Aspirational Naturalism

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CONTENTS

Philosophy, as I shall understand the word, is something intermediate between theology and science. Like theology, it consists of speculations on matters as to which definite knowledge has, so far, been unascertainable; but like science, it appeals to human reason rather than to authority, whether that of tradition or that of revelation. All *definite* knowledge-so I should contend-belongs to science; all *dogma* as to what surpasses definite knowledge belongs to theology. But between theology and science there is a No Man's Land, exposed to attack from both sides; this No Man's Land is philosophy. Almost all the questions of most interest to speculative minds are such as science cannot answer...

(Russell 2013, 1)

Philosophy investigates reality in the same way as science. Its methods are akin to scientific methods, and the knowledge it yields is akin to scientific knowledge.

(Papineau 2014 : 166)

Most of us don't worry about these questions [i.e. fundamental questions about the nature of the universe] most of the time. But almost all of us must sometimes wonder: Why are we here? Where do we come from? Traditionally, these are questions for philosophy, but philosophy is dead. [...] Philosophers have not kept up with modern developments in science. Particularly physics. Scientists have become the bearers of the torch of discovery in our quest for knowledge. [… new theories] lead us to a new and very different picture of the universe and our place in it.

(Stephen Hawking 2011, May)

Our current name [i.e. 'philosophy'] is harmful because it posits a big gap between the sciences and philosophy; we do something that is not a science. Thus we do not share in the intellectual prestige associated with that thoroughly modern word. We are accordingly not covered by the media that cover the sciences, and what we do remains a mystery to most people. But it is really quite clear that academic philosophy is a science. The dictionary defines a science as “a systematically organized body of knowledge on any subject.” This is a very broad definition, which includes not just subjects like physics and chemistry but also psychology, economics, mathematics and even “library science.”

(McGinn 2012, March 4)

Philosophy is a field that, unfortunately, reminds me of that old Woody Allen joke, ‘those that can’t do, teach, and those that can’t teach, teach gym.' And the worst part of philosophy is the philosophy of science; the only people, as far as I can tell, that read work by philosophers of science are other philosophers of science. It has no impact on physics what so ever. ... they have every right to feel threatened, because science progresses and philosophy doesn’t.

(Krauss 2012, April 23)

Although there are real methodological differences between philosophy and the other sciences, as actually practiced, they are less deep than is often supposed.

(Williamson 2007 : 3)

I see philosophy not as an *a priori* propaedeutic or groundwork for science, but as continuous with science. I see philosophy and science as in the same boat – a boat which, to revert to Neurath's figure as I so often do, we can rebuild only at sea while staying afloat in it.

(Quine 1969 : 126-27)

Now, it seems to me obvious, but apparently it needs to be stated that: a) philosophy and science are two distinct activities (at least nowadays, since science did start as a branch of philosophy called natural philosophy); b) they work by different methods (empirically-based hypothesis testing vs. reason-based logical analysis); and c) they inform each other in an inter-dependent fashion (science depends on philosophical assumptions that are outside the scope of empirical validation, but philosophical investigations should be informed by the best science available in a range of situations, from metaphysics to ethics and philosophy of mind).

(Piggliuci 2009 : Nov 19)

While science and philosophy do at times overlap, they are fundamentally different approaches to understanding.

(Friedland 2012 : April 5)

Search your mind, or pay attention to the conversations you have with other people, and you will discover that there are no real boundaries between science and philosophy—or between those disciplines and any other that attempts to make valid claims about the world on the basis of evidence and logic.

(Harris 2014 : Jan 14)

Chapter One

Aspirational Naturalism

Abstract

In this thesis I argue for *aspirational naturalism*. Aspirational naturalism is a metaphilosophical thesis that encourages a continuation of the interdisciplinary relationship between philosophy and science. It is a kind of methodological naturalism, a view about philosophical methodology that treats science with epistemic respect.

§1.1 Naturalism

As the above quotations make clear, the perceived relationship between philosophy and science is a vexed one; views run the gamut from science and philosophy being more-or-less indistinguishable bedfellows to their occupying separate, autonomous domains.

Whatever naturalism might be, it's popular. A recent survey of philosophical beliefs polling all members of faculty at 99 leading departments of philosophy, largely focusing on departments specialising in the analytic or Anglocentric tradition, found that 49.8% favour naturalism as a metaphilosophical view, whilst almost half as many favour non-naturalism (25.9%), and "other" views (24.3%) (Bourget and Chalmers 2014, 476). A 49.8% consensus on any philosophical issue is rather rare. Under my home country's (New Zealand's) political structure, a consensus in voting for a political party at this level, would all but allow a single political party to govern alone[1]. Many

[1] This is not strictly true, due to the possibility of separations between those voted in by way of electorate seats and party seats, under MMP, New Zealand's voting system. But for a party to not

philosophers commit to something called ‘naturalism’ but, as we shall see, there is very little agreement on what naturalism is.

Although there is nothing approaching an agreed definition of naturalism, we can nonetheless identify family resemblance: in particular, naturalist philosophical views have in common their relationship to the sciences. Naturalists like science[2]; they afford it some special epistemic, methodological or sometimes even metaphysical priority.

§1.2 Aspirational naturalism

Aspirational naturalism encourages the philosopher to make use of relevant scientific methods and findings where possible, and to exercise some caution when using some of their own traditional methods. As the thesis progresses, it will become evident that I am of the view that philosophy often does this already. I make no calls for wholesale reform of philosophical methods or domains as currently practiced, yet I offer some heuristics drawn from some naturalist critiques of philosophical method, and from observing scientific practice.

govern alone with 50% of the party vote is both a highly unlikely possibility, and an irrelevant digression.

[2] More specifically, my thesis is focused on scientific naturalism, which concerns the relationship between science and philosophy. For present purposes, I'm not interested in those free-spirited groups of people who like to express themselves by wearing no clothes, or naturalism in the context of the philosophy of religion (e.g. see Plantinga 1997) which explores the relationship between science and religion, or views descended from the later Wittgenstein (e.g. *Philosophical Investigations*, §258), which have also sometimes shared the name ‘naturalism’ (e.g. see McDowell (1998) , or Penelope Maddy’s mathematical naturalism (1997 : 184).

§1.3 The naturalistic starting point

My thesis assumes what I will call *the naturalistic starting point*. I define this as holding the assumptions that the sciences are successful epistemic enterprises, and that they do generate knowledge. I assume further, that the methods of the sciences are responsible for the generation of scientific knowledge. That is, I think that it is no mere accident of history that the sciences should be treated with epistemic respect; it isn't that the sciences have generated knowledge through sheer luck, but rather that the methods of the sciences viewed as systems of practice are to be treated as epistemically respectable. I will not argue for these assumptions here, as I am more interested in holding these views fixed, and focusing on consequences of the naturalistic starting point, to inform conversations regarding the applicability of scientific methods in philosophy. Holding that there is scientific knowledge has interesting consequences for one's epistemology, as it is often hard to square scientific knowledge with traditional philosophical accounts of knowledge. Thus, holding the naturalistic starting point can help illuminate what kinds of knowledge have been generated by the sciences.

Some methodological naturalists hold normative views about the methods of science and philosophy and the relationship between the two domains. One group of such views, I will call *reformist methodological naturalisms*. This group of views shares the belief that either philosophical methods ought to be reformed, so that they are made more like those of science, or that some domains of philosophical inquiry ought to be changed or abandoned, due to a perceived inapplicability of scientific methods to them. Reformist methodological naturalists rely on a descriptive claim - that philosophy and the sciences differ in some way, and a normative claim - that they ought not to differ, for the coherence of their views.

§1.4 Could philosophy be more scientific? If so, should it?

The reformist methodological naturalists[3] rely on the truth of the descriptive claim, for their normative claim to be non-redundant. That is, if philosophy and the sciences do not differ in some relevant sense, the normative claim, however true, has already been delivered on. If philosophical methods are already sufficiently scientific, there is no work to be done in terms of changing philosophical method to make them so. Other methodological naturalists argue that the descriptive claim that the reformist relies on is false, and that therefore no changes in philosophical methods are required in light of treating scientific methods with epistemic respect. These methodological naturalists I call *vindicatory methodological naturalists*; they argue that a careful inspection of scientific and philosophical methods vindicates the methods currently used by philosophers as sufficiently naturalistic.

This thesis takes science as its object of inquiry initially, observing science as practiced and looking at the methods that it uses. One component of implementing the naturalistic starting point, involves my trying to leave any *a priori* conceptions of science behind, trying as carefully as possible to observe science as actually practiced, and drawing conclusions about the methods of science from observing science itself. This I think is significant, in that many other methodological naturalisms that I discuss throughout, start from a different perspective, oftentimes starting with a preconceived view about what scientific methods are, and what scientific methods are not, and then discussing how various philosophical theories look according to these *a priori* views of science. I suspect that some of these theorists begin with a view of kinds of philosophy that they intuitively feel are non-naturalistic, and look to describe naturalism in a way that will rule those philosophical practices or theories out as epistemically inferior. This strikes me as a dangerous approach in many ways. If a theorist adopts the naturalistic starting point, it seems to be in conflict with their respect for the sciences for them to take these *a priori* intuitions of theirs about science as serious evidence as to what the methods of science are in the first place.

[3] Henceforth: the reformists.

§1.5 Scientific practice and pluralism

I pay close attention to a study of scientific practice, informed by both contemporary and historical examples drawn from science, to inform an analysis of scientific methods. From a study of scientific method based on observing science, I argue for *methodological pluralism*. That is, I argue that there is not one overarching account of scientific method that catches all of what it is that science does, rather there are a number of different methods employed throughout the sciences. I am importantly not arguing for methodological nihilism, a view that there are no methods that are used in the sciences, and nor am I arguing for methodological anarchism, a view that is sometimes ascribed to Feyerabend, probably due to his quoteworthy slogan that "anything goes" (Feyerabend 1975). Rather, I am arguing that there are a number of different methods which have been used in the sciences, many with some degree of success. To borrow a slogan from Hasok Chang, this is not the view that "anything goes", but rather that "many things go" (Chang 2012 : 285).

From here, the descriptive claim that the sciences and philosophy differ in a significant way, can be inspected more carefully. I outline two different ways in which we could find a descriptive difference in methods used by science and philosophy that the reformist requires for the cogency of their argument. We could either a) find significant scientific methods that are not being used by philosophy, or b) we could find a philosophical method that is not used in the sciences. I argue that a significant descriptive difference between the methods used in the two domains is hard to find at such an abstract level. Thus, the methodological naturalism that I defend in this thesis, is largely a vindicatory one.

§1.6 Progress in philosophy and the sciences

Despite arguing against the descriptive claim that the reformist relies on, I spend some time analysing the normative claim that is part of their view. I argue that the normative claim is largely a result of perceived views of *progress*. I think it is fair to characterise the overwhelming majority of reformists, as sharing the view that science makes more progress than philosophy does. On the basis of this idea, and the reliance on a significant descriptive difference between domains, a reformist argues that an adoption of scientific methods or an abandonment of non-scientific methods, or a combination of these two things, will lead to greater philosophical progress. Talk of progress in science and philosophy, and the thought that scientific methods might generate more progress than non-scientific methods in philosophy, assumes that to some extent philosophy and science share similar goals. Progress, I will argue, involves trajectory towards a goal. The assumption that philosophy and science have a shared goal, seems plausible to an extent. However, it is more accurate to say that philosophy and the sciences share similar epistemic aims. Here, I argue that observing scientific practice motivates pluralism about *epistemic aims*, in the sciences, and for philosophy.

§1.7 Methodological pluralism

Whilst ultimately dismissing a variety of naturalist critiques of philosophy, I draw from them several heuristics for methodological practice in philosophy that help to shape my positive view. I argue that observing scientific practice supports *methodological pluralism*. This is the view that there are a number of different scientific methods, and no one of them can reasonably be considered more important than the others in any straightforward and universal sense. I argue that non-observational methods should remain an important member of the toolkits of both the scientist and philosopher. In some of the more theoretical branches of both philosophy and the sciences, it seems implausible that stereotypically scientific methods, such as observation and controlled

experiment, should or even could have traction. Finding out which methods will be more or less suited for a given area will be a partly empirical matter, and a variety of methods might need to be trialed and compared across philosophical practices, as is instanced within the sciences. Use of a wider set of methods need not be chastised, nor should those more theoretical branches of philosophy suffer premature arboristry due to a perceived inapplicability of scientific methods.

§1.8 An alternative to the continuity models of naturalism: *alignment*

One novel feature of my view is my proposal to change the way the relationship between science and philosophy is viewed in discussions of naturalism. From at least as far back as Quine (1969, 126–7), naturalism in philosophy has often been discussed within a model of continuity. A philosophical theory is naturalistic if, and only if, it is continuous with the sciences - or so the claim goes. I argue that the continuity model faces serious challenges, and propose that instead we look at the relationship between philosophy and the sciences in terms of *alignment*. A philosophical theory can be aligned with the sciences in numerous ways, by way of implementing some or all of the various scientific methods, or by utilising the products of scientific methods to inform philosophical arguments.

I encourage philosophers to do their best to keep informed and make use of relevant scientific methods and their products that may have an impact on their positions. In many cases, the best way for this to be achieved is by continued dialogue in the structured fashion by which academic philosophy already works, e.g. via refereed journal publishing, and conferences, departmental seminars, and personal correspondence. However, philosophy should not blindly follow science, and it has further important roles as a critical spectator of, commentator on, and incubator for, science. These all provide further means for fruitful alignment between philosophy and

science, and give rise to a plurality of important roles for philosophy to play.

§1.9 Methodological heuristics

Throughout this thesis, I develop several methodological heuristics to do with various methods used in philosophy. The view I defend involves a consideration of *relevance*. Relevance involves treating the plurality of sciences as, in principle, potentially relevant to all domains of philosophical inquiry. Precisely when particular scientific methods or products of these methods will be relevantly applied to philosophical inquiry will be very much a case by case matter. This involves denying the assumption that philosophy has its own autonomous domain of inquiry, either isolated from, or somehow prior to, the sciences. This is more or less following Quine (1969 : 126-7), who argues for continuity between philosophy and science. My own view is differentiated from Quine's, when I develop my positive views in the most detail in chapter five. Whilst I depart from Quine in some respects, I think that my aspirational naturalism is Quinean in spirit. I discuss the implications this has for argumentative strategies in philosophy.

§1.10 Rejecting monism as an *a priori* assumption

Motivations for methodological pluralism stem from observing scientific practice. Empirical observation of the sciences supports pluralism; there are currently a pluarlity of different methods at play both between and within scientific domains. Many reformist naturalist arguments rely on assumptions that the sciences exhibit far less diversity, assuming that the sciences really exhibit some kind of monism. I follow Kellert et al. (2006), who describe the argument for monism as follows:

1. the ultimate aim of a science is to establish a single, complete, and

comprehensive account of the natural world (or the part of the world investigated by the science) based on a single set of fundamental principles;

2. the nature of the world is such that it can, at least in principle, be completely described or explained by such an account;

3. there exist, at least in principle, methods of inquiry that if correctly pursued will yield such an account;

4. methods of inquiry are to be accepted on the basis of whether they can yield such an account; and

5. individual theories and models in science are to be evaluated in large part on the basis of whether they provide (or come close to providing) a comprehensive and complete account based on fundamental principles.

(*x*)

I argue that observing scientific practice provides no support for the argument for monism, and that it is therefore a naturalistically unacceptable view. The various premises on which the monist argument relies on are not supported by empirical evidence. Even if it was an aim of science to establish a monist picture of the world, there is no evidence to suggest that such an aim could ever be achieved. As it stands now, the current state of science is patently pluralistic. Thus, at this stage in proceedings, accepting pluralism as a working hypothesis, is the most naturalistically acceptable view of the sciences.

Kellert et al. outline monism, in order to argue against it. I use their outline in a similar fashion, however I do not need to refute monism for my arguments to succeed. For my view to succeed, I merely need to establish that the argument for monism is not conclusive by naturalistic standards, and that the naturalistic starting point encourages us to treat the question of monism or pluralism as an open one. An accurate description of science as practiced shows us that the sciences are currently pluralistic, not just between sciences such as biology and physics, but also within these domains. I

illustrate that this is so using case studies in §3.4 of this thesis. Should evidence eventually weigh in favour of monism, naturalists could then happily endorse this view. For now, the prudent naturalist should treat this as an open question, and the continuation of both monistic and pluralistic programmes should be encouraged in order to make progress on settling this question.

§1.11 Taxonomising naturalisms

In arguing for aspirational naturalism, I'll first establish a taxonomy of naturalist views in chapter two, which in turn allows me to narrow my focus to methodological naturalism, which my view is a version of. As well as helping to narrow the scope of this thesis, the second chapter of this thesis adds a positive contribution to future discussions of naturalism, by way of clarifying a number of distinctions within a very large pool of naturalisms. This chapter merely sketches a number of positions, and I make no claims that it provides an exhaustive taxonomy. It does however outline a number of popular views within the literature that are relevant for the purposes of this thesis. In chapter five, I outline aspirational naturalism. This involves returning to some of views from the taxonomy of chapter two, by elaborating on the implications that my preferred methodological naturalism has for various of the views discussed there.

§1.12 Challenges for aspirational naturalism

It could be complained that the aspirational naturalism I defend is weak, in ways that concern Timothy Williamson when he criticises some variants of naturalism for "falling back on a more inclusive understanding of science that drastically waters down naturalism" (2014 : 30). If a broad survey of the sciences leads to a pluralistic

understanding of science, as I will argue it does, then the naturalist who aspires to align philosophy with the sciences has no plausible alternative but to offer a pluralistic understanding of naturalism, however watered down that may be considered to be. My positive contribution is an incomplete outline, and more work needs to be done to further develop this view.

Some commentators have complained that methodological pluralism collapses into either a vicious relativism, fact-constructivism, or methodological anarchism. I defend the methodological naturalism which I endorse from these criticisms. The naturalistic starting point involved first having epistemic respect for the sciences. If it turns out that the sciences are methodologically pluralistic, then there need be no worry that methodological pluralism will amount to a kind of vicious relativism, a dangerous constructivism, or methodological anarchism. Methodological pluralism both describes science as it is currently practiced, the area with which the naturalist approached with epistemic respect, and also includes the evaluative claim that the plurality that one finds descriptively is *okay*, and is perhaps even a *good thing*.

In earlier stages of my research, I was hesitant to consider my positive metaphilosophical position an instance of 'naturalism', due to having precisely the same concerns expressed here by Williamson:

> I am sometimes described as a naturalist. Why do I resist the description? Not for any religious scruple: I am an atheist of the most straightforward kind. But accepting the naturalist slogan without looking beneath the slick packaging is an unscientific way to form one's beliefs about the world, not something naturalists should recommend.
>
> (2014 : 29)

I take Williamson's concerns seriously, and do my best to resist assuming anything

sloganistic, attempting to carefully observe the sciences in order to help inform my views.

§1.13 Concerns about scope

The naturalist meta-philosophical views that I deal with in this thesis tend themselves to be very sweeping and wide-scope - or at the very least, they typically don't themselves give any reasons why they wouldn't be sweepingly wide in their scope. That being so, I need to be sweeping and wide-scope also, if I'm to chase these views down and hold them to account. Doing so necessarily implies also that I can only be brief and somewhat explanatory in each particular aspect of the large terrain that I canvass.

One of my thesis supervisors shared with me an anecdote from a contemporary of his at graduate school at Oxford University[4]. Apparently, David Owen used to say there are two sorts of thesis: those that tackle an unsatisfyingly narrow topic and consequently *might* advance it somewhat, and those that tackle a satisfyingly wide topic and consequently *won't* advance it. Regardless of the truth of Owen's claim, my thesis is surely an instance of the latter in terms of scope. I am confident that this thesis does advance various discussions of importance, but in this thesis I argue that a philosopher ought to be somewhat sceptical about relying on their own intuitions as sufficient evidence for any given claim. Given this caution, the reader will have to judge whether or not this thesis escapes the fate stated in 'Owen's dictum'.

Chapter summary

My aim in this thesis is to take Williamson's comments about naturalism seriously, looking beneath naturalism's "slick packaging" (2014 : 29), outlining the ways in which

[4] A. Moore, Personal communication, June 2017

philosophy could usefully be aligned with science, and illustrating some of the many ways in which it already is. There is much more work to do in terms of applying relevant methods from the sciences within philosophy, and this motivates important and exciting work.

Chapter Two

A taxonomy of naturalisms: methodological, ontological, and epistemological

Chapter abstract

As discussed in my introductory chapter, naturalism is a name that has been given to a number of different positions within the discipline of analytic philosophy. Naturalism, for my purposes, is to do with philosophy and its relationship to science. This thesis critically examines and defends a kind of *methodological naturalism*, a view to do with the relationship between philosophical methods and the sciences. There are two other broad kinds of naturalism which I discuss here: *ontological naturalism* - which involve scientifically informed metaphysical views, and *epistemological naturalism* - involving views about knowledge that are informed or shaped by the sciences. After outlining these views, I briefly examine the relationships among these three kinds of naturalism. In the final chapter of this thesis, I return to the relationships among methodological, epistemological, and ontological naturalism. I discuss how the methodological naturalism which I defend motivates, places constraints on, or otherwise affects, epistemological and metaphysical concerns (see §5.4, this thesis).

§2.1 Motivations for providing a taxonomy

David Papineau has commented that "'naturalism' has no very precise meaning in contemporary philosophy", and that "[d]ifferent contemporary philosophers interpret 'naturalism' differently" (Papineau 2010 : 1). He argues further that getting into

definitional issues about naturalism is unfruitful (ibid.). Whilst I agree with Papineau that there are many different views that could be placed under the umbrella of naturalism, each with various considerations at play, I disagree with his claim that getting into definitional debates about naturalism is unfruitful - in fact, I think a careful look at the details of different naturalisms could be most beneficial, by first drawing conceptual lines in the sand to help delineate different naturalisms, and subsequently to clarify one's own positive view, and compare this view with other naturalisms. I do not attempt to give an ultimate description of one true naturalism, but instead I introduce a number of positions, and critically discuss them.

Some brands of naturalism have a strong normative component, prescribing views about the practice of science and philosophy. The next chapter directly engages with arguments of this kind, and carefully analyses claims that philosophical methods *ought* to be reformed, in order to best implement methodological naturalism. More precise definitions of naturalism will help to analyse such arguments.

