



THE MULTIVERSE: A PHILOSOPHICAL INTRODUCTION

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Physics and Philosophy from 1600 to 1900

The main aim of this chapter is to review some aspects of philosophy and physics in the three centuries from about 1600 to 1900. That may seem a tall order. But we will only need to cover 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 the developments in the twentieth century that prepared the ground for the multiverse proposal. (The timeframes will vary between the three cases. For example, the relevant twentieth-century developments in logic and philosophy cover a century, from 1870 to 1970, while for cosmology, they happened in the sixty years from 1910 to 1970.)

The historical aspects up to about 1900, reviewed in this chapter, and the twentieth-century developments in the early sections of chapters 3, 4, and 5 will not just be 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. In this development, the main name will be David Hume. For physics, the chapter will mostly be about the rise of mechanics, for which the main name will be, of course, Isaac Newton.

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 into 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, Humean, 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? This question has been at the centre of twentieth-century philosophy, and we will return to it in chapter 3.

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 between their resulting answers.

Various developments from the eighteenth century onward 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 divided philosophy and science concerns 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 more details below.) As the eighteenth century went on, this success increasingly used technical notions and advanced mathematics, which, of course, led to intellectual specialization.

Agreed, great figures in physics, such as Euler, continued to write natural philosophy, as did some great figures of nineteenth-century physicists, 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, from 1700 to 1900, meant that such writings became less central to physicists' detailed research.

So much by way of a lightning-quick 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 fact that philosophy covers so many topics in addition to 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 (i.e., the theory addressing what knowledge), a self-conscious turning away from the details of physics and science in general. This occurred as part of the legacy of Kant (and so it occurred especially in German philosophy). The reason, in short, is that Kant formulated and defended 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, much of 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 drew physicists back to basic conceptual questions, like those I listed above, concerning the nature of space, time, matter, and causation. The ensuing debates in which philosophers, especially in Vienna and Berlin (which were centres of the new physics), participated had an enormous influence on philosophy. Indeed, they shaped and helped form the logical positivist movement, which 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 chapters 4 and 5) how, since 1970, the metaphysics and epistemology of physics—now referred to as “the philosophy of physics,”—has really taken off. But in this chapter, I will explore and develop three themes from natural philosophy in the earlier period, between 1650 and 1900. We will need these themes to understand our multiverse proposals, especially the first two proposals (from philosophy and 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 onwards, it became vigorous, which will lead us to chapter 3.

2. The Mechanical Philosophy

Several of the greatest natural philosophers of the seventeenth century believed that all 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” includes biological processes, such as the growth of plants. For some of these authors, it also included psychological processes, such as perception, in humans and 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 gearwheels 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.” (So here, the main meaning of “action at a distance” is causation between spatially separated bodies being unmediated by something in between them, rather than the causation being instantaneous—that is secondary.)

Proponents of this mechanical philosophy 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 among themselves. Some, such as Descartes, denied that there is a void (vacuum). They claimed 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. Yet some (including Descartes, once again) 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 up of atoms, which are separated from each other by empty space, i.e., a vacuum. (That said, 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’ view 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, never mind 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, or “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. Consider how the existence of atoms, and the way they comprise molecules, together with how they explain chemical processes, was understood only in the early twentieth century. And the understanding of biological processes, such as how muscles contract, how nerves send signals to the brain, or how genes are passed from parent to offspring, came only with the rise, in the 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 *cannot* explain all these processes. But I postpone quantum physics until 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 really common sense but was instead hard-won. So these will be my third and fourth points.

Third, the mechanical philosophers fashioned concepts to describe with quantitative accuracy the contact and collisions of bodies. These include the concepts of velocity, acceleration, mass, and momentum. Nowadays, these concepts are everyday notions: they are broadly familiar from, e.g., car travel, and are 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) That acceleration is change in velocity during a given interval of time, rather than change in velocity during traversal of a given distance. (ii) That mass is an intrinsic property of a body, different from its weight, that is a measure of its resistance to being accelerated. (iii)

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—it is still 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 that I have oversimplified. There is an elephant in the room: i.e., Newton and his theory of gravity

3. Newton's Theory of Gravity: Unbelievable?

Newton's theory *denies* 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 is utterly precise. What came to be called "Newton's universal law of gravitation" says that at any time, any two bodies attract one another with a force along the geometrical line between them at that time. Moreover, this mutual attraction is unmediated—it occurs even when there is a vacuum between them. (The law also describes how the force decreases with the distance between them, and how it depends on the bodies' masses. I will discuss these other features shortly.)

