

Is there Progress in Philosophy? A brief case for optimism *

Daniel Stoljar

Abstract: This chapter sets out an optimistic view of philosophical progress. The key idea is that the historical record speaks in favor of there being progress at least if we are clear about what philosophical problems are, and what it takes to solve them. I end by asking why so many people tend toward a pessimistic view of philosophical progress.

Key words: Progress, philosophical problems, Descartes, matter, Hume, induction, explanation, optimism, pessimism.

I

It is a cliché of intellectual life that philosophy makes no progress. Looking at the discipline from the outside, many will agree with Nobel Laureate Francis Crick that “if you ask how many cases in the past has a philosopher been successful at solving a problem, as far as we can say there are no such cases.”¹ And from inside the view is little better. Many philosophers provide despairing assessments of the chances of solving philosophical problems, and many more have the sinking feeling they might be right.² (One colleague once told me that the whole issue of progress made her feel sick.)

I am here to tell you that the cliché is false. I am an optimist about philosophical progress, but (of course) of a nuanced and sophisticated sort—a reasonable optimist, as I will say. I am not a reasonable optimist because I think that I and I alone have discovered some new and distinctive methodology that (finally!) puts philosophy on the sure path of science. Like many other contemporary philosophers I don’t think philosophy has a distinctive methodology; hence it has no distinctive methodology that is new and certainly none discovered by me. Rather I am a reasonable optimist because I think that in philosophy (as in many parts of inquiry) the historical record speaks in favor of reasonable optimism—at least it speaks that way *if* we are clear about (a) what philosophical problems are, and (b) what it takes to solve them. What follows is a very brief explanation and defence of this point of view.³

II

We may begin with two examples of (what I take to be) solved philosophical problems. I will describe the first briefly, the second even more briefly.

Exhibit A is perhaps the most famous philosophical problem of them all, namely, the mind-body problem as formulated by Descartes. As we all know, in the *Meditations* and other works, Descartes argued that the existence of consciousness or thought is inconsistent with the truth of materialism, and that consequently a certain sort of dualism is true. And his contemporaries, including Thomas Hobbes, Antoine Arnauld, and Princess Elisabeth of Bohemia, objected to what he said in various ways.

What happened to this dispute over the mind-body problem between Descartes and his critics? The standard answer—the pessimistic answer—is that the problem is still with us, and that philosophers are still banging on about it. In effect, this repeats the cliché that philosophy makes no progress. But this pessimistic answer pays no attention to the way Descartes formulated the problem. If we do pay attention to this, what emerges is that, contrary to what often seems to be supposed, the Cartesian mind-body problem has been solved. There are three points to make.

First, when Descartes talked of “matter” or “body”—when he talked of the physical, as we would put it—he explicitly identified matter and extension in space. In the *Principles of Philosophy*, he says (1985c, 210) “extension... constitutes the nature of corporeal substance,” and in *Meditations VI*, which contains his main argument for dualism, he writes (1985a, 54) that he has a “clear idea of body in so far as this is simply an extended, non-thinking, thing.”

Second, Descartes’ assumption that matter is extension is not merely an historical detail, unrelated to the underlying plausibility of his position. For the key argument against materialism and for dualism that he gave in *Meditation VI* relies crucially on there being available, as he expressed it, a clear and distinct understanding of what thought and matter are—the basic point being, to put it very roughly, that enough is known about both to know that they are distinct one from the other. In turn, if matter is extension, this assumption is *prima facie* plausible at least as regards the physical, since it is plausible to think that, courtesy of geometry, there was available to Descartes (and available to us for

that matter) a clear and distinct understanding of what extension is, at any rate as Descartes intended that notion.

Third, while Descartes himself assumed that matter is extension, and while this is crucial to his argument against materialism, this assumption was subsequently rejected by the philosophical and scientific community.⁴ Instead of supposing that matter is extension, philosophical and scientific opinion after Descartes's death converged on the idea that matter—that is, the brains and bodies of sentient creatures, and the cells that make up those brains and bodies, and the atoms that make up those cells—is something that fills in space, rather than being something that is space. Since it is an open question what it is that fills in space, it becomes no longer plausible to say that we have a clear and distinct idea of matter, and certainly it becomes no longer plausible to say this on the ground that Descartes did.

