The Multiverse: a Very Short Introduction

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Chapter 2: Physics and Philosophy from 1600 to 1900

The main aim of this Chapter is to review some aspects of philosophy and of physics in the three centuries from 1600 to about 1900. That may seem a tall order. But we will only need those aspects that will help me explain how by about 1970, both philosophy and physics were "ripe" for formulating the three multiverse proposals, on which Chapters 3, 4 and 5 will focus. So those Chapters will also review developments in the twentieth century. (The time-frames will vary between these three cases. For example, the relevant developments in logic and philosophy cover a century, from 1870 to 1970; while for cosmology, the period will be some sixty years, from 1910 to 1970.)

These historical aspects, reviewed in this Chapter, and in the early Sections of Chapters 3, 4 and 5, are not just stage-setting. They will also help us to assess the multiverse proposals.

For philosophy, this Chapter will mostly be about how a modest conception of scientific enquiry, separated from the idea of necessity and from the framework of logic, emerged in the eighteenth century. For physics, the Chapter will mostly be about the rise of mechanics.

I begin with the tradition of natural philosophy: a tradition from which both physics and philosophy, as we now conceive those disciplines, sprang. This will lead in to the rise of mechanics, especially Newton's mechanics. I will emphasize how accepting action-at-a-distance in Newton's theory of gravity paved the way to the modern, modest---one might even say: pessimistic---conception of what it is to understand the natural world. At the end of this Chapter, we will see that this conception, and the emergence in the nineteenth century of the distinction between applied mathematics and pure mathematics, contributed to the rise of logic as central to philosophy. These factors also prompt a philosophical question, 'What is pure mathematics about?': which has been at the centre of twentieth-century philosophy.

Chapter 2, Section 1: The tradition of natural philosophy

'Natural philosophy' is a venerable phrase. It refers to enquiry into the natural world. It encompasses enquiry that is empirical, including experiments as well as everyday observation; and also enquiry that is conceptual, including using quantitative e.g. mathematical methods. It is especially associated with the seventeenth century. Figures such as Galileo, Descartes and Newton all saw themselves as engaged in natural philosophy. Indeed, Newton's masterpiece that propounds his theory of mechanics and gravitation (published in 1687) is entitled: 'The Mathematical Principles of Natural Philosophy'. But this field of enquiry goes much further back, to ancient times: as the seventeenth century thinkers of course recognized. Thus when figures such as Aristotle, Lucretius, Aquinas, Galileo and Newton asked what was the nature of space, or of time, or of matter, or of causation, they shared a common field of enquiry---however great the disagreements of their resulting answers.

Various developments from the eighteenth century onwards broke up this intellectual unity between philosophy and what we now call 'the sciences', especially physics. The parting of the ways is symbolized by the invention of the word 'scientist' (by Whewell in 1834), and the phrase 'natural philosophy' falling completely out of use by the late nineteenth century. (But the phrase remains, even today, in some British universities' title of one of their Professorships of physics. This is, for anyone enthusiastic about the connections between physics and philosophy, an evocative reminder of yesteryear's synergy between the disciplines.)

The broadest of these developments that broke up the unity might be summed up as: the growth of knowledge. For our purposes, the main development within physics was the establishment of Newtonian mechanics as describing with increasing detail, and quantitative accuracy, not just astronomical observations but also many terrestrial phenomena. (I will give some more details below.) As the eighteenth century went on, this success increasingly used technical notions and advanced mathematics: which of course made for intellectual specialization.

Agreed: great figures in physics, such as Euler, continued to write natural philosophy: as did some great figures of nineteenth century physics, such as Helmholtz, Maxwell and Mach---despite the explosion of knowledge within physics, during the 1800s. But broadly speaking, the increasing specialization of physics over the course of these two centuries, 1700 to 1900, meant that such writings became less central to physicists' detailed researches.

So much by way of a lightning summary of how physics grew away from philosophy between 1700 and 1900. During the same period, philosophy also grew away from physics, and more generally from science. This was not only due to the obvious point that philosophy is about so many topics other than the natural world and our knowledge of it (such as moral and political philosophy). Also there was, even within metaphysics (i.e. the theory addressing the general nature of all entities) and epistemology (the theory addressing what knowledge is), a self-conscious turning away from the details of physics, and of science. This occurred as part of the legacy of Kant (and so it occurred especially in German philosophy). The reason in short is that Kant announced a new and ambitious conception of metaphysics and epistemology that rendered them autonomous from other disciplines, and in particular independent of the details of the sciences. Though Kant himself wrote a lot of natural philosophy, German philosophy got more and more separated from science after his death, in the work of figures like Fichte and Hegel. (In my opinion, it also got more and more obscure and high-falutin'.)

Then in the early twentieth century the quantum and relativity revolutions erupted, and brought tumult to physics; (more details in Chapter 4). The controversies about how to develop, and even how to "just" understand, these new theories threw physicists back to addressing basic conceptual questions, like those I listed above: what is space, what is time, what is matter, what is causation? The ensuing debates in which philosophers, especially in Vienna and Berlin (which were centres of the new physics), took part, had an enormous influence on philosophy. Indeed, they moulded the logical positivist movement; and this led to the idea of philosophy of science as a sub-discipline of philosophy. In this way, natural philosophy---under the new name 'the metaphysics and epistemology of the sciences'--- became again, by the mid-twentieth century, a research subject.

