Philosophical Scepticism

HELEN BEEBEE
THE 110TH PRESIDENTIAL ADDRESS

PHILOSOPHICAL SCEPTICISM

HELEN BEEBEE
UNIVERSITY OF MANCHESTER

MONDAY, 2 OCTOBER 2017
17.30 - 19.15

THE CHANCELLOR’S HALL
SENATE HOUSE
UNIVERSITY OF LONDON
MALET STREET
LONDON WC1E 7HU
UNITED KINGDOM

This event is catered, free of charge & open to the general public

CONTACT
mail@aristoteliansociety.org.uk
www.aristoteliansociety.org.uk

© 2017 THE ARISTOTELIAN SOCIETY
BIOGRAPHY

Helen Beebee is Samuel Hall Professor of Philosophy at the University of Manchester. She completed her PhD at King’s College London in 1996, and has previously held positions at Edinburgh, St. Andrews, UCL, The Australian National University, and Birmingham. Her research falls mostly within metaphysics, focussing primarily on causality, laws of nature, and freedom of the will. Her publications include a monograph on Hume (Hume on Causation, Routledge 2006) and a textbook, Free Will (Palgrave, 2013), and she is currently Principal Investigator for an AHRC project, ‘The Age of Metaphysical Revolution’, which focusses on the work and correspondence of David Lewis in the context of his role in the history of twentieth-century analytic philosophy. She has served as President of the British Society for the Philosophy of Science (2015-17), Director of the British Philosophical Association (2007-11), a member of the AHRC Advisory Board (2008-13), and a co-editor of Philosophy and Phenomenological Research (current), as well as a member of various executive committees of learned societies and journal editorial boards. She is a Patron of the Athena SWAN Charter.

This year’s Presidential Address marks the official inauguration of Helen Beebee as the 110th President of the Aristotelian Society.

EDITORIAL NOTE

The following paper is a draft version that can only be cited or quoted with the author’s permission. The final paper will be published in Proceedings of the Aristotelian Society, Issue No. 1, Volume CXVIII (2018). Please visit the Society’s website for subscription information: aristoteliansociety.org.uk.
I. PHILOSOPHICAL SCEPTICISM

IN RECENT YEARS a lot of attention has been paid to what I'll call the problem of philosophical scepticism. Philosophical scepticism is scepticism about philosophy: the claim is that philosophers (or, sometimes, just metaphysicians) don’t know, or perhaps do not so much as have warrant for, many of the substantive philosophical claims that they make. Philosophical scepticism contrasts with what I’ll call ‘garden-variety’ scepticism: the kind of scepticism that claims to cast doubt on more mundane beliefs, such as the belief that the external world exists, the belief that other minds exist, or the belief that the future will resemble the past.

A surprisingly large number of philosophers have recently endorsed, or at least come pretty close to endorsing, philosophical scepticism. Thus, for example, we have Hilary Kornblith: ‘It is just not clear that the philosophical enterprise has served as a source of knowledge’ (2013, 260), and William Lycan, who says that we have ‘only dribs and drabs’ of philosophical knowledge, here and there’ – ‘nothing to write a song about’ (2013, 120).

The challenge of philosophical scepticism has a variety of sources, of which I shall discuss two: what I’ll call the methodology challenge and the disagreement challenge. According to the methodology challenge – often aimed specifically at metaphysics – our methods can stake no claim to truth-conduciveness; thus we have no particular reason to think that the theories that those methods deliver are true (see e.g. Ross, Ladyman and Spurrett 2007, Ch.1; Ney 2012; Kriegel 2013). The disagreement challenge takes as its starting point the fact of what Sandford Golderg (2013) calls ‘systematic peer disagreement’. Pretty much whatever your philosophical view, you’ll find plenty of other philosophers who are just as good at philosophy and just as well informed as you who sincerely avow a view that’s incompatible with your own. And this – so the argument goes – undermines our epistemic entitlement to our own view.

The problem of philosophical scepticism is a problem because we like to think that we do have knowledge of substantive philosophical claims: knowledge not just of what follows from what, say, or of which philosophical theses are compatible or incompatible with other theses or with common sense or whatever, but theses about (for example) the
nature of reality. Moreover, philosophers tend to take for granted that
the aim of philosophy (or one of them) is also knowledge; so even if we,
individually, don’t know very much, we can take comfort in the thought
that, somehow or other, we are collectively making progress towards that
goal. Philosophical scepticism also puts pressure on conceiving of the aim
of philosophy as knowledge. If we have no reason to think that our methods
are truth-conducive, then it’s hard to see how we can be making progress
towards that goal. And if systematic peer disagreement undermines
knowledge, only the optimist who thinks that such disagreement might
eventually – and by rational means – disappear can hang on to the idea
that one day, in principle, knowledge might be achieved. And it’s unclear
what might warrant such optimism, given philosophy’s long history of
failing to resolve major points of substantive disagreement.

In fact, some aspects of contemporary philosophy are hard to square
with the claim that it is knowledge that we seek. Take David Lewis’s thesis
of Humean supervenience. When he introduces the thesis (1986, ix-xi), he
offers no argument for it. He doesn’t even attempt any informal motivation
for it, unless you can call a nod in Hume’s direction motivation. He himself
doesn’t even appear to be very interested in whether it’s true: ‘What I
want to fight’, he says, ‘are philosophical arguments against Humean
supervenience’ (1986, xi). And yet, despite the fact that it appeared on the
scene as – and to a great extent remains – a mere speculative hypothesis,
Humean supervenience has been the subject of an enormous amount of
philosophical attention over the last 30 years. That’s hard to explain, if
what we really care about is the pursuit of knowledge. Why did we not,
collectively, simply ignore it, pending a passable argument in its favour?

