**NATURALISM AND THE INTELLECTUAL LEGITIMACY OF PHILOSOPHY**

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1. There is a striking fact about the history of science: the sciences, at least in the last few hundred years, show a progressive history. (Boyd 1981; Putnam 1975) Later theories tend to make predictions which are both more accurate than earlier theories, and which cover a wider range of phenomena. They have greater explanatory power. In many cases, they allow for a wider range of technological applications. It is this fact about the progressive history of scientific theorizing that provides the best argument for realism about scientific theories. It provides the strongest reason for believing that our theories are at least approximately true. For this reason, there can be little doubt about the intellectual legitimacy of scientific theorizing.

 Philosophy, however, does not seem to have a progressive history. Philosophy has certainly changed over time, but it has not shown the kind of progress that we see in the sciences. The problem is not, of course, that philosophy has failed to produce predictions or explanations of physical phenomena, since such predictions and explanations seem to be no part of the goal of philosophical theorizing. Still less is the problem that philosophy has not spun off any technological innovations. The worry is an epistemic one. When problems arise within scientific theories, by and large, the problems are resolved over time. When there are competing theories within the sciences, by and large, further investigation and the accumulation of further evidence tends to resolve the conflict in favor of one theory rather than the other. Philosophy, however, doesn’t seem to be like this. The conflict, for example, between consequentialist moralities, and deontological views, and virtue theories shows no sign of resolution. The conflict between views of compatibilism and incompatibilism is much the same. The conflict between internalist views in epistemology and externalist views continues unabated. There are developments within each of these different competing theories; the best current version of each of these views looks different in important ways from earlier versions. But the fundamental conflict remains entirely unresolved. And this is utterly different from what one sees in the sciences.

 It will not do to suggest that the goal of philosophical theorizing is not to have approximately true theories but rather to achieve understanding. It is one thing to claim that understanding is *a* goal of philosophical theorizing; quite another to claim that having theories that are approximately true is not also such a goal. It is difficult indeed to see how understanding of knowledge, freedom, morality, and so on might be achieved without also achieving a good deal of knowledge about such phenomena. The claim that the goal of philosophical theorizing is understanding thus does not offer an alternative to the view that progress in philosophy requires progressively true theories.

 Because the progressive character of scientific theorizing provides our reason for thinking that current theories are at least approximately true, and therefore provides our reason for regarding the scientific enterprise as an intellectually valuable one, the lack of such a progressive history in philosophy is especially worrisome. It suggests that, whatever theories we may favor within philosophy, there is no good reason to believe that they are actually true or even approximately true. And this, in turn, suggest that there is reason to question the very intellectual legitimacy of philosophical theorizing.

2. Some will argue that philosophy does have a progressive history. Scott Soames (2014, 2017) offers a history of philosophy in the 20th century which paints it in progressive terms. Timothy Williamson (2014) offers such a progressive view as well. Robert Brandom (2009) takes a longer view, situating his progressive account of the history of philosophy in ideas found in Kant and Hegel. In moral philosophy, Derek Parfit (2011) offers an account of the history of ethics which sees it in progressive terms, ultimately uniting moral theories which others had seen as competitors. Many philosophers throughout the history of the discipline have offered progressive accounts of its history, typically, of course, culminating in their own work. No doubt each of the readers of this paper will have views about the ways in which philosophical theorizing has developed over time, making progress on the issues which concern them most. As a naturalist, there is a story about the history of philosophy that I sometimes tell myself which culminates, unsurprisingly, in the triumph of naturalism. So there is no shortage of accounts of the history of philosophy in progressive terms.

 This plethora of progressive accounts of the history of philosophy does not resolve the problem I raise; it is, instead, a symptom of the problem. There is a progressive account of the history of the sciences, but we do not have fundamentally different stories about the history of physics, or chemistry, or biology, culminating in radically different theories within each of these areas. Within the sciences, there is broad agreement about a single progressive story, and it is because of that agreement that theorizing in the sciences can draw on earlier theories in ways that have led to the many sorts of progress—in prediction, explanation, and technological application—mentioned above. It is the lack of this sort of agreement within philosophy that raises doubts about what we are doing in this field, whether we are, in the end, latching on to the truth, or nearly so, on the various topics we investigate. And it is the lack of this sort of agreement, the lack of a single progressive account, that raises doubts about the intellectual legitimacy of philosophy.

