin answered positively (*infra*, p. 231-232).

philosophy, and focusing on an anti-metaphysical agenda gives us a skewed picture, even more so today given that metaphysics has now returned to its central position, while "philosophy of language" has concomitantly fallen off the pedestal.[32] The latter still retains an important role, but the revolutionary days in which it could be wielded against bad metaphysics are over; they even were at the time of Royaumont. On the other hand, few on the continent would recognize themselves today as heirs to Bergson or Husserl on "intuition" and "essences", and, needless to say, the fad for existentialism has long gone. Given that much historical work on both analytic philosophy and the phenomenological movement has also been done that emphasized commonalities over points of divergence, we are now able to step back and take a fresh look at the *discussion*, noticing, of course, how it focused on this ill-begotten issue, but also looking for more interesting material to extract from it.

3. *The discussion and History of Analytic Philosophy*

Royaumont is also a precious document for historians of analytic philosophy, that has been poorly exploited simply because it is in French, and I would like briefly to conclude by providing two specific examples, taken from Austin's and Ryle's remarks, of the sort of details that make the *discussion* so rich a historical document – its translation being for this reason very much welcomed.

It is true that most of what Austin and others said during it is already known from what they had written elsewhere, for example when Austin replies to Perelman's opening remarks, presenting (*infra*, p. 218) his well-known suggestion in "A Plea for Excuses", that philosophers should try and reach agreement on "what we should say when" (Austin 1979: 182) or reprising and even expanding a bit (*infra*, pp. 218-219) on an equally well-known passage, also from "A Plea for Excuses", in which he claimed that ordinary language already

> […] embodies all the distinctions men have found worth drawing, and the connections they have found worth marking. (Austin 1979: 182)

Still, this repeated testimony underlines the depth of Austin's debt to John Cook Wilson, on an important point of interpretation, where one usually credits Moore or Wittgenstein.[33] As Cook Wilson put it:

[32] See, e.g., (Williamson 2007).

[33] On Cook Wilson and his influence on Oxford philosophy, including Austin, see Marion (2000) and Marion (2015).

[…] a philosophical distinction is *prima facie* more likely to be wrong than what is called a popular distinction, because it is based on a philosophic theory which may be wrong in its ultimate principles. This is so far from being appreciated that the reverse opinion is held and there is a tendency to regard the linguistic distinction as the less trustworthy because it is popular and not due to reflective thought. The truth is the other way. Reflective thought tends to be too abstract, while the experience which has developed the popular distinctions recorded in language is always in contact with the particular facts. (Cook Wilson 1926: 874-875)

Here, Austin says:

We simply discover facts that those who have been using our language for centuries have found worth noting, have retained in passing as worthy of note, and preserved within the evolution of our language.

[…] if a language perpetuated itself in the speech and the writings of civilized men, if it had been serviceable in all circumstances of their lives across ages, it is likely that the distinctions it draws, just like the connections it establishes in its multiples turns of phrases, are not entirely devoid of any value. At least one will discover in it familiar things […] worth being noted, and which seem to me to be, at the end of the day, infinitely richer in variety and common sense […] than the sort of reverie in which I used to abandon myself between lunchtime and five o'clock, when I was spending my energy trying to solve riddles of the universe, just as our fine teachers had encouraged us to do. (pp. 333-334)[34]

It is to be regretted that participants did not debate this Cook Wilson-Austin point in any depth, since it obviously impinges on the very possibility of establishing *via* an "intuition" or "experience", metaphysical distinctions that the hosts apparently wanted to safeguard.

Austin is also adamant here that "analysis" as he understands it is not a mere superficial study of language, such as grammar or phonetics; it has almost the un-Wittgensteinian ambition, to paraphrase the *Tractatus*, 6.52, to touch the problems of life:

[…] the diversity of expressions that could be used draws our attention on the extraordinary complexity of situations in which we are called upon to speak. That is to say that language sheds light on the complexity of life.

I believe that it becomes evident from all of this, that our study does not end at *words*, whatever one understands by this: I suppose that one thinks here about what phoneticians, semanticists, grammarians do. But I would never wish to imply that this is what we are doing. We use words to learn about the things we talk about when we use words. Or, if one finds this definition too naïve: we use words in order better to understand the totality of the situation in which we find ourselves using them. (*infra*, p. 218)

[34] Austin also repeats the point later on *infra*, pp. 231-232.

It seems to me that this is a precious testimony about Austin's ultimate philosophical ambitions.

I have already quoted Ryle quasi-Collingwoodian comment on the philosopher's task to describe underlying "conceptual schemes" and tensions between or within them (*infra*, p. 228). Ryle is here explicitly referring to Quine's use of that expression earlier in the discussion (*infra*, p. 225), but he is not repeating Quine's point. Ryle's comment is rather an echo of his inaugural lecture, "Philosophical Arguments" (1945), when he wrote that:

> The philosopher may, perhaps, begin by wondering about the categories constituting the framework of a single theory or discipline, but he cannot stop there. He must try to co-ordinate the categories of all theories and disciplines. (Ryle 1971, vol. II: 195)

This was written, however, as a comment on Collingwood and his idea of metaphysics as the study of "constellations of absolute presuppositions" in *An Essay on Metaphysics* (1940). If it was not clear at first blush, that Ryle actually agreed with it, this passage confirms it: although he did not wish to recognize any influence from Collingwood (Ryle 1970: 13), it seems Ryle was, on this very point, influenced by him.[35] It is thus worth recalling that Collingwood's *Essay on Metaphysics* was explicitly written as a reply to Ayer's critique of metaphysics in *Language, Truth and Logic*: according to Collingwood, every theoretical activity is composed of chains of questions and answers, with a set of absolute presuppositions standing at the beginning of them, of which it is the task of a metaphysics to investigate. Metaphysics is thus "descriptive" in this sense,[36] but it is also within its remit to study tensions within such "constellations" and how they might be revised by a change from one "constellation" to another (Collingwood 1940: 48, n. 73). This is the idea that finds an echo in Ryle's comment in the *discussion*, an idea that could have gone a long way to mend the rift between some of the participants at Royaumont; a rift that was, as we saw, inflated for dramatic purposes, with the unhappy consequences that we all know too well.[37]

Mathieu Marion
marion.mathieu@uqam.ca
Université du Québec à Montréal

[35] William Lyons (1980: 13) takes the above-quoted passage (Ryle 1971, vol. II: 195) as reminiscent of the logical positivist program of a "unified science", but the context makes it plain that he is taking his lead from Collingwood.

[36] See D'Oro (2002).

[37] I would like to thank Aude Bandini, Nick Griffin and Guido Bonino for help in writing this paper, and the latter also for kindly inviting me to write it.

References

Alquié, Ferdinand, 1969, *Science et métaphysique chez Descartes*, Centre de documentation universitaire, Paris.

Alquié, Ferdinand, 1981, *Le rationalisme de Spinoza*, Presses Universitaires de France, Paris.

(Anon.), 1962, *La philosophie analytique*, Éditions de Minuit, Paris.

(Anon.), 1980, *Les philosophes anglo-saxons par eux-mêmes*, *Critique*, n. 399-400.

Austin, John Langshaw, 1962, "Performative-Constative", in Caton, C.E., ed., *Philosophy and Ordinary Language*, University of Illinois Press, Champaign (IL): 22-54.

Austin, John Langshaw, 1970, *Quand dire c'est faire*, Éditions du Seuil, Paris.

Austin, John Langshaw, 1979, *Philosophical Papers*, 3rd ed., Clarendon Press, Oxford.

Ayer, Alfred Jules, 1956, *Langage, vérité et logique*, Flammarion, Paris.

Benveniste, Émile, 1963, "La philosophie analytique et le langage", in *Les Études philosophiques*, 66: 3-11.

Béra, Marc-André, 1948, *A.N. Whitehead. Un philosophe de l'expérience*, Hermann, Paris.

Berger, Gaston, 1941, *Le cogito dans la philosophie de Husserl*, Aubier, Paris.

Breton, Stanislas, 1959, *Situation de la philosophie contemporaine*, Emmanuel Vitte, Paris-Lyon.

Collingwood, Robin George, 1935, *An Essay on Philosophical Method*, Clarendon Press, Oxford (rev. ed. 2006).

Collingwood, Robin George, 1940, *An Essay on Metaphysics*, Clarendon Press, Oxford (rev. ed. 1998).

Cook Wilson, John, 1926, *Statement and Inference*, 2 vols., Clarendon Press, Oxford.

Critchley, Simon, 2001, *Very Short Introduction to Continental Philosophy*, Oxford University Press, Oxford.

Derrida, Jacques, 1973, "Signature événement contexte", in *La Communication. Actes du XVe Congrès de l'Association des sociétés de philosophie de langue française. Université de Montréal 1971*, Éditions Montmorency, Montréal: 49-76.

Dummett, Michael, 1993, *Origins of Analytical Philosophy*, Duckworth, London.

D'Oro, Giuseppina, 2002, *Collingwood and the Metaphysics of Experience*, Routledge, London.

Glendinning, Simon, 2006, *The Idea of Continental Philosophy: A Philosophical Chronicle*, Edinburgh University Press, Edinburgh.

Glock, Hans-Johann, 2008, *What is Analytic Philosophy?*, Cambridge University Press, Cambridge.

Hadot, Pierre, 1959, "Réflexions sur les limites du langage à propos du 'Tractatus Logico-philosophicus' de Wittgenstein", in *Revue de métaphysique et de morale*, 63: 469-484.

Hadot, Pierre, 2004, *Wittgenstein et les limites du langage*, Vrin, Paris.

Lévinas, Emmanuel, 1930, *La théorie de l'intuition dans la phénoménologie de Husserl*, Félix Alcan, Paris.

Lyons, William, 1980, *Gilbert Ryle. An Introduction to his Philosophy*, Harvester Press, Brighton.

Marion, Mathieu, 2000, "Oxford Realism: Knowledge and Perception", in *British Journal for the History of Philosophy*, 8: 299-338, 485-519.

Marion, Mathieu, 2003, "Husserl et le réalisme britannique", in Fisette, Denis, Sandra Lapointe, eds., *Aux origines de la phénoménologie. Husserl et le contexte des* Recherches Logiques, Vrin – Presses de l'Université de Laval, Paris – Québec: 255-286.

Marion, Mathieu, 2015, "John Cook Wilson", in *The Stanford Encyclopedia of Philosophy* (Spring 2016 Edition), Edward N. Zalta (ed.), URL = <https://plato.stanford.edu/archives/spr2016/entries/wilson/>.

Marion, Mathieu, Claudia Berndt, 2006, "Vie et œuvre d'un rationaliste engagé: Louis Rougier (1889-1982)", in *Philosophia Scientiae*, 10 (2): 11-90.

Mays, Wolfe, Stuart C. Brown, eds., 1972, *Linguistic Analysis and Phenomenology*, Macmillan, London.

Merleau-Ponty, Maurice, 2005, "Phenomenology and Analytic Philosophy (1960)", in Silverman, H.J., ed., *Texts and Dialogues: On philosophy, Politics, and Culture*, Humanity Books, New York: 59-73.

Overgaard, Søren, 2010, "Royaumont Revisited", in *British Journal for the History of Philosophy*, 18: 899-924.

Pap, Arthur, 1956, "Logical Empiricism and Rationalism", in *Dialectica*, 10: 148-166.

Rougier, Louis, 1955, *Traité de la connaissance*, Gauthier-Villars, Paris.

Russell, Bertrand, 1914a, *Our Knowledge of the External World*, Allen & Unwin, London.

Russell, Bertrand, 1914b, "The Relation of Sense-Data to Physics", in *Scientia*, 16 (4): 1-27 (reprinted as chap. VII of *Mysticism and Logic*, Allen & Unwin, London, 1917).

Russell, Bertrand, 1971, *La méthode scientifique en philosophie. Notre connaissance du monde extérieur*, Payot, Paris.

Ryle, Gilbert, 1970, "Autobiographical", in Wood, Oscar P., George Pitcher, eds., *Ryle. A Collection of Critical Essays*, Anchor Books, Garden City (NY): 1-15.

Ryle, Gilbert, 1971, *Collected Papers*, 2 vols., Hutchison, London.

Quine, Willard Van, 1985, *The Time of My Life. An Autobiography*, MIT Press, Cambridge (MA).

Rée, Jonathan, 1993, "English Philosophy in the Fifties", in *Radical Philosophy*, 65: 3-21.

Shalom, Albert, 1956-7, "Y a-t-il du nouveau dans la philosophie anglaise?", in *Les Études philosophiques*, 11: 653-664 and 12: 47-63.

Shalom, Albert, 1958, "Wittgenstein, le langage et la philosophie", in *Les Études philosophiques*, 13: 486-494.

Taylor, Charles, 1964, "Review of La Philosophie Analytique", in *The Philosophical Review*, 73: 132-135.