Even in methodological mode, our own accounts of what we are
doing sometimes sit uneasily with a self-conception as knowledge-seekers.
Consider the way in which the theoretical virtues are normally discussed.
Generally speaking, the claim is made that some virtue such as ideological
or ontological parsimony or simplicity is a ground for theory choice or
preference. It is very rarely claimed explicitly that they are grounds
for thinking the theory in question true – that is, that theory choice or
preference itself is itself a matter of choosing the theory that is most
likely to be the true one. When that claim is explicitly interrogated, the
conclusion reached is normally a negative one (see e.g. Huemer 2009,

Of course, philosophers are not unaware of the problem of connecting
the theoretical virtues with truth-conduciveness. But, given the prominence
of the theoretical virtues in discussions in contemporary ontology in
particular, if ontologists really think what they’re up to is figuring out the
truth then it is surprising, to say the least, that they spend so much time avoiding – to use David Armstrong’s expression – a compulsory question on the examination paper.

I consider the methodology challenge in §2 and the disagreement challenge in §3. The upshot is that we know, both individually and – more importantly – collectively – very little by way of substantive philosophical theses. Moreover, the lesson of the methodology challenge is that very many of the systematic peer disagreements that undermine our current claims to know substantive theses are unlikely to be tractable. Hence if knowledge is the aim of philosophy, then we should be sceptical of the possibility, even in principle, of ever reaching it. In §4 I sketch a position, which I call ‘equilibrism’, which, drawing on some well-known remarks of David Lewis’s, proposes a more modest aim for philosophical enquiry: to find out ‘what equilibria there are that can withstand examination’. In §5, I address what I call the ‘commitment problem’: the problem of giving an account of the kind of attitude we need to have towards substantive philosophical theses if we are to engage in the practice of philosophy, given that in many cases we lack warrant for believing those theses.

2. THE METHODOLOGY CHALLENGE

Start from the assumption that metaphysics is to be pursued in a broadly naturalistic vein (in some sense of ‘naturalistic’): that is, the methods it deploys are not substantially different from those deployed in the sciences. We construct theories on the basis of our evidence, and we choose between rival theories on the basis of the theoretical virtues: explanatory power, simplicity, parsimony and the like. (This, I would say, is the dominant conception of the method of metaphysics – and certainly of ontology – in contemporary philosophy.) But now, the worry goes, how does metaphysics fare in comparison to the sciences? Answer: spectacularly badly. Our evidence base is woefully bereft, consisting largely as it does of intuitions about whose alleged connection with the facts of the matter we lack any good story; and we have no grounds for thinking that, as deployed in metaphysics, maximisation of the theoretical virtues is truth-conducive.

Responses to the methodology challenge have largely focussed on the status of intuitions, and in particular those intuitions that are elicited by thought experiments. I shall briefly consider a particular response to the challenge, namely that of Timothy Williamson (2005), who argues that the ‘intuitions’ typically thought to be evoked by thought experiments are merely the upshot of our common-or-garden counterfactual thinking – something we are, in general, pretty good at. Hence there is no special
problem of accounting for the provenance or veracity of our intuitions.¹

Well, perhaps Williamson is right. But how far does that get us, really? Even if we have on the table a plausible story about how we come by the judgements elicited by thought experiments, that story does not help us at all in the very many cases where we disagree with each other: plenty of disputes remain. You might think that Designed Ernie in Alfred Mele’s ‘zygote argument’ is not morally responsible, and conclude that non-historicist compatibilism is false (Mele 2006, 189). Or you might not (Fischer 2011, 271). You might come up with what you take to be a cast-iron case of the failure of the transitivity of causation (McDermott 1995, 524). Someone else will inevitably disagree (Lewis 2000, 194). You might think you have described two different possible worlds that agree with respect to the distribution of matters of particular fact but disagree with respect to the laws, thus refuting the claim that the latter supervene on the former (Carroll 1994, 57-68). Or you might not (Beebee 2000, 586-92). And so on.²

In the case of run-of-the-mill counterfactuals that we deploy outside of philosophy, disagreements can often be resolved. I say that if your alarm clock hadn’t gone off (as it did at 7.45), you would have missed the bus. You point out that in fact you woke up before the alarm went off anyway. I revise my judgement. Or you point out that, while you were indeed woken up by the alarm clock, you never sleep past 8 o’clock so you would have woken up at most 15 minutes later, and you waited at the bus stop for 20 minutes, so – again – you would have caught it anyway. Again, I revise my judgement in the light of this new information. And so on.

We have no such method for resolving metaphysicians’ clashes of intuitions. When I say that Designed Ernie is morally responsible for his crime or that in our transitivity case $x$ really was a cause of $z$, and you disagree, we have no agreed way of reconciling our differences. There are no empirical facts about which you might correct me, in the light of which I would recognise my mistake and change my mind. I have not illegitimately held fixed some facts that I should not have held fixed, or

¹ Williamson generally eschews using the term ‘intuition’ for the judgments elicited by thought experiments, noting that in general the ‘main current function’ of the term ‘intuition’ and its cognates ‘is not to answer questions about the nature of the evidence on offer but to fudge them, by appearing to provide answers without really doing so’ (2007, 220). I here persist in using the term merely for the sake of expediency and not to signal any disagreement with him in that regard.

² Williamson says: ‘Levels of disagreement over thought experiments seem to be significantly lower among fully trained philosophers than among novices’ (2007, 191). I can only report – from the corners of philosophy where I spend my time – that they do not seem so to me.
vice versa – not by my lights, anyway. We might try to convince each other otherwise, of course, and sometimes one of us succeeds; but often we both fail. Often we both fail simply because each of us is holding fixed some element of our own background philosophical theory that the other rejects. But then our thought experiment serves only to provide an example of the different consequences of our respective theories; it cannot adjudicate between them. So even if Williamson is right, the ‘intuitions’ that are thus vindicated are a relatively circumscribed set – viz, the ones that philosophers by and large agree about.