We will see later (in Chapter 4) how since 1970, the metaphysics and epistemology of physics has really taken off: being nowadays called 'the philosophy of physics'. But in this Chapter, I will develop three themes from natural philosophy in the earlier period, between 1650 and 1900. We will need these themes in order to understand our multiverse proposals, especially the first two proposals (from philosophy, and from quantum physics).

The first theme is about what the natural philosophers believed. The second is about their optimism that, by adopting their views, humans could achieve an understanding of the natural world that was completely clear and satisfying. Here my point will be that this optimism was dashed by the success of Newton's physics, and indeed, by Hume's philosophy. The third theme is the status of logic during these three centuries. In short, logic was for most of this period in the doldrums; but from the mid-nineteenth century, it was vigorous.

Chapter 2, Section 2: The mechanical philosophy

Several of the greatest natural philosophers of the seventeenth century believed that all the processes in the natural world would ultimately be explained in terms of objects' parts, including their tiniest parts, interacting with one another by causal "pushes and pulls". Here, the phrase 'the natural world' was to include biological processes, such as the growth of plants. For some of these authors, it was to include also psychological processes, such as perception, both in humans and in animals. And for most of these authors, the causal "pushes and pulls" required that the two objects (usually called 'bodies') touch one another, i.e. be in contact, like the gear-wheels in a machine such as a windmill. Hence the phrase 'mechanical philosophy'. Other jargon: the principle that there could be no interaction without contact came to be called 'the principle of contact action'. Another slogan for the same idea was that there should be no 'action-at-a-distance'.

Advocates of this view included Galileo, Hobbes and Descartes. They knew, of course, that their view had ancient precursors: in particular, the ancient atomists, Democritus and Lucretius, who maintained that matter ultimately consisted of small indivisible lumps in a void (vacuum). Thus Democritus (ca. 460-370 BC) said: 'The first principles of the universe are atoms and empty space; everything else is merely thought to exist . . . Sweet exists by convention, bitter by convention, colour by convention; atoms and Void [alone] exist in reality.' (Here of course, 'by convention' means something like 'as a result of how humans' sensory organs happen to work'; rather than the modern meaning, viz. 'by a human decision that could have been made otherwise'.)

Of course, the mechanical philosophers disagreed amongst themselves. Some, such as Descartes, denied that there is a void (vacuum). They said instead that matter fills space completely (even on the tiniest length-scales), and what seems to be empty space, e.g. between the planets, is filled with a "very thin" fluid through which solid objects pass easily, rather like a boat through water. And some (including again Descartes) denied that all biological and-or psychological processes could be thus explained. So they limited the scope and ambition of their mechanical philosophy to explaining, in terms of contact-action, all processes in the inanimate world: or all processes in either the inanimate world or within organisms that are not sentient.

Nowadays, the conception of matter as lumps in the void is the everyday conception. It is so-called 'common sense'. For we all learn in primary school that matter is made of atoms, which are separated from each other by empty space i.e. a vacuum. (Agreed: our atoms differ from Democritus', in that they can be divided.) But it is important to recognize that this vision was hard-won over the centuries. At the time, the mechanical philosophers' vision seemed not just radical, but thoroughly implausible.

Indeed, there are four points here.

First: to the extent that the lumps-in-the-void conception of matter is true, it is not at all obvious that it is true. This is so even for inanimate objects, nevermind the objects involved in biological and psychological processes. It is far from obvious that air and other gases are mostly empty space; or indeed that liquids and solids consist of tiny particles jostling each other, with interstices between them. On the contrary, the naïve appearance of all the gases, liquids and solids we see around us is that they consist of matter that fills space completely (even on the tiniest length-scales): such matter is called 'continuous'.

Second: even if we accept that all matter consists of tiny lumps in a void, these philosophers' other claim---that all processes can be explained as accumulations of microscopic interactions, "pushes and pulls", occurring when the lumps are in contact---by no means follows. Indeed, it seems a radical, even thoroughly implausible, speculation. One naturally asks: how could all the variety and complexity of the processes we see, including biological and even psychological processes, be explained in such simple terms? It is really only in the last hundred years or so that the claim has become believable---indeed well-confirmed by countless pieces of detailed evidence. Think of how the existence of atoms, and the way they compose molecules, was understood only in the early twentieth century, together with how they explain chemical processes. And the understanding of biological processes, such as how muscles contract, how nerves send signals, or how genes are passed from parent to offspring, came only with the rise, in mid-twentieth century, of physiology and molecular biology.

There is, of course, another aspect to these achievements. Namely, they require much more subtle interactions between the tiny lumps in the void than the phrase "pushes and pulls", i.e. collisions or impacts, suggests. In fact, they require quantum physics with its strange features.

A classical physics of "tiny billiard balls bouncing off each other" certainly <u>cannot</u> explain all these processes. But I postpone quantum physics till Chapter 4. At the moment, I want to stress two other ways in which the claims of the mechanical philosophers were radical: and so, at the time, hard to believe. Again, the moral will be that we should recognize that their later acceptance was not "common sense", but was hard-won. So these will be my third and fourth points.