Taylor, Charles, Alfred Jules Ayer, 1959, "Symposium: Phenomenology and Linguistic Analysis", in *Proceedings of the Aristotelian Society. Supplementary Volume*, 33: 93-124.

Vrahimis, Andreas, 2013a, *Encounters between Analytic and Continental Philosophy*, Palgrave MacMillan, Basingstoke.

Vrahimis, Andreas, 2013b, "Is the Royaumont Colloquium the *Locus Classicus* of the Divide between Analytic and Continental Philosophy? Reply to Overgaard", in *British Journal for the History of Philosophy*, 21: 177-188.

Vuillemin, Jules, 2015, "Un Français peut-il encore comprendre les philosophes d'outre-Manche?", in *Les Études philosophiques*, 2015 (1): 17-30.

Wahl, Jean, 1920, *Les philosophies pluralistes d'Angleterre et d'Amérique*, Paris, Alcan (2nd ed. Les Empêcheurs de penser en rond, Paris, 2005; Engl. transl.: *The Pluralist Philosophies of England and America*, Open Court, London 1925).

Wahl, Jean, 1932, *Vers le concret. Études d'histoire de la philosophie contemporaine. William James, Whitehead, Gabriel Marcel*, Vrin, Paris (2nd ed. 2004)

Wahl, Jean, 1938, *Études kierkegaardiennes*, Aubier, Paris.

Wahl, Jean, 1947, *Petite histoire de l'existentialisme*, Éditions du club Maintenant, Paris (Engl. transl.: *A Short History of Existentialism*, Philosophical Library, New York 1949).

Wahl, Jean, 1952, *La pensée de l'existence*, Flammarion, Paris.

Wahl, Jean, 1953, *Traité de métaphysique*, Payot, Paris.

Wahl, Jean, 1954, *Les philosophies de l'existence*, Armand Colin, Paris.

Wahl, Jean, 1965a, *L'expérience métaphysique*, Flammarion, Paris.

Wahl, Jean, 1965b, "Préface", in Wittgenstein, Ludwig, *Le cahier bleu et le cahier brun*, Gallimard, Paris: I-XIX.

Wahl, Jean, 1972, *Tableau de la philosophie française*, new ed., Gallimard, Paris.

Williamson, Timothy, 2007, *The Philosophy of Philosophy*, Blackwell, Oxford.

Royaumont Colloquium, 1958 – Analytic Philosophy

General discussion*

Mr. Perelman

Before beginning with this general discussion, I would like to tell our English friends that we have been following this session devoted to analytic philosophy with great interest. It allowed some to come into contact and some to become more familiar with it, and they are now better able to distinguish the analytic philosophers from each other. In any case, we have seen that the method used by analytic philosophers offers new perspectives in philosophy, that are often very interesting even for those who prefer a different type of philosophy. The various lectures helped us to see how this method can be applied to all areas of philosophy – from the most general metaphysics to the history of philosophy, spanning epistemology, theory of values and its application to ethics and political philosophy. I personally believe that the reader of the forthcoming volume would be very interested in seeing these lectures complemented by a short bibliography of the main works, books and articles that have been conceived in the spirit of analytic philosophy.

I believe that the general discussion should have only one goal, which is to give us a better understanding of the ideas and method of analytic philosophy. Through papers presented by truly brilliant figures we sensed a definite common attitude among these different philosophers without being able to say exactly on what points they agree and, sometimes, on what points they do not, when dealing with general questions relating to the method and conception of philosophy.

R.P. Van Breda

How much time do you plan to allot to the discussion of different issues?

Mr. Perelman

Let's say about one hour for each aspect.

* "Discussion générale", in Cahiers de Royaumont, 1962, *La philosophie analytique*, Éditions de Minuit, Paris: 330-380.

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I

Mr. Perelman

The first issue that I am taking up is as follows: What do you mean by analytic philosophy? What is the goal that you set yourself while practicing this method? How do you practice it? To what extent is it an analysis of language? To what extent do you go beyond the analysis of language in using this method? What does it presuppose *outside of* language? Are there experiences or certain extra-linguistic knowledge that are assumed as given as you practice this method? And, finally, what are the criteria of success for your method?

This is the first issue, and now I would like to ask Prof. Austin, for example, to tell us how he would respond to this.

Mr. Austin

It seems to me that there are a number of issues that are a bit entangled. Let's first take the label: "analytic method". I would begin by saying – and I believe that there are a certain number of my colleagues who would at least share my view – that to my mind the expression isn't particularly precise, and that I don't even think it is a label that can aptly describe what we are doing or the manner in which we are going about doing it. All you can say, if you like, is that no one yet has come up with a more satisfactory term or at least a term we agreed on, and, for my part, I do not see anything wrong with being labelled thus – this label is just as worthy as any other. But I believe it should be stated clearly that none of us is going to say that it is a method I could modestly claim for everyone who is inspired by it and, as a consequence, that there is anything specific about it. I, for my part, would add that the method I would modestly claim for myself and would like to see more widely applied could be described as a certain way of dealing with problems as they present themselves, with help of a certain number of tricks or techniques, so that this would ultimately be an art, and that one can learn bit by bit, step by step, to apply it not just to traditional problems of philosophy but to a field that has remained as of yet unexplored, to be found on the fringes of philosophy and encompassing all problems that have been excluded, ignored or neglected, but whose solution could perhaps enable us to go back to the traditional problems of philosophy with a more assured footing, problems that are – here I agree with you – difficult, worthy of respect and certainly deserving every bit of the attention we give to them.

Having said that, what are the methods so characteristic that are ascribed to us, and why do we say that they have a certain relationship to language? – Is it that I'm deviating from my subject or am I right in the middle of it? Well,

I believe that one can indeed say that we employ language for our investigations – that we put it to work in the majority of cases. At least that's what I do. To a very great extent this is also what many of my colleagues do. To put it differently, as I think I have explained, we begin by drawing up a list of everything that relates, in language, to the subject we are investigating: all the words we use, all the expressions in which these words appear. It is essential that this choice be sufficiently representative, and to insure that our inventory is complete enough we use methods that I have already described, so I shall not come back to these. We also make sure that we are to deal with a problem of sufficiently limited scope. So this, for me, is what is essential: a complete, meticulous inventory of everything relating to the topic we are investigating, and the choice of a restricted topic at least to begin with.

Then I could perhaps try and answer the question you raised, that has come up a number of times, notably in Father Van Breda's interventions. To what extent do we apply criteria that are not strictly linguistic? In which sense are we dealing with phenomena that are not strictly speaking linguistic, when we go beyond the point we have reached so far?

What we ordinarily do or perhaps, I could say, even specifically do, is to ask ourselves in what circumstances we use each of the expressions that we have noted. We have before us a complete, but chaotic list of all kinds of expressions. To what case does each one of them apply? In other words, we use the multitude of expressions provided by the richness of our language to direct our attention to the multiplicity and richness of our experiences. We use language to observe through it the living facts that constitute our experience, which we would often tend simply to ignore without it.

One quickly discovers, as soon as one directs one's attention to these things – or at least one quickly succeeds in formulating the hypothesis – that nothing happens without reason. If there are two expressions in language, one will discover something in the situation that prompts us to use one or the other, something that explains our choice. It could be that the choice seems arbitrary, but very often we note a clear preference for one particular expression, over the other. And we base our case on the hypothesis that if this preference exists there must be something in the overall context that would explain, if one were to discover it, why, in this particular case, we give preference to one, while we give preference to a second one in another case.

When we try to explain the choice that determined the use of this or that expression as opposed to another one, we very quickly notice, however, that there are many more facts or groups of facts of various types and kinds in the overall context in which a speech act occurs, than we would have initially believed. If our list is sufficiently long from the outset, the diversity of expressions that

could be used draws our attention to the extraordinary complexity of situations in which we are called upon to speak. That is to say, language sheds light on the complexity of life.

I believe that it becomes evident from all of this, that our study does not end at *words*, whatever one understands by this: I suppose that one thinks here about what phoneticians, semanticists, grammarians do. But I would never wish to imply that this is what we are doing. We use words to learn about the things we talk about when we use words. Or, if one finds this definition too naïve: we use words in order better to understand the totality of the situation in which we find ourselves using them.

I hope that I have responded to the question: "Do we go farther than words?" As far as I am concerned, I have nothing more to add.

Let us now move to, and perhaps we can come to an end here, this other question that has come up repeatedly: "What is the criterion for a good analysis?" I can only speak for myself without pretending to express the view of my colleagues but, if you like, when we tackle a problem and try to get to the bottom of it, this is roughly how we proceed. In any case, the important thing for me is, at the outset to reach an agreement on the question: "What should we say when…?" To my mind, experience amply proves that agreement can be reached on "what should we say when?", on this or that thing, even if I would concede to you that it is often a long and difficult process. No matter how long this takes, you will manage, and it is on the basis of this agreement, on this given, on this achievement, that we can begin to reclaim our little corner of the garden. I should add that this is all too often what is missing in philosophy: a preliminary "datum" on which an agreement can be reached at first.

I do not say that one can expect to proceed in every case from a given data that is seen by everyone as secured. We are all in agreement at least in thinking that this is desirable. And I would go so far as to say that some of the experimental sciences have found their point of departure and a good direction to proceed in precisely this manner, that is, by coming to an agreement on the way to determine certain data. In the case of physics, by use of the experimental method. In our case, by the impartial search for a "What should we say when?", which gives us a point of departure because, as I have already pointed out, an agreement on "What should we say when?" already constitutes agreeing on a particular way one describes and grasps facts. The only thing I would be tempted to add, if I did not fear anticipating what could emerge in the second part of our discussion, I would thus only tell as a warning: that we dot not pretend to discover in this way the entire truth there is concerning any thing. We simply discover facts that those who have been using our language for centuries have found worth noting, have

retained in passing as worthy of note, and preserved within the evolution of our language.

I know that one will say that this is not much, with respect to the eternity and the totality of history. All the same, if a language perpetuated itself in the speech and the writings of civilized men, if it had been serviceable in all circumstances of their lives across ages, it is likely that the distinctions it draws, just like the connections it establishes in its multiples turns of phrases, are not entirely devoid of any value. At least one will discover in it familiar things concerning all aspects of life, envisaged from every angle, and with the most conflicting goals, that are worth being noted, and which seem to me to be, at the end of the day, infinitely richer in variety and common sense – but here I might be mistaken – than the sort of reverie in which I used to abandon myself between lunchtime and five o'clock, when I was spending my energy trying to solve riddles of the universe, just as our fine teachers had encouraged us to do.

Mr. Perelman

Thank you very much, Mr. Austin. I have the impression that Professor Ryle wishes to tell us something, to complement your exposé.

Mr. Ryle

First of all, I would like to echo the words of Mr. Austin. If one absolutely has to use the expression "analytic method", there isn't anything necessarily wrong with that. But this does not mean that there is a particular formula or a set of tricks of which one could say: "Anyone who deals with what could be vaguely called philosophical issues in a different way does not have the right method that would allow us to find a solution to a problem <…>[1]

By "analytic method", I understand any method that allows us to find a solution to a typical philosophical problem. And I understand "philosophical" in the ordinary sense. And I also do not have the intention of going through all the meanings that the word "philosophy" has taken in the course of history.

To illustrate what I mean by a philosophical problem, I shall take a really banal example, where I think you will all recognize the type of things that may puzzle us and the type of problems that cannot be resolved by recourse to one or the other exact science.

Let us take the case of two persons: I'll call the first William, the other John. They both have good eyes. Both are looking at headlines of the same journal. They are equally well positioned to see what they are reading. The lighting is the same for both. Now, it turns out that one of the words in one of

[1] [Editors' note: the text is corrupted, ending abruptly in the middle of the word "problem"].

the headlines, let us say PARIS, is poorly printed and it turns out to be spelled P.A.R.R.I.S. John knows the proper spelling, while William doesn't. In these circumstances how can you say that John sees something (a misprint), that William does not see, while saying that what is visible for John is also visible for William? In this type of situation, there could be a very minor defect in the paper, let us say a comma that does not show up well in print, of which we could say: this is visible for John but not William, because John has better eyesight than William or because one is closer to the journal than the other. But this is not the case. I have stated that, by hypothesis, everything which is visible for John is also visible for William. Still, John sees a mistake in print; William does not see it. And the only difference between the two is that John knows the spelling, while William does not.

You will tell me that we are dealing here with a rather minor problem. One could say that it concerns the notion of things that are visible and the notion of things that are perceived. One can easily imagine this type of example coming up in an examination, on the topics of sensation and perception. But it is not the label that interests me. As I see it, this is a problem that could be intriguing to many; I know many who have been attracted to it – and I was myself caught by it. And by "analytic method" I mean any method that is capable of solving a problem of this type, capable of clearly explaining to us why we face this apparent contradiction that allows us to say that everything that is visible for John is also visible for William, while, on the other hand, we also say that John sees something, a misprint, that William does not see. I do not want to say that this problem is important but taking an important problem as an example would have taken up too much time, and this one allows me to characterize the analytic method in one word: that is, to say once again, *any operative method* that allows one to solve this type of problems, problems that are – I hasten to add – usually much more interesting than the one I have just provided.

