Introduction

1 'What is philosophy?'

On 29 November 1912 the 23-year-old Ludwig Wittgenstein read a paper on 'What is philosophy?' to the Cambridge University Moral Sciences Club. The minutes say that his paper lasted 'about 4 minutes'[1] and that, in it, 'philosophy was defined as all those primitive propositions which are assumed as true without proof by the various sciences', adding that 'this definition was much discussed, but there was no general disposition to adopt it'. There still isn't. The definition is too rarely even considered, or its plausibility as a definition of metaphysics might stop that subject's content, feasibility, and utility being as contentious as it is (Chalmers, Manley *et al.* 2009 ch. 3). It might, for example, stop some philosophers denying that metaphysics can enhance a physics which does not ingest it (Ladyman, Ross *et al.* 2009), and stop others contradicting the entangled ontology of modern microphysics by denying that objects are ever more than the mereological sums of their parts (see chapter 11). A metaphysics that satisfies the young Wittgenstein's definition can and does add to his 'various sciences' without being either entailed by or inconsistent with them.

This applies not only to the empirical sciences but to logic and mathematics. No system of either can establish what Bob Hale (1996) calls the 'absolute necessity' of its theorems, because it cannot prove its basic axioms or rules of inference: there are no premise-free proofs. So they too need 'primitive propositions … assumed as true without proof' that must be argued for in other ways. That is why the necessary truth for all P of 'P or not-P' is as philosophically debatable as that of 'Water is H_2O': logics that assume or abjure the law of excluded middle can no more prove or disprove it than physics and chemistry can prove or disprove that water not only contains H_2O but is identical to it. It is why, in their preface to *Principia Mathematica* (1913), Russell and Whitehead say that

> … the chief reason in favour of any theory on the principles of mathematics must always be inductive, i.e. it must lie in the fact that the theory in question enables us to deduce ordinary mathematics.

[1] Two weeks earlier the Club had resolved that 'The whole object of the papers read shall be, as a general rule, to open a discussion, and therefore no paper shall last longer than seven minutes, except by previous permission of the Chairman on a special occasion'. Those were the days: I doubt if the Club's present practice, of letting speakers take ten times longer than the young Wittgenstein to say rather less, improves its discussions.

In short, while Russell and Whitehead do not take 'ordinary mathematics' to entail their philosophical theory of it, they do require that theory to entail and hence be consistent with it.

Of course, to ensure that our philosophical theory of a science is consistent with it, we must take care not to misread it, as some have done. Take the simple kinetic theory on which a gas's absolute temperature is proportional to its particles' mean kinetic energy. This theory does not say that even in gases, let alone in anything else, temperature *is* mean kinetic energy, which would imply, among other things, that all gas particles at rest are at absolute zero, which they aren't, and can be heated just by accelerating them, which they can't (see chapter 6.9). Philosophers should not keep asserting this evidently false identity when there are equally evident true ones: for example, that all light is electromagnetic (e-m) radiation, and that an object's inertial mass is identical with its gravitational mass.

2 Untestable identities

These two true identities illustrate how far physics, as well as metaphysics, exceeds what could be tested by observation. Take the identity of light with radiation. The undisputed evidence for this is that all light is accompanied by e-m radiation that shares its velocity, is reflected, refracted, and diffracted in the same way, and whose frequency distribution determines its colour. But this fits two theories. One says that light is *not* e-m radiation, merely correlated with it by deterministic laws of nature. To this, the identity theory, that light *is* e-m radiation, is an empirically untestable addition. Yet all physicists accept it for the simple and parsimonious explanation it gives of the correlation it entails. Similarly for the theory that gravitational mass, defined by the laws of gravity, is not merely correlated with but identical to inertial mass, defined by the laws of motion. Similarly also, and even less deniably, for the identity of inertial mass itself, as follows.