Moreover, in many cases philosophers are prepared to concede the counter-intuitiveness of some aspect of their view but persist in holding the view because it has other merits. In Williamsonian terms, such approaches might seem to amount to accepting the truth of some counterfactual (i.e. some thought experiment rendered in counterfactual mode) while simultaneously refusing to modify one’s view accordingly – which would clearly be irrational. But in this kind of context counter-intuitiveness is normally conceived in terms of conflict with common-sense theory; correspondingly, one might reasonably respond that the alleged truth of the relevant counterfactual turns on the assumption that common-sense theory is true, and we have no general grounds for thinking for that that theory is true.

What about the theoretical virtues? Responses to this aspect of the methodology challenge have been rather thin on the ground. My sense of the matter is that L. A. Paul’s claim that ‘if simplicity and other theoretical desiderata are truth conducive in scientific theorizing, they are truth conducive in metaphysical theorizing’ (2012, 22) operates as the default position amongst metaphysicians. But that claim is contestable, and indeed contested. First, as is well known, the claim that the theoretical virtues are truth-conducive in the sciences has been contested by Bas van Fraassen (1980). So far as I am aware, that claim has not been refuted. Indeed, as James Ladyman notes, ‘it is widely acknowledged that it is difficult to argue that simplicity or elegance are direct evidence for the truth of a theory so [scientific] realists have tended towards the view that all the superempirical virtues are subsumed under explanatory power so that the solution to the underdetermination problem is IBE [inference to the best explanation]’ (Ladyman 2012, 42).

So suppose we grant (as of course van Fraassen does not) that IBE is indeed a truth-preserving inference in the sciences. May we infer that the same is true in metaphysics? Ladyman claims that the role of IBE as a means of theory-choice in science has been overstated: even in oft-cited

---

3 An exception is Cowling 2013.
alleged cases of IBE, such as evolutionary theory and the choice of General and Special Relativity over their rivals, ‘[e]xplanatory power plays the role that it does in [scientific] theory choice because of the relationship between theoretical explanation and the empirical virtues of scientific theories. We have inductive grounds for believing that pursuing simplicity and explanatory power in science will lead to empirical success, but no such grounds where we are dealing with distinctively metaphysical explanations, since the latter is completely decoupled from empirical success’ (2012, 46). In other words, to the extent that – and only to the extent that – the best explanation in science has turned out to be predictively superior to its rivals, we have inductive grounds in the scientific case for thinking that the best explanation is true. But no such inductive grounds exist in the case of metaphysics.

Finally, what if we set all this aside and granted that the theoretical virtues are, individually, truth-conducive – or, roughly equivalently, that IBE is a truth-preserving inference in metaphysics? Would we then have a method for deciding between rival metaphysical theories? Well, we would if we had some agreed way of trading the virtues off against one another (correspondingly: a method for determining which of various rival explanations that make different trades is the best). But of course we don’t – and plenty of disputes in metaphysics arise from differences in our trade-offs. Paul herself says: ‘While choosing one theory over its competitors based on a particular balancing of theoretical virtues is perfectly rational, we will almost always have a range of alternative theories to choose from: this is because there are many different ways of balancing respective theoretical virtues and hence many different ways of arriving at reflective equilibrium’ (2012, 22). But the sceptical worry here is precisely how it can be rational to believe one theory rather than another when we have no grounds for thinking that our particular balancing act is more likely to deliver the truth than those of our competitors.

Consider Argle and Bargle’s dispute about the nature of holes (Lewis & Lewis 1970). At the end of their discussion about whether or not holes are material objects, Bargle (who thinks that holes are not material objects) accuses Argle (who thinks they are) of violating common sense in his description of a kitchen-roll tube that has a small hole in its side: ‘you must pay a great price in the plausibility of your theories’. The conversation continues:

Argle: Agreed. We have been measuring the price. I have shown that it is not so great as you thought; I am prepared to pay it. My theories can earn credence by their clarity and economy; and if they disagree a little with common opinion, then common opinion may be corrected even by a philosopher.
Bargle: The price is still too high.

Argle: We agree in principle; we are 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 ... of the nature of our customary disputes. (Lewis & Lewis 1970, 211-12.).

Argle and Bargle have reached stalemate in their cost-benefit analysis of the claim that holes are material objects. They agree on what the costs and benefits of the view are: on the benefit side we have ‘clarity and economy’, and on the cost side we have dissonance with common sense. But they disagree over what the costs and benefits are worth: they weigh them up and get different results. So it is unclear why Argle is entitled to hold that his theory ‘earns credence’ by its clarity and economy despite the cost, when he has no response to Bargle’s claim that it doesn’t.

The methodology challenge, then, is that we have no grounds for trusting our intuitions – perhaps at all, or, conceding the point to Williamson, when they conflict with those of our philosophical peers. And we have no grounds for thinking that the theoretical virtues are truth-conducive; indeed, even if we grant that they are, we have no agreed methodological standard for trading them off against one another, and hence, where such trade-offs are what divide us, we have no more reason to believe our own theory than we do a theory whose (alleged) justification comes from making the trade differently.