One thing, in any case, is perfectly clear. Even though we are dealing here with questions that involve among other things a problem of vision, our problem does not figure among those that we could present to an ophthalmologist, saying: "Doctor, you know so much about eyesight and vision, how is it that everything that is visible to John is also visible to William and that still John sees something that William does not see, even though the only difference between the two is that one knows how to spell while the other does not?" Such a problem is of no interest either to the eye specialist or to the optician.

If we want to use a technical term, the omnivalent term of "philosophy", to designate this type of problem in order to distinguish it from others – and I am not saying that one wouldn't have good reasons to do so in certain cases – then,

I would say that we have here a poor example of a philosophical problem that cannot be reduced either to a scientific problem, to a historical problem or to a theological problem.

Mr. AYER

I do not disagree either with Mr. Ryle or with Mr. Austin. Yet it still seems to me that they have given us a description of the method of analytic philosophy which is a bit too broad. And at the same time they have not succeeded in explaining or, to be honest, they have not even tried to explain why we think that these methods are the only methods – we do not say this because one ought to show prudence, but we still act as if it were the case – that allow for progress in philosophical reflection. I would like to say a few words about these two points.

It seems to me that Russell, with whom the entire movement began, worked at a meta-linguistic level, and that he did this first of all for ontological reasons. He wanted to get rid of expressions that have no meaning. This is where his reflection begins. According to him, one must first eliminate all expressions that do not denote real things. He also wanted to get rid of expressions denoting things whose existence struck him as epistemologically dubious. And the method he applied was one of translation: translating phrases in which these expressions occurred in phrases with expressions denoting things whose existence is not open to doubt; for an empiricist, this means reducing, at least in an ideal sense, all types of propositions to propositions about sense data.

As to the criterion, it is also empirical. No one can postulate rules according to which one can judge whether a translation is good or not. The verification will take place by examining counter-examples. If one does not find counter-examples to the proposition that one has reached, it will be judged satisfactory. But, obviously, no proof is ever absolute. One can never be dogmatic, at best, one can state that this or the other translation is certainly not valid. This is a solution – as Mr. Austin has said – which is purely provisional.

Since Russell we have given up the ideal of translation, the practical application of which seemed difficult or arbitrary to some; and in most cases the empiricist theses that inspired Russell were also given up. Still, great resemblances remain between today's analysts and those of the past: if we have renounced translating we still try to describe how expressions that one wants to eliminate or simply explain actually function. And we still choose, even now, these expressions at least in part for ontological reasons. We do not rely on a theory of meaning that simply consists in saying that the words have, let us say, an eidetic meaning. We do not believe that there exist, properly speaking,

meanings. This is simply because we do not think of this as an explanation. Mr. Quine and myself are very close on this point. Explaining the sense of a word through its meaning is not explaining anything.

One is no longer looking for a translation assuming a preliminary agreement on identity of meaning, but for a description that tends to make puzzling expressions less puzzling.

A word now on the issue of the analysis of language. I find myself entirely in agreement with Mr. Austin, in saying that this is very misleading. But we have found nothing better. Let us take for example the work of Ryle, if he doesn't mind me doing so. One could say that in his *The Concept of Mind*, Ryle studies the way in which we use words such as: belief, intelligence, will, knowledge, action, etc. But one could also say that he studies the material phenomena of knowing, believing, acting, etc. His way of proceeding, as it appears to me – and he is here to contradict me if I am wrong – is to ask what happens typically or by necessity when someone believes something, when someone wants something, when someone acts of his own free will, etc. Obviously, we can ask ourselves the same questions in a different way by asking what these words are supposed to mean. But one is not on the same side of the net, if I can say so. One plays either the phenomenological game or the linguistic one. Whatever side one chooses, one always finds oneself at fault. Because if we say "phenomenology", we seem to cling to Husserl. If we say "linguistic", we are open to another type of misunderstanding, which would lead us to believe that we are striving to do a word by word scientific analysis of language, as linguists do, and this is not what we do.

R.P. Van Breda

Could you elaborate on this point of difference?

Mr. Ayer

That is to say, that the purely linguistic, scientific and empirical attitude leads you to state, for instance, that in Scotland the words "assiet", "giggot" are used that are not used in southern England; the regional variations of the pronunciation of the words are studied through phonetics, etc. – these are all empirical notions that have nothing to do with what people like Ryle or I do and certainly even less with what Austin does.

But we can still note in Ryle's work, and even more in Austin's work, that in studying grammar, in the most literal sense of the word, one finds twists and turns that supply clues or threads that lead to phenomenological discoveries. We find them in Ryle, for example in connection with his study of *knowledge*; we would probably find even more in Austin, for whom this investigation is not

only useful but also one of the most important. It is, however, simply a matter of degree.

Finally, if I may add another word, one of the reasons that we insist even more on saying that philosophy is an activity that bears on language is that we are convinced that philosophy is not capable of competing directly with science, that it is a second-order activity, so to speak, meaning by this that it does not bear directly on facts, but on the way one talks about facts. And this is why, although we are, as you have been able to see, deeply divided on other issues, we find ourselves in complete agreement here. There is no reason to consider that what the French call "la réflexion philosophique" is thinking that bears directly on facts, and not on the way one describes facts. In other words, for us there is no point in trying to see philosophy as a kind of supra-scientific discipline. That's all I have to say for now.

Mr. PERELMAN

Mr. Urmson would, no doubt, like to add something regarding these links between current analytic philosophy and Russell.

Mr. URMSON

I don't wish to say anything other than to express my almost perfect agreement with Mr. Austin in what he has said. If I had to discuss these things in particular with him, I would perhaps like to begin discussing certain points. But certainly today, and at the general level on which we are forced to remain, I accept what he has said to us.

Mr. PERELMAN

Even regarding Russell?

Mr. URMSON

I believe that Professor Ayer has defined Russell's position very well, remaining at the level of generalities in which we placed ourselves, which makes it impossible for us to go into details. I am entirely in agreement with the idea that Ayer approaches philosophy from the same angle, or at least more or less, as Russell did. When I say that the analysts distanced themselves from Russell, I immediately make an exception for Ayer, who is much closer to Russell than most of us. I think that it is always ontological reasons that prompt him to do what he does. You will tell me that this is also the case for many among us. Still there are nuances, degrees: Professor Austin, for example, who is certainly an extreme case in the opposite sense. It is to these extreme positions that I was thinking of when I compared today's school of Oxford and Russell's position

of fifty years ago. Extreme positions once again, such as that of Professor Austin, mine certainly, and that of several others. But many more feel much closer in heart and spirit to Russell.

Mr. Perelman

Mr. Hare wishes to add something.

Mr. Hare

Two points of detail. First of all, the word "ontology" has come up again and again in our conversations over the last eight days, and each time with a multiplicity of meanings, leading to a lot of confusion. I have no intention of entering now into a classification of all the meanings of this word. I suggest that we simply put it aside from the discussion, as long as we have not been able to clarify its meaning. I am certain, for instance, that Professor Gewirth and Professor Quine would use the word in a completely different meaning than the one shared among continental philosophers. Perhaps Professor Quine will enlighten us on this point, when he gets a chance to speak. But it seems to me that he uses it as if analysts could devote themselves to ontological studies without leaving the essential domain of analysis. I believe that this would not be true, if we were to understand the word as certain philosophers on the continent understand it.

Secondly, I would like to take this opportunity to come back to a question that has been asked to me by Professor Quine, precisely regarding therapeutic positivism. With that which one can call *esprit de l'escalier*, I thought of a better response than the one I gave him on the spot. I said yesterday that I see myself as a therapeutic positivist but I wasn't in agreement with the methods used by the most illustrious representatives of this doctrine. Upon further reflection, I would prefer to say that I am not what one could call a positivist therapist because by this term people generally mean that philosophy is only about looking for a cure to ills of this type. I would prefer to say that if our attention is often attracted to a philosophical problem because of the difficulties that arise through ordinary language, the philosopher should not content himself to resolving these difficulties but should try, as Aristotle has shown, to write a general treatise to show how one could solve them, or to give a medical analogy, to write a treatise of pathology and general medicine, which would also tell us how these arise. For me, this is the essence of philosophy. And I believe that this position is in formal disagreement with what we generally view as therapeutic positivism.

This does not stop me from being in agreement with therapeutic positivism on one essential point, upon which I have distanced myself from a good number

of my Oxford colleagues, in the sense that I believe that we should, first and foremost, restrict ourselves only to philosophical problems that leave us truly puzzled. I also believe that we should not cast our nets too far, at least at the beginning, and that it would be good to only take up topics in our research that we reasonably believe will help us to solve the difficulties that we have encountered.

Mr. PERELMAN

Professor Quine has been mentioned so often that I shall ask him to open this discussion that will start now. I ask you to raise your questions, objections and requests for elaboration here, on the comments regarding method that have been presented to you.

Mr. QUINE

I have been asked whether I agree with Ryle and Austin. I certainly agree with them to a very large extent, and in particular regarding everything that one could refer to as method. In what I do or try to do, I am perhaps not so close to them and their group or what has been called the Oxford School, even if we have common views on some points. In any case, I believe that a common trait unites us: that our activities are focused on language. I believe that one of the main reasons that we prefer to focus on language is that if we deal directly with the problems of the foundations of reality, we are in danger of introducing a set of presuppositions that touch underlying conceptual schemes relating to the most deeply rooted habits of thinking and feeling, so that none of the participants can oppose their own point of view to that of the others without seemingly being guilty of a petition of principle. You can go on discussing faculties and entities forever, that no one conceives in the same way. Everyone will maintain his own point of view, proceeding from an opposing conceptual scheme. Now, a philosophical retreat to language is an approach that helps us escaping such vicious circles. Let us see how.

The central and primordial function of language is to deal with common objects, of common size, of familiar use, of the kind one finds at the market. It is here that interlocutors can come to a perfect reciprocal understanding, even though they might disagree on ontological matters. Now, *words* themselves are one of those kinds of common objects, of common size, and therefore people can agree with each other rather well when they talk about words, in spite of all ontological disagreement. Now, here's the trick: transposing the ontological discussion to a discussion of language in such a way as to insist no longer on this or that presumed irreducible ontological *facts*, but more on the methodological assets or goals that favor this or that discursive ontological

theory. The trick is to avoid a direct discussion of the fundamental features of reality, so as to turn, rather, to the discussion of the pragmatic virtues of theories about reality.

The usefulness of such an approach that prompts us to retreat from the conceptual to the semantic level, and focus on the way we speak about things instead of focusing on things spoken of, remains, even if one thinks, as I continue to think, that the fundamental problems regarding conceptual schemes are of the same kind as the fundamental problems of physical science or of mathematical logic.

Mr. Perelman

Thank you. Mr. Jean Wahl, if you could please come forward to the podium…

Mr. Jean Wahl

I would like to be very brief. I was not always able to follow thoughts being reduced to a "swift movement of lips" from a behaviorist point of view. But I did notice two expressions, "retreat" and "avoid", in what Mr. Quine said. There is thus first of all a certain approach, a preliminary approach, by means of which you avoid certain things. For certain reasons… I would really like for these reasons to be explained. It is about this retreat and this avoidance that I request clarifications.

R.P. Van Breda

First of all, I would like to express my satisfaction at having heard my friend Ayer accentuating the negative attitude of analytic philosophy towards all undertakings of continental philosophy. It is obviously the absolute right of English philosophers to have no interest in what is happening elsewhere. But the discussion is clarified a lot by stating this openly. Quite often they meant to be understood as saying "You certainly do something different. Carry on if this interests you. That's very good". For my part, I believe that there's an implicit value judgment here, which exists by the way on both sides, and that it is worth formulating it openly. When we meet we are often too polite and show very little honesty. It is the truth, pure and simple, I believe, that there are many continentals who do not have any real interest in your philosophy. I would dare say that it is the same thing for you regarding the continentals.

There remains a very important issue, raised by the three or four speakers who have spoken this morning. By these four speakers I mean Mr. Austin, Mr. Ryle, Mr. Ayer and Mr. Quine. All four have raised an issue, which strikes me as very important: When is a problem philosophical? Mr. Austin is not inter-

ested in this question. Mr. Ryle, by contrast, has clearly addressed it. He went to pains to define the method, which should help us recognize when a problem is authentically philosophical. Mr. Ayer, for his part, said "one must distinguish oneself from the sciences and we are not interested in the first-degree type of reflection on the facts", which at least constitutes a draft version of a definition of what constitutes the problem of philosophy. Mr. Quine, for his part, advocated semantic schemas, which would be of quasi-universal application.