Third: the mechanical philosophers fashioned concepts with which to describe with quantitative accuracy the contact, and collisions, of bodies. I mean concepts such as velocity, acceleration, mass, momentum, and energy. Nowadays these concepts are everyday notions: broadly familiar from e.g. car travel, and taught in detail in school. But we should remember how non-obvious they are. For example: it took decades for physics to settle on each of the following. (i): The idea that acceleration is change in velocity during a given interval of time; rather than change in velocity during traversal of a given distance. (ii): The idea that mass is an intrinsic property of a body, different from its weight, measuring its resistance to being accelerated. (iii): The idea that although in most cases the sum of bodies' momenta is evidently not conserved (i.e. constant over time) before and after a collision---think of a car-crash, or more safely, of throwing two marshmallows together---nevertheless, it is useful to consider the special, simple cases where the sum of the bodies' momenta is indeed the same before and after the collision.

Fourth: So far, I have summarized mechanical philosophy's claims that matter consists simply of lumps, and that mere contact-action among such lumps (especially tiny ones), theorized in terms of momentum etc., can explain the great variety we see around us. And I have praised these claims as bold and implausible, at the time---but vindicated by history. But I confess: I have over-simplified. There is an elephant in the room. Namely, Newton and his theory of gravity.

<u>Chapter 2, Section 3: Newton's theory of gravity: unbelievable?</u>

For Newton's theory <u>denies</u> the principle of contact action, that bodies cannot interact unless they are in contact. And in its description of gravity, the theory's replacement for this principle could not be more precise. What came to be called 'Newton's universal law of gravitation' says that any two bodies at any time attract one another with a force along the line <u>between them</u>. (The law also says how the force decreases with the distance between them, and how it depends on the bodies' masses. I will discuss these other features in a moment.)

So the law proclaims <u>action-at-a-distance</u>: exactly what Galileo, Descartes and the other mechanical philosophers denied. It is indeed very hard to believe. Take as the two bodies, the Sun and the Earth; and think of how light takes eight minutes to travel from the Sun to the Earth. Newton's law says that if somehow you could shift the entire Sun during the course of, say, a minute, by some distance, say a thousand miles, then the direction along which the Sun pulls the Earth would be different at the end of the minute---before the light arrived. Indeed, the direction would change instantaneously during the course of the minute, sweeping through the sky like a lighthouse-beam. Of course, the angle through which it sweeps would be tiny, because the Sun-Earth distance is so much greater than a thousand miles. But that is only because I imagined shifting the Sun by a "modest" astronomical distance. If instead I had imagined shifting the Sun by, say, half the radius of the Earth's orbit, then the direction of the Sun's pull would have changed---again, instantaneously during the course of the shift---by about half a right angle, i.e. 45 degrees. So the important point is that the change in direction is instantaneous.

This law is all the more hard to believe when one notices that it claims this instantaneous attraction-at-a-distance occurs between <u>any</u> two bodies; for example, between an apple and a planet.

Besides, the law says that the attractive forces are <u>equal in size</u> (though opposite in direction). So the apple pulls on the planet with exactly the same "strength" that the planet pulls on the apple. That is well-nigh incredible. In particular, notice that it goes far beyond the familiar anecdote about Newton seeing the apple fall and thinking of the Earth as pulling the apple down. In that anecdote, as it is usually told, there is no suggestion that the apple pulls the Earth upward---let alone with equal force.

Newton's explanation of our not noticing the Earth's upward acceleration is that the Earth's mass is so vastly greater than the apple's mass, that its acceleration is correspondingly smaller. For according to Newton, force = mass times acceleration. So the equal and opposite forces make the small mass of the apple accelerate enough for us to observe and measure; but they make the vast mass of the Earth accelerate only a minuscule, and unobservable, amount.

What are we to make of all this? As regards physics and its history, the verdict has been, in short: gradual acceptance that was eventually so entrenched that by about 1800 or 1850, physicists were sanguine, even placid, about the idea of action-at-a-distance, and about the idea that the gravitational forces between any two bodies are equal in size (though oppositely directed).

Let me spell this out in a bit more detail. Initially, Newton's readers were unbelieving. Besides, Newton himself wrote that he had endeavoured to find a means, a mechanism, by which gravity acted, and had been unsuccessful. (I will shortly return to this admission.) He also argued, with considerable justice, that as regards the gravitational force between each planet and the Sun, the detailed astronomical observations he had in hand (especially those encapsulated in what became known as 'Kepler's laws') meant that he could <u>deduce</u> that the gravitational force acted along the instantaneous line between the planet and the Sun.

Furthermore, he could <u>deduce</u> that the force decreased with distance in the precise way his law of gravitation said. (Namely, by what is called 'the inverse-square'. This means: doubling the distance reduces the force by a factor of 4, i.e. multiplies it by a quarter; tripling the distance reduces the force by a factor of 9, i.e. multiplies it by a ninth; and so on.)

This quantitative precision of Newton's theory meant that, once combined with astronomical observations, it made precise predictions about the planets' movements. Many of which were tested in the decades following his theory's publication (i.e. after 1687), and they turned out to be true. These successes depended of course on developing the calculus that Newton and Leibniz had invented, and applying it in ever greater detail to mechanics and astronomy. (These successes were mostly achieved, not in Britain, but in continental Europe by figures such as Euler and Lagrange.)