So, the law explicitly proclaims *action at a distance*: 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 through the vacuum from the Sun to the Earth. Newton's law states 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 of the pull 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 by a much larger amount—but again, instantaneously during the course of the shift. So, the important point is that the attraction is unmediated, and that the change in direction is instantaneous.

This law is yet harder to believe when one notices that it claims this instantaneous attraction at a distance occurs between *any* two bodies, for example, between an apple and a planet.

Besides, the law states that the forces of attraction are *equal in size* (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 that it does so with a force equal in size to the force exerted by the Earth.

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 the Earth's acceleration is correspondingly smaller. For according to Newton, force equals mass times acceleration. So, the equal and opposite forces make the small mass of the apple accelerate fast enough for us to observe and measure it, 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. Indeed, the acceptance was eventually so complete 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. Newton himself favoured the principle of contact action. For example, in a now-famous letter to Bentley, he wrote:

That gravity should be innate inherent and essential to matter so that one body may act upon another at a distance through a vacuum without the mediation of any thing else by and through which their action or force may be conveyed from one to another is to me so great an absurdity that I believe no

man who has in philosophical matters any competent faculty of thinking can ever fall into it.

Besides, he wrote in a scholium (i.e., a comment) added to his *Principia* that he had endeavoured to find a means, a mechanism, by which gravity acted, and had been unsuccessful. (I will return to this admission at the end of section 6.) In short, it is no wonder that initially, Newton's readers were incredulous about his theory.

But Newton also argued, with considerable justice, that as regards the gravitational force between each planet and the Sun, the detailed astronomical observations which he had in hand (especially those encapsulated in what became known as "Kepler's laws") implied that he could *deduce* that the gravitational force acted along the instantaneous line between the planet and the Sun. Furthermore, he argued that he could *deduce* 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 four, i.e., it multiplies it by a quarter; tripling the distance reduces the force by a factor of nine, 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 such predictions were tested in the decades following his theory's publication (i.e., after 1687), which turned out to be true. In time, this impressive quantitative success trumped people's doubts, in particular, their incredulity about action at a distance.

(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 about these claims.

Agreed, there were dissenting voices, such as the physicist-philosopher Ernst Mach. And calm comes before a storm. In 1915, Einstein (inspired in part by Mach's misgivings) created an amazing new theory of gravity, which he called "general relativity." According to this theory, there is no action at

a distance. Gravitational influence *does* take time to propagate across space: namely, it travels at the same speed as light. So, for my imaginary example above, in which the entire Sun is shifted by a thousand miles during the course of a minute, Einstein's general relativity says that eight minutes must elapse (from the beginning of the Sun's shift) before the direction along which the Sun pulls the Earth begins to change. For eight minutes is the travel time from the Sun to the Earth, for gravity as well as for light.

In chapters 4 and 5, I will briefly return to general relativity. But for this book's purposes, it is the *philosophical* 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.

4. Optimism about Understanding Nature: "We Will Soon Deduce the Effect from the Cause"

We have seen that the mechanical philosophers had a bold and ambitious vision: to understand all the processes of nature as mechanical. Some of them, including Galileo and Descartes, were also accomplished proselytizers—one might say, propagandists—for this 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,

God is meant to have created the natural world. But they also believed 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, including 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 founded the subject, mainly by classifying *valid patterns* of argument.

Thus, recall what it means to say that an argument with premises and a conclusion is *valid*: in other jargon, it is an argument in which 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 1: All As are Bs. Premise 2: 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 (based on Aristotle’s work) had been a basic part of university studies. (Along with grammar and rhetoric, the three disciplines together comprised the “trivium” or the “three ways.”) So, it was natural for the mechanical philosophers to conceive the complete understanding that they were proclaiming to be 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, i.e., the *content* 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 exactly this in his famous *Meditations* (in 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 *ex nihilo*, 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.

5. Lowering Our Sights: Hume

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. These reasons are the topic of this section and the next one.

Hume argued (in his *Treatise of Human Nature* (1739) and his *Enquiry Concerning Human Understanding* (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 *deduction* of effect from cause. For a deduction of a proposition E stating the effect from a proposition C stating the cause would require that *if* C is true, then it is logically impossible for E to be false. This implies that no matter how detailed one's description of the cause, it is hopeless to aim for a deduction. For it is logically possible that the cause as described occurs, with the effect being absent. In support of his claim of logical possibility, Hume takes two examples: (i) that an impact of a body, say of a billiard ball, causes another body, another ball, to move, and (ii) that bread causes nourishment. For these examples, he points out that one can conceive, i.e. 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 chemical composition) without getting nourished.