Barry Stroud (1996) has commented that naturalism is a vague term, and that there is no agreed usage for the term. Stroud goes on to make a witty analogy, comparing naturalism to the idea of world peace. He argues that both naturalism and world peace are things that many would declare to be in favour of, but when pressed on the finer details of how they are to be realised, vast disagreements ensue about how best to implement such an ideology, or even what these ideologies are precisely (43). As a naive pacifist, I once conversed with a friend and peace and conflict studies academic, and argued that war and peace were mutually exclusive. It was pointed out to me that there is empirical evidence to suggest that war is sometimes the least harmful way to achieve peace[5]. We both agreed that world peace was important, but the view that I naively held as a means to best implement this shared ideology was radically different to hers. Luckily, purely academic philosophical warfare is much cheaper and involves much less loss of life than military endeavours, and as philosophers of naturalism we can ponder the best ways to interpret, and to implement naturalism from our armchairs and laboratories, and leave the world-peace debate to some other sub-set of

[5] L. Quinger, Personal correspondence, 2012.

philosophers who wish to engage with those ethical domains. For now, I leave myself with the more modest but still important job of discussing ways to best interpret and implement naturalism.

Owen Flanagan jokes that he has found more kinds of naturalism than Paul Simon had ways to leave his lover (2006 : 431). He presents fifteen different slogans, which he considers to be key components of various kinds of naturalism:

1. Philosophy should 'respect', 'be informed by', 'whole-heartedly accept' the methods and claims of science.
2. When a well-grounded philosophical claim and an equally well-grounded scientific claim are inconsistent (whatever 'equally well-grounded means), the scientific claim trumps.
3. Philosophical questions are not distinct from scientific questions - they differ, if they do differ, only in level of generality.
4. Both science and philosophy are licensed only to describe and explain the way things are.
5. Both philosophy and science are, in addition to the business of description and explanation, in the business of giving naturalistic justifications for epistemic and ethical ideals and norms.
6. There is no room, or need, for the invocation of immaterial agents or forces or causes in describing or accounting for things.
7. Mathematics and logic can be understood without invoking a Platonic (non-naturalistic) ontology.
8. Ethics can be done without invoking theological or Platonic foundations. Ethical norms, values, and virtues can be defended naturalistically.
9. Naturalism is another name for materialism or physicalism; what there is, and all there is, is whatever physics says there is.
10. Naturalism is a form of non-reductive physicalism; there are genuine levels of nature above the elemental level.
11. Naturalism is a thesis that rejects both physicalism and materialism; there are natural but 'non-physical' properties, e.g. informational states.

12. Naturalism claims that most knowledge is a posteriori.
13. Naturalism is indifferent to claims about whether knowledge is a priori or a posteriori, so long as whatever kind of knowledge exists can be explained, as it were, naturalistically.
14. Naturalism is, first and foremost, an ontological thesis that tells us about everything that there is.
15. Naturalism is, first and foremost, an epistemic thesis, which explains, among other things, why we should make pronouncements about 'everything there is'.

(430-431)

Whilst Flanagan lays out a number of slogans that have been associated with or pronounced by naturalists over the years, his article does not yet do much in the way of clearly organising naturalisms into kinds. As can be seen from these fifteen pronouncements of naturalism, there is a wide variety of views that make claim to fall under the umbrella of naturalism.

Timothy Williamson has argued that the term naturalism is vague in ways that are damaging (Williamson 2016, Feb 11). He suggests that the normative force behind many contemporary naturalistic views is strong, but that the unclarity of naturalism makes the details of these views difficult to analyse. Elsewhere, he accuses some naturalists of having "oscillating" views, suggesting such naturalists are involved in "dogmatic equivocation" (2014 : 30).

I agree with Papineau, Stroud, and Flanagan, that definitions of naturalism are unclear. I further agree with Williamson that this unclarity can be dangerous. For this reason, I think a more careful taxonomy is required for more fruitful discussions about naturalism. In the rest of this chapter I will lay out and critically discuss a number of distinct naturalistic positions, as held by various philosophers within the literature. I hope that with some of these distinctions in place, future discussions focussed towards naturalism can be more clearly defined and hence more easily understood. I take this as a positive part of my thesis, in that it responds to the calls of Stroud, Williamson,

Papineau, and Flanagan, by providing a more comprehensive taxonomy than exists elsewhere.

I distinguish three broad kinds of naturalism that are already somewhat delineated in the literature: methodological, ontological, and epistemological. This thesis takes methodological naturalism as its main concern, but it is worthwhile to look at other kinds of naturalism for several reasons. It is useful to separate naturalisms into these kinds for reasons of clarity, for reasons discussed above, and to refine the scope of my particular interest in this thesis: *methodological naturalism*. Moreover, it is interesting to look at the relationships among the various naturalisms and see whether they are complementary, as sometimes assumed (e.g. Forrest 2000), or whether certain interpretations of naturalism might be in conflict with one another (e.g. Boudry et al. 2012).

§2.2 Methodological Naturalism

Methodological naturalism, as I shall understand the term, names a family of views to do with philosophical practice and its relationship to scientific practices[6]. I show two different ways in which philosophical practice[7] could align with the sciences, which provides a distinction between *results-naturalism* and *methods-naturalism*.

[6] There are groups of views within the philosophy of religion that share the name "methodological naturalism", claiming something about the lack of a need for invocation of supernatural agents. These views are somewhat related, but are importantly different to the methodological naturalisms in which my thesis is interested, although they are somewhat relevant to discussions of ontological naturalism. These kinds of 'methodological naturalism' are to do with the relationship between science and religion, whereas the kinds I am interested in are to do with the relationship between science and philosophy.
[7] I often talk about 'philosophical practice' in the singular, but of course there are really a number of philosophical practices. I intend the term to include the various philosophical practices in general, unless I specify otherwise.

Results/methods distinction

The first distinction within methodological naturalisms that I will discuss is between the *results* and the *methods* of science. Brian Leiter (1998) suggests two ways in which philosophical methods can be aligned with science, either by way of employing or emulating the methods of science directly, or by making use of the products of scientific methods as a component of philosophical methods - i.e. bringing scientific results to bear upon philosophical work. I outline each of these axes of alignment with science, and provide examples to illustrate their implementation.

Methods-alignment

One way of realising methodological naturalism, is by way of aligning philosophical *methods* with scientific methods. Recent philosophical literature encompasses a developing trend in a branch of philosophy called *experimental philosophy* (for the classic presentation, see Weinberg et al. 2001). Experimental philosophers express scepticism about the epistemic credibility of using one's intuitions as evidence to support conclusions about 'folk' beliefs. In light of this scepticism, experimental philosophers instead suggest that we begin with predictions of folk beliefs, and test them using experiments, usually in the form of surveys and polls, to gather data to support findings about what it is that the 'folk' really do believe. Experimental philosophy is an example of a philosophical method emulating the methods of psychology, by way of testing individuals in a fashion that is relevantly analogous to a psychological examination. Thus, experimental philosophy aligns itself with methods from the sciences, and especially psychology, and so is an instance of methods-alignment naturalism.

Results-alignment

A different way of aligning the methods of philosophy with science, is by utilising

scientific *results* as part of one's philosophical method. An example of this can be found In the debate between A-theorists and B-theorists in the philosophy of time literature (e.g. Sklar, 1976, 1985; Balashov and Janssen, 2003). A-theorists believe there is an ontological distinction between the present, the past, and the future. B-theorists, on the other hand, think there is no unique ontological distinction between any times. Thus, B theorists think that all times are equally "real".

In some of these debates, it has been argued that there are contradictions between consequences of Einstein's special theory of relativity[8] and consequences of the A-theory of time. Many B-theorists argue that this purported inconsistency between the A-theory and the special theory of relativity lends weight to the truth of the B-theory, thus bringing scientific results to bear on philosophical debates. An argument structure for such a view is illustrated below:

P1) Either the A-theory or the B-theory is the correct metaphysical theory of time
P2) The A-theory is inconsistent with the STR, and the B-theory is consistent with the STR
P3) We should prefer theories which are consistent with well-established scientific results to ones that are not
∴ On these grounds, we should prefer the B-theory of time

Results-alignment is not a necessary condition for methodological naturalism

Results-alignment is one way a philosophical theory can do what methodological naturalism recommends, as I have shown above. However, it is not the case that all philosophical theories have to draw upon scientific results to be considered instances of what methodological naturalism endorses. We don't expect physicists to draw upon the results of other sciences, for them to be considered as using naturalistic methods. I suggest it would be unreasonably harsh to impose a constraint on philosophical methods that is not required by the sciences themselves, in order that they be considered suitable

[8] Henceforth: STR

by methodologically naturalistic standards. It can't be a requirement that all philosophy must engage with some result from science at all times, in order to exhibit the recommendations of methodological naturalism. However, when scientific results are relevant to a particular philosophical debate, it would be careless to pay them no attention. Aspiring to utilise the results of the sciences when they are relevant to philosophy is to be encouraged as a methodological heuristic. Philosophy can implement methodological naturalism by either using scientific methods directly, or by drawing upon the products of those methods.

Distinctions within sciences and their impacts on methodological naturalism

We can make still finer distinctions between kinds of methodological naturalisms, by focusing on the methods or results of particular sciences. With such distinctions in mind, some commentators propose a distinction between natural and social science, or between experimental and historical sciences. Some naturalists have tied their naturalist masts to the "natural" or "experimental" sciences (e.g. Rosenberg 2014, Ladyman and Ross 2007), claiming that there is some primacy or greater epistemic credit belonging to these particular areas of scientific inquiry, that makes them more appropriate for drawing upon for methodological purposes. I will not develop these views further presently, but in chapter three of this thesis I argue that such restrictions are untenable from the naturalistic starting point, as they rely on intuitions about science that have no evidential support from observing the sciences themselves.

Reformist and vindicatory naturalisms

As well as descriptive views about the methods the sciences and philosophy use, there are also normative views about what these methods *should* be. Some methodological naturalists argue that philosophical methods need to be changed, in order to become suitably naturalistic (e.g. Rosenberg 2014). Others argue that particular domains of inquiry are epistemically inaccessible using naturalistic methods, and think that these

domains of inquiry should be abandoned as a result (e.g. Ladyman and Ross 2007). Reformist naturalists often argue that if naturalistic methods are not appropriate for a domain of inquiry, no set of methods will be (e.g. Rosenberg 2014, Ladyman and Ross 2007), and therefore that there is something dubious about these domains of inquiry. Those who argue for either changes of philosophical method, or abandonment of areas of philosophical inquiry due to a perceived inapplicability of naturalistic methods, I call *reformist methodological naturalisms*, due to their suggesting that philosophy is in need of reform for purportedly naturalistic methodological reasons.

Others disagree that philosophical methods need to be reformed in light of naturalistic critiques, yet still take the naturalistic starting point seriously (e.g. Papineau 2014, Williamson 2014). Views of this kind I call *vindicatory methodological naturalisms*. This thesis mostly sides with the vindicatory methodological naturalists, in that it makes no call for wholesale methodological overhauls, nor abandonment of domains of inquiry, within philosophical practice. However, neither does it write the philosopher a blank methodological cheque. Throughout the thesis I develop some methodological heuristics, and these are presented collectively in §5.3 of this thesis.

In chapter three, I present and critically discuss what I take to be the most common argument for reformist naturalism, which often points to a perceived lack of progress in philosophy, when compared to science. For now, I have sketched various methodological naturalisms: methods-naturalism, results-naturalism, reformist naturalism, and vindicatory naturalism. These distinctions will be returned to throughout the remainder of the thesis, as I motivate aspirational naturalism, which involves methods-naturalism, aspires towards results-naturalism, and sides mostly with the vindicatory naturalists in terms of the appropriateness of philosophical practice as it stands. For the rest of this chapter I sketch some other views, *epistemological naturalism*, and *ontological naturalism*, in order to clarify the discussions of my preferred methodological naturalism, and its relationship with these substantive naturalistic perspectives that are relevant throughout this thesis.

§2.3 Substantive naturalisms

With distinctions within methodological naturalism in place, I look at two other kinds of naturalism found in the philosophical literature: *ontological* and *epistemological*, and briefly explore the relationships they each have with methodological naturalism and with each other.

Ontological naturalism

Ontological naturalisms are substantive meta-metaphysical views, drawing inspiration from science. Ontology is the study of what there is, or more simply: existence. To claim that birds exist is to include birds in one's ontology. There are a number of nuanced variations of ontological naturalism that I will discuss here. I begin this section with several pronouncements of what ontological naturalism has been taken to mean by various philosophers, to give a flavour of the position, before discussing some more specific views throughout the remainder of this section:

> Ontological S[ubstantive] naturalism is the view that there exist only *natural* or *physical* things.
>
> (Leiter 1998 : 80)

> I define [naturalism] as the doctrine that reality consists of nothing but a single all-embracing spatio-temporal system.
>
> (Armstrong 1978 : 261)

> A central thought in ontological naturalism is that all spatiotemporal entities must be identical to or metaphysically constituted by physical entities. [...] They hold that there is nothing more to the mental, biological and social realms than arrangements of physical entities.
>
> (Papineau 2016, Winter edition)

> Naturalism is the realist ontology that recognizes only those objects required by the explanations of the natural sciences.
>
> (Linsky and Zalta 1995 : 1)

Only the entities posited by the sciences exist

A common view of ontological naturalism is that ontology can be somehow read off our best science. If one subscribed to such a view, one can countenance such things as quarks, electrons, waves, and fields, via the ontology of physics. Other sciences provide their own ontologies, e.g. chemistry contains chemical elements, biology contains species and organisms, mathematics contains sets and categories. Depending on which sciences the naturalist takes ontologically seriously, different entities may be posited. As was the case with methodological naturalism, some ontological naturalisms argue that particular sciences are to be favoured when it comes to claims of existence, often placing physics in the box seat, perhaps seeing quarks, or strings, as the fundamental constituents of reality (e.g. Rosenberg 2014). Less austere ontological naturalisms could also countenance such things as psychological illnesses, debt crises, child poverty, and global warming, to name only a few of the myriad possibilities.

E**ntities that are required to make sense of the sciences exist**

Some think that there are existing entities beyond the ones the sciences have currently posited, and evidence for their existence is connected to scientific ontologies in some way. Such naturalists might countenance entities that they claim are either necessary for, assumptions of, consequences of, or otherwise indispensable to, the sciences.

Linsky and Zalta express such a view:

> Naturalism is the realist ontology that recognizes only those objects required by the explanations of the natural sciences. But some abstract objects, such as

> mathematical objects and properties, are required for the proper philosophical account of scientific theories and scientific laws.
>
> (Linsky and Zalta 1995 : 1)

Many mathematical realists, those who consider that mathematical objects exist, do so based on *indispensability arguments* (e.g. Quine (1976; 1980a; 1980b; 1981a; 1981b), Putnam, (1979a; 1979b), and Colyvan (1998, 2001)). Evidence is provided to support positing the existence of mathematical objects due to claims that mathematics is indispensable to science, and that a realist ontology of mathematics underpins this. There is a variety of mathematical-realist positions, ranging from those of Quine, Putnam, and Colyvan, who posit mathematical objects as abstracta, to those placing them in space and time (e.g. Armstrong 1989, Bigelow 1988). All of these kinds of mathematical realism could claim to be endorsed by ontological naturalism in the sense that their ontologies are purportedly necessary to make sense of the sciences.

Instances of the theoretically based positing of entities occur in the sciences, very commonly within physics. The Higgs Boson, initially suggested as a consequence of the particle physics theory in 1964 (Higgs 1964), is an example of physics postulating the existence of an entity, which at that stage was not amenable to empirical enquiry, for theoretical reasons. It was only recently, in 2012, that a series of experiments have lent observational evidence for the existence of the Higgs Boson (CMS collaboration 2012). It was initially postulated due to its being a theoretical consequence of some of our best current science. So, positing the existence of theoretical entities, or entities that are not yet known by observation, is something that happens within the sciences themselves, and is not a special conjuring trick used merely by philosophers. The positing of unobservables does not, in and of itself, require non-naturalistic ontological commitments.

Naturalistic nominalisms

Another group of metaphysical views which sometimes claim support from naturalistic constraints are labelled nominalisms. Nominalists, as opposed to realists, hold that the entities for which they are nominalists about, do not exist. They are often called anti-realists. Oftentimes such views involve arguing that the entities in question are unavailable to naturalistic inquiry, and hence we should conclude that the entities they posit do not exist.

Whereas the mathematical realists discussed in the previous section argued that the sciences require an ontology of mathematical objects for a successful interpretation of the sciences, nominalists about mathematical objects argue that the sciences do not require such ontologies. Hartry Field (2016) argues for nominalism about mathematical objects, demonstrating that a small part of physical theory can be interpreted without an assumption of mathematical objects such as sets, or categories. It is not my intention here to adjudicate between realists and anti-realists in any of these debates, but it is important to note that both can claim to align their ontologies with findings from science, and hence have some endorsement for their views via naturalistic concerns. Nominalists and realists might agree on some indispensability of mathematics thesis, but disagree about the ontological commitments, if any, that such indispensability requires.

Scientifically informed constraints on ontology

David Armstrong endorses a kind of ontological naturalism based on spatio-temporality, arguing that everything that exists, does so in space and time (1978 : 261). He claims to draw inspiration from science for this view, arguing that the sciences are only concerned with entities that exist in space and time, and that philosophers should adopt the same constraints for positing their ontologies. For these reasons, he disagrees with the naturalist-mathematical-platonists discussed above, due to their positing of abstract entities supposedly floating free from the spatio-temporal world.

One peculiarity of Armstrong's view, is that he doesn't have many other scientifically informed constraints on the types of entities that he can posit, apart from spatio-temporality. For example, as a proposed solution to the problem of universals in metaphysics, Armstrong posits immanent universals, existing as non-spatio temporal parts of objects, yet still existing within space and time (1989). Whilst intuitively, immanent universals such as these seem atypical kinds of things to study using typically scientific methods, they meet his spatio-temporal constraints, and he claims the constraint is justified by science. This seems tricky at best. These entities meet Armstrong's condition because they exist in space and time just in virtue of being non-spatio-temporal parts of entities that do (*really* do, one is tempted to say) exist in space and time.

Leaving these specific details of Armstrong's ontologies aside, there is a further problem with his overall spatio-temporality constraint. Despite claiming to draw this ontological constraint from the sciences, it doesn't seem in keeping with contemporary scientific views, as Ladyman and Ross point out:

> [C]ontemporary physics takes very seriously the idea that spacetime itself is emergent from some more fundamental structure.
>
> (Ladyman and Ross 2007 : 23)

Ladyman and Ross's point is that this fundamental structure therefore cannot itself exist in space or time, and so does not meet Armstrong's condition. On the basis of this consideration, Armstrong's view is not a plausible view of ontological naturalism as it seems to have adopted *a priori* constraints on ontology that are at conflict with currently accepted science, or at least with what current science "takes very seriously" (Ladyman and Ross 2007 : 23). Such a view is not justifiable from the naturalistic starting point.

Suppose however, there were some constraints that science did support about existence. Were that the case, an ontological naturalist could limit their domain of ontology accordingly.

An example of this occurs within David Lewis' (1986a) arguments against Armstrong's *structural universals*. Lewis points out that consequences of David Armstrong's views entail that the properties of 'being Methane' and 'being Butane' are identical on Armstrong's account. Whatever one's metaphysics of properties is, it should be able to account for differences in properties that are detectable to the sciences. Chemists can discern the difference between what it is to be methane and to be butane - they are different chemical properties. On those grounds, David Armstrong's view is dismissed by Lewis. Properties are supposed to explain resemblance between various entities. If Armstrong's view cannot explain what it is that makes different molecules of butane resemble each other in a way that differentiates butane from methane, it cannot be a thorough account of the properties of chemistry.

Supervenience naturalism

Supervenience of properties describes a necessary relation between two sets of properties, whereby there *cannot* be a difference in one set, without there being a difference in the other. There seems to be a supervenience relation between the amount of beer I consume, and my body mass, for instance.

Supervenience naturalism is a kind of ontological naturalism. It requires of its ontology that it commit only to entities that supervene upon the natural world, the area which the naturalist claims is best explored by science. I will illustrate the idea using a view of David Lewis's, inspired by a framework of Hume's, which he calls *Humean supervenience* (1986b ; 1994). Lewis presents his view as follows:

> [A]ll there is to the world is a vast mosaic of local matters of particular fact, all else supervenes on that.

(1986b, *ix-x*)

Presumably, Lewis takes "local matters of particular fact" to mean something like synthetic facts, i.e. facts about the world, the kinds of things that the sciences investigate and have generated knowledge about. Lewis's idea is that necessarily, there's no difference of any other entity without some difference of 'local mosaic' entity.

So, for a supervenience naturalist, the entities that are posited do not have to be directly amenable to scientific inquiry *per se*, it merely has to be argued that they supervene upon the kinds of things that are amenable to scientific inquiry.

Reduction naturalism

Reduction naturalists are a particular kind of supervenience naturalist, who claim that all entities in their ontologies can be reduced to something natural. Reduction is a kind of supervenience relation, whereby one set of properties is reduced to some other set of properties. The bare minimum for a supervenience claim is that there is a difference in the supervening set of properties only if there's a difference in the base set of properties. Identity is one kind of reduction. If we hold a reductionist view towards two groups of properties A and B, suggesting that A and B are identical, or that B is somehow composed of or otherwise reduced to A, a change in A would necessarily entail a change in B, and *vice-versa*. In the philosophy of mind literature, there are various views that argue for the mind supervening on the natural world. An instance of this view is the mind-brain identity theory (see Smart 2017), which holds that the mind and the brain are one and the same. Thus, the mind is reduced to something amenable to the sciences, i.e. the brain. Eliminativist reductionists (e.g. Chuchland 1981) argue that such an identity relation is somehow asymmetrical, in that the mind is *really* some bit of the brain, but it's not the case that this bit of the brain is *really* the mind.

Ontological reduction and explanatory reduction

An important distinction that helps to clarify some of these reductionist views is that between *ontological reduction* and *explanatory reduction*.

Reductionists about ontology make claims about existence, reducing a phenomenon such as 'the mind' to some more fundamental entity, such as 'the brain', for instance. One might be an ontological reductionist, but that does not entail a commitment to one's also being an explanatory reductionist. Explanatory reductionists think that not only is a particular phenomenon reducible in an ontological sense to some natural entity, but that an explanation of the more basic item which the phenomenon can purportedly be reduced to, also provides an adequate (often, better) explanation of the particular phenomenon itself. Eliminative materialists, such as Paul Churchland (1981), argue that we should eliminate explanations of the mind (e.g. psychology), and that the neurosciences will provide adequate and exclusive descriptions of the phenomena that theories of mind have entertained[9].

To explain how one might be an ontological reductionist, but not an explanatory reductionist, I will illustrate with an example from economics. Within economics, explanations are given for things such as a *financial crisis*. Whilst one might agree that all there is to the world - ontologically speaking - is combinations of quarks and electrons (or probably something more fundamental still, perhaps parts or strings - or whatever the ontology some future fundamental physics provides us with), they may not agree that a description or explanation of these fundamental entities will adequately explain phenomena such as financial crises. It seems obvious, at least presently, that a quantum mechanical theory could not provide an explanation of a financial crisis in a way that is superior to economics.