Thus by about 1800, most natural philosophers accepted the claims that gravity involved action-at-a-distance, and equal and opposite forces that decreased with distance according to the inverse-square. And so it went. The theory garnered more and more successes; so that by 1850, physicists---by then professionally identified as such---were sanguine, even placid, about these claims.

Agreed: there were dissenting voices, such as the physicist-philosopher Ernst Mach. And calm comes before a storm. In the early twentieth century, Einstein (inspired in part by Mach's misgivings) created an amazing new theory of gravity, which he called 'general relativity' (1915). According to this theory, there is no action-at-a-distance. Gravitational influence <u>does</u> take time to propagate across space: namely, it travels at the same speed as light.

In Chapters 4 and 5, I will briefly return to general relativity. But for this book's purposes, it is the <u>philosophical</u> consequences of Newton's postulate of action-at-a-distance that will matter---more than its two centuries of success, followed by its demise at the hands of Einstein.

For as I shall explain in the next Section, the success of Newton's theory also contributed to the decline of the mechanical philosophers' extraordinary "cognitive optimism" about our ability to understand nature's innermost workings. Another factor in that decline was David Hume's philosophy: which, together with Newton's theory, paved the way for what is now the mainstream "modest", or "pessimistic", picture of how much humans can understand nature.

Chap 2, Section 4: Optimism about understanding nature: 'we will soon deduce the cause from the effect'

We have seen that the mechanical philosophers had a bold and ambitious vision, about understanding all the processes of nature as mechanical. Some of them, including Galileo and Descartes, were also accomplished proselytizers---one might say, propagandists---for the movement. They confidently proclaimed that detailed and successful mechanical explanations would soon be achieved---"if not tomorrow, then the next day". (And of course, they promised that the explanations would conform to their own principles, rather than some rival's favoured principles.)

But there was also another strand to the mechanical philosophers' confidence. It is about the quality of understanding that such prospective explanations were expected to provide. Crudely and metaphorically: the quality was going to be the very best. To go beyond metaphor, I need to invoke a topic that my account has so far suppressed. It is the ultimate "elephant in the room": namely, God and God's understanding of nature.

Thus the mechanical philosophers believed that God had complete insight into the innermost workings of nature; any natural process was completely "intellectually transparent" to God. So far, so unsurprising. After all, He is meant to have created the natural world. But they believed also that we humans, being made 'in the image of God' (Genesis 1:27), can hope to emulate this complete insight and understanding. Of course, we are finite creatures, and God is infinite. So we cannot hope for such understanding all at once, and for all of nature: to hope for that would be grossly hubristic. But for individual "patches" of nature, perhaps "small" ones---for example: the collisions of solid bodies moving in straight lines---we can attain an insight and understanding, as complete and intellectually transparent as God's own. (Or rather, most of the mechanical philosophers believed these claims. Of course, theological controversies abounded as much as philosophical ones; even to the extent that some of them, for example Hobbes, were accused of atheism.)

One main way in which this idea of complete understanding was made more precise was in terms of deduction. So here at last we broach the discipline of logic. In the Western tradition, Aristotle had founded the subject, mainly by classifying valid patterns of argument.

Thus recall what it means to say that an argument with premises and conclusion is valid: in other jargon, that one can deduce the conclusion from the premises, or that the premises imply, or entail, the conclusion. All these different jargons are synonymous. Namely: if all the premises are true, or (supposing them to be in fact false) if they were true, then the conclusion must be true. That is: any way in which all the premises are made true must also make the conclusion true.

In many cases, the validity of an argument turns on the placing within the premises and conclusion of words like 'all', 'some', 'none', 'and', 'or' and 'not', irrespective of the other words. In such cases, we say there is a valid pattern. An elementary Aristotelian example turns on the behaviour of the word 'all'. Consider the argument-pattern: 'Premise: all As are Bs. Premise: all Bs are Cs. Therefore, Conclusion: All As are Cs'. This pattern is evidently valid, whatever 'A', 'B' and 'C' stand for, i.e. whatever plural nouns or noun phrases ('horses', 'red things' etc.) one puts in for them.

Since medieval times, logic had been a basic part of university studies. (Along with grammar and rhetoric, the three disciplines together comprised the 'trivium', the 'three ways'). So it was natural for the mechanical philosophers to conceive the complete understanding, that they were proclaiming as imminent in their description of nature, in terms of deduction.

Besides, there is a tempting metaphor that yields an analogy between, on the one hand, the relation between premises and conclusion, and on the other, the relation between cause and effect. Namely, the metaphor of containment. Since the premises being true forces the conclusion to be true, it is natural to say that the conclusion is contained in the premises. Or rather, since the premises and conclusion are sentences, i.e. pieces of language, we should express this as: the proposition expressed by the conclusion, the <u>content</u> of the conclusion, is contained in the conjunctive proposition expressed by all premises taken together. And analogously for causation. It is natural to say that the effect is contained in the cause: at least, provided that the cause is described in sufficient detail so that all relevant factors are included.

This analogy suggests that from a sufficiently detailed description of the cause, one should be able to deduce a description of the effect: rendering the effect completely comprehensible "to the light of reason". Indeed, Descartes says just this in his famous <u>Meditations</u> (the Third Meditation). He writes: 'Now it is already clear by the light of nature that the complete efficient cause must contain at least as much as the effect of that cause. For where, pray, could the effect get its reality if not from the cause? And how could the cause supply it, without possessing it itself?'