Newton's laws of motion say that any force F acting on any object o of mass M will give o an acceleration A proportional to F, and in the same direction, provided F does not alter M, e.g. by knocking bits off o. With this proviso, a 1 kg mass will be accelerated at 1 m/sec^2 by a force of 1 newton, at 2 m/sec^2 by 2 newtons, and so on, for infinitely many different forces and directions. In other words, o's having the mass M makes true infinitely many conditionals saying that F would accelerate o at $A=F/M$, all of which are logically independent.[2] So each of them *could* be made true

[2] Those who deny that conditionals like this are either true or false (e.g. Edgington 1986; Levi 2002) may replace 'makes true' by (e.g.) 'supports': it makes no odds to the argument. Nor do the facts that (a) to embody the proviso that F does not alter M, the conditionals must be reduction sentences (chapter 6.4) and (b) as the laws of motion bind M and F into a package where neither is independently definable, they need other theories, like Newton's theory of gravity, to make M and F empirically detectable by linking them to observable facts, like the orbits of the planets.

by a different property: with M_1 making 1 newton accelerate o at $1/M$ m/sec^2; M_2 making 2 newtons accelerate it at $2/M$ m/sec^2; and so on. A theory postulating this plethora of properties, with values made equal merely by a correlation, would again be empirically indistinguishable from the identity theory, that all these conditionals are made true by o's having the single inertial property M. Yet no one thinks that o has more than one such property at a time, or that there is no fact of the matter about how many it has. And since no physicist doubts that we can know that, and when, o has a single mass property M, no philosopher can claim scientific backing for a verificationism which entails that no one could know that.

3 Science and metaphysics

I have said that the metaphysics of a science should be consistent with it. But that is only because our beliefs cannot all be true if some are inconsistent with others. All an inconsistency shows is that something is wrong: it does not tell us what. In particular, it does not tell us that our metaphysics must always give way when it contradicts our science, though often of course it should. Mereologists, for example, should accept that a thing's parts can change over time, as the parts of our bodies do. They should accept too that irreducibly probabilistic sciences, from genetics to epidemiology, as well as the microphysics referred to in §1, show that the properties of few if any things with parts supervene on the properties and relations of those parts (see chapters 10 and 11).

But science does not always trump metaphysics. Take Richard Feynman's (1949) theory of positrons as electrons travelling backward in time. This rests on two facts: that positrons only differ from electrons in being positively rather than negatively charged, and that same charges repel and opposite ones attract. These facts make positrons move away from positive charges, just as electrons move toward them, so that a video of either movement, played backward, looks just like a video of the other played forward. Feynman's explanation of this assumes 'as true without proof' the metaphysical thesis that time gets its direction from that of irreversible processes, in this case those of electrons moving toward positive charges and away from negative ones.

This process is not the only so-called 'arrow of time' that has been said to give time its direction. Others include the expansion of the universe, the increase of entropy in isolated systems, and the divergence of waves from point sources. But none of them will do, because they all rule out reversals of processes that even physicists agree only *contingently* always or mostly go one way: the universe *could* contract, entropy can and sometimes does decrease in isolated systems, and waves often converge on points, as they do whenever eyes and camera lenses make light converge to form sharp images. (See chapter 15.)

Similarly, some electron-like entities both can and do move away from positive charges and toward negative ones. They are the ones we call 'positrons' and credit

with the positive charge that distinguishes them from (other) electrons, a distinction that Feynman's time-travel theory cannot explain: since if that theory does more than relabel the fact to be explained, it is false. A positron cannot, for example, be travelling backward in time as Dr Who's time machine TARDIS does: for if it did, electric charges that deflected it at a time *t* would affect its positions at times that for us are earlier than *t*; and they don't (see chapter 14 figure 4). Positrons are not backward time-travelling electrons in this or any other serious sense, any more than electrons are time-travelling positrons, or than the anti-clockwise clock I have is a time-travelling clockwise one. Backward time travel worth the name would not be that easy even if it were possible, which it isn't (see chapter 14).

My other example of metaphysically flawed physics is Martin Rees's 'multiverse' hypothesis (1997), that our universe is only one of many. This is meant to explain why our universe's constants and initial conditions are 'fine-tuned' for life, i.e. have the *a priori* very improbable values that permit supposedly intelligent beings like us to exist in it. The explanation is that, as a multiverse of universes with all physically possible values of these constants and conditions is bound to contain *some* universes we can live in, it is not improbable but certain that we will be in one of these. This is offered as a secular alternative to the theory that a supernatural designer gave the one and only universe the constants and conditions that let us exist in it.