[9] Sometimes I wonder whether Paul or Mary Churchland tell the other they love them, given that love is a folk psychological concept that they think doesn't really refer. Elimination of entities is one thing. Elimination of the associated self-indulgence is another.

One could provide a complete physical description, by pointing out that a financial crisis is a conjunction of physical things: people (biological individuals) and their psychological states (brain states), economic policy (pieces of paper, ink, computers) implemented by way of democratic process (utterances (sound-waves) of people and their physical movements, more paper, more ink, computers), and the psychological states (brain states) and physical behaviours of financial institutions (physically constituted by groups of people, buildings made of various matter, money (pieces of paper, and metal), utterances (more sound waves), and behaviours of groups of people (physical motions of individuals, more soundwaves). An ontological reductionist could attempt to reduce the various entities that I included in the parenthesis to those posited by a quantum mechanical theory, describing the entirety of the constituents of a financial crisis in this way, but refuse to countenance that a financial crisis could be *explanatorily* reduced via a quantum mechanical definition of its physical constituents. I suggest that the following beginnings of a definition of a financial crisis, serve pretty well as an explanation of a debt crisis, without warranting explanatory reduction to quantum mechanics.[10]

> We begin by developing working definitions of what constitutes a financial crisis, as well as the methods—[...] the boundaries drawn are generally consistent with the existing empirical economics literature, which by and large is segmented across the various types of crises considered (e.g., sovereign debt, exchange rate, etc.). Two approaches are used to identify crisis episodes. One, which can be applied to inflation and exchange rates crises, is quantitative in nature, while the other, which we apply to debt and banking crises, is based on a chronology of events.
>
> (Reinhart and Rogoff : 1677-8)

[10] One might rightly insist that economics is a science, and that as such this is already explanatorily naturalistic. However, my intention was merely to show the distinction between explanatory and ontological reduction, and my example succeeds in illustrating this distinction.

Views about explanatory reduction are not ontological views, they are epistemological views. This distinction is discussed here in order to clarify what it is that ontological reduction involves, and what it does not.

Structural realism

In this section I discuss a group of views that could be described as *structural realisms*. The locus classicus of this view is found in John Worrall's 'Structural Realism: the best of both worlds?' (1989). Worrall argues that his position can accommodate the best arguments for and against traditional forms of scientific realism. The two arguments in question are Larry Laudan's pessimistic meta-induction, an argument for scientific anti-realism (see Laudan 1981), and the no-miracles argument for scientific realism (see Putnam 1975, and Musgrave 1988).

Laudan's (1981) argument draws upon instances of theory-change in the sciences. He argues that the history of science shows us that we often overturn widely accepted scientific views, and replace them with new ones. Examples of this can be found via the various scientific revolutions, including the Copernican revolution, involving the shift in acceptance of the Ptolemaic geocentric model of astronomy to the Copernican heliocentric model, and the chemical revolution, which involved a shift of acceptance of the phlogiston theory of combustion, to Lavoisier's oxygen theory of combustion, widely regarded as being the beginnings of modern chemistry.

Laudan argues that the overturning of previously accepted scientific theories is rife throughout the history of science, and by induction, that we should expect this process to continue. He argues that this provides observational evidence that our current scientific theories are quite likely false, and that future ones will likely be also. Thus, the pessimistic meta-induction is the view that we ought to be pessimistic with regards to our prospects of science obtaining truths, or converging to the truth. As such, we ought to be sceptical of the posits of ontologies of scientific theories, such as electrons,

or oxygen, as we will likely find the theories in which these ontologies are embedded within to be false.

The no-miracles argument for scientific realism suggests that as our scientific theories have generated such a high degree of predictive success, it would be a miracle if none of them genuinely referred to entities to which those theories are committed. Given the predictive success of physics, proponents of the no-miracles argument might argue that it would be a miracle if there were no such things as electrons, say. The entities that a proponent of the no-miracles argument is committed to is somewhat up for grabs. A useful discussion of the argument, and the issue of which ontologies one might be a realist about is found in Musgrave (1992), but for now I will present the structural realist view in sketch form.

John Worrall draws upon instances of theory change in science, and argues that even during radical scientific theory change, structural features of the theories can stay intact. For instance, when the chemical revolution took place, what Lavoisier proposed as oxygen, might be considered structurally analogous to what Priestly, a proponent of the superseded phlogiston theory, had called dephlogistonated air.

Worrall agrees with Laudan, that science will likely continue to experience theory change, and that we are right to be pessimistic about the various entities that our currently accepted theories commit us to. However, he also agrees with the no-miracles argument, at least in terms of claiming that the predictive success of our theories, combined with the shared structure that can be found between accepted and overturned scientific theories, means it would be a miracle if our theories hadn't uncovered truths about the *structure* of the natural world. He thus thinks that we ought to be realists with regards to the structure represented by some scientific theories. Contemporary defences of structural realism include Ladyman and Ross's *Ontic-Structural Realism* (2007). A relevantly similar view is Daniel Dennett's *Real Patterns* (1991), where Dennett argues we should be realists about patterns, rather than objects or entities. Earlier expressions of similar views include Poincare (1905, 1906), and Bertrand Russell's *The Analysis of Matter* (1927). A comprehensive historical survey of structural realisms can be found

in Gower (2000). For criticism of the no-miracles argument, and structural realism, see Hoyningen-Huene (2011).

Perspectival realism

Another group of ontological views motivated by the sciences are what I will call *perspectival realisms*. Views of this kind take an egalitarian approach, at least as a principled starting point, towards the various sciences, and argue that there are a number of unique ways of accurately describing aspects of the world that give rise to various scientific frameworks from which to motivate ontological conclusions. These views are usually influenced by a pluralistic understanding of the sciences, and maintain that observing the sciences indicates that there are different scientific theories, models, methodologies, or perspectives, through which we can describe the world, and that no one of these should be considered more fundamental than any other, unless we have strong evidence to think otherwise.

On this approach, one could be a realist about electrons, say, from the perspective of physics, and might also be a realist about organisms, from the perspective of biology, without committing to some kind of supervenience or reduction view about biological organisms and their relationship with the ontologies of physics. Importantly, perspectival realism denies fundamentalism. Fundamentalism was outlined in chapter one, section *x* of this thesis, and I will restate the first premise from Kellert et al's (2006) presentation of the argument for monism to remind the reader.

> [T]he ultimate aim of a science is to establish a single, complete, and comprehensive account of the natural world (or the part of the world investigated by the science) based on a single set of fundamental principles[.]
>
> (*x*)

Denying fundamentalism implies that different scientific systems of practice, or perspectives, focus on different objects of inquiry. On this view, the reductionist and

supervenience claims that were discussed in the previous chapter are less plausible *tout court*. Unless we have strong evidence that a particular ontology can be reduced to something more basic, which has happened on occasion in the sciences (famously with Einstein's (1945) reduction of Newtonian mechanics), we should not assume that there is a hierarchy of sciences, with fundamental physics at the bottom level of reality. Whilst theoretical unification might be promoted as a positive goal for various sciences, or for science as a whole, it should be considered an open question whether the world is such that such a goal could be achieved, or that the human enterprise of science could achieve such a goal, given the limited nature of our cognitive structures, even if it were possible in principle. At present, the sciences are radically disunified. The only justification for thinking they will turn out otherwise seems to stem from *a priori* metaphysical assumptions to the effect that the world is such that it an accurate description of it would turn out to cohere with this fundamentalist picture, and epistemological speculations about what kinds of knowledge the sciences will be able to produce[11].

Ronald Giere (2006) argues that the natural world is incredibly complex, and that a unified theory of everything via one unified model would be remarkably difficult to achieve. He also argues that various scientific observations are performed using different scientific instruments, and that as such, observational data should be considered relative to those particular instruments.

> To say that scientific observation is perspectival relativizes observations to the perspective of the relevant instrument. There is no such thing, for example, as the way the Milky Way looks. There is only the way it looks to such and such instrument.
>
> (2006 : 30)

[11] The reader is directed to appendix 1, which contains a comic-illustration of the notion of fundamentalism. The illustration was created by a close friend after discussing some of these ideas. It is included here to add colour, but is not to be taken as an argumentative device.

Views of this kind are becoming more popular within contemporary philosophy of science literature, and other related examples are found in Hasok Chang's "active realism" (2012), Michaela Massimi's "scientific perspectivism" (2012), John Dupre's "promiscuous realism" (1996), and Nancy Cartwright's view of a "dappled world" (1999). I come back to develop a similar view of my own in §5.4 of this thesis, which I call *aspectival realism*.

Summary of ontological naturalisms

In the first part of this section, I have spent time taxonomising a number of distinct views that might reasonably be considered instances of ontological naturalism - various ontological views that take the sciences seriously. As can be seen, different commentators have drawn significantly different metaphysical conclusions, despite all claiming to ground their ontologies in a scientifically respectable way. The next section discusses epistemological naturalisms. These are views about knowledge, that are inspired by an appreciation for the epistemic success of the sciences.

Epistemological naturalisms

For the rest of this section, I will outline a number of distinct positions within a group of views that might be broadly labelled as epistemological naturalisms. Epistemology is the study of knowledge. Epistemological naturalisms are views about knowledge that align themselves with the sciences. Most epistemologically naturalistic views incorporate some elements of the naturalistic starting point which I outlined in the introductory chapter, such as treating the sciences with epistemic respect. I begin with several quotations that outline some of the variety of views that have been put forward under this banner, before spending some time characterising useful distinctions within this broader range of views.

> Naturalism is the philosophical theory that treats science as our most reliable source of knowledge and scientific method as the most effective route to knowledge.
>
> (Rosenberg 2014 : 32)

> [T]here is only one reliable method of reaching the truth about the nature of things ... this reliable method comes to full fruition in the methods of science
>
> (Hook 1965 : 183)

> [W]e take naturalism to be the view that only natural science deserves full and unqualified credence.
>
> (Wagner & Warner 1993 : 1)

> [N]aturalism [is] … the claim that the methods and techniques of natural science are **the** source of knowledge about the world.
>
> (Hylton 1994 : 261, **emphasis** mine)

Degrees of strength

From the quotations presented above, it is clear that epistemological naturalisms come in varying degrees of strength. A modest epistemological naturalism might claim that the sciences instantiate plausible routes to knowledge. I take it that this should be a minimal and uncontentious constraint on epistemological naturalisms. Stronger epistemological naturalisms may claim that science is our most reliable route to truth (Rosenberg 2014, Hook 1965), that it deserves unqualified credence that other areas of inquiry do not (Wagner & Warner 1993), or stronger still, that it is our only source of genuine knowledge about the world (Hylton 1994).

Domain specificity

Some epistemological naturalists favour particular sciences over others, and afford them more epistemic credibility, arguing that the knowledge these domains generate is more secure than the conjectures offered by other domains. The epistemological naturalist makes claims about how philosophical, psychological, economic, etc, inquiry does or[12] *ought* to proceed as knowledge-seeking practices.

Alex Rosenberg suggests that the "natural" sciences (usually taken to refer to at least physics, and usually chemistry and biology) provide us with our most secure claims to knowledge, and that their tests of knowledge are:

> the experimental/observational methods all the natural sciences share, the social sciences increasingly adopt, and that naturalists devote themselves to making specific.
>
> (Rosenberg 2014 : 33)

I will argue in §3.4 why I think domain specific epistemological naturalisms are difficult positions to maintain. In the next chapter I will argue for methodological pluralism about the sciences, arguing, *pace* Rosenberg, that there is no secure set of "experimental/observational methods [that] all the natural sciences share" (Rosenberg 2014 : 33), there are rather a number of different systems of practice used by different sciences, and even within singular sciences - including the natural sciences - and that many of these can be viewed as epistemically successful in their domains. Whether a particular system of practice can be applied to a particular domain of inquiry is a domain specific matter, and we should not assume that one set of methods will work in any one area. Here I mean to raise doubt about whether one method can work in *all* areas of inquiry (e.g. in both ethics and also mathematics). Particular scientific methods might be usefully implemented within some domains of inquiry, but may not be effective for others.

[12] This disjunction is intended to be interpreted inclusively.

I certainly do not think it is a bad idea to attempt to use observational or experimental methods within philosophy, and I encourage philosophers to apply these methods where possible. However, I note that the sciences are not unified in their methods presently, neither between sciences, nor within specific scientific domains (the case studies presented in §3.4 provide evidence for this claim). I see no reason why the methods of philosophy should be unified in a way that those of the sciences are not, in order that they be considered either naturalistic or sufficiently good. Some reasonably obvious challenges for these narrower domain specific naturalisms include a correct treatment of the methods of mathematics and history within such a narrow framework. Given that mathematics and history are both drawn upon with gusto within "natural" and "experimental" scientific practices[13], it is a challenge for these domain-specific naturalists to maintain that the methods of their favoured domains are both generally superior and appropriate for all areas of inquiry.

Connection to ontological naturalisms in previous sections

Epistemologically naturalistic views can be used as parts of arguments for various ontological naturalisms. One could argue for realism about electrons, for instance, with an argument along the following lines:

1) Physics posits entities including electrons
2) We have good reasons to believe that the entities posited by physics exist
∴ We have good reasons to believe that electrons exist

At other times, constraints endorsed by epistemological naturalism are used as parts of arguments for nominalism, by arguing that we should be sceptical about some realist positions due to their entities posited being inaccessible to science. Colin Cheyne's arguments for scepticism about the existence of mathematical objects, due to the causal

[13] Physics and biology use extraordinary amounts of mathematics, and the building of knowledge of any system of practice requires having a history from which to build upon.

isolation of such alleged objects from us, are instances of this kind of view (Cheyne 2001).

§2.4 Relationships amongst naturalisms

For the remainder of this chapter, I look at the relationships between the various naturalistic views outlined in previous sections. Some commentators have claimed that ontological naturalism is an assumption of methodological naturalism (e.g. Forrest 2000). Others have argued that the two are independent of one another (e.g. Williamson 2014, Boudry et. al 2012). I will argue that methodological naturalism and epistemological naturalism have a symbiotic/mutually reinforcing relationship with one another, in that with more knowledge and progress generated via the systems of practice that constitute the sciences, more credence for the epistemological naturalist is gathered. I will argue that many variants of ontological naturalism are in conflict with methodological naturalism, but in §5.4 of this thesis, I outline an ontological framework that I take to be currently best supported by naturalistic methodological and epistemological concerns.

The mutually reinforcing relationship between methodological and epistemological naturalisms

As has been mentioned throughout, this thesis assumes the naturalistic starting point, which is accepting that the sciences have generated knowledge, and that they have done so via the methods of their various systems of practice. This is really just the claim that methodological naturalism supports epistemological naturalism through repeated generations of scientific knowledge. Methodological naturalism and epistemological naturalism reinforce one another. I take it as a contingent fact about the world that the sciences have generated knowledge, and with successive generations of knowledge by the systems of practice of the sciences, the epistemologically naturalistic claim that the

sciences generate knowledge is lent more support over time. Thus, the two views can be seen as having an active and mutually supportive relationship, to the extent that the practices recommended by methodological naturalism yield knowledge. The claims that the epistemological naturalist makes, that the sciences generate knowledge, can be justified by activities that adhere to methodologically naturalistic standards. We can observe the various systems of practice at play in the sciences, and recognise the knowledge they accumulate, to support this position. This in turn can lend support to the worthiness of using these methods.

Potential conflicts regarding ontological naturalisms

Whilst I have argued that the relationship between methodological naturalism and epistemological naturalism is one of mutual reinforcement, I do not think the same holds between methodological naturalism and ontological naturalism, if ontological naturalism is defined in any substantive way.

Some commentators have argued that ontological naturalism is a metaphysical assumption of methodological naturalism:

> [m]etaphysical[14] naturalism is a constitutive ontological principle of science in that the general empirical methods of science, such as observation, measurement and experiment, and thus the very production of empirical evidence, presuppose a no-supernature principle.
>
> (Mahner 2012 : 1437)

I am unsure how to make sense of claims of this kind. I can only assume that an endorsee of a view like this means something like "we assume there is no spooky stuff out there". Others have argued along similar lines, claiming that the sciences operate assuming atheism:

[14] Mahner uses 'metaphysical naturalism' to mean the same as what I mean by 'ontological naturalism'.

> If a Watchmaker is thus carefully excluded at the beginning, we need not be surprised if no Watchmaker appears at the end. The dice have been loaded against him.
>
> (Macbeth 1974 : 126)

Macbeth is claiming that the sciences operate excluding a designer, and that therefore the sciences will not discover that there is one. I think these claims are false. The sciences have turned out to generate knowledge that tell us the world is a very bizarre place, much different to a naive commonsense understanding of it. There are many things that once might have been considered "spooky stuff", that the sciences are happy to countenance. For example, the various ontologies of quantum mechanics are very contrary to common sense. An example from the history of science is useful to illustrate this point. Einstein attacked Bohr over the principle of action at a distance in quantum mechanics, on the grounds that it was "spooky" (see Nikolic 2012, for a useful discussion). Quantum physicists today are very happy to countenance action at a distance.

It is much more plausible to think of the sciences as being agnostic, and as looking at the evidence for various hypotheses, before concluding either way on such matters. If we view the sciences in this way, we can reasonably easily show by example that the claims of Mahner and Macbeth above are misguided. Macbeth talks about excluding a watchmaker at the beginning, and if this is re-interpreted as being agnostic about a watchmaker at the beginning, as I suggest is a more plausible interpretation of a starting point of the sciences, we can point to various ontologies that the sciences were once agnostic about, but which they now take very seriously. You could more or less pick any entity from the sciences to demonstrate this point, but let's take the electron as an example. The sciences were not anti-realists about electrons at any stage, there was just not sufficient evidence for their existence until modern physics. Now that there is sufficient evidence for the existence of electrons, contemporary science counts them amongst its ontology very readily. The dice were never loaded against the electron, and neither are they loaded against the existence of a deity. If there were sufficient evidence

for the existence of a deity, the sciences should just as happily countenance it amongst its ontology.

Timothy Williamson expresses this same point as follows:

> [methodological] naturalism is not as restrictive as it sounds. For example, some of its hard-nosed advocates undertake to postulate a soul or a god, if doing so turns out to be part of the best explanation of our experience, for that would be an application of scientific method. Naturalism is not incompatible in principle with all forms of religion. In practice, however, most naturalists doubt that belief in souls or gods withstands scientific scrutiny.
>
> (Williamson 2014 : 30)

The systems of practice of the sciences have no need to make overarching metaphysical assumptions. Often sciences work with an assumed fundamental ontology, but they do not consider it fixed *a priori*, and leave it subject to revision over time. The sciences, by and large, share a commitment to discovering the true nature of the world, and I see no evidence that they begin with fixed *a priori* ontological assumptions that rule anything out by fiat. If it can be shown where sciences currently do this, it would be important to subject those assumptions to scrutiny, and treat them as fallible.

Fishman and Boudry (2013) claim that *a priori* metaphysical assumptions should not be included in accounts of methodological naturalism:

> Science, at least ideally, is committed to the pursuit of truth about the nature of reality, whatever it may be, and hence cannot exclude the existence of the supernatural *a priori*, be it on methodological or metaphysical grounds, without artificially limiting its scope and power.
>
> (2013 : 929)

I agree with Williamson and Fishman and Boudry. Ontological naturalism is not an assumption of methodological naturalism. I see no plausible way to give an account of

ontological naturalism as a substantive metaphysical view, that coheres with the views of Mahner and Macbeth. I argue that the sciences do not involve a commitment to fixed *a priori* metaphysical assumptions. Rather, the sciences often start out with revisable assumptions, and change them as new evidence comes to light that is relevant to their various metaphysical commitments.

Is ontological naturalism a consequence of methodological naturalism?

Barbara Forrest has argued that ontological naturalism - in the same sense of the term used by Macbeth and Mahner - is the only plausible consequence of practices endorsed by methodological naturalism. She describes her view as follows:

> [Ontological] naturalism is emphatically not an arbitrary philosophical preference, but rather the only reasonable metaphysical conclusion--if by reasonable one means *both* empirically grounded and logically coherent.
>
> (2000 : 7)

By ontological naturalism, Forrest seems to mean something like non-supernaturalism. As above, when I argued that ontological naturalism is not a working assumption of methodological naturalism, I find no plausible way to view it as a non-trivial conclusion of methodological naturalism either. If she means that there exist no entities that are not yet posited by the current sciences, her view is surely false, as of course the sciences are not complete and in many respects might well not be correct either. If she means that some future science will eventually settle the ontological nature of the world, it is neither informative - as who are we to know what some ideal science might settle upon, and nor is it naturalistically justifiable - as there is no means by which we could provide evidence for or against this claim. Of course there are various ontologies posited by the sciences, and on methodologically naturalistic grounds there is good evidence for them, but these cases are local, and Forrest's thesis is unworkable as a claim that is general and substantive.

Ontological naturalism is neither an assumption, nor a consequence of, methodological naturalism, in any way that understands the view to involve holding fixed metaphysical views about the nature of reality. In §5.4 of this thesis, I outline an ontological framework called *aspectival realism*, a view that I argue is well motivated by methodological naturalism and its symbiotic relationship with epistemological naturalism. This view is most closely related to the groups of views I discussed earlier in this section under the title of *perspectival realism*. For now, I hope to have shown the difficulty of justifying many kinds of ontological naturalism on methodologically naturalistic grounds.

Chapter summary

In this chapter I have discussed a number of different views that have gone under the broad label of naturalism. I have spent some time discussing three more specific kinds of naturalism: methodological, ontological, and epistemological. This has involved discussing various other specific views within these kinds. This taxonomy is surely not exhaustive, but I think it provides a cursory glance at some of the predominant views within the grab-bag of naturalistic views, that allows my discussion to proceed within these conceptual frameworks. Methodological naturalism has something of a symbiotic relationship with epistemological naturalism; the more good work that is the product of the sciences, the more evidence for the epistemic credibility of their methods is gathered. I have argued that ontological naturalism is a hard view to substantiate, in any way that makes it a robust and general metaphysical framework. However in §5.4 of this thesis, I develop a metaphysical view called *aspectival realism*. This is a view most closely related to the perspectival realisms that were discussed in section three of this chapter.

In the next chapter, I narrow my focus to methodological naturalism, and argue that the sciences exhibit methodological pluralism. This discussion begins to shape my positive view, that methodological naturalism is best understood as encouraging a plurality of

systems of practice, and that many, but not any, different methods can amount to achieving different epistemic aims across different systems of practice.