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 following lines: this impact is followed by the second ball moving; my eating this bread is followed by my being nourished.

Hume agrees that once such an extra premise, either general or specific, is supplied, there is undoubtedly a valid argument from the cause to the effect. Indeed, these arguments will illustrate the simple and familiar pattern of *modus ponens*, viz, "Premise 1: P; Premise 2: If P then Q; Conclusion: Therefore, Q." For we have here the pattern: "Premise 1: The cause occurs; Premise 2: If the cause occurs, then the effect occurs; Conclusion: Therefore, 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 to justify 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.

But without such a second premise, there is no valid deduction of the effect. The inference to the effect remains inductive. Hence, Hume’s challenge came to be called “the problem of induction.”

All this is nowadays so obvious to us that it is tempting to criticize Hume for 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 that earlier state. Only on the assumption of a suitable linkage, like the extra premises above, will 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 urged in the last section, 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 deductively valid inference to the conclusion, can be put in terms of a 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 that the person will be nourished? Of course, we all believe the effect will follow from the cause. But why? And is it rational to do so?

When we put the question 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 a deductively valid inference 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 (in addition, of course, to the deductive consequences of the collection)? That is, what principles, besides 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. (Indeed, Hume himself addresses it, in passages less well-known than his formulation of the problem of induction.) For example, some believe probability must be the central idea for answering the question, while others 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 does not deny—and he has no reason to deny—that there are principles about belief-formation that go well beyond deductive rationality.

For his main critical point remains that his predecessors thought they did *not* need to formulate and assess any such principles. They thought their new science would need *only* 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 that it does not usher in any such complete understanding. And second, Hume can and does accept (along with the rest of us) that there are principles about the non-deductive formation of beliefs. And he, like us, investigates what they are.

6. Newton Again

So much by way of expounding and defending Hume. I turn to my second reason why the mechanical philosophers’ optimism died away 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 during the course of the eighteenth century, it gathered 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 indeed, it is not intelligible that a change in the position of the Sun would, without an intervening medium, and instantaneously, alter the direction of pull felt by the Earth. For any process propagating from the change of position would surely require a medium and would take time to arrive at Earth. For example, a process at the speed of light would take eight minutes. But though unintelligible, the 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 that the effect is 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 follows 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 of enquiry, and for some aspects of nature—here, mechanics) not fruitful.

This lowering of one’s sights about what our 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 states:

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. Indeed, this force arises from some cause that penetrates as far as the centres of the sun and planets,

without any diminution of its power to act . . . I have not as yet been able to deduce from phenomena the reason for these properties of gravity, and I do not feign hypotheses. . . . And it is enough that gravity really exists, and acts according to the laws that we have set forth and is sufficient to explain all the motions of the heavenly bodies and of our sea.

Newton's final words neatly sum up this discussion. His magisterial "it is enough" (*satis est* in Latin) 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 set the path for the future of physics.

Besides, the subsequent history of physics has vindicated him, in the sense that amazing quantitative accuracy has been achieved in various fields of physics while giving up, not just intelligibility in the specific sense above, viz. requiring contact action, but also in other senses. In chapter 4, we will see examples of this. The first will be the treatment of matter by classical physics (from about 1750) as made up of point particles, i.e., masses that are not just tiny but extensionless—located at just one point of space. (So, their density, understood in the usual way as the mass per unit volume—in other words, as mass divided by volume—is infinite.) But the main example will be quantum theory, with its paradoxical combination of amazing quantitative accuracy and notorious interpretative difficulties.

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 (from 1850 onwards). 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, from 1600 to around 1850. 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, it was in the doldrums 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.

Logic was also in the doldrums in a second, and more subtle and controversial sense (which is connected with the first). In short, philosophers in this period tend not to address what for us are natural philosophical questions about logic, questions such as what exactly the nature of logical necessity is 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 exactly makes them necessary is not a question that they really explore. (I shall return to this sort of question shortly.)

I admitted that this second sense is subtle or controversial because the work of each of two great philosophers, Leibniz and Kant, prompts a qualification. 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 its 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*. Setting these theological aspects aside, chapter 3 will, of course, return to assess the idea that necessity consists of truth in all possible worlds.