Putting these points together, the Cartesian mind-body problem is a solved problem. We might perhaps agree with Descartes that thought or consciousness is not a function of extension as he understood it, and for precisely the reasons he gave. But that gives us no reason to deny that thought is a function of matter, since matter is not extension. Hence it gives us no reason to deny materialism, which is precisely Descartes's intended conclusion. Likewise it gives us no reason to endorse dualism, at least if that is understood (as dualism is usually understood) as entailing that thought or consciousness is a fundamental feature of reality, somewhat like space or time or gravity, something not derivative on anything else. After all, even if thought is not a function of extension, it doesn't follow that it is not a function of anything; hence it does not follow it is fundamental.

III

So much then for (the very famous) Exhibit A—Exhibit B is almost as famous; it is the problem of induction as formulated by David Hume. As we all also know, in the *Treatise* and other works, Hume argued (among many other things) that induction was irrational; that is, it is never rational to infer (for example) that all emeralds are green from the premise that all observed emeralds have been green. It is this and related points of Hume's that famously spurred Kant to awake from his dogmatic slumbers and write the *Critique of Pure Reason*.

What happened to the problem of induction as formulated by Hume? Once again, the standard answer is that it is still with us, and philosophers are still discussing it. But again, this answer pays no attention to the way in which Hume formulated the problem. For, as has been pointed out by twentieth-century commentators, the problem as Hume formulated it assumes (what is sometimes called) deductivism, according to which it is rational to infer the conclusion that all emeralds are green from the premise that all observed emeralds are green *only if* the premise logically or a priori entails that conclusion.⁵ But since it is clear that the premise does not logically or a priori entail the conclusion—there is no contradiction in supposing both that all observed emeralds are green and yet not all emeralds are—deductivism yields Hume’s skeptical conclusion. On the other hand, the falsity of deductivism is pretty much an article of faith of contemporary philosophy: a conclusion may be rationally inferred from a premise if, for example, that conclusion is adequately explained by the premise or rendered sufficiently probable by the premise, and both of these things can obtain *even if* the premise does not a priori entail the conclusion. If deductivism is false, we no longer have Hume’s reason for saying that induction is irrational. Conclusion: the Humean problem of induction, like the Cartesian mind-body problem, is a solved problem.

IV

Crick suggests there are “no cases” in which philosophers have solved their problems. I have just provided two cases in which this has occurred. At least one of us therefore must be wrong. In a moment I will consider whether I’m the one making the mistake. But first, I want to point out that these two examples conform to a pattern, and that thinking about this pattern itself makes reasonable optimism plausible.

Philosophical problems often, but not always, involve a conflict between three theses. The first thesis, which we may call a boundary thesis, proposes a boundary or limit on truth or knowledge or reasonable belief. The second thesis says, of some potential truth or item of knowledge or reasonable belief, that it exceeds that boundary, or at least would do so were it to exist or obtain. The third thesis says that the potential thing does indeed exist or obtain. To solve problems

of this sort—we may call them boundary problems—you need to resolve the apparent tension: either reject or modify one or other of these theses.

The two examples we have just looked at are both examples of boundary problems, or at least can be reasonably interpreted that way. In the Cartesian case, the first thesis, the boundary thesis, is that all truths are either truths about extension or a function of truths about extension—this is the version of materialism that Descartes opposes. The second thesis is that truths about consciousness or thought, if there are such, are neither truths about extension nor a function of truths about extension. The third thesis is that there are truths about consciousness or thought—for example, I am a thinking conscious being. In the Humean case, the first thesis is that all reasonable beliefs, at least if they concern contingent matters of fact, are either perceptual beliefs or follow a priori from perceptual beliefs. The second thesis is that reasonable beliefs about the future, if they exist, are neither perceptual beliefs nor follow a priori from such beliefs. The third thesis is that there are reasonable beliefs about the future—for example it is reasonable for me to believe that I will not fly to the moon in ten minutes or that I will have dinner tonight.

So the two examples conform to a pattern—but not only that, the solutions to these problems conform to a pattern too. For in both cases, the problem was solved by rejecting or modifying the boundary thesis constitutive of it. (Of course this is not to say that that Descartes or Hume tried to solve their problems in this way; I am talking about how the problems were in fact solved.) In the Cartesian case it turned out to be false that every truth is a truth about extension; in particular even truths about matter are not of that sort. In the Humean case, it turned out to be false that reasonable beliefs concerning matters of fact are either perceptual beliefs or follow a priori from such beliefs, for this idea entails the deductivism I mentioned above.