This argument (with, of course, variations in its exact formulation) occurs frequently in the writings of Descartes and his contemporaries. As an illustrative example, one variation appeals to the idea that there can be no creation <u>ex nihilo</u>, i.e. creation out of nothing; (except of course by God, as in the creation of the world). So the effect with 'its reality' (as Descartes puts it) must somehow be latent in what occurred before: which prompts the argument above.

Thus the common theme is that over the next hill---"if not tomorrow, then the next day"---there will be a science ("mine, not that of my rivals") whose concepts and claims will be so clear to the light of reason that they do not merely command our assent, but also provide complete understanding and certain knowledge. In particular, this science will provide deductions of effects from their causes: the premises describing the causes will entail the conclusion describing the effect.

<u>Chap 2, Section 5: Lowering our sights: Hume</u>

In the eighteenth century, this optimistic view withered away. Two main reasons for this were Hume's critique of the view, and the success of Newton's theories.

Thus Hume argued (in his <u>Treatise of Human Nature</u> (1739) and his <u>Enquiry concerning Human Understanding</u> (1748)) that, whatever the concepts and claims of a successful science might turn out to be, there is no hope at all of a genuine <u>deduction</u> of effect from cause. No matter how detailed one's description of the cause, it is hopeless to aim for a deduction. For one can conceive, i.e. imagine, that the cause occurs as described, with the effect being absent. Thus two of Hume's examples are that an impact of a body, say of a billiard ball, causes another body, another ball, to move; and that bread causes nourishment. He points out that one can imagine that the impact occurs without the second ball moving away; and that I eat the bread (just as it is, in look, smell and composition) without getting nourished, but instead, say, poisoned.

To make a genuine deduction of the effect, i.e. a valid argument whose conclusion states that the effect occurs, one obviously needs an extra premise. This could be a general premise. It could be along the lines: all impacts of such-and-such a kind (including the one in question) are followed by the second ball moving. And similarly for the bread example: anyone eating a loaf of such-and-such a kind, is later nourished. Here, I say 'followed by' and 'is later', since for the aim of validly implying that the effect occurs, the extra premise need not claim a causal relation. It is enough---but essential---that it claims the effect's occurrence. Or the extra premise could be a specific one, along the lines: this impact is followed by the second ball moving; my eating this bread is followed by my being nourished.

With such an extra premise, either general or specific, there is undoubtedly a valid argument to the effect. Indeed, these arguments will illustrate the simple and familiar pattern, <u>modus ponens</u>, viz: 'Premise: P; Premise: if P then Q. Therefore, Conclusion: Q'. For we have here the pattern: 'Premise: The cause occurs; Premise: If the cause occurs then the effect occurs. Therefore, Conclusion: The effect occurs'. Here, the second premise, the if-then premise, can be either general or specific; as we have seen. And in both premises 'the cause' is to be understood, i.e. described, in sufficient detail as to warrant the second premise. The impact must be sufficiently forceful, the first ball rigid enough (not made of jelly) etc.; the bread must be made from wheat or barley or … but not from cyanide.

All this is nowadays so obvious to us that it is tempting to criticize Hume as flogging a dead horse. One thinks: 'Of course, the later effect---the second ball moving away, the person being nourished---does not follow with sheer logical necessity from the earlier state, no matter how detailed our specification of it. Only on the assumption of a suitable linkage, like the extra premises above, can there be a deduction.' Agreed: that is so. But it being so does not mean that Hume's critique was misdirected, i.e. that all Hume's predecessors acknowledged the point. As I have urged: they did not.

This point is often put in terms of the idea of rationality. For the topic of whether or not there is, in cases like the billiard balls or the bread, a deduction can be put in terms of the question: why is it rational to believe that, given a sufficiently detailed specification of the impact or of the bread, the second ball will move, or the person be nourished? Of course we all do believe this. But why? And is it rational to do so?

When we put the question in this way, the temptation to criticize Hume is very likely to be expressed as follows. 'Yes it is rational to believe these propositions: though it is not a matter of deduction, as Hume emphasizes. But Hume's emphasizing this shows that he is using an unduly narrow notion of rationality. He recognizes only deductive rationality: that is, the obligation on rational beings to believe the deductive (sheer logical) consequences of what they believe. But not everything that it is rational for us to believe follows by sheer logic (a deductively valid argument) from other propositions we already believe. In short: Hume should loosen up about what rationality requires of us.'

To which I reply, on behalf of Hume---at least, the historical Hume---as follows. Once we set aside deductive rationality---which is a notion, and an obligation, that we can surely all agree on---rationality is a contested concept, in the sense I discussed in Chapter 1. For let us ask: given some collection of propositions we already believe, what else should we believe (additional of course to the deductive consequences of the collection)? That is: what principles, additional to deduction, should govern the formation of beliefs from other beliefs, taken as evidence? That is a very difficult and multi-faceted question which philosophers, and of course scientists and statisticians, have addressed in many different ways since Hume's time. For example, some appeal to probability, some abjure it. Large bodies of theory, with specialist names like 'inductive logic', 'statistical inference' and 'causal inference', have been developed---and debated. For the jury is still out concerning how best to make this question precise (perhaps as several sub-questions); and accordingly, about what the answers are.