Both theories misinterpret the *a priori* improbability of a universe containing life as a low *physical* probability, like that of a tossed coin landing on edge, which might make us suspect a non-chance process (see chapter 8). It is not: it is merely the low *epistemic* probability which the 'life' hypothesis is given by the mere forms of our universe's laws and a flat probability distribution over consistent values of their constants and its initial conditions. That the epistemic probability of this hypothesis is very low on that skimpy evidence and as high as it can be—one—on our actual evidence, which includes the fact that we *do* exist, is not a mystery that needs explaining. It is a triviality that neither multiverse nor design theories either can or should be asked to explain. And as with Feynman, what tells us this is not physics but metaphysics, in this case the metaphysics of probability.

The moral of these examples is not that scientists need to be metaphysicians, any more than metaphysicians need to be scientists. All each party needs to do is attend to the other, and not assume that, whenever they disagree, the scientists must be right and the metaphysicians wrong.

But where is the boundary, if all scientific theories contain presuppositions that Wittgenstein's definition makes philosophical? It would be as silly to say that this fact makes all scientific theories philosophical as to say that field theories, e.g. of gravity, which conform to the metaphysical principle of 'no (unmediated) action at a distance', make that principle scientific. Philosophy and science can be distinct without being independent, as Wittgenstein tells us. What he does not tell us is where to draw the line between them: precisely because, as he says, it is to be drawn, not *a priori*, but by what 'the various sciences' presuppose, which changes as

they do. Euclidean geometry, for example, was a metaphysical presupposition of Newtonian mechanics until the advent of other geometries, and of physical reasons to let light define straight lines in space, moved spatial and spacetime geometries from metaphysics to physics. Similarly with the developments in microphysics that replaced the seventeenth century's metaphysical atoms, first with the nineteenth century's chemical atoms and physical fields, and then with the twentieth century's quantum ensembles. This is another reason why metaphysicians are as ill-advised to ignore science as to surrender to it.

4 Methodology

The familiar fact that metaphysical theories often turn into scientific ones does not, *pace* Ladyman *et al.* (2009), imply that metaphysics is, or ever will be, redundant: Wittgenstein's 'various sciences' will always need presuppositions. But this does not require the methodology of metaphysics to differ from that of the sciences. The methods of the sciences vary, of course, with their subject matter: biologists have as little use for telescopes as cosmologists have for microscopes.. More generally, logic, mathematics, and metaphysics respond less, or less directly, to observational data than empirical sciences do. But this does not stop all these subjects sharing an objective that their methods are meant to achieve: namely, to generate and support theories that will explain the apparent facts of their domains—'apparent' because theorists who cannot explain some of these facts may trade scope for success by denying or excluding them.

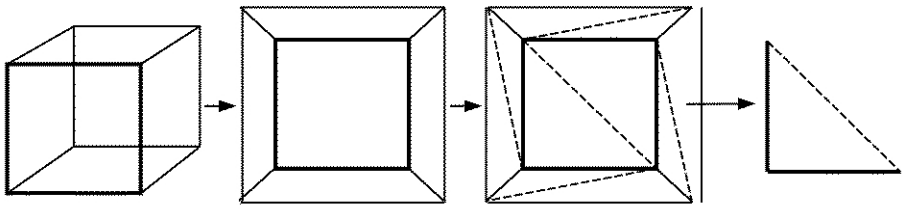
Take for example Euler's conjecture, that for all polyhedra, e.g. the cube in Figure 1(a) below,

(EC) $V–E+F = 2$,

where V is the number of vertices, E the number of edges and F the number of faces. This apparent fact may be explained by the following theoretical deduction (DEC) (Lakatos 1976 ch. 1.2).

First, turn the cube (a) into the 2-D figure (b) by stretching and flattening it after removing a face, thereby reducing $V–E+F$ by 1. Next, add a diagonal 'edge' to each face, thereby adding as many faces as edges (c). This leaves $V–E+F$ unchanged, as does the final step, of removing in turn all but one of the triangles so formed, since each removal reduces F and E by 1 (or, at the end, V and F by 1 and E by 2). Then since, for the remaining triangle (d), with three vertices, three edges and one face, $V–E+F = 1$, it follows that (EC) is true of the cube (a).