Of course these are only two examples. But they are very central ones. Indeed, it is hard to think of two problems more squarely philosophical than the mind-body problem *chez* Descartes and the induction problem *chez* Hume. Moreover, the examples are, I think, illustrative of a very common way in which philosophical problems are ultimately solved: they get solved by expanding the possibilities of what a truth is or what a reasonable belief is; they get solved, that

is, when we realize that a boundary thesis that seems or seemed plausible needs to be modified or given up altogether.

V

The conclusion to which we are heading is this: reasonably many philosophical problems have been solved in the past, and we therefore should expect on general grounds that reasonably many will be solved in the future. Since this last claim is definitive of the thesis about progress I have in mind, we arrive at the general suggestion that the historical record speaks in favor of reasonable optimism.

How might one object to this suggestion? As with any topic in philosophy, the issues here are complex and there are many highways and byways to pursue. But for reasons of space, I will concentrate on three main lines of criticism.

VI

The first objection says that what I have asserted can't be right because, while it might be true that we have solved a particular problem in the past, there is often a successor problem that can be formulated. That is, when we reject a boundary thesis there is often a related, or successor, thesis that is true, or apparently true. In the mind-body case, for example, it may be said that, while the problem as Descartes formulated it has been solved, it is possible to raise a successor problem on the same topic. Indeed, one successor is presumably the problem that is being discussed by contemporary philosophers of mind when they talk about materialism and consciousness.

However, while it is true that there is often a successor problem, this by itself is no criticism of reasonable optimism. For we should not suppose that we make progress on some problem in philosophy *only if* no successor problem can be formulated. That is a very implausible idea, and the reason is that, if applied consistently, it would entail that there is no progress in any field whatsoever. Suppose, to adopt an absurdly simplified example, that a historian makes a proposal about what caused World War II—World War I, say. Obviously various successor problems can be formulated—for example, what caused World War I,

how exactly did World War I cause World War II, and so on. Even so, it would be quite wrong in that case to reject the first answer as constituting no progress.

It might be replied that the point is not merely that a successor problem can be formulated, but that, at least in many cases, the successor is the *same* problem as the original, perhaps put in a different way. After all, suppose Q is some big philosophical question currently under discussion, and P is a problem debated in the past to which Q is the successor. If Q is *identical* to P, it follows that P is open if Q is, and since Q is open (or so we may assume) the history of that problem is the history of an unsolved problem.

It is true that *if* the successor problem is the same as the original, then reasonable optimism is in trouble, but it is also true that at least in many cases the successor problem is not the same as the original. As we have seen, for example, the contemporary problem about materialism and consciousness is distinct from that posed by Descartes. For Descartes the problem turns on the identification of matter and extension, whereas for contemporary philosophers it certainly does not turn on that.

It might be surprising to learn that the mind-body problem that was discussed by Descartes is distinct from the problem discussed by contemporary philosophers—isn't it a common idea that philosophical problems are perennial? Yes, this is a common idea, but on reflection it is also mistaken; indeed, this is an important point in the general case for optimism. It is true perhaps that the *topics* or *subject matter* of philosophy are perennial. Different philosophers at different periods of history, and in different cultures, have been interested in these topics—for example, both Descartes and contemporary philosophers of mind are interested in a general sense in the relation between the mental and the physical. But it scarcely follows that they are asking the same big question about that topic. After all, scientists and historians are asking questions on the same topics that their forebears asked—the fall of Rome, for example, or the origin of the universe—but it does not follow that they are asking the same big questions about these topics. And if they are not asking the same big questions, it does not follow that they have made no progress on the topics that interest them.

The second objection says that the cases I have pointed to involve merely *negative* progress, i.e., cases in which we have rejected something. Isn't the important issue whether we can make progress of a positive sort? This suggestion is in one form or another very common; pessimists or people worried by it often concede that philosophy makes progress, but only of a negative sort. Peter van Inwagen for example writes "if there is any philosophical thesis that all or most philosophers affirm, it is a negative thesis: that formalism is not the right philosophy of mathematics, for example, or that knowledge is not (simply) justified, true belief" (2004, 334-5).