Chapter Three

Methodological naturalisms and the argument for reform

Chapter preface

In this chapter, I narrow the focus of my discussion to methodological naturalism. I begin by outlining the argument for reformist naturalism, which was introduced in the preceding chapter. The argument for reform relies on two substantial claims. One is a claim that there is a significant difference between philosophy, or parts thereof, and the sciences. There are two ways in which a relevant difference between the domains might be had, either by finding significant differences in methodological practices between domains, or by finding that there is an inapplicability of methodologically naturalistic practices within philosophy or some part thereof. The second substantial claim that the argument for reform requires, is that there is something unsatisfactory about this difference between the domains. The usual complaint regarding a purported difference is that whatever constitutes said difference is responsible, at least in some part, for a lack of progress in philosophy.

This chapter focuses on the claims of descriptive difference between philosophy and the sciences, and concludes that pinpointing a significant difference is less than straightforward due to the plurality of methods exhibited in the sciences. Further, the systems of practice in the sciences tailor their methods depending on the phenomena they are interested in, and their particular epistemic aims. It is not straightforwardly obvious how various methods might be usefully applied within philosophy. However, I discuss a number of epistemic aims that are typical of various sciences, and argue that many of them will be relevantly similar to those of various philosophical domains. The lack of clear application of suitably naturalistic methods within philosophy is not seen as an area of concern, however. In fact, it motivates a need for work in the philosophy

of science, epistemology, metaphysics, and probably most other parts of philosophy, to conduct a careful study of various philosophical domains of inquiry, and various scientific systems of practice that might be relevantly drawn upon for appropriate applications of methods. This will involve practitioners of philosophy being careful in describing the various epistemic aims of their areas of inquiry, and aspiring to implement naturalistic methods where they might be fruitful. If philosophers clearly state their aims, we can evaluate the effectiveness of their methods in terms of how well they bring about their specific epistemic aims. This will be at least a partly empirical matter, which might best be tested by implementing a plurality of methods, and observing their effectiveness in realising various epistemic aims. Whether or not it is simply an empirical matter depends on whether the full description of those methods is itself simply empirical. If the full description is itself partly evaluative (e.g. if it refers to evidence, without 'naturalising' that evaluative notion), then it's not obvious that the evaluation to be considered will be simply empirical.

Throughout this discussion, I begin to motivate and sketch the methodological naturalism that I go on to advance. The following chapter looks more carefully at the normative component of the reformist naturalist argument to do with progress. With the discussion of various epistemic aims that are motivated throughout this chapter, measuring progress is made clearer, by way of determining how effectively a philosophical domain achieves its various aims. In this chapter, I often discuss philosophy as if it were one domain, which of course it is not. Some reformist arguments are targeted at specific parts of philosophy. I discuss some of these arguments that are directed specifically at metaphysics. The reader is encouraged to substitute some part of philosophy (e.g. metaphysics, epistemology, philosophy of science, etc.) with philosophy in case there is a thought that these arguments could be applied to other specific areas. I aspire to, and don't rule out, an extension of the scope of my argument to all of analytic philosophy, but due to the nature of this thesis, I can't *demonstrate* that comprehensiveness of scope of application. This is left for further work.

§3.1 The argument for reformist naturalism

The argument for reformist-naturalism has a structure as follows:

P1) There is a significant difference between philosophy and the sciences
P2) This difference is responsible for a comparative lack of progress in philosophy
P3) Philosophy ought to do what it can in order to make more progress
∴ Philosophy ought to reform by way of eliminating this significant difference

In this chapter I am interested in the first premise of the argument for reform, and consider first whether there are significant methodological differences between philosophy and the sciences. I then go on to consider whether there is a significant difference in domain between philosophy and the sciences, such that scientific methods might be inappropriate for application in philosophy. I argue that methods exhibited in the sciences are many, and that pinpointing significant differences between scientific and philosophical domains in terms of method is not straightforward. I then argue that neither is it straightforward to pinpoint significant differences between the domains themselves, such that naturalistic methods might be considered inapplicable to philosophy, or some of its constituent parts. From these discussions, a positive account of aspirational naturalism, the view that philosophy should aspire to use appropriate naturalistic methods where possible, is developed. Precisely what those methods should be, will be domain specific, depending on the objects of inquiry and the epistemic aims of a particular philosophical system of practice. This is how methodological naturalism works in the sciences, and how we should expect it to work in philosophy also. Methodological naturalism entails a plurality of philosophical practices, with methods tailored to best fit the aims, and objects of interest, of particular philosophical inquiries. Despite arguing that the first premise in the argument for reform is false, or at best too vague to be made both determinate and credible, I promote some methodological heuristics which form part of my positive view. I devote chapter four of this thesis to discussions on scientific and philosophical progress.

§3.2 Methods, methodologies, and systems of practice

Before looking into the details of the argument for reform, I wish to make clear my intended use of the terms 'methodology' and "methods". Here I follow Sandra Harding, who differentiates *method* from *methodology* in the following way:

> Methods are "techniques for gathering evidence," whereas methodology is "a theory and analysis of how research should proceed." Epistemology is the "theory of knowledge or justificatory strategy" that underlies the methodology (Harding 1987, 2).

Underlying a methodology, is an epistemology, which Harding defines as the "theory of knowledge or justificatory strategy" (1987 : 2). This should not be viewed as an epistemological foundation for the methodology, but more a framework of adjustable epistemic aims that set success conditions for the methodology. I take "methods" to be understood broadly, following Michael Dickson, who understands methods to:

> include such things as principles of experimental design, methods for determining causal relations, methods for analyzing data, and even techniques for proof in mathematics and theoretical physics.
>
> (2006 : 45)

Further, I encourage a wider understanding of both scientific and philosophical domains than more traditional accounts that treat them as bodies of propositions and focus mostly on justificatory strategies offered in the support of their propositional content, to include a larger focus on actual practice, which involves attention to certain aspects that this propositional focus allows on its own.

Hasok Chang (2014) encourages a similar wider understanding, and argues that the more restrictive traditional accounts of science leave out important features of scientific practice. He complains that:

> Standard Anglophone philosophical analyses of science have been unduly limited by the common habit of viewing science as a body of propositions, focusing on the truth-value of those propositions and the logical relationships between them. The premier subject of discussion in such philosophy of science has been theories as organized bodies of propositions. This has led to the neglect of experimentation and other non-verbal and non-propositional dimensions of science in philosophical analyses.
>
> (2014 : 67)

Chang's move to step away from viewing the sciences as solely consisting of bodies of propositional knowledge which can be tested for truth or falsity, enables an account to be given of scientific knowledge that can include a much richer description of the growth of science. Various systems of practice achieve their epistemic aims by generating techniques of how best to measure, test, predict, model, or theorise about their objects of inquiry. These important parts of science and its practice are not easily reduced to propositional knowledge that is usefully thought of as being true or false, and are thus not captured by viewing of scientific knowledge as merely propositional. Hasok Chang borrows a distinction from Gilbert Ryle (1945), between knowledge-that - i.e. propositional knowledge, and knowledge-how - which Chang labels active-knowledge, and takes the latter to be an important aspect of scientific knowledge which is missing from purely propositional accounts.

Chang's work here is novel and exciting, but might face criticisms by those who argue that knowledge-how can itself be reduced to knowledge-that. However, whether such a reduction of Chang's active-knowledge to propositional knowledge is possible should be treated as an open question, and his conceptual distinction usefully illuminates important aspects of scientific practice that seem, *prima facie*, to be overlooked when the sciences are viewed solely through the lenses of discovery and justification. With the distinction between *ontological* and *explanatory* reduction that was discussed in the §2.3 of this thesis in mind, Chang's active-knowledge would have to be shown to be

explanatorily reducible to propositional knowledge, for it to be satisfactorily done away with.

Much of the twentieth century philosophy of science involved focusing on how scientific theories are justified. Once a theory is proposed, such philosophers are interested in analysing bodies of evidence for the theory, performing controlled experiments to compare observational data with predictions, checking for logical consistency, and looking at how the theory fits with currently accepted science. The process of scientific discovery, the circumstances and considerations under which a scientific theory is produced, were thought to be uninteresting, or at least uninformative, in terms of highlighting the important epistemic features of the sciences. Moreover, social factors that contribute to scientific knowledge such as interaction and peer review, are excluded. These seem like obvious candidates for contributing factors to eliminating personal bias, a strategy which many of the sciences employ in the hope to attain greater *objectivity*. These are not easily captured when viewing the sciences as bodies of propositions. Further, the discovery/justification distinction is not a very neat fit when observing how scientific work is carried out. Temporally, there is not a clear distinction between discovery ending, and justification beginning. The conceptual distinction between discovery and justification is indeed a useful one. With it in place, we can focus on evidence for various theories or models, or the predictions they make, but it does not provide a natural understanding of how scientific work is carried out in practice, and nor does it capture everything that is important in contributing to the epistemic success of the sciences.

In more recent philosophy of science literature, more attention is being given to a study of scientific practice. Rather than imposing *a priori* conceptual distinctions and using them to analyse science, more attention is being paid to observing what it is that scientists do. Given that the naturalistic starting point involved admiring the sciences, and observing what it is that they do, carefully trying to withhold *a priori* analysis as far as possible, this focus on scientific practice seems in keeping with the motivations of the naturalist. This chapter considers methodological naturalism from both *a priori* and practice-focused empirical perspectives.

§3.3 Williamson's dilemma

In a recent dialogue between Timothy Williamson and Alex Rosenberg (see Williamson 2014, and Rosenberg 2014), Williamson spells out a problem for reformist methodological naturalism. He argues that the reformist naturalist faces a dilemma. On the one hand, they can restrict a definition of naturalism so that it can genuinely offer the significant descriptive difference between philosophy and the sciences that the reformist requires for their argument to be plausible. An unwanted consequence of restricting a set of naturalistic methods in this way, is that it would end up ruling out methods that are used successfully in the sciences as being non-naturalistic. This is in conflict with the naturalistic starting-point. Rather than accepting the methods of science that we can observe, such a view starts with *a priori* intuitions about scientific method either as practiced, or as it allegedly should be practiced in an 'ideal' or 'future' science, and ends up ruling out successful naturalistic methods as illegitimate. On the other hand, if the reformist adopts the naturalistic starting-point, and describes scientific methods as practiced, Williamson argues that their definition loses bite, leaving the definition of methodological naturalism as prescribing a vague grab-bag of practices, that no longer offers any argument for genuine reform.

This chapter explores the two horns of Williamson's dilemma, and offers a considered third option. That is, I argue that Williamson's dilemma is instead a trilemma, and I advocate this newly minted 'third way' as a solution. I argue that methodological pluralism is a realistic description of the sciences as practiced. Whilst this doesn't amount to offering a precise and generally implementable account of naturalistic methods suitable for all domains of inquiry immediately, it does give the naturalist some guidelines as to various methods that can stake claim for epistemic credibility drawn from their successful application in the sciences, and encourages a plurality of approaches to be implemented. Further, it allows that some methods might be criticised in local contexts.

Rather than offering significant reform of philosophical domain or method, I take on board some of the criticisms that various reformist naturalists have made and sketch some methodological heuristics which help to shape my positive view. I am of the view that many philosophical methods are perfectly suitable by naturalistic lights already, and thus do not require that the naturalism that I endorse provide a silver-bullet, to provide "bite". Rather than trying to bite off more than one can chew, the methodological naturalism that I am in favour of is more interested in local nibblings. Williamson seems to suggest that methodological naturalism must either be all inclusive, or implausibly restrictive. The pluralism which I endorse lies somewhere in the middle. I spend more time defending this middle ground in §5.5 of this thesis, when I defend methodological pluralism from criticisms that it collapses into relativism.

Williamson describes the dilemma for the reformist naturalist as follows:

> If they are too inclusive in what they count as science, naturalism loses its bite. Naturalists typically criticize some traditional forms of philosophy as insufficiently scientific, because they ignore experimental tests. How can they maintain such objections unless they restrict scientific method to hypothetico-deductivism? But if they are too exclusive in what they count as science, naturalism loses its credibility, by imposing a method appropriate to natural science on areas where it is inappropriate. When on the attack, they assume an exclusive understanding of science as hypothetico-deductive. When under attack themselves, they fall back on a more inclusive understanding of science that drastically waters down naturalism. Such manoeuvring makes naturalism an obscure article of faith. I don't call myself a naturalist because I don't want to be implicated in equivocal dogma. Dismissing an idea as 'inconsistent with naturalism' is little better than dismissing it as 'inconsistent with Christianity'.
>
> (2014 : 30)

I begin by discussing the second horn of WIlliamson's dilemma, and I agree that reformist naturalists often offer an implausibly restrictive description of methodological naturalism that lacks credibility. I demonstrate this by showing examples of reformist naturalists who propose such austere definitions of naturalism, and by showing that they are implausibly restrictive. Such definitions, I will argue, often appeal to methodological monism, the idea that there is an overarching singular scientific method. Such appeals to monism lack evidentiary support from the sciences. A careful inspection and description of scientific methods yields methodological pluralism. Rather than there being one singular scientific method, there are a number of different systems of practice, with a number of different epistemic aims, implementing a number of different methods, many of which can stake claim to epistemic merit.

After dismissing kinds of naturalism that fail on these grounds, I look to forge a middle ground, whereby I propose a credible methodological naturalism, that is not an obscure article of faith. The aspirational naturalism which I defend is rather inclusive, but it does not find itself "implicated in equivocal dogma" (Williamson 2014 : 30) either. The methodological heuristics which I outline in section four of this chapter draw support from their successful application in the sciences. They do not offer a cut and dry monistic philosophical methodology, but neither do they collapse into a vicious relativism, or anarchism, thus avoiding the first horn of Williamson's dilemma. The methodological pluralism of the sciences can stake claim to epistemic respectability through its output. If this results in a somewhat vague collection of methods, without a definitive overarching rule for when they are to be applied, this need not concern the methodological naturalist. The naturalistic starting-point involved first respecting the methods of the sciences, and claiming that they are credible. Thus, whatever these methods are, pluralistic or otherwise, they are methods which the naturalistically inclined philosopher should be content with.

§3.4 Reformist methodological naturalisms

Alex Rosenberg describes methodological naturalism thus:

> Naturalism is the philosophical theory that treats science as our most reliable source of knowledge and scientific method as the most effective route to knowledge.
>
> (2014 : 32)

Within Rosenberg's claim that "scientific method [is] the most effective route to knowledge" (32), is an implicit assumption of methodological monism. Science is described as having a singular method, and its method is described as being a singular most effective route to knowledge. When pressed on defining the scientific method, Rosenberg stipulates that it is:

> the experimental/observational methods all the natural sciences share, the social sciences increasingly adopt, and that naturalists devote themselves to making specific.
>
> (2014 : 33)

That he includes 'methods' in the plural does not amount to an endorsement of pluralism, as he is monistic with regards to there being a singular set of methods that "all the natural sciences share" (33).

Others who have defended reformist naturalist positions often restrict their criteria for application to one particular area of philosophy. Quine famously argued that epistemology was in need of reform (Quine 1969). James Maclaurin and Heather Dyke (2012) have proposed methodological constraints within metaphysics by stipulating that naturalistic metaphysics must directly engage with observational methods. Maclaurin and Dyke's definition of non-naturalistic metaphysics shares a strong resemblance to

the verificationism about meaning proposed by A.J. Ayer in his *Language, Truth and Logic* (1952). Maclaurin and Dyke's proposal is as follows:

> [W]e define [non-naturalistic metaphysics] as any philosophical theory that makes some ontological claim (as opposed to conceptual claim), where that ontological claim has no observable consequences.
>
> (2012 : 291)

As well as tying their methodological naturalism with respect to metaphysics to observation, Maclaurin and Dyke stipulate one further feature of non-naturalistic metaphysics. They argue that appeals to intuition as part of a justificatory strategy are "discontinuous with science" (291). For now I consider the first part of their claim, to do with their requirements about observation. In section four of this chapter, I consider whether some aspects of philosophical method - including the use of intuition - are somehow antithetical to the sciences.

In a recently published paper, Amanda Bryant defines an area that she calls "free-range metaphysics" (2017), raising a host of methodological concerns for philosophers involved in this fowl practice. She describes her philosopher belongs in a battery farm view as follows:

> Free range metaphysics is metaphysics that science has only a nominal role in constraining. Academic metaphysics that floats entirely free of science does not really exist, since most educated people have undergone at least some basic level of science education, which contributes to the background of belief against which they form metaphysical judgments. But in free range metaphysics, science plays a minimal role. In the construction of free range metaphysical theories, the institutional products of science — data, theories, books and journal articles — are not directly or explicitly appealed to. That is, free range metaphysics does not directly engage with science. Instead, it is constrained primarily by logical demands, such as the demand for consistency, aesthetic

> demands, such as the demand for simplicity, and psychological demands, such as the demands for intuitive plausibility and explanatory power.
>
> (Bryant 2017 : 2)

On Bryant's view, free-range philosophers respond to logical, aesthetic, and psychological demands, which she demarcates from scientific demands. I think it is fair to interpret her as meaning that these demands alone do not constitute employing naturalistically acceptable methods, on the grounds that there is no direct engagement with the findings of science. Bryant seems to be suggesting a necessary criterion of results naturalism (see §2.2, this thesis), in that she requires that the kind of metaphysics she takes to be naturalistically acceptable directly engages with the institutional products of science[15].

Aaron Novick (2016) shares concerns for the methods of metaphysics, but his concerns are quite different to, and seem to be in tension with, those of Bryant's. Novick argues that Inference to the Best Explanation[16] is a method which cannot of its own accord be considered a naturalistic method, and thus that employing such a method does not amount to methodological naturalism, in and of itself. Implicit in this claim is that IBE is not a sufficient condition for an implementation of naturalistically suitable methods. Novick further adds that:

> at least in many cases, the successful reliance on the theoretical virtues in scientific contexts shows only that the theoretical virtues are truth-conducive within those local contexts, and not that they are truth-conducive generally.
>
> (Novick 2016 : 1)

This seems to be an argument against imperialistic naturalism. Novick argues that from the observed merit of deploying these desiderata in scientific practice, one cannot reliably infer that imperialist expansion of that scientific practice into hitherto

[15] At least, it seems to me as though this is a fair interpretation of her claim. Her passage describes something that she also says doesn't exist in pure form, which makes an interpretation difficult.
[16] Henceforth, IBE.

non-scientific domains of inquiry will generate similar observable merit. Whereas Bryant criticised metaphysics that merely appeals to certain sorts of success factors while ignoring scientific findings, Novick seems to claim that the sciences themselves successfully deploy these very same success factors, and cautions against inferring from this that 'doing as the scientists do' in other areas will be likewise successful.

I will show that all of these instances of reformist naturalism are implausible as they impose methodological restrictions that are far too restrictive. There are numerous examples of the sciences breaking the various methodological demands for which these reformists require philosophy or parts of philosophy to maintain. They impose a restriction on naturalism that is unwarranted, unsuitable, and unscientific. I use these cases to illustrate the failure of these restrictive sorts of naturalistic reform. I think it is likely that other instances of narrowly construed reformist methodological naturalisms are likely to fail for similar reasons. This is a hypothesis which draws inductive support from observed cases, and which is empirically tractable. It could be viewed as my *pessimistic meta-induction against reformist-naturalism*. Whereas Laudan (1981) demonstrated various failed attempts of demarcating science from non-science, and inferred from this his pessimistic meta-induction against any future attempt at demarcation, the case studies which I discuss show the failures of various restrictive reformist naturalisms, from which I infer a pessimistic meta-induction against any future attempts at successful implementation of such restrictions. If one accepts that Laudan has shown that demarcation of science from non-science will likely fail, it should be no surprise that attempts to demarcate naturalistic (scientific) methods from non-naturalistic (non-scientific) methods will likely fail for similar reasons.

Pluralism about method

It is a commonly held view within science studies subjects, that there is no such thing as a singular scientific method:

> [S]cholarly studies of the history and practice of science long ago showed that, in fact, there is no such thing as the Scientific Method.
>
> (Kidd & McKinnel 2015 : 163)

Similar views stem back to at least Paul Feyerabend (1975), from his aptly titled *Against Method*. Ladyman and Ross (2007) elaborate:

> there is no such thing as 'scientific method', by which we mean: no particular set of positive rules for reasoning that all and only scientists do or should follow. There are many observed prohibitions...but these apply to all sound reasoning.
>
> (28)

This is very much a commonplace view in the philosophy of science since Larry Laudan's argument highlighting the various failures of demarcating science from non-science (1981). There is no observational evidence that conveys overall support on the claim that there is one overarching unified 'scientific method'. A careful inspection of the sciences as practiced reveals that there are a plurality of systems of practice, embodying a vast plurality of methods, working towards achieving a plurality of epistemic aims. Assuming that some ideal science would eventually find one singular method (or one fixed set of methods) is not plausible from the naturalistic starting point, given that the sciences are currently radically pluralistic in practice.

I see no good reason why philosophy should attempt to meet a restrictive methodological standard that the sciences themselves cannot attain. The naturalist began with respecting science for its epistemic credibility, and by observing the sciences, we find that there are a plurality of methods used both across and within various different scientific domains (for a thorough defence of this view, see Kellert et al. 2006). Thus, if the methodological naturalist proposes to use scientific methods, they will have a plurality of suitably naturalistic methods at their disposal, and I will point toward some of these during this chapter, before developing them further, and outlining my more specific methodological views in chapter five. For now, I will

illustrate methodological pluralism in the sciences with a case study from within the behavioural sciences. In light of this, we can return to the reformist naturalists claims about philosophy, and see that they are implausibly restrictive.

Pluralism in behavioural sciences

Helen Longino (2006) has recently researched the extent of pluralism within the sciences of human behaviour. Longino investigates a variety of different approaches to behaviour that draw on different bodies of scientific investigation. These include quantitative behavioural genetics - which draws primarily on classical genetics, molecular behaviour genetics - which draws on contemporary molecular biology, work on neurophysiology of behaviour - which draws on work within neuroscience, and social environment oriented psychology. Many previous meta-analyses of these areas of research concluded that one of these approaches is the right approach to the study of behaviour; however little agreement has been reached as to which approach is correct (see Longino 2006 for details). Rather than looking for a singular best approach, Longino looks at what each approach can accomplish, and also at the limitations each approach exhibits, and concludes that the different approaches are not fairly evaluated as being contenders for giving the one true account of behaviour. Rather, each approach had its own set of questions, experimental and observational strategies for answering those questions, different patterns of argument, and a range of alternative hypotheses. She finds that each approach is more focused on generating questions of interest and building a body of results, than on trying to confirm its underlying theories. Different approaches investigate different potential causes of behaviour within a population.