3. In this paper, I propose a solution to this problem. I offer an argumentative strategy for philosophy which is recognizably naturalistic, which is well-represented in the literature, and which puts philosophy, or at least philosophy which adopts this argumentative strategy, in good epistemic standing. I present examples of a wide range of philosophers who have adopted this common strategy in order to show that my ideas here are anything but idiosyncratic. The strategy I propose is widely, though certainly not universally, adopted. I would not defend all of the conclusions that the various philosophers I cite have reached. One may agree that the strategy these philosophers adopt is the right one for legitimate philosophical theorizing, and yet disagree about the details of the particular implementation of that strategy.

 I will not argue that this is the only strategy one might pursue if one wishes to defend the intellectual legitimacy of philosophy. Perhaps there are others as well. All the same, the legitimacy of philosophy is surely under threat for the reasons sketched above, and if we can fkind even one approach which will escape this argument, that will be a real achievement. If others wish to suggest that there are additional ways in which the threat can be avoided, so much the better.

3.1 Jerry Fodor’s defense of the language of thought (1975) makes the following argument. The cognitive sciences constitute a rich body of theorizing about the mind, and work within this field, just in the two decades prior to Fodor’s work, have shown a progressive history: they have made progress in understanding how cognition works, offering ever greater and more accurate predictions and explanations. These theories offer computational accounts of cognition, and there is a presupposition underlying such theories which Fodor brings out here. Computation is not possible without mental representations; representation requires a representational medium; any such medium can only do the work required if there is a language of thought.

 Let us forget, for the purposes of this paper, whether Fodor is right about the language of thought. What I am interested in here is his argumentative strategy. Fodor begins with work in a well-established scientific tradition and argues that his conclusion—that there is a language of thought—is presupposed by that work. The fact that the scientific tradition he takes as his point of departure is well-established, that it is not some out of the way wildly controversial approach to the topics it addresses, speaks for itself: work in this field has the authority of science. It is not that apparently successful approaches within the sciences cannot, in the fullness of time, lead to a dead end and need to be rejected. But to the extent that there is a well-established research tradition within some science that has a progressive character, there is currently every reason to view the results of that tradition as presumptively true. One can do no better than this. This provides one with excellent reason to believe that the results of such theorizing are at least approximately true. But now if these theories within the cognitive sciences share the presupposition which Fodor has brought to light, that is the best sort of reason one could have for believing that the presupposition is true.

 The language of thought hypothesis, on this way of viewing things, is a well-confirmed scientific theory. What does it have to do with philosophy? There can be little doubt that this view is deeply relevant to a wide range of philosophical issues about the mind which have been the subject of philosophical theorizing for centuries. Questions about innateness, about the a priori, about the nature of inference and its justification, and, of course, the nature of thought, are all illuminated by the language of thought hypothesis. So the theory which Fodor offers makes an important philosophical contribution. By deriving this view from well-established work in the sciences, the intellectual legitimacy of the enterprise in which Fodor is engaged is thereby assured. Here we have work which has the epistemic standing we were looking for in philosophical theorizing, and it achieves that standing by being rooted in independently well-established science.

3.2 One more example from Fodor may be found in his work on the modularity of mind (1983). Fodor’s work here is influenced in a very direct way by Chomsky’s account of language acquisition (1957, 1964). Earlier theorists, such as Skinner (1957), viewed the learning of language as a product of domain-general learning strategies. On this view, we learn language by way of the very same learning strategies that allow us to learn to tie our shoes, to learn physics, to learn auto mechanics or plumbing, or to learn anything else. There is a set of learning strategies which work equally well on any of the topics we are capable of learning. Chomsky, famously, argued that language learning is not like this. We have an innate set of cognitive capacities which allow us to learn language and nothing else. Chomsky’s devastating arguments against Skinner’s account of language learning (1959), together with the positive development of his account of how language learning proceeds, led to a revolution in linguistics and a highly developed research tradition with an impressive record of progress.