To which I say: 'More power to your elbow: tough work, and we should all look forward to, and value, the answers'. But what is relevant here is that Hume also, not just we moderns, can say this. He need not deny---he has no reason to deny---that there are principles about belief-formation that go beyond deductive rationality.

For his point is that his predecessors thought they did <u>not</u> need to formulate and assess any such principles. They thought their new science would need <u>only</u> deductive rationality. Thus the objection that Hume is using an unduly narrow notion of rationality, and should loosen up about what rationality demands, is doubly wrong. For first: Hume is merely examining the same deductive notion that his predecessors touted as both sufficient for science, and as promising a complete understanding of cause-effect relations. Hume shows that it is not sufficient, and does not usher in such understanding. And second: Hume can and should accept (along with the rest of us) that there are principles about non-deductive formation of beliefs. And he, like us, can investigate what they are.

<u>Chap 2, Section 6: Newton again</u>

So much by way of expounding, and defending, Hume. I turn to my second reason why the mechanical philosophers' optimism died way: why, to use the jargon above, eighteenth century natural philosophers stopped claiming that their description of an effect, or of how it came about, was 'clear to the light of reason'. This second reason is: the success of Newton's theory of gravity with its action-at-a-distance.

We saw above how radical, indeed unbelievable, Newton's theory was. The point now is that in the eighteenth century it had ever more empirical successes, so that the conclusion became inescapable: our most successful framework for quantitative empirical knowledge explicitly abjures there being any intelligible ('clear to the light of reason') causes of gravitation.

For it is not intelligible that a change in the position of the Sun would instantaneously alter the direction of pull felt by the Earth; since any process propagating from the change of position would take time to arrive at Earth. For example, a process at the speed of light would take eight minutes. But though unintelligible, this conditional proposition, 'If the Sun were to move …, the Earth's direction of pull would instantaneously alter', is a consequence of our well-confirmed theory---and we should accept it. Thus the idea of understanding the effect as necessarily connected to the cause, in particular by its being somehow contained in the cause, withers away. We must accept that the effect is just an event that invariably succeeds the cause.

This situation shows that intelligibility in the above sense is not a sine qua non, a necessary condition, of exact empirical knowledge. And it shows that, although you might want intelligibility in your scientific theory, seeking intelligibility is sometimes (i.e. at some stages in enquiry, and for some aspects of nature---here, mechanics) not fruitful.

This lowering of one's sights about what understanding of nature should involve was formulated already by Newton himself in a famous passage in the General Scholium that he added to the second edition (1713) of his 1687 masterpiece The Mathematical Principles of Natural Philosophy. The passage includes his reporting that (as I mentioned above) he had tried, but not succeeded, to understand gravity other than as action-at-a-distance. Thus he writes:

'Thus far I have explained the phenomena of the heavens and of our sea by the force of gravity, but I have not yet assigned the cause of gravity. This force must arise in any case from some cause that penetrates to the very centres of the sun and planets, without suffering the least diminution of its power . . . But hitherto I have not been able to deduce from phenomena the reason for these properties of gravity, and I do not contrive hypotheses. . . . And it is enough that gravity really exists, and acts according to the laws which we have explained, and sufficiently accounts for all the motions of the celestial bodies and of our sea.'

Newton's final words neatly sum up this discussion. His magisterial 'it is enough' (in Latin: 'satis est') lowers our sights about what to require in scientific theories: we cannot require intelligibility in the strong sense above. But it also offers the solace that even without such intelligibility, we can achieve amazing quantitative accuracy. Thus Newton resolutely sets the path of the future of physics.

Chapter 2, Section 7: Logic in the doldrums---and its revival

For this Chapter, one task remains: to describe the changing fortunes from 1600 to 1900 of the discipline of logic. In short, it went from bad times (1600 to 1850) to good (1850 to 1900). But the revival of logic in the late nineteenth century can only be understood as part of a wider change within mathematics: namely, the emergence of the distinction between applied mathematics and pure mathematics. So the next (and final) Section will be about that distinction, and its impact on the landscape of logic and philosophy.

But let us start with the earlier period, after 1600. I have said that the mechanical philosophers knew their logic, and envisaged a science in which one could validly deduce the effect from the cause. Nevertheless, it is fair to say that in their time, and more generally in the period 1600 to 1850, logic was in the doldrums. Indeed, in two senses.

First, it was generally regarded as a completed subject, in which the last word had been said. Logic was of course to be respected in any discourse. But there was no recognition that valid patterns of argument additional to those codified by Aristotle and his successors (including medieval successors) might yet be discovered and codified. (For the mechanical philosophers, this indifference was part of their rebellion against the Aristotelian tradition.) Thus in philosophical texts, the teachings of logic were sometimes summed up as the principle of 'excluded middle', i.e. 'P or not-P'; or as the principle of non-contradiction, i.e. 'not both P and not-P'. Venerable principles indeed: but there is so much more to the subject.

Second, and of course connected with the first point: philosophers in this period do not address what for us are natural philosophical questions about logic: such as what exactly is the nature of logical necessity, and exactly which propositions are indeed necessary. Their discussions of such questions often presuppose that Euclidean geometry and arithmetic are both, indeed, necessary; but what makes them necessary is not a question that is much engaged with. (I shall return to these questions shortly.)