Each approach has instruments and equipment designed to measure certain kinds of things, but these differ between approaches. This involves having a variety of methods for conducting measurement, depending on the potential causal field an approach is interested in. Further, the methods for measuring the strength of association of variation in that field, and variation in behaviour, vary between approaches, and also

shape the field of investigation and the association measures that are possible from such an approach. From the molecular genetic approach, investigation produced data that were sufficient to distinguish between different molecular genetic hypotheses, but methods for determining the association between some genetic mutation with a behavioural trait of interest could not determine the degree of association of variation with respect to neurophysiological factors, nor social environmental factors. Methods of each approach require dividing the epistemic labour of investigating the various causes of behaviour between alternate approaches. Each approach produces different and incommensurable measurements of behaviour. Further, no approach can produce sufficient data to establish that it is a superior theory.

From this, Longino concludes a number of things. She finds that different approaches measure variation in different construals of causal space, that there is no measurement which is common between different potential causal factors, that the approaches are evidentially incommensurable, and that they cannot be taken to be empirical competitors in any substantial way. Thus, we have incommensurability at the level of measurement and evidence, and incompatibility at the level of theoretical pronouncement.

Nevertheless, Longino argues that these approaches are all productive, in that each is able to to generate replicable results and has practical use by way of implementation in cognitive and other projects. Each approach reveals significant causal relationships that cannot readily be revealed by any other approach. The lesson Longino draws from this is one of pluralism. That is, an attitude or stance of pluralism should be taken towards these approaches to the study of behaviour. We have no grounds to expect one theory to be able to integrate and explain behaviour adequately, unifying all of these approaches, without losing insights that the approaches as a plurality provide. The incommensurability and incompatibility between these various approaches, should not be viewed as problematic, but perhaps be lauded for the different aspects of behavioural knowledge each illuminates.

A recent edited collection of Kellert et al. (2006) comprises an excellent defence of scientific pluralism that involves discussion of the case made here by Longino, but also presents chapters that demonstrate the pervasiveness of pluralism within quantum mechanics, mathematics, and economics. These case studies are used to support the view that useful methods are various, depending on specificities to do with the particular objects of interest and aims specific to various scientific systems of practice.

I take it that these case studies help illustrate the wider point that an accurate description of the sciences finds that they are methodologically pluralistic. Longino uses her case study to illustrate another significant point, noting that despite the various models of measuring behaviour involving observation, the observations *recorded do not provide evidentiary support for their theories*. These studies from the behavioural sciences make ontological claims about human behaviour, and model ways to measure causal relationships between possible contributing factors, but do not require that the ontological claims themselves have observable consequences.

Methodological heuristics

Care with assuming that views are in genuine competition with one another

It is reasonably common for many philosophical arguments to motivate a positive view, in part by arguing against "competing" views. An important heuristic we can draw from Longino's examples is to do with the fact that she has presented different kinds of theories that can each claim to successfully explain some aspects of a phenomenon: human behaviour. Given that there is a plurality of cogent naturalistic models that provide different causal explanations for behavioural traits, one might wonder whether different models that purport to address a particular area of philosophical inquiry, might be viewed in a similar way to these models - not necessarily as competitors for the one true view about an area of inquiry, but perhaps as focusing on different aspects of somehow related phenomena. Care needs to be taken when using certain kinds of argumentative strategies to favour a particular model or theory in a domain of

philosophical inquiry. It may be the case that "competing" views are in genuine conflict with one another, and careful work should be done to investigate whether this might be the case in particular domains. One take on this is that on many topics that are complex, multi-faceted, and hard to access, overall increase of knowledge is often maximised by capturing fragments of such knowledge through use of multiple and diverse methods. There is a very large set of truths, with multiple paths of access, and it's possible that no individual path can access the whole of the set of truths.

Observation

Many within the philosophy of science literature view observational methods of justification as often important but ultimately unnecessary components of naturalistic methods. Observation and carefully controlled experiment are certainly important features of many methods found within the sciences, but observation is not a necessary condition of naturalistically acceptable methods. I will briefly rehearse two examples from twentieth century philosophy of science that illustrate that observation is a complicated feature of a naturalistic methods, and that empirical adequacy cannot be considered the only, or the most important, epistemic aim of scientific systems of practice.

The theory-ladenness of observation

One problem with relying on observation-based data alone, is due to the fact that observation is theory-laden. That is, there is no such thing as theory-free observation. Various experimental evidence appears within a bigger theory with an established body of work behind it, and with a number of theoretical assumptions. The significance of this, is that all observational data is viewed through some or other theoretical lens. It is not as if scientific theory is written by osmosis from simply observing the world. Observation does not straightforwardly describe a unique set of methods that apply in a clear and general way (see Kuhn 1962). This is only a very brief outline of a very large

topic with a significant quantity of literature, and I cannot provide a full and thorough account of it presently. I merely wish to point to the less than straightforward nature of appeals to observational methods, which is all I need to do for now in the overall context of my argument.

Underdetermination of theory by data

Perhaps more importantly, Duhem (1914) pointed out long ago that scientific theories are underdetermined by way of observational data. As a simple logical point, an infinite number of theories could be made empirically equivalent to any one theory. There are a number of instances from the history of science where competing theories have had observationally equivalent predictions of phenomena, and have used non-observational criteria to guide theory choice. Here, considerations of elegance, parsimony, and unification with other accepted theories, have played important roles in scientific theory choice. Important moments from this history of science, such as the Copernican revolution, where a heliocentric model of astronomy took over from the geocentric Ptolemaic model as the preferred scientific theory, are regarded as important instances of scientific progress, where observation played no initial role in adjudication between theories. Helen Longino (1996) has illustrated how rife underdetermination in the sciences really is. Longino's arguments are in tension with the commonly held belief that genuine instances of underdetermination in the sciences are rare.

Criteria such as novel prediction, and explanatory breadth have played important roles throughout the history of science, and they ought to be considered naturalistically acceptable epistemic aims, alongside empirical equivalence. Non-observation based epistemic virtues as components of criteria for theory choice are important to science, and using these criteria is naturalistic, in so far as they can be shown in practice to help generate scientific knowledge. Applying naturalistic methods more widely is to be encouraged, but this does not imply knowing in advance which methods will be effective in realising a given system's epistemic aims, and it does not imply that those methods will always be centred on observation.

Science that plainly does not involve observation

There are contemporary scientific systems of practice that do not involve observation at all. String theory, for instance, seems to be autonomous from observational evidence. In physics, string theory provides a theoretical model in which point-like particles of particle physics are replaced by one-dimensional objects called strings. The theory describes how strings interact and how they spread through space. There are no recorded observations that could provide evidence for string theory's truth or falsity, or at least, there is currently no evidence to support a claim that observation-centred methods could have traction. String theory is often criticised on these grounds. If the naturalist demands that observation-centred methods are necessary, string theory looks patently non-naturalistic. The methods employed by much of theoretical physics, do not enlist observation in any obvious way.

The reformist naturalist, if requiring that observation-centred methods are necessary, can respond to this in three ways. Their options seem to be: (1) accept their own criteria generally, and assert that string theory is non-naturalistic, (2) claim string theory does use observation, so it meets any observation requirement, or (3) say that string theory doesn't use observation, but it needn't, though insist that philosophy (or some relevant part thereof) still does need to - then specify the relevant difference between philosophy and string theory that justifies this different treatment, and thereby also this scope restriction on the observation requirement. None of these three options seem very plausible from the naturalistic starting point, but I do not take this brief discussion to involve a knock-down argument. The challenge remains for the reformist to point out why it is that philosophy, or some its parts, require special treatment in terms of the naturalistically acceptable methods at its disposal. Imposing restrictions on philosophy due to naturalist critiques, that are not also restrictions placed on the sciences themselves, requires explanation.

Mathematics

For any reformist naturalist who insists on the necessity of observation-centred methods, the systems of practice of mathematics provide a troubling case. The methods of working mathematicians involve starting with *a priori* axioms, and generating proofs without employing observation. Mathematics plays a supremely important role within the sciences. If the reformist requires observation as a necessary feature of all sets of naturalistic methods, they will need to explain why the practices of mathematics have been so useful both for their own purposes, and for their application to the sciences, given that they do not meet this restrictive methodological constraint. If a reformist naturalist insists that observation is necessary for philosophy, or for some branch of philosophy, they will need to explain why mathematics is a domain which is exempt from such requirements, and why philosophy, or a particular domain of philosophy, is to be treated differently. There might well be good arguments to do just this, but this is a challenge that the reformist must rise to. I see no other way than to consider the methods of mathematics as a subset of the plurality of naturalistic methods at first glance.

One reply a reformist might give would be to allow for an 'indispensability to science' exemption that extends to a lot of mathematics, but doesn't extend to a lot of philosophy. This will again require spelling out the relevant difference between mathematics and philosophy that justifies this different treatment, and thereby a scope restriction on the observation requirement for different systems of practice.

Science that doesn't engage with other science - results naturalism

In §2.2 of this thesis, I introduced results naturalism. Results naturalists align themselves with the sciences by engaging with the results of the sciences. Amanda Bryant's (2017) discussion of "free-range metaphysics" involved criticising instances of

metaphysics that did not directly engage with the results of science, and is thus an instance of results naturalism. Bryant's view involves placing restrictions on certain philosophical views that are not placed upon the sciences themselves. We do not expect that a theoretical physicist should engage with the results from some other scientific system of practice, e.g. psychology, in order to exhibit naturalistic methods. As was the case with regards to the above discussions surrounding observational methods, the reformist will need to clarify why it is that philosophy, or metaphysics, or whichever system of practice is attributed more restrictive methodological requirements than others, is to be singled out in this regard.

Do specific domains have local and exclusive epistemic aims?

The last of the reformist naturalist views outlined earlier in this section was that of Aaron Novick, who claimed that various epistemic virtues are only truth-conducive in local contexts (2016). Novick's paper is a reply to a paper of Laurie Paul's (2012), where Paul argues that metaphysics and the sciences share an overarching methodology of *inference to the best explanation*, and thus that metaphysics employs suitably naturalistic methods. Novick's paper involves a discussion of two case studies from biology, that exhibit a methodological constraint of *vera causa*.

Vera causa is a methodological principle that was first described by Newton, and literally translates to *true cause*. Vera causa is a constraint that suggests that we can determine the *best* explanation from a number of empirically equivalent theories if one of the theories makes use of a previously accepted cause. The theory that uses the previously accepted cause is to be preferred to others that do not. Novick (2016) demonstrates two case studies from biology that make use of the vera causa principle. Novick argues further that this tells us something interesting about biology, in that it uses IBE in a specific way, by employing vera causa. From here Novick suggests that this shows that local domains (of which he takes biology to describe one such) have local epistemic aims, and that we should therefore be sceptical about applying the method of IBE within metaphysics. According to Novick, metaphysics cannot stake

claim to naturalism, merely by employing IBE. He argues that the challenge is now on the metaphysician to justify the suitability of using IBE for their purposes. Novick takes it that he has demonstrated that biology is justified in using IBE, but expresses scepticism at the use of IBE in metaphysics.

I've already argued that a current description of the sciences includes a plurality of systems of practice, each with specific and sometimes different epistemic aims, with different underlying methodologies that purport to realise the aims of a practice. I think a charitable interpretation of Novick would suggest that he means something like this, when he talks about local domains having local epistemic aims. However, that we should be sceptical about metaphysics employing a method that is often instanced in various scientific practices does not obviously follow from his discussion. If Novick's examples and arguments are taken to be universally applied, they would result in a general scepticism of every scientific system of practice, and every other broadly epistemic system of practice, except biology - or maybe even of every single theory other than the two that he discusses. For example, he seems to be arguing that showing how *vera causa* works in these cases does only this, it provides justification for using IBE qua vera causa in these cases, and these cases alone. If he takes it that this leads to a scepticism for metaphysics, due to showing that an epistemic aim is unique to a particular theory, then it would follow that we ought to be sceptical of any theory that uses IBE that is not one of the two he discusses. The list of theories to be sceptical of, if Novick's argument generalises as he suggests, is quite large. If Novick's criteria is only supposed to apply to metaphysical theories, he needs to provide more detail that shows why metaphysics should be singled out in in this regard. To make a claim about some parts of biology in order to argue for domain specific aims, to then claim a special restriction on methods for different specific domains, is so far an incomplete argument.

I encourage metaphysicians to clearly express the epistemic aims of their system of practice, and to make use of methods that have been used in the sciences where possible. We cannot know *a priori* that a method from one system of practice will be usefully applied to a different system of practice, but we can be optimistic that methods that have been used successfully elsewhere might be usefully applied more widely. A

system of practice can be criticised by demonstrating that its underlying methodologies are inappropriate for the achievement of its aims. By showing that biology sometimes uses a particular kind of IBE does not provide evidence for scepticism about the methods of metaphysics, in and of itself.

The aspirational naturalism which I defend encourages the appropriate alignment of the epistemic aims of philosophy with those of the sciences. I cannot see any argument for ruling out particular aims as being appropriate for a particular philosophical practice *a priori*, and it would be antithetical to the naturalistic starting point to claim otherwise. Thus, the prudent methodological naturalist encourages alignment with the sciences in a variety of ways, and does not rule any methods out *a priori*.

Heuristics around observation and scientific results

Despite dismissing a requirement of observation, or a direct engagement with the results of science to be included as necessary conditions for implementing the view that methodological naturalism endorses, I offer a methodological heuristic that forms part of my positive view. I argue that methodological naturalism should include a theoretical commitment to *relevance*. I define relevance, as holding that, at least in principle, philosophical theorising should commit to the belief that various scientific findings or scientific methods, observational or otherwise, could be usefully implemented within their systems of practice. If some product of scientific methods, or the implementation of some observational or otherwise scientific method, could be utilised usefully within philosophical systems of practice, philosophers are encouraged to do so. Whether particular methods will be useful in local contexts will be partly an empirical matter, and an *a priori* insistence on favoured methods is to be treated with scepticism.

What to make of reformist naturalism?

In light of the discussion of this chapter, we can return to look at the restrictive instances of reformist naturalism that were introduced earlier. Alex Rosenberg's (2014) insistence on a singular scientific method is implausible, in light of a current description of scientific practice as exhibiting methodological pluralism. Maclaurin and Dyke's (2012) claims that metaphysics must by necessity engage with observation imposes restrictions for metaphysics that are not met by various sciences. Amanda Bryant's (2017) requirement that metaphysics must directly engage with scientific results again imposes restrictions that various sciences themselves do not live up to. Aaron Novick's (2016) claim is as yet incomplete, and relies on *a priori* assumptions about science and philosophy which are not obviously well-motivated. These four naturalisms all prescribed different variants of reform for philosophy that are implausibly restrictive.

Having rejected these instances of reformist naturalism, I single out several aspects of philosophical practice that have been criticised at times for purportedly being non-naturalistic, in case these might provide the significant descriptive difference between philosophy and the sciences that the reformist seeks to identify and deploy.

The role of intuition and thought experiment in philosophy

Intuition

Various philosophers have criticised the use of intuition within analytic philosophy. Ladyman and Ross (2007) and Maclaurin and Dyke (2012) have argued that the use of intuition in philosophy (their complaints are specifically directed toward the use of intuition in metaphysics) establishes a "discontinuity" with the sciences. A growing number of philosophers have adopted the method of experimental philosophy, which was initially motivated by a scepticism of the reliability of accurately intuiting folk beliefs (e.g. Weinberg, Nichols and Stich 2001). Other commentators have downplayed the role of intuition within philosophical systems of practice. Cian Dorr (2010) suggests that often when philosophers claim that a proposition, *P*, is intuitively plausible they are rather announcing an assumption that *P* is the case, without providing

further argument. Timothy Williamson (2004) suggests that often when philosophers claim that a proposition, *P*, is intuitively plausible, they are instead announcing that they are making a judgment that *P*, or perhaps making an abductive inference to *P*, based on available evidence (Williamson 2004).

I do not wish to take sides in this debate by offering a diagnosis of the "genuine" role of intuition in philosophical practice, but instead I offer some methodological heuristics that we can draw from the various commentators in this debate. These heuristics form part of the aspirational naturalism which I advance, and outline in more detail in chapter five. Philosophers should take more care with regards to using the term 'intuition'. Dorr is probably right that there are times when a philosopher uses the term 'intuitively plausible', when rather they mean that they will make an assumption for which they will not provide an argument. A philosopher who intends this usage is encouraged to make it explicit that they do so. One should state clearly that they are making an assumption, and make plain what that assumption is, rather than using the term 'intuitively plausible'. An argument's having assumptions, and working with downstream consequences is not necessarily problematic. All scientific systems of practice have working assumptions, there is nothing non-naturalistic about doing so. But, an epistemic aim that philosophy rightly values is an aim for clarity and precision, and one ought to be clear and precise as to what one is doing, especially given the current negative press that intuition has received in the literature. Likewise, if Williamson is right that sometimes the term 'intuition' is used to indicate that one is making a judgement, which seems a plausible description for at least some uses of the term, one should stipulate this explicitly for precisely the same reasons. This will curtail any potential straw-man attacks on those who misuse the term in these innocent contexts.

If a philosopher is genuinely relying on their intuitions as serious and exclusive evidence for a philosophical claim, one might rightly express some degree of scepticism in certain cases. We might consider the following questions, when evaluating the strength of such evidence. Is the philosopher an expert in a particular domain, such that relevant intuitions might be theory-guided, or are they merely throwing caution to the

wind and saying they intuit something for which they can provide no other justification? Could these intuitions provide reliable evidence about the matter in question? Intuitively plausible ideas can often make for good hypotheses, or starting points for an investigation, and there is no reason why they can't be relied on to contribute towards such practices, but the epistemic weight we might place on a given agent's intuition might rightly be questioned in particular circumstances.

Laurie Paul outlines a fallacious argument structure which involves a reliance on using intuitions as conclusive evidence for the matter in question. She names this the *homunculus fallacy* (2010). According to Paul, this is committed when one concludes that a proposition is true, only on the basis that one finds the proposition intuitively plausible. Arguments that commit the 'homunculus fallacy' have the following structure:

P1) *X* is intuitively plausible
∴ *X*

Some reformist naturalists express scepticism toward the using of intuition as a means of providing evidentiary support for various philosophical arguments (e.g. Maclaurin and Dyke 2012, Ladyman and Ross 2007, Weinberg et. al 2001). Philosophers should make explicit the intended use of the term 'intuition' (e.g. as a judgement, an assumption, an intellectual seeming, etc.). The meaning of 'intuitions' that both the reformists express scepticism towards and that Laurie Paul's homunculus fallacy describe, is close to the meaning attributed to the everyday use of the term, where it describes something like an *instinctive feeling*[17], or an *intellectual seeming* (e.g. Bealer 1998). Judgements and assumptions are not subject to these sceptical arguments. More attention to detail by way of clearly defining one's terminology, enables the wheat to be sorted from the chaff, as it were, and help ensure that the arguments presented by sceptics about intuition are not misapplied. This allows more fruitful conversations about specific usages of the term 'intuition' to take place. This is to be encouraged. I next look at the role of thought experiments, which themselves involve intuition. After

[17] Oxford dictionary. Retrieved from https://en.oxforddictionaries.com/

this, I present some findings from psychological research, that might help clarify certain circumstances in which intuitions are more reliable than others.

The role of thought experiment

Thought experiments have played a colourful role within philosophical practice. Famous examples include the Gettier cases (1963) - where many took Gettier to have successfully refuted the traditional epistemological conception of knowledge as justified true belief, Kripke's twin earth thought experiments (1972) - which were important arguments against the reference theory of meaning, various trolley-themed thought experiments in ethics - which place the reader in an imaginary scenario in order to illuminate consequences of particular ethical theories (see Singer 2005 for discussion), Rene Descartes' evil demon hypothesis (1641), and David Chalmers' arguments for the possibility of zombies - physical duplicates of agents that lacked consciousness - which were arguments against various materialist theories in the philosophy of mind (2002).

Using thought experiment as an argumentative strategy in philosophy has been criticised at times, for precisely the same considerations that concern sceptics about the reliability of intuition. Thought experiments are sometimes offered as attempts to refute claims about necessity, by constructing a hypothetical situation which would provide a counterexample to the necessity claim, if it could be demonstrated that the purported situation were possible. If a thought experiment can convince a test subject as to the possibility of a counterexample to some necessity claim, then a rational subject must dismiss the necessity claim on logical grounds. It cannot be the case that *X* is necessarily true, and also possible that *X* is false. On the contrary, if *X* is necessarily true, then it is impossible that *X* is false. Thought experiments may be otherwise employed as intuition pumps, allowing a greater understanding of some of the central concepts of a philosophical theory, by highlighting kinds of scenarios that might have to be accommodated by particular conceptual distinctions (see Dennett 2013).

Again, I do not proffer any adjudication on the epistemic credibility of the various thought experiments listed above, but I suggest that thought experiments of the former kind discussed above, are epistemically credible to the degree to which they establish the possibility of their putative counterexamples. Thought experiments involve counterfactual reasoning, a cognitive process which is carried out reliably in many situations. Suppose an employer levels the following counterfactual to an employee: "If I let you off work early, will you come and have a drink with me at the pub?". This proposal consists of a counterfactual scenario in which an employee's intuitions are likely to be pretty reliable. The employee might make a number of considerations whilst thinking of how they will reply. They might consider whether they had any previous engagements around the time of the proposed drink, or whether any other relevant personal factors might impact on their making a decision one way or the other. Perhaps the employee drove to work that day and drinking alcohol would prevent their ability to get home safely. Maybe they consider their boss to be of dubious character and could think of nothing worse than their spending social time together. I see no good reason for a general scepticism about the ability of each party to reliably cognize about such counterfactual scenarios - these scenarios seem very *familiar* to day to day experiences. Hence, there is no good reason to be sceptical about the ability to reliably cognize about counterfactuals in general. When considering something less familiar to our day to day experiences, e.g. the metaphysical possibility of philosophical zombies - i.e. physical duplicates of humans that lack consciousness -, it is not obvious that intuitions can gain much traction. This is intended to make the wider-scope point that the reliability of and hence evidentiary weight attributed to an agent's intuitions will vary according to their familiarity with the antecedent conditions of the counterfactual conditional. I next make this suggestion more precise, and illustrate with a psychological case study of the reliability of intuitions.

Psychologist Daniel Kahnemann (2011) describes a project conducted for determining circumstances where intuitions are at their most reliable. Kahnemann found that forming reliable intuitive judgments is an acquired skill, and that these skills are best developed within an environment that is sufficiently regular. Above, I had suggested that the reliability of intuitive judgments is correlated with familiarity. Where I used

the term ‘antecedent conditions’, Kahnemann uses the term ‘environment’ analogously. Familiarity with the antecedent conditions is most likely to increase when those conditions exhibit greater regularity. Regularity of the antecedent conditions makes systematic prediction possible, and affords the opportunity for prolonged practice, in order for an agent to best learn what these regularities are. The more regularities learned, the more familiar one is with the antecedent conditions. According to Kahnemann’s study, the more regular the antecedent conditions are, the more reliable our intuitions about them are likely to be. He doesn’t use the term ‘familiarity’, but I think it is a useful conceptual addition to his claim that increased regularity leads to more reliable intuitions. More carefully, increased regularity of antecedent conditions enables the agent to acquire greater familiarity, and it is this greater familiarity that increases the reliability of the agent’s intuitive judgments about the matter in question.