The second qualification is that although Kant said little about necessity as such, he said a great deal about how a proposition could have that apparently similar and associated feature, of being *a priori*, meaning, roughly, knowable (and so true) independently of any experience. For he believed

that the propositions of both Euclidean geometry and of arithmetic are *a priori*. But he also believed they are informative about the world. (His label for this was “synthetic” (in contrast with “analytic,” which he construed as “the predicate being contained in the subject,” though his usage corresponds well to the common modern formulation of “being true in virtue of the meanings of the words.”) How a proposition could be both *a priori* and synthetic thus became his central question, which his masterpiece, *The Critique of Pure Reason* (1781), answered by claiming this is possible from the way our human cognitive constitution shapes, or imposes structure on, raw experience.

About this answer, the jury is still out. Agreed, we all accept, in everyday thought as much as technical science, that our concepts, and so what we perceive, believe, and even know, reflect contributions from the side of the subject, us, as well as from the side of the object thought about or perceived. We also accept that such contributions can be common to the species (and so presumably of biological origin), to a population (and so of historical or cultural or linguistic origin), or specific to an individual (and so idiosyncratic in a non-pejorative sense). But Kant’s conception of these contributions was less straightforward and less empirical than this. He maintained that by philosophical reflection, he could formulate the contributions as being a necessary condition of objective experience, and that he could thus justify, not just that there are some synthetic *a priori* propositions, but also that Euclidean geometry and arithmetic consist of them.

I myself think that even his first claim is wrong: there are no synthetic *a priori* propositions. (This, of course, goes with my admiration for Hume, evident in the previous sections.) But even if his first claim is right, his second claim fails. For as I will report in the next section, in the nineteenth century, mathematicians developed various non-Euclidean geometries and showed their consistency: the world, and our experience of it, *could* be described by each such geometry. (Kant also claimed that the so-called “law of causality,” that every effect has a cause, and several principles of Newtonian mechanics were synthetic *a priori*: claims that were doomed to fail with the advent of twentieth-century physics.)

But this is not the place to belabour Kant. Instead, I wanted just to record his work as prompting a qualification of my summary statement that logic was in the doldrums.

As to *why* logic was thus sidelined between 1600 and 1850, one obvious reason was its association with the Aristotelian tradition, so that it became

a target of the mechanical philosophers' rebellion. But another reason arises from the cognitive optimism of those philosophers. As I discussed in section 4, they believed their forthcoming science would be *certain* and intellectually transparent or "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 science would indeed deduce an effect from a cause would prompt the confident reply that, yes, the doctrines are necessary. After all, to every valid argument—say, P1, P2; therefore, 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 from the cause 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 algebra in the sense of equations about numbers with variables 'x' and so on, and later, the calculus), 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 from a philosophical viewpoint. For no matter how certain we may agree its claims to be, resulting as they do from rigorous mathematical proofs, there is the question: how do we come to have such knowledge? 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 of 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 around 1850, i.e., after the applied/pure distinction was 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.

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., $1/3 = 0.333\dots$ with the 3s going on forever. Here, the idea of recurrence includes eventually settling down to a finite sequence of digits that then repeats forever, e.g., $137.95421372137213721372137\dots$, where “2137” repeats indefinitely. But as the ancient Greeks discovered, there are also *irrational* numbers, such as 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 π (the Greek letter “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 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 from 1690 onwards, in 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. By the end of the nineteenth century, “formalizing” came to mean 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. This language contained a precisely specified vocabulary (usually a very small one), 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 a conclusion) count as an allowed inference. Accordingly, “axiomatizing” came to mean that, having written all the claims of a theory in a formal language of this kind, one finds a (usually 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 selected subset is 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 also prompted by two other developments, additional to the efforts at rigorizing the calculus. The first was the rise of what one might call “heterodox” theories of numbers and geometrical figures; the second was the rise of the theory of *sets*.

First, new mathematical theories were proposed that surprised, even shocked, mathematicians by the fact that their subject matters (numbers and geometrical figures) explicitly disobeyed the familiar postulates and rules that had traditionally been considered necessary for numbers and figures. 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, which were developed in the 1830s. As I mentioned in the last section, the consistency of these geometries and the fact that the world and our experience of it *could* be described by them spelled trouble for Kant's view that the propositions of Euclidean geometry are synthetic *a priori*.

In addition to the new geometries, there were also new notions of quantity different from the familiar real numbers, which obeyed strange rules. 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 $x \text{ times } y = y \text{ times } x$. So, these new theories were, to put it mildly, unintuitive. The new ideas of quantity underlying the strange rules 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 similar to that for geometry: just follow the postulated rules, and you will see they lead to a novel, but consistent and even elegant, algebra. And the claim of consistency, the reassurance, could be secured more easily if the theory were written in a formal language, with its precise rules of grammar and inference.