The main qualification to what I have just said lies in the philosophy of Leibniz; (admittedly a major qualification). This is in part because Leibniz aimed to reconcile the insights of the new learning of the mechanical philosophers with the doctrines of the Aristotelian tradition. And famously, his philosophy engaged with the nature of necessity. He proposed that there is a realm of all possible worlds, and a proposition's being necessary is a matter of it being true in any, and so in all, of them. Similarly, a contingent proposition is true in some but not all worlds; and an impossible proposition is true in none. He also said that God in his omnipotence created just one world; and in his benevolence, created the best possible world---a claim later satirized devastatingly in Voltaire's Candide.

As to why logic was thus side-lined, one obvious reason was its being associated with the Aristotelian tradition, so that it became a target of the mechanical philosophers' rebellion. But another reason arises from the cognitive optimism of these philosophers. As I discussed above, they believed their forthcoming science would be certain, and "intellectually transparent": clear to the light of reason. Being convinced of achieving such certainty naturally prompted them to ignore questions about whether their science's doctrines are necessary. Besides, if such questions had been pressed, the doctrine that the science would indeed deduce an effect from a cause would prompt the confident reply, that Yes, the doctrines are necessary. For after all: to every valid argument---say: Premise: P1, Premise: P2; Therefore, Conclusion: C---there corresponds a necessary proposition, viz: 'If P1 and P2, then C'. In short: if these philosophers' optimistic programme had succeeded, the proposition saying that the effect is deducible would indeed be necessary.

The broader context of mathematical, indeed scientific, thought is that from ancient times until the mid-nineteenth century, mathematics was taken to consist of, on the one hand, the study of numbers (arithmetic, and later algebra); and on the other hand, the study of space, viz. Euclid's geometry. And both these were universally regarded as providing an absolutely certain body of knowledge, that could never be overturned.

Here, I use the single word 'mathematics' very deliberately. For no distinction was made, as we now do, between: (i) applied mathematics, which describes the physical world, objects in space and time, using mathematical concepts (numbers and geometrical concepts); and (ii) pure mathematics, which is about numbers, triangles etc. "in themselves", regardless of what is in the physical world.

From a modern philosophical viewpoint, the first thing to say about this distinction is that pure mathematics, so understood, is obviously problematic: no matter how certain its claims may be, resulting as they do from rigorous mathematical proofs. For we are physical organisms, embodied in space and time. So presumably our ideas, beliefs and knowledge originate in our experience of the physical world. But since the subject-matter of pure mathematics (numbers, triangles etc.) is not in the physical world, i.e. not located in space and time, it is then a pressing question how we come to have any ideas about that subject-matter. Besides, assuming we have such ideas: how can we come to believe, even know, propositions about this subject-matter, by following mathematical proofs?

This is a central, perhaps the central, question of the philosophy of (pure) mathematics since about 1850, i.e. after the applied/pure distinction gets articulated. I will briefly discuss this question in the next Section and the next Chapter. But to philosophers and mathematicians of the eighteenth century, this question was simply invisible. One main reason for this was that they conceived numbers in terms of lines within physical space. This conception, we will see, led to trouble.

Chapter 2, Section 8: Houses built on sand---and how to repair them

To introduce this, let us recall how we learn in school that besides the integers, positive and negative, there are, firstly, the rational numbers, where 'rational' stands for ratio or proportion. These numbers are a ratio of integers like 1/3, 2/5, 10/2 (= 5), or - 42/9. Expressed as a decimal, they either terminate, e.g. 2/5 = 4/10 = 0.4, or recur, e.g. 1/3 = 0.333… with the 3s going on forever. Here, the idea of recurring includes eventually settling down to a finite sequence of digits that then repeats forever, e.g. 137.95421372137213721372137… . But as the ancient Greeks discovered, there are also irrational numbers, e.g. the square root of 2, that cannot be expressed as a ratio of integers. When expressed as a decimal, these numbers neither terminate nor recur. The decimal expression goes on forever. But it never settles down into a repeating digit, nor even into a repeating sequence of digits. For example, the square root of 2 begins as 1.41421… but it never settles down. Another example is **pi**, defined as the ratio of the length of a circle's circumference to its diameter. It begins as 3.14159… but it never settles down. The set including all rational numbers (taken as including the integers, as in 10/2 = 5), and also all irrational numbers, is called the set of real numbers.

So for our purposes, the point here is that until the mid-nineteenth century, philosophers and mathematicians conceived of real numbers in an intuitive way, as segments of physical space. For example, they took the square root of 2 to be the diagonal of a square whose sides are of length 1. With this intuitive treatment, mathematicians produced amazing developments in the theory of real numbers and the calculus that Newton and Leibniz had invented, and in the applications of these theories within mechanics and astronomy.

But it was a house built on sand. For the intuitive treatment I have just sketched led to paradoxes. One could construct apparently valid arguments within the calculus whose conclusions were contradictions. Agreed: with talent and care, mathematicians could insulate their work from these paradoxical arguments. But they remained as discomforts, so to speak; and they prompted efforts in the nineteenth-century to make the calculus more rigorous, and thereby expunge the paradoxes.