 Fodor argues that not only Chomsky’s account of language learning, but available accounts of perceptual input systems, offer a picture of the large-scale structure of the mind. Input systems are cognitive modules which are able to perform the rapid processing of information required of them precisely because of their modular structure. The output of these modules is then delivered to central processing where the results of the different input systems may be brought to bear on one another. Fodor offered a rich and detailed account of the structure of the mind, one which had obvious implications for a wide range of philosophical issues.

 Once again, my interest here is not in the details of Fodor’s argument or in defending his account of modularity. Rather, my interest is in the argumentative strategy which Fodor employs in developing the philosophical conclusions he reaches. By starting with a well-developed scientific theory, and arguing that his philosophical conclusions are consequences of that theory, Fodor assures the intellectual legitimacy of the enterprise he is engaged in.

 I will be far briefer in my discussion of other theorists.

3.3 Ruth Millikan’s work (1984, 1993, 2005, 2006, 2019) takes a different approach to theorizing about the mind. Throughout this large body of work, Millikan situates her ideas about the nature of mind in biology, showing its implications for an extraordinarily rich body of philosophical issues. The epistemic standing of biological theorizing is beyond question. The status of her philosophical conclusions is then assured, at least if her derivation of these views from biology is accurate. These philosophical views earn their epistemic standing by being rooted in well-established scientific theorizing.

3.4 Tyler Burge presented a wide range of arguments in favor of an anti-individualist account of mental content, many of which did not employ the argumentative strategy I am concerned with here. (see, for example, his 1979) But Burge did present an argument (1986) for anti-individualism which used exactly the argumentative strategy with which I am concerned. Burge examined David Marr’s theory of vision (1982), and he argued that one can only make sense of the explanations Marr offers by supposing that the content of the mental representations involved in visual processing have anti-individualist content. A well-confirmed scientific theory is used here, once again, as a source of philosophically rich ideas, and the philosophical views thus earn their legitimacy by way of the scientific theory in which they are rooted. Burge’s more recent work in philosophy of mind (2010, 2022) makes extensive use of this strategy.

3.5 The argument for scientific realism due to Hilary Putnam (1975) and Richard Boyd (1981), with which I began this paper, itself involves an application of the strategy under discussion. In examining the history of successful sciences, Putnam and Boyd detect a crucial feature of theory change: later theories tend to entail the approximate truth of the theories they replace. Crucially, it is not just that observational predictions of later theories largely contain and expand upon the observational predictions of earlier theories. Rather, the unobservable objects, properties, and causal relations posited by the theories are approximately preserved in successor theories. The fact that successor theories, which are arrived at as a product of such a constraint, tend to be more successful than earlier theories in prediction, explanation, and technological application then provides us with a reason for thinking that the realist methodological presupposition which led to these theories must itself be true.

3.6 Putnam’s (1973) defense of the causal or historical theory of reference depends on use of this strategy as well. It is important to distinguish Putnam’s argument for this view from Kripke’s (1980). Kripke, famously, sees his argument for the causal theory of reference as rooted in an appeal to intuition. Much as many others have expressed serious doubts about the epistemic status of the kinds of intuitions to which philosophers often appeal in support of their theories (see, for example, many of the papers in DePaul and Ramsey 1998), Kripke holds that intuitions about hypothetical cases provide an exceptionally secure foundation for philosophical theorizing.

Some philosophers think that something’s having intuitive content is very inconclusive evidence in favor of it. I think it is very heavy evidence in favor of anything, myself. I really don’t know, in a way, what more conclusive evidence one can have in favor of anything. (Kripke 1980, 42)

But Putnam’s argument for the causal theory of reference is not rooted in appeals to intuition. Rather, Putnam’s argument is part of his response to views in the philosophy of science, such as Feyerabend’s (1975) and Kuhn’s (1962), which raise doubts about the objectivity of science. Putnam argues that descriptions theories of reference fail to make sense of the history of science. In particular, scientists who hold differing views about some phenomenon, such as Newton and Einstein, are viewed as talking about different things, since the referents of their terms are determined by the descriptions they associate with them, rather than disagreeing about some single thing. When Newton says that mass is independent of velocity and Einstein says that it is, instead, dependent on velocity, a descriptions theory of reference makes the referent of Newton’s use of the term ‘mass’ different from the referent of Einstein’s use of that term. Instead of disagreeing, this view thus has the two of them talking past one another. As Putnam makes clear, any such view makes a hash of the history of science. A constraint on a theory of reference that makes sense of the history of science is that it allows for commonality of reference across theoretical disagreement, and it is this that leads Putnam to the causal theory. Putnam’s argument for the view, unlike Kripke’s, is an empirical argument rooted in the sciences.