Here, I should stress that until these new theories were proposed, the only mathematical theory that had been conceived as an axiomatized theory was Euclidean geometry. And if one adopted these new nineteenth-century standards of *formal* and *rigorous*, then the venerable textbook that had been used for two millennia, Euclid's *Elements*, was very informal and not rigorous, whether written in English or Latin. Accordingly, mathematicians developed axiomatizations 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 "a is a point," "b is a line," "c lies on d," (ascribed to a point and a line) and "e is between f and g" (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.

The second development that motivated formalization and axiomatization arose from another new theory from the late nineteenth century, which led to paradox, to another house built on sand. Namely, mathematicians developed the theory of *sets*. Its basic ideas are simple, and nowadays familiar from school mathematics: e.g., the intersection of two sets of objects is the set of those objects that are elements of both the given sets. But considering infinite sets, i.e., sets with infinitely many elements, led, like the calculus

had done earlier, to paradoxes. Again, one could construct apparently valid arguments, with plausible premises about infinite sets, whose conclusions were contradictions.

Taken together, these various problems—about calculus, the new theories of geometry and algebra, and 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, and 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 into 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 terminology, 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 Gottlob Frege created the *logicism* programme. He proposed that all of pure mathematics was really logic. This proposal promises a ready explanation of why pure mathematics is necessary. For Frege took logic to be a body of necessarily true propositions. We saw the idea here near the end of section: It is that to every valid argument say: Premise: P1, Premise: P2; Therefore, Conclusion: C—there corresponds a necessary proposition, viz: “If P1 and P2, then C.” Thus, where Aristotle and countless logicians after him saw the study of logic as the investigation of valid arguments, Frege took it as the production of necessary truths. So, if mathematics is “just” a part of logic, so understood, it is guaranteed to be necessary.

So, Frege aimed to show that the necessary propositions of logic are the axioms and theorems of a formal axiomatic system. And from this system,

all of pure mathematics (e.g., arithmetic, calculus, geometry, etc.) could be derived merely by adding appropriate definitions of the various mathematical symbols (like numerals). This came to be called the *reduction* of mathematics to logic.

Logicism was enormously influential in philosophy from 1900 to 1930, partly through the writings of Russell and Whitehead, and later, the logical positivists. To cut a long story short, the details of this turned out to depend on writing pure mathematics in terms of a (paradox-free) theory of sets, and then arguing that this theory of sets is really logic in disguise. Broadly speaking, the first aim was achieved: all of pure mathematics could indeed be presented in terms of sets, and nowadays most textbooks proceed in this way. But Frege and the other logicians failed in their second, philosophical aim, i.e., showing that the theory of sets is really logic in disguise. Nowadays, the consensus is that, after all, the theory of sets is not really logic, and so logicism failed.

But for our purposes, what matters is not so much the failure of logicism as its historical role 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.

9. Notes and Further Reading

9.1 Primary Sources

For the topics of this chapter, the best reading list is, of course, the original masterpieces themselves. Though daunting, one should at least dip into them. For Newton, Hume, Kant, and Frege, I recommend the following editions.

NEWTON

Newton, Isaac. *The Principia: Mathematical Principles of Natural Philosophy*, trans. I. B. Cohen and A. Whitman, with a Guide by I. B. Cohen (University of California Press, 1999). In section 6 above, I quoted this translation of Newton's *General Scholium*, which is discussed in chapter 9 of Cohen's Guide.

HUME

Hume, David. *A Treatise of Human Nature* (1739) and *An Enquiry concerning Human Understanding* (1748). Both works are available in many editions. For example, the *Treatise* is published by Penguin (1969), edited by E. Mossner, and the *Enquiry* by Oxford University Press (1894 onwards), edited by L. Selby-Bigge. The passages most relevant to Hume's discussion of causation and inductive inference (discussed in section 5 above) are *Treatise*, Book I, Part I, sections 1, 4, and 5, and Part III, sections 12 and 14. For the *Enquiry*, consult sections 3–7.

KANT

Kant, Immanuel. *Critique of Pure Reason* (1781; second edition 1787), available in many English translations. The most important passages concerning Kant's claim that geometry and arithmetic consist of synthetic a priori propositions are found in the Preface, Introduction, and the Transcendental Aesthetic.