I can only state that a discussion on what would constitute for these four speakers the content of a philosophical reflection seems impossible to avoid. One cannot elude it by referring to historical disagreements on this issue. These historical dissensions have never stopped a number of people from believing themselves to be philosophers and us from referring to them as such. This problem is also one for each of us, and as the problem should be solved before one can determine the "subject-matters" of philosophy (to take up the expression used by Mr. Quine), I'm afraid it has to be solved beforehand, and in a definite fashion. I add that what I heard here, this morning, as describing what the analysts deem philosophical, is neither very clear nor satisfactory to me. I do not presume that your school does not recognize some characteristics of philosophy as such, but I admit that I myself am unable to clearly distinguish them.

Mr. Perelman

Before giving the floor to professors Austin, Ryle, Ayer and Quine to respond, I would like to ask a last question myself which relates, I believe, to what Professor Austin said. We look for turns of phrase that we can find in English, because this group of expressions has seemed sufficiently important to a cultural group that uses this language, and this is enough for us, he tells us, to attach importance to it. The question I would like to ask is the following. I have the impression that there are reasonable beings outside England and apart from English speakers, who also have very different expressions in their own language, and if we wish to go beyond a conception that is not just the one that has been found to be important in English, we would have to do the same work in all of the cultures that have been able to find that certain distinctions are important and reveal something about the structure of reality.

Right now, I open the discussion to the floor for those who would like to respond. I suggest that Professor Ryle answer the questions that have been directed to him and to which he wishes to respond. I don't think he will want to respond to all of them. I would then give the floor to Professor Austin, to Professor Ayer and to Professor Quine.

Mr. Alquie

Can we ask that the responses be quite short so that we have enough time to address other issues?

Mr. Perelman

Yes, but their responses will be shorter than we would like.

Mr. Ryle

I do not think that any of the questions was addressed specifically to me. The question raised by Father Van Breda was directed to us four. He was anxious to hear us explaining what philosophy is. I do not really see what satisfaction he could derive from a response that would be very brief. But he seemed to find something terribly negative in simply saying that nowadays a philosopher, in the sense we now understand philosophy, is no longer an astronomer, for instance, or a chemist or an ophthalmologist, though a chemist, an astronomer or an ophthalmologist could be a philosopher at the same time. This reply does not satisfy him. It is not enough for him either that one could cite several problems that I would say are typically philosophical as opposed to scientific problems.

He is thus asking us to provide some general formula, which would make the audience shudder, at least, I hope, as it makes its author shudder. What would he say of this one? Professor Quine has mentioned – apparently without making anyone shudder – *conceptual schemes*. This is not the exact expression he used. He has spoken of *underlying conceptual schemes*. Well, I suppose that we could say that one task of the philosopher would be to examine the structures and the interconnections of the conceptual schemes, more particularly in the case in which we sense a certain difficulty in connecting a given part of our conceptual schemes with another one. Or something to that effect. Of course, we would have simply shifted the problem, because you would be entitled to ask me what these splendid conceptual schemes consist of, what my understanding of this grandiose expression is. I would much more prefer to offer you a varied selection of all the problems which have always been considered and stand a great chance of being considered for a long time as typically philosophical. But I am really afraid that Father Van Breda will only say that I am hiding behind the examples, since what he wants is a formula.

R.P. Van Breda

Let me ask you simply, Mr. Ryle, whether you would be willing to accept, on this list, as one of the central problems of philosophy, the problem that one must decide on thought, on philosophical reflection…

THE PRESIDENT

With your permission, we would like to keep your reply for the second part of our discussion, so that we do not already begin with this second part. Are there any further questions you would like to respond to?

Mr. RYLE

I think that's all I had to say for right now.

Mr. AUSTIN

I will not deal with questions Professor Ryle has already answered, that either have to do with the boundaries between what we do, no matter how we name it, and philosophy, or call for a definition of philosophy. I said everything that I had to say on this earlier and I will not repeat myself. Moreover, I did not realize that we were to address this question at this stage in our debate. For the same reasons, since I already said everything I had to say and since I do not think this question relates to the topic at hand, I will not try to give a response regarding the boundaries that separate linguistics and analytic philosophy, whatever one might understand by this. I would simply say, if you like as a sort of warning, that I, for one, do not think that the boundary is so strictly defined as Professor Ayer led us to believe.

Therefore, to take up questions that seem to me to be more directed to me, let me begin with the first one. It has to do, once again, with the method of analytic philosophy. We are being asked why we do what we are trying to do when we act the way we do. When one asks me this question, I feel a bit as if in the situation of one of my colleagues, a father, who every time he was about to punish his children was held back by the fact that he could not remember the reasons one has for punishing children. The same holds for me. When someone asks me why I do what I am doing, I remain silent. Everything that I can say, in the most insistent way, is that the word "method" displeases me. I much prefer the word "technique", and even more the plural "techniques". On this point, I feel that it is important to make a distinction when we examine our relations, as friendly they may be, with someone like Professor Quine. Everything he has said about the way one has to consider language and the importance of this inquiry for philosophy, strikes me as extremely appealing. I love what he does. I appreciate what he has achieved. But I am perfectly aware of the fact that he uses techniques that are completely different from mine.

Mr. Goldmann's question relates again to method. He asked me what this agreement that we are seeking consists of, and, first of all, an agreement between whom? This question seems to me entirely legitimate. One could also ask a physicist: with whom would a physicist seek an agreement? I would say,

first of all, an agreement among all those who would like to go through the trouble of seeking one. And if we come to an agreement, then all the better. And if we don't, all we have to do is look elsewhere, try something else, aim for a different experiment on the basis of which we could hope to come one day to an agreement. The best we can do is to choose a certain number of sharp-witted colleagues of a quarrelsome nature.

But I might also add something to what I just said earlier about the importance of finding a *datum*, on the basis of which consent could be reached. There is a third party we could call up – one that would help us to reassure ourselves that the agreement we have obtained is not in vain, that it will stand the test of critique and facts. This would consist of inviting a group of outsiders to our group, but ones who would know how the thing works, to repeat the experiment, to resume the investigation of the same expressions, to see if they would reach the same results as we did. Here, I wouldn't know a better way to proceed than to say again what I said regarding Mr. Apostel's demonstration the other day. Nothing can be more encouraging for me than to see how someone else tries the same things and proves that he is able to reach similar results, just as well or even better than I could do. This is what we have always recognized as the characteristic approach of scientific thought, even when applied to a limited field.

Both Professor Ayer and Professor Urmson spoke about Russell, what separates us from him and what brings us closer to him. To this I would add that there are still great differences between us. My general line of thought aligns itself very closely to that of Mr. Urmson: take what comes and take it easy. But when Ayer comes along and tells us with a certain virulence that, if we do not take the road of language, we must be shown a different road that allows us to get to the facts without the detour of language; when he adds that this attempt has already been made a number of times and that history shows that it has been in vain, dangerous and sometimes quackery, well, then I should say that I agree wholeheartedly. In Socrates' time one would customarily say: "Why does he waste his time with words when he should be dealing with the nature of things?" And already Socrates responded in a way that seemed right; I still agree with what he says.

This was not the only reproach that was directed at him. You will remember that Aristophanes found it frivolous that Socrates would waste his time measuring leaps of fleas. If others after him had passed their time measuring these leaps like Socrates, they would have invented physics several centuries earlier. And I would say, in the same vein, that if people since Socrates had followed his example and opted for the path of language, sticking to it instead of beating around the bushes in all directions in search of the hidden paths of

things, philosophy as we know it, which does not seem so bad to me, would have been invented many centuries ago, as it partly was in Athens. Indeed, we are rediscovering it.

One word in response to what Mr. Hare said. Well, he certainly knows very well that I do not agree with him when he says that we should confine ourselves to the central part of philosophy, when choosing the words we are going to examine. I know he is not in agreement either when I say we should try to find our topics in less septic, less bitterly contested regions. I for my part see three good reasons: first, we warm up without getting too hot; secondly, the great problems that have resisted all frontal assaults could be breached if we attack them at an angle; thirdly, and this seems to me by far the most important, isn't there any risk in claiming to know in advance what the most important problems are? And this even if we suppose, what still remains to be seen, that we could claim to know the best method to take them on? I believe that, by taking a step back, we would have a greater chance of perceiving the outline of the peaks and of finding the best path, as we go along. Here, the example of physics is once again instructive. By tinkering right and left with instruments, as Faraday did, we have more chances of hitting upon something really important than by saying one day: "Let's tackle some great problem: let us ask ourselves, for example, what is our universe made of?"

Mr. Meteroc has asked two questions. He wonders first how we apply the concept of the totality of language and if it includes this crepuscular thing that is called "artistic language". He also asks how we can find a principle of classification for parsing such a diverse and also cumbersome collection of expressions, since it brings together something like fifteen thousand concepts that are so many varieties of one and the same kind, for each one of the attempts that we make. I believe that one can respond with just one word to two questions as Professor Ryle did: *ambulando.* As with physics or in the natural sciences. You will find the principle of classification of your beetles only by arranging all the beetles that you find, by counting the number of species and different varieties and looking at them well enough to identify them. There is no other way to proceed. In any case I am sure that one cannot say anything in advance. The same goes for the notion of the totality of language and the crepuscular fringes where daylight plays with shadows: we would only know how to tackle them by going there to look and see. Far be it from me to want to exclude them from the field of our research. Their hour will come. I do not feel capable of tackling them right now, that's all. I understand that this response may not be completely satisfactory.

Finally, coming to our president. Mr. Perelman asks: "Why English rather than other languages? And if you must consider all languages, would your inquiry ever come to an end?" If I am not mistaken, these questions cancel each

other. I want to say that, up to a certain point, if we do what we are engaged in first in English, it is because we think that this is already a lot; to venture out into other languages would lead us much further. But I am completely in agreement with Mr. Apostel in thinking that other languages should be subjected to the same inquiry, simultaneously, if possible, or else consecutively. The great principle to keep in mind is that every language that has survived up to our time and all expressions that have survived within each language bear witness, just as they deserved to survive, if we accept the law of evolution that only respects the fittest. In my view, they all deserve our attention or at the very least, as in my case, my respect.

Mr. Ayer

Two words, since Mr. Austin has already responded to most of the questions that interested me. Mr. Goldmann asked me if what he calls "second-degree facts" – an expression which doesn't seem very felicitous to me, but I do not wish to appear as a dictator in matters of terminology; I would rather simply say "semantic facts" – and if our focusing more specifically on these facts wouldn't lead us to reintroduce a sort of metaphysics, in the guise of a "meta-fact", which he, for his part, finds shocking.

Mr. Goldmann

I did not speak of metaphysics, I spoke of *meta-facts*.

Mr. Ayer

These *meta-facts* are an expression used by you to describe semantic facts – second-degree facts if you like. I don't see how we could avoid focusing on these facts whatever name we use to call them.

As for Mr. Béra, I have not understood what he is chastising us for. He says that we assume that speech can be reproduced in written words. I think so, perhaps wrongly…

Mr. Bera

Do you believe in an *exact* equivalence between the two? Do you believe that one can see written signs as the exact equivalent of words in action?

Mr. Ayer

Let us come to an understanding. Perhaps in a given language, the language that one finds in certain newspapers does not correspond exactly to the everyday spoken language. Only it doesn't follow that one cannot reproduce it faithfully by means of written signs…

Mr. Bera
What is your take on this?

Mr. Ayer
…But I, for my part, do not see, even if we make this objection, that there are detrimental consequences for the proof.

Mr. Bera
You can see very well that we are not talking about the same thing.

Mr. Quine
I do not understand why, but it seems to me that I have not succeeded in getting my thoughts across. For this reason, I am going to try to restate this in French. It was, though, a precise, limited idea.

When philosophers discuss fundamental points of their system, there is always the danger of a vicious circle. One trick for avoiding or reducing this danger consists in transforming their ontological sentences into two sentences that deal with words and that compare their suitability with respect to the two conflicting conceptual theories. Every phrase can be transformed into a usual one that deals with words. Usually, this transformation does not pose any difficulty. But when the discussion concerns foundations, this translation into usual language has the effect of diminishing, even of completely reducing, the problematic vicious circle. Starting from this, there are philosophers such as Carnap who have thought that the fundamental problems of conceptual systems are essentially practical problems of language. But this does not necessarily follow.