From here on, I shall shift my attention from metaphysics in particular to philosophy in general, and will assume without argument that a version of the methodology challenge applies to other areas of philosophy as well. That is, admittedly, a big assumption, since the kind of ‘naturalistic’ approach at which the methodology challenge is aimed is largely (though not entirely) confined to contemporary metaphysics – and not even to the whole of metaphysics, given that not all metaphysicians sign up to this way of proceeding. On the other hand, a version of the Argle’s and Bargle’s situation is pretty commonplace even outside metaphysics. Simpler, more elegant theories – whether in ethics or aesthetics or philosophy of language or wherever – have an annoying tendency to throw up some counter-intuitive consequences, thus generating disputes about whether the solution to that problem is to add epicycles to the theory in order to save the intuitions or rather to attempt to explain away, or bite the bullet with respect to, the recalcitrant intuitions. More generally, at least in many areas of philosophy it is hard to see how other conceptions of appropriate philosophical methods are going to be any less susceptible to worries about the truth-conduciveness of those methods.
3. THE DISAGREEMENT CHALLENGE

The disagreement challenge starts from the obvious and undeniable fact of pervasive systematic peer disagreement. In §2, I briefly considered two sources of such disagreement: divergent intuitions with respect to thought experiments and differences in how we are to trade costs against benefits. How – so the challenge goes – can we claim to know or be justified in believing our philosophical views, when we know that equally capable and well-informed philosophers disagree with us – and often continue to do so despite our best efforts to persuade them of the error of their ways?

One kind of response to the disagreement challenge is to try to explain how a given philosopher $S$ can be legitimately claim to know or be justified in believing some proposition $p$ even in the face of persistent disagreement from $S$’s peers about whether or not $p$. Broadly speaking, such responses try to argue that whatever evidence that the belief in $\neg p$ by one’s philosophical peers might in principle provide for the falsity of $p$, that evidence can, at least in some contexts or circumstances, legitimately be ignored.

Thus for example David Lewis, in a letter to Richard Cartwright (1989), says:

Subject to a qualification, I also say that the one who’s right in a philosophers’ impasse typically knows that he’s right …

The qualification is that the knowledge may temporarily vanish in the course of philosophical discussion because ‘alternatives’ are made ‘relevant’ that normally are not … [Lewis refers to his 1979, 355 here; see also Lewis 1996]. Maybe I cannot truly say, in the midst of a discussion of far-fetched sceptical hypothesis, that I know I have hands. Attending to these hypotheses temporarily raises the standards for what may be called knowledge. But I can truly say, even then, that under more ordinary circumstances I can truly say that I know I have hands.

The context of the discussion is Graham Priest’s view that contradictions can be true; Lewis, of course, is firmly of the view that they cannot. And he says that he knows that he is right (and that his view is reasonable) and Priest is wrong (and that his view is unreasonable), even though there is no serious possibility of establishing this to Priest’s satisfaction – it is a genuine ‘philosophers’ impasse’ – since part of what is at issue between them is what counts as ‘reasonable’. So Lewis is not merely interested here with sceptical scenarios – evil demons and the like. Rather, he is claiming that his contextualist account of knowledge applies just as well to philosophers’ impasses, where the alternative hypothesis is something that is actually believed, and by their own standards reasonably so, by actual philosophers as is does to mere sceptical scenarios.
Another example: in his book *The Strife of Systems* – about which I shall say a bit more later – Nicholas Rescher argues that persistent disagreement is an inevitable feature of philosophy because philosophers inevitably have different cognitive values – and even when they share the same cognitive values, they won’t all get weighed against each other in the same way. Nonetheless, he holds that one can perfectly well be said to *know* philosophical theses (1985, 208); ‘from the vantage point of one’s own cognitive-value perspective – the only one that one has – only one optimally adequate position on philosophical issues is rationally warranted’ (1985, 265). Again, then, philosophical knowledge is entirely possible: one can know *p* by one’s own lights, while fully appreciating that by different lights *p* does not constitute knowledge (and indeed is not even rationally warranted).

Suppose for now that some such response is viable. What these kinds of response to the sceptical problem have in common is that the challenge they are attempting to meet is that of vindicating the knowledge claims of individual philosophers. But there is a sense in which those responses fail to address a broader problem. We often take the bearer of knowledge to be not an isolated individual but a community of some sort. When, at the point in the TV crime drama where the detective gathers her team in the incident room and asks, ‘what do we know?’, she is asking for a pooling of resources: her aim is to compile a list of (purported) facts about the suspect that can be used as a basis for further investigation. That list will generally consist of individuals’ items of knowledge, of course, but it would be of no help at all to the investigation if it were to turn out that different team members’ items of knowledge only *counted* as knowledge in the absence of salient alternatives. Were PC Smith to convey her (purported) knowledge – based on excellent evidence – by saying ‘the suspect lives with his mother’, and PC Jones to convey her (purported) knowledge – equally well grounded in a different set of evidence – by saying ‘the suspect shares a flat with his sister’, the detective would quite rightly conclude that the team collectively does not know what the suspect’s living arrangements are. Similarly, when we ask ‘what do we know about climate change?’, or ‘how much do we know about the causes of rheumatoid arthritis?’, or whatever, any coherent answer to our question will be a set of purported facts whose members are consistent with one another. No proposition *p* that is a matter of persistent peer disagreement can make it onto the list, since there will be no grounds for having *p* rather than ~*p* on the list.

What would happen if the philosophy detective were to gather the philosophy team – the epistemic community of philosophers – in the incident room and ask the question, ‘what do we know?’? Well, the list of purported facts – at least when it comes to substantive philosophical
theses – is likely to be rather short. And that is not because we contain within our midsts garden-variety sceptics intent on raising their sceptical hypotheses, thereby challenging our claims to know that the external world exists, or that there are other minds, or whatever. Perhaps there are such sceptics lurking amongst us; even so, we can, I think, banish them from the philosophy incident room, just as our police detective will ban the police sceptic from hers. In the context of aiming to solve a crime, it is not helpful to pay attention to those who point out that all of our evidence is consistent with some contrived scenario, cooked up solely for the purposes of undermining the crime team’s claims to know anything about the perpetrator. The situation in the philosophy incident room still leaves us with little by way of collective knowledge of substantive philosophical theses, even with the garden-variety sceptic banished. The ‘sceptical scenarios’ brought to the room by those with whom we systematically disagree are not at all like those of the garden-variety sceptic – put forward with the sole aim of challenging our claims to know. They are consequences of well worked-out philosophical theories that, at least in many cases, are the results of different choices of starting assumptions or methodological principles.