However, while this negativity objection is common, it is also subject to two straightforward replies, at least if we construe it as an objection to the line of thought we have been considering. First, it misunderstands the structure of boundary problems. These problems are inconsistent triads, and so solving them will always involve giving up something. That is how the issues are structured. So it is a confusion to say that solving them by rejecting something is merely negative progress. Indeed, this is a general feature of any problem that may be presented as a sort of paradox. If we have a set of plausible premises that lead to an unbelievable conclusion, we know a priori the premises and the negation of the conclusion cannot all be true. And this means that to resolve the paradox you must say something negative. If you insist on solving the problem *and* on not saying anything negative, you have insisted on not solving the problem.

Second, while it is true that rejecting a boundary thesis is negative, there is something positive that it immediately brings in its train. To reject or modify a boundary thesis is to reassess or expand the bounds of the possible as to what can constitute a truth, or a knowable fact, or a reasonable belief. But to expand the bounds of the possible in this sense *is* to do something positive. For example, at one point in his discussion of Hume's attachment to deductivism, D.C. Stove remarks that rejecting it had "something of the quality of waking from a nightmare" (1970, 97). Perhaps the "rejecting deductivism" bit here is negative, but the "waking from a nightmare" bit surely is not!

The third objection says that, while boundary problems may indeed constitute one important type of philosophical problem, this type of problem does not exhaust the field. Don't we want a theory or explanation of the various phenomena philosophers are interested in, not simply resolutions of various paradoxes that these phenomena involve?

In response, I think we should grant that philosophers often want theories or explanations of various phenomena they have been interested in. But I also think that when we reflect on this issue, here too it emerges that philosophers have been successful.

To bring this out, it is helpful to have before us a well-known general view of what explanation might be; I have in mind David Lewis's view of causal explanation (see Lewis 1986). Lewis first makes a metaphysical assumption, namely, that the world consists in, or contains, a huge causal structure. He then argues that to explain something causally is to provide some information about this structure. Lewis goes on to point out that providing information is something that can be done well or badly (see Lewis 1986, 226-7). Other things being equal, to provide information well entails providing information that is (a) correct or mostly correct; (b) not too abstract, in the sense that it does not leave open too many possibilities; (c) justified in the sense that the person providing the information has good reason or evidence for doing so; (d) *new* information, in that it does not simply repeat what is already known; (e) responsive to current concerns of the questioner; (f) packaged in a way that is relatively easily understandable or assimilated; and (g) corrects previous falsehood.

Lewis's view is not clearly applicable to philosophy, since philosophers, while they are certainly interested in what constitutes causal explanation, are not so often interested in providing causal explanations of the phenomena that concern them. Still, it is possible to adjust Lewis's picture *mutatis mutandis*: just as there are causal relations between events, there are also other sorts of relations, for example, the existence and nature of some events necessitate, supervene on, realize, or ground, the existence and nature of other events.⁶ If we gather together these relations under the general heading of "constitution" we may say that, just as the world consists in or contains a huge causal structure, it also consists in or contains a huge constitutive structure. Likewise, we may say that, just as to explain causally is to provide information about the causal structure of the world,

to explain constitutively is to provide information about the constitutive structure of the world. Once again Lewis's point about providing information well or badly applies. In fact, as he himself says (1986, 227), this point applies no matter what structure we have in mind. It works for the Melbourne train system. It works for huge causal structures. And it works too for huge constitutive structures.

Philosophers may not be interested in causal explanations, but they are certainly interested in constitutive explanations. When we ask, for example, what consciousness is, or what morality is, or what knowledge is, I think it is indeed plausible to say that what we want is some information about the constitutive structure of the world, or at least of those parts of the structure that contain, or seem to contain, consciousness, morality and knowledge. Have we made progress on problems of this sort? Well, once we have the Lewis-inspired idea about explanation before us, this question becomes whether the information that we currently provide about these constitutive structures is an improvement on the information that we have provided in the past.

But if that is the way the question is asked, it is difficult to deny that it should be answered in the affirmative. Surely the information that we provide in philosophy of mind, for example, about the nature of conscious states is better along the several dimensions Lewis sets out in his discussion of explaining well or badly. Consider the idea that conscious states consist of awareness of properties, rather than simply the instantiation of properties, which is an idea that in one or another form dominates contemporary attempts at providing constitutive explanations of consciousness.⁷ This is a very plausible idea, one that is assumed in many otherwise different approaches to philosophy of mind. Even philosophers who deny it typically deny only that it is applicable to all conscious states; very few deny it is applicable to many.⁸ Hence, this sort of view about mental states provides better information about constitutive structures associated with consciousness than we had before. It is, in short, an example of progress on a problem that is not a boundary problem. To the extent that it too is representative, we have reasonable optimism.