Mr. Williams
I would be very tempted to enlarge a bit on this last point. I will limit myself to a brief response to Mr. Béra, to whom the translation of my talk on the *Cogito* is much indebted. He recalled that we had several difficulties with this translation and he takes issue with us for treating language as if it were a monologue. I would have thought exactly the opposite. But this is not the question, because what I say or think is perhaps not enough. In any case, we are certainly constantly aware of the fact that language is an instrument of communication, manifestations of which appear in a complex array of questions and responses where a number of interlocutors intervene. And this, as I have very briefly underlined in my talk, is intimately linked to the fundamental problems of philosophy, since all the difficulties that Descartes stumbled upon in elaborating the *Cogito* stem precisely from the fact that he, by contrast, had committed the

crucial error of treating language as a monologue. Here I believe to be in full agreement with what Mr. Strawson said elsewhere and also, broadly speaking, with Mr. Merleau-Ponty, when they see in the Cartesian effort an attempt to isolate oneself from the world and not to wonder what the relations between the self and the world, the self and others could be. This attitude leads to a dead end, where the only way out is to consider language as a tool of communication.

II

Mr. Perelman

We can now proceed to the second part of our discussion. I will first ask professors Ryle and Austin to be so kind as to respond to a question that was asked by Father Van Breda.

Mr. Ryle

As far as I can remember, the question was specifically the following: What is the relationship between the analytic method and philosophy? Or something like that. Am I mistaken?

Mr. Perelman

That was my question, not Father Van Breda's.

Mr. Ryle

Whatever the case may be, I would prefer, for my part, to ask, for example: What is the relationship between the method or methods used by mathematicians and mathematics? I would understand it better in that case. If you discuss questions of a certain kind, there is more than one way to find an answer that will lead you nowhere. If the problems that you are discussing are of a philosophical nature the only way to obtain a satisfactory response, if you are able to, is to use the appropriate method and not another one. Let us consider again my rather simple and rather tedious example of John and William. If you were vexed by the same problem and tried to solve it by means of the instruments of an optician, you would certainly be doing something silly. You have to examine the apparent contradiction between the fact that what is visible for John is also visible for William and the fact that John, however, sees a misprint whereas William does not. If you wonder where the contradiction lies, where it shows up, then and only then are you embarking upon a philosophical inquiry, something distinct, if you like, from a scientific investigation that could be undertaken by an optician.

Mr. PERELMAN

I will now turn over to Mr. Austin.

Mr. AUSTIN

I will only say a few words. I said, and I can only repeat it here once more, that for me philosophy has always been the name that one gives to a holdall in which we provisionally put all the problems lying about, for which no one has yet found respectable use, a unanimously accepted method for dealing with them. All we claim to have done is to have found a technique for clearing a small corner of the bushes. In English we often say: "Be your age". We could add: "Be your size". One must be of one's age and size. We are modest-sized, we begin modestly. If we were giants such as Descartes or Husserl, I would then say: let us begin from the beginning.

Mr. ALQUIE

I would like to express a wish rather than ask a question. One of the main goals of this colloquium is for us "continentals" to become acquainted with analytic philosophy. I believe that one of the things which separate us from our English colleagues is the habit that we have of constantly referring to the history of ideas. For us, philosophy is, first and foremost, what Descartes, Spinoza, Kant, etc. have said. Now, it is very striking that in the discussions we have heard the references to these authors are extremely rare. Yesterday I had the opportunity to ask Mr. Hare how his conception relates to Kant's and it seems that what he said to this matter shed considerable light on the question for us Frenchmen. I would like to express the wish that in the responses to the questions that will be asked, our English colleagues try to explain the relationship of their philosophy to that of English philosophers with whom we are more familiar: Berkeley and Hume. In many of the formulations that have been presented to us, we thought that we recognized echoes of opinions we know or believe to know to be those of these authors. Thus, when Mr. Ayer just said moments ago that one of the sources of analytic philosophy was the aspiration to eliminate expressions that do not possess any effective thought content, we are reminded of Berkeley and his critique of the notion of matter. Because this critique consists entirely in showing that behind the word "matter" we are, strictly speaking, thinking nothing. And Hume also shows that when we talk of "cause" we have in mind something completely different from what we believe we have in mind. But among these authors there is a fundamental difference between, on the one hand, the analysis of language alone and, on the other, the much more important opposition between language and intuition. Because the last word, for them, belongs to intuition and

it is ultimately in the name of mental experience that they criticize language. The fundamental question of philosophy is: "What do we really have in mind when we say that…" This is why, in Mr. Ayer's talk, there is a point that has remained obscure for me: Mr. Ayer declares that the analytic method is, in a certain sense, phenomenological and in another sense linguistic. Now, at least for us Frenchmen, there's a contradiction here. It seems to us that we have to choose between a method that takes language as a guide and analyses it, and a method, which, on the contrary, criticizes language in the name of intuition. It is for this reason that a clarification of the relationship between the analytic method and that employed by Berkeley and Hume would be valuable to us.

Mr. Poirier

May I share here some reflections, some impressions that have been suggested by the lectures in this colloquium, that have brought us into more direct contact with a school, of which some ideas were, of course, not entirely unknown to us? One could group them as follows: total agreement on the very point of analysis of language; several reservations regarding some of its methods and norms; a lot of doubts regarding the philosophical scope.

I. We should not be inclined to oppose a project that has ultimately nothing revolutionary about it, in spite of the title of a rather well-known collection of Oxford papers,[2] that impresses us essentially by virtue of the art with which it is realized. Its initial aim is actually in agreement with those of a number of classics[3], and its conclusions actually seem to continue those of other recent schools: how can one be surprised to see neo-empiricists considering them as epigones or phenomenologists not formulating any objections about principles? Aren't we all more or less analysts without knowing it, just as Mr. Jourdain was a writer of prose?

Who will dispute, for instance, that a study of philosophical problems should begin with an analysis of fundamental notions and the elimination of ambiguities? Wasn't it Descartes who wanted the problem of essence to be addressed before that of existence and all thoughts be defined? We all know that the most familiar words, truth, falsity, demonstration, value, mind, mass have different, often irreconcilable, meanings, and that one should not uncritically apply to them common formulas. The famous principles of logic are the very example of equivocal sentences. The philosopher, the logician must therefore, before anything else, analyze language and make it unequivocal. This is the reason why there is an analytic and linguistic task in logic that precedes the

[2] *The Revolution in Philosophy* [ed. by Alfred J. Ayer, London, Macmillan, 1956].

[3] It could legitimately claim to be that of Aristotle.

actual synthetic or constructive task. Moreover, moving from alpha to omega, one can say that the definition of the word *matter* is the definitive task of physics, the definition of the word *universe* that of cosmology, that each science ultimately defines its subject-matter and that its goal is to establish a natural language just as Duhem said that its goal was to establish a natural classification.

The analysts, however, essentially adhere to ordinary language. That is in fact a natural point of departure, but only with the purpose of correcting it and passing from actual usage to a normative, ideal use of words. Isn't this the perennial method of Socrates and Aristotle: going from everyday to rational or scientific language, from a bad to a better language?

As for knowing if one should speak of analyzing language or thought, of defining words or ideas, the names or the things or the concepts, this is hardly important.[4] We all know that thought only materializes, only becomes something real and concrete within a discourse, that discovering a true thought beneath a misleading language means substituting clear phrases that are well understood and universally accepted as expressing the authentic meaning to misleading expressions which were not felt satisfactory. But this in turn presupposes that there is something deeper that determines and justifies this substitution, something we call thought, meaning, idea, in the same way as there is something that determines our perceptions, our experiences, our physical measures and that we call physical reality, no matter how difficult it is to define it. One can always, for logical purposes, speak of all of this in terms of behaviors, dimensions, discourse, that which will once be referred to as behaviorism or as experimentalism, or as nominalism, or phenomenalism or even idealism. It is a fact that I don't know who my neighbor is, or not even who I am, and I can only relate to visible body the audible words of my neighbor, and that his being, his mind, his personal character reveal themselves only as a law governing his physical actions. This law must itself exist in a certain way and it is altogether much simpler and undoubtedly truer to say: I speak with X, or I love X, than to say: the words of my body correspond to the words of body X, the emotions and the feelings of my body correspond to the presence and the gestures of body X. The same holds true for language. So, let us leave aside these squabbles over words, and speak freely in terms of thought, in accordance with the classical usage, or in terms of language, following the present style, since they are equivalent.

Let us no longer ask if the philosophical problems have a linguistic or mental foundation, if the problem of God, for instance, is born from an inner belief

[4] After all, a collection of papers by the most well-known analysts is entitled *Essays on Conceptual Analysis* [ed. by Anthony Flew, London, Macmillan, 1956].

or anxiety (well-founded or not) or from the fact that one finds the word God in dictionaries or books. All of this has little importance.

And perhaps it is also useless to wonder if the analysis of language stems from logic, linguistics, psychology or sociology. But here the problem becomes more complex, because this brings us to questions of methodology regarding which there is less spontaneous and much less universal agreement.

II. It is indeed difficult to escape the feeling that some of our Oxford friends take language, and ordinary language in particular, to be something absolute, within which the different meanings of an utterance and the rules of its usage, once they have been properly analyzed, are perfectly determined, thus *ipso facto* determining our judgment regarding its value and its truth. It's a bit like in Freudianism, where becoming aware of conflicts and complexes has, in and of itself, a healing virtue. It is also a kind of anti-psychologism, extending that of the majority of logicians: one would say there are natural, objective, that is, necessary implications of language in itself, while in the eyes of the majority of people these implications are those that have been attributed to it by various persons, based on the psychological meaning that they give to words and expressions. There is here something misleading and quite paradoxical for which one finds an equivalent in phenomenology. If we don't relate language to ideas, essences existing in themselves (or in relation to a divine thought), and if we don't relate it either to individual minds that speak it, it is difficult to see where it stands and what implications it determines. Here, all that one can say is that I cannot see what you are talking about and to what you are referring. But I fear that I am over-stretching here the thoughts of these authors and distorting a bit their theses, so as to be better able to assert their originality.

As a matter of fact, the meaning of a discourse is something very indeterminate and something that varies with a time, a milieu, the circumstances, the interlocutor, his culture and intellectual orientation, in a word, with the entire logical, psychological and social context. One does not isolate a phrase from the tone in which it is spoken, nor from the attention and from the resonances that it elicits in the listener as in the speaker (these are not always the same, and this is the source of quite a few misunderstandings!). And this is why an analysis of ordinary language, even for the words of everyday usage, is often risky. We saw this very well in the attempt to analyze diverse French semi-synonyms, an analysis that is both subtle and incisive and yet it has been convincing only to a certain number of us. And if this is the case in ordinary language, what to say about the writings of philosophers? Will one purport to apply ordinary semantics to them? The objection was made with great pertinence.

Now, according to which rules do we analyze language, following which methods and which norms?

We evoke the etymological criterion only for the record, since it is clearly unusable in general.

Are we appealing to a purely positive and linguistic criterion of universal assent? Yet we know how fragile it is, how relative meanings are, and how illusory an exegesis based on "common sense" would be.

Do we proceed in a purely empirical and individual way according to what we know of the author and the context of "discourse"?

In fact, in a philosophical discussion we manage to reach an agreement on the words and the expressions only by referring implicitly to experiences and theoretical evidence, which presuppose the shared principles, a common interpretation of facts and ideas that have a normative status. Analysis can only take place within the framework of a doctrine: Thomists only have a common understanding within Thomist philosophy, Marxists only within the Marxist dialectics, which they consider to be ideally required. But we seem to be on a rather different level from the one within which the "analysts of language" proceed.

The notion of norm also enters into play almost inevitably, from two points of view: there are well-made languages and there are incorrect ones, and there are good and bad ways of speaking a given language. There is a sense *de jure* and a sense *de facto* of a given phrase and of a given discourse, but the sense *de jure*, the theoretical sense, is not fixed *ne varietur* and depends on the fact that at least an elite has adopted and preserved it. It is thus quite difficult to remain on a level of pure description and not to bring value judgments to bear in the analysis of language.

I would also say that I was surprised to see that two notions were never invoked, those of historical evolution of language and virtual meanings.

If we want to draw philosophical consequences from the analysis of language, we must envisage its history and its effective or ideal progress. Without this we could just as well philosophize by analyzing prehistoric language (supposing it is known) or that of the pygmies or the Andamans. Language is not simply an established fact which poses static problems: how, through which instinct, which preadaptation, which divinations have we been able to understand and to speak a given language? how can we translate one language into another, that is to say, what does a given discourse in a given language mean? It is also something that is renewed, it re-invents itself, transcends itself, deepens, along with the thought that it represents, and understanding how this is possible and how it emerges is the dynamic problem of language.

When I speak of virtual meanings, I mean to say this: a word, a sentence do not simply have a current meaning, they do not translate something prepackaged, they are the indicators of a meaning that will be constructed later on,

they correspond to needs and promises of meaning, just as many propositions contain the promises of a truth that can only become actual to the extent that the meaning of the terms that appears in them becomes more precise. These are certainly not clear ideas, far from it, but this does not prevent them from being well-founded. In any case, intending to analyze language only in frozen actual meanings would be tantamount to the psychological atomism of the last century, that is, to the most illusory of empiricisms.