When aimed at the claim that we collectively know a lot of substantive philosophical theses, then, the problem of systematic disagreement is not met by appealing to the idea that we can, in the face of such disagreement salvage our individual claims to know such theses. But I suggest that the difference between garden-variety sceptical scenarios and the kinds of ‘sceptical scenario’ which constitute rival substantive philosophical theses that are subject to systematic peer disagreement serves to put pressure on even our individual claims to know any such theses. Consider the stalemate that is the outcome of Argle and Bargle’s dispute about the nature of holes again. According to Lewis’s contextualist solution to the problem of philosophical scepticism, insofar as Argle can claim to know that holes are material objects, Bargle’s contention that they are not constitutes a sceptical challenge: a relevant alternative to Argle’s view that, once made salient, Argle cannot rule out. Conversely, Argle’s contention that holes are material objects constitutes a sceptical challenge to Bargle’s view that they aren’t.

Collectively Argle and Bargle do not know whether holes are material objects are not. But do they even individually know – when not discussing holes with one another – whether or not holes are material objects? They are members of the same epistemic community, on any reasonable conception of ‘epistemic community’. (They might be the best of friends, or inhabitants of adjacent offices. They might even be married to each other.) Argle knows – not just while discussing holes with Bargle, but all
the time – that Bargle can, by her own lights, legitimately claim to know the denial of the thesis that Argle claims to know – and similarly for Bargle. That is very different to knowing that there are garden-variety sceptical scenarios that one’s evidence does not rule out. It therefore strikes me as decidedly odd to claim that Argle can legitimately claim to know, when Bargle is not around (or not talking about holes anyway), that holes are material objects. Claiming to know that the animal in front of me at the zoo is a zebra, while yet not being able to rule out the hypothesis that it is a mule painted to look like a zebra, is one thing. Claiming to know that it’s a zebra while knowing that some of one’s epistemic peers legitimately – by their own lights – claim to know that it is a painted mule is another thing entirely.

Moreover, once Argle and Bargle agree, as they should, that they don’t collectively know whether or not holes are material objects, it is unclear why Argle should care whether or not he knows (when holes are not under discussion) that holes are material objects. Argle has a well-worked-out view on the matter. It coheres, we may suppose, with his wider body of philosophical views. He has measured the price of his view to his own satisfaction, and found the price to be worth paying. The same goes for Bargle. Why shouldn’t that be enough for each of them?

The thought here is that if (and, I grant, it’s a big if) our individual claims to knowledge do not contribute to our collective knowledge – which in cases of systematic peer disagreement they don’t – it is not clear why our entitlement to individual knowledge claims is something we should care about. What purpose does it serve, exactly? I suggest that the only thing we really need to care about is our ability to sincerely take a view. Proper engagement in the business of philosophy does, I think, require that. But perhaps we can make sense of sincerely taking a view without conceiving the view in question as an item of knowledge or justified belief. Perhaps we do not even need to conceive it as a matter of belief. I’ll return to this suggestion in §5.

First, however, let’s return briefly to an issue raised in §1: what is the aim of philosophy? As I said, we might like to – and many philosophers do – think that philosophy aims at knowledge. That, surely, is a collective and not an individual aim: it is the aim of philosophy qua intellectual endeavour, pursued by a broad epistemic community that spans not only the globe but several millennia. This being so, even if we could salvage the claim that substantive philosophical theses can be known even in cases of systematic peer disagreement, that would not help us at all in conceiving ourselves as making our own individual contribution to that goal. Again, if I can reasonably claim to know that \( p \) (perhaps only when
you, a naysayer, are safely out of the way) and you can equally reasonably claim to know that not-\( p \) (perhaps only when I am safely out of the way), collectively we know nothing about whether or not \( p \), and are therefore not contributing at all to our collective aim.

If systematic peer disagreements were generally tractable, then perhaps this would not matter much. Perhaps you and I are contributing to the collective aim of knowledge not by contributing knowledge of our own, but by playing a necessary part in a dispute – perhaps a protracted one that has already been going on in one form or another for centuries – that will, eventually, be resolved. The methodology challenge, however, suggests that this would be wishful thinking. If we had grounds for thinking that our methods are truth-conducive, we would no doubt have grounds for thinking that all systematic peer disagreements will, eventually, get resolved, even if the resolution is years or even centuries away – or at least would get resolved given sufficient work. (We can all agree that philosophy is hard.) But we do not have such grounds. So if philosophy’s aim is knowledge, then our methods are not fit for purpose. Nor do we have the least idea how we might change or improve our methods in such a way as to deliver reasonable confidence that collective knowledge is after all attainable.

4. EQUILIBRISM

What to do, then, if we collectively do not know very much at all when it comes to substantive philosophical theses, and that there are no serious prospects of realising the aim of collective knowledge of such theses even in the long run? Well, we could aim for something else instead: something achievable in principle, and – ideally – something that does not require us to change the way we go about our business in order to make progress with respect to that aim.