Whether (DEC) explains (EC) at all is a very moot point, but not the one I want to raise. The issue here is whether (EC) is refuted by such apparent counter-examples as a cylinder, with three faces, two edges and no vertices, for which $V–E+F = 1$ and to which (DEC) cannot apply, since its faces cannot be divided into triangles. To reject this counter-example we must deny that a cylinder *is* a polyhedron and hence



(a) $V–E+F = 2$ (b) $V–E+F = 1$ (c) $V–E+F = 1$ (d) $V–E+F = 1$

FIGURE 1: Deduction of Euler's conjecture

a shape to which (EC) and (DEC) apply. And failing a better theory that applies to cylinders too, it may be as reasonable to protect (EC) and (DEC), by limiting their scope in this way, as to protect the gills theory of how fish breathe by denying that aquatic mammals are fish.

On a more serious and positive note, one virtue of a theory of continental drift is its ability to explain otherwise inexplicable and hence suspect similarities between land-bound animal species, e.g. in the Americas and Africa, as an effect of past migrations. Similarly in metaphysics: theories of causes and effects as facts, that put events (e.g. a movement) and non-events (a staying still) on a causal par, can credit the latter with causes and effects more readily than theories which, for other reasons, limit causes and effects to events (see chapter 5.2–3).

In short, the criteria of scope and success used to judge metaphysical theories are the same as those used in science and mathematics. There is nothing peculiar about the methodology of metaphysics. Indeed there is very little to it beyond a few platitudes that apply equally to all secular non-fiction: admit no unchallengeable authorities; write relevantly, clearly, and concisely; don't assert or infer what you don't believe; and don't use jargon or be needlessly technical.

Not only is the methodology of metaphysics neither special nor problematic, it helps its practitioners as little as that of most other subjects helps theirs. No one, I trust, thinks studying the methodology of drama would have made Shakespeare a better playwright; merely a less prolific one. The methodology of science is no more helpful to scientists, not even in scientific revolutions. For even then, what rescues or replaces a faltering theory, like classical mechanics in the nineteenth century, is usually not a methodological but a metaphysical thesis: for example, that the same laws of nature apply everywhere and always to all things, whatever their relative motions: a thesis which, if the laws give something (e.g. light) an invariant finite speed, entails the Lorenz transformations of special relativity (Minkowski 1908 p. 79, Lange 2013).

However, as no one is perfect, and authors in different subjects may fall short in different ways, a few peculiarly philosophical bad habits may be worth deploring.

One is that of relying too much on analysis, i.e. on clarifying our concepts without questioning factual assumptions built into them. To take a reported example of William Kneale's, it is built into our concept of kittens that they are (a) the offspring of cats and (b) grow into cats. That is mostly true, but what makes it so is biology, not our concepts. And if cats evolved from simpler organisms, as they did, then (a) and (b) cannot both be true of all of them. Some ancestors of cats were not cats, just as some of our ancestors were not human beings. Similarly, the logically possible 'hypothesis that the world sprang into being five minutes ago, with a population that "remembered" a wholly unreal past' (Russell 1921 p. 159), is not refuted just by our concept of the world being of something that is millions or at least (for Biblical literalists) thousands of years old.[3]

The second bad habit on my list is that of requiring philosophical disputes to be won by a knockout, i.e. by showing that no rival theory *could* be true. While that may happen in logic and mathematics—though if 'P or not-P' is disputable, what is not?—it rarely if ever happens in philosophy or the empirical sciences. Cartesian scepticism, for example, can no more be absolutely disproved than can creationism, or the theory that Christopher Marlowe wrote Shakespeare's plays. But that is no excuse for being, or posing as, a Cartesian sceptic, a creationist or a Marlovian: for in all these cases other theories—that there *is* an external world, that species *do* evolve, and that *Shakespeare* wrote the plays published in his name—win, if not by a knockout, then decisively on points. Of course it does happen in philosophy, as in science and history, that even the best theory of a subject faces serious objections; but that is no excuse for having equal credence in others that face worse ones. To vary the sporting metaphor, no sane bookmaker will offer the same odds on every horse in a race, and nor should we on every competing theory of a scientific or philosophical subject.