The conclusion to all this is that language is the expression of a thought which is half-real, half-virtual, in the minds, in a culture, in a society, in an experience, in a history, in a process of progress, and can only be understood as such. The analysis of language can thus only bear fruits, if it is not purely linguistic (in the sense of an autonomous science as opposed to psychological and epistemological considerations). What is more meaningful in language is its law of development and its implicit promise. Perhaps Oxford should be inspired here by Newman.

III. One the last problem remains: that of results. No matter how interesting the description and elucidation of terms and sentences is and no matter how appealing the work of glossarists can be, where does all this lead us and in what way does it constitute or proclaim itself as philosophy? I have just alluded to the "Freudian" hypothesis according to which becoming conscious of the meaning of language would *ipso facto* cure philosophical illnesses, but we would still like to have some example of such cures and I would like to know how the analysts think here to put their method to work.

First of all, as far as language itself is concerned, do they claim to rectify its use and structure, given that they are the opposite of artificial languages, of formal symbolism? Would they accept an eventual codification?

One would happily grant in general that, apart from deductive formalisms, artificial languages can only represent schemes that are much too flimsy and too conventional to be adapted to any concrete reality. Uprooted from a tradition of thought that is not without biases or dangers but which reminds us, with respect to each word, each expression, of the diversity of their meanings and of the problems and arguments associated with them, we would not be able to fix arbitrarily, through some simple conventions, the meanings of concepts that history has surrounded with resonances and caveats. Replacing old terms by new symbols is not enough, and in losing a tyrant one also loses an advocate, an advisor and a guide, because language is the daughter of history and of all thought. It is thus reasonable to proceed from the experience of everyday language, rather than to construct formalisms off the bat and somehow identify them in retrospect with a reality as best as one can. Do we, in order to create a resembling statue, geometrically arrange cubes or prefabricated polyhedrons?

Moreover, we would not be able to codify ordinary language in a rigorous way.

First, because it is directed to real people, in given circumstances, in a general human context. Even if we intend to "speak our language well", we speak to be understood, that is, in relation to the interlocutor and the moment.

Second, because everything depends on what we want to say. Is it a matter of proving something in a rigorous, logical way? Then we need rigid and mechanical concepts into which a formalized language fits. If, by contrast, it is a matter of persuading, inferring intuitively, becoming aware of a reality, describing an experience, deploying our *esprit de finesse*, then we need a living language, with its richness, its dynamism, its overlapping meanings, I would even say, its provisional indeterminacies.

We would thus not be able to dream of reducing discourse to a systematic assemblage of terms with a defined meaning independent from the context, because each and every discourse is a living organism. And one could no longer ideally endow language with contradictory qualities: being easy to learn and fixing the exact nuances of meaning by means of the grammar, being both rigid and fluent, fixing thought and renewing it, being logical and poetic.

We add that it is actually enough, at least in French, to know one's language well, to seek the true expression and possibly take some precautions or use some tricks to eliminate ambiguities that are linguistic in nature. But no grammar, no vocabulary, would ever give us the "true" usage of words such as God, mind, I, body, soul, nor any other important word.

Let us consider now philosophical problems or, to be frank, metaphysical problems. What attitude does the school of logical analysis suggest adopting for this subject?

Here it seems to us that we can get a glimpse of some nuances in the Oxford school, in particular in Mr. Strawson's descriptive metaphysics, which we hope to gain a better understanding of later. On the whole, though, one has the impression that the conclusions do not differ essentially when one goes from logical positivists to Oxford analysts. Whereas the former think that everything that goes against the axiomatization and the formalization of artificial languages and is not defined in terms of physical experience is nothing but pseudo-problems and *flatus vocis*, the latter seem to say that the analysis of the empirical meanings of metaphysical concepts and problems shows us an interesting diversity, towards which it is advisable to feel a kind of sympathy and a benevolence tinged with aestheticism. The practical difference is not very big. What goes beyond language does not seem to touch, for instance, Mr. Austin very much. And as far as religious problems are concerned, in general there seems to be even greater reticence.

Of course, one is free to have no interest in metaphysics, especially given that compared to the aggressiveness of the Vienna Circle and its epigones, the analysts seem really accommodating and almost condescending toward possible philosophical or ontological longings. But one would like to know if all the differences in the conclusions can be reduced to their being a-metaphysical rather than anti-metaphysical, and to their considering philosophers (in the strict sense) to be dreamers rather than delirious persons. Isn't metaphysics also for the analysts an illness of language, just as religion was in the eyes of Max Müller? Should we conclude that the notions and the traditional problems have so much sense that it is almost as if they did not have any?

If not – and to the extent that one still recognizes the legitimacy of such philosophical problems – the only method is, certainly, to prove movement by walking, and we wish that one day our Oxford colleagues will come back and bring us some examples of dealing, by means of the analytic method, with these problems that are still kept alive as legitimate, and that they will at least allow us to become cognizant of solutions that can be brought to bear on them, in this new framework.

The remarkable application of the "analytic" method by Mr. Hare to a moral problem nicely shows all the difficulties of linguistic analysis itself, without speaking of its conclusive value in the concrete cases. Here I have also felt embarrassed as when Socrates showed me, by the analysis of the words happiness, virtue, etc. that the bad person is always unhappy and the sage always happy. To be sure, if a student came to us, declaring with despair that "nothing has importance" we, too, would all respond by saying: "My poor friend, what is that supposed to mean?" I should say, however, that one could probably not get rid of moral nihilism by purely grammatical means. It is not like the case of contradictory formulas: all the judgments are wrong, nothing has logical meaning, etc. In fact, one has to consider the real sense of the formula (assuming that there is a defined and different grammatical sense): nothing has in itself, speaking absolutely, any importance; nothing merits being judged important, life signifies nothing more than a rain shower. In these conditions, we would not readily accept that "nothing has importance" implicitly entails "for me" or "for such or such". I add that if by applying the methods of the analysis of language one would succeed in making people accept that the idea of importance "in itself" does not make sense, for this very reason one would justify, I'm afraid, the affirmation (formula or idea) that "nothing is important".

Mr. E. Weil

I don't know if I should intervene in this debate because the question raised has always seemed to me void of any meaning. If one wishes to discuss method,

then one should first have a method to decide the method for such a discussion. This seems to me to be a typical case of infinite regress. I believe that philosophy is done by philosophizing and that the methods have to be regarded, if one wants to speak about them at all costs, as sediments of this living activity.

So here, if you like, a first negative point. Now, concerning the linguistic approach, it seems to me to constitute less a real method, properly speaking, than the choice of a point of attack. Of course, one must always choose one's point of attack. But – and this is not a critique, it is a question – I wonder whether this linguistic point of attack is the best possible point of attack, if, speaking of language, one does not want actually to speak of communication, and whether there are not modes of communication that are at least just as important as language.

What one could say, of course, by advocating the linguistic attack, is that it is the easiest point of attack. I would reply to this that it is in any case not clear that the easiest attack is necessarily the most fruitful.

Mr. GALIMBERTI

I have often thought, in the course of our discussions over these last days, that the vaults of Royaumont, if they hadn't had to be rebuilt when they were in danger of becoming ruins, might have had something to teach us. They may still be haunted by the spirits of those who came here centuries ago seeking shelter. We have attended discussions that resemble in a striking way those that took place in the age of scholasticism. To take an adjective that gets to the very heart of a question that is, as you know, quite nuanced, I think that in spite of all the efforts, the spirit of Oxford is still after all the spirit of the nominalists and that to this spirit of nominalism the continent is still opposing a realist spirit. I ask myself whether we aren't guilty of the same error as those people. When Father Van Breda, for example, and I am only naming him because he was the fiercest adversary, says that this cannot interest him much because the problem lies beyond, is it really his own cause that he is defending? I mean is it the cause of his clothes and of the religion in the name of which he is speaking? Because in the end this religion is always the one that has expressed his most secret message in the fourth Gospel where it is written: "In principio erat Verbum". Does he fear that he will get lost talking about the *Logos*?

But, on the other hand, there's also the strange fate of nominalism which was supposed to act as the advocate of words and which finds itself in the position of an adversary of words. They fear them. They, too, want things. Here on both sides, we want just things, we are afraid of words. Finally, it is our fate that we must always justify our words by means of things. In so doing we forget that actually, if we want to speak of a philosophical truth, we have to figure out

how to do that: "In principio erat Verbum", since ultimately we are ourselves the Word, the *Logos*. The principle of all our incomprehension, now as then, remains the same. We have a sort of veil before our eyes, which shrouds the real meaning, or better, the linguistic meaning, of language. This is why I think – no matter what Father Van Breda says of this – that the efforts of the Oxford group should be followed with great interest. Perhaps the only thing that remains to be desired is that it finds the way to elaborate a language that has also value for us or that, conversely, we are capable of constructing such a language.

Mr. Perelman

If you allow me, we will now give the floor to Professor Ryle who has to catch his flight. But this will not stop us from following the discussion so that we can address all aspects without letting any participant feel robbed of his right to present objections.

Mr. Ryle

Prof. Alquié has rightly reminded us that what is called philosophy on the continent is most often reduced in practice to the history of ideas. What he claims as true about the continent is, to my knowledge, also true of the entire world – I mean the parts of this world with which I am acquainted. I do not claim to know how history of philosophy is taught in France. I have seen how it is taught elsewhere, in different places, in particular in Toronto. The teaching of the history of philosophy consists in exhuming the bodies of all known philosophers and doing an autopsy on them. The result would pretty much resemble a collection of obituaries, in the best case.

Mr. Alquié has asked us what we think and what I, in particular, think of the history of philosophy, and what connection we have established between what we have done and the thought, in particular, of Hume or Berkeley. I do not have the intention of embarking at such a late hour on a lecture on Hume or Berkeley. I will limit myself to a brief remark, on the subject of Hume.

For many excellent historical reasons, Hume was completely unaware in his time of the boundaries that for us divide what we call psychology and the discipline that deals with what I would personally call philosophical problems or if you prefer conceptual problems. From the very title of his work, he seems concerned with the problem of the advancing of empirical knowledge on human nature. And this definition would encompass a whole range of things, psychology, sociology, anthropology, political economy, etc. I am not saying that he did not contribute to the advancement of these disciplines. But I would compare his contribution to these disciplines, a contribution many aspects of which seem ephemeral to us today, to the contribution he made to

a different field and for which he is rightly famous. So now the perspective changes.

When he tells us, for instance, that essentially the relationship of cause and effect is not of a logical nature, what kind of affirmation is he putting forward? Certainly not the kind that for him would merit the recognition of future generations of psychologists, sociologists, chemists, physicists or biologists. It is also equally clear that, by affirming that causality is not a logical relation – once again I am not citing his exact words, I'm summarizing –, it didn't occur to him to intersperse his text with quotation marks. If we had told him that he was doing a linguistic analysis he would have been the first to be surprised. Nevertheless, he made an important contribution, which remains valuable to us. There is a difference between saying "The butter is going to melt *because* it is in the sun" and stating "Tomorrow is Sunday *because* today is Saturday". There are assertions that we can include among the logical inferences or purely formal ones: there is an essential, crucial difference between making assertions of this type and asserting a causal relation between, for example, the heat of the sun and the fusion of butter.

In this domain, although he has certainly did not make use either of our methods or of our language, Hume's contribution is crucial, even if it does not increase in any of its dimensions the sum of our empirical knowledge of human nature. To say that Hume, for this reason, should be counted among the philosophers of language would still be absurd, since he uses neither quotation marks nor expressions borrowed from semantics.

Professor Weil has told us that for him the linguistic approach expresses the choice of a point of attack, which is only one point of attack among others and not necessarily the best. I love this strategic metaphor. If we had time to dwell on this, I would like to ask him to describe for us a point of attack that would not, from our point of view, be a linguistic one. Personally, I have always taken great care to avoid the use of such words, language and linguistic, which is responsible for a great part of the confusion in our debate.

Professor Wahl has put his finger on an interesting point: in certain respects, it seems quite true that certain philosophers on the continent are quite close to Anglo-Saxon philosophers, while in other respects they seem very distant. The symptoms of these differences are recognizable. One could suggest several diagnoses. I would perhaps venture to frame one. We, too, for our part, first felt a need in the still recent history of the movement to elaborate, if not a system, at least a group of responses to very general, overly general questions that concern philosophers, though ready to go back to details and find out how the system would apply to this or that domain. My readings and the meetings in which I have recently participated have left me with the impression that this

conception is still widespread among classical philosophers, many of whom think that it is their task to elaborate as soon as possible something that can be considered to be their system; and if their efforts do not go beyond volume I, they would be allowed to leave aside anything they could say concretely about the application of their system in detail. Our own conception would tackle the problem the other way around. We tend to believe that if you are not capable of attacking head on problems about details, it is futile to pretend you possess a skeleton key that would give you access to the Problems written with a capital letter. If you would like another example: when someone comes along and tells us that he knows how to extract food from soil and then we discover with time that he confuses all grains and that he does not know how to dig his own patch of the garden, we suspect that he is being lazy and is trying to make a name for himself the easy way. Whatever the case may be, and I do not want to be too brutal, this tendency exists and manifests itself also on our side of the Channel. We think we are cured from it, even if sometimes the patient becomes used to his treatment… There are these addictions that make the symptoms resurface, as you well know.