I want to propose an aim that fits the bill by drawing on some well-known remarks of Lewis from the Introduction to Volume I of his *Philosophical Papers*:

The reader in search of knock-down arguments in favor of my theories will go away disappointed. Whether or not it would be nice to knock disagreeing philosophers down by sheer force of argument, it cannot be done. 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. But when all is said and done, and all the tricky arguments and distinctions and counterexamples have
been discovered, presumably we will still face the question which prices are worth paying, which theories are on balance credible, which are the unacceptably counterintuitive consequences and which are the acceptably counterintuitive ones. On this question we may still differ. And if all is indeed said and done, there will be no hope of discovering still further arguments to settle our differences.

It might be otherwise if, as some philosophers seem to think, we had a sharp line between ‘linguistic intuition’, which must be taken as unchallengeable evidence, and philosophical theory, which must at all costs fit this evidence. If that were so, conclusive refutations would be dismayingly abundant. But, whatever may be said for foundationalism in other subjects, this foundationalist theory of philosophical knowledge seems ill-founded in the extreme. Our ‘intuitions’ are simply opinions; our philosophical theories are the same. Some are commonsensical, some are sophisticated; some are particular, some general; some are more firmly held, some less. But they are all opinions, and a reasonable goal for a philosopher is to bring them into equilibrium. Our common task is to find out what equilibria there are that can withstand examination, but it remains for each of us to come to rest at one or another of them. (1983, x)

I propose that we conceive what Lewis sees as our common task as our aim: that is, our collective aim is to ‘find out what equilibria there are that can withstand examination’. Intractable disagreements, on this picture, make for different equilibria: Argle and Bargle agree on a great many things, we may suppose, and to that extent their respective equilibria will overlap. But they disagree about the nature of holes – and, we can assume, on other issues besides, where their different cost-benefit trade-offs deliver different views – and those differences fall outside the intersection of their respective equilibria.

With our aim thus conceived, philosophical scepticism, conceived as a threat to our current state of collective knowledge of substantive philosophical theses – and indeed as a threat to our ability to achieve such knowledge in the long term – turns out not to pose a threat at all. That we bring to the table different sets of substantive assumptions and different methodological principles – the kinds of differences that generate intractable disagreements – is no threat to our ability to contribute to the aim of philosophy. On the contrary: such differences are precisely what generate the plurality of equilibria that it is our collective aim to uncover. And if our individual aim is to find an equilibrium position of our own – or at any rate a partial one, given the vastness of the task – then it no longer really matters if our substantive philosophical views are unjustified. Argle need not hide under his desk when Bargle is around, fearing that Bargle’s insistence on reminding him of his view of holes will temporarily
rob him of his knowledge that holes are material objects; insofar as Argle and Bargle are convinced that their argument has run its course and each is occupying a stable position, neither poses any kind of epistemic threat to the other.

I’ll call the combination of this broadly Lewisian conception of the aims of philosophy and philosophical scepticism – understood as the thesis that collective knowledge of at least very many substantive philosophical theses is impossible – equilibrism. Equilibrism is, I hope, a relatively conservative position with respect to the practice of first-order philosophy. I take it that at least a very large part of what most individual philosophers do just is a matter of trying to find a (partial) point of equilibrium, at which one is happy to – as Lewis puts it – come to rest. (Whether or not that is what they think of themselves as doing is another question, of course; I don’t expect equilibrism to be conservative with respect to that.) In particular, equilibrism does not in the least undermine the philosopher’s stock-in-trade of argument and counter-argument (see §5 below). Mostly, then, equilibrism merely recommends an alternative account of why it is we’re doing all of this: of what purpose we are serving in going about our business in the way that we generally do.

It’s important to stress that even the aim of bringing all of one’s own opinions into equilibrium is an exceptionally tall order – one that surpasses the lifespan and cognitive powers of any actual philosopher. For one thing, our own positions are only ever partial; they never span the whole of philosophical space. You might have your view in some area of metaphysics nicely buttoned down, or so it seems – but there is always the possibility (indeed, likelihood) that when you attempt to bring your guiding assumptions and methodological principles to bear on some new area – the philosophy of language, say, or meta-ethics or the philosophy of physics – you find yourself forced to accept some unpalatable claims: equilibrium is disturbed and a rethink is required.

Nor should the range of ‘equilibria that can withstand examination’ be overstated. To the extent that there can be said to be some common knowledge and a common set of very general methodological standards in philosophy, there will be overlap between equilibria. Nor does just any old philosophical disagreement over whether p, no matter how persistent, necessarily engenders competing equilibria that differ with respect to whether p. Some persistent disagreements do get resolved eventually: it really does turn out that one side was right and the other was wrong. It is of course hard to tell, while disagreement persists, whether or not it will eventually be resolved; and trying to resolve such disagreements is no less recommended by equilibrism than it is by a conception of philosophy.
as aiming at knowledge. Equilibrism recommends only that – when the occasion demands – stopping the argument and moving on is a legitimate move to make in pursuit of our collective aims.

5. THE COMMITMENT PROBLEM

Lewis himself conceives the aim of the individual philosopher as a matter of bringing all of one’s ‘opinions’ into equilibrium. But if knowledge of substantive theses can be claimed neither by us collectively nor even by each of us individually, doesn’t that leave us without any rational entitlement to hold opinions at all? Indeed, doesn’t it undermine our very claim to be engaging in a rational activity? Rescher (1985) defends a view, ‘orientational pluralism’, that is in some respects a much fuller version of what I am calling ‘equilibrism’. Rescher spends a good deal of time on the question of how we should respond to the pluralism that is the inevitable lot of philosophy, and criticises Nozick, whose view has in common with both equilibrism and orientational pluralism the claim that there are ‘various philosophical views, mutually incompatible, which cannot be dismissed or simply rejected. Philosophy’s output is the basketful of these admissible views, all together’ (1981, 21). What Rescher objects to in Nozick’s view is the further claim that (as Rescher puts it): ‘The aim is not securing answers to philosophical questions but gaining insight and “understanding” through the comparison and contrast of various possible explanations’ (1985, 254).