3.7 My (2002), *Knowledge and its Place in Nature*, offers a defense of a reliabilist account of knowledge which makes use of this argumentative strategy as well. In substance, my account of knowledge is very much like Alvin Goldman’s (1986), but while Goldman offers his account of knowledge as a matter of conceptual analysis—an analysis of the folk concept of knowledge—I argue that knowledge is a natural kind and Goldman’s account offers the best available view of the nature of that kind. I argue that knowledge plays a crucial explanatory role in theories in cognitive ethology, and the fact that our best theories of animal behavior appeal to such a thing gives us reason to believe that knowledge is the very state about which theorists of animal behavior give an account. The account they give, I argue, bears a striking resemblance to the view which Goldman arrives at by other means. More than this, just as the referent of the term ‘water’ was H2O even before the chemical composition of water was known, the referent of the term ‘knowledge’ was reliably produced true belief even before theorists of animal behavior developed their evolutionary account of the functional role of the mental states of animals. As I see it, such a foundation for the reliabilist account of knowledge provides a far more secure grounding of that view than appeals to conceptual analysis. This is just an application of the argumentative strategy under discussion here.

3.8 The KK Thesis, the view that knowing that *p* entails knowing that one knows that *p*, has been very widely rejected in recent years. Dan Greco (2014) has offered a striking defense of the view. Greco offers a variety of considerations in favor of the KK Thesis, but one important strand of his argument makes use of our argument strategy. Greco argues that the notion of common knowledge plays an important explanatory role in computer science, in game theory, and in explanations of social conventions in a variety of social sciences. Common knowledge involves the infinite hierarchies of knowledge required by the KK Thesis and which many who object to it have argued are never actually instantiated. By rooting these infinite hierarchies in successful theories in the sciences, Greco makes use of the strategy under discussion.

3.9 Well-being plays an important role in many moral theories, as well as in accounts of human flourishing. Some have sought to illuminate the nature of well-being by conceptual analysis, but a variety of recent theorists have offered accounts of well-being which are grounded in the social sciences. Work by Michael Bishop (2015), Daniel Haybron (2008), and Anna Alexandrova (2017) are representative. These philosophers see any defense of an account of well-being as requiring just such scientific backing. And this is just an application of our argumentative strategy.

3.10 The strategy has been employed, as well, in explaining the nature of the good and the right in moral theory by Cornell realists such as Richard Boyd (1988), Nicholas Sturgeon (1985), and Peter Railton (1986).

3.11 Work in philosophy of science, and especially in philosophy of physics and philosophy of biology, has long been a home for precisely this kind of inferential strategy. See, for example, Dupre (1993), Kitcher (2003), Ladyman and Ross (2009), Maudlin (2011), Sklar (1977), Sober (1994, 2000), Sterelny (2001, 2014), Wallace (2014).

3.12 Penelope Maddy (2007) presents a version of this inferential strategy as a general method for philosophical theorizing.

4. I turn now to a consideration of some objections.

4.1 One might object that this argumentative strategy leaves philosophy hostage to the vicissitudes of science. Although it is true that science has a progressive history, it is also true that scientific claims which are well-supported at one time may be given up when new evidence arises; scientific progress is not a simple matter of adding new claims to old without any revision. As a result, building philosophy on our best available scientific theories leaves open the possibility that our philosophical views will be based on falsehoods rather than truths. It is for this reason that some would see the strategy proposed here as leaving philosophy a hostage to the inevitable revisions in current scientific claims. I would put this differently: this strategy leaves philosophy hostage to the facts. It would certainly be unwise to defend philosophical theories which are rooted in highly controversial scientific views,[[1]](#footnote-1) but that is not what is being suggested here. Rather, the suggestion is that philosophical views should be grounded in our best available scientific theories. This provides an exceptionally secure, even if fallible, foundation for philosophical theorizing. Since even our best available scientific views may be revised with the passage of time and the availability of new evidence, our philosophical views are subject to revision in the very same way. But far from being a fault in the proposed strategy for philosophical theorizing, this is a source of its strength. If philosophy can be given a foundation which makes its views no less secure than views in our best available sciences, this provides us with a robust response to the worry that the history of philosophy fails to show the kind of progress that we see in the sciences. By adopting the proposed argumentative strategy, philosophy can achieve a record of progress that leaves no room for doubt about its intellectual legitimacy.