FREGE

G. Frege's logicism is best approached through *The Foundations of Arithmetic* (1884), available in several editions and translated into English by J. L. Austin (Blackwell, Oxford, 1950 onward). Frege's proposed definitions of the numbers 0, 1, 2, and the positive whole numbers are presented in Part IV, following his critique of earlier accounts of arithmetic, including Kant's. A more comprehensive selection of Frege's writings, including substantial extracts from *The Foundations of Arithmetic*, is collected in *The Frege Reader*, ed. M. Beaney (Wiley-Blackwell, 1997).

9.2 Internet Sources

Many of these works, often in authoritative editions, are available online. Newton, Hume, and Kant are particularly well served. I recommend the following resources:

- **Newton:** The Newton Project, <https://www.newtonproject.ox.ac.uk>.
- **Hume:** Hume Texts Online, <https://davidhume.org>

- **Kant:** Project Gutenberg edition of the *Critique of Pure Reason*, <https://www.gutenberg.org/files/59023/59023-h/59023-h.htm>
- **Frege:** *The Philosophical Writings of Gottlob Frege*, ed. P. Geach and M. Black (1960), available at: <https://archive.org/details/the-philosophical-writings-of-gottlob-frege>

9.3 Secondary Sources

For secondary literature, there are excellent entries on all the topics of this chapter in major online reference works.

- *The Stanford Encyclopedia of Philosophy*, mentioned in the “Notes and Further Reading” section of chapter 1. Among these excellent entries are many about natural philosophy, and even the history of physics, in its philosophical aspects. In particular, G. Smith’s entry on Newton’s *Principia* is highly recommended: <https://plato.stanford.edu/entries/newton-principia/> (last modified Fall, 2024).
- A wide-ranging and insightful survey of the role of philosophical ideas in the historical development of physics—including twentieth-century developments such as relativity and quantum theory—is provided by J. Cushing, *Philosophical Concepts in Physics* (Cambridge University Press, 1998), available at: <https://www.cambridge.org/core/books/philosophical-concepts-in-physics/F285F13FE71F225BD8BE01F754F8C2E5>
- I also recommend the British Academy’s Dawes Hicks series in the history of philosophy, including both lectures and symposia, all of which are available online: <https://www.thebritishacademy.ac.uk/events/lectures/listings/dawes-hicks-lectures-philosophy/>

For this chapter, in particular, I recommend:

- Hacking, Ian. *Leibniz and Descartes: Proof and Eternal Truths* (1973), <https://www.thebritishacademy.ac.uk/documents/2191/59p175.pdf>

- Smiley, Timothy, ed. *Mathematics and Necessity: Essays in the History of Philosophy*, containing essays by J. Bennett, M. Burnyeat, and I. Hacking, <https://www.thebritishacademy.ac.uk/publishing/proceedings-british-academy/103/>.

Finally, beyond historical questions, the justification of induction and the nature of inductive inference remain central topics in contemporary philosophy of science.

- Norton, John D. *The Material Theory of Induction* and *The Large-Scale Structure of Inductive Inference* represent the current state of the art. Both are available in the British Society for Philosophy of Science Open Access series at: <https://press.ucalgary.ca/series/bsps-open/> (last modified April 7, 2025).

9.4 Reflections and References on Three Themes

In this section, I provide some more commentary and references about three of this chapter's themes, as follows. 1. My interpretation of Hume (my sections 4 and 5). 2. The development of physics from 1600 to 1900 (my sections 1, 2, 3, and 6). 3. The nineteenth-century development of logic (sections 7 and 8).

INTERPRETATION OF HUME

In sections 4 and 5, I reported Hume's critique of the cognitive optimism of his predecessors, such as Descartes. I should add here that although my account is part of an interpretation of Hume that is widely endorsed, there is a rival interpretation.

The difference, in short, is between (i) arguing that we can know about some problematic concept X, because once we analyze X carefully, we see that there is less to know than we first thought—and so no problem; and (ii) arguing that indeed we cannot know about X, while still saying we do have the original concept X.

In reading Hume, the main example of X one needs to consider is the concept of causation. Thus, (i) becomes his view that once we analyze our concept of causation, we see that it is really the concept of the cause and effect invariably accompanying one another (simultaneously or one soon after the other). That is, the property by which we specify the cause, e.g.,

this object being bread, is invariably accompanied by the property by which we specify the effect, e.g., this object being nourishing. In Hume's famous phrase, causation is *constant conjunction*.