Rescher finds Nozick’s approach anaemic: ‘To project one’s pacifist ideology onto philosophy is to emasculate the subject. It is not by accident that Athena, goddess of wisdom and patroness of philosophy, presides over the arts of war as well. The strife of systems is relentless – the destiny of philosophy is not peace but the sword’ (1985, 259). Or, to put the point in rather less combative terms, it is part and parcel of the philosophical enterprise to offer arguments and justification, to refute and deny, to defend one’s position with passion and rigour. The cosy activity of dispassionately comparing and contrasting that he sees Nozick as advocating is one that does not lend itself to the kind of vigorous debate that is not only a desirable but a necessary feature of the philosophical endeavour.

Rescher’s own view is that ‘from the vantage point of one’s own cognitive-value perspective – the only one that one has – only one optimally adequate position on philosophical issues is rationally warranted’ (1985, 265); so he relativises justification to one’s ‘perspective’. What he ignores, I think, is the extent to which it is possible to make sense of our daily business while jettisoning the quest for justification. We can, I claim, make sense of the philosophical enterprise, as it is actually practised,
while maintaining the agnostic stance towards substantive philosophical theses that is recommended by philosophical scepticism. In other words, agnosticism about a given philosophical thesis need not lead to the kind of philosophical pacifism that Rescher envisages.

Suppose, for example, that you offer an argument for \( p \). It looks very much as though – and indeed you might explicitly claim that – you are offering a justification for \( p \), or a reason to believe \( p \). Surely, one might think, the point of argument is precisely to figure out which claims are justified and which are not – otherwise, what is the point? Well, your argument for \( p \) constitutes a challenge to an audience that endorses \( \neg p \) – and also an invitation to an audience that has no view one way or another to endorse \( p \). Philosophical debate ensues. Your audience may attempt to find fault with your argument: it is a bad argument (perhaps, according to them, there are clear cases of arguments of that form that have true premises and false conclusions), one or more of its premises are false (according to them – though of course you may disagree), or whatever. They may or may not succeed by their own lights, or indeed by yours. Adjustments ensue, wherever they need to be made, in order for equilibrium to be restored to whoever's position was challenged by the original argument itself or by the ensuing debate. We move a tiny step closer to philosophy's aim by filling out a small corner of the space of equilibria. All of this is entirely rational.

But this still depends on the idea that it is legitimate to 'endorse' a position – to have an 'opinion', in Lewis's terms. Rescher is surely right to say that it is crucial to the philosophical endeavour that we take a view. So does equilibrism require us to rescind from taking a view at all? I say it doesn't – but a story is needed.

There has been a fair amount of discussion recently concerning whether the agnosticism that is (allegedly) recommended by the phenomenon of systematic peer disagreement is compatible with taking a view. Sandford Goldberg argues that the appropriate doxastic attitude in the face of systematic peer disagreement is that of 'attitudinal speculation', which is 'a matter of having a degree of confidence above .5 but below the threshold warranting outright belief' (2013, 285). Goldberg argues that attitudinal speculation does all the work that we might have thought we needed belief to do, including making sense of the idea that, when we assert philosophical claims, we do so sincerely. And he takes 'having' a view, or regarding a view as defensible, as amounting to attitudinal speculation plus a certain kind of attachment: 'One who endorses and defends a philosophical view is typically more motivated to persist in defense of the view when challenged, than is one who merely speculates that \( p \).
(We are more committed, and perhaps more emotionally attached, to our philosophical views, than we are to our speculations.)’ (2013, 284). Zach Barnett objects that the requirement of a degree of confidence above 0.5 is too demanding, and argues for a version of Sandford’s view according to which ‘a person’s philosophical views should be her disagreement-insulated inclinations’ (f/c, 13), where one’s ‘inclination’ towards \( p \) is insulated, specifically, from the evidence against \( p \) that arises from the fact that whether or not \( p \) is a matter of systematic peer disagreement. Barnett argues that this somewhat weaker condition on having a view nonetheless satisfies the sincerity requirement.

Goldberg’s and Barnett’s proposals are broadly friendly to equilibrism – after all, they both seek to make sense of the idea that sincere commitment to a philosophical view is compatible with the absence of full-blown belief (and hence justified belief). I would not be sorry if a view along these lines turned out to be viable, and perhaps it will. On the other hand, from the equilibrist perspective, I think, Goldberg and Barnett are overly concerned about the evidential status of the fact of systematic disagreement. Recall our common aim on the equilibrist view – to ‘find out what equilibria there are that can withstand examination’ – and the corresponding individual aim (or at least one aim that one might reasonably have, given the common aim) of ‘coming to rest’ at one of them. That one’s own position is subject to systematic peer disagreement is, from the point of view of those common and individual aims, not something that presents a problem: the fact that other people disagree with us (and for good reasons, by their own lights) on matters of philosophical substance is exactly what we should expect. Argle and Bargle disagree over whether holes are material objects because they have an irresolvable disagreement about how to trade consonance with common sense against theoretical economy. For each of them, at this point, the fact of this disagreement is simply not a threat. Argle does not need to establish that his own view is worthy of degree of belief of > 0.5; nor does he have any need to insulate himself from Bargle’s disagreement. He may get on with the business of pursuing his own individual aim of finding an equilibrium at which he can ‘come to rest’ while being fully aware of the fact that Bargle is aiming to find a different equilibrium of her own that enshrines claims that he, Argle, disagrees with. From the point of view of the collective aim of metaphysics, this, again, is all well and good.