4.2 One might object that the kind of empirical grounding of philosophical theorizing, whatever its successes in some areas, will not cover all areas of philosophy. For example, it seems implausible as a strategy for explaining the nature of mathematical knowledge.

 I do think that it is an open question as to how far the strategy I endorse may be pushed, and how much of philosophy may be grounded in such a strategy. At the same time, I believe there is some reason for optimism here. As indicated above, some areas which one might have thought would be especially resistant to this strategy, such as work in moral philosophy, have actually been developed by a number of philosophers in encouraging ways. It would simply be a mistake to suggest that the current strategy obviously fails for normative inquiries within philosophy. Any such claim would have to overlook not only the work of moral realists, but the work of naturalistic epistemologists as well. There are even promising accounts of mathematical knowledge which make use of this strategy. Phillip Kitcher’s (1983) makes a case for seeing mathematical progress as rooted in the empirical sciences, just the sort of thing which one might have thought was antecedently implausible. At the same time, it is important to acknowledge that it remains an open question as to just how much of philosophy may be amenable to this approach. If there are areas of philosophy which prove resistant to the argumentative strategy recommended here, then some other account of the intellectual legitimacy of those areas will be needed.

4.3 It may be objected that the proposed argumentative strategy doesn’t answer distinctively *philosophical* questions. Instead, it changes the subject and simply offers us something to do *instead* of doing philosophy.

 It’s hard to know precisely how to respond to such an objection. The many illustrations I have given above all involve views not only put forward by people who are, by every reasonable account, philosophers, but also views which address familiar philosophical issues, such as the nature of thought, of the a priori, of the mental, of knowledge, of the good and the right, and so on. These traditional philosophical topics are, admittedly, addressed by non-traditional means,[[2]](#footnote-2) but this does not make them any less philosophical. I believe that this objection relies on an overly narrow and contentious view of what philosophy is, but it deserves a more detailed response, to which I now turn.

5. There is a story about the origins of analytic philosophy which is widely told, one which would support the objection that the argumentative strategy I propose provides us, not with a way of lending intellectual legitimacy to philosophy, but, instead, with a way of doing something other than philosophy. Because the story is so widely told, and, indeed, so widely believed, it deserves some real attention.

 The origins of the analytic tradition are frequently traced to Frege. One of the many important themes in Frege’s work, and one which played a central role in the development of the analytic tradition, was Frege’s attack on psychologism. Martin Kusch (1995) provides a fascinating and detailed history of the debate over psychologism in the German-speaking world at the close of the nineteenth century, with Frege and Husserl (1900/2001, 1901/2001) taking the anti-psychologistic side of the debate. As Kusch makes plain, exactly what psychologism was is often quite unclear in much of this debate, both in those who defend it, and in those, such as Frege and Husserl, who reject it. For some, psychologism was the view that the subject matters of logic and mathematics—the truth-makers of claims in those fields—were themselves psychological. For others, psychologism was a view about the nature of epistemic justification, one which tied questions of justification to psychological questions, rather than to features of the quality of arguments, which were viewed as entirely independent of psychology. It was widely held by partisans on both sides of the debate about psychologism that the resolution of this issue had important implications for the very nature of philosophy. Rejecting psychologism was seen as a necessary condition for defending the autonomy of philosophy, while the proponents of psychologism were seen as rejecting any such claim of autonomy.