There is no doubt that much of Hume's writing supports this interpretation. For he maintains in general that the analysis of a concept requires tracing its origin in our experience (in his terminology: the analysis of an *idea* requires tracing the *impressions* from which it originated). And then he argues at length that our idea of causation can be traced back to our experience of constant conjunction. There is no more to it than that. We have no experience of, nor insight into, a necessary—in particular, a deductive—link from cause to effect.

So, in the bread example, not only do we believe bread causes nourishment—in ordinary language: “this piece of bread will be nourishing”—because of our previous experiences. This “because” claim is naturally understood as a surely uncontentious causal claim about our psychology. Hume is also arguing that the *content* of our belief is only that being bread and being nourishing accompany one another. There is no more to the content of the belief than that. (Again, the example can be varied without affecting the issues. The accompaniment can be either simultaneous or soon thereafter; the belief can be either about this new piece of bread, not yet tasted, or about bread in general.) In other words, we have no concept of “necessitation-in-nature”: a sort of ontological “oomph” by which the cause “forces” the effect to occur—as alleged by Hume's predecessors, such as Descartes.

As I said, this interpretation of Hume is widely endorsed. One persuasive statement of it, which makes the connection with Newton (my sections 3 and 6), and is connected with my later theme (chapter 4) of probability, is in I. Hacking's *The Emergence of Probability* (Cambridge University Press, 1975), chapter 5. For more detailed support of this interpretation of Hume, I especially recommend E. Craig's *The Mind of God and the Works of Man* (Oxford University Press, 1987), chapters 2 and 3. This book is a superb overview of philosophy from 1600 to the present day. As its title hints, it articulates two dominant philosophical themes: the first about knowledge, and the second about action. The first, from 1600 till about 1800, is that we finite creatures can, and should, aspire to know nature, or rather parts of it, with the full understanding that God enjoys for all of nature (cf. my section 4). The second, from 1800 till now, is that we (“Man”) are not passive knowers of nature, but active agents in it, imposing our will on it. The transition between these themes is, very neatly, Kant. For his doctrine that

our cognitive constitution imposes structure on raw experience (cf. my section 7) keeps the first theme's stress on knowledge but adds the idea of the human mind as active.

However, there are passages in Hume's texts that suggest the interpretation that I labelled (ii), again with X taken as the concept of causation. That is, some passages say—or seem to say—that we *do* have a concept of necessitation-in-nature, of ontological “oomph,” but that, nevertheless, we cannot know anything about how it “works.” A recent full defence of this sort of interpretation is in G. Strawson's *The Secret Connexion: Causation, Realism and David Hume* (Oxford University Press, 1989, revised edition 2014). For recent scholarly work (including assessments of Craig's and Strawson's interpretations), cf. the papers at the Hume Texts Online website mentioned above. In particular, go to <https://davidhume.org/scholarship/millican> (accessed January 13, 2026).

THE DEVELOPMENT OF PHYSICS FROM 1600 TO 1900

In treating physics from 1600 to 1900, this chapter has focused solely on mechanics, especially as applied to astronomy (sections 1, 2, 3, and 6). I shall now add some details about mechanics, mention other fields of physics, and return to the topic at the end of section 6, about physics after 1700, having forsaken intelligibility in senses additional to forsaking contact action (i.e., to requiring action at a distance, as Newton's gravity did).

Although these references focus on the history of physics, I choose them for their emphasis on philosophical issues.

- A magisterial study of theories of motion, from Aristotle to Newton and beyond, focused on the contrast between “absolute” and “relative” conceptions of motion, is Barbour, Julian. *Absolute or Relative Motion?* (Cambridge University Press, 1989). Barbour favours the relative conception, in the spirit of Ernst Mach (mentioned at the end of section 3). The book was reprinted by Oxford University Press in 2001, entitled *The Discovery of Dynamics*. It is available at <https://academic.oup.com/book/54639>
- An excellent popular book describing physics' changing conceptions of the vacuum, not just from 1600 to 1900, but also in contemporary physics, is Weatherall, James

Owen. *Void: The Strange Physics of Nothing* (Yale University Press, 2016). It provides many philosophically important references and can be found at: https://archive.org/details/voidstrangephysi0000weat_u9b4