There are other factors that constrain the reliability of an agent’s intuitive judgments, to do with characteristics of individual agents, other than the regularity of the antecedent conditions. The extent to which reliable intuitive judgments about particular matters will depend upon the cognitive make-up of individual agents. Scepticism regarding the reliability of the intuitive judgments of a particular agent will be warranted, should the agent be cognitively impaired in some way. The reliability of the intuitive judgments of a sufferer of a cognitive disease such as Alzheimer’s - a degenerative neurological condition, with symptoms of confusion and memory loss - will be questionable, regardless of the regularity of the environment. This provides another reason for including ‘familiarity’ as a useful addition to Kahnemann’s conceptual scheme. An agent’s ability to gain familiarity with any antecedent conditions, regardless of regularity, is constrained by the cognitive make-up of individual agents. Thought experiments need not be merely hypothetical scenarios. Timothy Williamson (2007) performed real Gettier cases on his own lecture theatres, for example, thus actualising them.

A number of famous thought experiments are to be found within the history of science. Einstein’s various *gedankenexperiments* (see Cohen 1989), and Newton’s cannonball experiment (see Velentzas and Halkia 2013) are examples of such. Use of thought

experiment is not exclusive to philosophy, nor is it alien to the sciences. Epistemic scepticism is sometimes warranted towards thought experiments, to the extent that the possibility of their proposed counterexamples can be reliably ascertained through cognition. That something is a thought experiment in and of itself, does not establish this skepticism.

After discussing the use of intuition and thought experiment within philosophical practice, I have argued that there is nothing particularly unusual in the fact that philosophy uses them, since the sciences do so too. However, I have not addressed *how* philosophy or science uses them in detail. I have discussed and commented on some aspects of the reliability of intuition and thought experiment in certain circumstances. This provides at least some of the groundwork for further examination into the appropriate uses of intuition and thought experiment in philosophy and the sciences. Despite requiring further detailed work to make these claims more precise, I have offered some methodological heuristics of care when using intuition and thought experiment which are a starting point:

> 1) The intended meaning of the term 'intuition' should be made explicitly and as precisely as possible. If other terms more accurately express the author's intended meaning, these should be favoured instead (e.g. 'assumption', 'judgement', or 'inference').
>
> 2) Outlining both the intended evidentiary role and justificatory weight that the author intends to attribute to intuition and/or thought experiment should also be made as explicit and precise as possible. A thought experiment might be intended to *demonstrate* the possibility of a particular state of affairs. Less ambitious roles for thought experiments include using them as intuition pumps, in order to *highlight* interesting aspects of a particular theory. Outlining the epistemic aims and methodologies of a philosophical system of practice with greater precision, makes evaluating the methods of intuition and thought experiment more tractable on a case by case basis.

3) Laurie Paul's 'homunculus fallacy' occurs within arguments that afford intuition too great an evidentiary role. Paul's fallacy is committed when one concludes *x*, from merely intuiting *x*. In general, we might be sceptical about arguments of this form[18].

This chapter has so far dismissed a number of reformist naturalist views, that attempted to rule out various philosophical practices as non-naturalistic. I argued that the various criteria that reformist naturalist arguments impose on philosophical practice are untenable, in that they offer restrictions on philosophy that are too harsh. The restrictions suggested are not endorsed by the sciences, and neither are they restrictions that the sciences themselves employ. After dismissing these views, I have looked at other reformist criticisms that expressed scepticism towards the use of intuition and thought experiment. I have shown that the use of intuition and thought experiment, in and of itself, does not provide grounds for scepticism on naturalistic grounds. However, relevant findings from psychology give us some guidance as to conditions when intuitions will be more reliable than other times.

I have argued that reformist arguments do not succeed by focusing on various methods of the sciences and philosophy to provide the significant difference between domains that the argument for reform requires. For the remainder of this chapter I consider some more abstract considerations on behalf of the reformist, to see if these might provide the relevant difference between the sciences and philosophy that they require for the argument for reform to be successful.

Is philosophy some kind of analytic *a priori* activity, autonomous from the sciences?

[18] Not all instances of this argument pattern are naturalistically unreliable, however. A subject's intuitions might be taken as reliable evidence for a medical practitioner providing a medical diagnosis, for example.

Some commentators have claimed that there is a significant difference between philosophy and the sciences at a sufficiently abstract level, and that the domains can be demarcated from one another by philosophy's having an exclusive realm of interest by way of the analytic *a priori*, whilst the sciences are thought to reign over the synthetic *a posteriori*. I will briefly introduce these distinctions, before discussing commentators who hold related views.

Different philosophers often work with different definitions of these terms, as I will highlight below. I will introduce these terms in what I take to be a fairly conventional and non-controversial way. I do not claim that this is the correct way to use the term, or that there is one uniquely correct usage. The analytic-synthetic distinction is a semantic distinction to do with what it is that makes a particular proposition true or false. An *analytic* proposition is made true or false by definition. For instance, the proposition 'all bachelors are unmarried' is true by definition, whilst the proposition 'all bachelors are married' is false. The proposition 'all bachelors are unmarried' is analytically true, whilst the proposition 'all bachelors are married' is analytically false. Contrasting this, a *synthetic* proposition is made true or false by the way the world is. For instance, the proposition 'the sun is the gravitational centre of our solar system' is made either true or false (true, I believe!) in so far as it provides an accurate description of the world.

The *a priori*/*a posteriori* distinction is an epistemological one, to do with how we can know the truth of propositions. If a proposition is knowable *a priori*, its truth or falsity can be ascertained without observing the world. Our old friend, the bachelor, can come to the party again to illustrate this point[19]. The truth of the proposition 'all bachelors are unmarried' can be ascertained without observing the world. One does not have to count each and every bachelor, carefully checking whether they are also unmarried before knowing the proposition is true. On the other hand, we cannot ascertain the truth or falsity of the proposition 'the sun is the gravitational centre of our solar system' simply by understanding the various terms involved. Astronomers provide evidence for the truth of this proposition by observing the world.

[19] A bachelor party, if you will!

Colin McGinn's *Truth by Analysis* (2012) makes the case that philosophy has its own unique role, and that it is solely to perform conceptual analysis. He argues that it is exclusively focused on making *analytic* claims, and justifying them by *a priori* means. He adds further, that this in itself gives no reason to think that philosophy is unscientific, and indeed that it should rightly be considered a science of its own. Frank Jackson has argued along similar lines at various times (e.g. 1998), claiming that there is room for a distinct role for philosophy as conceptual analysis. Jackson thinks this role will be analytic and *a priori*.

In Timothy Williamson's introduction to his *The Philosophy of Philosophy* (2007), a similar view is described.

> [W]e may put the difference to a first approximation thus: the current methodology of the natural sciences is *a posteriori*; the current methodology of philosophy is *a priori*.
>
> (1)

Williamson goes on to reject this approximation, claiming that it cannot be neatly applied to philosophy or science in any significant way.

David Papineau has advanced a quite different view to Jackson and McGinn:

> [p]hilosophy investigates reality in the same way as science. Its methods are akin to scientific methods, and the knowledge it yields is akin to scientific knowledge.
>
> (2014 : 166)

Papineau goes on to add that philosophical claims are synthetic in nature, and that philosophical knowledge is *a posteriori* rather than *a posteriori*. Interesting features of this view include regarding various methods of justification for philosophical theory choice - such as considerations of elegance or parsimony - as being *a posteriori*, rather than a more typical categorization that considers them *a priori*.

I will not weigh in heavily on these debates. It is not my task to provide a correct description of the analytic and synthetic, or the *a priori* and the *a posteriori*. For what it's worth, I think that McGinn and Jackson largely misdiagnose their own practices, but I will not argue for this view presently. It seems to me obvious that philosophy is often, if not usually, interested not just in conceptual analysis, but also in making arguments about how particular concepts can denote features of the world. Epistemologists who perform analyses of knowledge, are trying to describe a phenomenon in the world: knowledge. Philosophers of mind are trying to describe an object in the world: the mind. Metaphysicians of time are trying to describe the nature of some phenomenon in the world: time. These seem to me to be patently synthetic claims. Regarding whether various methods of justification are rightly considered *a priori* or *a posteriori*, it does not matter for my purposes. My interest is whether such methods of justification are naturalistic or not. Neither the analytic/synthetic distinction, not the *a priori*/*a posteriori* distinction provide the significant difference between the sciences and philosophy that the argument needs to succeed.

Does philosophy, or some part thereof, have a distinctive subject matter?

Laurie Paul (2012) has advanced a view about metaphysics, claiming it has a "distinctive subject matter" (1), compared to the sciences. She claims that metaphysical questions are more general in nature, than those that concern the sciences (ibid.). However, Paul thinks that metaphysics still makes use of naturalistically respectable methods. If there is a significant difference between some part of philosophy and the sciences in terms of subject matter, the argument for reform might yet succeed. For this to be the case, the reformist needs to establish the truth of both: 1) that there is a significant difference between this part of philosophy and the sciences, and 2) that naturalistic methods are unable to be applied to this particular domain of inquiry. Paul maintains that the first of these conditions is met, but not the second. Like Paul, I think that metaphysics, by and large, uses naturalistically respectable methods. *Pace* Paul,

however, I do not think that there is a clear sense in which the truth of her *generalness of questions* description of metaphysics can be established.

There are many instances of useful connection between metaphysics and the sciences that cast doubt over this claim. Much contemporary philosophy of mind engages with findings from the neurosciences and psychology (e.g. Baron-Cohen et al. 2013). Philosophy of time literature has found important connections between its subject matter and the special theory of relativity (see §2.3, this thesis). The philosophy of properties has involved various crossover with sciences such as chemistry (e.g. Lewis 1986). In this chapter I have argued for methodological pluralism, and take it that, at least in principle, there is no evidence for the claim that reality can be viewed through more or less fundamental lenses. I have outlined why I prefer to think of different scientific systems of practice as having different objects of inquiry, investigating different aspects of reality. The sciences ask a great many questions about a great many aspects of reality, from quantum mechanical theories that try to describe very general features of the world, through to much more specific matters, such as those that focus on partial causes of specific human behaviours.

I think that Paul's view can be usefully interpreted as making the point that metaphysical systems of practice are interested in investigating certain aspects of reality, with epistemic aims specific to their practice. As a wider point, I think the same applies to pretty well all parts of philosophy, and the sciences. I see no reason to think of metaphysics as being a unique instance of this point. Thus, I have no *a priori* views over which methods are appropriate for different systems of practice. Appropriate methods for a system of practice, scientific or philosophical, will depend on its epistemic aims, and the objects of inquiry it is interested in.

Many scientific methods have demonstrated success, and philosophers are encouraged to align themselves with relevantly useful methods from the sciences. Many reformist arguments try to make broad claims about the whole of philosophy or science, or sometimes the more restricted, but still rather broad domains such as metaphysics, or the natural sciences. These claims are hard to make good at this level of abstraction,

due to the plurality of systems of practice within these broad categories that exhibit extremely diverse aims and methods. If reformist naturalist arguments are to succeed, their focus needs to be narrowed further, and directed at specific systems of practice. Criticising a particular system of practice might be warranted if it can be shown that the practice in question has inferior methods that cannot bring about its aims. So far, the argument for reform is found wanting, until these more specific cases are made.

Chapter summary

In this chapter I have argued that reformist naturalists impose implausibly restrictive criteria for philosophical methods, ones that are inappropriate for philosophy, and ones not even required of various scientific systems of practice. I have considered whether there is a relevant descriptive difference between philosophy and the sciences that might warrant a call for reform of philosophy, and concluded that such differences are not to be found at first glance, and unlikely to succeed as such broad claims.

Throughout this discussion, I have motivated methodological pluralism, and have begun to shape this view with various methodological heuristics. I've argued that philosophers should aspire to use appropriate methods, and that aligning with the various scientific methods is to be encouraged. However, given the plurality of methods the sciences exhibit, application isn't a straightforward matter. Different systems of practice have their own epistemic aims and objects of inquiry. The appropriate application of methods for philosophical systems of practice will be context dependent, and can be evaluated in terms of how well its methods succeed in satisfying a system's epistemic aims. We should assume, at least in principle, that scientific findings and methods might be relevantly applied within all areas of philosophy, unless we have good evidence to suppose otherwise.

In the following chapter I discuss progress in philosophy and the sciences, and consider how it might be measured, suggesting that methodological pluralism might lead to greater progress if implemented in philosophy more widely.

Chapter Four

Progress in philosophy and the sciences

Chapter preface

In the last chapter I outlined and appraised the argument for reformist naturalism. Reformist naturalists call for some kind of a reform in philosophy in order for it achieve greater progress. I have already expressed my grounds for scepticism about the descriptive claims on which the argument relies, and these undermine the reformists claims about progress. Nonetheless, philosophers should happily welcome discussions about progress, given that they are working in an epistemic domain, and as careful critical thinkers, they should encourage useful models for improvement. This chapter looks at some models of philosophical progress that measure progress by convergence. I argue that this measure will not do, if it assumes that what is true is reducible to one answer in any given domain, due to its begging the question against pluralism. Further, it assumes that the sciences are convergent, and seems to beg the question about the extent of pluralism in the sciences as practised. In chapter three, I spent some time arguing that pluralism is an accurate description of the sciences at present, if not an accurate description for the way the sciences will turn out. The pluralism found in the sciences undermines the convergence measure of progress. There isn't a clear way to describe the sciences as making progress by way of convergence. A different model of progress will be required. I lay some of the groundwork for a better model of progress, outlining some considerations that this model will need to take into account, but I leave a complete development of this model for further work.

§4.1 What constitutes progress?

To make sense of comparisons between philosophy and the sciences in terms of progress, the beginnings of the discussion of epistemic aims from the preceding chapter can be usefully drawn upon and expanded somewhat. Progress, in the usual sense, is best thought of as trajectory towards a goal. On this reading, progress can be evaluated by determining how effectively a system of practice meets its epistemic aims. This will support inter-inquiry but not intra-inquiry comparisons of progress. For now, I narrow the scope of my discussion to inter-inquiry comparisons of progress. Intra-inquiry comparisons of progress might be fair in cases where it is clearly the case that the domains in question share epistemic aims. Further details of what intra-inquiry measures of progress may amount to are largely left for further work, but some considerations for such a measure are discussed in section six of this chapter.

The claim that the reformist makes about philosophy's making more progress through utilising scientifically inspired methods is plausible to the extent that philosophy and the sciences share similar aims. I will argue that there are clear instances where philosophy and the sciences do share similar aims. However, it seems to me that philosophy sometimes has some quite specific epistemic aims, including conceptual clarification, structural analysis of arguments, and broadly generating understanding, alongside striving to generate knowledge in a more straightforwardly "scientific" way.

There are two clear senses in which we could talk about progress, in terms of *quantity* or *quality*. It is clear that the reformist is not intending to be considering quantitative progress, which we could easily measure by counting published journal articles, conferences, books, and the like, and very easily prescribe a remedy for (i.e. do more!). The reformist argues that philosophy makes insufficient qualitative progress. I propose a number of suggestions for what qualitative progress might amount to. One obvious suggestion is that philosophy and science achieve qualitative progress when they discover truths about the world. As plausible as it may be that the discovery of truths is

an aim for both philosophy and the sciences at times, measuring a goal like this seems difficult. Viewing philosophy and the sciences as bodies of propositional knowledge, and adopting something like the classic epistemological definition of knowledge, knowledge is generated when true and justified propositions are the outputs of systems of practice. First-order instances of knowledge of this kind might be achievable by the various systems of practice if some of their outputs were both true and justified. But measuring progress by the attainment of first-order instances of knowledge would require the second-order claims that we know which of the outputs of various systems of practice are actually true. Measurement of these second-order knowledge claims would be easy from a "God's-eye" view-point, where we could sift through the various findings of the sciences and philosophy, comparing them with the *real* truths about the world, and compare philosophy with the sciences in terms of how well their outputs match up with some list of *real* truths. Without the power of omniscience, analysing such measures of progress seems problematic at best.

David Chalmers (2015) proposes such a measure of progress, and is aware of the difficulties regarding second-order knowledge evaluation. Chalmers attempts to sidestep these concerns by using a logical generalisation from convergence in general, to convergence to the truth, but I argue that his proposal does not succeed. I express scepticism about models of progress that attempt to measure truth by convergence in general.

I go on to demonstrate cases where it is clear that philosophy and the sciences have different aims than attaining truth. Oftentimes the sciences work within models that they know to be, strictly speaking, false. They use these models because they achieve other epistemic aims, such as usefulness, generating understanding, making something measureable, the clearing up of conceptual confusions, generating plausible hypotheses, or even sometimes - the sending of rockets to the moon. The sciences demonstrate a plurality of epistemic aims, which require different measures of progress. A particular system of practice can be viewed as progressive in so far as it meets its various epistemic aims, which are in part determined by the phenomena, or object of inquiry, it is interested in.

I consider other causal factors involved with progress that should be taken into consideration when attempting to measure intra-inquiry progress, however that may be done. In terms of population size, there are far more working scientists than philosophers. In terms of financial investment, the sciences receive more funding than philosophy does by order of magnitude. Any measure of progress between the two domains needs to factor in these important causal and scale factors. For example, if philosophy progresses far less than science, this may be merely proportional to the highly unequal human and monetary resources each domain has at its disposal. One implication of a commitment to scientific naturalism is to be at least minimally clear and determinate in one's own empirical or semi-empirical and meta-philosophical claims. Notably indeterminate claims about the comparative progress of philosophy and science do not meet this standard. One way to be notably indeterminate, for example, is not to make it clear whether or not one is making this comparison *per unit of resource* devoted to each of these forms of inquiry.

David Papineau (2017) has recently proposed that a reason for philosophy's slower comparative progress is that it is very hard. These considerations should be taken into account in terms of having a reasonable level of expectation for philosophy's progress. This motivates some work for epistemologists, in terms of observing various systems of practice, and attempting to demonstrate what a given system's epistemic aims are. It also motivates some work for metaphysicians, in terms of analysing the ontological assumptions and entailments that are attributable to a system of practice, or to determine whether there is overlap and interaction between different systems. If we have this information at hand, we can make informed and more precise claims about progress, which might then motivate suggestions for suitable measures of it. As much as the claim that philosophy progresses less than the sciences may seem intuitive, in order that one can provide evidence for such a claim or to prescribe possible methodological remedies, more clarificatory work about the plurality of epistemic aims must be done.

Progress is achieved when a particular system of practice is effective in meeting its epistemic aims. Towards the end of this chapter I sketch a number of epistemic aims, drawing from the work of Hasok Chang (2012), Thomas Kuhn (1962), and John Dupre (2012), and adding some suggestions of my own. These are to be included in my positive account of methodological naturalism, as potentially viable epistemic aims for various systems of practice in general. Showing that they are viable epistemic aims on more local scales is not achieved here. Different systems of practice will call for using different methods suitable for their aims, and more work will need to be done within philosophical systems of practice to provide insight into which methods will be usefully drawn upon. This chapter does not provide a complete and clear way to measure progress between philosophy and the sciences, but it demonstrates ways that will not work, and provides a sketch for further work in terms of making meta-progress.

§4.2 Dissatisfaction with philosophy

It is all too common to hear complaints about the lack of progress in philosophy these days. Such comments sometimes come from scientists, who look down scathingly upon us petty philosophers, locked in our rooms with our books, our armchairs, and a curious nature. Nobel prize winner and famed physicist Richard Feynman made a number of scathing comments about philosophy throughout his career, describing his distaste for philosophy, and especially the philosophy of science, which he described as being as useful to scientists as ornithology is to birds (discussed in Wilkins and Ebach 2013). My own hunch is that he could be correct her, but only because ornithology is rather useful to birds - for instance, through its indirect contribution to conservation. When describing philosophical practice, Feynman makes the following colourful anecdote:

> We can't define anything precisely. If we attempt to, we get into that paralysis of thought that comes to philosophers… one saying to the other: you don't know what you are talking about! The second one says: what do you mean by 'talking'? What do you mean by 'you'? What do you mean by 'know'?

(Feynman 1963, April)

Similar sentiments are expressed by other contemporary scientists[20], including such luminaries as Stephen Hawking, who claimed that "philosophy is dead" (2011, May 16). Neil deGrasse Tyson also provides an illuminating illustration of the practices of the philosopher, comparing their practice to the scientist, and arguing that science is superior:

> if you are distracted by your questions so that you can't move forward, you are not being a productive contributor to our understanding of the natural world. And so the scientist knows when the question "What is the sound of one hand clapping?" is a pointless delay in our progress.
>
> (2014, March)

Lawrence Krauss echoes in the footsteps of Feynman, and claims that

> Philosophy is a field that, unfortunately, reminds me of that old Woody Allen joke, those that can't do, teach, and those that can't teach, teach gym.' And the worst part of philosophy is the philosophy of science; the only people, as far as I can tell, that read work by philosophers of science are other philosophers of science. It has no impact on physics what so ever. [...] [T]hey have every right to feel threatened, because science progresses and philosophy doesn't.
>
> (2012, April 23)

At this stage, one might be forgiven for thinking, *pace* Feynman, that the metaphilosophy espoused by physicists is as useful to philosophy as football is to birds. Other philosophical wonderings come to mind, such as, does a strawman really burn if nobody is watching? Do these scientists think they have a ready method for measuring these claims of progress, or will asking them this get them involved in some kind of paralysis of thought?

[20] Often physicists, oddly!

Should we philosophers take these 'folk' metaphilosophical views seriously? They are awfully easy philosophical targets to thought-paralyse. I contend that we should take them seriously. We live in a time where proving our value to outsiders is important. Our careers and livelihoods are sometimes on the line due to various funding cuts across universities globally. The philosophy department of which I am a student, is facing examination in the next round of proposed funding cuts for the humanities at the University of Otago (Taylor 2016, Aug. 3). The popular scientists discussed are popular public figures, with wide audiences. A quick glance at numbers of views on youtube with comparably "famous" philosophers can illustrate this point[21].

§4.3 Progress as convergence to the truth

We should engage with these 'folk' metaphilosophical views, but thankfully we can find better formulated versions as expressed by philosophers themselves. We can tip our hat to the popular scientists, and lead the discussion towards better metaphilosophical views about progress, that state more carefully and more plausibly, views that are relevantly similar to what these 'folk' views have in mind. I do wonder why it is that such eminent individuals in the large and high-prestige fields of the sciences are so aggressive towards the small and low-prestige field of philosophy, but I leave these psychological curiosities to the side and focus on better formulated versions of scepticism about philosophical progress.