 As Kusch (1995) argues, there was a tremendously important sociological vector playing a role in this debate. The close of the nineteenth century saw the rise of experimental psychology as an independent science. In German-speaking universities, and in much of the world, psychology was housed in philosophy departments, and it is only late in the nineteenth century that psychology separated off from philosophy into a separate department. The rancor which characterized the debate over psychologism, Kusch argues, had a good deal to the intensely hierarchical structure of universities in the German-speaking world, with a very small number of Professors in each department. As psychology was emerging as a separate discipline, with its own disciplinary standards and methods, but before it was housed in a separate university department, the issue of whether professorships would go to philosophers or psychologists was, understandably, extremely fraught. As Kusch tells it, the debate over psychologism, and over the autonomy of philosophy, was lent a good deal of fuel by concerns over the autonomy of philosophy *departments*.

 This is not to deny that there is a legitimate question about the extent to which issues in epistemology about the nature of justification should be seen as dependent on psychological facts or, instead, independent of them. This is, in my view, an exceptionally important issue. As the analytic tradition emerged and began to flower in the work of Russell and Moore, and, still later, in the logical positivists, Frege was held up as the champion of an idea which was central to the development of that tradition, namely, the autonomy of philosophy, and the divorce between questions about epistemic justification and psychological questions about how beliefs are actually formed. Indeed, analytic philosophy became tied to a view about philosophy in general, which saw philosophical issues and their resolution as radically independent of empirical issues and their resolution.

 The methodological strategy I have proposed in this paper is thus a break from that particular aspect of the analytical tradition. But it is important to see that the feature of the analytic tradition from which my proposed methodology does indeed separate itself is a feature of a relatively brief period in one recent philosophical tradition, not a feature of the history of philosophy generally. If I am right, the kind of empirical grounding I have suggested for philosophical claims can serve as one important way to defend the intellectual legitimacy of philosophy. Right now, I know of no other.

6. I have argued that there is reason to question the intellectual legitimacy of philosophy, and the problem arises from the fact that philosophy does not display the kind of progressive history that we see in the sciences. This is a problem because the central reason for believing that the claims of the sciences are at least approximately true is dependent upon seeing our current views as emerging from just such a progressive history. It is not the mere fact that current scientific theories provide us with roughly accurate predictions of the results of experiments that justifies our beliefs in scientific claims. Rather, it is the fact that, over time, scientific theories have become ever more successful in their experimental predictions, in their explanations of phenomena, and in their technological applications. The historical development of scientific theories has depended upon a methodology which builds on earlier theory, not only taking on its observational consequences, but its theoretical commitments as well. And the fact that such a methodology has been instrumental in producing a history of progress is reason for thinking that the presuppositions upon which that methodology was built are at least approximately true. It is this pattern of progress which is at the heart of any defense, not only of realism in philosophy of science, but of the truth, or approximate truth, of scientific claims, and, along with that, of the intellectual legitimacy of science.

 The obvious progress of scientific theorizing has not left much room for serious questioning of the intellectual legitimacy of science. But the lack of progress in philosophy should, as I have argued, make us worry about the legitimacy of the philosophical enterprise. I have thus tried to lay out a methodology for philosophy which allows us to respond to that challenge by showing how philosophical issues may be addressed in a way that emerges from the sciences, thus allowing our philosophical work to inherit its legitimacy from the scientific theories on which it may be based. I have, in addition, tried to make out the case that the methodology I propose is one which is already present in a good deal of work in a wide range of areas in philosophy. I believe that philosophical theorizing has been slowly emerging, beginning in the mid- to late twentieth century, from the misguided myth of the autonomy of philosophy, a myth which was a crucial part of the founding of the analytic tradition.[[3]](#footnote-3)

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1. It is not at all unreasonable to consider what the philosophical implications of such views are so long as one recognizes the highly tentative nature of any such implications. [↑](#footnote-ref-1)
2. I would actually want to qualify this. As I will argue in the next section of this paper, the idea that philosophy is independent of the sciences, much as it was part of the self-image of analytic philosophy for much of the first half of the twentieth century, was, in my view, an historical aberration. For an extended defense of the kind of view of philosophy I favor, and to which I am much indebted, see Kitcher 1992. [↑](#footnote-ref-2)
3. I am indebted to two anonymous referees and to members of the audience at the conference on Naturalism in Philosophy in Sofia for helpful comments on an earlier version of this paper. [↑](#footnote-ref-3)