These efforts went along with a more general movement towards rigour, and especially rigorous proof, and therefore towards formalizing and axiomatizing mathematical theories. Here, 'formalizing' means writing the theory, not in a natural language such as English or Latin (augmented of course with technical terms like 'isosceles triangle' or 'limit of a sequence'), but in an artificial language with: a precisely specified vocabulary (usually a very small one); and precise rules of grammar dictating exactly which sequences of vocabulary items count as grammatical sentences; and precise rules of inference dictating exactly which passages from finite sequences of such sentences (thought of as premises) to another sentence (thought of as conclusion) count as an allowed inference. Accordingly, 'axiomatizing' means that, having written all the claims of a theory in a formal language of this sort, one finds a small subset of these claims with the feature that any claim of the theory can be inferred from some choice of finitely many elements of the subset as premises, using only the proclaimed rules of inference. Thus the small subset are the axioms, and all the other claims are theorems. (In almost all cases, the set of axioms is not unique: even assuming a fixed formal language, there are several equally good ways to axiomatize the theory.)

This movement towards formalization and axiomatization was prompted by two other developments, additional to rigorizing the calculus.

First: new mathematical theories were proposed that surprised, even shocked, mathematicians by their subject-matters (numbers and geometrical figures) explicitly disobeying the familiar postulates and rules, that had traditionally been considered necessary. Yet these new theories seemed consistent: scrutinizing the arguments within the theories revealed no contradictions. The best-known examples of such theories are the non-Euclidean geometries. But there were also new notions of quantity different from the familiar real numbers. For example, both Hamilton and Grassmann introduced (in two different ways) theories in which multiplication of numbers was not commutative, i.e. did not obey the rule that x times y = y times x. So these new theories were, to put it mildly, unintuitive. They were hard to understand; and indeed, hard to accept as correct mathematics. To do so, mathematicians needed to adopt, and did adopt, an abstract and formal approach. The idea was: "Just follow the postulated rules, and you will see they they lead to a novel, but consistent and even elegant, geometry or algebra". And the claim of consistency, the reassurance, could be secured more easily if the theory was written in a formal language, with its precise rules of grammar, and of inference.

Here I should stress that until these developments, the only mathematical theory that had been conceived as an axiomatized theory was Euclidean geometry. And viewed by these new nineteenth-century standards of 'formal' and 'rigorous', the venerable textbook that had been used for two millennia, Euclid's Elements, was very informal and unrigorous---whether written in English or in Latin. Accordingly, mathematicians developed axiomatisations of Euclidean geometry in the modern style. Thus their formal languages had very small vocabularies. For example, the language might have just four basic predicates, such as '… is a point', '… is a line', '… lies on …' (ascribed to a point and a line), and '… is between … and …' (ascribed to three points). From a few axioms making claims, using only this tiny set of predicates, about points and lines, all the hundreds of theorems of Euclidean geometry would follow.

Second, another new theory from the late nineteenth-century led to paradox: to another house built on sand. Namely, mathematicians developed the theory of sets: and although its basic ideas are simple (e.g. the intersection of two sets of objects is the set of those objects that are in both the given sets), it led, like the calculus had done earlier, to paradoxes. Again, one could construct apparently valid arguments, with plausible premises about sets (especially about infinite sets), whose conclusions were contradictions.

Taken together, these various problems---about calculus, about the new theories of geometry and algebra, about sets---amounted to a crisis in the foundations of mathematics. In response, over the years 1870 to 1930, various different "repairs" were proposed. That is, several mathematical research programmes were launched, with distinctive proposals about how to rigorize, and thereby vindicate as free of paradoxes, all these mathematical fields: the real numbers, the new geometries and algebras, the theory of sets. Thus ensued a vigorous multi-faceted debate, that lasted some sixty years.

And in all this, the role of logic was second to none. This was not just because diagnosing the errors in paradoxical arguments is obviously a job for logic. Also, paradoxes apart, the effort to rigorize proofs was a matter of breaking them down in to simpler steps that can be explicitly checked as conforming to some announced rule of inference: clearly, a matter of logic.

Furthermore, there were deep similarities between logic and set theory. Since arguments can be about anything, and sets can be made up of any objects, both fields seem to have no specific subject-matter. In philosophical jargon, they are topic-neutral. And the truths of the two fields seem similar, or even the same. For example, recall the valid argument pattern I mentioned: 'All As are Bs, and all Bs are Cs; therefore all As are Cs'. That corresponds exactly to the truth of set theory that if a set A is a subset of a set B, and B is a subset of another set C, then A is a subset of C.

Indeed, these similarities inspired one of the research programmes mentioned above. The great German logician Frege created the logicism programme. He proposed that all of pure mathematics was really logic. And since he saw logic as consisting of necessary propositions, this explains why pure mathematics is necessary. The details of how this was meant to work depended on writing all of mathematics, e.g. arithmetic, the calculus, geometry etc., in terms of a (paradox-free) theory of sets, and then arguing that this theory of sets is really logic in disguise.

Logicism was enormously influential in philosophy from 1900 to 1930, partly through the writings of Russell and Whitehead, and later, the logical positivists. Nowadays, the consensus is that it failed: essentially because, after all, the theory of sets is not really logic. But for our purposes, what matters is the historical role of logicism, and its legacy. In this Section, we have sketched how it arose from the applied/pure mathematics distinction, and the concurrent crisis in the foundations of mathematics. In the next Chapter, we will see its legacy: placing logic, and so the nature of logical necessity, at the centre of philosophy.