In (tentatively) rejecting Goldberg’s and Barnett’s proposals, however, I have of course just made life harder for myself: equilibrism still needs an account of what it is to endorse a metaphysical claim – in effect, an account of what it is to disagree about anything, as Argle and Bargle disagree with each other over the nature of holes. I suggest – again, tentatively – that something
like van Fraassen’s view about the ‘acceptance’ of scientific theories can be made to solve the problem. Constructive empiricism faces a similar problem to equilibrism: given that science does not aim at the truth, and hence knowledge, of scientific theories, how can we make sense of the fact that scientists do (and indeed must, for the purposes of pursuing that aim) make assertions that apparently commit them to – or apparently express belief in – claims about unobservables for which they have no justification? Van Fraassen’s answer, in short, is that ‘acceptance’ and belief in such claims are two distinct phenomena.

Here is what, for van Fraassen, acceptance amounts to:

Acceptance of theories (whether full, tentative, to a degree, etc.) is a phenomenon of scientific activity which clearly involves more than belief [in empirical adequacy]. One main reason for this is that we are never confronted with a complete theory. So if a scientist accepts a theory, he thereby involves himself in a certain sort of research programme. That programme could well be different from the one acceptance of another theory would have given him, even if those two (very incomplete) theories are equivalent to each other with respect to everything that is observable – in so far as they go.

Thus acceptance involves not only belief [in empirical adequacy] but a certain commitment. Even for those of us who are not working scientists, the acceptance involves a commitment to confront any future phenomena by means of the conceptual resources of this theory. It determines the terms in which we shall seek explanations. If the acceptance is at all strong, it is exhibited in the person’s assumption of the role of explainer, in his willingness to answer questions ex cathedra. Even if you do not accept a theory, you can engage in discourse in a context in which language use is guided by that theory – but acceptance produces such contexts. There are similarities in all of this to ideological commitment. A commitment is of course not true or false: The confidence exhibited is that it will be vindicated. (1980, 12-13)

Roughly, then, the idea is that in ‘accepting’ a scientific theory that is ontologically committed to unobservables, the scientist does not (or at least need not) adopt the attitude of belief towards what the theory says about those unobservables – or indeed any attitude that is a kind of watered-down belief-substitute such as Goldberg’s ‘attitudinal speculation’ or Barnett’s ‘inclination’. Rather, the scientist takes on a kind of practical commitment – a commitment to ‘confront any future phenomena by means of the conceptual resources of this theory’, a ‘willingness to answer questions ex cathedra’ and to assume ‘the role of explainer’, and so on – in other words, to speak and write and act as though the theory is true (or at least, insofar as the theory is incomplete, true or close to true in
broad outline).

This connects, I think, with Goldberg’s and Barnett’s concern about sincerity: if we are not entitled to believe the claims of our own theories, in what sense can they truly be said to be our theories? How can we sincerely endorse the claims those theories make? Acceptance, I take it, is supposed to deliver sincerity. The attitude of acceptance does not, of course, constitute sincere belief; but it is sincere nonetheless. The working scientist adopts a theoretical standpoint, works hard to accommodate the existing evidence and explore further consequences of her theory, makes adjustments where necessary, and so on. And she can do all of this entirely sincerely while yet merely accepting rather than believing her own theory, according to van Fraassen. All of this, I suggest, amounts to the scientist’s taking a view in as much of a sense of ‘taking a view’ as is required of her.

If something like van Fraassen’s notion of acceptance really can constitute a legitimate sense in which one might ‘take a view’ (again, this is a big if), then it can, I think, be applied to the working philosopher no less than to the working scientist. The aims of science (according to van Fraassen) and the aims of philosophy (according to me) differ, of course: the aim of empirical adequacy in science is very different to the pluralist aim in philosophy of discovering the equilibrium positions that can withstand examination. But in each case the acceptance of a theory that one cannot rationally believe serves a purpose relative to that aim: in the case of science, the aim of empirical adequacy demands that theories that posit unobservables are developed and tested, and in the case of philosophy the aim of the discovery of equilibria demands that we take on board a set of core assumptions and methodological prescriptions in order to develop and scrutinise an equilibrium position of our own that can withstand examination.4

4 A somewhat different notion of ‘acceptance’ is discussed in the philosophy of religion in the context of non-doxastic faith (see Alston 2007 and Audi 2008). I lack the space to consider whether Alston’s conception of ‘acceptance’ might work out as an alternative to van Fraassen’s in the context of having a philosophical view.
REFERENCES


www.projects.socialsciences.manchester.ac.uk/lewis/letter-month-

549-67.


206-12.


the Philosophy of Science, 46: 523-44.

University Press.

53-78.

University Press.

Philosophical Studies, 160: 1-29.

Rescher, N. (1985), The Strife of Systems. Pittsburgh: The University of
Pittsburgh Press.

J. Ladyman & D. Ross, with D. Spurrett & J. Collier, Every Thing

University Press.

165-81.

Counterfactual Thinking’, Proceedings of the Aristotelian Society,
105: 1-23.

PRESIDENT: Tim Crane (Cambridge)

PRESIDENT-ELECT: Helen Beebee (Manchester)

HONORARY DIRECTOR: Rory Madden (UCL)

EDITOR: Guy Longworth (Warwick)

LINES OF THOUGHT SERIES EDITOR: Scott Sturgeon (Oxford)

EXECUTIVE COMMITTEE: Clare Chambers (Cambridge) / Tim Crane (Cambridge)
    James Harris (St Andrews) / Ulrike Heuer (Leeds) / Sacha Golob (KCL)
    Elinor Mason (Edinburgh) / David Owens (KCL) / Jo Wolff (Oxford)

MANAGING EDITOR: Josephine Salverda (UCL)

ASSISTANT EDITOR: David Harris

DESIGNER: Mark Cortes Favis

ADMINISTRATOR: Nikhil Venkatesh (UCL)