Before taking leave from you, I would like to tell you how much pleasure I have derived from this meeting. The simple fact of being together, trying to see, apart from what people publish, always belatedly, what they think and the way they think what they think, to observe the faces, to grasp the inflections of voice, is of inestimable value. The discussions that have taken place here, which have been organized with a great care that we all appreciate, has given us a valuable and daunting revelation of the points of contact that bring us closer as well as of the differences that separate us. I will not speak at length of the charm of the location, the cordial atmosphere that reigned throughout. I will simply say that for those of us who have had the opportunity to participate in these discussions, Royaumont will remain an enduring memory. I would like to thank Professor Jean Wahl in particular for his welcome he has extended to us here on behalf of all his colleagues.

Mr. GEWIRTH

I am neither a continental nor an Englishman. I have come here as a neutral observer, or if you prefer, as one who is geographically neutral. I have come to listen and take note of the issues. And the impression that stands out for me from this experience is one of confusion.

On the one hand, it seems in fact that the English have apparently said such accurate, such reasonable things that it would be difficult not to be in agreement with them. They have defended their case very well. They have given us solid reasons to believe that they are innocent of a number of things that they

have been accused of, most notably, an excessive obsession with language. Yet, at the same time, I cannot but think, surrounded by all the *Gemütlichkeit* (to place myself on another neutral ground) that has continued to reign throughout all these debates, that the underlying differences between the different speakers tended to become confused, and that one lost a lot of clarity in that respect.

I will draw inspiration from one of the last remarks of Professor Ryle, to find my own point of departure for a new attempt of this type, to point at least to a possible direction of convergence or at the very least of a better mutual understanding of the divergent points of view.

As the discussion has progressed, and again and again on various occasions throughout these days, we have sensed that one or the other representative of philosophy on the continent was refraining from saying or would actually say: "But you are addressing the same problems as us!" This is true, I believe, at least for the similarity of topics. But, to take up once again the oversold expressions, method or technique, the differences are flaring up in the manner in which both of you address the same subjects. And in these differences of method or technique, I would, for my part, see more than differences of style, real divergences of approach.

As striking as they are, these differences remain in my view very often expressed in a negative way. One cannot deny that the English display, towards what is happening in philosophy in the rest of the world, a kind of contempt. And this disdain surprises us, to the extent that some of us ask on what criteria they rely, so as to reject wholesale all other ways of conceiving philosophy. It goes without saying, I believe, that the English feel only suspicion towards the undertakings of a Heidegger. We see this mistrust gradually spreading to other philosophers. On what is it based? What justifies it?

It seems – and I can speak more freely now that the specialist Mr. Urmson has left – that one could write the history of the analytic movement, at least a great part of it, by addressing the criteria that, one after the other, the analytic philosophers have put forward for almost one hundred years by now to reject, condemn or disparage other styles than theirs of conceiving philosophy. One of the most famous examples of this type of criterion can be found in the theory of verification as applied to meaning. As soon as this theory was on the market, and I seem to remember that it did not hold very long, it gave logical positivist philosophers an easy weapon to stigmatize all those who didn't think like them. How would a Hegel, how would a Heidegger, how would, should I know who else?… could ever verify one single assertion that he makes?

However, the difficulty that we face today, along with the contemporary English representatives of this school, is considerably different in my view. Here, we no longer have persons with a clear and simple formula that they use

to justify their refutation, their condemnation or their mistrust of any philosophical method other than theirs. Thus, in his talk, Professor Ryle wanted to identify the differences that oppose him to Husserl, saying that the latter appealed to essences or to intuitions. But these claims were less clear than they seemed at first glance, in part because he did not elaborate on the reasons of this disagreement, in part because Ryle himself, here and elsewhere, seems to be referring to essences or to an intuition. In part, finally, because, and this third point is crucial for me, he did not clarify what the other alternative would be.

I think that in order to understand what is happening right now one should stick to what Mr. Austin has said. Working in the concrete, piecemeal so to speak, English philosophers have not yet reached the point where they could say – and perhaps, at least this is what I strongly suspect, they are by nature and by vocation little inclined to ever actually reach this point – why, for what reasons summed up in a simple and general expression, they think what they think and condemn or simply avoid every other conception of philosophy.

Moreover, whatever the case may be, the findings that they offer us as fruits of their investigations are often so new and so convincing, so apt at provoking reflection and stimulating further investigations, that we can only welcome their efforts – for which I am, for my part, extremely grateful.

Mr. Perelman

Before giving the word to Mr. Austin I myself would like to ask him, as well as Mr. Ayer, a question, pertaining to the links that they conceive between science, philosophy and the analysis they deal with.

If I have understood correctly, one of the essential criteria for differentiating them is that there is agreement in science, and that there is no such an agreement in philosophy, and that inasmuch as it would be possible, they would like to transform certain domains of philosophy into domains of science, dealing with the problems by means of methods on which an agreement is possible.

I wonder whether, in this matter, they do not feel clearly obliged to limit their conclusions, and to limit them so that they stop being interesting to other philosophers. In the sense that, when one examines linguistic structures, one can be completely in agreement, but from the moment when these linguistic structures must serve as a way of arguing in favor of the existence of certain categories or of certain structures transcending language – and this is certainly Professor Ryle's point of view, since he has told us again and again: "I am not dealing with English or French, or any other languages, but through analysis of language I try to understand and to reveal facts that are not purely linguistic" –, in this case there is already an adventure – called by Whitehead *Adventures of Ideas* – precisely where disagreement becomes possible. I also wonder in this

connection whether Professor Austin and those who are closer to him than to Professor Ryle wish or not to embark on this adventure.

Mr. Devaux
This risk…

Mr. Austin
I shall first echo a comment by Mr. Jean Wahl who is surprised to see us be so close and so distant at the same time. In a certain sense I share his surprise, but on the other hand I wonder why we should be so surprised by the distance that separates us. It so happens that we all are standing on different grounds, and in spite of being so near, our respective grounds have managed to stay different for a very long time, with remarkable ease. I think it is hardly likely that we will succeed in finding a complete rapprochement that soon. Perhaps, with the help of time, we will succeed in gradually coming closer.

I will now turn to Mr. Gewirth, the linesman who has given us his impartial judgment on the game of the two sides. He began by saying that we stigmatize philosophers who do not think the way we do, especially those that come from Europe. I do not think that one can say that we, or at least most of us, are spending most of our time stigmatizing anyone. But I suppose that one can accuse us of the sin of not greeting people on the street. I agree with you that this lack of politeness is worse in a sense than a direct provocation. One can appeal to mitigating circumstances: we are simply too busy. We pride ourselves on having found an entertaining occupation, which we find profitable.LeL Add to this that provoking people under their noses, which would be better after all, I agree with you, than not responding to someone greeting you on the street, puts you at risk of being drawn into a nasty quarrel. Quarrels don't lead anywhere, they make you lose precious time at what could be a decisive moment. Think of the time lost in the 19th century, in futile debates between Darwinians and their opponents!

I will add to my response that is directed to Professor Gewirth, who knows very well what I am talking about, that we ourselves already have our fair share of such quarrels. We have already spent a lot of time quarreling with each other. If we are a movement – or if one can believe that we are a distinct trend in contemporary philosophy – it is indeed because we have come to believe, mistakenly of course, that our most immediate colleagues are the only ones with which it is worth showing ourselves in disagreement.

I would thus say, if one is to chide us for our unusual impertinence, and our way of stigmatizing people without appearing to be doing precisely that: "A thousand apologies, but we're pressed for time and life is so short!"

In response to Professor Alquié, who asks us what we make of history, my response would be in very general terms, both on my own behalf and on behalf of all my colleagues, that we have too much respect for the history of philosophy to wish for it to be neglected. We devote a lot of time to it, sometimes too much time. And I share Professor Ryle's view when he says that there is a danger of devoting too much time to historical studies in philosophy. But let us not give names.

Mr. Alquié wanted to know what we think of Hume and Berkeley. This choice was not made by chance, but I am not sure whether he wanted to know our opinion of these two authors generally speaking, or if his question does not have more to do with the present relationships of analytic philosophy to each of the two. I will attempt to respond briefly to both points.

Of Berkeley and Hume, in a general sense, I would say, for my part, that the former offers us a lot more material for reflection than the latter, at least as regards epistemology, for which Berkeley's work is a valuable lode, not yet completely exhausted today. However, I profess the greatest admiration for Hume's moral philosophy. If, on the other hand, we put the two authors into historical perspective, we cannot deny that Hume undertook an enormous operation of cleansing for his time, that is, a hygienic operation that was then indispensable, as it is still today, for each of us, at one or the other moment in one's life. But I do not believe that one gains a great deal from a meticulous study of his philosophical writings. As a writer he was not capable of writing a prose solid enough to withstand such treatment, whereas Berkeley was, at least to a greater extent and to a degree that was certainly remarkable for his time.

Now, to what extent can one link analytical philosophy with either Hume or Berkeley? I think that both did something, in their time, that was quite similar to certain things that we are doing today. But we can say that the results that they obtained in the areas in which they were engaged are completely obsolete today for the simple reason that they did not have at their disposal either the techniques or the time necessary to complete the studies that they had undertaken, always on very vast problems without going into enough detail nor deeply enough.

To come now to the remarks presented by Professor Galimberti – I hope I will distort neither his name nor his thought – they tend to suggest that there could exist certain proclivities, certain nominalist tendencies in a good part of what is practiced in the schools that are described as analytic. Here I wonder whether I dare speak on behalf of my colleagues, and Professor Ryle in particular, but if someone came up to me and said straight into my face: "Don't you have a little proclivity towards nominalism?" without wanting to attribute to me – not that I think that Galimberti had the intention of so doing – an at-

tachment to a particular doctrine, my God, I believe I could only do one thing, which is, admit my weakness; there is nothing so terrible about that.

Mr. Shalom asked me what we should do with the propositions uttered by classical philosophers. What would one say about them? I do not see how one can respond to such a general question and I suppose that Mr. Shalom expects that I respond to this in equally general terms. There are people that are called philosophers, in the classical sense of the term. They have all put forward a certain number of propositions, belonging to very different types. There are good ones, there are bad ones, of all types and species. What more would you do if, for one or the other reason, you happened to develop a special interest for what a "classical" philosopher had to say, in addition to taking the propositions that seem to you to be the most remarkable and examining each one for its merits? I would simply add this: the fact that any given remark made at a given time in history by this or another philosopher would strike us today as completely erroneous does not prove anything against this other fact, that the same remark, located in its context, could offer us an extremely rich subject for reflections; nor would it also stop us from saying that it was made by a genius, even though, in our eyes, he was wrong. One needs a genius to clash head-on with common sense.

I would respond to Mr. Goldmann who surprised me a bit – just as he took Mr. Ayer by surprise, who will, I hope, have something to say about this – by appearing to be saying, if I understood him correctly, that what we did went against a scientific investigation – let's say psychological, behavioristic if you like – of what happens in the situations in which speech acts take place, examined from their various aspects. Must it be reaffirmed here in the strongest possible terms that I have nothing against this type of studies? I am convinced on the contrary that psychology, along with a number of other scientific disciplines of this type, can discover a great number of things that would be completely missed by linguistic analysis. This is also true of situations in which we find ourselves when we say something. I am thus in favor of this type of research and I can only refer you to an article of mine where I expressed my credo on this, an article that also has a very apt title: "A Plea for Excuses"; since roughly speaking my credo boils down to making excuses for not doing what is certainly not something I intended to do.

I now proceed to the question raised by our president. He asked me, not without reason, if I am not thinking that at the end of our studies, once we will have fulfilled the task we set ourselves by means of the techniques we defined and that one could call, if you like, linguistic philosophy, there would remain, of our own confession, a great number of unsolved problems, as for instance, that of the categories in which one would have to sort out the phenomena

which for us constitute our universe, or even the categories that offer us an exact, correct description of the universe in general; and if the residue of our action wouldn't precisely be discovering these categories that would escape the control of linguistic techniques. Well, I would say, as far as I'm concerned, that I believe that Mr. Perelman is perfectly entitled to say and to think this. I am convinced that if one could press this orange to the last drop there would still remain a good deal to be done. There would still remain, in our holdall, quite a few things left which we wouldn't have touched.

