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

Frederique Janssen-Lauret and 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-

ISSN (print) 2281-8618-ETS

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: 134-5). 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: 298-9). 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.

Acknowledgements. We are grateful to Stephanie Lewis for permission to access and quote from Lewis's correspondence and unpublished papers. We also are grateful to Stephen Barnett, Sean Crawford, Chris Daly, Jane Heal, David Liggins, Kevin Mulligan, Francesco Orilia, Jeffrey Paris, Paul Teller, Thomas Uebel, Ann Whittle, and audiences at the European Congress of Analytic Philosophy held in Munich, and departmental seminars at the University of Macerata and the University of Manchester. This research was supported by the AHRC project grant 'The Age of Metaphysical Revolution: David Lewis and His Place in the History of Analytic Philosophy'.

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

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

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