It is in these ways, therefore, that the historical record indicates that reasonable optimism is true in the case of philosophy. Of course what I have said is extremely brief; as I have noted, there are many further issues to take up, which won't be pursued here. Instead let me finish by returning to the cliché we started with. If what I have said is true, or even near the truth, a good question is why this cliché is a cliché in the first place. Why, in other words, do so many people hold or tend to hold a pessimistic attitude about philosophical progress?

When thinking about this question, it is helpful to separate non-philosophers from philosophers. In the case of non-philosophers it is unfortunately hard to escape the conclusion that such people are pessimistic simply because they know so little about philosophy, and related to this have numerous false beliefs about it. One very common mistake, for example, is that philosophers are by definition people who make unfounded speculations about the topics they are interested in—this idea seems to be motivating Crick in the passage I began with. From this point of view, having departments of philosophy is just silly; it is like having departments of speculation. But my reaction to this is predictable: say what you like about professional philosophers, the idea that they sit around making speculations is laughable, as a glance at any philosophical journal will show.

Another common mistake is that philosophy is a very old discipline, the implication being that it is well past its use-by date. But if one means a discipline in the modern sense of a department of institutionalized learning, then philosophy is no older than other disciplines and indeed may even be younger than related disciplines such as psychology.⁹ And if one means that people have been very interested in philosophical topics for a long time, that is true but the same is true in many fields.

Of course that people who are not philosophers (including very brilliant people who are not philosophers) know so little about philosophy, and have false beliefs about it, is a striking fact about the intellectual environment we find ourselves in. It would be interesting to ask what explains this fact. But I am not going to do that here. Instead, let me turn to philosophers themselves: people employed in universities and similar institutions to teach and research philosophy construed as a field of study. Why are they so often pessimistic, or at least tending to pessimism?

In this case it cannot be that they know so little about philosophy; on the contrary some philosophers who tend to pessimism know as much about philosophy as anyone who ever lived. Instead, I think the answer is that philosophers, even excellent ones, tend to have highly entrenched but mistaken expectations about what solutions to philosophical problems are like. In the case of boundary problems for example, it is common to assume that a solution to the problem will be *either* a declaration that the facts in question are fundamental *or* an explicit reductive account of them *or* a denial of their existence. Now I would agree that boundary problems are not often solved in this way. Hence *if* you restrict attention to potential solutions of this sort, you would be inclined to infer that the problems have not been solved, and hence that pessimism is justified. But the problem with this position is that the restriction itself is unjustified. First, it is certainly possible to solve philosophical problems in another way—denying a boundary thesis is a way of solving them that involves none of the options just mentioned. Second, as I have been emphasizing, the history of the subject affords a number of cases in which this is precisely what has happened.

Of course, that people who are philosophers (including very brilliant people) have false expectations about how to solve philosophical problems is a striking fact as well. It would be interesting to go on ask what explains this fact. But I will not attempt that here. The main point has been that, despite the views of many people both inside and outside the discipline, if we are clear enough about what philosophical problems are, and what it takes to solve them, what emerges is a reasonable optimism about philosophy's future.

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Notes

¹ This quotation is from Blakemore 2005, 75, but I learnt of it from Sytsma and Livengood 2012, 145.

² Two prominent examples are Peter van Inwagen and David Chalmers: see van Inwagen 2004 and Chalmers 2015.

³ The position set out here is developed in much greater detail and a different point of view in Stoljar forthcoming.

⁴ A point of this sort that has been greatly emphasized by Chomsky in a number of his works; see e.g. Chomsky 2000.

⁵ A classic discussion of Hume and deductivism is Stove 1970.

⁶ For some discussion of issues closely related to this, see the essays in Correia and Schneider, B 2012.

⁷ For a recent defense of this sort of picture, see Siegel 2010

⁸ See, e.g., Kind 2013

⁹ For some discussion of the founding of philosophy departments in America, see Menand 2001.