In a recent collection on the question of progress in philosophy, Eric Dietrich argued that philosophy makes none at all:

> Except for a patina of twenty-first century modernity, in the form of logic and language, philosophy is exactly the same now as it ever was; it has made no progress whatsoever. We philosophers wrestle with the exact same problems the

[21] A cursory glance comparing Peter Singer's most popularly viewed youtube video, a TED conference talk (212,954 views), compared to Neil deGrasse Tyson's *Everything Wrong with Gravity* (7,232,196 views) yields one such relevant statistic. The popularity of Singer's clip is unusually high compared to other notable philosophers, whereas Neil deGrasse Tyson's is typical of many other popular scientists.

> Pre-Socratics wrestled with. Even more outrageous than this claim, though, is the blatant denial of its obvious truth by many practicing philosophers. The No-Progress view is explored and argued for here. Its denial is diagnosed as a form of anosognosia, a mental condition where the affected person denies there is any problem.
>
> (2011 : 334)

Dietrich's article is charming and witty, and without wanting to attribute myself a psychological illness, I think it gives more of a colourful sociological account of philosophy, than it does to demonstrate that philosophy does not progress. However, at one point in Dietrich's article, he fleshes out the claim in a little more detail, explaining a feature that is taken as evidence for his claim that philosophy does not progress. Dietrich takes a lack of *convergence* to provide sufficient evidence that philosophy exhibits a lack of progress. When arguing for his no-progress thesis, Dietrich supports his argument by stating that "no philosophy theory is true, or at least no theory is regarded as true by [a] significant and large majority of philosophers" (2011 : 335). In the same collection of papers, Zach Weber suggests that progress is to be measured in terms of, or is otherwise attributable to, agreement:

> If philosophers started accepting each others' results, we would make progress. We could have proofs in philosophy -- say, Evans' proof that there are no vague identities, or Williamson's proof that everything that exists does so necessarily. We have, from Tarski, a nice formal answer to 'what is truth[?]'. But for any P, there is someone (sometimes that someone is me), who says not P. It is temperament that sets us apart.
>
> (2011 : 199)

Where Dietrich diagnosed that optimists about philosophical progress are suffering from a psychological condition, Weber attributes a pyschological factor in being causally relevant to a lack of convergence, arguing that philosophers are disagreeable by nature. Weber takes this lack of convergence a causal factor for philosophy's lack of progress. Performing a psychoanalysis of philosophers is beyond the scope of this

thesis, but each of these views seems to claim that convergence is a sufficient condition for progress.

Another view that progress and convergence are importantly related is outlined, but not endorsed, by Thomas Kuhn, in his *Structure of Scientific Revolutions*:

> If we doubt, as many do, that non-scientific fields make progress, that cannot be because individual schools make none. Rather, it must be because there are always competing schools, each of which constantly questions the very foundations of the others. The man who argues that philosophy, for example, has made no progress emphasizes that there are still Aristotelians, not that Aristotelianism has failed to progress.
>
> (Kuhn 1962 : 162-3)

It is implicit in this claim that progress is had by way of convergence, to singular philosophical schools. Weber's convergence is focused on convergence on results, which implies something like convergence on fewer propositions. Deitrich's focus is on convergence on theories. This shows that appeal to convergence alone isn't fully determinate, and a better account of progress as convergence will need to settle on which convergence is the relevant kind. A measure of progress that appeals to convergence of any of these kinds will need to do more than just this if they want to deny that schools of fundamentalist christians who take a literal interpretation of the bible do not come out on top of the sciences and philosophy in the progress polls.

In a more formal attempt to measure philosophical progress, David Chalmers (2015) extrapolates from results he gathered whilst co-conducting a survey of faculty at 99 leading departments of philosophy internationally (Bourget and Chalmers 2009). Chalmers' measure of progress in philosophy is "collective convergence to the truth" (2015 : 9). His proxy measurement for collective convergence to the truth is collective convergence to one philosophical position on the questions which he takes to be the "big questions" of philosophy. He argues that philosophy doesn't exhibit collective

convergence to singular answers to the "big questions", so concludes that it therefore doesn't exhibit collective convergence to the truth.

This measure of progress begs several questions. It is implicit in these arguments, that a) the sciences are convergent in a way that is relevantly similar to Chalmers' "big questions", b) that this kind of convergence is a sufficient measure of progress in the sciences, and c) that the progress of philosophy can be relevantly compared with the progress of the sciences using a commensurable measurement of convergence. I will argue that each of these assumptions is implausible, or at best, in need of much more clarification and evidentiary justification.

The problem with the first of these assumptions is that it assumes monism both about the sciences as a description of current practice, and as a regulatory aim for the sciences. In §3.4 of this thesis, I discussed Helen Longino's (2006) meta-analysis of the scientific studies of human behaviour. Each study Longino investigated used unique theoretical models, measurements, and hypotheses, and each resulted in different findings. Longino's careful analysis of these studies motivates the case for pluralism about human behaviour - that there is not one "true" description of what is causally responsible for human behaviour. Rather, there is a plurality of descriptions about human behaviour that each provide a useful contribution towards understanding different aspects of the complex phenomena that human behaviour comprises.

Insofar as these case studies reflect the general nature of the sciences, they demonstrate that the sciences do not present anything like convergence to simple and singular instances of knowledge. Pluralism, rather than monism, is an accurate description of the sciences at present. If pluralism demonstrates convergence in any straightforward sense, it is implausible that it aligns with Chalmers' measure of "big question" convergence. According to pluralists, "big" questions in the sciences are tackled by a plurality of systems of practice, often investigating different aspects of related phenomena. Chalmers assumes that an account of convergence can be specified in a determinate enough manner, and that a reliably operationalisable measure of it can be collected in its terms. Kuhn's convergence on 'schools', and Deitrich's convergence on

theories indicate that 'truth' might not necessarily play a central role in all ways of specifying the above matters.

§4.4 Pluralism about epistemic aims

During the last chapter I began to sketch a view regarding a plurality of epistemic aims. Not only is it the case that the sciences do not currently neatly converge to monistic answers to the questions they purport to answer, it is not evident that they are even attempting to do so. Newtonian mechanics has not disappeared from physics textbooks, even though it is considered to be, strictly speaking, false. The reason Newtonian mechanics is still taught, and the reason physicists are actively engaged within this system of practice, is because it is useful, despite being false. Science makes progress when it implements Newtonian mechanics, a theory it knows to be false, and uses it to send rockets to the moon.

Different systems of practice have epistemic aims other than truth, and their methodologies are often successful at realising these aims. To the extent that Newtonian mechanics can still meet epistemic aims, we ought to consider it progressive, and for quite good reason. From this example we can also see that epistemic aims might change over time. For some time, it had been quite reasonable to think that Newtonian mechanics was aiming to truthfully describe reality, but now that we know it can not achieve this, there is still other work that the theory can assist with. Hasok Chang's inaugural professorial lecture at Cambridge involved a nice example of various different systems of practice being combined to achieve something that seems to me to be patently progressive. His example provides a lovely description of plurality at work, by describing the workings of GPS technology:

> GPS uses satellites we keep in place by Newtonian physics, an atomic clock ruled by quantum mechanics and corrected by special and general relativity; it

> maps the surface of the round earth on a geostatic grid and gives advice to people on the ground from a flat earth point of view. It all works beautifully. (2012, October 11)

Contemporary uses of Newtonian mechanics are no longer trying to accurately describe the world, but they have plenty of other useful work to do. Chang encourages those engaged in the history and philosophy of science[22] literature to think carefully about their epistemic aims, spending some time spelling out precisely what they hope to achieve, and implementing various of the scientific methods in pluralistic fashion, in the hope that they can achieve greater progress (2012, October 11).

He goes on to mention one kind of pluralism already at play in HPS that has been an undercurrent throughout much of this thesis. Viewing the sciences through the lens of scientific practice, resulted in new and useful ways of thinking about scientific knowledge, illuminating that there is a plurality of epistemic aims that various systems of practice are striving towards. Viewing the sciences through the twentieth-century philosophy of science lens that focused on sciences as bodies of propositional content, allowed us to focus on and evaluate various methods of justification for scientific findings. With a pluralistic stance taken towards philosophy of the sciences, different points of interest can be isolated, or focused on. This amounts to progress in our understanding of the sciences. There is no need to think of viewing the sciences in terms of practice, or as bodies of propositional knowledge, as mutually exclusive competitors purporting to give the one true account of the sciences. Both perspectives have something insightful to offer. Chang has encouraged pluralism about methods for HPS, and this is an example of where pluralism can lead to progress in understanding. I think we have good reason to expect pluralism to be usefully applied more broadly to philosophy. If the methodological naturalist wants to align themselves with the sciences, in the hope of greater progress, a pluralism of methods is what they should foster, for that is what the sciences exhibit. This will take some conceptual shifting in the way we often think about different philosophical views within various domains.

[22] Henceforth, HPS.

To make this point clear, let's think about functionalism and the mind-brain identity theory for a moment. Perhaps it is incorrect to view them as competing theories of mind, each purporting to give the one genuinely true account of the mind. The brain and the mind clearly have a lot to do with each other, so it makes sense to be interested in the brain, if one is interested in explaining the mind. However, there are other aspects of the mind that one might be interested in asides from its material composition, such as the way the mind functions. This allows one to focus on human emotion and behaviour, merely another aspect of interest, in the plurality of insightful ways we could think about the mind[23]. Indeed, most contemporary research on Artificial Intelligence focuses on these functional aspects of the mind - it is not trying to physically replicate a brain - but that doesn't undermine the findings of the neurosciences in any way. Pluralism seemed an appropriate way to think about Helen Longino's meta-study of human behaviour, where it seemed wrong to think about the various systems of practice discussed as competitors for giving the one true account. Rather, each system of practice focused on its own object of inquiry, and generated knowledge of aspects of human behaviour in a patently scientific way.

As Chang encouraged practitioners of HPS to spend time carefully thinking about and articulating their epistemic aims, I encourage philosophers in general to adopt this approach. This will involve some conceptual realigning in terms of argumentative strategies. Typically, philosophical systems of practice involve both a destructive and a constructive aspect, whereby a thesis is defended as the one true answer to a philosophical question, via constructing a positive view of the object of inquiry, and also arguing why other theses in the same ball-park fail. On the view that I advocate, the constructive and destructive aspects of an argument can still be viewed as separately useful, but not necessarily related to one another. There is of course still merit in destruction, and falsification of theories is an obvious example of this. Pluralism allows us to take what is useful from Popper's falsificationism (1957), despite scepticism about the thesis that it tells us everything about science. Falsificationists think science makes progress when it rules out false theories, whilst never being able to have genuine

[23] It might give Paul and Mary Churchland a means to both truly claim to love each other, whilst also thinking the mind is made of the stuff that physics is interested in, without contradiction!

knowledge that our best theories are true. Indeed, if we can prove that a theory is false, we have generated knowledge, albeit knowledge of negative propositions. Just as we think we make progress cleaning our house when we discard leftover detritus, we can think of knowledge of falsehoods analogously.

But the thought that showing the falsity of one theory lends support to the idea that a "competing" theory is true will have to be very carefully investigated. Pluralism does not involve a commitment to thinking that there is never genuine competition between systems of practice. If genuine commensurability between theories can be shown, we have good reason to think that different theories might *really* be in competition with one another. But this will not be the case in all, and maybe even many instances. This provides an alternate hypothesis to Weber's suggestion that a lack of progress in philosophy can be attributed to the nature of philosophers' temperaments. Weber suggested that for any *P*, we have somebody who argues for not *P* (2011 : 199). Perhaps it isn't really the psychology of individual philosophers, and their never-ending desire to disagree that is responsible for a lack of progress. Rather, it may be the misguided assumption of monism that is the main contributing factor. More carefully, I suggest that mine and Weber's views are not genuinely competing with each other - perhaps Weber is right, and perhaps I am right as well. Perhaps an explanation of the causal factors of philosophical progress will look relevantly similar to Longino's study of various behavioural sciences, it seems plausible to me that there will be multiple causal factors involved.

If philosophers move away from thinking that in order to prove the worth of one theory, they have to also prove that somebody else's theory is wrong, and instead spend time carefully articulating their epistemic aims, looking for appropriate methods that they might implement in order to do so, looking at their opponent rather as a potential contributor to providing understanding about what might well be a complex phenomenon requiring a plurality of aspectival research, more progress might be had. I note that pluralism is practiced to an extent in various philosophical literatures presently, and a few examples include various debates in the philosophy of biology to do with units of selection (Wilson 2003), and definitions of species concepts (Mishler

and Donoghue 1982, Ereshefsky 1998), and the logical pluralist view that there is more than one good sense of logical consequence (Beall and Restall 2006). I propose that this programme be applied more widely across philosophical domains.

In the final chapter, threads of this thesis are drawn together in the form of my positive account of methodological naturalism as aspirational methodological pluralism. We should aspire to implement a plurality of suitable methods within philosophical practice. For now I suggest the following sketch from John Dupre as a starting point:

> [T]he best we can do is to draw up a list of epistemic virtues and apportion our enthusiasm for knowledge-claiming practices, to the extent that they meet as many as possible of such criteria. Such epistemic virtues will include certainly coherence with empirical data and with other things we take ourselves to know, and these virtues will be subject to detailed elaboration. They will surely include other things: perhaps aesthetic virtues such as elegance and simplicity, perhaps even moral virtues. There will no doubt be an unavoidable element of boot-strapping in the project.
>
> (Dupre 2012 : 26)

§4.6 Other contributing causal factors of progress

So far in this chapter I have argued that the accounts of progress I have discussed have begged the question of monism. Progress in the sciences is not obviously achieved by the realisation of truths by monistic research programmes, and thus using convergence to monistic programmes as a measure of progress is problematic as it doesn't reflect the true natures of the sciences. Even if the accumulation of monistic truths was a goal or achievement of the sciences, measuring the achievement of truths would be all but impossible without the super-power of omniscience. I have suggested that instead, progress might be more complicated and messy, and that various systems of practice might have different ways by which they can make progress through attainment of their

respective epistemic aims. Leaving this to the side for now, I note some other important considerations for any measure of intra-inquiry progress, be it pluralistic as I have suggested, or monistic, as assumed by the various commentators whose views I have discussed in this chapter.

Financial and human resources

The sciences receive far greater financial funding than philosophy does. This comes in the form of both public funding, and private funding. In private funding, the disparity is likely to be much greater than in public funding, which itself is likely to be greatly disparate. It would be most unfair to compare the progress of the sciences to that of philosophy, without taking this massive financial factor into mind. Various data will need to be gathered in order to calculate a measure of progress that reflects this. Given that the sciences receive financial support by orders of magnitude greater than philosophy, even if it could be established that they are more progressive than philosophy by some appropriate measure, this might merely be proportional to the human and monetary resources available to these domains. Expecting philosophy to progress as much as the sciences without factoring financial resources in, would be like expecting a philosopher riding a bicycle to win a race against a scientist driving a Bugatti Veyron Super Sport[24] car.

Further, the sciences have far larger numbers of humans involved in their enterprises, individually and collectively working to make progress. A measure of progress between the sciences and philosophy needs to factor human resources into account, in order to be a fair measure of output to input.

Hardness

In a recent popular piece, David Papineau presented a possible reason for philosophy's lack of progress when compared with the sciences, claiming that the methods or kinds

[24] These are currently the fastest cars on this planet.

of questions are not at issue, but that that particular philosophical problems are harder to settle than scientific ones:

> If you ask me, the relative inconclusiveness of philosophical debate does nothing to discredit the discipline. It is the natural upshot of the task facing philosophy. Most people don't enjoy banging their heads against nasty paradoxes. It's a dirty job, but someone has to do it. Given this, it is unsurprising that philosophical problems aren't easy to settle. The difficulty of philosophy doesn't stem from its peculiar subject matter or the inadequacy of its methods, but simply from the fact that it takes on the hard questions.
>
> (Papineau 2017, June 1)

I think that Papineau is close to touching on something that has been prominent throughout the discussions of this chapter. Perhaps we could plausibly interpret Papineau as saying that some parts of philosophy have epistemic aims whose achievement is not as easily determined as that of empirical adequacy. Having aims other than empirical adequacy is not unique to philosophy, and is certainly not alien to science, but there may well be something to Papineau's comment if we took it as a rough generalisation. My encouragement for philosophers to spend time thinking about and articulating their epistemic aims, considering appropriate methods to employ in the hope of success, can only help in the quest to measure progress in philosophy.

Chapter summary

In this chapter I discussed a number of expressions of pessimism about philosophical progress, both from popular scientists and from philosophers themselves. Many of the commentators who raised these criticisms assumed that progress is indicated by convergence to monistic truths. I question this assumption, and think that a measure of progress should at the very least not beg the question of monism, given the various arguments I have presented throughout this thesis. I've sketched the beginnings of a different conception of progress, whereby progress is measured according to how well a

given system of practice meets its epistemic aims. In the final chapter of this thesis, when presenting and defending my positive account of methodological naturalism, I spend some time articulating in a little more detail what at least some of these epistemic aims are likely to be. Once matters on this are clearer, we will have a better idea of how to measure progress. If scientific progress is to be compared with philosophical progress, we need to take into account important causal factors such as financial and human resources, and the relative importance and difficulty of achieving different epistemic aims. Some say that the pen is mightier than the sword. I say that this is not the case if your epistemic aim is the practical performing of surgery. It is unsurprising that a scientist has a better chance of landing a rocket on the moon than a philosopher throwing a pen, but it does not take a rocket-scientist to figure that out.

Chapter Five

An outline of aspirational methodological pluralism

Chapter preface

In this final chapter, I develop and defend methodological naturalism, which I suggest is best implemented as *aspirational methodological pluralism*. The methodological position that I advance, is to aspire to implement a pluralism of methods which will best suit specific philosophical systems of practice, given their epistemic aims. This has some implications for philosophical practice, although it doesn't amount to a significant reform of philosophy as currently practiced. The naturalistic starting point involved admiring the sciences, and then observing their practices. From a study of the sciences as practiced, pluralism is an accurate description of the various systems of practice exhibited. Given that different systems of practice will have different epistemic aims and objects of inquiry, further work will need to be done, carefully articulating these more precisely. This motivates both an epistemological programme focussed on aims, and a metaphysical programme focussed on objects, of various systems of practice. I defend the methodological pluralism that I advance from claims of vaguery, and relativism. This chapter is a mere sketch of methodological pluralism, and motivates much important further work.

§5.1 A sketch of methodological naturalism

I suggest that methodological naturalism should assume *relevance*. I define relevance as the working assumption that scientific methods, and the products of scientific methods, are, at least in principle, relevant to all areas of philosophical inquiry. I do not

think philosophy can justify the view that it has its own autonomous domain, in which it can maintain a unique and specific set of methods. Given pluralism in the sciences, it will not always be obvious where and when particular scientific methods, or the products they generate, will be relevant to a given philosophical system of practice. The articulation of epistemic aims of philosophical inquiries will be a good place to start, before looking to both philosophy and the sciences to inform judgements about which methods might be of use.

This motivates a large epistemological programme for philosophy, whereby specific systems of philosophical practice can specify their aims precisely, and look for the most plausible methodological candidates that will likely be useful for achieving them. It should not be expected that this can be achieved overnight. The successful methods will not be knowable *a priori*, but we can reasonably speculate hypotheses, with the best evidence we have available, and treat them fallibly. The methods of current philosophical practice should be retained, so useful comparisons can be made with others that we want to test. If new methods are developed, we should not straightforwardly assume that they are replacements for the ones with which we are comparing them. The sciences tell us that oftentimes having a plurality of systems of practice is the best way to describe aspects of some phenomena. Viewing the sciences through a plurality of lenses, such as Chang's practice centred science studies (2012), or through the more traditional lens of seeing sciences as bodies of propositional content, suggests that epistemology has more to do than presenting an account of propositional knowledge, but not that traditional methods of epistemology are inferior. Appreciating that there are a plurality of epistemic aims motivates the fact that we need more accounts of knowledge to accommodate them. This amounts to a richer pluralism than previously dominant views allowed, in the *objects* of knowledge (e.g. it's not only the knowledge-that constituted by the content of scientific theories), and there is more work to be done articulating the different aspects of knowledge that can contribute to a description of knowledge in this broader sense.

Thinking about knowledge in this pluralist sense, indicates a need to move away from *continuity* models of naturalism. Much of the discussion of naturalism in philosophy

has involved this model, with claims along the lines that philosophy is naturalistic by way of being continuous with science. The continuity metaphor is unhelpful. There is an historical continuity between philosophy and science that is easily recognisable, due to the fact that the sciences grew out of philosophy, as natural philosophy. Other kinds of continuity are not much more help - given the pluralistic nature of the sciences, continuity with some part of science comes cheap, and it is not obvious precisely which way philosophy and the sciences are supposed to be continuous. My worry here is that 'continuity' is too vague to be helpful. Unless carefully spelled out, it's no more illuminating than the observation that absolute zero is continuous with a million degrees centigrade. The methodological pluralism which I endorse is better thought about in terms of *alignment*. A philosophical system of practice can align itself with various scientific systems of practice in many different ways. That this will be a good idea will be a case by case matter, and precisely how it will be a good idea will be somewhat up for grabs. We can reasonably believe that aligning ourselves with the successful practices of the sciences will be a good idea, likely to yield new aspectival knowledge. Of course there are many ways in which philosophy and the sciences are already aligned. From the discussions of previous chapters, I draw together a number of methodological heuristics that will be a good starting point for my positive view.

Perhaps what we should do in the first instance is just to develop somewhat further an *account* of how best to proceed. In this chapter I outline some central elements in that account. Full spelling out of that account, and full evaluation of it, will then have to be largely business for the future. Any strong recommendation to implement it in the practice of the sciences and philosophy might also need to be endorsed or withheld well down the track, depending on just how those future developments, of first spelling out the account, pan out.

I offer a sketch of a metaphysical programme, which I call *aspectival realism*. Systems of practice are thought of as aspectival investigations, and I offer two different semantics that might be usefully implemented for understanding this view. This is developed further in section four of this chapter.

Throughout this thesis I have advanced pluralism, but I do not think this view has many consequences for the plausibility of monistic pursuits. There have been instances of theory-reduction and unification in the sciences, and we can be confident there will be more. Whether the sciences are ideally pluralistic or monistic is to be treated as an open question. The goal of monism is a noble one. If a unified theory of everything could be had, it would be a wondrous achievement. The take home lesson for monism, however, is that it should treat its research programme as an empirical hypothesis, not an *a priori* truth. A current description of the sciences shows us that they are pluralistic, and we have no good evidence to assume that the future will yield a monistic account of science. However, evidence either way will only accumulate through observing the success of both monistic and pluralistic systems of practice, and thus the pluralist should not discourage attempts at unification, or reduction, insofar as the monist can clearly demonstrate commensurability between whatever it is they hope to unify. One cautious suggestion, is that such attempts at unification or reduction should at first be attempted on much more local scales. That unification could be had in particular local areas is itself not obviously true, and the thought that it could be found on a much larger scale is far more difficult to justify at this stage in proceedings. This means we cannot assume that metaphysics can be justified in attempting to give complete foundational, or universally general accounts of the world. Nor can we assume that an ideal ethical theory can give us the one true account of morality that holds universally. This doesn't have negative implications for more local studies, however. In fact, it motivates a vast research programme attempting an understanding of the relationships between the various objects of inquiry from different systems of practice.