But do take note of this: the question is whether we admit that another philosophical method – I am thinking here perhaps of a very particular one, which is rather fashionable at the moment on the continent – could be the right one? That it could tackle problems that we see as unsolvable and slay them in one go? My answer is no, twice no. First, I do not believe that the time has come – I did not say that it will never come – to address the type of problems you have alluded to. And then I believe *even less* that any of the philosophical methods presently held in favor has the slightest chance of succeeding in coming up against them.

You see, I have so often said that philosophy is the name we give to all the residual problems that still elude the proven methods of science, that I am unable to give this up. When the time comes, if it ever does, to address the problems, which professor Perelman thinks, rightly or in any case with a lot of plausibility, will arise one nice day, then, I wonder again if it will be philosophy that will still be in a position to tell us how we can go about solving them. I rather believe that even in that case we will have to invent our own methods, and when they are perfected, we will discover that they are scientific.

And this brings me to the last question, raised by Mr. Weil, who tells us that our way of conceiving philosophy seems to him the easiest, but that he does not believe it proved for that reason that it is the most fruitful. I do not believe to be mistaken in saying that here Mr. Weil leaves it to be understood that in his view it is not, indeed, the most fruitful one.

Let me say to you first of all that this is certainly the type of question that could have been directed, say, to Descartes, with respect to his geometry, or to Galileo with respect to his physics. I do not say that it was wrong to ask them; they were told: "All of this is nice, really easy. But isn't there a way to proceed much faster and to reach more important results?" And the verdict of history is no, there is no other way.

Furthermore, in what sense are you claiming this is easier? I personally do not find that this is as easy as you claim it is. You need a long time to get familiar with it, a long time to learn to handle these techniques with enough awareness, modesty and, I would add, joy. You need a lot of care. You also need

a sense of team work, which is something that is not given to all philosophers. The only sense in which one could say that these methods are easier is that if one applies them with enough care, one becomes aware that they will provide reasonable assurance against risks of error. I did not say assurance against strokes of genius. But that is a different story.

I would put forth a last argument in favor of this method that you claim is easy, and which seems to be a decisive advantage to me. In general, when someone proposes a new method in philosophy, the first thing that people must do after developing their thesis is try to convince their peers, as it turns out their colleagues in their department. But, you know very well what happens in philosophy: if you manage to convince someone, this person will have all the less faith in you. The big advantage of our method, then, is that it does not force us to convince anyone of its excellence. It is enough for us to say: "Why don't you try…"

Mr. Ayer

I would only add a few supplementary words, to provide nuances on certain points that Mr. Austin has said and to elaborate my own proposal. I am not entirely in agreement with him, most notably in his response to Mr. Alquié's question regarding Hume. I agree with him in thinking that our moral philosophy follows from Hume, but I attach more importance than he seems to do to the theory of knowledge. In any case, I believe that it is clear that Russell's work is very close to Hume's, which explains why those like myself, who continue to follow very closely Russell's work are able to relate more directly and more willingly to Hume's work than those like Austin who have abandoned most of the positions endorsed by Russell.

The second question has to do with language. For Mr. Alquié the issue is knowing if we are speaking about language or if we think that we reach facts through language. If you don't mind, let us take an example: What is understanding? You can frame this question the following way: What do we mean when we say that someone understands something? A first interpretation, both naïve and hasty, of the functioning of language, would be to say that here "understanding" means a mental act, and that every time that someone understands something, just as I hope Mr. Alquié understands me right now, there is something happening in his brain and this something is something exceptional, peculiar. At this moment what we are trying to do is look at the typical cases. Typical cases for which one would say correctly that someone is in the process of understanding something. At this point one realizes that specifically mental processes are not necessary. It is in describing these typical cases where the phrase "understanding something" applies, that one could

succeed in grasping what the word "understanding" means – or if you prefer, what understanding is. This is what I meant by saying that one can express it in two ways. And I think that it will be easier for us to come to an agreement in this regard.

Now, Mr. Goldmann certainly misunderstood me, I probably did not express myself very well or the translation from English to French has betrayed my thoughts. I'll take another example: let us say that we are searching for a thief. Money has disappeared. Several banknotes had been marked and one finds some of the marked bills in a suspect's pocket. And one says: this is evidence, though not conclusive evidence, that he is the pickpocket. So where are the facts of the first order? They are, I would say, the fact that the bills are in his possession, that these bills had disappeared, and that they were marked. Now, one could also say of the fact that the suspect is in possession of the marked bills that it is proof that he had stolen them. This is for us of the type of the second-order facts. Not the fact that he has the bills on him, but the fact that this first fact constitutes evidence for the theft. So, for me philosophy consists in asking oneself, principally or exclusively, what one wants to say in saying that this or that fact constitutes the proof of this or that other one – or to raise questions of the same order, concerning second-order facts. If Mr. Goldmann doesn't like the expression one could easily replace it with another one.

Mr. GOLDMANN

The entire problem is knowing, precisely, whether this science is a science like any other or whether, in your opinion, it has a privileged status.

Mr. AYER

Of course, it has a privileged status. It is a science like others, but it differs, in essential points, which have to be noted, from sciences like physics or chemistry. But if you want by all means to call it scientific, perhaps it is less misleading than to deny it this character.

Finally, with regard to Mr. Perelman's question I believe that Mr. Austin has already said what I could have said. Obviously, we have here methods at our disposal, which, in our view, lend themselves to solving important problems. In particular, the problems that have been raised in the course of the history of philosophy. It seems to me that the classical issues of epistemology, the problem of realism or of idealism, the problem of free will, and so on, can be dealt with – at least we hope so – by our methods, and potentially can be solved by these methods. Those who claim that they have at their disposal other methods, easier or more difficult ones – it is irrelevant, only the efficacy will count – only have to try and use them. To provide a convincing proof of

their superiority, they have to bring problems, real problems that are amenable to solution by means of their methods and not by ours. It is true that one could then debate what constitutes real problems. Professor Quine is next.

Mr. QUINE

I will respond briefly to the remarks that have been addressed especially to me, since my last intervention. Most notably to Mr. Goldmann with respect to the example which he has suggested to decide what seems in his view to be a major difficulty. He asks us to consider the case of the publisher and if, according to us, we have to define the concept of publishing on the basis of what the publisher can say about it, on the basis of language, or more generally, on the basis of the publisher's real situation with regard to the books that he sells. My answer is that we have to take both into account as *descriptions*. Because I remain skeptical regarding the value of the distinctions that one could establish between essential and accidental characteristics – just as I remain skeptical regarding the value of definitions. Perhaps this is related to my profession: when one works too long with logic, one ends up contracting a kind of allergy with regard to oversold clichés, like that of the sense of a definition.

But I would also like to say that I am pleased that I came from so far away to attend this meeting, which has helped us get to know each other better. I am especially grateful to those who have provided multiple signs of friendship towards me.

Mr. AUSTIN

May I add a word? My colleagues have already expressed their joy and gratitude for being brought together. I would like to add the name of Mr. Béra to those whom we have to thank. I have the feeling that he has contributed in large measure to the success of this undertaking and that he had to show a lot of patience over the course of these days, the burden of which he had shouldered.

Mr. PERELMAN

I would like to join these acknowledgements, on behalf of my Belgian colleagues, to thank all of the managing committee and, in particular Mr. Béra for all the care he has shown in organizing this conference. I hope that it will be followed by many more, equally successful ones.

Translated from French by Camilla R. Nielsen

Citation Style

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Substantive notes are numbered, references are parenthetical (in-text, in round brackets), bibliography is compulsory. Compared to Harvad style, *philinq* uses a simplified punctuation and a "European" style with regard to Publisher-City order.

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Books, essays, published/unpublished conference papers, etc... are all referred to through Author and year.

Commas are used to separate different (multiple) authors. Colons are used instead of "p." or "pp.". Semicolons are used to distinguish different works and the original edition from a translation in use (if applicable). Letters are used to distinguish different works written by the same author in the same year.

Single author:

> Sophisticated searching techniques are important in finding information (Rossi 1994).
>
> Rossi (1994) claimed that sophisticated searching...
>
> Rossi states that "sophisticated searching techniques are important in finding information" (1994: 33).

Multiple authors: add *et al.* (always *italicised*)

> Sophisticated searching techniques are important in finding information (Rossi *et al.* 1994).

Translation:

> Rossi states that "sophisticated searching techniques are important in finding information" (1994: 33; Eng. tr. 1998: 28).
>
> *abridged form*
>
> Rossi states that "sophisticated searching techniques are important in finding information" (1994: Eng. tr. 28).

Multiple works:

> Sophisticated searching techniques are important in finding information (Rossi 1994; 1996).
>
> Sophisticated searching techniques are important in finding information (Rossi 1994; Bianchi 1999).
>
> Sophisticated searching techniques are important in finding information (Rossi 1994a; 1994b).

Avoid redundancies:

> Rossi has been working on this subject for almost a decade. In his essay on searching techniques (1994), he states that they are "extremely important in finding information" (33).
>
> Rossi has been working on this subject for almost a decade. In his essay on searching techniques (1994), he states that they are "extremely important in finding information" (Eng. tr.: 28).

Omonimies: use initial(s)

Sophisticated searching techniques are important in finding information (Rossi G. 1994).

Edited books

In their work on searching techniques (Rossi 1994), the Italian team on Information Engineering...

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References are ordered alphabetically by last name. First personal names ought to be in extended form. Multiple works from the same author will be ordered by year, most recent to oldest.

Elements of the citation

- Books

  Author of book – last name, first name abridged, Year of publication, ed(s). if applicable, *Title of book – italicised*, Translator if applicable, Edition if applicable, Publisher, Place of publication.

  Rossi G., 1994, ed., *Information Engineering*, ETS, Pisa.

MULTIPLE AUTHORS (UP TO THREE)

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Rossi G., Bianchi F., Verde G., *Information Engineering*, ETS, Pisa.

MULTIPLE AUTHORS (MORE THAN THREE): YOU CAN USE ET AL.

Rossi G. *et al.*, *Information Engineering*, ETS, Pisa.

TRANSLATIONS (WHEN QUOTED)

Ricœur, P., 1983, *Temps et récit*, Seuil, Paris; Eng. tr. by D. Pellauer 1990, *Time and Narrative*, University of Chicago Press, Chicago.

PLEASE NOTE THAT ABBREVIATIONS FOR LANGUAGES KEEP THEIR CAPITAL LETTER (Eng., Fr., Ger., It., etc...).

- Chapter(s) in a book

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  Bhabha H.K., 1985, "Signs Taken for Wonders: Questions of Ambivalence and Authority under a Tree Outside Delhi, May 1817", in *Critical Inquiry*, 12, 1: 144-165.

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  - ...*Critical Inquiry*, 12: s1-s18.
  - ...*Critical Inquiry*, 12, 1 bis: 1-18.
  - ...*Critical Inquiry*, 12, 1, Spanish edition: 1-18.

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Over the last thirty years historical attention has been directed toward analytic philosophy. Yet this historiographical perspective mainly focuses on the origins of analytic philosophy or on the central decades of the 20th century. By contrast, a proper historical investigation of *late* analytic philosophy – analytic philosophy in the last forty years – is greatly needed. This special issue of *Philosophical Inquiries* is intended to be a stimulus for such an investigation.

The issue includes a series of interviews with contemporary philosophers – Thomas Baldwin, Michael Beaney, Cora Diamond, Han-Johann Glock, Matthew Haug, Cheryl Misak, Philip Pettit, Nicholas Rescher, John Skorupski, Brian Weatherson, Timothy Williamson, Jonathan Wolff – based on a fixed set of questions:

(1) What are the main philosophical and metaphilosophical similarities and differences between early analytic philosophy and late analytic philosophy?
(2) Is it possible to identify a mainstream in late analytic philosophy? If so, what are its main (cultural, ideological, philosophical, methodological, metaphilosophical) features?
(3) What are the main critical and controversial aspects of late analytic philosophy?

In addition to five articles, which investigate the topic under discussion from widely different points of view, the issue also includes the first English translation of the "Discussion générale" of the fourth Colloque philosophique de Royaumont (1958), accompanied by an introduction by Mathieu Marion.

Guido Bonino teaches history of philosophy at the University of Turin. He is the author of *The Arrow and the Point. Russell and Wittgenstein's Tractatus* (Ontos, 2008) and *Anatomia del realismo. Saggio su Gustav Bergmann* (Il Mulino, 2009). His main interests are the history of analytic philosophy, British Idealism and analytic ontology.

Paolo Tripodi teaches history of philosophy at the University of Turin. He is the author of *Dimenticare Wittgenstein. Una vicenda della filosofia analitica* (Il Mulino, 2009) and *Storia della filosofia analitica. Dalle origini ai giorni nostri* (Carocci, 2015); a book on *Analytic Philosophy and the Later Wittgensteinian Tradition* is forthcoming for Palgrave Macmillan. His main interests are the history of analytic philosophy, the later Wittgenstein and Wilfrid Sellars.

Currently, both of them are trying to apply distant reading techniques to the history of philosophy.