This brings me to a third bad habit, of wasting too much time on theories no one believes, like Cartesian scepticism. I do not mean it is never worth discussing what is wrong with arguments for such theories, only that their internal consistency is not a sufficient reason to take the theories themselves seriously. Why this seems to happen more often in philosophy than elsewhere, if it does, I am not sure, but I can see no need or excuse for it. Evolutionists and Shakespeareans have real opponents, as I do when I argue in chapter 14 against those who think backward time travel is

[3] Compare Thersites's comment on Troilus's reaction, in Shakespeare's *Troilus and Cressida*, to his eavesdropping on Cressida's night-time tryst with Diomed:

TROILUS: This she? no, this is Diomed's Cressida:
If beauty have a soul, this is not she;
If souls guide vows, if vows be sanctimonies,
If sanctimony be the gods' delight,
If there be rule in unity itself,
This is not she.

THERSITES: Will he swagger himself out on's own eyes?

possible or even, as we saw in §3, actual. These are serious debates about what to believe, which debates about Cartesian scepticism are not.

The cure for this habit is, of course, to heed one of the platitudes listed above. For since philosophy, like logic, science, and mathematics, is not a branch of fiction, philosophers have no academic (as opposed to personal, clinical, or political) excuse for asserting things they do not believe: e.g. 'There is no external world', 'Language is not used to describe the world', 'We can always create an entity by creating a name for it', or 'Animals cannot feel pain because they have no concept of pain'.

5 Meta-philosophy

'I have far more confidence in the one man who works mentally and bodily at a matter than in the six who merely talk about it' (Michael Faraday, Letter to John Tyndall 19 April 1851, in F. A. J. L. James (1999) *The Correspondence of Michael Faraday*, vol. 4 letter 2411).

The last bad habit on my list is that of discussing philosophy instead of doing it, i.e. of doing meta-philosophy, or the philosophy of philosophy, whose remit I take to be what philosophy is and how to do it. About what it is, I think Wittgenstein's definition in §1 says all that need or can usefully be said. The topic of how to do it I take to comprise its methodology, discussed in §4, and its methods, which, like those of other subjects, are best mastered by emulating exemplars. That after all is why philosophers, like scientists and mathematicians, as well as historians, lawyers, artists, and motorists, mostly start as apprentices, learning their trade by doing it under supervision: these days, in discursive subjects, usually by writing theses that assess and add to existing work *in* that subject, not meta-work *about* it.

It is a fact that this process, of mastering a subject by learning how to do it, rarely needs to include learning a rationale for its scope and methods. Hence the lack of interest of most practitioners of most subjects in their subjects' definition and methodology. Why then is philosophy different, as it seems to be: why do so many philosophers indulge in meta-philosophy? Perhaps because, unlike the philosophy of other subjects, it is a part of the subject it is about. But that is hardly a good enough reason to do it unless it is an enlightening or important part, which I have never found it to be.

Nor is it necessary, since we can do philosophy perfectly well without doing meta-philosophy, just as we can do other subjects without doing their philosophy. But not *vice versa*, because philosophy, like mathematics and languages, is not a spectator sport: its products, unlike those of chefs and poets, can only be judged by those who can do it. So would-be meta-philosophers must first learn to do first-order philosophy. And anyone who can do that, i.e. add seriously to metaphysics, ethics, aesthetics, political philosophy, or the philosophies of science, mathematics, logic, language, history, or law, has better things to do than meta-philosophy.

Finally, to the charge that this introduction is itself a piece of meta-philosophy, I can only reply that it is meant to be aversion therapy, i.e. to do just enough meta-philosophy to deter readers from doing any more. It may well fail, just as my and Tim Crane's 'There is no question of physicalism' (1990) failed to stop colleagues wasting time on that question. But our failure did not make what we said false, not worth saying, or self-refuting, and nor is what I have said about meta-philosophy. Anyway, I have not said much. I have never been guilty of lecturing, or editing or writing a book, on the subject. And there is no more of it in this book: what follows, for better or worse, is just philosophy.