One criticism of my view is that the pluralism I advance amounts to a kind of relativism, nothing more than a Feyerabendian style "anything goes" (1975). I defend my view against these criticisms, borrowing the expression from Hasok Chang (2012), not that anything goes, but rather that *many things go*. A critic might respond that a fully satisfactory reply from me here will eventually need to say more about the natures of (at least some of) the things that *don't go*. I do illustrate in this chapter some ways in which certain systems of practice can be rightly criticised, and although pluralism about epistemic aims makes adjudicating a more murky enterprise, it does not make it

impossible. Besides, pluralism is an accurate description of the sciences as practiced. The methodological naturalist wanted to share the methods of the sciences due to their epistemic credibility, and the murkiness will be only as great as current scientific knowledge is; the very knowledge with which the naturalist started out with admiration and respect for.

§5.2 Aspirational methodological pluralism developed

The naturalistic starting point involved approaching the sciences with respect as epistemic enterprises. Observing the sciences as practiced yields a pluralistic description of the sciences, incorporating a vast array of systems of practice, many of which appear *prima facie* progressive. It seems plausible that incorporating as many learnings as we can attain by way of observing the sciences within philosophical systems of practice is a good idea, insofar as there seems to be a lot in common between these broad domains in terms of their being epistemic enterprises, seeking to build substantial and useful bodies of knowledge. The plurality of the sciences means implementation of their methods within philosophy is not a straightforward matter. How easy it would be if there was one genuine scientific method, and implementing naturalism would simply be using that very method. Broad statements such that philosophy is unscientific, or even more precise statements such as that metaphysics is unscientific, are simply too vague at such an abstract level to make sense of. One worry about the sketch of the aspirational pluralism that I provide in this chapter is that it's similarly vague. An important difference, I think, is that I am programmatically sketching a path of development for future work that will take the arguments of my thesis further forward. In contrast, those I am here calling to account are using vague lines of argument to attempt grand-scale dismissal of entire practices and modes of inquiry (such as philosophy). The appropriate demands of determinacy should be much higher for their putative weapons of mass destruction than for my programmatic sketches of future constructive work.

It is clear that philosophy already implements many methods that look patently scientific. Aspirational naturalism welcomes philosophical systems of practice to aspire to implement new relevant scientific methods. Will philosophical systems of practice fail? Surely, just as unsuccessful systems of practice have in the sciences.

Otto Neurath saw philosophy and science as being onboard the same boat, rebuilding its foundations whilst out at sea (1921). Quine was fond of this metaphor also, and uses it when arguing for his views about the continuity between philosophy and the sciences in his *epistemology naturalized* (1969). I think we can extend Neurath's boat metaphor, but think of different systems of practice as each having their own boat, rebuilding its epistemic aims and methods whilst at sea, and sailing in the various great oceans of knowledge. Different boats will be better suited for different conditions. As we can learn from yachting, sometimes boats will be better driven by different helmspeople. Some boats will hit icebergs along the way, and will become shipwrecks. The aspirational naturalist may not have the resources to win the America's cup, but paddling down a river in a kayak is an achievement in itself, and provides good exercise.

At various points of discussion throughout, I have recommended a number of plausible methodological heuristics that shape parts of my positive view, and I will summarise these below. The list is surely incomplete, and perhaps some of the heuristics may be misguided, but I suggest they are good starting points that require further follow up.

§5.3 Methodological heuristics for aspirational naturalism

Relevance

One important component of aspirational naturalism is *relevance*. In aspiring to use naturalistic methods, we take it that the methods of the sciences, and the products of those methods, are *prima facie* relevant to philosophical endeavour as a matter of

principle. We have no good reason to think otherwise, and if we are to embody naturalism in philosophy, we do so because we think philosophy and the sciences are relevantly similar in ways that learning from the sciences will assist philosophy to progress. Whilst it is not always obvious when particular scientific systems of practice will have useful crossover with philosophy, we think it likely that there will be relevant connections between the domains. A careful articulation of each system of practice's epistemic aims is a good place to start. If we find that a philosophical system of practice and a scientific system of practice share a number of epistemic aims, and if we observe that the methods exhibited by the scientific practice successfully realises its aims, it is reasonable to think that these methods might be relevantly applied to the philosophical system. The success of this implementation of methods can be evaluated by observing philosophical systems of practice, to establish whether certain methods are indeed relevant. Relevance does not entail that any scientific method or result will always be usefully implemented.

Continuity or alignment?

Literature on naturalism has been dominated by the continuity model, whereby naturalism is said to involve continuity between philosophy and the sciences. Quine (1969) spoke of continuity between philosophy and the sciences. Continuity does not do justice to the vastly complex nature of the sciences and the ways in which they stand to one another. The continuity metaphor implies that philosophy and the sciences are on the same level, but the pluralist denies that there is evidence to suggest there is anything meaningful beyond this metaphorical level-speak. Instead, the plurality of sciences exhibit a plurality of aspectival knowledge. Nature is far more complex than the continuity model allows, and thinking of the various ways in which philosophy and the sciences can be aligned is much better. The ways in which the recommendations of methodological naturalism can successfully be implemented in a given system of practice is better viewed in terms of alignment between philosophy and the sciences, rather than continuity.

Epistemic aims

The last two chapters have involved arguing that observing scientific practice yields a plurality of epistemic aims. Viewing science through the traditional lens of twentieth century philosophy of science tended to focus on just one of these: truth. Surely truth is an epistemic aim of various scientific systems of practice. Many scientific systems of practice attempt to accurately describe the world; physics involves attempts to describe the fundamental particles of the observable world, biology involves attempts to describe the complexity of the natural world, economics involves attempts to describe our monetary system and behaviours. And so on. Underdetermination of theory by data, and arguments from theory change in science show us that measuring the success of the sciences in terms of its attainment of genuine truths is problematic. That truth is one aim of some of the sciences seems obvious, but various scientific systems of practice are not trying to generate knowledge by discovering truths, and are rather aiming for other kinds of knowledge generation. Some scientific systems of practice adopt abstract models which they know to be false, doing so for many practical reasons, such as being measurable and thus quantitatively implementable, or because they function usefully for some other purpose, as Newtonian mechanics still does for us today.

In the last chapter I outlined a number of epistemic aims suggested by John Dupre as a starting place, and his list included the following: coherence with empirical data and with other things we take ourselves to know, aesthetic virtues such as elegance and simplicity, and moral virtues (Dupre 2012). Hasok Chang (2011) suggests we might also think about fruitfulness, usefulness, and operationalism (being measurable). From Popper we can borrow falsification or refutability (1957). Other aims, I suggest, could include repeatability, novel predictive success, innovation, and creativity. On the face of it, many of the successful scientific practices involved success by way of attaining these aims. Epistemic aims will be specific to a system of practice.

We can then follow Dupre's suggestion and apportion enthusiasm towards those systems of practice which successfully attain their aims (Dupre 2012 : 26). Further

work will be needed in determining what these aims are, for any given practice. It's not clear that they can in any simple empirical way just be read off the practice in question. Hasok Chang (2011) suggests a framework for analysis of epistemic activities which he intends to be a basis for evaluating scientific systems of practice. I suggest it can be usefully applied to philosophical systems of practice also, in terms of implementing methodological naturalism. He describes *coherence* as being an overall measure of the success of a given system of practice involving various factors, and this is explained in more detail in section four of this chapter.

As a means to viewing the sciences, Chang is establishing a growing body of work on epistemic aims and systems of practice as a means to view the sciences, and has begun to implement them himself, focusing on HPS as his object of inquiry. This material provides an excellent starting point for thinking about epistemic aims in a wider context for potential implementation in other areas of philosophy. This motivates much exciting and important new work for philosophy, of which the present outline is merely a very incomplete sketch.

Care around the assumption of monism

In philosophy, it is common to find arguments that attempt to establish the plausibility of a positive philosophical view by attempting to show why "competing" views are implausible. Observing the sciences has shown us that it is not always the case that different theories in a given subject area are in genuine competition with each other. I illustrated this point using Helen Longino's meta-analysis on behavioural sciences (2006), where she argues convincingly that looking at different scientific theories of behaviour as being competitors is sometimes misguided. Rather, it appeared there were a number of theories, each investigating different aspects of the complex phenomena that could be broadly described as behaviour. Throughout this thesis I have taken two different aspects of the sciences into consideration. Treating the sciences as bodies of

propositional knowledge - whose claims can be evaluated in terms of justification, and also taking a practice-centred approach - paying more attention to observing the sciences as they unfold, provide useful insights into different important aspects of the sciences. Both of these frameworks are useful ways of discussing the sciences, and shouldn't be thought of as competing views for giving a monistic theory of science in general.

From these discussions, a more general heuristic can be drawn. Care needs to be taken to decipher whether different philosophical theories are genuinely competing with one another, sharing the same epistemic aims, and focusing on the same objects of inquiry. It seems likely that this will sometimes be the case, but it shouldn't be assumed to hold in general, and careful attention to the motivations of different systems of practice will need to be given, before the general argumentative strategy that not *P*, therefore *Q* will be usefully applied. Such a strategy can succeed only if the two theories are genuine competitors. This doesn't inhibit deconstructive argumentative strategies in philosophy however. Showing that not *P* can still generate the knowledge that *P* is false, but that *Q* follows from not *P* will require careful argument.

Careful use of the term 'intuition'

In the third chapter of this thesis I urged greater attention to detail when using the term 'intuition' within philosophy. Cian Dorr (2010) suggests that often it is used as shorthand for indicating an assumption of one's argument. Timothy Williamson suggests it is often used to mean that somebody is making a judgement based on some evidence (2011). Others have criticised certain ways of doing philosophy for giving (what they think is) too big a role to intuitions (Maclaurin and Dyke 2012, Ladyman and Ross 2007, Weinberg, Stich, and Nichols 2001). Skeptics about intuition are right to question the epistemic status of a rational faculty that would allow the generation of facts by merely intuiting from the armchair. Dorr and Williamson are right, in that

people sometimes use the term intuition to mean something far less epistemically dangerous than what these skeptics think objectionable.

Part of a system of practice's careful articulation of its epistemic aims will involve spelling out explicitly and precisely what assumptions are being made, and will involve setting forth the epistemological programme by which it can hope to succeed. We could rightly be skeptical towards a system of practice that hoped to obtain facts about the world, simply by blindly intuiting whatever it pleased. Care with the use of the term 'intuition' is required, and if the term 'assumption' or 'judgement' could be used to more precisely denote one's intended meaning, using these terms instead is to be encouraged. Thought experiments, whether thought of as intuition pumps or as having some other methodological role should be treated analogously. Daniel Kahnemann's (2011) provides some evidence for when intuitions will be more or less reliable. This was discussed in more detail in §3.4 of this thesis.

§5.4 Implications of aspirational naturalism for epistemology and metaphysics

I briefly return to the topics of the second chapter, involving epistemology and metaphysics, and the relationships between methodological naturalism and these domains.

Epistemology

There are a number of implications for epistemology that result from aspirational naturalism that I have discussed at length already. Firstly, there is much work to be done carefully describing epistemic aims, appropriate implementation of methods, and the ways by which we might measure how a given system of practice succeeds in

meeting its aims. Employing Chang's *system of practice* framework (2011) for analysing the sciences is a start in an attempt to describe the various aims and methods within the sciences. We can employ this framework to analyse philosophical practice as well, and look for relevant comparisons between different systems of practice. A plurality of epistemic aims is available for worthwhile epistemological engagement. A practice centred approach steps away from the useful lens of viewing science as a body of propositional knowledge, and as such it sheds light on other kinds of knowledge. Applying this approach when taking philosophical systems of practice as our objects of inquiry might assist us to make more sense of notions of progress, which are presently ill-defined.

Metaphysics

I will now briefly sketch a metaphysical programme which I think is well motivated in light of the findings of this thesis, which I call *aspectival realism*. It is a view that is relevantly similar to Ronald Giere's *perspectival realism* (2006) and Hasok Chang's *active realism* (2012). Whereas Giere thinks of scientific knowledge as perspectival (as do Helen Longino (2015), and Massimi (2012)), I prefer to think of it as *aspectival*. JC Beall (2006) has argued for a kind of logical pluralism about the treatment of negation, which he calls *aspectival dialetheism*. Beall thinks there are two aspects of the behaviour of negation that need to be taken into account, and thinks that a classical treatment of negation works well for most occasions, but there are other occasions where negation needs a different treatment, for which he suggests dialetheism (i.e. logics which allow for contradiction) is more appropriate. At times Beall refers to his view as "double-aspect dialetheism" (169), and indicates that there are different aspects of negation that are better understood within different logical frameworks. The details of Beall's paper are not especially relevant here, but his usage of the term 'aspectival' is relevantly similar to what I have in mind with my aspectival realism. Indeed, I would suggest that Beall's stance towards the logical treatment of negation is a member of a broader set of aspectival realisms.

I suggest two different semantics for aspectival realism, treating knowledge claims counterfactually, or indexically. On one interpretation, we could think of the claim 'electrons exist' as a counterfactual claim, with the system of practice represented by the antecedent of a conditional, and the particular claim of interest as the consequent. For example, 'If the theory of physics is true, then the proposition 'electrons exist' is true'. If one preferred to not think in terms of truth, other epistemic aims may be used in its place. Hasok Chang's *active realism* provides a criteria of *coherence*, by which we might evaluate the antecedent of the counterfactual:

> The coherence of a system of practice has several layers to it: (i) Is each constituent activity of the system coherent within itself, in serving its inherent purpose? (ii) Do the inherent purposes of different activities constituting the system not interfere with each other? (iii) Are the presuppositions or implications of different activities consistent with each other? (iv) Are the external functions of the activities coordinated so as to achieve the overall aims of the system?
>
> (Chang 2011 : 74)

To the extent that a system of practice is coherent, we might apportion enthusiasm for the knowledge claims that are represented by the consequent of the conditional in this counterfactual treatment of aspectival realism.

Other semantics might also be usefully applied, such as treating the knowledge claims of a system of practice *indexically*. Here we think of knowledge claims as being relative to a particular index; the system of practice from which they are maintained. I suggest one interpretation of this indexical treatment using a centred-worlds semantics (for an application of centred-worlds semantics to indexical knowledge in general, see Egan 2006), where centred-worlds are ordered pairs $\langle o, w \rangle$, with an object, o, and a world, w. This semantics might plausibly be interpreted such that a world, w, is a system or practice, and an object, o, is an epistemic output of the practice. In this semantics, truth is achieved when w = a robust aspect of the actual world, and when the function $\langle o, w \rangle$ is satisfied. Of course worlds could represent a plurality of epistemic

aims other than truth, for the numerous systems of practice that are not remotely interested in achieving truth in the first place. Various systems of practice have much less lofty goals than finding some rock bottom truth about reality, such as building a weapon of mass destruction that could destroy the earth several times over, or better, curing cancer. So rather than judging the success of a given index by attempting to compare the degree to which it accurately describes reality, we can let worlds represent whatever epistemic aims we see fit. This is merely the beginnings of a sketch of aspectival realism, and much more work will need to be done to further develop the position.

§5.5 Defending aspirational pluralism from charges of vicious-relativism

In this thesis I have defended methodological naturalism as aspirational methodological pluralism, encouraging the implementation of a plurality of suitable methods. This involves encouraging different systems of practice to make contributions to the production of knowledge. It might be thought that this view collapses to a vicious kind of relativism, as is sometimes suggested problematic for pluralistic views in general. I have presented the view that *many things go*, but does this just amount to the claim that *anything goes*? What makes a given index a sufficiently credible one from which we could hope to generate knowledge? It might be thought that my view amounts to subjectivism, whereby anybody could construct any old index, and claim they can generate knowledge.

Firstly, anybody could construct an index, in theory, and indeed the scientific bodies of knowledge have been constructed through the hard work of humans. But that a particular knowledge claim is somehow relative to an index does not make is dangerously subjective. The following is an indexical proposition, in the classic linguistic sense of the term: 'My parents house is 273 kilometres away from my house'. It is an indexical proposition, which is (approximately) true when uttered by me, as long as my parents and I remain living in our current houses. It seems to me to be

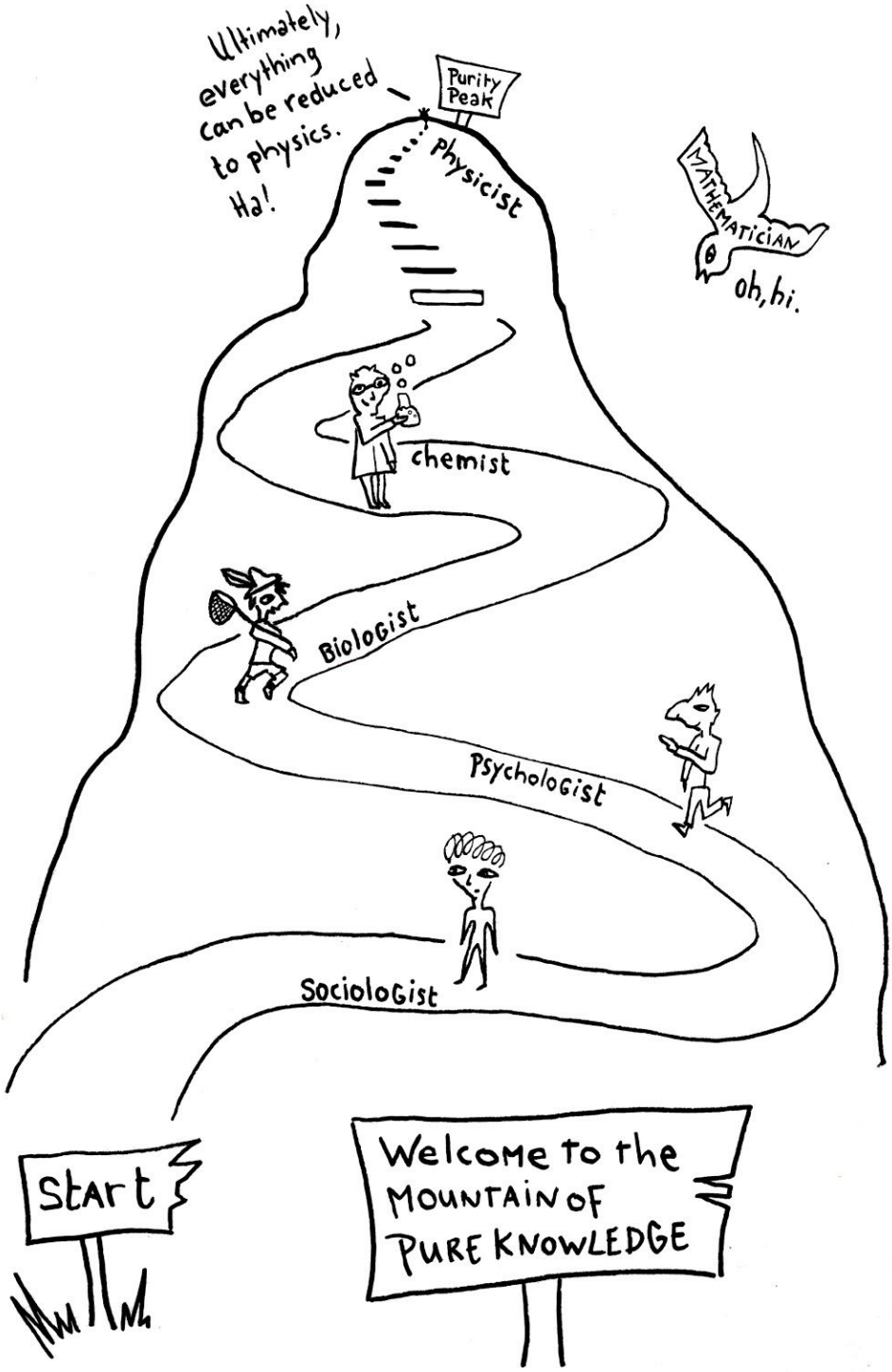
objectively true. It's not a fact that is up for grabs, despite involving theoretical assumptions of measurement, invoking the abstract concept of a straight-line, and representing the world in merely two dimensions. There is nothing viciously relativistic or subjective about this indexical fact.

Let's take a slightly different example. In medieval times, 'witch' was a concept said to refer to biological females with sadistic tendencies and metaphysical superpowers. It's possible that witches also had black cats and broomsticks as well, but of these details I am less sure. What's more, a measurement was proposed by which we could test for witchiness. So here we have a theory that has an object of inquiry - witches, that had epistemic aims such as determining witchiness, that had operationalised a means of measuring for witchiness using a proxy of buoyancy. If the suspected witch was buoyant when thrown into a river, and did not die by drowning, then indeed, they were a witch. The method of detecting witchiness by measuring buoyancy was testable, repeatable, and patently observational. It might be thought that viewing witch-detection as a system of practice could meet the various criteria I have outlined that need to be met in order that something be considered acceptable by methodologically naturalistic lights in the ways in which I have advanced.

But in terms of its robustness, or coherence as a system of practice, witch-detection can be quite easily dismissed. Witch-detection theory involved an aim of detecting witchiness, and its methodology involved measuring buoyancy as a proxy of witchiness. There are serious problems with the epistemological rigour of the claim that measuring buoyancy will also measure witchiness. There are serious problems with the metaphysical assumption that witches have supernatural powers. That a given proxy for measurement is a suitable one does not come for free. Part of evaluating the success of a system of practice is critiquing how well its methods bring about its epistemic aims, and there are serious flaws with the methodological programme that purports to measure witchiness by measuring buoyancy. This is why we don't see many scientific journal articles publishing witch-detection theory and publishing witchiness data.

The sciences as practiced are currently pluralistic, yet we do not think of the knowledge they produce as being dangerously relativistic. The naturalist first approached the sciences with respect for their epistemic credibility, not with distrust for their dangerous relativism. That there are many viable systems of practice does not mean that any system of practice is viable. The sciences operate well within this pluralistic framework, generating a plurality of knowledge. We can plausibly assume, as a working hypothesis, that implementing methods that have generated success in these relevantly similar epistemic domains might generate success in ours too. It seems to me that we have already implemented a number of these, and that they work pretty well. Could we do better? Let's try.

Appendix 1. Illustration by Suus Agnes, 2017. Reprinted with artist's permission.



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