- For details of Newton himself, two excellent biographies are Westfall, Richard S. *Never at Rest* (Cambridge University Press, 1980) and Hall, A. Rupert. *Isaac Newton: Adventurer in Thought* (Cambridge University Press, 1992). In this press' *Companion* series (usually for philosophers), Bernard, Cohen, Smith, George E. *The Cambridge Companion to Newton*, is available at <https://www.cambridge.org/core/books/cambridge-companion-to-newton/B92293E01C97D041CA42B30396E2EA22>, while Oxford University Press has Eric Schliesser, Smeenk, Chris, eds. *The Oxford Handbook of Newton*, which is available at <https://academic.oup.com/edited-volume/34749>
- Hesse, Mary. *Forces and Fields: The Concept of Action at a Distance in the History of Physics* (Philosophical Library, London, 1961) is a fine overview of the struggles from 1700 onwards to accept action at a distance, as in Newton's theory of gravity, and its gradual replacement, from 1850 onwards, by the concept of an all-pervasive field, as in Maxwell's electromagnetism concept (discussed in chapter 4). It is available at <https://archive.org/details/forcesfieldsconc0000hess>
- Harman, P. M. *Energy, Force and Matter* (Cambridge University Press, 1982) is a general history of nineteenth-century physics. It is available at: <https://www.cambridge.org/core/books/energy-force-and-matter/00A35E995E821EEF4A20A7AE1D37202F>
- Jed Z. Buchwald, Fox, Robert, eds. *The Oxford Handbook of the History of Physics* (Oxford University Press, 2013) is a fine anthology whose Parts I, II, and III cover the period from 1600 to 1900. It is available at <https://academic.oup.com/edited-volume/38638>

Section 6 ended by saying that physics after 1700 had forsaken intelligibility in senses additional to forsaking contact action (i.e., in addition to requiring action at a distance, as Newton's gravity did). I provided the example of point particles (introduced ca. 1750) and mentioned that quantum theory (chapter 4) will add more. There are two further points worth making here.

First, in the early days of quantum theory, it was much debated whether a physical theory should be visualizable (*anschaulich* in German), in the way that Schrödinger's version of quantum theory (wave mechanics) seemed to be, and Heisenberg's version (matrix mechanics) was not.

Second, recently, philosophers of science have sought a general account of intelligibility of a theory (usually, under the label "understanding"): accounts that usually do not require visualizability, nor subsume understanding as just an aspect or result of having a scientific explanation (which is chapter 5's topic).

In this trend, a good and influential book is H. de Regt's *Understanding Scientific Understanding* (Oxford University Press, 2017), which discusses nineteenth-century mechanical models, as well as Newton's action at a distance, and the failure of *anschaulichkeit* in quantum theory. It is available at: <https://academic.oup.com/book/36363>

THE NINETEENTH-CENTURY DEVELOPMENT OF LOGIC

About the nineteenth-century development of logic, and the rigour and formalization in pure mathematics (sections 7 and 8), there is an enormous literature, and excellent coverage in philosophy curricula and internet resources like *The Stanford Encyclopedia of Philosophy*. So, I will be brief (also because the next chapter will give ample references to logic). Thus, apart from Frege (above), I recommend two superb overviews and two superb specialist books.

- Kline, Morris. *Mathematical Thought from Ancient to Modern Times* (Oxford University Press, 1972), a 1200-page book now available very conveniently as three paperbacks. For our topics, chapters 36, 37, 40–43, and 51 are relevant. It is available at <https://global.oup.com/academic/product/mathematical-thought-from-ancient-to-modern-times-9780195061376?lang=en&cc=gb>

- Much more recent (and emphasizing logic rather than mathematics) is Potter, Michael. *The Rise of Analytic Philosophy, 1879–1930* (Routledge, 2020). As the dates (and the subtitle, “From Frege to Ramsey”) hint, this book also discusses not just Frege, but also Russell, Wittgenstein, and Ramsey (in roughly equal measure). It is available at: <https://www.taylorfrancis.com/books/mono/10.4324/9781315776187/rise-analytic-philosophy-1879–1930-michael-potter>
- Coffa, J. Albert. *The Semantic Tradition from Kant to Carnap: to the Vienna Station* (Cambridge University Press, 1991) is a deep study of the origins of logical positivism. (So, its details about German and Austrian philosophers apart from Frege make a good complement to the book by Potter, just above.) It is available at: <https://www.cambridge.org/core/books/semantic-tradition-from-kant-to-carnap/E448B2413A076ED2275A87C87811D419>
- For philosophers of physics, geometry provides a central “subplot” in the story of increasing rigour and formalization, from 1850 onwards. A deep study of this is Torreti, Robert. *The Philosophy of Geometry from Riemann to Poincaré* (Springer, 1978). It is available at: <https://archive.org/details/philosophyofgeom0000torr>

